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The Impacts of Introducing Computers in Schools in Developing Countries: Evidence from Peru

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Abstract

In education circles a lively debate has ensued regarding the effectiveness of ICT to enhance the learning process. This paper contributes to the limited quantitative evidence regarding this issue in developing countries by analyzing the impacts of increasing ICT access in secondary schools in Peru. We exploit rich administrative censal data between 2001 and 2006 as well as institutional knowledge regarding the execution of a specific program that deployed computers in 350 schools. Results indicate null impacts of increasing ICT access on repetition, drop out rates and initial enrollment. The large sample sizes allow ruling out even very modest effects. These findings can be explained by the inherent difficulties in successfully implementing ICT interventions due to the critical nature of several different inputs (hardware, software, connectivity, training and support).

I. Introduction

There is widespread consensus on the critical role that education plays in achieving sustained improvements in welfare for developing countries. Fueled by this view, important efforts have been exerted to generate improvements in coverage as well as in quality of education. In primary education, because many developing countries have almost attained universal coverage, the current emphasis lies in how to improve quality (see Duflo, 2009). The picture for secondary schools is different. Coverage has increased markedly (net enrollment ratios have increased from 46 percent in 1999 to 53 percent in 2005) but there is a long way ahead. In terms of quality, significant improvements are still needed. For example, in TIMSS 2003, 20 to 90 percent of grade 8 students in low- and middle- income countries did not attain the lowest benchmark level (UNESCO, 2005). 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. The lack of knowledge regarding effectiveness of alternative policy options can produce significant funding misallocations which, given the nature of the process of production of human capital, can have large and long-lasting consequences. Consequently, information on the ability, and also the inability, of different strategies can be highly valuable for education policy makers in the developing world.

One specific intervention has been highlighted as having the potential of achieving the twin objectives of improving both quality and coverage: the introduction of ICT in schools. Regarding quality, ICT has the potential to revolutionize the way that the learning process is conducted by providing a stimulating, richer environment for students. The quality of education, i.e. how much students learn while they are in schools, can be raised because of two factors. First, ICT can improve the productivity of education at constant levels of students' effort given its ability of providing better tailored content and education materials to students. For example, Computer Assisted Instructional (CAI) programs have the potential of diagnosing students' competences, identifying weaker areas and, focusing lessons on these particular areas in which there is greater room for learning. Second, in a widespread view shared by educational researchers, policy makers and other relevant actors, the potential of ICT lies in the fact that it can boost motivation among students and, consequently, it can directly increase a key factor in the learning process: students' effort.

The mentioned attractiveness of ICT to students makes it plausible to think that the introduction of technology to schools can also increase coverage by raising enrollment to secondary school and reducing drop-out. The value that students give to having computers at school is in many cases shared by parents and even directors. There is widespread qualitative evidence about cases in which significant efforts are put forward by parents and school administrators to purchase and install computers in schools, efforts that are not so common for other types of educational inputs. Therefore, the hypothesis that arises is that schools can act as "magnets" drawing (or in many cases retaining) students and, in this case, contributing to the continued accumulation of human capital. This view is also highlighted by Banerjee et al. (2005) as they argue that "Computers have the potential to both directly improve learning and indirectly increase attendance by making school more attractive".

Somewhat surprisingly, the current quantitative literature regarding the impact of ICT on educational outcomes has disproportionally focused on the question about whether the introduction of technology can enhance learning. From our quite thorough review of the literature, we have identified a number of studies that adopt a quantitative approach to explore the issue about the effectiveness of technology in the classroom (see Table 1 for a list of those studies). From the 23 studies identified, none tests whether the introduction of technology has an effect on enrollment to secondary school or drop-out rates at this level. This gap in the literature is even more surprising considering that the attractiveness of ICT as a tool to improve education has fueled an amazingly vast qualitative literature in education aimed at theoretically analyzing how, why and in which contexts ICT can be effective.[1]

This paper aims to contribute to the candid debate regarding the effectiveness of ICT in education in developing countries, which ultimately can only be settled empirically, by providing important evidence regarding the ability of ICT of inducing improvements in secondary school coverage via increases in initial enrollment and drop out rates. Also, it aims to provide additional evidence to the limited literature on the impact of ICT on learning in the context of developing countries by analyzing impacts of increased technology access on repetition rates. To that end, the paper exploits a very rich data set from Peru which contains longitudinal information for virtually all secondary schools for the period 2001 to 2006 on the mentioned outcomes and a host of educational inputs.

The focus of the paper on factors that ultimately determine years of completed education instead of test scores seems warranted for several reasons. First, as noted, in developing countries improving secondary school coverage is an important objective. Second, in many of these countries high repetition rates are pervasive and generate a host of problems related to heterogeneity in classrooms and further pressure to drop-out among those lagging behind. Third, the strong evidence on the widespread benefits of education is virtually in all cases derived from studies linking years of completed education (as

[1] This vacuum in the literature can be rationalized considering the very small share of the studies that adopt a quantitative approach (only 23 identified from a very large literature) and the fact that the literature is tilted towards analyzing interventions in developed countries.

opposed to test scores) to relevant outcomes. Thus, to assess the impacts of introducing technology in classrooms on welfare outcomes (e.g. earnings), it seems more plausible to extrapolate from studies that estimate the impact of the introduction of ICT on years of completed education rather on test scores. Fourth, measured impacts on test scores are not directly and easily interpretable given a host of issues regarding the way exams are designed and implemented (e.g. dependency of results on the set of skills evaluated). On the contrary, years of completed education have a specific meaning allowing easier interpretation of the results.

To answer the question posed regarding the impact of ICT access intensity (measured in terms of potential weekly hours of use per student) on secondary school enrollment at first grade, drop out and repetition rates, we execute two different analyses. First, we exploit the plausibly exogenous increase in the number of computers per student produced by a program, funded by the Inter-American Development Bank (IADB), which distributed 10 computers in 350 large public secondary and primarily urban schools in 2004. Information obtained from Ministry of Education former staff involved in the program implementation was used to identify a plausible comparison group. It included schools that received earlier hardware deployments from the previous government, and that was deemed ineligible to the mentioned intervention.

We provide several of evidence suggesting the ability of the empirical strategy of providing consistent estimates of the treatment effects. First, the treated and comparison groups are quite similar in observable dimensions to start with and more so after applying propensity score reweighting. Second, the program produced a significant increase in ICT access to the treated group concomitant to almost no change in the comparison group. Third, trends in other observed educational inputs were quite flat and similar across the two groups. Finally, estimated impacts were quite robust to the addition of different set of controls.

Results indicate no statistically significant impact of the increase in ICT access intensity on the outcomes considered. Even though the sample size in terms of students-year available is large, under this empirical strategy we are not able to rule out impacts that are economically significant. For example, we can only rule out that by 2006 the program did not decrease average drop out and repetition, from their mean levels, more than 30 and 10 percent, respectively.

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 by estimating fixed-effects models. As before, the estimated impacts point towards the inability of ICT access to reduce repetition and drop out rates as well as increasing enrollment at the entry level. However, in this case we are able to rule out very small impacts. In the case of the impact of repetition rates we can rule out impacts larger than 0.2 percentage points or roughly 2 percent of the baseline rates of increases in one hour of ICT access per week. Similarly, we can rule out impacts larger than three percent in the case of the drop out rate and larger than one percent for enrollment in first grade (in terms of baseline rates). We proceed to perform a variety of specification checks, trying different modeling assumptions regarding the variable of interest but overall we arrive to the same qualitative conclusions. Finally, we perform a number of exercises to check the robustness of the empirical strategy and the evidence supports the methodology followed (e.g. trends outcomes in initial years do not predict trends in ICT access in later years).

The results found, in general, match other studies that have found limited measurable impacts of ICT in education outcomes. In many instances these failures have been explained in terms of poor implementation of the interventions which led to important deficits in the actual use of ICT in the learning process. This type of interventions requires precise coordination of very different inputs (hardware, software, connectivity, training, support). Failure in providing any particular input generates the almost complete failure of the whole intervention. Not only the provision of every input is needed but also the timing and sequence of the deployments is critical. There are many real-world examples where the ideal sequence was not followed (e.g. training teachers was provided before the computers arrived) that generated important logistic problems and also widespread skepticism about the overall initiative.

