Vaccines at Work: Experimental Evidence from a Firm Campaign

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This version: April 19, 2023

Abstract

Firms are increasingly implementing health campaigns to reduce employee absenteeism, but it remains unclear to what extent these campaigns could be a successful policy. Based on a natural field experiment, we provide causal evidence on the determinants and consequences of participating in a firm campaign encouraging employees to vaccinate against influenza. We find that opportunity costs and peer behavior matter to increase vaccination take-up. However, contrary to the firm's expectations, increasing take-up did not reduce sickness absence during the flu season. Furthermore, vaccinated employees may exhibit riskier behaviors concerning their health, which could decrease the benefits of such campaigns.

JEL Classification: D90, I12, J22, M50, N36

Keywords: Sickness-Related Absence, Health Intervention, Flu Vaccination, Field Experiment, Random Encouragement Design, Moral Hazard, Technology Adoption,

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Acknowledgements: The authors are grateful for comments from Ragan Petrie, Marco Castillo, Joanna Lahey, Laura Dague, Steve Puller, Jason Lindo, Abu Siddique, and Corey White as well as the participants of the Economics Research Laboratory of Texas A&M University. This paper has also benefited from comments by participants at the SAAER 2018, TExAS 2018, SEA 2018, ESA World 2018, North-American ESA 2018, Advances with Field Experiments 2018, AHEC 2018, Stata Texas Empirical Microeconomics 2018, ASSA/AEA 2019, WEAI 2019, ESPE 2019, EEA/ESEM 2019, German Economic Association 2019, IAB Workshop on Field Experiments in Policy Evaluation 2019, EALE/SOLE/AASLE 2020, the 8th IZA Workshop: Environment, Health and Labor Markets 2021, SEEDEC 2021 and presentations at the University of Konstanz, University of California, Irvine, University of Vechta, University of Melbourne, Boston College, Duisburg-Essen University, and University of Alberta. This project is registered (AEARCTR-0005479), and the research received IRB approval (IRB2017-0657D) from Texas A&M University and ethical approval in Ecuador.

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1. Introduction

Company management is interested in reducing employee sickness and absence since it is costly to the firm. Employees on sick leave commonly receive compensation while not providing any productive benefit to the firm. A possible way to reduce the sickness-related absence of employees is by leveraging health campaigns. In particular preventive health campaigns to vaccinate against influenza (the flu) could be a low-cost way to achieve this goal. This aligns with recommendations of public health institutions like the Centers for Disease Control and Prevention (CDC), encouraging employers to offer flu vaccines at the workplace (CDC, 2022) based on the notion that the flu is a major health concern. The World Health Organization (WHO) estimates that the flu is associated with three to five million cases of severe respiratory illnesses per year worldwide (WHO, 2018). In the United States, the flu is associated with an economic burden of approximately $34.7 billion per year (Rothman, 2017) and 16 million days of lost productivity (Molinari et al., 2007). Hence, flu vaccination seems to be a promising approach to tackle a significant health problem and its economic consequences for companies in the form of employee absenteeism.

However, individual behavior can counter the potential benefits of vaccination. First, according to public health institutions, vaccination rates are usually below recommended levels, ranging from 2% to 70% in European countries (Mereckiene, 2015) and 38.5% among adults in the United States during the 2017–2018 flu season (Srivastav et al., 2018). Therefore, it is essential to understand the determinants of vaccination, especially in populations with low take-up rates, like working-age adults. Second, economic theory and empirical evidence suggest that adopting protective technologies may induce individuals to undertake riskier behaviors. Individuals may overestimate the protection that the vaccine grants and engage in risky behaviors such as waiting longer before visiting the doctor when feeling sick and taking fewer protective measures to prevent illnesses, be it flu-related or not. Such forms of moral hazard could counter the benefits of adopting a preventive medical technology like flu vaccination in a company workforce or other contexts.

