Does the Second Teacher Matter? Effects on Enrollment and Grade Completion in Primary Single-Teacher Schools in Rural Peru

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Abstract

This paper evaluates the impact of adding a second teacher to primary single-teacher schools in rural Peru where more than one in five enrolled students fails class every year. Matched difference-in-difference analysis shows a positive enrollment effect of about 14 percent, mainly from increased grade completion and reduced between-year drop-out *before* treatment, i.e., in anticipation of improved schooling. Grade completion *levels* are increased after treatment due to the enrollment effect; the actual decrease in the student-teacher ratio of almost 40 percent, however, does not lead to a further significant improvement in grade completion *rates*. Increasing teacher quantity is thus unlikely to solve Peru's problem of educational inefficiency.

JEL Codes: I21
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1.1 Introduction

In the process towards universal primary education – Millennium Development Goal No. 2 – many developing countries are scaling up their primary school coverage while the quality of the system deteriorates as educational expenditures do not increase alike. In international student achievement tests, some developing countries with high coverage perform dismally. As quality lags behind, the system becomes clogged by students that do not progress through school in time, a phenomenon often termed *educational wastage*. Many students keep repeating the same grades because they are not promoted to higher grades, creating a vicious cycle: over-aged students become a drain on the remaining class by diverting scarce education materials and teacher attention away from others. Also, repeaters are more likely to drop out of school permanently with insufficient education (UNESCO 1998). Peru is a poignant example of an economically advancing developing country with such problems in the education sector. Through steady enrollment increases, Peru has almost achieved universal primary school coverage. Nevertheless, educational inefficiency is very high – 18 percent of primary students failed to complete the grade in 2004, and only 73 percent of 12 to 15 year olds had completed the 6-year cycle of primary education in 2003 (MINEDU 2005).

This paper analyses the effect of a reduction in the student-teacher ratio in primary single-teacher schools in rural Peru when a second instructor is added. The effectiveness of teachers should be under close scrutiny since they consume most of the small educational budgets in developing countries. Particularly, researchers still controversially and inconclusively debate about the importance of the student-teacher ratio, i.e., the average number of teachers per student. At the same time, changes in this ratio have huge budgetary consequences and can bind or free up resources for other educational inputs. For example, the World Bank (2007) estimates that in 2005, Peru spent 83 percent of current and 75 percent of total expenditure on wages and salaries. Also, it maintains a student-teacher ratio of about 24 which is close to the average of Latin America while its GDP per capita is considerably lower than the Latin American average. The World Bank concludes that Peru's student-teacher ratio may be too low, considering that there is little proof of the effectiveness of more teachers on student outcomes.

Since indicators of enrollment, learning achievement and grade completion are lowest in poor rural communities of developing countries, more teachers may be most effective in this context, if at all. In sparsely populated rural areas, children often acquire education in small multi-grade schools where teacher teach multiple grades at a time. In the extreme, only one teacher is responsible for the whole school. In these single-teacher schools, the addition of a second teacher reduces multi-grade teaching and class size and may thus be a strong driver of improvement. Theory implies that increases in school inputs have a non-decreasing effect on the level of enrollment and an ambiguous effect on grade completion levels and rates if they improve school quality. Findings on the impact of changes in the student-teacher ratio in schools at the bottom of the quality distribution can inform policymakers on the trade-off between teacher quantity and other educational inputs. While much empirical work has addressed the effect of school inputs on cognitive educational achievement (cf. Hanushek 2003), less effort has been devoted to the equally important questions of their impact on enrollment and school progression.

In order to inform about the effect of lower student-teacher ratios, I employ matched difference-in-difference estimates using a unique longitudinal school census data set from Peru. Difference-in-difference estimation allows understanding the addition of a second teacher as a treatment to single-teacher schools and calculating its effect on educational outcome variables. Before estimating, however, I employ propensity score matching to mitigate possible bias of results by creating an appropriate control group in observational data.

Matched difference-in-difference estimates show a positive enrollment effect of about 14 percent which translates into increased grade completion *levels*. The analysis suggests that most of the enrollment effect is caused by reduced grade failure in the first year and lower between-year drop-out *before* introducing the second teacher in treated schools, possibly in anticipation of improved schooling conditions. Via increased enrollment, treated schools produce significantly more grade completers. Nevertheless, the analysis also shows that there is no additional significant after-treatment effect on grade completion *rates* despite an almost 40 percent improvement in the mean student-teacher ratio.

There are several possible reasons why the analysis does not show an after-treatment impact on grade completion rates: first, treatment keeps more students in school who would have dropped out in the absence of smaller classes and have high propensities to fail. Second, additional teachers willing to teach in remote areas may be from the bottom of the teacher quality distribution such that teacher training, e.g., on multigrade teaching, may be more effective than reducing class sizes. Indeed, I find that second teachers have relatively more non-permanent positions and work fewer hours even though this may be unrelated to teacher quality. Third, out of school factors rooted in the economic and social environment of children may play a predominant role in poor rural areas, such as low and volatile household incomes. These factors may be unrelated to educational policy and thus harder to address.

1.2 Background

1.2.1 Inefficiency in Primary Education in Peru

The Peruvian school system is divided into pre-primary, primary, secondary and higher education. Primary education consists of 6 grades and starts at age 6. In principle, primary and secondary education in Peru is free and compulsory, but households face substantial costs of education[1] and enforcement of attending school is difficult in remote areas.

There are three categories of primary school according to the relative number of teachers present: complete, multi-grade, and single-teacher schools. In the first case, the number of teachers equals or exceeds the number of classes. In the second case, at least two teachers are present in school, however, there are more grade levels than teachers thus resulting in grouping of classes. In the last case, there is also multi-grade teaching but only one teacher exists for all students of all grades, typically teaching them altogether in one classroom. Sparsely populated regions, especially in the Andes and the Amazon basin, inhibit appropriate schooling conditions for many students in those remote places. As a result, multi-grade schools are wide-spread (Hargreaves et al. 2001).

Table 1 summarizes school characteristics in 2004 by type of public schools, adding private schools as an additional category. Single-teacher and multi-grade schools account for about two thirds of the school universe in Peru and host about one third of students. They are predominantly rural and more than 60 percent of all schools are located in the poorest quintile of districts.[2] Almost all urban schools are complete schools, and more than 90

[1] See Saavedra and Suárez (2002).
[2] Poverty was calculated based on the Peruvian national census 2005 using a district deficiency index which includes share of illiterate women, children under 12, undernourished people, and households without access to water, electricity, sanitation.

percent of private schools are urban. All public school types have similar average student-teacher ratios, between 24 and 25, but single-teacher schools have the highest variance: at the 5th percentile there are 10 students per teacher, at the 95th percentile there are 50. Even though the Ministry of Finance has intended an average student-teacher ratio of 35 in urban and 20 in rural areas, with some variations by level and for remote areas[3], both urban and rural schools have close to 25 students per teacher (not shown).

Table 1. Summary Statistics by School Type, 2004

	Public Single-Teacher		Public Multi-Grade		Public Complete		Private (All Types)	
Share of Schools	0.255		0.394		0.167		0.184	
Share of Students	0.068		0.300		0.440		0.192	
Enrollment	24.9	(13.0)	69.5	(45.1)	245.0	(208.3)	95.9	(113.7)
Teachers	1.00	(0.00)	2.89	(1.28)	10.37	(6.48)	7.27	(5.52)
Student-Teacher Ratio	24.90	(12.97)	23.86	(8.72)	24.73	(6.81)	12.48	(8.51)
Lowest Community Quintile	0.66	(0.48)	0.61	(0.49)	0.36	(0.48)	0.05	(0.21)
Rural	0.99	(0.09)	0.96	(0.21)	0.52	(0.50)	0.07	(0.26)
N	8182		12622		5367		5896	

Source: Own estimates based on school census data 2004. Note: Means in the left column, standard deviations in brackets.

