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RAFAEL DE SOUSA CAMELO

HUMAN RESOURCE POLICIES IN PUBLIC EDUCATION: EMPIRICAL EVIDENCES FOR BRAZIL

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Campo de conhecimento: Economia da Educação

Orientador: Prof. Dr. Vladimir Pinheiro Ponczek

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ABSTRACT

This thesis brings three empirical exercises on human resources issues in Brazilian public schools, taking advantage of a large policy implemented in São Paulo state school system. This policy raises the wages for teachers working in poor urban schools and its assignment, based on an arbitrary cutoff on a socioeconomic index, allows the identification of causal impacts.

In sum, the three papers point that allowances policies are able to, in fact, maintain teachers in disadvantaged schools and this effect, in turn, improves students' academic performance. Besides, we also find that this policy also reduces teacher absenteeism. However, as a consequence of the policy design, there are no evidence that this allowance improves the profile of teachers allocated in those disadvantaged schools.

The first paper evaluates the impacts of this policy on teacher turnover, students' grades and teachers' profile. We find that the wage compensation reduced the turnover rate by 7.2 percentage points, which means a drop of 15% over the pre-treatment average. In a reduced form model, we also find evidence that this policy can positively impact students' performance.

The second paper further analyzes the impacts on student learning, focusing on three possible mechanisms: i) the turnover itself; ii) the quality of teachers; iii) the wage increase. Estimates show that the only channel through which this compensatory policy affects students' performance is the reduction in teacher turnover. By reducing turnover rate in one standard deviation, the policy reduced the proportion of low performance students in about 50% of a standard deviation.

The third paper evaluates how the wage differentiation created by this policy affects teacher absenteeism. Results show that, after controlling for teachers' and schools' fixed effects, paying a higher wage (on average a raise of 26%) causes a drop in teachers' absent days of 8-22%. Absences that do not lead to salary discount, like for medical leaves, don't respond to the wage differentiation and the impact is larger for teachers that receive a higher incentive.

RESUMO

Esta tese traz três exercícios empíricos sobre questões de recursos humanos em escolas públicas brasileiras, aproveitando-se de uma ampla política implantada na rede estadual de São Paulo. Esta política aumenta os salários para os professores que trabalham em escolas urbanas pobres e sua regra de alocação, baseada em um corte arbitrário em um índice socioeconômico, permite a identificação de impactos causais.

Em resumo, os três artigos apontam que políticas de subsídios são capazes de, de fato, manter professores nas escolas mais pobres e este efeito, por sua vez, melhora o desempenho acadêmico dos alunos. Além disso, concluímos também que esta política também reduz o absenteísmo dos professores. No entanto, como consequência do desenho dessa política, não há evidências de que o subsídio melhora o perfil dos professores alocados nessas escolas.

O primeiro artigo avalia os impactos dessa política sobre a rotatividade dos professores. Concluímos que a compensação salarial reduziu a taxa de rotatividade em 7,2 pontos percentuais, o que significa uma queda de 15% sobre a média pré-tratamento. Em um modelo em forma reduzida, encontramos também evidências de que esta política pode impactar positivamente o desempenho dos alunos.

O segundo artigo analisa os impactos sobre a aprendizagem dos alunos, com foco em três possíveis mecanismos: i) a rotatividade; ii) a qualidade dos professores; iii) o aumento do salário. As estimativas mostram que o único canal através do qual esta política compensatória afeta o desempenho dos alunos é a redução da rotatividade dos professores. Ao reduzir taxa de volume de negócios em um desvio-padrão, a política reduziu a proporção de alunos de baixo desempenho em cerca de meio desvio-padrão.

O terceiro artigo avalia como a diferenciação salarial criada por esta política afeta absenteísmo dos professores. Os resultados mostram que, após controlar efeitos fixos de professores e escolas, pagar um salário mais elevado (em média 26% a mais) provoca uma queda de 8-22% nas faltas dos professores. Ausências que não levam a desconto de salário, como por licenças médicas, não respondem à diferenciação salarial e o impacto é maior para os professores que recebem maior incentivo.

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Chapter 1

Teacher Turnover and Financial Incentives in High-Poverty Schools: Evidence from a Compensatory Policy in Brazil

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Abstract

This chapter evaluates the impacts of a wage compensation policy, which provides a 28% wage premium to teachers at disadvantaged schools, on teachers' turnover, students' grades and teachers' profile in the largest Brazilian public school system (São Paulo). Treated schools are assigned by an arbitrary rule based on a socioeconomic index and a discontinuity at the probability of treatment provides the source of exogenous variation that allows the identification of a local causal effect. We find that the wage compensation reduced the turnover rate by 7.2 percentage points, which means a drop of 15% over the pre-treatment average. In a reduced form model, we also find evidence that this policy can positively impact students' performance. We could not find evidence that the intervention attracted or retained more tenured teachers in treated schools. All results are robust to variations in model specification and placebo tests.

Keywords: teacher labor markets, turnover rates, compensating differential.

JEL codes: I28, J38, J45.

1. Introduction

Teachers are one of the most relevant inputs for education, so human resource issue regarding teachers, such as attraction and retention, shall have important influence on educational results. For instance, turnover of teachers can be costly for students in terms of academic performance (Ronfeldt, Loeb and Wyckoff, 2013; Boyd et al, 2008), as it breaks the routine of classes and substitute teachers are, in general, less experienced (Hanushek et al, 2005; Clotfelter et al, 2008) and less effective (Boyd et al, 2008; Hanushek, Kain and Rivkin, 2004).

When deciding in which school to teach, teachers have preferences on factors such as students' profile (Hanushek, Kain and Rivkin, 2004; Boyd et al, 2008; Scafidi, Sjoquist and Stinebrickner, 2007; Kasmirski, 2012) and school's location (Boyd et al, 2008).

In general, these preferences tend to disadvantage the poorest and the most remote schools (Loeb, Beteille and Kalogrides, 2012; Boyd et al, 2008). Schools with underperformance students are the least likely to attract and retain teachers (Hanushek et al, 2005; Hanushek, Kain and Rivkin, 2004; Ronfeldt, Loeb and Wyckoff, 2013).

The literature shows that changes in human resource management practices that foster teachers' professional development and rationalize the teacher-school matching can reduce turnover (Loeb, Beteille and Kalogrides, 2012). Besides micromanagement practices, another way to reduce teacher turnover is through compensatory policies, which seek to create a compensating differential for teachers working in schools with harsh environments.

The fundamental idea behind this kind of policy is that teachers, beyond the preferences over schools' attributes, also respond to monetary stimulus. In fact, Hanushek, Kain and Rivkin (2004) show that teachers' turnover in Texas is strongly associated with both the profile of the students and the wage gap between schools, but the wage differences do not offset those other negative effects. Kasmirski (2012) comes to similar results in Brazil: the turnover is correlated to both the students' profile and the wage policies, such as bonuses for performance.

Despite the evidence of correlation between salary differentials and turnover, there is still little evidence about the causal impacts of wage compensation policies on teacher retention. Clotfelter et al (2008) use hazard models to evaluate the impact of a wage premium for teachers in underperforming schools of North Carolina and conclude that the reduction in average turnover was 12% and the impact was higher for the most experienced teachers. Pugatch and Schroeder (2014) offer evidence of impacts from a hardship allowance for primary school teachers in Gambia. They use a geographic

discontinuity in the policy's implementation to find that the intervention raised the share of qualified teachers in remote schools.

This chapter evaluates a wage compensation policy in the context of a large public school system in Brazil (the São Paulo's state school system), which serves over 4 million students with 200 thousand teachers. Teachers allocated in disadvantaged schools receive a wage premium (varying from 19% to 34%) and those schools are chosen by an arbitrary rule based on a socioeconomic index. This provides a regression-discontinuity design that allows us to estimate the causal impact of this policy.

We conclude, from our benchmark models, that an extra payment reduces teachers' turnover rate by up to 7.2 percentage points, which means a drop of 15% over the pre-treatment average. Results are robust to parametric and nonparametric specifications. In a reduced form model, we also find evidence that this policy can potentially impact students' performance. However, we have no evidence that this intervention attracted or retained better quality teachers in treated schools.

The chapter proceeds as follows. The next section describes the main institutional rules for hiring and allocating teachers in São Paulo's state schools. Section 3 describes the database, section 4 the compensation wage policy and section 5 the identification strategy. Then, the following sections bring the results and robustness test. Finally, last section resumes the conclusions.

2. Institutional background

The state of São Paulo has the largest public education system in Brazil, with over 4.2 million students and about 200,000 teachers in 5,600 schools, including elementary, middle and high school. As in any public school system in Brazil, teachers' career is governed by a wide set of rules defined, mostly, by the central administrative body, the State Department of Education (*Secretaria Estadual de Educacao* – SEE).

Those teachers can be divided into two broad functional categories, according to the type of employment contract: about 54% of teachers have permanent contracts and the remaining has fixed-term contracts (they are usually called temporary teachers). Teachers of the first group, as most of the civil servants in Brazil, are admitted by a public tender and have a permanent contract with job stability guaranteed by law[1]. Those with fixed-term contacts are admitted by a simpler selection process, but are contracted for a period

[1] Job stability is a very common characteristic of civil servants' contracts in Brazil. It means that the employee has the right to remain in the job even against the will of the employer and can only be dismissed for cause.

12 months and do not have the same stability neither the benefits of the first group[2]. Usually, these teachers are contracted to fill vacant classes or replace permanent teachers on leave. After the term, those teachers may be contracted again in the next year, but not necessarily.

Both types of teachers are also different in terms of mobility among schools. The allocation of teachers in schools and classes is a process that takes place annually, before the begging of the school year (in February) and, in general, follows like this. First, permanent teachers choose their schools and classes until they fulfil the contracted workload[3].

Then, the temporary teachers choose among classes unfilled by the permanent ones. Among the permanent teacher, as well among the temporaries, the priority for choosing the schools is given by a scoring system based on teachers' tenure and certification. It is important to underline that this process is fully centralized by SEE and depends solely on teachers' preferences and attributes, so that, principals have little power to interfere.

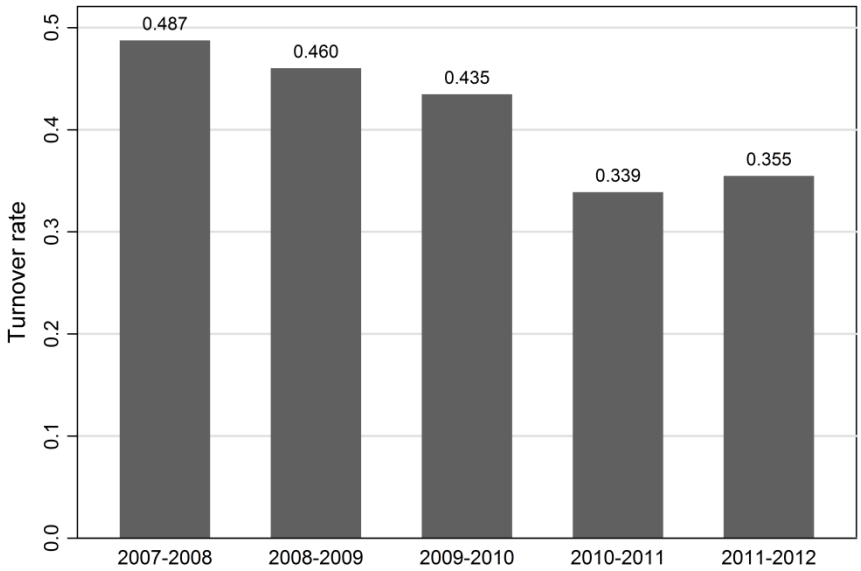
Therefore, by these rules, one can imagine that the turnover of teachers in São Paulo's public school system has two profiles. For the most experienced permanent teachers the turnover decision is mostly voluntary, given by their preferences. In contrast, the less experienced temporary teachers may change schools involuntarily, because they can eventually lose their classes if a permanent teacher (or a higher priority temporary teacher) decides to take them.

Graph 1 shows how the turnover rates behave in this context. The turnover rate throughout the chapter is measured as the proportion of teachers replaced in a school between two years. That is, on average, São Paulo's state schools switched, between 2007 and 2008, 48.7% of their teachers. This rate has been dropping since 2007 and in 2011-2012 it reached 35.5%.

This continuous drop may be associated to the introduction of policies to incentivize the stability of teachers in schools. Since 2008, in order to be eligible to the performance bonus teachers must remain in the same school the whole year. Also in 2008, teachers in the most disadvantage schools (about 25% of all São Paulo state schools) started to receive a wage premium for permanence. In 2009, teachers' length of stay in the same school became an eliminatory criterion for promotion.

[2] Benefits such as retirement with full salary, vacation, allowances among others.

[3] The contracted workload may be of 40, 30, 24 or 12 hours per week (including teaching and office hours).



Graph 1 – Average school turnover rate

3. Data

The turnover in a schools s between years t and $t+1$ here is calculated by the following formula: $Turn_{s,t,t+1} = \min[leavers_{s,t+1}, arrivers_{s,t+1}]/teachers_{s,t}$. Where the base $teachers_{s,t}$ is the number of teachers in schools s at the beginning of year t, $leavers_{s,t+1}$ is the total of teachers that moved from school s in $t+1$ and $arrivers_{s,t+1}$ are the ones that moved to school s in $t+1$.

The result of this formula is interpreted as the rate of teaches that left school s between t and $t+1$. The minimum function at the numerator just adjusts the measure for the creation or destruction of classes. For instance, if $leavers > arrivers$ it means that school s is closing classes, so part of the turnover is not due to teachers' decision.

This variable is calculated using the Brazilian School Census, which surveys all public and private schools, collecting information on infrastructure, students and teachers. Since 2007, this database allows the identification of teachers across schools and time. So one can map where each teacher works and to which schools she moves every year.

The Schools Census does not collect much personal and background information on teachers and students, so we also use specific database from State Department of Education.

The databases of the State School Performance Assessment System (*Saresp*) bring information on performance in Language and Math exams for students at 5th, 7th and 9th

grades of elementary school and 3rd grade of high school. The assessment also surveys those students, collecting socioeconomic data such as age, gender, retention, parents' education.

These exams have different scales for each grade, so in order to have a unique measure of performance for the whole school we use the proportion of students below the basic level of proficiency, which is a standardized level of achievement for all grades. In that sense, the performance of students in a school is measured as the proportion of low performers in that school, covering all grades.

Teachers' professional data, like type of contract, workload, tenure etc. come from the State Department of Education as well as the information on which school take part of the compensatory policy. The IPVS data, including its discrete version and schools' average index are provided by the State Statistical Bureau.

4. The Compensation policy

The Workplace Allowance (in Portuguese *Adicional por Local de Exercício*, so-called ALE) is a policy for reducing the turnover of school staff (including all teachers, coordinators, principals and support staff) applied to schools located in poor urban areas[4].

The policy consists in paying monthly allowance for every worker in eligible schools. All teachers working, even part-time, in those schools receive this wage increase, no matter their tenure, type of contract or position in career. These characteristics only determine the size of the increase, which ranges from 19% to 34% of their basic wage[5].

School's location is the only eligibility criterion. The policy applies to schools in the 39 cities of the Metropolitan Area of São Paulo and in other 14 large municipalities (over 300,000 inhabitants). Inside these municipalities, the policy aims schools in the poorest areas, defined by a socioeconomic index called IPVS (*Índice Paulista de Vulnerabilidade Social*).

IPVS is calculated by the state statistical bureau (*Fundação Seade*) and it intends to classify each census tract in the state of São Paulo according to a degree of social vulnerability. The index is composed of socioeconomic variables, such as household income, characteristics of household's head, family composition etc., and is calculated to each of the 50,000 census tracts. The indicator has a discrete scale ranging from 1 (no vulnerability) to 6 (very high vulnerability).

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[4] All schools in rural areas are also part of his policy, but since they do not have a suitable control group, they won't be analysed.

[5] Actually, the montly compensation values range from 135 to 450 BRL (81 to 268 USD of 2008), depending on the contracted workload. See Chapter 2 for more details.

Table 1 – IPVS scale and its respective vulnerability group

IPVS scale	Associated social vulnerability group
1	No vulnerability
2	Very low vulnerability
3	Low vulnerability
4	Median vulnerability
5	High vulnerability
6	Very high vulnerability

This scale was constructed by clustering procedures, that is, it just empirically groups similar census areas and the categories' names are merely judgmental, which means that there's no theoretical reason for considering an area 'no vulnerable' or 'highly vulnerable'.

Using this index, each state public school also gets its own vulnerability indicator, calculated as the average IPVS of the census tracts within 300 meters around the school's location. The map in Figure 1 illustrates how those areas around schools are defined.

