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Technology and Academic Achievement: Who Benefits? Evidence From Argentina

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"Tecnología y Logro Académico: ¿Quién se Beneficia? Evidencia del Plan Conectar Igualdad"

Resumen

Este documento proporciona una evaluación causal del efecto del acceso a computadoras e internet en la escuela secundaria sobre los logros académicos de los alumnos. Con este objetivo, aprovecho variación intertemporal y transversal en el acceso a estas tecnologías entre las escuelas de la Argentina, el primer país en implementar un programa nacional de una computadora portátil por niño en todos grados de la educación secundaria, y el programa más grande de este tipo. Contrariamente a la evidencia de que estos programas tienen efectos insignificantes en educación para los estudiantes de primaria y de ciclo básico, encuentro mejoras en las tasas de promoción y graduación en las escuelas secundarias públicas alrededor del momento de la intervención. También investigo si las escuelas que tenían más probabilidades de incorporar estas tecnologías fueron la que más se beneficiaron del programa.

Palabras clave: Educación, Desigualdad, Gasto Publico, Tecnología – TICs

“Technology and Academic Achievement: Who Benefits? Evidence From Argentina”

Abstract

This paper provides causal estimates of the effect of secondary school’s Access *to computers and the internet on academic achievement. I exploit intertemporal and cross-sectional variation in access to technology among schools in Argentina, the first country to implement a nationwide one-laptop-per-child program in all grades of secondary school, and the largest program of this kind. Contrary to the evidence that one-laptop-per-child programs have insignificant effects on educational achievement for primary and middle-school students, I find improvements in promotion and graduation rates in public secondary schools around the time of the intervention. I also investigate whether schools that were more likely to incorporate technology in education benefited the most from the program.*

Keywords: Education Policy, Education and Inequality, Government Expenditures and Education

Códigos JEL: [I21, I24, I28, H52]

1 Introduction [1]

Since the creation of the One Laptop per Child Organization in 2005, schools around the globe have experimented with providing personal computers or similar technologies to their students. One-laptop-per-child programs have been implemented in over 42 countries, and in some cases have been deployed at the national level, by governments that are increasingly concerned about the economic consequences of unequal access to technology and learning opportunities.[2]

Despite the popularity of these programs, policy evaluations of one-laptop-per-child initiatives, predominantly in primary schools, have found no short nor medium-term effects on a set of social, educational, and cognitive outcomes (Beuermann et al., 2015; Yanguas, 2018). Faced with this discouraging finding, some researchers have suggested that treatment effects may depend on how successful schools are at incorporating technology in the classroom. However, there is no direct empirical evidence on whether this is the case. Moreover, some studies suggest that one-laptop-per-child programs might be beneficial when implemented later in life (see Fairlie and London, 2012a), leaving an open question of what is the optimal educational stage for these interventions.

In this paper, I examine the effects of providing laptops with internet access to secondary schools on promotion and graduation rates, and investigate whether those most likely to incorporate technology in education benefit the most from the program. To this end, I use evidence from Conectar Igualdad in Argentina, the largest nationwide one-laptop-per-child program to date, and investigate its effect on promotion and graduation rates up to five years since the start of the program.[3] Starting in 2010 (and especially 2011), Conectar Igualdad delivered a personal laptop to each student in secondary schools within the public education system and equipped public schools with internet access. To the best of my knowledge, this is the first paper to consider the heterogeneous effects of a one-laptop-per-child program of this scale.

My analysis is based on tabulated administrative data from the Annual Census of Education

[1] E-mail: myanguas@ucla.edu. I thank Adriana Lleras-Muney for valuable comments and support for data collection; I thank David Atkin and Till von Wachter for valuable feedback in the early stages of this research. I thank the Ministry of Education of Argentina for providing me with district-level data. This project was supported by the California Center for Population Research at UCLA (CCPR), which receives core support (P2C-HD041022) from the Eunice Kennedy Shriver National Institute of Child Health and Human Development (NICHD).

[2] One-laptop-per-child programs have been implemented in over 42 countries. National partners of the One-Laptop-Per-Child organization include Uruguay, Peru, Argentina, Mexico, and Rwanda. Other significant projects have been started in Gaza, Afghanistan, Haiti, Ethiopia, and Mongolia. In the US, the most famous implementation was OLPC Birmingham (Alabama). For a review of technology-based approaches in education, see Escueta et al. (2017).

[3] Argentina is Latin America's fourth largest country and third largest economy, second largest in South America behind Brazil. It is ranked as a high-income country by the UN, with a population of 44 million people and a GDP per capita of $20,425 PPP.

provided by the Ministry of Education of Argentina. The data is available for each year between 2005 and 2016, by geographic district, school level and school type, for a total of 527 districts spread across 24 provinces. For the entire period the survey tracked each school's ownership of computers and other school characteristics including the measures of school quality, vulnerability among students, presence of computer labs, and enrollment, promotion, and graduation rates, allowing me to track access to technology in schools as well as the educational performance of their students.

I first document that the program was effective at increasing access to technology in public secondary schools —the rollout was complete by the end of 2014, and by 2015 the total number of laptops deployed surpassed the baseline secondary school population in all but five provinces; the average number of laptops per student available in schools doubled in the first year of the rollout, while internet connection had doubled by the end of 2015. Overall, I estimate that the program increased the number of laptops available in schools, per student, by almost 40% (15 percentage points).[4] The scale and scope of the program makes for a great setting in which to conduct this research.

To estimate the effect of the one-laptop-per-child program on school performance, I use two distinct methodologies. First, I perform a dynamic analysis, in which I estimate dynamic treatment effects for each year after the start of the program. I find that the strong increase in computer access is indeed accompanied by significant improvements in both promotion and graduation rates a few years down the line. However, I also find a significant drop in graduation rates in the year 2011, the first year after the start of the program.[5] I then perform a regression discontinuity analysis, where I estimate a break in trend on school performance after program implementation. The main assumption is that district-specific trends in the outcomes of interest prior to 2011 are a reasonable counterfactual for the subsequent evolution of these outcomes. Excluding 2011, I find a 3% increase in promotion and a 10% increase in graduation rates. These results are robust to economic conditions, as well as to changes in other school characteristics. When comparing the relative performance of public secondary schools (relative to private schools) or secondary schools (relative to primary schools) after the program, the effects on promotion rates disappear, but weak improvements on graduation rates persist. In other words, the schools that were targeted by the program experienced larger improvements in graduation rates than their counterparts, but did not experience larger improvements in overall promotion rates. This is consistent with cohort-specific heterogeneous effects, where the graduating cohorts are more likely to complete the academic year while younger cohorts are less

[4] This is a lower-bound estimate; students are allowed to take full ownership of their laptops after graduation in which case those laptops will no longer qualify as school equipment and will exit the dataset.

[5] This could be explained by short-term disruptions caused by the introduction of laptops in the education system, by a hike in public-school enrollment rates, or by deliberate retention. Further analysis is pending.

likely to make progress; an estimated 10% increase in dropout rates appears to confirm this hypothesis.[6]

To investigate whether the schools that were most likely to incorporate technology in education benefited the most from the program, I select districts based on three preintervention characteristics: household access to computers, computer labs in schools, and internet-aided instruction. First, I perform a dynamic analysis, in which I estimate dynamic treatment effects for each year after the start of the program, in districts with high technology at baseline relative to others. Then, I perform a regression discontinuity analysis, where I estimate a break in trend on school performance after program implementation that is exclusively attributed to high-technology districts. Public schools located in districts with high household access to computers prior to the program were less likely to experience an increase in computer access after 2011, and consequently were significantly less likely to improve their promotion rates. Schools located in districts with high rates of computer labs and teaching-aided instruction prior to the program did experience a higher increase in computer access after the intervention than their counterparts. These schools experienced larger improvements in promotion rates after 2011; there were no significant differences in graduation rates.

Due to the popularity of these interventions and newly available data, there is now abundant evidence on the effects of computers on learning. De Melo et al. (2014b) found that, two years after the intervention, Uruguay's Plan Ceibal had not influenced primary school student's math and reading scores. This is consistent with the findings of Cristia et al. (2017) 15 months into a similar intervention – although with no internet treatment – in primary schools in rural Peru; while the program increased computer use both at school and at home, together with some general cognitive skills, no evidence was found of effects on enrollment and math and language scores. While these programs intended children to use computers both at home and at school, their findings are in line with papers that explore the effects of home computers only. In a small-scale implementation in Peru that used the same devices for home-use only, Beuermann et al. (2015) found no effects on academic achievement or cognitive skills in the short run, although lower academic effort was reported by teachers. They found short-run improvements in proficiency at using the program's computer (which typically runs Linux) but no improvements in either Windows computer literacy or abstract reasoning. In a follow-up study Malamud et al. (2019) found that providing free internet access led to improved computer and internet proficiency, but there were no significant effects on math and reading achievement, cognitive skills, self-esteem, teacher perceptions, or school grades. More concerning, some studies, as Vigdor et al., 2014; Malamud and Pop-Eleches, 2011, found negative effects on academic achievement from interventions that are purely focused on expanding technology

[6] See Appendix Tables A14 and A15.

access. These findings contrast with positive effects found in alternative programs that use technology specifically for educational purposes (see Banerjee et al., 2007; Roschelle et al., 2016). This suggests that the effects of technology are likely to vary depending on when, where, how, and for what purpose children use their computers.[7] A few papers have also examined the effects of access to technology at more advanced stages of the education system. For instance, Cristia et al. (2014) found no statistically significant effects of high school computing labs on grade repetition, dropping out, and initial enrollment in Peru between 2006 and 2008, ruling out even modest effects. Dettling et al. (2015) examined effects of high-speed internet access in early adulthood on college-entry examinations and college applications; while broadband access generally increased applications to college, the effects were concentrated among high-income students potentially increasing preexisting inequities. Fairlie and London (2012b) found that donating laptops to recently enrolled community-college students lead to modest but statistical improvements on their academic performance, although Fairlie and Bahr (2018) found no effects on earnings or college enrollment seven years after the provision of laptops. In sum, the literature has typically found negligible effects of technology access on academic performance, with results ranging from negative to positive as the educational level of the target population rises.

