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

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Abstract

This paper provides the first causal estimates of the effect of children's access to computers and the internet on adult educational outcomes 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. However, college students who had been exposed to the program as children, were more likely to select majors with good employment prospects, and less likely to multi-major.

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1 Introduction

Governments around the globe have become increasingly concerned about the economic consequences of unequal access to technology among school children. One of the targets in Goal 9 of the United Nation's 2030 Agenda for Sustainable Development is to "significantly increase access to information and communication technology and strive to provide universal and affordable access to the internet in the least developed countries." One class of programs 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.[1]

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.[2] Despite the popularity of these programs, 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). However, there is no empirical evidence on the overall effects that these interventions may have on long-term human-capital accumulation. As children grow older they become responsible for a larger set of educational decisions, while more years of exposure to computers and the internet may increase their ability to use technology effectively.

In this paper, I examine the effects of providing laptops with internet access to school children on their adult educational outcomes. 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 implementation.[3] 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 the best of my knowledge, this is the first paper to consider the long-run effects of a one-laptop-per-child program of this scale.

To link participation in the program to children's adult educational outcomes, I combine survey and administrative data from the National Institute of Statistics of Uruguay, the Ministry of Education, and the main universities in the country. In particular, provin-

[1] 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).

[2] The 2017 *Measuring the Information Society Report* argues that recent advances in technology will enable innovations that have the potential to increase efficiency, productivity, and improve livelihoods around the globe.

[3] Uruguay is a small country in South America. It was ranked as a high-income country by the UN in 2013, with a population of 3.2 million people and a GDP per capita of $19,942 PPP.

cially 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 public university system allow me to track characteristics of university students and their academic choices.

To identify the causal effect of the intervention, I use information about an individual's cohort and location to approximate their likelihood of being exposed to the program. The cohorts of older 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 event-study identification strategy (also called an interrupted time-series) 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 required assumption is that the province-specific trend up to the first treated cohort is a good counterfactual for the outcomes of interest.

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% (up 20 percentage points from 70% to 90%), while internet access in public primary schools more than doubled (up 40 percentage points) between 2007 and 2009.[4] 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. Diverse specifications show that the program had no effect on educational attainment. I estimate that total years of education increased, on average, by only three weeks, a figure not statistically different from zero. To understand this finding, I explore the three main reasons for dropping out of high school as reported by students: lack of interest in education, finding employment, and, to a lesser extent, becoming a parent. I find that while most students use the internet for entertainment, very few of them report using it for learning activities. Similarly, the program does not appear to have increased employment among adolescents. However, I do find a considerable decrease in teen pregnancy rates among treated cohorts, which is consistent with both increased access to entertainment (and a

[4] Functioning internet connection was available in 26% of public primary schools in 2006 and 70% of the same schools in 2009. Home computer ownership among school-aged children increased from 35% in 2006 to 90% in December 2009.

lesser need to socialize) and increased access to information about contraceptives and family planning.

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, 36% of alumni would choose a different major were they given the chance to go back in time.[5] Access to information about the degrees offered and how they are valued by the market could improve the quality of the match between students and their major.[6] I find a significant decrease in the fraction of students who enroll in multiple majors. This is consistent with the hypothesis that students are more knowledgeable about the options and thus have a lesser need to explore by enrolling in multiple fields. This may have important implications for reducing congestion and increasing the quality of education, which is an important concern in the public university system.[7]

My findings suggest that the reform had some strong effects on the choice of area of study as well, leading students to enroll in majors with good employment prospects. In particular, the program was associated with a lower rate of enrollment in the arts and agrarian sciences, and a higher rate of enrollment in health-related majors. Although there are no statistically significant effects on enrollment in social sciences and science and technology, the coefficients indicate a relative increase in the latter. Besides access to information about employment, these findings could also be explained by differential returns to computer skills across courses.

This paper makes three main contributions to the literature. First, this is, to the best of my knowledge, the first paper to examine the effect of school children's access to the internet and personal laptops on their adult educational outcomes. Second, it is the first paper to examine the effects of technology access on choice of major. 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. Third, this paper exploits a large-scale quasi-experimental design; therefore, it is minimally affected by the concerns of external validity associated to randomized experiments and is particularly relevant for informing policy.

Due to the popularity of these interventions and newly available data, there is now

[5] Survey run among students who graduated from Universidad de la Republica in 2013. It is consistent with previous surveys. In addition, 9% of alumnae declared that their major is not related at all to their current occupation.

[6] In addition, there are many vocational tests that students can take on-line.

[7] I classify students as enrolling in multiple majors if they submitted more than one application form – one per major – in the period of interest. Note that this definition includes students who at some point decided to switch majors.

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 student's math and reading scores. Their finding is in line with other papers. In a small-scale implementation in Peru that used the same devices, 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. A greater concern is that some studies found negative effects on academic achievement from interventions that are purely focused on expanding technology access (see Vigdor et al., 2014; Malamud and Pop-Eleches, 2011), contrasting 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 how children use it.[8]

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, 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. They found that while broadband access generally increased applications to college, the effects were concentrated among high-income students. They worry that new technology may be increasing preexisting inequities. Fairlie and London (2012) studied the effects of donating laptops to recently enrolled community-college students on their academic performance. They found some evidence that the treatment group achieved better educational outcomes.

In sum, the literature has typically found negligible effects of technology access on academic performance, with results ranging from negative to positive depending on the educational level of the recipient. This is consistent with the hypothesis that results depend on the computers' intended use. College students are likely more inclined—either by nature or by context—to use computers for educational purposes. 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.[9] They found no evidence that computers have short- or medium-run effects on earnings or college enrollment. However, for many reasons, giving computer access to

[8] 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.

[9] This was the first study, to my knowledge, to have looked at medium-run effects of a one-to-one computer program on employment and college enrollment.

adults can be different from giving it to children. Besides developmental considerations (see Heckman 2006; Doyle et al. 2009) and the likely presence of an experience-curve (see Van Deursen et al., 2011) for computer and internet skills, 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.

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. 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. Moreover, technical skills may be more valuable in college than in primary and secondary school (see Escueta et al., 2017), thus increasing the likelihood of post-seccondary enrollment. Similarly, computer access may also affect students' career choices, by encouraging them to pursue professions that are more likely to involve or require computing technology. On the other hand, computer skills that are valuable in the labor market may discourage children from furthering their education.[10]

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 approach and technical details of the implementation. Section 5 presents the results. Section 6 considers intermediate outcomes. Section 7 concludes.

2 The One-Laptop-Per-Child Program in Uruguay: Plan Ceibal

One Laptop per Child (OLPC) is a nonprofit initiative founded in 2005 by MIT professor Nicholas Negroponte. Its mission is to empower the children of developing countries to learn by providing one internet-connected laptop to every school-age 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.

[10] These include searching for jobs in the Internet, networking with potential employers, producing adequate application materials, etc.

2.1 Implementation

In 2007, in partnership with OLPC, the government of Uruguay launched Plan Ceibal, an ambitious program designed to eliminate the existing 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. As of December 2016, 1.6 million laptops had been deployed, enough to double the number of children under 15 years old living in the country.[11]

Plan Ceibal was implemented in two phases, each lasting three years (see Figure 1). Within any province, each primary school was equipped with wireless internet access. Once internet access reached the 90% threshold, Plan Ceibal handed out a personal computer to each primary school student enrolled in that province's public education system. Uruguay has 19 provinces. One (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 gap in the timing of the program 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, 380,615 laptops were provided in primary schools.[12]

Phase 2 focused on secondary schools. In 2009, the pilot program was implemented in the province of Treinta y Tres, in which all students in middle school (grades 1, 2, and 3 of secondary school) received Windows laptops (donated by Microsoft), and more than 90% of the province's schools were equipped with wireless internet access. In 2010, after the implementation of this pilot was deemed successful, the rollout was extended to grade 2 students in the provinces of Montevideo and Canelones. In 2011, the rollout was extended to the rest of the country. At this point, the program was tasked with replacing the primary school laptops with newer laptops equipped with software that was geared towards 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 (grades 4 and 5 of secondary school) who had entered the technological track rather than the regular track—about 10% to 15% of all high school students—also received laptops. This adjunct program ended in 2014 due to financial constraints. In all, between 2009 and

[11] This number represents almost half of the entire population in 2016 (3.4 million). The explanation is that children would get two laptops in their lifetime: one in primary school and a different one in middle school, at which point the first laptop would go back to the state. Moreover, broken laptops had to be replaced.

[12] As a reference, 292,900 students were enrolled in public primary schools in 2009.

2011, 134,111 laptops were provided in secondary schools.

Official data provided by Plan Ceibal shows that by June 2010, 98% of primary public schools and 90% of public middle schools in the country had wireless connection. Public primary school census data from the Ministry of Education of Uruguay (ANEP) allows me to verify that internet connectivity increased significantly during the expansion period of the program. That data show that functioning internet connection was available in 26% of public primary schools in 2006 and 70% of them in 2009.[13]

Plan Ceibal was implemented successfully. Using data from Uruguay's monthly household survey (which I describe in more detail below), I track the fraction of individuals aged 6 to 15 who reported having a computer in their home: It increased 25 percentage points (from 50% to 75%) in the quarter in which the program was implemented in their province, and 40 percentage points (50% to 90%) when compared to the following quarter (Figure 2, Panel A).[14] Compellingly, there was no change at all around that time-frame in computer access for adults living with no children. Computer access among public school students had increased by 150% only two years after the intervention—I estimate an increase close to 90% in the first quarter of implementation alone (Figure 2, Panel B).[15] In effect, this increased access benefited only public school children; those enrolled in private schools experienced no significant discontinuities in computer access around that date.

Using the same data, Figure 3 shows variation in computer access across cohorts of individuals in a cross-section of 2011. 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 laptops, in striking contrast to private school students. Panels C and D show that this resulted in a 40% increase in computer access among all individuals in the relevant cohorts and a 50% increase when comparing public to private school students.

[13] see web Appendix Figure A2.

[14] 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, mentally capable, who can provide information about the home and rest of the household members. An individual is said to have reported a computer at home whenever the household informant reports a computer.

[15] The ECH survey does not provide data on school type for the years 2009 and 2010. To address this, I replace public school computer access by the average access in the student population, which is in its majority public sector. In the rest of the country, for which I do have data immediately after the intervention, the immediate increase in computer access was indeed about 90%.

