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Essays in Applied Microeconomics

A dissertation submitted in partial satisfaction of the

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in Economics

by

Maria Lucia Yanguas

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ABSTRACT OF THE DISSERTATION

Essays in Applied Microeconomics

by

Maria Lucia Yanguas

Doctor of Philosophy in Economics

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Professor Adriana Lleras-Muney, Chair

This dissertation contains three essays in Applied Microeconomics. Chapter 1 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. Chapter 2 provides the first empirical evidence of the historical effects of natural disasters on economic activity in the United States. Although the literature has focused on salient natural disasters, more than one hounded strike the country every year, causing extensive property destruction and loss of life. My coauthors and I construct an 80 year panel data set that includes the universe of natural disasters in the United States from 1930 to 2010 and study how these shocks affected migration rates, home prices and poverty rates at the county level. Severe disasters increased out-migration rates by 1.5 percentage points and lowered housing prices/rents by 2.5–5.0 percent, but milder disasters had little effect on economic

outcomes. Chapter 3 exploits the 1962 publication of *Silent Spring*, the first successful environmental science book, to investigate whether public information can influence popular demand for environmental regulation. Protecting the environment is often plagued by collective-action problems, so it is important to understand what motivates politicians to act. Combining historical U.S. congressional roll-call votes and census data, I find that the propensity of politicians to vote in favor of pro-environmental regulation increased by 5 to 33 percentage points after the publication of the book. The response to the informational shock varies with the constituency's level of education, income, and exposure to pollution.

The dissertation of Maria Lucia Yanguas is approved.

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I dedicate this dissertation to my mom, dad, and sister.

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Chapter 1

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

Maria Lucia Yanguas, UCLA[1]

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.

[1] Department of Economics, University of California, Los Angeles. E-mail: myanguas@ucla.edu. Link to most recent version: www.luciayanguas.com/research. This paper is the main chapter of my dissertation and my job market paper for 2018/2019. I am especially grateful to my advisers Adriana Lleras-Muney, Till von Wachter, Leah Boustan, and Michela Giorcelli for their guidance and support. I thank Moshe Buchinsky, Mauricio Mazzocco, Rodrigo Pinto, Ricardo Perez-Truglia, Sarah Reber, Manisha Shah, and Melanie Wasserman for helpful feedback. I thank my colleagues Elior Cohen, Brett McCulley, Bruno Pellegrino, and seminar participants at UCLA, Universidad de la Republica del Uruguay and the Evidence-Based-Economics 2018 conference for valuable comments. This project was supported by the California Center for Population Research at UCLA (CCPR), which receives core support (P2C- HD041022) from the Eunice Kennedy Shriver National Institute of Child Health and Human Development (NICHD).

1.1 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.[2]

Underlying the adoption of these programs is the idea that broadening access to computers among school children will increase their access to learning opportunities and decrease future inequalities.[3] 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.[4] Starting in 2007, Plan Ceibal delivered a personal laptop to each student in primary and middle schools within the public education system and equipped all public schools with wireless internet access. To 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

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

[3] 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.

[4] 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.

Ministry of Education, and the main universities in the country. In particular, provincially representative monthly household survey data (Encuesta Continua de Hogares; henceforth, ECH) allow me to track access to technology in the home as well as educational characteristics, and administrative data on all students enrolled in the 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 critical 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.[5] 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

[5] 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.

treated cohorts, which is consistent with both increased access to entertainment (and a 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.[6] 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.[7] 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.[8]

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

[6] 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.

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

[8] I classify students as enrolling in multiple majors if they submitted more than one application form — one per major — in the same year.

experiments and is particularly relevant for informing policy.

Due to the popularity of these interventions and newly available data, there is now abundant evidence on the short-term effects of computers on learning in primary and secondary school. De Melo et al. (2014) found that, two years after the intervention, Plan Ceibal had not influenced primary school 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.[9]

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

[9] 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.

computers.[10] 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 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 distract students 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-secondary 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.[11]

The rest of this paper proceeds as follows. Section 1.2 describes the program. Section 1.3 describes the data and summary statistics. Section 1.4 outlines the identification approach and technical details of the implementation. Section 1.5 presents the results. Section 1.6 considers intermediate outcomes. Section 1.7 concludes.

1.2 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

[10] 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.

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

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.

1.2.1 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.[12]

Plan Ceibal was implemented in two phases, each lasting three years (see Figure 1.1). 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.[13]

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

[12] 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.

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

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 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.[14]

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 1.2, Panel A).[15] 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 1.2, Panel B).[16] 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 1.3 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

[14] See web Appendix Figure A2.

[15] 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.

[16] 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%.

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.

1.2.2 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.[17]

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."[18]

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.[19]

Pricing for the XO was set to start at US$188 in 2006, with the goal to reach the $100

[17] 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.

[18] 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/.

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

mark in 2008. When the program launched, the typical laptop retailed for well north of $1,000.

1.2.3 Cost and Financing of the Program

As of December 2016, 1,681,830 devices had been dispatched by the program.[20] At $188 per laptop, this would imply a direct cost of about $300 million. However, the overall 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.[21] The ultimate cost of the program added up to approximately $600 per student.[22]

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.

1.3 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.

1.3.1 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

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

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

[22] 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.

dwellings each year.[23] 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, 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 1.2 and 1.3) 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.[24]

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.

[23] 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.

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

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.[25]

1.3.2 Summary Statistics

Table 1.2 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.[26] The average individual in this age 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.[27]

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).[28] Internet use is spread evenly between entertainment, information, and communication (about 30% each), while about 10% is for education or learning activities.[29]

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

[25] 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.

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

[27] 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%).

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

[29] See web Appendix Figure A5.

(55%) tops the list, followed by starting to work (20%), pregnancy (7%), and finding classes difficult (7%).[30] 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.

1.4 Identification Strategy

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

1.4.1 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

[30] See web Appendix Figure A5.

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 eventually exposed or not exposed to the program. Birth cohorts are imperfect indicators of who received a government laptop in a given province because repetition rates are relatively high. However, 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 1.3 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.[31] 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:[32]

$$Y_{isc} = \alpha + \eta_s + \gamma_s Trend_c + \beta(In\text{-}between_{sc}) + \boldsymbol{\theta}(After_{sc}) + \mathbf{X}'_{isc}\Gamma + \epsilon_{isc}\,, \qquad (1.1)$$

where Y_{isc} is the outcome of interest measured around age 19 for every cohort, i indexes

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

[32] 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.

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.[33]

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 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).[34] 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

[33] 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.

[34] 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.

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.[35]

1.4.2 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:

$$\begin{aligned} Y_{iscp} = \quad & \alpha + \gamma_s Trend_c + \phi Public_p + \delta(In\text{-}between_{sc}) + \kappa(After_{sc}) + \\ & \beta(Public_p * In\text{-}between_{sc}) + \boldsymbol{\theta}(Public_p * After_{sc}) + \mathbf{X}'_{iscp}\Gamma + \epsilon_{iscp}\,, \end{aligned} \tag{1.2}$$

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.

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

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).

1.4.3 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 1.5.1.3 and 1.5.2.4, is using the wrong functional form: a non-linear pre-trend could bias my results either way.

1.4.3.1 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.[36]

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.[37] 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 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.[38] 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.

1.4.3.2 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

[36] From the Ministry of Education of Uruguay. Refer to web Appendix Table A3 for more details.

[37] This simplifying assumption is supported by the vital statistics shown in web Appendix Table A2.

[38] **?** report that approximately 30% of sixth graders shared their government laptop with siblings in 2009.

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.[39]

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.[40] 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%).

[39] 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.

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

1.4.3.3 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 1.4 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 1.1 I estimate equation 1.1 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 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.

1.5 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 1.1 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 1.3 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 1.1 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).

1.5.1 Educational Attainment

1.5.1.1 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.[41] Therefore, if the program had any effect in the demand for university education, it would very likely translate into actual enrollment.

[41] 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.

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.[42] For students who drop out, it is possible that not enrolling in the first place would have been optimal.

Table 1.2 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).

1.5.1.2 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 approximately 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 1.4 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 seem to continue in their respective trends without major discontinuities around the treatment threshold.

Table 1.3 shows the main empirical results for this section. Panel A estimates equation 1.1 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

[42] 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.

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 1.1 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 1.4). 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.[43]

1.5.1.3 Robustness Checks

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

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

[43] 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.

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.

1.5.2 Choice of Major and Scholarship Applications

1.5.2.1 Summary Statistics

Table 1.2 (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.[44] 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.[45]

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 2006 to 2016, with two small spikes in 2010 and 2013. A subcategory of these including

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

[45] 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.

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.

1.5.2.2 Empirical Analysis

Table 1.6 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 1.7, 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.[46] 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.

1.5.2.3 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, followed closely by business, agricultural sciences, security services, and industrial ser-

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

vices, 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.

1.5.2.4 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.

1.6 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.[47] 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 1.5). 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 1.5).

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

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

2012, but it is also consistent with access to information, which may have helped increase take-up of abortion services after the law was enacted.

1.7 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.[48] 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.[49] Alternatively, with the

[48] 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.

[49] See Perera and Aboal (2017) for evidence of positive effects of the Ceibal Adaptive Math Learning

same resources (approximately 600 dollars per student), Uruguay could have employed full-time teachers in 100 schools, a mode of schooling that has shown promising results on educational outcomes of students of low socioeconomic status (Cardozo Politi et al., 2017).[50] 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.[51][52] 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.

Platorm (started 2013) on learning.

[50] 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.

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

[52] 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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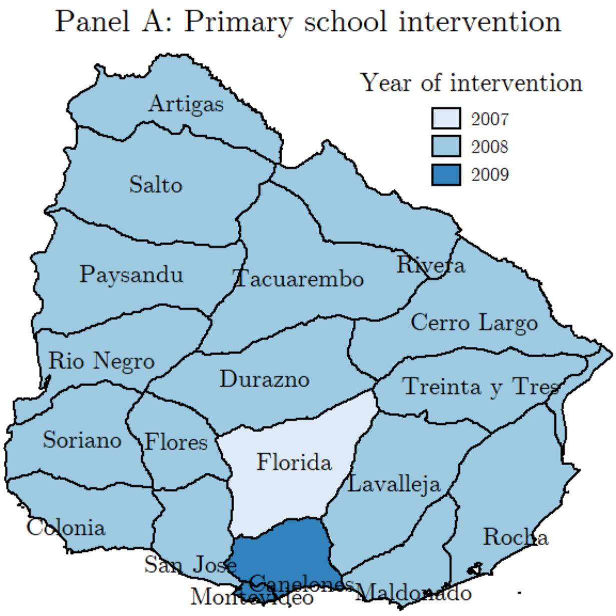
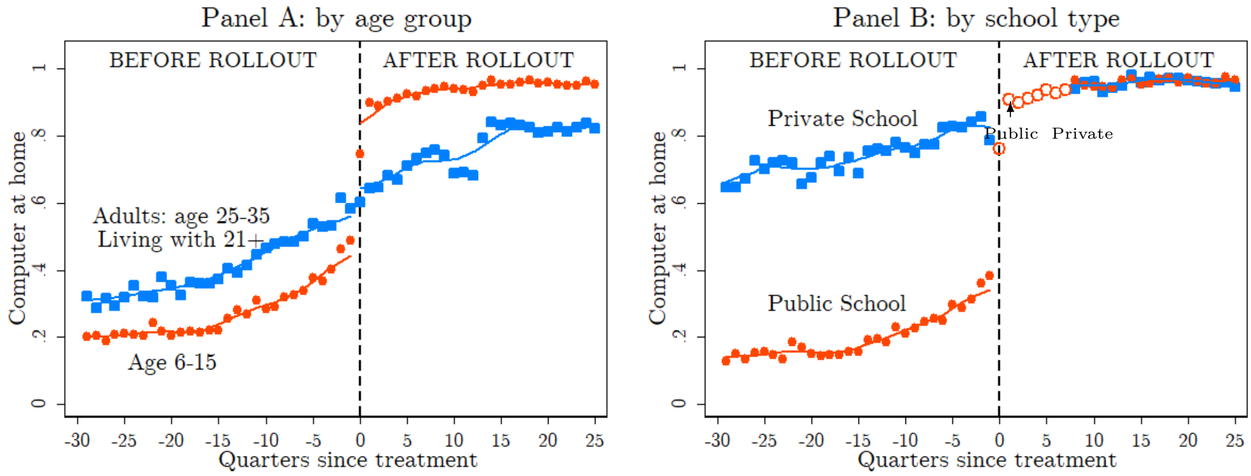
1.8 Figures and Tables

Figure 1.1: Year of Plan Ceibal's initiation in Uruguay by province



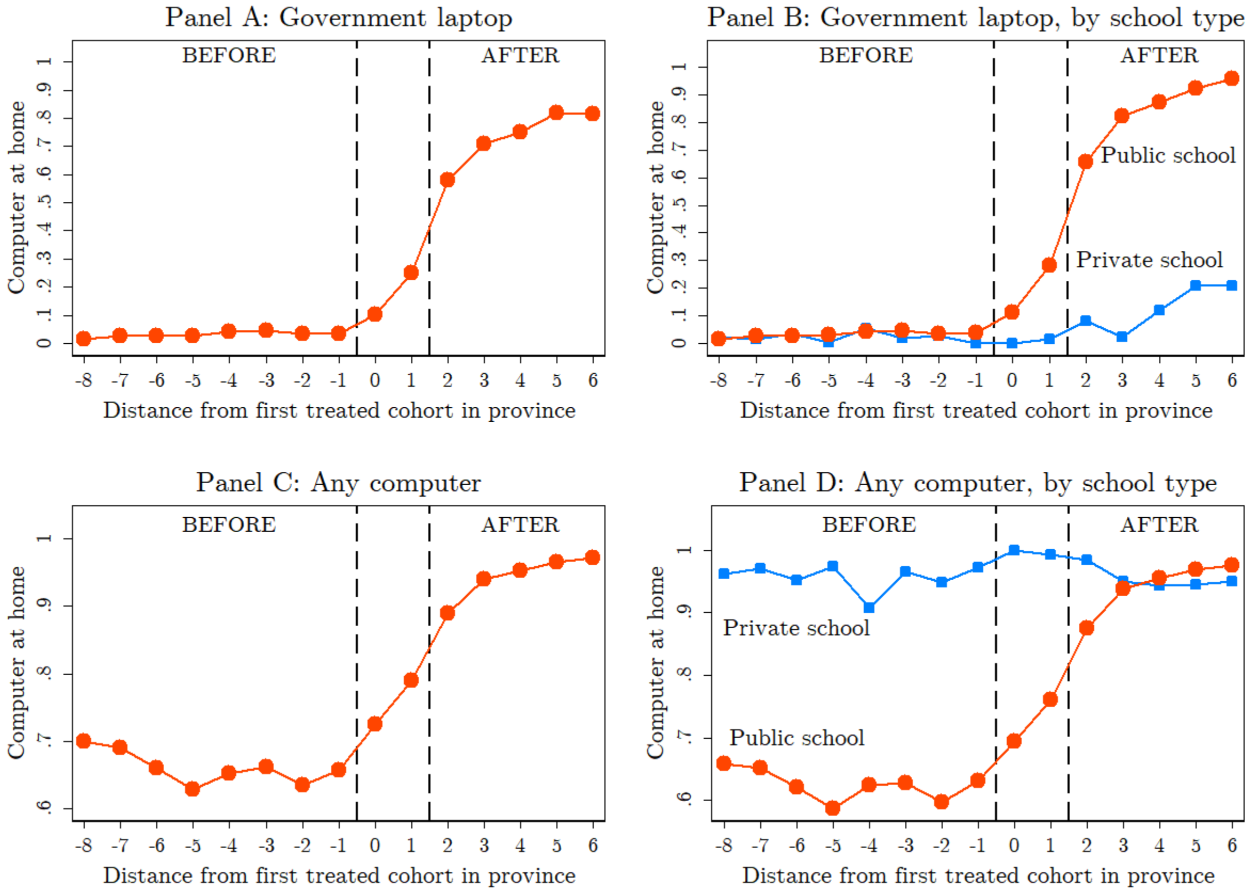
Notes: Panel A summarizes the rollout of Plan Ceibal in Uruguay among primary school students between 2007 and 2009, when full coverage was attained. Panel B summarizes the rollout of Plan Ceibal among middle school students between 2009 and 2011.

