Technology and Educational Choices: Evidence from a One-Laptop-per-Child Program

Maria Lucia Yanguas*

March 16, 2020

Abstract

This paper provides the first causal estimates of the effect of children's access to computers and the internet on educational outcomes in early adulthood, such as schooling and choice of major. I exploit cross-cohort variation in access to technology among primary and middle school students in Uruguay, the first country to implement a nationwide one-laptop-per-child program. Despite a notable increase in computer access, educational attainment has not increased; the schooling gap between private and public school students has persisted, despite closing the technology gap. Among college students, those who had been exposed to the program as children were less likely to enroll in science and technology.

JEL CODES: I21, I24, I28, H52

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

1 Introduction

Governments around the globe have become increasingly concerned about the economic consequences of unequal access to technology among school children.[1] One class of pro-

*E-mail: myanguas@ucla.edu. This paper is based on the main chapter of my doctoral dissertation at UCLA's Department of Economics. I am especially grateful to my advisers Adriana Lleras-Muney, Till von Wachter, Leah Boustan, and Michela Giorcelli, for their guidance and support. I am also grateful to Cory Koedel and two anonymous referees at the *Economics of Education Review* for their thoughtful comments. I thank Moshe Buchinsky, Mauricio Mazzocco, Ricardo Perez-Truglia, Rodrigo Pinto, Sarah Reber, Manisha Shah, and Melanie Wasserman for helpful feedback. I thank my colleagues Elior Cohen, Brett McCulley, Bruno Pellegrino, and seminar participants at UCLA, Universidad de la Republica del Uruguay and the Evidence-Based-Economics 2018 conference for their comments. 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).

[1] See Goal 9 of the United Nation's 2030 Agenda for Sustainable Development.

grams that has received considerable support and media attention is the one-laptop-per-child initiative, which provides personal laptops to school children and has thus far been implemented in at least 42 countries.[2] Underlying the adoption of these programs is the idea that broadening access to computers among school children will increase their access to learning opportunities and decrease future inequalities.[3] However, policy evaluations of one-laptop-per-child initiatives have found no short-term effects on a set of social, educational, and cognitive outcomes (Beuermann et al., 2015), and have not yet examined the overall, long-term effects on human-capital accumulation. As children grow older, they become responsible for a larger set of educational decisions, face higher opportunity costs of schooling, are exposed to developmental factors that shape their interactions with technology (Heckman, 2006; Doyle et al., 2009), and get more years of exposure to computers and the internet, which may increase their ability to use technology effectively (Van Deursen et al., 2011). Marginal differences in motivation and performance, barely noticeable in primary and middle school, may be amplified by the time the children reach young adulthood (Schweinhart et al., 2005).

In this paper, I examine the early-adulthood educational outcomes of students who were provided laptops and internet access as school children. To this end, I use evidence from Plan Ceibal in Uruguay, the first nationwide one-laptop-per-child program, and investigate its effect on children's educational attainment and choice of major one decade after its initial implementation.[4] Starting in 2007, Plan Ceibal delivered a personal laptop to each student in primary and middle schools within the public education system and equipped all public schools with wireless internet access. To my knowledge, this is the first paper to consider the long-run effects of a one-laptop-per-child program of this scale.

To identify the causal effect of the intervention, I first link participation in the program to children's early-adult educational outcomes, combining survey and administrative data from the National Institute of Statistics and the public university system of Uruguay. Provincially representative monthly household survey data (Encuesta Continua de Hogares; henceforth, ECH) allow me to track access to technology in the home as well as educational characteristics, and administrative data on all students enrolled in the pub-

[2] National partners of the One-Laptop-Per-Child organization include Uruguay, Peru, Argentina, Mexico, and Rwanda. Other significant projects have been started in Afghanistan, Ethiopia, Haiti, Mongolia, and Palestinian territories. In the United States, the most famous implementation was OLPC Birmingham (Alabama). For a review of technology-based approaches in education, see Escueta et al. (2017).

[3] The 2017 *Measuring the Information Society Report* argues that recent technological advances might enable innovations that increase efficiency and productivity and improve livelihoods around the globe.

[4] Uruguay is a small country in South America. In 2013, the United Nations ranked it as a high-income country, with a population of 3.2 million people and a GDP per capita of $19,942 PPP.

lic university system allow me to track characteristics of university students and their academic choices.[5] I then use information about an individual's cohort and location to approximate their likelihood of being exposed to the program: the cohorts of students who were finishing middle school when the intervention arrived in their province did not receive laptops, but the younger students did. I therefore use an interrupted time-series (also known as regression discontinuity in time) with province-specific trends to compare the educational attainment of individuals who were or were not exposed to the program over time. Identification comes from detecting discontinuities in province-specific trends around the first cohort exposed to the program in each province. The critical assumption is that the province-specific trend up to the first treated cohort is a good counterfactual for the outcomes of interest. I validate my findings using a controlled interrupted time series that compares cohorts of public and private school students over time.

I first document that the program was implemented successfully—the rollout was complete by 2009 for primary schools and 2011 for middle schools, and essentially everyone who was targeted received a laptop. I estimate that the program increased students' access to a home computer by almost 30% in 2011 (up 20 percentage points from 70% to 90%), while internet access in public primary schools more than doubled (up 40 percentage points from 26% to 70%) between 2006 and 2009. The unprecedented scope and scale of the program make for a great setting in which to conduct this research.

I then consider the effects of the program on educational outcomes, starting with educational attainment. I examine total years of education as well as high school, post-secondary, and university enrollment, and high school graduation rates. Contrary to what one might expect of a program designed to improve learning, diverse specifications offer no evidence that the program had any positive effect on educational attainment. I estimate that total years of education decreased, on average, by one month, although this figure is not statistically different from zero. To understand this finding, I explore the two main reasons for dropping out of high school as reported by students: lack of interest in education, and finding employment. I find that while most students use the internet for entertainment, very few of them report using it for learning activities, and the program does not appear to have increased employment among adolescents.

Next, I investigate whether the program had any effects on choice of major, conditional on attending university. I use administrative data on all incoming students to Universidad de la Republica, Uruguay's tuition-free, largely unrestricted public university system, which enrolls over 80% of the country's university students. According to a recent survey,

[5] I use the term province to refer to Uruguay's "departamentos," independent administrative divisions comparable to U.S. states.

36% of alumni would choose a different major were they given the chance to go back in time.[6] Access to information about the degrees offered and how they are valued by the market, as well as online vocational tests, could improve the quality of the match between students and their major. According to the survey, which groups students in three categories according to their field of study, overall satisfaction was highest among graduates of science and technology and lowest among the social sciences (the remaining category was health). In addition, some may expect access to computers to foster interest in technology-related majors. However, contrary to these expectations, I find that the intervention was associated with a significantly lower rate of enrollment in science and technology and a higher rate of enrollment in the social sciences. A more granular analysis suggests that the laptop program had a nonsignificant but generally negative association with enrollment in technology and computer-related majors. Consistent with my findings for overall educational attainment, I find a negative association but no significant effects of the program on the share of students who applied for a scholarship.

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. 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, which might not reflect immediately in test scores. It is unclear how this intervention will affect college entry: on the one hand, technical skills may be more valuable in college than in primary and secondary school (Escueta et al., 2017); on the other hand, computer skills that are valuable in the labor market may discourage children from furthering their education. In the longer run, prolonged exposure to information technologies might affect the way students learn about the costs and benefits of college and career choices.[7] For instance, Hoxby et al. (2013) show that providing students with semi-customized information on the application process and college's net costs, as well as no-paperwork application fee waivers, causes high-achieving, low-income students to apply and be admitted to more and better colleges. College students who overestimate costs are less likely to matriculate and more likely to drop out, while students who overestimate earnings enroll in programs with historically worse outcomes (Hastings et al.,

[6] Survey run among students who graduated from Universidad de la Republica in 2013. In addition, 9% of alumni declared that their major is not related at all to their current occupation.

[7] These include searching for jobs on the internet, networking with potential employers, and producing adequate application materials.

2016). Once students are in college, their familiarity with computers may encourage them to pursue professions that involve or require this technology.[8] On the other hand, internet-connected computers that enable users to access a wider range of information may encourage students to pursue lesser known professions, or to pursue majors that better fit their abilities, interests, and employment and income prospects.

This paper makes three main contributions to the literature. First, this is, to the best of my knowledge, the first paper to examine the joint effect of school children's access to personal laptops and the internet on their educational outcomes in early adulthood, and the first to explore its effects on choice of major.[9] Second, this paper examines a program that incorporates the use of computers both at school and at home, whereas much of the existing literature focuses on one or the other. These correspond to very different types of treatments in terms of the setting and the intensity of the intervention, the key supporting actors, and the different constraints on the use of the technology. Third, this paper exploits a large-scale quasi-experimental design; therefore, it is minimally affected by the concerns of external validity associated with randomized controlled trials and is particularly relevant for informing policy.