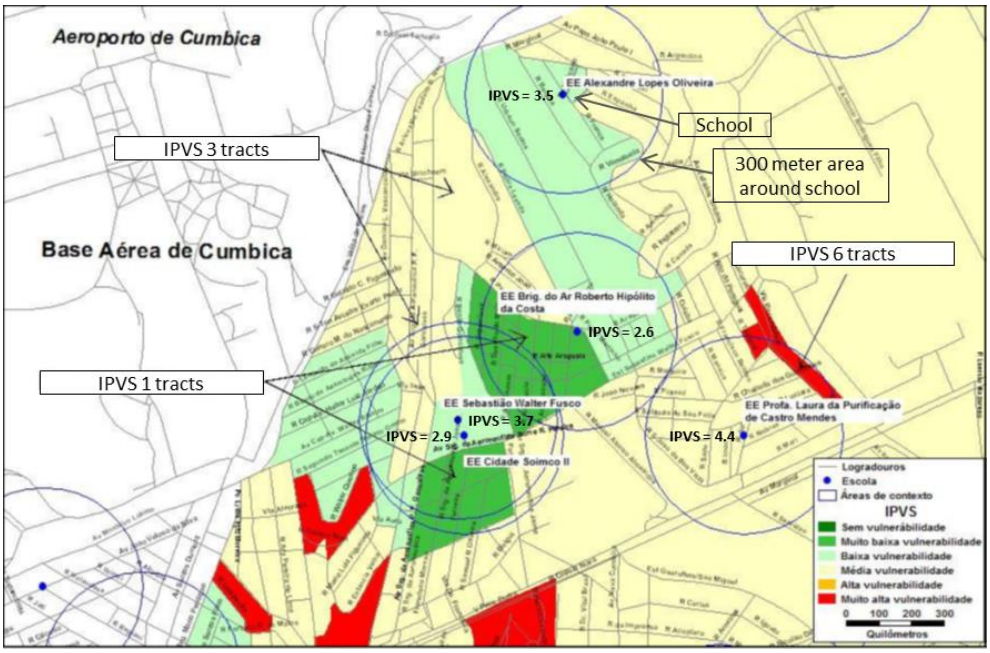


Figure 1 – Map containing census tracts, schools and their respective IPVS values

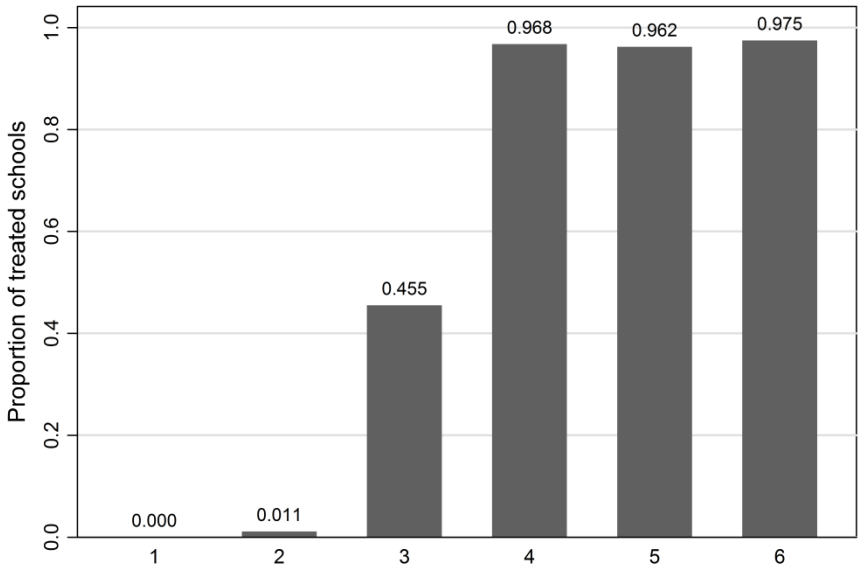
This map shows part of the municipality of Guarulhos. The green shaded areas are low vulnerability tracts, the yellow ones have medium IPVS and the red ones have the highest value. The blue spots represent schools and blue circle correspond to the 300 meter area around them. The map also shows each school's IPVS, which is the average of the discrete indexes inside the blue circle.

This average index was the criterion for the State Department of Education to decide which schools would take part of ALE. Graph 2 shows that the SEE included barely all schools with $IPVS \geq 4$ (the poorest ones), but also some schools below this threshold, generating a fuzzy assignment for schools with IPVS from 3 to 4 (low vulnerability ones).

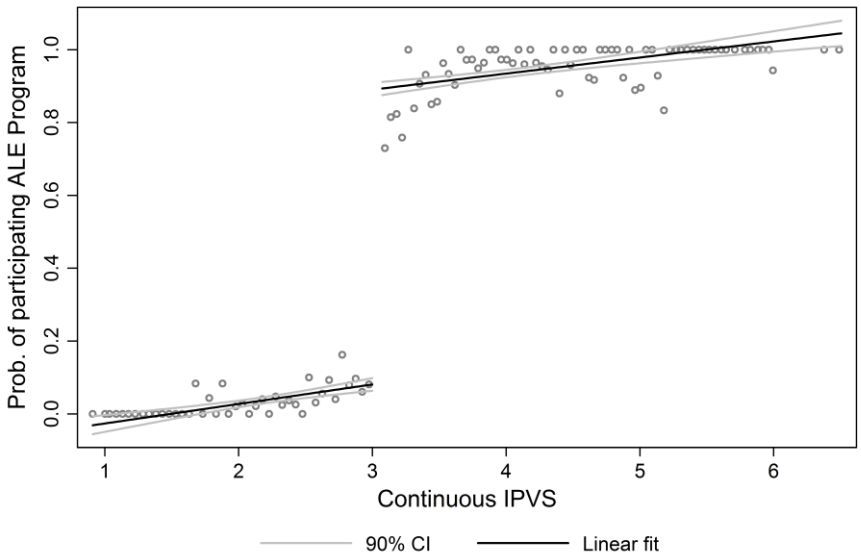
Although the statistical bureau considers schools with $IPVS \geq 4$ as the most vulnerable, the education office decided that the some other ones should be included as well. The reasons for that vary: pressure from school principals, local politics or even the belief of SEE technicians that some specific schools deserve to be included or excluded. We do not have any control on those reasons.

One can also see that almost no schools were included below $IPVS = 3$, which indicates that this value must be an important selection criterion. This is also confirmed by Graph 3, which shows the probability of treatment according to a continuous version of the IPVS. It shows a significant jump in this probability at $IPVS = 3$.

Table 2 shows that about half of the eligible schools receive ALE benefits and these schools have characteristics typically related to a higher turnover rate, such as higher proportion of low performing students, parents with lower education and less experienced teachers. Although these variables are not part of the selection criteria they are, as expected, correlated to the socioeconomic index of school's neighborhood.



Graph 2 – Treatment assignment by IPVS groups



Graph 3 – Treatment assignment by continuous IPVS

Table 2 – Summary statistics of pre-treatment characteristics

	Non-ALE schools	ALE schools	P-value of t-test
Turnover rate 2007-2008	0.467	0.527	0.000
Prop. of low performance students - Reading	0.229	0.310	0.000

Prop. of low performance students - Math	0.507	0.599	0.000
Prop. of male students	0.514	0.516	0.227
Prop. of retained students	0.113	0.137	0.000
Prop. of mothers with HS degree	0.479	0.328	0.000
Prop. of fathers with HS degree	0.499	0.346	0.000
Avg. teachers' age	41.43	39.21	0.000
Avg. teachers' tenure	13.39	11.94	0.000
Prop. of permanent teachers	0.730	0.721	0.011
Avg. teachers' workload	23.31	23.71	0.000
N. of schools	1,324	1,422	

5. Identification strategy

The policy assignment gives rise to a Fuzzy Regression Discontinuity Design. By this set, some schools (but not all) are induced to participate in ALE by the continuous IPVS, so it is possible to identify the following local treatment effect parameter:

$$\tau_{FRDD} = \frac{\lim_{x \downarrow c} E[Y \mid X = x] - \lim_{x \uparrow c} E[Y \mid X = x]}{\lim_{x \downarrow c} E[W \mid X = x] - \lim_{x \uparrow c} E[W \mid X = x]}$$

In which, Y is an outcome variable, X is the running variable (school's IPVS) and W is the treatment (binary) variable, defined as a probabilistic function of $W(X)$. In order to identify this estimand as a causal impact of treatment, we must assume that both $E[Y(0) \mid X = x]$ and $E[Y(1) \mid X = x]$ are continuous functions at $X = x$ (Hanh, Todd and Van Der Klaauw, 2001). Besides, this estimand gives a local causal treatment effect only for the complier schools (similar to the instrumental variable approach in Angrist, Imbens and Rubin (1996)), that is:

$$\tau_{FRDD} = E[Y_i(1) - Y_i(0) | \text{unit } i \text{ is a complier and } X_i = c]$$

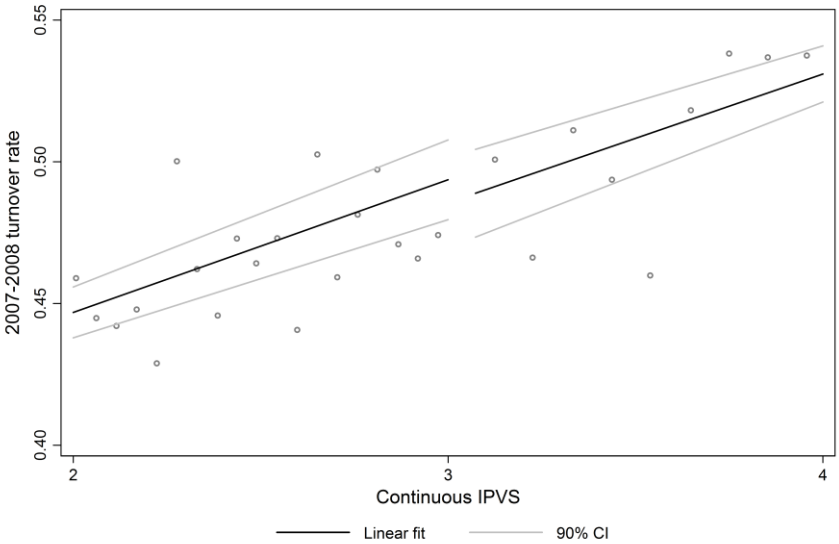
Where the i school is a complier if: $lim_{x \downarrow X_i} W_i(x) = 0$ and $lim_{x \uparrow X_i} W_i(x) = 1$.

As a benchmark, we estimate the interest parameter using the following linear models in an interval around $X = 3{,}0$:

$$Y_i = \beta_0 + \beta_1 X_i + \beta_3 W_i + u_i \quad (1)$$
$$W_i = \alpha_0 + \alpha_1 X_i + \alpha_3 Z_i + e_i \quad (2)$$

Where $Z_i = \mathbb{I}(X_i \geq 3{,}0)$ is the instrumental variable in this set. In this case, τ_{FRDD} is consistently estimated by: $\hat{\tau}_{FRDD} = \hat{\beta}_3/\hat{\alpha}_3$. As robustness check, we also ran a quadratic version of this set and also non-parametric models.

In order to generate arguments in favor of the continuity assumption we test whether treatment and control schools were similar in pre-treatment period around the cutoff point. Graph 4 and Table 3, along with Graphs A1 and A2 in the Appendix, show no evidence of significant differences in pre-treatment turnover rates and students' and teachers" average characteristics.



Graph 4 – Linear fit of pre-treatment turnover rate by continuous IPVS

Table 3 – Estimates of differences in pre-treatment characteristics

	Turnover rate	Performance in Reading	Performance in Math	Students Gender	Students Retention	Mothers' education
W (treatment dummy)	-0.012	-0.011	-0.024	-0.007	-0.008	-0.003
	(0.020)	(0.016)	(0.023)	(0.007)	(0.009)	(0.016)
X (continuous	0.048***	0.053***	0.071***	0.006	0.019***	-0.088***

IPVS)	(0.013)	(0.010)	(0.015)	(0.005)	(0.006)	(0.011)
Constant	0.351***	0.113***	0.350***	0.501***	0.068***	0.674***
	(0.031)	(0.023)	(0.034)	(0.011)	(0.013)	(0.025)
N. of schools	1,720	1,709	1,709	1,709	1,709	1,709

	Fathers' education	Teachers' age	Teachers' tenure	Temporary teachers	Teachers workload
W (treatment dummy)	-0.006	-0.273	-0.403	0.002	0.583
	(0.016)	(0.387)	(0.376)	(0.010)	(0.362)
X (continuous IPVS)	-0.086***	-1.070***	-0.555**	0.006	-0.212
	(0.011)	(0.253)	(0.242)	(0.007)	(0.236)
Constant	0.693***	43.612***	14.548***	0.167***	23.884***
	(0.025)	(0.591)	(0.561)	(0.016)	(0.546)
N. of schools	1,709	1,720	1,720	1,720	1,720

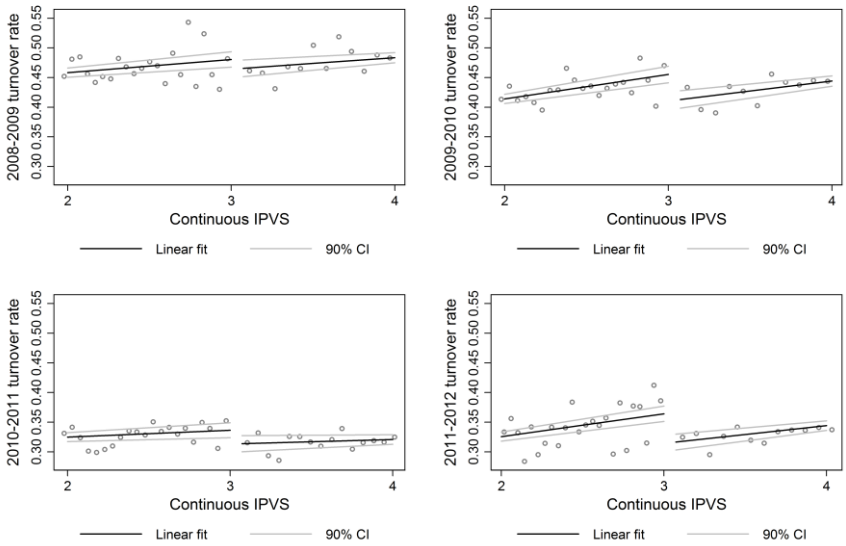
Results from the second stage estimation of equation (1), using (2) as first stage. Sample: schools with cont. IPVS = [2.0,4.0]. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

5. Results

In this chapter we are evaluating the impacts of ALE in three outcomes. First, we evaluate the impacts on school turnover, which is the aim of the policy. Also, we verify whether ALE had reduced form impacts on students' performance, as the literature points that teacher turnover might influence students' grades. Finally, we test whether ALE impacted the profile of teaching staff, that is, what kind of teachers this compensation policy attracted to treated schools.

Graph 5 and Table 4 show the impacts of ALE on schools' turnover rates during the first four years of policy. Although the policy started in 2008, teachers could only react to it in the next year, during the class allocation period. As one can see, the policy did not impact the turnover rate of treated schools in the first year, but it did in the following period. The compensation wage brought by ALE reduced the turnover rate in 2009-2010 by 6.4 p.p. In 2010-2011, the impact was lower (3.3 p.p.) but still significant and it was higher in 2011-2012 (7.2 p.p.).

Considering that the average turnover rate for schools with IPVS between 3.0 and 4.0 before the policy was 48.2%, ALE's premium was responsible for reducing school turnover in 13.3% in its first year.



Graph 5 – Linear fit of post-treatment turnover rates by continuous IPVS

Table 4 – Estimates of the impact of ALE on school turnover rates

	2008-2009	2009-2010	2010-2011	2011-2012
W (treatment dummy)	-0.024	-0.064***	-0.033*	-0.072***
	(0.018)	(0.020)	(0.018)	(0.018)
X (continuous IPVS)	0.024**	0.047***	0.015	0.045***
	(0.012)	(0.013)	(0.012)	(0.012)
Constant	0.410***	0.320***	0.296***	0.236***
	(0.028)	(0.029)	(0.027)	(0.027)
N. of schools	1,720	1,720	1,720	1,720

Results from the second stage estimation of equation (1), using (2) as first stage. Sample: schools with cont. IPVS = [2.0,4.0]. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5 (and Graph A3) brings estimates of the impact of ALE on students' performance, measured as the proportion of students with low performance in Reading and Math on the state assessment. It is hard to believe that ALE has a direct impact on students, as it is aimed exclusively to teachers, but one can suppose that raises in teachers'

salaries may impact students indirectly, via higher retention of teachers, attraction of better ones or even improvements in their motivation. So, one can consider that the results correspond to a reduced form of the impact of ALE on students.

Evidence show that ALE has a positive effect on students' performance, as it reduces the proportion of low performance students in between 2.8 and 4.5 p.p. This effect is detected in all year, with higher significant levels in 2009, 2011 and 2012.

Finally, we investigate whether ALE impacted the profile of teaching staff of treated schools. Table 6 (and Graph A4) shows that, in 2009, treated schools had a lower proportion of temporary teachers (about 2 p.p. less) and their teachers allocated, on average, 1 more hour per week.