2.2 The Computer

Plan Ceibal equipped each student with an XO-1 laptop, a small, durable, efficient, low-cost laptop that functions much like a normal PC.[16]

Reviews found in the internet tend to converge to one conclusion:

> "The XO-1 won't ramp up your digital productivity or amaze you with hi-def visuals. But (...) it celebrates its ability to communicate with people around the corner or around the world, access information, design programs and manipulate music, sound or pictures."[17]

The laptop features 128MB of RAM, 1GB of NAND flash memory (instead of a hard disk), a 7-1/2-inch dual-mode LCD, wireless networking, and a video camera. It's also designed to be operated by children and is therefore durable and rugged. In addition to a standard plug-in power supply, human power and solar power sources are available, allowing it to be operated far from a commercial power grid. The wireless technology supports both standard and mesh networking, which allows laptops to network peer-to-peer, without the need for a separate router. The XO-1 uses a GNU/Linux operating system, and all its software is free and open source. It comes with basic software installed. Plan Ceibal reported in 2009 that among schools with connectivity that used the laptops in class, 90% of students navigated the internet, 60% used the writing software, and 15% used the drawing software, with a smaller share using the calculator, chatting, reading a book, and memorizing concepts.[18]

Pricing for the XO was set to start at US$188 in 2006, with the goal to reach the $100 mark in 2008. When the program launched, the typical laptop retailed for well north of $1,000.

2.3 Cost and Financing of the Program

As of December 2016, 1,681,830 devices had been dispatched by the program.[19] At $188 per laptop, this would imply a direct cost of about $300 million. However, the overall

[16] The display is the most expensive component in most laptops, so the development of a new, cheaper display was instrumental to the creation of the XO. See http://wiki.laptop.org/images/7/71/CL1A_Hdwe_Design_Spec.pdf for more details.

[17] National-level programming competitions using the XO laptops began in 2010. There are several accounts of children creating/developing games in these laptops. While this does not mean the practice was universal, programming was certainly possible. See https://www.cnet.com/uk/products/olpc-xo-1-one-laptop-per-child/review/2/.

[18] https://www.ceibal.edu.uy

[19] This number includes laptops and tablets. Source: Memoria Explicativa de los Estados Contables al 31 de Diciembre de 2016, Centro Ceibal.

operational costs of Plan Ceibal were higher, about \$500 million by 2017. As a reference, this equates to an average of 3% of Uruguay's annual education budget and 0.4% of its annual federal budget since 2007.[20] The ultimate cost of the program added up to approximately \$600 per student.[21]

The program was financed mostly with taxpayer money, as Plan Ceibal got its own portion of the federal budget. There is no evidence that this implied a decrease in expenditures in other areas of education—in fact, the economy was growing and the overall education budget was rising. The Inter-American Development Bank helped finance the program through two loans: \$5 million in 2010 and \$30 million in 2017.

3 Data and Summary Statistics

In this study, I combine three datasets: (1) the 2001–2017 household survey data (ECH), which contains information on technology access and education; (2) tabulated enrollment data from 2001 to 2016 from the Ministry of Education and private universities, by year, province of origin, gender, and school type; and (3) administrative data from 2006 to 2016 for all 208,946 entering students in the public university system (Universidad de la Republica), which contains information about major of choice.

3.1 Data Sources

My main data source is the 2001–2017 Uruguay Continuous Household Survey (Encuesta Continua de Hogares; henceforth, ECH), which samples about 3.5% of particular dwellings each year.[22] This publicly available monthly survey comprises independent cross-sections, representative at the provincial level. It provides standard information on education and labor-market outcomes. The questionnaire has been continually revised over the last two decades, which has allowed for the timely incorporation of novel questions, including some on technology ownership and use (for example, the presence of a computer and/or the internet in the house). Moreover, since 2009 the questionnaire has incorporated a specific question about ownership of a laptop from Plan Ceibal. The survey also collects the number of years of education (attended and/or completed). Other useful variables include the type of primary and middle school institutions attended (public or private) and years of age, together with year and month of the survey. Moreover,

[20] From official Ceibal Financial records 2010-2016, the Institute of Statistics and the Government Budget 2006 and 2008.

[21] With 429,016 students enrolled in public primary and middle school in 2007 and assuming the number of students would have exactly duplicated by 2016.

[22] 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.

questions about migration are included as well: 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.

The household survey data is very convenient. Its main virtues: it allows me to document the effect of the program on computer access (as was demonstrated in Figures 2 and 3) and to estimate its impact on educational attainment. However, despite it being representative, it contains only a small sample of the population. Therefore, to validate my results I also collect aggregate 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. Due to migration concerns, – most of these educational establishments are in Montevideo; I cannot use this same data for postsecondary enrollment. Consequently, I contacted each university in Uruguay to collect tabulated data on their student demographics, including year of enrollment and province of origin in the 2010–2016 period. My resulting sample encompasses more than 95% of university students in Uruguay.[23]

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 between 2006 and 2016. This is the nation's largest university, attended by more than 80% of its university students. This dataset contains the specific majors chosen by the individual 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 private or public 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 several individual and parental characteristics.

Finally, in order to verify the expansion of internet access around the start of Plan Ceibal, I collect 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.[24]

3.2 Summary Statistics

Table 2 shows summary statistics for individuals aged 18–20 in 2011 to 2017 using the household survey data. Approximately half the sample is male, and one out of five individuals is nonwhite. In terms of socioeconomic status, one out of ten lives below the poverty line, and 42% claim to be employed.[25] The average individual in this age

[23] The sample includes the following universities: Universidad de la Republica (public), Universidad de Montevideo (private), Universidad Catolica del Uruguay (private), Universidad ORT (private).

[24] This information was not available in the web, I learned about it through an interview with the director of the research department in ANEP, who then had the data processed and sent to me.

[25] This is comparable to the US average for the entire population.

group lives in a four-person household, and four out of five individuals still live with their parents or grandparents. In addition, almost one out of five women have children.[26]

In terms of access to and use of technology, four out of five individuals 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 64% reported using the internet every day (this is consistent with the fact that only 42% of individuals age 15 to 20 had a smartphone at home in 2013).[27] Internet use is spread evenly between entertainment, information, and communication (about 30% each), while about 10% is for education or learning activities.[28]

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. The average years of education completed among individuals aged 18 to 20 is 9.9; only 60% ever attended a high school, and only 29% ever graduated from high school. Among the reported reasons for dropping out of secondary school, lack of interest (55%) tops the list, followed by starting to work (20%), pregnancy (7%), and finding classes difficult (7%).[29] Moreover, 12% attended technical school and 4% graduated from it. With respect to higher education, only 21% enrolled in any postsecondary education and only 18% enrolled in university. Finally, a considerable gap exists between public and private school students. Public school students have on average 9.7 years of education by age 20; private school students have on average 11.86, and almost all of them enroll in high school. Therefore, a large opportunity exists for increasing educational attainment.

4 Identification Strategy

This section outlines my empirical approach to identifying the causal effect of the one-laptop-per-child program.

[26] Adolescent births in Uruguay are well above the global average. According to World Health Organization, in 2015 4.7% of teenage women (age 15 to 19) had children globally, compared to 8.8% in Uruguay, which ranked right in between the averages for West Africa (11%) and Latin America and the Caribbean (6%).

[27] This question is not included in ECH. This data comes from the nationally representative EUTIC survey made in 2013.

[28] see web Appendix Figure A5.

[29] see web Appendix Figure A5.

4.1 Empirical Specification

To estimate the effect of the one-laptop-per-child program, I implement an event-study identification strategy (also called an interrupted time-series) that compares educational outcomes of individuals who were or were not exposed to the program over time. Thus, 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 would eventually receive one.

I start by documenting that school grade is a very precise indicator of whether an individual has 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 in Treinta y Tres were enrolled in 9th grade in 2009, while the oldest students to receive the intervention in Florida were enrolled in 6th grade in 2007 (expected to be in 9th grade in 2010), and the oldest ones to enter the program in the rest of the country were enrolled in 9th grade in 2011. Hence, there is a one year gap in access to the program between Treinta y Tres and Florida, and between Florida and the rest of the country (see web Appendix Table A1 for more details). In turn, this gap in access to the program across school grades (which is not easily observable for adults) extends across birth cohorts (which I can observe in my data): 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 the rest of the provinces.

In my analysis I focus on adults, and I have no information about 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 ever 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, I am able to observe the exact relationship between birth cohorts and school grade through the years, which allows me to track the exact 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 3 tracks the variation in access to computers across cohorts and provinces. 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 in 2011; (2) "before-intervention" cohorts, those who were 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 in 2011.[30] As mentioned above, 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 A3 for more details).

To estimate the effects of the program on adult educational outcomes, I concentrate on individuals born between May 1988 and April 1998 and estimate the following regression:[31]

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

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 exact 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.[32]

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 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

[30] A similar strategy was used in Havnes and Mogstad (2011).

[31] In the ECH survey I do not have date of birth, but I estimate it based on the age of the child in the month and year of the survey.

[32] Since the ECH survey does not report parental characteristics for 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 residing in the province were each adult individual was living 5 years ago, at around age 11.

reduced-form effects on all children from post-reform cohorts in each province. 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. Note that most siblings are 1–2 years apart, and so will be located in the "in-between" cohorts and above. As a robustness check, I also report the results of this specification where the in-between cohorts are dropped out of the sample within each province (sometimes called "doughnut" sample).[33] My results are robust to this change.

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. Given heterogeneity in the size of clusters, in the web Appendix I present regressions at the cluster level as well (see Abadie et al., 2017 and Athey and Imbens, 2017). Since Uruguay has only 19 provinces, I also report p-values from province-clustered wild-bootstrapped t-statistics to deal with the small number of clusters. This method has been shown to work well in Cameron et al. (2008), but MacKinnon and Webb (2017) show that wild-cluster bootstrapping severely under-rejects when the fraction of treated clusters is either very large or very small. Alternatively, I avoid the question of clustering completely and produce inference by randomization or permutation tests. The advantage is that these tests do not depend on assumptions about the shape of the error distribution. They work by shuffling the timing of the treatment in each province, generating placebos. I also report p-values generated from permuting treatment assignment among provinces and cohorts. My chosen approach leaves fixed the number of provinces treated for each cohort and permutes only the order in which provinces are treated, following Wing and Marier (2014). I go over all the potential combinations of provinces—342 repetitions in all.[34]

4.2 Alternative Specification: Exploiting School Type

Besides province and cohort, 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 difference-in-differences strategy specified below:

[33] Since the treatment effects of the program show some heterogeneity across provinces, the difference-in-differences estimate is hard to interpret and generalize to the entire population which is why I do not follow that identification strategy.