This is not the first paper to examine the effects of Conectar Igualdad. In a cross-sectional study based on a sample of 15 year old students, Alderete and Formichella (2016) found that those enrolled in schools that had participated in the program scored higher at the 2012 PISA exam than students from comparable schools that had not yet received their laptops, although these differences were not qualitatively significant.[8] Brambilla and Tortarolo (2018)investigated labor-market effects of the program, and found that companies that purchased computers experienced significant improvements in productivity, employment, and wages, and that skilled workers benefited relatively more. My paper contributes to the current understanding of the academic effects of Conectar Igualdad by offering a time-series analysis that captures the entire universe of schools in the country and allows me to compare measures of educational attainment both before and after the intervention. I control for potential labor-market effects of the program in my robustness checks, by incorporating province-specific annual unemployment rates. More generally, my paper contributes to filling a gap in the literature regarding the educational effects of one-laptop-per-child programs in secondary schools, and has important implications for policy makers who are looking for the optimal treatment group. Additionally,

[7] This is influenced by the level of parental supervision and teacher engagement. See Warschauer et al. (2011) for an analysis of the practical limmitations of one-laptop-per-child programs.

[8] A limitation of this study is that participating schools were of higher socio-economic status than non-participating schools in the sample; it is unclear whether propensity score matching on observable characteristics was able to completely eliminate unobserved advantages in the treatment group.

this is the first study to analyze the effects of a one-laptop-per-child program across schools with different propensities to integrate technology with education, and, by exploiting a large-scale quasi-experimental design, it is minimally affected by the concerns of external validity associated to randomized controlled trials and is particularly relevant for informing policy.

The direction of the effect of technology access on educational choices is not obvious. For instance, internet and computer access in schools might make the educational experience more enjoyable to children and may allow teachers to adapt more effectively to each student's level and needs. On the other hand, access to entertainment may encourage leisure and drive students to pay less attention in class. The school's propensity to incorporate technology in the classroom can enhance either of the two effects. These trade-offs can in turn affect students' daily decisions about whether to attend class and how much effort to put forth, as well as decisions with long-lasting effects such as whether to enroll or drop out of school. My findings suggest that one-laptop-per-child programs might be more beneficial among highschool students relative to younger populations and that school's propensity to incorporate technology in the classroom might not ensure higher benefits from the program.[9]

The rest of this paper proceeds as follows. Section 2 describes the program. Section 3 describes the data and summary statistics. Section 4 examines whether the program improved educational outcomes. Section 5 investigates what schools benefited the most. Section 6 concludes.

2 The One-Laptop-Per-Child Program in Argentina: Conectar Igualdad

One Laptop per Child (OLPC) is a nonprofit initiative founded in 2005 to empower the children of developing countries to learn by providing one internet-connected laptop to every school-aged child. The organization creates and distributes educational devices for the developing world and creates software and content for those devices. One-laptop-per-child programs have been implemented in partnership with the OLPC organization in at least 42 countries.

[9] Besides its focus on secondary schools, Conectar Igualdad differed from traditional one-laptop-per-child programs implemented in primary schools by equipping the laptops with the widely-used Windows operating system, rather than relying exclusively on Linux. The choice of operating system could potentially influence the educational effects of these programs. See Warschauer et al. (2011) for an analysis of the practical limitations of one-laptop-per-child programs.

2.1 Implementation

In 2010, the government of Argentina launched Conectar Igualdad, an ambitious program designed to eliminate the existing technological gap between private and public school students. Until its dismantling in 2016, Conectar Igualdad provided laptops with wireless modems to students and teachers in public secondary schools, special education schools, and teacher training institutes. As of July 2015, 5 million laptops had been deployed, enough to cover the secondary-school-aged population —12 to 18 years old— living in the country.[10] According to the National Survey on Access and Use of Information Technologies and Communication (ENTIC), the share of urban households with computer access increased from 56% in 2011 to 67% in 2015, and the share with internet access increased from 48% to 61%.[11]

The initial plan was to deliver 3.5 million laptops to the target population nationwide in three stages, within a period of approximately three years (2010, 2011, 2012). In practice, it took the program four years to achieve complete geographic coverage in December 2014 (see Web appendix Figure A1).[12] The implementation of the program involved federal and local authorities, school directors, parents, and students. The national and local ministries of education jointly determined which secondary schools would participate in the program at each stage. During the 2010 pilot program, distribution was focused in urban areas, schools with adequate infrastructure, and the most senior highschool cohorts were given priority to ensure their full participation. For the following years, students had to register in the program using an online application, then each school would make an official request for delivery of the equipment, specifying the total number of laptops needed, after which schools awaited for program authorities to determine the appropriate distribution stage for the school, verify that adequate infrastructure and connectivity were in place, and issue an order for delivery. Laptops were initially assigned to schools and lent to students; by design, students could take full ownership of their laptop upon completing secondary school. Most teachers did not receive individual laptops, but the number of laptops received by the school for teacher use was initially set to 10% of those requested for students.[13]

The program was financed by the National Treasure and from income generated by the Sus-

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[10] According to the Census of Population, 4,929,566 people were aged 12 to 18 in 2010. According to the Census of Education, 3,946,834 students were enrolled in secondary schools in 2015, of which 2,807,086 were enrolled in public schools.

[11] Computer and internet access measures are based on direct reports of a computer or internet connection in the dwelling.

[12] Former President of Argentina Cristina Fernandez de Kirchner announced in March 2015 that full coverage of secondary school students had been attained in 2014. See: http://www.telam.com.ar/notas/201503/96509-apertura-de-sesiones-cristina-conectar-igualdad.html.

[13] http://www.tic.siteal.iipe.unesco.org/politicas/859/programa-conectar-igualdad.

tainability Guarantee Fund, a sovereign investment fund that is part of the Integrated Argentine Pension System. Overall, Conectar Igualdad required an investment of approximately US$ 2.5 thousand million at 2005 prices.[14] In addition to financing the computers, investments were also directed towards adapting the infrastructure of schools.

Argentina's laptop program was effective at increasing access to computers in secondary schools throughout the country. Using official deployment data from Conectar Igualdad, I find that 5.3 million laptops had been set aside for deployment by December 2015. Only 7% of these laptops were actually deployed in 2010, while 60% of deployments were split between years 2011 and 2013. Combining province-level official deployment data with the 2010 Census of Population, I notice two facts: first, the program was very widespread geographically as all provinces received laptops every single year since 2010; second, by the end of 2015, the entire baseline secondary-school-aged population (aged 12–18 in 2010) had been covered in all but five provinces and was above 75% coverage in all of them (see Appendix Figure A1).

Using district-level data from Argentina's annual school census (which I describe in more detail below), I show that in 2010 —right before the start of the program— in most districts the share of computers available for secondary-school students was under 20%; in 2015 —after coverage was completed—, this share was above 60% (Figure 1). In the average district, there were enough computers in secondary schools to cover only 16% of students, with a range varying from 15% in public schools to 20% in private schools. In 2015, there were on average enough computers cover 65% of currently enrolled students, with a range varying from 72% in public schools to 14% in private schools.

Using the same annual data, I track the number of computers available in secondary schools by sector. Among public schools the number of computers available more than doubled (from under 0.5 million in 2010 to over 1 million in 2011) in the first year since the program was implemented, and reached 1.5 million in 2015 (Figure 2, Panel A).[15] Compellingly, there was no change at all around the threshold in the number of computers reported by private schools. In effect, this increased access benefited only public school children; those enrolled in private schools experienced no significant discontinuities in computer access. Moreover, this increase was driven by computers used for pedagogical rather than administrative purposes (Figure 2, Panel B). Appendix Figure A2 shows that the number of computers available in secondary schools doubled from 0.75 million to just above 1.5 million while the fraction of students with computer access doubled from 20% to just above 40% in the year 2011.

[14] Figure based on the National Budget records for the period 2010–2016: https://www.minhacienda.gob.ar/onp/presupuestos/2019.

[15] The specific question as it appears in the school survey is: *does this home have a personal computer?* The informant is the school director. Note that students, rather than the schools, become the owners of the laptops after graduation.

Access to internet also increased after the program: the share of secondary schools with internet access increased from 40% to 70% and more than doubled among public schools; although this gap had not been bridged by the end of 2015.[16]

2.2 The Computer

The laptops used were of the model Classmate PC, rugged netbooks for children developed by INTEL. These are small, durable, efficient, low-cost laptops function much like a normal computer and allow students to communicate with people around the world, access information, design programs, and manipulate music, sound or pictures.[17][18]

The original laptop has a 10-inch screen and a low-power Intel Atom N450 processor. It also has 1 GB of RAM (expandable to 2 GB), webcam, hard disk of 160 GB capacity and three USB ports. Internet connection can be established by Wi-Fi, or cable, with an Ethernet connector; additionally, the laptop includes a slot to insert a chip that allows surfing via 3G. Each computer is loaded with both Huayra Linux and Windows XP; the user is prompted to select the preferred operating system every time the laptop is started. In addition, the laptop includes Microsoft Office 2007 and OpenOffice. This is an important difference from other versions of this program which were mostly restricted to Linux.[19]

Among the software installed on the computer are applications for general and specific educational purposes. In order to prevent theft, the equipment includes a monitoring software. In addition, it includes a system that limits the contents that can be visualized as well as the schedules of use. The laptops weight 1.5 kg and contain a six-cell battery, which allows the user to work continuously for over five hours. When the program launched in 2010, similar laptops retailed under $500.[20]

A 2011 survey of secondary school students in the public sector, supported by the Ministry of Education, concluded that 80% of them had used the government laptop in class, 71% had

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[16] By the end of 2015, 90% of private schools had internet access in the average district, while only 70% of public schools had internet access.