Due to the popularity of these interventions, there is now abundant evidence on the short-term effects of computers on learning in primary and secondary school. De Melo et al. (2014) found that, two years after the intervention, Plan Ceibal had not influenced primary school students' math and reading scores. Similarly, Cristia et al. (2017) found that, one year after a similar primary school intervention (although with no internet treatment) in rural Peru, the program had increased computer use both at school and at home and some cognitive skills, but it had not influenced enrollment or math and language scores. These programs have yielded similar results to interventions focused on home computers only. For instance, Beuermann et al. (2015) found that home computers had no short-run effects on academic achievement or cognitive skills, while teachers reported lower student academic effort. 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 to no significant changes on math and reading achievement, cognitive skills, self-esteem, teacher perceptions, or school grades. More concerning, some studies found negative effects on academic achievement from interventions that are purely

[8] In psychology, this is known as the mere exposure effect, a phenomenon by which people tend to develop a preference for things merely because they are familiar with them.

[9] This is particularly critical in Uruguay, because—unlike in the United States—law and medical degrees are undergraduate options, and thus college majors are better predictors of career choice.

focused on expanding technology access (Malamud and Pop-Eleches, 2011; Vigdor et al., 2014). These findings contrast with positive effects found in alternative programs that use technology specifically for educational purposes (Banerjee et al., 2007; Roschelle et al., 2016). A few papers have 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, or initial enrollment. Dettling et al. (2015) found that, while high-speed internet access generally increased applications to college, the effects were concentrated among high-income students. Fairlie and London (2012) found some evidence that donating laptops to recently enrolled community-college students improves their educational outcomes.

The literature has typically found negligible effects of technology access on academic performance, with results switching from negative to positive with the educational level of the recipient. A potential explanation is that college students are likely more inclined, either by nature or by context, to use computers for educational purposes. Despite this, in a follow-up to the community-college experiment (Fairlie and Bahr, 2018), the authors matched students to employment and earnings records for seven years after the random provision of computers, and found no short- or medium-run effects on earnings or college enrollment. However, providing computer access to adults is different than providing it to children: developmental considerations and the likely presence of an experience curve mean that both the age at exposure and the years of exposure matter for treatment, while the effects of technology access on later-life outcomes such as income may operate through decisions made earlier in life, such as high school enrollment, graduation, and career choice. Once enrolled in tertiary education, there may be little scope for influencing educational attainment and career choices. In this paper, schooling and choice of major are observed at approximately age 19, but the treatment occurs in childhood; hence, there is enough time for children to develop technological skills and to face important educational milestones.[10] Primary and middle school education are mandatory and generally enforced by parents, while high school and tertiary education might not be enforced by parents and involve higher opportunity cost. Thus, the effects of the laptop program could be different in the longer run.

The rest of this paper proceeds as follows. Section 2 describes the program. Section 3 describes the data and summary statistics. Section 4 outlines the identification strategy and its implementation. Section 5 presents the results. Section 6 concludes.

[10] Over 80% of the young adults included in my analysis of household survey data are observed at age 19, while 18% are observed at age 20. Most entrants to Universidad de la Republica are aged 19 and under. I chose to look at outcomes around age 19 to maximize the number of cohorts included in my analysis. Replicating the analysis at around age 20 suggests that the laptop program had no significant effects on educational outcomes.

2 The One-Laptop-per-Child Program in Uruguay

One Laptop per Child (OLPC) is a nonprofit initiative founded in 2005 to empower the children of developing countries to learn by providing every school-age child with an internet-connected laptop and by creating and distributing educational devices and content. In 2007, the government of Uruguay, in partnership with the OLPC organization, launched Plan Ceibal, an ambitious program designed to eliminate the substantial technological gap between private and public school students. Plan Ceibal provides laptops with wireless modems to students and teachers in public primary schools, middle schools, and teacher training institutes. At its start, Plan Ceibal distributed the XO-1, a small, durable, efficient, low-cost laptop that functions much like a normal PC. Pricing was set to start at US$188 in 2006, when the typical laptop retailed for above $1,000. For more details about the laptop and program characteristics, refer to web Appendices A and B. As of December 2016, 1.6 million laptops had been deployed, double the number of children under age 15 living in the country.[11]

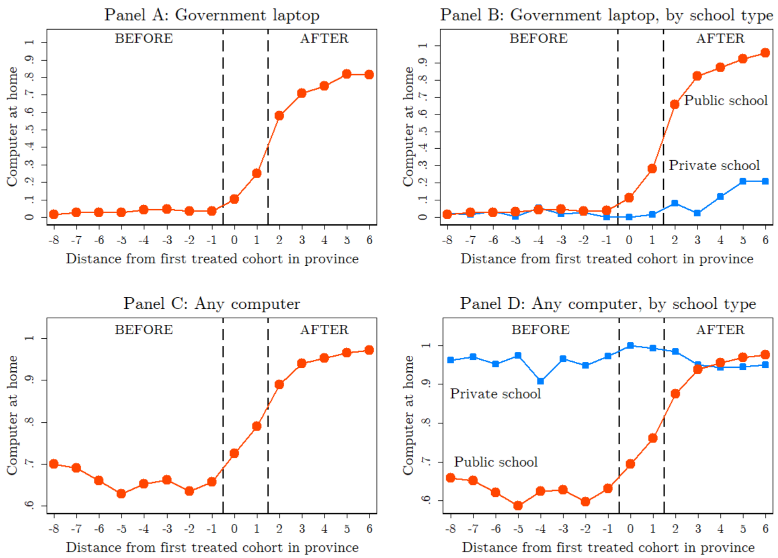
Plan Ceibal was implemented in two phases, each lasting three years (web Appendix Figure A1). Within each province, public primary schools were equipped with wireless internet access. Once internet access reached 90% penetration of these schools, Plan Ceibal distributed a personal computer to each primary school student enrolled in that province's public education system. Panel A summarizes the rollout of Plan Ceibal among primary school students between 2007 and 2009, when full coverage was attained. Uruguay has 19 provinces. The first (Florida) entered the program at the end of 2007; sixteen entered in 2008; finally, Canelones and Montevideo (where 40% of the population lives) entered at the start of 2009. This three-year spread yields three cohorts of students whose exposure to the program during primary school depended on their place of residence. Laptops were initially lent to these students; by design, they could take full ownership of their laptop upon completing primary school. Between 2007 and 2009, Plan Ceibal distributed 380,615 laptops in primary schools (as a reference, public primary schools enrolled 292,900 students in 2009).

Phase 2 focused on secondary schools. Panel B summarizes the rollout of Plan Ceibal among middle school students between 2009 and 2011 (Figure A1). In 2009, Plan Ceibal implemented a pilot program in the province of Treinta y Tres, where all middle school students received laptops and over 90% of the province's schools received wireless internet access. In 2010, the rollout was extended to second year middle school students in the

[11] Children would get two laptops in their lifetime: one in primary school and one in middle school, at which point the first laptop would be returned to the state.

provinces of Montevideo and Canelones. In 2011, the rollout was extended to the rest of the country. At this point, Plan Ceibal begun replacing the primary school laptops with newer laptops equipped with software that was geared toward middle school students. As with the primary school program, laptops were initially lent to students, who could take full ownership of them after completing middle school. In addition, from 2010 to 2014, some public high school students who had entered the technological track rather than the regular track (about 10%–15% of high school students) also received laptops. Between 2009 and 2011, Plan Ceibal distributed 134,111 laptops in secondary schools.

Figure 1: Differential access to computers in Uruguay as a result of the intervention
Variation across cohorts, provinces, and school types in 2011



Notes: This figure shows the fraction of individuals with a government laptop at home (Panels A and B) or any computer at home (Panels C and D) in a given cohort, stacked across provinces. A cohort is defined as the group of individuals that is expected to start primary school in the same academic year; it is estimated based on age, year, and month of the survey. In-between cohorts were exposed to the program to the extent that some individuals started primary school later than expected or repeated grades by the time the program arrived in their province. The sample is restricted to individuals living with no younger siblings between ages 5 and 18.
Source: ECH 2011.

