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Abstract

Many developing countries are allocating significant resources to expanding technology access in schools. Whether these investments will translate into measurable educational improvements remains an open question because of the limited evidence available. This paper contributes to filling that gap by exploiting a large-scale public program that increased computer and Internet access in secondary public schools in Peru. Rich longitudinal school-level data from 2001 to 2006 are used to implement a differences-in-differences framework. Results indicate no statistically significant effects of increasing technology access in schools on repetition, dropout and initial enrollment. Large sample sizes allow ruling out even modest effects.

JEL classifications: I21, I28
Keywords: Computers in education, Dropout rates, Repetition rates, Enrollment

1. Introduction

In the last 15 years, many developing countries have embarked on ambitious programs to expand computer access in schools. Among developing countries that participated in the OECD Programme for International Student Assessment (PISA) in 2001 and 2006, the ratio of computers per student increased 50 percent in just five years (OECD, 2007). This trend has recently accelerated, fueled by a number of programs that promote the distribution of laptops to students to improve educational outcomes. The most prominent initiative worldwide has been the One Laptop per Child (OLPC) program that has distributed about 2.5 million laptops in 41 countries.

The literature about the impacts of technology access on educational outcomes has mainly focused on whether the introduction of technology can enhance learning in traditional subjects such as Math and Language. Rigorous studies have produced mixed evidence, though they typically do not find evidence of effects. Angrist and Lavy (2002) analyzed a program that introduced computers in schools in Israel and found no impacts in Hebrew and some negative effects in Math. Goolsbee and Guryan (2006) estimated no impacts of increased internet access in the U.S. on test scores in Math, Reading and Science. Barrera-Osorio and Linden (2009) found no impacts of increased computer access on Math and Language in Colombia. Cristia et al. (2012) evaluated the OLPC program in primary rural schools in Peru and found no evidence of effects in Math and Language. As an exception, Machin, McNally and Silva (2007) found some positive impacts in English and Science but not in Math in the United Kingdom.[1]

A simple conceptual framework points to potential effects beyond learning in traditional subjects. Expansions in computer access in schools can have two direct effects. First, they might lead to increased learning in traditional subjects and to the development of computer-related skills. Second, computers in schools might make the educational experience more enjoyable to children. These changes in the gains and derived satisfaction of going to school might produce

[1] Another strand of the literature has focused on whether the use of interactive software that adapts the content and exercises to the particular user can generate improvements in tests scores (versus traditional instruction). Studies in this area have found more positive effects, especially when executed in developing countries (Banerjee et al., 2007; Linden, 2008; Barrow, Markman and Rouse, 2009).

different behavioral changes. They can affect permanent decisions including enrollment and dropout. Additionally, they can affect decisions made daily about attending school.[2]

Motivated by these theoretical considerations, we exploit rich administrative panel data from secondary schools in Peru to assess whether increased technology access affects repetition and dropout. We also measure effects on enrollment in grade 7, the first year in secondary schools, to explore whether families' decisions on enrolling children in school could also be affected.[3] We test whether increased computer access affects learning in regular subjects indirectly through exploring effects in the repetition rate. If computers increase subject learning, then they should reduce repetition rates. Additionally, we explore whether increased access to computers affects behavior focusing on initial enrollment and dropout decisions. Because of data limitations, we do not explore effects on test scores and attendance.

Simple comparisons between schools with high access to technology and those with low access might not be able to generate unbiased estimates because this variation might be correlated with a host of other educational inputs. To overcome this problem we exploit the exogenous variation in computer and Internet access generated by a large-scale public program implemented in Peru. The Huascarán program, implemented between 2001 and 2006, aimed to improve educational outcomes through introducing technology into schools. To assess the impact of the program, we focus on the sample of public secondary schools that had not benefitted from a technology in public education program by 2003. The treatment group includes schools that entered the program between 2004 and 2006. The comparison group consists of schools that remained untreated throughout the period. We estimate the effects of the program using a differences-in-differences framework and trimming and reweighting techniques to increase the similarity between the treatment and comparison groups in 2003, the baseline year. Results indicate no impacts of increased ICT access on the outcomes considered. The lack of differential pre-treatment trends in outcomes between the treatment and comparison groups provides support for the identification strategy used.

[2] This conceptual framework highlights that effects might be present for any of these outcomes. If students are more motivated to go to school with higher computer access, then attendance may rise assuming that they control this decision. On the other side, if parents expect larger gains of schooling with higher computer access, enrollment might increase if they make this decision.

[3] Enrollment in a school at a certain grade is affected by the decision of families to register their child in that school, and by dropout and the repetition rates. In lower grades, enrollment will be more affected by the registration decision although in higher grades it will depend more on the dropout and repetition rates. We measure effects on enrollment in grade 7 to focus on the registration decision.

The paper contributes to the literature in several ways. First, it is the first study to analyze the impacts of increasing ICT access on initial enrollment and dropout rates. By doing so, we can test the hypothesis of whether higher availability of computers in schools induces higher overall enrollment. Second, the use of large sample sizes generates precise estimates. This is particularly relevant to interpret the absence of statistically significant effects as definitive evidence of truly small impacts. Third, the study focuses on Peru, contributing to the scant literature for developing countries.

Our paper is closely related to another study that analyzes the effects of expanding computer access in secondary schools in Peru. Bet, Ibarrarán and Cristia (2010) use primary data from grade 9 collected in 200 secondary schools in Peru in 2008 to explore how increased access to computers affects computer use, ICT literacy and learning in Math and Language. The authors applied propensity-score matching to administrative data to identify two sets of similar schools that differ markedly in computer access. Then, primary data was collected from the selected schools to estimate effects. Results indicate that greater access led to higher computer use. However, the increased availability of technology only affected the time spent to teach digital skills, but it did not change the time the computers were used in Math and Language. Consistent with these patterns of use, the study showed positive significant effects in ICT literacy but no evidence of effects in Math and Language test scores.