An analogy can be drawn from interventions in ICT in education and the idea behind the O-ring theory propounded by Kremer (1993). This theory mainly aims to explain why general productivity is so different between developed and developing countries and argues that this can be explained by a production function characterized by a high degree of complementarities across inputs. This complementarity generates that total output decreases abruptly when only one of the involved inputs is missing. This is analogous to the situation of interventions in ICT. Acknowledging this intrinsic characteristic of this type of interventions is important for two reasons. First, as previously noted, careful and detail planning and monitoring is critical. Second, when evaluating the ex-ante benefits of an ICT intervention, the expected probability of not managing the highly synchronized implementation needed (and its consequences) has to be taking into account. This is particularly relevant in public educational systems with limited capacity in planning, executing and monitoring wide-scale reforms.

II. Background

II.1. 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, 2008. In terms of GDP per capita, is fares slightly better than the average middle income country (6,800 versus 5,400 2005 dollars in 2006). Its education indicators are slightly better compared to the average middle income country though it faces similar challenges regarding secondary education: increasing coverage, ensuring adequate students' progression and improving the quality of education.

School enrollment is almost universal in primary education though lower in secondary school. The net secondary enrollment rate is 72 percent due to the combined effect of quite high repetition rates and non trivial drop out rates. Moreover, coverage rates are significantly higher in urban compared to rural areas. There have been significant improvements in literacy rates throughout the last decades, but there is room for improvement: 11% of Peruvians do not know how to read and write (5.7% and 16.3% for males and females, respectively).

National and international examinations have repeatedly shown that the education system has been unable to ensure that students achieve high levels of learning. As an example, Peru was ranked last in reading comprehension among 41 countries participating in the Program for International Student Assessment (PISA) in 2000, though mainly developed countries participated (OECD, 2003). Fifteen year old students were evaluated. In this exam, students who do not achieve the benchmark level are considered as not being able to identify the main idea or specific information in a simple passage. Sadly, 54% of Peruvian students fell into this category. This performance compares poorly with a corresponding figure of 6 percent for developed countries but also against other participating Latin American countries (20, 23, 23 and 26 for Chile, Argentina, Brazil and Mexico, respectively).

Though the specific determinants of the low achievement levels are not well understood, certain potential explanatory factors have been identified. To start with, the amount of resources devoted to education are significantly lower in Peru compared to the group of middle income countries (2.8 versus 4.4 percent of GDP, respectively, in 2004). This comparison is even starker when focusing expenditure per student as a percent of GDP per capita (9.1 against 17.6 percent in 2004). Another suspected factor is the high proportion of the budget devoted to personnel (80 percent). Finally, policies regarding how teachers are selected, compensated, induced to provide good results and trained has also been noted as potential causes for low achievement.

II.2. Introduction of ICT in Education in Peru[2]

Until 1996 ICT played a small role as a tool to improve public education in Peru. Starting that year, 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 some investments by part of the participating schools to in order to be included in the program. These investments made by schools were typically funded by parents, private donations or other sources of funding (not public). This understandable requirement, given the low budgets of the ICT programs, promoted ownership and sustainability of the investments but at the expense of poor targeting (typically large public urban schools in more affluent areas received the ICT resources). In these cases, computers were mainly used for acquiring ICT skills (creating

[2] This subsection draws heavily from interviews with former and current government officials and government reports. The information was not rigorously cross-referenced with other sources. However, impressions from visits to a small number of schools in the country capital, Lima, were consistent with the information received.

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. It received prominence and a substantive slice of the budget. Its main objective was to increase coverage and quality in the educational sector by applying ICT in the learning process. The program mainly targeted secondary schools though some primary schools were also covered. To be eligible for the program the school needed to have: a) at least 5 computers (in certain cases the program provided some), b) a classroom that was secure enough to prevent thefts, c) an operating network. These eligibility requirements tilted the coverage of the program towards urban schools (besides the intended emphasis on secondary education). Table 2 shows statistics about coverage by school groups and types of inputs received.

The guiding principle behind the program was that ICT could be a significant tool to achieve the stated objectives and that special effort should be exerted to ensure the proper use of computers in the school. At the heart of the program was the “innovation classroom”. This was a computer lab meant to be used exhaustively by teachers in all subject areas (though in the beginning it was only used for Math and Science).

Aiming to ensure intensive access of the innovation classroom, the program stressed support, training and software over the provision of hardware and connectivity. This revealed the notion from the program designers that the first three mentioned inputs were key and also the idea that in many cases schools would generate the resources to purchase computers but not these other more “intangible” (but also critical) inputs. In fact, all schools that were included in the program received a paid innovation room coordinator, teacher training and certain software. Around 75 percent of schools received computers (on average 6 per school) and a similar proportion obtained internet connection (mostly DSL). Though it was not considered a necessary condition for a school to have internet connection, still an important effort was exerted to achieve significant increases in connectivity across schools.

The innovation room coordinator was seen as a key resource because he/she was in charge of ensuring the intensive and effective use of the innovation room and provided support towards integrating effectively ICT in the learning purposes. This person was selected based on first, a pedagogical background and second, knowledge and experience in using ICT.

Training was quite intensive and done sequentially. First, innovation room coordinators were trained in two different modules in four sites across the country. The first module was aimed at teaching how to use the computer in general and stressed the MS Office package and certain free educational software. This module lasted six days and the classes were given for nine hours a day. The second module was designed to teach how to integrate ICT in lessons and trained the coordinators in more specific educational software. The training included practical lessons where the coordinators had to design a complete class making use of the software available. Both modules were given by two facilitators: one with a pedagogical background and the second with an ICT technical background. The training to the actual teachers follow the same schedule described above though the classes were provided by the innovation room coordinator in their respective schools.

The software provided to participating schools was MS Office for each computer and a kit of CDs that contained educational videos as well as a number of multimedia material developed by specialists in the Ministry of Education and local firms contracted for this end. The multimedia applications covered specific topics in a subject and stressed multimedia aspects above all. There was almost no interactive software of the type that diagnoses the students' abilities and tailors the lessons based on that information. This seems a weakness of the program given that the capacity of ICT of tailoring material to students needs was not exploited. The emphasis was on using the multimedia aspects of the material (images, sound and movement) as a way to attract the student attention for learning purposes.

In terms of connectivity, governmental documents report that the program provided internet access to around a third of all secondary urban schools in the country and a fifth of the secondary rural schools. This expansion was important as schools that had internet connection seemed to have used this resource heavily. In terms of technical support, the program did not seem to have stressed this area. This is consistent with the fact that the program did not emphasize the delivery of hardware to schools.

Maybe the most revolutionary aspect of the program was that it was absolutely not allowed for the schools to use the innovation room to teach ICT skills (e.g. MS Office) except in very specific and restrictive cases. Allowing this type of (natural) use was considered a big risk: teachers and innovation room coordinators could fall in the temptation of using the computers to teach ICT skills and would not use them for the desired objective of enhancing the learning process in traditional subjects.

Regarding the use of the computers, from interviews and a small number of visits to schools the sense that emerges is that the innovation rooms seem to have been used a significant fraction of the available time, although clearly not all the time. However, the type of use stressed activities directed by teachers, little interactive use (consistent with the almost absence of CAI software) and heavily use of the internet, when available. Typically teachers would direct students to search for information about a particular topic, retrieve information and summarize it in a word document or power point presentation. In some cases, the computer seemed to have been used just to show previously designed power point presentations with little room for creativity and exploration on part of the students. However, all the limited (qualitative) evidence seems to point towards the idea that students enjoy greatly having access to computers and give some potential support to the view of computers as "magnets" to schools. However, it seems unlikely that the kind of use observed could produce significant improvements on students' skills beyond some marginal gains in reading abilities, familiarity with computers and acquisition of certain elements of general knowledge.

II.3. The IADB Program

In 1999 the Inter-American approved a loan for Peru aimed at improving the quality and coverage of secondary education. As part of this loan, 350 secondary schools were selected to receive an ICT package. Due to a number of administrative and logistical reasons, the actual deployment of the inputs suffered substantial delays and it finally took place between March and June of 2004.[3] This deployment can be seen as another action line within the Huascaran program aimed at improving the hardware stock of schools. The package funded included the lay-out of electrical infrastructure needed, 10 computers and the installation of a network. These schools entered the Huascaran program and, hence, they received an innovation room coordinator, training and the standard software. Moreover, the provision of internet access to these schools was prioritized.