For this research, we exploited the background of a company's vaccination campaign to present the first comprehensive study of the determinants and consequences of flu vaccination based on a natural field experiment.[1] In cooperation with a bank in Ecuador that conducts health campaigns to reduce sickness absence, we modified the company's 2017 vaccination campaign to create

[1] We follow the definition of a natural field experiment by studying behavior in an environment where subjects make their decisions naturally without knowing that they are participants in an experiment (Harrison & List, 2004).

exogenous variation in take-up. First, we introduced income-dependent subsidies and selected an income threshold at which the vaccine price for employees would change. Second, due to capacity constraints, assigning employees to be vaccinated during the workweek or on Saturday was necessary. By randomizing the assignment of employees for vaccination, we could manipulate the opportunity costs of participating in the campaign, as employees either had to arrange their weekend schedules to get to the firm’s location or did not have such extra costs when getting vaccinated during the workweek. Third, we varied the content of the invitation emails to appeal to altruistic or selfish motives.

The exogenous variation in vaccination rates generated by our campaign modification allows us to study the consequences of employees getting a flu shot. First, we analyze peer vaccination's effects on a co-worker's propensity to vaccinate. Second, we study individual and peer vaccination's impact on employees’ health and sickness-related absence, which also informs us of the economic benefit of the campaign. Third, we explore the behavioral implications of vaccinating employees to gauge the possibility of moral hazard behaviors when adopting medical technology.

Our design overcomes several challenges that arise when studying the consequences of vaccination. The first challenge is to provide causal evidence. To the best of our knowledge, no natural field experiment has yet been conducted to identify the causal effect of vaccinating against the flu. While the medical literature documents only modest positive health effects of flu vaccination, with no clear evidence for sickness absence, reviews of this research point out several limitations of the existing evidence (Demicheli et al., 2014; Demicheli et al., 2018; Jefferson et al., 2010; Osterholm et al., 2012; Østerhus, 2015). Despite well-known identification problems like the “healthy vaccine recipient effect” that could bias non-experimental evidence, observational studies are often preferred because of ethical concerns for randomized controlled trials (RCTs) with placebos in the context of health (Baxter et al., 2010; Sanson-Fisher et al., 2007). For the same reason, RCTs are often conducted using other types of vaccines instead of pure placebos to provide potential health benefits for experimental participants in the control group (Loeb et al., 2010). We present a methodological alternative that addresses ethical concerns by using the exogenous variation in vaccination rates generated through manipulating incentives to participate in a company campaign. To study the causal effects of vaccination, we thus employ a random encouragement design (Bjorvatn et al., 2020; List et al., 2017) as an innovative approach in the context of preventive medical technologies to circumvent the ethical dilemma of withholding a potentially effective treatment.

The second challenge we address is capturing the total health impact of vaccination, including behavioral effects which could indirectly affect health. While companies and policymakers are mainly interested in the total health effect, encompassing both medical and behavioral responses, medical research on vaccines focuses on the direct medical effect. Subjects in RCTs usually know that they are part of an experiment, but they do not know if they have received a specific type of vaccine, which eliminates any variation in behavioral responses to vaccination across experimental conditions. In contrast, our random encouragement design introduces no uncertainty in treatment, allowing us to study the total health effect and behavioral effects due to vaccination.

The bank's data also allow us to address a third challenge. There is a potential to underestimate the effectiveness of a vaccine due to externalities. Vaccinated peers could also encourage co-workers to get vaccinated, which may improve their health if the vaccine prevents them from getting sick. Furthermore, peer vaccination could indirectly affect health, even without individual vaccination, as previous research indicates positive health spillovers from the vaccinated to the unvaccinated in the broader population (White, 2021). While this idea of reduced disease transmission is unlikely to play a role in our setting, with flu vaccination rates in Ecuador fluctuating around 2% (ENSANUT, 2012), we can inspect the role of such externalities in a company workforce by using exogenous variation in peer vaccination across work units.

For our analysis of health-related outcomes, we utilize the access granted to the bank's administrative data and merge these with individual treatment assignment information. The data include detailed health information so that the resulting sick days can be identified for each employee. We also exploit information from medical diagnoses to distinguish flu-related sickness from non-flu-related sickness, which allows us to shed light on the behavioral effects of vaccination. Finally, employee surveys conducted before and after the campaign complement the administrative data and enable further inspections of the relevant mechanisms.