Table 5 – Estimates of the impact of ALE on students' performance

	2009		2010	
	Reading	Math	Reading	Math
W (assignement to ALE)	-0.029**	-0.030	-0.013	-0.023
	(0.013)	(0.020)	(0.017)	(0.021)
X (continuous IPVS)	0.064***	0.059***	0.054***	0.055***
	(0.008)	(0.013)	(0.011)	(0.013)
Constant	0.068***	0.219***	0.134***	0.249***
	(0.019)	(0.029)	(0.025)	(0.031)
N. of schools	1,718	1,718	1,717	1,717
	2011		2012	
	Reading	Math	Reading	Math
W (treatment dummy)	-0.029*	-0.037*	-0.028*	-0.045**
	(0.017)	(0.021)	(0.017)	(0.022)
X (continuous IPVS)	0.058***	0.060***	0.061***	0.073***
	(0.011)	(0.014)	(0.011)	(0.014)
Constant	0.119***	0.232***	0.105***	0.209***
	(0.025)	(0.032)	(0.025)	(0.032)
N. of schools	1,716	1,716	1,714	1,714

Results from the second stage estimation of equation (1), using (2) as first stage. Sample: schools with cont. IPVS = [2.0,4.0]. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Because we cannot find any impact on teachers' tenure, it is hard to infer anything about the quality of the teachers that remain in treated schools. So far we can only say that ALE attracted more permanent contract teachers – not surprisingly, as they have highest

priority in choosing where to teach – and they tend to rise the time allocated in treated schools – in order to maximize the gains.

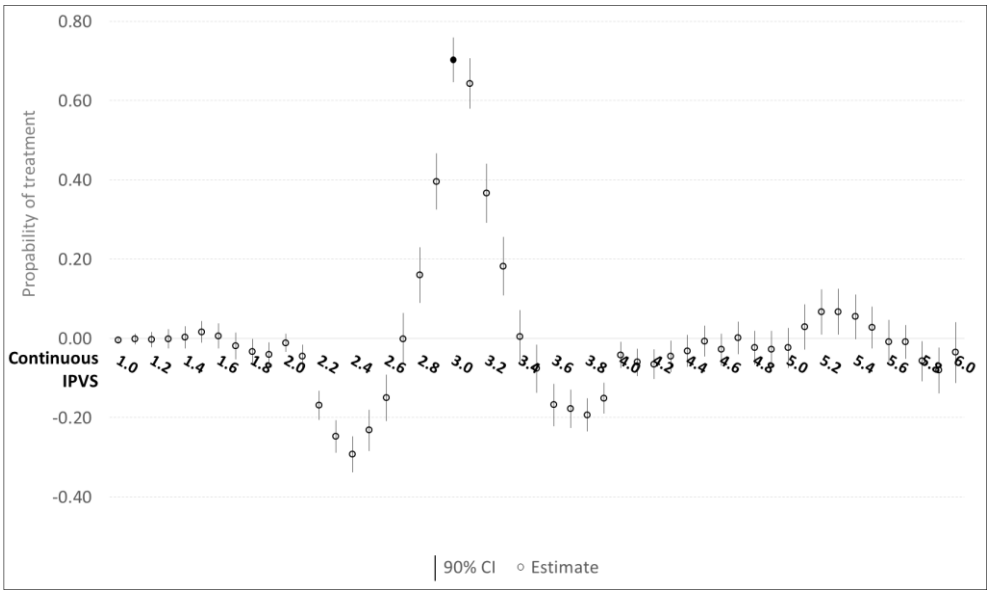
Table 6 – Estimates of the impact of ALE on teachers' characteristics

	Age	Tenure	Temporary	Workload
W (treatment dummy)	0.419	0.247	-0.021**	0.939**
	(0.389)	(0.381)	(0.010)	(0.406)
X (continuous IPVS)	-1.416***	-0.848***	0.016**	-0.140
	(0.251)	(0.245)	(0.007)	(0.266)
Constant	45.496***	16.542***	0.120***	22.163***
	(0.587)	(0.568)	(0.015)	(0.620)
N. of schools	1,720	1,720	1,720	1,720

Results from the second stage estimation of equation (1), using (2) as first stage. Sample: schools with cont. IPVS = [2.0,4.0]. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

6. Robustness tests

In order to test the robustness of the fuzzy design Graph 6 shows estimates for the jump on the probability, using linear parametric modeling, of treatment at a series of IPVS values, along with their confidence intervals. As one can see, the IPVS interval between 2.8 and 3.2 concentrates the positive shifts on the probability of treatment. These placebo tests confirms that this region is a valid fuzzy discontinuity set and that $IPVS = 3.0$ is the most relevant jump on treatment assignment, which reinforce the choice of this cutoff.

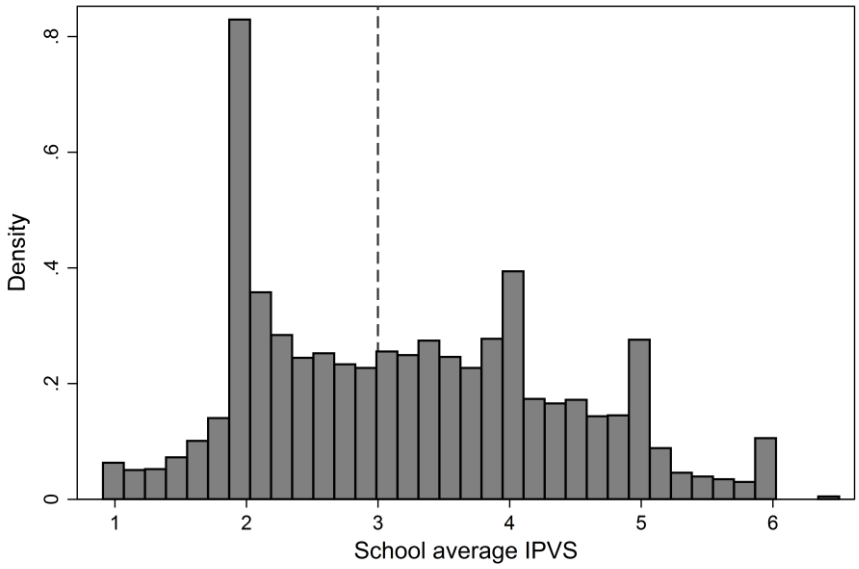


Graph 6 – Estimates and confidence intervals for discontinuities at probability of treatment by IPVS values

The manipulation of the running variable is also a concern for RD designs. In our case, one can suppose that schools would have incentives to inflate their own IPVS in order to receive ALE's wage premium.

However, that's an implausible concern. The State Statistical Bureau (full responsible for the index) is independent of the State Department of Education and therefore has no relationship with schools. In addition, IPVS consists of variables out of schools' control and the index was calculated in 2002, six years before its adoption as a criterion for ALE.

To demonstrate this, Graph 7 is the histogram for school average IPVS and shows no visual evidence of manipulation. Besides, we ran a manipulation test, proposed by Cattaneo, Jansson and Ma (2015). Results (in Table A6) show no evidence of differences in the density of running variable at the cutoff.



Graph 7 - Histogram for school IPVS

Finally, estimates are submitted to robustness tests on variations of econometric specification. Tables A7 to A14 in the Appendix reproduce the main estimates using a quadratic form of equations (1) and (2) and a nonparametric approach, following the methods of bandwidth selection and inference proposed in Calonico, Cattaneo and Titiunik (2014).

One can see that estimates are robust to these chances in model specification. Impacts on turnover are always negative and significant and results on student achievement also point to a reduction in low performers, although with less precision.

7. Conclusion

This chapter evaluates the impacts of a wage compensation policy intended to reduce teacher turnover in the largest Brazilian public school system. Teachers allocated in disadvantaged schools receive a wage premium and beneficiary schools are chosen by an arbitrary rule based on a socioeconomic index beyond their control. This provides a regression-discontinuity design that allows us to estimate the causal impact of this policy.

We conclude, from our benchmark models, that this extra payment reduces teachers' turnover rate by up to 7.2 percentage points, which means a drop of 15% of the pre-treatment average. Besides, we find that this policy can positively impact students' performance. On Chapter 2 we intend to investigate some mechanisms that can explain this effect.

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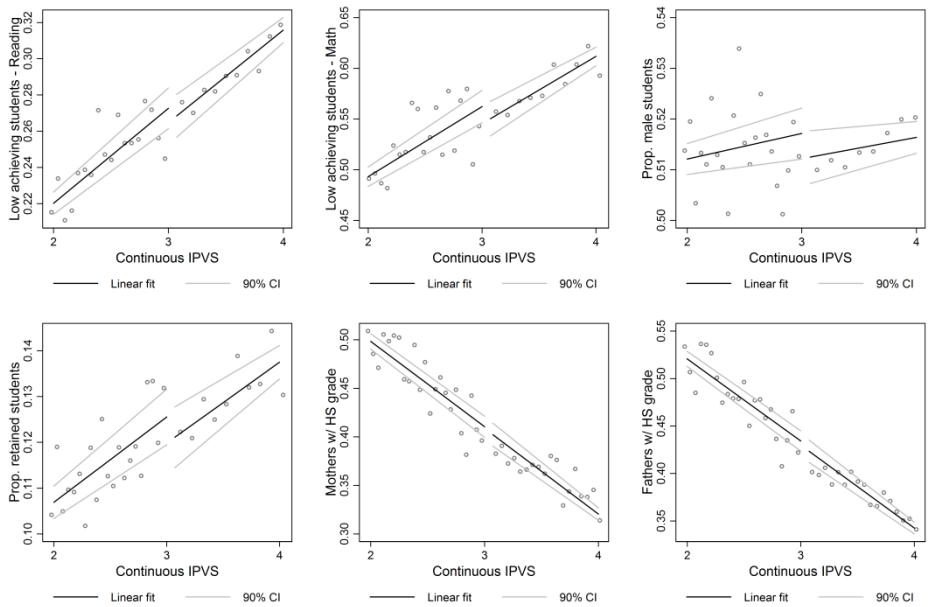
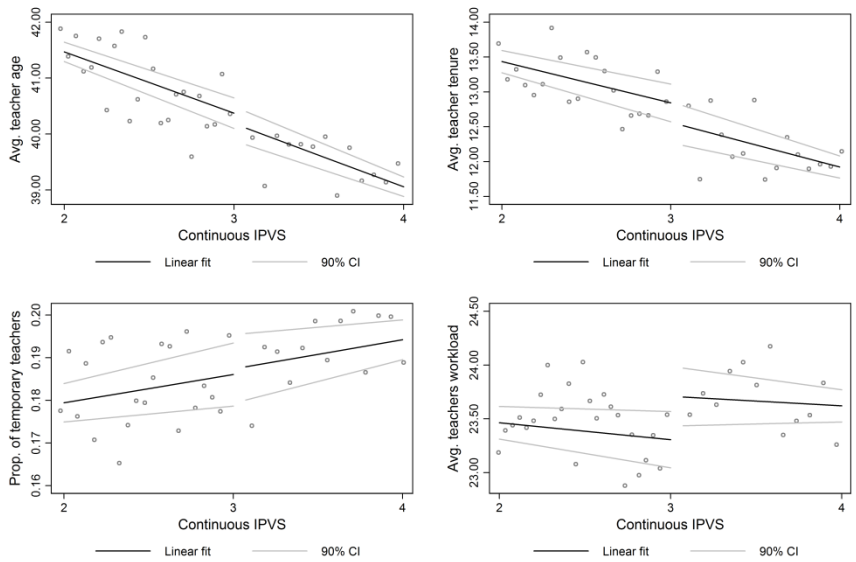
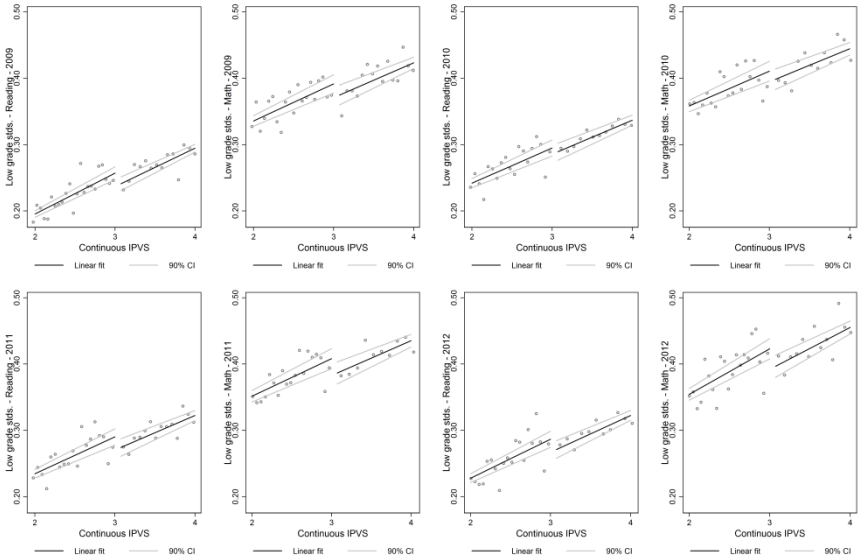
Appendix



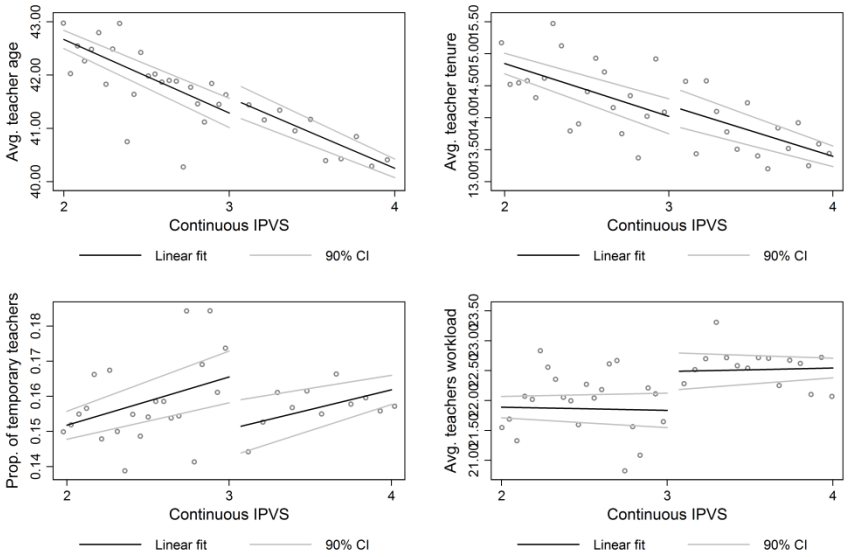
Graph A1 – Linear fit of pre-treatment students characteristics (2007) by IPVS



Graph A2 - Linear fit of pre-treatment teachers' characteristics (2007) by IPVS



Graph A3 – Linear fit of post-treatment students' grades by continuous IPVS



Graph A4 – Linear fit of post-treatment teachers' characteristics by continuous IPVS

Table A5 – Estimates of the discontinuity at the probability of treatment at IPVS=3

Parametric	
Linear	0.706***
	(0.033)
Quadratic	0.696***
	(0.034)
Nonparametric	
Conventional	0.649***
	(0.049)
Bias-corrected	0.617***
	(0.049)
Bias-corrected w/ robust variance	0.617***
	(0.058)

Parametric results from the estimation of equation (2) and its quadratic version. Sample for parametric estimation: schools with cont. IPVS = [2.0,4.0]. Nonparametric results from local-polynomial regression-discontinuity point estimators proposed in Calonico, Cattaneo and Titiunik (2014). Polynomial degree: 1. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A6 – Results of RD Manipulation Test

Method	**Test Statistic**	**P-value**
Conventional	0.014	0.989
Undersmoothed	0.399	0.690
Robust Bias-Corrected	0.915	0.360

RD Manipulation Test using local polynomial density estimation, following Cattaneo, Jansson and Ma (2015).