Plan Ceibal was implemented successfully. Official program data indicate that by June 2010, 98% of public primary schools and 90% of public middle schools in the country had a wireless connection. School census data from the Ministry of Education (ANEP) indicate that the share of public primary schools with a functioning internet connection increased significantly during the program's expansion, from 26% in 2006 to 70% in 2009. Using data from Uruguay's 2011 monthly household survey (which I describe in more detail below), I track the fraction of individuals aged 10 to 27 who reported having a computer in their home in 2011: Figure 1 shows the evolution of computer access across cohorts of individuals in a cross-section of 2011, after coverage was complete.[12] Panel A shows that access to a government laptop at home was around 60% among treated cohorts up to five years after deployment; Panel B shows that essentially all public school students had government laptops, in striking contrast to private school students. See web Appendix Figure A2 for evidence that computer access among public school students increased even more dramatically over time than across cohorts. Comparing access to computers for all cohorts in 2011 is more conservative, as it accounts for any general equilibrium effects and unrelated spread of technology over time. Panels C and D suggest that the laptop program resulted in an approximately 30% (20 percentage points) increase in computer access among all individuals in the relevant cohorts and an approximately 50% (30 percentage points) increase when comparing public to private school students. These visual estimates compare well to those obtained from the regression analysis later on in the paper (16–19 percentage points and 27 percentage points, respectively).

3 Data and Summary Statistics

3.1 Data Sources

In this study, I combine household survey data from 2001 to 2018, which contains information on technology access and education, and administrative data from 2006 to 2016 from the public university system, which contains information about choice of major.

To establish that the program increased access to computers among the target population and to examine how the program affected educational outcomes, I relied on the 2001–2018 Uruguay Continuous Household Survey (ECH), which samples about 3.5% of private dwellings each year.[13] This publicly available monthly survey comprises independent cross-sections, representative at the provincial level. It provides standard information on education and labor-market outcomes, including the number of years of

[12] The specific question as it appears in the household portion of the survey is: *Does this home have a personal computer?* The informant is a member of the household (excluding domestic service) over 18 years old and mentally capable. An individual is said to have reported a computer at home whenever the household informant reports a computer.

[13] ECH stands for Encuesta Continua de Hogares. The sample size was half this figure before 2006. Estimate based on the 2004 and 2011 Census of Population and Dwellings.

education attended and/or completed, and whether the individual attended a public or private primary or middle school. In addition, the questionnaire inquires about technology ownership and use, including questions such as whether there is a computer or internet connection in the house; since 2009, the questionnaire inquires about government laptops specifically. Other useful variables include years of age, year and month of the survey, and migration history (since 2007, the survey has been asking about the province of birth, and since 2012, it has asked about the province of residence five years prior).[14]

To examine how the program affected choice of major, I obtained access to restricted administrative data on 208,946 incoming students to the Universidad de la Republica, which enrolls over 80% of all university students in the nation, between 2006 and 2016. This dataset contains the specific majors chosen by each student as well as their exact province and date of birth, year of high school graduation, location of primary and secondary school, and whether those were in the public or private school system. It also contains information on whether the child applied for financial aid or had to move to study a specific major, as well as individual and parental characteristics. My main analysis excludes the department of physical education (sports), which was incorporated into the dataset only in 2010. To select technological (or technology-related) majors, I web-scraped the descriptions of all undergraduate degrees on the university's website, searching for specific keywords: "computer," "computing," "digital," "informatics," "telecommunications," "technology," and "technological." This task yielded 17 majors, most of which the university classifies as STEM, Science and Technology (see A2 for a complete list). The three non-STEM exceptions are communications (social sciences), electronic and digital arts (art studies), and photographic imaging (art studies). Of the 17 majors, two were created after the first treated cohort reached college: biological engineering (2013) and electronic and digital arts (2014). Enrollment in technological majors generally decreased from 2006 to 2016 (web Appendix Figure A13). I defined computer-related majors as a subcategory of technological majors encompassing electrical engineering, computing engineering, technologist in informatics, and electronic and digital arts; computer-related majors capture about 5% of total enrollment.[15]

[14] To validate my results with independent data, I also collected aggregate enrollment data on the population as a whole from the Ministry of Education, including tabulated enrollment by calendar and academic year, province of school location, gender, and school type, and contacted the main universities in Uruguay (public: Universidad de la Republica; private: Universidad de Montevideo, Universidad Catolica, Universidad ORT) to collect tabulated data on their student demographics, including year of enrollment and province of origin for each year between 2010 and 2016. The resulting sample encompasses more than 95% of university students in Uruguay. To verify the expansion of internet access around the start of Plan Ceibal, I collected data on the availability of internet access at schools from the annual census of public primary schools, which was conducted by ANEP from 2002 to 2009.

[15] The sample corresponding to technological and computer majors is smaller than the sample used to

3.2 Summary Statistics

In the web Appendix, Table A1 shows summary statistics for individuals aged 18 to 20 in 2011 to 2018 using the household survey data. Half the sample is male, and approximately one out of five individuals is non-white. In terms of socioeconomic status, one out of ten lives below the poverty line, and 60% are not employed, which is comparable to the U.S. average among all age groups. Also, four out of five young adults still live with their parents or grandparents. In terms of access to technology, four out of five young adults have a computer at home, three out of five have a regular (non government) computer at home, and three out of five have internet access at home. In-home computers are usually shared: there is about one computer for every two persons in a household. Overall, 75% used a computer in the month prior to the survey, and 68% reported using the internet every day (according to the national EUTIC survey, only 42% of individuals age 15 to 20 had a smartphone at home in 2013). Internet use is spread evenly between entertainment, information, and communication (about 30% each), while about 10% is for education or learning activities (see web Appendix Figure A3, Panel B). Despite generating significant cross-cohort variation in computer access, the gap associated with program participation gradually decreased over time, disappearing by age 18.

In terms of education, the public sector is widespread: 85% of people who ever enrolled in primary school, middle school, or university did so in a public institution. Educational attainment is lower in Uruguay than in the United States, the OECD, and Latin America and the Caribbean.[16] The average years of education completed among individuals aged 18 to 20 is 10.1; only 60% ever attended a high school, and only 29% ever graduated from high school. Moreover, 12% attended technical school and 4% graduated from it. With respect to higher education, only 23% enrolled in any post-secondary education and only 19% enrolled in university. A considerable gap exists between public and private school students. Public school students have on average 9.8 years of education by age 20; private school students have on average 12.2, and almost all of them enroll in high school. Therefore, a large opportunity exists for increasing educational attainment. Universidad de la Republica charges no tuition and, conditional on obtaining a high school diploma,

analyze overall enrollment across the broader areas of study (like Science and Technology) for two reasons. First, while the area of study is assigned based on a mandatory entry in the survey, filling in a major is optional and has more missing values. Second, the former are examined at the individual level, while the latter are examined at the individual-by-area-of-study level.

[16] In a sample of Uruguayans aged 25–34 only 56% had at least some high school education in 2015, and only 39% had obtained a high school degree, compared to 90% in the US/OECD and 59% in Latin America and the Caribbean; only 21% had some post-secondary education and only 9% had obtained a post-secondary degree compared to 37.5–47% in US/OECD and 16% in Latin America and the Caribbean. Source: US Census Bureau, OECD, SEDLAC.

has no restrictions to entry.[17] This facilitates translating demand for college into actual enrollment, with a few caveats. For instance, students in remote areas might face moving costs to attend the classes and services that are highly centralized in Montevideo, and the university is plagued by low graduation rates (Boado (2005) estimates that only 28% of students graduate in a timely manner), a reminder that the opportunity cost of attending university is often substantially larger than the direct costs. Table A1 (Panel B) shows descriptive statistics of incoming students at Universidad de la Republica in the 2012–2016 period, after reducing the sample to Uruguay-born students aged 18 to 20. The average age in this sample is 19: most people (60%) are 19 years old, followed by 18 (27%) and 20 (13%). More than 60% of entering students are female, more than 50% are born in Montevideo, 67% did their primary education in the public sector, and 61% did their secondary education in the public sector. Regarding family background, about 20% of students declared that a parent had completed post-secondary education, almost half of the sample are first-generation college students (i.e., students whose parents did not enroll in college), and 65% are first-generation university students. In terms of academic performance, 30% of the sample had applied for a college scholarship for financial aid, the most popular being Fondo de Solidaridad, which grants a monthly stipend equivalent to half of a person's legal minimum income.[18] In addition, 16% enrolled in a technological major, 5% enrolled in multiple fields of study, and 4% had previous post-secondary studies.

4 Identification Strategy

4.1 Empirical Specification

To estimate the causal effect of Plan Ceibal on educational outcomes, I implement an interrupted time-series approach (also known as regression discontinuity in time) with province-specific trends.[19] This results in a combination of multiple interrupted time-

[17] Only two schools have some restrictions in the form of entrance exam or limited space: Escuela Universitaria de Tecnología Médica and Educación Física y Tecnicatura en Deportes (excluded).

[18] http://becas.fondodesolidaridad.edu.uy. This fund is a public organization created by law in 1994; it provides scholarships for post-secondary education in public institutions.