[17] The first model was the Exomate X352, sold by the Argentine company EXO.

[18] The Classmate PC is similar in design to the XO used by the One-Laptop-Per-Child organization. See http://wiki.laptop.org/images/7/71/CL1A_Hdwe_Design_Spec.pdf for more details.

[19] The One-Laptop-Per-Child organization developed the first computer intended for children, the OLPC XO. This laptop originally contained only a Linux operating system. The Sugar-Linux interface on the XO has been criticised for being unfamiliar to users of Windows, Macintosh, or even most Linux operating systems (Warschauer et al., 2011).

[20] Dell's Netbook Inspiron (1 GB RAM, 250GB hard disk) sold for ARG$1,799 in 2010, around $450 at the time (see `http://www.redusers.com/noticias/dell-presento-sus-modelos-2010-y-prometio-la-llegada-de-su-tablet/`). ASUS' Netbook Eee PC sold for ARG$2,000 in 2010, around $ 500.

used office software, 32% had used the laptop to search information, 43% had used the laptop for homework, 30% had used educational software, and 11% had used social networks.[21]

2.3 Cost and Financing of the Program

By the end of 2015, 5,317,158 devices had been approved for dispatch by the program, of which 90% were targeted to secondary schools – 70% to standard schools and 20% to technical schools.[22] According to annual official reports for the period 2010–2015, the program required investments of roughly US$ 2.6 billion at fixed prices of 2010, with an implicit cost of $ 490 per laptop/ student.[23] [24] As a reference, this corresponds to an average of 5% of Argentina's annual education budget between 2010 and 2015 and 4% of its federal budget in 2010.

Conectar Igualdad got its own portion of the federal budget financed directly from the national treasure. Part of the initial investments in 2010 were financed through a loan from the Sustainability Guarantee Fund, a sovereign Argentine investment fund that is part of the Integrated Argentine Social Security System.[25] Overall, the country has been running a fiscal deficit since 2011 and a primary deficit since 2012, despite the fact that tax revenues as a share of GDP accelerated their increase from 23% in 2010 to 27% in 2015, driven by the rise in income tax collection.[26] There is no evidence that Conectar Igualdad implied a decrease in expenditures in other areas of education—in fact, the share of education in the national budget was quite stable at around 7.6% in subsequent years – compared to 7.1% in 2010 – with a slight fall to 7.0 in 2014. The country experienced a change of government in December 2015. The new administration transferred Conectar Igualdad from the hands of the Social Security Administration to those of the Ministry of Education in 2016, and engaged in an agreement with the International Monetary Fund in 2018, committing to achieve federal government primary balance by 2020.[27]

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[21] In total, 5263 students were interviewed in 205 public schools across all provinces in 2011 (Perczyk et al., 2011).

[22] Source: shared upon request by the central office of Conectar Igualdad within ANSES, July 2016.

[23] Source: own calculations based on annual reports of the National Budget Office 2010–2016 and Auditoria General de la Nacion (Proyecto Nº 121381). Total expenditures in local currency amounted to ARG$ 16,7 billion. See: https://www.minhacienda.gob.ar/onp/presupuestos/2019. All prices denoted in dollars of 2010. Billion defined as 10^9.

[24] The cost per student was obtained taking into consideration that 2,657,956 students were enrolled in public secondary school in 2010 and assuming the number of students would have exactly duplicated by 2015.

[25] https://chequeado.com/hilando-fino/quien-paga-las-netbooks/

[26] Based on reports on tax collection 2010–2015: https://www.argentina.gob.ar/hacienda/ingresospublicos/portri.

[27] https://www.imf.org/

3 Data and Summary Statistics

3.1 Data Sources

This study is based on tabulated data from 2005 to 2016 from the Annual Census of Schools conducted by the Ministry of Education, by year, geographic district, and school type. There are in total 527 districts spread across 24 provinces. For the entire period the survey tracked each school's ownership of computers and whether they were used for educational or administrative purposes. Since 2011, the questionnaire has incorporated a specific question about the number of laptops owned by the school. This is relevant because, prior to program completion, the government laptops lent to students were official property of the schools they attend. Other useful variables include public status of primary and secondary schools, the presence of a computing lab at school, total enrollment by grade, total promoted, and graduates. The survey takes place every year at the start of the academic year (March); consequently, outcome variables corresponding to the end of an academic year such as promotion and graduation totals are lagged one period.

This school survey data is very convenient. Its main virtues: it allows me to document the effect of the program on computer access in schools (as was demonstrated in Figure 2) and to estimate its impact on educational attainment on schools with relatively higher access to technology at the baseline. Due to its mandatory and census status, the response rate is very high with a coverage above 99% for primary and secondary schools.[28]

For my analysis I also use data from the annual household survey 2008–2016 (Encuesta Permanente de Hogares, henceforth EPH) and the Census of Population 2001 and 2010. The household survey data is representative annually by province; I use unemployment estimates available in this dataset to control for economic activity in my analysis. The Census of Population provides very accurate baseline information for the total number of computers available in households in each district before the program, as well as the total number of secondary-school aged population. This information allows me to cross-validate my school-census data on program effectiveness, as well as to compare the effects of the program on school performance in districts with higher and lower access to computers at the baseline.

3.2 Summary Statistics

Table 1 shows summary statistics for secondary schools in 2008 to 2015 using the Annual Census of Schools tabulated by district and year, compressing 4,216 observations. In this

[28] Source: Scope of the 2016 Annual Survey. in Common Education, Ministry of Education of Argentina.

period, there were on average 27 schools per district, 7,147 students, and 2,186 computers in schools, most of which were intended for educational purposes (73%). In addition, 60% of schools had internet connection, almost half used internet in class, and 55% had a computer lab. The public sector in Argentina is widespread: 80% of schools are publicly owned, and private schools tend to display higher socio-economic status. For instance, the share of schools with an internet connection was considerably higher among private schools (82% vs. 53%), as was the fraction that use internet in class is (70% vs. 43%) or contain a computer lab (79% vs. 56%). This technology gap was larger before the program: in 2008, the share of schools that used internet in class was 34% overall, 28% in public schools and 54% in private schools, while the share of schools with computer labs was 54% overall, 47% in public schools and 83% in private schools. The share of school-owned computers per student was, however, similar across sectors: 0.16 in public schools and 0.21 in private schools. Towards the end of the program, in 2015, the fraction of schools with internet connection in their classrooms was 31% in the public sector and 33% in the private sector.

Regarding other school characteristics, there was a similar number of students per school across public (248) and private schools (230), and the gender composition was pretty balanced as well, with a 52% female student body. There were on average 0.2 teachers per student overall, with a larger share among public schools (0.22 vs 1.8) and on average each teacher worked 8 hours a week (7.9 in public schools, 9 in private schools). The share of vulnerable students was significantly higher among public schools. For instance, the share of indigenous population is 2.8% in public schools vs. 0.08% in private schools, the fraction rural is 13% in public vs. 0.8% in private schools, and the fraction deprived of liberty was 0.01% in both. Defining vulnerability as surpassing percentile 90th in either of those categories, 20% of districts are vulnerable among public schools and 6% among private schools.

Regarding academic achievement, the fraction of students promoted to the next academic year (including graduating students) is 78% overall, 76% in public schools and 87% among private schools. The fraction graduated from secondary schools is 72% overall, 64% in public schools and 75% in private schools. The wide gap across public and private sector suggests there is a lot of room for improvement among public schools. Private school students had more access to computing labs and internet connection in their schools, while the number of laptops owned by the schools before this program was very similar. It is likely that a large share of private school students already had laptops at home.[29]

[29] According to Encuesta de Uso de Tecnologias de la Informacion y Comunicacion 2011 (EUTIC), 70% of individuals aged 10–29 had used a cellphone, computer, or the internet in the past three months, respectively. This share was positively correlated with education level, indicating that individuals with higher resources were more likely to use these technologies.

4 Did the program work?

4.1 Dynamic Approach

To estimate the effect of the one-laptop-per-child program on school performance, I first implement an event study. I estimate the following OLS regression on a year-by-district sample of public and private secondary schools between 2008 and 2015:

$$Y_{dt} = \eta_d + \delta_{\mathbf{t}} + \mathbf{X}'_{dt}\Gamma + \varepsilon_{dt}\,, \tag{1}$$

where Y_{dt} is the outcome of interest, d indexes the district, and t indexes the start of each academic year.

The vector of covariates $\mathbf{X_{dt}}$ includes district-by-year level characteristics such as student vulnerability, student gender, teachers per student, hours per teacher, and share of public schools, to make the estimates more precise and to try to control for district-specific trends. The regression includes district fixed effects. The parameter of interest δ_t captures the average causal effect of receiving computers for all students in secondary school, for each year after the program.

I interpret β as an intent-to-treat effect, since the regression model estimates the reduced-form effects on all districts from post-reform public schools. This specification does not capture the potential effects of other trends in education or the economy that could also be influencing education across time, and assumes that any variation in the outcomes of interest since 2010 is due to the program. The main specification uses robust standard errors, but, since the exact timing of program participation was non-randomly assigned across schools, and likely dependent on characteristics of school authorities and the student body, I also conduct robustness with province-clustered standard errors, with no changes in outcome.

The results of this specification are shown in Figure 3. The figure plots the estimated values of δ_t for three years prior and five years posterior to the start of the program, with vertical lines denoting 95% confidence intervals. As shown in Panel A, there was no significant trend in promotion rate before and up to 2011, but a persistent and significant increase in the promotion rate ensues since 2012. Panel B shows the same conclusions were true for graduation rates. The timing of the improvement in these outcomes, after controlling for variations in observed characteristics of schools and student body, suggests that it could be caused by the laptop program.