[34] My results are also robust to clustering standard errors by cohort or two-ways by cohort and province (see web Appendix).

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

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 who 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 private school students in the absence of the intervention. On the other hand, private and public school students are very different (private school student typically have higher income and more educated parents), and it's not clear that they would experience parallel 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 towards zero.

Since most of the private-school population resides in Montevideo, and for a differences-in-differences specification I need sufficient private-school observations in each province, I limit the sample to Montevideo residents for this specification. My results are reported in the web Appendix; they are very similar to the ones obtained with the main specification. Within Montevideo, I cluster standard errors at the neighborhood level (64 neighborhoods).

4.3 Threats to Identification

In this section I discuss two 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, which is not discussed here but in Subsections 5.1.3 and 5.2.4, is using the wrong functional form: a non-linear pre-trend could bias my results either way.

4.3.1 Cohort Assignment

My analysis relies heavily on my ability to distinguish between before- and after-intervention cohorts and their "distance" from treatment. The ideal way to classify individuals into cohorts would be to know exactly the *school grade* they were enrolled in when the program reached their province. This information would obviate the need for partially treated ("in-between") cohorts. Unfortunately, this information is not available in any of my data sources. In this subsection, I explain how I classify individuals into cohorts further from or closer to exposure to Plan Ceibal, and I show how my treatment of in-between cohorts addresses the concerns of attenuation bias.

In this paper I estimate students' date of exposure to the laptop problem based on their date of birth, assuming that children start primary school at the compulsory starting age to determine their grade at the time of the program. In Uruguay, children can begin primary school if they are at least six years old in March or turning six by the end of April. There is evidence that the regulation is respected: all students enrolled in the first grade of primary school in 2006 were at least 6 years old by April 30. Moreover, this age group represented more than 66% of entering students and an estimate based on the age law is the best predictor of being enrolled in grade 1 conditional on primary school enrollment.[35]

Because date of birth is not available in the ECH survey, in the first part of the paper I use information on age, month, and year of observation to determine a student's probability of turning six by April of a given year, under the assumption that births are uniformly distributed across the year.[36] For observations occurring in October, the probability of being in one cohort or the following one is exactly 50%. For this reason, I eliminate that month from my dataset when using this method and classify individuals in the cohort for which the probability surpasses 50%. This way, misclassification error stays well below 25%.

My methodology works well: about 80% of students who I classified in second grade were indeed enrolled in second grade. However, only about 50% remained enrolled in the right grade for their cohort by the end of middle school, which suggests that repetition is a non-negligible concern. More generally, almost 20% of students repeat grade 1, and only 40% of students enrolled in grade 12 in 2011 were in the correct age for the grade. But, conditional on starting middle school, 75% of students reached grade 12 at the expected time. I address this concern by identifying an in-between group in the analysis. In-between cohorts are those that would have never been exposed to the intervention if it weren't for the fact that a fraction of them were enrolled one or two years behind their age in school in their respective province. My empirical approach treats these cohorts differently (and even drops them) to ensure that my estimate is not biased toward zero.

Finally, even with a perfect cohort assignment, there could be a bias toward zero for

[35] From the Ministry of Education of Uruguay. Refer to web Appendix Table A3 for more details.
[36] This simplifying assumption is supported by the vital statistics shown in web Appendix Table A2.

individuals with younger siblings (50% of students have younger siblings aged 5 to 18 at home). Because students are encouraged to take their laptops home, program participants could affect their relatives. Even if this is not the case, younger siblings can be a problem when estimating the effect of the program on the presence of computers at home. To address this concern, I limit the sample to individuals with no younger siblings aged 5 to 18 in their household —in all regressions that document the treatment effect on computer access, and in the robustness section for the rest of the results.

4.3.2 Province Assignment

My analysis also relies on my ability to classify adults in their province of residence at the time of the intervention. Ideally, I would like to know the exact province in which everyone attended primary and middle school. Unfortunately, I have this information only for a limited number of years and only for the university microdata. For the other data, I must decide between province of birth, province of residence, and province of past residence. Misclassification error is likely to create a bias toward zero, but the bias could go either way if migration was differential by treatment. If, for example, treated cohorts from the least developed provinces were more likely to migrate to the richer provinces than the previous cohorts, the effects might be downward biased.

Uruguay is a highly centralized country—more than 40% of the population and educational opportunities are concentrated in Montevideo. Hence, cross-province migration exists and is likely to be correlated with educational choices. Using household survey data, I find two clear trend breaks in migration patterns by age. The probability of moving out of the province of birth is high before primary school (ages 0 to 5), plummets during formal education (ages 6 to 17), and spikes again after high school (ages 18–20). By the time they start primary school, 6% of students have already moved outside their birth-province; this percentage rises to 11% during the last year of high school and almost 15% at age 19. This trend suggests that individuals move to study or work after completing their formal education.

Since migration out of province of birth is already non-negligible by the start of primary school, my strategy for dealing with migration is to use the previous province of residence when measuring outcomes among adults and to use province of current residence when measuring outcomes among children. I also conduct robustness checks using province of birth (this information is available in all my datasets.) Cross-country migration is also a potential concern, but I will not be able to account for it in my data.[37]

[37] Net entries to the Carrasco Airport were increasing up to 2013, after which the trend reverts (net emigration represented 0.4% of the population in 2015). Unfortunately, the migration office is not able to separate this by age groups.

Finally, in one of my specifications I limit my dataset to Montevideo neighborhoods. Here migration is less of a concern, because treatment status does not depend on the neighborhood of residence, and because migrating for school or work is less necessary.[38] In 2011 the ECH survey included questions about cross-neighborhood migration: 83% of 18-year-old students who had lived in Montevideo for the past five years were still living in the same neighborhood as five years prior. This share is a bit higher among private school students relative to public school students (92% vs 80%).

4.3.3 Differences Between Older and Younger Cohorts

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 treated cohorts were already different at the baseline or experienced differential shocks before age 19.

Figure 4 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. Each scatter plot indicates the average value of an outcome according to distance from treatment in a province, while the dashed line is designed to be a linear fit for the cohorts that will never be exposed to the intervention in each province. Clearly, there are no significant variations from this linear trend among years of education, public school students, teacher employment, TV subscriptions, or parental education. Economic conditions, which generally vary over time, are a clear threat to identification. 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. I explore this relationship further and show that it is not a concern in the robustness section. Web Appendix Figure A4 plots a series of observable characteristics across cohorts in 2006, one year before the intervention. The last panel shows that household income is very similar across all cohorts. Web Appendix Figure A11 plots household income across cohorts for every age 11 to 19, to check that there were no obvious trend breaks at those critical ages (despite the 2016 economic downturn).

In Table 1 I estimate equation 1 for predetermined covariates and expect to find no effects. Panel A shows the regression results for 13 observable characteristics measured at age 11 (including the ones discussed above). As expected, none of these characteristics deviates significantly from trend. It is especially important to mention that there is no significant deviation from the trend for treated cohorts in employment or income

[38] Montevideo is small enough that it can be crossed from side to side in 1 hour by car, and has good public transport.

among teachers when students are around age 11. This is key for interpreting my results: it suggests that the program did not significantly affect the income or quantity of teachers, which was a potential concern. Panel B focuses on observable characteristics in 2006—the year before the program was implemented—when students of different cohorts have different ages. No significant difference exists among students in internet access at home, mobile phone ownership, government aid, household income, or the fraction of racial minorities. The only difference is that, if anything, treated cohorts (that were younger in 2006) were about 15% less likely to have a computer at home. But non-linear trends in ownership of technology across ages are present for all years before the start of the program.

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 nine cohorts of individuals living with no younger siblings – I use seven cohorts by province to guarantee three pre-intervention and two post-intervention cohorts in each province – in 2011. Panel A in Table 3 shows that the intervention increased access to a computer in the house among treated cohorts by about 17 percentage points (23%). Panel B estimates equation 1 in a doughnut sample that excludes the in-between cohorts in each province and the estimate is essentially unchanged. Results are significant at 1% level with and without controls, with robust and province-clustered standard errors, as well as with a p-value computed from a permutation test of treatment assignment and a province-clustered wild bootstrap. The estimated cross-cohort trend-break in 2011 is strictly positive in all but two provinces (see web Appendix Figure A7 and Figures A17–A19).

5.1 Educational Attainment

5.1.1 Summary Statistics

I start with background information on educational attainment in Uruguay. According to ECH data from 2015, only 56% of individuals aged 25 to 34 had at least some high school education, and only 39% had completed high school. Only 21% had at least some postsecondary education, and just 9% had earned a postsecondary diploma. At the university level, the numbers were even smaller: only 13% had any university education, and only 5.6% had earned an undergraduate degree. Clearly, there is ample margin to improve educational attainment in Uruguay. Moreover, Universidad de la Republica charges no tuition and has no restrictions to entry.[39] Therefore, if the program had any effect in the demand for university education, it would very likely translate into actual enrollment.

There are a few caveats. First, the university's classes and services are highly centralized in Montevideo; for students living in the rest of the country, there is a moving cost associated with studying for most university degrees. Second, the fact that enrollment is mostly costless results in a low graduation rate.[40] For students who drop out, it is possible that not enrolling in the first place would have been optimal.

Table 2 summarizes a set of descriptive variables for individuals observed around ages 18 to 20 using household survey data. In terms of access to technology, 80% of these individuals have a computer at home, and on average one computer is shared by every two people. This is what one might expect after learning how successfully Plan Ceibal was implemented. Although the program did generate significant cross-cohort variation, the gap gradually decreased, disappearing by age 18 (see web Appendix Figure A10 for details on this trend).

5.1.2 Empirical Analysis

I start this section by using household survey data. My outcomes are: years of education, high school enrollment, high school graduation, post-secondary enrollment and university enrollment. These outcomes are all measured at the same age (around age 19) for each cohort. This age corresponds to the survey year in which individuals should have been enrolled in the second year of college had they gone through the school system on time.

