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# Laptops in the Long-Run: Evidence from the One Laptop per Child Program in Rural Peru*

**Abstract**

This paper examines a large-scale randomized evaluation of the One Laptop Per Child (OLPC) program in 531 rural primary schools, as implemented by Peru starting in 2009. We use administrative data on academic achievement and grade progression through 2019 to estimate the long-run effects of greater computer access on i) school performance over time and ii) students' educational trajectories from primary school to university. Results suggest negative effects on grade progression and no improvement in academic achievement for treated schools over time. In turn, treated students had lower on-time primary and secondary completion, no higher academic achievement in secondary school, and no significant differences in university enrollment. Survey data from 2013 indicate that computer access significantly improved students' computer skills but not their cognitive skills; treated teachers received some training but did not improve their digital skills and showed limited use of technology in classrooms, suggesting the need for additional pedagogical support.

**JEL classifications:** I21, I24, I28
**Keywords:** Education, Technology, Evaluation

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# I Introduction

Developing countries have made large investments to expand access to technology in schools with the aim of improving educational outcomes. For example, between 2006 and 2012, 20 countries in Latin America and the Caribbean distributed nearly 10 million laptops to public school students (Arias Ortiz and Cristia, 2014). One highly publicized initiative that provided personal laptops to students was the One Laptop per Child (OLPC) program, which was launched in 2005 and implemented in 40 countries. These types of programs have garnered renewed interest during the COVID-19 pandemic as schooling was conducted remotely.[1] Notwithstanding the substantial public investments in these programs, experimental studies have generally not found short-term educational effects of providing personal computers to students (Mo et al., 2013; Beuermann et al., 2015).[2] A large-scale experimental evaluation of the OLPC program in Peru found no effects on academic achievement or enrollment in primary school after 15 months of program implementation (Cristia et al., 2017).

Despite the absence of impacts on academic achievement in the short term, expanding access to technology in schools could generate impacts on longer-term outcomes along two dimensions. On the one hand, there may be dynamic effects on *schools* over time if teachers and principals need time to learn how to use technology effectively for pedagogical purposes (Lakdawala et al., 2023). On the other hand, there could be dynamic effects on *students* as they progress through the educational system. Greater exposure to computers could lead to changes in students' attitudes, behaviors, and a broad range of skills with small effects on short-term academic achievement but larger effects on downstream educational outcomes (Yanguas, 2020a,b). This paper presents experimental evidence on the long-term effects of expanding school access to technology on both i) academic performance and grade progression in schools over time and ii) students' educational trajectories as they progress from primary to tertiary education.

We examine a large-scale randomized evaluation of the OLPC program as implemented by the Peruvian government in rural primary schools using administrative and survey data between 2007 and 2019. We focus on 531 public, primary, rural multigrade schools that were randomized into treatment and control groups. Treatment schools were assigned to participate in the OLPC program which provided students with personal laptops (called "XO laptops") starting in 2009. These low-cost durable laptops were specifically designed for learning in developing countries and

[1] For example, the government of the Dominican Republic distributed 1.5 million devices to students and teachers in 2020-2022 (Latino, 2023), while the governments of Kenya and Peru distributed 1.2 million and 1 million tablets, respectively, during this period (Standard, 2021; MINEDU, 2022).

[2] Another set of studies has assessed the effects of specific learning platforms on academic achievement with more positive results, especially when implemented after school and involving additional instructional time (Banerjee et al., 2007; Linden and MacLeod, 2008; Lai et al., 2012; Berlinski and Busso, 2013; Lai et al., 2013; Mo et al., 2014, 2015; Lai et al., 2015; Araya et al., 2019; Ferman et al., 2019; Muralidharan et al., 2019; Büchel et al., 2022; Ma et al., 2023).

came loaded with about 200 e-books and 39 applications. Teachers in the treatment group were expected to receive a one-week, 40-hour training that focused on how to operate the laptops and use them for pedagogical purposes. Using administrative data, we show that the program increased the ratio of XO laptops to students in treatment schools from 0 to 1 by the end of 2009. Starting in 2011, some XO laptops were also distributed to schools in the control group so that the average ratio of XO laptops per student in these schools eventually reached 0.4 by 2019. Still, the stark difference in access to XO laptops between treatment and control schools remained, and students in control schools never approached the 1-to-1 access to computers of students in treatment schools.

First, we analyze the effect of the OLPC program on schools' academic achievement from 2009 to 2016 using data from annual second grade national exams. Results indicate small and insignificant effects on mathematics and reading achievement without a clear pattern of differences in effects over time. Pooling data across all years, we estimate negative but insignificant effects on achievement, allowing us to rule out positive effects larger than 0.05 and 0.03 standard deviations in mathematics and reading, respectively, with 95 percent confidence. Thus, there is little evidence to suggest that the OLPC program improved school academic performance over time. We then estimate the effects on the fraction of primary students that advanced to the next grade between 2009 to 2016 using data from the annual school censuses. The pooled estimate suggests a negative effect of 1.0 percentage point on grade progression.

Second, we assess the effect of the OLPC program on students' educational trajectories using data from 4th and 8th grade national exams, 5th and 6th grade tests that we administered, and longitudinal administrative data on grade progression from primary to tertiary education. We estimate null impacts on 4th grade mathematics test scores but negative and marginally significant impacts on reading scores, and no significant effects in either mathematics or reading in 5th, 6th, and 8th grades. In terms of grade progression over time, we find negative and significant reductions in the likelihood of completing primary education on time of 2.2 percentage points. Nevertheless, there were no effects on completing primary education overall. There were also negative and marginally significant effects on completing secondary school and applying to university on time equivalent to 3.1 and 2.3 percentage points, respectively. Effects on enrolling in university are negative but insignificant. Note, the negative effects on on-time primary and secondary completion should be interpreted with caution since they are not significant after adjustments for multiple hypothesis testing (with $q$-values of 0.134).

Why did the OLPC program not improve academic achievement or educational attainment? To address this question, we use survey data collected from a subset of 140 schools in 2013. Teachers in treatment schools were 35 percentage points more likely to report receiving training in the use of the XO laptops than teachers in control schools (with a mean of 28 percent). However, there were no significant impacts on digital skills of teachers in the use of the XO laptops, PC computers, or

the internet. Furthermore, our results suggest that the OLPC program generated a relatively small increase in laptop and computer use in the classroom as reported by teachers.

Turning to student outcomes, we document that the program increased the use of XO laptops at home on the day prior to the survey by 20 percentage points, with no effects found on the use of other computers or the internet at home. Consistent with this, we find large positive effects of 0.40 standard deviations on their digital skills in using XO laptops, but only marginally significant effects on the use of PC computers and no effects on internet-related skills. Nor do we find positive effects on an index of cognitive skills based on the Raven's Progressive Matrices, verbal fluency and coding tests. Thus, the absence of positive impacts on academic achievement and educational attainment may be due to the limited adoption of XO laptops in schools for academic purposes and the small effects on students' intermediate outcomes beyond their digital skills.

This study builds on the experimental literature examining short-term educational effects of providing personal home computers to students (Mo et al., 2013; Beuermann et al., 2015; Cristia et al., 2017). However, our main contribution is to the nascent strand of the literature analyzing the long-term effects of expanding access to technology in schools. Regarding the dynamic effects on schools, Lakdawala et al. (2023) analyze how the provision of internet access to schools in Peru affects their performance over time. Using an event-study approach applied to the same administrative data on second grade academic achievement that we use, they document modest positive effects of 0.02 to 0.03 standard deviations one year after schools receive internet, but growing and larger effects of 0.06 to 0.11 standard deviations after five years.[3] Regarding effects on students, Yanguas (2020a) evaluates the long-term effects of an OLPC program in Uruguay among students exposed during childhood. Exploiting cross-cohort variation in exposure to the program, she finds no effects on high school enrollment and graduation or tertiary enrollment but some evidence for a shift from science and technology majors to social science majors in university.[4] Our study contributes to this literature by presenting the first experimental evidence on the long-term effects of expanding access to computers on both school performance over time and student trajectories as they progress through the educational system.

We proceed with Section II laying out the study design and the sample selection. Section III presents the data and empirical strategy, Section IV discusses the results, and Section V concludes.

[3] Consistent with Lakdawala et al. (2023), Malamud et al. (2019) find no short-term effects on academic achievement of providing personal laptops with internet for home use among primary school students in Lima, Peru.

[4] In a related paper, Yanguas (2020b) does observe improvements in promotion and graduation from secondary school from increased access to computers and internet in Argentina.

# II Study Design

## II.1 Education in Peru

Public education in Peru is free and compulsory for students from preschool through the end of secondary school, but enforcement is lax. Children aged 6 to 11 are expected to attend primary school in grades 1 to 6, and those aged 12 to 16 are expected to enroll in secondary school in grades 7 to 11. However, it is common to find relatively older students outside these ranges due to high rates of grade repetition.[5] Between 2009 and 2018, public spending per student in constant 2015 USD rose from $461 to $733 for primary education and from $544 to $957 for secondary education (World Bank, 2024). During this period, the proportion of students completing primary education increased from 91% to 96% and from 73% to 84% in secondary education (Inter-American Development Bank, 2022).

Academic performance in Peru has been improving, with the proportion of second grade primary students meeting the national mathematics standard increasing from 14% in 2009 to 34% by 2016, according to data from the national standardized exams (MINEDU, 2016). Reading proficiency also improved, with the percentage of second grade students reaching the national standard rising from 23% in 2009 to 46% in 2016. Nevertheless, educational outcomes reveal pronounced disparities among student groups, particularly when differentiated by residential location. For example, only 17% of rural students met the second-grade mathematics standard in 2016, compared to 37% of their urban counterparts (MINEDU, 2016).

## II.2 Intervention

The One Laptop per Child (OLPC) initiative, conceived by a team at the Massachusetts Institute of Technology Media Lab, aimed to provide affordable laptops to children in economically disadvantaged regions of the world. Announced in 2005, these XO laptops were initially touted as "$100 laptops" but governments eventually purchased them for approximately $200 each. The program saw its greatest uptake in Latin America, where 82% of the laptops were distributed, including the two largest deployments (Peru with 902,000 laptops, and Uruguay with 585,000 laptops).

The government of Peru launched its national OLPC program in 2008, distributing 40,000 laptops to about 500 schools. The program targeted the nation's most impoverished regions, ensuring that participating schools either had access to electricity or were provided with solar panels when necessary. The program aimed to provide an XO laptop to every student in these selected

[5] In grades two to six of primary school, children repeat a grade if they fail both Spanish and mathematics and do not pass the recovery program offered during summer vacations (promotion in the first grade is automatic). In secondary school, students repeat a grade if they fail four or more subjects and do not pass the recovery program. All evaluations for promotion or grade repetition are conducted by teachers based on competencies in the national curriculum. (MINEDU, 2005).

schools. In terms of software, the government equipped the laptops with 39 open-source applications, organized into five categories: standard (write, browser, paint, calculator, and chat); games (educational, including Memorize, Tetris, Sudoku, and a variety of puzzles); music (to create, edit, and play music); programming (three programming environments); and other (including sound and video recording and specific sections of Wikipedia). Furthermore, around 200 age-appropriate e-books, were preloaded onto the laptops. The lack of internet access and the laptops' incompatibility with Windows prevented students from installing video games or other software.

### II.3 Sample Selection

We focus on a sample of 531 primary schools in rural Peru that were randomized to receive the OLPC program, with 296 treated schools and 235 control schools. This sample is distinct from the one used by Cristia et al. (2017) to estimate the short-term effects of the OLPC program, although both are derived from the same initial set of schools.

The original sample targeted for randomization included 956 primary schools that were public, rural, multigrade, had administrative data on inputs for at least one year between 2005 and 2007, and were in the poorest districts within each region. Schools were randomized stratifying by region, fraction of overage students, and school size, with 567 schools selected for treatment and 389 assigned to the control group. We apply two restrictions to this initial set of schools. First, we exclude the 105 one-teacher schools because the government decided to include all of them in the OLPC program. Second, we exclude the 320 schools selected for evaluation in Cristia et al. (2017) because the government decided that the control schools in this sample would also participate in the OLPC program once their study was completed in 2011.[6] This yields the 531 schools for our study.

## III Data and Empirical Strategy

### III.1 Data

We estimate the long-term impacts of the OLPC program on academic performance and school progression using comprehensive administrative and survey data. To track treatment compliance and other school-level inputs over time, we utilize an annual school census in which principals report information to the Peruvian Ministry of Education. These databases contain school-level information on enrollment, teachers, resources, infrastructure, and technological inputs such as the number of computers and internet access. We analyze data from these annual databases from 2008, i.e., one year before the implementation of the OLPC program, until 2019.