Panel A of Table 2 shows this in more detail. While columns (1)–(2) show that computer access in secondary schools increased by 50% (20 percentage points) in 2011 had doubled (45 percentage points) by 2015, columns (3)–(4) show positive significant effects on promotion rates, starting in 2012. These increases are about 2.5% (0.02 percentage points) per year. Columns (5)–(6) show a strong, significant, 5% (4 percentage points) decrease in graduation rates in the year 2011, followed by a strong, significant recovery of about 10% (7 percentage points) in each of the following years. These findings are robust to inclusion of controls and province-clustered standard errors. Panel A.2 shows that the outcomes where not significantly different in 2008 and 2009 to 2010, suggesting that nothing was going on before the implementation of the program, and that, all else equal, nothing would have happened afterwards.

It is important to point out, however, that the economy of Argentina was undergoing a deceleration since 2008, with almost no growth since 2011, a concern given evidence that schooling decisions are counter-cyclical (Dellas and Sakellaris, 2003).[30] Could economic conditions account for the improved outcomes?

As mentioned above, a weakness of this specification is that any variations in educational outcomes that happen after 2010 are attributed to the program. Including a valid control group would reduce this concern, as long as the control group can be reasonable expected to be affected by similar shocks throughout the period.

In this light, I implement a difference-in-differences identification strategy that starts by comparing educational outcomes of public schools that were exposed to the program, to private schools that were not directly exposed to the program. Public schools were exposed to a treatment since 2011, but not in the previous years. Private schools were never targeted by the treatment. The most important assumption is that public and private schools would have had parallel trends in outcomes over time, in the absence of the treatment.

To estimate the effects of the program on school performance of public schools relative to private schools, I estimate the following OLS regression on a year-by-district-by-school-type sample between 2008 and 2015:

$$Y_{dtp} = \eta_{dp} + \gamma_t + \delta_{\mathbf{t}} Public_p + \mathbf{X}'_{dtp}\Gamma + \varepsilon_{dtp}\,, \tag{2}$$

where Y_{dtp} is the outcome of interest, p indexes the type of school, d indexes the district, and t indexes the start of each academic year.

The vector of covariates $\mathbf{X_{dtp}}$ includes time-varying district-by-sector level characteristics such as student vulnerability, student gender, teachers per student, hours per teacher, and share of public/private schools per district, to make the estimates more precise and to try to control for district and sector-specific trends. The regression includes district fixed effects. The parameter of interest δ_t captures the average causal effect of receiving computers for all students in secondary school, for each year after the program. I interpret these parameters as intent-to-treat

[30] See constant GDP per capita for Argentina 2005–2016, FRED.

effects, since the regression model estimates the reduced-form effects on all districts from post-reform public schools. This specification does not capture the potential effects of the program on private school students, who may have been induced to purchase laptops or may have benefited from the laptops of neighbors and friends. Since the exact timing of program participation was non-randomly assigned across schools, and likely dependent on characteristics of school authorities and the student body, I cluster standard errors at the district level.

The results of this specification are shown in Appendix Figure A3. Once again, dots denote estimates of treatment effects over time after including controls, while vertical lines denote 95% confidence intervals. This time, there is no visible effect on promotion rates for public relative to private secondary schools except for a visible decline in 2011 (Panel A), nor for graduation rates, although the pattern is similar to the one observed in the aggregate, featuring an insignificant decline in 2011 and insignificant recoveries moving forward (Panel B). Table summarizes results of this regression.

Panel B of Table 2 shows this in more detail. While columns (1)–(2) show that computer access in public secondary schools had doubled (30 percentage points) in 2011 and tripled (57 percentage points) by 2015 relative to private secondary schools, columns (3)–(4) show that promotion rates fell by 3% (2.6 percentage points) in 2011, with no significant effects in the following years. Columns (5)–(6) show no significant differences in graduation rates between public and private secondary schools after the implementation of the program. Panel B.2. shows that the pretrends for public and private schools were parallel.

Last, I repeat the difference-in-differences analysis above but this time declaring the treatment group as the set of all secondary schools and the control group as the set of all primary schools. The idea behind this approach is that the laptop program targeted secondary schools, so primary schools did not participate. On the one hand, primary schools are worse controls because students are of different ages, face different problematic, and because a reform from 2012 mandated automatic promotion for first-graders. On the other hand, if trends are deemed to be parallel despite their differences, it is a better control because it was less likely to be affected by the program than secondary private schools, which are direct competitors of secondary public schools. Moreover, private schools are on average richer, and hence, may have different effects from an economic downturn than public schools. Therefore, I conduct the OLS regression shown below, where now p indexes the school level:

$$Y_{dtp} = \eta_{dp} + \gamma_t + \delta_t Secondary_p + \mathbf{X}'_{dtp}\Gamma + \varepsilon_{dtp}\,, \tag{3}$$

Panel C of Table 2 shows the results of this regression in detail: significant increases in computer access, significant decreases in promotion rates, significant increases in graduation rates for secondary relative to primary schools. Negative effects on promotion, especially posterior to 2012, may be driven by a 2012 law that imposed automatic promotion in first grade. But, there is already a negative effect in 2011 column (4), so this might not be the driver. Decreases in promotion are consistent with increases in graduation since different years may be affected differentially by the program. However, Panel C.2 shows that pretrends were not parallel between primary and secondary schools before the implementation of the program in either of the outcomes, suggesting that another methodology is required to analyze how secondary schools compared to primary schools before and after the intervention.

4.2 Trend-break Approach

The validity of the event-study strategies listed above requires that there were no pre-trends in the outcomes of interest (equation 1) or that those pretends were parallel between treatment and control groups (equations 2 and 3) in the years leading to 2011. While, on average, it does seem to be the case that schools were not on a significant trend before 2011 (see Table 2, Panel B), and that public schools where not evolving significantly different than private schools leading up to 2011 (see Table 3, Panel B), there is also evidence that some provinces where on different trends, and non-significant differential trends could become accentuated over time.

To address this concern, I conduct the following three regressions that controls for district-by-group level trends. The program caused a large and significant break in trend in computer access among public secondary schools in the country, and, in the presence of a strong treatment effect, we would expect this break in trend around 2011 to reflect on the outcomes of interest. The main assumption behind this regression-discontinuity-in-time is that the trend up to 2011 is a good counterfactual for the trend in 2011–2015, and any deviations from it not captured by observable control variables, are due to the program itself. Because the program was distributed throughout 2011, 2012, 2013 and 2014, I allow for a sample that excludes the year 2011 as well, a technique that is common in this literature. The assumption is that there were no other shocks in 2011 at the same time as the rollout of the program that affected the treatment group differential in our outcomes of interest.

$$Y_{dt} = \eta_d + \delta_d Trend_t + \beta Post_t + \mathbf{X}'_{dt}\Gamma + \varepsilon_{dt}\,, \tag{4}$$

$$Y_{dtp} = \eta_{dp} + \delta_{dp} Trend_t + \theta Post_t + \beta Public * Post_t + \mathbf{X}'_{dtp}\Gamma + \varepsilon_{dtp}\,, \tag{5}$$

$$Y_{dtp} = \eta_{dp} + \delta_{dp} Trend_t + \theta Post_t + \beta Secondary_p * Post_t + \mathbf{X}'_{dtp}\Gamma + \varepsilon_{dtp}\,, \tag{6}$$

The dummy variable $Post_t$ is equal to one for all years since 2011.

Table 3 shows thee results for this section. Panel A focuses on outcomes for Regression 4 was run on a district-by-year sample of observations, with robust standard errors. While columns (1)–(2) show that computer access in secondary schools was 40% (15 percentage points) higher after 2011, columns (3)–(4) show no effects on promotion rates when looking at the entire period and only weakly significant improvements of 2.5% (0.02 percentage points) in promotion rates after dropping 2011. There is also no effect on graduation rates at 2011 unless deleting that year, when graduation rates jump by 13% (0.09 percentage points).

Panel B focuses on estimates resulting from running regression 5 on a district-by-public-by-year sample. While post-intervention computer access rates were 70% (19 percentage points) higher in public relative to private schools (and slightly higher than that after excluding 2011), post-intervention promotion rates fell significantly in public relative to private schools after 2011, with no effects after excluding 2011. Graduation rates show no effect in public relative to private schools when including 2011, but a weak increase of 6% (4 percentage points) when dropping 2011.

Panel C focuses on estimates resulting from estimating equation 6 on a district-by-level-by-year sample. While post-intervention computer penetration rates where significantly higher in secondary relative to primary schools (40%, 12 percentage points), post-intervention promotion rates where lower in secondary schools (a significant decline of 2%, 1.7 percentage points in the entire sample, and a 0.2% non-significant decline when excluding 2011). Although lower in the entire sample, graduation rates where significantly higher by around 10% 10% (0.8 percentage points) in secondary schools after the intervention relative to primary schools when excluding 2011.

In sum, using different specifications this section has shown evidence that school outcomes, particularly graduation rates, have modestly improved after the start of the laptop intervention, and slightly more so in groups of schools that were targeted by the program, especially when allowing for a one-year lag in educational outcomes. The sharp fall in graduation rates in 2011, followed by improvements in subsequent years, could be explained by students trying to continue enrolled to get access to the computers and graduating afterwards to become the sole owners. Further evidence is needed to confirm this hypothesis.

4.3 Robustness

The positive effect of the program on graduation rates remains statistically significant – both in the time-series and compared to primary schools – when using province-clustered standard errors (A4), when restricting the sample to the province of Buenos Aires (A5), and when controlling for economic activity (A6).

In Appendix Table A14 I replicate the analysis outlined in this section on a third outcome:

dropout rates. Declared dropout rates are qualitatively small in my sample (4% compared to 70% promotion and graduation rates) and possibly less reliable than my other outcomes, which is why they were excluded from the main analysis. However, understanding the effects of the program on dropout rates and whether those are in line with my other results can help validate my findings and illuminate their interpretation. Column (1) suggests that dropout rates increased by approximately 10% (0.5 percentage points) as a result of the program, both in the time-series and relative to public and primary schools. Excluding 2011 from the sample, the increase in dropout rates in the time-series is robust to the entire battery of robustness checks except for the inclusion of economic controls. When using other school groups as controls, the finding is robust to the entire battery of robustness checks except for province-clustered standard errors.