[19] Some researchers think of interrupted time series as a regression discontinuity in time (Hausman and Rapson, 2018); however the assumptions required in my setting are very different from a standard regression-discontinuity approach and have more to do with pretrends in the outcome being a good counterfactual for the post-intervention trends within provinces. This methodology is also essentially a difference-in-difference with province-specific trends rather than time fixed effects; this eliminates the cross-sectional dimension of a standard difference-in-difference approach and focuses entirely on the temporal dimension; again, the assumptions required in my setting are very different from a standard

series staggered over time, and allows me to compare educational outcomes of individuals who were or were not exposed to the program over time while allowing for differential trends across provinces. In my setting, identification comes from detecting discontinuities in province-specific trends around the first cohort exposed to the program in each province. The most important assumption is that the province-specific trend up to the first treated cohort is a good counterfactual for the outcomes of interest.

The strategy relies on the fact that students who were already in high school when the program arrived in their province did not receive a laptop, but those who were in primary school eventually received one. To assign treatment status to young adults, I start by documenting that school grade is a precise indicator of having received a government laptop within one year of the intervention in any given province. By combining the primary and middle school interventions in each province, I verify that the oldest students to enter the program were enrolled in ninth grade in 2009 in Treinta y Tres, expected to be enrolled in ninth grade in 2010 in Florida, and enrolled in ninth grade in 2011 in the rest of the country. Hence, there is a one to two-year gap in access to the program between Treinta y Tres, Florida, and the rest of the country (see web Appendix Table A3 for more details). This gap across school grades, which is not easily observable for adults, extends across birth cohorts: the oldest students to be exposed to the program in Florida and Treinta y Tres were on average one and two years older, respectively, than students in other provinces. In my analysis, I focus on young adults and do not observe the school grade they were enrolled in back when the program arrived in their province. Therefore, I must rely on their cohort of birth to classify individuals as eventually exposed or not exposed to the program. Birth cohorts are imperfect indicators of who received a government laptop in a given province because repetition rates are relatively high. However, by observing the exact relationship between birth cohorts and school grade through the years, I can track the proportion of treated individuals in each cohort. Based on this, I classify cohorts into three groups: those who were fully exposed to the program, those who were not exposed to the program, and those who were partially exposed to the program.

Figure 1 tracks the variation in access to computers across cohorts and provinces in 2011. Panel A shows the fraction of individuals (with no younger siblings) with a government laptop at home in 2011 (up to five years after the rollout) stacked by province. I classify cohorts into three groups within each province as a function of their degree of exposure to the program: (1) "after-intervention" cohorts, those with more than 60% access to a government laptop at home; (2) "before-intervention" cohorts, those who were

difference-in-difference approach, which would require trends across provinces to be parallel and remain parallel in the absence of the treatment.

not exposed to the program and had virtually no government laptops at home; and (3) "in-between" cohorts, those who were only partially exposed to the program in their respective provinces, with 10%–25% access to a government laptop (a similar strategy to Havnes and Mogstad (2011)). Partial exposure is the result of some individuals lagging behind in school for cohorts that would otherwise be classified as "before-intervention" cohorts (see web Appendix Figure A4).

In the first part of the paper, I observe outcomes at the combination of age and calendar month and year corresponding to exactly 13 years after starting primary school, which occurs at approximately age 19 and corresponds to the second year of post-secondary education for a student who started primary school at age 6 and experienced no delays nor interruptions in their education (ECH does not include date of birth). In the second part of the paper, I use birth dates to assign exposure to the program to individuals who enrolled in the public university system between ages 18 and 20. To estimate the effects of the program on early-adult educational outcomes, I narrow my sample to individuals born between May 1988 and April 1999 and estimate the following regression:

$$Y_{isc} = \alpha + \eta_s + \gamma_s Trend_c + \beta(In\text{-}between_{sc}) + \boldsymbol{\theta}(After_{sc}) + \mathbf{X}'_{isc}\Gamma + \epsilon_{isc}\,, \qquad (1)$$

where Y_{isc} is the outcome of interest measured around age 19 for every cohort, i indexes the child, s indexes the province, and c indexes the year in which the child was expected to start primary school. The vector of covariates $\mathbf{X_{isc}}$ includes individual-level characteristics such as age, race, and gender fixed effects to make the estimates more precise, and family income and parental education to try to control for province-specific trends that are not captured by individuals who are no longer living with their families, whenever I use this survey, I use average household income and parental education shares for individuals at around age 11, residing in the province where each young adult was living five years prior. Controlling for province-level trends is particularly relevant in Uruguay, where provinces differ considerably in size, wealth, and population characteristics, and educational outcomes are likely to evolve differently over time.[20]

The dummy variable $In\text{-}between_{sc}$ is equal to one for cohorts in the partially treated group within each province: students born between May 1994 and April 1996 in Treinta y Tres, May 1995 and April 1997 in Florida, and May 1996 and April 1998 in the rest of the country. The dummy variable $After_{sc}$ is equal to one for cohorts in the treatment

[20] Refer to web Appendix Table A5, and Tables A6–A7. Estimating equation 1 for the outcome "years of education" without control variables yields that (1) approximately half the province-specific trends are statistically significant at a 5% level and (2) approximately one fourth of the provinces show an underlying negative trend. Visual inspection also shows nonparallel pretrends across provinces. Heterogeneity in the treatment effect across provinces is further discussed in the results section.

group within each province: students born from May 1995 onward in Treinta y Tres, from May 1996 onward in Florida, and from May 1997 onward in the rest of the country. The regression includes province fixed effects and province-specific time-trends meant to control for potential differential trends across provinces. The parameter of interest θ captures the average causal effect of receiving a personal computer with internet access, for children of primary and middle school age, after the program.

I interpret θ as an intent-to-treat effect, since the regression model estimates the reduced-form effects on children from post-intervention cohorts in each province (for the effect of the treatment on the treated, see web Appendix Table A11). This specification does not capture the potential effects of the program on older cohorts of students, who may have been induced to purchase laptops or may have benefited from the laptops of younger relatives, neighbors, and friends. Most siblings are one to two years apart, and so most older siblings will belong in the "in-between" and "before-intervention" cohorts. As a robustness check, I also report results excluding the in-between cohorts from the sample (i.e., using a "doughnut" sample).[21]

Since program participation (and hence, treatment status) was assigned at the province level for all individuals in public schools, rather than randomly across individuals, I cluster standard errors at the province level. To address concerns associated with a small number of clusters (19 provinces), I base all p-values and confidence intervals on province-clustered wild-bootstrapped t-statistics. This method has been shown to work well in contexts where there are few clusters (Cameron et al., 2008) or in the presence of a moderate number of unbalanced clusters (MacKinnon and Webb, 2017). In section 5, I discuss robustness to a variety of ways of handling standard errors and address concerns about heterogeneity in the size of clusters (see Abadie et al., 2017 and Athey and Imbens, 2017).

4.2 Alternative Specification: Exploiting School Type

Controlled interrupted time-series designs, which compare trends in exposed and unexposed groups, can be used to strengthen causal inference. Besides province and cohort,

[21] Since the program exhibits heterogeneous treatment effects across provinces, a difference-in-differences (DID) with cohort and province fixed effects estimate is hard to interpret and generalize to the entire population (Goodman-Bacon, 2018). Additionally, there is very little cross-sectional variation in treatment timing (only two provinces had different timing), treatment was only one or two cohorts apart, the differentially-treated cohorts were treated at a slightly different age (two years apart), and the two out-of-sync provinces were considerably smaller and more rural and not likely to have evolved similarly to the other provinces in the absence of the intervention. Having said this, estimates from a classic DID approach with province and cohort fixed effects (instead of province-specific trends) are qualitatively consistent with those obtained using my preferred methodology.

school type is the third dimension along which the treatment varies. This approach considers this additional source of variation, assuming that whatever changes are observed among private school students are caused by other factors and that this group can provide a counterfactual trend. To exploit this additional source of variation, I implement the strategy specified below:[22]

$$\begin{aligned} Y_{iscp} = \quad & \alpha + \gamma_s Trend_c + \phi Public_p + \delta(In\text{-}between_{sc}) + \kappa(After_{sc}) + \\ & \beta(Public_p * In\text{-}between_{sc}) + \boldsymbol{\theta}(Public_p * After_{sc}) + \mathbf{X}'_{iscp}\Gamma + \epsilon_{iscp}\,, \end{aligned} \tag{2}$$

where $Public_p$ is an indicator for individuals who completed the majority of their primary or middle school education in the public system. Including a comparison group that is never treated, even among post-treatment cohorts, is useful given that all provinces are eventually treated. For the treatment effect on private school students to serve as a benchmark, it's necessary to assume that public school students would have experienced the same trend in educational outcomes as did private school students in the absence of the intervention. As discussed earlier, private and public school students are very different (private school students typically have higher income and more educated parents), and it's not clear that they would experience parallel trends. To address this, I provide estimates that include or exclude sector-specific trends. Another concern is that private school students may have been indirectly affected by the program; if true, this could bias my treatment-effect estimates toward zero.