Figure 1.2: Quarterly computer access for children aged 6–15
Variations across school type and age groups



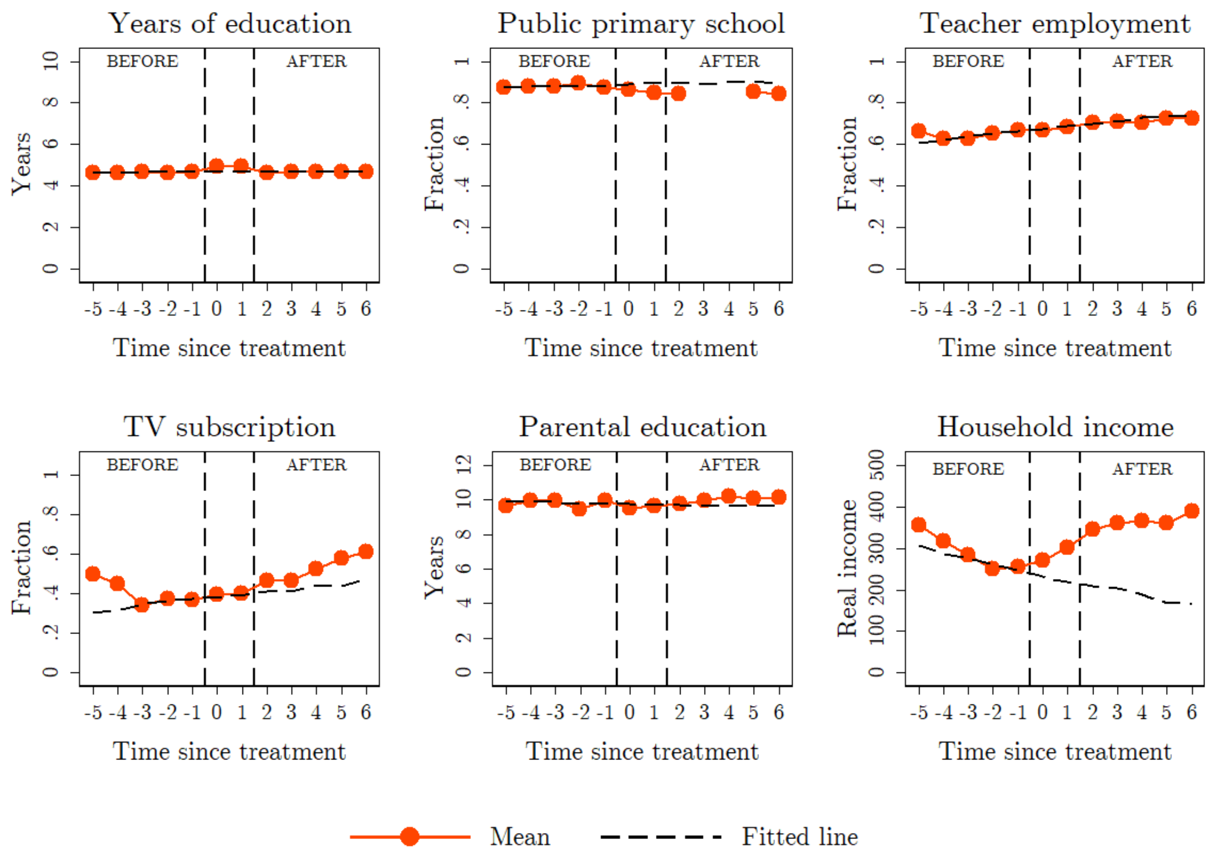
Notes: This figure shows the fraction of individuals with a computer at home for the population aged 6–15 at the quarterly level, stacked according to the timing of the primary school intervention in each province. The empty circles in Panel A correspond to the entire student population, for quarters 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 1.3: Differential access to computers in Uruguay as a result of the intervention
Variation across cohorts, provinces, and school types in 2011



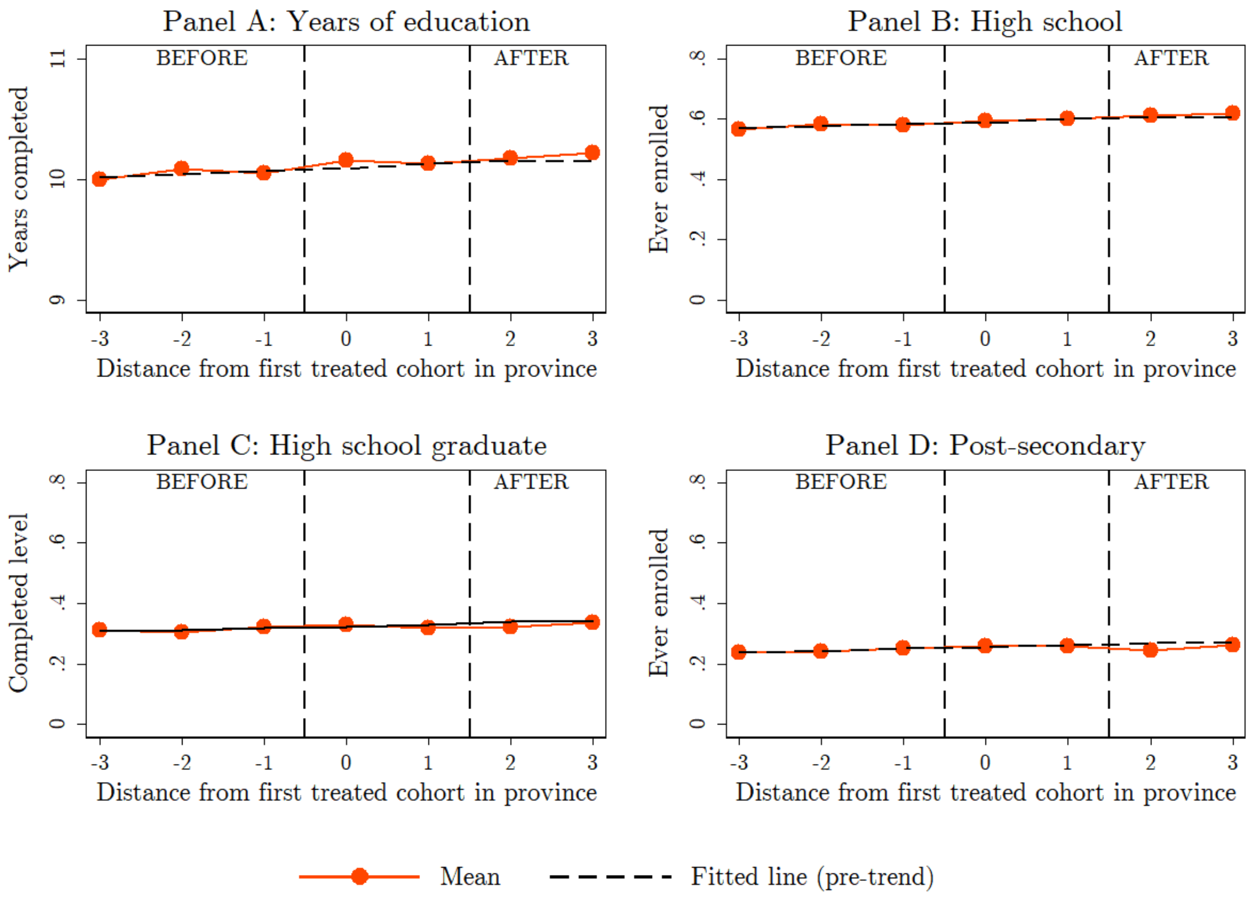
Notes: This figure shows the fraction of individuals with a government laptop at home (Panels A and B) or any computer at home (Panels C and D) in a given cohort, stacked across provinces. A cohort is defined as the group of individuals that is expected to start primary school in the same academic year; 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 1.4: 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 1.5.
Source: ECH 2001–2017.

Figure 1.5: 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 1.1: 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 1.1 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 1.2: 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 1.3: 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 1.1 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 1.4: 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 1.1 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 1.5: 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 1.1 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 1.6: 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 1.1 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 1.7: 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)
A. Complete sample										
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									
B. Doughnut sample										
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 1.1 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.

Chapter 2

The Effect of Natural Disasters on Economic Activity in US Counties: A Century of Data

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More than 100 natural disasters strike the United States every year, causing extensive property destruction and loss of life. We construct an 80 year panel data set that includes the universe of natural disasters in the United States from 1930 to 2010 and study how these shocks affected migration rates, home prices and poverty rates at the county level. Severe disasters increased out-migration rates by 1.5 percentage points and lowered housing prices/rents by 2.5–5.0 percent, but milder disasters had little effect on economic outcomes.

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[1] Link to most recent version: www.luciayanguas.com/research. We acknowledge helpful conversations with Martha Bailey, Hoyt Bleakley, Dora Costa, Richard Hornbeck, Suresh Naidu, Richard Sutch and Randall Walsh, and with workshop participants at UCLA and at the Property and Environmental Research Center. Paul Rhode is grateful for funding from the Michigan Institute for Teaching and Research in Economics (MITRE) and the assistance of Eleanor Wilking.

2.1 Introduction

Natural disasters regularly strike major cities in the United States, leading to numerous fatalities and billions of dollars of property and infrastructure damage each year. Recent examples include Hurricane Sandy, which hit New York City and the surrounding area in 2012, and Hurricane Harvey, which caused extensive flooding in Houston in 2017, each resulting in more than 100 deaths. Climate science suggests that as global greenhouse gas emissions increase, so too will the number and severity of natural disasters (**?**). Furthermore, as more economic activity clusters along America's coasts, a greater share of the population is now at risk of exposure to natural disasters (Changnon et al. 2000, Rappaport and Sachs 2003, Pielke Jr et al. 2008).

This paper analyzes an original dataset for which we compiled the universe of natural disasters in the United States from 1920 to 2010. Figure 1 displays annual counts of disaster events at the county level using this new series. Data is based on reports from the American National Red Cross (ARC) from 1920 to 1964, combined with counts from Federal Emergency Management Agency (FEMA) and its predecessors starting in the 1950s.[2] Though most of the century, the US experienced around 500 county-level disaster events each year. Since the early 1990s, there has been a clear acceleration in disaster counts, reaching around 1,500 county-level events per year by the 2000s.[3]

With this extensive new data in hand, we ask: what happens to local economies that are hit by a natural disaster? We view disasters as negative amenities or negative productivity shocks that, in spatial equilibrium, should encourage existing residents to leave (or prospective residents not to move in), leading to net out-migration and reductions in housing prices. We find that the presence of a severe disaster in a given decade led to heightened out-migration rates and lower housing prices/rents at the county level. Out-migration increases by 1.5 percentage points (8 percent of a standard deviation) and housing prices/rents fall by 2.5 to 5 percent. Our preferred specification considers a disaster to be "severe" if it leads to 25 or more deaths (the median value for disasters with known fatality counts), but results are quite robust to choice of fatality threshold.[4]

[2] By this measure, a disaster that affects multiple counties would be tallied multiple times. For example, the Great Mississippi Flood of 1927 affected 170 counties. Likewise, a county that experiences more than one disaster event in a decade would be counted more than once.

[3] A rise in the frequency of disasters after 1990 is also evident in global series, suggesting that it reflects a real uptick in weather events (see Munich 2012, Gaiha et al. 2015, Kousky 2014). In addition, the federal government may have become more expansive in their declaration of disaster events after Hurricane Andrew, which was especially salient, taking place during the 1992 presidential election campaign (Salkowe and Chakraborty, 2009).

[4] It would also be interesting to follow over time the individuals who were living in a county before a

The migration response to one severe natural disaster is around half as large as the estimated migration effect of a one standard-deviation reduction in local employment growth. Poverty rates also increase in areas hit by severe disasters, which is consistent with out-migration of households above the poverty line, in-migration of the poor (perhaps in response to lower housing prices), or a transition of the existing population into poverty. Our estimates capture the net effect of disasters on local economies, after any rebuilding, new investments, or disbursement of disaster relief funds.[5]

We find that the out-migration response to disasters has been increasing over the century, despite the growing coordination of federal disaster relief through FEMA (founded in 1979), which might root residents in place. One possibility is that the rising frequency of disaster events encourages residents to respond more readily to any given disaster, inferring from one event that future disasters may occur. In addition, rising disaster-related migration is consistent with Deryugina (2017)'s finding that non-disaster-related transfer payments increase substantially following disaster events, mainly in the form of unemployment insurance and medical spending. The presence of a safety net and of relief payments may allow residents of disaster-affected areas to relocate to another county.

On the margin, FEMA disaster declarations and the extent of disaster relief payments are affected by the political process Downton and Pielke Jr 2001; Garrett and Sobel 2003.[6] We provide suggestive evidence that our results are not being driven by biases that would arise if disaster events are declared more often in politically connected states. First, any political connection that would lead states to receive an unwarranted disaster designation should generate other flows of discretionary federal funds, thereby, if anything, leading to net in-migration. Thus, we would expect the political component of disaster declarations to bias against finding that disasters lead to out-migration or falling housing prices. Second, although the official designation of mild weather events as "disasters" may be subject to political manipulation, the largest disasters have all received federal disaster designations.[7] We show that the estimated effect of "severe disasters" is robust to various definitions, ranging from a threshold of 10 to 500 deaths. The association between large disasters and out-migration holds also when instrumenting for disaster activity with

disaster struck but this is not possible with our long-run historical data.

[5] Gregory (2013)and Fu and Gregory (2017) document that rebuilding grants have externality effects on the decision of neighboring households to remain in an area struck by a natural disaster.

[6] These papers show thatstates politically important to the president have a higher rate of disaster declaration, and that disaster expenditures are higher in states having congressional representation on FEMA oversight committees and during election years.

[7] Even Hurricane Maria, the severity of which was downplayed by the Trump administration after hitting Puerto Rico in 2017, did receive a disaster designation by FEMA and so would be included in our definition of a disaster event.

climate variables that are available historically (e.g., maximum and minimum temperatures), and is present regardless of whether the political party of the state's governor matches the party of the President.

Our work contributes to two strands of the literature in urban and environmental economics. First is a series of macroeconomic studies that use cross-country panel regressions to study how changing temperature, rainfall, and increased exposure to natural disasters conditions affects economic growth (Dell et al., 2012, 2014; Hsiang and Jina, 2014; Burke et al., 2015; Kocornik-Mina et al., 2015; Cattaneo and Peri, 2016).[8] These studies have not led to consensus, finding results ranging from long-lasting effects on national income to near-immediate recovery. By analyzing the effect of many natural disasters within a single country (the United States) over many decades, we are able to hold constant many core institutional and geographic features of the economy that may be otherwise correlated with disaster prevalence in a cross-country setting (e.g., democracy, temperate climate).

A second set of papers study the effect of specific major disasters in the US on existing residents (see, for example, Smith and McCarty (1996) and Hallstrom and Smith (2005) on Hurricane Andrew; Hornbeck (2012) and Long and Siu (2018) on the Dustbowl; Hornbeck and Naidu (2014) on the 1927 Mississippi flood; and Vigdor (2008), Sastry and Gregory (2014), Bleemer and Van der Klaauw (2017) and Deryugina et al. (2018) on Hurricane Katrina). Most of these case studies find large effects of a major disaster on out-migration or population loss. While it is important to study these major cases, most disasters are not as severe as these notable outliers. Our comprehensive dataset allows us to examine a much wider universe of disasters.

In a related paper, Strobl (2011) uses a county-level panel of coastal US counties to study how hurricanes affected local economic growth during the years 1970 to 2005. Quite consistently, he finds that hurricanes reduce economic growth in affected counties and encourage richer residents to move away. The advantage of Strobl's analysis is that, by focusing on a specific disaster type (hurricane) in the modern period, he can use detailed data on wind speeds and a scientific model of hurricane intensity to generate a proxy for local damage. The advantage of our paper is that we examine all disaster types – indeed, hurricanes represent less than 10 percent of disaster events – over a much longer historical period, before and after the creation of FEMA. Understanding the potential role of the creation of FEMA on the local economic impact of natural disasters requires

[8] There is some prior work using multiple disasters striking the same country. Feng, Oppenheimer and Schlenker (2012) study the effect of temperature on migration from rural US counties. Anttila-Hughes and Hsiang (2013) analyze more than 2,000 typhoons that struck the Philippines over 60 years.

a long time series.[9] We see our paper as a complement to his.

2.2 Natural Disaster Risk in a Static Spatial Equilibrium Model

Classic models of spatial equilibrium guide our empirical predictions (Mussa and Rosen, 1978; Roback, 1982). In the simplest version of these models, all people have the same preferences over private consumption and local public goods and face zero migration costs. Geographic areas differ by one exogenous attribute – say, winter temperature. From this local variation, a hedonic rental price equilibrium arises mapping out the representative agent's indifference curve between private consumption and temperature. There is no migration in equilibrium because utility is equalized across all locations. Residents of colder places are compensated in the form of lower rents.

Now suppose that a county experiences a natural disaster. A disaster event could disrupt local supply chains and product infrastructure (Carvalho et al., 2016), thereby lowering local labor market demand. In addition, the occurrence of a natural disaster could decrease the local amenity level – for example, by imposing some risk of mortality or property damage. All else equal, such natural disasters should push people to leave and discourage outsiders from moving in Topel (1986). In an economy featuring durable local housing, the housing supply will be inelastic in the medium run and this means that a downward shock to local quality of life will depress local home prices (Glaeser and Gyourko, 2005).[10] Lower home prices encourage some residents to stay in an area and others to move in; the price effect will be strongest for the poor who are more willing to trade off high real income for higher disaster risk. We thus predict that areas that are routinely hit by natural disasters will experience out migration, but such migration will be moderated by declining home prices. Falling prices could thus generate shifts in the income distribution of a local area, resulting in a larger share of residents who are poor.

2.3 Econometric Framework

To study how natural disaster events affect local economies, we stack data from county i in state j for decade t (t = 1930–2010) and estimate:

[9] Our paper follows in the tradition of Romer (1986), who seeks to understand the role of monetary and fiscal policy changes in the post-World War II period on the amplitude of business cycles.

[10] If disasters also destroy enough of the local housing stock, there could be a countervailing supply effect in the housing market, overwhelming the reduction in local housing demand.

$$Y_{ijt} = \mu_i + \xi_t + \beta_1 Disasters_{ijt} + \beta_2 \Delta Employ_{ijt} + X_{ij}t + U_{ijt}$$

Our set of dependent variables Y include the net migration rate, the logarithm of median housing prices (or rents), and the poverty rate (available from 1970), all of which are measured at the decade level from the Censuses of Population and Housing. [11] We control for county (μ_{ii}) and decade (ξ_t) fixed effects, state specific time trends, and an interaction between initial county population and a linear time trend. The vector X_{ij} includes both state fixed effects and initial county population, both of which are interacted with decade time-trend (t). We control for differential trends by initial population to account for the fact that sparsely populated areas (e.g., in the Mountain West) were unlikely to have declared disasters. Our main explanatory variables of interest is a vector of the number and severity of disasters in a local area ($Disasters_{ijt}$), which we will discuss in depth in the next section. In particular, we include an indicator for the presence of any severe disaster in the county and decade, and counts of all other disasters by type (e.g., hurricanes, fires). Standard errors are clustered by state.

Standard economic controls like the unemployment rate are not available at the county level over such a long period of time. Instead, we control for time-varying economic conditions by constructing an estimate of county employment growth from t–10 to t using initial industrial composition at the county level to weight national employment trends ($\Delta Employ_{ijt}$). This measure follows standard proxies for local economic growth pioneered by Bartik (1991) and Blanchard and Katz (1992) and is defined as:

$$\Delta Employ_{ijt} = \frac{\sum_{Ind=1}^{I} [Employ_{i,1930,ind} GR_{ind}]}{Employ_{i,1930}}$$

Equation (2) weights the national growth rate (GR) in employment in industry l for decade t by the share of workers in county i who worked in industry l in the base year (usually: 1930).[12]

[11] Data on poverty rates and house values/rents by county are taken from the National Historical Geographic Information System (NHGIS).