Figure 4 plots the fraction of individuals who graduated from high school (Panels C and D) or enrolled in post-secondary education (Panels A and B) for each value of "time since treatment." Time since treatment takes value 0 for the first cohort to be at least partially exposed to the program by age 19, in any given province. Time since treatment is –1 for the cohort that is immediately older in that given province and 1 for the cohort that is immediately younger in that given province. Once a cohort has been treated, all following (younger) cohorts are treated. Panels A and B show clearly that there is no change in trend among treated cohorts for both outcomes. Panels B and D compare students who attended public school vs. those who attended private school. Both series

[39] 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.

[40] According to Boado (2005) only 28% of students graduate in a timely manner. This percentage is lower in Engineering, followed by law, and higher in Medicine.

seem to continue in their respective trends without major discontinuities around the treatment threshold.

Table 3 shows the main empirical results for this section. Panel A estimates equation 1 in the complete sample. None of the estimated treatment effects associated with educational attainment are statistically significantly different from zero, which is robust across many different computations of the standard error. I estimate that the program was associated with only 0.06 additional years of education. This would correspond to only three additional weeks of instruction. On average, individuals in my sample have 10 years of education. The confidence interval for my estimate [–0.23, 0.35] implies a 2% decrease in schooling at its lowest bound and a 3.5% increase at its upper bound. The upper bound is not negligible; it corresponds to four additional months of education and represents 15% of a standard deviation (=0.35/2.42). However, it is possible to completely rule out increases of half a year of schooling or more. We can get the same takeaway from analyzing the magnitude of the coefficients corresponding to high school enrollment, high school graduation and university enrollment. Regarding post-secondary education, I estimate a statistically insignificant decrease of 2.3%, with a confidence interval of [–0.19, 0.14]. In web Appendix Figure A8, I plot the estimates province by province. The result is that Plan Ceibal had no statistically significant impact on college enrollment in any individual province. Moreover, the estimates are negative for about half the sample, and positive for the other half, which indicates that the direction of the effect is not clear and suggests that there was probably no effect of the program on schooling overall. Panel B estimates equation 1 in the restricted sample (without the in-between cohorts); the results are essentially unchanged.

In addition, I explore whether the effects of the program on years of education were heterogeneous among certain population groups (see Table 4). 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.[41]

5.1.3 Robustness Checks

In the web Appendix I go over various exercises that evaluate the robustness of these results along different dimensions.

[41] 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 below the median, positive for individuals with higher parental education and negative for individuals with lower parental education. This last finding is somewhat consistent with other findings in the literature, since parents with higher educational attainment are perhaps more likely to supervise their children's time using the computer and doing homework.

First, web Appendix Table A8 shows that my findings are robust to clustering the standard errors by cohort or to clustering two-way by province and cohort, while Table A9 shows they are robust to collapsing the sample by province and cohort.

Second, 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 (web Appendix Table A10). The results are unchanged.

Third, to address the concern that my results may be driven by functional form, I reproduce the empirical approach utilizing province-specific quadratic trends (web Appendix Table A11), or using a more standard aggregate linear trend (web Appendix Table A12). The later may correspond better to the visual illustration of my outcomes. The results are unchanged.

Fourth (web Appendix Table A13), I address the possibility that life-cycle income shocks affected educational choices. My main concern is the mild economic downturn in 2016, which occurred when the first post-intervention cohort was 19, the second cohort was 18, and the preintervention cohorts 20 and older. In web Appendix Figure A11, I show that this downturn was not very important in terms of affecting household income. But, although most schooling had been completed by this age, I wonder whether differential income patterns might have affected educational attainment for the small fraction of children who graduated from high school or enrolled in a postsecondary institution. I first check whether there were significant differences in enrollment in the education system by age 17 across cohorts. I estimate that treated cohorts were five percent (5 percentage points) more likely to remain enrolled in the education system by this age. However, the statistical significance is not robust to different ways of computing standard errors, and a graphical analysis shows that, if anything, the change in trend is happening among the preintervention cohorts. Alternatively, I run a specification at age 19 that caps years of education at 11 (only two years of high school), knowing that students are expected to complete 11 years of education by age 17. There are no effects in this regard either. Then, I focus on years of education completed by age 19 but exclude the second post-intervention cohort in every province. Because the first post-intervention cohort would have been 19 in 2016, and because I am considering only years of education completed, this should be a good robustness check. I find non-significant estimates that are similar in size to the original ones. Finally, I conduct my normal specification (years of education at age 19) but control explicitly by province-specific income trends at ages 18 and 19 across cohorts. The results are unchanged. I conclude that differential income shocks across the life cycle are not driving my finding of essentially no effects from the program on educational attainment.

In fifth place (web Appendix Table A14), I explore the effects of Plan Ceibal on years of education using cross-sectional data. First, I use the cross-section of cohorts in 2017. Of course, years of education does not follow a linear trend across cohorts in the cross-section, because cohorts are observed at different ages and educational attainment is non-linear on age. To address this, I use the cross-section of two previous years (2011 and 2013) as control groups. If I observe a change in trend among the treated cohorts with respect to the control group, I interpret this as an effect from the program. Using the cross-section allows me to include more cohorts in my analysis and thus better predict the pre-trend. It also guarantees that all cohorts are responding to educational attainment questions from the identical survey, so I don't have to worry about confounding year-specific shocks with cohort-specific shocks. The analysis that relies on the 2017 cross-section is only valid with the 2013 control group because the 2011 control group is inconclusive. The 2013 control group shows no significant effects from the program. I also explore using the 2016 cross-section. Here I find no statistically significant effects from the program, even after capping years of education at 12 (high school graduate). This additional evidence supports the fact that my findings are robust to the income shocks of 2016.

In sixth place (web Appendix Table A15), I show that the program also had no differential effect between public and private school students on years of education completed by age 19. The estimates would indicate a (weakly significant) decrease in public school children's probability of graduating from high school or enrolling in post-secondary education and university, relative to private school children after the program. 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 seventh place (web Appendix Table A16), I address the main limitation of the paper, which is a down-ward bias to the extent that older cohorts of individuals interact with the laptops of their younger siblings. I do this by restricting the sample to individuals living with no younger siblings. The results are unchanged.

Finally, in web Appendix Table A17, 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 (although positive in sign!) on university enrollment, as a fraction of individuals who made it to the last year of secondary school in their respective provinces. This is consistent with the rest of the results I obtained using the ECH data. The confidence interval allows for a decrease of about 5% to an increase of about 15% in university enrollment.

In sum, my findings regarding educational attainment seem robust to different specifications and to different ways of classifying students as exposed or not exposed to the intervention. With this in mind, I move on to the second part of my analysis, which explores how the program affected educational choices among students enrolled in the public university system.

5.2 Choice of Major and Scholarship Applications

5.2.1 Summary Statistics

Table 2 (Panel B) shows descriptive statistics of incoming students at Universidad de la Republica in the 2012–2016 period, after reducing the sample to recent high school graduates (students aged 18 to 20). The average age in this sample is 19.35.[42] More than 60% of entering students are female, more than 55% are born in Montevideo, 68% did their primary education in the public sector, and 63% did their secondary education in the public sector. More than 70% still live with their parents, and only 5% live alone, which is consistent with the age-group average in the ECH dataset. However, almost none of the individuals in this sample have children, which is consistent with the fact that pregnancy is among the main reasons for not completing high school. Analogously, only 13% of individuals in this sample were working at the time of enrollment, which is significantly lower than the average in the population for this age group (40% in the ECH dataset). Regarding family background, about 23% (30%) of students declared that their father (mother) had completed post-secondary education. Almost half of the sample (48%) are the first in their family to attend post-secondary classes, and 65% are the first to attend university. In terms of academic performance, 30% of the sample had applied for a college scholarship (financial aid), 18% enrolled in a technological major, 14% enrolled in multiple majors (multi-majored), and 2% had previous post-secondary studies. The most common of these scholarships, Fondo de Solidaridad, grants a monthly stipend equivalent to half of a person's legal minimum income.[43]

I defined technological majors as those that contain certain keywords in their description. Specifically, I web-scraped the descriptions of all undergraduate degrees on the Universidad de la Republica 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 (see web Appendix Table A19 for the complete list). The three non-STEM exceptions are communication (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). In web Appendix Figure A13, I show that enrollment in technological majors decreased from

[42] Most people (60%) are 19 years old, followed by 18 (27%) and 20 (13%).

[43] http://becas.fondodesolidaridad.edu.uy. This fund is a public organization, created by law in 1994, and its task is to provide scholarships for post-secondary education in public institutions.

2006 to 2016, with two small spikes in 2010 and 2013. A subcategory of these including computer engineering, technologist in informatics, and electronic and digital art, encompasses about 5% of total enrollment. These three spike in 2010 only and are flat in the years in which the first cohorts should be reaching college.

5.2.2 Empirical Analysis

Table 6 shows the main results for this section. I first test whether Plan Ceibal increased scholarship applications. I find no evidence that the program increased scholarship applications among enrolled students. It is possible that the scholarship was already sufficiently publicized, as students could be made aware of it at the time (or prior to the time) of physically completing the enrollment form and a about a third of them apply each year. In accordance with the first part of the paper, I do not find conclusive evidence of an effect of the laptop program on the probability of being the first in the household to enroll in postsecondary education. The coefficients vary in sign, size, and significance, depending on the specification and inclusion of control variables.

I then test whether enrollment in technology-related majors increased because of the increase in computer access. I find no evidence to support that hypothesis. For enrollment in both technology-related and computer-specific majors, my estimates are not statistically different from zero, and the signs are, if anything, negative. However, the one-laptop-per-child program appears to have strongly decreased the practice of enrolling in multiple majors at the same time. The magnitude of this effect is very large (29 percentage points, from a mean of 30%), implying that this practice virtually disappeared; this is statistically significant and robust to different ways of computing standard errors. I interpret this finding as evidence that students have become more knowledgeable about their majors beforehand and thus don't need to sit in on classes in different fields. As noted earlier, the Universidad de la Republica's website has been up since 2006 and has always showcased the complete list of available majors with their descriptions, suggesting that among these two factors, it's the students' access to information, not the mere existence of it, that has eliminated the practice of enrolling in multiple majors at the same time.