Since most of the private school population resides in Montevideo and this differences-in-differences specification requires sufficient private school observations in each province, I limit the sample to Montevideo residents. I report these results in the web Appendix; they are consistent with the main specification. While focusing on the single province of Montevideo, I use robust standard errors; results are also robust to clustering standard errors at the neighborhood level. Montevideo has 64 neighborhoods.

4.3 Threats to Identification

I assign exposure to the laptop program based on the student's date of birth or age and timing of the survey, assuming they started primary school at the compulsory age. In the household survey data, I use the province of residence five years prior to the survey as a proxy for where individuals attended school; in the university data, I use province of birth. Refer to web Appendix C for a detailed description of how I handle treatment

[22] This is not a standard difference-in-differences: I use time trends instead of cohort fixed effects; this relies on estimating breaks in trend at the threshold that are differential across educational sectors.

assignment in the various datasets and a discussion of miss-classification error. There are several threats to identification. First, exposure of the older cohorts to the program could generate a bias toward zero. This is likely to arise if there is error in assigning individuals to their correct province or cohort. Second, any unobserved differences between older and younger cohorts, when not captured by a linear trend, could bias the estimates. This is likely to arise if the post-treatment cohorts were already different at the baseline or experienced differential shocks before age 19. A third threat is using the wrong functional form; a nonlinear pretrend could bias my results either way.

Web Appendix Figure A5 shows that there were no variations from trend at the baseline (age 11; 6th grade) for a set of observable characteristics in the 2001–2014 period, including years of education, public school students, teacher employment, TV subscriptions, or parental education. Although not statistically significant, household income appears to experience an upward change in trend for younger cohorts that are completing primary school. This could be explained both by short-run effects of the program on household income (Marandino and Wunnava, 2017) and by exogenous time-series variations in economic growth, and is addressed in the robustness section. Web Appendix Figure A6 plots a series of observable characteristics across cohorts in 2006, one year before the intervention, and shows that household income is very similar across all cohorts. Web Appendix Figure A7 plots household income across cohorts for every age 11 to 19, to confirm that there were no obvious trend breaks at those critical ages. In web Appendix Table A4 I estimate equation 1 without controls on predetermined covariates. Panel A shows the regression results for 13 observable characteristics measured at age 11: as expected, none of these characteristics deviates significantly from trend, including employment and income among teachers. Panel B focuses on observable characteristics in a 2006 cross-section, the year before the program was implemented: no significant difference exists among students in internet access at home, mobile phone ownership, government aid, household income, or the fraction of racial minorities; if anything, because they were younger, treated cohorts were about 15% less likely to have a computer at home.

5 Results

I first show that the intervention increased ownership of computers in the targeted population, using information on the presence of a computer in the house from the monthly household survey in 2011. I start by estimating equation 1 in a sample of ten cohorts of individuals living with no younger siblings in 2011; I use eight cohorts per province, including three pre-intervention and three post-intervention cohorts. I control for a wide set of covariates including gender, race, parental education, and household income.

Table 1: Effect of laptop program on early-adult educational outcomes

		A. Complete sample			B. Doughnut sample		
	Mean[A]	ITT	CI	N	ITT	CI	N
Computer access in 2011	0.929	0.165***	[0.088, 0.232]	7,017	0.190***	[0.112, 0.257]	5,287
		(6.456)			(7.581)		
Years of education	10.29	−0.0793	[−0.447, 0.285]	12,775	−0.134	[−0.496, 0.26]	9,323
		(−0.574)			(−0.895)		
High school: enrolled	0.637	−0.0203	[−0.07, 0.058]	12,775	−0.0218	[−0.075, 0.055]	9,323
		(−0.840)			(−0.831)		
High school: graduate	0.349	−0.0299	[−0.099, 0.025]	12,775	−0.0395	[−0.108, 0.019]	9,323
		(−1.321)			(−1.576)		
Post-secon.: enrolled	0.271	−0.0384*	[−0.092, 0.011]	12,775	−0.0435*	[−0.101, 0.011]	9,323
		(−2.000)			(−2.056)		
University: enrolled	0.221	−0.00720	[−0.065, 0.037]	12,775	−0.0128	[−0.068, 0.031]	9,323
		(−0.403)			(−0.695)		
Factor index of levels reached	0.035	-0.0687	[-0.192, 0.039]	12,775	-0.0829*	[-0.212, 0.035]	9,323
		(-1.609)			(-1.755)		
Overall P-Value[B]			0.12			0.15	
Province FE			✔			✔	
Province trends			✔			✔	
Controls			✔			✔	

Notes: Panels A and B estimate equation 1 and show the estimate of θ. Controls include age, gender, and race fixed effects, as well as average household income and parental education for the cohort at the province of origin in the last grade of primary school. Province refers to province of residence five years prior except for past computer access, where province of residence in 2011 is used. Regressions include ten cohorts in total, with three pre-intervention and three post-intervention cohorts in each province. Past computer access is measured in 2011. All other outcomes are measured around age 19. T-statistics and confidence intervals from the wild cluster bootstrap are presented in brackets (clusters: 19 provinces).

Source: ECH 2001–2018.

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

[A] Mean among treatment cohorts.

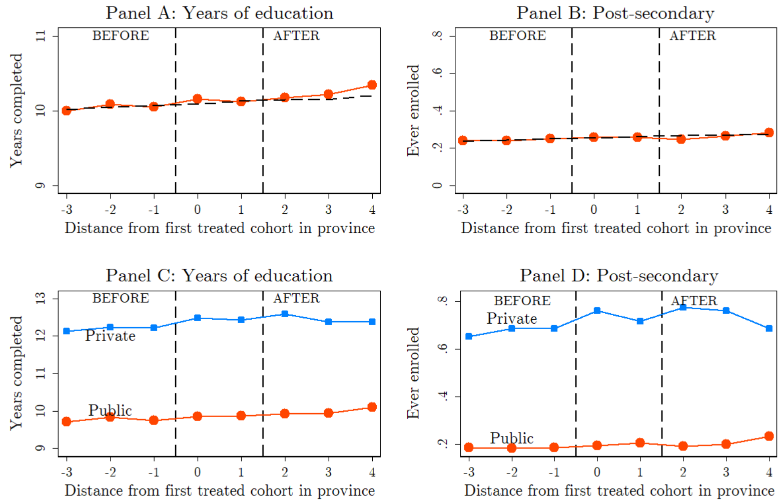
[B] P-values indicate the probability of obtaining the observed number of statistically significant coefficients or more by chance (at the 10-percent level) based on 3,000 bootstrap repetitions.

Panel A in Table 1 shows that the intervention increased access to a computer in the house among treated cohorts by about 25% (17 percentage points). Similarly, Panel B estimates equation 1 in a doughnut sample that excludes the in-between cohorts in each province; the estimate suggests that computer access increased by about 26% (19 percentage points). These results are statistically significant at the 1% level and robust to a series of checks, including various ways of handling inference and modeling underlying trends, collapsing the dataset, excluding controls, using province of birth rather than residence, or looking at government laptops specifically (see web Appendix Table A8). Moreover, the break in trend among the youngest cohorts is present in each province, and the treatment effect is strictly positive in all but two provinces at the chosen threshold (see web Appendix Figures A8 and A10).

5.1 Educational Attainment

5.1.1 Empirical Analysis

Figure 2: Evolution of educational attainment
Measured around age 19 across cohorts, provinces, and school types



Notes: This figure plots educational attainment by age 19 across cohorts (and school type) based on time since treatment in their respective provinces. Panels A and C plot the average schooling in the population (years completed). Panels B and D plot the fraction of individuals who enrolled in post-secondary education. A cohort is defined as the group of individuals who are expected to start primary school in the same academic year, and is estimated based on age, year, and month of the survey. In-between cohorts were exposed to the program to the extent that some individuals started primary school later than expected, or repeated grades by the time the program arrived in their province.
Source: ECH 2001–2018.

I start this section by using household survey data. The outcomes of interest are years of education, high school enrollment, high school graduation, post-secondary enrollment, and university enrollment. These outcomes are observed at essentially the same age (around 19 years old) for each cohort.

Figure 2 plots the average years of education attained (Panel A) and the share of individuals aged 19 that enrolled in post-secondary education (Panel B) for each value of "time since treatment", together with a black dashed line that represents the predicted trend based on province-specific linear pretrends. Time since treatment takes the value 0 for the first cohort to be at least partially exposed to the program by early adulthood, in any given province; it takes the value –1 for the cohort that is immediately older in that given province and the value 1 for the cohort that is immediately younger in that given province. Once a cohort has been treated, all following (younger) cohorts are treated. In these panels there is no visual evidence of a break in trend at the threshold; to the contrary, all variables appear to evolve smoothly across cohorts exposed or unexposed to the laptop program. Although there are signs of an upward trend in schooling, this is challenged by a slight drop in post-secondary enrollment rates. Panels C and D show the schooling gap between public school students who were directly exposed to the program and their private school counterparts, with no obvious signs of convergence and without major discontinuities around the treatment threshold. Refer to web Appendix Figures A11 and A9 for information about high school enrollment and graduation rates.