5 What schools benefited the most?

5.1 Dynamic Approach

A rooted question in this literature is what schools and what children benefit the most from one-laptop-per-child programs. Evidence from Malamud and Pop-Eleches (2011) suggests that parental control over time spent doing homework and using technology may matter. Analogously, several researchers have suggested that the pedagogical integration of computers and teacher training programs may improve the educational achievement effects of one-laptop-per-child programs (**?**De Melo et al., 2014a).

To examine this last hypothesis, I explore whether schools located in districts with relatively high rates of household computer access (above median), computer labs at schools (above percentile 75), or internet-aided-instruction (above median) at the baseline period performed relatively better after the program's implementation than their counterparts. I define baseline as 2001 – using data from the Census of Population 2001– for household computer access, and 2008 for the share of schools with computer labs and internet-aided instruction.[31] For each category, the district classifier is a binary variable defined as above. For this part of the analysis, I use a district-by-year dataset from 2008 to 2015, and implement a dynamic difference-in-differences methodology similar to the one described in the previous section. The object of interest is δ_t.

$$Y_{dt} = \eta_d + \gamma_t + \delta_t HighBaselineComputerRate_d + \mathbf{X}'_{dt}\Gamma + \varepsilon_{dt}\,, \tag{7}$$

[31] The Census of Population of 2010 was run in October, after the 2010 computers had been deployed. Although the 2010 pilot was small, I considered 2001 to be a safer choice. The results for 2010 are very similar and shown in the appendix.

$$Y_{dt} = \eta_d + \gamma_t + \delta_{\mathbf{t}} HighBaselineLabRate_d + \mathbf{X}'_{dt}\Gamma + \varepsilon_{dt}\,, \tag{8}$$

$$Y_{dt} = \eta_d + \gamma_t + \delta_{\mathbf{t}} HighBaselineInternetInstructionRate_d + \mathbf{X}'_{dt}\Gamma + \varepsilon_{dt}\,, \tag{9}$$

where Y_{dt} is the outcome of interest, d indexes the district, and t indexes the start of each academic year. The vector of covariates $\mathbf{X_{dtp}}$ includes time-varying district-by-sector level characteristics such as student vulnerability, student gender, teachers per student, hours per teacher, and share of public/private schools per district, to make the estimates more precise and to try to control for district and sector-specific trends. All regressions include district and year fixed effects.

The results of these specifications are summarized in Appendix Figures A5–A7. Once again, dots denote estimates of relative treatment effects over time after including controls, while vertical lines denote 95% confidence intervals based on district-clustered standard errors. From these graphs we can see a significant worsening of promotion rates after 2011 in districts with high household access to computers in 2001 relative to the rest, though we detect no difference in graduation rates. We an also see significant worsening of both promotion and graduation rates after 2011 in districts with high rates of schools with computer labs in 2008 relative to the others. There were no differences in treatment effects across districts with high or low shares of internet-aided-instruction.

Table 4 shows these results in more detail and provides additional context. Panel A shows results among districts with higher and lower access to computers in 2001. Columns (1)–(2) show that the districts with higher computer access at baseline were almost not treated; in fact, their counterparts experienced a much higher increase in computer access after the program. In this light, its surprising to find that graduation rates in columns (3)–(4) evolved similarly across districts. Panel B shows the results among districts with higher and lower shares of schools with computer labs at baseline. Columns (1)–(2) show essentially no differences in program participation, while columns (3)–(4) indicate worse effects on promotion rates and columns (5)–(6) indicate significantly worse effects on graduation rates. Panel C compares districts with higher and lower share of schools engaging in internet-aided instruction. Columns (1)–(2) show that high-technology districts were more likely to participate in the program, while columns (3)–(4) indicate no differences in promotion rates and columns (5)–(6) indicate modest relative worsening of graduation rates. These findings suggest that there is no positive correlation between technology adoption in schools at the baseline and the likelihood of benefiting from a subsequent one-to-one laptop program.

5.2 Trend-break Approach

In this subsection, I explore whether schools located in districts with relatively high rates of household computer access, computer labs at schools, or internet-aided-instruction at the baseline performed better after the program's implementation, using a district-by-year dataset from 2008–2015, and implement a break-in-trend methodology similar to the one described in Section 4. This is essentially a difference-in-differences identification strategy with district-specific time trends. The objective is to detect which groups of districts responded better to the program after 2011, while allowing differential trends by district. The most important assumption is that there was no shock in 2011, aside from this government intervention, that affected these groups of districts differentially over time.

To estimate the differential effects of the program on school performance across districts, I estimate the following OLS regressions:

$$Y_{dt} = \eta_d + \delta_d Trend_t + \theta Post_t + \beta HighBaselineComputerRate_d * Post_t + \mathbf{X}'_{dt}\Gamma + \varepsilon_{dt}\,, \quad (10)$$

$$Y_{dt} = \eta_d + \delta_d Trend_t + \theta Post_t + \beta HighBaselineLabRate_d * Post_t + \mathbf{X}'_{dt}\Gamma + \varepsilon_{dt}\,, \quad (11)$$

$$Y_{dt} = \eta_d + \delta_d Trend_t + \theta Post_t + \beta HighBaselineInternetInstructionRate_d * Post_t + \mathbf{X}'_{dt}\Gamma + \varepsilon_{dt}\,, \quad (12)$$

Y_{sdt} is the outcome of interest, s indexes the province, d indexes the district, and t indexes the academic year. The vector of covariates $\mathbf{X_{sdt}}$ includes district-by-sector level characteristics such as student vulnerability, student gender, teachers per student, hours per teacher, and fraction of public schools in district. The dummy variable $Post_t$ is equal to one for all years since 2011. The regression includes district fixed effects. The parameter of interest β captures the average causal effect of receiving computers for all students in secondary school, on schools located in districts with high access to technology, after the program. This specification does not capture the potential effects of the program on students from schools located in the other districts. Once again, I use robust standard errors; results are robust to province-clustered standard errors.

The results corresponding to this section are shown in Panel A of Table 5. The first 2 columns of Panel A show that public schools located in districts with high household computer access in 2001, were not more likely to experience a rise in laptops after 2011 than their counterparts when analyzing the whole sample, but much less likely (by 45%, or 20 percentage points) when dropping 2011 from the sample even though both types of districts had similar shares of computers per students at schools in 2008. The effect on promotion rates is also significantly negative in the order of 6% (5 percentage points) lower post-intervention for schools located in high-computer baseline districts. In other words, districts with low computer penetration in 2001, experienced a higher increase in laptop access after the program, and a higher rate of promotion among secondary-school students. Despite this finding, I find no differential effects on graduation rates.

Panel B shows that public schools located in districts with a high share of computer labs in schools in 2008, were 16 percentage points more likely to receive computers after the program in the whole sample, though no difference is discernible without 2011. This might reflect the fact that these schools possibly qualified for the program earlier, and were among the first to receive the government laptops during 2010 and 2011. In the whole sample, one can spot a significant 3% (2.5 percentage points) increase in promotion rates among the schools in districts with high baseline computer lab rates. However, this effect disappears after excluding 2011. There were no significant differences in post-intervention graduation rates across these two types of districts.

Panel C shows that public schools located in districts with a high share of internet-aided-instruction in schools in 2008, were 50% (20–30 percentage points) more likely to receive computers after the program. This might reflect the fact that these schools possibly qualified for the program and applied for the program earlier, and were among the first to receive the government laptops. For the most part, however, there were no significant differences in post-intervention promotion nor graduation rates across these two types of districts.

6 Conclusion and Discussion

This paper finds evidence of a positive effect of Argentina's secondary-school one-laptop-per-child program on school promotion and graduation rates. Further analysis of dropout rates indicates a worsening of dropouts after the program, indicating that it may have had different effects across cohorts. I analyze the hypothesis that schools that had more experience with technology before the intervention may have been more successful at incorporating it in schools for educational purposes, thus benefiting the most from the laptop program. I do not find strong evidence for this hypothesis. In fact, schools located in districts where the population was ex-ante more familiar with computers received comparatively less laptops and experienced relatively lower effects on promotion rates after the program than their counterparts; schools located in districts with high access to laboratories and teaching-aided-instruction received comparatively more computers but did not perform significantly better after 2011.

Argentina's one-laptop-per-child program came at a time of change in the education system.

In 2006, a reform declared secondary school mandatory in the entire territory.[32] Delayed efforts to abide by this regulation that are not captured in my observable school-quality characteristics could overlap with the laptop program, confounding results. A reform in 2012 eliminated primary school repetition in first grade; the discussion and approval of this law could have influenced teacher's decisions regarding promotion throughout the educational system.[33]

Conectar Igualdad serves as a case study for what would happen in a country that sets out to eliminate its digital divide among secondary-school students. On the one hand, I would expect my findings to be an upper bound to what would occur in other countries, since Argentina has a tuition-free and unrestricted public school and university system so financial restrictions to education may be less important compared to other countries. On the other hand, Argentina has a vast territory with varying quality among public schools, many of which may face higher frictions to incorporating technology in education.

In terms of implications for public policy, my findings suggest that expansions in access to technology may be more beneficial for educational attainment when implemented among older students (secondary school rather than primary school, with an emphasis on high-school students); policy makers should also consider equipping computers with a widely-used operating system (such as Windows). I did not find evidence to suggest that experience at using internet in the classroom is beneficial for students who participate in the program.

But school completion is not necessarily the most important educational outcome. Results from the international PISA evaluations among 15 year old students in 2012 showed that Argentina was in the bottom 8 out of 65 countries; 2/3 of Argentine students had not achieved the minimal requirements in math and 1/2 had not achieved the minimal requirements in Spanish and Sciences. A serious evaluation of one-laptop-per-child programs would require taking more outcomes and distributional concerns into consideration. Equal access to information and communication technologies might be seen as a goal in itself. [34][35]

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[32] Ley de Educación Nacional (LEN) N.º 26.206/2006.