Regarding the procedure employed to select the 350 schools into the program, from interviews with former government officials it seems that there were some criteria developed thought it was applied in an ad hoc manner. Still, eligible schools had to be

[3] Additionally, 52 schools received some equipment funded by the program due to the existence of remaining funds. However, these schools did not receive the complete package as compared with the original 350.

public and they should not have been covered by previous governmental programs (data checks showed that both requirements were fulfilled in all cases). From the eligible schools, three factors were considered to select the final set of schools: a) high enrollment, b) commitment by directors, teachers and parents to support and sustain the initiative, c) easy access to schools. Still, other considerations could have played a role in the final decisions.

III. Data

The data used in the study is compiled by the Ministry of Education from yearly surveys completed by secondary schools in the country. All private and public schools are required to answer these surveys though typically around 2 to 3 percent of schools do not submit the completed questionnaire in each year. The collected information is used to generate reports for planning and monitoring purposes. One questionnaire is answered by each school covering aspects such as enrollment and other dimensions related to the administration of educational inputs and results obtained. A different questionnaire covers aspects related to physical infrastructure, furniture, access to basic services (water, sanitation, electricity) and is completed per building (in rare instances two schools operate in the same building, typically at different shifts). From Ministry of Education sources as well as by executing basic data checks we learned that the quality of the information seems quite high for the school questionnaire thought not so high for the building questionnaire. Thus, as much as possible, data from the school questionnaire was used. In particular, data about ICT resources was typically available from both questionnaires and we prioritized the use of the school questionnaire.

Information available included: a) location, private/public type, year of creation, shifts; b) enrollment (per grade, gender and overage status), number of sections per grade;

c) administrative staff; d) teachers (number, gender, qualifications, ICT teachers); e) repetition and dropout rates; f) physical infrastructure, furniture available and number of textbooks; g) number of computers, in operation, for administration, network connection, internet access and existence of a computer lab.

We accessed data for years 2001 to 2007. Information on repetition and drop out rates was not available for years 2002 (some problems were faced in the collection process that year). Additionally, these variables are not available for 2007 as schools report them for the previous year (e.g. in June 2007 they report number of students that drop out in 2006). Consequently, we focus the empirical work on years 2001, 2003, 2004, 2005 and 2006.

We construct a panel data where the unit of observation is a school-year. We add administrative information relative to the participation of schools in different programs related to ICT to this data set. The school-year specification of the data is used to present summary statistics. However, for the regression analysis we reshape the data set and generate school-year-grade-sex observations to allow more flexibility in controlling with composition changes and heterogeneous effects across grades and sexes.

Table 3 presents summary statistics. The first column presents summary statistics in 2001 for the subset of schools that responded the surveys in all years used in the analysis (2001, 2003 to 2006). The third column shows corresponding statistics from 2006 data. 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 restrict our attention to the 7,319 schools that provided information in all years used in the analysis (denoted the Main Sample). Along the paper we calculate all statistics and estimates weighting school observations by the number of enrolled students. This assures that means of variables aggregated at the school level gives the same statistics as those directly constructed from student level data.

From this table we can see that imposing this restriction in general does not significantly affect the composition and representativeness of the Main Sample. As an exception, 17 percent of schools in the main sample are private as opposed to 20 percent of private schools from those answering the survey in 2006, suggesting that there recently have been a faster introduction of private schools.

In the top panel we observe that repetition rates are high though they have decreased around 10 percent in the period analyzed. However, the drop out rate remains virtually unchanged in the period. Note that the drop out rate represents the fraction of students enrolled in a particular year that leave the school before the end of the year. Strictly speaking it is the within-year drop out rates because it does not capture those students that finish a particular grade but fail to enroll in the following year. In this sense, the rate underreports the true rate but in some way it is better suited to answer the question at hand as individuals dropping out from a school within the year are more prone to correspond to real drop-outs whereas those failing to register for the following year will include at higher extent individuals transferring across schools.

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, with a computer lab from 39 to 76 percent and with internet access more than three-fold, from 16 to 55 percent. Similarly, the total number of computers doubles in the period under analysis.

We also present information for the variable *Students ICT Potential Access* (SIPA). This variable is the central variable of interest along 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 number of hours per week that a student would use computers if: a) all students have the same access to computers for learning, b) computers are used continuously, c) each computer is shared between two students at each point in time, d) students spend 25 hours per week at school. As its name indicates, it embodies the potential ICT access that students can have. This variable has a number of advantages over the typical variable computers-student ratio used widely for descriptive purposes. In particular, it is defined even 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.

The third panel presents a number of indicators related to school characteristics and availability of certain services in the school. These indicators present a much flatter trajectory and in general not substantial changes are observed. A number of summary school quality indicators are presented in the lower panel. The first indicator shows the number of classrooms in the group per 100 students. Similar indicators are derived for number of sections, teachers, administrative staff and blackboards. Higher numbers indicate higher availability of educational resources. In general these indicators are quite stable during the period under analysis.

Table 4 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. Note first that only 1 percent of schools are private rural. Hence, through the paper we do not focus our attention in this selected subgroup of schools but rather in the other three subgroups: public urban, private urban and public rural. Second, outcomes and characteristics vary widely between schools in the three groups mentioned. In particular, outcomes are significantly better in private urban schools in terms of both repetition and drop out rates compared with the other two groups. On the other hand, drop out rates are significantly lower in public urban schools compared with public rural but repetition rates are higher in the first mentioned group.

In terms of technology access there are very large differences between the three groups which could be ranked in this dimension as follows: first private urban, second public urban and third public rural. The differences across groups are extremely large. For example, in terms of SIPA, students in public rural schools have an average potential access of 0.3 hours per week compared to 0.8 for students in public urban facilities and 4.3 hours per week in private urban schools.

These differences are also mirrored in other educational inputs though they are not as stark. For example, 98 percent of private urban schools have an administrative office compared to 92 percent in public urban and only 73 percent in public rural. In other indicators, the two groups of urban schools present similar indicators though much better than those in rural schools (e.g. fraction of schools with electricity).

These patterns suggest substantial heterogeneity across schools in these mentioned groups especially in terms of technology access but also in other educational indicators. Given this, along the paper we proceed to execute separate analysis by the three groups in order to avoid comparing schools with high ICT access (typically private urban schools) with those with low ICT access (public rural) which will differ markedly in many other observable and unobservable dimensions. Moreover, in the paper we will focus the analysis primarily on public urban schools for three main reasons. First, this group accounts for the lion's share of students in the educational system. Second, the group corresponds to the main population targeted by the program analyzed in the next section. Third, from a public policy perspective, students in the public system are the ones that could be directly affected by educational policies. Among this group, which encompasses urban and rural students, it seems natural to focus on urban schools given that ICT penetration among public rural schools is so low that it seems difficult to make valid comparisons.

IV. Impacts of the IADB Program

IV.1. Empirical Strategy

In this section we estimate the impact of the IADB funded program. For reasons already discussed, we focus on the sample of schools that are public and urban. The identification strategy that we follow is to identify a suitable comparison group, apply propensity score reweighting to deal with differences in observed covariates and finally estimating fixed-effects models using the longitudinal data. To select the comparison group, we exploit the rich data that we have together with the institutional information available regarding the criteria followed to select the 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 have 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 selection of schools as beneficiaries of computers deployment between 2001 and 2006. From the analysis, several patterns become apparent. First, the main distribution of computers took place in 2004 (the IDB-funded intervention) thought there were some distribution before and after that year. Second, schools in almost all cases only received computers once in this period. Third, an important fraction of schools beneficiary 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 5 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. Regarding the first objective guiding the selection of the comparison group (that is, similarity in pre-treatment variables) the pre-Huascaran beneficiaries and the early Huascaran beneficiaries (Columns 2 and 3, respectively) fare better than the other two potential groups. In terms of school size, early Huascaran beneficiaries are more similar to the treatment group compared to the pre-Huascaran beneficiaries. However, in a number of important covariates such as fraction of overage students or percent of schools with library, the pre-Huascaran group seems more similar to the treatment group. However, in terms of the second objective (a flatter evolution of ICT intensity access), the pre-Huascaran beneficiaries clearly dominated all other groups (SIPA increases only 0.2 hours a week for this group compared to increase of around 0.5 for the other groups). Hence, the pre-Huascaran group of schools is selected as the comparison group for the analysis.

To increase the similarity of the treatment group and the selected comparison group we apply propensity score reweighting techniques. First, we run predict treatment using a logit regression including 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 reweight the comparison group by applying a weight of

1/(1-*ps*), where *ps* refers to the propensity score.[4] The effects of applying these steps can be observed in Table 6. After trimming and reweighting the sample, both groups seem remarkably similar in a number of important covariates.

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

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

where Y corresponds to the outcome variable, T indicates whether the school was treated, $Year_{04}$ 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. Note that in all regressions, standard errors are clustered at the school level and observations are weighted by enrollment.

