THREE ESSAYS ON APPLIED MICROECONOMICS

A Dissertation

by

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ABSTRACT

This dissertation presents three essays on the effects of different institutions, technologies, and shocks on health, education, labor and information outcomes using experimental and quasi-experimental research designs. Specifically, I consider the effects of social media, vaccination, and natural resources.

In the first essay "The Economic Effects of Facebook", joint work with Mofioluwasademi Odunowo, Trent McNamara, Xiongfei Guo, and Ragan Petrie, we study the effects of Facebook on news awareness, subjective well-being, and daily activities. We use a large field experiment with a validated Facebook restriction to document the value of Facebook to users and its causal effect on news consumption and awareness, well-being, and daily activities. Those who are off Facebook for a week reduce news consumption, are less likely to recognize politically-skewed news stories, report being less depressed and engage in healthier activities. One week of Facebook is worth $67, and this increases by 19.6 percent after experiencing a Facebook restriction.

In the second essay "Vaccines at Work", joint work with Manuel Hoffmann and Adrian Chadi, we study how behavioral factors can affect the effectiveness of flu vaccination. Flu vaccination could be a cost-effective way to handle the costs of this disease, but low take-up rates, particularly of working adults, and vaccination unintendingly causing moral hazard may decrease its benefits. We ran a natural field experiment with employees of a large bank in Ecuador where we experimentally manipulated incentives to participate in a flu vaccination campaign. We find that reducing the opportunity costs of vaccination increased take-up by 112 percent. Also, we find that the effect of vaccination on health outcomes is a precise zero with no measurable health externalities from coworkers. Using administrative records on sickness diagnoses and surveys, we find evidence consistent with vaccination causing moral hazard.

In the third essay "A Blessing or a Curse? The Long-term Effect of Resource Booms

on Human Capital and Living Conditions", I study if resource booms can reduce human capital accumulation. These booms can increase the opportunity costs of education by favoring low-skill jobs, which makes it optimal for some cohorts to interrupt their education. If these individuals do not resume their education, they may lose pecuniary and non-pecuniary benefits of education in their lifetime. For a country, lower human capital may constrain its long-term growth. I use proprietary individual-level data to study the long-term effects of exposure to the 1970s oil boom on human capital in a developing country. I exploit variation in the timing of the shock and geographic differences in the cost of college attendance and find that exposure to the boom decreased college completion and increased low-skill occupations - consistent with the idea that individuals shift into highly remunerative low skilled employment because the boom decreased college education returns. In line with this, I find no effects on wealth accumulation.

DEDICATION

To Maria Jose, my wife, for her constant support and help in everything. To my parents Alexis and Rocio, for always pushing me to be better.

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The analyses depicted in Section 2 were conducted in part by Mofioluwasademi Odunowo, Trent McNamara, Xiongfei Guo, and Ragan Petrie of the Department of Economics; the analyses depicted in Section 3 were conducted in part by Manuel Hoffmann of the Department of Economics and Adrian Chadi of Department of Economics of the University of Konstanz in Germany.

All other work conducted for the dissertation was completed by the student independently.

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1. INTRODUCTION

The efficient design of public policy requires knowing how institutions, technologies, and resource shocks affect outcomes of interest to society. In many cases, policymakers, academics, and society, in general, are not aware of how institutions affect daily life and how technologies or discoveries could have unintended consequences that affect living conditions and welfare in countries. With this general objective in mind, in this dissertation, I use both experimental and quasi-experimental methods from economics to study three issues that directly affect living conditions in many countries across the world.

In Section 2, together with Mofioluwasademi Odunowo, Trent McNamara, Xiongfei Guo, and Ragan Petrie, we study the causal effect of Facebook on daily life, captured by news awareness, subjective wellbeing, daily activities related to mood, and Facebook's value to its users. Social media usage has increased dramatically over the last 15 years, and Facebook has dominated the market. Facebook impacts components that go beyond building social networks and providing various forms of information. Some of these impacts are beneficial while others are harmful. Importantly, we do not know how people perceive and balance these costs and benefits. Because of this, Facebook's monetary value is critical to understand the total utility an individual receives from the platform. In this sense, knowing how much individuals value Facebook would be an essential measure for policymakers to address unintended negative consequences of Facebook use throughout many different aspects of daily life. Therefore, Facebook's monetary value along with its effects on news awareness and well-being is an important but under-researched aspect of the 21st century.

We used a field experiment with a randomized and validated Facebook restriction to investigate how Facebook may affect daily activities and news exposure and quantified how much users value access to it. Using an incentive-compatible procedure, we find that that one week of Facebook is worth about $67 to users, with a median value of $40. Regarding news, we find that Facebook is a major source of news exposure. Individuals restricted from Facebook are less aware of politically-skewed sources, and this is stronger for men than women. We show that a reduction in news consumption drives this result and that participants do not substitute towards other news sources or social media platforms when being off Facebook for a short period. Finally, regarding subjective well-being, we show no significant effect of using Facebook on overall life satisfaction. However, we do find a sizeable short-term reduction in feelings of depression when restricted from Facebook, especially for men. We build on existing research by studying the effect of Facebook on behaviors correlated with mood. We find that individuals restricted from using Facebook engage in healthier activities.

In Section 3, together with Manuel Hoffmann and Adrian Chadi, we study how economic factors affect working adults' decision to vaccinate, the effects of vaccination on health and whether flu vaccination can cause moral hazard. Seasonal influenza causes substantial morbidity and mortality every year around the world, and the flu vaccine can potentially be a cost-effective way to reduce its costs. However, individual behavior can counter the potential benefits of vaccination in two ways. First, vaccination rates in most countries of the world are substantially below the levels that could prevent epidemics. Second, vaccinated individuals may overestimate the protection that the vaccine grants and engage in risky behaviors. Thus, moral hazard could counter the benefits of adopting a preventive medical technology like the flu vaccine.

We ran a natural field experiment in cooperation with a major bank in Ecuador using a random encouragement design. We find that randomly assigning employees to get vaccinated during the workweek, which decreases the opportunity costs of vaccination, increased take-up by 112 percent of an eight percent baseline. Then we use the random assignment to the workweek to identify both the effects of vaccination on health and potential peer effects within units. First, we find that if the proportion of peers that get vaccinated increases by ten percentage points, take-up increases by 7.9 percentage points, and find evidence that suggests that employees react to social norms. Next, we study if flu vaccination is effective to improve working adults' health. We find no evidence that vaccination decreased the probability of getting sick due to the flu. While we cannot rule out that the vaccine did not match the prevailing strands of the flu virus, we explore if getting vaccinated can unintendingly cause moral hazard, which might independently decrease the effectiveness of the vaccine. We find that vaccinated individuals are less likely to go to the doctor when they feel the symptoms of the flu and that they forgo measures believed to protect against the flu. These results suggest that getting vaccinated can create a moral hazard problem that could reduce the effectiveness of flu vaccination.

In Section 4, I study if resource booms can reduce human capital accumulation in the context of developing countries. While we would expect that natural resources boost economic development, there is ample suggestive evidence that resource-rich countries tend to underperform in several dimensions. Of particular concern is the possibility that resource booms reduce human capital accumulation. These booms may affect labor market conditions favoring low-skill occupations. Standard human capital accumulation models show that an increase in productivity in low-skill occupations increases the opportunity cost of going to college and decreases the returns of education. Thus, during a resource boom, it might be optimal for some individuals to drop out of high school/college and enter the workforce. However, economic theory does not predict whether these effects are temporary or permanent. A permanent decrease in education could constraint wealth accumulation, decrease positive externalities of education, and limit a country's growth potential.

I use proprietary individual-level data to causally estimate the long-term effect of the 1970s' oil boom on educational attainment in the context of a developing country. In 1973, Ecuador started major oil production, and its price skyrocketed due to the Arab embargo. This boom increased productivity in low-skill occupations. I estimate the reduced form effects of exposure to this shock on college completion measured 40 years after the oil boom using an intensity difference-in-differences design. This design compares changes in outcomes across cohorts of individuals who turned 18 before and after 1973, to changes in outcomes across geographic regions with different costs of college attendance. I find that in the most affected cities, exposure to the oil boom before turning 18 decreased college completion by 2.9 percentage points, which represents 12.2 percent of baseline college completion for those who turned 18 just before the oil boom. Also, there is no effect in terms of wealth accumulation, which suggests that the long-term reduction of college completion is consistent with a model of rational individuals who reduce their educational attainment in response to lower returns of education in the long-run. While the results suggest that it was optimal for the exposed cohorts to interrupt their educational attainment, this does not necessarily imply that the boom was a blessing because it does not account the positive externalities of education. In particular, I find that exposure to the oil boom before turning 18 increased the number of children in the largest cities by 0.04 (1.7 percent of the baseline). This estimate, together with no apparent effect on wealth, suggests fewer resources per children that together with less educated parents may have affected their development. From the country's perspective, lower human capital levels may constrain the development of high-skill industries, which may hamper the country's long-term growth potential.

2. THE ECONOMIC EFFECTS OF FACEBOOK

2.1 Introduction

Social media usage has increased dramatically over the past decade, and Facebook has dominated the market. Almost 2.2 billion individuals worldwide have an active Facebook account, and nearly 1.4 billion log on daily (Facebook, 2017) for an average of 50 minutes per day (Facebook, 2016). Facebook not only provides means to connect with friends and build social networks and capital (Bailey et al., 2018b; Mayer and Puller, 2008; Cramer and Inkster, 2017), but it is also exposes users to a vast amount of information and news. Despite the potential influence of Facebook on an individual's behavior via information and content provision, there is surprisingly little known about its direct and comprehensive effects on news exposure and awareness, subjective well-being and day-to-day activities.

Facebook's platform has several characteristics that lend well to investigating its effects on an individual's exposure to news content as well as its impact on well-being. The platform consolidates information from many sources, making it an important and compelling place to go on the internet to keep up with news. People tap into Facebook for local, national and international news. Indeed, roughly two-thirds of Americans get at least some of their news from social media sources (Pew Research Center, 2017). While there is a concern that news transmitted through social media could be fake or skewed and affect political outcomes (Allcott and Gentzkow, 2017), these type of platforms could also serve to uncover corruption (Enikolopov et al., 2016). As individuals rely more on social media and news aggregators as a primary source of information, segregation may increase (Gentzkow and Shapiro, 2011) and voting behavior can be affected (DellaVigna and Kaplan, 2007; Bond et al., 2012; Martin and Yurukoglu, 2017). The consequences of this in terms of news awareness and biases - highlighted by political investigations regarding Facebook's involvement in the 2016 U.S. Presidential Election - are largely unknown.

More broadly, there is little consensus on Facebook's impact on well-being, especially in the context of daily behaviors and activities. Facebook is often used to connect with friends and family, organize events and share information and photos (Laroche et al., 2012; De Vries et al., 2012; Ashley and Tuten, 2015; Lee and Ma, 2012; Bailey et al., 2018a). Being able to seamlessly keep in touch with others might improve mood and happiness, but it might also induce negative emotions and habits from social comparison (Tromholt, 2016; Deters and Mehl, 2013). How Facebook directly affects well-being and mood in general and the correlation with daily activities is unclear.

Facebook's platform is provided for free to users and paid for by advertising, so the monetary value to users, as reflected in a market price, is untested. The platform facilitates building social networks and seamless access to relevant information. Usage rates, both in frequency and intensity, suggest this provides benefits to users. While the economic impact of Facebook on advertising has been estimated, the benefits to users and impact on behavior have been given more limited study.[1] Knowing the value of Facebook would inform an understanding of welfare effects and provide a monetary measure of the importance of Facebook to users.

We run a field experiment in the Spring of 2017 with a randomized, and validated, Facebook restriction to investigate how Facebook may affect daily activities and news exposure and quantify how much users value access. In total, 1,769 individuals from a large U.S. university participated in the study. Using an incentive-compatible procedure (Becker et al., 1964), we asked participants how much they would need to be paid to not use Facebook for one week. Qualified participants were then randomly assigned to either a one-week Facebook restriction group or a control group that faced no restriction.

Our design has several important and unique features worth noting. First, we can exploit the rich data collected on the distribution of Facebook's value to check for possible selection effects in our results. Second, we enforced and validated the restriction by logging partici-

[1] It is estimated that the impact of Facebook through advertising is $77.6 billion in the U.S. (Deloitte, 2015). Evidence on the value of Facebook is given in Brynjolfsson et al. (2018).

pants off Facebook and verified treatment compliance using an unobtrusive online monitoring procedure throughout the week. Our procedure was undetectable to the participant and did not involve direct contact which could potentially impact behavior. Finally, participants completed two surveys, the first prior to random assignment and a second survey one week later. These surveys were designed to provide a comprehensive view of behavior and measure the short-term effects of Facebook on news awareness and consumption, well-being, time allocation of daily activities and daily activities.

We have several key results. First, our study reveals that one week of Facebook is worth about $67 to users, with a median value of $40. This value is in line with other studies (Brynjolfsson et al., 2018; Corrigan et al., 2018; Allcott et al., 2019; Sunstein, 2018; Herzog, 2018), and represents a significant portion of a typical university student's weekly budget and expenses (roughly 30 percent according to Flood et al. (2017)).[2] Individuals place a nontrivial value on Facebook usage, and consistent with addiction or the compounding loss of information, the value increases 19.6 percent after not being able to use it for one week.

Second, our data document that Facebook is an important source of news exposure. Individuals restricted from Facebook are less aware of politically-skewed sources, and this is stronger for men than women. We show that this result is driven by a reduction in news consumption, and that participants do not substitute towards other news sources or social media platforms when being off Facebook for a short period of time. The causal estimates show that Facebook is an important conduit for news from non-mainstream outlets, and this echoes the findings of Allcott and Gentzkow (2017) who show that social media is correlated to the distribution of "fake news." Our results provide additional evidence that Facebook plays an important role in the acquisition of information by affecting what news is available to consume and thus an individual's ability to assess its veracity.

Third, our findings contribute to the literature that focuses on Facebook's effect on happiness and well-being. Early studies found mostly positive effects of social media on subjec-

[2]Participants in our study face a one in two chance of experiencing a Facebook restriction, and this may reduce bias in value estimates when using elicitation mechanisms coupled with implementation uncertainty.

tive well-being, perhaps through enhanced engagement, in cross-sectional studies (Ellison et al., 2007; Valenzuela et al., 2009; Gonzales and Hancock, 2011; Kim and Lee, 2011) and laboratory experiments (Sagioglou and Greitemeyer, 2014; Vogel et al., 2015; Verduyn et al., 2015). More recent studies have found mixed results using panel data (Shakya and Christakis, 2017) and Facebook use limitations (Tromholt, 2016).[3] Cross-sectional evidence on the effect of Facebook on depression is mixed. Feinstein et al. (2013) finds depressive feelings are driven by negative outcomes from social comparison, but other studies find no relationship between Facebook and depression (Steers et al., 2014; Jelenchick et al., 2013; Tandoc et al., 2015). We contribute to this literature by using a randomized and verified Facebook restriction and show no significant effect of using Facebook on overall life satisfaction. However, we do find a large short-term reduction in feelings of depression when restricted from Facebook, especially for men.

Finally, we build on existing research by studying the effect of Facebook on behaviors largely found to be correlated with mood. We find that individuals restricted from using Facebook engage in healthier activities. While our design does not allows us to recover the underlying mechanism, this finding is consistent with research in psychology (Salovey et al., 2000; Ostir et al., 2000; Fredrickson and Joiner, 2002; Blake et al., 2009; Kettunen, 2015; Newman et al., 2014; Sonnentag, 2001) that better mood is positively correlated with engagement in healthier behaviors.

Overall, the effects our study finds on news awareness, feelings of depression and daily activities show that Facebook has significant effects on important aspects of life not directly related to building and supporting social networks. Furthermore, almost two years after our experiment, Allcott et al. (2019) find similar results for news awareness and subjective well-being for a different population, which supports our findings. The effects of Facebook are far reaching, and our results provide a more comprehensive documentation of these impacts on the daily life of users. They seem to understand this and place a substantial value on the

[3] Tromholt (2016) uses a one-week, self-enforced Facebook restriction and finds a positive effect on overall life satisfaction.

experience that Facebook provides.

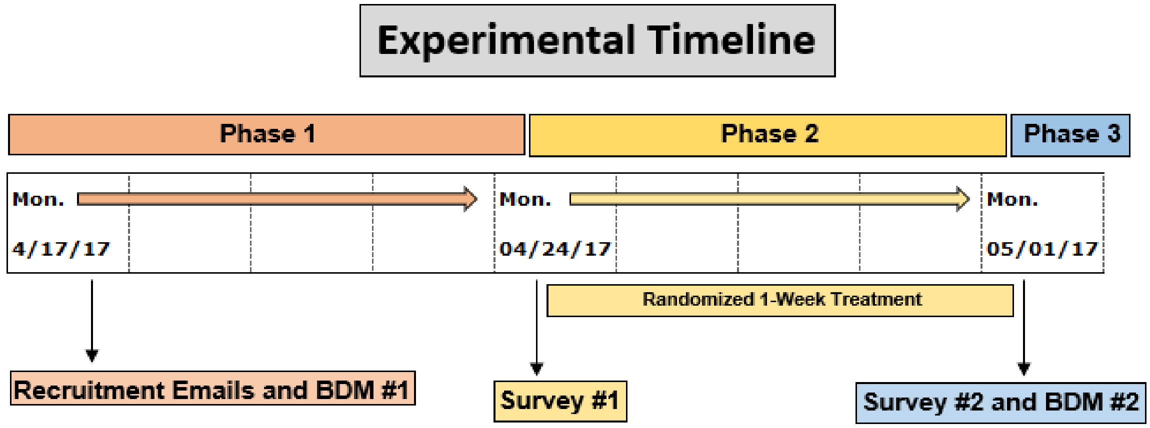
The paper is organized as follows. Section 2 describes the study design and implementation. Section 3 reports results on the value of Facebook to users and the effect of the Facebook restriction on news awareness, subjective well-being and activities. Section 4 continues with robustness checks on our main findings. Section 5 concludes.

2.2 Study Design

A direct approach to analyze the causal effects of Facebook on daily life would be to take the population of Facebook users, randomly restrict usage for some and not others and then examine behavior across the restricted and not restricted groups. This is difficult to achieve, however, absent a random event that blocks some comparable users from accessing Facebook for period of time and not others and then identifying those users to examine behavior. As an alternative, we adopt an approach where we recruit volunteers and then randomize a Facebook restriction among them. While feasible to implement, a challenge is the representativeness of the generated sample. Simply asking for volunteers willing to give up Facebook would likely result in a sample of low-value individuals. To address this issue, we collect additional information from our volunteers that allows us to account for this type of selection. Rather than merely asking for volunteers, we elicit an individual's value of Facebook for one week and then use the distribution of stated values to test if selection affects the results.

Our study occurs in three major phases, as outlined in Figure 2.1. In Phase 1, we elicit an individual's value of using Facebook for one week and recruit qualified participants into the Facebook restriction. In Phase 2, we administer a pre-treatment survey and then randomly assign participants into two groups – a group that experiences one week without Facebook and a group with no restriction. In Phase 3, participants return to complete a second survey and collect payments. In a surprise, we also re-elicit an individual's value of Facebook for one week. We ran this intervention between April and May 2017.

Figure 2.1: Timeline of Study Phases



2.2.1 Phase 1 - Recruitment and value of Facebook

We sent an invitation email to recruit participants. The email contained a short description of the study and a link to an online survey that asked basic demographic information, determined if the participant had a Facebook account (95 percent did) and elicited the participant's value for not using Facebook for one week.[4]

An individual's value of Facebook is revealed with the Becker-DeGroot-Marschak (BDM) mechanism (Becker et al., 1964) and determines eligibility for participation in subsequent phases of the study. The participant is asked to submit her value of one week of Facebook usage. A random counter offer is drawn and shown to the participant. If the participant's value is less than the counter offer, then the participant is eligible for the next phases of the study and would be paid the counter offer upon study completion. If the participant's value is higher than the random offer, then she is not eligible to participate in any of the subsequent phases of the study and does not receive payment. Several examples of how the procedure works were included in the instructions to make sure that participants understood the proce-

[4]The email text and online survey questions are in the Appendix, Sections A.1 & A.2.

dure prior to submitting a value.[5] To assure that reported values are not biased upwards, we follow the suggestion of Bohm et al. (1997) and leave the upper limit of the random offer unclear because that increases the validity of the BDM mechanism. This is implemented by informing participants that the minimum counter offer is $5 and the maximum is “our most reasonable estimate of the value of the time spent on Facebook.”[6]

All eligible participants were invited by email to attend the next phase (Phase 2) on Monday of the following week.[7] The email explained that the next phase involves completing a comprehensive survey and being randomly assigned to log off Facebook for one week. In addition, the participants were informed that they would need to come back a second time (one week later) to complete another survey and receive cash payments of the counter offer they received. The time and location of the session is indicated in the email, and participants confirm their attendance.

2.2.2 Phase 2 - Pre-survey and Facebook restriction assignment

Participants were required to show up in person to complete a short survey that collects information on social media usage, news awareness, consumption behavior, time allocation, and subjective well being (Appendix Sections A.3 and A.4). The questions on social media usage included time spent, frequency of postings and emotions felt while using the platform.

To capture news awareness, we tapped into a variety of news sources. In the week prior to the survey, we collected headlines from the front page of the eleven most popular newspapers as ranked by the Pew Research Center, including The New York Times, Washington Post, USA Today, Wall Street Journal, LA Times, New York Daily News, New York Post, Boston Globe, San Francisco Chronicle, The Chicago Tribune and The British Daily Mail. We used Breitbart as the source of skewed news.[8] There were no extraordinary news events during

[5]Our procedures made clear to participants that they would be paid the random offer upon study completion to mitigate any uncertainty bias (Horowitz, 2006).

[6]For budgetary reasons and expected participation rates, the random counter offers were drawn with the following probabilities: (5, 15.14%; 7, 15.14%; 9, 11.14%; 10, 11.14%; 12, 11.14%; 14, 11.14%; 16, 7.14%; 18, 6.14%; 20, 5.14%; 21, 5.14%; 24, 0.64%; 25, 0.64%; 28, 0.14%; 30, 0.14%). The expected offer is $11.58.

[7]Those who are ineligible for subsequent phases are not contacted.

[8]We chose Breitbart given that its internet traffic as of March 2017 surpassed other major skewed news

this period, like a mass shooting or major natural disaster, that might bias news knowledge. The participant is shown six headlines randomly chosen from the pool of mainstream sources and one randomly chosen from the skewed source and asked to identify if the event occurred or not. From the six mainstream sources, two headlines are changed slightly so as to make the headline false. All other headlines did appear on the front page of a newspaper or on Breitbart.[9]

Daily behavior is measured by presenting participants with a series of statements (e.g. "I save more money than I normally do", etc.) and asking them to identify on a scale of 1-5 whether they agree/disagree with the statement. Time allocation is measured with estimates for average time spent doing a variety of activities, such as working and exercising. Finally, our subjective well-being questions are constructed following the OECD Guidelines used to characterize the affective state of the respondent (OECD Better Life Initiative, 2013). These questions ask participants to respond on a 0-10 scale how frequently they feel a certain emotion (e.g. depression, happiness, etc.).

Upon completion of the survey, participants were randomly assigned to either a one-week Facebook restriction or no restriction based on the last digit of the participant's university-assigned ID number.[10] All participants complied with their assigned treatment and associated protocols.

The no restriction group is dismissed and asked to return the following Monday (one week later) to complete another survey and receive payment. The restriction group is required to log off of Facebook, and all its associated features, including Messenger, for one week. To validate compliance with the restriction, we created a Facebook account for the study and had treated participants become friends with our study account. As friends, we can monitor all access to their account through the "Last Active" feature in Facebook Messenger. This feature automatically updates as soon as someone logs on to Facebook, thus we

sources and was similar in magnitude to that of mainstream news sources such as The Washington Post according to data from alexa.com

[9]See the questionnaire in Appendix A.3

[10]The university randomly generates the last four digits of a student's ID number.

can validate if a participant complies or not with the restriction. A participant could go invisible, block or un-friend our Facebook account, but they would have to log in and we would observe this in our data. We saw no instances of this, and all participants complied with the restriction. After becoming friends with our Facebook account, participants logged off of all their active Facebook sessions on all their devices using Facebook's security settings. Finally, the restriction group was asked to return the following Monday (one week later) to complete another survey and receive payment.

2.2.3 Phase 3 - Post-survey and re-elicited value of Facebook

All participants returned one week later to complete another survey and receive payment. The survey is identical to the one given in Phase 2 and allows us to see how key indicators – social media use, news awareness and subjective well-being – have changed over the previous week.[11] After completing the survey, participants were instructed to go to a separate room for payment.

In the separate room, before receiving payment, we again elicited each participant's value for one week of Facebook usage. Up to this point, participants did not know they would again be asked their value of Facebook. This procedure gives us an unbiased measure of the change in Facebook's valuation following the restriction. We use the same BDM mechanism procedures as in Phase 1.[12] Afterwards, all participants receive a cash payment based on the counter offer from Phase 1 before leaving the session.

2.2.4 Implementation

Participants were recruited via email from a random sample of the undergraduate population at Texas A&M University during the Spring semester of 2017. Overall, 1,929 individuals initiated the Phase 1 online survey and 1,769 completed it, thus producing the distribution of

[11] We updated the news pool to reflect headlines from the previous week.

[12] Participants are asked to write down their valuation and informed that their payment today is unaffected by their response. Eligible participants from this second BDM go through the same process as in Phase 2, return for a third and final survey in one week, and are paid their counteroffers from the second BDM. We do not include this third survey in our estimates.

stated values used to estimate the value of Facebook and to test if selection affects results. When we compare the characteristics of the individuals who responded to the survey with the entire undergraduate population (based on year in school, home state and declared major), we find that our survey respondents are representative. Of those individuals who completed the Phase 1 survey, 562 were eligible for Phase 2 of the study, and eligibility does not depend on covariates.[13] Also, we find no evidence that participants who ended up being eligible or ineligible based on the randomly-drawn counter offer are different.[14]

All eligible participants were invited to Phase 2 of the study, and this session was held on main campus where participants came to complete the survey and be randomized into the Facebook restriction.[15] For the Phase 2 sessions, 167 participants showed up and completed the survey. Appendix Table A.1 shows the comparison between those who were eligible and showed up and those who did not. The only meaningful difference is that those who did not show up had a slightly lower value for Facebook.

Among the participants who completed the Phase 2 survey, fifty-four percent (n=90) were randomly assigned to the no restriction control group, and 46 percent (n=77) were assigned to the Facebook restriction treatment group. Comparing covariates of the control and treatment groups, we find there are significantly more women in the control group (71 percent) compared to the treatment group (57 percent), but otherwise, the two groups are balanced.[16] To address covariate differences by treatment assignment, our analysis controls for individual fixed effects so that treatment effects are identified through differences in changes in behavior before and after the one-week Facebook restriction across the treatment

[13]Eligibility for Phase 2 means that the submitted value was less than a randomly-selected counter-offer of no more than $30. This is by the design of the elicitation mechanism – so all those with submitted values higher than $30 were ineligible. Descriptive statistics for these groups are in Appendix Table A.1. In Section 2.4.2 of the paper, we test the robustness of the results to this design-induced selection.

[14]When we compare participants who submitted values less than or equal to $30, so they could have been eligible to participate in Phase 2, there is no significant difference by age or gender between those who ended up being eligible or ineligible based on the counteroffer. See Appendix Table A.1

[15]Participants were aware of this procedure prior to submitting their value of Facebook in the Phase 1 online survey. Holding this session on main campus minimizes travel costs that might have affected valuations for Facebook.

[16]Appendix Table A.2 shows the balance of covariates across the treatment and control groups.

and control groups.

After one week of treatment, 90 percent (n=151) of the participants from Phase 2 returned to complete the Phase 3 survey. There is no significant difference in covariates between the participants who returned for Phase 3 and those who did not, and attrition is not correlated with treatment status. Our monitoring process validates compliance with the restriction.[17] Those in the treatment group reduced their use of Facebook by 1.7 hours per day. Given a baseline Facebook usage of 1.9 hours per day, this illustrates that the treatment group complied with the restriction.

All sessions were completed in April-May 2017. Time to complete the Phase 1 online survey was approximately five minutes, and each subsequent in-person survey took about 10-15 minutes. Average payment to participants was $16.79 (s.d. $5.22) at the completion of Phase 3.

2.3 Results

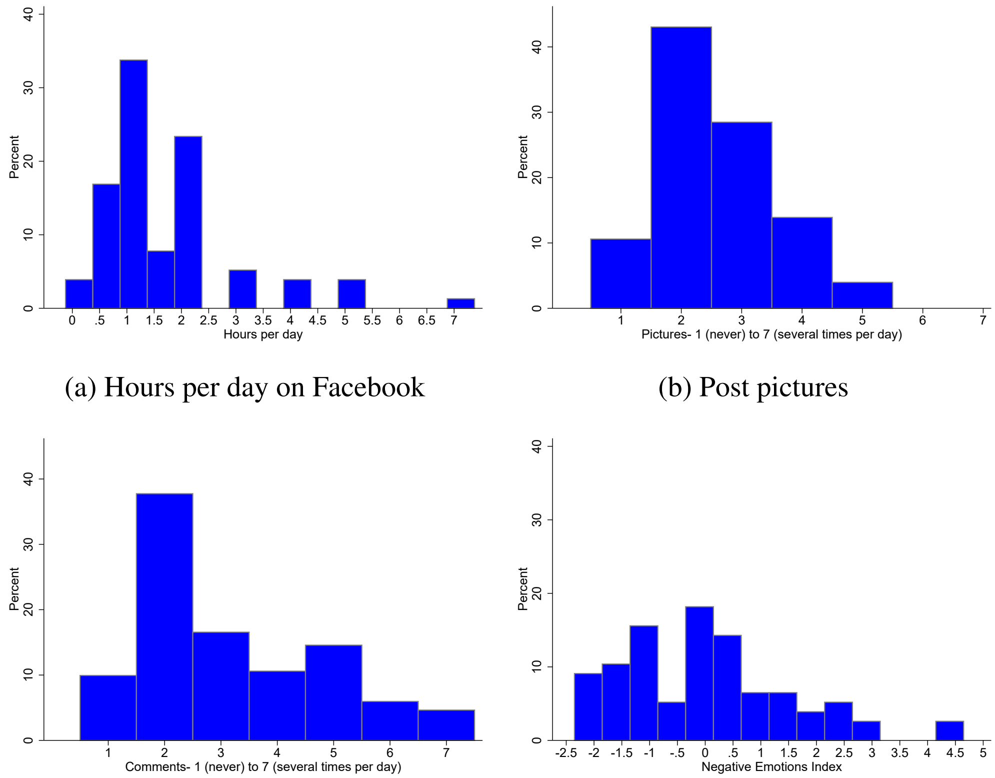
2.3.1 Description of the sample

In the baseline survey (Phase 2), participants report spending a mean of 1.9 hours per day on Facebook, including reading news feeds and news content (Figure 2.2, panel a). This is consistent with other surveys with college students that report an average of 2.6 hours spent on Facebook per day (EMarketeer, 2015), higher than the national average of 50 minutes per day on Facebook (Neilsen Company, 2016). Engagement on Facebook is measured by how often participants post pictures and comment. This activity was rated on a scale of 1 (never) to 7 (several times per day). About 52 percent never or rarely post pictures, 28 percent once or twice a month and the remainder post once a week or more (Figure 2.2, panel b). In terms of posting comments, 48 percent never or rarely comment, 18 percent once or twice a month and the remainder post once a week or more (Figure 2.2, panel c).

Other social media platforms are also used. On a daily basis, participants report spending close to two hours on Facebook, Snapchat and YouTube, over one hour on Instagram,

[17] Participants did not interact with the study account in any way.

Figure 2.2: Time Spent on Facebook and Facebook Usage



(a) Hours per day on Facebook

(b) Post pictures

(c) Post comments

(d) Negative Emotions Index

Notes: This figure presents descriptive statistics on Facebook usage. The x-axis in panels (b) and (c) represents: 1 never, 2 rarely, 3 1-2 times per month, 4 once a week, 5 2-4 times per week, 6 once a day, 7 several times per day.

less than one hour on Twitter, and very little on Tumblr and Vimeo.[18] This is consistent with the number of friends and followers reported across platforms. On average, there are more friends and followers on Facebook (641) and Instagram (452) than on Tumblr (87) and Twitter (182).

Information is also collected on where participants get their news and time spent acquiring news. Roughly, 15-30 minutes a day is spent reading or watching news, and most news is obtained from digital sources (e.g. online news, social media) as opposed to traditional outlets (e.g. cable tv, paper news, radio).[19] Participants reported their preferred news sources,

[18] Appendix Table A.3.

[19] Appendix Table A.3. While we cannot say what proportion of news participants get from Facebook, 81

and we rank each source's political bias on a scale of 1 (Left) to 5 (Right).[20] The average preferred news source has a political bias of 2.8 - slightly left of Center.

We further asked a variety of subjective wellbeing questions. On a scale of 0 (Never) to 10 (Very/Always), participants are generally satisfied with life (mean of 7.2) and responded with a mean of 3.4 to feelings of depression. These results are in line with the OECD's Better Life Initiative Survey for 2017 which reports an average overall life satisfaction score of 7.3.

Participants were asked to rate on a scale of 1 (never) to 6 (all the time) how often they felt certain negative emotions while using Facebook, such as envy/jealousy, loneliness, misery and annoyance. To generate a general measure of experiencing negative emotions while on Facebook, we take these four measures and combine them into a factor index that ranges from -2.35 to 4.37 using principal component analysis. A higher index indicates a participant feels more negative emotions (see Figure 2.2, panel d), and there is large variation in this index.[21]

2.3.2 Value of Facebook

According to the BDM lottery, participants report that one week of Facebook usage is valued at $24.84 on average ([23.02, 26.65] 95 percent confidence interval), and the median value is $15 ([12.70, 17.30] 95 percent confidence interval).[22] We evaluate how sensitive the mean is to outliers by trimming the distribution at $200, $100 and $50. With each cut, the mean BDM value changes to $22, $21 and $18, respectively. The median BDM value remains fixed at $15 with each cut of the distribution.[23] There is no bunching at $5 which

percent report opening up Facebook every day or several times a day to check their news feed.

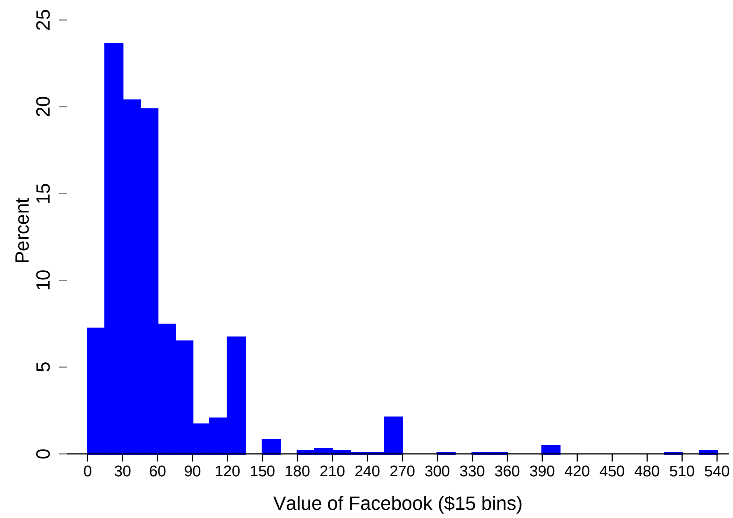
[20]We use the rankings on www.allsides.com. If a participant lists a news outlet that is not reported on allsides.com, we treat their preferred news outlet as missing. The top five first choice sources are CNN (28.1 percent), FOX (12.6 percent), BBC (8.3 percent), NYT (4.7 percent), and ESPN (4.7 percent). Breitbart was not listed as a first choice, however, news from this source could appear on a Facebook news feed.

[21]Appendix Figure A.1 shows the distribution of these emotions separately.

[22]We calculate the confidence intervals using bootstrap with 1000 replications.

[23]Our design also explored the willingness to pay (WTP) - willingness to accept (WTA) gap in the BDM mechanism (see Knetsch et al. (2001), Plott and Zeiler (2005), Horowitz (2006), and Brynjolfsson et al. (2018) for a discussion of this phenomenon). Half of the participants were asked the value in terms of selling participation in the study (WTA), "How much money would you need to be given to stop using Facebook for a week?" and half were asked in terms of purchasing participation (WTP), "What is the value of your weekly time on Facebook?" We find no significant difference in the reported value of Facebook from either solicitation method or by covariates across groups, so we pool the data in our analysis.

Figure 2.3: Distribution of the Value of Facebook (trimmed at $540)



indicates that participants did not try to manipulate the BDM mechanism to be eligible for the next stage of the study.

Our experiment introduces a lottery in which an individual has a 50 percent chance of being restricted from Facebook. This means that the reported BDM is underestimates Facebook's value. To account for this lottery, we make adjustments to obtain valuations. Under risk neutrality, the mean value of Facebook is $50 per week (median=$30) and $200 per month (median=$120). If individuals are risk averse, then we assume a CRRA utility function with a risk aversion parameter within a reasonable range (0.1-0.3), then the mean value of Facebook is $67 per week (median=$40) and $267 per month (median=$160). From here on, we report values adjusted for risk aversion. Figure 2.3 shows the distribution of these values.[24] By comparison, Brynjolfsson et al. (2018) and Corrigan et al. (2018) find lower weekly median values ($3.92 and $15 respectively), and Allcott et al. (2019) find median monthly values ($100-$180) similar to ours.[25]

[24] The distribution is trimmed at $540 because of a few outliers in the data – the maximum value is $2,153. We use the nontrimmed, full sample in our analysis.

[25] There are differences in our studies. Brynjolfsson et al. (2018) use an online sample, one out of every 200 participants are randomized into the Facebook restriction, and respondents who do not use Facebook are not screened out for their weekly estimate. Corrigan et al. (2018) use a series of second price auctions with a many

According to the Pew Research Center (2016), women are 8 percentage points more likely to use Facebook than men. Hence, we might expect to see differences in the value of Facebook across genders, however, we do not find a statistically significant difference. On average, one week of Facebook is worth $69.35 for men (median=$43.07) and $65.18 for women (median=$40.38). We also test for difference in the distributions of the value by gender and find no significant difference.

There is a positive correlation between the value of Facebook and age in our data. For those aged 21 years or younger, one week of Facebook is worth $62.95 (median=$40.38), while for those older than 21 years, Facebook is worth $78.37 (median=$53.84). This could reflect differences in income or that younger participants are more likely to use other social media. Indeed, those 21 years and younger spend more time on Twitter and Snapchat and have more Instagram followers.[26]

The value of Facebook changes across user types, with those who are more active reporting higher values. Facebook is worth 20 percent more for participants who use it for more than one hour a day and for those who post at least once per month. There is a positive, but not significant, correlation between the value of Facebook and having a large number of friends on Facebook, however, there is a positive and significant correlation between the value and having a large number of friends on other social media platforms. Those with a large number of friends on other social media also have a lot of friends on Facebook, so this likely reflects the larger value that active Facebook users place on using the platform. There is a negative correlation between feeling depressed or experiencing negative emotions while on Facebook and the value of Facebook, but these correlations are not significant.[27]

To put some perspective on the magnitude of the stated values of Facebook in our sample,

populations, using differing compensation schemes. Allcott et al. (2019) similarly use a BDM mechanism but on an online population.

[26]We did not ask questions on income but asked the zip code of where the participant lived at age 15. Using income data from this zip code, we find no significant difference in mean income for younger participants compared to older.

[27]Appendix Table A.4 presents the Pearson correlation coefficients between the value of Facebook and several measures that characterize Facebook users.

we compare its value with college students' mean income and some common expenses. The weekly average income of a college student is $224.28 (Flood et al., 2017), so a week of Facebook usage is worth 30 percent of income.[28] In addition, university students spend roughly $14 in clothing, $14 in personal care and $11.50 in technology (devices, plans and subscriptions) per week. Facebook is worth more than each of these and more than the average weekly expenditure of $20 on coffee (Tuttle, 2012). Facebook has a large value for our participants relative to their income and other purchases.

2.3.3 Effects of the Facebook Restriction

We explore the effect of not using Facebook for one week on five outcomes: social media usage, news consumption, news awareness, subjective well-being, daily activities and the value of Facebook. Throughout the paper, indices are constructed using the procedure of Anderson (2008). We demean each variable using the mean of the control group in Phase 2 and convert it into an effect size by dividing it by the standard deviation of the control group in Phase 2. The index is the weighted average of the transformed outcomes, where the weights are derived from the inverse of the covariance matrix of the transformed outcomes. A key advantage of our design is that we can verify that participants assigned to the Facebook restriction remained logged off without having to directly contact participants with reminders and possibly affect their behavior. Our compliance rate is 95 percent, and throughout the paper we report intent-to-treat effects.[29]

To examine the effects of Facebook on behavior, we exploit the fact that we ask the same questions in the pre and post-treatment surveys (administered in Phase 2 and Phase 3) and estimate the change in the outcome of interest and control for individual fixed effects. This

[28] In-state tuition at Texas A&M is $11,200 per year, or $350 per week, implying that participants value Facebook as much as 19 percent of the weekly cost of studying at the university. According to the College Board, the average university student in the U.S. spends $225 per week ($10,800 per year) on room and board. Facebook is then worth 30 percent of these expenses.

[29] All but three treated participants stayed off of Facebook for the entire week. The three who did log back into Facebook did so only once for less than an hour to communicate for a student organization via the organization's Facebook account. All three participants contacted the research team prior to logging in to inform us why they were logging back on. These participants are included in our intent-to-treat analysis. Instrumental variable estimates are 5 percent larger and slightly less precise.

approach identifies treatment effects based on changes in individual behavior and controls for any unbalancedness that might exist in covariates across the treatment and control groups. By relying on within-individual variation to identify effects, the only difference across individuals is random assignment to treatment and control.

Specifically, we estimate the following equation:

$$y_{it} = \beta_0 + \beta_1 PostSurvey_t + \beta_2 PostSurvey_t \cdot Treatment_i + \alpha_i + \varepsilon_{it} \tag{2.1}$$

where $PostSurvey_t$ is a dummy variable for the survey given in Phase 3 after the one-week Facebook restriction and $Treatment_i$ indicates if individual i is randomly assigned to the Facebook restriction group. β_2 is our coefficient of interest. Individual fixed effects are included and thus control for treatment assignment and fixed individual covariates. Standard errors are clustered at the individual level. We estimate equation (2.1) for the full sample and explore heterogeneous effects by gender and different classifications of Facebook users.

In addition to testing differences in means, we test whether Facebook usage has an effect on the distribution of outcomes. We test for equality of the distributions, as well as first and second order stochastic dominance.[30]

2.3.3.1 News Awareness

According to Gottfried and Shearer (2016), 64 percent of social media users access news from just one site, and on Facebook, 66 percent of users report getting at least some news while using the platform (Pew Research Center, 2016). This suggests that Facebook might play an important role in the distribution of news. If this is true, we should expect that logging individuals off Facebook for a week decreases awareness of current events. We use the news headlines quiz described in Section 2.2.2 to define three indicators that measure the effect of

[30] It would be important to test for effects at different quantiles, but we do not have enough power to estimate meaningful comparisons at the tails of the distribution. To test for distribution equality, let $F_{(1)}$ be the distribution of outcome y_{it} for the treated group and $F_{(0)}$ be the distribution of the control group. According to Abadie (2002), we define $F_{(1)}$ first order stochastic dominates $F_{(0)}$ if $\int_0^x dF_{(1)}(y) \leq \int_0^x dF_{(0)}(y)\ \forall x \geq 0$ and $F_{(1)}$ second order stochastic dominates $F_{(0)}$ if $\int_0^x \left(\int_0^z dF_{(1)}(y) \right) dz \leq \int_0^x \left(\int_0^z dF_{(0)}(y) \right) dz\ \forall x \geq 0$

Facebook usage on news awareness: the proportion of news headlines participants correctly recognized as having occurred, the proportion they got wrong and the proportion for which they were uncertain (i.e. they answered "I don't know"). We calculate these measures for the questions from mainstream sources (six questions) and for the skewed news source.

