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ABSTRACT

In policy circles a lively debate exists regarding the effects on educational outcomes of introducing computers in schools. A number of empirical studies have measured its effect on test scores. There is a lack of empirical evidence, however, on the effects of this type of intervention on drop-out and repetition rates, variables that have a direct impact on years of education. This paper aims to fill this gap in the literature. To this end, we analyze rich longitudinal censal data from Peru as well as information regarding a specific program that deployed computers in 350 schools in the year 2004. Results indicate null impacts of increasing computer access on repetition, drop-out rates and initial enrollment. The large sample sizes allow us to detect even very modest effects. These results, together with previous evidence on the lack of effects on tests scores, point to a limited potential of computers in improving education outcomes.

INTRODUCTION

There is substantial evidence on the critical role education plays in achieving sustained improvements in welfare for developing countries (Glewwe, 2002). Important efforts have therefore been exerted to generate improvements in coverage as well as in quality of education. In primary education, since most developing countries have almost attained universal coverage, the emphasis lies in how to improve quality (Duflo, 2009). The picture for secondary schools is different as coverage is far from universal, with net enrollment rates of 53 percent in the year 2005.[1] In terms of quality, significant improvements are still needed. Enough evidence of this is that between 20 and 90 percent of grade 8 students in low and middle income countries did not attain the lowest benchmark level in Mathematics and Science test in the year 2003.[2] Hence, for secondary education the challenge remains in determining ways to improve coverage as well as the quality of education.

Identifying specific interventions that are effective in attaining these goals is crucial for developing countries operating under limited budgets. One specific intervention has been highlighted as having the potential of achieving the twin objectives of improving learning and coverage: the introduction of computers in schools. Banerjee et al. (2005) argue that "Computers have the potential to both directly improve learning and indirectly increase attendance by making schools more attractive."

The literature regarding the impacts of Information and Communication Technology (ICT) on educational outcomes has mainly focused on whether the introduction of technology can enhance learning. Rigorous studies that have estimated the impacts of increasing ICT access on learning have in general found null impacts.[3] 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). For developing countries, results support the hypothesis that using interactive

[1] UNESCO (2008).

[2] Trends in International Mathematics and Science (TIMSS) (2003) and UNESCO (2005).

[3] Angrist and Lavy (2002) analyzed a program 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 US on test scores in Math, Reading and Science. Barrera-Osorio and Linden (2009) found no impacts of increased computer access on Math and Language. As an exception, Machin et al. (2007) found some positive impacts in English and Science but not in Math.

software can be effective when it replaces low-quality instructional time (Banerjee et al., 2005), but can produce negative effects when used in schools where traditional pedagogies are producing fast learning (Linden, 2008).[4]

This paper aims to contribute to the literature by providing evidence about the impacts of increasing computer access in secondary schools on drop-out rates and initial enrollment. If the hypothesis that computers can make schools more attractive is true, we should expect a positive impact on these dimensions. Additionally, the paper provides evidence regarding the effects of ICT on learning in developing countries by analyzing the impact of increased technology access on repetition rates. To that end, we exploit a very rich data set from Peru which contains longitudinal information on the aforementioned outcomes for virtually all secondary schools for the period 2001 to 2006, as well as a host of educational inputs.

The current paper contributes to the literature in several ways. To begin with, it is the first study to analyze the impacts of increasing ICT access on drop-out rates and initial enrollment. By doing so, we can test the hypothesis of whether a higher availability of computers in schools induces higher attendance. Second, the large sample sizes enable us to obtain extremely precise estimates, which is particularly important given the a priori expectations of small impacts. Finally, our results can be better interpreted given the complementary analysis found in Bet et al. (2010), who describe the way computers were used in the particular context of our study.

To analyze the impact of ICT access on the educational outcomes mentioned above, we follow two different approaches. We start by exploiting the plausibly exogenous increase in the number of computers per student generated by a program, funded by the Inter-American Development Bank (IDB), which distributed 10 computers in 350 public secondary schools in 2004. A suitable comparison group is constructed including schools that had received earlier hardware deployments from the previous government, and were therefore deemed ineligible for participation in the IDB intervention. Results indicate no impacts of increased ICT access on the outcomes considered. Even though the sample size in terms of students-year available is large, we are not able to detect economically significant impacts under this empirical strategy.

Aiming to increase the estimates' precision, we execute a second analysis in which we exploit the substantial variation in increases in ICT access in public urban schools during the analyzed period by estimating fixed-effects models. As

[4] For developed countries, a recent large-scale randomized-controlled trial in the US found evidence of no impacts of use of interactive software in Math and Reading (Dynarski et al., 2007).

before, results point towards the inability of increased ICT access to reduce repetition and drop-out rates, as well as changing initial enrollment. However, in this case we are able to detect even very small impacts. Results indicate that increases in one weekly hour of ICT access cannot decrease repetition rates by more than 0.2 percentage points or roughly two percent of the baseline rate. Similarly, we can detect impacts, in term of baseline rates, larger than three percent for drop-out rates and larger than one percent for enrollment in first grade. We try different modeling assumptions to check the presence of lag effects, non-linear impacts of ICT access on the analyzed outcomes and heterogeneous effects across different groups of children, but we always arrive to the same qualitative conclusions. Finally, we perform a number of exercises to check the robustness of our empirical strategy. The results support the methodology followed (e.g. trends in outcomes in initial years do not predict trends in ICT access in later years).