Both our study and Bet, Ibarrarán and Cristia (2010) seek to understand how increased computer access affects educational outcomes in the context of secondary schools in Peru. However, the studies differ in several dimensions. First, our study employs administrative data from 2001 to 2006 and a differences-in-differences strategy. In contrast, Bet, Ibarrarán and Cristia (2010) estimate effects by exploiting cross-sectional variation in computer access using primary data from 2008 and a propensity-score matching approach. Second, while we use data in all grades from at least 700 schools, Bet, Ibarrarán and Cristia (2010) use data from one grade in 200 schools. Finally, we estimate effects on repetition, dropout rates and initial enrollment, whereas Bet, Ibarrarán and Cristia (2010) focus on effects on computer use, ICT literacy and learning in Math and Language. Regarding qualitative findings, both studies are consistent and complementary. Bet, Ibarrarán and Cristia (2010) document no effects on Mathematics and Language consistent with our null effects on repetition rates. Additionally, our study documents no effects in dropout rates and initial enrollment although Bet, Ibarrarán and Cristia (2010) find positive effects on ICT literacy.

The paper proceeds as follows. Section 2 provides some institutional background, and Section 3 describes the data used. Section 4 lays out the empirical strategy, Section 5 presents results and Section 6 explores their robustness. Section 7 concludes.

2. Background

2.1. The Education Sector in Peru

Peru is considered an upper middle income country and ranks 79 out of 179 countries according to the Human Development Index for the year 2008. Its GNI per capita, based on PPP exchange rates, was slightly higher than the average middle income country (6,800 versus 5,400 dollars in 2006). Gross enrollment rates in secondary schools in Peru were 90 percent in 2007, whereas net enrollment was 75 percent (World Bank, 2013). The amount of resources devoted to education was significantly lower in Peru compared with other upper middle income countries, at 3.0 versus 4.9 percent of GDP in 2009 (World Bank, 2013).

2.2. ICT in Education in Peru

Until 1996 ICT played a small role as a tool to improve public education in Peru. Since then, several small-scale independent programs, mainly targeting secondary schools, were launched. These programs typically funded some ICT resources (hardware, software, training, and support) but required investments by participating schools to be included in the program. Computers were mainly used for acquiring ICT skills (creating documents, spreadsheets and presentations), browsing the Web and for communication purposes.

In 2001, a new ICT in education program was started, named Huascarán, which became one of the most publicized initiatives of the newly elected presidential government. Its stated objective was to increase coverage and quality in the educational sector by introducing ICT into the learning process. Schools selected into the program received hardware, software (Microsoft Office applications and digital media but not interactive software) and teacher training, and they were prioritized to receive Internet access. In addition, the program funded "innovation room coordinators," individuals trained in IT and pedagogy who were responsible for ensuring the effective use of computer labs in all subject areas. However, as noted above, Bet, Ibarrarán and

Cristia (2010) document that the overwhelming majority of time used was devoted to learning ICT skills and that increases in ICT access did not translate into higher use in subjects such as Math and Language.

Regarding the procedure employed to select schools into the program, interviews with former government officials suggest that there were some guidelines, but no strict protocol. Eligible schools had to be public, and they should not have been covered by previous governmental programs (data checks showed that both requirements were always fulfilled). Within eligible schools, three factors were considered to select the final set of schools: i) high enrollment levels, ii) ease of access to schools and iii) commitment by directors, teachers and parents to supporting and sustaining the initiative. Still, other considerations could have played a role in final decisions.[4]

3. Data

The data used in the study is compiled by the Ministry of Education from yearly surveys completed by almost all secondary schools in the country. Information available includes the following: location, private/public type, creation year, enrollment per grade, gender and overage status, number of sections per grade, administrative staff, teachers, repetition and dropout rates, physical infrastructure, textbooks, number of computers, network connection, Internet access and existence of a computer lab.

The data available for the study span from 2001 to 2007. Information on repetition and dropout rates was not available for 2007, as schools report them for the previous year (for example, in June 2007 they report the number of students that drop out in 2006). Additionally, data on these variables are not available for the year 2002.[5] Therefore, we focus the empirical work on years 2001, 2003, 2004, 2005 and 2006. To ensure the comparability of the analytic sample across time, we restricted our attention to schools that provided information in all years used in the analysis.

[4] Unfortunately, there is no documentation that we could access to further understand the selection procedure.

[5] According to sources at the Educational Statistical Unit of the Ministry of Education, a decision was made not to collect these data in 2003 (corresponding to repetition and dropout in year 2002) because the survey was going to be run every two years. However, this decision was later reversed and, therefore, data were collected annually for the period 2003-2006.

Table 1 presents summary statistics. The first column presents summary statistics for the year 2001, for the subset of schools that answered the surveys in all years.[6] The third column shows corresponding statistics for 2006. The second and fourth columns present statistics for 2001 and 2006, respectively, for all schools that answered the survey in those years. The differences across samples are small suggesting that restricting the attention to schools present in all years does not generate substantial bias in the representativeness of the sample.

In the top panel we observe that repetition rates are high, although they have decreased by about 10 percent in the period under consideration. The dropout rate, however, remains virtually unchanged in this period. The second panel, about technology access, shows significant increases in the availability of ICT over time. The fraction of schools having a computer increases from 68 to 85 percent, while the fraction of schools with a computer lab rises from 39 to 76 percent. The fraction of schools with Internet access more than tripled, increasing from 16 to 55 percent.

We also present information for the variable *Students ICT Potential Access* (SIPA). This is just a linear transformation of the student-computer ratio and it is computed as:

$$SIPA_{it} = \frac{Computers\ for\ Learning_{it}}{Enrollment_{i,2001}} * 2 * 25$$

where *i* and *t* index the school and year. *SIPA* represents the average number of hours per week that students would use computers if they were used continuously and shared between two students (students spend about 25 hours in school per week). Therefore, it expresses technology access in weekly hours that computers could be used. For example, in a school with 10 computers and 500 enrollees, if computers were used continuously by pairs of students, the average student would use them 1 hour per week (10/500*2*25=1).

As noted in the conceptual framework, enrollment is an endogenous variable and can be affected by an increase in computer access. Therefore, we fix enrollment in the year 2001 to compute the ratio. This means that changes in SIPA over time will only depend on variation in computer access. Between 2001 and 2006, SIPA increased from 0.8 to 2.2 hours per week.