To measure academic performance, we use national standardized examinations conducted by

[6]The sample in Cristia et al. (2017) was selected at the randomization strata level, so it remained internally valid. This also applies to the 531 schools in our study.

the Peruvian Ministry of Education, which evaluate students in mathematics and reading skills. These evaluations were carried out across different academic years and grade levels. At the primary level, students in second grade were assessed annually between 2007 and 2016, while those in fourth grade were evaluated in 2016 and 2018. At the secondary level, eighth-grade students were assessed in 2015, 2016, 2018, and 2019. Since the latter examinations were taken in secondary school, we matched these students with the primary school where they took their second-grade national examination. Thus, we evaluate academic achievement in secondary school among students who took the second-grade exam in one of the 531 experimental primary schools.

To assess students' primary and secondary school progression, as well as their application and enrollment to tertiary education, we leverage administrative enrollment data from the SIAGIE system. Launched in 2012 and administered by the Peruvian Ministry of Education, the SIAGIE system compiles the enrollment status of each student in the country at the primary, secondary, and tertiary levels, encompassing both public and private educational institutions. Specifically, for each year between 2012 and 2019, we observe every student's attended school, grade, passing status, and, when applicable, the student's application and enrollment status in tertiary education institutions.[7] As students are tracked annually across schools, we can identify those who attended one of the 531 primary schools in our experimental sample. When selecting students for analysis, we include those who attended an experimental school for at least one year. Since 98 percent of the students in our sample remain in the same primary school, this criterion is essentially equivalent to including students who have completed their entire primary education in one of the experimental schools.

Finally, in November 2013, we administered detailed surveys to principals, teachers, and students (attending fifth and sixth grade) in 70 treated and 70 control schools, randomly selected from our experimental sample of 531 schools.[8] We collected information on access and use of XO laptops and other devices, internet connectivity, teachers' technical training, and digital competencies. We also administered several test instruments to students examining mathematics and reading achievement as well as a wide range of student skills beyond those tested in national exams, such as digital and cognitive skills (including Raven's progressive matrices, a verbal fluency test, and a coding test).[9] These survey data, obtained four years following the start of the OLPC program

[7] We do not use SIAGIE data for 2020/2021 because the Ministry of Education adopted universal progression during the Covid-19 pandemic.

[8] Due to budgetary constraints, we sampled schools from 5 regions. Nonetheless, the sample remains internally valid as the selection was conducted at the randomization strata level.

[9] The Raven's Progressive Matrices measure nonverbal abstract reasoning: respondents are presented with a series of progressively more difficult exercises that require choosing the figure that completes a pattern. The verbal fluency test measures language functions (vocabulary): students are instructed to write as many words as they can that began with a certain letter in three minutes (Ruff et al., 1997). The coding test measures processing speed and working memory: 10 pairs of one-digit numbers and graphical symbols were shown to students, who then have to complete as many corresponding symbols as possible in three minutes.

implementation, help us explore potential mechanisms.

## III.2 Empirical Strategy

We assess the average treatment effect of the OLPC program by estimating the following OLS regression equation:

$$Y_{itsr} = \alpha + \beta \cdot OLPC_{istr} + \mu_{rt} + \varepsilon_{itsr} \tag{1}$$

where $Y_{itsr}$ represents the outcome of student $i$ from cohort $t$, who attended primary school $s$ that belong to randomization strata $r$. $OLPC_{istr}$ is an indicator that takes the value of 1 if school $s$ was randomly assigned to participate in the OLPC program, and 0 otherwise. We define cohorts based on the year in which students attended second grade because our primary measure for academic achievement is the second grade national examination. Following Bruhn and McKenzie (2008), we include randomization strata-by-cohort fixed effects ($\mu_{rt}$) to improve precision.[10]

Given that the treatment was randomly assigned at the school level, we cluster estimated standard errors at that level. Moreover, with so many indicators and samples, we also report $q$-values which are analogous to $p$-values when accounting for multiple hypothesis testing (Benjamini et al., 2006; Anderson, 2008). The $q$-values denote the lowest critical level at which a null hypothesis is rejected when controlling for the false discovery rate.

## III.3 Baseline Balance

Table 1 shows baseline balance using data from the 2008 school census and the 2007 and 2008 national second grade examinations. Panel A shows that access to computers and internet at baseline was minimal, with both treatment and control schools having 2 computers per 100 students. Panel B shows balance on other school-level inputs. While almost 90 percent of schools had electricity, less than half had access to piped water. In terms of pedagogical infrastructure, a fifth of schools had a library and almost none had a science lab. The average student-teacher ratio was around 20, the share of students who transferred from other schools was negligible, and the fraction of overage students at school entry was balanced across the treatment and control groups. Panel C shows that second grade students were balanced in terms of mathematics and reading performance in the 2007 and 2008 national examinations.[11] Panel D shows balance in the fraction of students who advanced to the next grade.

[10] Results without controlling for randomization strata remain qualitatively unchanged.

[11] Treated and control schools were also balanced on trends in these outcomes.

### III.4 Compliance

We examine compliance with the OLPC program in Figure 1, which shows the number of XO laptops per student for treated and control schools over time. From 2009 on, within a year of starting the OLPC program, treated schools were effectively saturated with one XO laptop per student. In contrast, control schools received almost no XO laptops through 2010. Starting in 2011, control schools did begin receiving some XO laptops, eventually reaching a ratio of over 0.4 laptops per student in 2019. [12] However, the difference in the intensity of treatment between treated and control schools remains large and significant throughout our period of analysis.[13]

We also explore whether the presence of the OLPC program triggered changes in other technology-related inputs or school characteristics. Appendix Table A.1 shows that while internet connectivity grew over time from virtually zero in 2009 to over 18 percent of schools with internet access by 2019, there were few significant differences between treated and control schools in any of the years. Appendix Table A.2 shows that there were no differences in access to desktops, other non-XO laptops, or on the presence of a computer lab in the school across the treatment and control groups. Appendix Table A.3 shows that the intervention did not generate differences in access to electricity, water, science lab, or library, or in the ratio of students per teacher, the share of students transferring from other schools, or overall school enrollment.

Our survey confirms the differential access to XO personal laptops between treatment and control groups among both students and teachers. It also shows that the program affected access to XO laptops at home as some students were allowed to take the devices with them.[14] However, there were no effects on either access to non-XO computers at home or internet access at school or home (Panel A of Appendix Table A.4). Thus, while the OLPC program generated significant and sustained access to personal XO laptops, it did not cause any significant impacts on other technology-related inputs or school characteristics that may have affected learning outcomes or grade progression.

## IV Results

We examine the long-run effects of exposure to the OLPC program in Peru on i) the academic performance and grade progression of schools over time by following successive cohorts in treatment and control schools and ii) the trajectories of student achievement and educational attainment by following students as they progress from primary school to university. We then use survey data to explore potential mechanisms explaining these long-run patterns.

[12] This reflected the government's desire to provide other rural schools with educational technology as well.

[13] Although control schools were not less likely to have at least one XO laptop than treatment schools by 2013, the experimental variation in XO laptops per student persisted over time (Appendix Table A.1).

[14] Since the allocation of XO laptops within control schools was uncorrelated with academic performance, this rules out potential biases arising from only high performers benefiting from technology.

## IV.1 School Performance and Grade Progression over Time

Panels A and B of Table 2 present the effects of exposure to the OLPC program on school-level performance in the eight years following treatment, as measured by second-grade national exams in mathematics and reading for cohorts who took the exam between 2009 and 2016. There is no discernible pattern over time, and none of the coefficients are significant at the 5 percent level. The magnitudes range from -0.10 to 0.08 standard deviations for mathematics and -0.12 to 0.03 standard deviations for reading (excluding the 2014 cohort which is marginally significant but appears to be an outlier). Pooling all the years together, we can rule out, with 95 percent confidence, effects larger than 0.05 and 0.03 standard deviations in mathematics and reading, respectively.[15]

Panel C of Table 2 displays effects on grade progression, defined as the fraction of primary students who advanced to the next grade each year. All estimated coefficients are negative and, when pooling all years, results indicate a negative effect of 1.0 percentage point on grade progression (relative to the control group mean of 86.9%).[16]

We conclude that there is no evidence of positive impacts of exposure to the program on school-level educational outcomes over time. These estimates represent the long-term effects of the OLPC program on schools rather than students, insofar as treated students in all cohorts were exposed to just two years of the program prior to taking the second-grade examination. The estimates suggest that schools were not able to leverage technology to improve test scores in the early primary grades. The negative estimated impacts on grade progression over time suggest that the program may have triggered increases in grade repetition that affected students' trajectories as they progressed through the education system.

## IV.2 Student Trajectories in Achievement and Educational Attainment

Table 3 summarizes the effects of the OLPC program on academic achievement and educational attainment for students. We link students who took the national second grade exam in treatment and control schools to their scores on the fourth and eighth grades examinations.[17] Similarly, we follow the school progression of students who attended treatment and control schools using the

[15] The fraction of schools administering these exams ranged from 59% to 71% between 2007 and 2013, but coverage fell to 39% by 2016. Appendix B explores this issue and reports that school participation in the exam was unrelated to treatment across years, that key baseline characteristics of covered schools were balanced between treated and control groups over time, that there were no compositional changes of schools over time, and that the results are robust to the exclusion of years with low coverage. In the main analysis we weight schools by enrollment. Appendix B shows that negative effects for reading become significant ($p$-value $< 5\%$) when running unweighted regressions.

[16] Grade progression is computed by dividing the number of students promoted to the next grade at the end of the school year by the number of students enrolled at the beginning of the school year (and capped at 1). Appendix Table A.5 shows that the results are robust to alternative ways of computing the grade progression rate. Appendix Table A.6 shows that the negative effects are concentrated among students attending third and fourth grades.

[17] Appendix Table A.7 shows that an average of 69% (48%) of students who took the second grade exam were matched to the fourth (eight) grade examinations and that treatment was unrelated to the likelihood of being matched.

SIAGIE data. We then evaluate effects pooling all cohorts with available data for each outcome.

Panel A shows results for academic achievement.[18] Estimates for second-grade achievement are negative but insignificant. The estimates for fourth-grade achievement are also negative, insignificant for mathematics, and only marginally significant for reading. The absence of significant impacts on academic achievement during primary school is also apparent in the mathematics and reading tests that we administered to fifth and sixth grade students in the 2013 survey (reported in Panel B of Appendix Table A.4). At the secondary level, the effects on eighth grade scores are small and insignificant at 0.03 and -0.01 standard deviations for mathematics and reading respectively. Once we adjust for multiple hypothesis testing, the corresponding $q$-values indicate no significant effects on academic achievement.

Panel B shows results for educational attainment. Students in the treatment group are significantly less likely to complete primary school on time, and for delays of up to 1 year, with effects of 2.2 and 1.8 percentage points, respectively. Students are not less likely to complete primary education overall, however, which suggests that the program decreased adequate grade progression but did not affect dropout rates.[19] We also observe negative effects on the likelihood of completing secondary school and applying to university on time. Nonetheless, the impact on secondary school completion with delay, although negative, is imprecisely estimated. Impacts on enrolling to university on time are also negative but insignificant. When adjusting for multiple hypothesis testing, results indicate that the negative effects on primary and secondary completion on time are marginally insignificant with $q$-values of 0.134.[20]

Overall, there were no long-term effects of the OLPC program on academic achievement. Yet there is some evidence, albeit inconclusive, pointing to negative effects on on-time graduation at the primary and secondary levels, with no effects on overall graduation rates with more than one year of delay.[21]

## IV.3 Potential Mechanisms

We explore potential mechanisms for the absence of positive long-term impacts of the OLPC program using survey data that we collected in 2013, after four years of treatment. These surveys

[18] Appendix Table A.8 presents academic achievement impacts by cohort.

[19] Appendix Table A.9 indicates that the negative effects on primary school completion (and fourth-grade achievement) are relatively stronger among students with less educated caregivers.

[20] Given that the SIAGIE started only in 2012, complete grade progression is not observed for cohorts that started school before 2012. Therefore, we compute on-time graduation based on the year in which students should have enrolled in first grade according to national rules. Reassuringly, Appendix Table A.10 shows consistent effects across different cohorts from the SIAGIE, and we also obtain consistent evidence of reduced grade progression from the school censuses (Table 2). Another concern is that treatment might have altered the age at which parents enroll their children in first grade. Appendix Table A.11 documents that there were no effects on the fraction of overage students enrolled in first grade, suggesting that this possibility is not driving our results.