The lack of impacts on drop-out rates and initial enrollment suggests that, although computers may be attractive to students, there are other more powerful forces at work. In the case of the absence of impacts on repetition rates, the results are consistent with recent evidence found in Bet et al. (2010). In that study increases in ICT access in secondary schools in Peru translates into increased weekly hours of computer use but only to learn ICT skills and not traditional subjects such as Math and Language. These results mimic those found by Barrera-Osorio and Linden (2009), who documented that increases in computer access in Colombia did not translate into higher usage to teach Math and Language. This is evidence of the strong barriers there exist to integrate ICT into traditional subjects.

BACKGROUND

The Education Sector in Peru

Peru is considered a medium development country and ranks 79 out of 179 countries according to the Human Development Index for the year 2008. Its GDP per capita is 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 98 percent in 2007, whereas net enrollment was 76 percent.[5] The amount of resources devoted to education is significantly lower in Peru compared to other middle income countries (2.8 versus 4.4 percent of GDP).

Introduction of ICT in Education in Peru

[5] World Development Indicators (2010).

Until 1996 ICT played a small role as a tool to improve public education in Peru. Since then, a number of 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 in order 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. During this period, there were significant investments in terms of hardware, software (Office applications and digital media but not interactive software), teacher training and connectivity. Also, the program funded "innovation room coordinators", individuals trained in IT and pedagogy who were responsible to ensure the intensive and 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.

The IDB Program

Between March and June of 2004, funded by a loan of the Inter-American Development Bank (IDB), 350 secondary schools were selected to receive an ICT package including the lay-out of the electrical infrastructure, 10 computers and the installation of a network. These schools entered the Huascaran program and, hence, they were assigned an innovation room coordinator, training and standard software. Additionally, the provision of internet access to these schools was prioritized.

Regarding the procedure employed to select the 350 schools into the program, interviews with former government officials suggest that it was carried out in an ad hoc manner. Still, eligible schools had to be public and they should not have been covered by previous governmental programs (data checks showed that both requirements were fulfilled in all cases). Within eligible schools, three factors were considered to select the final set of schools: a) high enrollment levels, b) commitment by directors, teachers and parents to support and sustain the initiative, c) easiness of access to schools. Still, other considerations could have played a role in final decisions.

DATA

The data used in the study is compiled by the Ministry of Education from yearly surveys completed by nearly all secondary schools in the country. Information available included among others: location, private/public type, creation year, enrollment per grade, gender and overage status, number of sections per grade, administrative staff teachers, repetition and drop-out 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 drop-out rates was not available for the year 2002. Additionally, these variables are not available for 2007 as schools report them for the previous year (e.g. in June 2007 they report the number of students that drop-out in 2006). Consequently, we focused the empirical work on years 2001, 2003, 2004, 2005 and 2006. We constructed a panel data where the unit of observation was a school-year-grade-sex.

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 used in the analysis.[6] The third column shows corresponding statistics for 2006. Additionally, the second and fourth columns present statistics for 2001 and 2006, respectively, for all schools that answered the referred survey in those particular years. To ensure the comparability of the analytic sample across time, we restricted our attention to the 7,319 schools that provided information in all years used in the analysis. Imposing this restriction does not significantly alter the composition of the sample.

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

Table 1: Summary Statistics - Main Sample and All Respondents Schools in 2001 and 2006

	2001		2006	
	Main Sample	Respondents in the Year	Main Sample	Respondents in the Year
Outcomes				
Repetition Rate	10.1	10.0	9.2	9.0
Drop Out Rate	5.7	5.7	5.6	5.7
Enrollment First Grade	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
Students Potential Access (in Hours per Week)	0.8	0.8	1.7	1.8
% 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 First Grade	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
Number of Schools	7319	8252	7319	10635

Note: The Main Sample contains schools that answered the surveys in all years used in the analysis (2001, 2003, 2004, 2005 and 2006). Statistics for Respondents in the Year corresponds to schools that answered the survey in the particular year (2001 or 2006).

In the top panel we observe that repetition rates are high, although they have decreased by around 10 percent in the period under consideration. However, the drop-out rate 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 goes from 39 to 76 percent. The fraction of school with internet access more than tripled, going from 16 to 55 percent.

We also present information for the variable Students ICT Potential Access (SIPA). This is the central variable of interest in the paper. It measures the number of potential hours that students can access computers in the school and it is computed as:

$$SIPA = \frac{Computers\ for\ Learning}{Enrollment} * 2 * 25$$

The variable represents the average number of hours per week that a student would use computers if they were used continuously and shared between two students. This variable has the advantages, over the computer-student ratio typically used, that it is defined for schools with no computers and it is linear in the number of computers in the school. Between 2001 and 2006, SIPA increased from 0.8 to 1.7 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. Private rural schools account for only one percent of schools. Hence, through the paper we do not focus our attention on this selected subgroup of schools but rather on the other three subgroups. Schools in the different categories vary widely in terms of repetition and drop-out rates, as well as in terms of technology access. To take this into account, we proceed to execute separate analyses of the three groups in order to avoid comparing schools with high ICT access (typically private urban schools) to those with low ICT access (public rural) which will differ markedly in many other observable and unobservable dimensions. For brevity, in the paper we will present results on public urban schools since this group includes 65 percent of students and are the main focus of educational ICT policies. Some results for private urban and public rural schools will also be described.