Table 2 presents the same set of indicators computed separately for different groups of schools, defined by the interaction of private/public and urban/rural, using data for 2004. Schools

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[6] Throughout the paper we calculate all statistics and estimates weighting school observations by the number of enrolled students.

in the different groups vary widely in terms of repetition and dropout rates, as well as in technology access. As expected, access to computers and Internet is markedly higher in private and urban schools. Because the Huascarán program targeted primarily public urban schools, we restrict the analysis to schools in this group.

4. Empirical Strategy

As noted previously, program administrators pointed to three main factors that influenced the decision to select a school into the Huascarán program: high enrollment, easy geographical access to the school, and strong commitment to support the ICT adoption process. This selection process suggests that beneficiary schools of the Huascaran program might be materially different from non-beneficiaries. In particular, schools might self-select into the program based on the leadership of their directors, motivation of teachers and support of parents. Therefore, cross-sectional comparisons between beneficiary and non-beneficiary schools might produce biased estimates of the effect of the program.

To tackle this problem, we adopt a differences-in-differences framework to estimate effects. We restrict the sample to schools that had not participated in an ICT public program by 2003. Our treatment group includes schools that entered the program between 2004 and 2006. The comparison group contains schools that had not entered a public program by 2006. This empirical strategy allows us to check differential pre-treatment trends between the treatment and comparison groups.[7]

Under this empirical strategy, schools in the treatment group are late entrants, as they were not selected for ICT programs before 2001, nor during the first stage of the Huascarán program (2001-2003). Therefore, they needed to show interest but they needed to apply (or be selected) late. Possibly, early entrants included schools clearly different from the rest. Then, the adopted strategy of only including schools in the sample not participating in an ICT program until 2004 might reduce the underlying differences between the treatment and comparison groups.

[7] An alternative comparison group would include schools that participated in an ICT program by 2003 (early entrants). However, if there are lagged effects of expanded access to technology, then early entrants will experience improvements in outcomes under the period of analysis (2004-2006). Under this plausible scenario, early entrants will not provide a valid counterfactual to treatment schools in the absence of the program. Hence, their inclusion in the comparison group would bias our estimates of treatment effects.

To explore patterns of selection into the program, we analyze observable characteristics of schools in the treatment group and those in the comparison group in 2003. Columns 1 and 3 in Table 3 document that schools in the treatment group tend to be larger, have better infrastructure and lower repetition rates than those in the comparison group. The identification assumption under a differences-in-differences framework is that outcomes in the treatment group would have evolved similarly to those in the comparison group in the absence of the treatment. This assumption is more likely to hold if the treatment and comparison groups are similar in pre-treatment observable covariates. This motivates the use of trimming and reweighting techniques, in our baseline specification, to increase the similarity between the treatment and comparison groups.

We start by estimating the treatment propensity score (PS) at the school level using a logistic regression and a large number of covariates from 2003.[8] Figure 1 plots the distribution of PS by treatment status. Few schools in the comparison group have a PS higher than 0.7. This motivates the selection of a common support in the interval between 0.3 and 0.7. That is, we drop from the sample all schools with a PS lower than 0.3 or higher than 0.7. After trimming the sample in this way, we proceed to reweight observations by 1/(1-PS). This procedure ensures that schools in the treatment and comparison group are balanced with respect to PS.[9]

Columns 4 to 6 in Table 3 document that the trimming and reweighting procedure generated treatment and comparison groups that are well balanced in terms of observable covariates in 2003. The differences in means across groups in the original sample (column 3) are substantially reduced and typically become not significant in the trimmed and reweighted sample (column 6). This result might be expected for variables included in the estimation of the propensity score. But it is present for other important variables, such as dropout and repetition rates, not included in the estimation of the propensity score.

[8] We predict treatment using deciles of enrollment in grade 7, students per section, students per teacher, tenured teachers per classroom, number of blackboards, chairs and tables, and indicators for having a principal, assistant principal, water, restrooms, gym, library, administrative office and teachers' lounge. Continuous variables enter linearly and squared. Because enrollment plays a central role in the selection process, we include the interaction between deciles of enrollment in grade 7 and students per section, tenured teachers per classroom, having a principal and having an assistant principal. Finally, to account for geographical aspects in the selection of schools, we include dummies at the department level (there are 25 departments in the country).

[9] In Section 5, we explore the robustness of the main results to adopting a simple differences-in-differences specification, specifying alternative common supports, not reweighting observations and applying propensity-score matching techniques.

Finally, we reshape the panel data to a structure in which the unit of observation is a school, year, grade and sex. The empirical strategy is executed estimating the following model on the trimmed and reweighted sample:

(1) $$Y_{itgs} = \alpha + \beta T_{it} + \gamma X_{itgs} + \mu_i + \eta_t + \pi_g + \chi_s + \varepsilon_{itgs}$$

where Y corresponds to the outcome variable, X is a vector of time-varying controls, and μ, η, π, χ correspond to dummies at the school, year, grade and sex levels, respectively. The treatment dummy T equals 1 for school i in year t if the school had been selected to participate in the Huascarán program by that year, zero otherwise. The indices i, t, g and s correspond to school, year, grade and sex, respectively. Time-varying controls include: enrollment, number of administrative staff, teachers per classroom, students per teacher, students per section, classrooms, blackboards, tables, desks and dummies indicating the school has principal, assistant principal, administrative offices, teachers' lounge, workshop, library, another lab (no ICT), gym, running water, sanitation and electricity. In all regressions standard errors are clustered at the school level.

5. Results

We start by checking whether participation in the Huascaran program did increase access to computers and the Internet. Results in Table 4 indicate that participating in the program produced positive and statistically significant effects on both SIPA and the Internet access indicator. The effect on SIPA is about 0.35 hours per week, and for Internet access the impact is 25 percentage points. Figure 2 graphically shows this finding by plotting the evolution of average SIPA and fraction of schools with Internet access by treatment status. Access to computers and the Internet was low and stable in the 2001 to 2003 period. Access to these resources increased substantially during 2004 and 2006 for schools in the treatment group. The figure also shows that there was an increase (though smaller) in access for schools in the comparison group during this period. This increase in the comparison group suggests that schools independently sought to acquire these resources, potentially through contributions from parents and donations from the private sector.