[12] We calculate employment in 143 industries by county using the 1930 IPUMS data and rely on the standardized 1950-based industry codes. Goldsmith-Pinkham et al. (2018) emphasize the identifying assumptions needed to use Bartik-style shift-share variables as instruments. In this case, we are simply using the shift-share measure to create a proxy for employment growth.

2.4 Data

2.4.1 Natural Disasters

We combine data from several sources to create a consistent series of disaster counts at the county level over the twentieth and the early twenty-first centuries. For each disaster, we record the geographic location (county), disaster type, month and year of occurrence, and fatality count.

Our most recent data is drawn from the list of "major disaster declarations" posted by FEMA and its predecessors, which begins in 1964 (fema.gov/disasters). We supplement the FEMA roster with information on disaster declarations published in the Federal Register back to 1958 and with archival records back to the early 1950s.[13] We extend our series back to 1918 using data on the disaster relief efforts of the American National Red Cross (ARC) documented in their Annual Reports and in lists of disaster relief operations.[14] We link these lists with the ARC's case files to document the date, type, and location of each disaster.[15]

Appendix Table 1 reports the number of disaster events in our dataset by type, as well as decade averages of disaster counts at the county level.[16] The most common disaster types in the data are floods and tornadoes, representing around 70 percent of the 10,158 total events. The typical county in our sample had 1.83 declared disasters in a decade, with the most common disasters being storms (0.73 in the typical county-decade), floods (0.49 in the typical county-decade) and hurricanes (0.31 in the typical county-decade).

Information on fatalities are drawn from the EM-DAT dataset or from the ARC records

[13] We use the archival records of the Office of Emergency Preparedness (Record Group 396) and of the Office of Civil and Defense Mobilization, the Office of Defense and Civil Mobilization, and the Federal Civil Defense Administration (Record Group 397) held at National Archives II at College Park, Maryland. The "State Disaster Files" in RG 396, Boxes 1–4 were especially useful.

[14] We use various versions of the ARC's "List of Disaster Relief Operations by Appropriation Number," held in Record Group 200 at National Archives II in College Park, MD (Records of the American National Red Cross, 1947–1960, Boxes 1635-37).

[15] The case files are located in RG200 Records of the American National Red Cross, 1917-34, Box 690-820; 1935-46, Boxes 1230-1309; 1947-60, Boxes 1670-1750.

[16] All disasters that may be influenced by economic activity, such as mine collapses, explosions, transportation accidents, arsons and droughts are excluded from the analysis. There is a debate about the extent to which droughts are caused by environmental conditions versus decisions about water use. We report results that include droughts in Appendix Table 7 and they are unchanged.

and are only available for disasters resulting in 10 or more deaths.[17][18] We create measures of disaster severity using fatality counts above various thresholds. Our preferred measure of a "severe" disaster is one with 25 or more deaths, the median count for disasters with known fatality numbers. Appendix Figure 1presents a histogram of disasters by fatality count. There are 292 disasters with 25 or more deaths in our dataset which constitute 2.9 percent of all events. These disasters tend to be geographically extensive, so that around 30 percent of counties experience a severe disaster in a given decade.

For a given disaster event, the number of fatalities is determined in part by the level of economic development in the location and the period (Kahn, 2005; Lim, 2016). For this reason, we avoid using actual fatality counts to measure the intensity of disaster severity in favor of a simple fatality threshold. Results are nearly identical if we instead define disaster severity as any disaster with fatalities above the 50th or above the 90th percentile of the decade average to allow for endogenous declines in fatalities over time (see Web Appendix, Table 5).

Figures 2.2 and 2.3 present maps of the spatial distribution of disaster prevalence. The first map reports the cumulative count of disasters of any type during the century, and the second map reports the number of decades in which the county experienced a severe disaster. Disasters are prevalent throughout Florida and on the Gulf of Mexico, an area typically wracked by hurricanes; in New England and along the Atlantic Seaboard, locations battered by winter storms; in the Midwest, a tornado-prone region; and along the Mississippi River, an area subject to recurrent flooding. There are comparatively few disasters in the West, with the exception of California, which is affected primarily by fires and earthquakes. Severe disasters follow similar geographic patterns but are more concentrated on the Atlantic Coast, in the Gulf of Mexico, and in large river valleys.

2.4.2 Migration

We obtain age-specific net migration estimates by decade for US counties from 1950 to 2010 from Winkler et al. (2013)(a,b). Gardner and Cohen (1992) provide similar estimates for 1930 to 1950. These data include estimates of net migration for each decade from US counties by five-year age group, sex, and race. The underlying migration numbers

[17] EM-DAT was created by the Centre for Research on the Epidemiology of Disasters (see http://www.emdat.be/).

[18] Our measure of fatalities includes the number of people who lost their lives because the event happened (dead) and the number of people whose whereabouts since the disaster are unknown, and presumed dead based on official figures (missing). In the majority of cases, a disaster will only be entered into EM-DAT if at least two independent sources confirm the fatality count. Note that the final fatality figures in EM-DAT may be updated even long after the disaster has occurred.

are estimated by comparing the population in each age-sex-race cohort at the beginning and end of a Census period (say, 1990–2000) and attributing the difference in population count to net migration, after adjusting for mortality. Any net inflow of immigration from abroad would be captured in this measure as an increase in the county's rate of net in-migration. This method has become standard practice to estimate internal migration in the United States, as originated by Kuznets and Thomas (1957). We divide estimated net migration to or from the county from time t to t+10 by population at time t to calculate a migration rate. Summary statistics of our outcome variables at the county-by-decade level are reported in Appendix Table 2.

2.5 Results

2.5.1 Disasters and Out migration

We find that severe natural disasters are associated with net out-migration from a county. Table 2.1 reports our main specification, which defines "severe disaster" as an event resulting in 25 or more deaths. The first column considers a county's net migration rate as an outcome. By this measure, experiencing a severe disaster leads to a 1.5 percentage point increase in net out-migration (8 percent of a standard deviation). Severe disasters are around half as disruptive to local population as a large negative employment shock. A one standard deviation decline in local employment growth increases out-migration by 3 percentage points.

Some categories of milder disasters have additional effect on net migration to a county. Floods attract in-migrants to an area, while wildfires and hurricanes lead to net out-migration (although the coefficient on hurricanes is not significantly different from zero in the main specification). Storms and tornadoes have no effect on migration flows. The positive effect of floods on in-migration is consistent with our earlier work, in which we found that migrants moved toward flooded counties before 1940 Boustan et al. (2012). We speculated that areas prone to flooding received new infrastructure in this period, which may have encouraged new use of previously marginal land. Below, we show that the positive effect of floods on migration in this series is present only in the first part of the century.

Our main specification is unweighted, allowing each county to contribute equally to the analysis. In this way, we treat each county as a separate economy that may be subject to a location-specific shock in a given period, corresponding to the cross-country regressions common to the climate economics literature. Appendix Table 3 aggregates counties into State Economic Areas and Appendix Table 4 instead weights the county-level results by county population in 1930. This specification puts more weight on disasters that take place in heavily populated urban areas. In both cases, the effect of a severe disaster on net migration is similar, but the coefficient is no longer statistically significant after weighting by county population. We prefer the unweighted results because weighted regressions put extra emphasis on large metropolitan areas. Appendix Table 5 uses a relative measure of disaster severity, defining severe disasters as any in the top 50 percent (or top 90 percent) of fatalities in a given decade. Results are nearly identical to the preferred specification.

We note that our estimates are net effects of disaster on migration activity after all private and government responses to the disaster event take place (e.g., infrastructure investment, transfer payments). A disaster at the start of a given decade may trigger infrastructure investments in flood control or early warning systems that mitigate future risk. New investments may attract people to an area both because of declines in natural disaster risk and because of short run jobs stimulus. Our results are unchanged by controlling for new dam construction in the decade, the largest of such infrastructure projects (see Appendix Table 6).[19]

Thus far, we treat disaster declarations as a reflection of true meteorological activity. However, there is a political process governing whether the government declares an official disaster or state of emergency after a given weather event. Ideally, we would have detailed climatological data to measure the intensity of wind speeds (for hurricanes), seismic activity (for earthquakes), and so on. However, it is not possible to gather such data for five major disaster types over a century. Instead, we present suggestive evidence that the coefficients are not driven by political factors.

First, we argue that any political connection that would lead states to receive an unwarranted disaster designation should generate other sources of discretionary federal funds, thereby, if anything, leading to net in-migration. Thus, we would expect the political component of disaster declarations to bias against finding that disasters lead to out-migration or falling housing prices.

Second, although the official designation of a mild weather event may be subject to political manipulation, it is hard to believe that the largest disasters (e.g., Hurricane Katrina) could be left without a federal declaration. It is not clear a priori how large an event would need to be before the disaster declaration was effectively depoliticized. Table 2.2 reports the coefficient on "severe disaster" for various fatality thresholds, starting with a threshold of only 10 fatalities, and increasing to an extreme threshold of 500 fatalities. We find a very consistent effect of facing a severe disaster on net out-migration

[19] Duflo and Pande (2007) study the productivity and distributional effects of large irrigation dams in India. They find that rural poverty declines in downstream districts but increases in the district where the dam is built.

(coefficients range from –0.011 to –0.017) for all definitions ranging from 20 deaths to 100 deaths. For larger thresholds, standard errors increase and the estimates are no longer statistically significant.

Third, we split the sample into disasters occurring in a state-year in which the state governor was of the same party as the President, and state-years in which he/she was not. If disaster declarations are driven by political considerations, we would expect that state-years with a same party governor would get more disaster declarations and the actual weather events underlying those declarations should be weaker, and thus should be less associated with out-migration. We find no relationship between having a same-party governor and the strength of the out-migration response to a severe disaster. Results are presented in Appendix Table 8.

Finally, we instrument for the presence of a severe disaster with the limited set of climatic variables that are available for the whole century. Our instruments are average maximum daily temperature, minimum daily temperatures and total precipitation by county and decade. Although the instruments do not rise to conventional levels of statistical power (F-statistics are around 5), we continue to find an association between the presence of a severe disaster and net out-migration from a county. Temperature and precipitation may have direct effects on migration decisions, beyond any effect on disaster prevalence, and so we caution that the instruments may not meet the necessary exclusion restriction. We include IV results for completeness in Appendix Table 9.

2.5.2 Disasters, Home Prices, and Poverty Rates

If natural disasters encourage net out-migration from a county, lowering demand for the area, we would expect an associated effect of disaster activity on housing prices and rents. The second and third column of Table 2.1 documents that the occurrence of a severe disaster lowers housing prices by 5.2 percent and rents by 2.5 percent. The implied elasticity of housing prices with respect to population – a 2.5 percent decline in rents for out-migration representing 1.7 percent of the population – is similar to standard estimates in the literature (e.g., Saiz (2007), which looks at the effect of foreign in-migration on rents). Appendix Table 10 demonstrates that the estimated effect of severe disasters on housing prices is robust to thresholds between 20 and 100 deaths (ranging from 3.8–5.3 percent); the estimated effect on rents is more sensitive but generally ranges between –1.0 and –2.6 percent.

Residents who leave an area after a natural disaster may represent a select subsample of the incumbent population. We suspect that rich households would have greater resources to leave an area struck by disaster. In the climate change adaptation literature, there is a broad consensus that the wealthy can access a wide range of strategies ranging from owning a second home, to accessing better quality food, medical care and housing to protecting themselves from shocks; the poor are thus more likely to bear the incidence of natural disasters (Dasgupta et al., 2001; Barreca et al., 2016; Smith et al., 2006). If the rich are more likely to leave an area after a disaster, net out-migration may serve to increase the local poverty rate. The fourth column of Table 2.1 shows that the occurrence of a severe disaster increases the local poverty rate by 0.8 percentage points (10 percent of a standard deviation).

2.5.3 Changing Responses to Disaster Events over Time

We may expect the effect of natural disasters on local economies to moderate as the federal response to disaster activity expanded over the century. From the 1920s through the 1950s, Washington responded to natural disasters on a case-by-case basis. In the 1950s, the federal government assumed a more systematic disaster relief role through a succession of Civil Defense agencies. The Federal Disaster Assistance Administration (FDAA) was created in 1973 under the Department of Housing and Urban Development and became an independent agency, the Federal Emergency Management Agency in 1978. We date the advent of a coordinated federal response to disaster activity (the creation of FDAA and FEMA) in the 1970s.

Table 2.3 tests for differences in the migration response to disaster events that occur before and after such coordinated federal response. If these agencies increased the reliability of federal disaster relief, we might expect net out-migration to decline as the receipt of federal funds makes an area more attractive. However, we note that the intensity of disaster activity increased over time, especially after 1990, in part due to environmental change (Figure 2.1), and so it is hard to disentangle the effects of policy change from the environment. We find no difference in the migration response to severe disasters before and after 1980. However, out-migration in response to mild disasters increases for nearly every disaster category 16 after 1980, including floods, hurricanes, and wildfires. Although floods attracted in-migrants before 1980, they had no effect on migration in the latter part of the century. Any drag on migration from the establishment of FEMA was swamped by changes in the nature of disasters, which may be becoming more damaging or more salient to households over time.[20]

[20] Any reduction in general migration costs would be absorbed into decade fixed effects. Yet national trends suggest that, if anything, internal migration has been falling over time, especially in the 1990s, and so we are unlikely to just be picking up greater responsiveness to any decline in local amenities Molloy et al. (2011).

Our finding that the establishment of FEMA did not anchor residents in place is consistent with Deryugina (2017), which documents that counties struck by hurricanes in the 1980s and 1990s received around $1,000 (2008 dollars) of additional federal transfers per capita in the decade after a hurricane event. Two-thirds of these funds were dispersed through unemployment insurance and income maintenance programs that are not tied to the recipient's location. The population in disaster-struck areas can move elsewhere and continue to receive income support.

2.6 Conclusion

During the past century, the United States has experienced more than 10,000 natural disasters. Some have been major newsworthy events, while others have been comparatively mild. We compile a near-century long series on natural disasters in US counties, distinguishing severe events by death toll, and find that these shocks affect the underlying spatial distribution of economic activity.We find that counties hit by severe disasters experienced greater out-migration, lower home prices and higher poverty rates. Given the durability of the housing capital stock, lower demand due to persistent natural disasters leads to falling rents and acts as a poverty magnet. We find little effect of milder disasters on population movements or housing prices.

Contrary to recent cross-country studies like Carvalho et al. (2016) and Kocornik-Mina et al. (2015) which find near-immediate recovery from large natural disasters, we find long-term (decadal) effects of severe disasters events on economic activity at the county level in the US. Yet, our estimates are much smaller than those arising from case studies of the nation's most extreme events, including Hurricane Katrina and the 1927 Great Mississippi Flood, both of which led to 12 percentage point increases in out-migration (Deryugina et al., 2018; Hornbeck and Naidu, 2014). Instead, we find that the typical severe disaster in the US was associated with a 1.5 percentage point increase in net out-migration from a county, and corresponding declines in housing prices/rents. Our estimates provide the net effect of disaster events on economic activity after all private and government responses take place, which can include transfer payments and new infrastructure investments. This comprehensive analysis, which is based on the universe of disaster activity in the US over nearly a century, provides a valuable benchmark against which future case studies of extreme disaster events can be compared.

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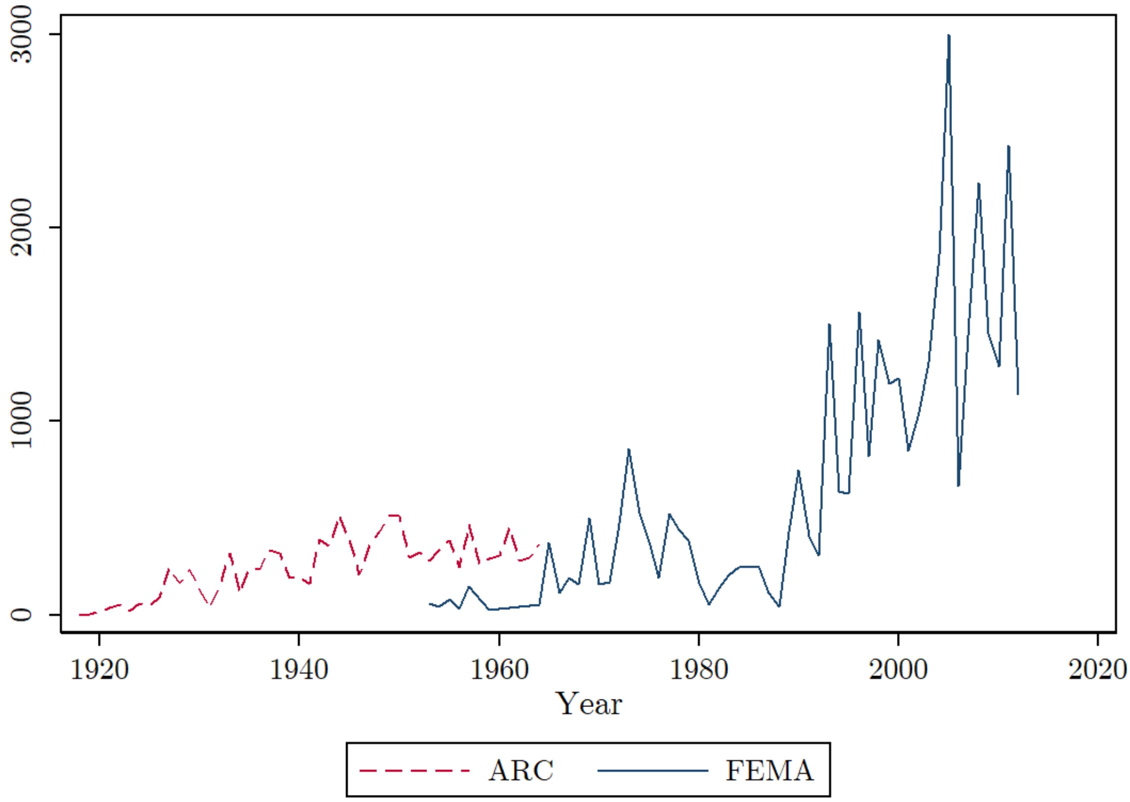
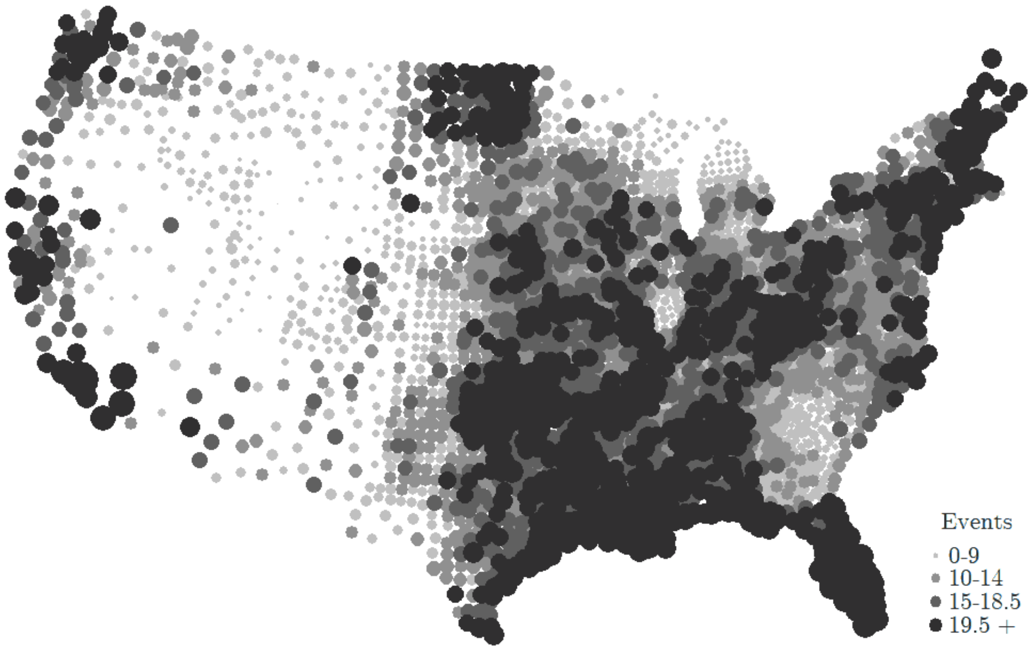
2.7 Figures and Tables