Table A7 – Estimates of differences in pre-treatment characteristics – quadratic models

	Turnover rate	**Stds. Perform. Reading**	**Stds. Perform. Math**	**Students Gender**	**Students Retention**	**Mothers' education**
W (treatment dummy)	-0.019	-0.011	-0.024	-0.009	-0.008	-0.007
	(0.021)	(0.016)	(0.023)	(0.007)	(0.009)	(0.016)
X (continuous IPVS)	-0.090	0.057	0.079	-0.021	0.026	-0.165***
	(0.056)	(0.041)	(0.059)	(0.019)	(0.023)	(0.044)
X^2 (sq. continuous IPVS)	0.024**	-0.001	-0.001	0.005	-0.001	0.014*
	(0.010)	(0.007)	(0.010)	(0.003)	(0.004)	(0.007)
Constant	0.540***	0.108*	0.340***	0.538***	0.059*	0.780***
	(0.079)	(0.058)	(0.086)	(0.028)	(0.033)	(0.066)
N. of schools	1,720	1,709	1,709	1,709	1,709	1,709

	Fathers' education	**Teachers' age**	**Teachers' tenure**	**Temporary teachers**	**Teachers workload**
W (treatment dummy)	-0.011	-0.410	-0.422	0.002	0.689*
	(0.016)	(0.394)	(0.384)	(0.011)	(0.367)
X (continuous IPVS)	-0.163***	-3.538***	-0.889	0.007	1.690*
	(0.045)	(1.054)	(1.002)	(0.028)	(0.971)
X^2 (sq. continuous IPVS)	0.014*	0.431**	0.058	-0.000	-0.332**
	(0.007)	(0.181)	(0.172)	(0.005)	(0.165)
Constant	0.799***	47.002***	15.006***	0.165***	21.271***
	(0.067)	(1.518)	(1.437)	(0.041)	(1.403)
N. of schools	1,709	1,720	1,720	1,720	1,720

Results from the second stage estimation of equation (1), using (2) as first stage. Sample: schools with cont. IPVS = [2.0,4.0]. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A8 – Estimates of the impact of ALE on school turnover rates – quadratic models

	2008-2009	**2009-2010**	**2010-2011**	**2011-2012**
W (treatment dummy)	-0.026	-0.068***	-0.035*	-0.074***
	(0.019)	(0.020)	(0.018)	(0.018)
X (continuous IPVS)	-0.029	-0.019	-0.014	0.004
	(0.050)	(0.052)	(0.049)	(0.048)
X^2 (sq. continuous IPVS)	0.009	0.012	0.005	0.007
	(0.009)	(0.009)	(0.009)	(0.008)
Constant	0.483***	0.411***	0.335***	0.292***
	(0.072)	(0.074)	(0.069)	(0.069)
N. of schools	1,720	1,720	1,720	1,720

Results from the second stage estimation of equation (1), using (2) as first stage. Sample: schools with cont. IPVS = [2.0,4.0]. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9 – Estimates of the impact of ALE on students' performance – quadratic models

	2009		2010	
	Reading	**Math**	**Reading**	**Math**
W (treatment dummy)	-0.025*	-0.028	-0.010	-0.023
	(0.014)	(0.020)	(0.017)	(0.021)
X (continuous IPVS)	0.134***	0.088*	0.113**	0.051
	(0.034)	(0.051)	(0.045)	(0.056)
X^2 (sq. continuous IPVS)	-0.012**	-0.005	-0.010	0.001
	(0.006)	(0.009)	(0.008)	(0.010)
Constant	-0.028	0.179**	0.053	0.255***
	(0.048)	(0.073)	(0.064)	(0.079)
N. of schools	1,718	1,718	1,717	1,717
	2009		**2010**	
	Reading	**Math**	**Reading**	**Math**
W (treatment dummy)	-0.026	-0.035	-0.025	-0.042*
	(0.018)	(0.022)	(0.017)	(0.022)
X (continuous IPVS)	0.103**	0.098*	0.120***	0.122**
	(0.044)	(0.056)	(0.044)	(0.057)
X^2 (sq. continuous IPVS)	-0.008	-0.007	-0.010	-0.009
	(0.008)	(0.010)	(0.008)	(0.010)
Constant	0.058	0.179**	0.025	0.141*
	(0.063)	(0.079)	(0.062)	(0.081)
N. of schools	1,716	1,716	1,714	1,714

Results from the second stage estimation of equation (1), using (2) as first stage. Sample: schools with cont. IPVS = [2.0,4.0]. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A10 – Estimates of the impact of ALE on teachers' characteristics – quadratic models

	Age	**Tenure**	**Temporary**	**Workload**
W (treatment dummy)	0.384	0.242	-0.021**	1.094***
	(0.396)	(0.387)	(0.010)	(0.413)
X (continuous IPVS)	-2.048*	-0.937	0.026	2.664**
	(1.045)	(1.013)	(0.026)	(1.094)
X^2 (sq. continuous IPVS)	0.110	0.016	-0.002	-0.490***
	(0.179)	(0.173)	(0.004)	(0.185)
Constant	46.365***	16.665***	0.107***	18.310***
	(1.511)	(1.463)	(0.038)	(1.590)

N. of schools	1,720	1,720	1,720	1,720

Results from the second stage estimation of equation (1), using (2) as first stage. Sample: schools with cont. IPVS = [2.0,4.0]. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A11 - Estimates of differences in pre-treatment characteristics – nonparametric models

Bandwidth choice:	**Turnover rate**	**Stds. performance in Reading**	**Stds. performance in Math**	**Students Gender**	**Students Retention**	**Mothers' education**
Conventional	-0.000	0.011	0.003	-0.003	-0.015	-0.014
	(0.027)	(0.027)	(0.042)	(0.010)	(0.012)	(0.026)
Bias-corrected	0.006	0.020	0.014	-0.000	-0.020	-0.016
	(0.027)	(0.027)	(0.042)	(0.010)	(0.012)	(0.026)
Bias-corrected w/ robust variance	0.006	0.020	0.014	-0.000	-0.020	-0.016
	(0.035)	(0.034)	(0.054)	(0.013)	(0.016)	(0.034)
N. of schools	1,351	962	881	952	1,250	858
	Fathers' education	**Teachers' age**	**Teachers' tenure**	**Temporary teachers**	**Teachers workload**	
Conventional	-0.024	-0.877	-0.364	-0.012	0.692	
	(0.026)	(0.648)	(0.524)	(0.015)	(0.502)	
Bias-corrected	-0.031	-1.142*	-0.392	-0.020	0.683	
	(0.026)	(0.648)	(0.524)	(0.015)	(0.502)	
Bias-corrected w/ robust variance	-0.031	-1.142	-0.392	-0.020	0.683	
	(0.034)	(0.821)	(0.678)	(0.019)	(0.655)	
N. of schools	846	957	1,225	1,060	1,316	

Results from local-polynomial regression-discontinuity point estimators proposed in Calonico, Cattaneo and Titiunik (2014). Polynomial degree: 1. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A12 – Estimates of the impact of ALE on school turnover rates – nonparametric models

Bandwidth choice:	**2008-2009**	**2009-2010**	**2010-2011**	**2011-2012**
Conventional	-0.048*	-0.066**	-0.038	-0.077***
	(0.027)	(0.026)	(0.025)	(0.029)
Bias-corrected	-0.058**	-0.069***	-0.043*	-0.078***
	(0.027)	(0.026)	(0.025)	(0.029)
Bias-corrected w/ robust variance	-0.058*	-0.069**	-0.043	-0.078**
	(0.035)	(0.034)	(0.032)	(0.038)
N. of schools	1,144	1,378	1,224	1,049

Results from local-polynomial regression-discontinuity point estimators proposed in Calonico, Cattaneo and Titiunik (2014). Polynomial degree: 1. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A13 – Estimates of the impact of ALE on students' performance – nonparametric models

Bandwidth choice:	**2009**		**2010**	
	Reading	**Math**	**Reading**	**Math**
Conventional	-0.030	-0.058*	-0.005	-0.022
	(0.020)	(0.033)	(0.027)	(0.034)
Bias-corrected	-0.035*	-0.077**	-0.002	-0.018
	(0.020)	(0.033)	(0.027)	(0.034)
Bias-corrected w/ robust variance	-0.035	-0.077*	-0.002	-0.018
	(0.026)	(0.042)	(0.035)	(0.044)
N. of schools	1,172	1,062	1,061	1,028
	2011		**2012**	
	Reading	**Math**	**Reading**	**Math**
Conventional	-0.020	-0.042	-0.019	-0.037
	(0.032)	(0.037)	(0.029)	(0.039)
Bias-corrected	-0.016	-0.042	-0.013	-0.030
	(0.032)	(0.037)	(0.029)	(0.039)
Bias-corrected w/ robust variance	-0.016	-0.042	-0.013	-0.030
	(0.041)	(0.048)	(0.037)	(0.050)
N. of schools	863	969	920	886

Results from local-polynomial regression-discontinuity point estimators proposed in Calonico, Cattaneo and Titiunik (2014). Polynomial degree: 1. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A14 – Estimates of the impact of ALE on teachers' characteristics – nonparametric models

Bandwidth choice:	**Age**	**Tenure**	**Temporary**	**Workload**
Conventional	0.393	0.383	-0.045***	1.335**
	(0.565)	(0.526)	(0.017)	(0.644)
Bias-corrected	0.399	0.481	-0.055***	1.411**
	(0.565)	(0.526)	(0.017)	(0.644)
Bias-corrected w/ robust variance	0.399	0.481	-0.055***	1.411
	(0.740)	(0.677)	(0.020)	(0.858)
N. of schools	1,163	1,210	920	1,192

Results from local-polynomial regression-discontinuity point estimators proposed in Calonico, Cattaneo and Titiunik (2014). Polynomial degree: 1. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Chapter 2

On the mechanisms of the impact of compensatory policies on performance

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EESP/FGV

Abstract

This chapter further analyzes the impact of ALE on student learning, focusing on three possible mechanisms, based on the program's design: i) the turnover itself; ii) the quality of teachers; iii) the wage increase. The program's assignment, based on an arbitrary rule, is used as an instrumental variable to identify the impacts of interests. Our estimates show that the only channel through which this compensatory policy affects students' performance is the reduction in teacher turnover. By reducing turnover rate in one standard deviation, ALE reduced the proportion of low performance students in about 50% of a standard deviation. Moreover, we find no evidence that ALE improved achievement through reallocation of teachers' quality or even via increase in teachers' earnings. All those results can be interpreted as a direct consequence of ALE's design, as its assignment does not take into account any teacher characteristic and the monetary incentive is a once-for-all premium, which is not supposed to affect teacher's productivity. Our estimates show important limitations on this policy's capacity of improving student performance.

Keywords: teacher labor markets, turnover rates, compensating differential.

JEL codes: I28, J38, J45.

1. Introduction

On Chapter 1 we found that a compensatory policy for teachers in São Paulo public schools is able to reduce turnover rates and we also indicated (in reduced form) that this incentive positively impacts the students' average performance in treated schools. This chapter aims to deepen this second result, investigating the channels through which this policy is capable of raising student learning.

Considering the policy design, we focus on three possible channels through which this incentive is impacting achievement: i) the turnover itself; ii) the composition of teachers; iii) the wage increase.

In the first channel, we investigate whether the reduction in teacher turnover caused by ALE can explain its impact on students' performance. Recent literature indicates that teacher turnover may *directly* harm achievement. Ronfeldt, Loeb and Wyckoff (2013) show that turnover has a negative effect on achievement, even after controlling for teacher quality and for students of the stayers (teachers that do not leave).

The authors argue that part of this negative effect comes from a disruption effect on school's organization, which is also the claim of Boyd et al (2008). When teachers leave a school there may be a loss of institutional memory related to teacher's knowledge about school's routines, principal's methods of work, relationship with other teachers, students' characteristics and other aspects that may affect teaching.

However, teacher turnover can also be associated to achievement through changes in teachers' profile. Several papers show that the substitute teachers are less experienced and less effective (Hanushek et al, 2005; Clotfelter et al, 2008; Boyd et al, 2008; Hanushek, Kain and Rivkin, 2004) and they tend to be allocated in the most disadvantaged schools (Loeb, Beteille and Kalogrides, 2012; Boyd et al, 2008).

Clotfelter et al (2008) show that experienced teachers responded more to financial incentives to reduce turnover and Pugatch and Schroeder (2014) estimated that a hardship allowance program in Gambia attracted more qualified teachers to treated schools, although the program has not impacted average achievement. So, our second channel investigates whether changes in the quality of treated school teachers caused by the compensation policy are leading to improvements in students' performance.

Finally, as ALE's incentive is exclusively monetary, we also investigate whether the increase in teacher earnings caused the improvements in achievement, although the link *teacher earnings–student performance* is not well established. Literature reviews, such as Podgursky (2011) and Hanushek and Rivkin (2006) show that the majority of estimates of

the effect of teacher salary on student performance are non-significant. A common argument for such results is that teachers' payments are, generally, not related to their productivity. However, even for pay-for-performance schemes the link to students' achievement is not clear (Figlio and Kenny, 2007, among others).

This chapter offers an interesting investigation on how teacher turnover may affect learning and, as a consequence, how a compensatory policy is capable of influencing student performance. That's because we have an exogenous variation in turnover rates caused by the introduction of ALE, already discussed in Chapter 1. That variation allows us to identify the relation between variations in turnover and improvements in achievement. Besides, this intervention, along with a rich set of data, permits the identification of both reallocation and income effects, two relevant mechanisms related to the program's design.

Our results point that this compensatory policy has impacted students' performance solely through reducing turnover. We find no evidence that ALE has impacted achievement via changes in teachers' quality or increases in teachers' remuneration.

These results are very consistent with the policy design, which focuses exclusively on teachers' retention, with no concerns about the profile of the retained ones or about their productivity. Besides, we found that this impact via turnover reduction only happens after a long time, which seems to fit to the argument that the retention of teacher would improve their interaction and teamwork capacity.

This chapter is divided into six sections. The next section exposes the wage policy for teachers in São Paulo public schools. Then, we discuss the identification strategy and empirical modeling. Finally, we present the results and the robustness tests, along with the concluding section.

2. Institutional background

Chapter 1 explains how teachers are allocated among schools every year. In sum, experienced permanent teachers have higher priority in choosing where to teach. This helps us understand how the turnover and the teacher quality channels should work. However, in order to discuss how the wage increase channel might act, it is important to know teachers' wage policies.

In São Paulo state public schools, teachers' hourly wage depends, generally, on two variables: tenure and certification. Both of them define what we call the "basic wage" and

beyond this remuneration some teachers receive grants, compensations and bonuses, depending on a set of situations[1,2].

The tenure determines teachers' salary by the so-called "quinquennial rule", a state law that determines a 5% increase on the basic wage for all public servants every 5 years of work, independently on any other characteristic, like career level, productivity etc.

Besides, teachers can raise their wages by career progression. The State Department of Education establishes that teachers' career can progress through 5 levels and at each progression the basic wage raises by 5%. This progression depends on teachers' certification. To be first admitted at this public schools system, teachers must, at least, be graduated in Pedagogy or in a specific area (like language arts, math, physics, history etc.). As teachers obtain additional certificates (in post-graduation courses, for example) they can apply to higher career levels.

Table 1 shows the basic wages for different levels in teacher career. As one can see, the 2008 hourly wage varied, for elementary school teachers, from 4.46 dollars of 2008 (for the less experienced and qualified) to 6.92 dollars (for the most experienced and qualified). As for secondary and high school teachers, basic earning ranged from 5.17 to 8.01 dollars/hour. This is a low remuneration even for Brazilian standards: in 2008, an average graduated worker earned 13.03 dollars/hour.

Table 1 – Teachers' basic wage per hour by tenure and career (in 2008 US$)

Career levels / Yrs. of tenure	Elementary school teachers					Secondary and high school teachers				
	I	II	III	IV	V	I	II	III	IV	V
[0, 5)	4.46	4.69	4.92	5.17	5.42	5.17	5.42	5.69	5.98	6.28
[5, 10)	4.69	4.92	5.17	5.42	5.69	5.42	5.69	5.98	6.28	6.59
[10, 15)	4.92	5.17	5.42	5.69	5.98	5.69	5.98	6.28	6.59	6.92
[15, 20)	5.17	5.42	5.69	5.98	6.28	5.98	6.28	6.59	6.92	7.27
[20, 25)	5.42	5.69	5.98	6.28	6.59	6.28	6.59	6.92	7.27	7.63
[25, 30)	5.69	5.98	6.28	6.59	6.92	6.59	6.92	7.27	7.63	8.01

Note 1: for currency conversion, we used the 2008 average exchange rate: 0.5453 US$/R$.

Note 2: teachers in São Paulo public schools usually retire after 30 years of work, for men, and 25 years, for women. Less than 1% of teachers have more than 30 years of tenure.

[1] Some of the additional remunerations are determined by Brazilian labor law, like the compensations for night shifts (20% over the basic wage).

[2] It is important to note that these wage policies varied a lot since 2010. In this study, we focus on the current rules between 2007 and 2009, period when rules did not change importantly.

On average, teachers work 30 hours per week, so a typical elementary school teacher earned in 2008 between 535.20 and 830.40 dollars per month. An average secondary/high school teacher earned between 620.40 and 961.20 dollars monthly.

As exposed on Chapter 1, teachers working in high-poverty schools take part of the Workplace Allowance (ALE) program and receive an additional payment. The value of this incentive is fixed (established in 2008), independently on teachers career level or tenure. In 2008 dollars, teachers earned US$ 1.53 per hour worked in ALE schools.

So, the importante of this incentive on teachers earning varies across career and tenure. More precisely, ALE's weight on earnings is decreasing in experience and career position. For example, for a level-I teacher beginning her career ALE's incentive raises the basic wage by 34% (elementary school teachers) or 29% (secondary/high school teachers). For the most experienced level-V teachers, ALE may increase basic wage by 22% (elementary school) or 19% (secondary/high school).