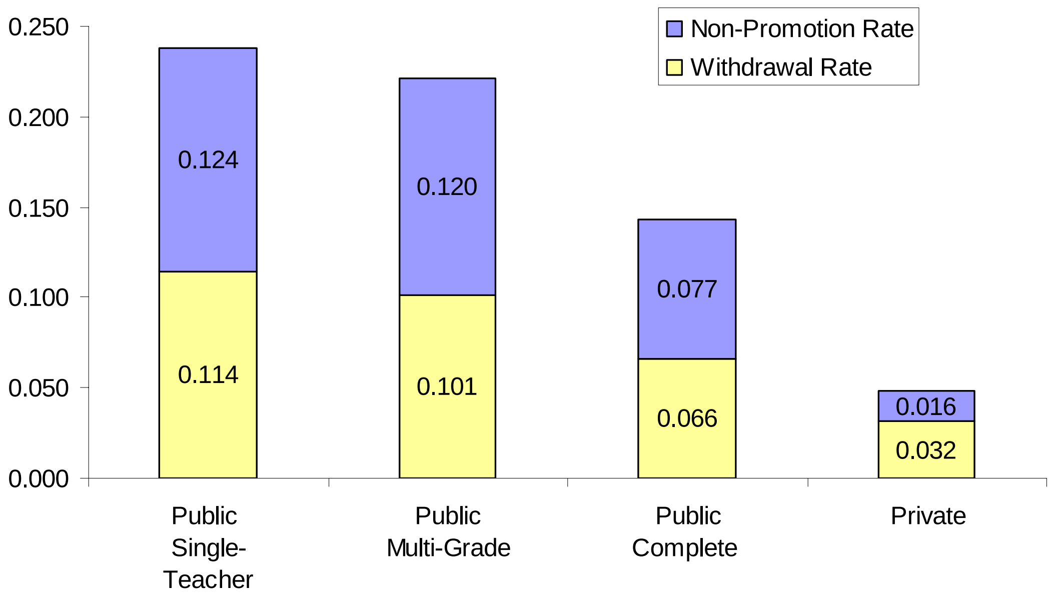
Peru has made significant progress in the expansion of primary school coverage for its population. Based on calculations from the national household survey ENAHO, according to the Ministry of Education (MINEDU 2005), in 2003, 96 percent of all children between 6 and 11 years old were enrolled in school. This figure distributes evenly between boys and girls, with a bias towards urban versus rural areas (98 to 93 percent). While among the non-poor, 99 percent of children were enrolled in school, this figure drops to 97 percent for poor children and 93 percent among the extremely poor.

Along with high coverage, educational *wastage* – inefficiency due to drop-out or grade repetition – is pervasive in Peru. In 2003 approximately 91 percent of 15- to 17-year-olds had completed primary education, but only 73 percent of 12- to 15-year-olds (MINEDU 2005). Taking into account that primary school can be completed at age 11, many students finish with significant delay. Among the extremely poor, figures are even more drastic, with 54 percent of the population between age 12 and 14, and 78 percent between age 15 and 17, having completed six grades of primary education.

[3] See World Bank (2001).

Peru's educational inefficiency resides strongly in high grade non-completion rates (see Figure 1). The non-promotion rate refers to the share of students enrolled and showing sufficient attendance but failing the grade due to non-promotion by decision of the teacher. The withdrawal rate denotes the share of students enrolled but failing the grade due to within year drop-out or insufficient attendance. The sum of non-promotion and withdrawal rate, i.e., the total share of students not completing the grade, is the failure rate. In single-teacher and multi-grade schools, more than 20 percent of all students each year fail to complete the grade. Withdrawal and non-promotion contribute almost equally to grade failure. This compares to around 14 percent grade failure in complete multi-grade schools, and 5 percent in private schools. Due to a national average failure rate of 18 percent, schools host many over-aged repeaters and by grade 6, public school students who have not dropped out are on average 1.3 to 1.4 years too old (not shown).

Figure 1. Summary Statistics by School Type, 2004



Source: Own estimates based on school census data 2004.

There are obvious economic reasons why one should care about drop-out and grade repetition: costs. UNESCO (1998) estimates that in developing countries, between 10 and 40 percent of total public current expenditure on education are spent on wastage before grade 5. Repeaters use more resources such as teaching time, space, textbooks, school meals, etc. which may be saved or used for other children, and create a heterogeneity in class that distorts normal instruction. Educational inefficiency puts a burden onto the whole economy, in the form of reduced growth perspectives. In developing countries, this is especially true for rural regions. Drop-out and grade repetition also result in costs at the individual level by causing low self-esteem, negative attitudes towards school and higher propensity for criminality. Droppers often relapse into illiteracy. Furthermore, there tends to be a reinforcement of discrimination as children from poorer households often remain uneducated.

1.2.2 School Quality, Enrollment and Grade Non-completion

Generally, two causes for school inefficiency can be distinguished: those rooted in economic and social environment of children, out-of-school reasons, and those rooted in the school system, in-school reasons (Randall, Anderson 1999). This paper concentrates on the latter, specifically teachers as school inputs and their effect on student enrollment and grade completion.

Enrollment

In a simple utility calculation a student weighs the benefits and costs of completing an additional grade in school.[4] Enrollment occurs if the value added from an additional year of schooling is positive, i.e., the benefits of schooling exceed the costs. Benefits of education typically include intrinsic valuation of schooling and the wage return after completing the additional year both of which depend on the quality of education in the respective grade. The costs of an additional year of schooling can be direct, such as school fees, transportation costs, or costs of learning materials, and indirect opportunity costs. A household sending its child to school faces an opportunity cost from losing a worker in the household or labor market. This cost increases in household deprivation and the wage equivalent for the student from not going to school.

Withdrawal

Why would children enroll for school and subsequently withdraw? One possibility is the presence of incentives, e.g. enforcement of penalties for not enrolling but no enforcement of attendance. Another potential explanation is that parameters in the utility calculation change during the year, e.g., with the occurrence of shocks to household wealth, the labor market or school quality. Furthermore, there may be uncertainty about the parameters necessary to decide on the additional year of schooling at the time of the enrollment choice. Students may enroll if their expected value of schooling was positive

[4] The setup abstracts from schooling choice, assuming one available school for each student.

before enrollment and withdraw during the year if uncertainty is resolved and the resulting utility outcome has turned negative. The quality of schooling may be one of these parameters for which uncertainty resolves after enrollment.[5] If increases in quality trigger increased enrollment, withdrawal rates may rise if the marginal students' propensity to withdraw is higher than that of previous students.

Non-promotion

As for students who do show sufficient attendance for possible promotion to the next grade, probability of promotion should be a non-decreasing function of the student's learning achievement. Learning achievement will weakly increase in educational quality, inputs and the student's ability, effort and attendance. However, in an attempt to optimize their use of time between leisure, studying and work, students may scale back studying effort as a response to an increase in educational inputs. Also, the decision of being promoted depends on the teacher's assessment of the student's achievement at the end of the grade which may have a non-meritocratic component attached to it. For example, evidence for discrimination based on social background and previous grade repetition has been found for Honduras (Marshall 2003, McGinn et al 1992). Educational inputs thus do not necessarily increase promotion probabilities among students.

A simple theory (cf. Manski 1989) predicts that increases in school quality should increase the returns to schooling and thus have a non-decreasing effect on enrollment. The effect on failure levels and rates is ambiguous because of quality effects on previous students, who would also have enrolled under the old quality level, and newly attracted students: if newly attracted students have some positive probability of failing, failure levels and rates may rise if they are not offset by decreased failure levels and rates of the previous students.[6] As a consequence, increases in quality may increase enrollment and change failure levels and rates in any direction.