Table 2: Summary Statistics by Public/Private and Urban/Rural in 2004 - Main Sample

	Total	Public Urban	Public Rural	Private Urban	Private Rural
Outcomes					
Repetition Rate	9.6	10.9	9.6	4.3	7.2
Drop Out Rate	5.7	5.2	10.3	2.3	6.1
Enrollment First Grade	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
Students Potential Access (in Hours per Week)	1.3	0.8	0.3	4.3	2.2
% 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 First Grade	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
Number of Schools	7319	2555	2666	2028	70

Note: The Main Sample contains schools that answered the surveys in all years used in the analysis (2001, 2003, 2004, 2005 and 2006).

IMPACTS OF THE IDB PROGRAM

Empirical Strategy

In this section we estimate the impact of the IDB funded program. The identification strategy that we follow is to pin down a suitable comparison group, apply propensity score reweighting to deal with differences in observed covariates, and finally estimate fixed-effects models using the longitudinal nature of the data. To select the comparison group, we exploit the rich data described above, together with the institutional information available regarding the criteria followed to select schools. Two objectives are sought in this decision: a) the comparison group should be as similar as possible to the treated group in terms of observed covariates, b) the group selected should present a post-2003 flat evolution in ICT access in order to generate sharper differences in this dimension.

To guide the identification of a suitable comparison group, we investigate the decisions taken within the Huascaran program by the Ministry of Education in terms of the selection of schools as beneficiaries of computer deployment between 2001 and 2006. From the analysis, several patterns become apparent. First, the main deployment of computers took place in 2004 (when the IDB-funded intervention was implemented) although some computers were distributed before and after that year. Second, schools in almost all cases only received computers once in the period. Third, an important fraction of schools benefitting of pre-Huascaran ICT interventions received computers before 2004 (196 out of 433) but none of them was selected for the IDB intervention or later deployment. Given these facts we considered four potential comparison groups of schools: a) beneficiaries of pre-Huascaran interventions, b) beneficiaries of hardware deployment before 2004 but not included in the previous group, c) beneficiaries of computers in 2005 or 2006, d) non-beneficiaries of publicly-funded computers.

Table 3 presents summary statistics for the treated group and the four potential comparison groups for pre-treatment educational inputs and outcomes as well as post-treatment ICT-related variables. Based on this information, we select the pre-Huascaran schools as the comparison group, as this group contains schools that present a relatively flat trend in SIPA and are similar in terms of observables dimensions to the treatment group.

Table 3: Summary Statistics - Treated and Potential Comparison Groups

	Beneficiary IDB Program	Potential Comparison Groups			
		Beneficiaries Pre-Huascaran	Beneficiary Huascaran before 2004	Beneficiary Huascaran after 2004	Non-beneficiary
Outcomes 2001					
Repetition Rate	12.1	11.8	12.2	10.7	11.2
Drop Out Rate	4.0	4.0	5.3	6.1	6.3
Enrollment First Grade	263.7	343.2	231.5	146.3	133.2
Outcomes 2003					
Repetition Rate	11.5	10.9	11.3	11.0	11.1
Drop Out Rate	4.9	4.1	6.1	6.5	6.8
Enrollment First Grade	240.5	310.2	215.1	134.3	121.0
Educational Inputs 2003					
Enrollment	1098.1	1490.9	946.3	593.6	540.1
% Overaged in First Grade	42.2	38.2	48.6	52.3	52.1
% Have Principal	95.2	97.0	95.2	88.8	87.9
% Have Teachers' Lounge	61.0	68.5	46.1	44.3	42.9
% Have Administrative Office	91.8	93.3	82.7	88.6	86.9
% Have Library	86.9	89.8	72.6	74.1	61.4
% Have Water	93.5	92.1	93.3	90.8	86.0
% Have Sanitation	98.7	99.6	98.1	97.1	94.5
% Have Electricity	94.8	94.1	92.6	91.1	86.1
ICT Indicators 2003					
Computers for Learning	7.6	26.7	7.2	3.8	4.3
Students Potential Access (Hs/Week)	0.4	1.0	0.5	0.4	0.4
% Have Computer Lab	70.1	90.3	54.6	40.5	34.9
% Have Internet Access	2.9	36.9	12.5	2.0	3.7
ICT Indicators 2005					
Computers for Learning	15.8	24.7	9.0	5.6	6.1
Students Potential Access (Hs/Week)	0.9	1.0	0.7	0.6	0.6
% Have Computer Lab	80.8	84.4	63.5	55.5	46.7
% Have Internet Access	63.0	67.4	37.9	6.2	7.5
ICT Indicators 2006					
Computers for Learning	18.7	29.0	12.6	11.1	8.1
Students Potential Access (Hs/Week)	1.1	1.2	0.9	1.2	0.9
% Have Computer Lab	93.3	96.2	86.3	86.1	59.3
% Have Internet Access	88.5	84.5	68.8	50.5	26.0
Number of Schools	267	456	187	427	1218