3. Empirical strategy

3.1. Channel 1: turnover

The strategy to test this first mechanism consists of estimating the following set of equations:

$$A_i = \gamma_0 + \gamma_1 X_i + \gamma_2 Y_i + \varepsilon_i \qquad (1)$$

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 W_i + u_i \qquad (2)$$

$$W_i = \alpha_0 + \alpha_1 X_i + \alpha_2 Z_i + e_i \qquad (3)$$

Where X_i is the continuous IPVS of ith school, $Z_i = \mathbb{I}(X_i \geq 3{,}0)$ is the instrumental variable, as discussed on Chapter 1, W_i is the treatment dummy, Y_i is the turnover rate and A_i is school i average achievement, measured by the proportion of low performance students.

The parameter of interest is γ_2, the partial effect of the channel on achievement, taking into account the exogenous variation caused by the treatment. In other words, γ_2 represents the partial effect of teacher turnover, after impacted by ALE, on achievement. A positive estimate may indicate that the impact of ALE on student performance is due, at least partially, to the variation on teacher turnover.

Naturally, it is expected expect that the error terms of those three equations to be correlated, so, in order to correctly identify their parameters, Z_i must be a valid instrument. As we argument on Chapter 1, Z_i is a valid instrument to treatment

assignment around the cutoff $X_i = 3{,}0$. For this reason, the set of equation is estimated at the IPVS interval $[2.0{,}4.0]$, the same we used for the benchmark estimates on Chapter 1.

The system of equations is estimated using three stage least square (3SLS). For robustness test we add to equation (1) control variables related to student achievement: gender; student's retention and parents' education. Moreover, also for testing robustness, we estimate the same models for pre-treatment data.

3.2. Channel 2: teacher quality

For the second channel we use a similar strategy, estimating equations (1) to (3), but replacing Y_i by a measure of the quality of teachers in school i: the average teacher's value-added.

Although the methods for measuring value-added are rather controversial (Rothstein, 2010; Chetty, Friedman and Rockoff, 2014), we attempt to follow some of the most traditional models, adapting to the available data. First, the following model is estimated using a teacher level data:

$$G_{tcs} = \alpha_1 S_{cs} + \alpha_2 B_t + \mu_s + \zeta_{tcs} \qquad (4)$$

In which G_{tcs} is the average grade of students in class c of schools s, where teacher t teaches, S_{cs} is a vector of average students characteristics (gender, age, parents education), B_t are teachers' characteristics (age, tenure, type of contract and career level) and μ_s is a school fixed effect (captured by school dummies).

The value-added is measured by the normalized residual (ζ_{tcs}) of this regression. Our available databases allow the estimation of this model for one year, 2007, so we obtain the value-added for each teacher t in this year. This is because the linkage between teachers and classes are only available in this year.

Then, we assign these 2007 values for the same teachers in the following years, 2008 and 2009. At last, we take the school average of this variable to obtain a measure of (average) quality of teacher working in that school.

As the state official exam focus on math and language, the teacher quality refers to the school's average value-added of language and math teachers. In this sense, we are not considering the whole set of teachers. On the other side, once we are evaluating effects on students' performance on language and math, this measure of quality should be, in fact, the most associated to our achievement variable.

Assigning this measure of teacher quality as Y_i in equation (2), the coefficient γ_2 in (1) again indicates whether the second channel is valid. To ensure that we are capturing a composition effect, we restrict our sample to teachers that are moving across schools.

3.3. Channel 3: teacher salary

In order to test the third channel, we adopt a different set of equations. As discussed on section 2 of this chapter, the weight of ALE on teachers' earnings is fully determined by tenure and career. Therefore, the equation (2) may be suppressed, generating a new system:

$$A_i = \delta_0 + \delta_1 X_i + \delta_2 W_i + \delta_3 V_i + \delta_4 W_i \cdot V_i + v_i \qquad (1')$$

$$W_i = \alpha_0 + \alpha_1 X_i + \alpha_2 Z_i + e_i \qquad (3')$$

To capture the effect of wage increase on achievement, we include in equation (3') the variable V_i, which measures the average value (as percentage of the basic wage) of ALE received by ith school's teachers. As this value is fully determined by teachers' characteristics, we can calculate it even for control schools' teachers. In this sense, V_i measures an average potential weight of ALE on the wages of ith school's teachers. So, the interaction $W_i \cdot V_i$ is the variable that, in fact, captures the importance of ALE on treatment school's teachers.

The coefficient of interest is δ_4. If this parameter is negative, we have evidence that, controlling for the participation in program and the characteristics that determine teachers' wage, the value transferred by ALE impacts students' performance.

In order to correctly identify this parameter as an evidence for this channel, we restrict the sample to teachers that have not moved across schools between 2008 and 2009. This way, we avoid confounding this mechanism with any composition effect.

4. Data

As explained on Chapter 1, data on ALE assignment and on schools' IPVS come, respectively, from the State Department of Education and the State Statistical Bureau. As well, student performance is calculated from Saresp (the official state exam) microdata and turnover rates are obtained from the Brazilian School Census. As both of these databases are updated annually, we are able to test the turnover channel for the period from 2009 to 2012.

Table 2 - Summary statistics of achievement and channels' variables for schools with IPVS in [2.0,4.0]

	Control	Treatment	P-value of t-test
Prop. of low performance students in reading			
2008	0.242	0.293	0.000
2009	0.220	0.272	0.000
2010	0.265	0.313	0.000
2011	0.258	0.300	0.000
2012	0.254	0.297	0.000
Prop. of low performance students in math			
2008	0.522	0.583	0.000
2009	0.360	0.401	0.000
2010	0.382	0.421	0.000
2011	0.376	0.411	0.000
2012	0.383	0.427	0.000
Turnover rate			
2007-2008	0.469	0.508	0.000
2008-2009	0.469	0.473	0.426
2009-2010	0.433	0.426	0.340
2010-2011	0.333	0.312	0.000
2011-2012	0.344	0.327	0.003
Teacher value-added – 2009**	-0.011	0.021	0.290
ALE weight on basic wage (in percentage) – 2009*	25.8	26.4	0.000
N. of schools	968	734	

* Considering only teachers staying in the same school in 2008-2009.

** Considering only teachers changing schools in 2008-2009.

To measure teacher's quality and ALE's wage increase we use database that links teachers to their classes, along with their students' performance and characteristics. This database is available only in 2007. Teachers' basic wage is inferred based on their tenure and career level. We do not have access to their full salary, that may add other payments (like extra hours, nightshifts etc.) over the basic value.

Table 2 brings some summary statistics of achievement and the channel variables over the schools of our relevant sample, with IPVS between 2.0 and 4.0. As expected, treated schools have a higher proportion of low performance students in both subjects. Our first channel, the teacher turnover, was higher for treated schools in pre-treatment period (2007-2008), but became lower a long time.

For the second channel, the measure of value-added for those teachers that changed schools in 2008-2009 is not statistically different between both groups, although with opposite signs. As for the third channel, we can see that on average ALE's incentive for stayers corresponds to about 26% of teachers' hourly wage. For control schools, we calculated a slightly lower value as a "potential" weight of ALE on hourly wage.

5. Results

Tables 3 to 5 summarize the results for each of the three channels. Complete results, including full estimates of the systems of equations, are reported in the Appendix (Tables A1 to A3). The table below shows the estimates for parameter γ_2 of equation (1), which indicates whether ALE impacted achievement through turnover.

Results indicate large positive coefficients in all years, although less precise in the first two years, which is expected, as the impact of ALE on 2008-2009 turnover rates and on 2010 achievement are not significant (see Chapter 1).

Table 3 – Estimates of the impact of ALE on achievement through changes in turnover (channel 1)

	Year = 2009		Year = 2010		Year = 2011		Year = 2012	
	Reading	Math	Reading	Math	Reading	Math	Reading	Math
Y (Turnover 08-09)	1.200	1.227	-	-	-	-	-	-
	(0.982)	(1.140)	-	-	-	-	-	-
Y (Turnover 09-10)	-	-	0.208	0.357	-	-	-	-
	-	-	(0.255)	(0.320)	-	-	-	-
Y (Turnover 10-11)	-	-	-	-	0.875	1.123	-	-
	-	-	-	-	(0.628)	(0.806)	-	-
Y (Turnover 11-12)	-	-	-	-	-	-	0.392*	0.621**
	-	-	-	-	-	-	(0.222)	(0.296)
X (continuous IPVS)	0.034***	0.028**	0.044***	0.038***	0.045***	0.044***	0.044***	0.045***
	(0.012)	(0.013)	(0.004)	(0.006)	(0.006)	(0.007)	(0.004)	(0.005)
Constant	-0.423	-0.282	0.067	0.135	-0.140	-0.101	0.013	0.063
	(0.433)	(0.503)	(0.104)	(0.130)	(0.213)	(0.274)	(0.074)	(0.099)
Control variables	NO	NO	NO	NO	NO	NO	NO	NO
N. of schools	1,718	1,718	1,717	1,717	1,716	1,716	1,714	1,714

Notes: performance is measured as the proportion of low performing students; estimates of parameter γ_2 of equation (1); standard errors in parentheses; $*** \ p < 0.01, ** \ p < 0.05, * \ p < 0.10$.

Coefficients are always higher for math performance, which makes sense, as it is a subject more dependent on teacher's work and on a regular routine of classes. In 2012, for example, a drop of 12 p.p. in turnover rate (1 standard deviation) is associated to a drop 4.7 p.p. in the proportion of low performers in language (43% of a standard deviation) and 7.4 p.p. in math (53% of a standard deviation). Moreover, this positive and significant relation is via an exogenous variation in the participation in ALE.

So, we confirm that this compensatory policy affects students' performance through reducing teacher turnover. Besides, our evidence is more precise after a few years of treatment, which seems compatible with the claim that a reduction in turnover could improve institutional knowledge and that, in turn, would lead greater achievement (Ronfeldt, Loeb and Wyckoff, 2013; Boyd et al, 2008). It is reasonable to suppose that it should take time for teachers to get used to school's routines, coordinator's methods of work etc., which might explain the less precise effects on short term.

Table 4 shows the results for testing the second mechanism. As one can see, we cannot accept the hypotheses that changes in faculty's quality caused by ALE are a channel for its impact on student performance. The sign of the coefficient has the expected direction, as it indicates that a higher average teacher quality, influenced by treatment compliance, is associated to a lower proportion of low achieving students. However, estimates have very low precision (p-value of 0.66).

Table A2 in the Appendix indicates the reason for this non-significant result. Estimates for equation (2) show that ALE has not changed the average quality of teachers in treated schools, so it would be hard to believe that this would be a mechanism behind ALE's impact on achievement.

Our result can be interpreted as a direct consequence of ALE's design. The program assignment does not generate incentives for attracting better teachers to treated schools. As we exploited on Chapter 1, the only teacher characteristic impacted by ALE is the proportion of permanent contract ones. This impact is related to the power of those teachers of choosing their schools, but not necessarily to their value-added.

Table 4 – Estimates of the impact of ALE on achievement through changes in teacher quality (channel 2)

	Year = 2009	
	Reading	Math
Y (Teacher value-added)	-0.575	-0.595
	(1.304)	(1.399)
X (continuous IPVS)	0.052**	0.043**
	(0.020)	(0.022)
Constant	0.101*	0.268***
	(0.057)	(0.061)
Control variables	NO	NO
N. of schools	1,454	1,454

Notes: the sample of teachers for measuring the value-added is restricted to those who moved across school in 2008-2009; performance is measured as the proportion of low performing students; estimates of parameter γ_2 of equation (3); standard errors in parentheses; $*** \ p < 0.01, ** \ p < 0.05, * \ p < 0.10$.

One can argue that more tenured teacher also have more power to decide where to teach and could be attracted to treatment schools, which could, in turn, positively affect students' performance. However, this effect of attracting tenured teachers might be offset by the fact that the more experienced ones receive a proportionally lower value of ALE. Chapter 1 showed that, in fact, ALE does not change average teacher tenure.

Table 5 shows results for channel 3, wage increase. One first interesting fact is that the estimate of the coefficient associated to the treatment dummy is barely equal the one we found on Chapter 1, where we haven't controlled for ALE weight on salary. So, in this modeling we still conclude ALE has a direct effect on achievement, but this cannot be

attributed to a wage increase. The sign of interaction's coefficient shows that the higher is average ALE's weight on salary the lower is the proportion of low performer, however estimates are not precise.

This result is not surprisingly, as the financial incentive received by those teachers is not related to any productivity criterion. That is, ALE gives a once-for-all premium, which is not expected to affect teacher's marginal productivity.

One can argue that, as we show on Chapter 1, ALE has raised the average amount of hours teachers devote to treated schools and that would be a measure of their productivity. However, this impact only indicates that teachers get more classes in treated schools, but it does not mean that students are having more instructional time or even that teachers are exerting more effort on those classes.

Table 5 – Estimates of the impact of ALE on achievement through changes in teacher salary (channel 3)

	Year = 2009	
	Reading	Math
W (assignement to ALE)	-0.029**	-0.029
	(0.014)	(0.020)
V (ALE weight on wage)	0.005	-0.003
	(0.008)	(0.014)
$W \cdot V$ (interaction)	-0.012	-0.011
	(0.016)	(0.023)
X (continuous IPVS)	0.065***	0.059***
	(0.009)	(0.012)
Constant	0.066***	0.220***
	(0.020)	(0.029)
Control variables	NO	NO
N. of schools	1,685	1,685

Notes: the sample of teachers for measuring the value-added is restricted to those who remained in the same school in 2008-2009; performance is measured as the proportion of low performing students; estimates of parameters δ_2, δ_3 and δ_4 of equation (1'); bootstrapped standard errors in parentheses; $*** \ p < 0.01, ** \ p < 0.05, \ * \ p < 0.10$.

In brief, we conclude this compensatory policy, that seeks to reduce teacher turnover in high poverty schools, seems to, in fact, drop the proportion of teachers moving from those schools (Chapter 1) and this impact seems to generate improvement in student performance. By reducing turnover in about one standard deviation, ALE improves students performance in about half standard deviation.

On the other side, ALE's effect on achievement seems limited by this only mechanism, as we cannot accept the hypothesis that ALE has changed student performance via teacher quality or wage raise. These results are expected, as this policy is not designed to attract or maintain high quality teachers and as the wage premium does not depend on any productivity criterion.

For testing the robustness of our results we, first, estimated both sets including control variable associated with achievement in equations (1) and (1'). Tables A4 to A6 show the results. For the first channel results are qualitatively the same, that is, we can find more precise evidence that ALE affects student performance via turnover in 2012. For the last two channels the new estimates also show no evidence that ALE acts via attraction of better teacher or wage increase. So, our conclusions are still valid after conditioning on some characteristics most related to student performance.

Another possible concern is whether the significant effect we found for the turnover channel is due to some previous difference between treatment and control schools. We have checked for several differences between treated and untreated schools around the cutoff on Chapter 1, but for checking it in this current context, we estimated the channel 1 model using pre-treatment variable (2008 achievement and students' characteristics and 2007-2008 turnover rate). As expected, we find no significant results for this pre-treatment period.

6. Conclusion

This chapter further analyzes the impact of ALE on student learning, focusing on three possible mechanisms, based on the program's design: i) the turnover itself; ii) the composition of teachers; iii) the wage increase.

Our estimates show that the only channel through which this compensatory policy affects students' performance is the reduction in teacher turnover. By reducing turnover rate in one standard deviation, ALE reduced the proportion of low performers in about half standard deviation. Those results are more precise along time, which seems compatible with the claim that a reduction in turnover could improve institutional knowledge among teachers.

We find no evidence that ALE improved achievement through reallocation of teachers or via increase in teachers' earnings. Both results are expected because of ALE's design. The program assignment does not take into account any teacher characteristic, so it wouldn't be expected some kind of teacher quality reallocation among schools. Besides, the program gives a once-for-all premium, which is not supposed to affect teacher's productivity.

Those results allow us to infer that the capacity of ALE to improve achievement is limited by its design. This policy is aimed exclusively to prevent teachers to leave poor schools and this, as the chapter shows, represents a relevant benefit to students' performance. However, this kind of policy does not incentivize the attraction or

maintenance more productive teachers. So, one cannot expect that it changes the fact that the more disadvantaged schools still tend to receive the lowest quality teachers, which can be an important limitation for the impacts on students.