School Inputs and School Quality

The quality of an educational system is often measured by its inputs since output is harder to quantify. This neglects the complex process which transforms these educational inputs into outputs. But even by doing so, it is hard to establish a causal relationship between

[5] This does not consider the case of a positive probability of failing the school year. Also, there is no distinction of school children according to their ability.
[6] If school quality deteriorates through higher enrollment and this decrease more than offsets the original increase in quality, failure levels may even increase among students who would have enrolled under the old quality level.

inputs and outputs since outputs may also affect the inputs into the system. For example, UNESCO (1998) correlates average student-teacher ratios in primary education by country with school efficiency as an outcome variable. The coefficient of correlation is -0.65, suggesting a strong influence of the input onto the output. Low school efficiency, however, also influences educational inputs – in this example via the channel of repeating students who clog the system and take up inputs, such as teacher time, away from others.

Although school resources are known to be poor measures of school quality (Hanushek 1995, 2003) the number of teachers is an interesting educational input to study. The student-teacher ratio is a measure of average class size or real resources devoted to schools, and has been used as its proxy in the literature (e.g. Case, Deaton 1999). The amount of teachers per student translates directly into current expenditures on education. Measuring the effect of this crucial input would allow improving their allocation in the context of developing countries with scarce budgetary resources. Teachers are among the most important determinants of children's education and even though teacher quality has been shown to affect outcomes more than teacher quantity (Rivkin et al. 2005), quantity is more easily observable and measurable.

In poor and sparsely populated regions of developing countries, enrollment, class-size and the student-teacher ratio often coincide when the entire student body is taught in one classroom by one teacher. Changes in the number of teachers in this setting can reasonably be assumed to be big changes in school quality: not only does the student-teacher ratio halve with an additional teacher, but students also gain from sharing their teacher with fewer other grades, creating more homogeneous classrooms. In this paper, I thus approximate a change in school quality by the addition of a second teacher to a single-teacher school.

1.2.3 Literature Review

Few analyses on enrollment and grade completion in developing countries examine in-school rather than out-of-school determinants. On the one hand, most of such analyses are performed at the individual level and examine individual, household and community factors, but usually not school characteristics, which drive school progression (e.g., Duryea et al. 2007 on income risk; Evans, Miguel 2007 on parent death; Pal 2004 on various factors; Meekers, Ahmed 1999 on pregnancy). On the other hand, analyses at the school or class level are often focused on learning achievement, not school progression (e.g., Krishnan et al 2005 on teacher absence, McEwan 2003 on peer effects; Kingdon 1996 on teacher and school characteristics). For example, international student assessments of learning achievement that sometimes include developing countries, such as PISA, collect tremendous information on individual, classroom and school characteristics at one point in time but not over time for the same observational units. One contrary example of an analysis on school progression using both individual and school-level data over time is Hanushek et al. (2006) who estimate a behavioral model of primary school drop-out behavior. They find that students act on differences in school quality measured as expected achievement improvements, and are more likely to drop out of low quality schools because of relatively lower labor market returns compared to high quality schools.

An additional hindrance to analyses on in-school determinants of enrollment and school progression, such as possibly the student-teacher ratio, is the endogeneity problem due to omitted variable bias and reverse causality. For example, low class-size schools could be high-quality according to many characteristics, of which some are not measured. Also, bureaucrats may react to the output of schools, either by specifically allocating resources to high- or low-efficiency institutions. There are thus only few convincing attempts to estimate the effect of the student-teacher ratio on educational outcomes in developing countries that use particularities of the respective countries' institutions. For example, Case and Deaton (1999) exploit student-teacher ratio differences before the end of Apartheid in South Africa and find strong significant effects on enrollment, attainment and test scores. Angrist and Lavy (1999) exploit discontinuities in class sizes induced by Maimonides' rule in Israel and find significant effects on test scores, but only in some grades. The evidence, however, is far from conclusive.

This paper thus contributes to the aforementioned strand of literature: it provides new evidence on student-teacher ratio effects on indicators of school progression in a developing country using panel data.

1.3 Empirical Implementation

1.3.1 Data

The Peruvian school census is collected on a yearly basis by the statistical unit of the Peruvian Ministry of Education. It covers all Peruvian educational institutions over time with questionnaires specific to the type and level of institution. Information is self-reported to reflect present school registers at the date of May 30. Only information on end-of-year results, such as grade completion, is collected for the previous school year. Thus, one needs to combine the census information of two consecutive years to build a profile of the end-of-year results for students covered in the first year. The analysis uses census information from 2004, 2005 and 2006 to fully cover the years 2004 and 2005. The subset used for estimation is a cleaned sample of formal non-adult primary schools.

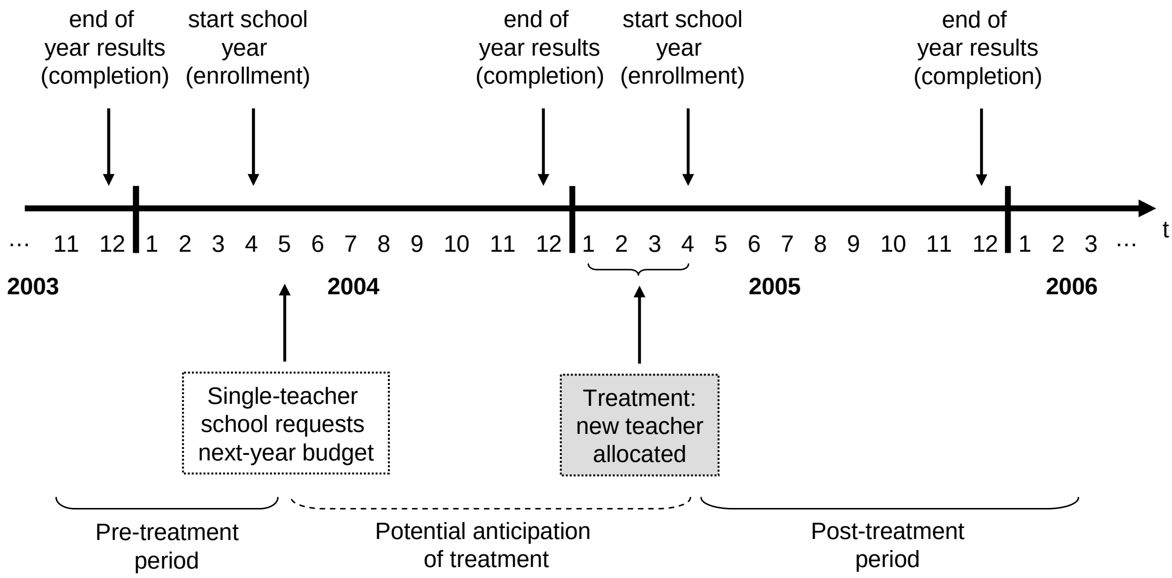
The information does not allow for individual student profiles but aggregation at the grade and school level. For example, information contains the grade structure of students according to gender, age, native language and repeater status but it is not possible to follow who exactly is failing the grade. Teacher information is collected at the school level for primary schools. School infrastructure information is also available but due to a change in questionnaire not comparable between 2004 and 2005.

By use of district identifiers, the school census data is complemented by a data set from 2005 containing district population information and proxy variables for poverty status of the communities, such as the share of households without water access or electricity.

1.3.2 Estimation Strategy and Analytical Framework

Given the difficulties to identify exogenous changes in class size or the student-teacher ratio, I use a quasi-experimental setting outlined in Figure 2 focusing on changes in the number of teachers as input changes. These changes, however, may also be prone to result from previous period outcomes, e.g., if additional teachers are allocated to particularly bad schools. This issue is addressed using retrospective data. Although experimental data are often considered more reliable, a retrospective setting does not suffer from a potential "Hawthorne" effect where participants are aware of being in an experiment and thus do not behave naturally (see Krueger 1999).