To increase the similarity of the treatment group and the selected comparison group we apply propensity score reweighting techniques.[7] First, we predict treatment using a logit regression containing a large set of covariates including provincial dummies. Next, we trimmed the sample dropping schools with a predicted participation lower than 0.3 or higher than 0.7. Finally, we reweighted the comparison group by applying a weight of 1/(1-ps), where ps refers to the

[7] Propsensity-score matching methods could have also been used. However, we implement propensity score reweighting due to recent work presenting evidence that this technique may outperform propensity-score matching in settings likely to be found in empirical practice (Busso et al., 2009)

propensity score.[8] The effects of applying these steps can be observed in Table 4. After trimming and reweighting the sample, both groups seem similar in terms of observable inputs different than ICT. But, crucial for the identification strategy followed, the treatment group experiences a significant increase in ICT inputs between 2003 and 2006.

[8] See Imbens (2004) for a discussion of propensity score reweighting estimators and its comparison to other semi parametric estimators.

Table 4: Summary Statistics - Treated and Comparison Groups Before and After Reweighting

	Treated		Comparison	
	All	Trimmed and Weighted	All	Trimmed and Weighted
Outcomes 2001				
Repetition Rate	12.1	12.3	11.7	11.1*
Drop Out Rate	4.0	4.2	4.0	4.5
Enrollment First Grade	266.8	259.3	342.9	252.8***
Outcomes 2003				
Repetition Rate	11.5	11.8	10.9	10.2**
Drop Out Rate	4.9	4.7	4.2	4.5
Enrollment First Grade	240.5	240.4	310.2	223.9
Educational Inputs 2003				
Enrollment	1098.1	1080.5	1490.9	1067.7
% Overaged in First Grade	40.8	40.5	36.3	36.8**
% Have Principal	95.2	95.7	97.0	95.6
% Have Teachers' Lounge	61.0	62.4	68.5	59.0
% Have Administrative Office	91.8	92.8	93.3	91.2
% Have Library	86.9	87.6	89.8	85.2
% Have Water	93.5	93.3	92.1	94.5
% Have Sanitation	98.7	99.2	99.6	98.9
% Have Electricity	94.8	95.1	94.1	95.5
ICT Indicators 2003				
Computers for Learning	7.6	7.1	26.7	19.0***
Students Potential Access (Hs/Week)	0.4	0.4	1.0	1.0***
% Have Computer Lab	70.1	70.6	90.3	88.3***
% Have Internet Access	2.9	3.7	36.9	31.1***
ICT Indicators 2005				
Computers for Learning	15.7	15.8	24.7	18.6**
Students Potential Access (Hs/Week)	0.9	0.9	1.0	1.0
% Have Computer Lab	80.3	81.5	84.4	84.5
% Have Internet Access	62.3	62.4	67.1	67.5
ICT Indicators 2006				
Computers for Learning	18.7	18.0	29.0	19.6
Students Potential Access (Hs/Week)	1.0	1.0	1.2	1.1
% Have Computer Lab	93.2	91.1	96.0	92.3
% Have Internet Access	88.5	85.9	84.2	78.3
Number of Schools	267	177	456	216

Note: Statistics for Trimmed and Weighted columns were obtained running logistic propensity-score regressions, trimming observations with probability of treatment outside the interval (0.3, 0.7) and computing weights using the predicted probabilities of treatment.
*, **, ***: Statistical difference between the Treated and Pre-Huascaran beneficiaries groups (trimmed and weighted) at the 10, 5 and 1 percent significance level.

Finally, we reshaped 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 reweighted sample:

$$Y_{itgs} (1) \alpha + \beta T_i * Year_{04} + \beta T_i * Year_{05} + \beta T_i * Year_{06} + \gamma X_{itgs} + \mu_i + \eta_t + \pi_g + \chi_s + \varepsilon_{itgs}$$

where Y corresponds to the outcome variable, T indicates whether the school was treated, Year04 is an indicator for 2004 (analogously for 2005, 2006), X is a vector of controls, and μ, η, π, χ correspond to dummies at the school, year, grade and sex levels, respectively. The indices i, t, g and s correspond to school, year, grade and sex, respectively. In all regressions standard errors are clustered at the school level.

Results

Table 5 presents the main results of the impact of the IDB program. Computers were distributed and installed in the first semester of 2004, hence results for that year correspond to the impacts of around 6 months after intervention. Results for the years 2005 and 2006 can be interpreted as the impacts at 1.5 and 2.5 years after intervention.