[21] Appendix Table A.12 reports effects by gender which were significantly different only for eight-grade achievement and marginally significantly different for applying to university. All other estimates are similar for males and females.

were administered to teachers and students in a random sub-sample of 70 treated and 70 control schools, and are presented in Table 4. Panel A reports that teachers in treatment schools were more than twice as likely to report receiving training in the utilization of the XO laptops than teachers in the control group (with a highly significant effect of 35 percentage points over the control mean of 28 percent). These significant effects of increased training also apply to specific categories such as learning about basic XO functions, XO activities and software, and learning activities using the XO laptops. While total training days were also substantially higher among treated teachers, the differences are entirely driven by the extensive margin, with no significant differences conditional on receiving training.[22]

Despite receiving some training, there is no evidence of positive effects on teacher's digital skills. The effect on XO-specific skills is 0.16 standard deviations, but it is not significant. The effects on general PC skills and internet use are negative and insignificant or marginally significant. Furthermore, in Panel C, we do not observe significant differences in teacher reports of computer use in the classroom between treatment and control schools. Teachers in treatment schools report using computers for only 0.8 hours per week more than teachers in control schools who use computers for 3.7 hours per week overall. These differences are even smaller in magnitude when looking at computer use in mathematics and reading/writing classes, although they are significant for reading classes where teachers in treatment schools report using computers for 0.4 hours more per week compared to the 1 hour per week in control schools. Still, even these differences are small in magnitude and may help explain why we fail to find effects of the OLPC program on academic outcomes.

Students in treatment schools were significantly more likely to use the XO laptops for entertainment, an increase of 14 percentage points.[23] They also exhibit a 10 percentage-point increase in use for academic purposes, though this effect is not statistically significant (see Panel D). Consistent with prior evidence on the OLPC program in rural Peru, we do find that exposure to the OLPC program has large and positive effects on computer skills. Panel E documents a highly significant increase of 0.41 standard deviations in XO-specific skills and a marginally significant increase of 0.17 in general PC skills, while there is no effect on internet-related skills. However, we do not find strong evidence of effects on cognitive skills (Panel F). While there is a marginally significant estimate of 0.19 standard deviations on verbal fluency, the effects on the Raven's Progressive Matrices and coding tests are small and insignificant. Following Kling et al. (2007), we compute a summary index of cognitive skills and find no significant effects.

[22] We also collected information on visits from IT specialists to teachers in the preceding 5 years. The differences between teachers in treatment and control schools are small and marginally significant, driven by differences in repair and maintenance rather than advice or training in the pedagogical use of XO laptops (Panel C of Appendix Table A.4).

[23] Consistent with the negative effects on school completion concentrated among students with less-educated parents, Appendix Table A.13 shows that use of XO laptops for entertainment is driven by students from low-SES households.

# V Conclusion

This paper presents results from a comprehensive, large-scale experimental evaluation of the One Laptop Per Child (OLPC) program in 531 rural primary schools, as implemented by the Peruvian government starting in 2009. We estimate null effects on schools' academic achievement through 2016 and on students when followed up until eighth grade. We also examine the effects on students' grade progression and find small negative effects on the fraction of primary students who advanced to the next grade between 2009 and 2016. Consistent with these results, we also find some suggestive evidence pointing to a small negative effect on the fraction of students who complete primary or secondary school on time, though there are no effects on eventually completing these education levels. Turning to mechanisms, we find that despite increased access and use of laptops at home, and some evidence of improved digital skills for students, there was limited adoption of laptops for pedagogical purposes at schools and no effect on teachers' digital skills. These results suggest that providing computers to students without sufficient pedagogical support may have limited effects on academic achievement and could even lead to negative effects on students' grade progression over time.

Our results contrast with those of Lakdawala et al. (2023), who document that increased school internet access in primary schools in Peru led to positive long-term effects on academic achievement and no effects on grade completion. It may be that the positive effects presented in Lakdawala et al. (2023) are due to the additional benefits of having internet access beyond just access to personal laptops. Alternatively, it may be that other factors, such as contextual differences or differences in empirical approaches, explain the contrasting findings. There is also evidence suggesting that access to personal laptops produces changes in student preferences regarding majors chosen in college (Yanguas, 2020a). However, in our setting, the fraction of students who enroll in tertiary education is only 6 percent, limiting our ability to investigate the effects on this educational decision.

With renewed interest in increasing access to computers and internet among governments in developing countries, it is important to extend the existing evidence on the short-term effects to longer-term evaluations. Though a large literature has documented that interventions in developing countries promoting the use of technology can produce positive effects on academic achievement when implemented in after-school programs with additional instructional time, the evidence is much more limited for programs integrated into the standard school curriculum. At the same time, increasing access to technological resources at schools and homes makes it crucial to understand how best to use these resources to improve educational outcomes. Looking forward, we expect future research to explore how recent advances in artificial intelligence may introduce opportunities to leverage technology in innovative ways to improve the delivery of educational services at low cost and large scale.

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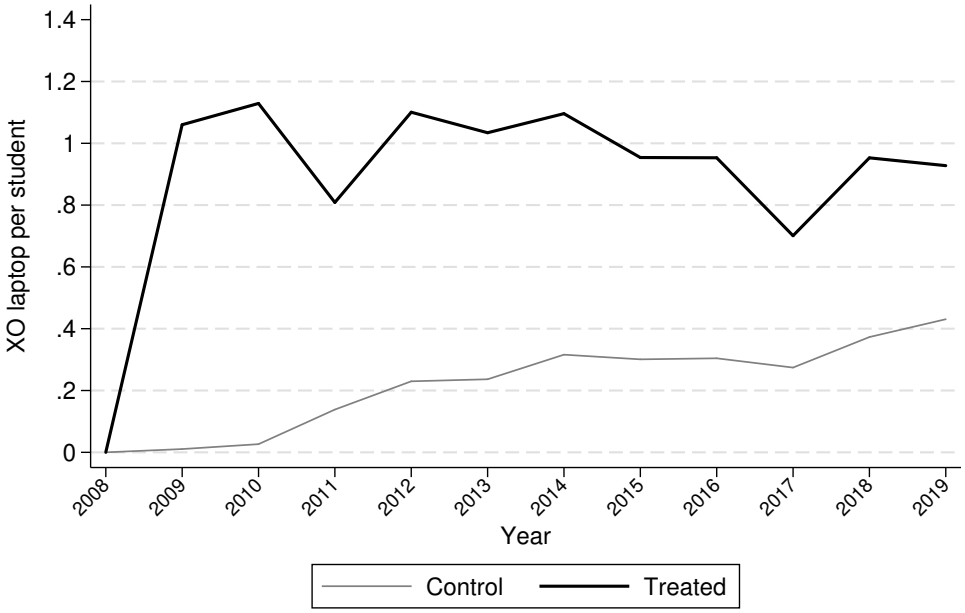
Figure 1: XO Laptops per Student over Time




*Notes*: This figure displays trends in terms of XO laptops per student at the school level according to administrative information from the Peruvian National Division of Technology in Education (2008-2010) and from the school census (2011-2019) differentiated by treatment status.

# Table 1: Baseline Balance

| | Treated mean | Control mean | Adjusted difference |
|---|---|---|---|
| | (1) | (2) | (3) |
| ***Panel A: Computers and connectivity*** | | | |
| Computers per student | 0.023 | 0.015 | 0.004 |
| | | | (0.003) |
| School has internet | 0.014 | 0.004 | 0.007 |
| | | | (0.006) |
| Observations | 296 | 235 | |
| ***Panel B: Other school characteristics*** | | | |
| Electricity | 0.882 | 0.872 | -0.001 |
| | | | (0.028) |
| Piped water | 0.490 | 0.477 | -0.011 |
| | | | (0.043) |
| Library | 0.216 | 0.183 | 0.021 |
| | | | (0.035) |
| Science lab | 0.010 | 0.009 | -0.000 |
| | | | (0.008) |
| Students per teacher | 20.539 | 20.796 | 0.492 |
| | | | (0.507) |
| Fraction of students transferred | 0.015 | 0.012 | -0.000 |
| | | | (0.002) |
| Fraction of overage students (first grade) | 0.178 | 0.159 | 0.020 |
| | | | (0.019) |
| Observations | 296 | 235 | |
| ***Panel C: Second grade national examination*** | | | |
| **2007** | | | |
| Second-grade mathematics | -0.046 | 0.000 | -0.055 |
| | | | (0.093) |
| Second-grade reading | -0.057 | 0.000 | -0.058 |
| | | | (0.091) |
| Observations | 226 | 159 | |
| **2008** | | | |
| Second-grade mathematics | -0.063 | 0.000 | -0.070 |
| | | | (0.097) |
| Second-grade reading | 0.013 | 0.000 | 0.017 |
| | | | (0.097) |
| Observations | 230 | 167 | |
| ***Panel D: School level grade progression*** | | | |
| Second grade | 0.770 | 0.779 | -0.008 |
| | | | (0.015) |
| Third grade | 0.774 | 0.789 | -0.023 |
| | | | (0.017) |
| Fourth grade | 0.847 | 0.822 | 0.024 |
| | | | (0.015) |
| Fifth grade | 0.824 | 0.811 | 0.014 |
| | | | (0.017) |
| Sixth grade | 0.874 | 0.872 | 0.002 |
| | | | (0.016) |
| Second - sixth grade | 0.814 | 0.813 | -0.002 |
| | | | (0.011) |
| Observations | 290 | 227 | |

*Notes:* This table presents baseline statistics and estimated differences between treatment and control schools. Columns 1 and 2 present means. Column 3 presents estimated coefficients and standard errors on the treatment indicator from OLS regressions that control for randomization strata fixed effects. Panels A, B, and D present school-level regressions using data from the 2008 school census. Panel C presents school-level regressions from the 2007 and 2008 national second grade examinations where individual-level performance was first standardized with zero mean and unit variance for the control group and then aggregated at the school level mean. Estimates displayed in panel C are weighted by the number of students who took the examination in each school and estimates in panel D are weighted by the enrollment in each school-grade. ***p<0.01, **p<0.05, *p<0.1.

Table 2: Effects for Schools over Time

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2009-2016 |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **Panel A: Second-grade mathematics performance** | | | | | | | | | |
| Effect | -0.053 | -0.105 | 0.022 | 0.043 | -0.079 | -0.196* | 0.079 | -0.093 | -0.044 |
| | (0.100) | (0.099) | (0.092) | (0.084) | (0.093) | (0.115) | (0.119) | (0.112) | (0.049) |
| Control mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | 360 | 335 | 329 | 380 | 335 | 299 | 254 | 220 | 2,512 |
| **Panel B: Second-grade reading performance** | | | | | | | | | |
| Effect | -0.071 | -0.123 | 0.015 | -0.004 | -0.050 | -0.204 | 0.029 | -0.050 | -0.056 |
| | (0.094) | (0.082) | (0.089) | (0.080) | (0.093) | (0.124) | (0.110) | (0.118) | (0.048) |
| Control mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | 360 | 335 | 330 | 380 | 335 | 299 | 254 | 220 | 2,513 |
| **Panel C: Grade progression** | | | | | | | | | |
| Effect | -0.009 | -0.020* | -0.021** | -0.003 | -0.007 | 0.000 | -0.014* | -0.003 | -0.010** |
| | (0.011) | (0.012) | (0.010) | (0.009) | (0.010) | (0.009) | (0.008) | (0.009) | (0.005) |
| Control mean | 0.859 | 0.825 | 0.857 | 0.853 | 0.865 | 0.893 | 0.920 | 0.912 | 0.869 |
| Observations | 530 | 531 | 531 | 528 | 531 | 529 | 529 | 526 | 4,235 |

*Notes:* Panels A and B show estimated effects from school-level regressions where individual-level performance was first standardized with zero mean and unit variance for the control group and then aggregated at the school level mean. Panel C shows estimated effects on grade progression (defined as the ratio of students promoted to the next grade with respect to those enrolled at the beginning of the school year in second to sixth grades - this ratio is capped at 1) using the yearly school census data. Each panel shows (a) the estimated coefficients and standard errors on the treatment indicator from OLS regressions that control for randomization strata fixed effects, (b) the control group means, and (c) the number of school-level observations. Regressions for individual years control for randomization strata fixed effects. Pooled regressions control for randomization strata by year fixed effects. Estimated standard errors are clustered at the primary school level. Estimates displayed in panels A and B are weighted by the number of students who took the examinations. Estimates in panel C are weighted by school (second - sixth grade) enrollment. ***p<0.01, **p<0.05, *p<0.1.