We next examine the effects of the Huascarán program on educational outcomes. Table 5 presents the estimated effects of the program on repetition, dropout rates and enrollment in grade

7 (the first year in secondary school). For repetition and dropout, the dependent variable was multiplied by 100 and, consequently, the impacts should be interpreted in terms of percentage points. We find no evidence that the program has affected the analyzed outcomes. Point estimates are close to 0 and robust to adding time-variant controls. Results indicate that participating in the program was associated with an increase in 0.01 percentage points in the repetition rate when no controls were added, and with a decrease in 0.03 percentage points when including time-varying controls. Similarly, the point estimates of effects on the dropout rate are 0.06 percentage points in the model without controls and 0.04 percentage points when including controls. In the case of initial enrollment, participating in the program is associated with an increase in initial enrollment of 0.032 students in the model without controls and 0.007 when including controls.

6. Robustness Checks

This subsection explores the robustness of the empirical findings. First, we check whether the results are robust to changes in the empirical specification. Second, we test whether there are differential trends in outcomes between the treatment and comparison groups during the pre-treatment period (2001 to 2003). Third, we examine whether during the treatment period (2004 to 2006) there are similar trends in educational inputs between the treatment and comparison groups.

In our baseline specification we generate results by focusing on schools with a propensity score between 0.3 and 0.7 and reweighting observations by 1/(1-PS). Table 6 shows results under alternative specifications regarding the common support imposed and whether observations are reweighted. The sample used in columns 1 and 2 includes all secondary public urban schools that had not participated in a program of technology in education by 2003. In columns 3 and 4 the sample is restricted to those schools with a propensity score between 0.1 and 0.9. In columns 5 and 6 (7 and 8), the sample is further restricted to include schools with a propensity score between 0.2 and 0.8 (0.3 and 0.7). In even-numbered columns, observations are reweighted by 1/(1-PS). Note that column 1 presents estimates when implementing a simple differences-in-differences model without trimming and reweighting. In all cases, there is no evidence that the program affected repetition, dropout rates or enrollment in grade 7. As an additional check, we estimate effects using a propensity-score matching differences-in-differences estimator.

Specifically, we implement nearest neighbor propensity-score matching with replacement. Table 7 shows that the qualitative results are robust to using this alternative estimation method.

The identification assumption in our empirical strategy is that, in the absence of treatment, average outcomes in the treatment group would have evolved similarly to those from the comparison group. We provide indirect evidence for this assumption by performing a placebo test. We keep observations for years 2001 to 2003 (the pre-treatment period) and defined a placebo treatment indicator equal to 1 in 2003 for those schools that participated in the program, zero otherwise. We generate estimates under the baseline specification and report results in Table 8. Results show that there are no statistically significant differences in pre-treatment trends in outcomes between both groups. This provides evidence supporting the empirical strategy followed.

Finally, we check whether there were significant changes in other educational inputs concomitant with the introduction of the program. If educational inputs evolved differently between the treatment and comparison groups, this would have raised doubts about the basic identification assumption. Figure 3 presents the results. Trends in these inputs are flat and similar across the two groups, giving further support to the empirical strategy followed.

7. Conclusion

This paper empirically addresses the policy-relevant question of whether increases in access to technology in schools can affect repetition, dropout rates and enrollment in grade 7. To contribute to the existing literature, we evaluate the effects of a large-scale program that increased computer and internet access in secondary schools in Peru. We generate differences-in-differences estimates exploiting rich longitudinal data between 2001 and 2006. We find no evidence that the program affected repetition, dropout or enrollment in grade 7.

As mentioned, Bet, Ibarrarán and Cristia (2010) document that total computer use increases substantially with higher ICT access in secondary schools in Peru. Therefore, it does not seem that the modest impacts on dropout rates and enrollment can be attributed to the inability of schools to use the additional resources. These findings give scant support to the hypothesis that the introduction of computers in schools could increase learning indirectly through increases in enrollment in schools. Moreover, it is commonly argued that computers increase students' motivation (InfoDev, 2005). In light of the results presented, the actual consequences of the potential increase in motivation might be limited, or at least not sufficient to affect these long-term decisions about initial enrollment and dropping out of school.

These results, complemented with the existing literature, suggest some tentative policy implications. First, it seems that the feasibility of improving coverage and quality of education in subjects such as Math and Language by using ICT is not straightforward. However, increases in ICT access induce the development of ICT skills, which could be valuable in the labor market. This suggests that some basic level of ICT access in all schools should be promoted and that devoting limited resources to teaching ICT skills might be ideal. Second, expansions beyond the referred basic level might not be optimal, at least if computers are used in the same way they have been used so far. Third, the versatility of computers suggests that alternative uses and arrangements might produce positive outcomes. Because successful models of use have not yet been clearly identified, experimentation and evaluation of alternative models can be particularly valuable for guiding public policy in this area.

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Table 1. Summary Statistics: Schools Responding in All Years and in 2001 and 2006

	2001		2006	
	Respondents in all Years	**Respondents in this year**	**Respondents in all Years**	**Respondents in this year**
	(1)	**(2)**	**(3)**	**(4)**
Outcomes				
Repetition Rate	10.8	10.7	9.7	9.4
Dropout Rate	5.6	5.6	5.6	5.7
Enrollment in Grade 7	187.8	186.0	165.9	159.0
Technology Access				
% Have Computer	67.9	67.8	84.9	83.2
Computers (Total)	11.1	11.1	21.4	20.5
Computers for Learning	9.4	9.4	17.5	16.8
SIPA (Hrs/Week)	0.8	0.8	2.2	2.2
% Have Computer Lab	39.1	39.2	75.6	73.7
% Have Internet Access	16.2	16.6	55.5	54.0
School Characteristics				
Enrollment	780.4	773.1	726.9	696.5
% Rural	16.8	16.9	18.0	19.1
% Private	15.4	16.2	16.6	20.3
% Have Teachers' Lounge	57.3	57.2	53.5	52.4
% Have Administrative Office	90.1	89.7	80.9	79.6
% Have Library	75.1	74.8	74.8	72.2
% Have Water	84.6	84.7	87.7	86.4
% Have Sanitation	95.0	94.8	97.4	94.9
% Have Electricity	83.8	83.9	93.2	92.1
N	7319	8252	7319	10635

Notes: This table presents means of the variables used in the paper. Each column corresponds to a sample of secondary schools. Columns 1 and 3 include schools that answered the surveys in all years used in the analysis (2001, 2003, 2004, 2005 and 2006). Columns 2 and 4 include schools that answered the survey in a particular year (2001 or 2006).

