XVII

Volume XVII, Number 1, May 2014

Journal of Applied Economics

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UCEMA

Edited by the Universidad del CEMA
Print ISSN 1514-0326
Online ISSN 1667-6726

Journal of Applied Economics. Vol XVII, No. 1 (May 2014), 89-112

DOES TECHNOLOGY IN SCHOOLS AFFECT REPETITION, DROPOUT AND ENROLLMENT? EVIDENCE FROM PERU

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Submitted June 2011; accepted May 2013

Many developing countries are allocating significant resources to expand technology access in schools. Whether these investments will translate into measurable educational improvements remains an open question because of the limited existing evidence. This paper contributes to fill this gap 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 is 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 classification codes: I21, I28
Key words: computers in education, dropout rates, repetition rates, enrollment.

I. Introduction

In the last fifteen years, many developing countries have embarked on ambitious programs to expand computer access in schools. Among developing countries that

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participated in the OECD Program for International Student Assessment (PISA) in 2001 and 2006, the ratio of computers per student increased 50% in just 5 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. One line of research has explored the effects of programs focused on increasing computers and internet access and found little evidence of impacts (Angrist and Lavy 2002; Goolsbee and Guryan 2006; Machin et al. 2007; Barrera-Osorio and Linden 2009; Cristia et al. 2012). 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 et al.2009).

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 different behavioral changes. They can affect permanent decisions including enrollment and dropout. Additionally, they can affect decisions taken daily about attendance to school.[1]

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

[1] This conceptual framework highlights the fact 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.

grade 7, the first year in secondary schools, to explore whether families' decisions about the registration of children to schools could also be affected.[2] 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 dropping out 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 used 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 Huascaran program, implemented between 2001 and 2006, aimed to improve educational outcomes through introducing technology in 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 education public 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 information and communications technology (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.

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, repetition 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

[2] Enrollment in a school at a certain grade is affected by the decision of families to register their child in that school, the 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.

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 at al. (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 on 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 higher 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.[3]

The paper proceeds as follows. Section II provides some institutional background and Section III describes the data used. Section IV lays out the empirical strategy, Section V presents results and Section VI explores their robustness. Section VII concludes.

II. Education in Peru

Peru is considered an upper middle income country that 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

[3] Both our study and Bet at al. (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 et al. (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 at al. (2010) use data from one grade in 200 schools. Finally, we estimate effects on repetition, dropout rates and initial enrollment, whereas Bet at al. (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 et al. (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 et al. (2010) find positive effects on ICT literacy

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 (3.0 versus 4.9 percent of GDP in 2009, World Bank 2013).

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 Huascaran, 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 in the learning process. Schools selected into the program received hardware, software (Microsoft Office applications and digital media but not interactive software), teacher training and they were prioritized to receive internet access. In addition, the program funded "innovation room coordinators", individuals trained in IT and pedagogy responsible to ensure the effective use of computer labs in all subject areas. However, as noted above, Bet et al. (2010) document that the overwhelming majority of time used was devoted to learn 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: a) high enrollment levels, b) ease of access to schools, c) commitment by directors, teachers and parents to support and sustain the initiative. Still, other considerations could have played a role in final decisions.[4]

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

III. 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: 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 spans 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 sample across time, we restricted our attention to schools that provided information in all years used in the analysis.

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.

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[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 was collected annually for the period 2003-2006.

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

Table 1. Summary statistics - schools responding in all years and in 2001 and 2006

		2001		2006	
		Respondents in all years (1)	Respondents in this year (2)	Respondents in all years (3)	Respondents in this year (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 (Hs/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
	% Overaged in Grade 7	45.5	45.3	38.5	38.5
	% Have Principal	86.2	85.7	90.2	88.2
	% 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		7,319	8,252	7,319	10,635

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

In the top panel we observe that repetition rates are high, although they have decreased by about 1 percentage point in the period under consideration. The dropout rate has remained virtually unchanged in this period, while average enrollment has decreased slightly. 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 67.9 to 84.9 percent, while the fraction of schools with a computer lab rises from 39.1 to 75.6 percent. The fraction of schools with internet access more than tripled, increasing from 16.2 to 55.5 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:

$$\mathrm{SIPA}_{it} = \frac{\text{Computers for Learning}_{it}}{\text{Enrollment}_{i,2001}} * 2 * 25,$$

where i and t indexes 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 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.[7] Schools in the different groups vary widely in terms of

[7] Results are similar along the period under analysis (2001-2006). For brevity we present results for 2004.

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 and lower in public and rural schools. Because the Huascaran program targeted primarily public urban schools, we restrict the analysis to schools in this group.

Table 2. Summary statistics by public/private and urban/rural status in 2004

		All	Public urban	Public rural	Private urban	Private rural
		(1)	(2)	(3)	(4)	(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 (Hs/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 Teachers' Lounge	55.3	57.3	20.6	85.2	55.8
	% Have Administrative Office	89.8	92.3	73.3	97.7	83.9
	% 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		7,319	2,555	2,666	2,028	70

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

IV. Empirical strategy

As noted previously, program administrators pointed to three main factors that influenced the decision to select a school into the Huascaran 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.[8]

Under this empirical strategy, schools in the treatment group are late entrants, as they were not selected for ICT programs before 2001, neither during the first stage of the Huascaran 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.