In Table 7, after grouping majors into five general areas (arts, agrarian sciences, social sciences, science and technology, and health), I further analyze whether Plan Ceibal exerts any effect on the choice of major. I find that the intervention was associated with a strong decrease in enrollment (about 30%; 0.05 and 2 percentage points, respectively) in the arts and agrarian sciences, and with a notable increase in enrollment (about 16%; 4 percentage points) in health. The program had no statistically significant effect on enrollment in social sciences or science and technology, although the coefficients suggest a 4% decrease in enrollment in the social sciences (2 percentage points) and a 2% increase in enrollment in science and technology, relative to enrollment in the other areas of study. The estimates are robust to the base category.[44] This suggests that students who were exposed to technology at a young age are more likely to select high-employment majors.

In a survey of former Universidad de la Republica students who graduated in 2010 and 2011 (see web Appendix Table A20), the university found that those who completed health-related majors were less likely to be unemployed, were more satisfied with their salary, and were less likely to regret having pursued a college degree. This suggests that access to technology over time may have given this cohort better access to information and communication when choosing their major.

5.2.3 Interpretation

In my empirical section I found that the program was associated to a lower probability of multi-majoring, as well as to a higher probability of selecting health-related majors as opposed to art-related majors.

One channel that could be at work is access to information through technology. This channel relies on the ability to find information online, which is enhanced through years of experience. The first thing I would like to know, is whether information about employment and income prospects for various occupations and majors was available online when both the "before-intervention" and "after-intervention" cohorts were entering college. For instance, I conduct a Google search for articles published between 2008 and 2010; the articles found emphasized the high employment rates in hospitals (health workers) and low employment rates and income in the arts. Therefore, students looking up what to study and deciding based on these economic factors would have been able to find this information on the web. Information about the content, duration, and requirements of majors has been available on the public university's website since 2006.

I computed additional summary statistics using household survey data for the 2012–2017 period for a population aged 30 to 40 with university degrees. STEM graduates have the highest income but also the highest unemployment. Students from health-related majors are the most likely to be employed. Regarding the popularity of different majors, in the web Appendix (Table A21) I show that most people choose medicine, business, law and social sciences & behavior–related majors. Women tend to choose these fields more than men—save for business. The highest-paying fields are engineering and informatics,

[44] The exception is enrollment in the Arts, which is problematic due to is small size: only 0.17% of students enroll in this category.

followed closely by business, agricultural sciences, security services, and industrial services, all of which tend to have high employment rates as well. The lowest paying fields are education, personal services, humanities, the arts, and life sciences. Journalism, the arts, and architecture have the highest unemployment. Moreover, health is the area with the highest employment growth from 2012 to 2016, while employment shrank in social sciences and science and technology.

In web Appendix Figure A13, I show that enrollment in technological majors has been decreasing over time (from 2006 to 2016), with two small spikes in 2010 and 2013. A subcategory of these including Computing Engineering, Technologist in Informatics and Electronic and Digital Art, encompasses about 5% of total enrollment. They show a spike in 2010 only, and are flat in the years in which the first cohorts should be reaching college. In web Appendix Table A22 I show descriptive statistics for the graduated student satisfaction survey conducted by the public university system among former students in 2010–2011. The survey grouped majors into three areas: natural sciences and technology; social sciences, humanities, and the arts; and health. It appears that students of social sciences, humanities, and the arts were the least satisfied with their salaries and the most likely to regret pursuing a university degree. On the other hand, health students expressed the highest salary satisfaction and were the least likely to regret pursuing a university degree. Natural sciences and technology students were the least likely to regret their major of choice.

Thus, I interpret my results as suggestive evidence that students who were exposed to the program were more likely to select majors with good employment prospects.

5.2.4 Robustness Checks

In the web Appendix I go over various exercises that evaluate the robustness of these results along different dimensions.

I start by demonstrating that the decrease in the practice of enrolling in multiple majors associated to the laptop program is very robust. First, web Appendix Table A23 shows that this finding is robust to clustering the standard errors by cohort or to clustering two-way by province and cohort, while Table A24 shows it's robust to collapsing the sample by province and cohort.

Second, I repeat my empirical approach using year of college enrollment rather than date of birth to assign treatment status (web Appendix Table A27). In panels C and D I address the possibility that the economic slowdown of 2016 may have affected educational choices that year by limiting the sample to 2006–2015. The result is unchanged.

Third, to address the concern that my results may be driven by functional form, I reproduce the empirical approach utilizing province-specific quadratic trends (web Appendix Table A25), or using a more standard aggregate linear trend (web Appendix Table A26). The result is unchanged.

Fourth, in Table A26 I address the fact that the public university system underwent some reforms that duplicated the total number of majors available to students from 2006 to 2009, by restricting the sample to the period 2009–2016. The negative effect in the practice of enrolling in multiple remains large and statistically significant.

In fifth place (web Appendix Table A29), I show that the program indeed seems to have had a differential effect between public and private school students regarding multiple enrollment. The estimates indicate a significant decrease in public school children's probability of enrolling in multiple majors at college, relative to private school children, after the program. This is further evidence that what I am capturing is not merely cohorts fixed effects, but that the effect is stronger among those who were exposed to the program.

Finally, in web Appendix Table A30 I restrict the sample to individuals living with no younger siblings and the results are unchanged.

In addition, I also demonstrate the robustness in the increase in enrollment in health-related majors associated to the program (based on a multinomial logit). First, web Appendix Table A31 shows that this finding is robust to clustering the standard errors by cohort.

Second, I repeat my empirical approach using year of college enrollment rather than date of birth to assign treatment status (web Appendix Table A34). In panels C and D I address the possibility that the economic slowdown of 2016 may have affected educational choices that year by limiting the sample to 2006–2015. The result is unchanged.

Third, to address the concern that my results may be driven by functional form, I reproduce the empirical approach utilizing province-specific quadratic trends (web Appendix Table A32), or using a more standard aggregate linear trend (web Appendix Table A33). The result is unchanged.

Fourth, in Table A33 I address the fact that the public university system underwent some reforms that duplicated the total number of majors available to students from 2006 to 2009, by restricting the sample to the period 2009–2016. The negative effect in the practice of enrolling in multiple remains large and statistically significant.

In fifth place (web Appendix Table A36), I show that the program indeed seems to have had a differential effect between public and private school students regarding multiple enrollment. The estimates indicate a significant decrease in public school children's probability of enrolling in multiple majors at college, relative to private school children, after the program. This is further evidence that what I am capturing is not merely cohorts fixed effects, but that the effect is stronger among those who were exposed to the program.

Finally, in web Appendix Table A37 I restrict the sample to individuals living with no younger siblings and the results are unchanged.

6 Intermediate Outcomes

This section provides context for interpreting my findings. The main reason adolescents and young adults don't complete secondary school is lack of interest. Since the internet is used mainly for entertainment, communication and information, one would expect that computers could make the educational process more interesting to students.[45] However, very few students report using the internet for learning or educational activities. I check also whether the program may have induced a different use of the internet, but that did not seem to happen. It appears that students who were exposed to the program are more likely to use the internet at age 19 overall. Thus, they were unconditionally more likely than their peers to use the internet both educationally and (proportionally more) as a source of entertainment.

More surprisingly, internet use remains higher for after-intervention cohorts, even after the older cohorts were equally likely to have a computer at home (see Table 5). This is consistent with previous evidence that years of experience using computers and the internet improve operational ability, and therefore are good predictors of internet skills, including the ability to conduct informational searches. This factor could be explaining Plan Ceibal's strong effects on university students. Previous evidence also finds that, beyond experience, education and age are strong predictors of the ability to search online for informational content. This is also consistent with the fact that Plan Ceibal's effects are concentrated among college students.

The second reason adolescents and young adults don't complete secondary school is that they start working. If computers help people find a job online or make them more appealing on the labor market, we would expect adolescent employment to rise. However, this does not appear to be the case (see Table 5).

The third reason is pregnancy (of the student or their partner). My analysis shows that the likelihood of being a parent by age 19 is significantly lower among cohorts that were exposed to the program. This could be due in part to the legalization of abortion in 2012, but it is also consistent with access to information, which may have helped increase

[45] Entertainment is defined as *"playing games, downloading music etc."*

take-up of abortion services after the law was enacted.

7 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 increased; however, the program appears to have had considerable effects on other margins. For instance, students who went on to university were more likely to select majors with good employment prospects. They were also less likely to enroll in multiple majors at the same time, thereby reducing congestion in the public university system.

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 and unrestricted public university system, and a larger margin for improving educational attainment in its population than other countries in the region.[46] On the other hand, Uruguayan children may face higher restrictions to primary and secondary education, limiting potentially positive effects of the program.

In terms of implications for public policy, my findings suggest that simply expanding access to technology (rather than the use of technology for educational purposes) does not necessarily improve educational attainment. Policymakers looking to improve years of schooling could complement one-laptop-per-child programs with activities that increase educational usage, investing in teacher training and educational software. The first few cohorts to be exposed to Plan Ceibal were in general not exposed to complementary programs later developed by the organization, some of which show a lot of promise and could contribute to improved outcomes in later generations.[47] Alternatively, with the same resources (approximately 600 dollars per student), Uruguay could have employed

[46] Source: (US) Census Bureau, OECD, and OECD and World Bank tabulations of SEDLAC (CEDLAS and the World Bank) for Latin America and the Caribbean. The last one is for 2014 and for population age 25-29.

[47] See Perera and Aboal (2017) for evidence of positive effects of the Ceibal Adaptive Math Learning Platorm (started 2013) on learning.

full-time teachers in 100 schools, a mode of schooling that has shown promising results on educational outcomes of students of low socioeconomic status (Cardozo Politi et al., 2017).[48] This would have targeted a smaller number of individuals, but with potentially positive long-run results.

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. 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.[49][50] 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.

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[48] From official Ceibal Financial records 2010-2016, the Institute of Statistics and the Government Budget 2006 and 2008. The estimation per student uses that 429,016 students were enrolled in public primary and middle school in 2007 and assuming the number of students would have exactly duplicated by 2016.

[49] World Summit on the Information Society, 2003.