Regarding the incentives for vaccination we introduced exogenously, the first stage results are as follows. A change of $2.48 in the vaccine's price did not affect the take-up rate. Conversely, assigning employees to vaccinate during the workweek increased take-up by about ten percentage points, which constitutes an increase of roughly 100 percent compared with Saturday appointments. It appears that reducing opportunity costs is found to have a remarkably strong effect on take-up for working adults. Finally, we find no effect from providing information on vaccination's altruistic or personal benefits.

Next, we exploit the exogenous variation created by randomly assigning employees to get vaccinated during the workweek to study the consequences of vaccination. First, we study the effect of peer vaccination on individual take-up. For this purpose, we use exogenous variation in the proportion of vaccinated co-workers in the same work unit. Given randomization at the employee level, by chance, some units have more employees assigned to the workweek than other units; hence, they could be more encouraged to get the vaccine than those in other units. According to our findings, when the proportion of peers vaccinated increases by ten percentage points, individual take-up rates increase by 7.9 percentage points.

Second, we investigate the most relevant consequences of vaccine take-up from the perspective of policymakers and firms that run health campaigns: whether flu vaccination effectively improves working adults' health, thereby reducing sickness absence. If influenza is a major driver of ill-health and vaccination is effective, encouraging a substantial number of employees to be vaccinated should reduce absence from work. However, our estimates show no evidence that exogenously triggered vaccination affects health in general or sickness absence. A back-of-the-envelope cost-benefit analysis shows that the campaign most likely did not generate any significant economic benefit to compensate the firm for the money invested in it.

There are several potential explanations for the lack of evidence for health improvements due to vaccination. It could be that we underestimate health benefits because of externalities. However, our results show that peer vaccination does not affect the probability of being diagnosed sick or taking a sick day, implying that health was not affected by increased take-up through peer effects, nor by any health spillovers from the vaccinated to the unvaccinated. As another explanation, the vaccine may have been medically ineffective, which we cannot inspect deeper in our study. As a related but separate explanation, the flu may not be the major driver of ill-health in a company with a working-age population, so any effort to tackle the disease cannot substantially affect sickness absence, even if the vaccine was effective. Finally, participation in a program to prevent illness may trigger riskier and thus health-threatening behavior as a general mechanism to affect the success of health interventions.

We, therefore, focus in the last part of our study on the behavioral impacts of vaccination. To test the hypothesis that individuals overestimate the protection against illness, we exploited the incidence of a national health emergency during our investigation in January 2018. That month, the Ecuadorian government launched a widespread media campaign encouraging people to visit their doctor if they felt any flu-related symptoms. To reveal behavioral effects, we exploit the data on medical diagnoses to investigate how vaccination affects diagnoses of non-flu-related respiratory illnesses, which share symptoms with the flu. The idea is that flu vaccination cannot medically affect the probability of being diagnosed with these diseases, but those employees who feel protected may refuse to visit a doctor and thus get fewer diagnoses of mild non-flu respiratory diseases. Indeed, we find a negative effect of assigning employees to be vaccinated during the workweek on the likelihood of being diagnosed with a non-flu respiratory disease in January, with no effect in other months. Apparently, vaccinated employees thought flu-like symptoms indicated just a minor respiratory illness without the need to follow the government's advice. This is supported by another result showing that workweek assignment for vaccination decreased visits to the bank's on-site doctor in January, with no effect in other months. Finally, evidence from the employee survey shows that workweek assignment for a flu shot reduces practices that are believed to help prevent sickness. This effect interacts with beliefs on vaccine effectiveness, which supports the idea of vaccinated individuals feeling protected and engaging in riskier practices.