[8] 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.

Table 3. Summary statistics in 2003 by treatment status

	All schools			Trimmed and re-weighted schools		
	Treatment	Comparison	Difference	Treatment	Comparison	Difference
	(1)	(2)	(3)	(4)	(5)	(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 Teachers' Lounge	53.9	42.7	11.2***	52.5	52.2	0.3
			(2.3)			(4.0)
% 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 (Hs/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	

Note: 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 re-weighed 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.

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 2 in Table 3 document that schools in the treatment group tend to be larger, have better infrastructure and lower dropout 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.[9] 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 of 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.[10]

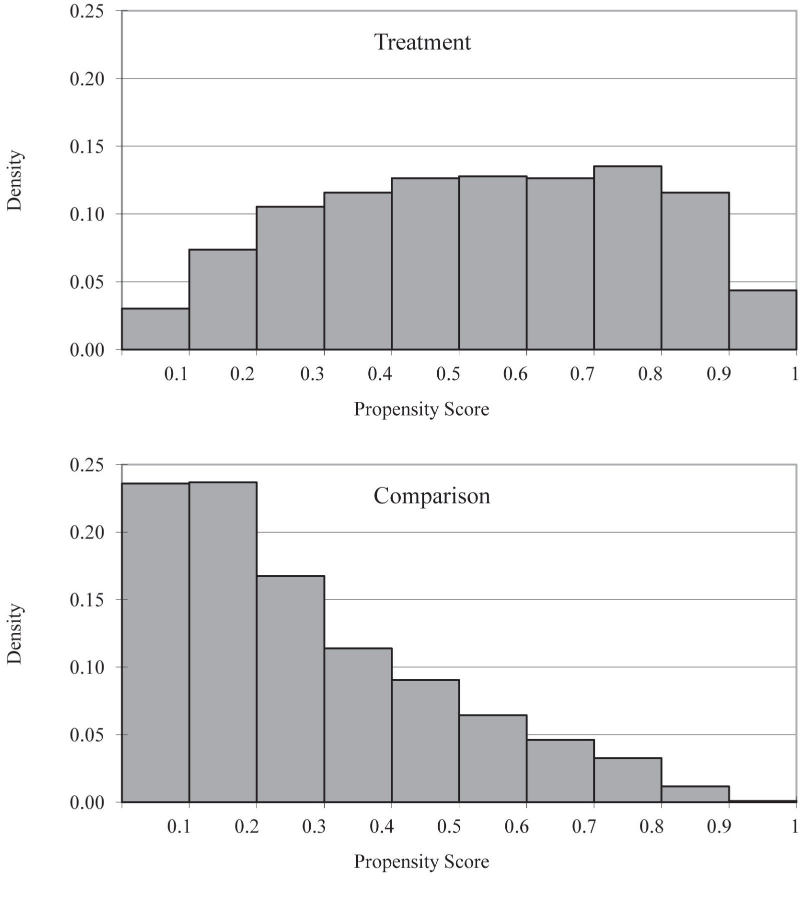
Columns 4 to 6 in Table 3 document that the trimming and reweighting procedure generated treatment and comparison groups that are well balanced in terms of observable covariates in 2003. The differences in means across groups in the original sample (column 3) are substantially reduced and typically become 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

[9] 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 enrolment 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 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).

[10] In Section VI, 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.

present for other important variables, such as the dropout and repetition rates, not included in the estimation of the propensity score.

Figure 1. Propensity score density for treatment and comparison groups



Note: 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 Huascaran program, zero otherwise. The sample includes secondary public urban schools that had not participated in a program of technology in education by 2003.

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:

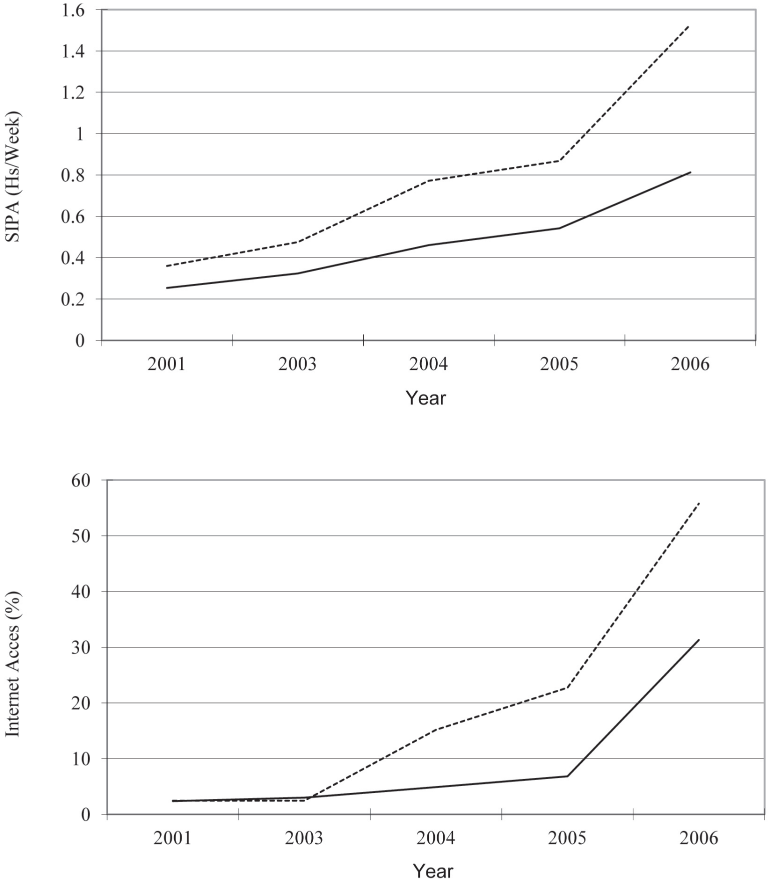
$$Y_{itgs} = \alpha + \beta T_{it} + \gamma X_{itgs} + \mu_i + \eta_t + \pi_g + \chi_s + \varepsilon_{itgs}\,, \tag{1}$$