[33] https://www.clarin.com/sociedad/titulo_0_Bkbg-cM3wQg.html

[34] The United Nations has argued that all people must be able to access the internet in order to exercise and enjoy their rights to freedom of expression and opinion and other fundamental human rights, and that states have a responsibility to ensure that internet access is broadly available (World Summit on the Information Society, 2003).

[35] This view appears to be shared by many: In 2012, 83% of the over 10,000 individuals in 20 countries interviewed by the Information Society agreed with the statement that "access to the internet should be considered a basic human right."

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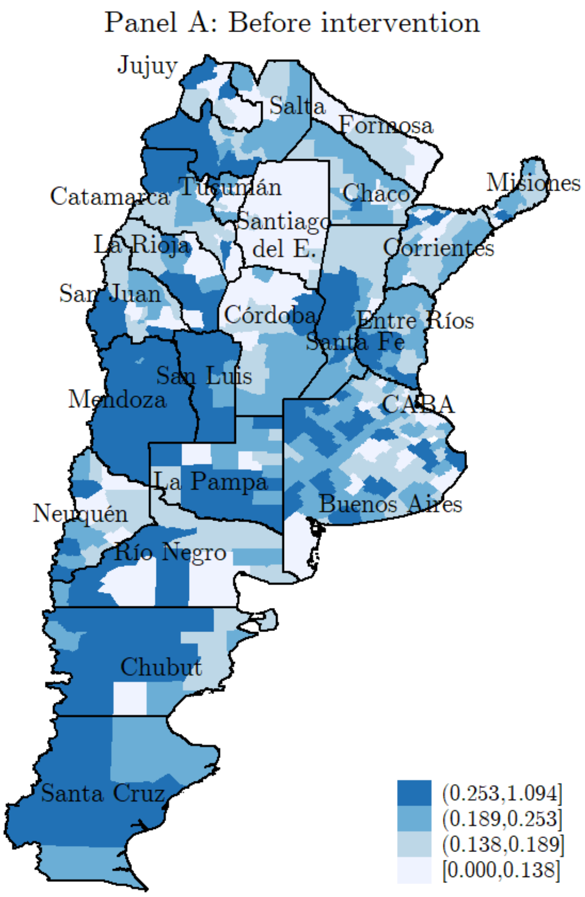
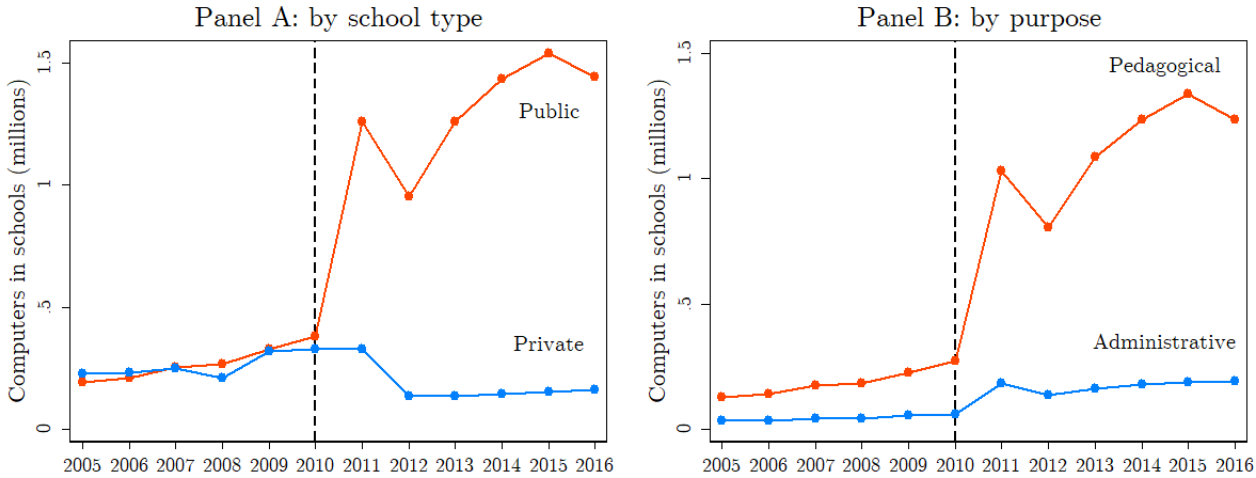
7 Figures and Tables

Figure 1: Computers per student, before and after Conectar Igualdad



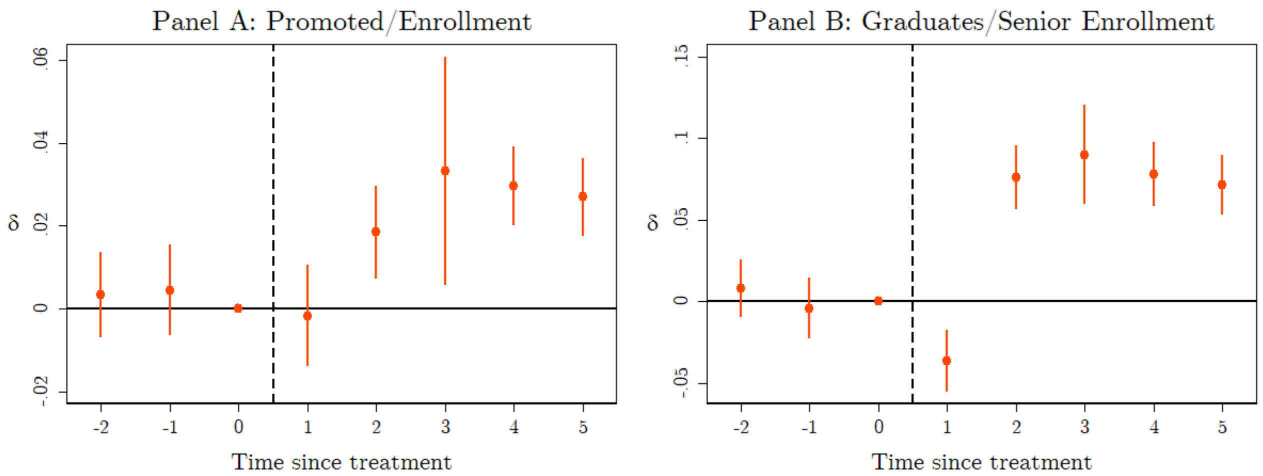
Notes: Panel A summarizes the geographic distribution of computer access in schools in Argentina among secondary school students at the start of 2010, right before rollout of Conectar Igualdad. Panel B summarizes the geographic distribution of computer access in schools in Argentina among secondary school students at the start of 2015.
Source: Relevamiento Anual 2010–2015, Dirección de Información y Estadística Educativa, Ministerio de Educación –Argentina.

Figure 2: Number of computers in secondary schools, variations across school type and purpose



Notes: This figure shows the number of computers (in millions) available in secondary schools of Argentina, before and after the start of the laptop program Conectar Igualdad. Panel A shows that the number of computers increased only in public schools; Panel B shows that the additional computers were mostly intended for pedagogical purposes.
Source: Relevamiento Anual 2005–2016, DiNIEE, Ministerio de Educación –Argentina.

Figure 3: Effect of the laptop program on secondary school performance, all schools 2008–2015



Notes: This figure plots the estimates of δ_t (annual treatment effects) for each year resulting from estimating equation 1 on a district-by-year sample between 2008 and 2015. The x-axis (time since treatment) corresponds to the number of years since the start of the program, in 2010. All variables are measured at the start of the academic year; consequently, the number promoted and graduated is lagged one period. Panel A focuses on the share of promoted students in secondary schools; Panel B focuses on the share of students who graduate from secondary schools. Vertical lines show 95% confidence intervals based on robust standard errors.
Source: Relevamiento Anual 2008–2016, DiNIEE, Ministerio de Educación –Argentina.

Table 1: Descriptive statistics across districts, secondary schools 2008–2015

	All schools	Public	Private
Schools per district	27	19	8
Students per district	7,147	5,127	2,700
Computers per district	2,186	1,768	419
Computers per student	0.408	0.442	0.194
Fraction public schools	0.809	–	–
Fraction computers for educational purposes	0.733	0.736	0.643
Fraction schools with internet connection	0.585	0.538	0.824
Fraction schools that use internet in class	0.481	0.434	0.697
Fraction schools with computer labs	0.557	0.510	0.787
Students per school	228	248	230
Fraction female	0.517	0.506	0.557
Teachers per student	0.208	0.217	0.184
Weekly hours taught per teacher	8.043	7.857	8.956
Fraction indigenous	0.027	0.028	0.008
Fraction rural	0.125	0.133	0.077
Fraction deprived of liberty	0.001	0.001	0.001
Fraction of vulnerable districts	0.184	0.194	0.060
Fraction promoted	0.783	0.763	0.873
Fraction graduated	0.724	0.641	0.756
Fraction schools with internet in classroom (2015)	0.314	0.306	0.326
Fraction schools that teach with the internet (2008)	0.336	0.276	0.543
Fraction schools with computer labs (2008)	0.538	0.474	0.827
Fraction computers per student (2008)	0.158	0.147	0.206

Notes: Summary statistics (means) computed on a district-by-year sample of secondary schools in Argentina. Vulnerable districts defined as those in which the secondary school student population is above 90th percentile rural (30 percent), indigenous (5 percent), or deprived of liberty (0.02 percent).
Source: Relevamiento Anual 2008–2016, DiNIEE, Ministerio de Educación –Argentina.