IV.2. Results

Table 6 presents the main results of the impact of the IADB program. Separate effects are estimated for years 2004, 2005 and 2006. Note that the computers were distributed and installed in the first semester of 2004, hence results for that year correspond to the impacts of around 6 months of intervention. Results for years 2005 and 2006 can be interpreted to the results at 1.5 and 2.5 years after intervention.

Column 1 presents the estimated effects of the program on repetition rates in 2004, 2005 and 2006. The dependent variable was multiplied by 100 and, consequently, the

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

impacts can be interpreted in terms of percentage points. Estimated impacts during 2004 and 2005 are quite close to 0 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. In Column 2 we present results for a similar specification but adding a large number of time-varying controls. The results are remarkably consistent suggesting small room for a role of unobservables in biasing the estimates. Columns 3 to 6 show that the introduction of computers generated by the program is not associated with statistically significant changes in the drop out rate and enrollment in first grade (in the following year).

Figure 1 reinforces these conclusions by plotting the evolution over time of the mean of the outcome variables separately for the treatment and control groups. In general the graphs show quite similar trends for both groups suggesting the absence of large impacts of the program on the analyzed outcomes.

IV.3. Robustness

In this subsection, we provide some evidence to gauge the plausibility of the empirical strategy followed. To start with, we analyze the evolution of the SIPA variable in the treatment and comparison groups. The larger the impact of the program on SIPA the more plausible that the observed changes in the treatment group (relative to the comparison group) can be attributed to the program. This is the idea motivating many quasi-experimental designs that focus on situations in which there are sharp breaks in a particular variable of interest with expected modest changes in other variables.

Figure 2 shows that in the treatment group SIPA increased substantially from 0.38 to 0.78 between 2003 and 2004 and continued increasing afterwards whereas for the comparison group a subtle increase was experienced. Hence, the substantial higher ICT intensity in the comparison group was almost completely wiped out by 2006.[5]

We also check whether there were significant increases in other educational inputs concomitant with the introduction of the program. Figure 3 presents the results. From the figures we note that trends in these inputs are quite similar across the two groups giving confidence to attribute changes in outcomes to the program (and the absence of changes to null impacts of the program).

V. Evidence from Longitudinal Variation in ICT Access

The identification strategy executed in the previous section may be seen as quite successful in consistently estimating the causal impacts of the program on the analyzed outcomes, given the results from the robustness checks. However, the estimates are not very precise. In fact, focusing on the impacts on repetition rates in 2006, we can only rule out at the 5 per cent significance level impact larger than 30 percent of the baseline rate. Given that to start with we do not expect that ICT would have a very large impact on this outcome, the results end up being not that informative in terms of affecting our a-prior expectations. Though for other outcomes we can rule out smaller impacts, still a case can be made stating that we are not able to identify smaller but still significant impacts.

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.

[5] To increase the relative impact of the program on SIPA we could have restricted to smaller schools in the pre-treatment period because all schools received the same number of computers and hence the impact on computer access per student was larger in smaller schools. However, given that precision was an important issue in the design of the empirical strategy and restricting the sample would have reduced the estimates precision we opt not to follow this empirical avenue.

The resulting estimates are far more precise than those from the previous section though the potential for bias may be greater.

V.1. Empirical Strategy

As in the previous section, we use the data in which the unit of observation is defined at the school, year, grade and sex level. Also, for reasons already discussed, we primarily focus on schools in the public urban sample. Two different specifications are used. In the first one, the impact of SIPA on the outcomes is modeled linearly as follows:

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

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 defined based on cut-offs at 1, 2 and 3 hours 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 within units over time. The use of these types of models is customarily motivated by stating that adding these indicators allows for controlling time-invariant differences across units. Though this is true, the addition of these dummies effectively wipes out two important sources of variation in the variable of interest (across units and over time). Consequently, if remaining bias exist after controlling for school and year effects (which it is difficult to argue against that possibility), reducing the variation substantially in the variable of interest may increase the extent of bias.[6] However, given that ICT penetrated strongly during the period, it is reasonable to think that this is a suitable situation to use fixed-effects models given that

[6] In the case of IV models, this possibility is widely recognized and in fact the term “large sample bias” is used to refer for cases when some correlation between the instrument and the error term is exacerbated by the low correlation between the instrument and the endogenous variable (Bound et al., 1995).

controlling for school and year effects only absorb a limited amount of variation in the variable of interest.

V.2. Results

Table 8 presents estimates of the impact of SIPA on the analyzed outcomes. Odd columns present results when the effect of SIPA is modeled linearly whereas in specifications in even columns SIPA entered in four categories as described previously. The results suggest that greater access to technology has no impact on the educational outcomes analyzed. However, because of the much larger sample sizes used, now the estimates are very precise and even modest impacts can be ruled out. Focusing on Column 1, we observe that an increase of SIPA in one hour per week is associated with a reduction of 0.006 percentage points in the repetition rate. Importantly, the standard error is very small and this implies that impacts larger than 0.2 percentage points are ruled out at the five percent significance level. Similarly, it is possible to rule out decreases in the drop out rate and the failing in December rate of 0.1 percentage points and an increase in more than 1.5 students in first grade. Focusing on the specifications in which SIPA enters categorically, yields similar qualitative results: no impact of technology access on the referred outcomes.

Table 9 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 and in even specifications SIPA lagged twice is also included. Again, we arrive to the same qualitative results of no impact of the introduction of technology on the outcomes. As an exception, for drop out rates the estimated coefficients for SIPA, SIPA lagged and SIPA lagged twice are negative and statistically significant from zero. However, the coefficient magnitudes are really small, and hence, the general conclusions remained unaltered.

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 10 presents the results. Again, 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.

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. Once more, we find no evidence of impact of ICT access on educational outcomes. However, in this case, we are able to rule out even smaller impacts of a one unit increase in SIPA given that the larger variation in 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 urban schools.

V.3. 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 results found. Simple data checks performed in the data, as well as reports from public officials from the Ministry of Education point towards not negligible amounts of measurement error in the variable number of computers available. Errors may arise mainly to two factors. First, this information is typically reported based on casual observations rather than from school records. Second, there are no procedures in place to check the accuracy of the reports of the school in this type of variable given that it is not used for general planning or informative purposes (unlike other variables such as enrollment). Because fixed effects models are used, the particular type of measurement error of concern is one that it is not fixed over time but rather it arises due to an idiosyncratic error. Substantive empirical checks are performed to explore this issue and in all cases the conclusions remain unaltered (see Annex 1 for a description of the checks performed).

VI. Discussion

The empirical analysis uncovers consistent evidence suggesting very modest impacts of ICT access on secondary school initial enrollment, drop out and repetition rates in the setting analyzed. To generalize these results to other contexts in developing countries is important to understand how computers were typically used in the country. Combining limited quantitative and some qualitative data drawn from visits to public urban schools and interviews to relevant actors the picture that emerges is that computers in this setting typically has been used to access information in the web, summarize this information and preparation of reports and group presentations. That is, the use has stressed using ICT to draw new and relevant content and to learn, due to its mere use, skills related to search of information, combination and presentation. On the contrary, the use of sophisticated CAI software appears to have been very limited. Interestingly, the qualitative evidence suggests that the national policy mandating the use of ICT resources for learning traditional subjects as opposed to the acquisition of ICT skills seems to have been achieved.

How can we link this type of use with the estimated impacts found? First, in terms of the ability of ICT to reduce repetition rates, this type of use does not seem particularly conducive to improve students' performance in subject tests (which are the main input used in decide whether students have attained the standards applied by teachers). In Peru, the decisions of progressing a student to the next grade or make him repeat the grade is largely based on students' performances in Math and Spanish. In fact, evidence from standardized testing has repeatedly shown the challenges faced by student to attain minimal levels in Math suggesting that this subject is a crucial hurdle to advance to the next grade. The type of ICT use promoted does not seem that could help students to master Math skills. However, it may have promoted some improvement of reading skills though limited impact on writing skills (students tend to over-use the copy-and-paste tools instead of summarizing with own words the information accessed). Still, some skills regarding search of web information and acquisition to general knowledge not specific to subject areas could have been impacted though it is difficult to measure this kind of improvements. Finally, it is important to put in context the fact that, as opposed in developed countries, in the case of Peru and other developing countries educational attainments deficits are quite acute in basic skills related to literacy and numeracy and consequently potential impacts on higher-order skills could be of second-order importance.