[50] 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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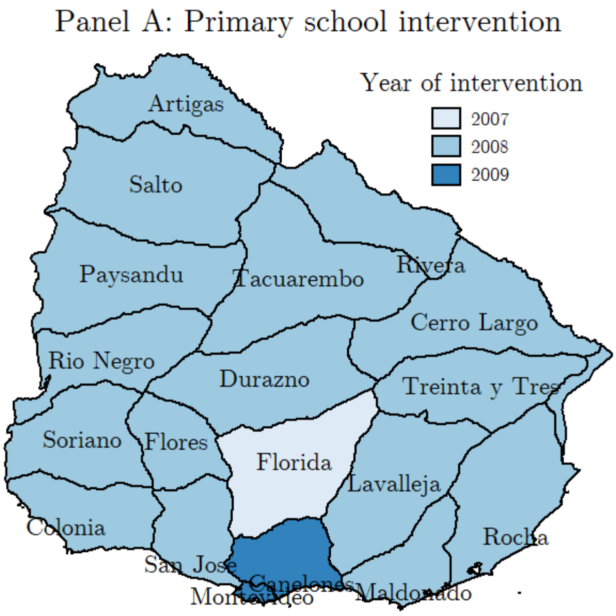
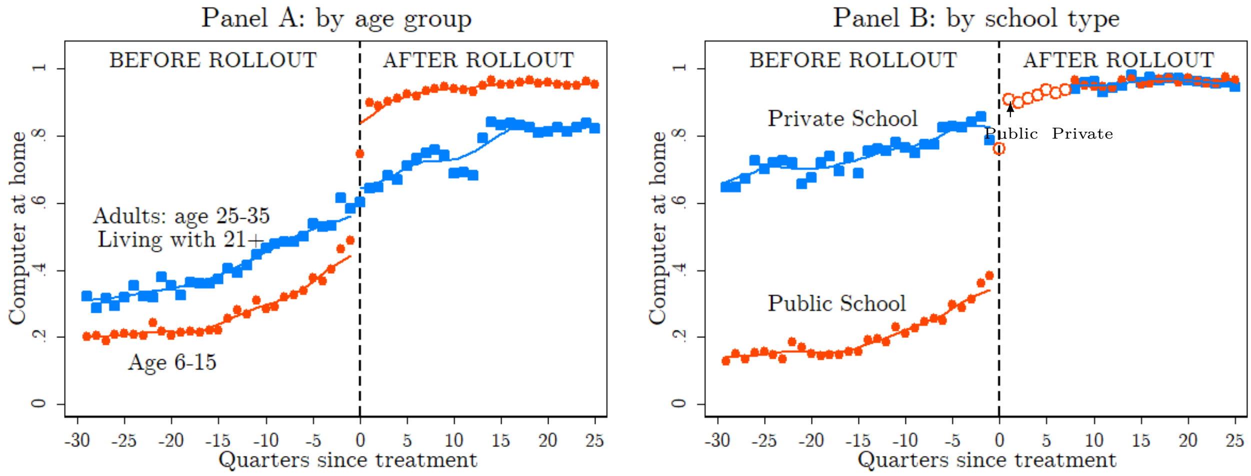
8 Figures and Tables

Figure 1: Year of Plan Ceibal's initiation in Uruguay by province



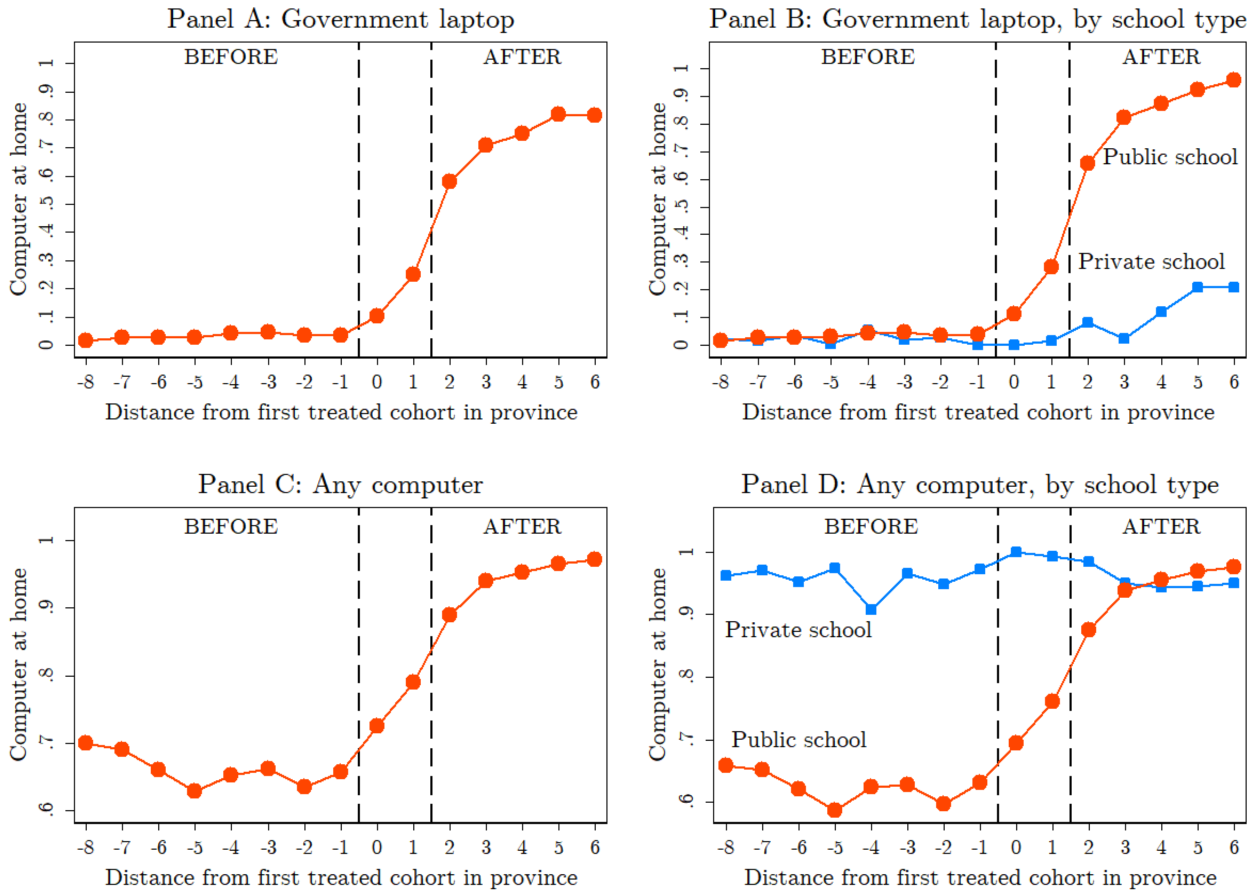
Notes: Panel A summarizes the rollout of Plan Ceibal in Uruguay among primary school students between 2007 and 2009, when full coverage was attained. Panel B summarizes the rollout of Plan Ceibal among middle school students between 2009 and 2011.

Figure 2: Quarterly computer access for children aged 6–15
Variations across school type and age groups



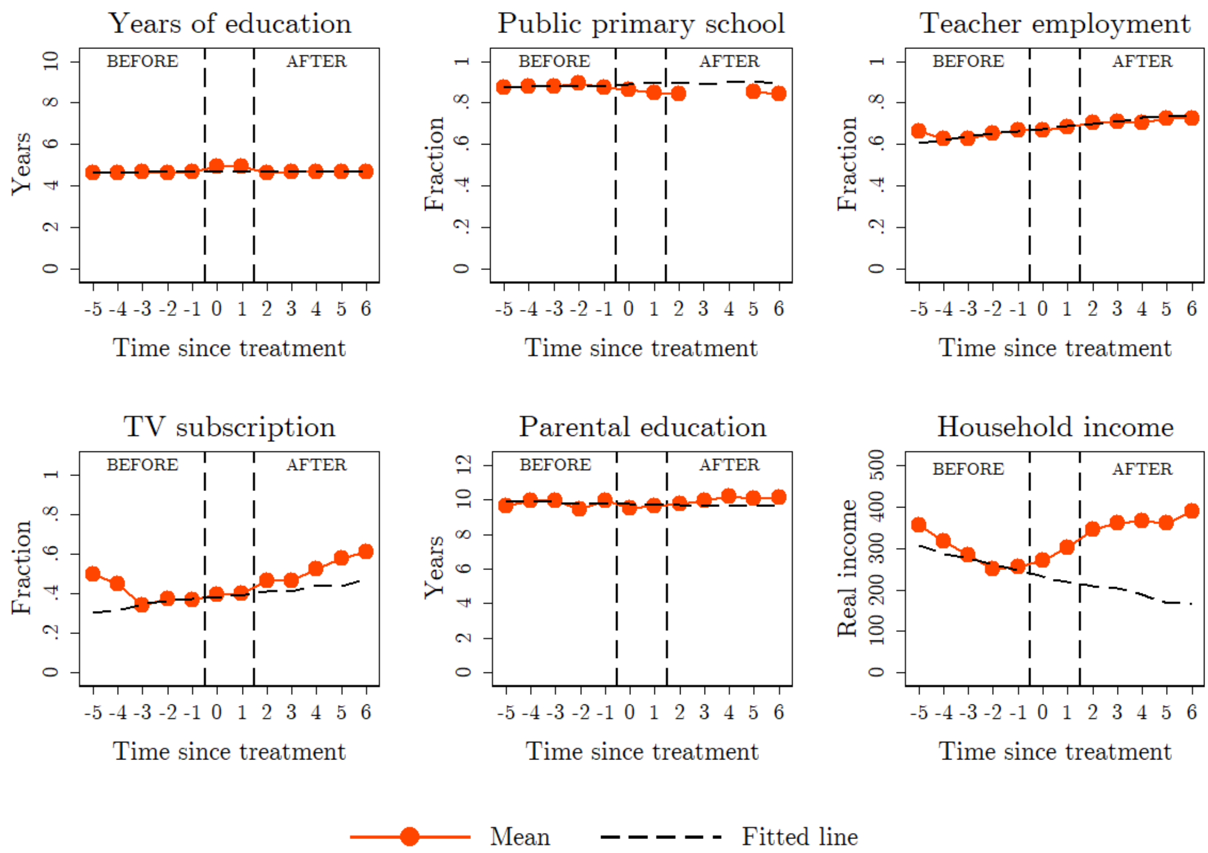
Notes: This figure shows the fraction of individuals with a computer at home for the population aged 6–15 at the quarterly level, stacked according to the timing of the primary school intervention in each province. The empty circles in Panel A correspond to the entire student population, for quarters in which data on school type is not available for most provinces. The majority of students are enrolled in the public school system. The sample includes only urban areas with 5000+ inhabitants.
Source: Encuesta Continua de Hogares 2001–2017.