Table 2: Effect of intervention on school performance —dynamic approach

	Computers/Enrollment		Promoted/Enrollment		Graduated/Senior Enrollment	
Panel A: All schools	(1)	(2)	(3)	(4)	(5)	(6)
A1. Treatment Effects						
Year 1 After LP	0.233***	0.249***	-0.00173	0.000579	-0.0364***	-0.0382***
	(0.0215)	(0.0219)	(0.00624)	(0.00605)	(0.00972)	(0.00993)
Year 2 After LP	0.227***	0.244***	0.0184***	0.0185***	0.0761***	0.0741***
	(0.0207)	(0.0215)	(0.00565)	(0.00603)	(0.00995)	(0.0105)
Year 3 After LP	0.343***	0.362***	0.0333**	0.0300***	0.0900***	0.0876***
	(0.0200)	(0.0210)	(0.0140)	(0.0102)	(0.0156)	(0.0136)
Year 4 After LP	0.388***	0.408***	0.0295***	0.0210**	0.0780***	0.0735***
	(0.0195)	(0.0212)	(0.00488)	(0.00889)	(0.00997)	(0.0128)
Year 5 After LP	0.432***	0.453***	0.0270***	0.0182**	0.0715***	0.0680***
	(0.0212)	(0.0232)	(0.00481)	(0.00876)	(0.00948)	(0.0124)
A2. Validity Check						
Year 2 Before LP	-0.0495***	-0.0534***	0.00333	0.00522	0.00788	0.000562
	(0.0181)	(0.0207)	(0.00523)	(0.00608)	(0.00896)	(0.0109)
Year 1 Before LP	-0.0214	-0.0187	0.00453	0.00829	-0.00418	-0.00558
	(0.0176)	(0.0179)	(0.00563)	(0.00598)	(0.00945)	(0.00980)
Mean	0.408	0.408	0.783	0.783	0.701	0.701
Observations	4,208	4,208	4,208	4,208	4,200	4,200
Number of districts	526	526	526	526	525	525
Panel B: Public v.s Private						
B1. Treatment Effects						
Year 1 After LP	0.287***	0.312***	-0.0261***	-0.0269***	-0.0283*	-0.0313*
	(0.0266)	(0.0258)	(0.00926)	(0.00989)	(0.0158)	(0.0161)
Year 2 After LP	0.325***	0.347***	-0.00681	-0.0121	0.0262*	0.0216
	(0.0314)	(0.0322)	(0.00775)	(0.00852)	(0.0133)	(0.0139)
Year 3 After LP	0.499***	0.523***	-0.000739	-0.00522	0.0283	0.0263
	(0.0311)	(0.0298)	(0.00856)	(0.00963)	(0.0208)	(0.0212)
Year 4 After LP	0.530***	0.555***	-0.00142	-0.00812	0.0287*	0.0228
	(0.0313)	(0.0299)	(0.00854)	(0.00994)	(0.0168)	(0.0170)
Year 5 After LP	0.573***	0.596***	-0.00334	-0.0104	0.0137	0.00849
	(0.0339)	(0.0321)	(0.00848)	(0.00988)	(0.0153)	(0.0156)
B2. Validity Check	0.0273***	0.0377***	-0.0104	-0.00811	0.0104	0.0122
Year 2 Before LP	(0.00955)	(0.0112)	(0.00997)	(0.00966)	(0.0158)	(0.0161)
	-0.00848	-0.00312	-0.00922	-0.00759	0.0288*	0.0293*
Year 1 Before LP	(0.00515)	(0.00699)	(0.0123)	(0.0117)	(0.0170)	(0.0171)
Mean	0.284	0.284	0.806	0.806	0.654	0.654
Observations	4992	4992	4992	4992	3128	3128
Number of districts	312	312	312	312	280	280
District FE	✔	✔	✔	✔	✔	✔
Trends by District	✔	✔	✔	✔	✔	✔
Controls	✘	✔	✘	✔	✘	✔

Notes: Panels A, B, and C show estimates of δ_t for each year resulting from estimating equations 1, 2, and 3 respectively between 2008 and 2015. Panel A estimates equation 1 on a district-by-year sample of secondary schools. Panel B estimates equation 2 on a district-by-sector-by-year sample of public and private secondary schools. Panel C estimates equation 3 on a district-by-level-by-year sample of primary and secondary schools. Controls include district-by-year level characteristics such as student vulnerability, student gender, teachers per student, hours per teacher, and share of public schools. All variables are measured at the start of the academic year; consequently, the number promoted and graduated is lagged one period. Robust (Panel A) and district-clustered (Panels B and C) standard errors are in parentheses.
Source: Relevamiento Anual 2008–2016, DiNIEE, Ministerio de Educación –Argentina.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2 (Continued): Effect of intervention on school performance —dynamic approach

	Computers/Enrollment		Promoted/Enrollment		Graduated/Senior Enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C: Secondary v. Primary						
C1. Treatment Effects						
Year 1 After LP	0.186***	0.213***	-0.0102	-0.0135**	-0.0479***	-0.0411***
	(0.0178)	(0.0196)	(0.00739)	(0.00544)	(0.0104)	(0.00939)
Year 2 After LP	0.141***	0.170***	-0.00195	0.00307	0.0680***	0.0717***
	(0.0168)	(0.0203)	(0.00480)	(0.00451)	(0.00819)	(0.00814)
Year 3 After LP	0.247***	0.257***	-0.0130***	-0.00906**	0.0709***	0.0632***
	(0.0181)	(0.0206)	(0.00473)	(0.00440)	(0.00869)	(0.00879)
Year 4 After LP	0.280***	0.283***	-0.00300	0.00406	0.0684***	0.0629***
	(0.0192)	(0.0218)	(0.00477)	(0.00470)	(0.00961)	(0.00899)
Year 5 After LP	0.286***	0.268***	-0.00536	0.0000977	0.0658***	0.0536***
	(0.0198)	(0.0225)	(0.00448)	(0.00468)	(0.00940)	(0.00921)
C2. Validity Check	-0.00129	-0.00646	0.0140***	0.00790	0.00525***	0.0244***
Year 2 Before LP	(0.00178)	(0.0111)	(0.00474)	(0.00502)	(0.000995)	(0.00883)
	-0.00176	0.00266	0.00893	0.0118**	-0.00351***	0.0143*
Year 1 Before LP	(0.00123)	(0.00368)	(0.00615)	(0.00558)	(0.000955)	(0.00825)
Mean	0.301	0.271	0.865	0.861	0.841	0.828
Observations	8112	6080	8112	6080	7032	5552
Number of districts	507	406	507	406	507	383
District FE	✔	✔	✔	✔	✔	✔
Time FE	✔	✔	✔	✔	✔	✔
Controls	✘	✔	✘	✔	✘	✔

Notes: Panels A, B, and C show estimates of δ_t for each year resulting from estimating equations 1, 2, and 3 respectively between 2008 and 2015. Panel A estimates equation 1 on a district-by-year sample of secondary schools. Panel B estimates equation 2 on a district-by-sector-by-year sample of public and private secondary schools. Panel C estimates equation 3 on a district-by-level-by-year sample of primary and secondary schools. Controls include district-by-year level characteristics such as student vulnerability, student gender, teachers per student, hours per teacher, and share of public schools. All variables are measured at the start of the academic year; consequently, the number promoted and graduated is lagged one period. Robust (Panel A) and district-clustered (Panels B and C) standard errors are in parentheses.
Source: Relevamiento Anual 2008–2016, DiNIEE, Ministerio de Educación –Argentina.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Effect of intervention on school performance —trend-break approach

	Computers/Initial Enrollment		Promoted/Enrollment		Graduated/Senior Enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
A. All						
Treatment Effect	0.145***	0.151***	-0.00305	0.00484	-0.0153	-0.00945
	(0.0195)	(0.0204)	(0.00682)	(0.00897)	(0.0103)	(0.0118)
Treatment Effect w/o 2011	0.127***	0.130***	0.0195*	0.0255*	0.0911***	0.0955***
	(0.0247)	(0.0243)	(0.0111)	(0.0135)	(0.0153)	(0.0168)
Mean	0.408	0.408	0.783	0.783	0.701	0.701
Observations	4208	4208	4208	4208	4200	4200
Number of Districts	526	526	526	526	525	525
B. Public vs. Private						
Treatment Effect	0.187***	0.193***	-0.0216**	-0.0198**	-0.0248	-0.0182
	(0.0295)	(0.0327)	(0.00850)	(0.00819)	(0.0169)	(0.0170)
Treatment Effect w/o 2011	0.245***	0.234***	-0.00634	-0.00782	0.0298	0.0374*
	(0.0409)	(0.0457)	(0.0107)	(0.0111)	(0.0198)	(0.0200)
Mean	0.284	0.284	0.806	0.806	0.654	0.654
Observations	4992	4992	4992	4992	3128	3128
Number of Districts	312	312	312	312	280	280
C. Secondary vs. Primary						
Treatment Effect	0.116***	0.164***	-0.0126**	-0.0170***	-0.0361***	-0.0136
	(0.0232)	(0.0228)	(0.00591)	(0.00438)	(0.00971)	(0.00901)
Treatment Effect w/o 2011	0.0886***	0.120***	-0.00437	-0.00454	0.0726***	0.0898***
	(0.0300)	(0.0289)	(0.00594)	(0.00574)	(0.0103)	(0.0101)
Mean	0.301	0.252	0.865	0.862	0.841	0.819
Observations	8112	5184	8112	5184	7032	5184
Number of Districts	507	337	507	337	507	337
District FE	✔	✔	✔	✔	✔	✔
Trends by District	✔	✔	✔	✔	✔	✔
Controls	✘	✔	✘	✔	✘	✔

Notes: Panels A, B, and C show estimates of β resulting from estimating equations 4, 5, and 6 respectively between 2008 and 2015. Panel A estimates equation 4 on a district-by-year sample of secondary schools. Panel B estimates equation 5 on a district-by-sector-by-year sample of public and private secondary schools. Panel C estimates equation 6 on a district-by-level-by-year sample of primary and secondary schools. Controls include district-by-year level characteristics such as student vulnerability, student gender, teachers per student, hours per teacher, and share of public schools. All variables are measured at the start of the academic year; consequently, the number promoted and graduated is lagged one period. Robust standard errors are in parentheses.
Source: Relevamiento Anual 2008–2016, DiNIEE, Ministerio de Educación –Argentina.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effect of intervention on school performance on districts with high technology at baseline —dynamic approach