Figure 2. Time Line and Treatment Setup



First, in order to exploit changes in the number of teachers in Peruvian primary schools, I only consider the sub-sample of single-teacher schools (in the first period, 2004) in rural areas. The schools employ one teacher who is responsible for teaching up to six grades simultaneously with class sizes between a few and several dozen students. This situation is typical for rural poor regions in developing countries which perform worst in enrollment rates, grade completion and learning outcomes and are thus the most interesting unit of analysis. Also, these schools are located in sparsely populated areas characterized by lack of school choice which mitigates concerns of interaction with neighboring schools (Hargreaves et al. 2001, Urquiola 2006).

Second, I consider the addition of a second teacher to rural single-teacher schools as a treatment for which I calculate the average treatment effect on the treated by difference-in-difference estimation. The considered outcomes are enrollment, promotion and failure levels and rates. The reason for analyzing the effect of the second teacher is that an additional teacher promises highest outcome changes in the considered single-teacher schools. Not only is the student-teacher ratio halved, students also enjoy the benefit of sharing their teacher with fewer other grades such that relevant teacher time is more than doubled. The effect of further teachers is likely to be non-linear and decreasing in more teachers. One more teacher in the schools with worst outcomes has thus the least budgetary consequences but the highest possible effect.

The idea of difference-in-difference estimation is to estimate the mean impact of treatment by calculating the difference between changes over time for the treatment and control group. The key assumption concerning selection bias is that the unobserved difference in mean counterfactual outcomes between treated and untreated units is constant over time. If so, outcome changes of the control group disclose the counterfactual outcome changes of the treated units. The assumption may be problematic if treatment units have been specifically selected on the promise of yielding different rates of outcome change than untreated units.

Consequently, we need to understand the process of teacher allocation to schools and the important determinants of this process which may also influence our outcome variables of interest. The budgeting process in the education system is quite fragmented in Peru. Every year in May, one to two months after the beginning of the school year, schools present budget requests for January of the following year to Educational Service Units. They consolidate them for the Regional Directorates, which forward aggregated budgets to Transitional Councils of Regional Administration, which are again consolidated by the Ministry of the Presidency and then presented to the Ministry of Finance (MoF).

For the MoF, the foremost budgetary priority is to cover teacher salaries and pensions before recurrent expenditures may be granted to Regional Directorates for other basic services. The loose formula for allocating budget to the regions is based on a desired student-teacher ratio of 20 in rural areas and 35 in urban areas. Other educational materials, such as textbooks, are generally bought by the MoF and distributed to Regional Directorates. Afterwards, the Regional Directorates have discretionary power over forwarding budget and materials to the schools (World Bank 2001).

If treatment is dependent on first applying for a second teacher and then being allocated sufficient funds by the Regional Directorates, schools which end up with an additional teacher may differ from those which remain single-teacher schools along important dimensions. In order to mitigate the bias arising from this selectivity, I employ propensity score matching of single-teacher schools (in 2004) which do and do not receive treatment (in 2005) to construct an appropriate control group along dimension which may matter both for treatment and outcome. For example, distance of the school to the next Regional Directorate may positively influence the probability to receive a second teacher but is likely to be irrelevant for the success of students. Previous year success of stduents, however, may influence both, the probability for treatment and outcomes this year. Matching reduces the bias in double-difference estimates by eliminating initial heterogeneity of observables between the treatment and comparison group. The method is superior to propensity score matching which assumes conditional exogeneity of unobservables with respect to treatment status conditional on observables and is prone to suffer from selection bias based on latent variables (Ravallion 2007).

On the matched and pooled sample, I estimate the difference-in-difference OLS equation (1) where the null hypothesis states that treatment does not have an effect on outcome. Outcomes can be the level of enrollment, the level of grade completers and failers as well as the share of completers and failers. The equation is of the form

$$(1) \qquad Y_{st} = \beta_0 + \beta_1 T_s + \beta_2 P_t + \beta_3 T_s * P_t + \beta_4 X_{st} + e_{st}$$

where the outcome (Y) in school (s) and year (t) is a function of being in the teacher-treatment group (T), a post-treatment dummy for the year 2005 (P), the interaction effect between being in the treatment group and being in the second year (T * P), a vector of control variables (X) which are mostly also used for matching, and a random error term (e). β_3 is the main coefficient of interest, the *average treatment effect on the treated* (ATT).[7] β_2 is also of interest since it denotes the *treatment group effect*. Given propensity score matching on relevant first-period variables it is zero if there are no effects in anticipation of treatment.

[7] While the ATT, β_3 , shows the average effect of adding a second teacher to single-teacher schools, we may also be interested in the actual student-teacher ratio effect. See appendix for an elaboration and estimation results using an instrumental variables.

Table 2. Summary Statistics – Unmatched Treatment and Control Groups before Treatment (2004)

	Treatment Group		Control Group		T-Test p-value
Enrollment	39.095	(15.36)	23.861	(11.15)	0.000
Teacher w/ Permanent Contract	0.774	(0.42)	0.753	(0.43)	0.380
Teacher Male	0.580	(0.49)	0.522	(0.50)	0.031
Teacher Hours	38.011	(4.00)	38.343	(3.78)	0.106
Teacher w/ Teaching Degree	0.815	(0.39)	0.816	(0.39)	0.953
Non-Promotion Share (2003)	0.129	(0.10)	0.125	(0.11)	0.497
Withdrawal Share (2003)	0.127	(0.12)	0.126	(0.12)	0.935
Share Repeaters in Class	0.051	(0.08)	0.055	(0.09)	0.420
Share Reentrants in Class	0.123	(0.11)	0.121	(0.12)	0.794
S.D. Age Distribution	2.421	(0.48)	2.289	(0.53)	0.000
Share Working	0.137	(0.34)	0.127	(0.32)	0.598
Share Not First Language	0.164	(0.35)	0.185	(0.36)	0.269
Share Male	0.508	(0.08)	0.516	(0.12)	0.263
Morning Classes	0.793	(0.41)	0.816	(0.39)	0.277
Food Program	0.736	(0.44)	0.697	(0.46)	0.114
Health Service	0.144	(0.35)	0.153	(0.36)	0.658
Language Other Native	0.071	(0.26)	0.062	(0.24)	0.486
Language Quechua	0.057	(0.23)	0.060	(0.24)	0.817
Bilingual School	0.128	(0.34)	0.143	(0.35)	0.441
Parents Committee	0.173	(0.38)	0.212	(0.41)	0.077
Rural	1.000	(0.00)	1.000	(0.00)	.
Share No Water (D)	0.557	(0.25)	0.541	(0.25)	0.251
Share No Sanitation (D)	0.411	(0.23)	0.455	(0.24)	0.001
Share No Electricity (D)	0.671	(0.24)	0.661	(0.25)	0.451
Share Illiterate Women (D)	0.252	(0.14)	0.262	(0.14)	0.208
Share Children 0-12 (D)	0.332	(0.05)	0.333	(0.05)	0.686
Share Malnutrition '99 (D)	0.439	(0.13)	0.440	(0.13)	0.980
N	367		5183		

Source: Own estimates based on school census data 2004 only for schools with full set of control variables available. Note: Means in the left column, standard deviations in brackets. (D) denotes variables measured at the district level.

1.3.3 Propensity Score Matching

Table 2 summarizes the variables used for propensity score matching between the single-teacher schools in 2004 that do and do not receive an additional teacher in 2005, i.e., the raw treatment and control group. On average, treated schools are significantly larger and thus more likely to receive an additional teacher. A t-test of mean comparison also reveals that treatment and control group differ along other dimensions at the 10 percent significance level, such as teacher gender, age heterogeneity of students, existence of a parent committee, and the share of sanitation in the district.