Figure 2.1: Annual disaster count in the US 1918–2012, by data source



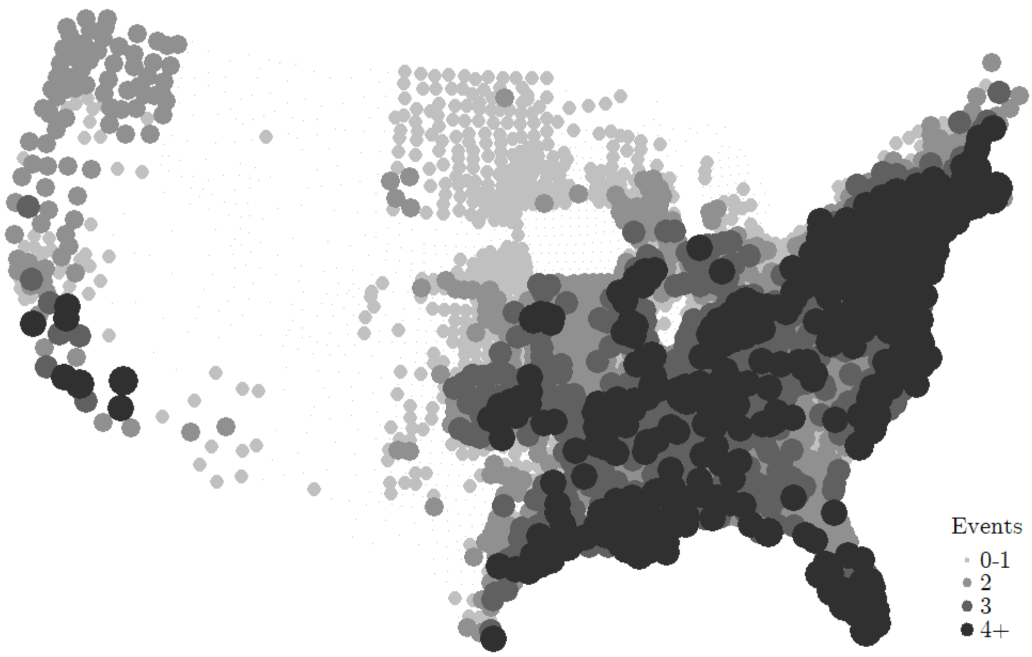
Notes: This figure plots the sum of county-level disaster counts by year and source between 1918 and 2012. Note that this measure will treat a given natural event that occurred in two separate counties as two different disaster events. The disaster count is truncated at 3,000. Sources: American National Red Cross (ARC) and various federal sources, including Federal Emergency Management Agency (FEMA). See text for details.

Figure 2.2: Disaster count by US county, 1930–2010



Notes: This map plots disaster counts within each county for the whole period 1930–2010. The marker size is increasing in number of events, while color represents quartiles of disaster counts. The maximum number of occurrences is 87. Sources: American National Red Cross and various federal sources, including Federal Emergency Management Agency.

Figure 2.3: Count of decades with a severe disaster event by US County, 1930–2010



Notes: This map shows the number of decades with severe events per county in the period 1930–2010. Severe events are disasters associated to 25 or more deaths. The marker size is strictly increasing in number of events, while color represents quartiles. The maximum number of occurrences is 7. Sources: American National Red Cross and various federal sources, including Federal Emergency Management Agency.

Table 2.1: Effect of disasters on county-level net migration by disaster type and severity

	(1) Migration rate	(2) House value (log median)	(3) House rent (log median)	(4) Poverty Rate
Severe disaster	-0.015***	-0.052***	-0.025**	0.008***
	(0.005)	(0.016)	(0.011)	(0.003)
Flood count	0.006**	0.007	0.007*	-0.002
	(0.003)	(0.006)	(0.004)	(0.001)
Storm count	-0.001	0.000	0.002	-0.000
	(0.002)	(0.005)	(0.003)	(0.001)
Tornado count	-0.002	0.011	0.015	-0.005**
	(0.004)	(0.011)	(0.010)	(0.002)
Hurricane count	-0.008	-0.005	-0.010	0.001
	(0.008)	(0.007)	(0.007)	(0.002)
Fire count	-0.013*	0.002	0.001	-0.004**
	(0.006)	(0.008)	(0.008)	(0.002)
Other disasters count	-0.029	-0.004	-0.022	0.005
	(0.030)	(0.030)	(0.021)	(0.008)
Employment growth rate	0.267***	0.264	0.234	-0.139***
	(0.028)	(0.174)	(0.140)	(0.028)
County FE	Y	Y	Y	Y
Decade FE	Y	Y	Y	Y
State FE * time trend	Y	Y	Y	Y
1930s population * time trend	Y	Y	Y	Y
Decades included	1930–2010	1970–2010	1970–2010	1970–2010
Observations	24,408	15,154	15,152	15,162

Notes: The reported regression of equation (1) is at the county-by-decade level. Net migration rates are from Winkler et al. (2013)(a,b) and Gardner and Cohen (1992). Housing values, rents and poverty rates are from NHGIS. Counts of natural disasters by type and severity are assembled from the ARC, FEMA and EM-DAT data. In this specification, a disaster qualifies as "severe" if it was associated with 25 or more deaths. We estimate the employment growth rate from IPUMS data using industrial composition and national employment trends (see equation 2); weights are based on county employment by industry in 1930 (column 1) and in 1970 (column 2). Standard errors are clustered by state.

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

Table 2.2: Effect of severe disasters on migration for different severity thresholds

	Severe Disasters	
Fatality Threshold	Coefficient	Standard Error
10	-0.008*	(0.004)
20	-0.015***	(0.005)
30	-0.012**	(0.005)
40	-0.015**	(0.006)
50	-0.012**	(0.006)
60	-0.012*	(0.007)
70	-0.014*	(0.007)
80	-0.014*	(0.007)
90	-0.016*	(0.009)
100	-0.017**	(0.008)
200	-0.013	(0.009)
500	-0.051**	(0.021)

Notes: Each row corresponds to a separate regression that follows the format of Table 2.1. We report coefficients on the indicator for "severe" disasters, varying the threshold required for a disaster to qualify as severe. Disasters qualify as severe if they were associated with more than the number of fatalities reported in column (1). All regressions include as controls counts of natural disasters by type, county and decade fixed effects, state-specific time trends and a 1930 population time trend. Standard errors are clustered by state.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3: Effect of disasters on net migration rates before and after 1980

	Migration rate	
	Coefficient	Standard Error
Severe disaster	-0.017**	(0.008)
Severe disaster, after 1980	0.003	(0.013)
Flood count	0.008**	(0.003)
Flood count, after 1980	-0.008*	(0.004)
Storm count	-0.006	(0.006)
Storm count, after 1980	0.005	(0.007)
Tornado count	-0.001	(0.005)
Tornado count, after 1980	-0.006	(0.010)
Hurricane count	0.006	(0.009)
Hurricane count, after 1980	-0.018***	(0.005)
Fire count	0.018	(0.013)
Fire count, after 1980	-0.031**	(0.015)
Other disasters count	0.005	(0.046)
Other count, after 1980	-0.047	(0.065)
Exp. employment growth rate, 1930 weights	0.266***	(0.028)
County FE	Y	
Decade FE	Y	
State FE * time trend	Y	
1930s population * time trend	Y	
Observations	24,408	

Notes: The reported regression is at the county-by-decade level (1930–2010). Net migration rates are from Winkler et al. (2013)(a,b) and Gardner and Cohen (1992). Counts of natural disasters by type and severity are collected from the ARC, FEMA and EM-DAT datasets. In this specification, a disaster qualifies as "severe" if it was associated with 25 or more deaths. We estimate the employment growth rate from IPUMS data using industrial composition and national employment trends (see equation 2); weights are based on county employment in 1930 by industry. Standard errors are clustered by state. We interact each disaster variable with an indicator for decade equal to or after 1980 (after the creation of FEMA). Standard errors are clustered by state.

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

Chapter 3

From Awareness to Action: Informational Shocks and Demand for Environmental Regulation

Maria Lucia Yanguas, UCLA[1]

Protecting the environment is often plagued by collective-action problems, so it is important to understand what motivates politicians to act. This paper exploits the 1962 publication of *Silent Spring*, the first influential environmental science book, to investigate whether public information can influence popular demand for environmental regulation. Combining historical U.S. congressional roll-call votes and census data, I find that the propensity of politicians to vote in favor of pro-environmental regulation increased by 5 to 33 percentage points after the publication of the book. The response to the informational shock varies with the constituency's level of education, income, and exposure to pollution.

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3.1 Introduction

How to control pollution and combat climate change in the face of increasing global demand for energy are among the most pressing policy challenges facing the world today.[2] The rapid growth of the world's population and the economic catch-up of developing countries have been closely accompanied by a steady increase in all forms of pollution, which threatens the health and well-being of current and future generations. Despite the importance of pollution, the world is failing to control it effectively, with the most powerful countries among the top CO_2 polluters.[3] Since protecting the environment is often plagued by collective-action problems, understanding what motivates politicians to act is crucial to create effective pro-environmental regulation. In this paper, I argue that public information increases awareness, investigate whether awareness can influence public demand for environmental regulation, and explore relevant channels.

Addressing the relationship between information and demand for a clean environment can be challenging because the stock of information held by any given individual is often endogenous, likely influenced by personal characteristics such as level of education, income, and exposure to pollution. Isolating potential mechanisms requires exogenous variation, which is especially hard to find in this context. To overcome this problem, I exploit a particular historical event in which an informational shock reached a wide range of the U.S. population, offering novel information in a reasonably exogenous, rapid, and homogeneous manner. Serialized in *The New Yorker* during the summer of 1962 and published that September, *Silent Spring* was the first environmental science book to succeed in disseminating environmental concerns among the American public. It did so by documenting the detrimental effects —particularly on birds— of the indiscriminate use of pesticides. It denounced that powerful synthetic insecticides such as DDT were poisoning the food chains, that they had been shown to cause cancer, and threatened wildlife. Though the book focused on pesticides, the main takeaway was broader: the importance and fragility of our environment, and the detrimental consequences of pollution. The book was highly publicized by the media and was credited with increasing awareness and public support for environmental protection (Parks, 2017).[4]

In this paper, I examine the effect of a sudden increase in the stock of environmental

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[2] See OECD green growth studies, Jänicke (2012), or Acemoglu et al. (2012) for additional references.

[3] China, United States, India, Russia, and Japan are the top carbon dioxide polluters according to the European Commission and Netherlands Environmental Assessment Agency (2015); they are also at the top of the National Power Index according to the United Nations (2007).

[4] It was later credited with the creation of the Environmental Protection Agency: `https://archive.epa.gov/epa/aboutepa/birth-epa.html`

information, generated by the publishing of *Silent Spring*, on actual demand for environmental regulation. To this end, I combine historical records of U.S. congressional roll-call votes held in the 84th through 91st Congresses (January 3, 1955 to January , 1971), census data, and cross-sectional estimates of pollution levels. I define demand for environmental regulation in terms of the total number of pro-environmental votes in Congress, i.e., positive votes for environmental bills.[5]

To identify the overall effect of the book on demand for environmental regulation, I use an event-study identification strategy that compares the fraction of pro-environmental votes before an after *Silent Spring* was published, controlling for an underlying linear trend. Identification comes from detecting discontinuities in the time-trend around the publication of the book. The most important assumption is that the trend up to the publication date is a good counterfactual for the outcomes of interest. I find that the propensity of politicians to vote in favor of environmental regulation by 5 to 33 percentage points after the book came out.

I then examine how information interacts with education, income, and exposure to pollution. To do this, I compare the characteristics of the population who lived in congressional districts in which environmental politicians were elected —defined as those who would later vote in favor of environmental regulation— with those of populations living in districts where "non-environmental" politicians were elected, before and after 1962, and including interactions of the informational shock with education, income, and pollution exposure. I find that the effect of the book on demand for environmental regulation was higher among less educated people. This finding is consistent with the idea that public information and education are substitutes in the production function of awareness, which in turn raises demand for environmental regulation. I complement this before-after analysis with an intensity-of-treatment approach that uses access to mass media (television) as a proxy for the intensity of the treatment.

The interactions between information, education, awareness, income, and pollution exposure, and their combined effect on overall demand for environmental regulation, are not, *ex ante*, clear. In Figure 3.1, I propose a framework for thinking about this. I hypothesize that information and formal education are inputs in the production function of awareness (knowing about the need for environmental regulation). Awareness, in turn, interacts with income and exposure to generate demand for a clean environment. That demand might be observed when it translates into various actions. Some actions are directed toward protecting against the harms of pollution. This may be achieved by, for example, moving out of a polluted neighborhood, buying water and air filters, or

[5] I did not find any explicitly anti-environmental bills in the period.

purchasing health insurance. Other actions might be directed toward reducing pollution; such as driving green cars, using public transportation, switching to solar energy, or participating in elections and voting for politicians who push for environmental regulation. In this paper, I focus on this last action, which I call demand for environmental regulation. Income enters the framework in two ways: first, it influences education attainment, and later, it influences demand and final action through its role in the budget constraint.

The magnitudes and directions of the interactions of education, income, exposure to pollution, and information in the production of demand for environmental regulation, are also unclear. Education affects one's ability to obtain, process, and act upon scientific knowledge. In the scenario that pollution poses a direct threat to human health, as long as individuals recognize this, advances in environmental knowledge (and access to information) will lead to greater environmental interest among the most educated people first, followed by eventual improvements among less educated people as the knowledge diffuses. This suggests a complementarity between education and information. It can be argued that this effect would be even stronger in the areas that are most polluted, or where local or regional environmental issues pose serious threats to human health, at the time of the election. Moreover, this theory may also apply when the worst consequences of environmental mistreatment lay in the future —posing a stronger threat in old age or even to future generations— to the extent that the highly educated are better at handling intertemporal trade-offs.

But perhaps well educated people are neutral or even less likely than lesser educated people to support collective action to protect the environment in response to an informational shock. In the extreme case in which information and education are perfect substitutes, the shock would have no impact at all on awareness, and therefore the cycle would stop. On the other hand, even if awareness increased, highly educated people tend to also be richer, and therefore better able to protect themselves from (short-run) effects of pollution. Some of these affluent people might also own, benefit from, or sympathize with the polluting firms and industries, so health concerns are not the only variable at play. The effect of income is not clear-cut either, since some low-income individuals might work for polluting industries. The same happens with exposure to pollution. The most exposed, and the poor, may face a trade-off between health and income. In this case, those individuals who have the most to gain from environmental regulation (because they are the most exposed to pollution) may also have the most to lose.

My empirical approach is likely to understate the demand for environmental regulation. Whether the channel is concerned constituents electing pro-environmental members to Congress, or the latter trying to better represent their constituency, the link between public environmental concern or willingness to act on that concern, and what their representatives actually do when elected to office, can sometimes be blurry. Nonetheless, to the extent that politicians represent their constituencies when voting, this setting can serve to measure demand for environmental regulation. I am not the first to measure demand in this way (see Kahn, 2002).

The rest of this paper is organized as follows. Section 3.2 describes the book and the regulatory context of the time, Section 3.3 summarizes the literature, Section 3.4 describes the data, Section 3.5 outlines the empirical approach, Section 3.6 presents the results, Section 3.7 discusses robustness checks, and Section 3.8 concludes.

3.2 The Book and Regulatory Context

Silent Spring is an environmental science book written by Rachel Carson and published on September 27, 1962. It documented the adverse environmental effects caused by the indiscriminate use of pesticides and advocated for environmental protection. This book was highly influential at the time and is widely credited with triggering popular ecological awareness in the United States.