Table 2. Summary Statistics by Public/Private and Urban/Rural Status in 2004

	All (1)	Public Urban (2)	Public Rural (3)	Private Urban (4)	Private Rural (5)
Outcomes					
Repetition Rate	10.9	12.3	10.7	4.8	8.1
Dropout Rate	6.2	6.0	10.4	2.6	6.6
Enrollment in Grade 7	171.5	224.3	55.9	76.9	52.3
Technology Access					
% Have Computer	78.5	86.5	37.7	90.0	56.8
Computers (Total)	16.8	17.6	2.1	30.0	11.2
Computers for Learning	14.5	15.3	1.7	25.3	9.8
SIPA (Hrs/Week)	1.5	0.8	0.4	5.6	2.7
% Have Computer Lab	60.7	67.7	15.7	80.6	48.7
% Have Internet Access	30.3	33.1	2.0	49.6	19.5
School Characteristics					
Enrollment	762.2	999.8	227.2	352.5	212.3
% Overaged in Grade 7	42.5	43.0	61.8	19.1	43.3
% Have Principal	89.1	92.8	81.5	82.0	84.7
% Have Library	71.7	79.8	32.3	80.3	68.2
% Have Water	82.8	88.1	56.4	89.9	55.2
% Have Sanitation	97.5	98.9	90.0	99.8	95.3
% Have Electricity	85.4	91.0	58.5	91.2	76.2
N	7319	2555	2666	2028	70

Notes: This table presents means in 2004 for schools that answered the survey in all years used in the analysis. Each column corresponds to certain group of secondary schools.

Table 3. Summary Statistics in 2003 by Treatment Status

	All Schools			Trimmed and Reweighted Schools		
	Treatment (1)	**Comparison (2)**	**Difference (3)**	**Treatment (4)**	**Comparison (5)**	**Difference (6)**
Outcomes						
Repetition Rate	11.1	10.9	0.2	11.1	11.5	-0.5
			(0.2)			(0.4)
Dropout Rate	5.4	6.8	-1.4***	5.5	5.9	-0.3
			(0.2)			(0.2)
Enrollment in Grade 7	195.6	122.1	73.5***	160.4	166.9	-6.5
			(4.8)			(8.2)
Educational Inputs						
Enrollment	884.6	545.0	339.5***	717.9	759.0	-41.1
			(22.6)			(39.4)
% Overaged in Grade 7	44.4	48.9	-4.5***	44.0	46.0	-2.0
			(0.8)			(1.4)
% Have Principal	92.5	87.9	4.6***	90.7	90.9	-0.2
			(1.4)			(2.3)
% Have Administrative Office	90.5	87.0	3.5***	90.7	93.4	-2.6
			(1.4)			(2.1)
% Have Library	81.5	61.2	20.3***	79.7	80.8	-1.1
			(2.0)			(3.2)
% Have Water	92.4	86.1	6.3***	92.3	93.5	-1.3
			(1.4)			(2.0)
% Have Sanitation	98.0	94.6	3.4***	98.5	97.6	0.9
			(0.9)			(1.2)
% Have Electricity	93.2	86.1	7.1***	93.4	93.4	0.0
			(1.4)			(2.0)
Technology Access						
Number of Computers	6.0	4.3	1.7***	5.6	5.0	0.6
			(0.4)			(0.6)
SIPA (Hrs/Week)	0.4	0.4	0.0	0.4	0.3	0.1
			(0.0)			(0.0)
% Have Computer Lab	57.5	35.2	22.3***	56.1	47.3	8.8**
			(2.2)			(4.0)
% Have Internet Access	2.6	3.8	-1.2	2.8	5.8	-3.0*
			(0.8)			(1.7)
N	694	1,220		330	376	

Notes: This table presents means and differences between the treatment and comparison groups. Columns 1 to 3 present statistics for secondary public urban schools that had not participated in a program of technology in education by 2003. In columns 4 to 6, the sample is further reduced to include schools that have a probability of treatment between 0.3 and 0.7 and observations are reweighted by 1/(1-PS) where PS corresponds to the probability of treatment. Significance at the 1, 5 and 10 percent levels is indicated by ***, ** and *, respectively.

Table 4. Estimated Program Effects on SIPA and Internet Access

	SIPA (Hrs/Week)		Internet Access	
	(1)	**(2)**	**(3)**	**(4)**
Treatment	0.345***	0.346***	0.247***	0.242***
	(0.042)	(0.040)	(0.028)	(0.027)
Constant	0.266***	0.570**	0.028***	0.009
	(0.016)	(0.235)	(0.010)	(0.184)
N	33,583	33,583	33,583	33,583
R^2	0.655	0.663	0.490	0.505
Time-Varying Controls	No	Yes	No	Yes

Notes: This table presents estimates of the effects of participating in the Huascarán program on SIPA and Internet access. The unit of observation is year-school-grade-sex. Each column corresponds to a separate regression. The sample includes secondary public urban schools that had not participated in a program of technology in education by 2003 and that have a probability of treatment between 0.3 and 0.7. All regressions control for year, school, grade and sex fixed effects. Regressions in even-numbered columns also include time-varying controls described in Section 4. Observations are reweighted by 1/(1-PS) where PS corresponds to the probability of treatment. Standard errors, reported in parenthesis, are clustered at the school level. Significance at the 1, 5 and 10 percent levels is indicated by ***, ** and *, respectively.