Regarding the impacts of ICT access on coverage, that is, on enrollment at entry level and drop out rates, in principle the type of use promoted seems to have been better suited, compared to the use of CAI software, to achieve impacts on these dimensions. That is, the stress on connectivity and relative freedom provided to students could have been conducive to improve the desire of students to choose, in the margin, attend school compared to other potential activities for their time. We emphasize that theoretically ICT could have some marginal impact on the decision of students to attend or not school. Similarly, for parents it could have had some marginal impact given the widespread view among them of the key role that mastering technology may have on future employment prospects for their children (specially in the urban settings analyzed). However, we do not identify impacts on these areas and, given the large sample sizes available, we can rule out even modest impacts. This raises doubts regarding the role that, in practice, increases in technology availability in schools may have on secondary education coverage.

Regarding the non-experimental empirical approach followed, a point can be made related to the ongoing discussion about the relative merits of experiments as compared to other non-experimental methods (see Deaton, 2008, Heckman and Urzua, 2008 and Imbens, 2009 for recent updates on this issue). The use of experiments are generally considered a very effective empirical method in situations where there is an absence of general equilibrium feedback effects and the treatment is a policy variable. However, an aspect of experiments that has not been stressed in the recent discussion is that experiments are not well suited in cases were the expected impacts are small. That is, though experiments are effective to tackle the ever present problem of bias, typically it is difficult to design and implement an experiment to detect small impacts. This limitation of experiments is clear among medical researchers trying to estimate the adverse effects of certain procedures (e.g. new drugs) given that the expected estimates are quite small. For the question posed in this paper, it seems very unlikely that an experiment at a large enough scale to detect impacts that are identified in this paper can be designed and implemented. To fix ideas, Barrera and Linden (2009) run a randomized evaluation of an ICT in education program in Colombia and use data on about 10,000 of students-year observations, which are several orders of magnitude smaller to the close 10 million student-year observations involved in our study.

Regarding the policy implications of our work, several points can be derived from the analysis. First, the evidence uncovered here points to a limited effectiveness of ICT to impact education quality and coverage, at least in the case of Peru and regarding the particular type of use proposed. However, the context and type of use seems to have been conducive to generate a potential positive impact on enrollment given that the type of use could be characterized as engaging for students as well as parents. Thus, evidence points against the, in principle theoretically plausible, view that the introduction of technology in schools can contribute to improving educational coverage.

Second, the results point towards the now common view that access to technology per se is not enough to obtain results; but significant effort and resources must to be devoted to ensure proper use. From this experience, it seems that the model followed, which can be characterized as relying heavily on connectivity, giving teachers the role of conduct classes in computer labs and very limited use of CAI programs, could be unsuccessful. It would be useful that future interventions stress a heavier use of CAI software providing more direct interaction between students and computer and maybe more limited role for teachers in computer labs, especially in light for quite positive impacts found in programs in India, reviewed by Banerjee et al. (2005). However, this model needs to be tested empirically as allowing students to interact freely with computers with CAI software does not preclude the possibility of students choosing to devote their time to low-education enhancing activities (e.g. chat, non-educational games). Finally, in contexts of limited access of ICT resources such as in the one studied, it seems reasonable to redirect the use of these resources to learn basic ICT skills that are both interesting for students and useful for job-related activities and a variety of other potential applications.

A final point can be raised regarding the role of increases in ICT access in public schools as a way to reduce the digital divide in society. As shown in Table 4, differences in ICT access across public and private schools in Peru are astounding: students in the latter schools have more than *five* times higher potential access to those in public schools. In principle, these findings suggest that the government should play a significant role to decrease these inequalities and invest resources to deploy significant ICT resources to public schools. However, ICT access has a "consumption" side that has to be taken into account. Private schools can have particular inputs at a much higher intensity level compared to public schools but this mere fact should not be the main motivation towards increasing these inputs in public schools. Instead, the public education system should continue to pursue the goal of achieving true learning of useful skills in the labor market and civic life. Hence, increases in certain educational inputs should only be warranted when there is knowledge, preferably based on empirical evidence, that this specific investment is cost-effective when compared to other alternative uses of public funds. Additionally, increased ICT resources can produce reductions in public-private educational input inequalities but can exacerbate the public-rural differences when resources are invested heavily in urban settings, as was the case in the program analyzed. Finally, increases in ICT resources can be unable to decrease these rural-urban inequalities in educational resources when additional inputs, like electricity, are necessary to make these investments and these necessary inputs are tilted toward more affluent segments of society.

VII. Conclusions

This paper empirically addresses the highly policy-relevant question of whether increases in ICT access can produce increases in completed years of education via analyzing impacts on repetition and drop out rates as well as entry-level enrollment in secondary schools in Peru. A rich longitudinal data set containing information on virtually all schools in the country in the period 2001 to 2006 is used together with information regarding a particular program implemented in 2004 which deployed significant ICT resources in around 350 mostly public urban schools. The empirical approach first analyzes the impact of the mentioned program and finds that there is no evidence of impacts on the outcomes analyzed. Motivated by the goal of providing more precise estimates of treatment effects, in the second part of the paper longitudinal variation in ICT access in public urban schools is exploited. Doing so, we effectively trade-off significant increases in precision with potential, thought unobservable, increase in bias. Again, we find no impacts of increased ICT access on the referred outcomes though in this case we are able to rule really modest effects.

Limitations of the study have to be taken into consideration when interpreting the evidence presented. In terms of internal validity, we cannot definitely rule out the possibility of certain types of bias contaminating the results. In particular, selection to the treatment group in the program analyzed or increases in ICT access over time are clearly not random events. Additionally, measurement error in the variable of interest could bias our results towards zero which is not a minor issue given that the results pointed towards null impacts. However, we try to deal with these issues in several ways. Regarding selection in unobservables, we implement empirical strategies that deal with time-invariant unobservable effects, i.e. fixed effects models, and consequently our results can be taken as significant more credible compared to cross-sectionals estimates. Additionally, we perform substantial robustness tests designed to gauge the existence of potential biases and we provide consistent evidence in favor of the empirical strategy followed. Finally, with respect to measurement error, we tackle this problem with a variety of alternative specifications (in general, providing estimates from aggregated-level data in the time or geographical dimensions). Again, we arrive at the same qualitative conclusions though these attempts come at a cost of reductions in precision.

Regarding external validity of the results, we stress that the contribution of this paper is to provide carefully obtained empirical estimates on a relevant policy question thereof almost unanswered. But by no means should it be expected that the results can be directly generalized to other settings. That is, it is completely plausible that in other contexts, in particular under different uses of ICT, qualitatively different conclusions can be reached. Thus, it will be valuable to tackle the question posed using data from other countries and exploiting other large intervention programs. These additional studies can significantly advance our understanding regarding the relative merits of investments in ICT towards achieving the twin goals of increasing coverage and quality in secondary education.

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Annex 1: Checks to Probe the Role of Measurement Error in the Results

A first approximation to this problem involves averaging observations across time. We re-run regressions presented in odd columns in Table 8 but constructing a panel data where the unit of observation is the school-period and the period refer to early (2001 to 2003) and late years (2004 to 2006). That is, only two observations per school. Results are reported in Column 2 of Table A.1. Second, we restrict the data set for years in 2001 to 2004, reshape the data set to school-years and re-run the fixed-effects models. The motivation for this exercise is that there was a change in the form used to report this information in 2005 and restricting to the mentioned time period ensures consistency in the way that the information was reported. Column 3 of Table A.1 shows these results. Additionally, Column 4 presents results when combining the two previous approaches: restricting to years 2001 to 2004 and constructing school-period observations (early period: 2001, late period: average 2003 and 2004). Finally, Columns 5 and 6 report results when averaging the data by district- and province-year level (there are around 1,800 districts and 150 provinces in Peru). Averaging the data in this geographical fashion allows dealing with idiosyncratic measurement error.

In all cases and for all outcomes we arrive to the same general conclusion of no evidence of impact of ICT on the analyzed outcomes (only in one case we find statistically significant results thought they point to ICT increasing drop out rates). These additional robustness checks, and the fact that we also find no evidence of impact when restricting to private schools which presumably should present a less pervasive extent of measurement error given the higher availability of administrative resources, point to the notion of very limited impact of technology on affecting the outcomes reviewed.