> Few books have been credited with inducing as much social and political change as Rachel Carson's *Silent Spring*. Published in September 1962, *Silent Spring* synthesized scientific research on the toxicity of chemical pesticides, most notably DDT, as the foundation for a polemic against unreflexive science and for a holistic, ecological approach to environmental management. Half a century later, the book is still widely cited in both academic journals and popular media, less for its contents than for its generally accepted role as a catalyst for environmental awareness, activism, legislation, and regulation that changed the character of people's relationship with government and nature. (Meyer and Rohlinger, 2012)

Indeed, the book's influence on public opinion was credited with spurring environmental regulation, from the Clean Air Act (1963, 1970) and Clean Water Act (1972) to the establishment of Earth Day (1971) to President Nixon's founding of the Environmental Protection Agency (EPA, 1970), a federal agency whose mission is protecting human health and the environment (Kline, 2011). In 1972, the U.S. banned the production of DDT and its agricultural use, and a worldwide ban on its agricultural use was formalized under the Stockholm Convention (see Figure 3.2 for a timeline of environmental regulation in relation to *Silent Spring*).

How did this happen? For starters, *Silent Spring* was widely publicized by the media. By the time *Silent Spring* was published, advance sales had already reached 40,000 copies, and the *The New Yorker* was serializing chapters of the book, resulting in more than 50 newspaper editorials and numerous news accounts and other stories. The book quickly made it onto the *New York Times*'s bestseller list and was then selected by the Book-of-the-Month Club, both of which extended its geographic reach. As it arrived in bookstores that fall, more news stories and book reviews appeared. Publicity included a note in the *Los Angeles Times* and a positive editorial in *The New York Times*, and excerpts of the book were published in the National Audubon Society's magazine, as well as various magazines and newspapers, such as *Chicago Daily News*. On April 3, 1963, CBS aired a one-hour telecast on Rachel Carson and her book in its documentary series, CBS Reports, further spreading the book's main message through television. The web Appendix contains pictures of articles and popular cartoons featuring *Silent Spring*—evidence that the book had became part of the popular culture and that a considerable portion of the population knew of the book and its message.

*Silent Spring*was not the first book to discuss these topics. Books on organic farming, nature, and wildlife conservation had been published before, though without nearly as much fanfare. In fact, only six months before Carson, Murray Bookchin published a book denouncing the harms of pesticides (*Our Synthetic World*), but it did not become popular, perhaps because it lacked the passionate sense of urgency embedded in Carson's writing, perhaps because it lacked a publication strategy.[6] Since the publication of *Silent Spring*, however, as shown in Gould and Lewis (2009) (reposted as web Appendix Figure A3), the number of new books listed in the Library of Congress under environmental headings began growing exponentially, together with the creation of regional and national environmental organizations. Gould and Lewis (2009) also point out an apparent compositional change in these organizations: after 1962, not only did more people become interested in the environment, but they also did so in a stronger, more radical way, pushing harder for changes in public policy. This finding is consistent with other studies. According to *The New York Times*'s Annual Event Index, the 1963–1967 period was characterized by higher rates of environmental protests than the 1960–1962 period, accompanied by a marked and steady decrease in public opinion about the state of the environment in 1964, 1965, and 1966, which never really recovered to 1963 levels until the 1980s (Agnone, 2007). Eventually, Congress passed the National Environmental Education Act of 1990, which promoted environmental education initiatives at the federal level.

It is central for this study to establish not only that the book's message reached a wide range of the population, but that this actually increased awareness about environmental issues. Unfortunately, there are no reliable surveys prior to the creation of the EPA,

[6] See Fox (1981), for a historical account of the conservation movement.

in 1970, that would allow me to track the levels of public support for environmental regulation before and after the publication of *Silent Spring*.[7]

Despite being heavily promoted, the book didn't immediately affect the amount of legislation voted on in Congress in regards to environmental protection, as one would initially expect, though there is evidence of a positive trend in the 1960s.[8] The first step toward environmental regulation is an entity capable of supervising and enforcing the regulations approved by Congress. Thus, the rise in the number of issues discussed previous to 1970 won't be a good measure of the demand for environmental regulation in the country: so many things changed in two decades that it would be hard to identify a clean effect of the book on issues discussed in the EPA era (1970 onward). However, the eight-year delay in producing certain types of environmental regulation provides an excellent setting for studying variations in the composition of demand for a clean environment. The fact that the nature and the number of environmental bills weren't changing significantly as a result of the informational shock strengthens the identification assumption of my model. By the time *Silent Spring* came out, some water-pollution regulation was already in place, but no equivalents existed for air pollution. By 1962, scientific research and interest in this topic was just nascent, and the population was generally unaware of (and uninterested in) the detrimental effects of pollution, let alone of how to help.

3.3 Literature

The literature on determinants of the demand for environmental regulation can be roughly divided into three (sometimes overlapping) categories: that which tries to infer preferences from surveys, that which tries to infer preferences from behavior (such as migration, housing prices, and sales of particular products), and that which directly examines willingness to take action on environmental regulation, looking at Congressional roll-call votes. In this section, I summarize some of the main findings of the literature.

Part of it has documented the demographic characteristics of green (pro-environment) voters, by attempting to understand the degree of correspondence between the actions of politicians and the preferences of their constituency. Kahn (2002) finds that demographic and economic changes may contribute to increasing aggregate demand for environmental

[7] Agnone (2007) creates an index of environmental public opinion from 1954 onward, but warns the reader about its limitations: it has many missing observations and combines surveys for different years, geographic scopes, and questions which are not necessarily comparable to one another.

[8] I classify regulation either as environmental or non-environmental by searching systematically for certain keywords in the synopsis of each issue discussed in Congress. I end up focusing on the words "pollution" and "clean". For more details, see Section 3.4.

regulation. Moreover, he finds that stated survey attitudes and actual choices consistently show that more educated people and minorities tend to be more pro-environment, and that manufacturing workers tend to oppose environmental regulation (minorities may be over-concentrated near environmental hazards and thus would benefit from cleanups).[9] This suggests that direct votes on environmental regulation may be consistent with reports from attitude surveys. Holian and Kahn (2015) find that household voting patterns mirror congressional voting patterns on national carbon legislation, and they argue that stated preferences based on microdata are highly informative for predicting local aggregate voting.[10] In addition, they conclude that political liberals and more educated voters favor environmental protection while suburbanites tend to oppose it. A narrower literature looks at the role of expertise in demand for environmental regulation. Morris and Smart (2012) reveal that doctors in 1980–2000 were not more willing to pay to live in less polluted areas than anyone else, once income and educational attainment are controlled for, rendering expertise an irrelevant determinant in demand for clean air.

A possible explanation is that nowadays information and awareness about environmental issues are sufficiently widespread across the population that being a physician does not make a difference anymore. A way to study whether educated people have better access to environmental information or are more able to process it, is to go back to a time when the general public knew little about environmental threats to human health, nature, wildlife, and the planet. In this regard, focusing on the 1950s and 1960s may be of value. The literature has focused mostly on data that emerged after 1970, and little is known about the period prior to the EPA, especially involving demographic and pollution variability across geographic areas.

A smaller line of research has addressed the potential effects of media coverage and/or salient events on demand for a clean environment. Kahn (2007) shows that unexpected events such as environmental catastrophes capture wide public attention. He finds that congressional representatives were actually less likely to vote in favor of bills tied to these events, probably because these bills were much more demanding than the standard ones discussed in normal years. He also finds that liberal representatives from the Northeast were most likely to increase their pro-environment voting in the aftermath of these shocks. Closer to my own question, Brulle et al. (2012) argue that extreme weather events, media coverage, and public access to accurate scientific information are not significant explanatory variables of public environmental concern, while elite cues, media advocacy,

[9] General Social Survey, conducted annually since 1972.

[10] This sets an optimistic precedent for my current methodology; see Guber (2001) for a more skeptical view.

and economic measures seem to be very important. In particular, the authors find that *The New York Times*'s mentions of *An Inconvenient Truth* —a proxy for the extent of overall media attention to the 2005 film— significantly boosted the public's perception of the urgency of climate change. Based on this finding, and by analogy, I would expect to see an important effect of *Silent Spring* on public interest and concern for the environment. However, Nolan (2010) finds that viewings of *An Inconvenient Truth* increased viewers' knowledge, concern, and reported willingness to reduce greenhouse gases, but that this impact was short-lived and not likely to lead to direct action.

In addition, it has been shown that cultural production may contribute to spreading interest and awareness. In a second paper, Brulle et al. (2012) argue that, by publishing ideas about environmental problems in books and magazine articles, and engaging in publicity campaigns, critical intellectuals and their responsive critical communities create the social networks and commitments that eventually generate new environmental movement organizations (EMOs). In a similar vein, Agnone (2007) uses time-series data from 1960 to 1998 to test hypotheses regarding the impact of protest (as measured by *The New York Times* event annual index) and public opinion (as measured by surveys) on the passage of U.S. environmental legislation. Evidence points to an amplification mechanism between environmental movement protest and public opinion, where public opinion affects policy above and beyond its independent effect when protest raises the salience of the issue among legislators.

Few papers have attempted to analyze the role of informational shocks or understand the channels through which they affect demand, and economists haven't reached a consensus yet regarding this topic. Moreover, reliance on surveys to elicit demand is widespread. I can complement these findings by inferring demand from actions and thus shed some light on the obscure link that connects awareness, concern, and action. In addition, understanding the role of education and the channels through which it acts remains an open question, specifically regarding whether educated people process information differently, and thus respond differently to informational shocks. And of course, climate change is a serious worldwide concern (Acemoglu et al., 2012) that requires regulation. Learning about how voters and politicians think and what motivates their actions is therefore crucial. When environmental awareness affects voting behavior (as opposed to just raising concern), carefully designed information campaigns are powerful means to expedite environmental protection.

3.4 Data

The Inter-university Consortium for Political and Social Research (ICPSR) houses data on all roll-call voting records for both chambers of the U.S. Congress.[11] The Senate comprises 100 members (senators), two per state, in office for six years; the House of Representatives comprises 435 members (representatives) in office for two years, where each state is represented in proportion to the size of its population and entitled to at least one representative. My unit of analysis is an individual member of Congress. Each ICPSR record contains a congressman's voting action on every roll-call vote taken during a specific Congress, along with identifying characteristics (name, party, state, district and most recent means of attaining office). In addition, the data contains descriptive information for each roll-call, including a synopsis of the issue, which I use to classify the legislation as either environmental or nonenvironmental. First, I identify issues that refer to environmental regulation by searching for specific keywords in their synopsis. Then, I review each issue to ensure a correct classification of the bill, keeping only those that propose pro-environmental regulation. In particular, I end up focusing on issues that contain the words "pollution" and/or "clean" in their synopsis. See web Appendix Table A1 for my complete selection of environmental issues.

To include characteristics of the constituencies, I merge the roll-code dataset with census data. Lewis et al. (2013) provide digital boundary definitions for every congressional district in existence between 1789 and 2012.[12] NHGIS provides the equivalent for census counties and tracts.[13] Using both maps, I merge congressional districts to census counties. County-level data on the Census of Population and Housing, Business, and Agriculture are also available in ICPSR.

Next, I add geographical variation in pollution. Measures of pollution before the EPA was created are scarce. The EPA provides diverse measures of air pollution by state and year for the 1950s and 1960s, and state-level emissions for SO_2 and NO_X since 1900. This data, however, is not available on a county or district level prior to 1970, and even so, it is not available for every U.S. county. Therefore, in this paper I use the available data from the 1970s to proxy for county-level pollution in my period of interest.[14] This data

[11] In the United States Congress, a bill is a piece of legislation proposed by either of the two chambers of Congress. Anyone elected to either body can propose a bill. After the House of Representatives approves a bill, it is considered by the Senate. After both chambers approve a bill, it is sent to the President of the United States for consideration.

[12] cdmaps.polisci.ucla.edu

[13] www.nhgis.org

[14] The EPA's county-by-year Air Quality Index for 1970 was retrieved from: https://www3.epa.

has some limitations. First, data collected in the early 1970s could have been affected by protective measures taken after the publication of *Silent Spring*; however, most of the effective actions to protect the environment took place after the creation of the EPA in 1970, so I think that using it is reasonable. Second, including pollution data from the 1970s costs me many observations, since 1970 data is available for only 30% of the sample. Another concern is the potential for selection in the counties that report their air quality: those counties that are suffering the most pollution or host the most environmentally-concerned residents might have more incentives to record pollution levels, generating a spurious relationship between pollution and other characteristics of the county. To address these concerns, in the paper I present several robustness checks with alternative pollution controls. For instance, the USDA Census of Agriculture provides data on farm labor, specified farm expenditures, and use of commercial fertilizer at the county level for 1954, 1959, 1964, and 1969. Alternatively, I use county-by-year level infant mortality (Census of Vital Statistics) and cause of death (National Center for Health Statistics, 1959–1971) as proxy for deleterious effects of pollution. The latter has the advantage of being available for all counties in a high frequency.[15]

3.5 Empirical Approach

3.5.1 Baseline

In this section, I use an event-study approach to assess the overall impact of the informational shock on a measure of demand for environmental regulation based on how politicians vote in Congress. My unit of observation is the vote of a specific member of Congress i representing state or congressional district c on a specific issue/bill b, at a specific date t (day, month, year) and chamber, for all environmental issues discussed from the 84th through the 91st U.S. Congresses.[16]

$$EVote_{pctb} = \alpha + \beta Post_t + +\delta Trend_t + \gamma X_{tc} + \epsilon_{itpc}, \tag{3.1}$$

`gov/ttn/airs/airsaqs/detaildata/AQIindex.htm`. The state-by-year SO_2 and NO_X emissions for 1950–1970 were sent to me upon request.

[15] Other sources of information may become available in the future. Clay et al. (2015) digitized new information on plant-level coal consumption, county-level air quality (TSP) measures and infant mortality rates for (the very few) counties with available information in the 1954–1962 period. Federal Power Commission Reports provide data on thermal-power-plant coal consumption and location. In the future, I might use industrial composition interacted with known pollution levels by industry as a proxy for local pollution levels.

[16] By environmental issue, I mean those that include the words “pollution” or “clean” in their synopsis.

where *EVote* is a binary variable representing a pro-environmental vote in any given (environmental) bill, *Post* is a binary variable indicating all time periods after the publication of *Silent Spring*, *Trend* is an annual trend used as a baseline control, and X is a set of controls at the national, state, and congressional district level that may include state fixed effects, district fixed effects, and state-specific trends.

The book was published in September 1962, at the end of the 87th U.S. Congress. I focus on the time window between the 84th and the 91st U.S. Congresses, from January 3, 1955, to January 3, 1971. No environmental issues are discussed in December 1970 or January 1971, implying that my sample effectively ends before the creation of the EPA on December 2, 1970.[17]

In my analysis, I run specifications with votes from both chambers of Congress first, and then limit the sample to bills discussed in the House of Representatives. The advantage of focusing on the House of Representatives is that more environmental bills are discussed in this chamber (not just the ones that are already pre-approved), that there is a larger number of representatives than senators and therefore a larger number of observations per bill, that representatives are in office for a shorter period of time and thus up for reelection more frequently (every two years), and that representatives are elected in congressional districts rather than states, which is adequate for analyzing the relationship between congressional votes and characteristics of the constituencies.

To address the concern that standard errors may be correlated within electorate regions, I cluster standard errors by state when running my analysis on both chambers, and by congressional district when focusing on the House of Representatives. To improve the precision of my estimates and absorb biases, I include a wide range of controls.

The key identification assumptions are that (i) all states and districts received equal treatment —no selection into treatment—, and (ii) the timing of the treatment was exogenous to demand for environmental regulation. The first assumption requires that news about the book or the general content of the book spread homogeneously across the population. It will fail to hold if, for example, talk about the book was more abundant in areas where more environmentally concerned people live. It could be that newspapers and magazines made more references to *Silent Spring* in counties where people's concern about the state of the environment was already relatively high. To address this concern, I

[17] The Congress starts and ends on the third day of January of every odd-numbered year.

use data on cross-county access to mass media, in particular to television.[18] I generate an index that serves as a proxy for cross-sectional intensity of the treatment, which is more likely to be exogenous to the model. This will also help address the second assumption. I argue that though small groups of the population were becoming aware of environmental matters, the exact date the book was published was exogenous. Other books of similar topics were published before and after, without capturing the public's attention. This may suggest that it wasn't so much that public interest pushed for the book to be published and communicated, but the other way round.

3.5.2 Channels

In this section, I propose an empirical approach to study the channels through which the publishing of *Silent Spring* may affect demand for environmental regulation.

To better understand what I'm trying to achieve, refer to Figure 3.1. Arrow 1 is a definition (or assumption) stating that the informational shock increases the stock of public information. All the other arrows illustrate testable hypotheses analyzed in this paper. I start by arguing that the book did, in fact, increase overall awareness in the population (an aggregation of arrow 2), by showing circumstantial evidence and analyzing the evolution of public-opinion surveys. Then, I conduct a direct-effect or baseline analysis, where I control for education, income, and exposure to zero out education in arrow 2, exposure to pollution in arrow 4, and both directions of the income effects in arrow 5, to try to identify arrow 3. The heterogeneity analysis follows, and I use interactions of the informational shock with education, income and exposure to learn more about the channels represented by arrows 2, 4 , and 5, respectively. Arrow 2 suggests that the combined stock of information and education determines the level of awareness. How this combination works is an empirical question.

In order to dissentangle the controbution of specific attributes of the population to the demand for environmental regulation in response to the book, I run the regression shown below. Once again, the unit of observation is the vote of a specific House member i on a specific issue/bill b, at a specific date t (day, month, year), for all issues related to the environment in my study period:

$$\begin{aligned} EVote_{pctb} = \alpha + \beta_1 Post_t + \beta_2 Educ_{ct} + \beta_3 Inc_{ct} + \beta_4 Expos_{ct} \\ + Post_t(\beta_5 Educ_{ct} + \beta_6 Inc_{ct} + \beta_7 Expos_{ct}) + \delta Trend_t + \gamma X_{ct} + \epsilon_{itpc}. \end{aligned} \tag{3.2}$$

[18] I would like to include radio and newspapers in a future iteration of this paper.