where Y corresponds to the outcome variable (enrollment, repetition and dropout rates), X is a vector of time-varying controls, while μ, η, π, and 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 Huascaran 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 sections, 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.

V. Results

As expected, results in Table 4 indicate that participating in the program produced positive and statistically significant effects on both SIPA and internet access. 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 graph 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.

Figure 2. Evolution of SIPA and internet access by treatment status



Note: 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 re- weighed by 1/(1-PS) where PS corresponds to the probability of treatment. Year 2002 is not included because administrative data is not available for that year.

Table 4. Estimated program effects on SIPA and internet access

	SIPA (Hs/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

Note: This table presents estimates of the effects of participating in the Huascaran 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 IV.1. Observations are re-weighed 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.

We next examine the effects of the Huascaran program on educational related 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 0.014 percentage points in the repetition rate when no controls were added, and with a decrease in 0.031 percentage points when including time-varying controls. Similarly, the point estimates of effects on the dropout rate are 0.060 percentage points in the model without controls and 0.038 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.

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

Note: This table presents estimates of the effects of participating in the Huascaran 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 IV.1. Observations are re-weighed 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.

VI. Robustness checks

This section 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 within 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 reduced 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.

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

Note: This table explores the robustness of the estimated effects of the Huascaran 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 re-weighed 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

Note: This table presents estimated program effects using propensity score matching and under alternative trimming bandwidths. 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 includes secondary public urban schools that had not participated in a program of technology in education by 2003. In columns 2 the sample is 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. Nearest neighbor with replacement is used to match treated with comparison schools. 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.

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.

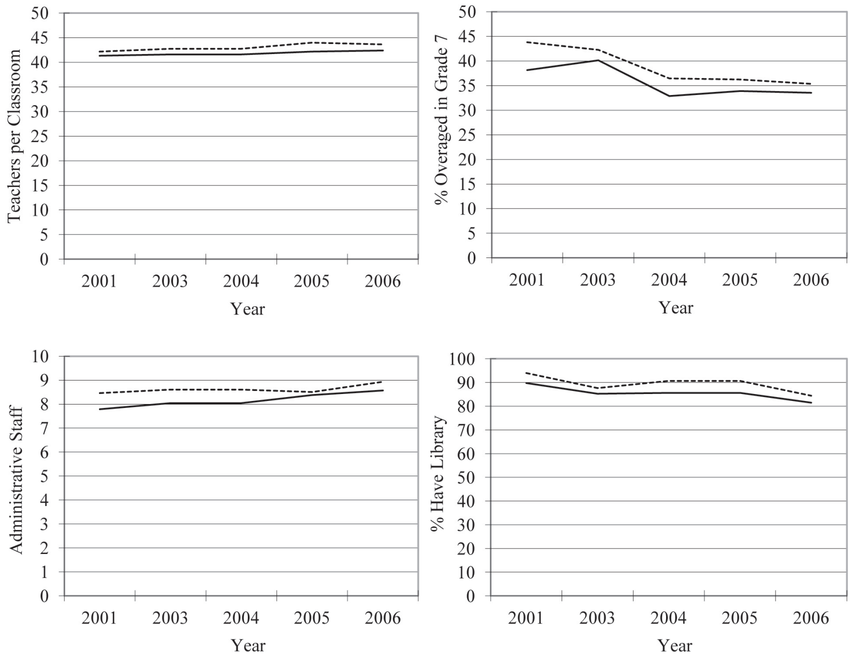
Table 8. Placebo test - estimated program effects during pre-treatment period

	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

Note: This table presents placebo tests to check whether there were 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 IV.1. Observations are re-weighed 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.

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.

Figure 3. Evolution of school inputs by treatment status



Note: 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 re-weighed by 1/(1-PS) where PS corresponds to the probability of treatment. Year 2002 is not included because administrative data is not available for that year.

VII. Conclusions

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 et al. (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 here, the actual consequences of the potential increase in motivation might be limited, or at least, not affecting these long-term decisions about initial enrollment and dropping out from school.

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