Figure 2.4 shows the effect of the Facebook restriction on these three measures for mainstream and skewed sources. There is no significant effect of the restriction on news awareness for headlines from mainstream sources.[31] However, there is significant uncertainty of the veracity of headlines from skewed news. Those who experienced a week off of Facebook are 22.1 percentage points more likely to be uncertain about whether or not a politically-skewed news headline is true or not. And, they are 15.6 percentage points less likely to answer correctly if the event actually occurred.[32]

2.3.3.2 Potential Mechanisms for the Reduction in News Awareness

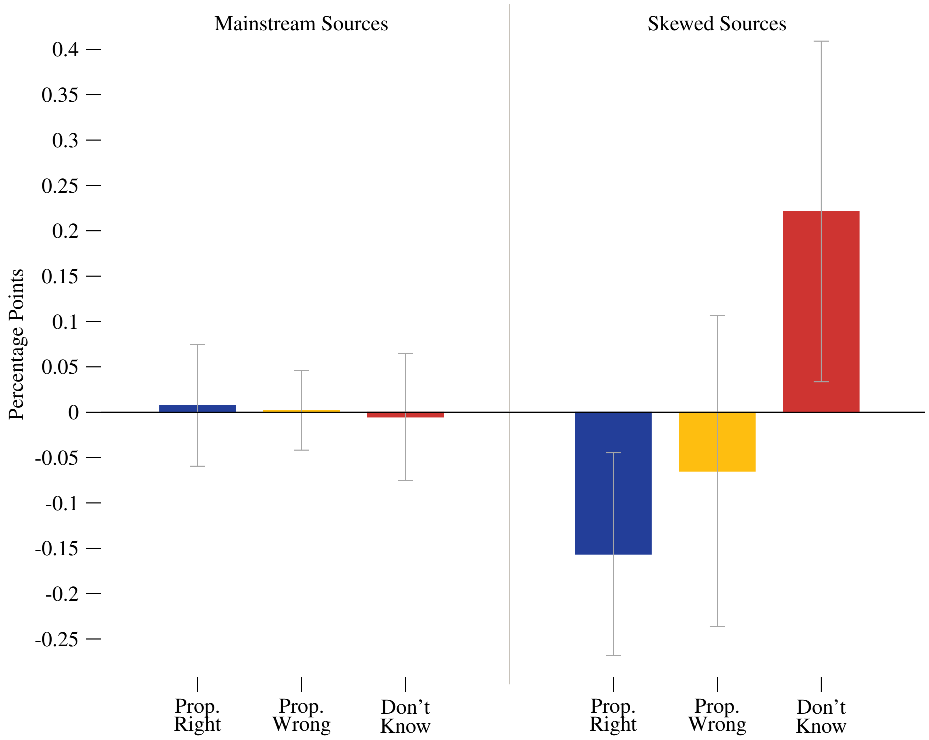
The reduction in news awareness should be correlated with an overall decrease in access and consumption of news. We analyze how being logged off Facebook for a week affects the frequency with which individuals access different news media and whether consumption of different types of news changes. Participants reported their answers for news consumption and types of news using a Likert scale ranging from "not at all" (1) to "all the time" (7). Following the procedure described in Section 2.3.3, we aggregate access to "traditional" news media (i.e. radio, newspapers, television and Internet sites) in one index (Traditional Media) and access to social media and news feeds into a second index (Social Media). We use the two indices to measure changes in access to news media.

The left panel in Figure 2.5 presents the effect of the Facebook restriction on access frequency to news media. On average, access to news through social media decreases by 0.66

[31] We tested whether the Facebook restriction had different effects for true headlines and the false headlines we created (by changing a few words) in the news quiz. For both types of headlines, the point estimates are similar to the main results and statistically insignificant.

[32] Gender differences do emerge. While both men and women are less likely to be aware of the veracity of skewed news when off of Facebook, the effect is much stronger for men than women. This suggest that men, more than women, are exposed to politically skewed news when on Facebook.

Figure 2.4: Effects on News Awareness

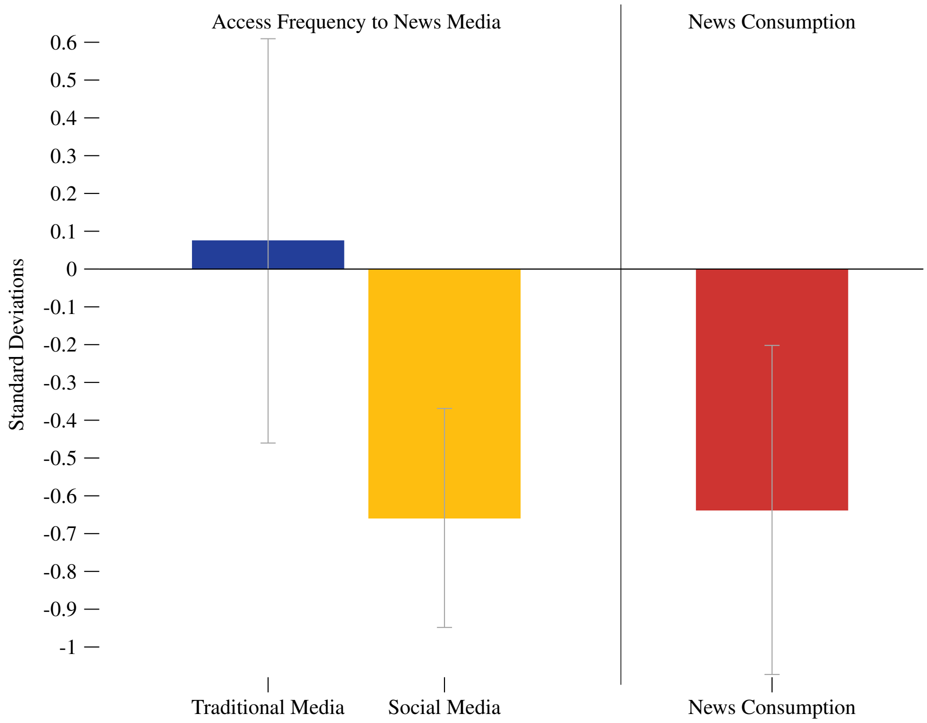


Notes: This figure presents the intent to treat effects of the Facebook restriction on news awareness. Prop. Right corresponds to the proportion of questions answered correctly on the news headlines quiz, Prop. Wrong corresponds to the proportions of questions answered incorrectly and Don't Know corresponds to the proportion of questions answered "I don't know". Estimates control for individual fixed effects. Each estimate corresponds to the change in the proportions of answers in each category. The figure displays the 95 percent confidence interval.

standard deviations (significant at the 5 percent level), while there is no statistically significant change in access to "traditional" news media. These results are consistent with the fact that participants in the restriction group reduced their Facebook usage to zero but they do not substitute by increasing use of traditional media.[33] We also find that the distribution of

[33]Our research design restricted usage of Facebook for those in the treatment group, but participants were not restricted in their usage of other social media platforms. We validate that those in the treatment group did reduce their use of Facebook – by 1.7 hours per day. Given a baseline Facebook usage of 1.9 hours per day, this illustrates that the treatment group did comply with the restriction. While the treatment group refrained from using Facebook, we find that they did not increase their usage of other social media (e.g. Instagram, Snapchat, Tumblr, Twitter). This is consistent with studies finding low cross-platform usage for social media and a significant cost to switch to alternatives for one week (Pew Research Center, 2016). Only one-third of Facebook users are active on other social media platforms, yet about 90 percent of users of other platforms are active on Facebook (Pew Research Center, 2016).

Figure 2.5: Effects on News Media Access



Notes: This figure presents the intent to treat effects of the Facebook restriction on access to two types of news media. Traditional media is an index that measures access to "traditional" news media (i.e. radio, newspapers, television and Internet sites). Social media is an index that measures access to news through social media and news feeds. Estimates control for individual fixed effects. Each estimate corresponds to the change in access frequency of a type of media. The figure displays the 95 percent confidence interval.

the social media index for the restriction group first order stochastic dominates the distribution of the non-restricted group. This indicates that access to news through social media decreases not only at the mean, but throughout the distribution (see Appendix Table A.5). We find no distribution differences for access to "traditional" media. These results indicate that Facebook is an important source of news for our participants, and in the short term, they do not substitute with other news sources.

The right panel in Figure 2.5 presents the effect of the Facebook restriction on news consumption. We asked how frequently the participants read political, business, sports, international, culture, science, local and weather news, and we aggregate these measures into an index (News Consumption) to capture overall news consumption. On average, participants in the Facebook restriction group significantly decrease their consumption of news by 0.64 standard deviations with respect to the baseline (p-value < 0.01), and this effect is consistent across all news types. The reduction in consumption of news decreases not only at the mean but also across the entire distribution (see Appendix Table A.5).

In summary, these results indicate that Facebook is an important conduit for news awareness, specifically from skewed sources, for college students. This is driven by the fact that news consumption decreases and there is no evidence of substitution to other news sources. In the next section we study the effects of Facebook on subjective well-being.

2.3.3.3 Subjective Well-being

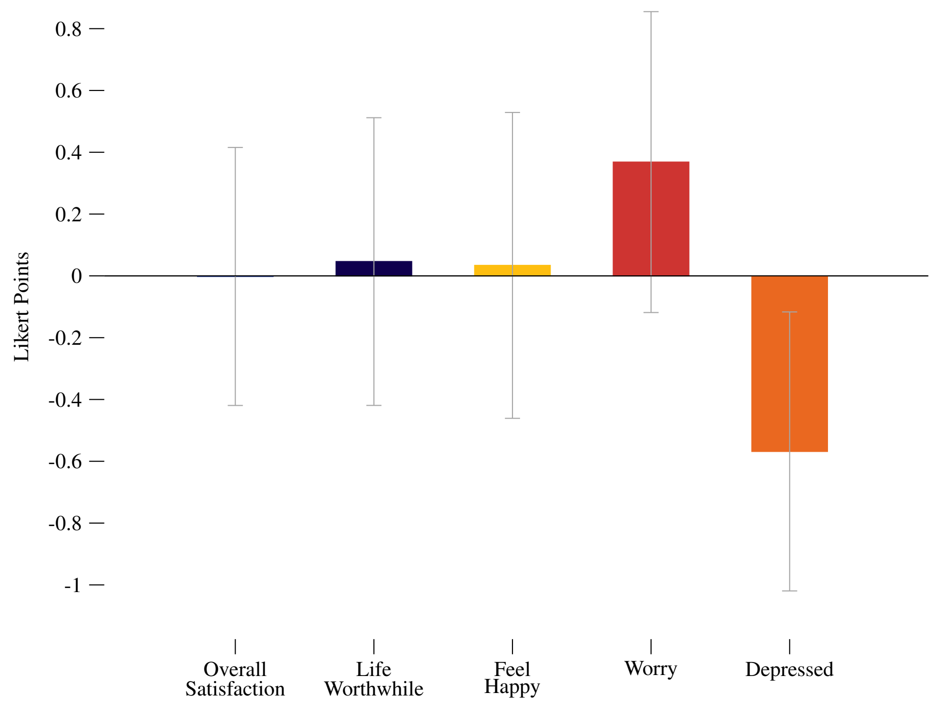
Previous studies have found mixed results on the effects of Facebook on happiness and well-being. We build on previous research by applying a validated Facebook restriction that does not interfere with participants during treatment, and by including a series of questions on daily habits and activities potentially correlated with well-being (Salovey et al., 2000; Ostir et al., 2000; Fredrickson and Joiner, 2002; Blake et al., 2009; Kettunen, 2015; Newman et al., 2014; Sonnentag, 2001).

We asked participants five subjective well-being questions (taken from the OECD Better Life Initiative) using a Likert scale (from 0-10). The questions assess overall life satisfaction, how worthwhile life is, happiness, level of worry, and depression.[34] Figure 2.6 presents the effects of the Facebook restriction on these measures. Estimates for overall life satisfaction, life is worthwhile, happiness and worry are small and statistically insignificant.[35] However, being off of Facebook does significantly reduce depression by 17 percent (0.57 points on

[34]The questions are: (i) Overall, how satisfied are you with life as a whole? (ii) Overall, to what extent do you feel that things you do in your life are worthwhile? (iii) How happy are you? (iv) How often do you worry? and (v) How often do you feel depressed? An alternative approach could have been to use the Day Reconstruction Method (Kahneman et al., 2004 and Kahneman and Krueger, 2006), however, to keep the survey short, we opted for the five OECD questions.

[35]Our results on life satisfaction are smaller than Tromholt (2016) who finds a significant effect of 0.26 standard deviations. The study's Danish sample is older (average age of 34 years) compared to our U.S. sample (average age of 20 years), and participants were contacted daily by the researcher team to follow their assigned treatment status.

Figure 2.6: Effects on Subjective Wellbeing



Notes: This figure presents the intent to treat effects of the Facebook restriction on five different measures of subjective well-being. Estimates control for individual fixed effects. The figure displays the 95 percent confidence interval.

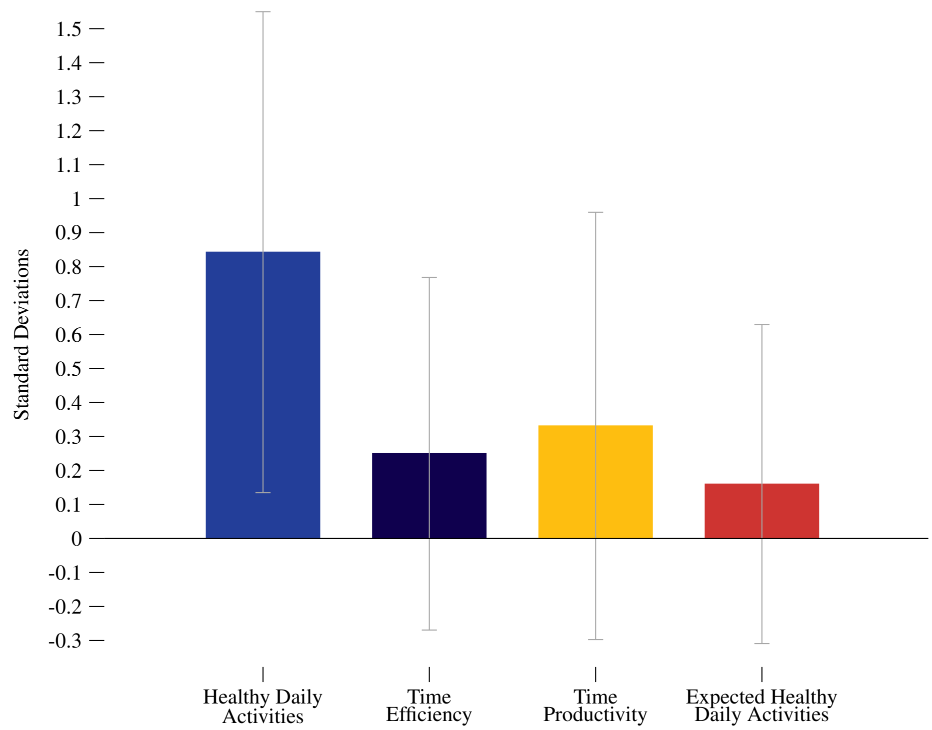
the Likert scale). This result is consistent with findings from the social psychology literature using cross-sectional data that shows Facebook increases feelings of depression (Steers et al., 2014 and Feinstein et al., 2013).[36] We do not find evidence of distribution shifts (see Appendix Table A.5).

Our results suggest that using Facebook induces feelings of depression, which plausibly decreases an individual's well-being.[37] One concern might be the presence of experimenter demand effects for this measure, however, this would imply that we should also observe an increase in positive measures of well-being. Our estimates reject significant improvements

[36]Subjective well-being measures can be sensitive to temporary events (e.g. the weather, long lines at a coffee shop, meeting somebody) (Krueger and Schkade, 2008), nonetheless, because our participants are randomly assigned to treatment, random shocks should be evenly distributed and our panel estimation allows us to directly control for events that affect both groups uniformly across time.

[37]Tromholt (2016) finds larger effects on life satisfaction for active users and for people who report feeling envy while on Facebook. We find qualitatively similar results in terms of feeling depressed.

Figure 2.7: Effects on Activities and Time Use



Notes: This figure presents the intent to treat effects of the Facebook restriction on four index measures of activities and time use. Healthy Daily Activities indexes engagement in "healthier" consumption/savings practices in the past week. Time Efficiency measures efficient time use. Time Productivity measure productive time use. Expected Healthy Daily Activities indexes the expected engagement in "healthier" consumption/savings practices the following week. Estimates control for individual fixed effects. The figure displays the 95 percent confidence interval.

in well-being.[38]

The reduction in feelings of depression from being logged off of Facebook could be driven by changes in behavior. To shed light on how people respond to losing Facebook access, we asked participants to report on a variety of activities the week prior to completing the pre and post-treatment surveys (Phases 2 and 3). Healthy behavior was measured by asking whether participants ate out less than usual, did less impulse buying, saved more money, ate healthier and exercised more.[39] We also asked what they expected their behavior

[38]Evidence of a negative correlation between happiness and depression is weak (Rezaee et al., 2016), hence, a significant decrease in depression is not inconsistent with no change in the positive measures of well-being.

[39]There is evidence that eating out is associated with excessive calorie intake (Urban et al., 2016), a less

would be the following week. Productive time use was measured by asking whether they spent more time studying, had time to relax and be with friends, and partied a lot. Time efficiency was measured by whether they wasted less time, achieved more than usual, were not late for class, were able to meet deadlines, were able to prevent distractions, discontinued wasteful activities, and procrastinated less.[40] Again, we use the procedure in Section 2.3.3 to aggregate these four categories of questions into four indices: healthy daily activities, time efficiency, time productivity and expected healthy daily activities.

Figure 2.7 reports the effects of the one week Facebook restriction on these four measures. Overall, we find evidence that people behave in a healthier manner. Healthy daily activities increase by 0.86 standard deviations with respect to the baseline, significant at the 5 percent level. We find positive, but not statistically significant changes for the other indices. There are no significant effects on the distributions (see Appendix Table A.5).

In summary, a one-week Facebook restriction decreased feelings of depression and increased engagement in healthier activities. While we are not able to tease out the exact mechanism, these results suggest that Facebook negatively affects components of daily live beyond the benefits of social media.

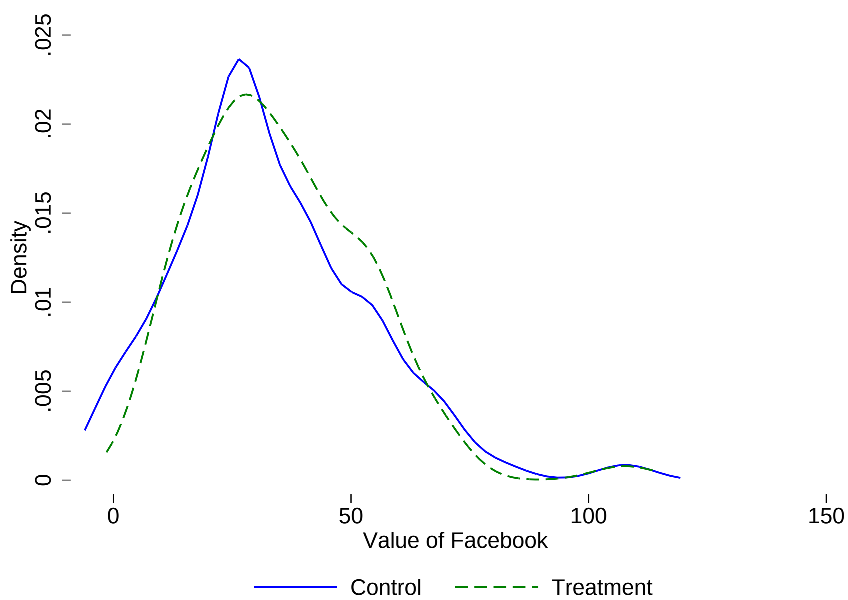
2.3.3.4 Change in the Value of Facebook

Being off Facebook for one week decreases news awareness and consumption, improves well-being by decreasing feelings of depression and promotes healthier behavior. If participants internalize these changes, we would expect a change in individuals' value of Facebook. Figure 2.8 shows the distribution of values for the restricted and unrestricted groups for those who completed the pre and post-surveys (Phases 2 and 3). Experiencing a week-long Facebook restriction increases the value of Facebook by 19.6 percent from $30.13 to $36.04,

healthy diet (Wolfson and Bleich, 2015), increased hypertension (Seow et al., 2015) and a higher exposure to phthalates (Varshavsky et al., 2018), which have been linked to asthma, breast cancer, type 2 diabetes and fertility issues. Diet is correlated with an individual's mental health (O'Neil et al., 2014).

[40]Participants were asked on a scale 1-5 to what extent they agreed with a particular statement, where 1: Strongly Agree, 5: Strongly Disagree. We adjust the coding so a higher value indicates a "healthier" response.

Figure 2.8: Distribution of the Value of Facebook after Treatment



Notes: This figure compares the distribution of the value of Facebook after a one-week Facebook restriction for the participants who attended both Phase 2 and Phase 3.

however, this effect is marginally significant at the 10 percent level.[41] We find no significant distributional treatment effects (see Appendix Table A.5).

There are several potential explanations for this increase in value. First, the reduction in access to news may simply not be compensated by a better mood and healthier activities. Individuals would then need a higher payment to be willing to be off of Facebook for another week. Second, the increase in value is consistent with withdrawal effects of an addictive good.[42] If being on Facebook creates addiction, then the week-long restriction should increase the desire to be back on Facebook. This would also explain the rise in value of Facebook. Finally, Facebook further affects other dimensions of daily live that were not captured

[41]Given our sample size we can detect effects up to 0.182 percentage points at the 5 percent level with a power of 80 percent.

[42]A key characteristic of an addictive good is that its consumption exhibits "adjacent complementarity" (Becker and Murphy, 1988, and Gruber and Köszegi, 2001), which means that past consumption increases the marginal utility of present consumption.

Table 2.1: Adjustments for Multiple Comparisons

	Unadjusted P-value	FDR Adjusted P-value
Facebook Use	0.000***	0.000***
News Media Index - Traditional Media	0.785	1.000
News Media Index - Social Media	0.000***	0.000***
News Consumption Index	0.004***	0.027**
Probability Right Answer - Mainstream News	0.826	1.000
Probability Wrong Answer - Mainstream News	0.926	1.000
Probability Not Sure Answer - Mainstream News	0.885	1.000
Probability Right Answer - Skewed News	0.006***	0.030**
Probability Wrong Answer - Skewed News	0.458	0.723
Probability Not Sure Answer - Skewed News	0.022**	0.052*
Overall Satisfaction	0.993	1.000
Life is Worthwhile	0.845	1.000
Feel Happy	0.893	1.000
Worry	0.139	0.228
Depressed	0.014**	0.048**
Consumption Index	0.020**	0.057*
Productive Time Index	0.302	0.499
Efficient Time Index	0.346	0.530
Expected Consumption Index	0.504	0.743
Value of Facebook	0.068*	0.125

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

Notes: This table shows how the significance of the main results changes when we control for multiple comparisons. The table present the unadjusted p-values of our main estimates (Column 1) and their corresponding values adjusted for multiple comparisons (Column 2). We apply a false discovery rate control as described in Anderson (2008).

in our study. For instance, we do not measure the effects of losing access to Facebook's messenger service. These aspects along with their interactions may be utility increasing, which could explain the increase in value for an additional week off of Facebook.[43]

2.4 Robustness Checks

2.4.1 Multiple Comparisons

Our analysis thus far tests for effects on a large number of outcomes. To check that our main findings are not due to chance, we adjust the p-values to account for multiple comparisons.[44] We apply the procedure defined by Benjamini and Hochberg (1995) and Benjamini et al. (2006), and the results are shown in Table 2.1. We see that all of our results remain statistically significant at the 5 percent level or less, with the exception of the probability of answering "Don't Know" for skewed news, the healthy activities index and the change in the value of Facebook.[45]

2.4.2 Sample Selection

Our approach of recruiting volunteers to log off Facebook may induce selection by over-sampling low-value participants and potentially bias the results. To address this, we use the distribution of the stated BDM value of Facebook to re-weight the sample using the inverse probability of being eligible to participate in Phase 2 conditional on the value. Table 2.2 presents these results. Columns 1 and 2 show that the results pertaining to news awareness and news consumption remain and are robust to sample selection. The point estimates are robust to re-weighting the sample and retain statistical significance. The point estimate of the effect on depression decreases from 0.57 (17 percent of baseline) to 0.39 (11 percent of baseline) Likert points and loses statistical significance. The same happens to the effect on daily activities. The point estimate decreases from 0.84 (17 percent of baseline) to 0.69 (11 percent of baseline) standard deviations.

[43] Appendix Figure A.3 shows that while the level of depression in the treatment group has decreased relative to control group, there is no evidence that suggests that treated participants are internalizing this benefit by lowering their value for Facebook.

[44] To do this involves a trade off between a Type I error and the power of the test (Anderson, 2008). We control for the false discovery rate to adjust our p-values and achieve a balance between these two factors.

[45] We also do a more robust adjustment controlling for the family-wise error rate. When we use the free step-down method described by Anderson (2008), only the effects on Facebook use, news access through social media, news consumption and the correct answer of skewed news are statistically significant at conventional levels.

This analysis suggests that the results on news consumption and awareness are robust to sample selection and representative of the broader population of college students. Conversely, the results on depression and daily activities speak to the population of college students who report having a BDM value of Facebook up to $30 per week (84.4 percent of the student population who uses Facebook).

Table 2.2: Weighting Adjustments

	Full Sample		Men		Women	
	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted
Facebook Use	-1.73***	-1.88***	-1.27***	-1.34***	-2.09***	-2.22***
News Media Index - Traditional Media	0.07	-0.05	0.47	0.77	-0.17	-0.48
News Media Index - Social Media	-0.66***	-0.61***	-0.81***	-0.68**	-0.53***	-0.55**
News Consumption Index	-0.64***	-0.59**	-1.01**	-0.59	-0.46*	-0.62**
Probability Right Answer - Mainstream News	0.01	0.04	-0.09	-0.03	0.03	0.05
Probability Wrong Answer - Mainstream News	0.002	0.005	-0.02	-0.04	0.02	0.02
Probability Not Sure Answer - Mainstream News	-0.01	-0.04	0.10*	0.06	-0.05	-0.07
Probability Right Answer - Skewed News	-0.16***	-0.14**	-0.32***	-0.33**	-0.09	-0.09
Probability Wrong Answer - Skewed News	-0.06	-0.03	-0.27**	-0.22**	0.06	0.06
Probability Not Sure Answer - Skewed News	0.22**	0.17*	0.59***	0.55***	0.03	0.03
Overall Satisfaction	-0.002	-0.02	0.12	0.21	-0.07	-0.10
Life is Worthwhile	0.05	-0.12	0.51	0.80**	-0.17	-0.42
Feel Happy	0.03	0.16	0.05	0.21	0.06	0.23
Worry	0.37	0.48*	0.42	0.28	0.37	0.57*
Depressed	-0.57**	-0.39	-0.82**	-0.90**	-0.44	-0.24
Healthy Daily Activities Index	0.84**	0.69	1.47**	1.16*	0.52	0.67
Productive Time Index	0.25	0.04	0.40	0.10	0.19	0.12
Efficient Time Index	0.33	-0.13	0.41	0.12	0.31	-0.18
Expected Healthy Daily Activities Consumption Index	0.16	0.22	0.28	0.12	0.14	0.32
Value of Facebook	0.20*	0.19**	-0.16	0.10	0.33**	0.19*

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

Notes: This table compares the main results with the weight-adjusted estimates. We use the inverse probability of being eligible as weights.

2.4.3 Gender differences

There is evidence to suggest that men and women use Facebook for different purposes with different frequencies. According to the Pew Research Center (2018) report, more women (74 percent) use Facebook than men (62 percent). Women are more likely to use it daily (69 percent) than men (54 percent) (Statista, 2018), and they post more comments and pictures and send more messages (Muscanell and Guadagno, 2012). This is also evident in our sample.[46] These differences in Facebook usage may imply heterogeneous responses to the Facebook restriction.

Splitting our sample by gender, Table 2.2 shows that for men one week off Facebook decreases feelings of depression by 0.82 Likert points, which increases to 0.90 after re-weighting. Both are statistically significant at the 5 percent level. There are no significant effects for women. While the point estimate of the effect on healthy daily activities decreases from 0.84 to 0.69 standard deviations in the full sample, losing statistical significance, the effect remains large and significant for men. Both the weighted and unweighted results show that the group, in this case men, that is less depressed also engages in healthier activities, confirming the influence of Facebook on other aspects of daily life.[47] This is also consistent with findings that men are more likely to feel depressed due to negative social comparisons (Steers et al., 2014).

Our finding on the reduction in awareness of skewed news is supported by the behavior of men. They are significantly less likely to be certain about the veracity of skewed news both in the weighted and unweighted samples, and women are unaffected. Women reduce their consumption of news via social media, as do men, but are otherwise not significantly affected by the Facebook restriction.

[46] In our sample, about 43 percent of women post comments on Facebook at least once a week, compared to 21 percent of men. Also, 23 percent of women post pictures at least once a week compared to 8 percent of men.

[47] The magnitude of the effects suggest incremental power issues due to re-weighting.

2.5 Conclusions

Social media and Facebook have become entities of global proportions. However, we know little about their economic value to users, the effects on daily activities, consumption behavior and news awareness. Using a randomized, and validated, Facebook restriction in a large field experiment, we provide an estimate of an individual's value of Facebook. One week on Facebook is worth about $67 for our participants – a relatively large value considering that it represents 30 percent of average weekly income. We also examine the direct effect of being logged off Facebook for one week on five outcomes: social media usage, news awareness, news consumption, subjective well-being, activities and the value of Facebook.

While individuals facing a Facebook restriction did refrain from using Facebook, they did not increase their usage of other social media. This is consistent with studies that find low usage across social media platforms and suggests that there is a significant switching cost between platforms.

In addition to not using other social media, participants did not look for news from other sources, even when the substitution cost for accessing news from other sources is low (i.e. turning on the television or radio or typing the web address of a news site instead of Facebook). Overall, awareness of news in most categories was not affected, but being off of Facebook resulted in more uncertainty about whether news from politically-skewed sources was fake or not. Those who experienced a week without Facebook were 22.1 percentage points more likely to be uncertain about a skewed news headline, and men's news awareness was most affected. These results imply that Facebook is an important source of news and may especially be a source of skewed news for men.

Our study has further implications. News aggregators that remove biases from news sources would better inform and educate the general public and could weaken the influence of skewed news. Facebook features (i.e. Instant Article, Trending News, etc.) suggest the company desired to serve as a news aggregation platform. However, recently Facebook eliminated these features out of concerns of propagating fake or skewed news, which goes in line with our finding on news consumption and awareness. While a news aggregator has the potential to provide an unbiased perspective of news and events (Mullainathan and Shleifer, 2005), our findings suggest that Facebook, as currently constructed, may not be well suited for this purpose.

Our results suggest that using Facebook induces feelings of depression, which plausibly decreases an individual's well-being. This effect is particularly pronounced for active Facebook users, for those who experience negative emotions while on Facebook and for men. Contrary to other studies (Tromholt, 2016; Valenzuela et al., 2009; Deters and Mehl, 2013), we find no effect with respect to reported overall life satisfaction. The reduction in depression we find from being off of Facebook might be explained by two mechanisms. First, being off Facebook encourages individuals to engage in more positive, healthy activities, such as exercising and eating out less often, which could explain the improvement in mood. Second, Facebook itself might be a channel for decreasing subjective well-being, and changes in activities and consumption patterns could be a result of feeling better. Untangling the direction of causality would be an important area for future research.

3. VACCINES AT WORK

3.1 Introduction

Seasonal influenza causes substantial morbidity and mortality every year around the world. The World Health Organization (WHO, 2018) estimates that the flu is associated with three to five million cases of severe respiratory illnesses and between 290,000 to 600,000 deaths per year worldwide. Rothman (2017) estimates that the flu has an economic burden of approximately $34.7 billion in the United States, most of it due to lives lost and foregone work. Molinari et al. (2007) associate 16 million days of productivity lost due to influenza in the United States. For both public health institutions and firms, flu vaccination has the potential to be a cost-effective way to reduce the incidence of the disease and its costs. From an immunological perspective, the flu vaccine increases the level of individual immunity by generating antibodies (Gross et al., 1989; Cox et al., 2004), which promises to reduce the transmission rate of the disease.

However, individual behavior can counter the potential benefits of vaccination in two ways. First, according to the World Bank, the Center for Disease Control, and other public health institutions, vaccination rates in most countries of the world are substantially below recommended levels.[1] Therefore, it is essential to understand the factors that affect vaccination take-up, particularly of working adults who are the least likely to get the vaccine. Second, economic theory and empirical evidence suggest that the adoption of protective technologies may induce individuals to behave riskier.[2] Vaccinated individuals may overestimate the protection that the vaccine grants and engage in risky behaviors like waiting

[1]Public health institutions recommend that everybody over six months should vaccinate against the flu. However, flu vaccination rates in European countries ranges from 2 percent to 70 percent (Mereckiene, 2015), and only 38.5 percent of adults 18 and older were immunized in the United States during the 2017-2018 flu season (Srivastav et al., 2018).

[2]There is a large literature that studies whether the adoption of any type of safety device leads individuals to adopt riskier practices (Peltzman, 1975; Richens et al., 2000; Auld, 2003; Cohen and Einav, 2003; Klick and Stratmann, 2007; Peltzman, 2011; Prasad and Jena, 2014; Talamàs and Vohra, 2018). There is a larger literature that studies moral hazard in insurance. For studies on moral hazard in medical insurance see Einav et al. (2013) and Einav and Finkelstein (2018).

longer before going to the doctor when feeling sick and taking fewer protective measures to prevent illnesses. Thus, moral hazard could counter the benefits of adopting a preventive medical technology like the flu vaccine.

In this paper we study how economic factors affect working adults' decision to vaccinate, the effects of vaccination on health and whether flu vaccination can cause moral hazard. In cooperation with a major bank in Ecuador that provides annual vaccination campaigns to improve its employees' health, we randomize incentives to get a flu shot. We follow the definition of a natural field experiment (Harrison and List, 2004) closely by studying individual behavior in an environment where subjects naturally make their decisions without knowing that they are participants in an experiment. Our design allows us to analyze how economic factors (price, opportunity costs, information, and peers) affect working adults' decision to vaccinate. In a second step, we use the exogenous variation on vaccination generated by the random incentives to study its impact on employees' health and their behavior. Thus, we use a random encouragement design (List et al., 2017) in a health-related context, which constitutes an ethical approach for evaluating the effects of adopting a medical technology that in our view is superior to existing empirical approaches.

Much of the medical research on vaccines relies on evidence from observational studies without randomization of the individual treatment. While reviews of the medical literature document the positive health effects of flu vaccination, many of the medical studies could be affected by selection and other biases, as pointed out by Jefferson et al. (2010), Osterholm et al. (2012), Demicheli et al. (2014), Østerhus (2015). For instance, researchers describe the problem of a "healthy vaccine recipient effect," which implies that healthier individuals are more likely to get vaccinated. Such positive selection bias could lead to an overestimation of the health effects. Still, observational studies are often preferred because of ethical concerns regarding experimental interventions in the context of health. This concern is also true for randomized controlled trials on vaccines, which, if conducted, rarely make use of placebos for ethical reasons (Sanson-Fisher et al., 2007; Baxter et al., 2010), but instead manipulate the type of vaccine across treatment and control groups. Besides these identification issues, the medical research on vaccines focuses only on the medical effects, without considering changes in behavior that may affect health. By design, participants of a randomized controlled trial know that they are in an experiment but do not know if they received the vaccine or not, which rules out changes in behavior. Since public institutions and companies are interested in the total effect of health interventions, we believe that our random encouragement design is superior in the sense that it captures both behavioral and medical effects from getting vaccinated and it circumvents the ethical dilemma of withholding a potentially effective medical treatment as in placebo-controlled interventions.

We introduced three modifications to the bank's 2017 vaccination campaign to influence vaccination take-up: We changed the vaccine's price at an income threshold, assigned weekdays for on-site vaccinations randomly across employees, and varied the content of the emails used to invite employees to vaccinate. Regarding price, employees who earned less than \$750 would receive a \$2.48 price discount from the bank if they got vaccinated. To implement the other encouragements, we randomly assigned all employees into four groups. Employees assigned to the control group were informed of the campaign via email about their assigned day during the workweek, time, and the vaccine's price. The first treatment group received the same information as the control, but employees were assigned to get vaccinated on *Saturday*. Assigning employees for vaccination on *Saturday* increases the opportunity costs of vaccination compared to the workweek because these employees would need to incur additional transportation costs and arrange their weekend schedule to get vaccinated. In contrast, assigning employees to the workweek minimizes their opportunity costs because the bank allows them to take time off their duties to get vaccinated. The second and third treatment groups received the same email as the control, plus an information nudge in the form of a short message summarizing the altruistic and individual benefits of the vaccine, respectively.

To investigate the determinants and consequences of vaccination, we have access to administrative data from the bank that we merge with information on treatment assignment at the individual level. The firm's data includes detailed medical diagnoses for each employee, so we can precisely identify flu diagnoses and the resulting sick days. We check if getting vaccinated affects these measures of health. Also, being able to distinguish flu-related sickness from non-flu-related sickness allows us to study behavioral effects, assuming that flu vaccination has no direct effect on non-flu-related sickness. Also, our empirical setting helps us with the issue of health spill-overs, as in principle unvaccinated individuals could benefit from vaccinated peers. First, general population flu vaccination rates in Ecuador fluctuate around 2 percent (INEC, 2012), which is far below the levels that grant herd-immunity effects. Second, we can empirically check whether peer effects in health are an issue by using the bank's organizational data. In our setup, the bank's units, which group the employees that work directly together every day, are the relevant social groups.[3] Given our intervention at the employee level, by chance, there are some units which have more employees encouraged to get the vaccine than in other units. We exploit this variation to study peer effects in take-up as well as potential health effects of having exogenously vaccinated peers. Finally, employee surveys before and after the vaccination campaign complement our dataset and allow inspection of possible mechanisms for the effects on employee health and behavior.

We find the following results on the factors that affect vaccination take-up. First, we assess how price and opportunity costs affect vaccination take-up on working adults. Economic theory identifies both monetary and opportunity costs as a relevant component in the decision to adopt medical technologies like vaccination (Brito et al., 1991; Geoffard and Philipson, 1997; Kremer and Miguel, 2007; Chen and Toxvaerd, 2014; Schaller et al., 2017). We find that a \$2.48 change in price did not affect take-up. Conversely, decreasing opportunity costs by assigning employees to get vaccinated during the workweek increased take-up by 14 percentage points, a 112 percent increase compared to Saturday. Thus, reducing op-

[3]Previous studies on peer effects in the adoption of medical technology rely on distance measures or on self-reported (incentivized and non-incentivized) networks of friends (Kremer and Miguel, 2007; Sato and Takasaki, 2015; Rao et al., 2017).

portunity costs has a significant effect on take-up for working adults. Other policy measures directed to increase vaccination rates of adults, such as advertising or commitment devices, have smaller effects than reducing these costs (Nowalk et al., 2010; Milkman et al., 2011).[4] Thus, public health institutions and firms can cost-effectively increase take-up by minimizing opportunity costs. Information on the altruistic or personal benefits of vaccination is another factor that could affect take-up, but we find no effect from providing this information. The coefficients are close to zero, negative and statistically insignificant, which is consistent with previous studies (Bronchetti et al., 2015; Godinho et al., 2016). Given that reducing opportunity costs has a substantial effect on take-up, it is plausible that supplying a sentence of additional information is not enough to further increase it.

Having found a significant determinant of vaccine take-up, we can use the random assignment to the workweek to identify both the effects of vaccination on health and potential peer effects within units. First, we study the causal effect of randomly vaccinated coworkers on individual take-up by exploiting exogenous variation in the proportion of peers who get vaccinated. While previous empirical work has revealed mixed results of peer effects on the adoption of medical technologies, we find a positive effect of peers on take-up.[5] The estimates indicate that if the proportion of peers that get vaccinated increases by ten percentage points, take-up increases by 7.9 percentage points. We explore potential mechanisms and find that peers are not changing information and beliefs about vaccination. Instead, our evidence suggests that employees react to social norms.

Next, we study the consequences of vaccine take-up by examining if flu vaccination is effective to improve working adults' health. If flu vaccination decreases flu cases, we expect

[4]Nowalk et al. (2010) find that increased advertising increases take-up by 29 percent in adults older than 50 years, with no effect on younger adults. Milkman et al. (2011) find that the use of commitment devices increases take-up by 13 percent.

[5]Theoretically, peer vaccination can either increase (Kremer and Miguel, 2007) or decrease individual take-up (Geoffard and Philipson, 1997; Chen and Toxvaerd, 2014). For flu vaccination, Rao et al. (2017) estimate a positive peer effect on flu vaccination for college students in Harvard. Regarding other medical technologies, Kremer and Miguel (2007) find that increased deworming of peers reduces deworming take-up in Kenya. Conversely, Sato and Takasaki (2015) find that having at least one friend who got vaccinated against tetanus increases the likelihood of tetanus vaccination of women in rural Nigeria.

that offering employees the opportunity to get vaccinated during the workweek would reduce the number of flu cases and thereby the incidence of sickness as well as absence from work due to higher vaccine take-up. However, we find that assigning employees to the workweek did not affect the probability of having a sick day or the incidence of a sickness per se. Using detailed data on medical diagnoses, we find no evidence that exogenously triggered vaccination decreased the probability of getting sick due to the flu. In particular, we can rule out an effect of -2.4 percentage points that correspond to the CDC's estimate of the effectiveness of the 2017-2018 flu vaccine.[6] Also, we explore whether peer vaccination affects employees' health. We find that peer vaccination does not affect the probability of having a sick day due to the flu, which is consistent with low unit vaccination rates that do not grant herd immunity.

We continue analyzing the consequences of vaccination by exploring if getting vaccinated can unintendingly cause moral hazard. As mentioned above, medical studies usually do not consider if vaccination could induce risky behavior (Prasad and Jena, 2014), while the few papers in economics that quasi-experimentally study moral hazard in the context of medical interventions have mixed results (Margolis et al., 2014; Moghtaderi and Dor, 2016; Doleac and Mukherjee, 2018). We find several pieces of evidence suggesting that getting vaccinated can indeed cause moral hazard. First, we test if getting vaccinated leads to individuals to feel more protected and to react differently than the unvaccinated when flu-like symptoms arise. Non-flu respiratory diseases have symptoms similar to the flu, but the flu vaccine does not provide any immunity benefit to prevent them. Thus, flu vaccination should not affect the probability of being diagnosed with a non-flu disease, so any effect on this probability would imply a change in how employees react when sick with a respiratory disease. In particular, if vaccinated employees feel more protected, they might

[6] The CDC calculates the effectiveness of the vaccine by comparing hospitalization rates due to the flu of vaccinated and unvaccinated individuals. In the 2017-2018 flu season, the CDC estimates that getting vaccinated decreased the probability of being hospitalized due to the flu by 36 percentage points. Scaling up this effect by the estimate of the effect of being assigned for vaccination in the workweek on vaccination take-up (6.7 percentage points with the most conservative first stage) yields a reduced form effect of -2.4 percentage points.

be less likely to go to the doctor when they feel flu-like symptoms, and the probability of being diagnosed with a non-flu disease would decrease. Consistent with this hypothesis, we find that assigning individuals to get vaccinated on the workweek decreased the likelihood of being diagnosed with a non-flu respiratory disease by 6.5 percentage points (20 percent of the baseline). Consistent with moral hazard, we find that assigning individuals to the workweek decreased the likelihood of going to the bank's on-site doctor for any reason.[7] Finally, we asked employees in the post-intervention survey about self-reported habits related to improving health. In line with the idea of riskier behavior among the vaccinated, we find that assigning employees for vaccination on the workweek decreased the frequency of people reporting to carry an umbrella by 18 percent of the baseline. As Ecuadorians and many other cultures around the world believe that carrying an umbrella could help prevent the flu and other respiratory diseases, this result suggests that vaccinated employees are less likely to engage in practices believed to prevent the flu. In summary, the results from our analyses suggest that getting vaccinated can create a moral hazard problem that could reduce the effectiveness of flu vaccination.