Our findings contribute to two strands of literature. The first is the literature on the determinants of take-up rates of vaccination and other medical technologies. Previous studies in this area mainly discuss how vaccination take-up rates are affected by laws, information, education, age, health status, health behavior, and lifestyle (Bradford & Mandich, 2015; Chang, 2018; Godinho et al., 2016; Maurer, 2009; Oster, 2018; Schaller et al., 2019; Schmitz & Wuebker, 2011). However, monetary and opportunity costs are likely relevant in adopting medical technologies such as vaccines (Brito et al., 1991; Chen and Toxvaerd, 2014; Geoffard and Philipson, 1997; Kremer and Miguel, 2007). Few studies have considered how compensating the opportunity costs of vaccination affects take-up rates. Those studies that consider these costs focus on children and vulnerable groups in rural areas in developing countries (Banerjee et al., 2010; Sato & Takasaki, 2018a) or populations with limited income (Bronchetti et al., 2015). Concerning the effect of peers on the adoption of medical technologies, theory predicts free-riding on vaccination benefits due to herd immunity, but empirical research based on non-hierarchical peer networks such as friendship groups or neighbors finds mixed results (Bouckaert et al., 2020; Chen & Toxvaerd, 2014; Geoffard & Philipson, 1997; Kremer & Miguel, 2007; Rao et al., 2017; Sato & Takasaki, 2018b).

We contribute to this literature by employing a unique setup that allows for variation in different types of cost. Our results indicate that reducing opportunity costs substantially affect the vaccination of working-age adults who are not constrained by income and live in locations where access to vaccines is not an issue. The estimates are similar to those of studies focusing on vulnerable populations, implying that opportunity costs are an important factor for any population. In contrast, a small change in the vaccine's price did not increase the take-up rate, suggesting that financial incentives must be substantial to be effective. Additionally, while some studies find that information nudges can be effective in changing health behavior in general (for a recent review, see Siddique et al., 2020), our evidence on insignificant effects aligns with other studies on vaccine take-up (Bronchetti et al., 2015; Godinho et al., 2016). As another driver of vaccine take-up, we document how the adoption of medical technologies in an organizational context can be strongly affected by co-workers, which is a peer group that has received much attention in research about peer effects on performance (Bandiera et al., 2010; Cornelissen et al., 2017; De Grip & Sauermann, 2012; Falk & Ichino, 2006; Mas & Moretti, 2009) but far less attention in our research context.

Second, our study contributes to the literature on the consequences of medical technologies (Alam & Wolff, 2016; Bütikofer et al., 2020; Duflo et al., 2019; Jeon & Pohl, 2019) as well as the literature on on-site health interventions (Belot et al., 2016; Jones et al., 2021; Just & Price, 2013; List & Samek, 2015; Milkman et al., 2011) and the broader literature on public health interventions (Bütikofer & Salvanes, 2020; Cawley, 2010). Our findings on the health effects of vaccine take-up add to an ongoing discussion that predominantly takes place in the medical literature, with some recent exceptions in the economics literature (Ager et al., 2017; Carpenter & Lawler, 2019; Lawler, 2017). Concerning flu vaccines, quasi-experimental studies do not show a clear picture as the average health effects are often small or insignificant, and positive effects can be found only in years when the flu vaccine matched well with the prevalent flu viruses (Anderson et al., 2020; Carrera et al., 2021; Van Ourti & Bouckaert, 2020; Ward, 2014; White 2021). Very few medical studies to date have considered the possibility of medical technologies unintentionally causing moral hazard (Prasad & Jena, 2014; Richens et al., 2000), while papers in economics exploring moral hazard in the context of health interventions present mixed results (Doleac & Mukjerjee, 2018; Einav et al. 2013; Klick & Stratmann, 2007; Margolis et al., 2014; Moghtaderi & Dor, 2021).

The present study contributes to this literature by employing a novel design for public health and medical interventions, allowing to circumvent measurement and ethical problems. In contrast to previous studies in the economics literature on vaccines, our experimental approach does not rely on assumptions, such as randomness in vaccine match quality. At the same time, our natural field experiment addresses potential issues of RCTs in medical research, including the problem of scrutiny and potential behavioral implications. Thanks to our setting, we can also inspect the possibility of externalities by using plausibly random variation in co-worker vaccination across work units. This indicates that an adult-working age population may not be subject to health spillovers compared to high-risk groups included in other research (White, 2021). We hope to encourage other researchers to use our methodological approach to obtain causal estimates of economically relevant health outcomes, such as sickness absence (Barmby et al., 2002; Bütikofer & Skira, 2018; De Paola et al., 2014; Pichler, 2015; Van Den Berg et al., 2019; Ziebarth & Karlsson, 2010). Furthermore, by relying on experimental variation, we provide behavioral evidence that getting vaccinated induces individuals to feel protected and thus to forgo preventive practices, which informs theoretical discussions on the role of risk compensation for vaccinations (Auld 2003; Talamàs and Vohra, 2020). By showing that preventive medical technologies can trigger riskier behaviors, we offer an alternative explanation for why health interventions may not always be as successful as expected in improving health outcomes.