Table 1: Summary of the Impact Evaluation Literature on ICT in Education

Author	Country	Methodology*	Estimated Effect	N
Panel A: Impact on Test Scores				
Author	**Country**	**Methodology***	**Estimated Effect**	**N**
Banerjee et al. (2005)	Developing	Experimental	+	230 Schools
Linden et al. (2003)	Developing	Experimental	+	111 Schools
Barrera Osorio (2009)	Developing	Experimental	0	5201 Students
Linden (2008)	Developing	Experimental	+/-	60 Schools
Waxman et al. (2003)	Developing	Meta-Analysis	+	7000 Students
Fuchs and Woessmann (2005)	Developing	Non-Experimental	0	174227 Students
Pirog and Kioko (2006)	Developing	Non-Experimental	-	59396 Students
Barrow et al. (2008)	Developed	Experimental	+	15 Schools
Cepni et al. (2006)	Developed	Experimental	+/0	52 Students
Rouse et al.(2004)	Developed	Experimental	0	374 Students
Dynarski et al. (2007)	Developed	Experimental	0	132 Schools
Machin et al. (2006)	Developed	Quasi-Experimental	+/0	1400 Schools
Angrist and Lavy (2002)	Developed	Quasi-Experimental	0/-	4779 Students
Cengiz and Demirtas (2005)	Developed	Non-Experimental	+	259 Students
Terrance et al (1996)	Developed	Non-Experimental	+	6000 Schools
Goolsbee and Guryan (2002)	Developed	Non-Experimental	0	5000 Schools
Alspaugh (1999)	Developed	Non-Experimental	0	525 Schools
Wenglisnky (1998)	Developed	Non-Experimental	+/-	13373 Students
Leuven et al. (2004)	Developed	Non-Experimental	-	5938 Schools
Waxman et al. (2002)	Developed	Meta-Analysis	+	4000 Students
Kulik (2003)	Developed	Meta-Analysis	+	n.a.
Panel B: Impact on Attendance				
Author	**Country**	**Methodology**	**Estimated Effect**	**N**
Banerjee et al. (2005)	Developing	Experimental	0/+	230 Schools
Terrance et al (1996)	Developed	Non-Experimental	+	6000 Schools
Alspaugh (1999)	Developed	Non-Experimental	0	525 districts
Panel C: Impact on Attitudes and Motivation				
Author	**Country**	**Methodology**	**Estimated Effect**	**N**
Kulik (2003)	Developed	Meta-Analysis	+	n.a.

* In terms of methodology, the studies were classified in three categories according to this criteria: a) Experimental, assignment of treatment was randomized; b) Quasi-Experimental, treatment assignment somewhat mimics a randomized setting; c) Non-experimental, impacts estimated comparing adjusted means across groups with varying intensity of treatment; d) Meta-Analysis, statistical summary of small-scale studies (typically non-experimental).

Table 2: Number of Public Schools Covered by Huascaran between 2001 and 2006

	All	Secondary Urban	Secondary Rural	Primary Urban	Primary Rural
Coverage (*N*)					
Public Schools in 2006	34726	2981	3612	4498	23059
Covered	3359	1347	1117	527	368
Coverage (*%*)	10%	45%	31%	12%	2%
Inputs Received by Schools Covered (*%*)					
Software, Training and Support	100%	100%	100%	100%	100%
Hardware OR Connectivity	89%	94%	93%	75%	80%
Hardware	70%	75%	75%	56%	57%
Connectivity	72%	77%	79%	57%	58%
Hardware AND Connectivity	53%	58%	61%	37%	35%

Note: All statistics for coverage correspond to the total number of schools that were covered between 2001 and 2006.

Table 3: Summary Statistics - All Schools and Permanent Schools in 2001 and 2006

	2001		2006	
	Main Sample	Respondents in the Year	Main Sample	Respondents in the Year
Outcomes				
Repetition Rate (Males and Females, Average)	10.1	10.0	9.2	9.0
Drop Out Rate (Males and Females, Average) *	5.7	5.7	5.6	5.7
Enrollment ((Males and Females, Grade 1)	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
School Quality Indicators (per 100 Students)				
Classrooms	4.9	5.0	4.8	5.3
Sections	3.3	3.4	3.5	3.7
Teachers	5.1	5.2	5.4	5.7
Administrative Staff	1.2	1.2	1.3	1.3
Blackboards	4.9	5.0	4.6	4.9
Number of Schools	7319	8252	7319	10635

Note: Main Sample is comprised by schools that answered the surveys in all years used (2001, 2003, 2004, 2005 and 2006). Statistics for Respondents in the Year corresponds to schools that answered the survey in the particular year (e.g. 2001).

* Fraction of students enrolled in a grade that drop out during the school year.

** Fraction of students enrolled in a grade that failed to approve up to 12/31 of the current year.

Table 4: Summary Statistics by Public/Private and Rural versus Urban in 2004

	Totals	Public Urban	Public Rural	Private Urban	Private Rural
Outcomes					
Repetition Rate (Males and Females, Average)	9.6	10.9	9.6	4.3	7.2
Drop Out Rate (Males and Females, Average) *	5.7	5.2	10.3	2.3	6.1
Enrollment ((Males and Females, Grade 1)	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
School Quality Indicators (per 100 Students)					
Classrooms	4.7	3.3	6.0	9.6	7.9
Sections	3.4	3.0	4.4	4.2	4.8
Teachers	5.2	4.4	6.0	7.7	6.3
Administrative Staff	1.3	1.0	0.8	2.8	1.4
Blackboards	5.0	3.4	6.0	10.7	8.6
Number of Schools	7319	2555	2666	2028	70

Note: Sample is comprised by schools that answered the surveys in all years used (2001, 2003, 2004, 2005 and 2006).

* Fraction of students enrolled in a grade that drop out during the school year.
** Fraction of students enrolled in a grade that failed to approve up to 12/31 of the current year.

Table 5: Summary Statistics - Treated Group and Potential Comparison Groups

	Beneficiary IDB (2004)	Potential Comparison Groups			
		Beneficiaries Pre-Huascaran	Benerificiary 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
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
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
Classrooms	2.8	2.5	3.2	3.8	4.3
Sections	2.8	2.7	3.0	3.0	3.2
Teachers	4.1	4.1	4.3	4.5	4.8
Administrative Staff	0.9	1.0	1.0	1.0	1.0
Blackboards	2.8	2.5	3.1	3.7	4.3
ICT Variables 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 Variables 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

Note: Weights was done it in base of specification 2 using Time-Varying Controls. Moreover, we eliminate the observations with scores outside the interval (0.3, 0.7) for the trimmed columns.

* Fraction of students enrolled in a grade that drop out during the school year.

Table 6: Summary Statistics - Treated and Comparison Group Before and After Reweighting

	Treated		Pre-Huascaran Beneficiaries	
	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	224.0
Indicators 2003				
Computers for Learning	7.6	7.1	26.7	19.1
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
Enrollment	1098.1	1080.5	1490.9	1067.7
% Overaged in First Grade	40.8	40.5	36.3	36.9
% Have Principal	95.2	95.7	97.0	95.6
% Have Teachers' Lounge	61.0	62.4	68.5	59.1
% 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	95.6
Classrooms	2.8	2.8	2.5	2.8
Sections	2.8	2.8	2.7	2.8
Teachers	4.1	4.1	4.1	4.1
Administrative Staff	0.9	0.9	1.0	0.8
Blackboards	2.8	2.8	2.5	2.8
ICT Variables 2005				
Computers for Learning	15.7	15.8	24.7	18.7
Students Potential Access (Hs/Week)	0.9	0.9	1.0	1.0
% Have Computer Lab	80.3	81.5	84.4	84.6
% Have Internet Access	62.3	62.4	67.1	67.6
ICT Variables 2006				
Computers for Learning	18.7	18.0	29.0	19.7
Students Potential Access (Hs/Week)	1.0	1.0	1.2	1.1
% Have Computer Lab	93.2	91.1	96.0	92.4
% Have Internet Access	88.5	85.9	84.2	78.4
Number of Schools	267	177	456	216

Note: Weights was done it in base of specification 2 using Time-Varying Controls. Moreover, we eliminate the observations with scores outside the interval (0.3, 0.7) for the trimmed columns.

* Fraction of students enrolled in a grade that drop out during the school year.

Table 7: Fixed Effects Estimates of Program Impacts

	Repetition Rate		Drop Out Rate		Enrollment in First Grade	
	(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: total enrollment, students per teacher, students per sections, teachers appointed per classroom, number of classroom, have gym, number of blackboards, have principal, have sub principal, number of administrative workers, have water, have drain, have electricity, have other lab, have workshop, have teacher's lounge, have administrative offices, have library, have sanitation, number of tables, number of chairs; as specification 1 showed, with the exception on columns 5 and 6, in which total enrollment was excluded as variable control. Also we used temporal controls in which 2001 is the base year. Standard errors are clustered at the school level. * 10%; ** 5%; *** 1%.