The factors that affect vaccination take-up and the causal impacts of flu vaccination on health have been of great interest to researchers in medicine and economics. In comparison to previous work, our intervention quantifies large effects of opportunity costs on vaccination of working adults and how peers affect take-up. Thus, we contribute to the ongoing research on the determinants of vaccine take-up, as an example of the adoption of preventive medical technology. According to previous studies, other factors that can affect vaccination take-up include information, education, age, health status, health behavior, and lifestyle (Maurer, 2009; Schmitz and Wübker, 2011; Godinho et al., 2016; Chang, 2018). We also contribute to the research on peer effects, which has important implications on workplace productivity (Mas and Moretti, 2009; Herbst and Mas, 2015) and has recently been recognized as an important aspect in human behavior concerning health. Our findings on the health ef-

[7] The on-site doctor is a convenient feature the bank offers its employees. Before the intervention 77 percent of all sick cases correspond to visits to these doctors.

fects of vaccine take-up add to an ongoing discussion that predominantly takes place in the medical literature, with few exceptions such as Ward (2014) and White (2018). Vaccination campaigns are seen as a way to tackle sickness-related absence, which is a topic of high economic relevance (Ziebarth and Karlsson, 2010; Ager et al., 2017; Bütikofer and Skira, 2018). Finally, with our investigation of a company-wide vaccination campaign, we contribute to the literature of on-site health interventions (Just and Price, 2013; List and Samek, 2015; Belot et al., 2016), whereas our findings may also inform the broader literature on public health interventions (Evers et al., 1997; Cawley, 2010; Bütikofer and Salvanes, 2018). By showing how preventive medical technologies can unintendingly cause moral hazard, we may offer a partial explanation why health interventions may not always be as successful as expected in improving people's health.

3.2 Experimental Design

We ran the field experiment in cooperation with a bank in Ecuador. This bank focuses on consumer credit and is one of the largest credit card issuers in the country. Its headquarters is in Quito, Ecuador's capital, and it has six branches across the country with over 1,300 employees, distributed in 31 divisions with 142 working units. The bank had run small vaccination campaigns in the past. These campaigns included only some employees in crowded areas and ran during the workweek in the bank's offices.[8] In 2017, the bank decided to extend its annual campaign to all its employees and allowed us to experimentally modify it to investigate how to increase take-up and the effects of vaccination. We implemented three interventions: we changed the vaccine's price for some employees using income-dependent subsidies, we randomized assignments for on-site vaccinations across weekdays, and we implemented information nudges by varying the content of the emails that were used to invite employees to vaccinate.

The bank decided to give the vaccine for free to areas that participated in campaigns in

[8] These areas included the call center and the collections departments, which only have few employees. We exclude the call center from our analysis of the 2017 campaign, as we have evidence that the call center supervisors pushed their employees into taking the vaccine leading to a take-up of almost 100 percent.

previous years and to partially subsidize it for new participants. We used the employees' income to allocate this subsidy. Employees who earned less than \$750 per month would pay \$4.95 to get vaccinated, while those who earned more than \$750 would pay \$7.43. Note that the vaccine's full price is \$9.99. The payment was directly deducted from the employees' paycheck if they opted to get vaccinated.

To examine the effects of opportunity costs and information, we randomly assigned all employees into one of four groups.[9] First, employees assigned to the control group (*Control*) received an email informing them about the campaign, their assigned day, time, and the price they would have to pay (see Figure B.3). These employees were assigned to get vaccinated during the workweek (Wednesday, Thursday or Friday) and were allowed to take time off their duties to get vaccinated. The specific day was selected randomly for each employee.

The first treatment increased the opportunity costs of vaccination by assigning employees to get vaccinated on *Saturday*. The employees usually do not work during the weekend, so they would incur extra transportation costs and would have to arrange their schedule to go to the bank and get vaccinated.[10] Otherwise, this group received the same information as the Control (see Figure B.6). This treatment was only applied in Quito because all the other branches are substantially smaller (82 percent of the employees work in Quito), and their employees could get vaccinated in a single day, which was not possible in Quito.[11]

We also implemented two information nudges. The first nudge highlights the social benefits of flu immunization (Altruistic Treatment). In addition to the information provided to the control group, the email included the phrase: "Getting vaccinated also protects people around you, including those who are more vulnerable to serious flu illness, like infants, young children, the elderly and people with serious health conditions that cannot get vaccinated" (see

[9]The bank requested that we exclude the CEO and another high executive from the intervention. We also excluded our counterpart in Human Resources and four employees who work in the local branches and did not have a company email address to deliver the treatments.

[10]Based on data from the employees' magnetic cards swipes to enter the bank, only 0.4 percent of the employees work regularly on Saturdays.

[11]Branches in the coastlands were randomly assigned to get vaccinated on Wednesday, and branches in the highlands were assigned to Thursday.

Figure B.4). The second nudge highlights the individual benefits of flu immunization (Selfish Treatment). In addition to the information provided to the control group, the email included the phrase: "Vaccination can significantly reduce your risk of getting sick, according to both health officials from the World Health Organization and numerous scientific studies" (see Figure B.5). Employees in these two treatments were assigned to get vaccinated during the workweek and the specific day was selected randomly.

Our intervention targeted the Ecuadorian flu season which usually covers the period from November to the end of February (Ropero, 2011). The bank ran a pre-intervention survey from October 25 to October 29, 2017. Human Resources sent the intervention emails on November 1, 2017, using its official email account. Employees were not aware that this study was taking place. For them, the campaign was just a regular activity organized by the Human Resources department. Employees are used to receiving emails from Human Resources and according to the Human Resources manager typically read these emails carefully. The bank sent out a reminder using the same email account a week later. The vaccination campaign ran from November 8 to November 11, 2017, at locations within the bank's offices in each branch. The bank hired an external medical team to supply and inject the vaccines. Finally, the bank conducted a post-survey during March and April 2018.[12]

3.3 Data

This section describes the data used in our analyses for assessing how economic factors can affect take-up and the effects of the flu vaccine on health. First, we have access to the firm's administrative records about its employees: gender, age, education level, and dependents; job and its position within the bank's organizational structure; tenure and income; and sickness diagnoses and sick days. Second, we collected vaccination take-up data from the bank's campaign records. Third, we use data from pre- and post-intervention surveys. These surveys asked employees about: previous illnesses and general health; knowledge and be-

[12]The geographic locations of the banks' branches are displayed in Appendix Figure B.1 and a depiction of the timeline is shown in Appendix Figure B.2. Appendix Figure B.7 provides information about the flu vaccine used and Appendix Figure B.8 shows an individual getting vaccinated during the campaign.

Table 3.1: Summary Statistics

	Full Sample	Control	Altruistic	Selfish	Saturday	F-test (p-value)
Monthly Income ($)	1,766	1,860	1,701	1,681	1,827	0.316
Company Tenure (years)	7.9	8.3	7.7	8.1	7.5	0.761
Prop. Women	0.49	0.51	0.52	0.46	0.47	0.497
Age (year)	36.6	37.2	36.4	36.6	35.7	0.553
Prop. College Education	0.91	0.92	0.91	0.90	0.93	0.759
Pre Survey Participation	0.48	0.50	0.50	0.47	0.40	0.171
Post Survey Participation	0.36	0.36	0.38	0.33	0.35	0.519
Diagnosed Sick	0.66	0.66	0.66	0.64	0.67	0.892
Granted a Sick Day	0.37	0.36	0.39	0.38	0.34	0.344
Diagnosed Sick Flu	0.11	0.09	0.13	0.13	0.10	0.348
Granted a Flu Sick Day	0.02	0.02	0.02	0.04	0.02	0.195
Vaccination Take-up	0.17	0.22	0.17	0.19	0.08	0.001
N	1,164	344	294	310	216	

Notes: This table presents characterizes the mean employee of the bank where we implemented our intervention. We present statistics for the full sample and the four treatment groups. The last column presents the p-value of a joint significance test to check whether there are significant differences across the treatment groups. The proportion of employees diagnosed sick or granted a sick day corresponds to the period between January 1 and November 7, 2017, before the vaccination campaign.

liefs about vaccination and the flu vaccine; habits related to health; relations with coworkers; opinions about the campaign; motivation; organizational attachment and work satisfaction; and risk and time preferences.

Table 3.1 presents the mean characteristics of the bank's employees (Column 1). On average, employees earn a total monthly income of $1,760. As a reference, in 2017 the average total income in Ecuador was $479, which implies that the bank's employees are in the three highest deciles of the Ecuadorian income distribution (INEC, 2017). The average employee has been in the company for more than seven years and is around 36 years old. The company employs roughly the same number of men and women, and more than 90 percent of its employees have at least some college education, close to education levels in developed countries. Almost 50 percent of the employees completed the pre-intervention survey, a high completion rate compared to previous surveys from Human Resources. The completion rate decreased to 36 percent for the post-intervention survey.

The administrative data include two measures of health: medical diagnoses and sick days. These measures come from two sources: the onsite doctors and medical certificates from outside doctors. It is important to note that Ecuadorian law establishes that employees must present a medical certificate to get a sick day.[13] Consequently, the onsite doctors report every visit they receive to Human Resources. The doctors report the diagnosis (the type of disease), whether they granted a sick day or not, and the number of sick days granted. Also, by law, if an employee takes time off work to go to an outside doctor, then she has to present to Human Resources a medical certificate that indicates the diagnosis and number of sick days granted if any.[14] Hence, in addition to sick days, we can also observe employees being diagnosed sick with no sick days granted for cases where a doctor did not consider the illness severe enough. Thus, sick days are a measure of more severe illness. From January to early November 2017, before the intervention, 14 percent of the employees were sick from any disease, and 6 percent had at least one sick day.

Table 3.1 also shows evidence on the balance of treatment assignment. Columns 2 to 5 present the mean employee characteristics across the four groups. All variables have almost identical means across groups. For each characteristic, Column 6 shows the p-value of a joint significance test of differences of means. We cannot reject the null hypothesis that the means are the same across the four treatments, which suggests that our randomization was successful. The Kruskal-Wallis rank test shows the same result. Finally, we test whether answering the pre and post surveys is different across treatments and find no statistically significant difference.

[13]By law employees in Ecuador also have up to one year of paid leave due to sickness and employers are not allowed to terminate employment during sick leave.

[14]Doctors diagnose their patient using a combination of a physical examination, blood tests and culture tests. The specific procedure is part of individual medical records and we do not have access to that data.

3.4 Analysis of Vaccination Take-up

In this section, we study how economic factors affect working adults' decision to vaccinate. Specifically, we consider the effect of opportunity costs, altruistic and individual information, and peers on take-up in detail. Regarding the income-dependent vaccine subsidy, we do not find any effect of the \$2.48 price difference on vaccination take-up.[15] We conclude that such price change may be too small to induce changes in take-up behavior.

The last row in Table 3.1 presents the flu immunization take-up rates for the different treatments during the campaign. The *Control* group has a take-up rate of 22 percent, the *Altruistic* treatment has a take-up of 17 percent, and the *Selfish* treatment has a take-up of 19 percent. Comparing across the three groups suggests that the information treatments were not sufficient to increase take-up. In contrast, being assigned to get vaccinated during the workweek increases take-up by 14 percentage points in contrast to Saturday (112 percent).[16] We extend the analysis of these effects in the next section.

3.4.1 Effects of Opportunity Costs, Altruistic and Individual Information on Individual Take-up

We model the effect of opportunity costs, altruistic information and selfish information on vaccination take-up for employee i in city c using the following equation:

$$Takeup_{ic} = \alpha + \gamma_c + \pi_1 Altruism_{ic} + \pi_2 Selfish_{ic} + \pi_3 Saturday_{ic} + u_{ic} \tag{3.1}$$

where $Takeup_{ic}$ is an indicator of vaccination take-up. We include Quito fixed effects γ_c to account for differences in implementation of the vaccination day assignment across cities, as discussed in Section 3.2. $Altruism_{ic}$, $Selfish_{ic}$ and $Saturday_{ic}$ are dummy variables

[15] Appendix Figure B.9 shows no visible discontinuity across the threshold. Regression discontinuity estimates also do not indicate any significant change in take-up at the cutoff (see Appendix Table B.1), which is robust to different bandwidths.

[16] In the post-intervention survey 59 employees report that they got vaccinated outside the campaign. Vaccination outside the campaign is not significantly different by treatment status. Also note that between November 2017 and February 2018, 20 treated employees quit the bank. Attrition is not correlated with assignment to the treatments.

Table 3.2: Effects of Treatments on Vaccination Take-Up

	Main	With Controls	Quito	Non-Compliance	Day of Week
Altruistic Information	-0.0260	-0.0209	-0.0493	-0.0262	
	(0.0310)	(0.0303)	(0.0332)	(0.0306)	
Selfish Information	-0.0032	-0.0011	-0.013	-0.0103	
	(0.0314)	(0.0316)	(0.0339)	(0.0308)	
Thursday					0.0002
					(0.0346)
Friday					-0.0356
					(0.0331)
Saturday	-0.0789***	-0.0791***	-0.0898***	-0.0671**	-0.0818***
	(0.0301)	(0.0304)	(0.0313)	(0.0298)	(0.0315)
Baseline take-up			0.1732		
N	1,164	1,164	929	1,152	929

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

Notes: Robust standard errors in parentheses. This table presents OLS estimates of the effect of the different treatments on vaccination take-up. All specifications control for city fixed effects. Column 1 presents our main estimates from equation (1) without adding additional controls. In Column 2 we test the robustness of the main estimates controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level. Column 3 presents the estimates using only employees in Quito, the city where we implemented our four treatments. In Column 4 we exclude 12 individuals who were assigned to vaccinate in the workweek but went to vaccinate on Saturday. In Column 5 we test for different effects across the different days of the week using only data from Quito that has all the treatments. Using clustered standard errors at the work unit level (142 clusters) yields similar standard errors with no loss of statistical significance.

that indicate treatment assignment. Thus, we estimate the effect of the different treatments relative to those individuals who were assigned to vaccination on the workweek and did not receive any information nudge.

Table 3.2 presents the effects of the different treatments on take-up. Column 1 shows baseline results of the effect of opportunity costs and information on vaccination take-up. The estimates indicate that assigning employees to *Saturday* decreased take-up by 7.9 percentage points compared to the *Control*. This effect is approximately 46 percent of the *Control's* take-up and is statistically significant at the 1 percent level. Hence, minimizing the opportunity costs associated with vaccination is a useful measure to increase take-up.

Conversely, we find that emphasizing either the altruistic or the selfish benefits of vaccination does not affect take-up. The coefficients are close to zero, negative and statistically insignificant. It is plausible that supplying a sentence of additional information is not enough to further increase take-up given that reducing opportunity costs has a substantial effect on it.[17] One interpretation of these results is that information would have to be very salient to accrue an effect on vaccine take-up in a company context such as this.

Columns 2-4 of Table 3.2 show the robustness of the results to the inclusion of controls, to the use of a restricted sample, and to controlling for non-compliance. Specifically, Column 2 shows that controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level does not affect the estimates. Column 3 addresses the fact that only employees who work in the bank's headquarters in Quito were assigned to vaccinate on Saturday. In this subsample, assigning employees to Saturday decreased take-up by almost nine percentage points (51 percent of the control group take-up), significant at the 1 percent level. This result is slightly larger than the main result, but we cannot reject that they are statistically the same. Both information treatments have small, negative and statistically insignificant effects. Column 4 shows the effect of controlling for non-compliance.[18] In this subsample, assigning employees to Saturday decreased take-up by 6.7 percentage points, significant at the 5 percent level. We cannot reject that this estimate is statistically the same as the baseline result. The estimates of the effect of the information treatments are practically the same as the main estimates.

Lastly, in Column 5 we check whether assignment to different days in the week affects take-up differentially. We exploit the fact that vaccination days are randomly assigned, and

[17] The post intervention survey asks if the employee recalls the altruistic and selfish information statements. Appendix Table B.3 shows that neither employees assigned to the *Altruistic* treatment nor those assigned to the *Selfish* treatment remember their respective statements better than the control. Another issue could be spillovers of information, but this is unlikely given that our design provides information directly to the treated individuals via email.

[18] We identified in the campaign records 12 people assigned to the workweek who vaccinated on *Saturday*. The bank asked the medical team in charge of the vaccination campaign to enforce the day assigned to each employee, but they failed to enforce this requirement on *Saturday* and were unable to send employees back home if they showed up that day. In contrast, nobody of those assigned to Saturday got vaccinated during the workweek.

we regress our indicator of vaccination take-up on dummies for the assigned day (*Wednesday*, *Thursday*, *Friday* or *Saturday*), using the Quito's subsample.[19] These estimates show that take-up on *Thursday* and *Friday* is not statistically different from take-up on *Wednesday*, while the effect of *Saturday* is substantially larger in magnitude and very close to the baseline estimate in Column 1.[20] These results do not support time-inconsistent preferences that would induce procrastination as the mechanism behind the *Saturday* effect and are consistent with increasing opportunity costs.[21]

3.4.2 Further Evidence on Opportunity Costs

We analyze heterogeneous treatment effects across different subgroups of our study population, which may yield further evidence that opportunity costs are driving the difference in take-up between being assigned to vaccinate on the workweek and *Saturday*.[22] We focus on differences across gender, distance to work, and employees with and without children.[23] Figure 3.1 shows that assignment to *Saturday* has larger effects for men than for women, although the difference is not statistically significant.

Distance to work reflects the transportation costs that an individual regularly incurs. The median employee lives 6.5 km away from work. Figure 3.1 shows that those who live further away than the median are slightly less likely to get vaccinated when they were assigned to *Saturday* than those who live closer to the bank, but this difference is not statistically significant. This result is consistent with increasing travel costs, but the magnitude suggests that travel costs are not the main factor driving the difference in take-up between employees assigned to the workweek and *Saturday*.

[19] Of the bank's employees in Quito, after excluding the call center, 23.4 percent where assigned to vaccination on *Wednesday*, 26.7 percent to *Thursday*, 26.5 percent to *Friday*, and 23.4 percent to *Saturday*.

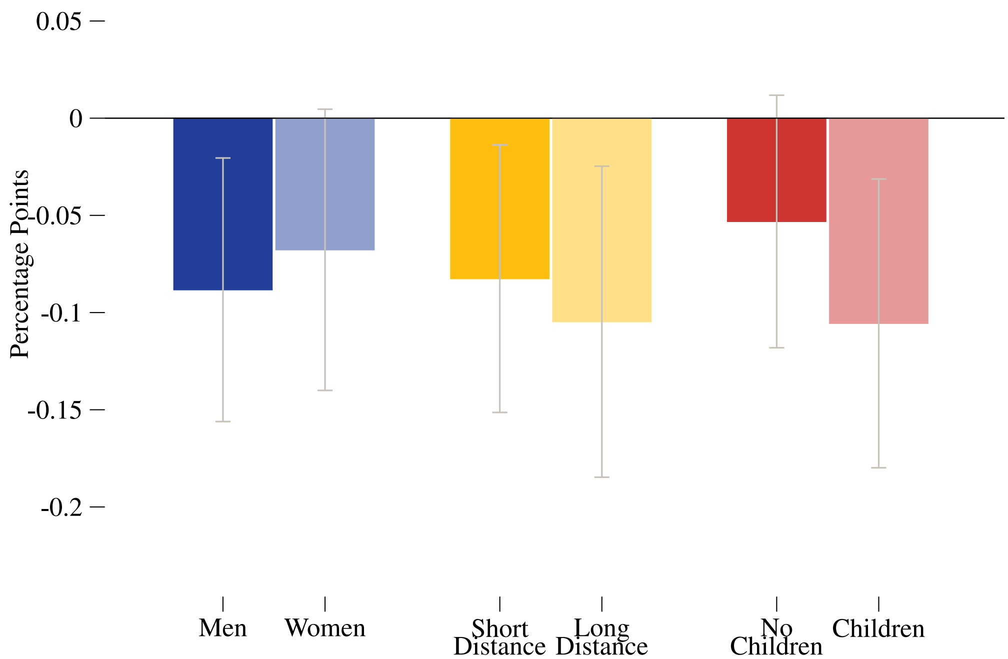
[20] While the effect of assignment to *Friday* is not significant, it is 44 percent of the effect of *Saturday* and two orders of magnitude larger than the effect of *Thursday*. Being assigned to *Friday* can slightly increase the opportunity cost of vaccination because it is only a 6-hour workday if employees finish their tasks.

[21] Also, the *Control* includes people assigned to *Wednesday*, *Thursday* and *Friday*, so any effect of procrastination is included in the comparison made in the baseline estimates.

[22] We find that the information treatments have no differential effect across subgroups. These estimates are small and statistically insignificant. See Appendix Table B.2.

[23] Distance to work was calculated with a geo-location service using employees' home addresses.

Figure 3.1: Heterogeneous Effects of Assignment to Vaccination on Saturday on Take-up



Notes: This figure presents the intent-to-treat effect of assignment to the Saturday on vaccination take-up for different subgroups in the sample. All specifications control for city fixed effects. The figure presents the point estimate and the 95 percent heteroscedastic robust confidence interval for each subgroup.

Finally, we consider differences in the effect between employees with and without children. Having children may imply higher opportunity costs at the weekend by increased family obligations. Figure 3.1 shows that assignment to *Saturday* decreased take-up by 10.6 percentage points for employees with children, while the effect is smaller (5.3 percentage points) and insignificant for employees without children. Although the difference between these two effects is not significant, its magnitude is consistent with the idea that opportunity costs increase for people assigned to *Saturday*.

In conclusion, these results suggest that the difference in take-up between employees assigned to the workweek and *Saturday* correspond to a change in the opportunity costs of vaccination. We use only this variation in take-up created by lowering opportunity costs as an instrument in the rest of our analyses.

3.4.3 Peer Effects on Vaccination Take-up

Peer effects may play an important role in vaccination behavior by either increasing or decreasing take-up. When a person gets vaccinated, the prevalence of the disease may decrease, making it less likely for others to get sick. Thus, if getting vaccinated has costs, then it may be optimal for some people not to do so if their peers got vaccinated. Theoretically, this free-rider problem can result in a Nash equilibrium where nobody takes the vaccine (Chen and Toxvaerd, 2014). Conversely, peers may increase take-up by exchanging information that affects individual beliefs about the flu and the vaccine. Also, individuals may imitate the health care behavior of their peers to conform to social norms (Kremer and Miguel, 2007).

The exogenous variation in take-up created by assigning people to get vaccinated in the workweek allows us to estimate the effects of groups who work together every day on vaccination. The bank's units define the social groups of employees that work directly together. Thus, we can identify the effect of social groups with whom adults share a large portion of their daily time on vaccine take-up. We will also use this approach to analyze peer effects in health caused by vaccinated peers below (Section 3.5).

We model the effect of the proportion of peers in unit j who take the vaccine on employee i's decision as

$$Takeup_{ijc} = \gamma_c + \beta_1 Prop.Takeup_{jc} + \beta_2 X_{ijc} + \beta_3 \bar{X}_{jc} + \pi_3 Workweek_{ijc} + u_{ijc} \quad (3.2)$$

where $Prop.Takeup_{jc}$ is the proportion of peers in unit j who get vaccinated and $\bar{X}_{jc}$ are the average observable characteristics of peers j. Manski (1993) shows that if we estimate equation (2) by OLS, self-selection, common environmental factors and reflection confound the true peer effects β_1 and β_3. However, in our design employees are randomly assigned to vaccination on the workweek independently of their unit. This creates an exogenous variation that affects the proportion of peers who get vaccinated independently of employee

i's decision to get vaccinated because by chance some units have more employees assigned to vaccinate in the workweek than other units. We can average equation 3.2 across unit j to obtain the first stage equation:

$$Prop.Takeup_{jc} = \frac{\gamma_c}{1-\beta_1} + \frac{\beta_2+\beta_3}{1-\beta_1}\bar{X}_{jc} + \frac{\pi_3}{1-\beta_1}Prop.Workweek_{jc} + \frac{\bar{u}_{jc}}{1-\beta_1} \quad (3.3)$$

where the proportion of peers in unit j who get vaccinated is a function of the proportion of peers who were randomly assigned to the workweek ($Prop.Workweek_{jc}$). Random assignment implies that $Prop.Workweek_{jc}$ is uncorrelated with both $\bar{X}_{jc}$ and $\bar{u}_{jc}$. Hence, the reduced form equation is

$$Takeup_{ijc} = \frac{\gamma_c}{1-\beta_1} + \frac{\beta_1\beta_2+\beta_3}{1-\beta_1}\bar{X}_{jc} + \frac{\beta_1\pi_3}{1-\beta_1}Prop.Workweek_{jc} + \pi_3 Workweek_{ijc} + \tilde{u}_{ijc} \quad (3.4)$$

In our design, the exclusion restriction holds because the proportion of peers that got vaccinated is the only channel through which the proportion of peers assigned to the workweek can affect the individual's vaccination decision. Hence, we can combine the estimates from equations (3.3) and (3.4) to obtain an IV estimate of the effect of the proportion vaccinated peers on the employee's take-up. The error term in equation (3.4) includes both the individual error from equation (3.2) and the average error from equation (3.3), so we cluster the standard errors at the unit level.

Panel A in Table 3.3 presents the main results. The first stage estimate in Column 1 indicates that a ten-percentage-point increase in the proportion of peers assigned to the workweek increased by 3.1 percentage points the proportion of peers that get vaccinated. The F-statistic is 16.48, so according to the results of Stock and Yogo (2002), the instrument is relevant. The estimates in columns 2-4 show that peer vaccination has a positive effect on individual take-up and that not accounting for endogeneity biases the effect downwards. The IV estimate in Column 4 indicates that a ten percentage points increase in the proportion of peers that get vaccinated increased take-up by 7.9 percentage points. The results are robust

Table 3.3: Effect of Peer Vaccination on Individual Take-up

	First Stage	Reduced Form	OLS	2SLS
		A. Main Effect		
Proportion of Peers:				
Assigned to the Workweek	0.3106***	0.0025***		
	(0.0765)	(0.0008)		
Vaccinated			0.0051***	0.0079***
			(0.0007)	(0.0018)
F-value	16.481			
N	1138	1138	1138	1138
		B. Heterogenous Effects		
Proportion of Peers:				
Same Gender Vaccinated			0.0041***	0.0076***
			(0.0008)	(0.0019)
Different Gender Vaccinated			0.0038***	0.0048*
			(0.0009)	(0.0025)

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

Notes: Standard errors clustered at the unit level in parentheses. The bank has 116 units with more than one employee. This table presents the effect of peers' vaccination take-up on the individual's vaccination decision. The estimates represent the effect of a one percentage point change in the proportion of peers. All estimates control for Quito fixed effects and individual assignment to the workweek. Panel A presents the main results. Column 1 presents the results for the first stage. Column 2 displays the results of the reduced form. Column 3 presents OLS estimates of the effect of a change in the proportion of peers that get vaccinated. Column 4 presents 2SLS estimates of the effect of a change in the proportion of peers that get vaccinated. Panel B reports 2SLS estimates of heterogeneous effects. For each row the instrument is the corresponding proportion of peers assigned to the workweek, the first stages have F-statistics greater than 10.

to controlling for the total number of people in the unit and for mean age and gender of the peers (Appendix Table B.4).[24]

[24]Mechanically, smaller units may have larger proportions. We also control for the proportion of peers in the unit who have some managerial position. The point estimates are not affected by including this control variable.

Table 3.4: Potential Mechanisms for Peer Effects

	Effect on	Baseline	N
a. Beliefs about the Flu, its Vaccine, and Interactions with Coworkers			
Vaccines Effective to Improve Health (1-5)	-0.0017	3.87	378
	(0.0049)		
Talked with coworkers about getting vaccinated (pp)	-0.0065***	1.07	360
	(0.0021)		
Went with coworkers to get vaccinated (pp)	0.0009	0.06	360
	(0.0014)		
Probability of Getting Healthy Without the Vaccine (0-100)	0.0010	44.17	367
	(0.0722)		
Probability of Getting Healthy With the Vaccine (0-100)	0.0319	54.00	367
	(0.0909)		
Informed about the Flu (0-100)	0.0098	69.03	372
	(0.0723)		
Informed about the Flu Vaccine (0-100)	0.0079	63.09	372
	(0.0977)		
Afraid of the Flu (0-100)	0.0452	33.69	372
	(0.1232)		
Afraid of the Flu Vaccine (0-100)	0.0959	17.20	372
	(0.1173)		
Would Get Vaccinated out of the Workplace (pp)	-0.0025	0.81	367
	(0.0020)		
Coworkers Convinced me to get Vaccinated (0-100)	0.0246	18.70	360
	(0.1266)		
I Convinced my Coworkers to get Vaccinated (0-100)	-0.0622	33.18	360
	(0.1343)		
b. Heterogeneous Effects for Extrinsic and Intrisic Motivated Individuals			
Vaccination of Extrinsic Motivated Individuals (pp)	0.0045***	0.24	247
	(0.0012)		
Vaccination of Intrinsic Motivated Individuals (pp)	0.0006	0.16	262
	(0.0017)		

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the reduced form effect of peers assigned to the workweek on a series of outcomes identified by the row headers. The measurement unit of each outcome is in parentheses next to the outcome's name. The estimates represent the effect of a one percentage point change in the proportion of peers. We define peers as all employees who work in the same unit. All estimates control for Quito fixed effects and individual assignment to the workweek.

The positive peer effect on individual take-up suggests that peers might be changing personal beliefs about vaccination or that individuals follow behavior that they deem socially acceptable. To disentangle these potential channels, we first explore how individual take-up responds to peers of similar or different gender. There is evidence that individuals react more to peers of similar characteristics (Akerlof and Kranton, 2000; Hoffman et al., 1996; Perkins, 2002). Thus, if peers with similar characteristics have a larger effect on take-up than peers with different characteristics, this would suggest that following social norms may be the mechanism behind the positive peer effect on take-up. Panel B in Table 3.3 shows that ten percentage points increase in the proportion of peers of the same gender who get vaccinated increased take-up by 7.6 percentage points. This effect is almost identical to the main estimate and is driven by men. The effect of peers of a different gender is 37 percent smaller and is not significant.

To study if peers might be changing personal beliefs about vaccination more directly, we exploit the post-intervention survey questions on beliefs and knowledge of flu vaccines and interactions with coworkers related to vaccination. Even though answering the post-intervention survey is not correlated with treatment assignment (Table 3.1), the first stage loses precision due to the smaller sample size in the survey. We focus on reduced form analyses to prevent issues with finite sample bias in the IV estimate. Panel A in Table 3.4 shows the results on a set of 12 outcomes. The proportion of peers assigned to the workweek only had a negative and significant effect on talking with coworkers about vaccination.[25] This negative effect could be a consequence of the fact that employees expect that events organized by the bank take place during the workweek, so they are less likely to mention this to their coworkers.[26] There is no significant effect on any of the questions regarding information or beliefs about the vaccine, nor on questions that measure how much coworkers influenced the vaccination decision. Moreover, the point estimates are small compared to

[25] This effect is robust to adjusting for the false discovery rate (FDR) as in Anderson (2008).

[26] Additionally, an employee who learns she is in a unit with a large proportion of assigned to *Saturday* might feel lucky that she was assigned to the workweek and get vaccinated. This would bias downwards the estimate of the effect of the proportion of vaccinated peers on take-up in Table 3.3.

the baselines, which suggests that peer behavior did not affect beliefs nor supplied new information about the vaccine and is not the driver of the positive peer effect we find.

To further test if employees following behavior that they deem socially acceptable is the driver of the peer effects on vaccination take-up, we check if extrinsically motivated employees are more likely to be affected by their peers. Intuitively, extrinsically motivated individuals are more likely to respond to stimuli from their surrounding environment, which implies that they should be more likely to follow their peers. The pre-intervention survey has questions to determine if employees are intrinsically or extrinsically motivated.[27] Panel B in Table 3.4 shows the reduced form effect of the proportion of peers assigned to the workweek on these subgroups. For extrinsically motivated employees, a ten percentage points increase in the proportion of peers assigned to the workweek would increase take-up by 4.5 percentage points, while intrinsically motivated employees' take-up would increase by only 0.6 percentage points. The difference between the subgroups is significant at the 5 percent level. These estimates suggest that the estimated peer effects are a consequence of individuals conforming with the norms of their work group.

3.5 Analysis of the Effects of Vaccination on Health and Risky Behavior

In this section, we exploit random assignment to get vaccinated in the workweek as an instrument to study if flu vaccination improved health by reducing cases of employees diagnosed sick days in our intervention. In order to shed light on one of the potential mechanisms underlying the health results, we then use the same approach to explore if getting vaccinated can induce moral hazard.

3.5.1 Effects of Flu Vaccination on Health

Flu vaccines may affect health through multiple avenues, direct and indirect. First and foremost, getting vaccinated could have a direct effect on health by increasing immunity

[27]The intrinsic motivation measure is a dummy variable based on a median split of a summation of four measures of motivation in the workplace where employees state how important is that they (i) learn something interesting, (ii) get motivated to think about things, (iii) gain a thorough understanding of content and (iv) feel that their opinions are considered.

against four strands of the flu virus. Besides, the results in the previous section show that if a person gets vaccinated, the likelihood that her close peers get vaccinated increases. This effect would imply that an employee's close peers are more protected against the flu, which may decrease the transmission rate of the disease. Thus, positive peer effects on vaccination take-up could create an indirect channel through which getting vaccinated might have a positive effect on health. While the overall vaccination rate in the firm's 142 units is far too low to provide herd immunity (see Table 3.1), the proportion of vaccinated peers by unit ranges substantially between 0 and 67 percent.[28] Thus, in some units, the proportion of vaccinated peers may be high enough to provide some protection. Ideally, we could estimate the effect of flu immunization on health outcomes (Y_{ijc}) - medical diagnoses and sick days- through these two channels as:

$$Y_{ijc} = \alpha + \gamma_c + \theta Takeup_{ijc} + \delta Prop.Takeup_{jc} + v_{ijc} \tag{3.5}$$

However, vaccination take-up and the proportion of peers who get vaccinated are potentially endogenous. For example, individuals with healthier lifestyles could be more likely to vaccinate and less likely to need a sick day, so the estimates of equation (3.5) by OLS would be biased downwards. Thus, we instrument take-up with an indicator of assignment to vaccination during the workweek, and we instrument the proportion of vaccinated peers in the unit with the proportion of peers assigned to the workweek. The first stage equations have F-statistics of 6.6 and 8.9, respectively, implying that IV estimates of equation (3.5) may have a problem of finite sample bias (Stock and Yogo, 2002). Thus, we focus on the reduced form estimates of regressing the health outcomes on the instruments, given that those estimates are valid.

Panel A in Table 3.5 presents the effects of flu vaccination on the probability of having a sick day between November 2017 and February 2018. The OLS estimate in Column 1 sug-

[28]Appendix Figure B.10 displays the number of employees by unit. The CDC and WHO indicate that vaccination rates over 75 percent grant herd immunity.

Table 3.5: Effects of Vaccination on Overall Sick Days

	OLS	Reduced Form	2SLS
a. Having a Sick Day			
Assigned to the workweek		0.0132	
		(0.0361)	
Prop. peers assigned to the workweek		0.00003	
		(0.0010)	
Vaccinated	-0.0407		0.2404
	(0.0298)		(0.7280)
Prop. peers vaccinated	0.0004		-0.0022
	(0.0009)		(0.0074)
Baseline (percentage points)		0.29	
b. Number of sick days			
Assigned to the workweek		-0.2610	
		(0.6195)	
Prop. peers assigned to the workweek		-0.0140	
		(0.0147)	
Vaccinated	-0.5114*		-3.9719
	(0.2899)		(12.2730)
Prop. peers vaccinated	-0.0075		-0.0137
	(0.0082)		(0.1272)
Baseline (days)		1.29	
N		1,120	

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the effects of flu vaccination on the probability of having a sick day in general between November 12, 2017, and February 28, 2018. Column 1 presents OLS estimates. Column 2 presents the reduced form estimates. Column 3 presents 2SLS estimates. The first panel presents the effect on the probability of having a sick day, and the second panel presents the effect on the number of sick days. The estimates include only units with two or more employees.

gests that getting vaccinated decreased the probability of having a sick day by 4.1 percentage points (14 percent of the baseline), although the effect is insignificant. Conversely, the reduced form estimates in Column 2 imply that getting vaccinated did not affect the probability of having a sick day. Being randomly assigned to the workweek, which increases vaccination take-up, increased the probability of having a sick day by 1.3 percentage points (5 percent of the baseline), insignificantly at conventional levels. Additionally, the results in columns 1 and 2 indicate that the proportion of vaccinated peers does not affect the probability of having a sick day. Panel B shows the effects of flu vaccination on the number of sick days. The OLS correlation suggests that vaccination decreases sick days, which is significant at the 10 percent level. However, the reduced form effect is no longer significant and sensitive to the presence of outliers.[29]

Total sick days include many diseases over which the flu vaccine has no immunity benefit. To address this issue, we exploit the data on medical diagnoses and estimate the effect of vaccination on both the probability of being diagnosed with the flu (Table 3.6 Panel A) and the probability of having a sick day because of the flu (Table 3.6 Panel B). The OLS estimates in Column 1 suggest that getting vaccinated decreases the probability of being diagnosed with the flu. However, the reduced form estimate in Column 2 indicates that being assigned to the workweek increased the probability of being diagnosed with the flu by 0.4 percentage points (9 percent of the baseline), not significant at conventional levels. This result further suggests that getting vaccinated was ineffective to decrease the probability of having the flu. Also, the estimates in columns 1 and 2 show that the proportion of vaccinated peers do not affect the probability of being diagnosed with the flu, which suggests that vaccination rates are too low to provide herd immunity. Thus, we drop the proportion of vaccinated peers in the following analyses.

Panel B presents the effects of assignment to the workweek on the probability of having a sick day because of the flu. These results are qualitatively the same as the effects on the probability of being diagnosed with the flu. The confidence interval of the effect of being assigned to the workweek rules out negative effects larger than 0.5 percentage points. In

[29]Sick days include severe illnesses, such as cancer, which leads to large numbers of sick days not related to the flu. If we exclude these outliers, the coefficient of the reduced-form changes and becomes positive, in line with our finding in panel A of Table 3.5 on the probability of having a sick day or not. Also, the effects do not change if we take out the proportion of peers and estimate only the individual effect of vaccination.

Table 3.6: Effects of Vaccination on Flu Diagnoses and Sick Days

	OLS	Reduced Form	2SLS
a. Being Diagnosed with the Flu			
Assigned to the workweek		0.0044	
		(0.0155)	
Prop. peers assigned to the workweek		-0.0003	
		(0.0006)	
Vaccinated	-0.0254*		0.1103
	(0.0151)		(0.2978)
Prop. peers vaccinated	-0.0001		-0.0020
	(0.0004)		(0.0033)
Baseline (percentage points)		0.05	
b. Granted a Sick Day because of the Flu			
Assigned to the workweek		0.0112	
		(0.0083)	
Prop. peers assigned to the workweek		-0.0002	
		(0.0003)	
Vaccinated	-0.0156		0.2309
	(0.0110)		(0.2194)
Prop. peers vaccinated	0.000003		-0.0026
	(0.0002)		(0.0025)
Baseline (days)		0.02	
N		1,120	

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the effects of flu vaccination on the probability of being diagnosed sick and being granted a sick day because of the flu. Column 1 presents OLS estimates. Column 2 presents the reduced form estimates. Column 3 presents 2SLS estimates. The estimates include only units with 2 or more employees.

particular, we can rule out a negative effect of 2.2 percentage points that correspond to the CDC's estimate of the effectiveness of the 2017-2018 flu vaccine.[30]

[30]In Appendix Table B.5 we check the robustness of these results to the inclusion of controls (gender, age, tenure and income) and to using a broader definition of flu-related illness, thereby increasing case numbers. The results are robust to these checks. Also, the results are robust to using a negative binomial or Poisson regression.

3.5.2 Can Getting Vaccinated Cause Moral Hazard?

The previous results imply that vaccinating employees against the flu appears to be ineffective. A simple explanation could be that the 2017-2018 vaccine did not match the prevailing flu strains in that particular flu season. The flu vaccine grants protection against three or four strands of the flu virus. If the vaccine does not match the prevailing strands of the flu virus, then vaccination would be ineffective in improving health. Taking into account that the quality of the flu vaccine could vary by year and by country, the bank and its employees may have had just bad luck. While our design does not allow us to test if the flu vaccine was immunologically effective, we can study if getting vaccinated induces people to engage in riskier practices, which may separately contribute to decreasing the effectiveness of flu vaccination.

As a first empirical test of the idea of behavioral changes due to flu vaccination, we inspect effect heterogeneity. The medical effectiveness of the vaccine does not depend on employee characteristics. Thus, if there is no change in behavior, assignment to the workweek should not have different effects across subgroups defined by gender, age, or having children. However, Appendix Table B.6 shows that assignment to the workweek increased the probability of having a flu-related sick day among subgroups who are more likely to be exposed to children, who are more likely to have the flu.

Vaccinated individuals could overestimate the protection of the vaccine and engage in riskier behaviors. As a consequence, it is possible that vaccinated people avoid going to the doctor or wait longer than unvaccinated people to do it when they feel flu-like symptoms. Also, vaccinated individuals could take fewer protective measures, such as washing hands less frequently, and these changes in behavior would expose individuals more to strands of the flu virus that the vaccine might not cover.

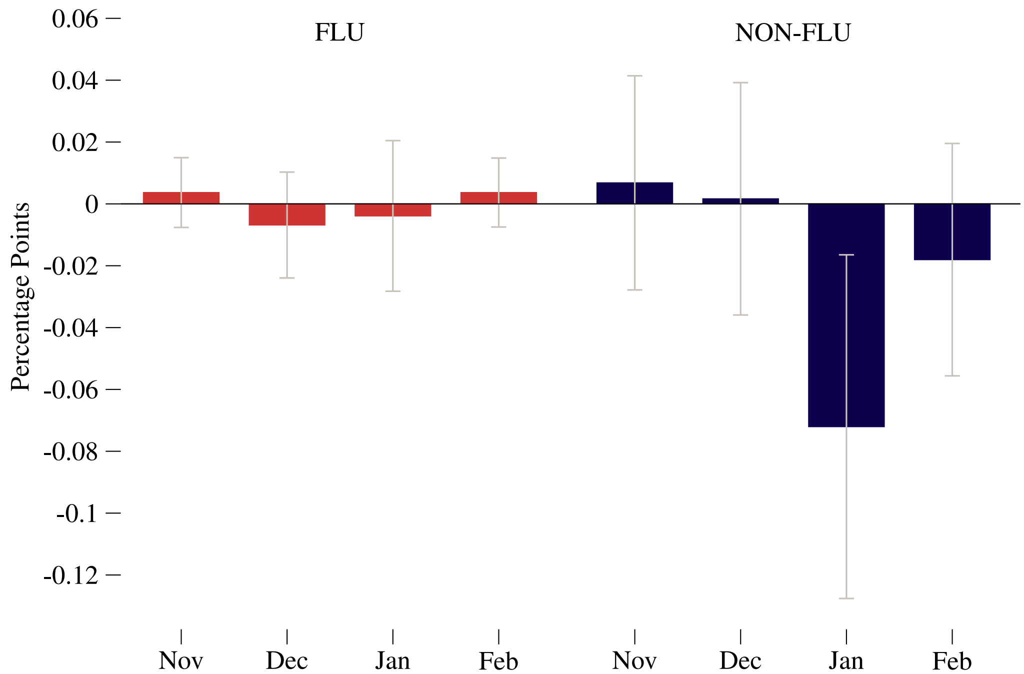
To further explore if flu vaccination may cause a moral hazard problem, we test if getting vaccinated makes people react differently than those unvaccinated when they feel flu-like symptoms. The idea here is that non-flu respiratory diseases have symptoms similar to the flu, but the vaccine does not provide any immunity benefit to prevent them. Thus, flu vaccination should not affect the probability of being diagnosed with a non-flu disease, so any effect on this probability would imply a change in how individuals react when becoming sick with a respiratory disease. In particular, if vaccinated employees felt more protected, they might have been less likely to go to the doctor when they felt flu-like symptoms, decreasing the probability of being diagnosed with a non-flu disease. In particular, this would concern cases of mild illnesses where it is up to the individual to decide to go to a doctor or not.

To implement this test, we exploit the richness of the data that differentiates between flu and non-flu respiratory diagnoses by exploiting a policy intervention of the Ecuadorian government that happened in our investigation period. In January 2018, as a result of a significant increment of flu cases nationwide, the Ecuadorian government launched a massive media campaign asking the population to go to the doctor if they felt any flu symptoms. If vaccinated individuals felt protected, we argue that they may not have followed the government's recommendation, resulting in fewer visits to the doctor and fewer non-flu diagnoses only in that month.