Last but not least, we assess the generalizability of the empirical results presented in our paper. Following the recommendation of List (2020), we discuss four relevant conditions in this respect: First, in terms of selection, our study population corresponds to a large bank in Ecuador. While this sample is more educated and has a higher income than Ecuador's general population, it speaks to the work context in developed and developing nations in the Western world. Therefore, the sample and the compliers to our experimental modification are particularly relevant from a policy-perspective when discussing how to increase participation in health campaigns. Ecuador, with its capital Quito located on the Equator Line, is a very interesting country for researching influenza vaccination. Since 2015, the flu season in Ecuador has matched the flu season in the United States and Europe (WHO FluNet, 2021). Moreover, the flu vaccines used in Ecuador match vaccines used in the United States and Europe, and working conditions in a bank or large company are similar across these countries. Second, in terms of attrition, we have perfect compliance in delivering the experimental treatments, and all the main outcomes are measured with administrative data. Third, considering the naturalness of the choice task, setting, and time frame, we use a natural field experiment. Thus, individuals are engaged in a natural and familiar task, unaware of the intervention, and not placed on artificial margins. Finally, in terms of scaling our insights, the magnitude of the effect of opportunity costs on vaccine take-up is comparable to other studies' results that vary costs in different settings (Banerjee et al., 2010). In terms of the effect of the vaccine on sickness absence, we do not expect a substantial change with scaling conditional on having a similar match quality of the vaccine and a comparable disease prevalence as during our investigation period.

2. Experimental Design

We conducted the field experiment in cooperation with a bank in Ecuador. The bank focuses on consumer credit and is one of the largest credit card issuers in the country. Its headquarters are in Quito (the capital of Ecuador), and it has six branches across the country with over 1,300 employees in total, distributed across 31 divisions with 142 work units. The bank had previously run small vaccination campaigns; these involved only some employees in crowded areas and were run in the bank's offices during the workweek.[2] In 2017, the bank decided to extend its annual campaign to all its employees, allowing us to experimentally modify the campaign to investigate the effects of vaccination and how to increase take-up rates. To trigger variation in vaccination take-up, we implemented three interventions: we changed the vaccine's price using income-dependent subsidies, randomized assignments for on-site vaccination across weekdays, and implemented information nudges by varying the email content to invite employees to get vaccinated. All interventions are orthogonal to each other, allowing us to evaluate them independently.

Given that health insurance does not cover flu vaccination, the bank decided to provide the vaccine for free to areas of the business that had participated in campaigns in previous years and

[2] These areas include the call center and the collections departments, which have small numbers of 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 rate of almost 100%.

to subsidize it for new participants partially. Since the company opposes randomized subsidies, we used information on employees' income to allocate this subsidy. An income threshold for the subsidy was chosen to maximize the sample size while passing density and covariate smoothness checks before the intervention. Employees who earned less than $750 per month would pay $4.95 to get vaccinated, while those who earned more than $750 would pay $7.49 (the total vaccine price is $9.99). Each employee was informed about the applicable price of the vaccine in their invitation email. This email included basic information about the campaign and informed employees that the payment for the vaccine would be deducted directly from their paychecks if they opted to get vaccinated. The email also contained the assigned day and time for their vaccination. Figure A1 shows an example of an invitation to receive the low-price flu shot on a Thursday morning.

To examine the effects of opportunity costs and information, we randomly assigned all employees into one of four groups.[3] First, employees assigned to the control group (*Control*) were invited to get vaccinated during the workweek (Wednesday, Thursday, or Friday). They were allowed to take time off their duties to get vaccinated. The specific day was selected at random for each employee.