Table 8: Fixed Effects Estimates of SIPA - Public Urban Sample

	Repetition Rate		Drop Out Rate		Enrollment in First Grade	
	(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: total enrollment, students per teacher, students per sections, teachers appointed per classroom, number of classroom, have gym, number of blackboards, have principal, have sub principal, number of administrative workers, have water, have drain, have electricity, have other lab, have workshop, have teacher's lounge, have administrative offices, have library, have sanitation, number of tables, number of chairs; as specification 1 showed, with the exception on columns 5 and 6, in which total enrollment was excluded as Time-Varying control. Also we used temporal controls in which 2001 is the base year. Standard errors are clustered at the school level. * 10%; ** 5%; *** 1%.

Table 9: Fixed Effects Estimates of SIPA, Qualitative Version - 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: total enrollment, students per teacher, students per sections, teachers appointed per classroom, number of classroom, have gym, number of blackboards, have principal, have sub principal, number of administrative workers, have water, have drain, have electricity, have other lab, have workshop, have teacher's lounge, have administrative offices, have library, have sanitation, number of tables, number of chairs; as specification 1 showed, with the exception on columns 5 and 6, in which total enrollment was excluded as Time-Varying control. Also we used temporal controls in which 2001 is the base year. Standard errors are clustered at the school level. * 10%; ** 5%; *** 1%.

Table 10: Fixed Effects Estimates of SIPA with Lagged Effects

	Repetition Rate		Drop Out Rate		Enrollment in First Grade	
	(1)	(2)	(3)	(4)	(5)	(6)
SIPA	-0.052	-0.041	-0.073*	-0.083**	0.370	0.413
	(0.143)	(0.145)	(0.041)	(0.041)	(0.404)	(0.415)
SIPA First Lag	-0.062	-0.053	-0.078	-0.086*	0.788	0.830
	(0.128)	(0.128)	(0.050)	(0.049)	(0.658)	(0.686)
SIPA Second Lag		0.143		-0.128**		0.544
		(0.132)		(0.054)		(0.761)
Constant	11.214***	11.107***	1.585*	1.681**	103.362***	102.975***
	(2.849)	(2.849)	(0.815)	(0.812)	(12.095)	(12.175)
N	96133	96133	96133	96133	19346	19346
R2	0.314	0.314	0.384	0.384	0.962	0.962

Note: Each column corresponds to one regression. Time-Varying Controls were used and these are: total enrollment, students per teacher, students per sections, teachers appointed per classroom, number of classroom, have gym, number of blackboards, have principal, have sub principal, number of administrative workers, have water, have drain, have electricity, have other lab, have workshop, have teacher's lounge, have administrative offices, have library, have sanitation, number of tables, number of chairs; as specification 1 showed, with the exception on columns 5 and 6, in which total enrollment was excluded as Time-Varying control. Also we used temporal controls in which 2001 is the base year. Standard errors are clustered at the school level. * 10%: ** 5%: *** 1%.

Table 11: Fixed Effects Estimates of SIPA - 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)

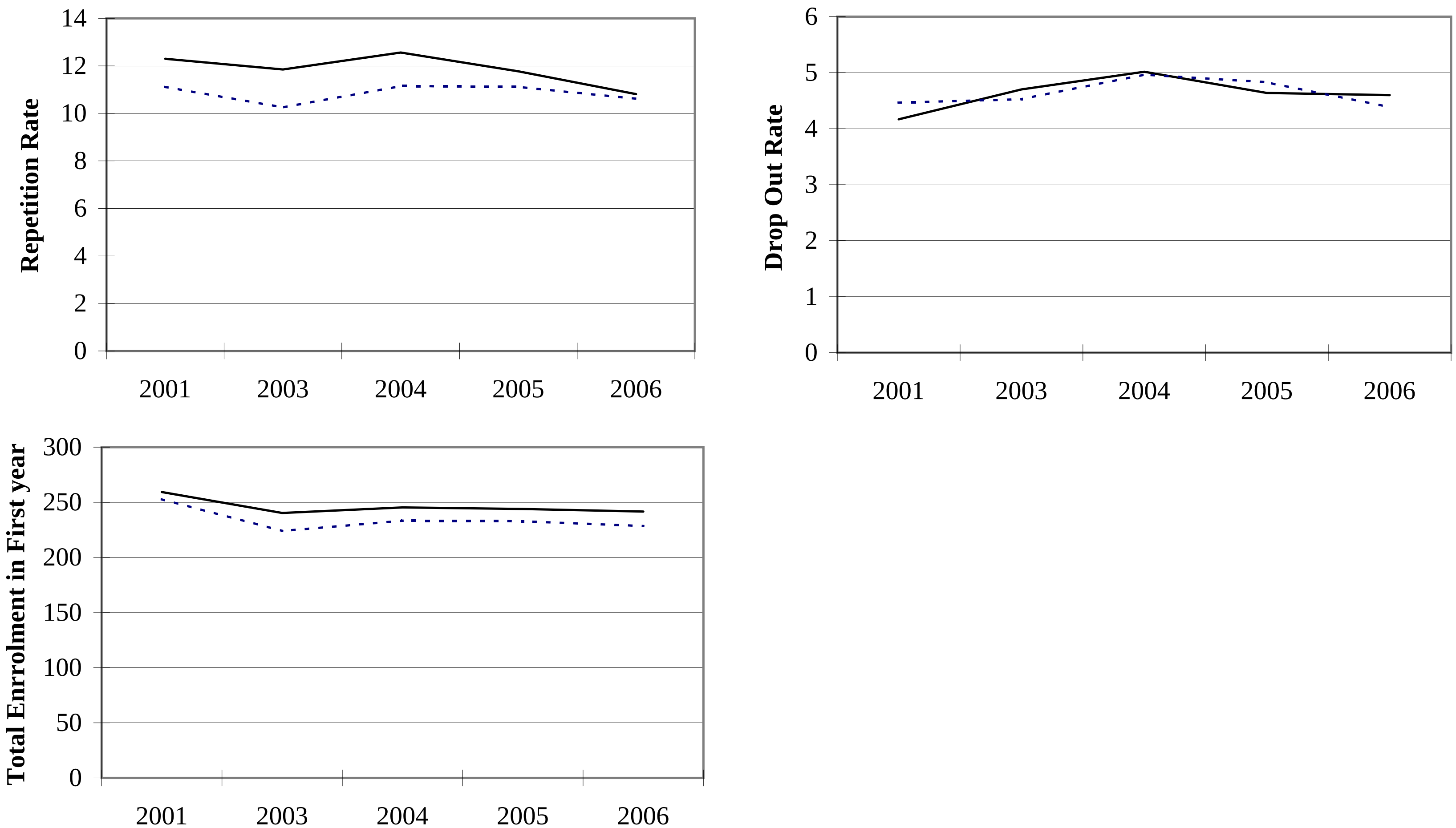
Note: Each coefficient corresponds to one regression. Time-Varying Controls were used and these are: total enrollment, students per teacher, students per sections, teachers appointed per classroom, number of classroom, have gym, number of blackboards, have principal, have sub principal, number of administrative workers, have water, have drain, have electricity, have other lab, have workshop, have teacher’s lounge, have administrative offices, have library, have sanitation, number of tables, number of chairs; as specification 1 showed, with the exception on columns 5 and 6, in which total enrollment was excluded as Time-Varying Control. Also we used temporal controls in which 2001 is the base year. Standard errors are clustered at the school level. * 10%; ** 5%; *** 1%.