Once again, *EVote* represents a pro-environmental vote, *Post* is a binary variable indicating all time periods after the publication of *Silent Spring*, *Trend* is an annual trend used as a baseline control, and X is a set of controls at the national, state, and congressional district level that may include state fixed effects, district fixed effects, and state-level trends. The innovation here is that I include an indicator for the level of education (*Educ*), median family income (*Inc*), and exposure to pollution (*Expos*) before and after the release of *Silent Spring*.

The main difference with the previous specification is that now we can distinguish between the overall effect of information that is associated with higher income, higher education, or higher exposure, respectively. A positive (negative) estimate of β_5 would suggest that information and education are complements (substitutes) in the production function of environmental awareness, which in turn increases pro-environmental voting.

Below, I present a full specification, which allows me to assess more specific channels by adding flexibility to the model.

$$\begin{aligned} EVote_{pctb} = \alpha &+ \beta_1 POST_t + \beta_2 Educ_{ct} + \beta_3 Inc_{ct} + \beta_4 Expos_{ct} \\ &+ \beta_5 Educ_{ct} Inc_{ct} + \beta_6 Educ_{ct} Expos_{ct} + \beta_7 Inc_{ct} Exp_{ct} \\ &+ POST_t(\beta_8 Educ_{ct} + \beta_9 Inc_{ct} + \beta_{10} Expos_{ct} + \beta_{11} Educ_{ct} Inc_{ct} \\ &+ \beta_{12} Educ_{ct} Expos_{ct} + \beta_{13} Inc_{ct} Exp_{ct}) + \delta Trend_t + \gamma X_{ct} + \epsilon_{itpc}. \end{aligned} \tag{3.3}$$

Controls may include district, race, and gender ratios, median age, population, schooling, death rates, state or district fixed effects, and state-level trends. I use robust standard errors in all regressions, clustered by congressional district.

The identifying assumption here is that, within each congressional district, and keeping all control variables fixed, the timing of the treatment is independent of any other variables that correlate with the evolution of the relevant interactions over time. This would be violated if, for example, the publication of *Silent Spring* coincided with actions from environmental activists who systematically sought support from the most educated and pollution-exposed individuals. This would bias upward the estimated coefficient β_{12} associated with $Post_{ct} Educ_{ct} Expos_{ct}$.

3.6 Results

3.6.1 Baseline

In this section, I present the baseline results. There were nine possible voting outcomes.[19] I use the standard binary Yes count for the main outcome and check the robustness of my results later using two alternative indicators.[20] Since the environmental regulation I work with is all pro-environmental, a Yes vote is equivalent to a pro-environmental vote.

First, I present a graphical analysis. Panel A in Figure 3.4 shows a scatter plot of the fraction of pro-environmental votes by chamber and environmental bill (defined as those that mention "pollution" or "clean" in their synopsis). Red squares represent issues voted on in the Senate, and blue circles represent issues voted on in the House of Representatives. Overall, no significant changes show up in trend before and after 1962. If anything, the fraction of pro-environmental votes seems to fall after the release of *Silent Spring*.[21] In closer inspection, we can see that there are very few observations from the Senate; these are driving the negative discontinuity. Panel B plots fitted values for the votes that correspond only to the House, and it shows the expected results: a jump upwards in the share of positive votes for pro-environmental regulation, accompanied by a rise in steepness. This might indicate positive reinforcement.

In figure 3.5, I narrow the set of environmental regulations to bills that are related to water pollution (the most common theme in the list). Both panels show a consistent increase in the share of positive votes after 1962, in both chambers of Congress. These results are robust to alternative voting indexes (see web Appendix Figure A7). I consider this to be a good selection of environmental issues, and I will continue to report results with both samples throughout the paper.

Then, I present results from my regression analysis. Panel A in Table 3.1 presents

[19] The nine categories for the variable vote in Congress are Yes [Y], Paired Yes [PY], Announced Yes [AY], Announced No [AN], Paired No [PN], No [N], General Pair [GP], Present [P], and Not Voting [NV]. When a member of Congress chooses to pair, she is pairing her own vote with an opposite vote, such that her vote gets neutralized. A general pair means that the congresswoman is not being associated with a yes or no answer, while a paired-yes and paired-no mean that she chooses explicitly to be associated with one side or the other, even though, at the end of the day, the vote won't contribute points to that side. The different categories may correspond to different strategies on the part of the member of Congress.

[20] Standard Binary Index: Yes=1=Y=PY=AY, No=0=N=PN=AN. Alternative Binary Index: Yes=1=Y=PY=AY, No=N=AN=PN=GP, Alternative Triple Index: Yes=1=Y=PY=AN, Neutral=0.5=GP=P, No=0=N=PN=AN.

[21] This is robust to using alternative voting indexes (see web Appendix Figure A6)

estimates corresponding to equation 1.1 for all environmental bills in my sample and both chambers of Congress. The unit of observation is given by pairing a bill with a senator or house representative. The treatment is a post-*Silent Spring* indicator, which takes value 1 after the book is released and 0 otherwise. The outcome is an indicator of each pro-environmental vote issued. Note that the graphical analysis implicitly weighs each bill equally, while the regression analysis assigns the same weight to each individual vote. In other words, in the graphical analysis the unit of observation was a bill, while in the regression analysis the unit of observation is a bill-congressman pair.

The results suggest that *Silent Spring* was associated with a significant rise in the probability that a member of Congress votes in favor of an environmental bill. This effect varies from 5 to 33 percentage points, depending on the specification. For instance, the inclusion of control variables in columns 4 and 6 increases the magnitude of the treatment-effect estimate.[22]

Panel B presents estimates corresponding to equation 1.1 for all water-related environmental bills emerging from both chambers of Congress. The resulting estimates are larger and more homogeneous than before, suggesting that *Silent Spring* was associated with a rise of 22 to 35 percentage points in the probability that a senator or representative votes in favor of (water-related) environmental legislation. This is to be expected because water-related bills before and after 1962 are more comparable to one another than to other environmental bills; the assumption that the only difference between votes before and after the publication of the book was the publication of the book itself is more credible when similar bills are being discussed.

Panel C presents estimates corresponding to equation 1.1 for all environmental bills in just the House of Representatives. As shown in the graphical analysis, the Senate gets fewer environmental bills and exhibits more volatile voting patterns, suggesting that more strategic behavior may be at play.[23] Thus, the rest of the paper focuses on the House of Representatives. Another advantage of restricting the sample to the House is that now I can include district-level fixed effects and controls. Although the initial specifications suggest that *Silent Spring* was associated with a 6 to 21 percentage-points increase in the probability that a representative votes in favor of an environmental bill, this effect completely disappears after including time-varying controls or district fixed effects.

[22] Controlling for total carbon emissions and per capita carbon emissions (reported annually by the EPA), and aggregate quarterly variables such as real GDP per capita, civil rate of unemployment, Consumer Price Index, and total monthly legislation discussed in Congress appears to raise the coefficient of interest.

[23] This might have something to do with the fact that most bills are born at the House, and must be approved by the House before they are discussed in the Senate.

In other words, Column 1 shows a specification without controls or fix effects, and presents the highest (and most significant) estimate in a manner perfectly consistent with the intuitions carried from the graphical analysis. However, the sign of the estimate becomes negative as soon as controls are included (Columns 5 and 6). Could it be that the controls are somehow not precise enough? As noted below the table, I account for pollution by including death rates and cause of death. I use the share of deaths by pneumonia, food poisoning, and cancer as an indicator of deaths that could be associated with pollution. This is a broad measure that might account for air pollution, water pollution, and soil pollution, and thus it seems reasonable for this analysis. Moreover, these data come with good geographic and temporal disaggregation, and thus I benefit from not having to drop many observations. However, the downside is that it's not a direct measure of pollution.

To inspect this further, Table 3.3 replicates column 5 of Panel C using alternative cross-section measures of pollution.[24] Column 1 replaces cause of death by the 1970 district-level Air-Quality Index, and column 2 uses 1980 district-level carbon emissions (both available from the EPA).[25] Both columns replace panel controls by their cross-section 1950 equivalents.[26] In contrast to Panel C, these cross-section controls do not eliminate the initially significant and positive treatment effect, which suggests that the publication of *Silent Spring* was associated with an increase of approximately 15 percentage points in the probability of voting in favor of environmental regulation. These direct and more accurate pollution controls, however, come at a cost in terms of observations. The 1970 data capture only 30% of the sample in Table 3.1, and the 1980 data about 50%. Moreover, this sample may be subject to selection: since most counties were not recording pollution back then, those for which we have data were likely the most polluted or the most concerned about pollution at the time. Thus, whatever results we see here might not be representative of the rest of the country. The last two columns go back to using only my indirect measure of pollution, with two caveats: Column 3 contains only cross-section equivalents of controls (comparable to Columns 1 and 2), and Column 4 contains time-varying controls, but uses only the sample of observations that would contain 1970 air-quality data. The coefficient in Column 3 comes out negative and insignificant, indicating that the different results I get, compared to Column 5 in Panel C, are exclusively related

[24] Its not possible to replicate column 6, since district fixed effects would be perfectly colinear with cross-section pollution, causing it to drop out of the regression.

[25] Maximum level of CO_2 in eight hours.

[26] Many Census questions show little variation from one decade to the next, and thus numbers might not be totally consistent across time. For this reason, I prefer cross-section controls, and I use time-varying controls mostly to include district fixed effects.

to my pollution data, and not to the nature (cross-section or time-variant) of the other controls. Finally, the coefficient in Column 4 comes out as significant and even larger in magnitude than the ones found in the first two columns. The takeaway is that the results are not robust to the sample under analysis, and the counties with 1970 and 1980 pollution data may just not be comparable to the rest of the country. This finding suggests that those districts that were most influenced by *Silent Spring*, later, and as a consequence, became the first to start recording their levels of pollution. It could also mean that the most polluted counties or those with the most environmentally sensitive population were the most affected by the book.

Finally, I conduct the baseline analysis for various subsamples of environmental issues. Table 3.4 shows the results. Columns 1 and 2 use a subsample of environmental bills without the two issues that shaped the the 1963 Clean Air Act. Since this was such an important act, these issues might have received more attention than other bills at the time. Moreover, they bills associated to the 1963 Clean Air Act were the first environmental bills to be discussed in Congress after the release of *Silent Spring*, and were voted on by representatives who were elected just months after the release of the book.[27] Thus, there are reasons to believe these bills might have been outliers. The results seem to sustain this theory with an insignificant (though positive) treatment coefficient in Column 1 (controlling for 1970s air quality) and actually significantly negative in Column 2 (the usual specification, controlling for cause of death and including district fixed effects). However, this is not the end of the story. Restricting the sample to only water-related environmental bills, the original result returns stronger than before, with highly significant coefficients that suggest a rise of as much as 43 percentage points in the probability that a representative votes pro-environmental. The conclusion is that the results are partly driven by the influential Clean Air Act but also appear in bills that are sufficiently similar before and after 1962. It is likely that the nature of the environmental issues being discussed in Congress changed from one period to the next.

3.6.2 Channels

The objective of this section is to learn about the arrows proposed in Figure 3.1. Once again, treatment is an indicator that takes the value 1 after *Silent Spring* was published, and the outcome variable is a standard binary vote index that takes value 1 for a pro-environmental vote, and 0 otherwise. The unit of observation is the vote made by a member of the House on a given bill.

[27] Members of Congress for 1963 were elected on December 1962, i.e., four months after *Silent Spring* came out and about six months from the early sales of the book.

Table 3.5 displays the estimates corresponding to equation 3.2 in a sample of all environmental issues discussed in the House. All regressions include a list of time-varying district-level controls, district fixed effects, and state-specific trends. The share of pollution-related deaths is a proxy for pollution. New variables and interactions of interest are added from one specification to the next for a clearer interpretation of the results. Column 1 shows that median family income has a significant and positive coefficient. The interpretation is that an increase of one dollar in the average value at the congressional district level, of the county median family income, is associated with a 0.0018 percentage-point increase in the probability that its representative votes in favor of environmental regulation. Pollution and schooling appear insignificant and have negative signs. It would seem that income is more decisive than education and pollution at the time of voting for environmental regulation. Column 2 includes interactions of these three variables with the post-*Silent Spring* treatment. The three interactions are significant. The income control loses significance and changes sign (becomes negative) in this alternative specification, suggesting that income is not inherently associated with environmental regulation. In fact, it seems the income effect was driven by *Silent Spring*. Perhaps richer counties had better access to the book or to information about the book, and thus responded significantly to its release.[28]

Exactly the opposite happens with median years of schooling: the sign of the control variable becomes positive (and insignificant), while the interaction with the treatment is negative and significant. This suggests that there is no such thing as an inherently negative association between education and demand for environmental regulation. In fact, the impression I got from Column 1 was misleading, and was driven by the fact that less educated people were the ones who reacted most strongly to the informational shock, perhaps because they were relatively less informed to begin with. This may suggest a substitutability (rather than a complementarity) between (private) education and (public) information in the production function of awareness (see Figure 3.1). Pollution displays a similar but even stronger pattern: the pollution control remains insignificant (though it becomes positive, indicating that, if anything, polluted counties would tend to favor environmental regulation) while the interaction with the treatment becomes positive and significant at the 1% level. It seems that *Silent Spring* had a major positive effect among polluted districts. Thus, it is the most exposed (because they have the need), the richest (because they have the means), and the less educated (because they now have more information) that react most strongly to the informational shock, leaving everything else constant in each case.

[28] This argument makes sense, especially because *Silent Spring* was publicized by *The New Yorker*.

Column 3 adds an extra layer of interactions to all variables of interest. Lets take income. Median family income appears now to be inherently positively associated with environmental regulation. It would appear that *Silent Spring* either encouraged poor people to demand environmental regulation, or discouraged the rich, and that income and education are substitutes in terms of voting for environmental regulation but became complements after *Silent Spring* came out. In other words, the positive effect of income in the demand for environmental regulation operates mainly through its interaction with education: the richest, most educated people tended to oppose environmental regulation before the shock, and are the ones who reacted most strongly in favor of environmental regulation after the informational shock. Last, Column 4 includes the most complete specification. Nothing comes out significant except for a positive effect of education, overall, on the propensity to vote green. However, all the signs from Column 4 remain unchanged. This specification contains fewer observations per variable of interest, which decreases the power of the statistical test to detect significant effects.

In short, the shock appears to have heterogeneous effects that wash out when we increase by too much the number of channels. The pattern is not necessarily robust to variations in the sample. See Table 3.6 for the results that correspond to the sample with 1970s air pollution data.

3.6.3 Intensity of Treatment

I complement the previous analysis using existing ICPSR data on county-level access to public media (television) to capture the intensity of the informational shock.[29] The idea behind this approach is that areas that didn't have access to this source of information were less likely to learn about *Silent Spring*, and thus, less likely to alter their voting patterns in elections. I expect to find a positive and significant coefficient when using access to media in a reduced-form analysis. The implicit assumption is that inhabitants of counties with less access to information were not any less (or more) inherently concerned about the environment than people living in other regions. This would not hold if, for instance, environmentally sensitive individuals tended to demand broader access to media. A product's early days, however, often give rise to variation in ownership that might depend on exogenous (remoteness) or observable (income) factors. In fact, between 1949 and 1969, the number of U.S. households with at least one television set rose from less than a million to 44 million, indicating that the lack of TV access could have been

[29] In the future, I might incorporate radio and newspapers.

due to difficulties in acquiring the product rather than to a lack of interest.[30] Moreover, there seems to be considerable overall and within-state variation in the distribution of the share of TV-owning households per county in 1960 (see web Appendix Figure A8).

Table 3.7 shows results for the intensity model, taking into account all environmental issues discussed in the House. The unit of observation and outcome are the same as in the baseline and heterogeneity analysis. The innovation lies in the treatment variable, which is now replaced by the interaction between the original *Silent Spring* treatment and the district mean of the share of TV-owning households per county in 1960. The first two columns replicate the baseline model with the new treatment, and the last two columns replicate the heterogeneity analysis. Odd columns measure pollution directly, using the 1970 Air Quality Index, and even columns use only a proxy, given by the share of pollution-related deaths. Column 1 shows the expected results: among districts that monitored pollution in 1970, the positive effect of *Silent Spring* on the probability of voting green is a complement of the degree of access to media (television), since the sign of the treatment is both positive and significant. This result, however, does not hold in the greater sample (Column 2). As happened with the baseline specification, this problem is once again solved when we restrict the sample to water-related environmental issues (see Table 3.8).

The heterogeneity specifications show that well educated people, richer people, and those who are more exposed to pollution respond more strongly to the informational shock but the size of the effect diminishes with the number of attributes. These findings remain after moving from the complete sample to the water-specific sample, but are not robust to the pollution control used. As above, it seems that districts with 1970s pollution data are intrinsically different from the rest.

3.7 Robustness Checks

I start by analyzing variations in timeframe. Table 3.9 shows the baseline specification for all environmental issues in two alternative windows of time. Columns 1–3 are based on the 86th to 90th Congresses (January 1959 to January 1967). This period encompasses exactly two Congresses elected before *Silent Spring* and two Congresses elected after *Silent Spring*. It also represents a narrowing of the time window used for the baseline study, since the complete data set encompasses the 84th through 91st Congresses (January 1955 to January 1971). For each timeframe, the first column includes 1970 pollution data, the

[30] By 1959, roughly 88 percent of U.S. households had least one TV; this figure reached 96 percent in 1970.

second column includes only cause of death as a proxy for pollution, and the third column adds district fixed effects to the previous one. In all six specifications, we get positive and significant treatment coefficients. The positive baseline effect of the informational shock on demand for environmental regulation seems to be larger when closer to 1962.