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Appendix

Table A1 - Full estimates of the impact of ALE on achievement through changes in turnover (channel 1)

	Equation (3) - Dep. Var.: W (assignement to ALE)							
	Year = 2009		Year = 2010		Year = 2011		Year = 2012	
	Reading	Math	Reading	Math	Reading	Math	Reading	Math
X (continuous IPVS)	0.139***	0.139***	0.139***	0.139***	0.139***	0.139***	0.138***	0.138***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
$Z\ (= 1, if\ IPVS \geq 3.0)$	0.706***	0.706***	0.706***	0.706***	0.706***	0.706***	0.708***	0.708***
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
Constant	-0.288***	-0.288***	-0.288***	-0.288***	-0.289***	-0.289***	-0.287***	-0.287***
	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)
	Equation (2) - Dep. Var.: Y (Turnover rate)							
	Year = 2009		Year = 2010		Year = 2011		Year = 2012	
	Reading	Math	Reading	Math	Reading	Math	Reading	Math
X (continuous IPVS)	0.025**	0.025**	0.047***	0.047***	0.015	0.015	0.045***	0.045***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
W (assignement to ALE)	-0.024	-0.024	-0.065***	-0.065***	-0.033*	-0.033*	-0.072***	-0.072***
	(0.018)	(0.018)	(0.019)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)
Constant	0.409***	0.409***	0.320***	0.320***	0.296***	0.296***	0.236***	0.236***
	(0.027)	(0.027)	(0.028)	(0.028)	(0.027)	(0.027)	(0.027)	(0.027)
	Equation (1) - Dep. Var.: A (Prop. of low performance students)							
	Year = 2009		Year = 2010		Year = 2011		Year = 2012	
	Reading	Math	Reading	Math	Reading	Math	Reading	Math
X (continuous IPVS)	0.034***	0.028**	0.044***	0.038***	0.045***	0.044***	0.044***	0.045***
	(0.012)	(0.013)	(0.004)	(0.006)	(0.006)	(0.007)	(0.004)	(0.005)
Y (Turnover rate)	1.200	1.227	0.208	0.357	0.875	1.123	0.392*	0.621**
	(0.982)	(1.140)	(0.255)	(0.320)	(0.628)	(0.806)	(0.222)	(0.296)
Constant	-0.423	-0.282	0.067	0.135	-0.140	-0.101	0.013	0.063
	(0.433)	(0.503)	(0.104)	(0.130)	(0.213)	(0.274)	(0.074)	(0.099)
N. of schools	1,718	1,718	1,717	1,717	1,716	1,716	1,714	1,714

Notes: parameters estimated via 3SLS; standard errors in parentheses; $*** \ p < 0.01, ** \ p < 0.05, * \ p < 0.10$.

Table A2 - Full estimates of the impact of ALE on achievement through changes in teacher quality (channel 2)

	Equation (3) - Dep. Var.: W (assignement to ALE)	
	Year = 2009	
	Reading	Math
X (continuous IPVS)	0.133***	0.133***
	(0.020)	(0.020)
$Z\ (= 1, if\ IPVS \geq 3.0)$	0.715***	0.715***
	(0.027)	(0.027)
Constant	-0.277***	-0.277***
	(0.048)	(0.048)
	Equation (2) - Dep. Var.: Y (Teacher value-added)	
	Year = 2009	
	Reading	Math
X (continuous IPVS)	-0.013	-0.013
	(0.059)	(0.059)
W (assignement to ALE)	0.042	0.042
	(0.091)	(0.091)
Constant	0.024	0.024
	(0.138)	(0.138)
	Equation (1) - Dep. Var.: A (Prop. of low performance students)	
	Year = 2009	
	Reading	Math
X (continuous IPVS)	0.052**	0.043**
	(0.020)	(0.022)
Y (Teacher value-added)	-0.575	-0.595
	(1.304)	(1.399)
Constant	0.101*	0.268***
	(0.057)	(0.061)
N. of schools	1,454	1,454

Notes: parameters estimated via 3SLS; standard errors in parentheses; $*** \ p < 0.01, ** \ p < 0.05, * \ p < 0.10$.

Table A3 - Full estimates of the impact of ALE on achievement through changes in teacher salary (channel 3)

	Equation (3') - Dep. Var.: W (assignement to ALE)	
	Year = 2009	
	Reading	Math
X (continuous IPVS)	0.139***	0.139***
	(0.020)	(0.020)
$Z\ (= 1, if\ IPVS >= 3.0)$	0.706***	0.706***
	(0.033)	(0.033)
Constant	-0.288***	-0.288***
	(0.046)	(0.046)
	Equation (1') - Dep. Var.: A (Prop. of low performance students)	
	Year = 2009	
	Reading	Math
X (continuous IPVS)	0.065***	0.059***
	(0.009)	(0.013)
W (assignement to ALE)	-0.029**	-0.029
	(0.014)	(0.020)
V (ALE weight on wage)	0.005	-0.003
	(0.008)	(0.013)
$W \cdot V$	-0.012	-0.011
	(0.015)	(0.023)
Constant	0.066***	0.220***
	(0.020)	(0.030)
N. of schools	1,685	1,685

Notes: bootstrapped standard errors in parentheses; *** $p < 0.01$,** $p < 0.05$,* $p < 0.10$.

Table A4 - Robustness test (control variables) - Estimates of the impact of ALE on achievement through changes in turnover (channel 1)

	Year = 2009		Year = 2010		Year = 2011		Year = 2012	
	Reading	Math	Reading	Math	Reading	Math	Reading	Math
Y (Turnover 08-09)	1.321	1.051						
	(0.941)	(0.948)						
Y (Turnover 09-10)			0.112	0.182				
			(0.231)	(0.283)				
Y (Turnover 10-11)					0.780	0.842		
					(0.607)	(0.720)		
Y (Turnover 11-12)							0.313	0.474*
							(0.210)	(0.278)
X (continuous IPVS)	0.012**	0.013**	0.022***	0.021***	0.025***	0.023**	0.025***	0.028***
	(0.006)	(0.006)	(0.004)	(0.005)	(0.009)	(0.011)	(0.004)	(0.006)
Students Gender	-0.004	0.232**	0.106**	0.306***	0.080	0.245**	0.099**	0.309***
	(0.080)	(0.091)	(0.051)	(0.062)	(0.085)	(0.103)	(0.050)	(0.065)
Students Retention	0.526***	0.992***	0.782***	1.118***	0.765***	1.121***	0.674***	0.984***
	(0.137)	(0.146)	(0.056)	(0.069)	(0.132)	(0.159)	(0.086)	(0.114)
Mothers' education	-0.087	-0.002	-0.092**	-0.014	-0.087*	0.036	-0.099**	-0.026
	(0.080)	(0.088)	(0.043)	(0.052)	(0.048)	(0.059)	(0.044)	(0.057)
Fathers' education	-0.075	-0.027	-0.034	-0.000	-0.012	-0.061	-0.007	0.005
	(0.049)	(0.061)	(0.054)	(0.066)	(0.045)	(0.056)	(0.044)	(0.057)
Constant	-0.406	-0.382	0.077	-0.028	-0.142	-0.200	0.008	-0.109
	(0.515)	(0.518)	(0.123)	(0.151)	(0.187)	(0.222)	(0.078)	(0.103)
N. of schools	1,718	1,718	1,717	1,717	1,716	1,716	1,714	1,714

Notes: performance is measured as the proportion of low performing students; estimates of parameters of equation (1); standard errors in parentheses; $*** \ p < 0.01, ** \ p < 0.05, * \ p < 0.10$.

Table A5 - Robustness test (control variables) - Estimates of the impact of ALE on achievement through changes in teacher quality (channel 2)

	Year = 2009	
	Reading	Math
Y (Teacher value-added)	-0.476	-0.383
	(0.698)	(0.629)
X (continuous IPVS)	0.026**	0.023**
	(0.010)	(0.010)
Students Gender	-0.026	0.215***
	(0.052)	(0.067)
Students Retention	0.575	0.990**
	(0.452)	(0.408)
Mothers' education	-0.074	0.032
	(0.178)	(0.165)
Fathers' education	-0.136	-0.101
	(0.167)	(0.157)
Constant	0.208***	0.121*
	(0.067)	(0.068)
N. of schools	1,454	1,454

Notes: performance is measured as the proportion of low performing students; estimates of parameters of equation (1); standard errors in parentheses; $*** \ p < 0.01, ** \ p < 0.05, * \ p < 0.10$.

Table A6 - Robustness test (control variables) - Estimates of the impact of ALE on achievement through changes in teacher salary (channel 3)

	Year = 2009	
	Reading	Math
W (assignement to ALE)	-0.026**	-0.019
	(0.011)	(0.018)
V (ALE weight on wage)	0.001	-0.003
	(0.007)	(0.012)
$W \cdot V$	-0.004	-0.008
	(0.012)	(0.019)
IPVS	0.036***	0.031***
	(0.007)	(0.012)
Students Gender	-0.029	0.198***
	(0.043)	(0.066)
Students Retention	0.562***	1.032***
	(0.042)	(0.054)
Mothers' education	-0.093**	0.029
	(0.038)	(0.054)
Fathers' education	-0.114***	-0.100*
	(0.038)	(0.055)
Constant	0.184***	0.101**
	(0.032)	(0.047)
N. of schools	1,685	1,685

Notes: performance is measured as the proportion of low performing students; estimates of parameters of equation (1'); standard errors in parentheses; $*** \ p < 0.01, ** \ p < 0.05, * \ p < 0.10$.

Table A7 - Robustness test (pre-treatment) - Estimates of the impact of ALE on achievement through changes in turnover (channel 1)

	Year = 2008			
	Reading		Math	
Y (Turnover 07-08)	0.786	0.922	0.795	-0.013
	(2.044)	(1.801)	(2.254)	(1.681)
X (continuous IPVS)	-	-0.010	-	0.020
	-	(0.046)	-	(0.043)
Students Gender	-	0.023	-	0.151**
	-	(0.065)	-	(0.069)
Students Retention	-	0.546**	-	0.896***
	-	(0.265)	-	(0.251)
Mothers' education	-	-0.056	-	-0.079
	-	(0.085)	-	(0.082)
Fathers' education	-	-0.087	-	-0.120
	-	(0.200)	-	(0.188)
Constant	-	-0.151	-	0.269
	-	(0.856)	-	(0.798)
N. of schools	1,718	1,718	1,718	1,718

Notes: performance is measured as the proportion of low performing students; estimates of parameters of equation (3); standard errors in parentheses; $*** \ p < 0.01, ** \ p < 0.05, * \ p < 0.10$.

Chapter 3

Teacher absences and financial incentives: evidence from a salary differentiation policy

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Abstract

Teacher absenteeism is an important issue for personal economics in education, with negative consequence on students' performance. Monitoring policies to address teacher absenteeism are generally pointed as effective solutions by the literature, although it depends on institutional design. On the other side, monetary incentives to teachers have less clear results on absenteeism. In this chapter we evaluate how a wage differentiation exogenously created by ALE policy affects teacher absenteeism. The policy design allows the estimation of a causal relation between remuneration and the decision of missing work. Results show that, after controlling for teachers' and schools' fixed effects, paying a higher wage (on average a raise of 26%) causes a drop in teachers' absent days of 8-22%. Absences that do not lead to salary discount, like for medical leaves, don't respond to the wage differentiation and the impact is larger for teachers that receive a higher incentive. Both results bring consistency to the monetary incentive argument.

Keywords: teacher labor markets, teacher absenteeism, compensating differential.

JEL codes: I28, J38, J45.

1. Introduction

Teacher absenteeism is claimed to be an important issue for personal economics in education, especially in undeveloped countries. The literature shows that absences rates are about 5-6% in the US, but can reach 25% in India, 14% in Ecuador and 11% in Peru (Chaudhury et al, 2006; Miller, Murnane and Willett, 2008).

Although precise data on teacher absenteeism in Brazil are not available, some proxies point that this is a persisting problem. For example, data from the Brazilian National Student Assessment (*Prova Brasil*) show that 45% of public school principals declare that teacher absenteeism is a problem in their schools. According to 2009 PISA, 30% of Brazilian students attend schools where absenteeism is considered an important problem. A study by Unibanco Institute (2010) that monitored a sample of public schools concluded that about 15% of classes did not happen because of teachers' absences.

Literature points that teachers are absent more often than other professionals because of their routine contact with children, that might carry infectious diseases, especially in poor and rural areas (Chaudhury et al, 2006; Miller, Murnane and Willett, 2008), but a variety of evidences shows that a great part of teachers' absences is discretionary.

For example, teachers' absences are strongly associated to institutional rules, like allowance for leaves, the strength of sanctions, the type of teacher's contract, norms for reporting absences etc. Schools' characteristics, such as working conditions, location and students profile are also related to absenteeism (Chaudhury et al, 2006; Miller, Murnane and Willett, 2008; Duflo, Hanna and Ryan, 2012; Banerjee et al, 2012).

As the educational technology is highly intensive in human capital, it is straightforward that teachers' absences negatively impact students' performance. When there are not substitute teachers, absences mean less instruction time, but even when absent teachers are replaced, there are reasons to believe that learning is harmed.

In general, substitute teachers are less qualified and less effective (Boyd et al, 2008; Hanushek, Kain and Rivkin, 2004), but even if substitutes were high quality teachers, absences create disruptions of the regular classroom routines and it is hard for students to establish a relationship with a "mobile" substitute teacher (Miller, Murnane and Willett, 2008).

In fact, literature shows the negative impact of teacher absenteeism on students' performance. Das et al (2007) estimate that, in Zambia, negative shocks associated to teacher absence (like illness, funerals etc.) lead to a decline of 20-30 percent in learning gains during a year. Clotfelter, Ladd, and Vigdor (2009) use a longitudinal dataset in North Carolina to control for teachers fixed effects and find that 10 additional absent days

decrease grades by 1-2% of a standard deviation. Duflo, Hanna and Ryan (2012) found a causal impact of teacher absences on students' scores through an experimental combination of monitoring and financial incentive policies in India.

Policies to address teacher absenteeism generally involve some combination of monitoring and incentives or punishments. The applied literature shows that monitoring policies shall be effective, although it depends on the consequences for absent teachers and on how external is the absence control (Banerjee and Duflo, 2006). For example, inspections by the Ministry of Education are associated to lower absence levels (Chaudhury et al, 2006). In São Paulo, an exogenous variation in schools' management practices reduced principals' perception about the severity teacher absenteeism (Tavares, 2015).

Nonetheless, monetary incentive schemes, like raising salaries or paying for high attendance etc., have less clear results on absenteeism. Differences in teachers' unconditional salary does not seem to affect teachers' absences (Ree et al, 2015 - evidence of an exogenous salary increase in Indonesia - Chaudhury et al, 2006 - evidence from cross-country differences in proxies for teacher salary).

Evidences for conditional monetary incentives are mixed. Glewwe, Ilias and Kremer (2003) found no evidence that incentives for teachers based on school performance impacts absenteeism in Kenya and Kremer, Miguel and Thornton (2004) show that a scholarship program in Kenya raises both students and teachers attendance. In India, Duflo, Hanna and Ryan (2012) found the most robust evidence of policies for addressing absenteeism, a combination of monitoring and financial incentives. Teachers were monitored every day by cameras and their salaries were based on attendance. The authors conclude that teacher absenteeism fell by 21 percentage points.

Theoretically one expects the impacts of financial incentives on absenteeism to be ambiguous, because incentives may not be strong enough or, even in the case of strong incentives, income effects may domain substitution effects and teachers would just stop attending once they have reached a target income. So, the role of financial incentives in reducing teacher absenteeism is still an empirical question. In this chapter we evaluate this question using an exogenous salary differentiation among public schools in São Paulo.

This differentiation comes from a compensatory policy that pays from 19% to 34% more for teachers that work in schools in poor areas. The allocation of this incentive is fully determined by schools' location and does not depend on teachers' behavior. So, this policy creates, inside the same system, schools paying different wages.

This is an interesting opportunity for testing that empirical question because, as explained in Chapter 1, we are able to observe an exogenous salary variation for some schools inside the same public system. In this case, teachers are also submitted to a set of rules on absences and are also monitored, but as those norms are applied indistinctly for all teachers, we can obtain the net effect of financial incentives on absenteeism. Moreover, the program also allows for testing the impact of marginal variations on the size this differentiation.

Results show that, after controlling for teachers' and schools' fixed effects, paying a higher wage (on average a raise of 26%) reduces about 8-22% absent days. Absences that do not lead to salary discount, like for medical leaves, don't respond to the wage differentiation and the impact is larger for teachers that receives a higher incentive. Both results bring consistency to the monetary incentive argument.

This chapter is divided in five sections. Next section discusses the rules for absences in São Paulo public schools. Section 3 brings the empirical strategy along with the description of the database. Results e conclusion are shown in sections 4 and 5.

2. Institutional background

Teacher absences and leaves in São Paulo public schools are ruled by local and federal norms, which establish, for each type of absences, its maximum duration, whether there are salary discounts and other eventual consequences.

In general, teacher absences may be *paid*, with no salary discounts (but with, eventually, other consequences), or *unpaid*, in which the teacher simply does not receive the daily salary of the missing day. That is, in São Paulo public schools there are not additional monetary punishment for teacher absences.