Table 3 shows the results of a probit analysis of treatment on the vector of observed control variables. In line with the MoF budgeting rules, enrollment has the biggest influence on the probability of being allocated a second teacher. Additionally, the following characteristics are significantly correlated with receiving a second teacher: having a teacher on a permanent position, having a lower share of repeaters, having a higher age heterogeneity of students, offering school meals, being a native Quechua school, and being located in a district with higher rates of illiteracy among women and higher shares of children.

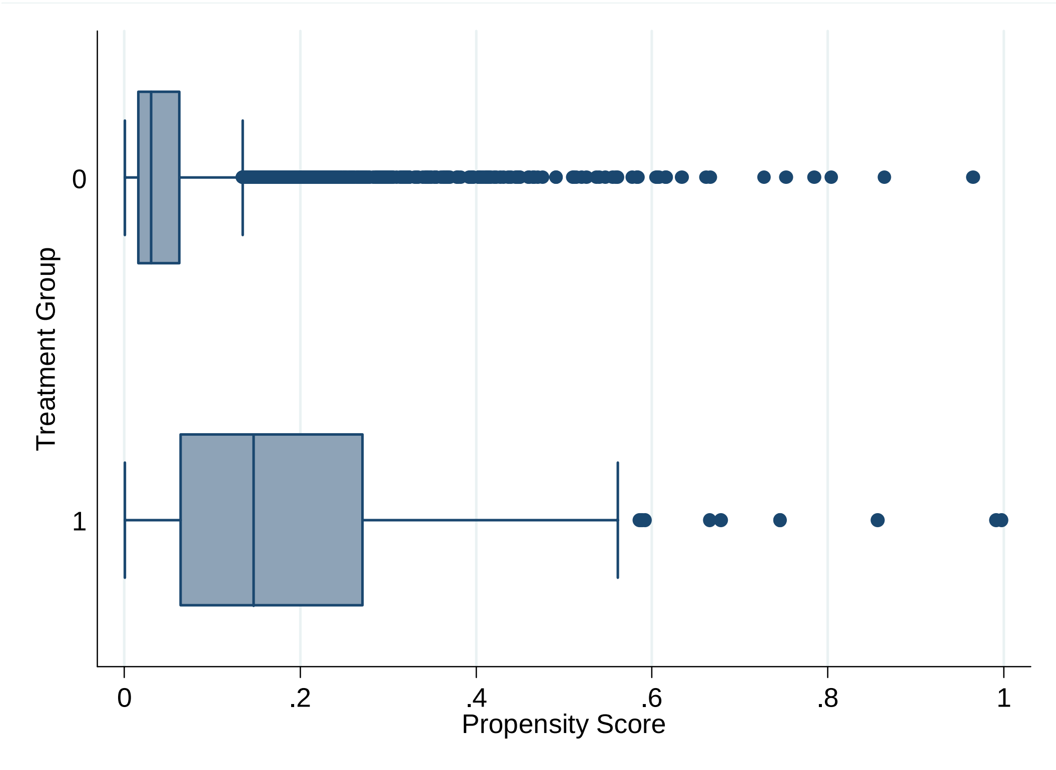
Table 3. Probit Regression of School Treatment

	Coefficient	S.E.
Enrollment	0.048 ***	(0.002)
Teacher w/ Permanent Contract	0.132 *	(0.075)
Teacher Male	0.070	(0.063)
Teacher Hours	-0.003	(0.008)
Teacher w/ Teaching Degree	0.047	(0.086)
Non-Promotion Share (2003)	-0.154	(0.419)
Withdrawal Share (2003)	0.062	(0.309)
Share Repeaters in Class	-1.002 **	(0.457)
Share Reentrants in Class	-0.169	(0.399)
S.D. Age Distribution	0.161 **	(0.063)
Share Working	0.010	(0.093)
Share Not First Language	0.099	(0.093)
Share Male	-0.489	(0.300)
Morning Classes	0.004	(0.083)
Food Program	0.130 *	(0.072)
Health Service	-0.032	(0.086)
Language Other Native	0.074	(0.146)
Language Quechua	0.310 **	(0.144)
Bilingual School	-0.038	(0.109)
Parents Committee	-0.082	(0.079)
Share No Water (D)	0.064	(0.155)
Share No Sanitation (D)	0.077	(0.158)
Share No Electricity (D)	0.192	(0.169)
Share Illiterate Women (D)	-1.110 ***	(0.360)
Share Children 0-12 (D)	-4.886 ***	(0.969)
Share Malnutrition '99 (D)	0.184	(0.390)
Constant	-1.509 ***	(0.446)
R-Squared	0.193	
Observations	5309	

Source: Own estimates based on school census data 2004. Note: Robust standard errors in brackets, significance levels: * p<0.10, ** p<0.05, *** p<0.01

If additional teachers are placed where outcomes are higher to begin with, or where the district has higher shares of literate women and lower fertility, there might be bias in difference-in-difference estimates, arising from the fact that the targeted schools may be able to show higher rates of productivity growth than their peers. As far as observable characteristics are concerned, this worry is taken care of by finding an appropriate control group via propensity score matching and making sure that covariates are balanced between the groups. A remaining concern to identification is heterogeneity between treatment and control group with respect to unobservables which induce different rates of outcome growth over time, conditional on treatment.

Figure 2. Distribution of Propensity Score by Treatment Status



Source: Own estimates based on school census data 2004.

Figure 2 shows the distribution of the estimated propensity score by treatment status. Non-treated schools are concentrated heavily in the lowest score quintile with a median of 0.03. However, due to the large number of non-treated schools, close matches for treated schools can even be found in high-score regions. Table 4 shows the resulting treatment and control group after performing nearest neighbor matching with 5 neighbors and replacement. The treatment group reduces to 349 schools after eliminating observations off the common support and those which were not allocated an appropriate neighbor within a caliper of 0.01. The control group consists of 1074 schools of which some are used more than once as an appropriate match. T-tests can never reject equality of weighted means between treatment and control group variables at the 10 percent significance level.[8]

Table 4. Summary Statistics –Matched Treatment and Control Groups before Treatment (2004)

	Trimmed Treatm. Group		Matched Control Group		T-Test p-value
Enrollment	38.384	(14.24)	38.663	(14.83)	0.777
Teacher w/ Permanent Contract	0.785	(0.41)	0.765	(0.42)	0.462
Teacher Male	0.579	(0.49)	0.581	(0.49)	0.945
Teacher Hours	38.023	(3.99)	38.106	(3.93)	0.752
Teacher w/ Teaching Degree	0.819	(0.39)	0.810	(0.39)	0.720
Non-Promotion Share (2003)	0.130	(0.10)	0.128	(0.10)	0.793
Withdrawal Share (2003)	0.127	(0.12)	0.127	(0.11)	0.930
Share Repeaters in Class	0.051	(0.08)	0.050	(0.07)	0.765
Share Reentrants in Class	0.126	(0.11)	0.128	(0.10)	0.742
S.D. Age Distribution	2.429	(0.48)	2.443	(0.52)	0.679
Share Working	0.138	(0.34)	0.134	(0.32)	0.852
Share Not First Language	0.164	(0.35)	0.177	(0.36)	0.560
Share Male	0.508	(0.08)	0.504	(0.10)	0.450
Morning Classes	0.794	(0.41)	0.801	(0.40)	0.792
Food Program	0.751	(0.43)	0.751	(0.43)	0.985
Health Service	0.152	(0.36)	0.145	(0.35)	0.765
Language Other Native	0.069	(0.25)	0.072	(0.26)	0.838
Language Quechua	0.060	(0.24)	0.063	(0.24)	0.860
Bilingual School	0.129	(0.34)	0.142	(0.35)	0.580
Parents Committee	0.181	(0.39)	0.193	(0.40)	0.630
Rural	1.000	(0.00)	1.000	(0.00)	.
Share No Water (D)	0.552	(0.25)	0.557	(0.24)	0.748
Share No Sanitation (D)	0.413	(0.23)	0.417	(0.25)	0.785
Share No Electricity (D)	0.673	(0.24)	0.675	(0.25)	0.905
Share Illiterate Women (D)	0.255	(0.14)	0.262	(0.14)	0.468
Share Children 0-12 (D)	0.332	(0.05)	0.334	(0.05)	0.480
Share Malnutrition '99 (D)	0.442	(0.13)	0.446	(0.13)	0.639
N	349		1074		
Sum of Weights	349		349		

Source: Own estimates based on school census data 2005. Note: Means in the left column, standard deviations in brackets. (D) denotes variables measured at the district level.