Then I move on to conducting placebo tests. So far, I have found positive effects of the informational shock on voting patterns that may be interpreted as increasing demand for a clean environment. However, are these effects particular to environmental regulation, or is there a general trend toward voting in favor of issues discussed in Congress? Finding an effect of *Silent Spring* in voting patterns for issues that are totally unrelated to environmental protection might signal that my previous results were spurious. Table 3.10 replicates the baseline and heterogeneity specifications on a sample of military issues discussed in the House between 1955 and 1971.[31] Table 3.11 does the same thing using treatment intensity. In the two tables, the heterogeneity analysis comes out without significant effects, as predicted. However, the baseline specifications shows a treatment coefficient that is both significant and comparable in magnitude to the ones obtained in the standard specifications. I get a similar result when I use the complete universe of issues after eliminating the environmental ones. This means that, while the evidence is consistent with a large effect of *Silent Spring* on demand for environmental regulation, I cannot rule out alternative causal factors correlated in time.

3.8 Conclusion and Discussion

The 1962 environmental science book *Silent Spring* is widely credited with spreading information about environmental issues among the U.S. population. In this paper, I document that the publication of the book was associated to a marked decrease in public confidence that the government was doing enough for the environment. I then show that this increase in environmental awareness was associated with a 5 to 33 percentage-point increase in the probability that members of Congress voted in favor of environmental regulation. The effect of the informational shock is not necessarily persistent across different specifications of pollution and county samples. I move on to show that the informational shock caused by the release of the book appears to have had heterogeneous effects across the population. It is associated with a decrease in the probability of voting green for districts with more schooling and income but with an increase for counties

[31] I obtained these by looking for the word "military" in the synopsis of each issue, making sure there was no overlap with environmental regulation, which there wasn't.

that are simultaneously rich and educated. When I ran a placebo analysis over a set of nonenvironmental bills I obtained similar results for other pieces of legislation. This suggests that, while the evidence is consistent with a large effect of *Silent Spring* on demand for environmental regulation, I cannot rule out other explanations for that rise.

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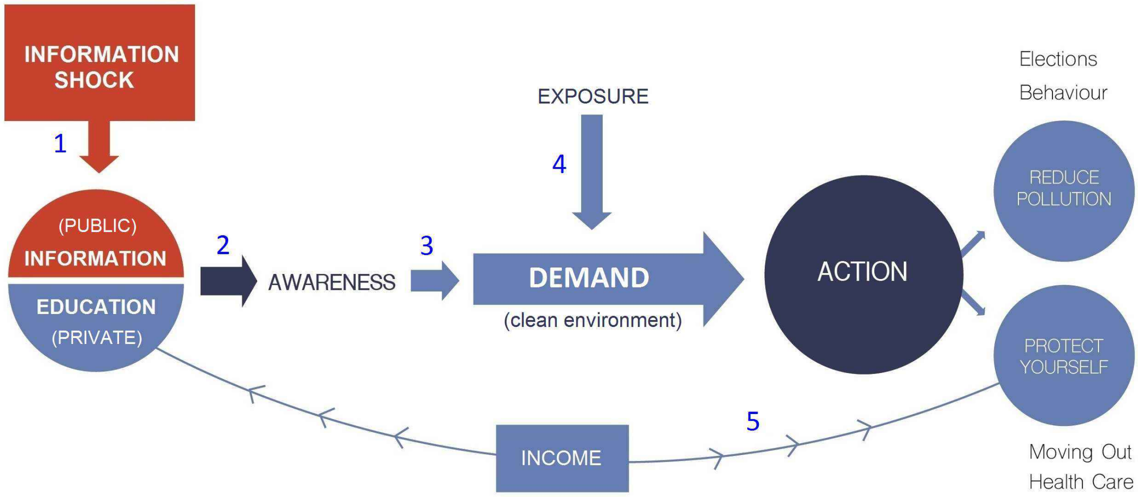
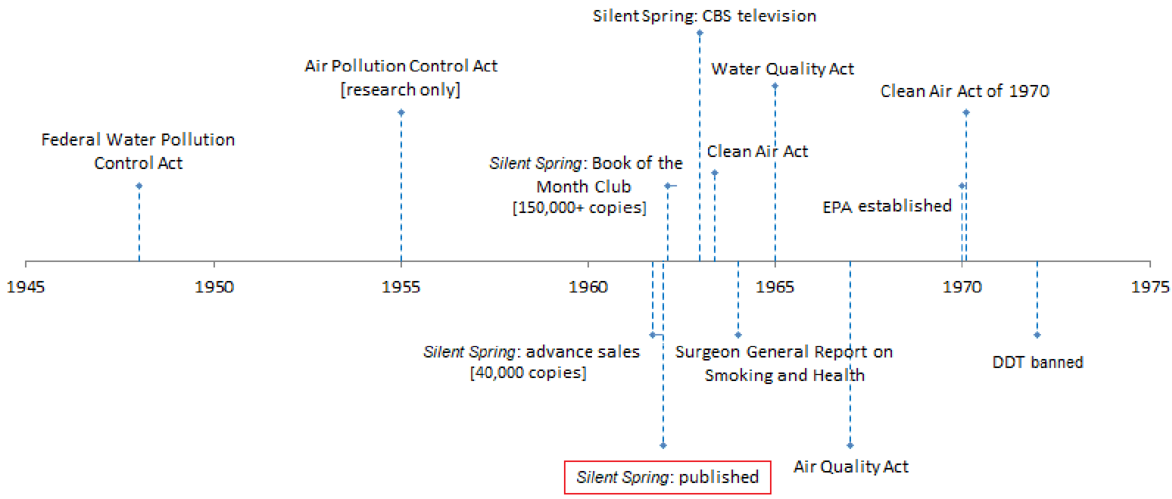
3.9 Figures and Tables

Figure 3.1: A theoretical framework for the response to information about environmental threats



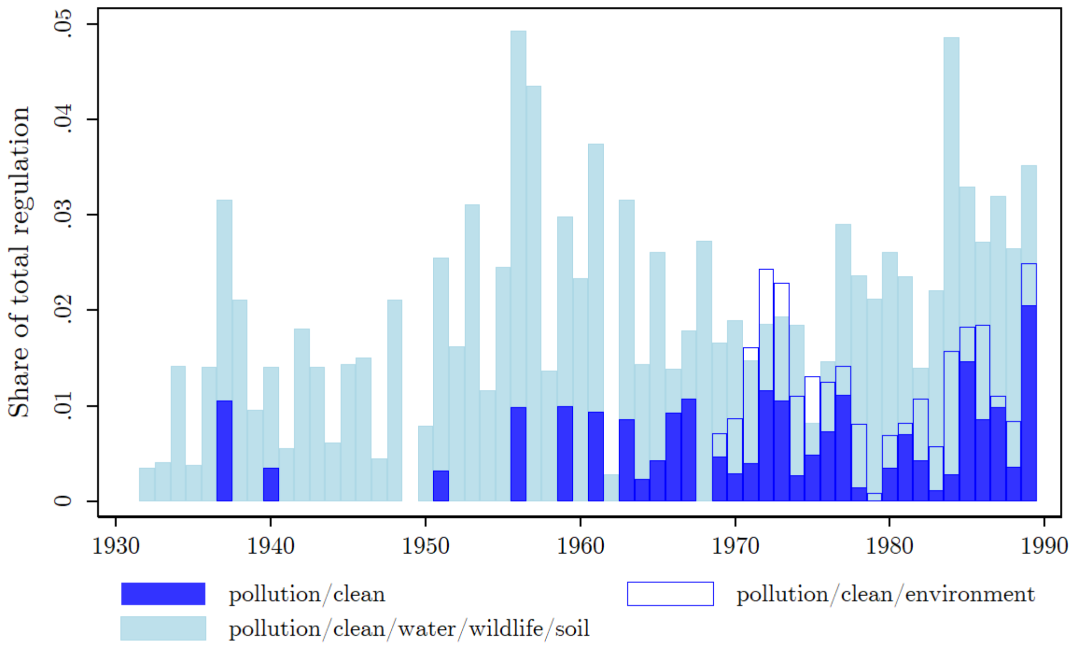
Notes: This diagram presents a framework to understand how informational shocks about environmental threats affect behavior. The shock increases the overall stock of information (1) to produce awareness about environmental issues (2). This awareness, in turn, influences the demand for a clean environment (3). Factors such as the degree of exposure to environmental threats also contribute to demand (4). The degree of demand for a clean environment takes the form of action towards reducing pollution directly or taking measures to protect oneself from environmental hazards. Income influences one's ability to engage in certain actions (5).

Figure 3.2: Timeline of *Silent Spring* and environmental regulation



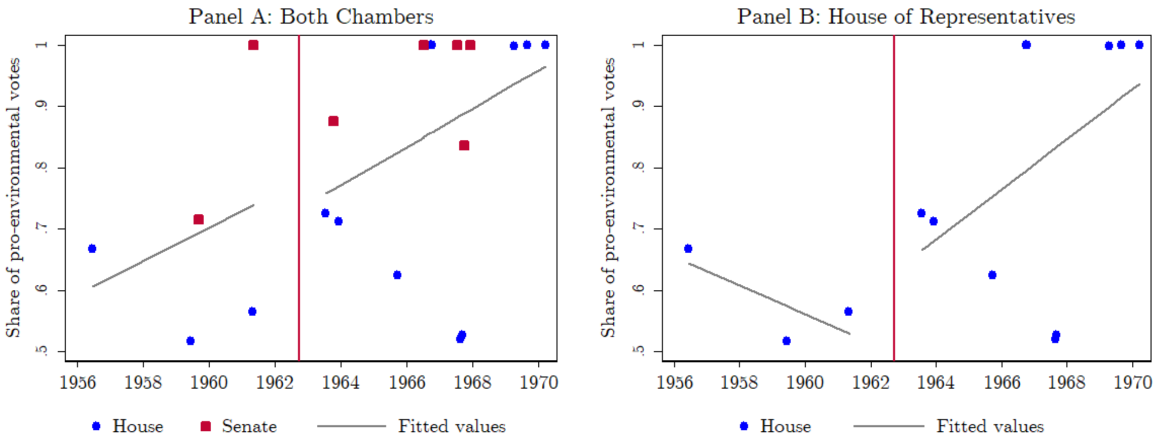
Notes: This diagram shows a timeline of events from the Federal Water Pollution Control Act of 1948, through the publication of *Silent Spring* in 1962, to 1972, when the harmful pesticide DDT was banned by the Environmental Protection Agency. Only two important pollution-control acts were passed between 1945 and 1962; six were passed between 1962 and 1975.

Figure 3.3: Share of environmental bills discussed in Congress



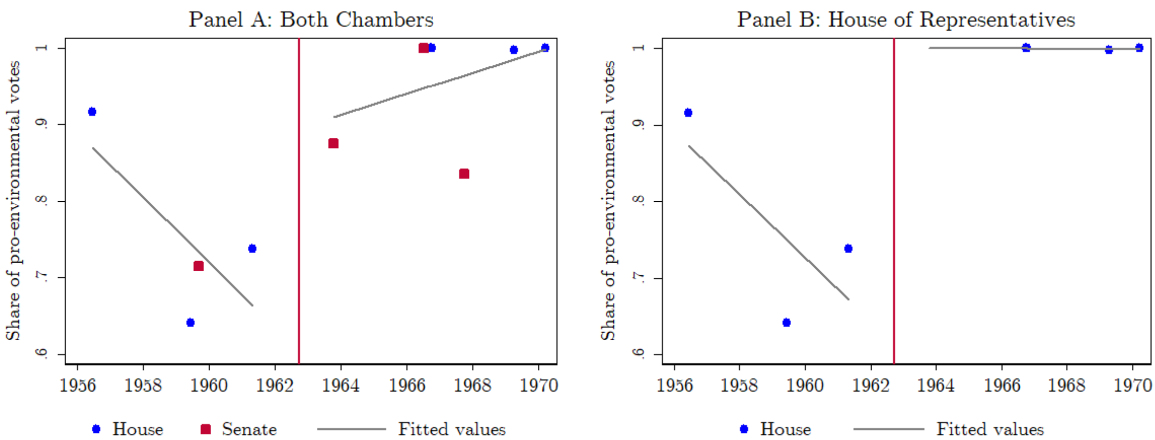
Notes: This figure shows the share of bills voted on by Congress that pertain to environmental regulation over time, as determined by the presence of three sets of keywords in their synopsis. The full blue bars at the bottom of the figure correspond to bills that contain the words "pollution" and/or "clean" in their synopsis; I refer to these bills as environmental bills throughout this paper. Other keywords often show up in unrelated contexts.
Source: Inter-university Consortium for Political and Social Research, and Congressional Quarterly, Inc. United States Congressional Roll Call Voting Records, 1789–1998.

Figure 3.4: Share of pro-environmental votes by bill 1956–1970
All environmental bills, Senate and House of Representatives



Notes: This figure plots the share of pro-environmental votes by environmental bill, defined as any bill containing keywords "pollution" and/or "clean." Vote based on a standard binary yes count. Panel A includes votes from both chambers of Congress. Panel B is restricted to the House of Representatives.
Source: ICPSR 1955–1971.

Figure 3.5: Share of pro-environmental votes by bill 1956–1970
Water-related environmental bills, Senate and House of Representatives



Notes: This figure plots the share of pro-environmental votes by chamber and bill containing "water pollution" and/or "clean water" and no motions to recommit, for enhanced comparability. Pro-environmental votes based on a standard binary yes count. Panel A includes votes from both chambers of Congress. Panel B is restricted to the House of Representatives.
Source: ICPSR 1955–1971.

Table 3.1: State-level baseline analysis for all environmental issues, both chambers

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. All issues						
POST *Silent Spring*	0.215***	0.214***	0.0497*	0.335***	0.0495*	0.333***
T-statistic	(20.18)	(20.05)	(2.39)	(10.16)	(2.38)	(10.09)
Observations	6,854	6,854	6,854	6,854	6,854	6,854
R-square	0.0561	0.0648	0.0763	0.129	0.0828	0.135
Panel B. Water issues						
POST *Silent Spring*	0.225***	0.224***	0.321***	0.357***	0.320***	0.351***
T-statistic	(19.85)	(20.10)	(12.05)	(7.04)	(12.15)	(6.82)
Observations	2,882	2,882	2,882	2,882	2,882	2,882
R-square	0.120	0.169	0.174	0.218	0.208	0.245
State F.E.	NO	YES	YES	YES	YES	YES
Annual trend	NO	NO	YES	YES	-	-
Controls	NO	NO	NO	YES	NO	YES
Trend by State	NO	NO	NO	NO	YES	YES

Notes: This table contains my complete sample of environmental issues discussed in both chambers of Congress from January 1955 through January 1971. I control for total and per capita carbon emissions by state-year, real GDP per capita, civil unemployment rate, and Consumer Price Index by quarter, and total monthly legislation discussed in Congress. Standard binary vote count: Y/PY/AY=1, N/PN/AN=0. I present t-statistics corresponding to robust and state-clustered standard errors in parentheses.
*p $<$ 0.05, **p $<$ 0.01, ***p $<$ 0.005
Source: ICPSR 1955–1971.

Table 3.2: Baseline analysis for all environmental issues, House of Representatives

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. All issues						
POST *Silent Spring*	0.214***	0.214***	0.0584**	0.0574**	-0.000557	-0.00535
T-statistic	(20.57)	(20.30)	(2.77)	(2.71)	(-0.02)	(-0.21)
Observations	6,086	6,086	6,086	6,086	4,893	4,893
R-square	0.0534	0.0630	0.0729	0.0810	0.119	0.159
Panel B. Water issues						
POST *Silent Spring*	0.418***	0.418***	0.566***	0.564***	0.413***	0.408***
T-statistic	(62.89)	(61.12)	(30.26)	(30.03)	(25.46)	(20.93)
Observations	3,742	3,742	3,742	3,742	2,720	2,720
R-square	0.201	0.210	0.215	0.222	0.293	0.322
State F.E.	NO	YES	YES	YES	YES	-
Annual trend	NO	NO	YES	YES	-	-
Controls	NO	NO	NO	YES	YES	YES
Trend by State	NO	NO	NO	NO	YES	YES
District F.E.	NO	NO	NO	NO	NO	YES

Notes: This table contains my complete sample of environmental issues discussed in both chambers of Congress from January 1955 through January 1971. I control for death rate, share of deaths associated with pollution, population, population density, share of urban population, median family income, median years schooling for persons 25+, labor force male, labor force employed in agriculture, median age in years, share of labor force working outside county of residence, share using public transport to work, share of occupied housing units with one automobile, share of manufacturing establishments with 100+ employees, share of non-white population, and tons of commercial fertilizer used in farms. Standard binary vote count: Y/PY/AY=1, N/PN/AN=0. I present t-statistics corresponding to robust and state-clustered standard errors in parentheses.

*p $<$ 0.05, **p $<$ 0.01, ***p $<$ 0.005

Source: ICPSR 1955–1971.

Table 3.3: Baseline analysis for all environmental issues with pollution controls, House of Representatives

	(1) Includes 1970 pollution	(2) Includes 1980 pollution	(3) Includes cause of death	(4) Includes cause of death, 1970 pollution
POST *Silent Spring*	0.102*	0.118**	0.00257	0.118*
T-statistic	(1.99)	(3.25)	(0.11)	(2.26)
Observations	872	1,724	4,776	778
R-square	0.136	0.121	0.121	0.167
State F.E.	YES	YES	YES	YES
Annual trend	YES	YES	YES	YES
Trend by state	YES	YES	YES	YES
District F.E.	NO	NO	NO	NO
Controls: cross-section	YES	YES	NO	YES
Controls: panel	NO	NO	YES	YES

Notes: I control for death rate, share of deaths associated with pollution, population, population density, share of urban population, median family income, median years schooling for persons 25+, labor force male, labor force employed in agriculture, median age (years), labor force working outside county of residence, share using public transport to work, share of occupied housing units with one automobile, manufacturing establishments with 100+ employees, share of non-white population, and tons of commercial fertilizer used in farms. Values from 1950 Census used in columns 1–3, and time-varying equivalents for column 4. This table contains my complete sample of environmental issues discussed in the House of Representatives from January 1955 through January 1971. Standard binary vote count: Y/PY/AY=1, N/PN/AN=0. I present t-statistics corresponding to robust and state-clustered standard errors in parentheses.
*p $<$ 0.05, **p $<$ 0.01, ***p $<$ 0.005
Source: ICPSR 1955–1971.