Table 1 shows the main empirical results for this section. Panel A estimates equation 1 in the complete sample. Despite what one might expect of a program that set out to improve learning, I find no evidence that the laptop program had a positive effect on educational attainment. None of the estimated treatment effects associated with educational attainment is statistically different from zero, except for a weakly significant decrease in enrollment in post-secondary education. I estimate that the program was associated with a 0.079 fall in years of education, which implies almost a month of lost instruction and represents just under 1% of overall schooling (10.2 years), or just above 3% of a standard deviation. The confidence interval for my estimate $[-0.447, 0.285]$ implies almost five fewer months of education at its lowest bound and almost three additional months of education at its upper bound; I can only rule out a large decrease of half a year of schooling or more, or a moderate increase of three months of schooling or more. The estimates corresponding to enrollment and graduation rates offer a similar conclusion. My estimates indicate a 3% (2 percentage point) decrease in high school enrollment and a 3% (0.7 percentage point) decrease in university enrollment, both statistically insignificant. On the higher end, I estimate an 8% (3 percentage point) decrease in high school graduation and a 12% (4 percentage point) decrease in college enrollment rates, with the latter being weakly significant at the 10% level with a confidence interval of $[-0.092, 0.011]$. Panel B estimates equation 1 in the restricted sample (without the in-between cohorts); the results are essentially unchanged. In web Appendix Figure A10, I plot the college enrollment estimates province by province: Plan Ceibal had no statistically significant impact in almost any individual province (the exception being Lavalleja). The estimates are negative for about half the sample, and positive for the other half, indicating that the direction of the effect is unclear and that the program had likely no effect on schooling overall. To address the fact that the study may not have enough statistical power to reject the null hypotheses, I combined the last four outcomes in a factor index using a principal components approach. Using this factor index as the outcome, I find that the laptop program had a negative effect on education persistence; this estimate is statistically significant in the doughnut sample.

Taken holistically, the findings in Table 1 overwhelmingly indicate a negative association between program exposure and educational outcomes: all the point estimates are lower than zero, and one is weakly statistically significant. However, all these variables are positively correlated, which makes it difficult to infer how likely it is that these results would arise by chance. To determine the likelihood of observing what we see in Table 1 by chance, I perform a simulation exercise following Koedel et al. (2017). I split the data panel vertically for the simulations, separating out the blocks of students' survey responses (dependent variables) and treatment assignment and characteristics (independent variables). I re-sort the survey response data using a random number and re-merge the dataset to randomly assign the education outcomes to students. At each iteration with the randomly assigned education outcomes, I estimate the five models and store the number of coefficients below zero. I repeat this 3,000 times; at the bottom of Table 1, I report the probability of observing at least as many coefficients lower than zero as I estimate with the real data by chance. Based on my simulations, the probability of observing at least one coefficient that is both negative and statistically significant at (at least) the 10% level, is 12% for the whole sample and 15% for the doughnut sample. Thus, the simulation-based statistical tests confirm that the visual patterns in the estimates reflect very weak differences in educational outcomes for students exposed to the treatment and support the conclusion that the program had no positive effects on educational attainment.

Web Appendix Table A9 shows what happens when public school students are compared to private school students over time. The findings are mostly in line with the previous analysis: across the different samples and specifications, I estimate that the program had a nonsignificant impact on years of education; the coefficients imply that the program resulted in about a month of lost instruction. This approach also yields nonsignificant reductions in university and post-secondary enrollment rates. I estimate a very large (30%) drop in high school graduation rates and a more modest (10%) rise in high school enrollment rates; although initially these appeared significant, the statistical significance was lost after including sector-specific trends. This is further evidence that eliminating the technological gap between private and public school students did not reduce (much less eliminate) the educational gap between them.

In addition, in the web Appendix I explore whether the effects of the program on years of education were different for certain groups (see Table A14). I find that the effects of the program were statistically insignificant among boys, girls, individuals with household income below or above the median, and individuals living with a father with or without a high school diploma.[23] Before moving on to the next section on public university students, I also show that the program had no significant effects on the likelihood of enrollment in the public university system, both unconditionally and conditional on university enrollment (see web Appendix Table A12).

5.1.2 Robustness Checks

In this section, I go over various exercises that evaluate the robustness of these results along different dimensions.[24] For brevity, I focus on the outcome "years of education"; the various checks are presented in web Appendix Table A10.

The first few rows show that my findings are robust to diverse ways of dealing with inference: robust standard errors, province-level clustered standard errors, clusters by cohort or clustering two-way by province and cohort, conducting permutation tests, or even collapsing the dataset at the province-by-cohort level, all result in nonsignificant treatment effects. I also show that my findings do not depend on the specific choice of covariates. To address the concern that individuals and households may migrate to follow opportunity, I repeat my empirical approach using province of birth rather than province of residence five years prior. To address the concern that my results may be driven by functional form, I reproduce the empirical approach utilizing province-specific quadratic trends, or using a more standard aggregate linear trend. To address the possibility of a downward bias to the extent that older cohorts of individuals interact with the laptops of their younger siblings, I restrict the sample to individuals living with no younger siblings. In all cases, the results are unchanged.

I go on to address the possibility that some confounding variables may be biasing the results. I control for whether students are living with a child of their own, for current household income, and for age-specific income trends by province; the results are unchanged. When excluding the youngest post-intervention cohorts from the analysis, the treatment effect turns positive but remains small and insignificant, in line with the notion that the program had no sizable effect on educational attainment. Focusing solely

[23] The relative signs and magnitude of the coefficients suggest that the program may have been positive for boys and negative for girls, more positive for households with income above the median, negative for individuals with higher parental education, and positive for those with lower parental education.

[24] A standard difference-in-difference approach (including province and year fixed effects) reveals that the laptop program had either no significant effect or caused a 4.5 month loss in educational attainment, depending on the sample used. Alternatively,refer to web Appendix Figure A12 and Table A13 for (a) an analysis based on intensity of treatment and (b) an analysis based on a difference-in-differences with balanced control and treatment groups in Montevideo. Overall, there is no evidence that the laptop program increased schooling.

on years of education completed or capping years of education at 11 both show similar results. I also check whether there was evidence of a break in trend in schooling by age 17 across cohorts. I estimate that treated cohorts had on average reached 0.19 more years of education than their counterparts, which is nonsignificant in the complete sample but weakly significant in the restricted sample. A graphical analysis shows that, if anything, the change in trend is happening among the pre-intervention cohorts. Although not reported, my findings are also robust to running the analysis in 2016 cross-sectional data. Since educational attainment is nonlinear on age, I use the cross-section of two previous years (2011 and 2013) as control groups. Once again, I find no significant effects from the program.

Finally, I discard the household survey data and make use of aggregate administrative data, which may be more precise. I find that the program had no significant effect on university enrollment, as a fraction of individuals who made it to the last year of secondary school in their respective provinces, consistent with my findings.

5.1.3 Interpreting My Findings

In this section, I discuss why the laptop program did not improve educational attainment.

Most importantly, the non-treated cohorts gradually caught up to the treated cohorts in terms of computer access at home; by age 18, there is no gap in access between cohorts. In fact, web Appendix Table A15 suggests that, at age 19, the treated cohorts were slightly *less* likely to have computers in their home relative to the expected trend, a figure non statistically different from zero. Still, program participants had a different experience with technology: they accessed a home computer at an earlier age, had time to benefit from the learning curve, experienced a high intensity of treatment through a personal laptop, and likely experienced supervised access at school. In addition, my findings are not driven by decisions made at ages 18 and 19; I have not found significant effects on outcomes that should have been realized before the convergence in technology, such as high school enrollment.

To provide further insights into why the laptop program may not have improved educational attainment, I go over the two main reasons adolescents and young adults don't complete secondary school, as self-reported in the household survey. I expect not to find any significant effects of the program on these factors. The first is lack of interest. If computers are a source of entertainment (defined as *"playing games, downloading music, etc."*), as well as information and communication, this technology could be potentially harnessed in the classroom to make the learning process more appealing to students. However, very few students report using the internet for learning or educational activities. While students who were exposed to the program remain unconditionally more likely to use the internet at age 19, the propensity to use the internet for information and education purposes, conditional on using the internet, is not significantly higher among the treated cohorts (Table A15). The second reason adolescents and young adults don't complete secondary school in Uruguay, is that they start working. Consistent with Autor et al. (2008) and Fairlie and Bahr (2018), I find no evidence that employment rates are significantly higher among treated cohorts and, if anything, the coefficients go in the opposite direction, indicating a 9% drop in employment relative to the predicted trend.