Table 5. Estimated Program Effects on Repetition, Dropout and Enrollment

	Repetition Rate		Dropout Rate		Enrollment in Grade 7	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment	0.014	-0.031	-0.060	-0.038	0.032	0.007
	(0.534)	(0.531)	(0.191)	(0.191)	(1.833)	(1.753)
Constant	11.790***	2.101	5.389***	5.462***	87.850***	79.784***
	(0.311)	(2.755)	(0.117)	(1.097)	(0.853)	(4.375)
N	33,583	33,583	33,583	33,583	6,749	6,749
R^2	0.247	0.256	0.298	0.299	0.914	0.915
Time-Varying Controls	No	Yes	No	Yes	No	Yes

Notes: This table presents estimates of the effects of participating in the Huascarán program on repetition, dropout and enrollment in grade 7. The unit of observation is year-school-grade-sex. Each column corresponds to a separate regression. The sample includes secondary public urban schools that had not participated in a program of technology in education by 2003 and that have a probability of treatment between 0.3 and 0.7. All regressions control for year, school, grade and sex fixed effects. Regressions in even-numbered columns also include time-varying controls described in Section 4. Observations are reweighted by 1/(1-PS) where PS corresponds to the probability of treatment. Standard errors, reported in parenthesis, are clustered at the school level. Significance at the 1, 5 and 10 percent levels is indicated by ***, ** and *, respectively.

Table 6. Estimated Program Effects using Propensity Score Reweighting: Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Repetition Rate								
Treatment	0.527	0.152	0.402	-0.063	0.272	-0.137	0.327	-0.031
	(0.337)	(0.437)	(0.348)	(0.446)	(0.392)	(0.475)	(0.474)	(0.531)
N	90,689	89,808	72,803	72,803	51,925	51,925	33,583	33,583
Panel B: Dropout Rate								
Treatment	0.265	0.113	0.230	0.091	0.143	-0.071	0.133	-0.038
	(0.163)	(0.146)	(0.164)	(0.148)	(0.182)	(0.162)	(0.211)	(0.191)
N	90,689	89,808	72,803	72,803	51,925	51,925	33,583	33,583
Panel C: Enrollment in Grade 7								
Treatment	-0.028	-0.388	0.597	0.311	0.932	0.469	1.231	0.007
	(0.510)	(1.301)	(0.535)	(1.265)	(0.627)	(1.421)	(0.764)	(1.753)
N	18,382	18,207	14,694	14,694	10,452	10,452	6,749	6,749

Notes: This table explores the robustness of the estimated effects of the Huascarán program under alternative specifications. The unit of observation is year-school-grade-sex. Each panel indicates the dependent variable in the regression. Each column in a panel corresponds to a separate regression. The sample used in columns 1 and 2 includes secondary public urban schools that had not participated in a program of technology in education by 2003. In columns 3 and 4 the sample is reduced to those schools with a probability of treatment between 0.1 and 0.9. In columns 5 and 6 (7 and 8), the sample is further reduced to includes schools with probability of treatment between 0.2 and 0.8 (0.3 and 0.7). All regressions control for year, school, grade and sex fixed effects. In even-numbered columns, observations are reweighted by 1/(1-PS) where PS corresponds to the probability of treatment. Standard errors, reported in parenthesis, are clustered at the school level. Significance at the 1, 5 and 10 percent levels is indicated by ***, ** and *, respectively.

Table 7. Estimated Program Effects using Propensity Score Matching: Alternative Specifications

	(1)	(2)	(3)	(4)
Panel A: Repetition Rate				
Treatment	0.050	0.027	0.246	0.087
	(0.437)	(0.444)	(0.495)	(0.554)
N	48,343	44,982	35,921	25,110
Panel B: Dropout Rate				
Treatment	0.183	0.180	0.043	0.074
	(0.153)	(0.156)	(0.177)	(0.195)
N	48,343	44,982	35,921	25,110
Panel C: Enrollment in Grade 7				
Treatment	-0.272	0.113	0.577	0.575
	(1.306)	(1.270)	(1.433)	(1.705)
N	9,735	9,055	7,226	5,043

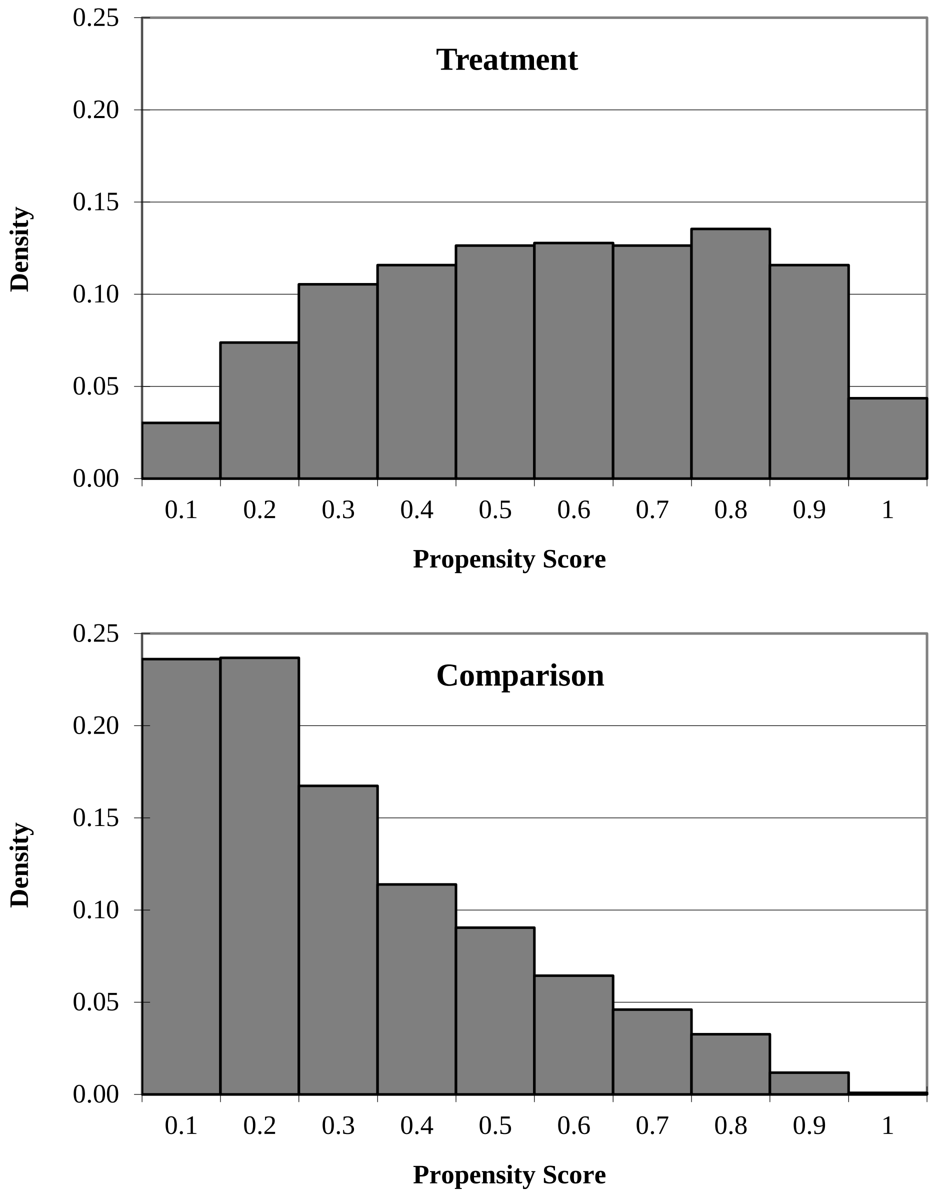
Notes: This table presents estimated program effects using propensity score matching under alternative trimming bandwidths. In all cases results are obtained using nearest neighbor propensity-score matching with replacement. The unit of observation is year-school-grade-sex. Each panel indicates the dependent variable in the regression. Each column in a panel corresponds to a separate regression. The starting sample includes secondary public urban schools that had not participated in a program of technology in education by 2003. In column 1 the sample is reduced to treatement and matched comparison schools. In column 2 the sample is further reduced to those schools with a probability of treatment between 0.1 and 0.9. In column 3 and 4, the sample is further reduced to includes schools with probability of treatment between 0.2 and 0.8 and 0.3 and 0.7, respectively. All regressions control for year, school, grade and sex fixed effects. Standard errors, reported in parenthesis, are clustered at the school level. Significance at the 1, 5 and 10 percent levels is indicated by ***, ** and *, respectively.