We estimate the reduced-form effects of vaccination by month during our investigation period. Figure 3.2 presents the effects of being assigned to the workweek on flu and non-flu diagnoses. As the main result, assigning employees to the workweek does not affect the probability of being diagnosed with the flu in any month. The point estimates are smaller than 0.7 percentage points in magnitude and insignificant at conventional levels. These results further confirm that vaccination was ineffective.

Regarding non-flu diagnoses, if vaccination did not induce people to feel more protected, we would expect to find no effect on the probability of being diagnosed with a non-flu respiratory disease. This is true in November, December, and February. However, in January when the government asked people to go to the doctor, being assigned to the workweek decreased the probability of being diagnosed with a non-flu respiratory disease by 7.2 per-

Figure 3.2: Reduced Form Estimates of the Effect of Vaccination on Diagnosed Sickness

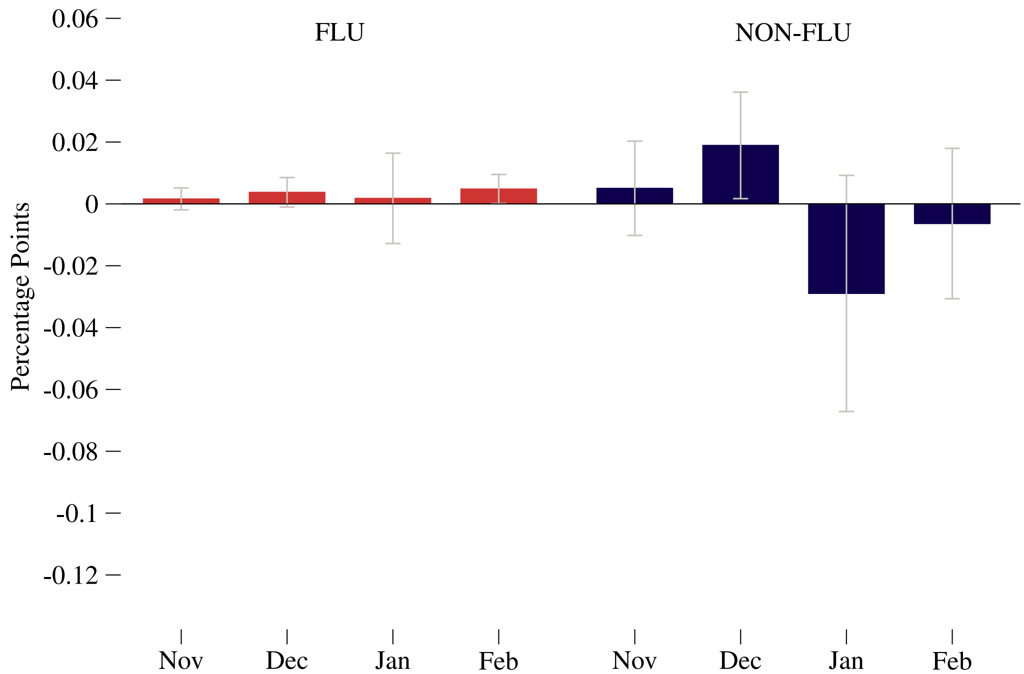


Notes: This figure presents the reduced form effect of being assigned to the workweek on the probability of being diagnosed sick by month. The left panel presents the effect of assignment to vaccination on the workweek on flu diagnoses, and the right panel presents the effect of assignment to vaccination on the workweek on non-flu respiratory diagnoses. The figure presents the point estimate and the 95 percent heteroscedastic robust confidence interval. November includes cases of diagnosed sickness detected since November 12, after the vaccination campaign.

centage points.[31] This result suggests that employees assigned to the workweek, who were more likely to get vaccinated, felt protected and went less to the doctor when they felt flu-

[31]We also estimate the effect of assignment to the workweek collapsing the data of the four months to a cross-section. In this specification, being assigned to the workweek decreased the probability of being diagnosed with a non-flu respiratory disease by 7.5 percentage points (Appendix Table B.7), almost identical to the effect in January. Another check concerns the fact that the data on sickness diagnoses correspond to employees who went to the onsite doctor or to an external doctor during working hours, while employees who went to an external doctor outside working hours, who were diagnosed sick but were not granted a sick day, are coded as healthy. This measurement error will not bias the previous estimates as long as it is uncorrelated with assignment to the workweek. However, if employees assigned to get vaccinated during the workweek are more likely to go to an external doctor after work, then this would overestimate the effect on non-flu respiratory diagnoses. We bound the effect to address this potential concern (Lee, 2009). First, we calculate the treatment-control difference in the proportion of healthy individuals. Then, we trim this difference from the control group (assigned to vaccination on *Saturday*) to obtain an upper bound, and we trim this difference from the treatment group (assigned to vaccination on the workweek) to obtain a lower bound. Appendix Table B.7 presents these results. The effect of being assigned to the workweek is always negative and bounded between 5.4 and 9.8 percentage points.

Figure 3.3: Reduced Form Estimates of the Effect of Vaccination on Sick Days

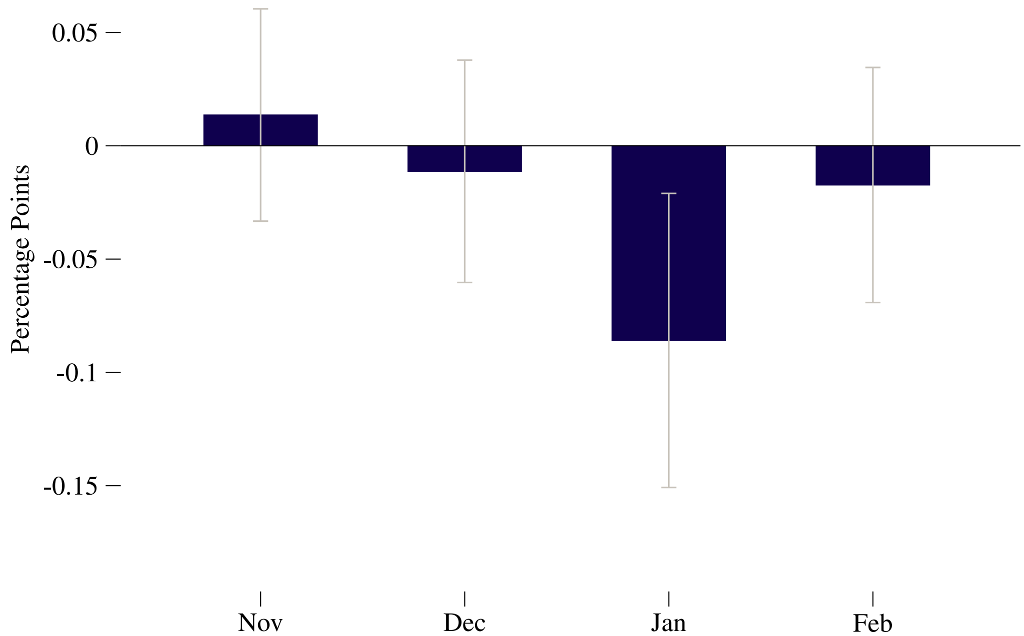


Notes: This figure presents the reduced form effect of being assigned to the workweek on the probability of being granted a sick day by month. The left panel presents the effect of assignment to vaccination on the workweek on flu sick days, and the right panel presents the effect of assignment to vaccination on the workweek on non-flu respiratory sick days. The figure presents the point estimate and the 95 percent heteroscedastic robust confidence interval. November includes sick days granted since November 12, after the vaccination campaign.

like symptoms. These estimates are consistent with the hypothesis of riskier behavior among vaccinated individuals, as they appeared to think that they are protected against the flu.

Figure 3.3 presents the reduced form effects of the assignment to the workweek on the probability of having a sick day because of the flu and other non-flu respiratory diseases. These results answer the question of whether cases of diagnosed sickness that we observe in Figure 3.2 were severe enough also to get a sick day granted. The results in Figure 3.3 are qualitatively similar to the effects on the probability of being diagnosed with these illnesses but less precise. Assigning employees to the workweek did not affect the probability of having a flu-related sick day. Regarding non-flu respiratory diseases, the point estimates are consistent with the results in Figure 3.2. While assigning employees to the workweek in-

Figure 3.4: Reduced Form Estimates on the Probability of Going to the Onsite Doctor



Notes: This figure presents the reduced form effect of being assigned to the workweek on the probability of going to the onsite doctor. The figure presents the point estimate and the 95 percent heteroscedastic robust confidence interval. November includes sick days granted since November 12, after the vaccination campaign.

creased the probability of having a non-flu respiratory sick day in December, the probability decreased in January by 2.9 percentage points. This reduction corresponds to the finding in Figure 3.2 but suggests that the vaccinated are less likely to go to the doctor in the presence of mild flu symptoms.

We can also check if vaccination affects the likelihood of going to the on-site doctor. The bank's on-site health center is a convenient feature for its employees because they do not have to ask for time off to go to the doctor as they can take a few minutes of their work time to go to the health center. Before the intervention, the on-site doctors account for 77 percent of all cases of diagnosed sickness. If vaccinated individuals felt more protected, they may have been less likely to visit these doctors when the government launched its media campaign. Figure 3.4 presents the effects of assigning employees to the workweek on the probability of going to the on-site doctor by month. There was no significant effect in

November, December, and February. In January, being assigned to the workweek for vaccination decreased the probability of going to the onsite doctor by 8.6 percentage points (21 percent of the baseline).

Table 3.7: Reduced Form Estimates on Health-Related Habits

	Baseline	Coefficient	N
Responses on scale from 1 ("never") to 10 ("all the time")			
How often do you exercise	5.93	-0.3145	359
		(0.4026)	
How often do you take dietary supplements	3.18	-0.6147	359
		(0.4372)	
How often do you carry an umbrella	6.85	-1.2190**	359
		(0.4856)	
How often do you wash your hands	9.25	0.0980	359
		(0.1836)	

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

Notes: Robust standard errors in parentheses. This table presents the reduced form effects of being assigned to the workweek on four daily habits and activities related to health and preventing the flu. Column 2 presents the reduced form estimates. Column 3 presents the number of individuals who answered the survey.

In our final test for moral hazard, we look at self-reported habits and cultural beliefs related to preventing the flu. In the post-intervention survey, the bank asked its employees how often they: (i) exercise; (ii) wash their hands; (iii) use an umbrella; and (iv) take nutritional supplements. Washing hands is a proven measure against the flu, exercising and taking nutritional supplement may improve overall health, and many people including Ecuadorians believe that carrying an umbrella helps to prevent the flu or other respiratory illnesses. Psychology research show that cultures across the world associate the fact that the flu virus survives longer on a cold and wet environment with the belief that people catch the flu by getting wet or cold (Au et al., 2008; Sigelman et al., 1993; Baer et al., 1999; Helman, 1978).[32]

[32] Also, since Quito is on the Equator Line, there are no marked seasons in the year. In Quito, temperatures in a day can fluctuate between the upper forties (°F) and the lower eighties (°F), and there are no accurate

Table 3.7 shows the effects of assigning employees to the workweek on these outcomes. Assigning employees to the workweek did not affect how often employees wash their hands (1 percent of the baseline), which is not surprising since they report that they wash their hands very frequently. Assigning employees to the workweek had a negative but statistically insignificant effect on how often employees exercise (5 percent of the baseline) and how often they take nutritional supplements (19 percent of the baseline). The effect on how often employees carry an umbrella is statistically significant. Being assigned to the workweek decreases the frequency of carrying an umbrella by 1.22 points (18 percent of the baseline) on a Likert scale where one means "never" and ten "all the time."[33] We can also investigate heterogeneous effects across individual's beliefs on the effectiveness of the vaccine using the pre-intervention survey. We find that the effect is driven by individuals who believe the vaccine is very effective to prevent the flu. Thus, this result suggests that vaccinated individuals feel protected, so they neglect other measures that they believe to be helpful in order to prevent respiratory illnesses.

3.5.3 Other Interpretations of the Results on Moral Hazard

In the previous section, we provide several pieces of evidence supporting the idea that flu vaccination caused a moral hazard problem. In the following, we discuss other interpretations of these findings focusing on whether other factors not related to moral hazard could explain these results.

Misdiagnoses could be a competing explanation. If doctors are not able to distinguish the flu from other non-flu respiratory diseases, then the non-flu cases could have been flu cases. If this were true, the results in Figure 3.2 would indicate that the vaccine was effective in January 2018 when the flu was prevalent in Ecuador. While we cannot directly observe how doctors diagnose the flu in the data, we observe diagnoses from 72 different doctors from different health centers and hospitals. It is unlikely that all doctors misdiagnosed the

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forecasts for rain.

[33]This effect is significant at the 5 percent level (p-value=0.012) and robust to adjusting for multiple comparisons following Anderson (2008).

flu. Also, the results are robust to using a broader definition of flu-related illness. Finally, misdiagnoses do not explain why vaccinated individuals are less likely to carry an umbrella as a cultural protective measure against the flu.

We could also think that doctors misdiagnose conditional on whether a person got vaccinated or not. When a doctor learns that a person who shows flu-like symptoms got vaccinated, the doctor might be more likely to misdiagnose those symptoms as a non-flu respiratory disease. However, the results in Figure 3.2 show that employees assigned to the workweek, who are more likely to get vaccinated, were diagnosed less with non-flu respiratory diseases.

Finally, an alternative to moral hazard is the idea of adverse selection: employees with higher risk tolerance regarding health are more likely to get vaccinated and to engage in risky health behavior. However, adverse selection cannot be a driver of our results because we use an exogenous source of variation on take-up. The marginal individual who gets vaccinated is a person who would not have gotten vaccinated if assigned to *Saturday*. This variation is uncorrelated with the underlying risk preferences of employees that could determine adverse selection.

3.6 Conclusions

Individual behavior may threaten the success of health interventions in multiple ways. First and foremost, individuals can decide not to participate. In this paper, we find that reducing opportunity costs has a substantial effect on participation in a vaccination campaign in the context of employees in working age who live in locations where access to vaccines is not an issue, as in most major cities in both developing and developed countries, and who are not affected by income constraints. Previous research finds effects of similar magnitudes in rural areas in developing countries (Banerjee et al., 2010; Sato and Takasaki, 2018) or populations with income constraints (Bronchetti et al., 2015). Regarding the health benefits of the intervention in our study, flu vaccination did not have a significant effect on any of our outcomes. While we cannot rule out that the flu vaccine was medically ineffective, we find evidence consistent with individuals adopting riskier behaviors after getting vaccinated. Moral hazard constitutes a second way through which individual behavior could limit the effectiveness of health interventions.

Our study provides several pieces of evidence that speak to the idea of riskier behaviors regarding health among vaccinated individuals. We argue that getting the vaccine is not relevant to determine the effects we find on diagnosed non-flu respiratory diseases, where the vaccine has no immunity effect. It appears that employees made different decisions about whether to go to the doctor or not, depending on being vaccinated. Furthermore, survey evidence on differences in the likelihood of carrying an umbrella illustrates potential changes in health-related behaviors following vaccination. These results suggest that getting vaccinated could cause moral hazard. Forgoing other protective measures and increasing risky behaviors could partially explain the ineffectiveness of vaccination and could help understand better why health interventions may sometimes fail. Regarding the interpretation of the health effects in our study, moral hazard undermining the effectiveness of vaccination is consistent with quasi-experimental evidence on the effectiveness of flu vaccination. For example, Ward (2014) finds for Canada that: (i) flu vaccination increased sickness absences in years when the flu vaccine had a bad match with the prevalent flu viruses; and (ii) flu vaccination had no effect in years when the flu vaccine had a good match with the prevalent flu viruses. The difference between these two results, which would control for moral hazard, points to the immunological benefits of the vaccine.

To answer the question of whether the vaccination campaign was economically successful for the company carrying out this health intervention, we can perform a back-of-the-envelope calculation of the net benefit of this campaign. This analysis has the limitation that we are not able to fully quantify all of the possible effects that vaccination may have on outcomes relevant to the bank, like morale and productivity.[34] Our calculation suggests that

[34] A channel pertaining to company morale is the perception of individuals that the company cares more about their health when assigned to the work-week which leads them to behave differently. However, we cannot find evidence for that channel using data on organizational perceptions from our post-survey. Appendix Table B.8 presents imprecise estimates on self-reported productivity and the duration of the workday measured

the net benefit of the campaign was negative regarding sick days. In the best-case scenario, the treatment may result in a net gain of \$0.17 regarding gains in work attendance during the flu season, which is not enough to compensate the bank for its costs that include vaccine subsidies of \$2.57, \$5.05 and \$9.99 per vaccine.[35]

Our study allows us to draw practical implications for health interventions in at least two regards. The presence of moral hazard in health-related behavior implies that firms and policymakers should consider this phenomenon in the design of interventions like vaccination campaigns. A promising mechanism to mitigate it could be to increase awareness that the proposed measure, such as flu vaccination, does not guarantee a 100 percent protection against illnesses. It might be necessary to remind people to continue making use of other protective measures against respiratory viruses and bacteria, instead of letting them rely only on the protection potentially provided by medical technology.

Another lesson learned from our investigation is how to raise participation in health interventions. In this paper, we could find two cost-effective measures that increase vaccination take-up in a workplace context where monetary aspects do not seem to play a significant role in people's willingness to participate in a health campaign. Decreasing opportunity costs is one option to increase participation drastically, which suggests using mobile campaigns in days and locations where people usually congregate. Also, since we find that peer behavior has an important effect on vaccination take-up, and that following social norm is the potential mechanism, employers can increase participation in health campaigns by using mechanisms to incentivize groups of employees. Small rewards for the entire unit when the unit takes

by the employees' magnetic cards swipes to enter and exit the bank. The point estimates suggest that assigning employees to get vaccinated in the workweek increased their perception on their productivity, while decreased the duration of their workday by about a third of an hour. Given that the bank pays a fixed salary, these effects could suggest an increase in productivity. However, in the absence of more precise measures of productivity, we cautiously conclude from this analysis that there is no sizable productivity premium. One could argue that from the perspective of a company, sick days have higher economic relevance, given that this often goes along with re-assignment of tasks, compared to when some employees are able to finish tasks and leave earlier than others.

[35] The estimate's confidence interval implies that at most assigning employees to the workweek could decrease the likelihood of having a flu sick day by 0.5 percentage points. We take the median wage of the bank (\$750), divide it by the average number of work days in a month (22), and we multiply this value by 0.005.

part could have significant effects on participation rates. Evaluating the role of such peer incentives in health-related contexts is a promising area for future research.

4. A BLESSING OR A CURSE? THE LONG-TERM EFFECT OF RESOURCE BOOMS ON HUMAN CAPITAL AND LIVING CONDITIONS

4.1 Introduction

Is resource abundance a blessing or a curse for a country? A priori, we would expect that natural resources boost economic development, but, since the work of Sachs and Warner (1999, 2001), there is ample suggestive evidence that resource-rich countries tend to underperform in several dimensions.[1] Of particular concern is the possibility that resource booms reduce human capital accumulation. These booms are the product of a combination of discoveries, technological changes, and demand shocks that affect prices. These changes may affect labor market conditions favoring low-skill occupations.[2] Standard human capital accumulation models (Becker, 1964; Black et al., 2005b; Charles et al., 2015) show that an increase in productivity in low-skill occupations increases the opportunity cost of going to college and decreases the returns of education. Thus, during a resource boom, it might be optimal for some individuals to drop out of high school/college and enter the workforce.

However, economic theory does not predict whether these effects are temporary or permanent. On the one hand, if individuals anticipate that the resource boom is temporary, they could plan to return to school at a later date. On the other hand, as time passes, events such as marriage or having children impose costs on returning to school. Also, the time horizon for the returns of education to realize decreases. These two factors make it less likely for individuals to resume their education. Hence, the decrease of human capital could be

[1]There is a large litearture that documents a negative cross-country correlation betwen resource abundance and economic growth (Sachs and Warner, 1999, 2001; Gylfason, 2001; Torvik, 2002; Papyrakis and Gerlagh, 2004, 2007; James and Aadland, 2011). More recent evidence suggests that the apparent negative correlation between resources and economic growth was the product of endogenous measures of resource abundance (Stijns, 2006; Smith, 2015). There is also country-level evidence that natural resource abundance is negatively correlated with educational attainment (Gylfason, 2001; Papyrakis and Gerlagh, 2004, 2007), although these results are sensitive to different measures of resource abundance (Stijns, 2006). See Van der Ploeg (2011) for a review of this literature.

[2]There is evidence that labor demand shocks from "fracking" favor the less educated (Bartik et al., 2017; Kearney and Wilson, 2017). In cases where the state owns mineral rights, government policies can facilitate the development of low-skill, labor-intensive occupations (De La Torre et al., 2015).

permanent.

In this paper, I use proprietary individual-level data to causally estimate the long-term effect of the 1970s' oil boom on educational attainment in the context of a developing country. In 1973, Ecuador started major oil production and its price skyrocketed due to the Arab embargo. In Ecuador, as in many countries, the state owns all mineral rights, so the government received a large influx of funds that it targeted to infrastructure spending and subsidies that affected the country uniformly (Acosta, 2006; World Bank, 1979a). Productivity in low-skill occupations increased 93 percent because subsidies and price controls lowered the cost of starting small businesses related to commerce and construction in the entire country (World Bank, 1979a). I estimate the reduced form effects of exposure to this shock on college completion measured 40 years after the oil boom.

I use an intensity difference-in-differences design that compares changes in outcomes across cohorts of individuals who turned 18 before and after 1973, to changes in outcomes across geographic regions with different costs of college attendance.[3] I focus on cohorts who by 1973 had already decided to go to college or not and compare them to cohorts that were still in high school and thinking about going or not to college. Specifically, I consider the cohorts who turned 18 between 1966 and 1979. Also, I show that differences in costs of attending college across regions imply that shocks that increase the opportunity cost of education affect these regions differently. Theoretically, individuals go to college when its benefits are greater than its costs. Lower costs allow students with lower ability and income to go to college. Hence, the marginal student who attends college from regions with low costs should have lower ability and lower income than the marginal student from regions with high costs. This implies that regions with low costs should have higher college attendance, and that its marginal student should be more sensitive to shocks that increase the productivity of low-skill jobs. Thus, exposure to the oil boom should affect college completion more in regions with low costs than in regions with high costs.

[3]Finkelstein (2007) uses a similar region-based approach to estimate the effect of the introduction of Medicare.

In Ecuador during the 1970s, differences in the costs of attending college stemmed from the fact that universities were located only in five cities of the country, with the two largest cities concentrating college supply.[4] This fact, together with drastic altitude differences across the country that increase transportation costs, implies that people born in regions without universities faced higher costs of college attendance than people born in regions with universities. I use regions without universities, which should be the least affected by the boom, as the baseline group and assign individuals to their region of birth to account for migration caused by the boom. Thus, this design recovers the change in college completion in the regions with universities in excess of the change of college completion in regions without universities. Thus, as long as the oil boom negatively affects college completion in the baseline regions, I recover a lower bound of the real effect.

I find that in the context of a developing country, exposure to a resource boom negatively affected completed educational attainment. This represents a stark contrast with studies of the effect of resource booms on educational attainment in the context of developed countries, which find that exposure to resource booms decreased high school enrollment in resource-rich areas in the short-term (Black et al., 2005b; Cascio and Narayan, 2017) but have no effect on completed educational attainment (Emery et al., 2012).[5] In contrast, I find that exposure to the oil boom before turning 18 decreased college completion in the long-term. Consistent with the previous theoretical predictions, exposure to the oil boom had heterogeneous effects across regions, and the decrease in college completion was driven by the cities that concentrated the majority of universities at the time (*Quito* and *Guayaquil*). In these cities, exposure to the oil boom before turning 18 decreased college completion by 2.9 per-

[4]No new universities opened around the time of the oil boom and most universities were public and free at the time. I use the terms *college* and *university* interchangeably.

[5]There is a larger quasi-experimental literature that considers the effect of natural resource shocks on: (i) non-resource economic activity (Black et al., 2005a; Michaels, 2011; Marchand, 2012; Allcott and Keniston, 2017); (ii) participation in disability programs (Black et al., 2002); (iii) family income and children education (Løken, 2010; Løken et al., 2012), (iv) effect of income on health spending (Acemoglu et al., 2013); and (v) public expenditure and corruption (Caselli and Michaels, 2013). A related literature studies the effects of shocks that increase the productivity of low-skill occupations, such as housing booms (Charles et al., 2015), large infrastructure projects (Carrington, 1996), technological changes (Fetzer, 2014; Bartik et al., 2017; Feyrer et al., 2017), and recessions (Kahn, 2010; Oreopoulos et al., 2012).

centage points, which represents 12.2 percent of baseline college completion for those who turned 18 just before the oil boom.

The long-term reduction of college completion is consistent with a model of rational individuals who reduce their educational attainment in response to lower returns of education in the long-run. I provide four pieces of evidence that support this potential mechanism. First, exposure to the oil boom increased college completion by 4.5 percentage points (58.5 percent of the baseline) in the Amazon region, where the boom had a direct positive effect on connectivity and local income because the oil fields are located there. A new highway connecting this region to the capital reduced the cost of attending college and spillovers from the oil industry into the local economy contributed to increasing employment in high-skill jobs from 25 percent in 1962 to 26.8 percent in 1982.

Second, consistent with higher productivity of low-skill jobs (World Bank, 1979a), employment in the country shifted towards these jobs after the oil boom and this change lasted long after the boom. Employment in commerce, low-skill services (food preparations, repairs, transportation, housekeeping), construction, and other low-skill occupations increased from 25.2 percent in 1962 to 39.4 percent in 1982 and 55.5 percent in 2010, while employment in manufacturing industries decreased from 13.6 percent in 1962 to 12.6 percent in 1982 and 10.1 percent in 2010. Consistently with the shifts in employments and the effect on education, I find that in the cities with full universities exposure to the oil boom before turning 18 increased the likelihood of working informally in 2012 by 0.8 percentage points (1.9 percent of the baseline) and decreased this probability by 1.7 percentage points (3 percent of the baseline) in the *Amazon* region.

Finally, the literature on the returns of education implies that lower educational attainment should have translated into lower wealth accumulation if the returns of education did not decrease in the long run.[6] However, I find that exposure to the oil boom before turning 18 did not affect two relevant measures of wealth: home ownership in 2010 and vehicle

[6]See Angrist and Krueger (1991), Card (1993), Harmon and Walker (1995), Card (1999), Card (2001), Duflo (2001), and Lemieux and Card (2001) for estimates of the effects of education on income.

ownership in 2013. The point estimates for home ownership in the cities with universities are smaller than 0.6 percentage points (1 percent of the baseline), and the standard errors rule out effects larger than two percentage points. Also, I find that exposure to the oil boom before turning 18 decreased vehicle ownership in these cities by 0.5 percentage points (2.8 percent of the baseline). Together, these results indicate that negative effects on wealth were limited, which suggests that the oil boom induced a long-term reduction of the returns of education.

This paper contributes to the literature on factors that affect the growth potential of developing countries. I show that exposure to the oil boom before turning 18 decreased educational attainment in the long-run. The results suggest that it was optimal for the exposed cohorts to interrupt their educational attainment. From an individual perspective, this does not necessarily imply that the boom was a blessing because it does not account the positive externalities of education. In particular, I find that exposure to the oil boom before turning 18 increased the number of children in the largest cities by 0.04 (1.7 percent of the baseline). This estimate, together with no apparent effect on wealth, suggests fewer resources per children that together with less educated parents may have affected their development. From the country's perspective, lower human capital levels may constrain the development of high-skill industries (Becker et al., 2011; Becker and Woessmann, 2010), which may hamper the country's long-term growth potential.

4.2 Stylized Facts about the Ecuadorian Oil Boom

In the early 1970s, Ecuador found oil in its *Amazon* region and started major oil production in 1972-1973. At the same time, Ecuador benefited from the rise of oil prices due to the Arab embargo in 1973-1974. We can see the effects of this oil boom in Figure 4.1. Ecuador's oil output increased from 0 to 28.6 million barrels in 1972.[7] In 1973, output more than doubled to 76.2 million barrels, and it fluctuated around this level in the rest of the decade. Also,

[7] At the beginning of the twentieth century, Ecuador found small oil deposits in its coast lands. The government leased this field to the Anglo-Ecuadorian Oil Company (now part of British Petroleum), who only paid taxes on its profits. Official statistics do not include output nor revenue from this field.

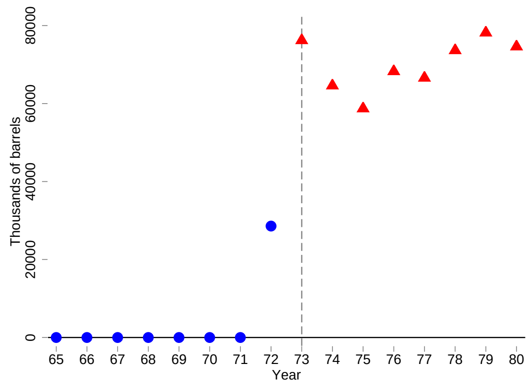
in 1974 oil's price raised from $4.20 per barrel to $13 per barrel (a 210 percent increase), and it remained at high levels until 1982.

These two shocks had profound implications for Ecuador's economy. GDP per capita increased from $503 in 1972 to $983 in 1974 (Figure 4.1, Panel e). Oil became a major source of fiscal revenue (Figure 4.1 Panels c and d) because in Ecuador the state owns all mineral rights and is the main actor in the oil industry. From 1973 to 1980, oil revenue represented 34 percent of total fiscal revenue, up from almost zero in the previous decade. Non-oil tax revenue continued to increase but at a slower rate. The World Bank (1979a) estimates that its share decreased from 14.4 percent of non-oil GDP in 1972 to 12 percent in 1977.

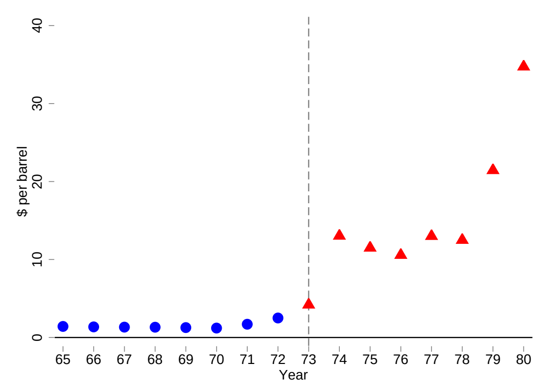
The government channeled these funds to new expenditures. According to Ecuador's Central Bank, the government's expenditures, mainly personnel expenses, increased 659 percent from 1972 to 1980. Capital expenditures grew 603 percent. These investments were focused on expanding the country's existing highway network and developing new infrastructure for the oil, electricity, agriculture (irrigation and storage), education, and health sectors (World Bank, 1979c). The government also financed interest rate subsidies for certain sectors; price controls on wages and agricultural products; and subsidies on gasoline and other fuels (World Bank, 1979a; Cisneros et al., 1988; Acosta, 2006). Trade policy included tariffs on imports of finished products and subsidized imports of intermediate products and capital goods (Larrea, 1989). All these transfers grew 851 percent from 1972 to 1980. Also, the abundance of oil funds facilitated an implicit transfer in the form of lax tax collection efforts.

Notably, the oil boom did not lead to an increase in the industrial sector's share in the economy. Until 1965, Ecuador's economy relied heavily on agriculture, in particular growing products for export. Ecuador was a major cacao exporter until 1917 and became the world's largest banana exporter since the 1950s (Acosta, 2006).

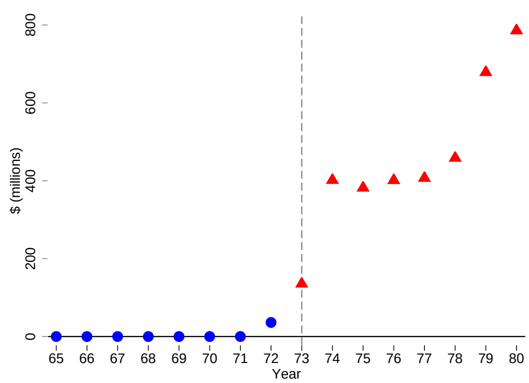
Figure 4.1: Ecuador’s Oil Boom



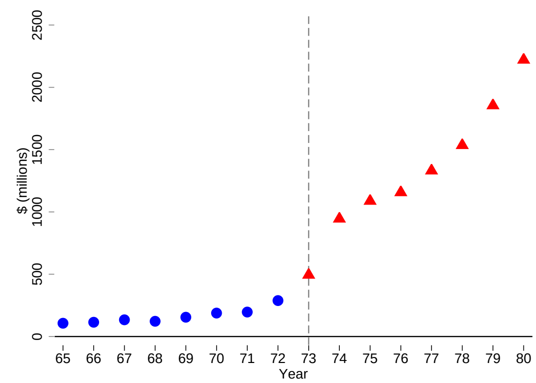
(a) Oil output



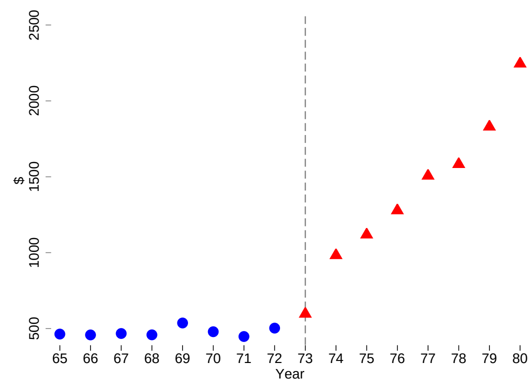
(b) Price ($ per barrel)



(c) Government’s Revenue (Millions of $)



(d) Government’s Oil Revenue (Millions of $)



(e) GDP per Capita

Notes: This Figure presents the evolution of Ecuador’s crude oil output, its price, the government’s revenue from oil exports, and GDP per capita from 1965 to 1980. Oil output data, government’s oil revenue and GDP per capita is reported by Ecuador’s Central Bank. Oil’s price from 1965 to 1971 is the average price of OPEC, from 1972 onward it corresponds to the average price of Ecuador’s oil exports as reported by Ecuador’s Central Bank.

In 1965, the country followed the rest of Latin America and adopted a series of policies to promote growth in the industrial sector by replacing imports of manufactured products with local production. Ecuador maintained these policies throughout the 1970s. However, these policies were not effective at expanding the manufacturing sector weight in the economy. According to the World Bank (1979c), the manufacturing sector's share of real GDP barely increased from 17 percent in 1970 to 18 percent in 1977. At the same time, the share of people working in manufacturing activities fell from 13.6 percent in 1962 to 12.6 percent by 1982 (Figure 4.2).[8]

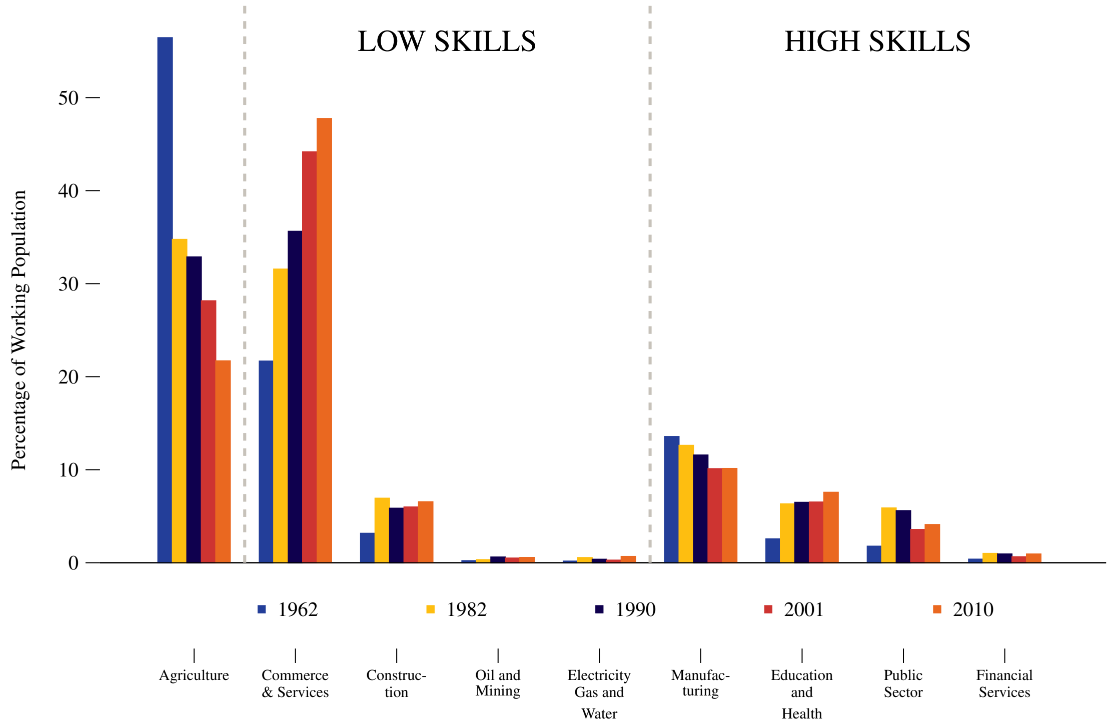
We may think that employment in the manufacturing sector fell due to the adoption of capital-intensive technologies that require fewer but more productive employees. However, the World Bank (1979a) estimates that value added per worker remained practically constant for the industrial sector between 1972 to 1975. At the same time, value added per worker in low-skill non-agricultural sectors increased 93 percent, from $806 to $1,556. Labor productivity of low-skill jobs increased after the oil boom because subsidies and price controls lowered the relative cost of capital in these occupations. For instance, it was cheaper to purchase small machinery (cooking appliances, drink dispensers, sewing machines) and vehicles than before the oil boom. Higher productivity implied higher earnings for people who worked in commerce, construction, and low-skill services. Consequently, individuals had the incentive to work in occupations with lower skill requirements.

Figure 4.2 shows that after the oil boom employment shifted from agriculture to other low-skill sectors, consistent with the increase in productivity mentioned above. Employment in agriculture decreased 21.7 percentage points between 1962 and 1982, while employment in low-skill occupations increased by 14.1 percentage points and employment in high-skill jobs increased by only 7.5 percentage points.[9] Commerce and services concentrated labor

[8] In 1965, Ecuador also started an agrarian reform with the objective of redistributing land from large landowners to their workers. According to the World Bank (1979c), this reform was not effective. In 1975, it distributed only 16 percent of the allocated land to 17 percent of the potential beneficiaries.

[9] Employment increased in "high-skill" jobs provided by the government (public administration, education, and health). It is important to note that at that time teachers did not require a college degree, and there were specialized high schools called *normales* that trained teachers.

Figure 4.2: Employment by Sector Before and After the Oil Boom



Notes: This Figure presents the proportion of the working population employed in each sector of the economy for 1962 and 1982. Data comes from Ecuador's 1962, 1982, 1990, 2001 and 2010 population censuses.

in the country. These dynamics suggest that, at least in the short-run, some individuals might have chosen to forgo a college education. Moreover, the fact that this change in employment's composition lasted until 2010 would be consistent with a long-term reduction in educational attainment. I study this issue in the rest of the paper.

4.3 Theoretical Framework

4.3.1 The Decisions to Drop Out and Return to School

Human capital accumulation models predict that some individuals may stop their education, at least in the short run, because of the large increase in productivity of low-skill jobs due to the oil boom (Becker, 1964; Black et al., 2005b; Charles et al., 2015). As low skills jobs become more appealing, particularly for young adults, the opportunity cost of finishing high school/going to college increases and the perceived returns of education decrease.

The long run effect of this type of shock is less clear from a theoretical perspective. It is plausible that individuals take advantage of booms to save and return to school in the future. This would imply zero long-run effect on educational attainment. However, age imposes costs on the decision to return to school. First, the horizon to receive the earnings premium of education decreases as we get older. Second, the cost of returning to school may increase as time outside school passes. Life events, such as marriage or having children, make it more difficult to go back to school. These costs imply that if the person has been out of school for more than a threshold number of years, then the optimal decision is to keep working. This threshold depends on the gap between high skills and low skills earnings. The smaller the difference, the shorter the period when it is optimal to return to school. Also, if agents expect that the natural resource boom will be a permanent shock, then the likelihood of returning to school decreases. Limited information may induce agents to believe that the shock could last for a long period.

In summary, the presence of long-term effects of natural resource booms on educational attainment is an empirical question. It is important to note that a negative long-term effect on education should imply a long-term increase in employment in low-skill occupations.

4.3.2 Heterogeneity Across Regions

A homogeneous shock that increases the opportunity cost of education or reduces the returns of education in a country can have heterogeneous effects across its regions if they have different costs of education. This result follows from the human capital accumulation model of Charles et al. (2015), who define a model for young adults who differ in ability θ_i that follows some distribution Φ_θ. In this setup, the authors define the lifetime payoff of going to college in year t as

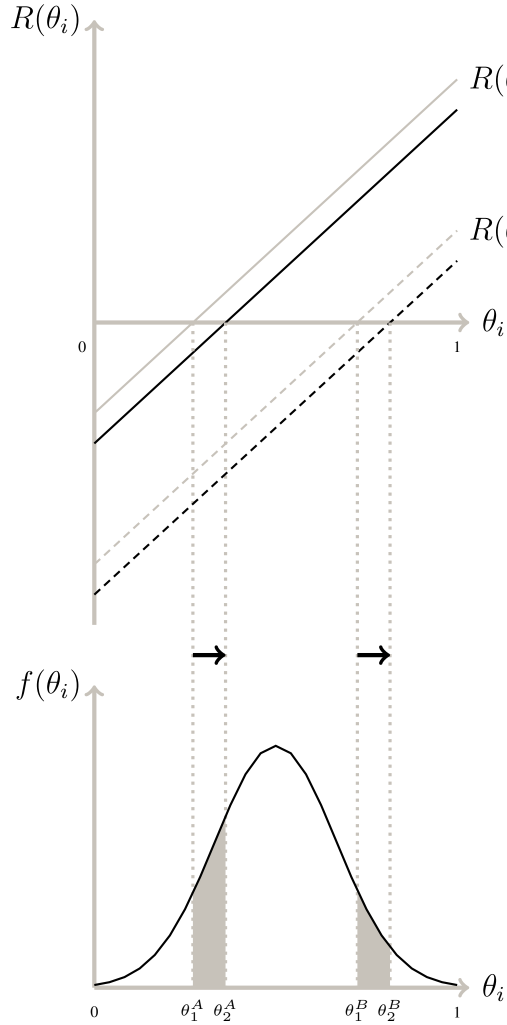
$$R_{it}(\theta_i) = \sum_{k=1}^{L-\alpha_i} E_t[\Pi_{t+k}|\Lambda_t] - (1+b)F - \kappa(1-\theta_i) - Y_t^0 \qquad (4.1)$$

where the first term ($E_t[\Pi_{t+k}|\Lambda_t]$) captures the expected returns of college attendance conditional on all information available Λ_t, the second term ($(1+b)F$) is the direct cost of college attendance (tuition, fees, living costs), the third term ($\kappa(1-\theta_i)$) is the psychological cost of education, and the last term (Y_t^0) is the opportunity cost of college attendance in the form of lost wages. The authors assume that the lifetime value of going to college is increasing in ability. This implies that there is an indifferent individual with ability θ^* such that all individuals with ability $\theta_i \geq \theta^*$ choose to go to college.

With this model, I show that a homogeneous shock that increases the opportunity cost of education or reduces the returns of education in a country can have heterogeneous effects across its regions. Let us suppose that there are two regions in a country, A and B with different direct costs of college attendance, $F^A < F^B$. This difference implies that for any underlying distribution of ability, college attendance is larger in region A than in region B. Now, suppose that both regions are affected by a shock that increases the opportunity cost of college attendance (Y_t^0) or decreases the returns of education ($E_t[\Pi_{t+k}|\Lambda_t]$) homogeneously across regions. College attendance will decrease in both regions, but unless the distribution of ability in the country is uniform, which region is the most affected depends on the magnitude of the difference in costs and the shape of Φ_θ. Given the shape of the distribution, the larger the difference in costs, the more likely it is that the cheaper region will be the most affected. Figure 4.3 presents this result assuming a linear lifetime value of college.