Paid absences include medical leaves, funeral leaves, family care leaves, jury or electoral duty, among others. Unpaid absences are classified into *justified* and *unjustified*. In the first case, teachers lose the missing day salary, but, after justifying the absence, there are no other consequences. If the teacher does not show any justification or the principal does not accept it, the absence is considered unjustified and, besides the salary discount, the recurrence of this absence may lead to teacher's dismissal.

Besides these two types of absences, teachers have the right to take *personal days*, paid absences that don't require any justification. Each year, teachers may be absent for up to 6 days for any personal reason, without justifying to the principal or school supervisor and with no salary discount neither risk of dismissal.

So, one can conclude that teachers in São Paulo public schools may be absent for six days per year (about 3% of a typical school year) at cost zero and beyond these days, absences have some positive cost for teachers, related to the salary discount or to the annoyance of justifying the absence.

In general, absences are reported directly to the principal, who is also responsible for evaluating the reasons for them. For example, in case of a medical absence the teacher handles to the principal the documents that prove her reasons (medical certificate, for example) and the principal decides whether the proof is valid or not. Then, the principal reports the absence to the human resources department (centralized in the capital city), which is responsible for calculating salary discounts, when applied.

Our database on absences comes from the human resources department and it reports the yearly total days of absence, from 2006 to 2008. So, for every teacher we have access to the number of absent days in each school, classified according the type of absence. The data are organized as a teacher-school combination, totalizing about 200,000 cases per year (190,000 teachers in 4,800 schools). It is also possible to identify teachers and schools every year, which allows the construction of a panel and the merge with the ALE and IPVS database (described in Chapter 1).

Table 1 shows data on the most frequent types of absences. It reports the frequency of each type of absence and the correspondent average absent days. Each occurrence is an absence of a teacher in a school, so the whole database shall be larger than the total number of teachers.

Table 1 - Summary statistics on the most frequent absences

	2007			2008		
	Frequency	Avg. days	Std. dev in days	Frequency	Avg. Days	Std. dev in days
All types	166,758	8.68	7.88	163,366	7.74	7.95
Personal days	162,951	4.02	1.84	160,978	3.95	1.85
Medical absence	96,110	3.66	2.91	39,800	1.87	1.38
Justified absence	80,684	4.43	4.88	79,925	5.39	5.47
Unjustified absence	7,601	8.11	8.79	11,671	8.71	9.15
Other types	14,938	1.45	0.86	14,553	1.47	0.79

Note: we are working with a database of 205,744 (2007) and 199,162 (2008) observations, meaning 190,485 (2007) and 187,973 (2008) teachers among 4,861 schools.

Adding up all kinds of absences, teachers were 8.7 days absent in 2007 (7.7 days in 2008). As expected, personal days are the most frequent type of absences, with about 80% of teachers exerting their rights and taking, on average, 4 days off for personal reasons.

Medical absences are also highly frequent, although the occurrences have dropped in 2008, after the imposition of limits for this kind of leave (6 days per year). On average, teachers take 3.7 (2007) and 1.9 (2008) days off for health reasons.

As for the unpaid absences, the most frequent are the justified ones. About 40% of teachers are 4-5 days absent for a justified reason, with a salary loss. The frequency of unjustified absences is considerably lower (less than 5%), as this type of absence may cause dismissal. On average, teachers are 8 days absent with unjustified reason.

All other kinds of absences added up represent less than 10% of observations. So, we focus our analysis on the three most frequent absences: personal days, medical absences and justified absences. Among them, one expects that the salary differentiation caused by ALE affects only the justified absences, as they cause a salary loss. The other two types are used as robustness test for the monetary mechanism in analysis.

4. Empirical strategy

Our first approach for identifying the effects of salary differentiation on teacher absenteeism is based on a classic differences in differences model:

$$Y_{ist} = \gamma_1 W_s + \gamma_2 d_t + \gamma_3 W_s \cdot d_t + \varepsilon_{ist} \qquad (1)$$

Where, Y_{ist} is the number of absent days for teacher i in school s in year $t = (2007, 2008)$; W_s is the dummy that indicates the participation of school s in ALE and $d_t = \mathbb{I}[t = 2008]$.

This specification controls for average unobserved factors common to schools and teachers in the ALE and non-ALE group. By design, both groups of schools are different in terms of the neighborhood socioeconomic status. In this sense, ALE schools shall, on average, suffer more with disruptive shocks related to teachers' absenteeism, like violence, incidence of infectious diseases, problems in school infrastructure etc. This model identifies the effect of ALE on absenteeism as long as those factors vary equally, on average, over time in both groups.

As the databases allow the identification of teachers and schools, our second empirical approach is based on a fixed effect model:

$$Y_{ist} = \gamma_2 d_t + \gamma_3 W_s \cdot d_t + u_i + v_s + \epsilon_{ist} \qquad (2)$$

Where u_i and v_s are teachers' and schools' time invariant unobserved effects. So, this approach controls for factors like teachers' individual effort, their health condition among other, all related to their willingness to miss work. The model controls as well schools' managerial and cultural characteristics correlated to the allowance of teacher absenteeism.

However, none of those models fully controls for the selection for treatment, as this is a time-variant event, specific for a group of schools. So, for both specifications, the variable $Z_s = \mathbb{I}(X_s \geq 3{,}0)$, X_s as the IPVS for school s, is used as instrument for W_s and the interaction $Z_s \cdot d_t$ as instrument for $W_s \cdot d_t$. As already discussed previously, in order to Z_s be a valid instrument, we restrict our sample to schools close to IPVS cutoff, that is, schools with $IPVS = [2.0; 4.0]$.

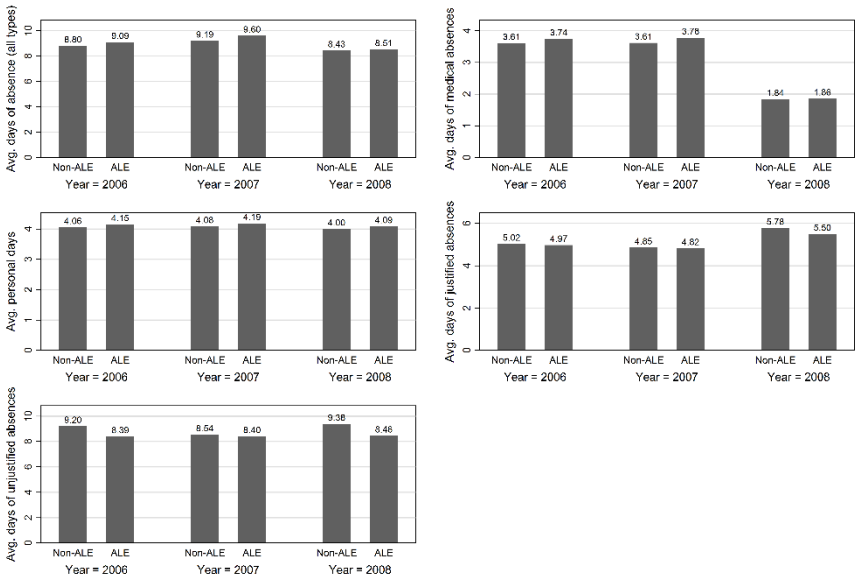
In practical terms, we are comparing teachers with different opportunity costs of missing work. On average, an absent day for ALE teachers is 26% more costly than for the non-ALE ones, however their hourly wage is also higher by the same amount. So, one can only expect that this wage differentiation to reduce absences (i.e. $\gamma_3 < 0$) if the substitution effect domains the income effect.

To test our hypothesis the variable Y_{ist} assumes the values of: i) the number of annual absent days (considering all types); ii) the number of *justified* absences and iii) the number of *unjustified* absences. As these two kinds of absence are the only with salary discounts, they are the most likely to be affected by difference in teachers' opportunity costs.

As a robustness test for the salary differentiation hypothesis, we also estimate the same models using *personal days* and *medical absences* as Y_{ist}. As both types of absences have no consequences on wages, it is expected that γ_3 to be non-significant.

Although ALE transfers a fixed amount for teacher, the weight of the financial incentive on teachers' wages vary, so it is also possible to test whether the impacts are heterogeneous according to the size of the wage differentials.

Graph 1 and Table 2 show the general behavior of teacher absences in treatment and control school, that is, ALE and non-ALE schools within IPVS interval [2.0,4.0]. In general, treatment schools have higher absences, on average, in pre-ALE period and this pattern remains the same in the first ALE year.



Graph 1 - Average absent days by type of absence and ALE and non-ALE schools

Paid absences, however, have the opposite pattern. They were lower for ALE schools before the program and became even lower in first year of treatment. Table 2 brings tests on whether those pre-treatment patterns were significant, for schools in the benchmark window. Both paid absences (medical and personal) diminished between 2006 and 2007. Even so, the path of all types of absences for both group of schools were parallel in this pre-treatment period, which is a relevant result in favor of our empirical strategies.

Table 2 - Differences in differences estimates of the impact of ALE on teachers' absent days for the pre-ALE period

	All types	Medical absence	Personal days	Justified absence	Unjustified absence
Treatment dummy	0.284**	0.133***	0.092***	-0.049	-0.810**
	(0.116)	(0.036)	(0.024)	(0.084)	(0.394)
Year dummy (=1, if 2007)	0.390***	0.005	0.027*	-0.169***	-0.664**
	(0.055)	(0.025)	(0.014)	(0.056)	(0.307)
Interaction Treat. x Year	**0.122**	**0.029**	**0.009**	**0.016**	**0.673**
	(0.090)	**(0.039)**	**(0.022)**	**(0.085)**	**(0.459)**
Control variables	NO	NO	NO	NO	NO
Constant	8.804***	3.609***	4.058***	5.024***	9.204***
	(0.081)	(0.024)	(0.016)	(0.058)	(0.277)
N. of Obs.	125,574	75,092	123,006	59,571	6,550

Notes: standard errors clustered by school in parentheses; all models estimated with schools in the IPVS interval [2.0; 4.0]; excluded instruments: $z = 1[IPVS >= 3.0]$ and $z * year\ dummy$; $*** \ p < 0.01, \ ** \ p < 0.05, \ * \ p < 0.1$.

5. Results

Table 3 brings results for DID model (1) estimated by OLS without control variables and by 2SLS, including the following control variables: school IPVS; teacher profile (number of schools she works, workload, gender, tenure, type of contract); principals' profile (age, tenure); students' average profile (proportion of boys, retained students, parents education).

The OLS estimates show that, on average, treatment schools reduced teacher absenteeism significantly more than control schools. Considering all types of absences, ALE schools near the cutoff reduced their absenteeism in about 0.3-0.5 day more than non-ALE schools did. Breaking into the most common types of absence, one can conclude that this result is much stronger for the unpaid absences.

Table 3 - Differences in differences estimates of the impact of ALE on teachers' absent days

	All types		Medical absence		Personal days		Justified absence		Unjustified absence	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Treatment dummy	0.405***	-0.328	0.163***	-0.108	0.101***	-0.039	-0.033	-0.125	-0.138	-0.366
	(0.120)	(0.332)	(0.035)	(0.089)	(0.023)	(0.064)	(0.084)	(0.226)	(0.348)	(0.748)
Year dummy (=1, if 2008)	-0.763***	-0.233**	-1.777***	-1.803***	-0.084***	-0.027	0.922***	1.207***	0.840***	0.210
	(0.061)	(0.113)	(0.026)	(0.040)	(0.014)	(0.023)	(0.058)	(0.089)	(0.289)	(0.387)
Interaction Treat. x Year	**-0.324*** **	**-0.491*** **	**-0.143*** **	**-0.163*** **	**-0.015**	**-0.039**	**-0.241*** **	**-0.364*** **	**-0.786* **	**-0.910** **
	(0.092)	**(0.109)**	**(0.039)**	**(0.045)**	**(0.022)**	**(0.025)**	**(0.086)**	**(0.100)**	**(0.402)**	**(0.437)**
School IPVS	-	0.825***	-	0.189***	-	0.175***	-	0.275*	-	0.189
	-	(0.212)	-	(0.055)	-	(0.041)	-	(0.140)	-	(0.490)
Control variables	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Constant	9.194***	5.908***	3.614***	1.809***	4.084***	2.843***	4.855***	3.480***	8.540***	7.435***
	(0.081)	(0.862)	(0.023)	(0.256)	(0.015)	(0.169)	(0.054)	(0.572)	(0.239)	(2.293)
N. of Obs.	130,850	129,298	55,922	55,212	128,323	126,803	69,260	68,486	9,590	9,526

Notes: standard errors clustered by school in parentheses; all models estimated with schools in the IPVS interval $[2.0; 4.0]$; excluded instruments: $z = 1[IPVS \geq 3.0]$ and $z * year\ dummy$; $*** \ p < 0.01, \ ** \ p < 0.05, \ * \ p < 0.1$.

Adding control variables and instrumenting by the rule of assignment do not change results qualitatively. ALE schools reduced about 0.16 day of medical absence, 0.36 day of justified absence and almost 1 day of unjustified ones. These results are consistent with the argument that the wage gap generated by ALE may reduce absenteeism, although we detect significant results for medical absences, which are not supposed to be impacted by differences in salary.

It might be the case that this model is not controlling for every bias. Specifically in the case of medical absences, teachers' individual health issues are not fully partialled out in a DID model. Table 4 brings the estimates for panel model (2). In this model control variables are reduced to number of schools the teachers work, teachers' workload and students' profile, because the remaining variables are invariant or almost invariant in time.

Table 4 - Panel estimates of the impact of ALE on teachers' absent days

	All types		Medical absence		Personal days		Justified absence		Unjustified absence	
Year dummy (=1,	-1.616***	-1.692***	-3.383***	-3.367***	-0.278***	-0.309***	0.751***	0.710***	2.791***	2.697***

if 2008)	(0.049)	(0.080)	(0.047)	(0.079)	(0.013)	(0.021)	(0.061)	(0.099)	(0.465)	(0.771)
Interaction Treat. x Year	**-0.386*** **	**-0.370*** **	**-0.111**	**-0.122**	**-0.020**	**-0.019**	**-0.365*** **	**-0.353*** **	**-1.805** **	**-1.875** **
	(0.079)	**(0.080)**	**(0.074)**	**(0.075)**	**(0.021)**	**(0.021)**	**(0.097)**	**(0.098)**	**(0.758)**	**(0.768)**
Control variables	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Constant	10.008***	9.873***	4.205***	3.465***	4.310***	4.429***	5.019***	5.090***	7.645***	5.811
	(0.023)	(0.378)	(0.014)	(0.369)	(0.006)	(0.098)	(0.027)	(0.463)	(0.225)	(3.620)
N. of Obs.	125,099	124,659	54,350	54,137	122,953	122,524	66,644	66,412	9,241	9,223

Notes: all models control for teachers' and schools' fixed effects; all models estimated with schools in the IPVS interval $[2.0; 4.0]$; excluded instruments: $z = 1[IPVS \geq 3.0]$ and $z * year\ dummy$; $*** \ p < 0.01, \ ** \ p < 0.05, \ * \ p < 0.1$.

One can see that after controlling for schools' and teachers' fixed effects, estimates for medical absences lose significance. So, we cannot affirm that the wage differentiation brought by ALE affects the paid absence, as expected. On the other side, estimates for the unpaid absences are still negative and very precise.

Then, the whole set of estimates confirms that ALE has reduced teacher absenteeism and this effect is, in fact, driven by the monetary incentive. On average, the point estimates mean that raising teacher hourly wage by 26% reduced justified absences by 0.3 day, a drop of 6% over the pre-ALE average, and the unjustified ones by 1.9 day, 23% over the pre-ALE average. In the overall effect, the reduction in absenteeism is 4%.

Besides, estimates show that teachers react by reducing more the unjustified absence than the justified ones. This can be explained by the fact that the first one is (potentially) more costly, as it may cause teacher's dismissal. So, unjustified absences shall have a higher expected cost for teachers, although the immediate cost of wage discount is the same for types of absences.

Table 5 - Panel estimates of the impact of ALE on teachers' absent days separated by ALE's weight on wage

	All types		Medical absence		Personal days		Justified absence		Unjustified absence	
	Above median	Below median	Above median	Below median	Above median	Below median	Above median	Below median	Above median	Below median
Year dummy (=1, if 2008)	-1.504***	-1.809***	-3.354***	-3.334***	-0.300***	-0.296***	0.869***	0.469***	3.331***	1.165
	(0.118)	(0.114)	(0.117)	(0.114)	(0.030)	(0.031)	(0.138)	(0.156)	(1.086)	(1.225)
Interaction Treat. x Year	**-0.456*** **	**-0.289*** **	**-0.099**	**-0.109**	**-0.050**	**0.010**	**-0.407*** **	**-0.328** **	**-2.459** **	**-0.865**
	(0.123)	**(0.111)**	**(0.111)**	**(0.110)**	**(0.031)**	**(0.030)**	**(0.140)**	**(0.149)**	**(1.111)**	**(1.150)**
Control variables	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constant	9.805***	10.200***	3.229***	3.785***	4.497***	4.437***	4.377***	6.361***	4.487	12.400**
	(0.561)	(0.561)	(0.541)	(0.561)	(0.143)	(0.150)	(0.640)	(0.754)	(5.198)	(5.810)
N. of Obs.	70,782	53,874	30,295	23,841	69,405	53,118	39,507	26,902	5,605	3,619

Notes: median of ALE's weight is 26% (weights equal 26% were included in above median); all models control for teachers' and schools' fixed effects; all models estimated with schools in the IPVS interval $[2.0; 4.0]$; instruments: $z = 1[IPVS \geq 3.0]$ and $z * year\ dummy$; $*** \ p < 0.01, \ ** \ p < 0.05, \ * \ p < 0.1$.