Table 5: Fixed Effects Estimates of IDB Program Impacts

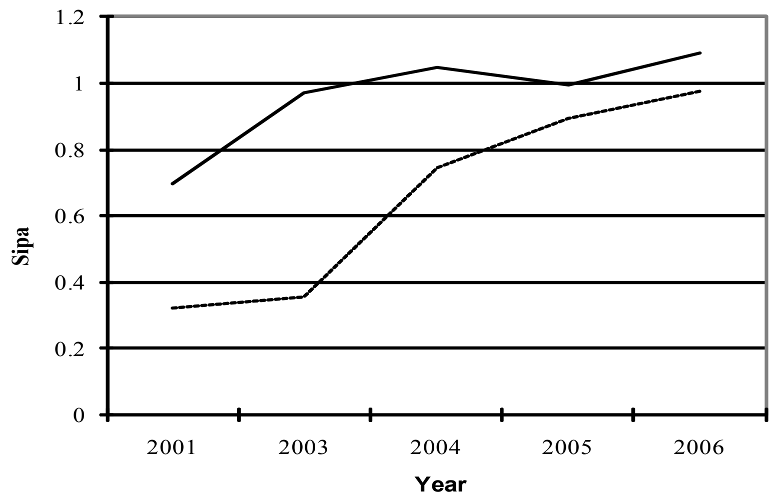
	Repetition Rate		Drop Out Rate		Enrollment in First	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment*Year 2004	-0.041	-0.028	-0.015	0.008	0.272	0.276
	(0.904)	(0.910)	(0.253)	(0.256)	(2.340)	(2.265)
Treatment*Year 2005	-0.616	-0.545	0.271	0.296	0.966	1.110
	(1.072)	(1.039)	(0.304)	(0.308)	(2.762)	(2.678)
Treatment*Year 2006	-1.339	-1.195	0.242	0.280	-0.727	-1.230
	(0.813)	(0.825)	(0.317)	(0.305)	(3.316)	(3.230)
Constant	11.841***	9.513	4.285***	4.530***	128.749***	105.546***
	(0.343)	(6.527)	(0.100)	(1.028)	(1.078)	(11.456)
N	18049	18049	18049	18049	3628	3628
R2	0.264	0.273	0.324	0.330	0.869	0.870
Time-Varying Controls	No	Yes	No	Yes	No	Yes

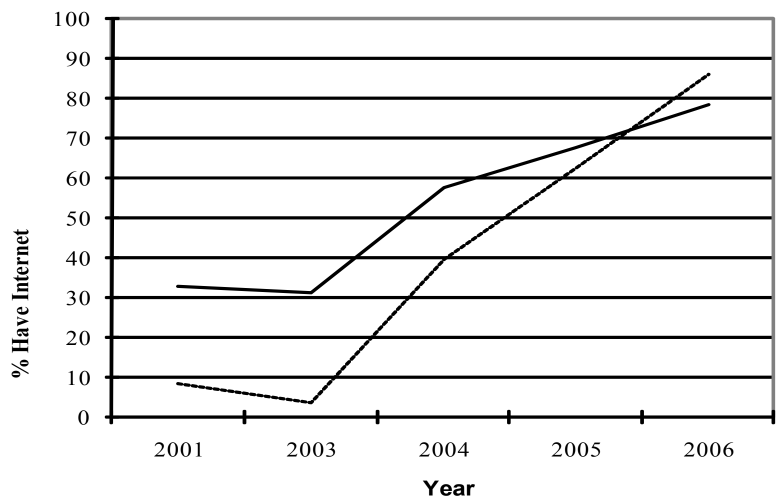
Note: Each column corresponds to one regression. Time-Varying controls are: enrollment, number of administrative staff, teachers appointed per classroom, students per teacher, students per sections, number of classrooms, number of blackboards, number of tables, number of student desks and dummies indicating the school counts with: principal, sub principal, administrative offices, teachers' lounge, workshop, library, other (no ICT) lab, gym, running water, sanitation, electricity. In columns 5 and 6 total enrollment was excluded as a control variable. Standard errors are clustered at the school level. * 10%; ** 5%; *** 1%.

Column 1 presents the estimated effects of the program on repetition rates. The dependent variable was multiplied by 100 and, consequently, the impacts can be interpreted in terms of percentage points. Estimated impacts during 2004 and 2005 are close to zero, but in 2006 participation in the program is associated with a 1.3 percentage point decrease in the repetition rate. However, it is not possible to reject the null of no impact at standard significance levels. Column 2 shows that the results are robust to adding a large number of time-varying controls, suggesting a small role for unobservables in biasing the estimates. Columns 3 to 6 show that the introduction of computers related to the program is not associated with statistically significant changes in the drop-out rate and enrollment in first grade.

Implicit in the above analysis is the idea that treated schools received an increase in access to ICT resources in the 2004 to 2006 period compared to the pre-intervention period. Figure 1 provides evidence on this issue. SIPA in the treatment group increased substantially from 0.38 to 0.78 between 2003 and 2004 and continued to increase afterwards, whereas the comparison group experienced only a subtle increase. Hence, the substantially higher ICT intensity in the comparison group was almost completely wiped out by 2006. Still, these are not significant changes in terms of size given that they correspond to increases in hours of potential use in a week.