In developing countries, universities are concentrated in few cities increasing the costs of attending college. In this setup, it is likely that differences in costs are large enough such that the marginal student from high-cost regions has substantially higher ability and income than the marginal student from low-cost regions. Thus, the marginal student from high-cost regions should be less sensitive to shocks, like the oil boom, that increase the productivity of low-skill jobs. Thus, exposure to the oil boom should affect college completion less in regions with high costs than in regions with low costs. I use this result to define the base region to estimate the effects of exposure to the 1973 oil boom with an intensity difference-

Figure 4.3: Heterogeneity Across Regions with Different Costs of College Attendance



Notes: This Figure shows how differences in costs of attending college between regions can lead to differences in the proportion of the populations that discontinues their education in the presence of a shock that increases the opportunity cost of college attendance homogeneously across regions. Figure adapted from Charles et al. (2015).

in-differences design.

4.4 Data

I have access to proprietary data from a financial services company that works in Ecuador. This company collects comprehensive demographic data of the adult population in the country to develop credit scoring models.[10] I observe gender, year of birth, marital status, number of children, canton of birth,[11] canton of residence, highest completed education level, type of occupation, income for employees, and car ownership. Also, I use home ownership data from Ecuador's 2010 Population Census.[12] I focus on the cohorts born in Ecuador between 1948 and 1961 (1,711,538 individuals) to estimate the long-term effects of exposure to the oil boom before turning 18.

Ideally, to fully control for fluctuations from the life-cycle, we would be able to observe these cohorts at different points in time when they have the same age. However, the demographic information corresponds to 2014, car ownership to 2013, labor market data to 2012, and home ownership to 2010. This concern is not likely to alter the results because the observed outcomes should be determined for these cohorts. For instance, the probability of owning a house should not depend on age, since individuals in the sample were between 49 and 62 years old in 2010. We would expect that the decision of owning a house happened for most individuals before they turn 49. Also, at 62 individuals are young enough to live independently in their own home, even if they decided to downsize. Hence, the home ownership measure should not be affected by age for these cohorts.[13] Similar reasoning applies to the other outcomes in Table 4.1.

Table 4.1 presents descriptive statistics of the main sample. Women represent 51 percent of these cohorts, and on average, these individuals were 57 years old in 2014. Table 4.1 also splits the cohorts into two groups: those individuals who turned 18 years old before the oil boom in 1973 (born in 1948-1954) and those individuals who turned 18 after the oil boom (born in 1955-1961). There is no difference in the proportion of women between the two

[10]Sources include banks, other financial institutions, and web scrapping to fill gaps. Credit applications collect demographic information from Ecuador's national identification cards.

[11]A canton is an administrative division similar to a U.S. county.

[12]The complete census database is publicly available from Ecuador's national statistics agency and it can be downloaded from http://www.ecuadorencifras.gob.ec/base-de-datos-censo-de-poblacion-y-vivienda/. See Rivadeneira and Zumarraga (2011) for the complete description of the Census and the information it collects. I also use 10 percent random samples from Ecuador's population censuses of 1962, 1974 and 1982 to construct labor participation statistics reported in Section 4.2. These data are reported by Minnesota Population Center (2017), which collected the data from Ecuador's national statistics agency.

[13]Home value could be changing, but this variable is not present in the data.

Table 4.1: Sample Means

	Full Sample	Born in 1948-1954	Born in 1955-1961
Women	0.51	0.51	0.51
Age	56.73	60.78	53.84
No Education	0.15	0.18	0.12
Primary Education	0.47	0.49	0.45
Secondary Education	0.26	0.21	0.28
College Education	0.13	0.12	0.14
Years of education	8.18	7.60	8.59
Informal Workers 2012	0.53	0.56	0.51
Employees 2012	0.15	0.13	0.17
Professional Workers 2012	0.32	0.31	0.33
Monthly Wage Employees 2012	974.90	1039.17	939.50
Vehicle Owners 2013	0.17	0.16	0.17
Average age of vehicle 2013	16.09	16.92	15.54
Home Owners 2010	0.78	0.81	0.76
Home Owners with More than 2 Rooms 2010	0.57	0.59	0.56
Home Owners with Home above Median Quality 2010	0.33	0.34	0.32

Notes: this Table presents sample means for a subset of variables that characterize the data. The data corresponds to 2014 unless otherwise noted. Column 1 presents means for the full sample, that is individuals who were born in Ecuador between 1948 and 1961. Column 2 considers individuals born between 1948 and 1954, that is the cohorts who turned 18 years old before the oil boom in 1973. Column 3 considers individuals born between 1955 and 1961, that is the cohorts who turned 18 years old after the oil boom in 1973. Informal workers are people who work in low skills occupations, often self-employed, and who are not fully declaring taxes. Employees are people who work for a firm and receive a monthly wage. Professional workers are people who work independently and are registered with the Ecuadorian tax office.

groups. The proportion of people with primary education or less is smaller for those cohorts exposed to the oil boom before turning 18, while the proportion of people with secondary education or more is larger.

Table 4.1 also presents labor participation indicators as of 2012 to ensure that the majority of the sample is still active in the labor market.[14] The data have three labor participation categories: (i) informal workers, people who work in low skills occupations, often self-employed, and who are not fully declaring taxes; (ii) employees, people who work for a firm and receive a monthly wage; and (iii) professional workers, people who work independently and are registered with the Ecuadorian tax office. More than 50 percent of these cohorts were informal workers in 2012, with a slight drop for those who turned 18 after the oil boom.

[14]In 2012, those individuals born in 1948 were 64 years old, below the legal threshold for retirement.

I use home and vehicle ownership as proxies of wealth. We can observe that only 17 percent of the individuals born between 1948 and 1961 own at least one vehicle. This proportion is similar for the younger cohorts. Home ownership rate is close to 80 percent. It is important to note that in developing countries the quality of housing is a relevant issue. Hence, home ownership decreases to 57 percent if we consider owning a house with more than two rooms. I also use principal components analysis to combine data from the Census on the type of construction, materials used, water source, type of sewage and garbage disposal into an index of housing quality. Home ownership of houses above the median of the quality index is 33 percent. There are no substantial differences in home ownership between individuals who turned 18 years old before and after the boom.

4.5 The Long-Term Effect of the Oil Boom on Human Capital Accumulation

4.5.1 Descriptive Analysis

While Table 4.1 is useful to characterize the sample, it hides the evolution of the different variables across birth cohorts, which is important to identify the effect of being exposed to the oil boom before turning 18. Figure 4.4 presents the evolution of the highest completed schooling level for individuals born in Ecuador between 1940 and 1961.[15] For example, if a person dropped high school, then her highest completed education level is elementary school. The Figure shows that the proportion of people with no education or elementary education was decreasing, while the proportion of individuals with secondary education or college was increasing for those born until 1955.[16] These trends are expected for a developing country.[17]

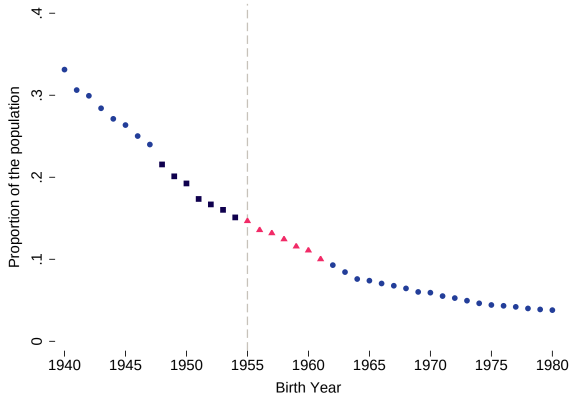
However, there is a major kink in educational attainment for cohorts who turned 18 after

[15] Cohorts born in 1962 and after are affected by a series of additional shocks: a short war in 1981, the oil bust and a declaration of default in 1982, and an earthquake in 1986 that destroyed the only oil pipeline in the country. These shocks confound each other, so I focus on the effect of the oil boom.

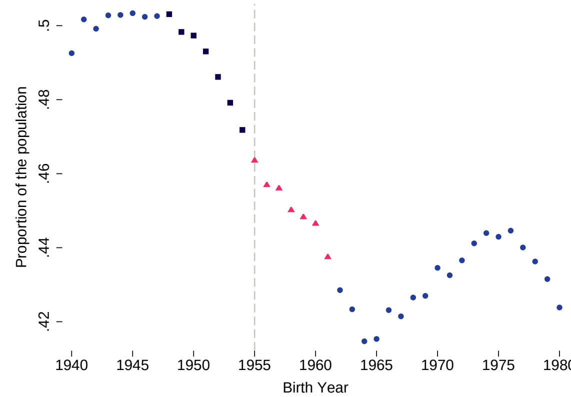
[16] Elementary school includes grades 1 to 6, secondary school includes grades 7 to 12.

[17] Economic conditions were fairly stable until 1973, and the economy grew at an average annual rate of 5.4 percent from 1960 to 1973. It is important to note that a civilian dictator ruled Ecuador from June 1970 to February 1972, who was replaced by a military dictatorship from February 1972 to August 1979. However, this was a peaceful period in contrast with other dictatorships in Latin America. Also, there are no dips in college completion in 1970, 1971 and 1972 which would have been consistent with repression at the beginning of a dictatorship.

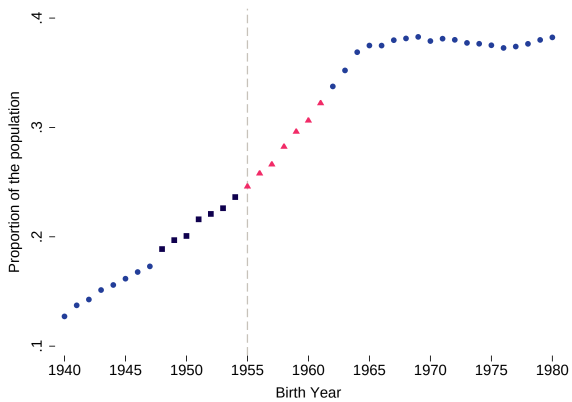
Figure 4.4: Highest Level of Education Attainment by Birth Cohort



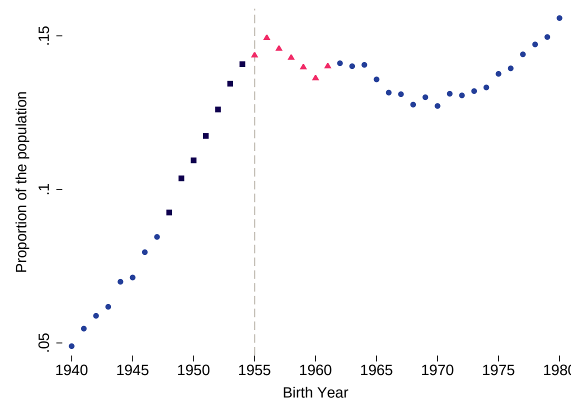
(a) No Education



(b) Completed Elementary Education



(c) Completed Secondary Education



(d) Completed College or More

Notes: This Figure presents the evolution of the highest completed schooling level for the cohorts born in Ecuador between 1940 and 1961. For example, if a person dropped high school before completing 12th grade, then she completed elementary school. The cohorts born between 1955 and 1961 (red triangles) turned 18 during the oil boom in the 1970s.

the oil boom. College completion flattens and decreases for the cohorts born between 1955 and 1961 (red triangles in Figure 4.4). Naively, if we extend the pre-1955 trend, Figure 4.4 Panel (d) suggests that exposure to the oil boom at the end of high school decreased college completion by around two percentage points. Consequently, the positive trend of secondary education becomes steeper (Panel c), and the negative trend of elementary education flattens (Panel b). These changes suggest that college completion drops due to a mix of people who choose not to enter/complete college and people who choose to drop out of high school.[18]

This abrupt change in college completion is consistent with the increase in productivity of low-skill jobs induced by the oil boom. As discussed in Section 4.3.1, higher productivity of low skill occupations increases the opportunity cost of education, which induces some individuals to interrupt high school/college completion in the short term. However, if the returns of education decreased in the long-run, the cost of returning to school were increasing in age, or agents believed that the shock was going to last long, then some people could have chosen not to return to school, leading to the observed drop in college completion in 2014.

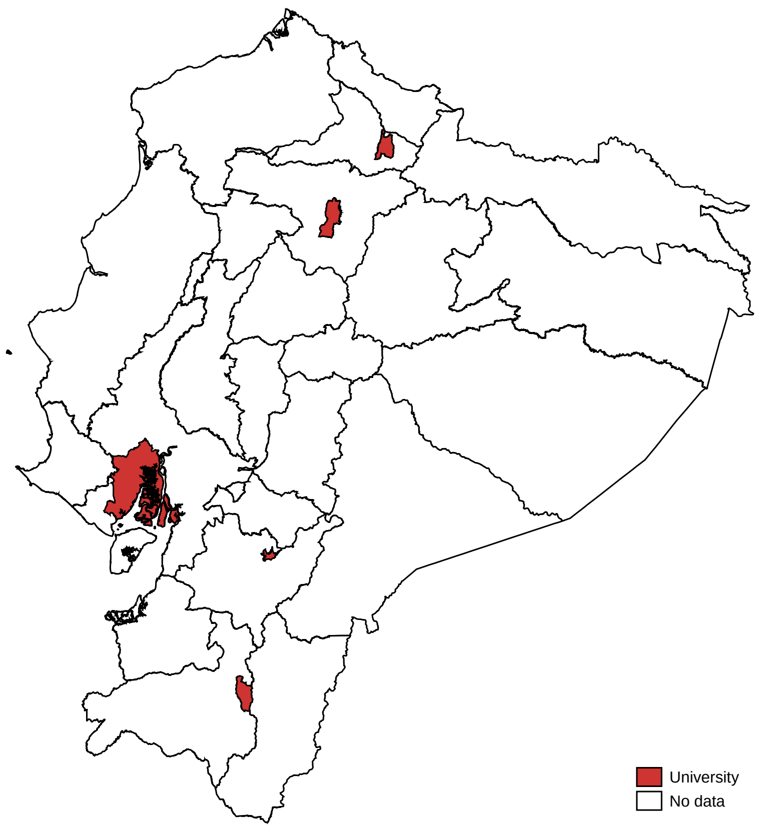
Figure 4.4 also shows that there is no apparent change in the trend of people with no education for the cohorts born between 1948 and 1961. This lack of change suggests that there were no other shocks that affected educational attainment of these cohorts when they were younger that could explain the reduction in college education. Hence, the only difference for cohorts who turned 18 years old around 1973, is that for some people the boom occurred when they were already attending college, while for others it occurred when they were completing high school and thinking about going to college. In the next sections, I develop an empirical strategy to estimate this effect more rigorously.

4.5.2 Regional Variation in the Costs of College Attendance

In Section 4.3.2, I show that if regions in a country have different costs of college attendance, then we should expect that a shock that increases low-skill productivity affects more regions with low costs than regions with high costs regarding college completion. In Ecuador during the 1970s, geographic differences in these costs stem from the fact that universities were located only in five cities of the country with no new openings in that period (Figure 4.5). Four universities where located in *Quito*, the capital, three in *Guayaquil*, three in *Cuenca*, and one in *Loja* that also had a second campus in the north to the country. Moreover, only universities in *Quito* and *Guayaquil* offered majors in every field of study, the

[18]Formal child labor was illegal, but children were allowed to work informally in agriculture or other low-skill jobs.

Figure 4.5: Cities with Universities in Ecuador Before the Oil Boom



Notes: This Figure shows the geographical distribution of the cities with universities in Ecuador before the oil boom.

other cities only had access to majors related to law and the humanities (liberal arts). The rest of the country only had agricultural technical schools.[19]

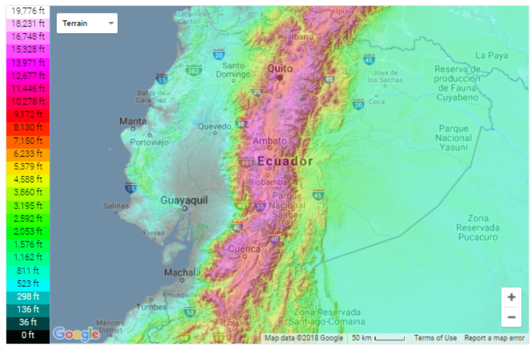
Attending college was cheaper for individuals who lived in the cities with universities due to lower living, travel, and information costs (most universities were public and free at that time). In Ecuador, it is very common for young adults to live with their parents until their early 30s, specially while they are still studying.[20] Thus, people born in a city with universities could live with their parents while they studied, significantly lowering the cost

[19]Only one new technical school opened in 1973. These schools are coded as secondary in Figure 4.4. Appendix Table C.2 lists all universities and technical school that functioned until 1989.

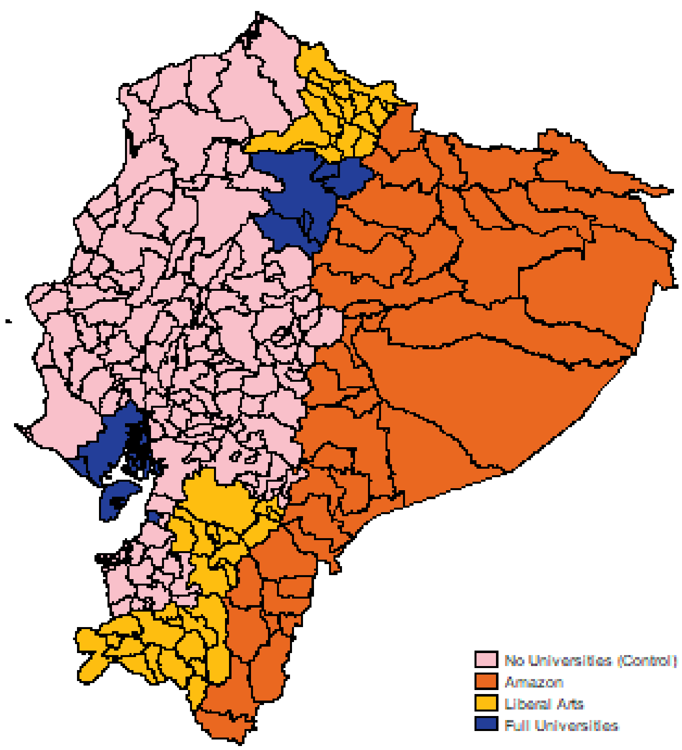
[20]In 1974, 36.5 percent of all adults between 18 and 30 years old lived with their parents. This share increases to 46.5 percent if we consider people aged 18 to 24. For comparison, Vespa (2017) reports that in 1975, 26 percent of adults aged 18 to 34 lived with their parents or in college dorms in the United States.

of education. These students did not have to rent a place to live (Ecuadorian universities do not offer dorms).

Figure 4.6: Regions by Cost of College Attendance



(a) Altitude Map



(b) Regions by Cost of College Attendance

Notes: Panel (a) presents an altitude map for Ecuador. Roughly, the country can be divided into 4 regions by the mountain ranges that cross the country. In Panel (b) I combine geographic regions with the location of universities before the oil boom to divide the country in 4 different areas.

Altitude differences partly determined the distribution of universities in the country and increased transportation costs, which lowered the cost of attending college for people who lived in cities with universities. The Andes mountains split the country into four regions (Figure 4.6 Panel a). Altitude fluctuates sharply across these regions. For example, in a horizontal distance of 190 miles, altitude increases from sea level to 16,000 feet and drops to 800 feet. This last region corresponds to the *Amazon* jungle, which was sparsely populated before the oil boom, had minimal agricultural activities, and had a deficient road network within the region and with the rest of the country (World Bank, 1979b).[21] The sharp differences in altitude increased travel times and costs within relatively short distances. For instance, travel between the two largest cities of the country in the 1970s took half an hour by airplane or 10 hours by car.

I define four areas with different costs of college attendance combining the location of the universities with the four geographic regions (Figure 4.6 Panel b). The first area corresponds to regions without universities that had the highest costs of college attendance. The second area corresponds to the *Amazon* region. While this region did not have universities at the time, the oil fields were located there. The government built a new highway that connected the *Amazon* region directly to the capital city of *Quito* to access the oil fields (Acosta, 2006). Also, spillovers from the oil industry into the local economy and direct transfers from the government to municipalities in this region contributed to increasing employment in high-skill occupations from 25 percent in 1962 to 26.8 percent in 1982. These two factors suggest that the cost of college attendance might have decreased in this region for the cohorts who turned 18 after the oil boom compared to the rest of the country that did not have universities. The third region in Figure 4.6 Panel (b) corresponds to areas of influence of the cities of *Cuenca*, *Loja*, and *Ibarra* that had access to liberal arts colleges. The last region corresponds to the cities of *Quito* and *Guayaquil* that had access to full universities and the lowest costs.

[21] Ecuador has an additional region, the Galapagos Islands that lie 1,000 kilometers of its coast. I do not include this region in the analysis because these islands had almost no population and were isolated from the continent at the time of the oil boom.

4.5.3 Empirical Strategy

To estimate the effect of exposure to the oil boom before turning 18 on college completion, I use an intensity difference-in-differences design that compares changes in outcomes across cohorts of individuals who turned 18 before and after 1973, to changes in outcomes across geographic regions with different costs of college attendance.[22]

In line with higher costs of attending college and the theoretical discussion in Section 4.3.2, Appendix Figure C.2 shows that for cohorts who turned 18 before the oil boom, regions without universities had substantially lower levels of college attendance than regions with full universities. Almost seven percent of the cohort born in 1948 in regions without universities completed college, while 16 percent of their peers born in the major cities achieved this goal. This gap of eight percentage points increased to 12 percentage points for the cohort of 1954. These magnitudes suggest that differences in the cost of attendance were large enough such that the marginal student who attended college from regions without universities had substantial higher ability and income than the marginal student from regions with universities. As predicted by the theoretical model, this implies that regions without universities should be the least affected by the oil boom. I use these regions as the baseline group for the intensity difference-in-differences design.

This design estimates the change in college completion in the regions with universities in excess of the change of college completion in regions without universities. Hence, as long as the oil boom negatively affects college completion in the baseline region (suggested by Appendix Figure C.2), I recover a lower bound of the real effect. Specifically, for individual i, born in region r in year t, I estimate

$$college\ completion_{irt} = \alpha_r + \alpha_t + \lambda_r t + \sum_{r \neq NoU} \sum_{t>1948}^{1961} \theta_{rt} region_r \cdot year_t + u_{irt} \tag{4.2}$$

[22]Finkelstein (2007) uses a similar design to evaluate the effects of the introduction of Medicare comparing highly affected regions in the United States to less affected regions. For other applications of this type of design see Acemoglu et al. (2004), Gregg et al. (2006), Baez (2011), and Felfe et al. (2015).

where I include region and year of birth fixed effects, α_r and α_t. The coefficients θ_{rt} capture the effect of exposure to the oil boom. We can interpret these estimates as the change in college completion since 1948 in each region in excess of the change observed in the regions without universities. Finally, I control for differential trends in outcomes for cohorts who turned 18 before the oil boom, $\lambda_r t$.[23] Appendix Figure C.2 shows that regions with full universities not only had a higher level of college completion but also had a steeper trend across birth cohorts than regions without universities. These differences in trends are consistent with the fact that regions with full universities had the lowest costs of college attendance and were the richest cities in the country. Also, these trends capture any remaining variation from age differences and the life cycle across cohorts.

The identification assumption is that if exposure to the oil boom had no effect, then any difference in college completion across regions would have continued on the same trends. Given this assumption, there are two main concerns to interpreting θ_{rt} as the causal effect of exposure to the oil boom before turning 18. Migration across regions due to the oil boom presents the first threat to identification. Velasco (1988) using data from the 1962, 1974 and 1982 population censuses shows that during the oil boom *Quito* and *Guayaquil* received an influx of immigrants from the rest of the country. The author argues that increased earnings for low-skill occupations drove migration, implying that cities with full universities have a larger proportion of people who did not go to college and would not have gone regardless of the oil boom. In this case, using the current place of residence would overestimate the effect. To address this concern, I assign individuals to regions using their canton of birth.

The second identification challenge is that there could be other shocks that have different effects across cohorts or regions. For example, an earlier shock that increased fertility after 1955 in areas without universities could have increased the proportion of people with no education in this region, which mechanically decreases the proportion of people who completed college. This shock would bias the estimates downwards. Conversly, the estimates

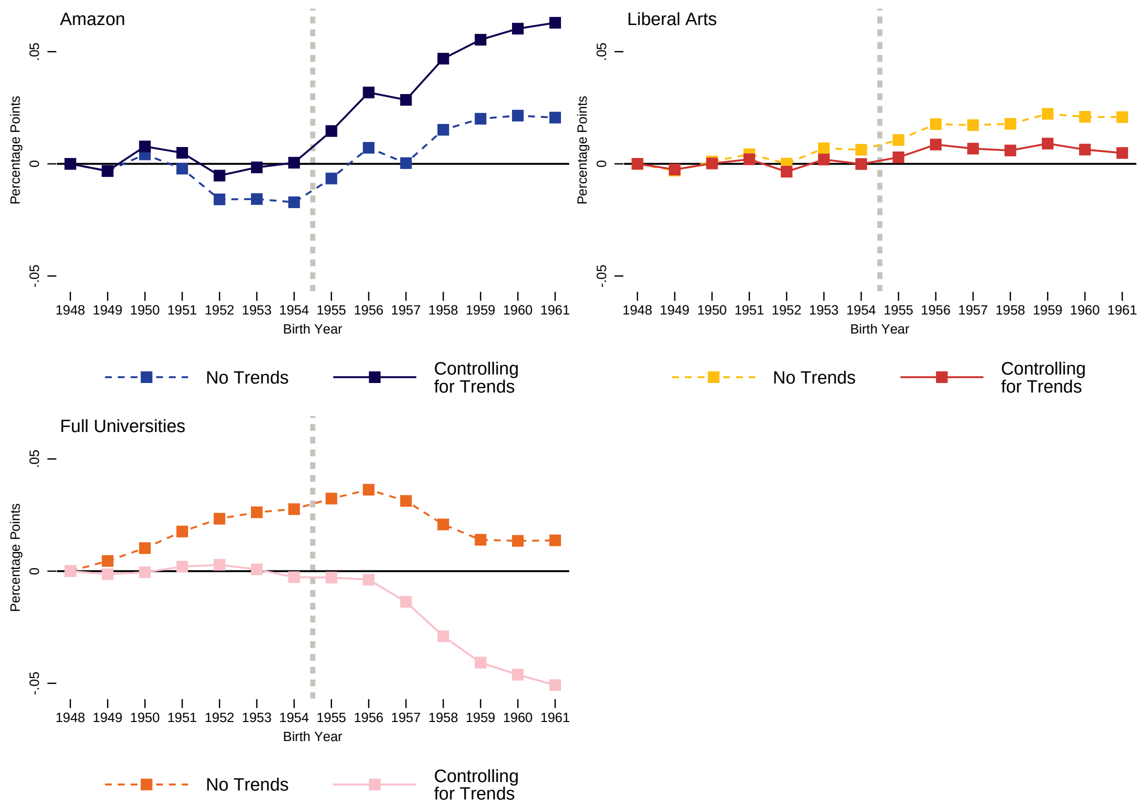
[23]These trends capture the fact that in developing countries younger cohorts are increasingly more educated than their older peers.

would be biased upwards if college educated individuals born in cites with full universities after 1955 were more likely to migrate to other countries than older cohorts. To asses if shocks that affect the population's composition are a concern, I apply a version of McCrary (2008) population density test. This type of shocks would create discontinuities or kinks in the distribution of the population who turned 18 years old around the oil boom. Appendix Figure C.1 shows that there are no significant discontinuities nor changes in the trend of the proportions of the population who turned 18 before the oil boom (born in 1948-1954) and after the oil boom (1955-1961). The trends are almost identical across regions. These results suggest that shocks related to changes in the population (fertility, migration) are not driving the results.

I follow Abadie et al. (2017) to determine the appropriate way to calculate standard errors. The data in this paper is a cross-section, where the outcomes for the different cohorts are measured at the same point in time. In this case, Abadie et al. (2017) find that in models that include fixed effects we should use cluster robust standard errors if either (i) there is clustering in the sample and there are heterogeneous treatment effects; or (ii) there is clustering in treatment assignment, and there are heterogeneous treatment effects. In this research, we can rule out (i) because the sample consists of the entire population born between 1948 and 1961. Concerning (ii), Abadie et al. (2017) define that there is clustering in treatment assignment when the probability that individual i is assigned to treatment is correlated with assignment to the treatment of other individuals in the same region. The extreme case would be that all individuals in a region have the same treatment assignment. In this research, treatment is turning 18 after 1973. Thus, in the absence of past regional shocks that affect fertility, (Appendix Figure C.1), in any given region the probability that one individual turns 18 after the oil boom should not be not correlated with other individuals in the region turning 18 before or after the boom. Hence, heteroskedastic robust standard errors should be sufficient since neither (i) nor (ii) hold. For robustness, I also report standard errors clustered at the canton level (215 clusters) that would account for unobserved, local shocks to fertility.

4.5.4 Results

Figure 4.7: Effects of Exposure to the Oil Boom Before Turning 18 on College Completion



Notes: This Figure presents dynamic difference in difference estimates of the effect of exposure to the oil boom before turning 18 on the probability of graduating from college. The region without universities is the base region.

Figure 4.7 and Table 4.2 present the estimates of the effect of exposure to the oil boom before turning 18 on college completion. Figure 4.7 confirms that college completion had different trends across regions for the cohorts who turned 18 before 1973. Regions with full universities and liberal arts colleges had steeper trends than regions with no universities, and the *Amazon region* had a lower trend than regions with no universities. These differences are consistent with the different costs of college attendance across regions discussed in Section

Table 4.2: Effects of Exposure to the Oil Boom Before Turning 18 on College Completion

Born in:	1955	1956	1957	1958	1959	1960	1961	1955-1961
Full	-0.0028	-0.0038	-0.0137	-0.0290	-0.0408	-0.0461	-0.0508	-0.0286
Universities	(0.0038)	(0.0041)	(0.0045)***	(0.0049)***	(0.0053)***	(0.0057)***	(0.0062)***	(0.0043)***
	(0.0031)	(0.0021)*	(0.0034)***	(0.0043)***	(0.0067)***	(0.0093)***	(0.0079)***	(0.0047)***
Liberal	0.0029	0.0086	0.0068	0.0060	0.0090	0.0063	0.0048	0.0064
Arts	(0.0034)	(0.0037)**	(0.0040)*	(0.0043)	(0.0047)*	(0.0050)	(0.0055)	(0.0037)*
	(0.0027)	(0.0038)**	(0.0049)	(0.0061)	(0.0092)	(0.0095)	(0.0112)	(0.0064)
Amazon	0.0146	0.0318	0.0285	0.0469	0.0553	0.0603	0.0629	0.0450
Region	(0.0083)*	(0.0092)***	(0.0097)***	(0.0107)***	(0.0116)***	(0.0124)***	(0.0134)***	(0.0092)***
	(0.0105)	(0.0095)***	(0.0122)**	(0.0137)***	(0.0127)***	(0.0203)***	(0.0194)***	(0.0133)***

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

Notes: This Table presents the effect of exposure to the oil boom before turning 18 on the probability of graduating from college for the cohorts born in 1955-1961. The first seven columns present the effect for each cohort. The last column shows the average of these effects across cohorts using using population as weights. Standard errors are in parentheses. The first row of standard errors corresponds to heteroskedastic robust standard errors. The second row of standard errors are clustered at the canton level for robustness (215 clusters). The estimates control for different trends across regions for the cohorts who turned 18 before 1973. The estimation sample includes all individuals born in Ecuador between 1948 and 1955 ($n = 1,711,538$).

4.5.2 and disappear after controlling for linear trends in the regression. Thus, from this point onward, I control for trends in all estimates.

Table 4.2 presents the estimates of the effect of exposure to the oil before turning 18. The first seven columns show the coefficients plotted in Figure 4.7 for the cohorts born between 1955 and 1961, and the last column averages these effect across cohorts weighting the estimates by population. In all regions, the effects get larger for the youngest cohorts in the sample. This pattern is consistent with longer exposure to the oil boom, which gives younger individuals more time to see the shift in the labor market towards low-skill jobs and perceive decreasing returns of education. A longer exposure may also bias their perception regarding the expected duration of the boom. Thus, it is possible that younger individuals were more likely to believe that the boom was a permanent change in the economy.

Table 4.2 shows that regions with full universities were the most affected by the oil boom, which is consistent with the model in Section 4.3.2. On average, exposure to the oil boom decreased college completion by 2.9 percentage points for the cohorts born in 1955-1961 in these cities. This change represents 12.2 percent of the college completion rate of individuals who turned 18 in 1954.[24] In contrast, college completion did not significantly change for the region with liberal arts colleges.

The estimates in Table 4.2 also show that exposure to the oil boom increased college completion in the *Amazon* region by 4.5 percentage points for the cohorts born in 1955-1961, which represents 58.5 percent of the baseline. As mentioned above, in 1973, the government built a new road connecting the *Amazon* region with *Quito* as part of the construction of an oil pipeline. This road decreased the cost of attending universities located in the capital, which could explain why education increased for people born in that region. Also, spillovers from the oil industry into the local economy and direct transfers from the government to municipalities in this region contributed to increasing employment in high-skill occupations, which could also explain the increase in college completion, particularly for the younger cohorts.

Appendix Table C.3 presents the effect of exposure to the oil boom before turning 18 by gender. The point estimates for both regions with full universities and the *Amazon* region are larger for men than for women, although most of the differences are not statistically significant. On average, for the cohorts born in 1955-1961, exposure to the oil boom decreased college completion by 1.8 percentage points (9.9 percent of the baseline) for women in cities with universities and by 3.9 percentage points for men (13.7 percent of the baseline). Exposure to the oil boom affected women in the younger cohorts (born in 1958 and after), while affected almost all the men who turned 18 after the boom (born in 1956 and after). This difference is consistent with men being affected first by the increase in infrastructure spending and construction that followed the oil boom, while women were affected by the increase in low-skill productivity that followed. In the *Amazon* region, exposure to the oil boom increased college completion by 3.6 percentage points (56.3 percent of the baseline) for women and by 5.4 percentage points for men (59.9 percent of the baseline). There is no significant effect in the regions with liberal arts colleges for both genders.

[24]Throughout the paper I will refer to the 1954 cohort as the baseline for all comparisons

4.5.5 Further Evidence for the Validity of the Research Design

This section reports the results from two additional tests on the validity on the previous results.

4.5.5.1 Unobserved Shocks on Early Educational Attainment

Other shocks that had different effects across cohorts or regions could be the main threat to identification. In Section 4.5.3, I present results that suggest that shocks associated with fertility or migration outside Ecuador are not driving the results. However, changes that directly affect early educational attainment could still be a concern. Specifically, two types of shocks would prevent us from interpreting the estimates in Table 4.2 as the causal effect of exposure to the oil boom before turning 18. First, if for some reason the proportion of people with no education increased more in cities with full universities than in regions with no universities, then the effects in Table 4.2 would be a consequence of this shock and not of exposure to the oil boom. For instance, in 1965, Ecuador started an agrarian reform to redistribute land from large landowners. In that year the cohorts born in 1955-1961 had ten years or less, and their educational attainment may have stopped if their parents decided to take advantage of this policy. Second, the same interpretation concern would arise if some policy decreased the proportion of people with no education in the *Amazon* region more than in regions with no universities. At that time, missionaries frequently visited the *Amazon* region to improve education.

I check for effects of unobserved shocks on the probability of not completing any educational level for those individuals who turned 18 years old after 1973. Table 4.3, Appendix Figure C.3, and Appendix Figure C.4 show these results. These estimates go in the opposite direction of the hypothesized concerns. Compared to regions without universities, the proportion of people with no education decreased in cities with full universities (1.3 percentage points, 23.4 percent of the baseline) and increased in the *Amazon* region (5.1 percentage points, 26.9 percent of the baseline). Thus, if there is any confounding effect, unobserved

Table 4.3: Effects on the Probability of Not Completing Any Educational Level

Born in:	1955	1956	1957	1958	1959	1960	1961	1955-1961
Full	-0.0111	-0.0085	-0.0116	-0.0151	-0.0128	-0.0155	-0.0119	-0.0125
Universities	(0.0027)***	(0.0029)***	(0.0032)***	(0.0036)***	(0.0039)***	(0.0043)***	(0.0047)**	(0.0033)***
	(0.0028)***	(0.0030)***	(0.0034)***	(0.0044)***	(0.0048)***	(0.0051)***	(0.0058)**	(0.0039)***
Liberal	-0.0081	0.0009	-0.0028	-0.0028	-0.0002	-0.0077	-0.0036	-0.0035
Arts	(0.0037)**	(0.0040)	(0.0044)	(0.0049)	(0.0053)	(0.0058)	(0.0063)	(0.0044)
	(0.0044)*	(0.0053)	(0.0053)	(0.0056)	(0.0065)	(0.0078)	(0.0086)	(0.0058)
Amazon	0.0085	0.0252	0.0263	0.0416	0.0591	0.0813	0.0885	0.0508
Region	(0.0118)	(0.0128)**	(0.0141)*	(0.0156)***	(0.0172)***	(0.0190)***	(0.0208)***	(0.0147)***
	(0.0149)	(0.0158)	(0.0169)	(0.0213)*	(0.0222)***	(0.0261)***	(0.0316)***	(0.0204)**

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

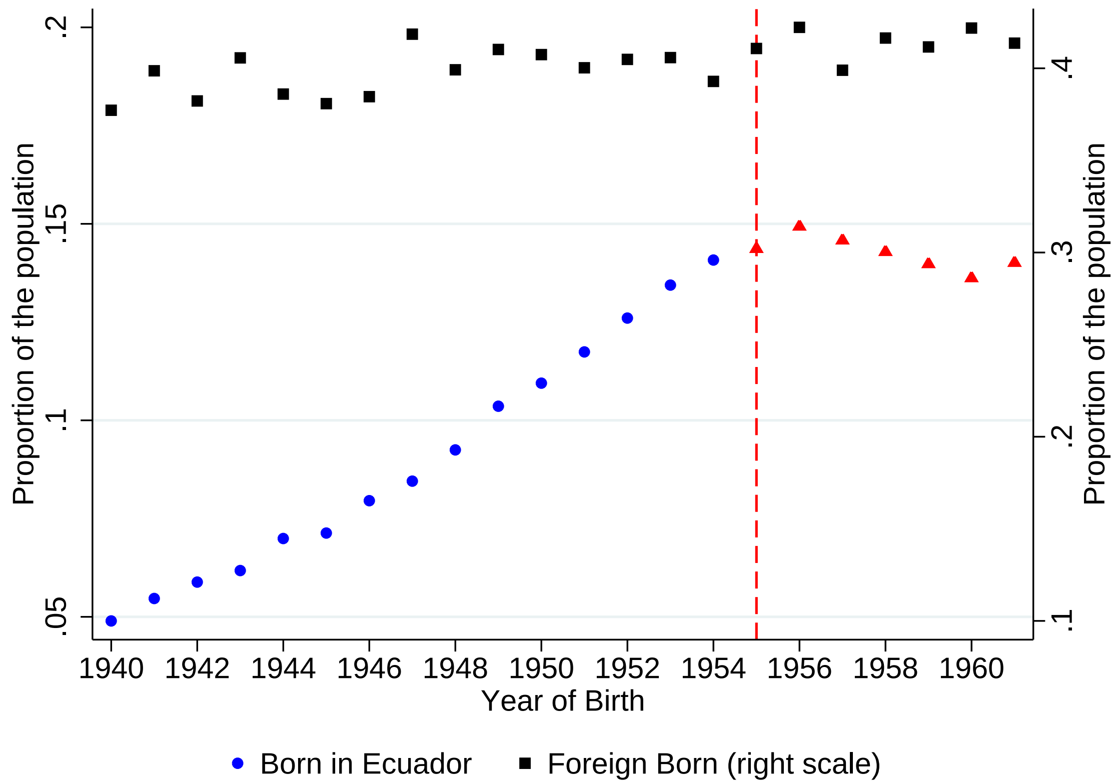
Notes: This Table presents the effects of unobserved shocks on the probability of not completing any educational level for the cohorts born in 1955-1961. The first seven columns present the effect for each cohort. The last column shows the average of these effects across cohorts using using population as weights. Standard errors are in parentheses. The first row of standard errors corresponds to heteroskedastic robust standard errors. The second row of standard errors are clustered at the canton level for robustness (215 clusters). The estimates control for different trends across regions for the cohorts who turned 18 before 1973. The estimation sample includes all individuals born in Ecuador between 1948 and 1955 ($n = 1,711,538$).

shocks that affect early educational attainment are attenuating the effect of exposure to the oil boom before turning 18 on college completion.

4.5.5.2 Are the Estimates a Lower Bound of the True Effect?

As mentioned above, the results in Section 4.5.4 are the change in college completion in the different regions in excess of the change of college completion in the regions with no universities. If exposure to the oil boom before turning 18 had a small, negative effect on educational attainment in these regions, as suggested by Appendix Figure C.2, then the estimates are a lower bound of the real effect. However, if exposure to the oil boom had a positive effect on educational attainment in the regions without universities, then the estimates in Section 4.5.4 would overstate the effect. To address this concern, I re-estimate Equation 4.2 using people who became Ecuadorian by naturalization as the control group. According to Ecuador's 2010 census, 82.3 percent of these individuals entered Ecuador after they turned 18 and 75.7 percent after they turned 24. While 18 percent of naturalized Ecuadorians could have been exposed to the oil boom, Figure 4.8 shows that there is no change in the level or trend of college completion between foreign-born individuals who

Figure 4.8: College Completion for Native Born and Foreign Born



Notes: This Figure presents the evolution of college completion for individuals born between 1940 and 1961. Blue circles and red triangles represent people born in Ecuador. The cohorts born between 1955 and 1961 (red triangles) turned 18 years old during the oil boom in the 1970s. The black squares represent people born outside of Ecuador, who became Ecuadorians later in life, most likely after the oil boom.

turned 18 years old before and after 1973. Thus, it is likely that for the majority of this group the decision to go to college was not affected by Ecuador's oil boom.[25]

Table 4.4 presents these results. The estimates follow the same pattern but are two to four times larger than the estimates in Table 4.2. Cities with full universities are still the most affected region. On average, exposure to the oil boom decreased college completion by 7.7 percentage points for the cohorts born in 1955-1961 in these areas (32.9 percent of the baseline). Additionally, these results indicate that exposure to the oil boom decreased

[25] However, it is also possible that these individuals became Ecuadorians because of the oil boom. Thus, it is not valid to use naturalized Ecuadorians as a control for outcomes related to the labor market and wealth.

Table 4.4: Effects on College Completion using Foreign-Born Ecuadorians as Control

Born in:	1955	1956	1957	1958	1959	1960	1961	1955-1961
Full	-0.0218	-0.0392	-0.0357	-0.0776	-0.0950	-0.1215	-0.1224	-0.0772
Universities	(0.0121)*	(0.0132)***	(0.0144)**	(0.0159)***	(0.0173)***	(0.0189)***	(0.0204)***	(0.0139)***
	(0.0027)***	(0.0010)***	(0.0024)***	(0.0030)***	(0.0058)***	(0.0083)***	(0.0067)***	(0.0038)***
Liberal	-0.0161	-0.0268	-0.0152	-0.0426	-0.0452	-0.0690	-0.0668	-0.0417
Arts	(0.0119)	(0.0131)**	(0.0143)	(0.0157)***	(0.0172)***	(0.0187)***	(0.0202)***	(0.0136)***
	(0.0022)***	(0.0033)***	(0.0043)***	(0.0053)***	(0.0085)***	(0.0085)***	(0.0104)***	(0.0058)***
Amazon	-0.0044	-0.0036	0.0065	-0.0017	0.0011	-0.0151	-0.0087	-0.0041
Region	(0.0141)	(0.0155)	(0.0168)	(0.0185)	(0.0202)	(0.0219)	(0.0236)	(0.0162)
	(0.0104)	(0.0093)	(0.0119)	(0.0134)	(0.0122)	(0.0198)	(0.0189)	(0.0130)
No	-0.0190	-0.0354	-0.0220	-0.0486	-0.0543	-0.0753	-0.0716	-0.0481
Universities	(0.0117)	(0.0128)***	(0.0140)	(0.0154)***	(0.0168)***	(0.0183)***	(0.0198)***	(0.0133)***
	(0.0016)***	(0.0019)***	(0.0024)***	(0.0030)***	(0.0035)***	(0.0042)***	(0.0042)***	(0.0027)***

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

Notes: This Table presents the effect of exposure to the oil boom before turning 18 on the probability of graduating from college for the cohorts born in 1955-1961 using Foreign-Born Ecuadorians as control. The first seven columns present the effect for each cohort. The last column shows the average of these effects across cohorts using using population as weights. Standard errors are in parentheses. The first row of standard errors corresponds to heteroskedastic robust standard errors. The second row of standard errors are clustered at the canton level for robustness (215 clusters). The estimates control for different trends across regions for the cohorts who turned 18 before 1973. The estimation sample includes all individuals born in Ecuador between 1948 and 1955 ($n = 1,754,059$).

college completion by 4.8 percentage points for the cohorts born in 1955-1961 in regions without universities (42.9 percent of the baseline). This effect is similar to that of regions with liberal arts colleges, where exposure to the oil boom decreased college completion by 4.2 percentage points. The difference in the point estimates between these two regions is not statistically significant for any cohort, consistent with the main results. These results confirm that we can take the main estimates in in Table 4.2 as a conservative measure of the true effect.