Table 3: Effects on Student Educational Trajectories

| | Control mean | Effect | Observations | Cohorts | $q$-values |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| ***Panel A: Academic achievement*** | | | | | |
| Second-grade mathematics | 0.000 | -0.044 | 22,861 | 2009-2016 | 0.688 |
| | | (0.045) | | | |
| Second-grade reading | 0.000 | -0.056 | 22,888 | 2009-2016 | 0.688 |
| | | (0.045) | | | |
| Fourth-grade mathematics | 0.000 | -0.108 | 3,207 | 2014, 2016 | 0.688 |
| | | (0.072) | | | |
| Fourth-grade reading | 0.000 | -0.133* | 3,207 | 2014, 2016 | 0.688 |
| | | (0.073) | | | |
| Eighth-grade mathematics | 0.000 | 0.026 | 6,024 | 2009-2010, 2012-2013 | 0.749 |
| | | (0.045) | | | |
| Eighth-grade reading | 0.000 | -0.010 | 6,025 | 2009-2010, 2012-2013 | 0.749 |
| | | (0.050) | | | |
| ***Panel B: Educational attainment*** | | | | | |
| Primary completion on time | 0.696 | -0.022** | 28,516 | 2009-2015 | 0.134 |
| | | (0.011) | | | |
| Primary completion with up to one year of delay | 0.862 | -0.018** | 24,939 | 2009-2014 | 0.134 |
| | | (0.009) | | | |
| Primary completion overall | 0.957 | -0.002 | 20,953 | 2009-2013 | 0.314 |
| | | (0.005) | | | |
| Secondary completion on time | 0.664 | -0.031* | 7,749 | 2009-2010 | 0.134 |
| | | (0.016) | | | |
| Secondary completion with up to one year of delay | 0.710 | -0.014 | 3,750 | 2009 | 0.292 |
| | | (0.018) | | | |
| Applied to university on time | 0.168 | -0.023* | 3,750 | 2009 | 0.134 |
| | | (0.013) | | | |
| Enrolled in university on time | 0.060 | -0.011 | 3,750 | 2009 | 0.158 |
| | | (0.008) | | | |

*Notes:* Column 1 displays control group means. Column 2 displays estimated coefficients and standard errors on the treatment indicator from OLS regressions that control for randomization strata by cohort fixed effects. Column 3 displays the number of individual-level observations in each pooled regression. Column 4 displays the cohorts included in each pooled regression where cohorts are labeled with the year in which students were enrolled in second-grade. Column 5 shows the adjusted $q$-values (where outcomes are grouped for each panel). Mathematics and reading performance have been standardized at the individual level with zero mean and unit variance for the control group. Estimated standard errors, reported in parentheses, are clustered at the primary school level. ***p<0.01, **p<0.05, *p<0.1.

# Table 4: Potential Mechanisms

| | Control mean | Effect | N | $q$-values |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ***Panel A: Teachers' digital training*** | | | | |
| Received training in XO laptop utilization | 0.280 | 0.350*** | 167 | 0.001 |
| | | (0.074) | | |
| Basic XO functionalities and operation | 0.268 | 0.270*** | 167 | 0.001 |
| | | (0.074) | | |
| General activities and software of the XO | 0.207 | 0.349*** | 167 | 0.001 |
| | | (0.071) | | |
| Learning activities using the XO | 0.134 | 0.175*** | 167 | 0.005 |
| | | (0.062) | | |
| Total training days | 1.313 | 1.135** | 168 | 0.009 |
| | | (0.457) | | |
| Total training days (conditional on receiving training) | 4.542 | -0.566 | 76 | 0.118 |
| | | (1.167) | | |
| ***Panel B: Teachers' digital skills*** | | | | |
| Self-reported digital skills | 0.000 | 0.256 | 168 | 0.269 |
| | | (0.163) | | |
| XO test | 0.000 | 0.157 | 168 | 0.269 |
| | | (0.157) | | |
| PC test | 0.000 | -0.255 | 168 | 0.269 |
| | | (0.180) | | |
| Internet test | 0.000 | -0.317* | 168 | 0.269 |
| | | (0.163) | | |
| ***Panel C: Teachers' use of computers in the classroom*** | | | | |
| Weekly hours using computers or laptops in class (typical week) | | | | |
| Total | 3.681 | 0.797 | 168 | 0.206 |
| | | (0.698) | | |
| Mathematics | 0.723 | 0.254 | 168 | 0.197 |
| | | (0.181) | | |
| Reading and Writing | 0.952 | 0.398** | 167 | 0.15 |
| | | (0.195) | | |
| ***Panel D: Students' use of computers in the school (previous week)*** | | | | |
| Used for entertainment | 0.518 | 0.139** | 2,128 | 0.058 |
| | | (0.062) | | |
| Used for academic purposes | 0.566 | 0.101 | 2,125 | 0.066 |
| | | (0.065) | | |
| ***Panel E: Students' digital skills*** | | | | |
| XO test | 0.000 | 0.405*** | 2,128 | 0.001 |
| | | (0.090) | | |
| PC test | 0.000 | 0.170* | 2,128 | 0.069 |
| | | (0.091) | | |
| Internet test | 0.000 | 0.078 | 2,128 | 0.152 |
| | | (0.091) | | |
| ***Panel F: Students' cognitive skills*** | | | | |
| Cognitive skills index | 0.000 | 0.136 | 2,134 | |
| | | (0.093) | | |
| Raven's progressive matrices | 0.000 | 0.046 | 2,128 | 0.854 |
| | | (0.082) | | |
| Verbal Fluency | 0.000 | 0.187* | 2,130 | 0.266 |
| | | (0.102) | | |
| Coding | 0.000 | 0.034 | 2,105 | 0.854 |
| | | (0.086) | | |

*Notes:* Column 1 displays control group means. Column 2 displays estimated coefficients and standard errors on the treatment indicator from OLS regressions that control for randomization strata fixed effects. Column 3 displays the number of observations in each regression. Column 4 shows the adjusted $q$-values (where outcomes are grouped for each panel). Computer utilization for entertainment includes drawing, playing, listening to music, and watching videos or movies. Computer utilization for academic purposes includes reading, writing, organizing information (e.g., conceptual frameworks), using the calculator, and doing homework. Data corresponds to the survey implemented by the team in the year 2013. Estimated standard errors, reported in parentheses, are clustered at the primary school level. ***p<0.01, **p<0.05, *p<0.1.

# Appendix: NOT FOR PUBLICATION

## Appendix A. Supplemental Tables

Table A.1: Effects on School-Level XO Computers and Internet by Year

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2017 | 2018 | 2019 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| ***Has XO*** | 0.924*** | 0.840*** | 0.182*** | 0.093*** | 0.021 | 0.008 | -0.039 | 0.040 | -0.057 | -0.043 | -0.028 |
| | (0.017) | (0.023) | (0.041) | (0.034) | (0.035) | (0.034) | (0.038) | (0.035) | (0.039) | (0.036) | (0.037) |
| Control mean | 0.009 | 0.111 | 0.549 | 0.796 | 0.824 | 0.839 | 0.779 | 0.838 | 0.839 | 0.828 | 0.809 |
| Observations | 531 | 531 | 508 | 515 | 503 | 520 | 518 | 469 | 434 | 525 | 513 |
| ***XO per student*** | 1.053*** | 1.106*** | 0.715*** | 0.911*** | 0.870*** | 0.829*** | 0.694*** | 0.771*** | 0.535*** | 0.641*** | 0.587*** |
| | (0.024) | (0.028) | (0.042) | (0.047) | (0.056) | (0.064) | (0.069) | (0.084) | (0.063) | (0.080) | (0.087) |
| Control mean | 0.011 | 0.026 | 0.145 | 0.238 | 0.250 | 0.323 | 0.306 | 0.338 | 0.334 | 0.375 | 0.438 |
| Observations | 531 | 531 | 508 | 515 | 503 | 520 | 518 | 469 | 434 | 525 | 513 |
| ***Has internet*** | 0.008 | -0.005 | 0.044* | 0.014 | 0.049** | 0.030 | 0.015 | 0.053 | 0.010 | 0.027 | 0.019 |
| | (0.005) | (0.014) | (0.025) | (0.032) | (0.022) | (0.030) | (0.029) | (0.038) | (0.035) | (0.033) | (0.035) |
| Control mean | 0.000 | 0.027 | 0.058 | 0.120 | 0.036 | 0.113 | 0.121 | 0.168 | 0.141 | 0.138 | 0.183 |
| Observations | 502 | 488 | 508 | 515 | 503 | 520 | 518 | 440 | 434 | 525 | 513 |

*Notes:* For each outcome-year, this table presents (a) the estimated coefficient and standard error on the treatment indicator from OLS regressions that control for randomization strata fixed effects, (b) the control group mean, and (c) the number of school-level observations. ***p<0.01, **p<0.05, *p<0.1

Table A.2: Effects on School-Level Desktops, Other Laptops, and Computer Lab by Year

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2017 | 2018 | 2019 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| ***Has desktop*** | 0.049 | 0.026 | 0.003 | 0.000 | 0.004 | -0.036 | 0.010 | 0.023 | 0.003 | -0.017 | 0.015 |
| | (0.044) | (0.043) | (0.044) | (0.044) | (0.042) | (0.041) | (0.043) | (0.041) | (0.041) | (0.041) | (0.040) |
| Control mean | 0.446 | 0.612 | 0.625 | 0.636 | 0.599 | 0.639 | 0.563 | 0.710 | 0.750 | 0.677 | 0.700 |
| Observations | 502 | 488 | 508 | 515 | 503 | 520 | 518 | 469 | 434 | 525 | 513 |
| ***Desktops per student*** | 0.002 | 0.019** | 0.020 | 0.010 | 0.001 | -0.015 | 0.004 | -0.002 | -0.038 | -0.007 | -0.028 |
| | (0.003) | (0.009) | (0.016) | (0.013) | (0.010) | (0.017) | (0.023) | (0.020) | (0.037) | (0.028) | (0.026) |
| Control mean | 0.022 | 0.037 | 0.054 | 0.058 | 0.062 | 0.096 | 0.091 | 0.125 | 0.181 | 0.159 | 0.193 |
| Observations | 502 | 488 | 508 | 515 | 503 | 520 | 518 | 469 | 434 | 525 | 513 |
| ***Has other laptops*** | | | -0.005 | -0.027 | -0.034* | -0.006 | -0.008 | -0.024 | -0.017 | -0.029 | -0.036 |
| | | | (0.016) | (0.022) | (0.020) | (0.023) | (0.033) | (0.035) | (0.036) | (0.030) | (0.033) |
| Control mean | | | 0.027 | 0.067 | 0.059 | 0.074 | 0.290 | 0.324 | 0.354 | 0.427 | 0.448 |
| Observations | | | 508 | 515 | 503 | 520 | 518 | 469 | 434 | 525 | 513 |
| ***Other laptops per student*** | | | 0.001 | 0.000 | 0.000 | 0.001 | -0.009 | -0.009 | 0.001 | 0.001 | 0.002 |
| | | | (0.006) | (0.006) | (0.004) | (0.002) | (0.009) | (0.007) | (0.007) | (0.008) | (0.010) |
| Control mean | | | 0.005 | 0.006 | 0.003 | 0.002 | 0.033 | 0.040 | 0.037 | 0.052 | 0.058 |
| Observations | | | 508 | 515 | 503 | 520 | 518 | 469 | 434 | 525 | 513 |
| ***Has Computer lab*** | | | | | 0.026 | 0.034 | 0.021 | -0.044 | 0.015 | -0.018 | -0.018 |
| | | | | | (0.030) | (0.035) | (0.035) | (0.032) | (0.035) | (0.027) | (0.029) |
| Control mean | | | | | 0.128 | 0.170 | 0.170 | 0.163 | 0.184 | 0.103 | 0.128 |
| Observations | | | | | 531 | 529 | 529 | 526 | 527 | 526 | 527 |
| ***Total computers per student*** | 1.055*** | 1.124*** | 0.736*** | 0.921*** | 0.871*** | 0.815*** | 0.688*** | 0.761*** | 0.499*** | 0.634*** | 0.561*** |
| | (0.024) | (0.029) | (0.044) | (0.048) | (0.059) | (0.072) | (0.079) | (0.094) | (0.072) | (0.089) | (0.097) |
| Control mean | 0.031 | 0.061 | 0.204 | 0.302 | 0.315 | 0.422 | 0.431 | 0.502 | 0.552 | 0.586 | 0.689 |
| Observations | 531 | 531 | 508 | 515 | 503 | 520 | 518 | 469 | 434 | 525 | 513 |

*Notes:* For each outcome-year, this table presents (a) the estimated coefficient and standard error on the treatment indicator from OLS regressions that control for randomization strata fixed effects, (b) the control group mean, and (c) the number of school-level observations. Blank cells denote missing information for the measured characteristic. ***p<0.01, **p<0.05, *p<0.1