In this table, teachers are divided into two groups, according to the proportion of the hourly wage increase. Teachers above the median (26%) receive, on average, an increase of 28.8% in hourly wage and those below the median receive an average increase of 24.5%.

The table shows that the amount of wage increase matters. Teachers receiving higher increases react by reducing more their absences. This is valid for the whole set of absences as well as for justified and unjustified absences. So, this heterogeneity reinforces our main argument, that teachers' decision of being absent may be influenced by monetary incentives.

7. Conclusion

Teacher absenteeism is an important issue for personal economics in education, with negative consequence on students' performance. Monitoring policies to address teacher absenteeism are generally pointed as effective solutions by the literature, although it depends on institutional design. On the other side, monetary incentives to teachers have less clear results on absenteeism.

Theoretically, the impacts of financial incentives on absenteeism shall be ambiguous, so its effectiveness is an empirical question. In this chapter we evaluate this issue through a policy that exogenously raised wages for part of the teachers in São Paulo public school system. The allocation of this incentive is fully determined by school location and does not depend on teachers' behavior. This design allows the estimation of a causal relation between remuneration and the decision of missing work.

After controlling for teachers' and schools' unobserved fixed effects, estimates show that an average increase of 26% in hourly wage leads to a reduction in overall absenteeism of 4%. Significant estimates are only valid for absences with wage discount, which reinforces the hypothesis that teacher absenteeism reacts to monetary incentives. We also find stronger impacts for absences that may cause higher costs for teachers and for teachers that receive higher increases.

In sum, this chapter brings interesting evidence on how teacher absenteeism may be influenced by monetary incentives. However, for a policy discussion, it is necessary to point that this policy only reduces the unpaid absences, which are practically costless for government.

Besides, the effects seem quite low: the reduction of 1% in absenteeism is associated to more than 4% wage increase. Although the literature helps us to argue about the benefits of less teacher absences for students (by avoiding losing classes or taking it with substitute teachers), it is not possible to confirm it with our available data.

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Appendix

Table A1.1 - Differences in differences full 2SLS estimates of the impact of ALE on teachers' absent days

	All types	Medical absence	Personal days	Justified absence	Unjustified absence
First stage for variable 'treatment dummy'					
Year dummy (=1, if 2007)	-0.0276	-0.0212	-0.0280	-0.0308	-0.0147
	(0.0021)	(0.0035)	(0.0022)	(0.0030)	(0.0078)
Z (IPVS >3.0)	0.7158	0.7135	0.7162	0.7058	0.7381
	(0.0031)	(0.0045)	(0.0031)	(0.0043)	(0.0113)
Z * Year dummy	0.0008	0.0019	0.0009	0.0072	-0.0078
	(0.0025)	(0.0042)	(0.0026)	(0.0035)	(0.0090)
School IPVS	0.1181	0.1201	0.1175	0.1222	0.1084
	(0.0022)	(0.0033)	(0.0022)	(0.0030)	(0.0078)
Number of schools teacher works	-0.0077	-0.0086	-0.0080	-0.0071	-0.0030
	(0.0013)	(0.0020)	(0.0013)	(0.0018)	(0.0041)
Teacher workload	0.0002	0.0003	0.0002	0.0003	0.0003
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Teacher gender (female = 1)	-0.0056	-0.0012	-0.0057	-0.0077	-0.0025
	(0.0016)	(0.0025)	(0.0016)	(0.0020)	(0.0046)
Teacher tenure	0.0001	0.0000	0.0001	0.0000	-0.0006
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0003)
Type of contract (permanent = 1)	0.0022	-0.0014	0.0021	0.0043	0.0033
	(0.0014)	(0.0021)	(0.0014)	(0.0019)	(0.0048)
Principal age	-0.0011	-0.0013	-0.0011	-0.0012	-0.0011
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0003)
Principal tenure	0.0009	0.0008	0.0009	0.0006	-0.0004
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0006)
Prop. male students	0.0467	0.0661	0.0456	0.0004	0.0111
	(0.0139)	(0.0217)	(0.0140)	(0.0192)	(0.0483)
Prop. retained students	-0.1171	-0.0989	-0.1163	-0.0909	0.1116
	(0.0132)	(0.0202)	(0.0133)	(0.0182)	(0.0454)
Avg. mother education	-0.0189	-0.0530	-0.0202	-0.0379	-0.0708
	(0.0126)	(0.0191)	(0.0127)	(0.0175)	(0.0468)
Avg. father education	-0.2021	-0.1683	-0.2021	-0.1856	-0.1871
	(0.0127)	(0.0197)	(0.0129)	(0.0179)	(0.0461)
Constant	-0.0826	-0.0948	-0.0808	-0.0671	-0.0601
	(0.0119)	(0.0182)	(0.0120)	(0.0165)	(0.0424)

Notes: standard errors clustered by school in parentheses; all models estimated with schools in the IPVS interval [2.0;4.0].

Table A1.2 - Differences in differences full 2SLS estimates of the impact of ALE on teachers' absent days

	All types	Medical absence	Personal days	Justified absence	Unjustified absence
First stage for variable 'interaction treat. x year'					
Year dummy (=1, if 2007)	0.0280	0.0396	0.0278	0.0263	0.0246
	(0.0015)	(0.0020)	(0.0015)	(0.0021)	(0.0063)
Z (IPVS >3.0)	-0.0829	-0.0530	-0.0832	-0.0831	-0.1162
	(0.0022)	(0.0025)	(0.0022)	(0.0031)	(0.0091)
Z * Year dummy	0.8820	0.8833	0.8820	0.8854	0.8968
	(0.0018)	(0.0024)	(0.0018)	(0.0025)	(0.0072)
School IPVS	0.0601	0.0382	0.0602	0.0592	0.0775
	(0.0016)	(0.0019)	(0.0016)	(0.0022)	(0.0063)
Number of schools teacher works	-0.0017	0.0004	-0.0020	-0.0002	0.0025
	(0.0009)	(0.0011)	(0.0010)	(0.0013)	(0.0033)
Teacher workload	0.0000	0.0000	0.0000	0.0000	0.0001
	(0.0000)	(0.0001)	(0.0000)	(0.0001)	(0.0002)
Teacher gender (female = 1)	-0.0018	0.0005	-0.0018	-0.0029	-0.0055
	(0.0011)	(0.0014)	(0.0011)	(0.0015)	(0.0037)
Teacher tenure	0.0000	0.0000	0.0000	0.0000	-0.0005
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0003)
Type of contract (permanent = 1)	0.0022	0.0005	0.0023	0.0045	0.0031
	(0.0010)	(0.0012)	(0.0010)	(0.0014)	(0.0039)
Principal age	-0.0005	-0.0003	-0.0004	-0.0005	-0.0009
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0003)
Principal tenure	0.0004	0.0001	0.0004	0.0002	-0.0004
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0004)
Prop. male students	-0.0305	-0.0182	-0.0311	-0.0645	-0.1199
	(0.0099)	(0.0122)	(0.0100)	(0.0138)	(0.0389)
Prop. retained students	-0.0659	-0.0327	-0.0659	-0.0659	0.0582
	(0.0094)	(0.0114)	(0.0095)	(0.0131)	(0.0366)
Avg. mother education	0.0599	0.0397	0.0594	0.0546	0.0477
	(0.0090)	(0.0108)	(0.0091)	(0.0126)	(0.0377)
Avg. father education	-0.1604	-0.1006	-0.1606	-0.1580	-0.1973
	(0.0091)	(0.0111)	(0.0092)	(0.0128)	(0.0371)
Constant	-0.0467	-0.0362	-0.0466	-0.0253	-0.0073
	(0.0085)	(0.0102)	(0.0086)	(0.0118)	(0.0341)

Notes: standard errors clustered by school in parentheses; all models estimated with schools in the IPVS interval [2.0;4.0].

Table A1.3 - Differences in differences full 2SLS estimates of the impact of ALE on teachers' absent days

	All types	Medical absence	Personal days	Justified absence	Unjustified absence
			Second stage		
Treatment dummy	-0.3280	-0.1082	-0.0386	-0.1246	-0.3656
	(0.3322)	(0.0885)	(0.0643)	(0.2263)	(0.7476)
Interaction Treat. x Year	-0.4907	-0.1626	-0.0391	-0.3643	-0.9096
	(0.1092)	(0.0449)	(0.0245)	(0.0996)	(0.4368)
Year dummy (=1, if 2008)	-0.2328	-1.8031	-0.0269	1.2072	0.2097
	(0.1131)	(0.0399)	(0.0233)	(0.0890)	(0.3866)
School IPVS	0.8246	0.1893	0.1752	0.2748	0.1889
	(0.2124)	(0.0550)	(0.0412)	(0.1403)	(0.4902)
Number of schools teacher works	1.2439	0.2224	0.2397	0.5677	0.6628
	(0.0589)	(0.0218)	(0.0120)	(0.0440)	(0.1592)
Teacher workload	-0.0062	0.0097	0.0059	-0.0125	-0.0182
	(0.0024)	(0.0013)	(0.0006)	(0.0021)	(0.0081)
Teacher gender (female = 1)	-1.6903	0.3149	-0.0925	-1.2128	-1.6495
	(0.0848)	(0.0281)	(0.0147)	(0.0586)	(0.2142)
Teacher tenure	0.0269	0.0109	0.0126	0.0230	0.0468
	(0.0041)	(0.0016)	(0.0009)	(0.0032)	(0.0144)
Type of contract (permanent = 1)	1.7741	0.3465	0.5540	0.9910	3.5248
	(0.0723)	(0.0264)	(0.0159)	(0.0555)	(0.2234)
Principal age	-0.0346	-0.0031	-0.0058	-0.0086	0.0029
	(0.0079)	(0.0020)	(0.0014)	(0.0051)	(0.0181)
Principal tenure	0.0134	-0.0022	-0.0001	0.0019	0.0034
	(0.0130)	(0.0033)	(0.0025)	(0.0079)	(0.0304)
Prop. male students	-3.1748	0.4469	-0.3328	-1.8685	1.2595
	(0.7764)	(0.2778)	(0.1622)	(0.5591)	(2.2862)
Prop. retained students	13.8375	2.1087	1.9283	6.6650	-2.0477
	(0.9992)	(0.2678)	(0.1995)	(0.6498)	(2.3779)
Avg. mother education	3.7693	0.1792	0.6209	2.0087	1.8306
	(0.8604)	(0.2426)	(0.1697)	(0.6582)	(2.4014)
Avg. father education	-0.1439	-0.2634	-0.0846	0.1139	-4.9588
	(0.8505)	(0.2530)	(0.1680)	(0.6161)	(2.4088)
Constant	5.9077	1.8090	2.8429	3.4799	7.4351
	(0.8617)	(0.2561)	(0.1693)	(0.5716)	(2.2926)
N. of Obs.	130,850	55,922	128,323	69,260	9,590

Notes: standard errors clustered by school in parentheses; all models estimated with schools in the IPVS interval [2.0;4.0]; excluded instruments: z=1[IPVS>=3.0] and z*year dummy; instrumented: treatment dummy and treat. x year; *** p<0.01, ** p<0.05, * p<0.1.

Table A2 - Panel full estimates of the impact of ALE on teachers' absent days

	All types	Medical absence	Personal days	Justified absence	Unjustified absence
Year dummy (=1, if 2008)	-1.693***	-3.367***	-0.310***	0.709***	2.697***
	(0.080)	(0.079)	(0.021)	(0.099)	(0.771)
Interaction Treat. x Year	-0.370***	-0.122	-0.019	-0.352***	-1.875**
	(0.080)	(0.075)	(0.021)	(0.098)	(0.768)
Number of schools teacher works	0.324***	-0.127*	-0.005	0.315***	0.544
	(0.070)	(0.067)	(0.018)	(0.081)	(0.622)
Teacher workload	0.018***	0.019***	0.005***	0.002	-0.021
	(0.003)	(0.004)	(0.001)	(0.004)	(0.034)
Prop. male students	-1.089**	1.125**	-0.345***	-1.017	4.957
	(0.507)	(0.480)	(0.131)	(0.624)	(4.869)
Prop. retained students	0.279	-0.572	0.141	0.183	6.788
	(0.760)	(0.728)	(0.197)	(0.916)	(7.277)
Avg. mother education	-0.680	0.514	0.022	-0.096	-1.627
	(0.571)	(0.544)	(0.148)	(0.724)	(6.227)
Avg. father education	0.280	-0.719	-0.205	0.083	-2.619
	(0.572)	(0.545)	(0.148)	(0.713)	(6.224)
Constant	9.870***	3.465***	4.428***	5.089***	5.810
	(0.378)	(0.369)	(0.098)	(0.463)	(3.620)
N. of Obs.	124,663	54,137	122,527	66,408	9,222

Notes: all models control for teachers' fixed effects; all models estimated with schools in the IPVS interval [2.0;4.0]; excluded instruments: z*year dummy; instrumented: treat. x year; *** p<0.01, ** p<0.05, * p<0.1.

Table A3 - Panel full estimates of the impact of ALE on teachers' absent days separated by ALE's weight on wage

	All types		Medical absence		Personal days		Justified absence		Unjustified absence	
	Above median	Below median	Above median	Below median	Above median	Below median	Above median	Below median	Above median	Below median
Year dummy (=1, if 2008)	-1.5***	-1.81***	-3.35***	-3.34***	-0.30***	-0.29***	0.86***	0.468***	3.33***	1.165
	(0.118)	(0.115)	(0.117)	(0.114)	(0.030)	(0.031)	(0.138)	(0.156)	(1.086)	(1.225)
Interaction Treat. x Year	-0.4***	-0.28***	-0.099	-0.104	-0.050	0.009	-0.40***	-0.327**	-2.45**	-0.865
	(0.123)	(0.111)	(0.111)	(0.110)	(0.031)	(0.030)	(0.140)	(0.149)	(1.111)	(1.150)
Number of schools teacher works	0.38***	0.126	-0.128	-0.20**	0.001	-0.035	0.28***	0.278**	0.855	-0.739
	(0.100)	(0.106)	(0.093)	(0.104)	(0.026)	(0.028)	(0.108)	(0.132)	(0.844)	(1.048)
Teacher workload	0.01***	0.012*	0.02***	0.011	0.01***	-0.000	0.002	0.009	-0.028	-0.008
	(0.004)	(0.007)	(0.005)	(0.008)	(0.001)	(0.002)	(0.005)	(0.010)	(0.037)	(0.092)
Prop. male students	-1.348*	-0.945	1.039	1.157	-0.53***	-0.091	0.096	-2.86***	6.849	1.840
	(0.777)	(0.700)	(0.706)	(0.707)	(0.198)	(0.187)	(0.887)	(0.946)	(7.118)	(7.160)
Prop. retained students	0.744	-0.466	-0.267	-0.723	0.029	0.291	0.592	-0.807	0.118	14.879
	(1.111)	(1.107)	(1.044)	(1.089)	(0.283)	(0.295)	(1.240)	(1.472)	(9.999)	(11.61)
Avg. mother education	-0.802	-0.306	0.257	0.997	0.027	-0.006	-0.358	0.579	4.508	-13.669
	(0.864)	(0.797)	(0.798)	(0.793)	(0.220)	(0.213)	(1.001)	(1.129)	(8.857)	(9.482)
Avg. father education	0.455	-0.007	-0.439	-1.020	-0.350	0.037	0.447	-1.018	-7.162	-0.524
	(0.859)	(0.809)	(0.781)	(0.807)	(0.219)	(0.216)	(0.978)	(1.123)	(8.870)	(9.709)
Constant	9.80***	10.2***	3.229***	3.783***	4.498***	4.438***	4.377***	6.359***	4.489	12.39**
	(0.561)	(0.561)	(0.541)	(0.560)	(0.143)	(0.150)	(0.640)	(0.754)	(5.198)	(5.810)
N. of Obs.	70,792	53,871	30,299	23,838	69,413	53,114	39,508	26,900	5,604	3,618

Notes: the median of ALE's wegiht is 26%; all models control for teachers' fixed effects; all models estimated with schools in the IPVS interval [2.0;4.0]; instruments: z*year dummy; instrumented: treat. x year; *** p<0.01, ** p<0.05, * p<0.1.