Table 8. Placebo Test: Estimated Program Effects during Pre-Treatment Period

	Repetition Rate		Dropout Rate		Enrollment in Grade 7	
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo Treatment	0.922	1.048	0.052	-0.012	-1.470	-1.628
	(0.806)	(0.786)	(0.287)	(0.278)	(2.042)	(1.971)
Constant	11.824***	5.321	5.403***	7.093***	87.708***	81.398***
	(0.219)	(5.361)	(0.080)	(2.166)	(0.610)	(8.647)
N	13,286	13,286	13,286	13,286	2,690	2,690
R^2	0.329	0.339	0.357	0.360	0.925	0.927
Time-Varying Controls	No	Yes	No	Yes	No	Yes

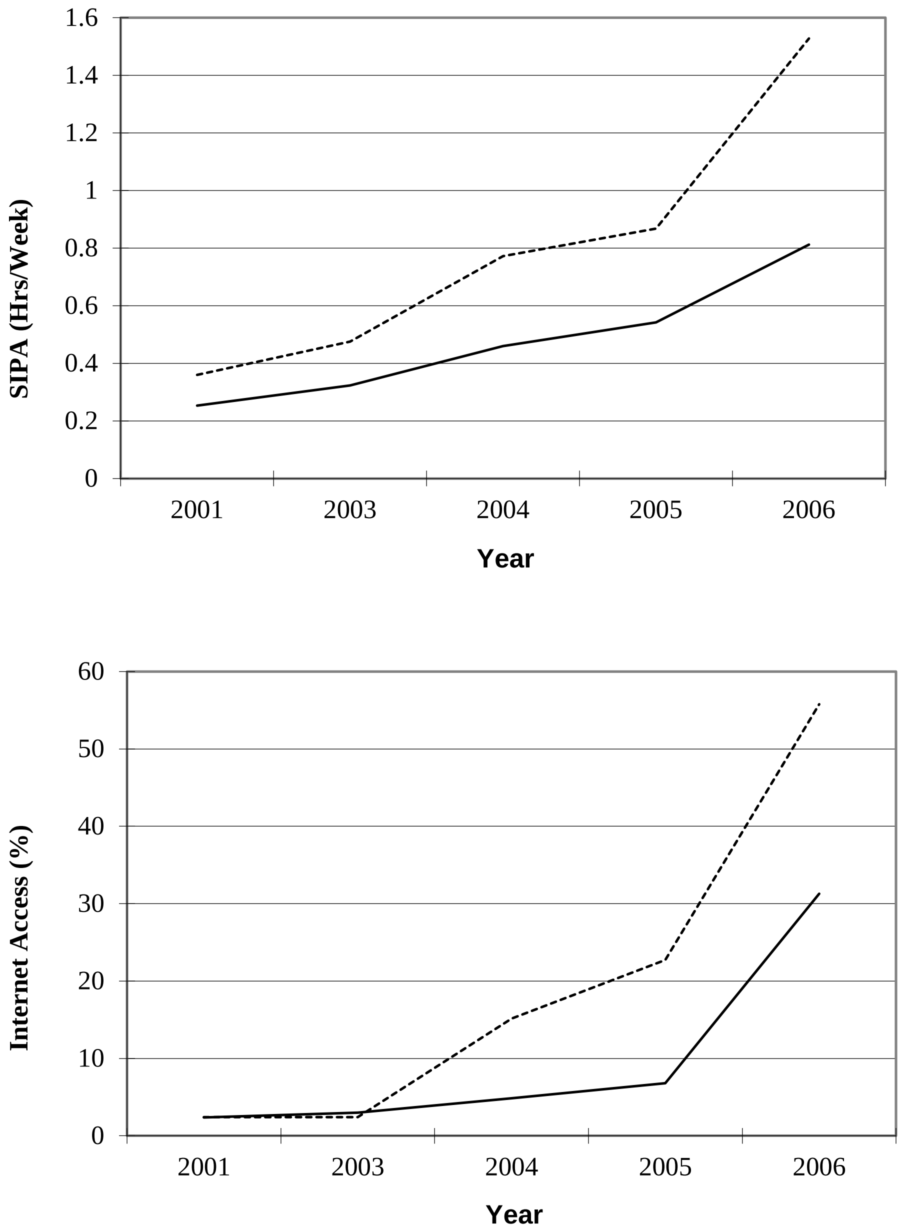
Notes: This table presents placebo tests to check whether there are pre-intervention differential trends in outcomes between treatment and comparison schools. The unit of observation is year-school-grade-sex. Each column corresponds to a separate regression. The sample includes secondary public urban schools that had not participated in a program of technology in education by 2003 and that have a probability of treatment between 0.3 and 0.7. All regressions control for year, school, grade and sex fixed effects. Regressions in even-numbered columns also include time-varying controls described in Section 4. Observations are reweighted by 1/(1-PS) where PS corresponds to the probability of treatment. Standard errors, reported in parenthesis, are clustered at the school level. Significance at the 1, 5 and 10 percent levels is indicated by ***, ** and *, respectively.