Table A.3: Effects on Other School Characteristics by Year

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2017 | 2018 | 2019 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| ***Electricity*** | -0.021 | 0.035 | -0.001 | 0.012 | -0.016 | 0.004 | 0.002 | -0.001 | -0.007 | | 0.005 |
| | (0.029) | (0.024) | (0.022) | (0.019) | (0.020) | (0.015) | (0.017) | (0.014) | (0.019) | | (0.016) |
| Control mean | 0.866 | 0.909 | 0.929 | 0.942 | 0.955 | 0.965 | 0.961 | 0.970 | 0.957 | | 0.965 |
| Observations | 502 | 488 | 508 | 515 | 503 | 520 | 518 | 522 | 525 | | 513 |
| ***Piped water*** | -0.082* | -0.013 | -0.050 | 0.033 | -0.051 | -0.007 | -0.038 | -0.044 | -0.055 | -0.001 | -0.051 |
| | (0.045) | (0.044) | (0.043) | (0.043) | (0.042) | (0.042) | (0.040) | (0.039) | (0.042) | (0.044) | (0.044) |
| Control mean | 0.549 | 0.612 | 0.589 | 0.569 | 0.604 | 0.583 | 0.667 | 0.704 | 0.654 | 0.517 | 0.396 |
| Observations | 502 | 488 | 508 | 515 | 503 | 520 | 518 | 522 | 525 | 525 | 513 |
| ***Science lab*** | 0.001 | 0.008 | -0.003 | 0.003 | 0.005 | -0.002 | 0.009 | 0.004 | 0.008 | 0.016 | 0.029 |
| | (0.011) | (0.014) | (0.014) | (0.011) | (0.006) | (0.012) | (0.014) | (0.016) | (0.014) | (0.014) | (0.018) |
| Control mean | 0.009 | 0.018 | 0.022 | 0.013 | 0.005 | 0.017 | 0.017 | 0.026 | 0.021 | 0.017 | 0.022 |
| Observations | 502 | 488 | 508 | 515 | 503 | 520 | 518 | 522 | 525 | 525 | 513 |
| ***Library*** | 0.091** | -0.038 | 0.088** | 0.047 | 0.073* | 0.061 | 0.008 | 0.017 | -0.006 | -0.004 | -0.017 |
| | (0.038) | (0.044) | (0.043) | (0.032) | (0.041) | (0.042) | (0.042) | (0.033) | (0.037) | (0.039) | (0.041) |
| Control mean | 0.179 | 0.320 | 0.268 | 0.116 | 0.257 | 0.296 | 0.325 | 0.142 | 0.209 | 0.263 | 0.274 |
| Observations | 502 | 488 | 508 | 515 | 503 | 520 | 518 | 522 | 525 | 525 | 513 |
| ***Students per teacher*** | 0.009 | -0.015 | 0.067 | -1.042* | 0.014 | -0.428 | 0.230 | 0.438 | 0.296 | -0.058 | 0.200 |
| | (0.479) | (0.474) | (0.461) | (0.613) | (0.431) | (0.415) | (0.422) | (0.355) | (0.615) | (0.334) | (0.329) |
| Control mean | 19.849 | 18.756 | 15.954 | 15.755 | 13.676 | 13.174 | 12.293 | 11.164 | 14.016 | 10.531 | 10.121 |
| Observations | 531 | 531 | 531 | 528 | 531 | 529 | 529 | 526 | 501 | 526 | 526 |
| ***Fraction of students transferred*** | 0.001 | -0.007 | -0.009 | 0.002 | 0.001 | 0.003 | 0.000 | -0.000 | -0.006 | -0.004 | -0.004 |
| | (0.005) | (0.005) | (0.014) | (0.004) | (0.005) | (0.006) | (0.008) | (0.005) | (0.005) | (0.006) | (0.010) |
| Control mean | 0.022 | 0.029 | 0.056 | 0.024 | 0.021 | 0.022 | 0.032 | 0.031 | 0.039 | 0.041 | 0.053 |
| Observations | 531 | 531 | 531 | 528 | 531 | 529 | 529 | 526 | 527 | 526 | 527 |
| ***School enrollment*** | 0.813 | -0.125 | -1.194 | 0.002 | 0.053 | -0.183 | -0.029 | 0.306 | 0.492 | 0.511 | 1.438 |
| | (1.988) | (2.043) | (2.107) | (2.126) | (2.165) | (2.207) | (2.341) | (2.285) | (2.452) | (2.489) | (2.514) |
| Control mean | 61.336 | 59.621 | 53.749 | 48.180 | 45.179 | 41.549 | 39.417 | 37.730 | 35.769 | 34.442 | 33.419 |
| Observations | 531 | 531 | 531 | 528 | 531 | 529 | 529 | 526 | 527 | 526 | 527 |

*Notes:* For each outcome-year, this table presents (a) the estimated coefficient and standard error on the treatment indicator from OLS regressions that control for randomization strata fixed effects, (b) the control group mean, and (c) the number of school-level observations. Blank cells denote missing information for the measured characteristic. ***p<0.01, **p<0.05, *p<0.1

Table A.4: Effects on Technology Access, Academic Achievement, and IT Support

| | Control Mean | Effect | N | Respondent |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ***Panel A: Technology access*** | | | | |
| *Computers* | | | | |
| Student has own XO laptop in school | 0.291 | 0.448*** | 2,129 | student |
| | | (0.046) | | |
| Teacher has own XO laptop in school | 0.133 | 0.321*** | 167 | teacher |
| | | (0.069) | | |
| Student takes the XO laptop home | 0.019 | 0.194*** | 2,142 | student |
| | | (0.036) | | |
| Student has non-XO laptop or computer at home | 0.100 | 0.018 | 2,137 | student |
| | | (0.017) | | |
| *Internet* | | | | |
| School has internet | 0.057 | 0.057 | 140 | principal |
| | | (0.047) | | |
| Student has internet at home | 0.026 | -0.002 | 2,127 | student |
| | | (0.007) | | |
| ***Panel B: Students' academic achievement*** | | | | |
| Mathematics | 0.000 | 0.061 | 2,123 | student |
| | | (0.095) | | |
| Reading | 0.000 | 0.040 | 2,144 | student |
| | | (0.086) | | |
| ***Panel C: IT support for teachers*** | | | | |
| Received visit from IT specialist | 0.096 | 0.102* | 168 | teacher |
| | | (0.054) | | |
| Training in computer or laptop utilization | 0.060 | -0.007 | 168 | teacher |
| | | (0.035) | | |
| Advise on use (after being trained) | 0.048 | -0.021 | 168 | teacher |
| | | (0.029) | | |
| IT support (repair or maintenance) | 0.036 | 0.085** | 168 | teacher |
| | | (0.040) | | |

*Notes:* Column 1 displays control group means. Column 2 displays estimated coefficients and standard errors on the treatment indicator from OLS regressions that control for randomization strata fixed effects. Column 3 displays the number of observations in each regression. Column 4 displays the respondent for each outcome analyzed. Data corresponds to the survey implemented by the team in the year 2013. Estimated standard errors, reported in parentheses, are clustered at the primary school level. ***p<0.01, **p<0.05, *p<0.1.

Table A.5: Effects on Alternative Definitions of School-Level Grade Progression by Year

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **Panel A: Denominator is enrollment at the beginning of the academic year (ratio is capped at 1)** | | | | | | | | | |
| Effect | -0.009 | -0.020* | -0.021** | -0.003 | -0.007 | 0.000 | -0.014* | -0.003 | -0.010** |
| | (0.011) | (0.012) | (0.010) | (0.009) | (0.010) | (0.009) | (0.008) | (0.009) | (0.005) |
| Control mean | 0.859 | 0.825 | 0.857 | 0.853 | 0.865 | 0.893 | 0.920 | 0.912 | 0.869 |
| Observations | 530 | 531 | 531 | 528 | 531 | 529 | 529 | 526 | 4,235 |
| **Panel B: Denominator is enrollment at the beginning of the academic year (ratio is not capped)** | | | | | | | | | |
| Effect | -0.021 | -0.022* | -0.021** | -0.009 | -0.006 | 0.003 | -0.013 | -0.006 | -0.013** |
| | (0.018) | (0.012) | (0.010) | (0.011) | (0.011) | (0.010) | (0.009) | (0.010) | (0.006) |
| Control mean | 0.889 | 0.828 | 0.857 | 0.865 | 0.866 | 0.898 | 0.927 | 0.918 | 0.877 |
| Observations | 530 | 531 | 531 | 528 | 531 | 529 | 529 | 525 | 4,234 |
| **Panel C: Denominator is enrollment at the beginning of the academic year (missing if ratio >1)** | | | | | | | | | |
| Effect | 0.001 | -0.018 | -0.023** | -0.000 | -0.008 | 0.002 | -0.010 | -0.002 | -0.008 |
| | (0.011) | (0.012) | (0.010) | (0.009) | (0.010) | (0.009) | (0.008) | (0.008) | (0.005) |
| Control mean | 0.832 | 0.820 | 0.856 | 0.848 | 0.862 | 0.884 | 0.913 | 0.904 | 0.860 |
| Observations | 465 | 520 | 519 | 504 | 514 | 484 | 485 | 488 | 3,979 |
| **Panel D: Denominator is students in the end-of-year report (Promoted + Failed + Dropped Out + Transferred)** | | | | | | | | | |
| Effect | 0.006 | -0.026** | -0.019* | -0.006 | -0.007 | 0.000 | -0.012 | -0.001 | -0.008* |
| | (0.011) | (0.011) | (0.010) | (0.008) | (0.010) | (0.009) | (0.008) | (0.009) | (0.005) |
| Control mean | 0.851 | 0.820 | 0.862 | 0.853 | 0.863 | 0.880 | 0.906 | 0.897 | 0.862 |
| Observations | 530 | 531 | 531 | 528 | 531 | 529 | 529 | 525 | 4,234 |
| **Panel E: Denominator is the maximum between enrollment and end-of-year report** | | | | | | | | | |
| Effect | 0.002 | -0.020* | -0.021** | -0.001 | -0.009 | -0.001 | -0.011 | -0.004 | -0.008 |
| | (0.012) | (0.012) | (0.010) | (0.009) | (0.010) | (0.009) | (0.008) | (0.009) | (0.005) |
| Control mean | 0.827 | 0.807 | 0.848 | 0.836 | 0.850 | 0.876 | 0.903 | 0.894 | 0.850 |
| Observations | 530 | 531 | 531 | 528 | 531 | 529 | 529 | 525 | 4,234 |

*Notes:* This table shows estimated effects on grade progression using alternative definitions. In all panels, the numerator of the grade progression ratio is the number of students promoted to the next grade at the end of the academic year who attended second to sixth grades. The different panels present alternative denominators and censoring approaches. Each panel shows (a) the estimated coefficients and standard errors on the treatment indicator from OLS regressions that control for randomization strata fixed effects, (b) the control group means, and (c) the number of observations. Regressions for individual years control for randomization strata fixed effects. Pooled regressions control for randomization strata by year fixed effects. Estimated standard errors are clustered at the primary school. Estimates are weighted by school (second - sixth grade) enrollment. ***p<0.01, **p<0.05, *p<0.1.

Table A.6: Effects on School-Level Grade Progression by Grade and Year

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **Second grade** | | | | | | | | | |
| Effect | 0.011 | -0.013 | -0.017 | 0.010 | -0.013 | 0.009 | 0.001 | 0.022 | 0.001 |
| | (0.016) | (0.017) | (0.015) | (0.017) | (0.018) | (0.016) | (0.016) | (0.016) | (0.007) |
| Control mean | 0.795 | 0.751 | 0.805 | 0.778 | 0.808 | 0.842 | 0.877 | 0.854 | 0.807 |
| Observations | 526 | 531 | 529 | 517 | 520 | 516 | 512 | 502 | 4,153 |
| **Third grade** | | | | | | | | | |
| Effect | -0.017 | -0.043** | -0.034** | -0.012 | -0.010 | 0.018 | -0.025* | -0.020 | -0.018*** |
| | (0.015) | (0.018) | (0.017) | (0.016) | (0.017) | (0.017) | (0.014) | (0.015) | (0.007) |
| Control mean | 0.827 | 0.795 | 0.810 | 0.818 | 0.830 | 0.848 | 0.902 | 0.890 | 0.834 |
| Observations | 530 | 528 | 529 | 524 | 522 | 510 | 513 | 503 | 4,159 |
| **Fourth grade** | | | | | | | | | |
| Effect | -0.017 | -0.029* | -0.032** | -0.014 | -0.005 | -0.010 | -0.016 | -0.015 | -0.018*** |
| | (0.015) | (0.017) | (0.015) | (0.014) | (0.018) | (0.014) | (0.013) | (0.011) | (0.006) |
| Control mean | 0.871 | 0.839 | 0.877 | 0.871 | 0.869 | 0.900 | 0.914 | 0.926 | 0.879 |
| Observations | 530 | 531 | 528 | 521 | 524 | 521 | 513 | 509 | 4,177 |
| **Fifth grade** | | | | | | | | | |
| Effect | -0.002 | -0.008 | -0.024 | 0.015 | 0.008 | -0.008 | -0.027* | -0.012 | -0.006 |
| | (0.017) | (0.018) | (0.015) | (0.017) | (0.017) | (0.015) | (0.014) | (0.015) | (0.007) |
| Control mean | 0.849 | 0.828 | 0.863 | 0.848 | 0.852 | 0.899 | 0.924 | 0.913 | 0.868 |
| Observations | 526 | 529 | 526 | 517 | 520 | 516 | 516 | 505 | 4,155 |
| **Sixth grade** | | | | | | | | | |
| Effect | -0.012 | -0.012 | 0.006 | 0.007 | -0.027** | 0.006 | 0.004 | 0.001 | -0.004 |
| | (0.016) | (0.017) | (0.013) | (0.015) | (0.012) | (0.012) | (0.009) | (0.009) | (0.006) |
| Control mean | 0.895 | 0.897 | 0.912 | 0.904 | 0.941 | 0.941 | 0.953 | 0.950 | 0.922 |
| Observations | 526 | 526 | 527 | 520 | 520 | 515 | 512 | 509 | 4,155 |
| **Second - sixth grade** | | | | | | | | | |
| Effect | -0.009 | -0.020* | -0.021** | -0.003 | -0.007 | 0.000 | -0.014* | -0.003 | -0.010** |
| | (0.011) | (0.012) | (0.010) | (0.009) | (0.010) | (0.009) | (0.008) | (0.009) | (0.005) |
| Control mean | 0.859 | 0.825 | 0.857 | 0.853 | 0.865 | 0.893 | 0.920 | 0.912 | 0.869 |
| Observations | 530 | 531 | 531 | 528 | 531 | 529 | 529 | 526 | 4,235 |

*Notes:* This table shows estimated effects on grade progression for each grade. In all panels, the numerator of the grade progression ratio is the number of students promoted to the next grade at the end of the academic year; while the denominator is the number of students enrolled in each grade at the beginning of the academic year. When the ratio exceeded unity, it was capped at 1. Each panel shows (a) the estimated coefficients and standard errors on the treatment indicator from OLS regressions that control for randomization strata fixed effects, (b) the control group means, and (c) the number of observations. Regressions for individual years control for randomization strata fixed effects. Pooled regressions control for randomization strata by year fixed effects. Estimated standard errors are clustered at the primary school. Estimates are weighted by school enrollment. ***p<0.01, **p<0.05, *p<0.1.