5.2 Choice of Major and Scholarship Application

In this section, I investigate whether the program affected educational choices, conditional on attending university. This analysis might yield different results, because there is evidence that providing computers to college students, as opposed to providing computers to school children, has improved educational outcomes, and that the years of exposure to technology improve the ability to use the internet and to find information.

Technology could affect college outcomes through information and preference channels. The information channel indicates that students are more likely to find information about their options (content of majors, duration, requirements, and job-market prospects), their interests, and the costs and benefits of education. A survey of former students who graduated in 2010 and 2011, indicates that overall satisfaction (having no regrets) was slightly higher among those who majored in science and technology and health, relative to the social sciences (web Appendix Table A16). Similarly, an independent analysis of household survey data indicates that employment has been persistently highest in health, followed by science and technology, social sciences, and the arts. I expect technology to increase scholarship applications and enrollment in areas of study that have high satisfaction rates, such as science and technology and health relative to the social sciences. The preference channel indicates that access to computers may have shaped the preferences of students over time, perhaps increasing their taste for technology, in which case I would expect enrollment in technological or computer-related majors to increase.

Table 2 shows the main results for this section. I find no evidence that the laptop program increased scholarship applications among enrolled students, contrasting my expectations; my estimates suggest that the share of applicants fell by 5% (1.9 percentage points) with respect to the pre-intervention trend, a figure that is not statistically different from zero. It is possible that the scholarship was already sufficiently publicized, as it was already popular among students.. In accordance with the first part of this paper and with the fact that college students tend to be better off than the general population, I find no evidence that the program had a significant effect on the probability of being a first-generation university student; my estimates indicate a 2% to 4% decrease in this population. Similarly, I find no significant discontinuity on the likelihood that students had a previous college experience; my estimates suggest a 10% (0.002 percentage points) nonsignificant increase in this population.

Table 2: Effect of the laptop program on educational outcomes of university students

		A. Complete sample			B. Doughnut sample		
	Mean[A]	ITT	CI	N	ITT	CI	N
Sample of individuals							
Technological major	0.156	−0.0109	[−0.031, 0.042]	52,168	−0.0162	[−0.041, 0.046]	36,882
		(−0.743)			(−0.917)		
Computer major	0.043	−0.0057	[−0.018, 0.023]	52,168	−0.00746	[−0.02, 0.027]	36,882
		(−0.882)			(−0.949)		
Enrollment across departments	0.049	0.00345	[−0.026, 0.013]	52,168	0.00354	[−0.029, 0.023]	36,882
		(0.49)			(0.489)		
Previous college	0.02	0.00187	[−0.005, 0.019]	52,168	0.0023	[−0.009, 0.021]	36,882
		(0.49)			(0.539)		
Scholarship application	0.319	−0.0186	[−0.054, 0.027]	39,908	−0.0238	[−0.074, 0.04]	24,705
		(−1.64)			(−1.469)		
First generation	0.628	−0.0175	[−0.057, 0.038]	52,168	−0.00993	[−0.058, 0.052]	36,882
		(−1.36)			(−0.704)		
Sample of individuals by area of study							
Social sciences	0.401	0.0377	[−0.044, 0.071]	53,041	0.0574**	[−0.034, 0.097]	37,497
		(1.995)			(2.664)		
Science and technology	0.26	-0.033	[−0.056, 0.029]	53,041	−0.0471**	[−0.072, 0.023]	37,497
		(−2.004)			(−2.616)		
Health	0.339	−0.00432	[−0.058, 0.038]	53,041	−0.0103	[−0.068, 0.04]	37,497
		(−0.336)			(−0.751)		
Province FE			✔			✔	
Province trends			✔			✔	
Controls			✔			✔	

Notes: Panels A and B estimate equation 1 and and show the estimate of θ. Controls include age, gender, and parental characteristics; the first generation outcome excludes parental characteristics. Province refers to province of birth and cohort is computed based on date of birth. T-statistics and confidence intervals from the wild cluster bootstrap are presented in brackets (clusters: 19 provinces).
Source: Universidad de la Republica del Uruguay, incoming student survey, 2008–2016.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
[A] Mean among treatment cohorts.

I also explore the effect of the program on choice of major. I start by looking at whether students enroll in multiple departments; they are required to complete a new enrollment form every time they enroll in a new school or department within the university. If early access to information technology increased knowledge about characteristics of majors, the program could have led students to decide up front, thus enrolling in a single department; conversely, information about majors offered by the university could have fueled their curiosity about lesser-known majors, therefore encouraging them to enroll in multiple fields. My estimates indicate that the laptop program had no significant impact on this choice, although the magnitude of coefficients suggests a 6% (3 percentage point) increase. These two findings suggest that these students are probably enrolling in these majors simultaneously rather than switching majors over time. This is robust to using a province-by-cohort sample or accounting for province-specific quadratic trends.

I then test whether the increase in computer access has fueled enrollment in technology or computer-related majors. One might expect the laptop program to increase students' interest in computing or technology-oriented majors, or to increase demand for local experts in computing technology, creating jobs. On the other hand, nonuniversity post-secondary technical degrees can be obtained through other public institutions, allowing graduates to take up jobs in technology without the need to enroll in university majors. I find no evidence that the program affected enrollment in technology-related majors; my estimates are non statistically significant and suggest a 6% (1 percentage point) decrease in technological majors and a 50% (0.5 percentage point) decrease in computer-related majors. The latter estimate seems unreasonably large; only 4% of students enroll in computer-related majors, so a small group of students moving in or our of this major can greatly influence the total share of students in the group.

I then group departments into three general areas: the social sciences, science and technology, and health.[25] In the bottom rows of Table 2, I investigate whether the laptop program exerted any effect on the choice of major along this dimension. When considering all observations (Panel A), my estimates suggest that the program had no statistically significant effect in choosing an area of study; the program is associated with a 10% (4 percentage points) increase in social sciences at the expense of a comparable (3 percentage points) drop in science and technology and, to a lesser extent, a 1% (0.4 percentage points) drop in health. When considering the doughnut sample, all three coefficients become larger in magnitude and the effects on social sciences and science and technology are statistically significant, suggesting a 15% rise in social sciences at the expense of a 20% drop in science and technology. These two coefficients are not always statistically significant, but their directions are robust across a series of checks. Extending the number of pre-treatment cohorts with quadratic trends by province yields a positive but insignificant impact on health. These findings contradict my initial hypothesis; technologies are complex, and there is no direct link between having a computer and choosing a specific

[25] Social sciences includes humanities and the arts; health includes veterinary school.

major (especially when almost everyone in a cohort is treated).

Web Appendix Table A18 shows the estimates that result from comparing public to private school students over time. In line with the schooling results, these estimates indicate that the laptop program had no significant effects on scholarship applications; the general direction of the coefficients suggests a positive association but becomes negative after controlling for sector-specific trends. More surprisingly, my estimates suggest a weakly significant 4% (2.4 percentage point) increase in the share of first-generation students, but the statistical significance disappears after including sector-specific trends. My estimates also suggest that the program had no significant effect on the share of students who had previous college experience; there is a positive association that becomes negative after controlling for sector-specific trends. A difference with the prior analysis is that the probability of enrolling in multiple departments is lower for public school students relative to private school students after the program; this becomes significant when controlling for sector-specific trends, indicating that this practice was cut almost by half (4 percentage points) as a result of the program. This could suggest that treated individuals have become better at selecting majors in advance, with less of a need to shop around. However, the program remains ineffective at influencing enrollment in technology or computer-related fields; there was no differential break in trend for the public school students who participated in the program relative to private school students.

Table A19 shows how the program affected enrollment across the three broad areas of study when public and private school students are compared over time. When controlling for sector-specific trends, my estimates are similar to my previous findings: the program seems to be positively associated with enrollment in the social sciences and negatively associated with enrollment in science and technology, with ambiguous although nonsignificant effects on health; the 13% (4 percentage points) decline in science and technology is weakly statistically significant, irrespective of sampling, whereas the 5%–14% (2–6 percentage point) rise in the social sciences is only statistically significant in the doughnut sample. These findings are similar both inside Montevideo and in the rest of the country as a whole. Excluding the sector-specific trends, my estimates remain negative and significant for science and technology, but are ambiguous for the other two areas of study.

In web Appendix Tables A20 and A21, I conduct various exercises that evaluate the robustness of these results along different dimensions. For this purpose, I focus on the two main takeaways from this section: (1) there is no evidence that the program affected scholarship applications (and if anything, the association appears to be negative), and (2) there is some evidence that the program was associated with a decline in enrollment in the broadly defined area of science and technology. Overall, the tables corroborate these findings. Refer to web Appendix D for more details.