In this specification, exposure to the oil boom did not affect college completion in the *Amazon* region. The fact that in this region there is no effect when compared to foreign-born individuals while there is a positive effect when compared to the other regions without universities in Ecuador suggests that the presence of the oil industry countered the increase of low-skill productivity that affected the country. As mentioned above, the oil industry may have unintentionally decreased the cost of college attendance in the *Amazon* region by improving connectivity with *Quito* and enhancing the local economy.

In conclusion, the previous results show that exposure to the 1973 oil boom before turning 18 caused a drop in educational attainment on those who turned 18 years old after the oil boom. In the next Section, I discuss the potential mechanisms behind this long-term reduction in human capital.

4.6 Potential Mechanisms Behind the Long-term Reduction in Educational Attainment

The results in Section 4.5 provide quasi-experimental evidence that a natural resource boom can cause a permanent decrease in completed educational attainment. However, the theoretical models in Section 4.3.1 imply that there could be more than one mechanism behind this effect and its long-term consequences on welfare. On the one hand, if the increase of low-skill earnings is large enough, it may compensate for the loss of human capital over a person's lifetime. In the same line, the temporary resource boom could create a permanent shift in the structure of the economy towards low-skill jobs, lowering the long-term returns of education. In these cases, it would be optimal for rational individuals not to return to school, and there would be no long-term effect on wealth. On the other hand, limited information about the resource boom or time inconsistent preferences (Sutter et al., 2013; Castillo et al., 2011; Cadena and Keys, 2015) can lead to a long-run reduction in educational attainment and wealth. Lack of information or present-biased preferences can make individuals overstate the expected duration of the boom. As discussed in Section 4.3.1, when myopic agents realize that the boom ended, age related costs may prevent them from resuming their education. In this case, we would observe a negative long-term effect on educational attainment, and potentially a decrease in lifetime wealth, creating a "lost generation".

To bring light on the mechanism, in this Section I discuss four pieces of evidence: (i) the effects of college completion in the *Amazon region*, (ii) general shifts in employment from 1962 to 2010, (iii) the effect of exposure to the oil boom of working in low-skill occupations, and (iv) the effect of exposure to the oil boom on wealth.

4.6.1 College Completion Increased in the *Amazon* Region

In Section 4.5.4, I find that exposure to the oil boom before turning 18 increased college completion in the *Amazon* region. This region is the only in Ecuador where the oil boom plausibly had a positive effect on jobs that require higher skills. Thus, it is possible that the returns of education increased after the oil boom in this region. If this is the case, rational individuals would increase their educational attainment.

Before the oil boom, the *Amazon* region was sparsely populated before the oil boom and poorly connected with the rest of the country. It only had one highway that connected it to the rest of the country. Agriculture, commerce, and other low-skill services concentrated 70 percent of employment in the region. This situation changed dramatically after the oil boom. The oil boom had a direct positive effect on connectivity and income in the *Amazon* region because all the new oil fields that started production in 1973 are there. Hence, the government built a new highway that connected the *Amazon* region directly to the capital city of *Quito* to access the oil fields. It also passed a new law establishing that municipalities in the *Amazon* region receive 10 percent of fiscal revenue from oil. This transfer plus spillovers from the oil industry into the local economy (Bartik et al., 2017) contributed to increasing employment in high-skill occupations from 25 percent in 1962 to 26.8 percent in 1982, the highest change in the country.

4.6.2 The Structure of the Labor Market Shifted to Low-skill Occupations

As discussed in Section 4.2, the oil boom led to an increase in productivity of low-skill occupations (93 percent between 1972 and 1975) because oil revenue enabled the government to finance subsidies and price controls that lowered the cost of starting small businesses related to commerce and construction in the entire country (World Bank, 1979a). As a consequence, Figure 4.2 shows that employment composition in Ecuador changed after the oil boom. Employment in commerce, low-skill services (food preparations, repairs, transportation, housekeeping), construction, and other low-skill occupations increased from 25.2

percent in 1962 to 33.44 percent in 1982, while employment in manufacturing industries decreased from 13.6 percent in 1962 to 12.6 percent in 1982. More generally, Figure 4.2 shows that after the oil boom employment mainly shifted from agriculture to low-skill services. Employment in agriculture decreased 21.7 percentage points between 1962 and 1982, while employment in low-skill occupations increased by 14.1 percentage points and employment in high-skill jobs increased by only 7.5 percentage points in jobs provided by the government (public administration, health, and education).

This composition change in employment did not revert nor switch to high-skill occupations after the oil boom. Low-skill jobs importance in the economy grew until 2010. Figure 4.2 shows that agriculture's share in employment decreased until 2010, which is expected for a developing country as it improves living conditions. However, employment shifted to low-skill services and not to occupations that require higher skills. In particular, in 1982 the share of non-agriculture low-skill jobs was 39.4 percent and increased to 55.5 percent in 2010, while the share of high-skill occupations decreased from 25.8 percent to 22.8 percent in the same period. These changes in employment's composition suggest that the oil boom changed the structure of the labor market by enhancing the importance of low-skill jobs. This change would decrease the returns of education in the country in the long-run.

4.6.3 Are Long-term Labor Market Effects Consistent with the Changes in Educational Attainment?

The theoretical discussion in Section 4.3 implies that a long-term reduction in educational attainment should come in hand with a long-term increase in the probability of working in low-skill jobs. This response should be more marked if employment in the country shifted towards these occupations. Ideally, I would estimate the effect of exposure to the oil boom on the probability of working on these jobs, but the available data do not report specific occupations. The data have an aggregate measure that classifies a person as an "informal" worker if at least one of the three following conditions holds: (i) the person works in a low-skill job (as an employee or self-employed); (ii) the person is retired; or (iii) the person has

Table 4.5: Effects of Exposure to the Oil Boom Before Turning 18 on Informal Employment

Born in:	1955	1956	1957	1958	1959	1960	1961	1955-1961
Full	0.0013	0.0042	-0.0020	0.0095	0.0092	0.0163	0.0151	0.0082
Universities	(0.0047)	(0.0052)	(0.0057)	(0.0062)	(0.0068)	(0.0074)**	(0.0081)*	(0.0056)
	(0.0043)	(0.0049)	(0.0056)	(0.0059)	(0.0094)	(0.0178)	(0.0170)	(0.0094)
Liberal	-0.0035	-0.0029	-0.0037	-0.0024	0.0001	0.0037	0.0016	-0.0008
Arts	(0.0051)	(0.0056)	(0.0061)	(0.0067)	(0.0073)	(0.0079)	(0.0086)	(0.0059)
	(0.0043)	(0.0066)	(0.0074)	(0.0078)	(0.0090)	(0.0099)	(0.0117)	(0.0074)
Amazon	0.0095	-0.0166	-0.0282	-0.0188	-0.0216	-0.0191	-0.0169	-0.0166
Region	(0.0149)	(0.0161)	(0.0177)	(0.0193)	(0.0211)	(0.0229)	(0.0249)	(0.0174)
	(0.0165)	(0.0156)	(0.0232)	(0.0239)	(0.0262)	(0.0362)	(0.0351)	(0.0240)

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

Notes: This Table presents the effect of exposure to the oil boom on the probability of working informally informally in 2012 for the cohorts born in 1955-1961. The first seven columns present the effect for each cohort. The last column shows the average of these effects across cohorts using using population as weights. Standard errors are in parentheses. The first row of standard errors corresponds to heteroskedastic robust standard errors. The second row of standard errors are clustered at the canton level for robustness (215 clusters). The estimates control for different trends across regions for the cohorts who turned 18 before 1973. The estimation sample includes all individuals born in Ecuador between 1948 and 1955 ($n = 1,711,538$).

working age but is not registered with the Ecuadorian tax office. I estimate the effect of exposure to the oil boom on this measure of informality in 2012. Given the definition of informality in the data, retired individuals would bias the estimates towards zero because they are classified as informal workers. However, in 2012, the majority of the sample was still active in the labor market. In that year, those individuals born in 1948 were 64 years old, below the legal threshold for retirement.

For the cohorts born in 1955-1961, exposure to the oil boom before turning 18 increased the probability of working informally by 0.8 percentage points (1.9 percent of the baseline) in the cities with full universities and decreased this probability by 1.7 percentage points (3 percent of the baseline) in the *Amazon* region (Table 4.5, Appendix Figure C.5, and Appendix Figure C.6). There is no statistically or economically significant change in the probability of working informally in regions with liberal arts colleges, where there is no effect on college completion.[26] Overall, these results are consistent with the changes in educational attainment

[26]According to Ecuador's Labor Survey of December 2012, informal workers earn on average $195 per

in these regions and with a labor market oriented to low-skill jobs.

4.6.4 Long-term Effects of Exposure to the Oil Boom on Wealth

If the returns of education did not decrease in the long run, the literature on the returns of education implies that a reduction in educational attainment should have translated into lower levels of wealth. However, if wealth is not affected in the long-run, this would be consistent with rational individuals optimally choosing to stop their education. To analyze the long-term effects of exposure to the oil boom, I proxy wealth through home ownership in 2010 and vehicle ownership in 2013. As discussed in Section 4.4, fluctuations from the life-cycle should not be an important concern, given these individuals' age in 2010 and in 2013.

In Ecuador and other developing countries, the quality of housing is a relevant issue to determine wealth. It is common that poor households split their land to give their children a place to build a small house when they marry. Thus, home ownership rate is close to 80 percent for those born in 1948-1961, but it does not necessarily reflect wealth. To better capture wealth, I follow two approaches. First, I estimate the effect of exposure to the oil boom on the probability of owning a house with more than two rooms. Second, I construct an index of housing quality combining data on the type of construction; materials used in floors, walls, and ceilings; water source; type of sewage; and garbage disposal. In Ecuador, brick and mortar houses are of higher quality than wood houses, and the type of water source (tap/well/creek) and the type of waste disposal (sewage/septic tank/open) also signal higher wealth.

Exposure to the oil boom did not affect home ownership in the regions with full univer-

month, while formal workers earn on average $470 per month. While there is a gap in earnings, I am not able to estimate the reduced form effect of exposure to the oil boom on income because the labor survey does not report place of birth and the data I use only reports earnings of individuals who work formally as employees in companies. The fact that exposure to the oil boom increases the probability of working informally in some regions implies that I would need to account for sample selection to estimate the effect of the oil boom on earnings of employees. However, the fact that informal employment decreases for the *Amazon* region violates monotonicity across regions. Hence, I cannot use bounding procedures as in Lee (2009) to account for selection in the sample.

Table 4.6: Effects of Exposure to the Oil Boom Before Turning 18 on Home Ownership

Born in:	1955	1956	1957	1958	1959	1960	1961	1955-1961
	a. Owning a house with more than two rooms							
Full	-0.0109	-0.0010	-0.0059	-0.0051	-0.0063	0.0033	-0.0072	-0.0045
Universities	(0.0053)**	(0.0058)	(0.0064)	(0.0071)	(0.0077)	(0.0083)	(0.0091)	(0.0062)
	(0.0037)***	(0.0045)	(0.0042)	(0.0040)	(0.0052)	(0.0078)	(0.0058)	(0.0039)
Liberal	-0.0036	-0.0058	-0.0086	-0.0022	0.00003	-0.0015	-0.0037	-0.0036
Arts	(0.0057)	(0.0063)	(0.0069)	(0.0076)	(0.0083)	(0.0089)	(0.0097)	(0.0065)
	(0.0054)	(0.0061)	(0.0063)	(0.0066)	(0.0074)	(0.0083)	(0.0096)	(0.0060)
Amazon	0.0039	0.0208	0.0069	0.0120	0.0001	0.0268	-0.0004	0.0101
Region	(0.0162)	(0.0178)	(0.0196)	(0.0215)	(0.0236)	(0.0254)	(0.0278)	(0.0192)
	(0.0149)	(0.0126)*	(0.0148)	(0.0175)	(0.0211)	(0.0219)	(0.0226)	(0.0155)
	b. Owning a house of quality above the median of the quality index							
Full	-0.0032	-0.0033	-0.0062	-0.0042	-0.0027	-0.0013	-0.0066	-0.0039
Universities	(0.0053)	(0.0058)	(0.0063)	(0.0070)	(0.0076)	(0.0082)	(0.0090)	(0.0062)
	(0.0038)	(0.0053)	(0.0029)**	(0.0040)	(0.0047)	(0.0068)	(0.0049)	(0.0035)
Liberal	-0.0014	-0.0014	-0.0078	-0.0041	0.0050	-0.0089	-0.0096	-0.0043
Arts	(0.0055)	(0.0061)	(0.0066)	(0.0073)	(0.0080)	(0.0085)	(0.0093)	(0.0063)
	(0.0053)	(0.0057)	(0.0053)	(0.0072)	(0.0078)	(0.0077)	(0.0104)	(0.0061)
Amazon	0.0012	0.0289	0.0140	0.0324	0.0391	0.0611	0.0433	0.0332
Region	(0.0128)	(0.0144)**	(0.0155)	(0.0172)*	(0.0189)**	(0.0204)***	(0.0222)*	(0.0154)**
	(0.0154)	(0.0162)*	(0.0168)	(0.0212)	(0.0209)*	(0.0273)**	(0.0281)	(0.0198)*

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

Notes: This Table presents the effect of exposure to the oil boom on the probability of owning a home with more than two rooms (Panel a) and on the probability of owning a home of quality above the median of the quality index for the cohorts born in 1955-1961. Home ownership is measured in the 2010 census. The first seven columns present the effect for each cohort. The last column shows the average of these effects across cohorts using using population as weights. Standard errors are in parentheses. The first row of standard errors corresponds to heteroskedastic robust standard errors. The second row of standard errors are clustered at the canton level for robustness (215 clusters). The estimates control for different trends across regions for the cohorts who turned 18 before 1973. The estimation sample includes all individuals born in Ecuador between 1948 and 1955 ($n = 1,711,538$).

sities (Table 4.6 and Appendix Figures C.7, C.8, C.9, and C.10). Panel (a) shows that for the cohorts born in 1955-1961, exposure to the oil boom decreased the probability of owning a home with more than two room by 0.4 percentage points (0.7 percent of the baseline), insignificant at conventional levels. The standard errors rule out effects larger than two percentage points in any direction. In the *Amazon* region, where college completion increased, exposure to the oil boom before turning 18 increased the likelihood of owning a house with more than two rooms by one percentage point (1.8 percent of the baseline). The points esti-

Table 4.7: Effects of Exposure to the Oil Boom Before Turning 18 on Vehicle Ownership

Born in:	1955	1956	1957	1958	1959	1960	1961	1955-1961
Full	-0.0011	0.0010	0.0003	-0.0111	-0.0120	-0.0075	-0.0059	-0.0055
Universities	(0.0037)	(0.0040)	(0.0044)	(0.0049)**	(0.0053)**	(0.0058)	(0.0063)	(0.0043)
	(0.0020)	(0.0025)	(0.0035)	(0.0024)***	(0.0038)***	(0.0048)	(0.0055)	(0.0032)*
Liberal	0.0010	0.0048	0.0067	-0.0010	0.0091	0.0061	0.0045	0.0046
Arts	(0.0040)	(0.0044)	(0.0048)	(0.0052)	(0.0057)	(0.0062)	(0.0067)	(0.0046)
	(0.0038)	(0.0046)	(0.0048)	(0.0056)	(0.0068)	(0.0065)	(0.0076)	(0.0052)
Amazon	0.0044	0.0148	0.0121	0.0195	0.0289	0.0327	0.0260	0.0209
Region	(0.0096)	(0.0105)	(0.0114)	(0.0125)	(0.0137)**	(0.0148)**	(0.0160)	(0.0111)*
	(0.0125)	(0.0112)	(0.0163)	(0.0133)	(0.0177)	(0.0228)	(0.0207)	(0.0156)

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

Notes: This Table presents the effect of exposure to the oil boom on the probability of owning at least one vehicle in 2013 for the cohorts born in 1955-1961 The first seven columns present the effect for each cohort. The last column shows the average of these effects across cohorts using using population as weights. Standard errors are in parentheses. The first row of standard errors corresponds to heteroskedastic robust standard errors. The second row of standard errors are clustered at the canton level for robustness (215 clusters). The estimates control for different trends across regions for the cohorts who turned 18 before 1973. The estimation sample includes all individuals born in Ecuador between 1948 and 1955 ($n = 1,711,538$).

mates are imprecise and fluctuate across cohorts. There is no significant effect on the region with liberal arts colleges.

Panel (b) in Table 4.6 presents the effect of exposure to the oil boom on the probability of owning a house of quality above the median of the quality index. Again, exposure to the oil boom did not affect home ownership in the regions with full universities and liberal arts colleges. The point estimates have similar magnitudes to those in Panel (a). For the *Amazon* region, exposure to the oil boom before turning 18 increased the probability of owning a house of quality above the median by 3.3 percentage points (17 percent of the baseline). This result is consistent with the increase in educational attainment in this region.

Table 4.7, Appendix Figure C.11, and Appendix Figure C.12 show that exposure to the oil boom before turning 18 did not affect on the probability of owning at least one vehicle in cities with full universities. For the cohorts born in 1955-1961, exposure to the oil boom decreased the probability of owning a vehicle by 0.5 percentage points (2.8 percent of the baseline). The point estimates are small and statistically indistinguishable from zero. For the

Amazon region, the point estimates are imprecise but indicate that exposure to the oil boom before turning 18 increased the likelihood of owning a car by 2.1 percentage points (17.8 percent of the baseline), which goes in line with the effects on educational attainment.

In summary, the four pieces of evidence together suggest that the long-term reduction in educational attainment is consistent with a model of rational individuals who reduce their educational attainment in response to lower returns of education in the long-run. The long-term reduction in human capital together with no significant change in wealth is consistent with the hypothesis that the oil boom changed the structure of the Ecuadorian labor market by enhancing the importance of informal low-skill jobs.

4.7 Discussion

By analyzing the Ecuadorian oil boom of the 1970s, this paper provides some ground work for understanding how natural resource boom can affect human capital accumulation in developing countries. The results indicate that educational attainment decreased without affecting wealth accumulation. This finding is consistent with a model of fully informed rational individuals who reduce their educational attainment in response to a shock that decreases the returns of education by increasing the productivity of low skill jobs. Thus, that rather than a "lost generation", the cohorts exposed to the oil boom before turning 18 were just busy at work.

The results suggest that fiscal policy has role in the propagation of natural resource shocks in developing countries, in particular in cases where the state owns resource rights. The long-term reduction in educational attainment in Ecuador is consistent with policies that increased low-skills productivity at the time and were applied again in Latin America in the last resource boom in the 2000s. De La Torre et al. (2015) find descriptive evidence that low-skill productivity increased in resource-rich Latin American countries during the commodity price boom in the 2000s, driven by public spending policies similar to the ones Ecuador implemented in the 1970s.[27] How the government spends the extra money can influence

[27] Additionally, Caselli and Michaels (2013) find that oil revenue increases budgeted spending for public

the effects of the natural resource shock on the economy, particularly in the long term. The case of the *Amazon* region suggests that if a government wants a natural resource boom to increase educational attainment, it needs to invest to reduce costs of college attendance.

Also, it is plausible that policies that increase productivity in high-skill occupations produce a different effect. The case of Indonesia, another oil producing developing country, gives suggestive evidence in this direction. During the 1960s and 1970s, the Indonesian government started promoting industrialization to increase exports of manufactured goods (Elias et al., 2011). Appendix Figure C.15 shows that there is no drop in college completion after the oil boom. There is an increase in the trend for those individuals who turned 18 years old after the oil boom. More studies are needed to fully understand how fiscal spending can determine the effects of natural resource booms. Evaluating the counterfactual of what would have happened if the exposed cohorts completed more education and received the effects of the oil boom is an important question for future research.

Given these results, can we tell if natural resources a blessing or a curse? The estimates suggest that those individuals who turned 18 years old after the oil boom did not fare worse than their older peers regarding wealth accumulation. However, the drop in educational attainment caused by the oil boom could decrease social capital and civic engagement (McMahon, 2010; Dee, 2004; Milligan et al., 2004; Huang et al., 2009); have negative effects on health (Silles, 2009; Brunello et al., 2016); affect safety in the country (Lochner and Moretti, 2004; Groot and van den Brink, 2010; Buonanno and Leonida, 2009); and negatively affect the well-being of the next generation (Behrman and Rosenzweig, 2002; Currie and Moretti, 2003; Mine Güneş, 2015). These factors can constrain a country's long-term growth potential. To preview these potential negative consequences, Appendix Table C.4, Appendix Figure C.13 and Appendix Figure C.14 present the effect of exposure to the oil boom on the number of children per person.[28] Exposure to the oil boom increased the number of children by 1.7 percent of the baseline in cities with full universities. This result and the fact

services in Brazil, but it does not affect living conditions, suggesting that corruption might be a problem.

[28]There is no effect on the extensive margin of having children nor on the probability of never marrying.

that wealth did not decrease may imply fewer resources per children, which may lower their educational attainment and other future outcomes. In the *Amazon* region, exposure to the oil boom decreased the number of children by 4.5 percent of the baseline.

From a macroeconomic perspective, the results indicate that the oil boom decreased Ecuador's stock of human capital and may have affected its capacity to accumulate human capital for the next generation, which constrains the country's long-term growth potential. There is evidence that the drop in educational attainment can constraint the development of high-skill industries (Becker et al., 2011; Becker and Woessmann, 2010). Hence, a resource boom may not be a curse regarding individual wealth, but it can be a curse for society regarding the next generation's outcomes and lost growth potential.

5. SUMMARY AND CONCLUSIONS

The three essays in this dissertation use experimental and quasi-experimental methods to study how institutions, technologies, and natural shocks can affect living conditions. In Section 2, we find that people place a high value on Facebook. One week on Facebook is worth about $67 for the participants in our intervention - a relatively large value considering that it represents 30 percent of their average weekly income. We also find that Facebook is an important source of news. When we logged participants off Facebook, they did not look for news from other sources, even when the substitution cost for accessing news from the radio, television or the internet is low. Notably, being off of Facebook resulted in more uncertainty about whether news from politically-skewed sources was fake or not. Finally, we find that using Facebook induces feelings of depression and that participants switch to healthier activities when they cannot access Facebook.

In Section 3, we study how individual behavior may affect the effectiveness of medical technologies, in the context of flu vaccination. We find that reducing opportunity costs increases vaccination take-up substantially and that peer behavior can influence participation in vaccination campaigns. These results suggest that decreasing opportunity costs is one option to increase participation drastically and that employers and policymakers can increase participation in health campaigns by using mechanisms to incentivize groups of people. We also find evidence consistent with vaccination causing a moral hazard problem. Our study provides several pieces of evidence that speak to the idea of riskier behaviors regarding health among vaccinated individuals. Forgoing other protective measures and increasing risky behaviors could partially explain the ineffectiveness of vaccination and could help understand better why health interventions may sometimes fail.

In Section 4, I find that a natural resource boom can decrease human capital accumulation in the long-run in developing countries. While the results show no effect on wealth accumulation, affected individuals are losing non-monetary benefits from education including social capital and civic engagement, health, safety, and the well-being of the next generation. These factors, together with the fact that lower human capital constraints the development of high-skill industries, can negatively affect a country's long-term growth potential.

These analyses highlight the importance of understanding the unintended consequences institutions, technologies, and shocks should have in policy design. The case of Facebook highlights that causal knowledge on its adverse effects is a missing piece of information for the ongoing policy debate on social media. Also, how individuals react to a policy, program or shock can lead to outcomes unforeseen by policymakers and it is important to account for these possibilities. For instance, in the context of flu vaccination, policymakers should account for ways to counter riskier behaviors in the design of vaccination campaigns and policies that reduce the cost of college attendance can be used to increase human capital accumulation during resource booms.

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APPENDIX A

THE ECONOMIC EFFECTS OF FACEBOOK

A.1 Phase 1 Recruitment Email

Howdy "Student's Name",

Did you know that the average person spends about 50 minutes a day on Facebook? Over an individual's lifetime, this will amount to 5 or more years.

To date, the impact of this usage is unclear. Texas A&M's Department of Economics is seeking current TAMU students who are Facebook users to participate in a research study. You are receiving this email because you are on A&M's email list. Our team is examining the effects of Facebook on everyday life, and we are looking for students to help us out.

If you have an active Facebook account, you may be eligible to participate in this paid research study. In an unusual turn of events, we are asking **you** to tell us how much money you would need to be paid to stay off Facebook for a week. Please note that if selected for this study, staying off Facebook for one week will be a part of the protocol.

Participation in this study involves:

- Cash payouts based on an auction
- Coming to the Evans Library on main campus to complete two surveys
- The potential to be without Facebook for a week

If you are interested in participating in this study, please click the link below for more information.
Take the survey

If you have any questions or would like more information about this study, please contact the research team by email at rpetrie@tamu.edu.

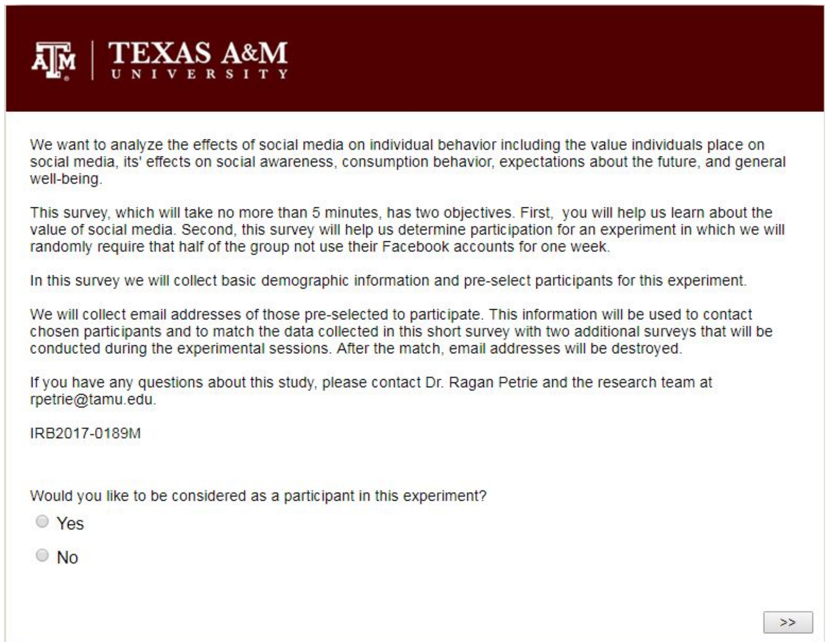
Thank you,

Prof. Ragan Petrie
TAMU Department of Economics
3035 Allen Building, College Station, TX 77845

Study Title: The Behavioral Effects of Social Media
IRB2017-0189M

Follow the link to opt out of future emails:
Click here to unsubscribe

A.2 Phase 1 Survey



TEXAS A&M
UNIVERSITY

We want to analyze the effects of social media on individual behavior including the value individuals place on social media, its' effects on social awareness, consumption behavior, expectations about the future, and general well-being.

This survey, which will take no more than 5 minutes, has two objectives. First, you will help us learn about the value of social media. Second, this survey will help us determine participation for an experiment in which we will randomly require that half of the group not use their Facebook accounts for one week.

In this survey we will collect basic demographic information and pre-select participants for this experiment.

We will collect email addresses of those pre-selected to participate. This information will be used to contact chosen participants and to match the data collected in this short survey with two additional surveys that will be conducted during the experimental sessions. After the match, email addresses will be destroyed.

If you have any questions about this study, please contact Dr. Ragan Petrie and the research team at rpetrie@tamu.edu.

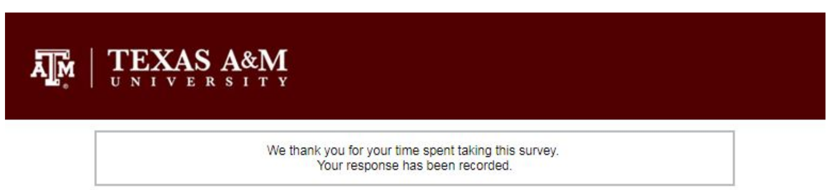
IRB2017-0189M

Would you like to be considered as a participant in this experiment?

- Yes
- No

>>

If no, the next screen shows



TEXAS A&M
UNIVERSITY

We thank you for your time spent taking this survey.
Your response has been recorded.

If yes, the next screen shows

TEXAS A&M UNIVERSITY

Thank you for your interest in helping us learn more about the effects of social media. Before continuing, please tell us a little bit about yourself.

What is your gender?

- Male
- Female

What is your age in years?
Please round up.

Which college are you currently enrolled in?

What is your major?

Did you live in the United States when you were 15 years old?

- Yes
- No

>>

TEXAS A&M UNIVERSITY

What was the zip code of your address when you were 15 years old?

>>

TEXAS A&M UNIVERSITY

What country did you live in when you were 15 years old?

>>

TEXAS A&M UNIVERSITY

Do you have an active Facebook account?

- Yes
- No

>>

TEXAS A&M UNIVERSITY

Let's play a game!

Please think carefully about your value of the time you spend on Facebook over a week. You will be asked to enter this value later.

Afterwards, we will present a counter-offer! This counter-offer will be randomly drawn from an interval of $5 to a maximum that is our most reasonable estimate of Facebook's value over a week.

If our counter-offer is greater than or equal to your valuation YOU WILL BE CONSIDERED TO PARTICIPATE IN THE EXPERIMENT. We will randomly select the final participants from this group. FINAL PARTICIPANTS will be paid the value of our random counter-offer.

If our counter-offer is lower than your valuation we will not be able to compensate you fairly. YOU WILL NOT BE CONSIDERED TO PARTICIPATE IN THE EXPERIMENT.

The next screen provides examples.

>>

TEXAS A&M UNIVERSITY

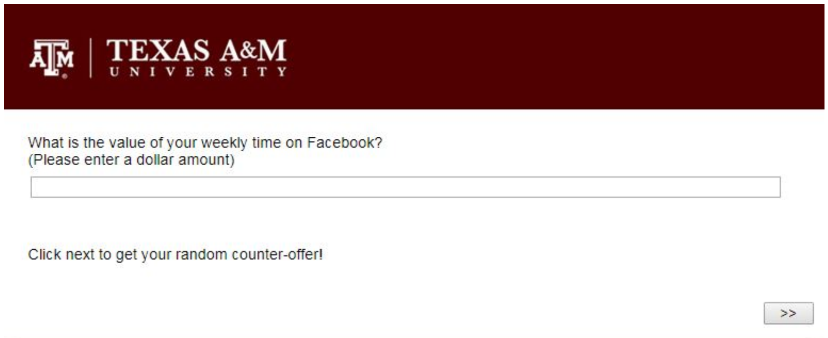
Please read the following examples of this game:

1) Mary values her weekly time on Facebook at $20. She enters this value in the following screen and clicks next. Then she receives our random counter-offer of $15. Since our counter-offer is lower than her valuation, she will not be considered to participate.

2) John values his weekly time on Facebook at $8. He enters this value in the following screen and clicks next. Then he receives our random counter-offer of $10. Since our counter-offer is higher than his valuation, he will be considered as a potential participant. If John is selected to participate he will be paid $10, the value of our counter-offer, at the end of the experiment.

Click next to continue.

>>



TEXAS A&M UNIVERSITY

What is the value of your weekly time on Facebook?
(Please enter a dollar amount)

Click next to get your random counter-offer!

>>

† The screen above represents the WTP setting. Half of the subjects received this wording while the other half were asked "How much money would you need to be given to stop using Facebook for a week?", which reflects the WTA setting.

TEXAS A&M UNIVERSITY

Our counter offer is $10.

Congratulations! You have been pre-selected to participate in this experiment.

On Thursday, April 20, we will send you an email if you are randomly selected as a final participant.

If selected, we will further explain the details of the experiment on Monday, April 24, at Evans Library. We will email you the exact time and room number. There will be a second session on Monday, May 1, at Evans Library. At most you will spend 1 hour between the two sessions. You will be paid $10 - our counter-offer - at the end of the experiment.

Remember, we will randomly require that half of the final participants do not use their Facebook accounts by any means for one week.

Please enter your preferred email address below:

Thank you for your time!

>>

For the case where the counter offer is less than the valuation:

Our counter offer is $10.

Sorry! You are not pre-selected to participate. Thank you very much for completing this short game. We are sure that your answers will help us learn more about the effects of social media on our lives.

Thank you for your time!

>>

A.3 News Quiz

News Quiz in phase 2 (before treatment)

A1	Read the following list of events. Did these events happen in the previous week?			
		Definitely happened	I do not know	Definitely did not happened
A11	Serena Williams, the best women's tennis player, is expecting her first child and will not play again until next year.	❑	❑	❑
A12	Thousands of people gathered in the rain Saturday on the soggy grounds of the Washington Monument to turn Earth Day into an homage to science.	❑	❑	❑
A13	Facebook killer, Steve Stephens, was arrested in Ohio.	❑	❑	❑
A14	Vice President Mike Pence visited the demilitarized zone as the U.S. kept its options open on North Korea.	❑	❑	❑
A15	Stanford University, said that it would permit the conservative author Ann Coulter to speak on campus in early May, just one day after it canceled her appearance.	❑	❑	❑
A16	MSNBC analyst calls for ISIS to bomb Trump property.	❑	❑	❑
A17	General Motors has become the latest multinational company to pull out of Venezuela after it says government authorities illegally seized its plant there.	❑	❑	❑

News Quiz in phase 3 (after treatment)

A1	Read the following list of events. Did these events happen in the previous week?			
		Definitely happened	I do not know	Definitely did not happened
A11	Bulls bow out of playoffs with blowout loss to Celtics in Game 6.	❑	❑	❑
A12	Federal agencies take actions to implement President Trump's order to strip fund from municipal governments that refuse to cooperate fully with immigration agents.	❑	❑	❑
A13	Obama begins new phase of public life with Chicago visit.	❑	❑	❑
A14	Tens of thousands of people protested the president's rollback of rules protecting the environment.	❑	❑	❑
A15	President Trump has instructed his advisers to keep the corporate tax rate close to 30 percent.	❑	❑	❑
A16	In France's most consequential election in recent history, voters on Sunday chose Emmanuel Macron and Marine Le Pen to go to a runoff to determine the next president.	❑	❑	❑
A17	Trump wants to send astronauts to Mars during his presidency.	❑	❑	❑

A.4 Survey Questionnaire

Date: May 1, 2017 Time: __ : __ PM

UIN	Please enter your TAMU UIN:	☐☐☐☐☐☐☐☐☐	Please enter your TAMU Email:

N1 How much time did you spend reading or watching the news per day last week?

- ❑ Less than 15 min
- ❑ More than 15 minutes but less than 30 minutes
- ❑ More than 30 minutes but less than 1 hours
- ❑ More than 1 hour but less than 2 hours
- ❑ More than 2 hours

N2 Please indicate how frequently you used the following types of news media last week. Please answer on a scale of 1 to 7, where 7 is a type of media that you used frequently, and 1 is a type of media you used infrequently.

		◄ Not at all						All of the time ►
		1	2	3	4	5	6	7
N21	Cable TV	❑	❑	❑	❑	❑	❑	❑
N22	Paper news	❑	❑	❑	❑	❑	❑	❑
N23	Radio	❑	❑	❑	❑	❑	❑	❑
N24	Online news	❑	❑	❑	❑	❑	❑	❑
N25	Social media	❑	❑	❑	❑	❑	❑	❑
N26	News feed	❑	❑	❑	❑	❑	❑	❑
N27	Other 1: ______________	❑	❑	❑	❑	❑	❑	❑
N28	Other 2: ______________	❑	❑	❑	❑	❑	❑	❑

N3 Please indicate how frequently you used the following methods to obtain news last week. Please answer on a scale of 1 to 7, where 7 is a method you used frequently, and 1 is a method you used infrequently.

		◄ Not at all						All of the time ►
		1	2	3	4	5	6	7
N31	Watch	❑	❑	❑	❑	❑	❑	❑
N32	Read	❑	❑	❑	❑	❑	❑	❑
N33	Listen	❑	❑	❑	❑	❑	❑	❑

N4 List the top 3 news outlets/sources you got your news from last week.

1st Choice: ______________________	2nd Choice: ______________________	3rd Choice: ______________________

N5 What type of news did you frequently read, watch or listen to last week? Please answer on a scale of 1 to 7, where 7 is a type you used frequently, and 1 is a type you used infrequently.

		◄ Not at all						All of the time ►
		1	2	3	4	5	6	7
N51	Political	❑	❑	❑	❑	❑	❑	❑
N52	Sports	❑	❑	❑	❑	❑	❑	❑
N53	Business	❑	❑	❑	❑	❑	❑	❑
N54	International	❑	❑	❑	❑	❑	❑	❑
N55	Local news	❑	❑	❑	❑	❑	❑	❑
N56	Culture	❑	❑	❑	❑	❑	❑	❑
N57	Science	❑	❑	❑	❑	❑	❑	❑
N58	Weather	❑	❑	❑	❑	❑	❑	❑

N6 For the following sources, indicate how frequently you used each last week. Please answer on a scale of 1 to 7 where 7 is a source you used frequently, and 1 is a source you used infrequently.

		◄ Not at all						All of the time ►
		1	2	3	4	5	6	7
N61	Battalion	❑	❑	❑	❑	❑	❑	❑
N62	KBTX	❑	❑	❑	❑	❑	❑	❑
N63	MSC website	❑	❑	❑	❑	❑	❑	❑
N64	Local radio	❑	❑	❑	❑	❑	❑	❑
N65	Local newspaper	❑	❑	❑	❑	❑	❑	❑
N66	National newspaper	❑	❑	❑	❑	❑	❑	❑
N67	Online news	❑	❑	❑	❑	❑	❑	❑
N68	Online social network	❑	❑	❑	❑	❑	❑	❑
N69	Friends	❑	❑	❑	❑	❑	❑	❑

M1	Last week, how much time did you spend <u>each day</u> doing the following activities?				
M11	Sitting in a library on campus	_______ Hours	M17	Attending class	_______ Hours
M12	Studying	_______ Hours	M18	Sleeping (average number of hours per night)	_______ Hours
M13	Working for pay	_______ Hours	M19	Attending a party or social event (fill in for time you spent in total last week)	_______ Hours
M14	Exercising	_______ Hours	M110	At what time do you typically go to bed?	☐☐ : ☐☐ ❑ AM / ❑ PM
M15	Hanging out with friends	_______ Hours	M111	At what time do you typically wake up?	☐☐ : ☐☐ ❑ AM / ❑ PM
M16	Reading news	_______ Hours			
M2	Last week, how much time did you spend <u>each day</u> on the following types of social media?				
M21	Facebook	_______ Hours	M27	Vimeo	_______ Hours
M22	Instagram	_______ Hours	M28	YouTube	_______ Hours
M23	Twitter	_______ Hours	M29	Other 1: _______________	_______ Hours
M24	Tumblr	_______ Hours	M210	Other2 : _______________	_______ Hours
M25	Snapchat	_______ Hours			
M3	How many friends do you have on Facebook? (Feel free to open your FB account to check)				
M4	How many followers do you have on Instagram?				
M5	How many followers do you have on Tumblr?				
M6	How many followers do you have on Twitter?				

F1	How often do you do the following on Facebook?							
		Never	Rarely	1-2 times per month	Once a week	2-4 times per week	Once a day	Several times per day
F11	Open up FB to check your news feed	❑	❑	❑	❑	❑	❑	❑
F12	Read news feed content	❑	❑	❑	❑	❑	❑	❑
F13	Post pictures	❑	❑	❑	❑	❑	❑	❑
F14	Post comments	❑	❑	❑	❑	❑	❑	❑

F4	When you are on Facebook, how often do you feel the following?						
		Never	Rarely	Sometimes	Frequently	Often	All the time
F41	Envy/jealousy	❑	❑	❑	❑	❑	❑
F42	Happiness	❑	❑	❑	❑	❑	❑
F43	Misery	❑	❑	❑	❑	❑	❑
F44	Satisfaction	❑	❑	❑	❑	❑	❑
F45	Connected with friends	❑	❑	❑	❑	❑	❑
F46	Up to date on my friends' activities	❑	❑	❑	❑	❑	❑
F47	Lonely	❑	❑	❑	❑	❑	❑
F48	Annoyed	❑	❑	❑	❑	❑	❑
F49	Inspired	❑	❑	❑	❑	❑	❑

Please think about what you did last week as you answer the following questions.

C1	(1-Strongly agree, 2-Agree, 3-Neither agree nor disagree, 4-Disagree, 5-Strongly disagree)					
		1	2	3	4	5
C11	I ate out less than I normally do	❑	❑	❑	❑	❑
C12	I did less impulse buying than usual	❑	❑	❑	❑	❑
C13	I saved more money than I usually do	❑	❑	❑	❑	❑
C14	I ate healthier than usual	❑	❑	❑	❑	❑
C15	I exercised more than usual	❑	❑	❑	❑	❑
C2	(1-Strongly agree, 2-Agree, 3-Neither agree nor disagree, 4-Disagree, 5-Strongly disagree)					
		1	2	3	4	5
C21	I wasted less time than I normally do	❑	❑	❑	❑	❑
C22	I achieved more than I normally do	❑	❑	❑	❑	❑
C23	I spent more time studying and doing school related work	❑	❑	❑	❑	❑
C24	I was not late for classes, meetings or work	❑	❑	❑	❑	❑
C25	I was able to meet deadlines without rushing at the last minute.	❑	❑	❑	❑	❑
C26	I was able to prevent distractions from achieving high priority tasks.	❑	❑	❑	❑	❑
C27	I discontinued any wasteful or unprofitable activities or routines.	❑	❑	❑	❑	❑
C28	I had time to relax and be with friends	❑	❑	❑	❑	❑
C29	I procrastinated less than I normally do	❑	❑	❑	❑	❑
C210	I partied a lot	❑	❑	❑	❑	❑

Please think about what you are going to do this coming week as you answer the following questions.