Table A.7: Matching Rates Across National Examinations by Cohort

| | 2009 | 2010 | 2012 | 2013 | 2014 | 2016 | Pooled |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Matched with fourth-grade mathematics** | | | | | | | |
| Effect | | | | | 0.013 | -0.023 | -0.003 |
| | | | | | (0.038) | (0.032) | (0.027) |
| Control mean | | | | | 0.606 | 0.787 | 0.690 |
| Observations | | | | | 2,428 | 2,192 | 4,620 |
| **Matched with fourth-grade reading** | | | | | | | |
| Effect | | | | | 0.014 | -0.021 | -0.002 |
| | | | | | (0.038) | (0.032) | (0.027) |
| Control mean | | | | | 0.606 | 0.786 | 0.689 |
| Observations | | | | | 2,428 | 2,192 | 4,620 |
| **Matched with eighth-grade mathematics** | | | | | | | |
| Effect | 0.008 | -0.038 | -0.033 | -0.012 | | | -0.019 |
| | (0.025) | (0.024) | (0.024) | (0.023) | | | (0.016) |
| Control mean | 0.380 | 0.442 | 0.553 | 0.565 | | | 0.484 |
| Observations | 3,316 | 3,229 | 3,311 | 2,876 | | | 12,732 |
| **Matched with eighth-grade reading** | | | | | | | |
| Effect | 0.008 | -0.039 | -0.032 | -0.013 | | | -0.019 |
| | (0.025) | (0.024) | (0.024) | (0.022) | | | (0.016) |
| Control mean | 0.380 | 0.442 | 0.552 | 0.566 | | | 0.484 |
| Observations | 3,316 | 3,229 | 3,311 | 2,876 | | | 12,732 |

*Notes:* This table shows estimated effects on individual-level matching rates across national examinations by cohort. The national second-grade examination is taken as the base to compute the individual-level match indicator with fourth and eight-grade national exams. Cohorts (displayed in each column) indicate the year when students were enrolled in second-grade. All estimations include strata fixed effects and estimated standard errors are clustered at the primary school level. ***p<0.01, **p<0.05, *p<0.1.

Table A.8: Effects on Individual-Level Academic Achievement by Cohort

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **Second-grade mathematics** | | | | | | | | | |
| Effect | -0.053 | -0.105 | 0.022 | 0.043 | -0.079 | -0.196* | 0.079 | -0.093 | -0.044 |
| | (0.093) | (0.092) | (0.086) | (0.079) | (0.087) | (0.107) | (0.109) | (0.103) | (0.045) |
| Control mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | 3,314 | 3,221 | 3,161 | 3,308 | 2,872 | 2,427 | 2,368 | 2,190 | 22,861 |
| **Second-grade reading** | | | | | | | | | |
| Effect | -0.071 | -0.123 | 0.015 | -0.004 | -0.050 | -0.204* | 0.029 | -0.050 | -0.056 |
| | (0.088) | (0.076) | (0.083) | (0.075) | (0.086) | (0.115) | (0.101) | (0.108) | (0.045) |
| Control mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | 3,312 | 3,228 | 3,176 | 3,307 | 2,876 | 2,428 | 2,370 | 2,191 | 22,888 |
| **Fourth-grade mathematics** | | | | | | | | | |
| Effect | | | | | | -0.182 | | -0.040 | -0.108 |
| | | | | | | (0.115) | | (0.090) | (0.072) |
| Control mean | | | | | | 0.000 | | 0.000 | 0.000 |
| Observations | | | | | | 1,524 | | 1,683 | 3,207 |
| **Fourth-grade reading** | | | | | | | | | |
| Effect | | | | | | -0.218* | | -0.056 | -0.133* |
| | | | | | | (0.116) | | (0.084) | (0.073) |
| Control mean | | | | | | 0.000 | | 0.000 | 0.000 |
| Observations | | | | | | 1,525 | | 1,682 | 3,207 |
| **Eighth-grade mathematics** | | | | | | | | | |
| Effect | 0.066 | -0.006 | | 0.063 | -0.019 | | | | 0.026 |
| | (0.093) | (0.071) | | (0.059) | (0.067) | | | | (0.045) |
| Control mean | 0.000 | 0.000 | | 0.000 | 0.000 | | | | 0.000 |
| Observations | 1,281 | 1,352 | | 1,769 | 1,622 | | | | 6,024 |
| **Eighth-grade reading** | | | | | | | | | |
| Effect | -0.101 | -0.042 | | 0.102 | -0.035 | | | | -0.010 |
| | (0.080) | (0.075) | | (0.064) | (0.067) | | | | (0.050) |
| Control mean | 0.000 | 0.000 | | 0.000 | 0.000 | | | | 0.000 |
| Observations | 1,281 | 1,353 | | 1,768 | 1,623 | | | | 6,025 |

*Notes:* This table shows estimated effects on primary education outcomes for each of the cohorts that were pooled in the estimations shown in Table 3. Cohorts (displayed in each column) indicate the year when students were enrolled in second-grade. All estimations include strata fixed effects and estimated standard errors are clustered at the primary school level. Mathematics and reading exams have been standardized with zero mean and unit variance for the control group. ***p<0.01, **p<0.05, *p<0.1.

Table A.9: Effects on Student Educational Trajectories by Caregiver's Educational Attainment

| | Control mean | Incomplete Primary or less | Complete Primary or more | *p*-value (2) = (3) | N | Cohorts |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ***Panel A: Academic achievement*** | | | | | | |
| Second-grade mathematics | 0.000 | -0.071 | -0.014 | 0.231 | 11,319 | 2012-2016 |
| | | (0.068) | (0.061) | | | |
| Second-grade reading | 0.000 | -0.076 | -0.013 | 0.188 | 11,323 | 2012-2016 |
| | | (0.068) | (0.061) | | | |
| Fourth-grade mathematics | 0.000 | -0.461*** | -0.129 | 0.005 | 1,386 | 2014 |
| | | (0.149) | (0.125) | | | |
| Fourth-grade reading | 0.000 | -0.522*** | -0.150 | 0.000 | 1,386 | 2014 |
| | | (0.127) | (0.131) | | | |
| Eighth-grade mathematics | 0.000 | 0.084 | -0.059 | 0.058 | 3,873 | 2009-2010, 2013 |
| | | (0.079) | (0.056) | | | |
| Eighth-grade reading | 0.000 | 0.023 | -0.114* | 0.054 | 3,873 | 2009-2010, 2013 |
| | | (0.072) | (0.059) | | | |
| | | (0.055) | (0.057) | | | |
| ***Panel B: Educational attainment*** | | | | | | |
| Primary completion on time | 0.705 | -0.039*** | -0.014 | 0.063 | 27,993 | 2009-2015 |
| | | (0.014) | (0.011) | | | |
| Primary completion with up to one year of delay | 0.877 | -0.034*** | -0.014 | 0.098 | 24,426 | 2009-2014 |
| | | (0.012) | (0.009) | | | |
| Primary completion overall | 0.975 | -0.009 | -0.004 | 0.465 | 20,481 | 2009-2013 |
| | | (0.006) | (0.004) | | | |
| Secondary completion on time | 0.686 | -0.049** | -0.032* | 0.480 | 7,519 | 2009-2010 |
| | | (0.021) | (0.018) | | | |
| Secondary completion with up to one year of delay | 0.736 | -0.010 | -0.021 | 0.725 | 3,618 | 2009 |
| | | (0.026) | (0.021) | | | |
| Applied to university on time | 0.173 | -0.025 | -0.017 | 0.721 | 3,618 | 2009 |
| | | (0.016) | (0.019) | | | |
| Enrolled to university on time | 0.060 | 0.000 | -0.013 | 0.350 | 3,618 | 2009 |
| | | (0.009) | (0.011) | | | |

*Notes:* Column 1 displays control group means. Column 2 displays estimated coefficients and standard errors on the treatment indicator for student's with caregivers who had not completed primary education from OLS regressions that control for randomization strata by cohort fixed effects. Column 3 displays estimated coefficients and standard errors on the treatment indicator for student's with caregivers who have completed primary education from OLS regressions that control for randomization strata by cohort fixed effects. Column 4 displays the *p*-values for the null of equality of effects between columns (2) and (3). Column 5 displays the number of individual-level observations in each pooled regression. Column 6 displays the cohorts included in each pooled regression where cohorts are labeled with the year in which students were enrolled in second grade. As information on caregiver's educational attainment was not available for years 2009-2011, effects on second-grade examinations are only estimated for cohorts 2012-2016. In addition, caregiver's educational attainment was not captured in year 2018. This corresponds to the year in which the 2016 cohort wrote the fourth-grade exam and in which the 2012 cohort wrote the eight-grade exam. Therefore, these cohorts are not included in the analysis for these outcomes. Estimated standard errors, reported in parentheses, are clustered at the primary school level. ***p<0.01, **p<0.05, *p<0.1.

Table A.10: Effects on Individual-Level Educational Attainment by Cohort

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | Pooled |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Primary completion on time** | | | | | | | | |
| Effect | -0.006 | -0.030* | -0.007 | -0.045*** | -0.020 | -0.025 | -0.015 | -0.022** |
| | (0.018) | (0.018) | (0.018) | (0.017) | (0.017) | (0.017) | (0.015) | (0.011) |
| Control mean | 0.605 | 0.632 | 0.663 | 0.693 | 0.735 | 0.763 | 0.790 | 0.696 |
| Observations | 3,750 | 3,999 | 4,342 | 4,494 | 4,368 | 3,986 | 3,577 | 28,516 |
| **Primary completion with up to one year of delay** | | | | | | | | |
| Effect | -0.013 | -0.035** | -0.015 | -0.023* | -0.012 | -0.011 | | -0.018** |
| | (0.016) | (0.014) | (0.014) | (0.013) | (0.011) | (0.013) | | (0.009) |
| Control mean | 0.814 | 0.830 | 0.859 | 0.862 | 0.894 | 0.910 | | 0.862 |
| Observations | 3,750 | 3,999 | 4,342 | 4,494 | 4,368 | 3,986 | | 24,939 |
| **Primary completion overall** | | | | | | | | |
| Effect | -0.006 | 0.002 | 0.002 | -0.006 | -0.001 | | | -0.002 |
| | (0.006) | (0.008) | (0.008) | (0.009) | (0.009) | | | (0.005) |
| Control mean | 0.969 | 0.955 | 0.953 | 0.955 | 0.954 | | | 0.957 |
| Observations | 3,750 | 3,999 | 4,342 | 4,494 | 4,368 | | | 20,953 |
| **Secondary completion on time** | | | | | | | | |
| Effect | -0.024 | -0.039** | | | | | | -0.031* |
| | (0.019) | (0.019) | | | | | | (0.016) |
| Control mean | 0.657 | 0.670 | | | | | | 0.664 |
| Observations | 3,750 | 3,999 | | | | | | 7,749 |
| **Secondary completion with up to one year of delay** | | | | | | | | |
| Effect | -0.014 | | | | | | | -0.014 |
| | (0.018) | | | | | | | (0.018) |
| Control mean | 0.710 | | | | | | | 0.710 |
| Observations | 3,750 | | | | | | | 3,750 |
| **Applied to university on time** | | | | | | | | |
| Effect | -0.023* | | | | | | | -0.023* |
| | (0.013) | | | | | | | (0.013) |
| Control mean | 0.168 | | | | | | | 0.168 |
| Observations | 3,750 | | | | | | | 3,750 |
| **Enrolled to university on time** | | | | | | | | |
| Effect | -0.011 | | | | | | | -0.011 |
| | (0.008) | | | | | | | (0.008) |
| Control mean | 0.060 | | | | | | | 0.060 |
| Observations | 3,750 | | | | | | | 3,750 |

*Notes:* This table shows estimated effects on primary education outcomes for each of the cohorts that were pooled in the estimations shown in Table 3. Cohorts (displayed in each column) indicate the year when students were enrolled in second grade. All estimations include strata fixed effects and estimated standard errors are clustered at the primary school level. Mathematics and reading exams have been standardized with zero mean and unit variance for the control group. ***p<0.01, **p<0.05, *p<0.1.