For an elaboration of the characteristics of new compared to old teachers, see appendix.

[8] Estimation results in the next section were qualitatively very similar when using nearest-neighbor matching with 1 neighbor or kernel matching instead of 5-nearest neighbor matching. Results are available from author upon request.

1.4 Results

Tables 5 to 11 show the results of estimating equation (1), i.e., difference-in-difference estimates with robust standard errors on the pooled sample of schools on different measures of outcome: enrollment, the student-teacher ratio, completion, failure, non-promotion and withdrawal levels and rates at the school level.[9]

Table 5 shows an important finding: the addition of a second teacher to rural single-teacher schools increases enrollment significantly: on average by 5.3 students (column 1). At 38.4 students before treatment this represents an enrollment increase of about 14 percent. The effect is almost the same (5.4 students) when including the set of control variables (column 2). At the same time, the student-teacher ratio drops considerably – the treatment effect of -15.2 students per teacher (column 3) is equivalent to a decrease of almost 40 percent.

Table 5. Matched Difference-in-Difference Estimates: Treatment Effect on Enrollment and the Student-teacher Ratio

	[1]	[2]	[3]	[4]
Dependent Variable	Enrollment		Student-teacher ratio	
Treatment*2005	5.290***	5.378***	-15.150***	-15.064***
	(1.448)	(1.318)	(1.250)	(1.143)
Treatment Group	-0.279	0.030	-0.279	-0.009
	(0.983)	(0.902)	(0.983)	(0.902)
Year 2005	-2.795***	-2.896***	-2.795***	-2.894***
	(0.897)	(0.810)	(0.897)	(0.811)
Controls	No	Yes	No	Yes
Adj.R-Squared	0.013	0.200	0.239	0.382
Observations	2846	2846	2846	2846

Source: Own estimates based on school census data. Note: Robust standard errors in brackets, significance levels: * p<0.10, ** p<0.05, *** p<0.01

There are several possibilities why enrollment increases in treated schools: (i) schools are allocated a second teacher when there is a large cohort one year before enrollment in first grade, (ii) treated schools attract students from other schools, (iii) treated schools attract

[9] Control variables are used as follows: regressions on enrollment and outcome *levels* include school and district variables; regressions on outcome *shares* include student, school and district variables. Student level: standard deviation of age distribution, share working, share with other first language than school language; School level binary variables: morning classes, food program, health service, language Quechua, language other native, bilingual school, parents committee; District level: share households without water, without sanitation, without electricity, share of illiterate women, share of children age 0-12, share of malnourished children in 1999.

formerly not enrolled students, and (iv) treated schools have lower drop-out rates between 2004 and 2005.

(i) Table 6 shows that treatment is not allocated in anticipation of a large new cohort in grade 1. The dependent variable is enrollment in grades 1 to 6 (columns 1 to 6). If (i) was the case we would observe most of the treatment effect in grade 1. Instead, we observe that the enrollment effect is spread out over 5 of 6 grades.

Table 6. Matched Difference-in-Difference Estimates: Treatment Effect on Enrollment in Grades 1 to 6

Enrollment in grades 1-6	[1]	[2]	[3]	[4]	[5]	[6]
Treatment*2005	1.293***	1.159***	0.234	0.959***	0.815***	0.918***
	(0.424)	(0.403)	(0.406)	(0.317)	(0.305)	(0.265)
Treatment Group	-0.052	-0.352	0.081	0.127	0.298	-0.073
	(0.305)	(0.284)	(0.317)	(0.223)	(0.212)	(0.185)
Year 2005	-1.287***	-0.642**	-0.440	-0.258	-0.159	-0.110
	(0.275)	(0.254)	(0.283)	(0.194)	(0.182)	(0.164)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj.R-Squared	0.129	0.170	0.120	0.113	0.081	0.082
Observations	2846	2846	2846	2846	2846	2846

Source: Own estimates based on school census data. Note: Robust standard errors in brackets, significance levels: * p<0.10, ** p<0.05, *** p<0.01. The sum of treatment effects does not perfectly match the treatment effect of table 5 due to small reporting inconsistencies between retrospective and present-year figures.

(ii) Table 7 indicates that the enrollment effect is not merely due to attracting students from other schools. The dependent variable is enrollment, split up into different student categories: students that were promoted from a lower grade or enter grade 1 for the first time (column 1), students that repeat last year's grade due to non-promotion (column 2) or withdrawal (column 3) or that were reincorporated after not being enrolled (column 4) the year before. Panel A shows students coming from a different school, panel B those coming from the same school. The sum of effects in panel A shows that treatment attracts only an insignificant 0.3 students from other schools on average. The bulk of increasingly enrolled students is thus not just pulled away from other schools. This is reasonable as the sampled rural schools can be considered monopolistic entities, far away from other schools.

(iii) Also, the treatment effect does not work by attracting students that were previously not enrolled. The effects of column 4 in panel A and B are close to zero and insignificant.

(iv) Instead, most of the increased enrollment effect can be found in columns 1 to 3 of panel B, i.e., enrollment is increased by students of the treated school that were promoted in the year before (3.9) or that repeat the grade (0.6). Consequently, increased enrollment in treated school results from fewer students dropping out between 2004 and 2005 compared to untreated schools, possibly in anticipation of increased school quality. Note that these students would have dropped out without treatment anticipation even though they had completed the previous grade.

Table 7. Enrollment Effects by Student Status

Panel A: Students from Different School

Origin of student: different school	[1] Promoted	[2] Repeater	[3] Reentrant	[4] Reincorpo-rated
Treatment*2005	0.337	-0.097	0.041	0.027
	(0.302)	(0.100)	(0.100)	(0.048)
Treatment Group	0.249	0.098	0.067	0.025
	(0.209)	(0.068)	(0.049)	(0.037)
Year 2005	-0.122	0.034	-0.050	-0.035*
	(0.169)	(0.066)	(0.034)	(0.018)
Controls	Yes	Yes	Yes	Yes
Adj.R-Squared	0.033	0.012	0.012	0.004
Observations	2846	2846	2846	2846

Panel B: Students from Same School

Origin of student: same school	[1] Promoted	[2] Repeater	[3] Reentrant	[4] Reincorpo-rated
Treatment*2005	3.869***	0.596	0.173	0.129
	(1.097)	(0.427)	(0.289)	(0.093)
Treatment Group	0.335	-0.125	-0.033	-0.089
	(0.771)	(0.308)	(0.206)	(0.082)
Year 2005	-1.346**	-0.427	-0.044	-0.190***
	(0.673)	(0.267)	(0.182)	(0.069)
Controls	Yes	Yes	Yes	Yes
Adj.R-Squared	0.169	0.125	0.042	0.011
Observations	2846	2846	2846	2846

Source: Own estimates based on school census data. Note: Robust standard errors in brackets, significance levels: * p<0.10, ** p<0.05, *** p<0.01. The sum of treatment effects does not perfectly match the treatment effect of table 5 due to small reporting inconsistencies between retrospective and present-year figures.