	Computers/Enrollment		Promoted/Enrollment		Graduated/Senior Enrollment	
Panel A: Household computers	(1)	(2)	(3)	(4)	(5)	(6)
A1. Complete sample						
Year 1 After LP	-0.0379	-0.0288	-0.0110	-0.0155	0.00993	-0.000534
	(0.0465)	(0.0461)	(0.0191)	(0.0152)	(0.0221)	(0.00240)
Year 2 After LP	-0.331***	-0.322***	-0.0154	-0.0292**	0.00488	0.00230
	(0.0531)	(0.0531)	(0.0168)	(0.0115)	(0.0185)	(0.00226)
Year 3 After LP	-0.479***	-0.474***	-0.0383	-0.0383	0.0169	0.00207
	(0.0545)	(0.0549)	(0.0347)	(0.0326)	(0.0206)	(0.00243)
Year 4 After LP	-0.495***	-0.488***	0.00556	0.00254	0.00745	-0.00111
	(0.0501)	(0.0502)	(0.0164)	(0.0117)	(0.0222)	(0.00246)
Year 5 After LP	-0.562***	-0.562***	0.00868	0.0111	-0.0111	-0.00333
	(0.0531)	(0.0542)	(0.0159)	(0.0113)	(0.0213)	(0.00238)
A2. Validity Check						
Year 2 Before LP	0.00169	-0.00619	-0.00182	0.00338	0.0227	0.0294
	(0.00935)	(0.0169)	(0.0169)	(0.0137)	(0.0187)	(0.0199)
Year 1 Before LP	-0.0220***	-0.0192***	0.0163	0.0166	-0.00236	-0.00244
	(0.00614)	(0.00730)	(0.0193)	(0.0140)	(0.0182)	(0.0182)
Mean	0.446	0.446	0.764	0.764	0.635	0.0455
Observations	4072	4072	4072	4072	2752	4072
Number of districts	509	509	509	509	344	509
Panel B: Computer Labs						
B1. Complete sample						
Year 1 After LP	0.180***	0.170**	-0.0112	0.00283	0.00713	0.00899
	(0.0662)	(0.0668)	(0.0338)	(0.0164)	(0.0237)	(0.0236)
Year 2 After LP	0.00267	-0.00870	-0.0239	-0.00784	-0.0262	-0.0231
	(0.0524)	(0.0525)	(0.0337)	(0.0161)	(0.0214)	(0.0216)
Year 3 After LP	0.0399	0.0283	-0.0669*	-0.0505**	-0.0600***	-0.0570***
	(0.0570)	(0.0580)	(0.0394)	(0.0211)	(0.0193)	(0.0194)
Year 4 After LP	0.0751	0.0608	-0.0418	-0.0177	-0.0636***	-0.0610***
	(0.0536)	(0.0539)	(0.0330)	(0.0150)	(0.0205)	(0.0205)
Year 5 After LP	0.0895	0.0794	-0.0463	-0.0158	-0.110***	-0.107***
	(0.0588)	(0.0594)	(0.0334)	(0.0164)	(0.0229)	(0.0234)
B2. Validity Check						
Year 2 Before LP	0.0303	0.0539	-0.0376	-0.00894	0.0115	0.00716
	(0.0464)	(0.0474)	(0.0340)	(0.0160)	(0.0208)	(0.0208)
Year 1 Before LP	0.00656	0.0173	-0.0456	-0.00929	0.0262	0.0254
	(0.0438)	(0.0437)	(0.0350)	(0.0173)	(0.0245)	(0.0250)
Mean	0.446	0.446	0.767	0.767	0.631	0.631
Observations	3376	3376	3376	3376	2176	2176
Number of districts	422	422	422	422	272	272
District FE	✔	✔	✔	✔	✔	✔
Time FE	✔	✔	✔	✔	✔	✔
Controls	✘	✔	✘	✔	✘	✔

Notes: Panels A, B, and C show estimates of δ_t for each year resulting from estimating equations 10, 11, and 12 respectively on a district-by-year sample of public secondary schools between 2008 and 2015. Controls include district-by-year level characteristics such as student vulnerability, student gender, teachers per student, hours per teacher, and share of public schools. All variables are measured at the start of the academic year; consequently, the number promoted and graduated is lagged one period. Robust standard errors are in parentheses.

Source: Relevamiento Anual 2008–2016, DiNIEE, Ministerio de Educación –Argentina; Census of Population 2001.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4 (Continued): Effect of intervention on school performance on districts with high technology at baseline —dynamic approach

	Computers/Enrollment		Promoted/Enrollment		Graduated/Senior Enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C: Internet-aided Instruction						
C1. Treatment Effects						
Year 1 After LP	0.204***	0.194***	0.0239*	0.00821	0.0125	0.0138
	(0.0724)	(0.0730)	(0.0142)	(0.0139)	(0.0297)	(0.0295)
Year 2 After LP	0.316***	0.310***	0.00710	-0.0103	-0.00959	-0.00913
	(0.0823)	(0.0824)	(0.0140)	(0.0142)	(0.0246)	(0.0246)
Year 3 After LP	0.350***	0.340***	-0.00256	-0.0100	-0.0200	-0.0185
	(0.0765)	(0.0770)	(0.0209)	(0.0208)	(0.0238)	(0.0239)
Year 4 After LP	0.278***	0.270***	-0.00210	-0.0165	-0.0487*	-0.0467*
	(0.0652)	(0.0660)	(0.0138)	(0.0133)	(0.0283)	(0.0282)
Year 5 After LP	0.295***	0.292***	0.00179	-0.0104	-0.0525**	-0.0502**
	(0.0769)	(0.0763)	(0.0135)	(0.0136)	(0.0236)	(0.0234)
C2. Validity Check						
Year 2 Before LP	0.00958	0.0153	0.00794	0.00803	-0.0102	-0.0125
	(0.0644)	(0.0653)	(0.0163)	(0.0162)	(0.0240)	(0.0243)
Year 1 Before LP	0.0150	0.0188	0.0000817	0.00338	-0.00476	-0.00440
	(0.0642)	(0.0648)	(0.0140)	(0.0141)	(0.0239)	(0.0240)
Mean	0.442	0.442	0.763	0.763	0.631	0.631
Observations	4208	4208	4208	4208	2880	2880
Number of districts	526	526	526	526	360	360
District FE	✔	✔	✔	✔	✔	✔
Time FE	✔	✔	✔	✔	✔	✔
Controls	✘	✔	✘	✔	✘	✔

Notes: Panels A, B, and C show estimates of δ_t for each year resulting from estimating equations 10, 11, and 12 respectively on a district-by-year sample of public secondary schools between 2008 and 2015. Controls include district-by-year level characteristics such as student vulnerability, student gender, teachers per student, hours per teacher, and share of public schools. All variables are measured at the start of the academic year; consequently, the number promoted and graduated is lagged one period. Robust standard errors are in parentheses.

Source: Relevamiento Anual 2008–2016, DiNIEE, Ministerio de Educación –Argentina; Census of Population 2001.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effect of intervention on school performance on districts with high technology at baseline —trend-break approach

	Computers/Enrollment		Promoted/Enrollment		Graduated/Senior Enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Computers in 2001						
Treatment Effect	0.0310	0.0430	-0.0434***	-0.0451***	0.0196	0.0213
	(0.0449)	(0.0447)	(0.0121)	(0.0120)	(0.0213)	(0.0214)
Treatment Effect w/o 2011	-0.202***	-0.200***	-0.0517***	-0.0521***	0.0308	0.0329
	(0.0512)	(0.0506)	(0.0126)	(0.0129)	(0.0246)	(0.0253)
Mean	0.428	0.428	0.757	0.757	0.635	0.635
Observations	3864	3864	3864	3864	2752	2752
Number of Districts	483	483	483	483	344	344
B. Computer Labs in 2008						
Treatment Effect	0.175***	0.167***	0.0267**	0.0234**	0.0313	0.0260
	(0.0630)	(0.0638)	(0.0121)	(0.0117)	(0.0254)	(0.0257)
Treatment Effect w/o 2011	0.0603	0.0516	0.00400	0.00107	0.0120	0.00635
	(0.0606)	(0.0609)	(0.0146)	(0.0145)	(0.0297)	(0.0300)
Mean	0.423	0.423	0.760	0.760	0.631	0.631
Observations	3176	3176	3176	3176	2176	2176
Number of Districts	397	397	397	397	272	272
C. Teaching with Internet in 2008						
Treatment Effect	0.212***	0.199***	0.0205*	0.0164	0.0343	0.0340
	(0.0682)	(0.0689)	(0.0110)	(0.0114)	(0.0291)	(0.0292)
Treatment Effect w/o 2011	0.332***	0.323***	0.00372	0.00282	0.0163	0.0155
	(0.0911)	(0.0915)	(0.0141)	(0.0142)	(0.0316)	(0.0319)
Mean	0.424	0.424	0.757	0.757	0.631	0.631
Observations	4000	4000	4000	4000	2880	2880
Number of Districts	500	500	500	500	360	360
District FE	✔	✔	✔	✔	✔	✔
Trends by District	✔	✔	✔	✔	✔	✔
Controls	✘	✔	✘	✔	✘	✔

Notes: Panels A, B, and C show estimates of β resulting from estimating equations 10 11 and 12 respectively on a district-by-year sample of public secondary schools between 2008 and 2015. Controls include district-by-year level characteristics such as student vulnerability, student gender, teachers per student, hours per teacher, and share of public schools. All variables are measured at the start of the academic year; consequently, the number promoted and graduated is lagged one period. Robust standard errors are in parentheses.

Source: Relevamiento Anual 2008–2016, DiNIEE, Ministerio de Educación –Argentina; Census of Population 2001.

* p < 0.1, ** p < 0.05, *** p < 0.01