Figure 1: Evolution of SIPA and Internet Access by Treatment Status

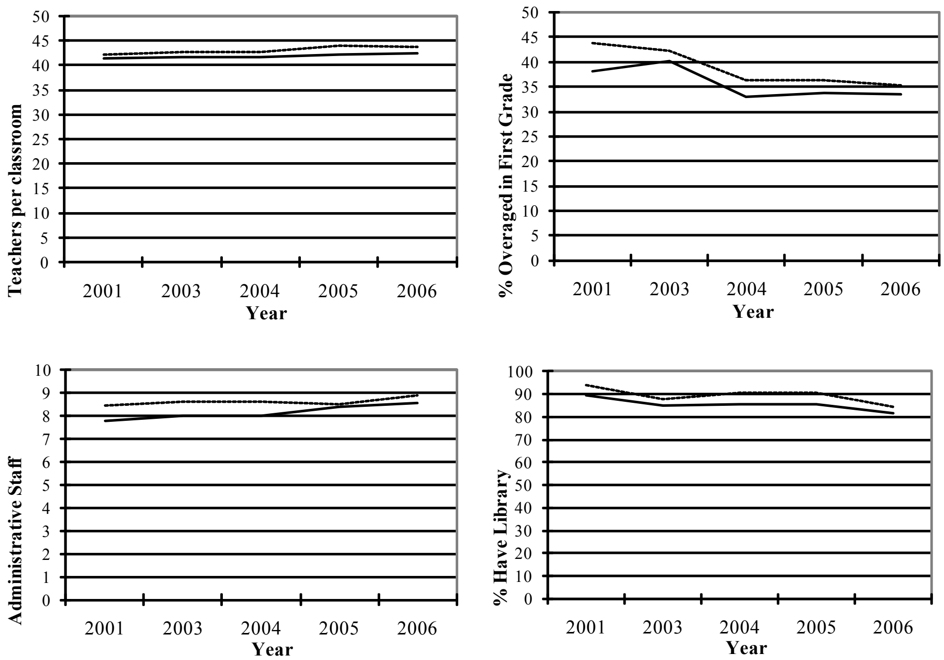




Note: The solid (dotted) line represents the average indicators by year for the Treatment (Comparison) group. Averages were computed using the Main Sample. Treatment and Comparison schools with predicted probability of treatment outside the interval (0.3, 0.7) were dropped. Schools in the Comparsion group were re-weighted applying the factor PS/(1-PS) where PS is the predicted probability of treatment for each school.

Finally, we checked whether there were significant changes in other educational inputs concomitant with the introduction of the program. Figure 2 presents the results. Trends in these inputs are quite flat and similar across the two groups, giving support to the hypothesis that the lack of results cannot be attributed to compensating changes in other inputs.

Figure 2: Evolution of School Inputs by Treatment Status



Note: The solid (dotted) line represents the average indicators by year for the Treatment (Comparison) group. Averages were computed using the Main Sample. Treatment and Comparison schools with predicted probability of treatment outside the interval (0.3, 0.7) were dropped. Schools in the Comparsion group were re-weighted applying the factor PS/(1-PS) where PS is the predicted probability of treatment for each school.

EVIDENCE FROM LONGITUDINAL VARIATION IN ICT ACCESS

The identification strategy executed in the previous section may be successful in consistently estimating the causal impacts of the program. However, the estimates are not very precise. For example, focusing on the impacts on repetition rates in 2006, we can only detect impacts larger than 30 percent of the baseline rate at the 5 per cent significance level. Since from the beginning we did not expect ICT to have a very large impact on this outcome, the results end up being little informative in terms of affecting prior expectations.

In this section, we exploit the rich data set available and estimate fixed-effects models using the whole longitudinal variation in ICT access in the public urban sample. The resulting estimates are far more precise than those from the previous section though the potential for bias may be greater.

Empirical Strategy

We estimate the following regression on the sample of public urban schools:

$$Y_{itgs} = \alpha + \beta\, SIPA_{it} + \gamma\, X_{itgs} + \mu_i + \eta_t + \pi_g + \chi_s + \varepsilon_{itgs} \quad (2)$$

where all variables and indices are defined in the same way as in equation (1).

Additionally, we run a second specification in which we estimate differential effects for four categories for SIPA. The definition of these categories is based on cut-offs at 1, 2 and 3 hours of potential ICT access per week.

In both cases, we run fixed effects models in which we add dummies for school and year, exploiting changes of the variable of interest over time within units. Results may be biased if there are concomitant changes in other inputs that are correlated with changes in SIPA. However, Table 1 shows that during the analyzed period there were substantial changes in ICT access coupled with small changes in other educational inputs. This suggests that the potential for bias in the estimates may be smaller compared to a situation where the variable of interest experiences modest changes, while the other variables are changing strongly.

Results

Table 6 presents estimates of the impact of SIPA when the effect is modeled linearly. Odd columns present results when time-varying controls are not included and even columns when these controls are included. The results suggest that greater access to technology has no impact on the educational outcomes analyzed. However, the large sample size allows us to detect even modest impacts. Focusing on Column 2, we observe that a one-hour per week increase in SIPA is associated with a reduction of 0.006 percentage points in the repetition rate. Importantly, the standard error is very small, which implies that impacts larger than 0.2 percentage points can be detected at the five percent significance level. Similarly, it is possible to detect decreases in the drop-out rate of 0.1 percentage points, as well as an increase in more than 1.5 students in first grade.