Table 8 shows the results for levels of grade completion (column 1), failure (column 2), non-promotion (column 3) and withdrawal (column 4) with control variables. Treatment effects from column 1 and 2 add up to the enrollment effect of column 2, table 5.[10] Increased enrollment levels due to treatment thus translate into increased grade completion and failure levels. Column 1 shows that the treatment interaction is positive and significant, i.e., there is an estimated effect of 3.7 additional grade completers after treatment according to the specification including control variables. Similarly, there are an estimated 1.7 additional grade failers after treatment; about 0.2 from non-promotion and 1.5 from withdrawal.

Furthermore, being a treated school affects grade completion as shown by the treatment group effect in columns 1 and 2, which was close to zero and insignificant in the enrollment estimation (table 5). The explanation is that enrollment is determined in April, at the beginning of the school year; grade completion is determined in December. As budgeting requests are filed in May, students enroll in 2004 without possibly anticipating a second teacher in 2005. Since schools were also matched on size in 2004, there is no significant treatment group effect on enrollment levels. Nevertheless, students may anticipate some time after May but before December the addition of a second teacher in 2005, which in turn could influence their perception of school quality and increase their willingness to complete. This seems to be the case: given similar enrollment levels, about one more student was successful in treated schools already in 2004, possibly in anticipation of treatment.

Table 8. Matched Difference-in-Difference Estimates: Completion and Failure Levels

	[1]	[2]	[3]	[4]
Dependent Variable	Completed	Failed	Non-Promoted	Withdrawn
Treatment*2005	3.704***	1.674**	0.190	1.467***
	(1.072)	(0.660)	(0.415)	(0.463)
Treatment	1.004	-0.974**	0.093	-1.063***
	(0.757)	(0.489)	(0.304)	(0.346)
Year 2005	-1.697**	-1.199***	-0.255	-0.939***
	(0.659)	(0.448)	(0.255)	(0.327)
Controls	Yes	Yes	Yes	Yes
Adj.R-Squared	0.149	0.134	0.171	0.056
Observations	2846	2846	2846	2846

Source: Own estimates based on school census data. Note: Robust standard errors in brackets, significance levels: * p<0.10, ** p<0.05, *** p<0.01

[10] The treatment effects on non-promoted and withdrawn students (columns 3 and 4) do not exactly add up to the treatment effect on failed students (column 2) due to a small number of deceased students who fail the grade but are neither considered non-promoted nor withdrawers.

Table 9 documents the effects of treatment on grade completion and failure shares. As before, there is a positive and significant treatment group effect on the completion *share*. Whhen comparing columns 3 and 4 of table 9, we see that the treatment group effect significantly alters the share of those withdrawing but not those non-promoting. The treatment interaction effect on the completion share, however, is negative and insignificant. Point estimates indicate that the effect of receiving a second teacher is a 2.7 percentage point increase in the completion share *before* treatment, and 1.8 (2.7-0.9) percentage points *after* treatment. This means that the actual presence and class size reduction of an additional teacher could not lift the completion share significantly above the share achieved by the anticipation of that teacher. Standard errors are not large in the estimates: an after-treatment effect of about 2.4 percentage points, i.e., about one more completed student, would be significant. Instead, the effect may even be negative.

Table 9. Matched Difference-in-Difference Estimates: Completion and Failure Shares

Dependent Variable	[1] Share Completed	[2] Share Failed	[3] Share Non-Promoted	[4] Share Withdrawn
Treatment*2005	-0.009	0.009	-0.006	0.014
	(0.012)	(0.012)	(0.009)	(0.010)
Treatment	0.027***	-0.027***	-0.004	-0.023***
	(0.009)	(0.009)	(0.006)	(0.007)
Year 2005	0.012	-0.012	-0.001	-0.010*
	(0.008)	(0.008)	(0.005)	(0.006)
Controls	Yes	Yes	Yes	Yes
Adj.R-Squared	0.099	0.099	0.111	0.075
Observations	2846	2846	2846	2846

Source: Own estimates based on school census data. Note: Robust standard errors in brackets, significance levels: * p<0.10, ** p<0.05, *** p<0.01

In summary, the results indicate that students may anticipate a second teacher before the actual treatment and thus be less inclined to leave school. Possibly, they perceive that a quality increase will raise their utility from schooling, e.g., through higher labor market returns from education. Treatment anticipation thus increases the share of grade completers even before treatment, especially via reduced withdrawal. One possible explanation for this is that the anticipation of treatment does not increase learning achievement itself and thus the probability to be promoted. Instead, it may increase expected future school quality and thus induce fewer children to leave presently. Treatment anticipation also increases enrollment in the second period. After treatment, there are two presumably counteracting effects: a positive effect through class size reduction and a negative side effect in that more students are in school who would not have enrolled in absence of treatment and who may thus be more likely to fail than their peers. The net impact of these two effects shows up insignificantly in the after-treatment effect on completion shares.

When examining the effects on completion rates for each grade separately (Table 10), we see that significant treatment group effects are found in grades 1 to 3 where children benefit longest from future increases in school quality. There is, however, a significant negative after-treatment effect in grade 1, indicating that here the positive impact of reduced class sizes is dominated by the negative effect of attracting new students with a relatively high probability to fail. The sum of treatment effect point estimates is highest in grades 2 (0.028), 3 (0.022) and 6 (0.022). Possibly, opportunity costs from going to school in poorly developed labor markets are not yet very pronounced for young children. Their decision between enrolling and dropping may be a closer one, and a second teacher more likely to increase utility from schooling above the threshold needed to enroll and continue. For older children, only those shortly before completing the 6-year cycle may be inclined to stay due to treatment.

Table 10. Matched Difference-in-Difference Estimates: Completion Share in Grades 1 - 6

Share completed in grades 1-6	[1]	[2]	[3]	[4]	[5]	[6]
Treatment*2005	-0.038*	-0.004	-0.023	-0.013	-0.026	0.032
	(0.020)	(0.019)	(0.019)	(0.020)	(0.020)	(0.022)
Treatment Group	0.040***	0.032**	0.045***	0.010	0.018	-0.010
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.017)
Year 2005	0.031**	0.016	0.014	0.015	0.002	-0.005
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj.R-Squared	0.057	0.063	0.071	0.063	0.044	0.028
Observations	2667	2728	2705	2641	2215	2014

Source: Own estimates based on school census data. Note: Robust standard errors in brackets, significance levels: * p<0.10, ** p<0.05, *** p<0.01

Table 11 summarizes all unconditional effects, i.e., those equivalent to estimating equation (1) without control variables. The last row shows double-difference effects between end-of-2003 and end-of-2005 outcomes. We see that treatment has increased the level of completers by 3.9, i.e. about 10 percent of initial enrollment in 2004. The table also illustrates that the treatment group effect and the after-treatment effect jointly decrease the non-promotion share by about 1.2 percentage points, and the withdrawal share by about 0.7

percentage points, leading to an overall effect of -1.9 percentage points compared to the end-of-year results of 2003.

Table 10. Summary of Unconditional Effects

Means	Year	Enrollment	Student-teacher Ratio	Completed	Share Non-Promoted (%)	Share Withdrawn (%)	Share Failed (%)
Control Group	2003	.		26.96	12.77	12.68	25.45
	2004	38.66	38.66	28.40	12.87	12.76	25.63
	2005	35.87	35.87	26.77	12.60	11.56	24.16
Treatment Group	2003	.		27.51	12.95	12.74	25.69
	2004	38.38	38.38	29.25	12.31	10.36	22.67
	2005	40.88	20.44	31.25	11.56	10.90	22.46
Effect	**Year**	**Enrollment**	**Student-teacher Ratio**	**Completed**	**Share Non-Promoted (p.p.)**	**Share Withdrawn (p.p.)**	**Share Failed (p.p.)**
Single Difference	2003	.	.	0.55	0.18	0.06	0.24
	2004	-0.28	-0.28	0.85	-0.56	-2.40	-2.96
	2005	5.01	-15.43	4.48	-1.04	-0.66	-1.70
Time Trend	2003/04	.	.	1.44	0.10	0.08	0.18
	2004/05	-2.79	-2.79	-1.63	-0.27	-1.20	-1.47
Difference-in-difference	2003/04	.	.	0.30	-0.74	-2.46	-3.20
	2004/05	5.29	-15.15	3.63	-0.48	1.74	1.26
	2003/05	.	.	3.93	-1.22	-0.72	-1.94

Note: p.p. stands for percentage points.