## Table A.11: Effects on Overage at Entry (First Grade)

| Census year: | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | Pooled |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **Fraction of overage students** | | | | | | | | | |
| Effect | -0.001 | 0.020 | 0.025 | 0.017 | 0.007 | 0.017 | -0.007 | 0.004 | 0.010 |
| | (0.018) | (0.019) | (0.020) | (0.020) | (0.026) | (0.025) | (0.024) | (0.015) | (0.009) |
| Control mean | 0.174 | 0.156 | 0.139 | 0.135 | 0.194 | 0.233 | 0.107 | 0.050 | 0.149 |
| Observations | 530 | 529 | 522 | 513 | 513 | 511 | 498 | 500 | 4,116 |

*Notes:* This table shows estimated effects on overage enrollment in first grade by year. Overage is the fraction of first grade students enrolled at 7 years of age or more. The school census collected enrollment by age with a cutoff date of June 30th up to the year 2014. Starting in 2015, the census varied the cutoff date to March 31st. Regressions for individual years control for randomization strata fixed effects. Pooled regressions control for randomization strata by year fixed effects. Estimated standard errors are clustered at the primary school. ***p<0.01, **p<0.05, *p<0.1.

## Table A.12: Effects on Student Educational Trajectories by Gender

| | Control mean | Males | Females | *p*-value (2) = (3) | N | Cohorts |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ***Panel A: Academic achievement*** | | | | | | |
| Second-grade mathematics | 0.000 | -0.041 | -0.047 | 0.825 | 22,861 | 2009-2016 |
| | | (0.047) | (0.047) | | | |
| Second-grade reading | 0.000 | -0.051 | -0.061 | 0.740 | 22,888 | 2009-2016 |
| | | (0.046) | (0.048) | | | |
| Fourth-grade mathematics | 0.000 | -0.069 | -0.141* | 0.338 | 3,207 | 2014, 2016 |
| | | (0.079) | (0.083) | | | |
| Fourth-grade reading | 0.000 | -0.132 | -0.134 | 0.984 | 3,207 | 2014, 2016 |
| | | (0.081) | (0.082) | | | |
| Eighth-grade mathematics | 0.000 | 0.059 | -0.004 | 0.194 | 6,024 | 2009-2010, 2012-2013 |
| | | (0.052) | (0.050) | | | |
| Eighth-grade reading | 0.000 | 0.042 | -0.064 | 0.044 | 6,025 | 2009-2010, 2012-2013 |
| | | (0.055) | (0.057) | | | |
| ***Panel B: Educational attainment*** | | | | | | |
| Primary completion on time | 0.696 | -0.023* | -0.020 | 0.824 | 28,516 | 2009-2015 |
| | | (0.012) | (0.013) | | | |
| Primary completion with up to one year of delay | 0.862 | -0.023** | -0.013 | 0.318 | 24,939 | 2009-2014 |
| | | (0.010) | (0.011) | | | |
| Primary completion overall | 0.957 | -0.001 | -0.003 | 0.828 | 20,953 | 2009-2013 |
| | | (0.006) | (0.006) | | | |
| Secondary completion on time | 0.664 | -0.027 | -0.036* | 0.723 | 7,749 | 2009-2010 |
| | | (0.019) | (0.021) | | | |
| Secondary completion with up to one year of delay | 0.710 | -0.006 | -0.020 | 0.678 | 3,750 | 2009 |
| | | (0.023) | (0.025) | | | |
| Applied to university on time | 0.168 | -0.004 | -0.045** | 0.083 | 3,750 | 2009 |
| | | (0.017) | (0.018) | | | |
| Enrolled to university on time | 0.060 | -0.007 | -0.015 | 0.578 | 3,750 | 2009 |
| | | (0.010) | (0.012) | | | |

*Notes:* Column 1 displays control group means. Column 2 displays estimated coefficients and standard errors on the treatment indicator for males from OLS regressions that control for randomization strata by cohort fixed effects. Column 3 displays estimated coefficients and standard errors on the treatment indicator for females from OLS regressions that control for randomization strata by cohort fixed effects. Column 4 displays the *p*-values for the null of equality of effects between males and females. Column 5 displays the number of individual-level observations in each pooled regression. Column 6 displays the cohorts included in each pooled regression where cohorts are labeled with the year in which students were enrolled in second grade. Estimated standard errors, reported in parentheses, are clustered at the primary school level. ***p<0.01, **p<0.05, *p<0.1.

Table A.13: Effects on Students by Socio-Economic Status

| | Control mean | Below median SES | Above median SES | $p$-value (2) = (3) | N |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| ***Panel A: Technology Access*** | | | | | |
| Student has own XO laptop in school | 0.291 | 0.492*** | 0.419*** | 0.157 | 2,129 |
| | | (0.057) | (0.050) | | |
| Student takes the XO laptop home | 0.019 | 0.185*** | 0.198*** | 0.689 | 2,142 |
| | | (0.040) | (0.039) | | |
| Student has non-XO laptop or computer at home | 0.100 | 0.010 | 0.015 | 0.851 | 2,137 |
| | | (0.013) | (0.023) | | |
| Student has internet at home | 0.026 | -0.003 | -0.004 | 0.929 | 2,127 |
| | | (0.004) | (0.010) | | |
| ***Panel B: Students' use of computers in the school (previous week)*** | | | | | |
| Used for entertainment | 0.518 | 0.234*** | 0.079 | 0.012 | 2,128 |
| | | (0.073) | (0.065) | | |
| Used for academic purposes | 0.566 | 0.160* | 0.063 | 0.140 | 2,125 |
| | | (0.082) | (0.065) | | |
| ***Panel C: Students' digital skills*** | | | | | |
| XO test | 0.000 | 0.454*** | 0.371*** | 0.464 | 2,103 |
| | | (0.121) | (0.094) | | |
| PC test | 0.000 | 0.300** | 0.083 | 0.022 | 2,103 |
| | | (0.115) | (0.090) | | |
| Internet test | 0.000 | 0.162 | 0.026 | 0.123 | 2,103 |
| | | (0.106) | (0.094) | | |
| ***Panel D: Students' cognitive skills*** | | | | | |
| Cognitive skills index | 0.000 | 0.142 | 0.120 | 0.852 | 2,107 |
| | | (0.110) | (0.107) | | |
| Raven's progressive matrices | 0.000 | 0.005 | 0.060 | 0.595 | 2,101 |
| | | (0.099) | (0.091) | | |
| Verbal Fluency | 0.000 | 0.241* | 0.158 | 0.540 | 2,106 |
| | | (0.129) | (0.117) | | |
| Coding | 0.000 | 0.036 | 0.025 | 0.904 | 2,085 |
| | | (0.107) | (0.091) | | |
| ***Panel E Students' academic achievement*** | | | | | |
| Mathematics | 0.000 | 0.107 | 0.009 | 0.399 | 2,092 |
| | | (0.126) | (0.096) | | |
| Reading | 0.000 | 0.013 | 0.029 | 0.887 | 2,125 |
| | | (0.105) | (0.094) | | |

*Notes:* Column 1 displays control group means. Column 2 displays estimated coefficients and standard errors on the treatment indicator for student with below median socio-economic status (SES) from OLS regressions that control for randomization strata by cohort fixed effects. Column 3 displays estimated coefficients and standard errors on the treatment indicator for student with above median SES from OLS regressions that control for randomization strata by cohort fixed effects. Column 4 displays the $p$-values for the null of equality of effects between columns (2) and (3). Column 5 displays the number of observations in each regression. Socio-economic status was calculated with an equally weighted average index of household assets and services (TV, radio, gas or electric stove, electric iron, cellphone, wheelbarrow, electricity, piped water, sewer, fixed line phone, internet, non-XO computer or laptop, video games). In this index, we excluded assets associated with agricultural work (i.e., wood stove, plow, lamp, rake, or axe) as they might be correlated negatively with SES. However, results are consistent when including them in the SES index. Data corresponds to the survey implemented by the team in the year 2013. Estimated standard errors, reported in parentheses, are clustered at the primary school level. ***p<0.01, **p<0.05, *p<0.1.

# Appendix B. Robustness Checks: Effects for Schools over Time

## Potential Compositional Changes of Schools Covered in the Second Grade National Examinations

The second grade national exam is intended to cover all schools with at least five students enrolled in second grade. Because of this, not every school in our sample was covered across all years (Appendix Table B.1). Out of the 531 schools in the sample, the post-treatment year with the highest coverage was 2012 (with 380 schools covered), and the year with the lowest coverage was 2016 (with 220 schools covered). Conditional on being included in the examination, the average number of test takers per school varied between 8 and 10. Appendix Table B.1 shows that the OLPC treatment did not affect the likelihood of being covered or, conditional on being covered, the number of test takers. Nonetheless, to rule out potential compositional changes over time, we perform baseline balance tests for some key characteristics of Table 1 considering only the set of schools covered in the examination every year. Results in Appendix Table B.2 show both no baseline differences between treated and control and stable baseline compositions of schools covered over time (as the control group means are largely stable).

## Reduced Coverage of Schools from 2014 Onwards

Up to the year 2013, the coverage rate of our experimental schools oscillated around 60-70 percent. However, starting in 2014, coverage rates started to decrease significantly (Appendix Table B.1). Given this, Appendix Table B.3 shows that our pooled results remain qualitatively unchanged if we either exclude years 2014-2016 or 2015-2016, years which have relatively lower exam coverage.

## Unweighted Effects and Heterogeneity by School Size

We also estimate unweighted regressions. Results in Appendix Table B.4 show similar effects on grade progression when compared to the main weighted estimates (negative effects of 1.3 vs 1 percentage points). However, negative effects on performance are larger in magnitude and acquire significance on the pooled estimate for reading. The absence of effects for the weighted regression coupled with stronger unweighted effects suggest possible heterogeneous effects by school size. Therefore, we assess differential effects for schools below and above the median baseline enrollment within our sample. Results are shown in Appendix Table B.5. While the pooled estimates suggest relatively stronger negative effects among smaller schools, we are unable to reject the null of equality of effects, and the yearly estimates suggest that this pattern is heavily driven by the first two years of the program (2009 and 2010).

Table B.1: School-Level Coverage in Second Grade National Examinations

| | 2007 | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| **School is covered in second grade national examination** | | | | | | | | | | |
| Effect | 0.029 | 0.017 | 0.053 | 0.027 | 0.013 | 0.000 | -0.045 | 0.016 | 0.032 | 0.008 |
| | (0.031) | (0.035) | (0.038) | (0.040) | (0.042) | (0.038) | (0.038) | (0.040) | (0.041) | (0.036) |
| Control mean | 0.677 | 0.711 | 0.613 | 0.587 | 0.604 | 0.706 | 0.647 | 0.545 | 0.455 | 0.387 |
| Observations | 531 | 531 | 531 | 531 | 531 | 531 | 531 | 531 | 531 | 531 |
| **Number of test takers (among schools included in the national examinations)** | | | | | | | | | | |
| Effect | -0.034 | 0.225 | -0.535 | -0.125 | -0.484 | -0.151 | 0.473 | -0.152 | -0.553 | 0.302 |
| | (0.492) | (0.410) | (0.532) | (0.577) | (0.584) | (0.520) | (0.599) | (0.633) | (0.791) | (1.015) |
| Cont. Mean | 10.044 | 9.174 | 9.611 | 9.761 | 9.641 | 8.669 | 8.184 | 8.172 | 9.355 | 9.901 |
| Observations | 385 | 397 | 360 | 335 | 330 | 380 | 335 | 299 | 254 | 220 |

*Notes:* This table shows estimated effects on whether the school was covered in the second grade national examinations and on the number of test takers (among schools included in the examinations). Years (displayed in each column) indicate the year of the second grade national examination. Each panel presents (a) the estimated coefficients and standard errors on the treatment indicator from OLS regressions that control for randomization strata fixed effects, (b) the control group mean, and (c) the number of school-level observations. ***p<0.01, **p<0.05, *p<0.1.