The first treatment increased the opportunity costs of vaccination by assigning employees to get vaccinated on a *Saturday*. The bank's employees usually do not work during the weekend, so individuals in this group would incur extra transportation costs and have to arrange their schedules to travel to the bank and get vaccinated.[4] Otherwise, this group received the same information as the *Control* group (see Figure A2). This treatment only applied to Quito because 82% of the bank's employees work in Quito; all the other branches are substantially smaller, and their employees could all get vaccinated in a single day, which was not possible in the capital.[5] Also, keep in mind that being able to get vaccinated on a Saturday was, despite higher opportunity costs, still a unique opportunity since vaccination during the weekend is not commonly available in Ecuador.

We further implemented two information nudges. We kept the additional messages as unobtrusive as possible to prevent confounding the effect of information with salience or other

[3] The bank requested that we exclude the CEO and another high-level executive from the intervention. We also excluded our contact in the Human Resources department and four employees who work in the local branches and do not have a company email address.
[4] Based on data from the employees' magnetic swipe cards that they use to enter company buildings, only 0.4% of the bank's employees work regularly on Saturdays.
[5] Branches in the coastal areas were assigned for vaccinations to be carried out on a Wednesday, and branches in the highlands were assigned to Thursday.

behavioral factors. The first nudge highlighted the social benefits of flu immunization (*Altruistic*). In addition to the information provided to the *Control* group, the email included the following wording: "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 Figure A3). The second nudge highlighted the individual benefits of flu immunization (*Selfish*). In addition to the information provided to the *Control* group, the email included the following wording: "Vaccination can significantly reduce your risk of getting sick, according to health officials from the World Health Organization and numerous scientific studies" (see Figure A4). Employees in these two treatments were assigned to get vaccinated during the workweek, while 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; WHO FluNet, 2021). The bank ran a pre-intervention survey from October 25 to October 29, 2017. The human resources (HR) department sent the intervention emails on November 1, 2017, using its official email account. The employees were not aware that this study was taking place. For them, the campaign was just a regular activity organized by the HR department. Employees are used to receiving emails from HR; according to the HR manager, they typically read them carefully. A reminder was sent out 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-intervention survey during March and April 2018.[6]

3. Data

This section describes the data used in our analyses for assessing how monetary and non-monetary determinants can affect take-up rates and the effects of flu vaccination. First, we were granted access to the firm's administrative records about its employees, including gender, age, education level, children, tenure, income, medical diagnoses, and sick days. The records also provide information about the employee's job and work unit, i.e., their position within the bank's

[6] The geographic locations of the banks' branches are displayed in Figure A5, and a depiction of the timeline is shown in Figure A6. Figure A7 provides information about the flu vaccine used, and Figure A8 shows an individual getting vaccinated during the campaign.

organizational structure. The company has predetermined work units, established more than two decades ago. Second, we collected vaccination take-up data from the bank's campaign records. Third, we gathered data from the pre- and post-intervention surveys. These surveys asked employees about their previous illnesses and general health, knowledge and beliefs about vaccination and the flu vaccine, habits related to health, relationships with co-workers, opinions about the campaign, motivation, organizational attachment and work satisfaction, and risk and time preferences.

--- Table 1 about here ---

Table 1 presents the mean characteristics of the bank's employees (Column 1). On average, the employees earn a total monthly income of $1,766. As a reference, in 2017, the national average monthly income in Ecuador was $479, implying that the bank's employees are in the three highest deciles of the Ecuadorian income distribution (ENEMDU, 2017). The average length of employment with the bank is more than seven years, and the average age of the employees is around 36 years. The company employs roughly the same share of men and women. Fifty percent of the employees have children. The average distance that employees live from work is 7.58 km, and a work unit consists of approximately 29 employees. Almost 50 percent of the employees completed the pre-intervention survey, representing a high completion rate compared to previous surveys from HR. The completion rate decreased to 36% for the post-intervention survey.