Table A.1: Robustness Check - Impact of SIPA using Longitudinal Variation

	School in years 1,3,4,5,6 (1)	**School in averaged years (1, 3), (4,5,6)** (2)	**School in years 1,3,4** (3)	**School in averaged years 1, (3,4)** (4)	**District in years 1,3,4** (5)	**Province in years 1,3,4** (6)
Repetition Rate	-0.067	0.056	-0.149	-0.207	-0.193	0.292
	(0.173)	(0.356)	(0.228)	(0.455)	(0.225)	(0.492)
Drop Out Rate	-0.003	-0.072	0.134*	0.305**	0.021	0.068
	(0.051)	(0.119)	(0.080)	(0.132)	(0.095)	(0.168)
Enrollment in First Grade	0.741	2.073	0.665	0.387	0.491	0.232
	(1.030)	(3.378)	(1.250)	(2.661)	(0.761)	(0.745)
Fixed Effect	By School	By School	By School	By School	By District	By Province

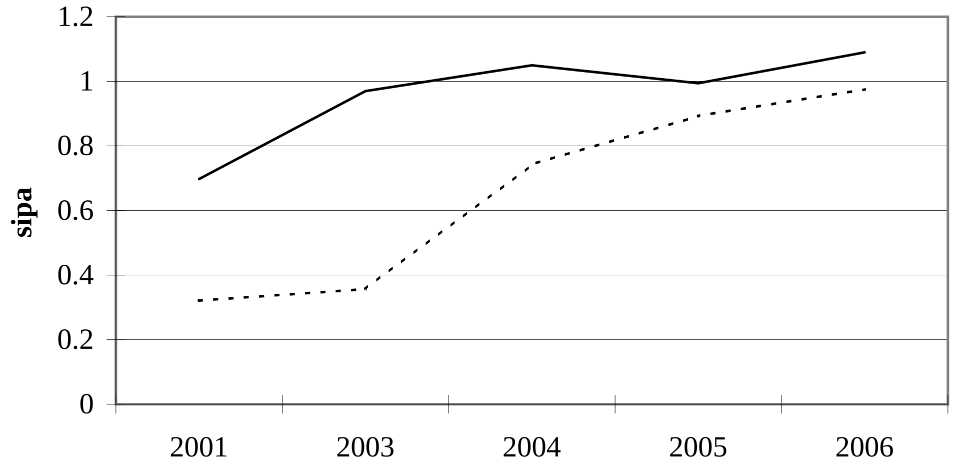
Note: Each coefficient corresponds to one regression. We use those schools were reported to have electricity in all the years. Time-Varying Controls were used and these are: total enrollment, students per teacher, students per sections, teachers appointed per classroom, number of classroom, have gym, number of blackboards, have principal, have sub principal, number of administrative workers, have water, have drain, have electricity, have other lab, have workshop, have teacher's lounge, have administrative offices, have library, have sanitation, number of tables, number of chairs; as specification 1 showed, with the exception on columns 5 and 6, in which total enrollment was excluded as Time-Varying Controls. Also we used temporal controls in which 2001 is the base year. Standard errors are clustered at fixed effect level. * 10%; ** 5%; *** 1%.

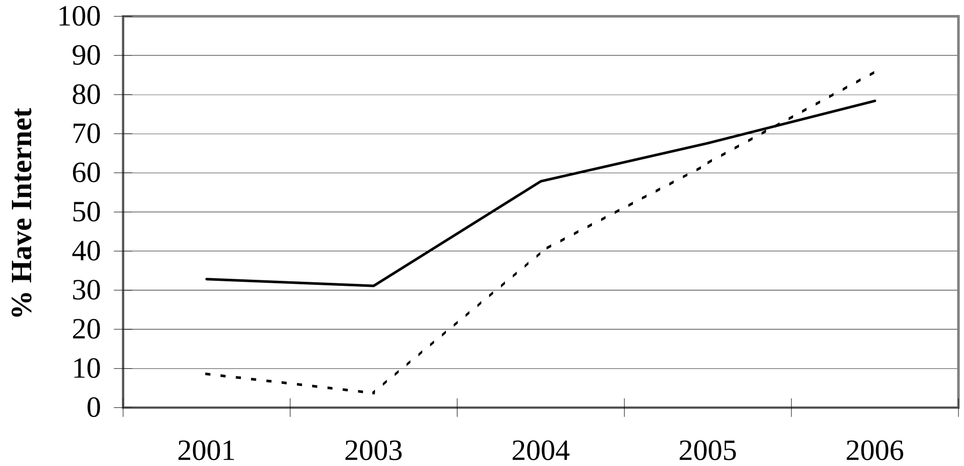
Figure 1: Evolution Outcomes by Treatmetnt Status



Note: Control group (dotted line) and Treatment group (solid line) are exposed in each graph. Means were estimated usign total enrrolment as a weighting. We used public urban sample and we eliminated schools that belongs to any program different to IADB program.

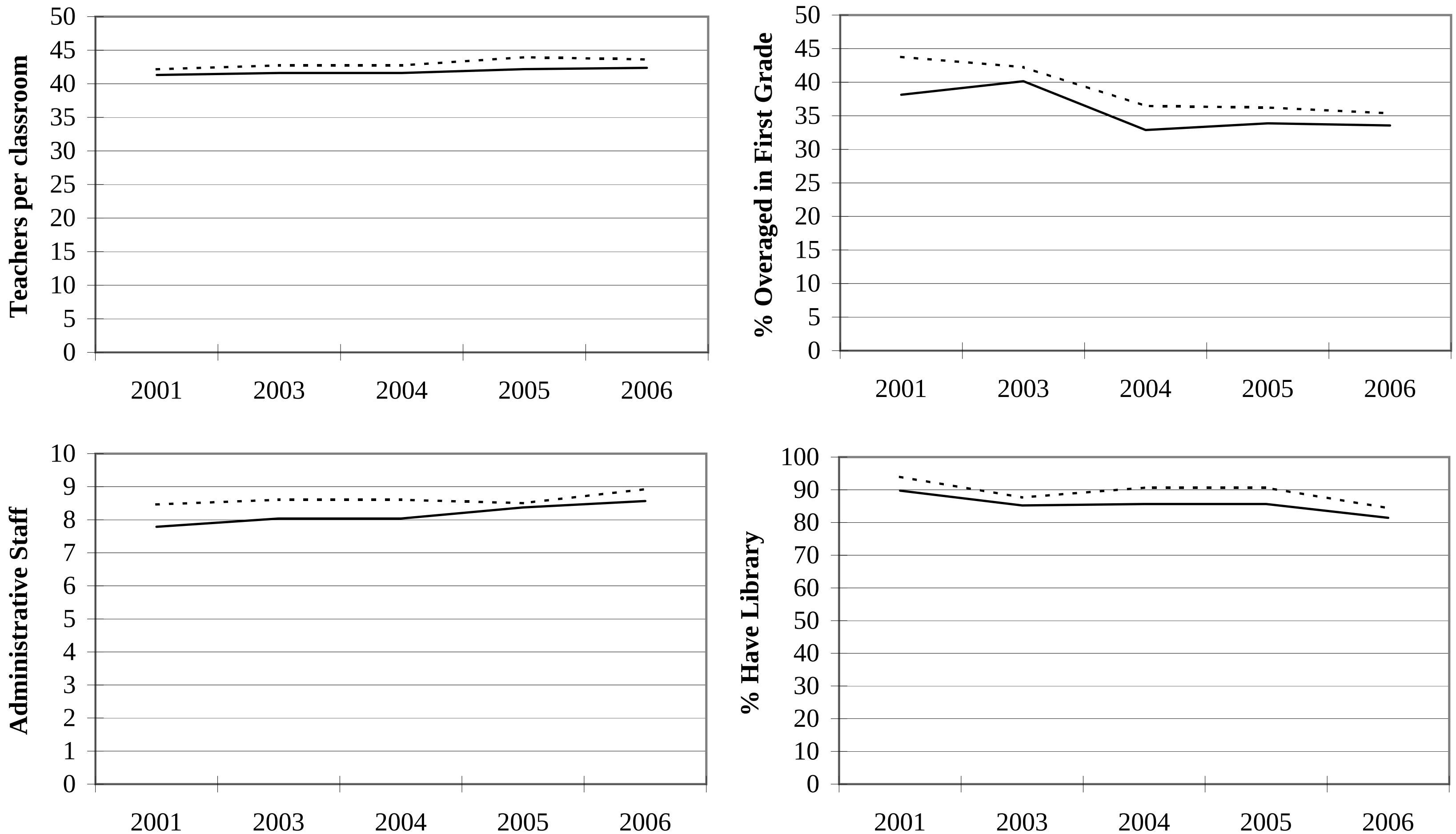
Figure 2: Evolution of SIPA and Internet Access by Treatment Status





Note: Control group (solid line) and Treatment group (dotted line) are exposed in each graph. Means were estimated usign total enrrolment as a weighting. We used public urban sample and we eliminated schools that belongs to any program different to IADB program.

Figure 3: Evolution School Inputs by Treatment Status



Note: Control group (solid line) and Treatment group (dotted line) are exposed in each graph. Means were estimated usign total enrrolment as a weighting. We used public urban sample and we eliminated schools that belongs to any program different to IADB program.