C3	(1-Strongly agree, 2-Agree, 3-Neither agree nor disagree, 4-Disagree, 5-Strongly disagree)					
		1	2	3	4	5
C33	I expect to spend less on eating out and hanging out with friends	❑	❑	❑	❑	❑
C34	I expect to save more money	❑	❑	❑	❑	❑
C35	I will cut down on my impulse buying	❑	❑	❑	❑	❑
C36	I will spend more time studying	❑	❑	❑	❑	❑
C37	I will eat more healthy food	❑	❑	❑	❑	❑
C38	I will exercise more than I normally do	❑	❑	❑	❑	❑

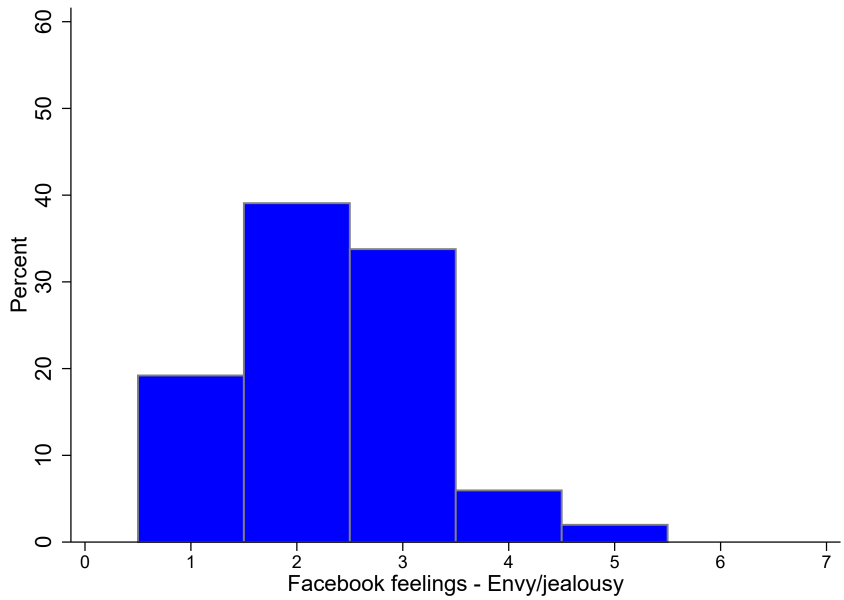
S1	**Overall, how satisfied are you with life as a whole?**
	◄ Not at all satisfied … Completely satisfied ►
	0 ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 ☐
S2	**Overall, to what extent do you feel the things you do in your life are worthwhile?**
	◄ Not at all worthwhile … Very worthwhile ►
	0 ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 ☐
S3	**How happy are you?**
	◄Very unhappy … Very happy ►
	0 ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 ☐
S4	**How often do you worry?**
	◄ Never … All of the time ►
	0 ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 ☐
S5	**How often do you feel depressed?**
	◄ Never … All of the time ►
	0 ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 ☐

D1	**What is your race?**		
	☐ White ☐ Black/ African American ☐ American Indian/ Alaskan Native	☐ Asian ☐ Native Hawaiian/ Other Pacific Islander ☐ Other: ___________________________	
D2	**What is your ethnicity?**	☐ Hispanic/ Latino	☐ Not Hispanic/ Latino

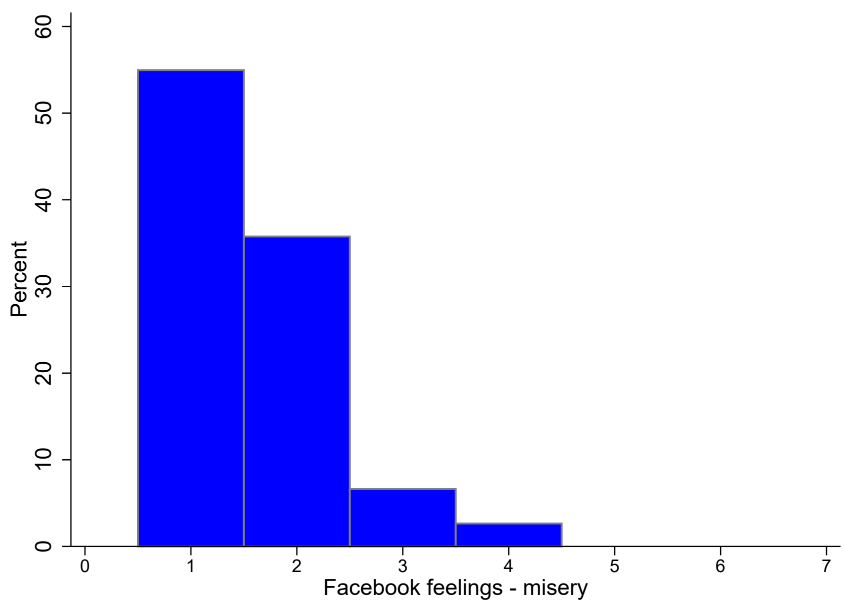
O1	**Is there anything else you would like to tell the research team?**

A.5 Additional Results

Figure A.1: Facebook Negative Emotions

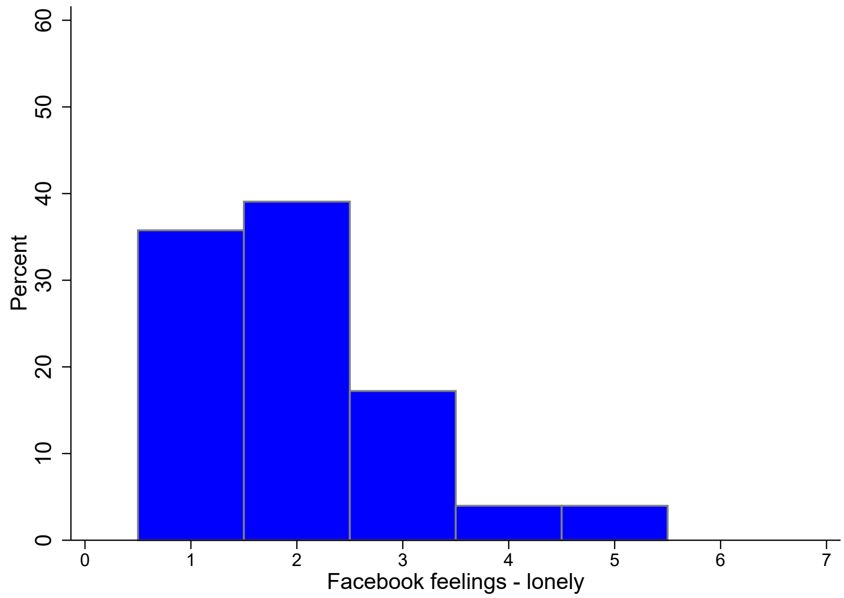


(a) Envy/Jealousy

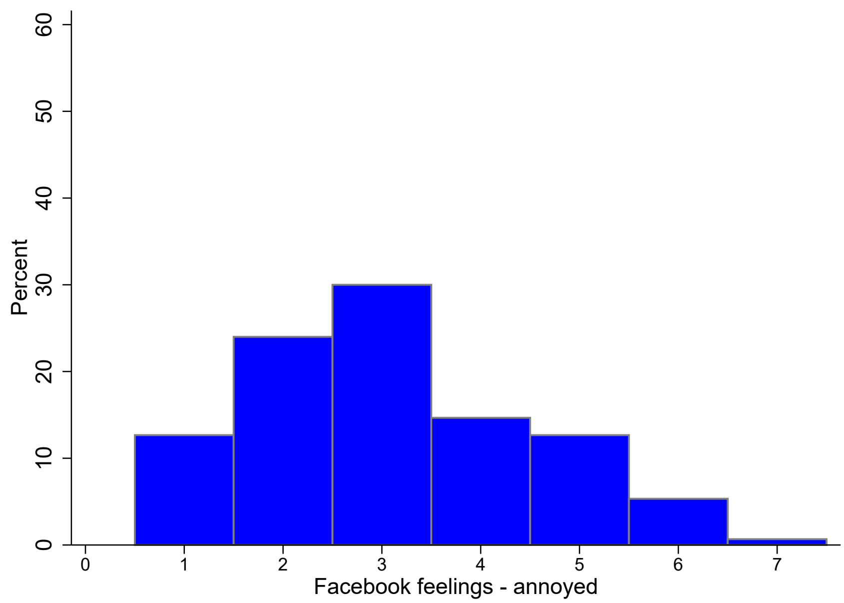


(b) Misery

Figure A.2: Facebook Negative Emotions Cont.



(a) Lonely



(b) Annoyed

Figure A.3: Change in Reported Depression and Change in the Value of Facebook

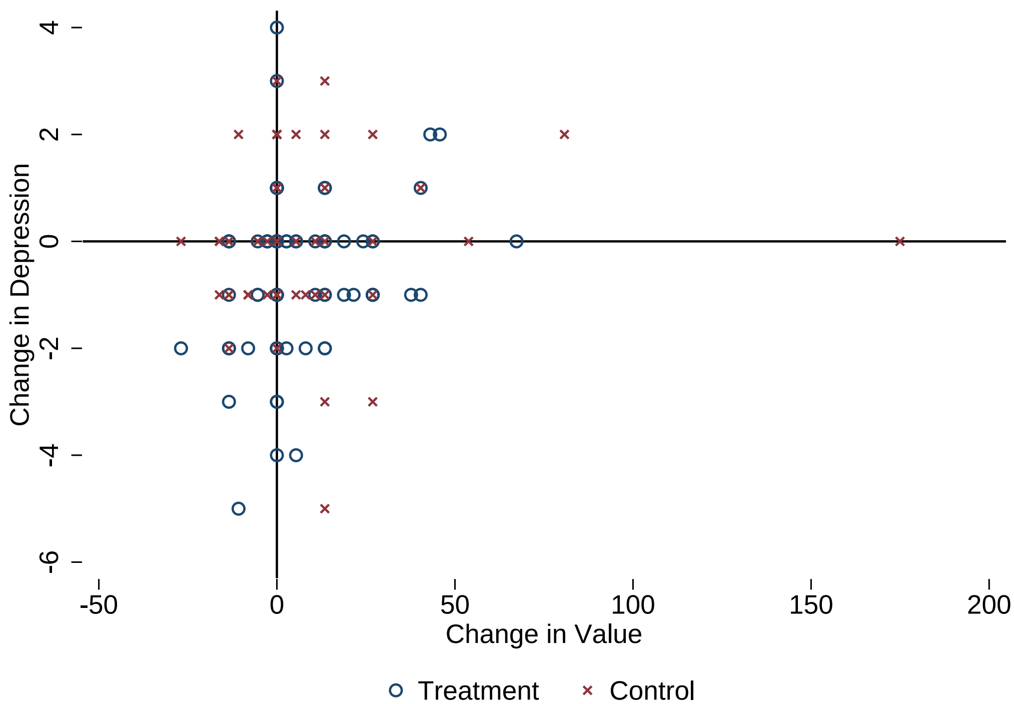


Table A.1: Descriptive Statistics by Survey Phases

	Ineligible	Eligible	P-value	Eligible (Show)	Eligible (No-Show)	P-value
Value of Facebook	85.35	27.11	0.000	28.97	26.33	0.025
	(119.88)	(12.72)		(12.98)	(12.55)	
Woman	0.60	0.59	0.720	0.65	0.57	0.089
	(0.49)	(0.49)		(0.48)	(0.50)	
Age	20.77	20.55	0.009	20.59	20.53	0.693
	(1.65)	(1.68)		(1.99)	(1.53)	
Income ($)	67,204	71,761	0.109	69,509	72,286	0.512
	(55,192)	(68,778)		(63,207)	(71,032)	
N	1,207	562		167	395	

Notes: This table presents the means for eligible and ineligible participants from the Phase 1 survey and for the eligible participants that showed up to complete the Phase 2 survey and those that were eligible but did not show up for phase 2. The p-values represents the difference of means for each group. Standard deviations are in parentheses.

Table A.2: Facebook Restriction - Balance of Covariates

	Treatment	Control	P-value
Value of Facebook	28.42	29.43	0.618
	(11.27)	(14.33)	
Woman	0.57	0.711	0.060
	(0.50)	(0.46)	
Age	20.69	20.51	0.569
	(2.41)	(1.56)	
Income($)	67,900	75,986	0.482
	(55,988)	(68,904)	
N	77	90	

Notes: The first two columns present the means of different observables characteristics for the Facebook restriction treatment group and the no restriction control group. Columns 3 presents the p-values of the difference of means between these groups. Standard deviations are in parentheses.

Table A.3: Phase 2 Survey - Summary Statistics

	Mean	Median	Std. Dev.
Daily Time Reading or Watching News (1-5)[1]	2.15	2	1.19
Frequency of Use (1-7)[2]			
Cable TV	1.93	1	1.49
Paper News	1.31	1	0.67
Radio	2.46	2	1.66
Online News	4.55	5	1.73
Social Media	5.60	6	1.56
News Feed	4.14	4	1.99
Political Nature of Preferred News (1-5)[3]	2.81	3	0.97
Daily Social Media Usage (hours)[4]			
Facebook	1.87	1	2.21
Instagram	1.28	1	1.60
Twitter	0.86	0	2.06
Tumblr	0.35	0	1.57
Snapchat	1.95	1	3.02
Vimeo	0.03	0	0.16
YouTube	1.85	1	2.65
Social Media Friends and Followers (number)[5]			
Facebook	640.99	538	442.04
Instagram	452.36	350	511.77
Tumblr	87.32	0	571.74
Twitter	182.12	0	333.80
Subjective Well-Being (0-10)[6]			
Satisfied with life	7.15	8	1.92
Things in life are worthwhile	7.37	8	1.88
How happy are you	7.17	8	2.12
How often do you worry	6.79	7	2.33
How often do you feel depressed	3.40	3	2.63

Notes: [1]Responses to the question "How much time did you spend reading or watching the news per day last week?" Response options: 1) Less than 15 min, 2) More than 15 minutes but less than 30 minutes, 3) More than 30 minutes but less than 1 hour, 4) More than 1 hour but less than 2 hours, and 5) More than 2 hours. N=167 obs. [2]Responses to the question "Please indicate how frequently you used the following types of news media last week." Scale was from 1 to 7 where 1 indicates "Not at all" and 7 indicates "All of the time." N=167 obs. [3]List top news outlets/sources from the previous week. We categorized each 1st choice as either being 1) Left, 2) Left-Center, 3) Center, 4) Right-Center, or 5) Right based on www.allsides.com. N=57 obs. [4]Time spent each say on various social media platforms. [5]How many friends and followers on various social media platforms. [6]Subjective well-being questions, with 0 indicating "never and" 10 "very/always."

Table A.4: Correlations between the Value of Facebook and User's Characteristics

	Value of Facebook	High Time	High Engage	Depressed	High Negative	High Friends in Facebook	High Friends in other Social Media
Value of Facebook	1.00						
High Time	0.23***	1.00					
High Engage	0.20**	0.32***	1.00				
Depressed	-0.11	0.23***	0.05	1.00			
High Negative	-0.06	0.17**	0.09	0.32***	1.00		
High Friends on Facebook	0.06	0.10	0.21***	-0.02	0.01	1.00	
High Friends on other Social Media	0.17**	0.18**	0.38***	-0.10	0.01	0.42***	1.00

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

Notes: This table presents the Pearson correlation coefficients between the stated value of Facebook and characteristics of its users based on Phase 2 survey responses. High Time refers to individuals who on average use Facebook for more than one hour per day; High Engage refers to individuals who post pictures and comments on Facebook at least once or twice per month; Depressed refers to individuals who reported feeling depressed above the reported median value; High Negative refers to individuals who are above the median of the factor index that combines measures of feeling envy, misery, lonely and annoyed while on Facebook; High Friends in Facebook refers to individuals who have more than 564 friends in Facebook (median number of friends); and High Friends in other Social Media refers to individuals who have more than 529 friends in Facebook (median number of friends in other social media).

Table A.5: Distribution Shift Tests

	Equality	FSD C-T	SSD C-T	FSD T-C	SSD T-C
Facebook Use	0.00***	0.00***	0.00***	0.93	1.00
News Media Index -Traditional Media	0.41	0.22	0.10*	0.60	0.57
News Media Index -Social Media	0.00***	0.00***	0.00***	0.94	1.00
News Consumption Index	0.07*	0.04**	0.00***	0.95	0.75
Probability Right Answer - Mainstream News	0.37	0.77	0.80	0.18	0.23
Probability Wrong Answer - Mainstream News	0.61	0.70	0.55	0.33	0.34
Probability Not Sure Answer - Mainstream News	0.55	0.29	0.21	0.58	0.51
Probability Right Answer - Skewed News	0.01***	0.01***	0.01***	0.54	0.99
Probability Wrong Answer - Skewed News	0.46	0.50	0.75	0.23	0.23
Probability Not Sure Answer - Skewed News	0.37	0.51	0.81	0.19	0.19
Overall Satisfaction	0.25	0.11	0.20	0.58	0.76
Life is Worthwhile	0.28	0.14	0.09*	0.62	0.79
Feel Happy	0.17	0.09*	0.11	0.93	0.82
Worry	0.21	0.90	0.79	0.10*	0.11
Feel Depressed	0.32	0.16	0.22	0.91	0.98
Consumption Index	0.03**	0.97	0.89	0.01**	0.00***
Productive Time Index	0.10	0.97	0.90	0.05*	0.02**
Efficient Time Index	0.10*	0.98	0.90	0.05*	0.01***
Expected Consumption Index	0.07*	0.79	0.63	0.03**	0.01***
Value of Facebook	0.47	0.70	0.99	0.25	0.14

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

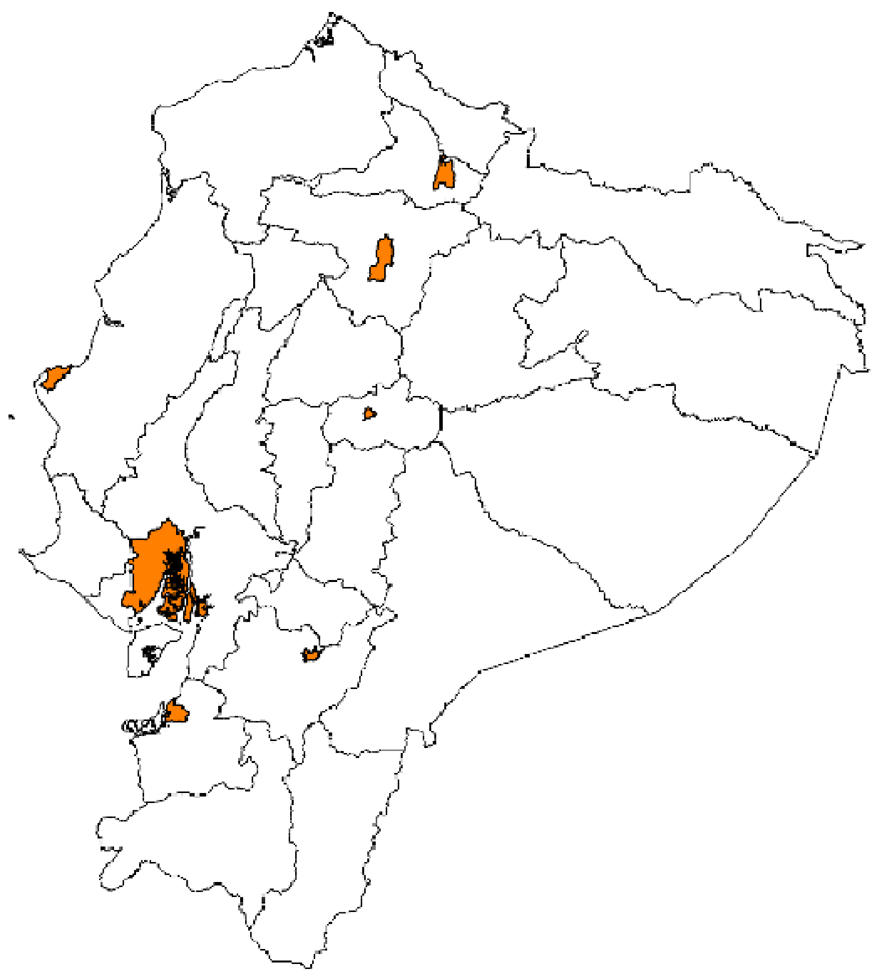
This table presents the bootstrap p-values of Kolmogorov-Smirnov statistics that test for equality of distributions, first order stochastic dominance and second order stochastic dominance between treatment and control after a one week Facebook restriction. In column 1 the null hypothesis is that the distributions are the same, in column 2 the null hypothesis is that the treatment group first order stochastic dominates the control group, in column 3 the null hypothesis is that the treatment group second order stochastic dominates the control group, in column 4 the null hypothesis is that the control group first order stochastic dominates the treatment group, and in column 5 the null hypothesis is that the control group first order stochastic dominates the treatment group.

APPENDIX B

VACCINES AT WORK

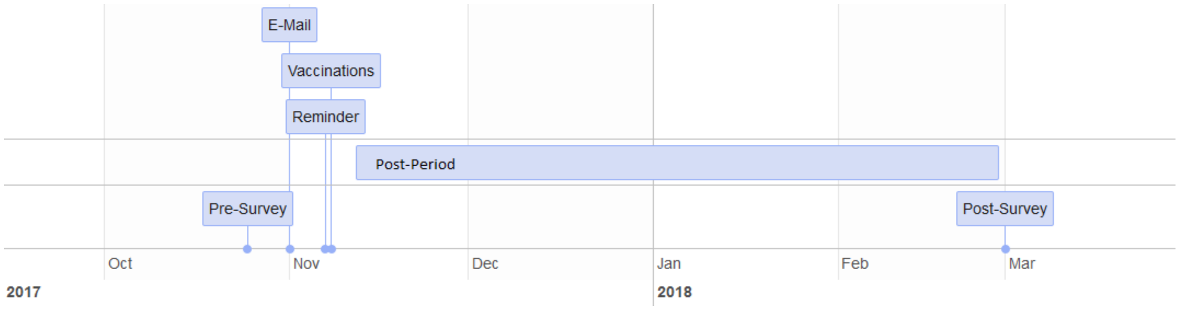
B.1 Additional Figures

Figure B.1: Locations of the Bank in Ecuador



Notes: The map contains the locations of the bank in Ecuador (orange) where we implemented our intervention.

Figure B.2: Timeline of Experiment Implementation



Notes: The bank sent the pre-intervention survey on October 18. The bank sent emails with the different treatments on November 1 using Human Resources mailing account. Furthermore, it sent a reminder on November 7. The vaccination campaign took place between November 8 and November 11. The post-treatment period (Ecuadorian flu season) went from November 13 to March 1. The bank sent the post-intervention survey during March and April 2018.

Figure B.3: Treatment Message: Control

Notes: The above image portrays the email sent to the control group. Translation: Dear Employee, Diners Club of Ecuador is running an influenza vaccination campaign in November. You are eligible for a flu shot on Thursday, November 9 from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, the price is \$4.95, that will be deducted from your payroll if you choose to get vaccinated. If you have questions, please contact. Let's get vaccinated!

Figure B.4: Treatment Message: Altruism

Notes: The above image portrays the email sent to the "Altruistic Treatment" group. Translation: Dear Employee, Diners Club of Ecuador is running an influenza vaccination campaign in November. You are eligible for a flu shot on Thursday, November 9 from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, the price is $4.95, that will be deducted from your payroll if you choose to get vaccinated. Getting vaccinated yourself also protects people around you, including those who are more vulnerable to severe flu illness, like infants, young children, the elderly and people with dangerous health conditions that cannot get vaccinated If you have questions, please contact. Let's get vaccinated!

Figure B.5: Treatment Message: Selfish

Notes: The above image portrays the email sent to the *"Selfish Treatment"* group. Translation: Dear Employee, Diners Club of Ecuador is running an influenza vaccination campaign in November. You are eligible for a flu shot on Thursday, November 9 from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, the price is $4.95, that will be deducted from your payroll if you choose to get vaccinated. Vaccination can significantly reduce your risk of getting sick, according to both health officials from the World Health Organization and numerous scientific studies. If you have questions, please contact. Let's get vaccinated!

Figure B.6: Treatment Message: Opportunity Cost (Saturday)

Notes: The above image portrays the email sent to the *"Saturday Treatment"* group. Translation: Dear Employee, Diners Club of Ecuador is running an influenza vaccination campaign in November. You are eligible for a flu shot on Saturday, November 11 from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, the price is $4.95, that will be deducted from your payroll if you choose to get vaccinated. If you have questions, please contact. Let's get vaccinated!

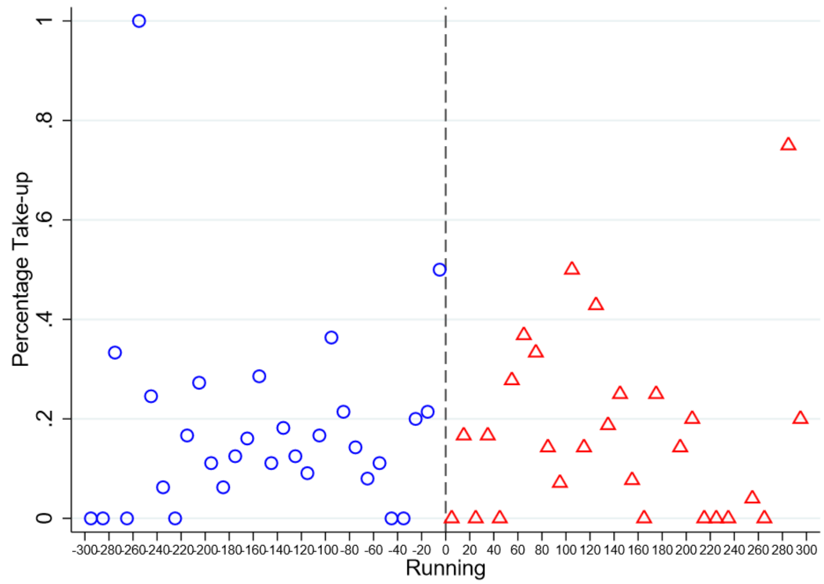
Figure B.7: Vaccination Campaign: Influenza Vaccine

Notes: The above package contains the influenza vaccine used in the campaign. This vaccine protects against four strands of the flu, two from type A and two from type B.

Figure B.8: Vaccination Campaign: Flu Shot in Action

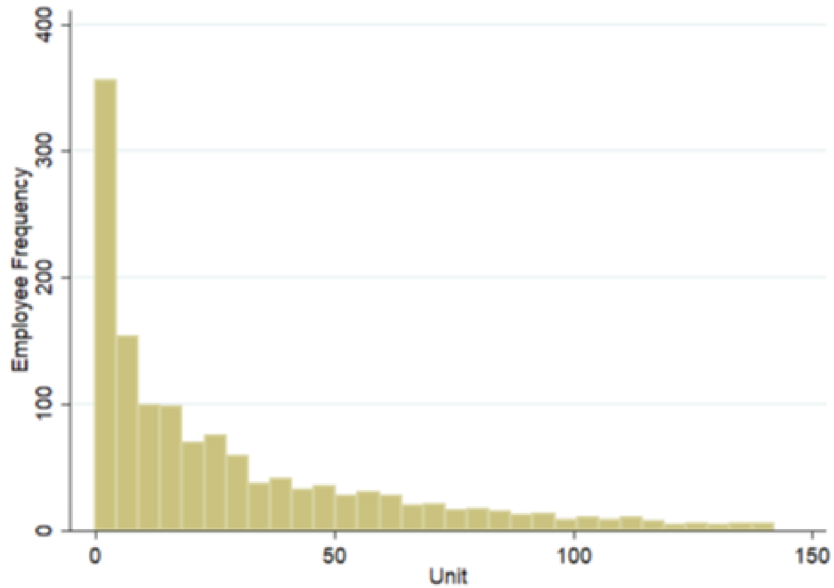
Notes: Immunization at the firm.

Figure B.9: Vaccine Take-up around $750 Wage Threshold



Notes: This figure presents the evolution of vaccine take-up around the $750 threshold with a bin size of $10. Individuals who earn more than $750 paid $7.49 for the vaccine, while employees whose wage is below this threshold paid $4.99.

Figure B.10: Distribution of Employees in Units



Notes: This figure presents the number of employees in each of the 142 units.

B.2 Additional Tables

Table B.1: Regression Discontinuity Effects of Higher Price on Vaccination Take-Up

	Baseline	With Controls	Quito Sample	Non-Compliance
Threshold	0.0590	0.1603	0.0655	0.0400
	(0.0730)	(0.1514)	(0.0786)	(0.0722)
N	608	608	461	604

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

Notes: Robust standard errors in parentheses. This table presents the local average treatment effects of a small price change on vaccination take-up. We report the normalized coefficient at a wage of $750 and a bandwidth of $300. Individuals who earn more than $750 paid $7.49 for the vaccine, while employees whose wage is below this threshold paid $4.99. There is no visible discontinuity across the threshold. All specifications control for city fixed effects. Column 1 presents our main estimates without adding additional controls. In Column 2 we test the robustness of the main estimates controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level. Column 3 presents the estimates using only employees in Quito, the city where we implemented our four treatments. In Column 4 we exclude 12 individuals who were assigned to vaccinate in the workweek but went to vaccinate on Saturday. Reducing the bandwidth in steps of $50 to $150 does not change the results.

Table B.2: Heterogeneous Treatment Effects on Vaccination Take-up

	Men	Women	Short Distance	Long Distance	No Children	Children
Altruistic Information	-0.0017	-0.0508	-0.0564	-0.0477	-0.0163	-0.0368
	(0.0452)	(0.0429)	(0.0441)	(0.0521)	(0.0421)	(0.0454)
Selfish Information	0.0098	-0.0166	-0.0074	-0.0291	0.0188	-0.0253
	(0.0439)	(0.0451)	(0.0460)	(0.0527)	(0.0435)	(0.0452)
Saturday	-0.0883**	-0.0677	-0.0825**	-0.1047**	-0.0531	-0.1056**
	(0.0413)	(0.0441)	(0.0420)	(0.0488)	(0.0396)	(0.0453)
N	593	571	446	449	556	608

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

Notes: Robust standard errors in parentheses. This table presents the effect of the different treatments on vaccination take-up for different subgroups in the study's population.

Table B.3: Recall Information Statements

	Heard Altruistic Statement	Heard Selfish Statement
Altruistic Information	-1.5050	-8.6603**
	(4.9361)	(4.1577)
Selfish Information	-4.1349	-0.2413
	(4.9398)	(4.0169)
Saturday	-3.9293	-2.8269
	(6.2201)	(5.0108)
Baseline	69.95	78.21
N	378	378

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

Notes: Robust standard errors in parentheses. This table presents the effects of the different treatments on measurements of recalling the altruistic and selfish statements. The post-intervention survey collects these measures on a scale from 0 to 100.

Table B.4: Robustness Check on Peer Effects Estimates

	Unit Size	Peer Characteristics
A. Main Effect		
Proportion of peers:		
Vaccinated	0.0079***	0.0071***
	(0.0018)	(0.0019)
N	1138	1138
B. Heterogenous Effects		
Proportion of peers:		
Same Gender Vaccinated	0.0075***	0.0072***
	(0.0020)	(0.0020)
Different Gender Vaccinated	0.0048**	0.0043*
	(0.0024)	(0.0025)

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

Notes: Standard errors clustered at the unit level in parentheses. The bank has 116 units with more than one employee. This table presents the effect of peer vaccination take-up on the individual's vaccination decision. We measure the proportion of peers vaccinated and the proportion of peers assigned to the workweek in percentage points. Thus, the estimates represent the effect of a one percentage point change in the proportion of peers. We define peers as all employees who work in the same unit. All estimates control for Quito fixed effects and individual assignment to the workweek. Column 1 controls for the number of employees in each unit. Column 2 controls for the number of employees in each unit, and peer age and gender.

Table B.5: Robustness Check on Effects of Vaccination on the Flu

	Controls		Broader Definition of Flu	
	Reduced Form	2SLS	Reduced Form	2SLS
a. Being Diagnosed with the Flu				
Assigned to the workweek	0.0011		-0.0118	
	(0.0160)		(0.0173)	
Vaccinated		0.0183		-0.1729
		(0.2434)		(0.2922)
b. Granted a Sick Day because of the Flu				
Assigned to the workweek	0.0095		0.0082	
	(0.0086)		(0.0106)	
Vaccinated		0.1458		0.1218
		(0.1366)		(0.1679)
N	1,148		1,148	

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

Notes: Robust standard errors in parentheses. This table presents the robustness of the effects of flu vaccination on the probability of being diagnosed sick and being granted a sick day because of the flu to the addition of controls (gender, age, tenure, and income) and using a broader definition of the flu. Column 1 presents the reduced form estimates. Column 2 presents 2SLS estimates.

Table B.6: Reduced Form Heterogeneous Effects

	Men	Women	23-45	>45	No Children	Children
			A. Being Diagnosed with the Flu			
Assigned to the workweek	0.0008	0.0049	0.0131	-0.0632	0.0089	-0.0021
	(0.0177)	(0.0271)	(0.0167)	(0.0517)	(0.0235)	(0.0216)
			B. Granted a Sick Day because of the Flu			
Assigned to the workweek	-0.0041	0.0263***	0.0176**	-0.0370	0.0018	0.0198***
	(0.0141)	(0.0087)	(0.0081)	(0.0364)	(0.0149)	(0.0074)
N	585	563	982	166	544	604

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

Notes: Robust standard errors in parentheses. This table presents the robustness of the effects of flu vaccination on the probability of being diagnosed sick and being granted a sick day because of the flu to the addition of controls (gender, age, tenure, and income) and using a broader definition of the flu. Column 1 presents the reduced form estimates. Column 2 presents 2SLS estimates.

Table B.7: Bounds

	Diagnosed with Flu			Diagnosed with Non-flu		
	Main	Lower Bound	Upper Bound	Main	Lower Bound	Upper Bound
Workweek	0.0032	0.0002	0.0023	-0.0748**	-0.0982***	-0.0540
	(0.0160)	(0.0168)	(0.0161)	(0.0363)	(0.0378)	(0.0368)
N	913	899	860	913	899	860

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

Notes: Robust standard errors in parentheses. This table presents bounds for the effect of being assigned to the workweek on the probability of being diagnosed with the flu and other non-flu respiratory diseases. All specifications control for Quito fixed effects.

Table B.8: Reduced Form Effects on Productivity

	Post-Survey		Swipe-Cards		
	General Productivity	Productivity Post-Intervention	Entry to Work	Exit from Work	Duration at Work
Assigned to the workweek	0.1684 (0.1357)	0.1534 (0.1718)	-0.1492 (0.1945)	-0.4879 (0.3487)	-0.3387 (0.4004)
N	378	378	403	403	403

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

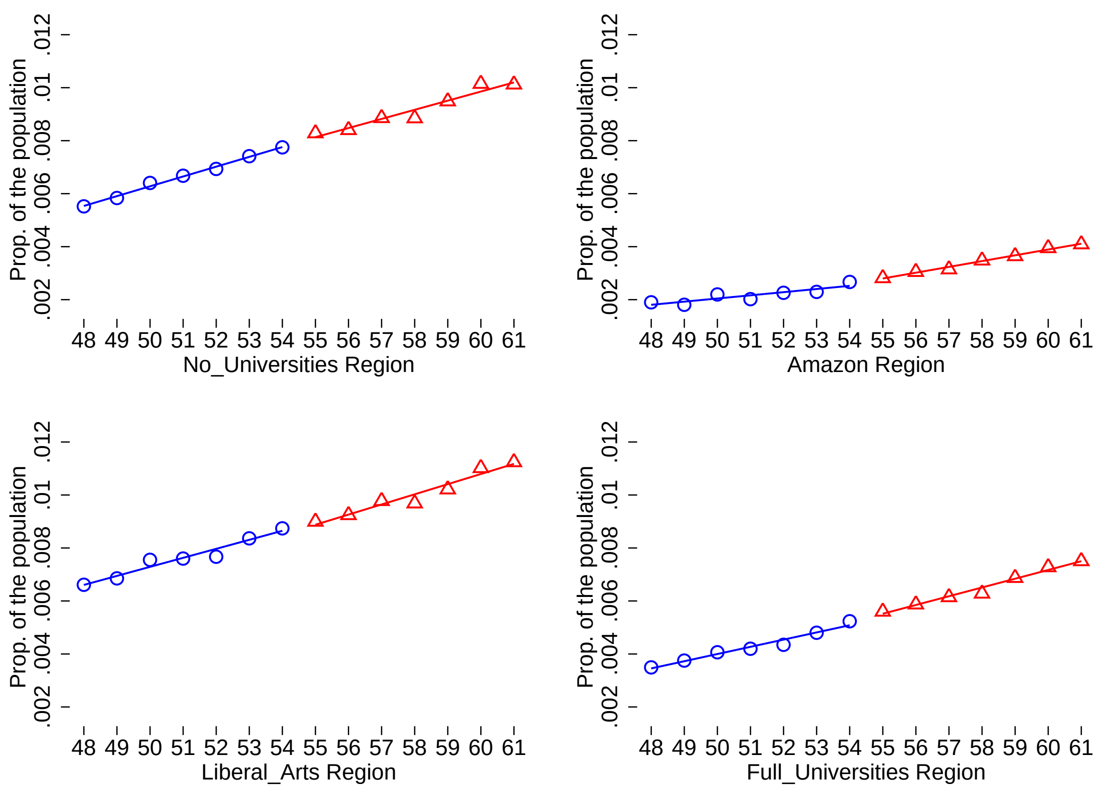
Notes: Robust standard errors in parentheses. This table presents the effect of the assignment to the workweek on self-reported measures productivity and duration of the workday. The post-intervention survey collects these self-reported measures on a scale from 0 to 10. The swipe card information corresponds to January and is measured in hours.

APPENDIX C

A BLESSING OR A CURSE? THE LONG-TERM EFFECT OF RESOURCE BOOMS ON HUMAN CAPITAL AND LIVING CONDITIONS

C.1 Design tests

Figure C.1: Population Distribution by Birth Cohort and Region



Notes: This Figure presents the distribution of the population in Ecuador for the cohorts born between 1948 and 1961 by region of birth. The cohorts born between 1955 and 1961 (red triangles) experienced the effects of the exploitation of oil in the Amazon region.

C.2 Universities and Colleges during the 1970s

Table C.1: Young Adults Living with their Parents in Ecuador

	18-30 years old	18-24 years old
1962	33.8%	45.4%
1974	36.5%	46.5%
1982	37.4%	48.3%
1990	39.4%	51.0%
2001	40.6%	51.0%
2010	40.7%	51.5%

Notes: This Table presents the proportion of young adults who live with their parents according to Ecuador's population censuses of 1962, 1974, 1982, 1990, 2001 and 2010.

Table C.2: Universities and Colleges in Ecuador during the 1970s

	Open since	Province
Universidad de Cuenca	1867	Azuay
Universidad del Azuay	1968	Azuay
Universidad Catolica de Cuenca	1970	Azuay
ESPOCH*	1973	Chimborazo
Universidad Tecnica de Machala*	1969	El Oro
Universidad Tecnica Luis Vargas Torres de Esmeraldas*	1970	Esmeraldas
Universidad de Guayaquil	1883	Guayas
Escuela Superior Politecnica del Litoral	1958	Guayas
Universidad Laica Vicente Rocafuerte de Guayaquil	1966	Guayas
Universidad Nacional de Loja+	1943	Loja
Universidad Tecnica Particular de Loja*	1971	Loja
Universidad Tecnica de Babahoyo*	1971	Los Rios
Universidad Tecnica de Manabi*	1959	Manabi
Universidad Central del Ecuador	1621	Pichincha
Escuela Politecnica Nacional	1869	Pichincha
Escuela Politecnica del Ejercito	1922	Pichincha
Pontificia Universidad Catolica del Ecuador	1946	Pichincha
Universidad Tecnica de Ambato*	1969	Tungurahua

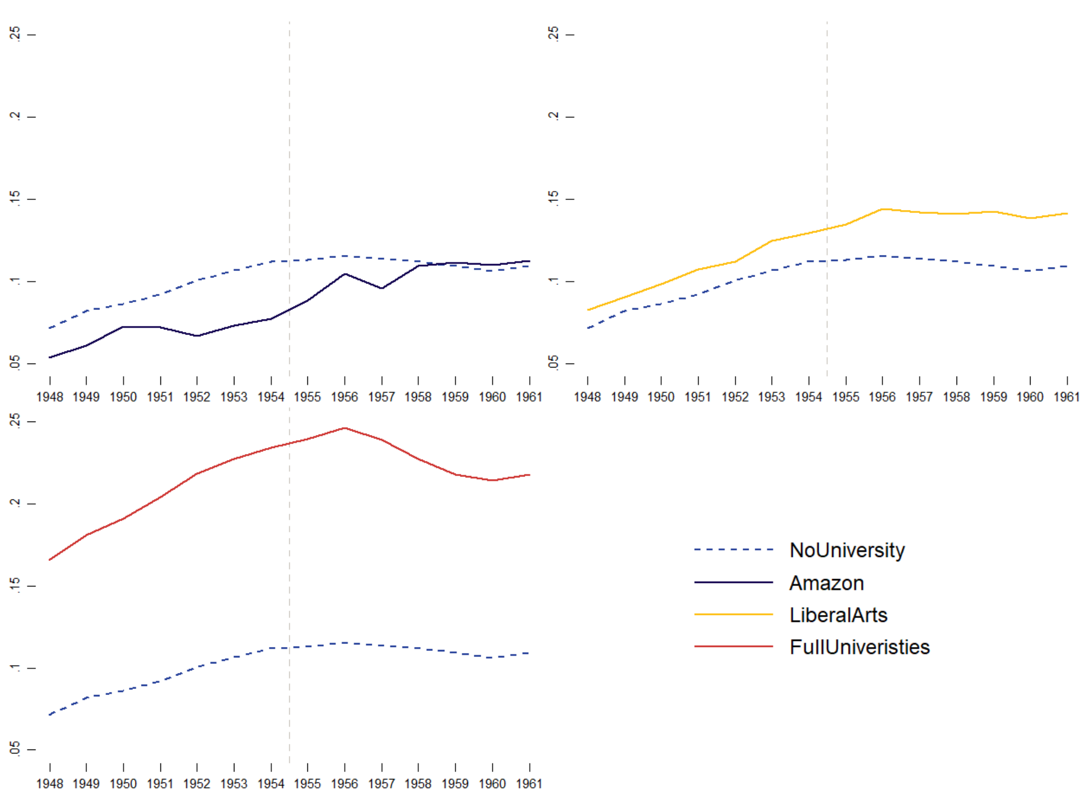
*Technical school focused on agriculture during the 1970s

+ Had a second campus in the Imbabura province in the north of the country

Notes: This Table presents list of universities and technical colleges that functioned in Ecuador during the 1970s. Technical colleges focused on agriculture at that time. The Table lists the institution's name, its opening date and the province where it is located.

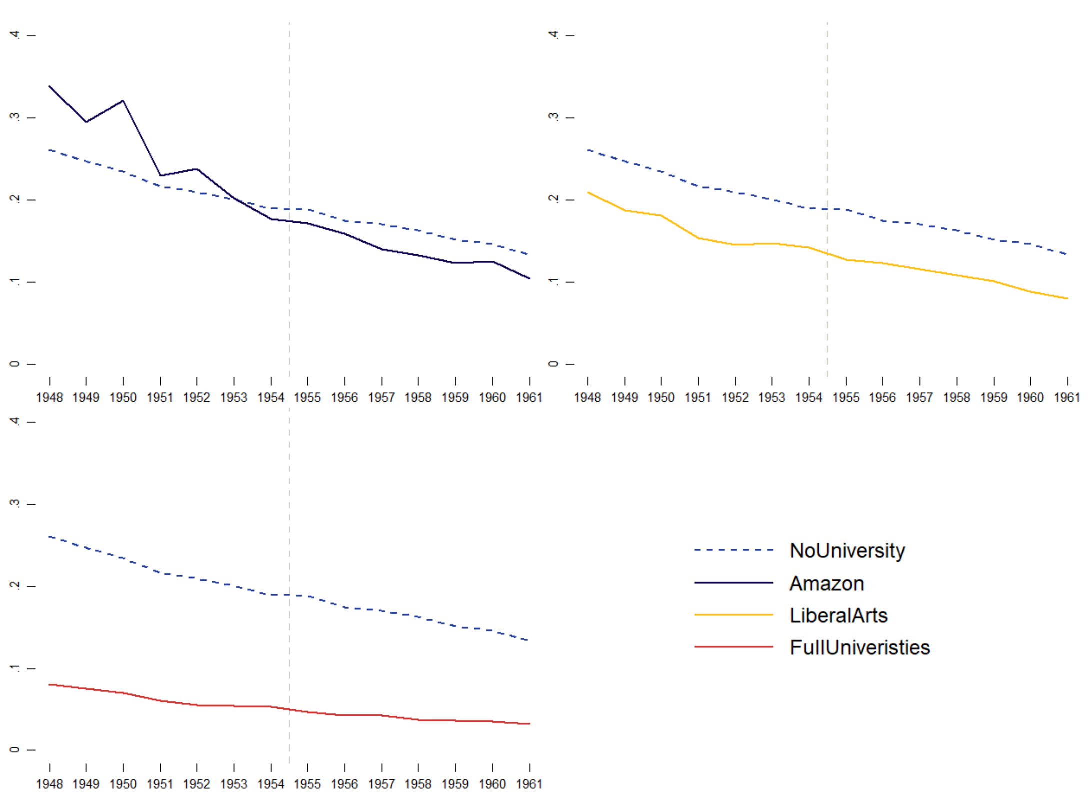
C.3 Additional Graphs and Tables

Figure C.2: College Completion by Birth Cohort



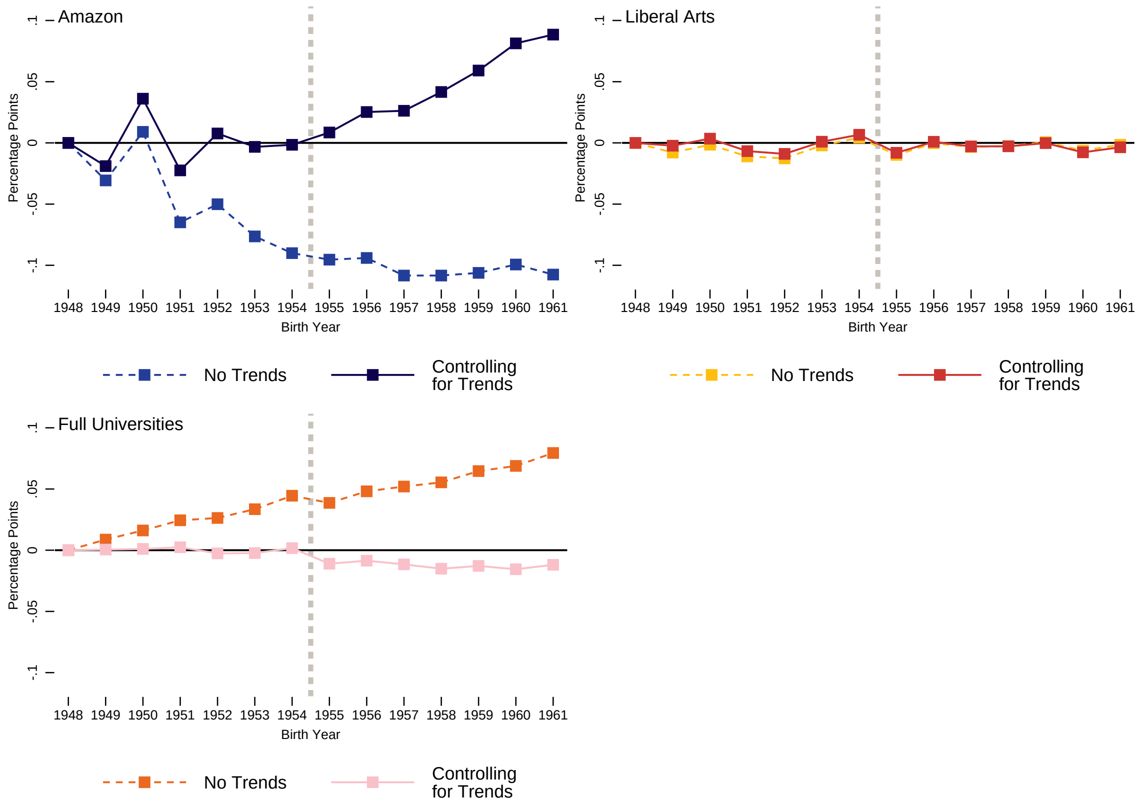
Notes: This Figure presents the evolution of the proportion of the population who graduated from college in Ecuador for the cohorts born between 1948 and 1961. The horizontal axis plots the year of birth. The country is divided into four regions depending on the geographic location and type of universities before the oil boom. The cohorts born between were exposed to the oil boom before turning 18 years old.