1.5 Conclusion

A matched difference-in-difference analysis presented in this paper shows that the addition of a second teacher to rural primary single-teacher schools in Peru increases enrollment by about 14 percent mainly because of fewer between-year drop-outs in treated schools, possibly in anticipation of higher future school quality. Part of this effect comes from a higher share of grade completers, about 2.7 percentage points or a 10 percent reduction, at the end of the first year, i.e., even before treatment. Consequently, completion *levels* in the second period are significantly increased after treatment due to this enrollment effect.

Nevertheless, the analysis also shows that there is no additional significant after-treatment effect on completion *rates*. This disappointing result is a net effect of decreasing the student-teacher ratio on average by about 40 percent and inducing more students to enroll who would not have remained in school in the absence of smaller classes and are thus the first ones to fail. Unfortunately, we cannot say how strong these effects are respectively.

Since rural single-teacher schools tend to lay in the poorest areas improvements in school quality decrease drop-out and thus increase enrollment among those groups of the population where it is lowest. In-school factors thus seem to matter for those parts of the population that are so far excluded from the educational system. This finding is along the lines of Hanushek et al. (2003): increased school quality keeps children in school longer. Nevertheless, the finding implies that increases in quality would have to be even more significant and costly to close the gap towards universal primary education. Even though this could not be explicitly tested in the paper, it may be a more promising avenue to invest in teacher quality rather than quantity, such as training specifically designed to deal with multi-grade teaching.

Furthermore, the results also suggest that out-of-school reasons may be significant determinants of grade non-completion rates in poor rural areas in developing countries. Households with financial constraints or volatile income flows face high opportunity costs of sending their children to school permanently. These constraints may be lifted by programs such as conditional cash transfers, which support the poorest families financially conditional on sending their children to school. Estimating the impact of such measures on educational inefficiency remains an imperative area for future research.

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Appendix

A1 Comparison of New and Old Teacher Characteristics

Could it be possible that the new teachers are different from old teachers, for example because only teachers of the lowest quality go to remote rural areas? Table A1 addresses this issue by comparing average observable teacher characteristics – gender, contract and education – between treatment and control group before and after treatment. The upper panel A shows that characteristics before treatment are balanced due to trimming of the treatment group and selection of appropriate control schools via propensity score matching. About 58 percent of teachers are male, more than 80 percent have obtained a teaching degree and almost 80 percent of teachers have a permanent contract. Also, most teachers work the maximum of 40 hours. In contrast, panel B suggests that second teachers are more predominantly female and work on fixed rather than permanent contracts. Also, additional teachers work fewer hours than their colleagues such that the treatment effect on the number of *students per teacher hour* is less the effect on the student-teacher ratio. While teacher education does not significantly differ after treatment between treated and control schools, fixed term contracts may have a different motivational effect than permanent appointments. It is unknown if these differences indicate lower teacher quality of second teachers which could explain the lack of clear positive effects on grade completion rates.

Table A1.1. Average Teacher Characteristics Before and After Treatment

	Trimmed Treatm. Group		Matched Control Group		T-Test p-value
A. Before Treatment					
Teacher Male	0.581	(0.49)	0.571	(0.50)	0.759
Teacher Female	0.419	(0.49)	0.429	(0.50)	0.759
Teacher with Teaching Degree	0.814	(0.39)	0.824	(0.38)	0.676
Teacher without Teaching Degree	0.186	(0.39)	0.176	(0.38)	0.676
Teacher with Permanent Contract	0.778	(0.42)	0.736	(0.44)	0.142
Teacher with Fixed Term	0.222	(0.42)	0.264	(0.44)	0.142
Teacher Hours	38.083	(3.94)	38.156	(4.16)	0.785
B. After Treatment					
Teacher Male	0.500	(0.39)	0.548	(0.50)	0.094
Teacher Female	0.500	(0.39)	0.452	(0.50)	0.094
Teacher with Teaching Degree	0.833	(0.31)	0.833	(0.37)	0.991
Teacher without Teaching Degree	0.167	(0.31)	0.167	(0.37)	0.991
Teacher with Permanent Contract	0.628	(0.36)	0.701	(0.46)	0.006
Teacher with Fixed Term	0.372	(0.36)	0.299	(0.46)	0.006
Teacher Hours	34.142	(3.07)	38.236	(4.31)	0.000

Source: Own estimates based on school census data. Note: Robust standard errors in brackets.

A2 Instrumental Variable Estimate of the Student-teacher Ratio Effect

While the ATT, β_3, shows the average effect of adding a second teacher to single-teacher schools, we may also be interested in the actual student-teacher ratio effect, i.e. γ_1 in

(2a) $Y_{st} = \gamma_0 + \gamma_1 C_{st} + \gamma_2 X_{st} + \varepsilon_{st}$

where the student-teacher ratio (C) and control variables (X) influences outcomes (Y).

As mentioned before, the student-teacher ratio itself is endogenous and we need to circumvent the problem of endogeneity to estimate the causal effect on educational outcomes, for example by instrumental variables (IV) techniques. IV requires an instrument, Z, which influences the endogenous explanatory variable of interest independently of all other explanatory variables in (2a) and which is uncorrelated with ε_{st} (Wooldridge 2002). Z is then used to predict C in a first stage regression,

(2b) $C_{st} = \delta_0 + \delta_1 Z_{st} + \delta_2 X_{st} + \mu_{st}$.

The predicted value from this first stage, $\hat{C}$, is subsequently substituted into (2a) instead of C to estimate γ_1.

I argue that the interaction effect $T_s * P_t$ from (1) is a suitable instrument: by construction, the treatment of an additional teacher decreases the student-teacher ratio in the second period. Furthermore, propensity score matching creates a control group such that in treatment is conditionally exogenous in the sample of matched treatment and control group.

Instrumental variable estimates of the student-teacher ratio effect do not yield significant results (table A2). Column 1 shows first stage results where the student-teacher ratio is instrumented with the interaction "treatment group * post-treatment year". The instrument is highly significant (t-statistic: 11.67). In the second stage, we cannot reject that there is no significant effect of the student-teacher ratio on grade completion shares. Given a point estimate of 0.0006 and a standard error of 0.0008, we can exclude with 95 percent certainty that a 10 person decrease in the student-teacher ratio would increase grade completion shares by more than 1 percentage point.

Table A1.2. Instrumental Variable Estimates of Student-teacher Ratio Effects

	1st Stage	2nd Stage
Dependent Variable	Student-teacher ratio	Share Completed
Treatment*2005	-15.2454***	-
	(1.1399)	-
Treatment	0.0081	0.0271***
	(0.8972)	(0.0091)
Year 2005	-2.6739***	0.0134
	(0.8123)	(0.0091)
Student-teacher ratio	-	0.0006
	-	(0.0008)
Controls	Yes	Yes
Adj.R-Squared	0.390	0.085
F-Statistic	83.2	-
Chi2	-	215.2
Observations	2846	2846

Source: Own estimates based on school census data. Note: Robust standard errors in brackets, significance levels: * p<0.10, ** p<0.05, *** p<0.01