Table 6: Fixed Effects Estimates of ICT Access - Linear Effects Specification - Public Urban

	Repetition Rate		Drop Out Rate		Enrollment in First	
	(1)	(2)	(3)	(4)	(5)	(6)
SIPA	-0.042	-0.006	-0.068*	-0.063	0.505	0.358
	(0.119)	(0.117)	(0.038)	(0.039)	(0.546)	(0.569)
Constant	12.128***	12.609***	5.060***	4.702***	138.528***	117.487***
	(0.159)	(1.774)	(0.053)	(0.565)	(0.997)	(8.514)
N	119168	119168	119168	119168	24125	24125
R2	0.272	0.274	0.359	0.359	0.957	0.957
Time-Varying Controls	No	Yes	No	Yes	No	Yes

Note: Each column corresponds to one regression. Time-Varying controls are: enrollment, number of administrative staff, teachers appointed per classroom, students per teacher, students per sections, number of classrooms, number of blackboards, number of tables, number of student desks and dummies indicating the school counts with: principal, sub principal, administrative offices, teachers' lounge, workshop, library, other (no ICT) lab, gym, running water, sanitation, electricity. In columns 5 and 6 total enrollment was excluded as a control variable. Standard errors are clustered at the school level. * 10%; ** 5%; *** 1%.

Table 7 presents estimates when SIPA is modeled as a four-valued categorical variable as described above. For all specifications and outcomes analyzed, we arrive at the same qualitative conclusions: no impact of technology access on the referred outcomes.

Table 7: Fixed Effects Estimates of ICT Access - Non-linear Impacts - Public Urban Sample

	Repetition Rate		Drop Out Rate		Enrollment in First	
	(1)	(2)	(3)	(4)	(5)	(6)
SIPA(1-2 hours per week)	0.001	0.048	-0.087	-0.076	0.216	0.205
	(0.266)	(0.264)	(0.075)	(0.076)	(1.441)	(1.491)
SIPA(2-3 hours per week)	0.077	0.146	-0.139	-0.127	1.159	1.193
	(0.447)	(0.450)	(0.136)	(0.139)	(1.771)	(1.824)
SIPA(3-+ hours per week)	0.059	0.045	-0.286	-0.264	0.676	0.431
	(0.645)	(0.663)	(0.212)	(0.215)	(2.680)	(2.727)
Constant	12.108***	12.590***	5.044***	4.703***	138.689***	117.496***
	(0.158)	(1.768)	(0.051)	(0.566)	(1.008)	(8.501)
N	119168	119168	119168	119168	24125	24125
R2	0.272	0.274	0.359	0.359	0.957	0.957
Time-Varying Controls	No	Yes	No	Yes	No	Yes

Note: Each column corresponds to one regression. Time-Varying controls are: enrollment, number of administrative staff, teachers appointed per classroom, students per teacher, students per sections, number of classrooms, number of blackboards, number of tables, number of student desks and dummies indicating the school counts with: principal, sub principal, administrative offices, teachers' lounge, workshop, library, other (no ICT) lab, gym, running water, sanitation, electricity. In columns 5 and 6 total enrollment was excluded as a control variable. Standard errors are clustered at the school level. * 10%; ** 5%; *** 1%.

Table 8 presents estimates when exploring lagged effects of increases in technology access on outcomes. Odd columns show results when relating the dependent variable to current and previous values of SIPA. Even columns present specifications with current and lagged SIPA. Once again, we arrive at the same qualitative results.

Table 8: Fixed Effects Estimates of ICT Access with Lags - Public Urban Sample

	Repetition Rate		Drop Out Rate		Enrollment in First	
	(1)	(2)	(3)	(4)	(5)	(6)
SIPA	-0.050	-0.052	-0.070*	-0.073*	0.341	0.370
	(0.143)	(0.143)	(0.041)	(0.041)	(0.398)	(0.404)
SIPA First Lag		-0.062		-0.078		0.788
		(0.128)		(0.050)		(0.658)
Constant	11.790***	11.214***	1.781**	1.585*	104.015***	103.362***
	(2.957)	(2.849)	(0.835)	(0.815)	(12.175)	(12.095)
N	96,133	96,133	96,133	96,133	19,346	19,346
R2	0.314	0.314	0.384	0.384	0.962	0.962

Note: Each column corresponds to one regression. Time-Varying controls are: enrollment, number of administrative staff, teachers appointed per classroom, students per teacher, students per sections, number of classrooms, number of blackboards, number of tables, number of student desks and dummies indicating the school counts with: principal, sub principal, administrative offices, teachers' lounge, workshop, library, other (no ICT) lab, gym, running water, sanitation, electricity. In columns 5 and 6 total enrollment was excluded as a control variable. Standard errors are clustered at the school level. * 10%; ** 5%; *** 1%.

Next, the existence of heterogeneous effects is explored by focusing on a number of different subpopulations defined by sex, grade, fraction of students overaged, internet access outside school and class size. Table 9 presents the results. Once more, the results indicate no impact of technology: in all but two regressions the coefficients are not statistically significant, and in those two cases the estimated coefficients are quite small. Note also that even in the absence of true effects, some rejections should be expected because 40 regressions are run.