6 Conclusion and Discussion

Governments and organizations around the globe are seeking to expand children's access to computers and the internet as the United Nations calls for efforts to eliminate the digital divide. However, little is known about the effects this expansion may have on long-run human capital accumulation. This paper estimates the causal effect of access to computers and the internet on educational attainment and choice of major. To establish a causal link, I exploit variation in access to computers and the internet across cohorts and provinces among primary and middle school students in Uruguay, the first country to implement a nationwide one-laptop-per child program. Despite a notable increase in computer access, educational attainment has not improved, and my findings suggest that the laptop program may have lessened educational attainment. Among public university students, those who were exposed to the program were not more likely to apply for scholarships and were less likely to enroll in technology-related majors, relative to health and the social sciences.

Uruguay's Plan Ceibal serves as a case study for what would happen in a country that succeeds in eliminating its digital divide. On the one hand, I would expect my findings to be an upper bound to what would occur in other countries, since Uruguay has a tuition-free, unrestricted public university system, and a larger margin for improving its citizen's educational attainment than other countries in the region. On the other hand, Uruguayan children may face higher restrictions to secondary education, while the universal expansion of technology in the last decade could have reduced the impact of the program. My findings suggest that simply expanding access to technology (rather than using technology for educational purposes) does not necessarily improve educational attainment as measured in young adulthood. To increase schooling, policymakers may consider complementing one-laptop-per-child programs by promoting activities that increase educational usage, and by investing in teacher training and educational software. The first few cohorts exposed to Plan Ceibal were not exposed to complementary programs later developed by the organization, some of which show promise and could improve outcomes in later generations (Perera and Aboal, 2017). Alternatively, with the same resources (approximately 600 dollars per student), Uruguay could have employed full-time teachers in 100 schools, a mode of schooling that has shown promising results for students of low socioeconomic status (Cardozo Politi et al., 2017).[26] This would have targeted a smaller number of individuals, but with potentially positive long-run results.

[26] Based on official 2010–2016 Ceibal Financial records, the Institute of Statistics, and the 2006 and 2008 government budget. The estimation uses that 429,016 students were enrolled in public primary and middle school in 2007 and assumes the number of students would have doubled by 2016.

A serious evaluation of one-laptop-per-child programs, however, would require taking more outcomes and distributional concerns into account. Equal access to information and communication technologies might be seen as a goal in itself: Many argue 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 (2003 World Summit on the Information Society). Access to computers and the internet could increase social welfare through positive network effects, or affect other outcomes that are valuable to society and have not been analyzed in this paper. Moreover, my findings do not capture any effects that the program may have on educational decisions made after age 19 (which might involve returning to school or pursuing online tertiary education later in life).

References

Alberto Abadie, Susan Athey, Guido W Imbens, and Jeffrey Wooldridge. When should you adjust standard errors for clustering? Technical report, National Bureau of Economic Research, 2017.

Susan Athey and Guido W Imbens. The econometrics of randomized experimentsa. In *Handbook of Economic Field Experiments*, volume 1, pages 73–140. Elsevier, 2017.

David H Autor, Lawrence F Katz, and Melissa S Kearney. Trends in us wage inequality: Revising the revisionists. *The Review of economics and statistics*, 90(2):300–323, 2008.

Abhijit V Banerjee, Shawn Cole, Esther Duflo, and Leigh Linden. Remedying education: Evidence from two randomized experiments in india. *The Quarterly Journal of Economics*, 122(3):1235–1264, 2007.

Diether W Beuermann, Julian Cristia, Santiago Cueto, Ofer Malamud, and Yyannu Cruz-Aguayo. One laptop per child at home: Short-term impacts from a randomized experiment in peru. *American Economic Journal: Applied Economics*, 7(2):53–80, 2015.

Marcelo Boado. Una aproximacion a la desercion estudiantil universitaria en uruguay. 2005.

A Colin Cameron, Jonah B Gelbach, and Douglas L Miller. Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics*, 90(3):414–427, 2008.

Santiago Cardozo Politi, Elisa Borba, Gimena Castelao, and Diego Forteza. Evaluacion de impacto de las escuelas de tiempo completo en uruguay 2013-2016. *Administracion Nacional de Educacion Publica*, 2017.

Julian Cristia, Alejo Czerwonko, and Pablo Garofalo. Does technology in schools affect repetition, dropout and enrollment? evidence from peru. 2014.

Julian Cristia, Pablo Ibarrarán, Santiago Cueto, Ana Santiago, and Eugenio Severín. Technology and child development: Evidence from the one laptop per child program. *American Economic Journal: Applied Economics*, 9(3):295–320, 2017.

Gioia De Melo, Alina Machado, and Alfonso Miranda. The impact of a one laptop per child program on learning: Evidence from uruguay. 2014.

Lisa J Dettling, Sarena Goodman, and Jonathan Smith. Every little bit counts: The impact of high-speed internet on the transition to college. 2015.

Orla Doyle, Colm P Harmon, James J Heckman, and Richard E Tremblay. Investing in early human development: timing and economic efficiency. *Economics & Human Biology*, 7(1):1–6, 2009.

Maya Escueta, Vincent Quan, Andre Joshua Nickow, and Philip Oreopoulos. Education technology: an evidence-based review. Technical report, National Bureau of Economic Research, 2017.

Robert W Fairlie and Peter Riley Bahr. The effects of computers and acquired skills on earnings, employment and college enrollment: Evidence from a field experiment and california ui earnings records. *Economics of Education Review*, 63:51–63, 2018.

Robert W Fairlie and Rebecca A London. The effects of home computers on educational outcomes: Evidence from a field experiment with community college students. *The Economic Journal*, 122(561): 727–753, 2012.

Andrew Goodman-Bacon. Difference-in-differences with variation in treatment timing. Technical report, National Bureau of Economic Research, 2018.

Justine S Hastings, Christopher A Neilson, Anely Ramirez, and Seth D Zimmerman. (un) informed college and major choice: Evidence from linked survey and administrative data. *Economics of Education Review*, 51:136–151, 2016.

Catherine Hausman and David S Rapson. Regression discontinuity in time: Considerations for empirical applications. *Annual Review of Resource Economics*, 10:533–552, 2018.

Tarjei Havnes and Magne Mogstad. No child left behind: Subsidized child care and children's long-run outcomes. *American Economic Journal: Economic Policy*, 3(2):97–129, 2011.

James J Heckman. Skill formation and the economics of investing in disadvantaged children. *Science*, 312(5782):1900–1902, 2006.

Caroline Hoxby, Sarah Turner, et al. Expanding college opportunities for high-achieving, low income students. *Stanford Institute for Economic Policy Research Discussion Paper*, (12-014), 2013.

Cory Koedel, Jiaxi Li, Matthew G Springer, and Li Tan. The impact of performance ratings on job satisfaction for public school teachers. *American Educational Research Journal*, 54(2):241–278, 2017.

James G MacKinnon and Matthew D Webb. Wild bootstrap inference for wildly different cluster sizes. *Journal of Applied Econometrics*, 32(2):233–254, 2017.

Ofer Malamud and Cristian Pop-Eleches. Home computer use and the development of human capital. *The Quarterly Journal of Economics*, 126(2):987–1027, 2011.

Ofer Malamud, Santiago Cueto, Julian Cristia, and Diether W Beuermann. Do children benefit from internet access? experimental evidence from peru. *Journal of Development Economics*, 138:41–56, 2019.

Joaquin Marandino and Phanindra V Wunnava. The effect of access to information and communication technology on household labor income: Evidence from one laptop per child in uruguay. *Economies*, 5 (3):35, 2017.

Marcelo Perera and Diego Aboal. Evaluación del impacto de la plataforma adaptativa de matemática en los resultados de los aprendizajes1. 2017.

Jeremy Roschelle, Mingyu Feng, Robert F Murphy, and Craig A Mason. Online mathematics homework increases student achievement. *AERA Open*, 2(4):2332858416673968, 2016.

Lawrence J Schweinhart, Jeanne Montie, Zongping Xiang, W Steven Barnett, C Belfield, and Milagros Nores. The high/scope perry preschool study through age 40. *Ypsilanti, MI: High Scope Educational Research Foundation*, 2005.

Alexander JAM Van Deursen, Jan AGM van Dijk, and Oscar Peters. Rethinking internet skills: The contribution of gender, age, education, internet experience, and hours online to medium-and content-related internet skills. *Poetics*, 39(2):125–144, 2011.

Jacob L Vigdor, Helen F Ladd, and Erika Martinez. Scaling the digital divide: Home computer technology and student achievement. *Economic Inquiry*, 52(3):1103–1119, 2014.