Figure C.3: Population with no Completed Education by Birth Cohort



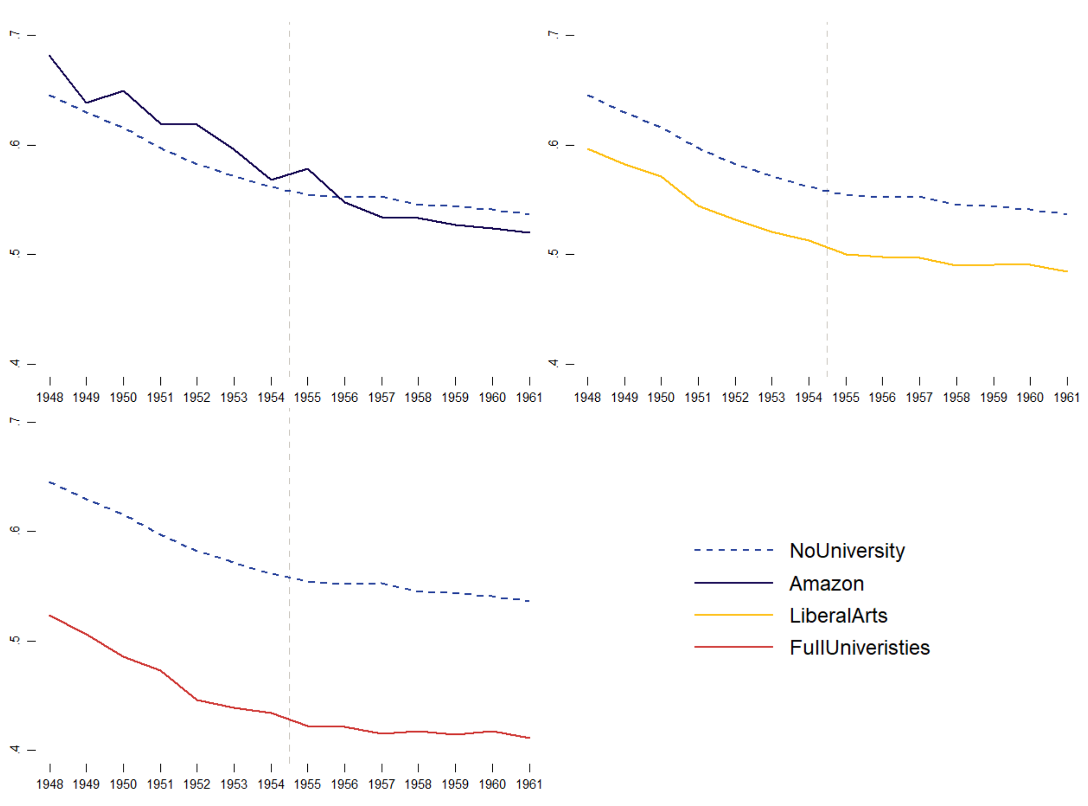
Notes: This Figure presents the evolution of the proportion of the population with no completed education in Ecuador for the cohorts born between 1948 and 1961. The horizontal axis plots the year of birth. The country is divided into four regions depending on the geographic location and type of universities before the oil boom. The cohorts born between were exposed to the oil boom before turning 18 years old.

Figure C.4: Effects on the Probability of Not Completing Any Educational Level



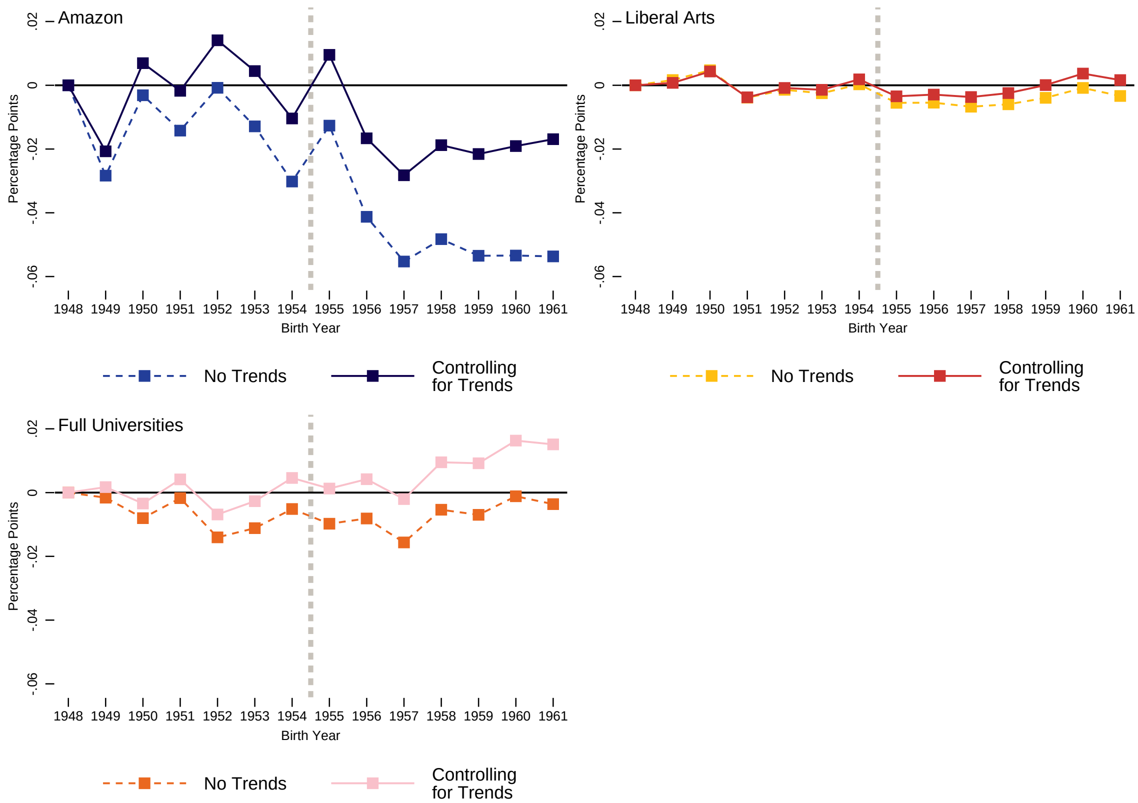
Notes: This Figure presents dynamic difference in difference estimates of the effects of unobserved shocks on the probability of not completing any educational level. Dashed lines present conventional difference in difference estimates, and solid lines control for differential trends. These estimates take the the region without universities as the base region.

Figure C.5: Population Working Informally in 2012 by Birth Cohort



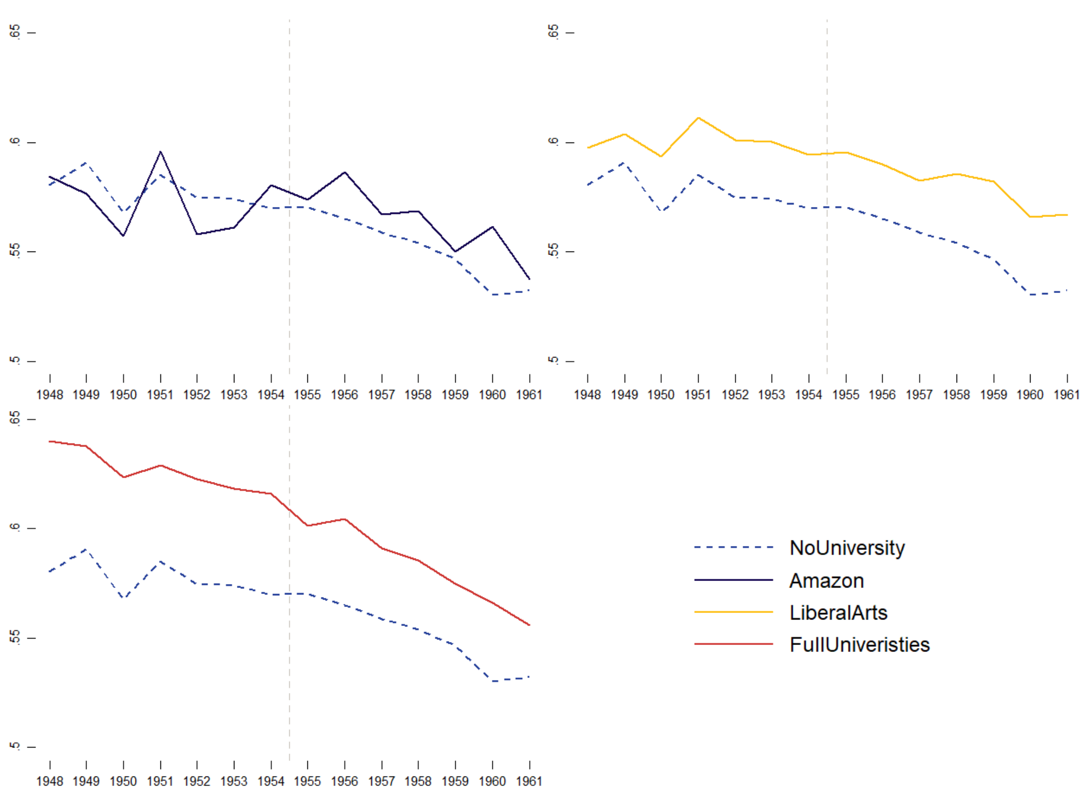
Notes: This Figure presents the evolution of the proportion of the population working informally in Ecuador for the cohorts born between 1948 and 1961. The data correspond to 2012. The horizontal axis plots the year of birth. The country is divided into four regions depending on the geographic location and type of universities before the oil boom. The cohorts born between were exposed to the oil boom before turning 18 years old.

Figure C.6: Effects of Exposure to the Oil Boom Before Turning 18 on Informal Employment



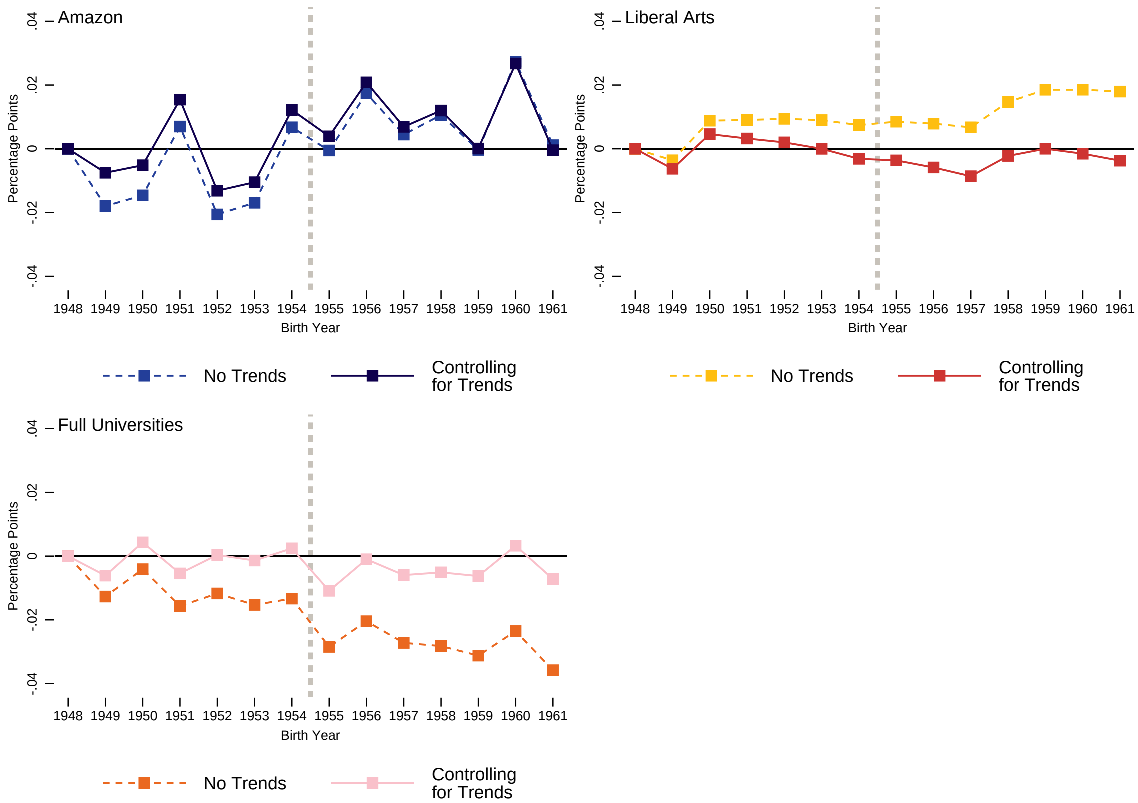
Notes: This Figure presents dynamic difference in difference estimates of the the effect of exposure to the oil boom before turning 18 on the probability of working informally in 2012. Dashed lines present conventional difference in difference estimates, and solid lines control for differential trends. These estimates take the the region without universities as the base region.

Figure C.7: Population Owning a Home with more than two Rooms in 2010 by Birth Cohort



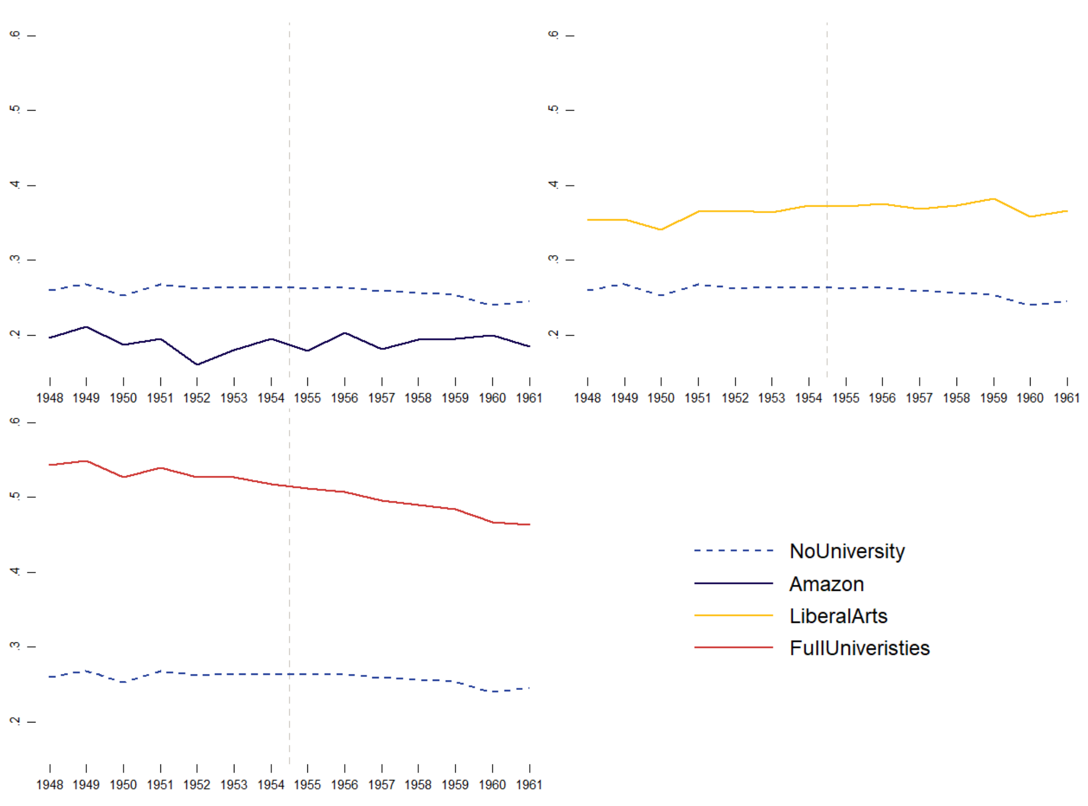
Notes: This Figure presents the evolution of the proportion of the population who owns a home with more than two rooms in Ecuador for the cohorts born between 1948 and 1961. The data correspond to 2010. The horizontal axis plots the year of birth. The country is divided into four regions depending on the geographic location and type of universities before the oil boom. The cohorts born between were exposed to the oil boom before turning 18 years old.

Figure C.8: Effects of Exposure to the Oil Boom Before Turning 18 on the Probability of Owning a Home with more than two Rooms



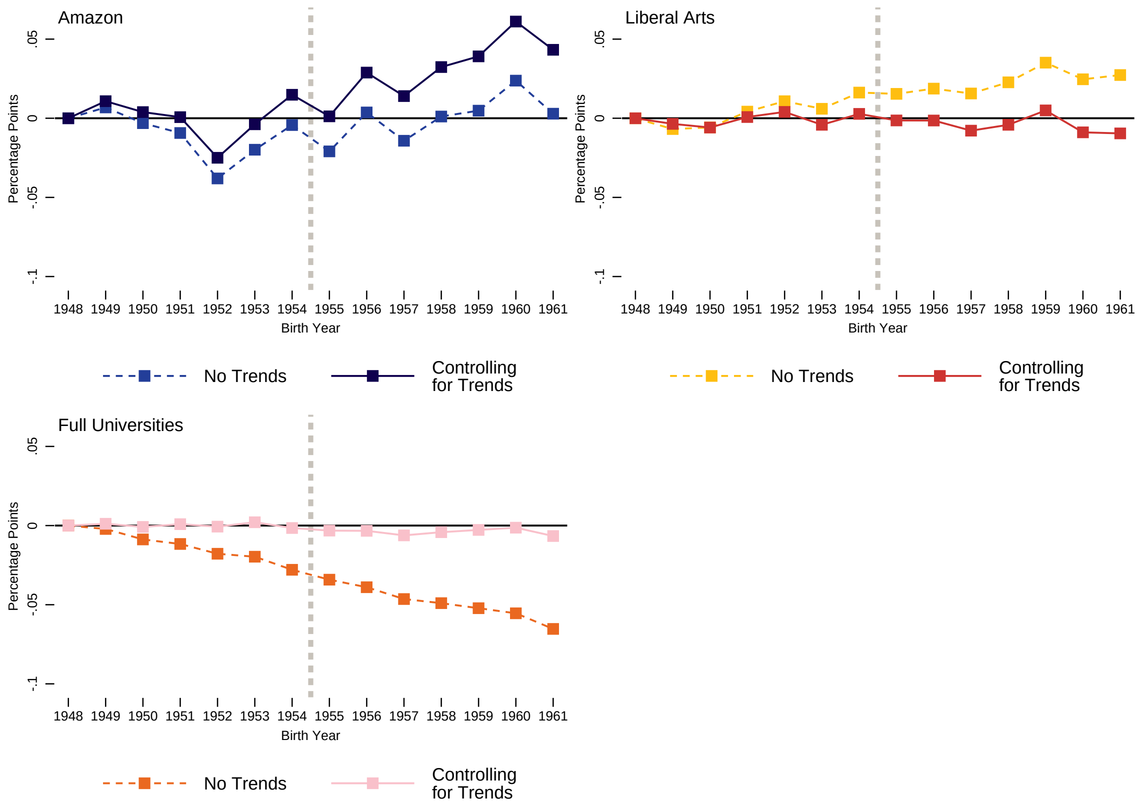
Notes: This Figure presents dynamic difference in difference estimates of the the effect of exposure to the oil boom before turning 18 on the probability of owning a home with more than two rooms in 2010. Dashed lines present conventional difference in difference estimates, and solid lines control for differential trends. These estimates take the the region without universities as the base region.

Figure C.9: Population Owning a Home of Quality above the Median in 2010 by Birth Cohort



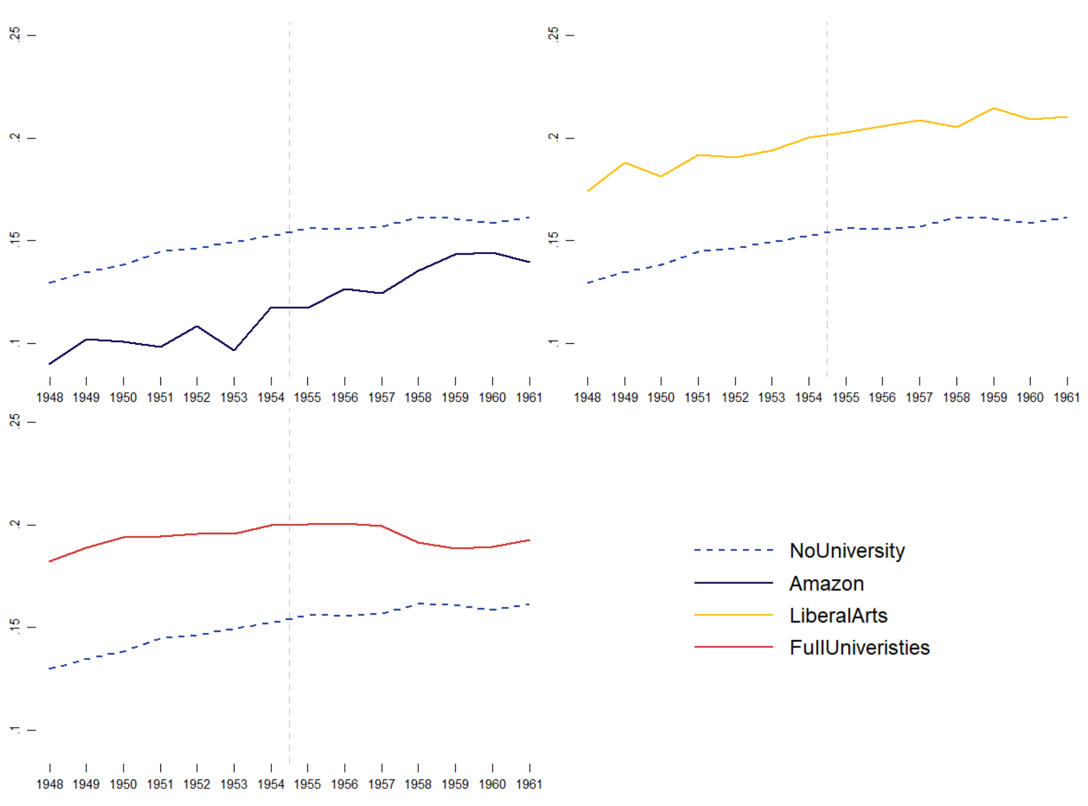
Notes: This Figure presents the evolution of the proportion of the population who owns a home of quality above the median of the quality index in Ecuador for the cohorts born between 1948 and 1961. The data correspond to 2010. The horizontal axis plots the year of birth. The country is divided into four regions depending on the geographic location and type of universities before the oil boom. The cohorts born between were exposed to the oil boom before turning 18 years old.

Figure C.10: Effects of Exposure to the Oil Boom Before Turning 18 on the Probability of Owning a Home of Quality above the Median



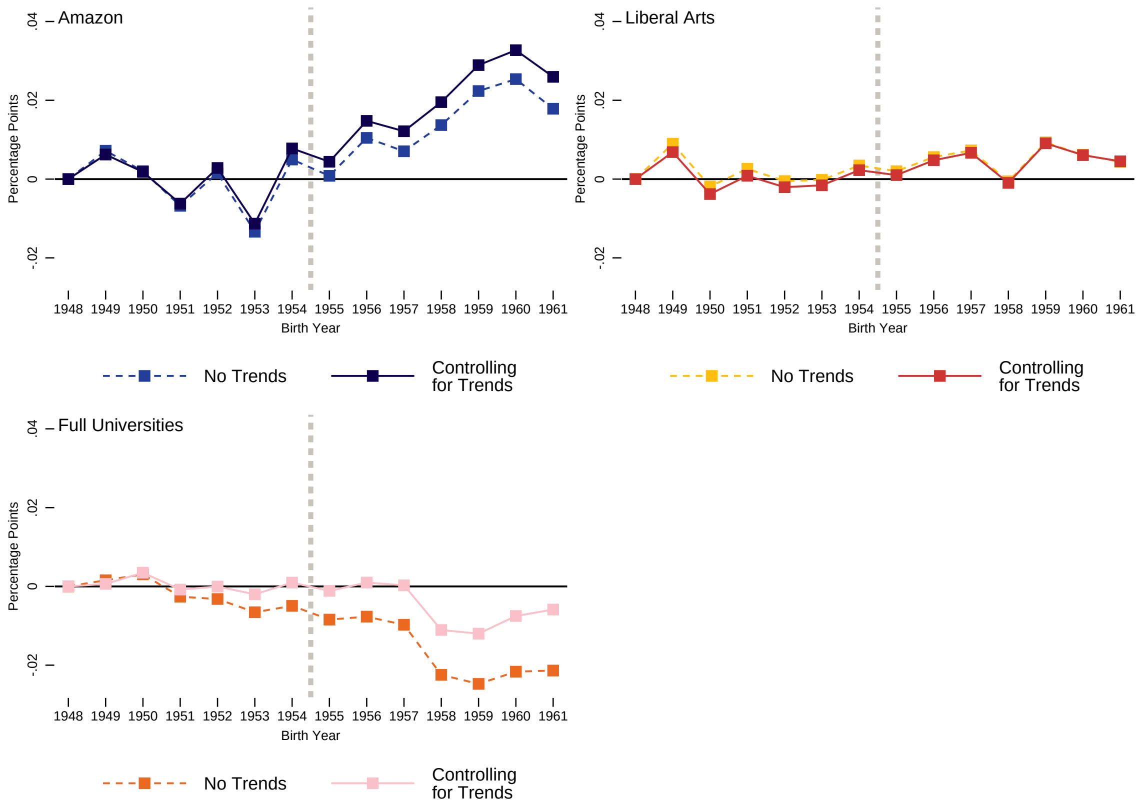
Notes: This Figure presents dynamic difference in difference estimates of the the effect of exposure to the oil boom before turning 18 on the probability of owning a home of quality above the median of the quality index in 2010. Dashed lines present conventional difference in difference estimates, and solid lines control for differential trends. These estimates take the the region without universities as the base region.

Figure C.11: Population Owning a Vehicle in 2013 by Birth Cohort



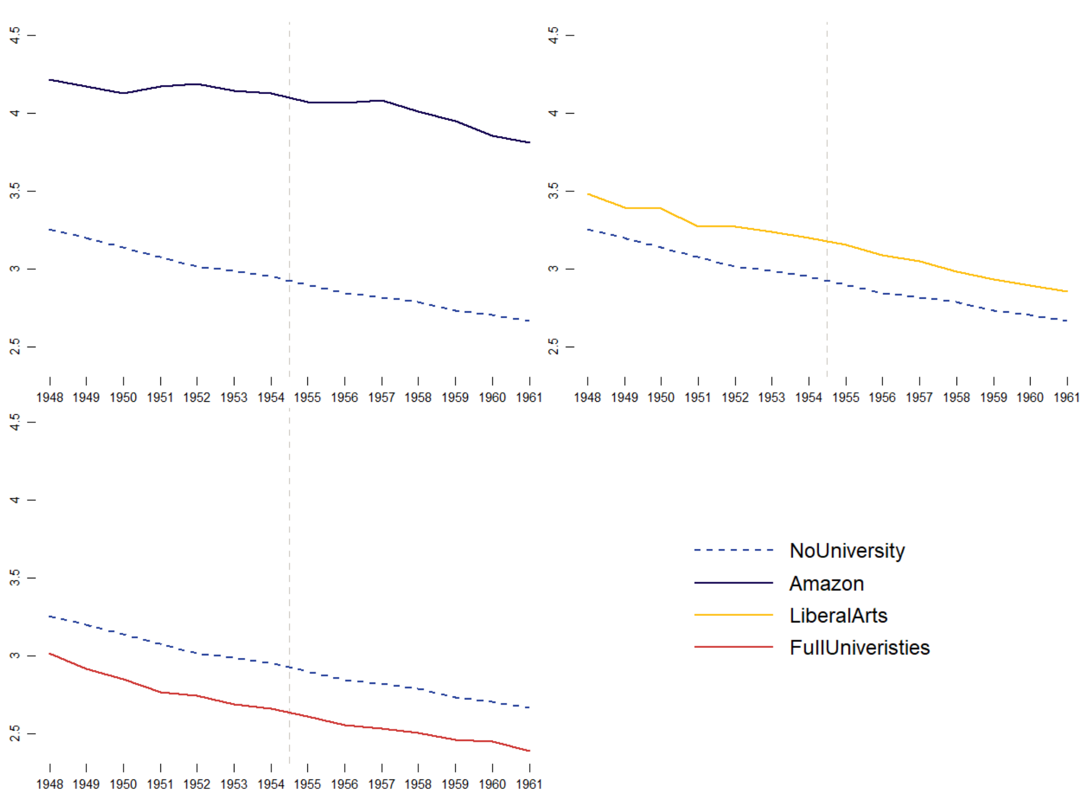
Notes: This Figure presents the evolution of the proportion of the population who owns a vehicle in Ecuador for the cohorts born between 1948 and 1961. The data correspond to 2013. The horizontal axis plots the year of birth. The country is divided into four regions depending on the geographic location and type of universities before the oil boom. The cohorts born between were exposed to the oil boom before turning 18 years old.

Figure C.12: Effects of Exposure to the Oil Boom Before Turning 18 on the Probability of Owning a Vehicle



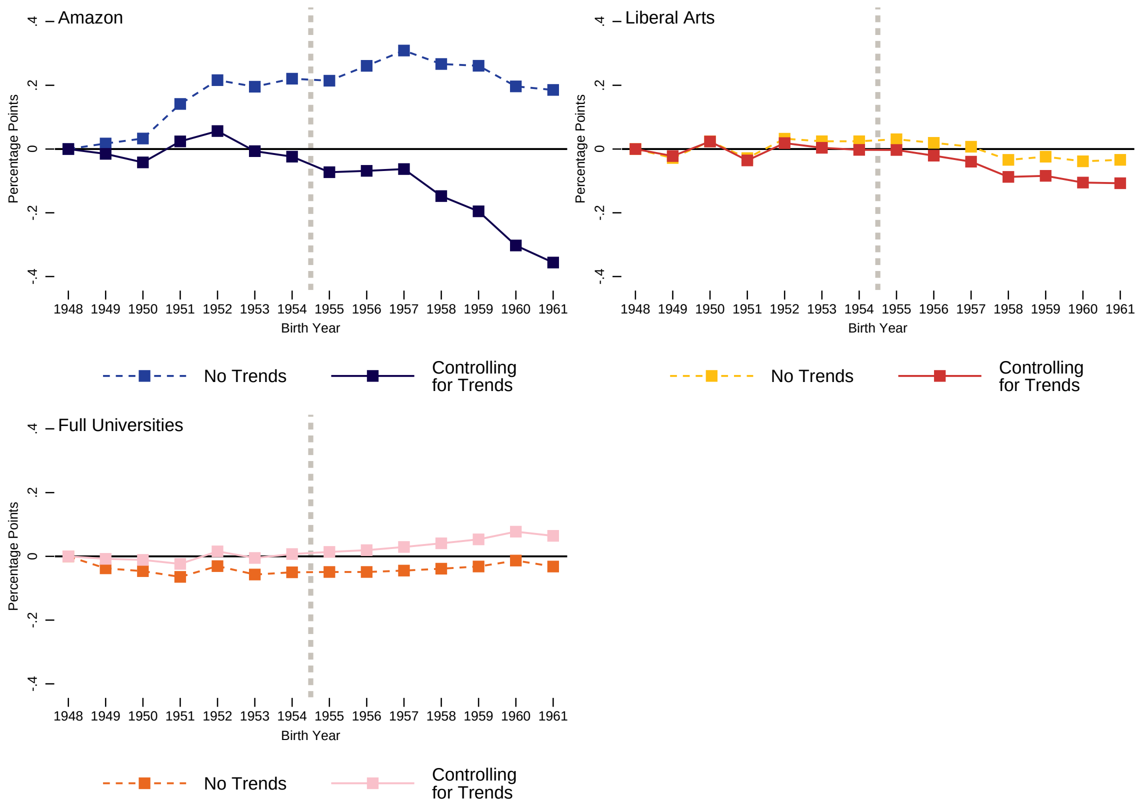
Notes: This Figure presents dynamic difference in difference estimates of the the effect of exposure to the oil boom before turning 18 on the probability of owning a vehicle in 2013. Dashed lines present conventional difference in difference estimates, and solid lines control for differential trends. These estimates take the the region without universities as the base region.

Figure C.13: Number of Children by Birth Cohort



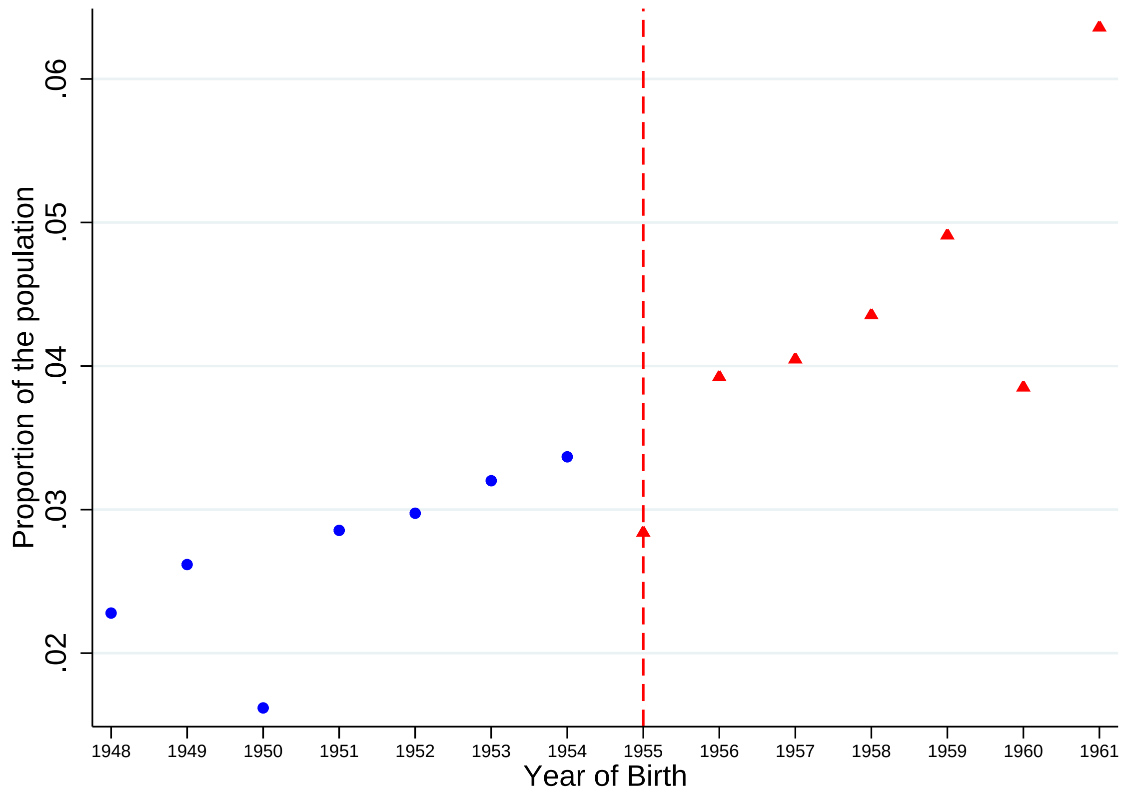
Notes: This Figure presents the evolution of the number of children for the cohorts born between 1948 and 1961. The data correspond to 2014. The horizontal axis plots the year of birth. The country is divided into four regions depending on the geographic location and type of universities before the oil boom. The cohorts born between were exposed to the oil boom before turning 18 years old.

Figure C.14: Effects of Exposure to the Oil Boom Before Turning 18 on the Number of Children



Notes: This Figure presents dynamic difference in difference estimates of the the effect of exposure to the oil boom before turning 18 on the number of children in 2014. Dashed lines present conventional difference in difference estimates, and solid lines control for differential trends. These estimates take the the region without universities as the base region.

Figure C.15: College Completion by Birth Cohort in Indonesia



Notes: This Figure presents the evolution of college completion for the cohorts born in Indonesia between 1948 and 1961. The cohorts born between 1955 and 1961 (red triangles) turned 18 years old during the oil boom in the 1970s. This Figure uses data from a 10 percent random sample of Indonesia's 2010 population census (Minnesota Population Center, 2017). The drops correspond to birth years that are multiples of five. Apparently, individuals with low education round their age/year of birth to the closest multiple of five. I have found evidence of this rounding in self-reported data sets from other developing countries.

Table C.3: Effects of Exposure to the Oil Boom Before Turning 18 on College Completion by Gender

Born in:	1955	1956	1957	1958	1959	1960	1961	1955-1961
				Women				
Full	0.0014	0.0072	-0.0014	-0.0142	-0.0261	-0.0405	-0.0431	-0.0184
Universities	(0.0049)	(0.0054)	(0.0058)	(0.0063)**	(0.0068)***	(0.0073)***	(0.0080)***	(0.0055)***
	(0.0024)	(0.0039)*	(0.0088)	(0.0118)	(0.0137)*	(0.0171)**	(0.0166)***	(0.0107)*
Liberal	0.0011	0.0006	0.0102	0.0092	0.0129	0.0116	0.0078	0.0078
Arts	(0.0043)	(0.0047)	(0.0051)**	(0.0056)	(0.0060)**	(0.0064)*	(0.0070)	(0.0047)*
	(0.0036)	(0.0049)	(0.0056)*	(0.0070)	(0.0105)	(0.0116)	(0.0121)	(0.0073)
Amazon	0.0122	0.0159	0.0307	0.0313	0.0544	0.0408	0.0528	0.0357
Region	(0.0107)	(0.0115)	(0.0127)**	(0.0135)**	(0.0149)***	(0.0157)***	(0.0170)***	(0.0116)***
	(0.0109)	(0.0117)	(0.0116)***	(0.0130)**	(0.0145)***	(0.0187)**	(0.0214)**	(0.0124)***
				Men				
Full	-0.0074	-0.0151	-0.0263	-0.0441	-0.0552	-0.0522	-0.0575	-0.0387
Universities	(0.0057)	(0.0062)**	(0.0067)***	(0.0074)***	(0.0080)***	(0.0087)***	(0.0095)***	(0.0066)***
	(0.0060)	(0.0040)***	(0.0118)**	(0.0097)***	(0.0083)***	(0.0069)***	(0.0098)***	(0.0076)***
Liberal	0.0053	0.0175	0.0034	0.0027	0.0054	0.0011	0.0023	0.0051
Arts	(0.0052)	(0.0057)***	(0.0061)	(0.0066)	(0.0072)	(0.0078)	(0.0084)	(0.0058)
	(0.0036)	(0.0058)***	(0.0068)	(0.0073)	(0.0096)	(0.0099)	(0.0126)	(0.0072)
Amazon	0.0172	0.0479	0.0261	0.0631	0.0561	0.0787	0.0744	0.0544
Region	(0.0127)	(0.0143)***	(0.0148)*	(0.0166)***	(0.0177)***	(0.0193)***	(0.0209)***	(0.0144)***
	(0.0161)	(0.0147)***	(0.0168)	(0.0178)***	(0.0195)***	(0.0255)***	(0.0243)***	(0.0179)***

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

Notes: This Table presents the effect of exposure to the oil boom before turning 18 on the probability of graduating from college for the cohorts born in 1955-1961 by gender. The first seven columns present the effect for each cohort. The last column shows the average of these effects across cohorts using using population as weights. Standard errors are in parentheses. The first row of standard errors corresponds to heteroskedastic robust standard errors. The second row of standard errors are clustered at the canton level for robustness (215 clusters). The estimates control for different trends across regions for the cohorts who turned 18 before 1973. The estimation sample includes all individuals born in Ecuador between 1948 and 1955 (870,046 women and 841,492 men).

Table C.4: Effects of Exposure to the Oil Boom Before Turning 18 on the Number of Children

Born in:	1955	1956	1957	1958	1959	1960	1961	1955-1961
Full	0.0139	0.0195	0.0295	0.0410	0.0533	0.0775	0.0643	0.0446
Universities	(0.0176)	(0.0193)	(0.0214)	(0.0236)*	(0.0259)**	(0.0285)***	(0.0311)**	(0.0216)**
	(0.0116)	(0.0223)	(0.0213)	(0.0348)	(0.0502)	(0.0447)*	(0.0529)	(0.0325)
Liberal	-0.0029	-0.0208	-0.0396	-0.0874	-0.0841	-0.1051	-0.1072	-0.0662
Arts	(0.0211)	(0.0229)	(0.0250)	(0.0277)***	(0.0302)***	(0.0330)***	(0.0359)***	(0.0247)***
	(0.0194)	(0.0224)	(0.0251)	(0.0333)***	(0.0333)**	(0.0410)**	(0.0432)**	(0.0280)**
Amazon	-0.0726	-0.0684	-0.0628	-0.1475	-0.1954	-0.3025	-0.3560	-0.1854
Region	(0.0758)	(0.0812)	(0.0898)	(0.0979)	(0.1077)*	(0.1167)***	(0.1268)***	(0.0890)**
	(0.0613)	(0.0762)	(0.0805)	(0.1019)	(0.1064)*	(0.1255)**	(0.1351)***	(0.0869)**

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

Notes: This Table presents the effect of exposure to the oil boom on the the number of children per adult for the cohorts born in 1955-1961. The number of children is measured in 2014. The first seven columns present the effect for each cohort. The last column shows the average of these effects across cohorts using using population as weights. Standard errors are in parentheses. The first row of standard errors corresponds to heteroskedastic robust standard errors. The second row of standard errors are clustered at the canton level for robustness (215 clusters). The estimates control for different trends across regions for the cohorts who turned 18 before 1973. The estimation sample includes all individuals born in Ecuador between 1948 and 1955 with children ($n = 1,366,190$).