Figure 3: Differential access to computers in Uruguay as a result of the intervention
Variation across cohorts, provinces, and school types in 2011



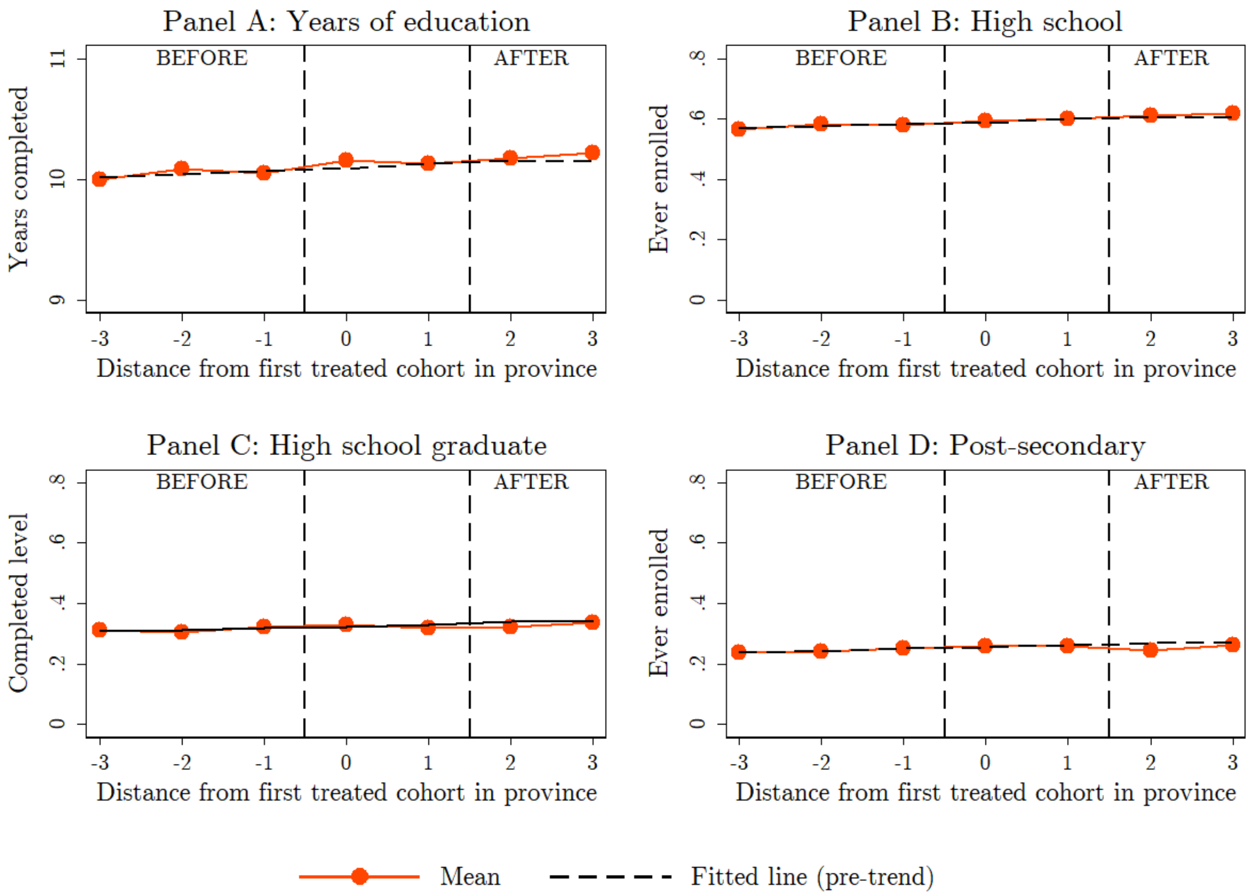
Notes: This figure shows the fraction of individuals with a government laptop at home (Panels A and B) or any computer at home (Panels C and D) in a given cohort, stacked across provinces. A cohort is defined as the group of individuals that is expected to start primary school in the same academic year; 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.

Figure 4: No major discontinuities in other variables
Measured around age 11 (grade 6)



Notes: This figure plots potential confounding variables across cohorts for individuals age 11, based on distance from treatment in their respective provinces of residence. The dotted line represents a fitted line estimated among pre-intervention cohorts within each province, excluding any additional controls. I explore the the evolution of household income and show that it is not a concern in section 5.
Source: ECH 2001–2017.

Figure 5: Evolution of fraction enrolled in high school and post–secondary education
Measured around age 19 across cohorts and provinces



Notes: This figure plots educational attainment by age 19 across cohorts based on time since treatment in their respective provinces. Panel A plots the average schooling in the population (years completed). The subsequent panels plot the fraction of individuals who enrolled in high school (B), who graduated from high school (C) and who enrolled in postsecondary education (D). 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 2009–2017.

Table 1: Analysis of baseline characteristics

	Complete sample				Doughnut sample		
	Mean	Test	SE Clustered	SE Robust	Test	SE Clustered	SE Robust
A. Around age 11 (5 years of education)							
Male	0.526	-0.00982	(0.0198)	(0.0262)	0.532	(0.0190)	(0.0267)
Public school	0.864	-0.0405	(0.0225)	(0.0205)	0.865	(0.0233)	(0.0210)
Lagging behind	0.355	0.0146	(0.0258)	(0.0249)	0.344	(0.0258)	(0.0255)
Years of education	4.722	-0.0404	(0.0543)	(0.0606)	4.669	(0.0538)	(0.0619)
Younger siblings	0.467	-0.00784	(0.0203)	(0.0262)	0.460	(0.0224)	(0.0267)
Household size	4.997	-0.117	(0.0954)	(0.102)	4.987	(0.102)	(0.104)
Parent w/high school	0.767	-0.0386	(0.0350)	(0.0222)	0.772	(0.0348)	(0.0226)
Parent w/college	0.175	0.00975	(0.0228)	(0.0195)	0.175	(0.0225)	(0.0199)
Parental education (years)	9.870	-0.0527	(0.283)	(0.215)	9.942	(0.274)	(0.220)
Household income ($ UY)	26,834	2,955	(2363.0)	(1624.6)	3,276	(2,592)	(1,663)
TV subscription	0.457	0.0120	(0.0590)	(0.0250)	0.471	(0.0581)	(0.0255)
Teacher employment	0.684	0.0140	(0.0159)	(0.0023)	0.686	(0.0162)	(0.00238)
Teacher income (> p50)	0.622	0.0108	(0.0103)	(0.00229)	0.0148	(0.0102)	(0.00229)
			N=16,271			N=11,409	
B. Before program, in 2006							
Computer at home	0.290	-0.0538	(0.0120)	(0.0117)	-0.0557	(0.0126)	(0.0117)
Internet connection	0.139	-0.0126	(0.0135)	(0.00912)	-0.0130	(0.0130)	(0.00916)
Mobile phone (not smart)	0.609	0.0120	(0.00873)	(0.0117)	0.0127	(0.00919)	(0.0118)
Government aid	0.294	0.00317	(0.0102)	(0.0111)	0.00218	(0.0111)	(0.0111)
Household income ($ UY)	20,809	66.73	(404.3)	(576.7)	120.5	(416.8)	(579.2)
Nonwhite	0.157	-0.0119	(0.0116)	(0.00881)	-0.0141	(0.0119)	(0.00884)
			N=55,608			N=47,216	

Notes: The first column reports the average values for each variable. The other columns report estimates of θ obtained from estimating equation 1 without control variables. Regressions include nine cohorts in total, including three pre-intervention and two post-intervention cohorts in each province. This classification is based on current province of residence. Robust and province-clustered standard errors are reported (clusters: 19).
Source: ECH 2001–2017.

Table 2: Descriptive Statistics: individuals aged 18–20

Household Survey Data [2011–2017]		Administrative Data From Public University System [2012–2016]	
Variable	Mean	Variable	Mean
Males	0.511	Age	19.35
Nonwhite	0.171	Male	0.383
Below poverty line	0.133	Born in Montevideo	0.55
Household size	4.31	Public primary school	0.68
Lives with parents	0.832	Public secondary school	0.631
Has children	0.177	Children	0.002
Employed	0.417	Lives with parents	0.725
Has computer at home	0.806	Lives alone	0.044
Has internet at home	0.628	Father post secondary education	0.234
Has a non-Ceibal computer at home	0.628	Mother post secondary education	0.307
Computers per person	0.488	First to attend post secondary	0.478
Used computer last month	0.758	First to attend university	0.653
Uses internet every day	0.64	Works	0.128
Primary school was public	0.861	Scholarship	0.304
Middle school was public	0.848	Technical major	0.178
University was public	0.86	Multiple majors	0.141
Ever enrolled in high school	0.588	Previous post-secondary studies	0.02
Graduated from high school	0.285		
Ever enrolled in technical school	0.121		
Graduated from technical school	0.039		
Ever enrolled in post-secondary education	0.218		
Ever enrolled in university	0.179		

Notes: Summary statistics (means) for individuals aged 18–20.
Source: ECH 2011–2017 and Universidad de la Republica del Uruguay 2012–2016.