Figure 1. Propensity Score Density for Treatment and Comparison Groups



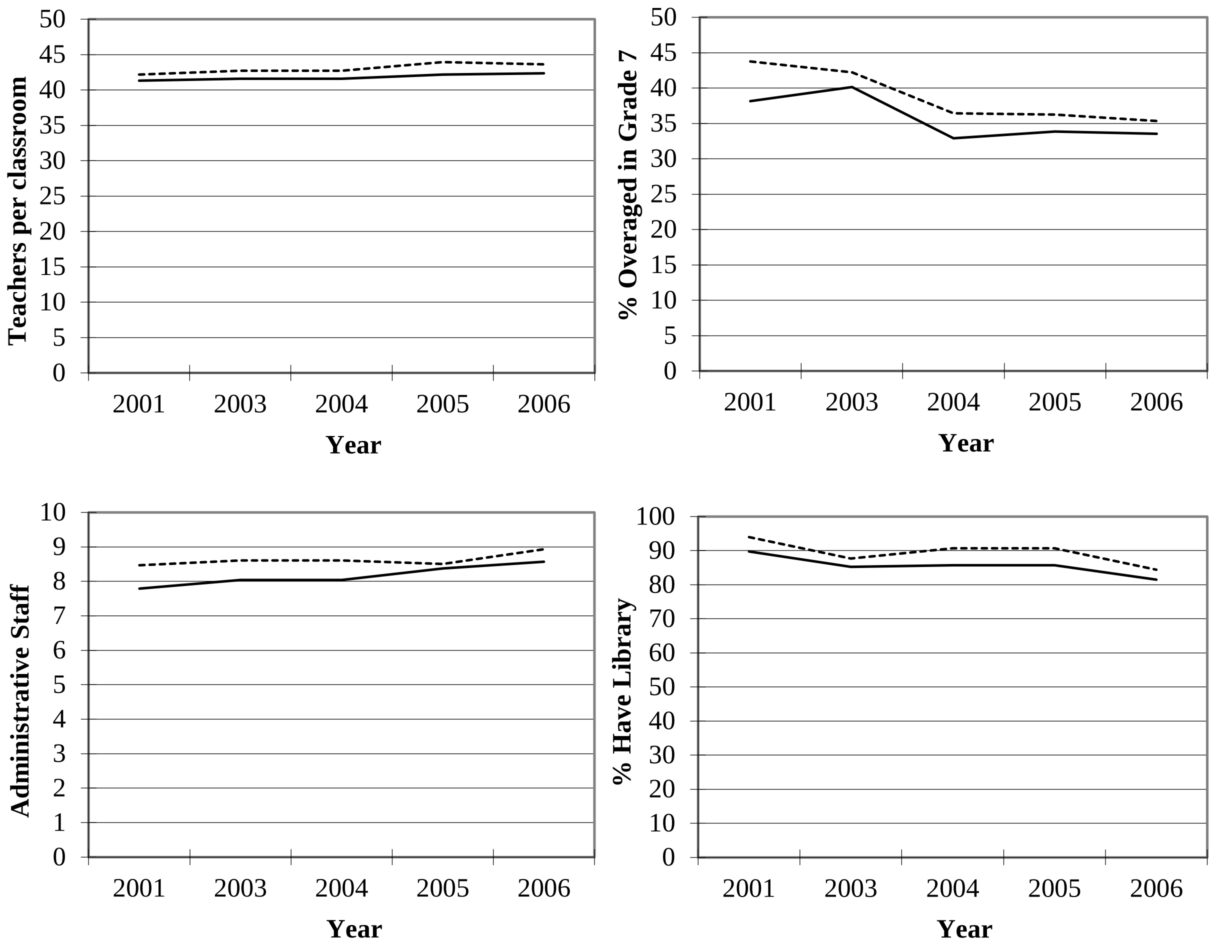
Notes: The figures show the density of the propensity score for treatment and comparison schools. The histograms were constructed using the predicted scores from the estimation of a logistic regression for the year 2003 where the dependent variable takes value one if the school participated in the Huascarán program, zero otherwise (see Section 4 for the list of explanatory variables). The sample includes secondary public urban schools that had not participated in a program of technology in education by 2003.

Figure 2. Evolution of SIPA and Internet Access by Treatment Status



Notes: The figures show the evolution of SIPA and Internet access over time. The dotted (solid) line represents averages by year for the Treatment (Comparison) group. The sample includes secondary public urban schools that had not participated in a program of technology in education by 2003 and that have a probability of treatment between 0.3 and 0.7. Observations are reweighted by 1/(1-PS) where PS corresponds to the probability of treatment. Year 2002 is not included because administrative data are not available for that year.

Figure 3. Evolution of School Inputs by Treatment Status



Notes: The figures show the evolution of school inputs over time. The dotted (solid) line represents averages by year for the Treatment (Comparison) group. The sample includes secondary public urban schools that had not participated in a program of technology in education by 2003 and that have a probability of treatment between 0.3 and 0.7. Observations are reweighted by 1/(1-PS) where PS corresponds to the probability of treatment. Year 2002 is not included because administrative data are not available for that year.