Table B.2: Baseline Balance and Compositional Changes of Schools Covered in National Examinations over Time

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Computers per student** | | | | | | | | |
| Effect | 0.006 | 0.004 | 0.003 | 0.002 | 0.004 | -0.001 | 0.005 | -0.004 |
| | (0.005) | (0.004) | (0.004) | (0.004) | (0.005) | (0.005) | (0.006) | (0.007) |
| Control mean | 0.015 | 0.013 | 0.015 | 0.015 | 0.015 | 0.016 | 0.013 | 0.016 |
| Observations | 360 | 335 | 330 | 380 | 335 | 299 | 254 | 220 |
| **Electricity** | | | | | | | | |
| Effect | 0.028 | 0.033 | 0.030 | 0.038 | 0.029 | 0.007 | -0.003 | -0.015 |
| | (0.036) | (0.037) | (0.034) | (0.033) | (0.036) | (0.037) | (0.043) | (0.044) |
| Control mean | 0.868 | 0.848 | 0.880 | 0.867 | 0.862 | 0.875 | 0.888 | 0.868 |
| Observations | 360 | 335 | 330 | 380 | 335 | 299 | 254 | 220 |
| **Piped water** | | | | | | | | |
| Effect | 0.015 | 0.011 | 0.016 | 0.015 | 0.050 | 0.007 | -0.044 | -0.031 |
| | (0.053) | (0.055) | (0.056) | (0.049) | (0.053) | (0.056) | (0.066) | (0.067) |
| Control mean | 0.507 | 0.529 | 0.486 | 0.482 | 0.493 | 0.547 | 0.551 | 0.527 |
| Observations | 360 | 335 | 330 | 380 | 335 | 299 | 254 | 220 |
| **Enrollment (first - sixth grade)** | | | | | | | | |
| Effect | -3.179 | 0.481 | 0.922 | 0.242 | -0.350 | 0.146 | -0.568 | -2.815 |
| | (3.861) | (3.763) | (3.874) | (3.540) | (3.760) | (4.077) | (4.715) | (5.240) |
| Control mean | 82.095 | 82.352 | 82.800 | 81.395 | 84.855 | 87.893 | 91.266 | 96.469 |
| Observations | 360 | 335 | 330 | 380 | 335 | 299 | 254 | 220 |
| **Second-grade mathematics performance** | | | | | | | | |
| Effect | -0.041 | -0.086 | -0.047 | -0.036 | -0.034 | 0.001 | -0.129 | 0.011 |
| | (0.075) | (0.085) | (0.083) | (0.077) | (0.088) | (0.094) | (0.106) | (0.104) |
| Control mean | -0.067 | 0.015 | 0.011 | -0.004 | -0.025 | -0.043 | 0.028 | -0.028 |
| Observations | 345 | 315 | 308 | 348 | 305 | 277 | 235 | 207 |
| **Second-grade reading performance** | | | | | | | | |
| Effect | 0.003 | -0.020 | -0.010 | 0.040 | 0.059 | 0.013 | -0.042 | 0.090 |
| | (0.075) | (0.083) | (0.080) | (0.076) | (0.085) | (0.091) | (0.097) | (0.095) |
| Control mean | -0.019 | 0.027 | 0.019 | 0.007 | 0.016 | -0.032 | 0.033 | -0.021 |
| Observations | 346 | 316 | 308 | 349 | 306 | 278 | 236 | 208 |
| **Grade progression (second - sixth grade)** | | | | | | | | |
| Effect | -0.009 | 0.008 | 0.011 | 0.002 | 0.002 | 0.014 | 0.025 | 0.032* |
| | (0.013) | (0.014) | (0.014) | (0.013) | (0.014) | (0.014) | (0.017) | (0.017) |
| Control mean | 0.816 | 0.806 | 0.804 | 0.815 | 0.809 | 0.805 | 0.796 | 0.803 |
| Observations | 352 | 322 | 319 | 369 | 327 | 292 | 246 | 213 |

*Notes:* This table presents baseline statistics and estimated differences between treatment and control schools for the set of schools covered in the second grade national examinations every year. Each panel shows (a) the estimated coefficients and standard errors on the treatment indicator from OLS regressions that control for randomization strata fixed effects, (b) the control group means, and (c) the number of observations. Results for second grade mathematics and reading are estimated effects from school-level regressions where individual-level performance on the 2007 and 2008 evaluations were first standardized with zero mean and unit variance for the control group for each year and then aggregated at the school-year level mean. Then the school-level average for 2007 and 2008 was computed. The other outcomes present school-level regressions using data from the 2008 school census. Estimates for second grade mathematics and reading are weighted by the number of students who took the examination and estimates for school grade progression are weighted by school (second - sixth grade) enrollment. ***p<0.01, **p<0.05, *p<0.1.

Table B.3: Effects for Schools with Alternative Pooling Periods

| | Pooled school-year-level | | |
|---|---|---|---|
| | 2009-2013 | 2009-2014 | 2009-2016 |
| | (1) | (2) | (3) |
| **Panel A: Second-grade mathematics performance** | | | |
| Effect | -0.033 | -0.054 | -0.044 |
| | (0.051) | (0.051) | (0.049) |
| Control mean | 0.000 | 0.000 | 0.000 |
| Observations | 1,739 | 2,038 | 2,512 |
| **Panel B: Second-grade reading performance** | | | |
| Effect | -0.047 | -0.066 | -0.056 |
| | (0.049) | (0.050) | (0.048) |
| Control mean | -0.000 | -0.000 | -0.000 |
| Observations | 1,740 | 2,039 | 2,513 |
| **Panel C: Grade progression** | | | |
| Effect | -0.012** | -0.011* | -0.010** |
| | (0.006) | (0.006) | (0.005) |
| Control mean | 0.851 | 0.857 | 0.869 |
| Observations | 2,651 | 3,180 | 4,235 |

*Notes:* Panels A and B show estimated effects from school-level regressions where individual-level performance was first standardized with zero mean and unit variance for the control group and then aggregated at the school level mean. Panel C shows estimated effects on grade progression (defined as the ratio of students promoted to the next grade with respect to those enrolled at the beginning of the school year in second to sixth grades - this ratio is capped at 1) using the yearly school census data. Each panel shows (a) the estimated coefficients and standard errors on the treatment indicator from OLS regressions that control for randomization strata by year fixed effects, (b) the control group means, and (c) the number of school-year observations. Estimated standard errors are clustered at the primary school level. Estimates displayed in panels A and B are weighted by the number of students who took the examinations. Estimates in panel C are weighted by school (second - sixth grade) enrollment. ***p<0.01, **p<0.05, *p<0.1.

Table B.4: Effects for Schools over Time (Unweighted)

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2009-2016 |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **Panel A: Second-grade mathematics performance** | | | | | | | | | |
| Effect | -0.202** | -0.166* | 0.053 | 0.037 | -0.105 | -0.144 | 0.121 | -0.153 | -0.072 |
| | (0.090) | (0.094) | (0.091) | (0.082) | (0.087) | (0.103) | (0.111) | (0.103) | (0.045) |
| Control mean | 0.085 | 0.028 | 0.040 | 0.016 | -0.000 | 0.042 | -0.007 | -0.028 | 0.025 |
| Observations | 360 | 335 | 329 | 380 | 335 | 299 | 254 | 220 | 2,512 |
| **Panel B: Second-grade reading performance** | | | | | | | | | |
| Effect | -0.181** | -0.196** | 0.030 | -0.019 | -0.113 | -0.178* | 0.075 | -0.102 | -0.090** |
| | (0.086) | (0.085) | (0.087) | (0.081) | (0.087) | (0.101) | (0.101) | (0.102) | (0.045) |
| Control mean | 0.068 | 0.044 | 0.022 | 0.015 | -0.004 | 0.033 | -0.055 | -0.060 | 0.013 |
| Observations | 360 | 335 | 330 | 380 | 335 | 299 | 254 | 220 | 2,513 |
| **Panel C: Grade progression** | | | | | | | | | |
| Effect | -0.012 | -0.017 | -0.022** | -0.001 | -0.013 | -0.006 | -0.020*** | -0.014* | -0.013*** |
| | (0.011) | (0.011) | (0.010) | (0.010) | (0.010) | (0.008) | (0.008) | (0.008) | (0.005) |
| Control mean | 0.866 | 0.827 | 0.862 | 0.857 | 0.876 | 0.909 | 0.932 | 0.926 | 0.882 |
| Observations | 530 | 531 | 531 | 528 | 531 | 529 | 529 | 526 | 4,235 |

*Notes:* Panels A and B show estimated effects from school-level regressions where individual-level performance was first standardized with zero mean and unit variance for the control group and then aggregated at the school level mean. Panel C shows estimated effects on grade progression (defined as the ratio of students promoted to the next grade with respect to those enrolled at the beginning of the school year in second to sixth grades - this ratio is capped at 1) using the yearly school census data. Each panel shows (a) the estimated coefficients and standard errors on the treatment indicator from OLS regressions that control for randomization strata fixed effects, (b) the control group means, and (c) the number of observations. Regressions for individual years control for randomization strata fixed effects. Pooled regressions control for randomization strata by year fixed effects. Estimated standard errors are clustered at the primary school level. Estimates are not weighted. ***p<0.01, **p<0.05, *p<0.1.

Table B.5: Effects for Schools over Time by Baseline School Enrollment

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2009-2016 |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **Panel A: Second-grade mathematics performance** | | | | | | | | | |
| Effect Below | -0.323** | -0.401** | 0.049 | 0.031 | -0.238* | -0.088 | -0.038 | -0.240 | -0.156** |
| | (0.146) | (0.157) | (0.147) | (0.128) | (0.130) | (0.150) | (0.205) | (0.217) | (0.071) |
| Effect Above | 0.096 | 0.031 | 0.024 | 0.042 | -0.020 | -0.228 | 0.111 | -0.062 | -0.007 |
| | (0.123) | (0.124) | (0.115) | (0.111) | (0.120) | (0.144) | (0.141) | (0.127) | (0.058) |
| *p*-value | 0.03 | 0.03 | 0.89 | 0.95 | 0.22 | 0.50 | 0.55 | 0.48 | 0.11 |
| Observations | 360 | 335 | 329 | 380 | 335 | 299 | 254 | 220 | 2,512 |
| **Panel B: Second-grade reading performance** | | | | | | | | | |
| Effect Below | -0.220 | -0.411*** | 0.011 | 0.051 | -0.127 | -0.191 | 0.067 | -0.310 | -0.131* |
| | (0.134) | (0.138) | (0.145) | (0.125) | (0.135) | (0.138) | (0.177) | (0.220) | (0.071) |
| Effect Above | 0.018 | 0.008 | 0.016 | -0.032 | -0.019 | -0.206 | 0.017 | -0.006 | -0.031 |
| | (0.120) | (0.100) | (0.111) | (0.105) | (0.119) | (0.159) | (0.132) | (0.133) | (0.057) |
| *p*-value | 0.19 | 0.02 | 0.98 | 0.62 | 0.55 | 0.94 | 0.82 | 0.23 | 0.28 |
| Observations | 360 | 335 | 330 | 380 | 335 | 299 | 254 | 220 | 2,513 |
| **Panel C: Grade progression** | | | | | | | | | |
| Effect Below | -0.004 | -0.015 | -0.016 | -0.005 | -0.029* | -0.011 | -0.021* | -0.002 | -0.013 |
| | (0.019) | (0.019) | (0.016) | (0.016) | (0.015) | (0.013) | (0.013) | (0.013) | (0.009) |
| Effect Above | -0.010 | -0.025* | -0.025** | -0.003 | 0.002 | 0.005 | -0.011 | -0.003 | -0.010 |
| | (0.013) | (0.015) | (0.013) | (0.011) | (0.014) | (0.012) | (0.010) | (0.011) | (0.006) |
| *p*-value | 0.81 | 0.68 | 0.65 | 0.89 | 0.14 | 0.36 | 0.52 | 0.97 | 0.79 |
| Observations | 530 | 531 | 531 | 528 | 531 | 529 | 529 | 526 | 4,235 |

*Notes:* Panels A and B show estimated effects from school-level regressions where individual-level performance was first standardized with zero mean and unit variance for the control group and then aggregated at the school level mean. Panel C shows estimated effects on grade progression (defined as the ratio of students promoted to the next grade with respect to those enrolled at the beginning of the school year in second to sixth grades - this ratio is capped at 1) using the yearly school census data. Each panel shows (a) the estimated coefficients and standard errors on the treatment indicator differentiated by whether the school was below/above the median baseline enrollment from OLS regressions that control for randomization strata fixed effects, (b) the *p*-value of the null for the equality of the estimated effects by baseline enrollment, and (c) the number of school-level observations. Regressions for individual years control for randomization strata fixed effects. Pooled regressions control for randomization strata by year fixed effects. Estimated standard errors are clustered at the primary school level. Estimates displayed in panels A and B are weighted by the number of students who took the examinations. Estimates in panel C are weighted by school (second - sixth grade) enrollment. ***p<0.01, **p<0.05, *p<0.1.