Table 9: Fixed Effects Estimates of ICT Access - Heterogeneous Effects

	Female	Male	First and Second Grade	Third, Fourth and Fifth Grade	Higher Fraction of Overaged	Lower Fraction of Overaged	With Internet Boot	No Internet Booth	Large Sections	Small Sections
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Repetition Rate	-0.026	0.026	-0.001	-0.005	-0.170	-0.122	0.294**	-0.436**	-0.029	0.019
	(0.144)	(0.131)	(0.131)	(0.121)	(0.250)	(0.175)	(0.146)	(0.171)	(0.232)	(0.102)
Drop Out Rate	-0.079*	-0.049	-0.089*	-0.042	-0.118	0.003	-0.044	-0.095	-0.095	-0.003
	(0.046)	(0.045)	(0.049)	(0.041)	(0.111)	(0.053)	(0.049)	(0.061)	(0.059)	(0.050)
Enrollment in First Grade	1.335	-0.630	0.358	-	0.293	0.233	0.749	0.268	0.115	0.078
	(0.905)	(0.723)	(0.569)	-	(0.526)	(1.172)	(0.812)	(0.680)	(1.286)	(0.184)

Note: Each cell corresponds to one regression. In all cases time-varying controls are included. Time-Varying controls are: enrollment, number of administrative staff, teachers appointed per classroom, students per teacher, students per sections, number of classrooms, number of blackboards, number of tables, number of student desks and dummies indicating the school counts with: principal, sub principal, administrative offices, teachers' lounge, workshop, library, other (no ICT) lab, gym, running water, sanitation, electricity. In regressions were the dependent variable is enrollment in first grade, total enrollment was excluded as a control variable. Standard errors are clustered at the school level. * 10%; ** 5%; *** 1%.

In results not reported here we test whether the introduction of technology gains is associated with improvements in outcomes when restricting to the sample of private urban schools. Again, we find no evidence of impact of ICT access on educational outcomes. However, in this case, we are able to detect even smaller impacts of a one unit increase in SIPA since the larger variation of this variable in this subsample leads to substantially smaller standard errors. Similarly, we find evidence of no impact when focusing on the sample of public rural schools.

Robustness

Several pieces of evidence point towards the robustness of the results. First, similar results are obtained when different subsets of controls are added. Second, there are low correlations between trends in outcomes in the early period (2001 to 2003) and trends in ICT in the final period (2004 to 2006). This suggests that schools with faster ICT introduction did not have a secular different baseline trajectory in outcomes.

Lastly, we consider whether the extent of measurement error in the variable of interest, which leads to attenuation bias, may be the source of the lack of identification of impacts. Simple checks performed in the data, as well as reports from public officials from the Ministry of Education, point towards some level of measurement error in the variable reporting the number of computers available. Following Swaffield (2001), we tackled this issue by averaging school observations across years and running regressions on the resulting data set. If errors are not serially correlated, this approach will reduce the bias generated by measurement error. In additional specifications, we average school observations across larger geographical units. In all cases the findings remain unaltered.[9]

CONCLUSION

This paper empirically addresses the policy-relevant question of whether increases in ICT access can induce improvements in completed years of education. Though a number of studies have analyzed the effects of increasing ICT access on test scores, which should have an effect on repetition rates, this is the first paper to estimates impacts on drop-out rates and initial enrollment. A rich longitudinal data set containing information on virtually all secondary schools in Peru for the period 2001-2006 is used, together with information regarding a particular program implemented in 2004 which deployed significant ICT resources in around 350 schools. The empirical approach first analyzed the impact of the mentioned program and found that there is no evidence of impacts on the outcomes under consideration. Motivated by the goal of providing more precise estimates of treatment effects, in the second

[9] Results are available from the authors upon request.

part of the paper longitudinal variation in ICT access was exploited. Again, the results suggest null impacts, although in this case we can test for the existence of small impacts.

The lack of impacts found on repetition rates are consistent with results from Bet et al. (2010) who document that increases in ICT access in public secondary schools in Peru translate into higher usage to teach ICT skills but have no impact on the time used for Math and Language. Moreover, Bet et al. (2010) find no impacts on tests scores in Math and Language but substantial positive impacts on ICT skills. However, as it is documented that total use increases substantially with higher ICT access, it does not seem that the lack of impacts on drop-out rates and enrollment can be attributed to the inability of schools to utilize the additional resources. These findings give no support to the hypothesis that the introduction of computers in schools could increase learning indirectly via increases in attendance. Moreover, it is commonly argued that computers increase students' motivation (InfoDev, 2005). In light of the results presented, the real consequences of the potential increase in motivation may be limited.

The results presented, coupled with those from the rest of the literature, suggest some tentative policy implications. First, it seems that the ability of ICT to improve coverage and quality of education in subjects such as Math and Language is limited. 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 may be optimal. Second, expansions beyond the referred basic level may 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 may produce positive outcomes. Since successful models of use have not yet been clearly identified, experimentation and evaluation of particular arrangements may turn the promise of the revolution of ICT in education into reality.

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