Table 3: Effect of intervention on computer access and educational attainment around age 19

	Computer access in 2011		Years of education		High school: enrolled		High school: graduate		Post-secondary: enrolled		University: enrolled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Complete sample												
ITT	0.161***	0.178***	0.0655	0.0621	0.00806	0.0151	-0.00429	-0.00900	-0.0187	-0.0259	0.00908	0.000486
Cluster SE	(0.0391)	(0.0288)	(0.149)	(0.147)	(0.0311)	(0.0282)	(0.0286)	(0.0302)	(0.0212)	(0.0216)	(0.0177)	(0.0179)
Robust SE	(0.0441)	(0.0406)	(0.171)	(0.170)	(0.0342)	(0.0342)	(0.0322)	(0.0323)	(0.0298)	(0.0299)	(0.0275)	(0.0278)
WB/PT p-value	0.004/0.018	0.004/0.015	0.665/0.5	0.696/0.6	0.798/0.55	0.597/0.65	0.88/0.34	0.78/0.27	0.384/0.24	0.335/0.27	0.599/0.8	0.977/0.9
Mean	0.762		10.12		0.595		0.321		0.251		0.204	
Observations	6,038		11,421		11,421		11,421		11,421		11,421	
B. Doughnut sample												
ITT	0.196***	0.218***	0.0175	0.0185	0.00725	0.0230	-0.0164	-0.0223	-0.0265	-0.0329	0.00419	-0.00903
Cluster SE	(0.0381)	(0.0296)	(0.163)	(0.166)	(0.0358)	(0.0353)	(0.0314)	(0.0338)	(0.0233)	(0.0242)	(0.0187)	(0.0182)
Robust SE	(0.0483)	(0.0446)	(0.188)	(0.191)	(0.0374)	(0.0383)	(0.0352)	(0.0362)	(0.0325)	(0.0336)	(0.0300)	(0.0314)
WB/PT p-value	0.0016/0.000	0.00210/0.029	0.92/0.51	0.92/0.64	0.84/0.63	0.528/0.8	0.61/0.29	0.57/0.28	0.29/0.15	0.29/0.16	0.83/0.9	0.68/0.07
Mean	0.762		10.11		0.593		0.320		0.248		0.204	
Observations	4,308		7,970		7,970		7,970		7,970		7,970	
Province FE	✔	✔	✔	✔	✔	✔	✔	✔	✔	✔	✔	✔
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 5 years prior except for past computer access where province of residence in 2011 is used. Regressions include nine cohorts in total, with three pre-intervention and two post-intervention cohorts in each province. Past computer access is measured in 2011. All other outcomes are measured around age 19. Robust and province-clustered standard errors are in parentheses (clusters: 19); p-values from province-clustered wild-bootstrapped t-statistics and from province-by-cohort permutation tests are also presented.

Source: ECH 2001–2017.

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

Table 4: Heterogeneity — effects of the intervention on years of education around age 19

	Geography		Gender		Income		Parental Education	
	Montevideo	Elsewhere	Boys	Girls	Below median	Above median	High school degree	No high school degree
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Complete sample								
ITT	0.485	0.129	0.0577	-0.108	0.0388	-0.178	-0.222	0.198
Cluster SE	(0.842)	(0.210)	(0.229)	(0.167)	(0.272)	(0.268)	(0.313)	(0.185)
Robust SE	(0.871)	(0.210)	(0.222)	(0.213)	(0.244)	(0.196)	(0.218)	(0.194)
WB/PT p-value	–	0.583/0.63	0.81/0.57	0.53/0.34	0.90/0.48	0.823/0.45	0.86/0.36	0.30/0.49
Mean	10.28	10.04	9.711	10.50	9.368	10.70	11.80	9.424
Observations	3,755	7,666	5,522	5,559	5,079	6,002	3,199	7,882
B. Doughnut sample								
ITT	0.153	0.0776	0.114	-0.260	-0.0812	-0.168	-0.287	0.121
Cluster SE	(1.079)	(0.0690)	(0.193)	(0.203)	(0.279)	(0.335)	(0.368)	(0.186)
Robust SE	(1.116)	(0.233)	(0.242)	(0.236)	(0.270)	(0.216)	(0.239)	(0.214)
WB/PT p-value	–	0.570/0.43	0.553/0.3	0.378	0.8/0.75	0.94/0.54	0.78/0.78	0.52/0.61
Mean	10.30	10.01	9.696	10.49	9.330	10.70	11.80	9.416
Observations	2,616	5,354	3,812	7,970	7,970	4,168	2,183	5,494
Province FE	✔	✔	✔	✔	✔	✔	✔	✔
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 5 years prior. Regressions include nine cohorts in total, with three pre-intervention and two post-intervention cohorts in each province. Outcomes are measured around age 19 for every cohort. Robust and province/neighborhood-clustered standard errors are in parentheses (19 provinces, 64 neighborhoods in Montevideo); p-values from province-clustered wild-bootstrapped t-statistics and from province-by-cohort permutation tests are also presented.
Source: ECH 2001–2017.

Table 5: Understanding the findings — effects of the intervention on early parenthood, employment, and technology use by age 19

	Teen Parent		Employed		Current computer access		Computer & internet use		Internet for information & education	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Complete sample										
ITT	-0.0704***	-0.0628***	-0.0306	-0.0356	-0.0702***	-0.0706***	0.0411	0.0234	0.0403*	0.0280
Cluster SE	(0.0140)	(0.0169)	(0.0261)	(0.0302)	(0.0213)	(0.0224)	(0.0384)	(0.0357)	(0.0227)	(0.0257)
Robust SE	(0.0226)	(0.0222)	(0.0345)	(0.0339)	(0.0275)	(0.0279)	(0.0337)	(0.0338)	(0.0304)	(0.0305)
WB/PT p-value	0.008/0.43	0.018/0.42	0.24/0.36	0.26/0.35	0.026/0.04	0.032/0.04	0.327/0.92	0.565/0.93	0.178/0.54	0.334/0.57
Mean	0.110		0.446		0.800		0.576		0.739	
Observations	11,421		11,421		11,421		11,421		11,421	
B. Doughnut sample										
ITT	-0.0683***	-0.0612***	-0.0502	-0.0545	-0.0953***	-0.0896***	0.0559	0.0532	0.0151	0.00473
Cluster SE	(0.0114)	(0.0145)	(0.0291)	(0.0347)	(0.0210)	(0.0240)	(0.0396)	(0.0380)	(0.0250)	(0.0296)
Robust SE	(0.0247)	(0.0248)	(0.0377)	(0.0381)	(0.0302)	(0.0317)	(0.0370)	(0.0381)	(0.0335)	(0.0344)
WB/PT p-value	0.002/0.75	0.005/0.72	0.07/0.497	0.152/0.64	0.002/0.16	0.002/0.17	0.160/0.58	0.201/0.71	0.546/0.69	0.875/0.74
Mean	0.110		0.444		0.794		0.554		0.736	
Observations	4,308		7,970		7,970		7,970		7,970	
Province FE	✔	✔	✔	✔	✔	✔	✔	✔	✔	✔
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, where province of origin refers to province of residence five years prior. Regressions include nine cohorts in total, with three pre-intervention and two post-intervention cohorts in each province. Outcomes are measured around age 19. Robust and province-clustered standard errors are in parentheses (clusters: 19); p-values from province-clustered wild-bootstrapped t-statistics and from province-by-cohort permutation tests are also presented.
Source: ECH 2001–2017.

Table 6: Effect of intervention on major choice, scholarship application, and intergenerational mobility in education among students in the public university system

	Technological major		Computer major		Multiple majors		Scholarship application		First to attend college	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Complete sample										
ITT	0.00373	-0.0120	-0.00850	-0.00943	-0.286***	-0.288***	-0.0320**	-0.0188	-0.0113	0.0844***
Cluster SE	(0.00832)	(0.00848)	(0.00650)	(0.00559)	(0.0716)	(0.0622)	(0.0134)	(0.0126)	(0.00675)	(0.00811)
Robust SE	(0.00522)	(0.00536)	(0.00288)	(0.00292)	(0.00859)	(0.00890)	(0.0135)	(0.0129)	(0.00692)	(0.00612)
WB/PT p-value	0.696/1	0.691/0.000	0.847/0.000	0.671/0.000	0.0071/0.000	0.006/0.000	0.0897/0.000	0.301/0.000	0.052/0.000	0.0004/0.000
Mean	0.163		0.0424		0.313		0.301		0.447	
Observations	110,023		110,032		110,032		56,324		110,032	
B. Doughnut sample										
ITT	0.00397	-0.0109	-0.00869	-0.0109*	-0.300***	-0.290***	-0.0472**	-0.0316*	-0.00512	0.0914***
Cluster SE	(0.00819)	(0.00773)	(0.00669)	(0.00578)	(0.0749)	(0.0618)	(0.0187)	(0.0166)	(0.00683)	(0.00859)
Robust SE	(0.00527)	(0.00550)	(0.00290)	(0.00299)	(0.00862)	(0.00884)	(0.0166)	(0.0164)	(0.00698)	(0.00625)
WB/PT p-value	0.674/1	0.669/0.000	0.873/0.000	0.612/0.000	0.00680/0.000	0.00600/0.000	0.0128/0.000	0.0897/0.000	0.426/0.000	0.0004/0.000
Mean	0.164		0.0430		0.288		0.301		0.445	
Observations	86,190		86,194		86,194		32,726		86,194	
Province FE	✔	✔	✔	✔	✔	✔	✔	✔	✔	✔
Controls	✘	✔	✘	✔	✘	✔	✘	✔	✘	✔

Notes: Panels A and B estimate equation 1 and and show the estimate of θ. Controls include age, gender, and parental characteristics. Province refers to province of birth and cohort is computed based on date of birth. Robust and province-clustered standard errors are in parentheses (clusters: 19); p-values from province-clustered wild-bootstrapped t-statistics and from province-by-cohort permutation tests are also presented.
Source: Universidad de la Republica del Uruguay, incoming student survey, 2006–2016.

Table 7: Effect of intervention on area of study at university

	Arts		Agrarian Sciences		Social Sciences		Science and Technology		Health	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<u>A. Complete sample</u>										
ITT	-0.007***	-0.005***	-0.018**	-0.019**	-0.025*	-0.021	0.020	0.005	0.030***	0.041***
Cluster SE	(0.001)	(0.001)	(0.007)	(0.008)	(0.015)	(0.015)	(0.015)	(0.015)	(0.006)	(0.007)
Robust SE	(0.002)	(0.002)	(0.003)	(0.003)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Mean	0.017		0.063		0.437		0.227		0.255	
Observations	109,978									
<u>B. Doughnut sample</u>										
ITT	-0.006***	-0.005***	-0.018**	-0.019**	-0.027*	-0.022	0.020	0.005	0.031***	0.041***
Cluster SE	(0.001)	(0.001)	(0.008)	(0.008)	(0.015)	(0.016)	(0.015)	(0.015)	(0.007)	(0.007)
Robust SE	(0.002)	(0.002)	(0.003)	(0.003)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Mean	0.017		0.064		0.439		0.228		0.251	
Observations	86,146									
Province FE	✔	✔	✔	✔	✔	✔	✔	✔	✔	✔
Controls	✘	✔	✘	✔	✘	✔	✘	✔	✘	✔

Notes: This table reports the marginal effects resulting from estimating equation 1 using a multinomial logit model. The largest category, social sciences, is used as the baseline. Province refers to province of birth and cohort is computed based on date of birth. Controls include age, gender, and parental characteristics. Robust and province-clustered standard errors are in parenthesis (clusters: 19).

Source: Universidad de la Republica del Uruguay 2006–2016.