Table 3.4: Baseline analysis for subsamples of environmental issues with pollution controls, House of Representatives

	Excluding Clean Air Act		All water-related		Water-related, no motions to recommit	
	(1)	(2)	(3)	(4)	(5)	(6)
POST *Silent Spring*	0.0893	-0.0764**	0.433**	0.428***	0.374**	0.341***
T-statistic	(1.09)	(-2.65)	(3.34)	(7.51)	(2.85)	(5.39)
Observations	794	4062	511	2,629	401	1,917
R-square	0.212	0.178	0.332	0.324	0.366	0.585
Pollution: 1970 Air Quality Index	YES	NO	YES	NO	YES	NO
State F.E.	YES	YES	YES	YES	YES	YES
District F.E.	NO	YES	NO	YES	NO	YES

Notes: All models contain state-specific trends and time-varying controls for: death rate, share of deaths associated with pollution (pneumonia/cancer/food poisoning), population, population density, share of urban population, median family income, median years of schooling for persons 25+, share of male labor force, share of labor force employed in agriculture, median age (years), share of labor force working outside county of residence, share using public transport to work, share of occupied housing units with one automobile, manufacturing establishments with 100+ employees, share of non-white population, and tons of commercial fertilizer used in farms. Values from the 1950 Census are used in columns 1–3, and time-varying equivalents for column 4. This table contains subsamples of environmental issues discussed in the House of Representatives from January 1955 through January 1971. Standard binary vote count: Y/PY/AY=1, N/PN/AN=0. I present t-statistics corresponding to robust and district-clustered standard errors in parentheses.

*p < 0.05, **p < 0.01, ***p < 0.005.

Source: ICPSR 1955–1971.

Table 3.5: Heterogeneity analysis for all environmental issues, House of Representatives

	(1)	(2)	(3)	(4)
POST *Silent Spring*	-0.00963	0.217	1.543**	1.222
	(-0.34)	(1.27)	(3.17)	(1.48)
Share of pollution-related deaths	-0.121	4.461	8.696	6.124
	(7.07)	(0.14)	(1.00)	(0.65)
Median family income	0.0000186**	-0.0000142	0.000309***	0.000358
	(2.78)	(-0.69)	(3.56)	(1.75)
Median years schooling, persons 25+	-0.0126	0.0276	0.159**	0.177*
	(-1.03)	(1.29)	(2.74)	(2.02)
(POST)*(Income)		0.0000393*	-0.000262**	-0.000189
		(1.97)	(-2.98)	(-0.92)
(POST)*(Schooling)		-0.0569*	-0.155**	-0.131
		(-2.55)	(-2.67)	(-1.47)
(POST)*(Pollution)		3.256***	-1.320	5.860
		(3.44)	(-0.27)	(0.41)
(Income)*(Schooling)			-0.0000265***	-0.0000316
			(-3.78)	(-1.64)
(Income)*(Pollution)			-0.000909	-0.00215
			(-0.98)	(-0.55)
(Schooling)*(Pollution)			-0.0574	-0.492
			(-0.08)	(-0.34)
(POST)*(Income)*(Schooling)			0.0000231**	0.0000177
			(3.31)	(0.92)
(POST)*(Income)*(Pollution)			0.00157	-0.000244
			(1.61)	(-0.06)
(POST)*(Schooling)*(Pollution)			-0.339	-0.893
			(-0.43)	(-0.56)
(Income)*(Schooling)*(Pollution)				0.000122
				(0.33)
(POST)*(Income)*(Schooling)*(Pollution)				0.00014
				(0.37)
Interactions	NO	NO	YES	FULL
Observations	4,721	4,721	4,721	4,721
R-square	0.171	0.176	0.181	0.182

Notes: Income denotes median family income; schooling denotes median years of schooling for persons 25+; pollution denotes the share of deaths associated with pollution (pneumonia/food poisoning/cancer). All columns contain a set of state-specific trends and and district fixed effects. I control for death rate, population, population density, share of urban population, share of male labor force, share of labor force employed in agriculture, median age (years), labor force working outside county of residence, share using public transport to work, share of occupied housing units with one automobile, manufacturing establishments with 100+ employees, share of non-white population, and value of chemicals used for insect control on livestock. This table contains my complete sample of environmental issues discussed in the House of Representatives from January 1955 through January 1971. Standard binary vote count: Y/PY/AY=1, N/PN/AN=0. I present t-statistics corresponding to robust and district-clustered standard errors in parentheses.

*$p < 0.05$, **$p < 0.01$, ***$p < 0.005$.

Source: ICPSR 1955–1971.

Table 3.6: Heterogeneity analysis for all environmental issues
1970s pollution controls, House of Representatives

	(1)	(2)	(3)	(4)
POST *Silent Spring*	0.193**	-0.0617	-0.885	-7.305*
	(3.34)	(-0.14)	(-0.45)	(-2.27)
Pollution in 1970: Air Quality Index	0.000337	0.000483	-0.00111	-0.0527*
	(1.18)	(1.42)	(-0.15)	(-2.45)
Median family income	0.0000481**	0.0000402	-0.000291	-0.00154*
	(-3.10)	(-0.64)	(-0.67)	(-2.20)
Median years of schooling, ages 25+	-0.0398	-0.0541	-0.118	-0.773*
	(-1.16)	(-0.91)	(-0.63)	(-2.48)
Share of pollution-related deaths	6.937***	7.011***	7.105***	7.096***
	(-5.10)	(-4.85)	(-4.77)	(4.89)
(Treatment)*(Income)		0.0000045	0.000293	0.00138*
		(-0.07)	(-0.69)	(2.05)
(Treatment)*(Schooling)		0.024	0.0335	0.631*
		(-0.47)	(-0.19)	(2.12)
(Treatment)*(Pollution)		-0.000242	-0.000736	0.0449*
		(-0.89)	(-0.10)	(2.18)
(Income)*(Schooling)			0.0000197	0.000132*
			(-0.63)	(2.28)
(Income)*(Pollution)			0.00000084	0.00000960*
			(-0.77)	(-2.47)
(Schooling)*(Pollution)			-0.000351	0.00440*
			(-0.35)	(-2.09)
(Treatment)*(Income)*(Schooling)			-0.0000157	-0.000115*
			(-0.52)	(-2.07)
(Treatment)*(Income)*(Pollution)			-0.00000089	-0.00000841*
			(-0.80)	(-2.23)
(Treatment)*(Schooling)*(Pollution)			0.000564	-0.00373
			(-0.53)	(-1.80)
(Income)*(Schooling)*(Pollution)				-0.000000795*
				(-2.38)
(Treatment)*(Income)*(Schooling)*(Pollution)				0.000000697*
				(-2.14)
Interactions	NO	NO	YES	FULL
Observations	914	914	914	914
R-square	0.192	0.193	0.194	0.197

Notes: Income denotes median family income; schooling denotes median years of schooling for persons 25 years old and up; pollution denotes the 1970 Air Quality Index reported by the EPA. All columns have annual trend, trend by State, controls and district fixed effects. Controls include death rate, share of pollution-related deaths, population, population density, share of urban population, share male labor force, share labor force employed in agriculture, median age (years), labor force working outside county of residence, share using public transport to work, share of occupied housing units with 1 automobile, manufacturing establishments with 100+ employees, share of non-white population and value of chemicals used for insect control on livestock. This table contains my complete sample of environmental issues discussed in the House of Representatives from January 1955 through January 1971. Standard binary vote count: Y/PY/AY=1, N/PN/AN=0. I present t-statistics corresponding to robust and district-clustered standard errors in parentheses.
*p $<$ 0.05, **p $<$ 0.01, ***p $<$ 0.005.
Source: ICPSR 1955–1971.

Table 3.7: Intensity of treatment for all environmental issues, House of Representatives

	Baseline		Heterogeneity	
	1970 pollution & cause of death	Cause of death	1970 pollution & cause of death	Cause of death
	(1)	(2)	(3)	(4)
Treatment intensity	0.00204**	-0.000279		
	(3.25)	(-0.85)		
1970 Air Quality Index	0.000345			
	(1.20)			
Share of pollution-related deaths	6.880***	2.542***	7.185***	
	(5.08)	(7.06)	(4.99)	
Median family income	0.0000476**	0.0000174**		
	(3.06)	(2.60)		
Median years schooling, persons 25+	-0.0395	-0.0123		
	(-1.15)	(-1.01)		
(Treatment intensity)*(Income)			0.0000188*	-0.0000003
			(2.56)	(-0.14)
(Treatment intensity)*(Schooling)			0.00981**	-0.000981
			(3.26)	(-1.16)
(Treatment intensity)*(Pollution)			0.000641**	0.210
			(2.81)	(1.44)
(Treatment intensity)*(Income)* (Schooling)*			-0.00000166**	5.95e-08
			(-2.79)	(0.33)
(Treatment intensity)*(Income)*(Pollution)			-0.000000112**	-0.0000363
			(-2.62)	(-0.87)
(Treatment intensity)*(Schooling)*(Pollution)			-0.0000563**	-0.0201
			(-2.62)	(-1.40)
(Treatment intensity)*(Income)*(Schooling)*(Pollution)			9.75e-09**	0.00000399
			(2.74)	(1.12)
State F.E.	YES	NO	YES	NO
District F.E.	NO	YES	NO	YES
Observations	914	4,720	914	4,720
R-square	0.405	0.589	0.445	0.592
Adj. R-square	0.274	0.442	0.301	0.442

Notes: Treatment intensity is computed as the interaction between the post-1962 indicator, and and the share of TV-owning households by county in 1960. Income denotes median family income; schooling denotes median years of schooling for persons 25+; pollution denotes either the 1970 Air Quality Index reported by the EPA or the share of pollution-related deaths as indicated in the headline. All columns contain state-specific trends and time-varying controls. I control for death rate, share of pollution-related deaths, population, population density, share of urban population, share of male labor force, share of labor force employed in agriculture, median age (years), share of labor force working outside county of residence, share using public transport to work, share of occupied housing units with one automobile, manufacturing establishments with 100+ employees, share of non-white population, and value of chemicals used for insect control on livestock. This table contains my complete sample of environmental issues discussed in the House of Representatives from January 1955 through January 1971. Standard binary vote count: Y/PY/AY=1, N/PN/AN=0. I present t-statistics corresponding to robust and district-clustered standard errors in parentheses.
*$p < 0.05$, **$p < 0.01$, ***$p < 0.005$.
Source: ICPSR 1955–1971.

Table 3.8: Intensity of treatment for water-related environmental issues, House of Representatives

	Baseline		Heterogeneity	
	1970 pollution & cause of death	Cause of death	1970 pollution & cause of death	Cause of death
	(1)	(2)	(3)	(4)
Treatment intensity	0.00409**	0.00394***		
	(2.76)	(5.38)		
1970 Air Quality Index	-0.000323			
	(-0.93)			
Share of pollution-related deaths	-0.110	-1.086*	-0.549	
	(-0.05)	(-2.07)	(-0.26)	
Median family income	-0.0000349	-0.0000324*		
	(-1.52)	(-2.21)		
Median years schooling, persons 25+	-0.0355	0.00782		
	(-0.63)	(0.52)		
(Treatment intensity)*(Pollution)			0.000907**	-0.0307
			(1.96)	(-0.10)
(Treatment intensity)*(Income)			0.0000161*	0.00000011
			(2.33)	(0.04)
(Treatment intensity)*(Schooling)			0.0125**	0.00159
			(2.96)	(1.15)
(Treatment intensity)*(Income)*(Schooling)			-0.00000165**	-0.00000012
			(-2.88)	(-0.46)
(Treatment intensity)*(Income)*(Pollution)			-0.000000132**	0.0000175
			(-3.09)	(0.27)
(Treatment intensity)*(Schooling)*(Pollution)			-0.0000855**	-0.00438
			(-3.01)	(-0.17)
(Treatment intensity)*(Income)* (Schooling)* (Pollution)			1.28e-08***	-0.00000021
			(3.37)	(-0.04)
State F.E.	YES	NO	YES	NO
District F.E.	NO	YES	NO	YES
Observations	401	1,917	401	1,917
R-square	0.365	0.584	0.403	0.590

Notes: Treatment intensity is computed as the interaction between the post-1962 indicator, and and the share of TV-owning households by county in 1960. Income denotes median family income; schooling denotes median years of schooling for persons 25+; pollution denotes either the 1970 Air Quality Index reported by the EPA or the share of pollution-related deaths as indicated in the headline. All columns contain state-specific trends and time-varying controls. I control for death rate, share of pollution-related deaths, population, population density, share of urban population, share of male labor force, share of labor force employed in agriculture, median age (years), share of labor force working outside county of residence, share using public transport to work, share of occupied housing units with one automobile, manufacturing establishments with 100+ employees, share of non-white population, and value of chemicals used for insect control on livestock. This table contains a subsample of water-related environmental issues discussed in the House of Representatives from January 1955 through January 1971. Standard binary vote count: Y/PY/AY=1, N/PN/AN=0. I present t-statistics corresponding to robust and district-clustered standard errors in parentheses.

*$p < 0.05$, **$p < 0.01$, ***$p < 0.005$.

Source: ICPSR 1955–1971.

Table 3.9: Baseline specification for all environmental issues, alternative time periods, House of Representatives

	01/03/1959– 01/03/1967 (± 2 Congresses)			01/01/1960–12/31/1965 (± 3 years)		
	(1)	(2)	(3)	(4)	(5)	(6)
POST *Silent Spring*	0.343***	0.0874*	0.0882*	0.662***	0.196**	0.196**
	(-4.01)	(-2.25)	(-2.10)	(-3.54)	(-3.22)	(-2.66)
Observations	643	3152	3152	366	1916	1916
R-square	0.205	0.127	0.181	0.205	0.0692	0.183
District fixed effects	NO	NO	YES	NO	NO	YES

Notes: All models contain state-specific trends and time-varying controls for: death rate, share of deaths associated with pollution (pneumonia/cancer/food poisoning), population, population density, share of urban population, median family income, median years of schooling for persons 25+, share of male labor force, share of labor force employed in agriculture, median age (years), share of labor force working outside county of residence, share using public transport to work, share of occupied housing units with one automobile, manufacturing establishments with 100+ employees, share of non-white population, and value of chemicals used for insect control on livestock. This table includes the complete sample of environmental issues discussed in the House of Representatives from January 1955 through January 1971. Standard binary vote count: Y/PY/AY=1, N/PN/AN=0. I present t-statistics corresponding to robust and district-clustered standard errors in parentheses.
*p < 0.05, **p < 0.01, ***p < 0.005.
Source: ICPSR 1955–1971.

Table 3.10: Placebo test using military issues, baseline and heterogeneity, House of Representatives

	Baseline		Heterogeneity	
	(1)	(2)	(3)	(4)
POST *Silent Spring*	0.259***	0.264***	0.347	-0.402
	(-8.10)	(-20.15)	(-0.19)	(-1.07)
(Treatment)*(Income)			0.00000239	0.0000722
			(0.01)	(0.76)
(Treatment)*(Schooling)			0.0279	0.0830*
			(0.17)	(2.05)
(Treatment)*(Pollution)			0.00158	-1.996
			(0.13)	(-0.35)
(Treatment)*(Income)*(Schooling)			-0.00000581	-0.00000875
			(-0.20)	(-0.96)
(Treatment)*(Income)*(Pollution)			-0.00000113	0.000713
			(-0.51)	(0.41)
(Treatment)*(Schooling)*(Pollution)			0.000197	0.142
			(0.16)	(0.23)
(Treatment)*(Income)*(Schooling)*(Pollution)			5.12e-08	-0.0000881
			(0.26)	(-0.54)
District fixed effects	NO	YES	YES	NO
Controls	YES	NO		
Observations	3625	19286	3625	19286
R-square	0.119	0.0932	0.129	0.0962

Notes: Income denotes median family income; schooling denotes median years of schooling for persons 25 years old and up; pollution denotes the 1970 Air Quality Index reported by the EPA (in odd columns) or share of pollution-related deaths (in even columns). All specifications include annual trend, trend by State, and time-varying controls (the same as in the main regressions). This table includes only military-related environmental issues discussed in the House of Representatives from January 1955 through January 1971. Standard binary vote count. I present t-statistics corresponding to robust and district-clustered standard errors in parentheses.

*p < 0.05, **p < 0.01, ***p < 0.005.

Source: ICPSR 1955–1971.

Table 3.11: Placebo test using military issues, intensity of treatment

	Baseline		Heterogeneity	
	(1)	(2)	(3)	(4)
Treatment intensity	0.00272***	0.00287***		
	(7.74)	(19.07)		
(Treatment intensity)*(Income)			-0.00000032	-0.0000001
			(-0.08)	(-0.10)
(Treatment intensity)*(Schooling)			0.0000867	0.000506
			(0.05)	(1.18)
(Treatment intensity)*(Pollution)			0.0000633	-0.0603
			(0.43)	(-0.86)
(Treatment intensity)*(Income)*(Schooling)			-3.37e-08	-4.37e-10
			(-0.10)	(-0.00)
(Treatment intensity)*(Income)*(Pollution)			-1.96e-08	0.0000173
			(-0.78)	(0.92)
(Treatment intensity)*(Schooling)*(Pollution)			-0.00000172	0.00688
			(-0.12)	(0.96)
(Treatment intensity)*(Schooling)*(Pollution)			1.17e-09	-0.00000237
			(0.52)	(-1.37)
District fixed effects	NO	YES	NO	YES
Observations	3625	19286	3625	19286
R-square	0.117	0.0916	0.128	0.096

Notes: Treatment intensity is computed as the interaction between the post-1962 indicator, and and the share of TV-owning households by county in 1960. Income denotes median family income; schooling denotes median years of schooling for persons 25 years old and up; pollution denotes the 1970 Air Quality Index reported by the EPA (in odd columns) or share of pollution-related deaths (in even columns). All specifications include state-specific trends and time-varying controls (the same as in the main regressions). This table includes only military issues discussed in the House of Representatives from January 1955 through January 1971. Standard binary vote count. I present t-statistics corresponding to robust and district-clustered standard errors in parentheses.

*p < 0.05, **p < 0.01, ***p < 0.005.

Source: ICPSR 1955–1971.