The administrative data include medical diagnoses and sick days as two health measures. These measures come from two sources: on-site doctors and medical certificates from external doctors (72 different physicians in total). It is important to note that Ecuadorian law establishes that employees must present a medical certificate to receive a sick day.[7] Consequently, the on-site doctors report every patient's visit to HR, including the diagnosis (the type of disease), whether they granted sick days or not, and the number of sick days granted. Furthermore, if an employee takes time off work to go to an outside doctor, they must present a medical certificate to HR that indicates the diagnosis and the number of sick days granted (if any). Hence, in addition to sickness absence, we also observe milder health problems when employees were diagnosed with an illness,

[7] By law, employees in Ecuador are also entitled to up to one year of paid leave due to sickness. Employers are not allowed to terminate employment during sick leave.

but the doctor did not consider the condition severe enough to grant a sick day. Between January and early November 2017 (before the intervention), two out of three employees had some illness, and 37% had at least one sick day (see Table 1).

The doctors diagnose their patients using a combination of a physical examination, blood tests, and culture tests. The procedures undertaken are recorded in individual medical records, to which we do not have access. Diagnoses that name the patient's illness as "flu" provide us with a narrow definition of flu-related sickness. If flu cases occur with complications, the data report the complication as the diagnosis and thus does not mention the flu explicitly. To address this issue, we use an extended definition of flu-related sickness, which includes diagnoses that could likely indicate complications caused by the flu, according to a third-party physician (who also provided us with an even broader definition used for robustness checks). Any other respiratory disease not classified by this doctor as flu is listed as a non-flu respiratory disease. A second physician has verified these measures to ensure confidence regarding the distinction between flu-related and non-flu-related health problems.

Table 1 also shows evidence on the balance of treatment assignment. Columns 2–5 present the mean employee characteristics across the four groups; all variables have almost identical means across all 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 treatments, implying that the randomization was successful. Using the Kruskal–Wallis rank test yields the same result. Finally, we test whether participation rates in the pre- and post-intervention surveys are different across treatments; no statistically significant difference is detected.[8]

4. Analysis of the Determinants of Vaccination Take-Up

This section studies how monetary and non-monetary determinants affect working adults' vaccination decisions. Specifically, we consider the effect of opportunity costs, information nudges, and peers on take-up rates. We do not find any effect of the $2.48 price difference from

[8] A further inspection of the available data shows that survey participants have similar characteristics compared to non-participants and hence could be regarded as representative of the initial sample. We also note that there is minor attrition of employees who left their jobs at the bank between November 2017 and February 2018. According to an additional check, attrition of employees is not affected by treatment assignment.

the income-dependent vaccine subsidy on vaccination take-up rates.[9] We thus conclude that this price change may be too small to induce changes in take-up behavior.

The last row in Table 1 presents the flu vaccine take-up rates for the different treatments during the campaign. The *Control* group shows a rate of 22%, the *Altruistic* treatment shows a rate of 17%, and the *Selfish* treatment shows a rate of 19%. Comparing the three groups suggests that the information treatments were insufficient to increase take-up. In contrast, being assigned to get vaccinated during the workweek increases take-up by 14 percentage points compared to the *Saturday* treatment (112%).[10] We extend the analysis of these effects in the next section.

4.1 Effects of Opportunity Costs and Information on Individual Take-Up

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

$$Takeup_{ic} = \alpha + \gamma_c + \pi_1 Saturday_{ic} + \pi_2 Altruism_{ic} + \pi_3 Selfish_{ic} + u_{ic}, \qquad (1)$$

where $Takeup_{ic}$ is an indicator of getting vaccinated. We control for whether the branch was in Quito or not (γ_c) to account for differences in the implementation of the vaccination day assignment across branches (as discussed in Section 2). $Saturday_{ic}$, $Altruism_{ic}$, and $Selfish_{ic}$ are dummy variables that indicate treatment assignment. We estimate the effect of the different treatments relative to those individuals in the *Control* group who were assigned to vaccination on a day during

[9] Figure A9 shows no visible discontinuity across the threshold. Regression discontinuity estimates also indicate no significant change in take-up at the cutoff, which is robust to a variety of checks (see Table A1). If anything, the coefficient suggests that higher prices result in insignificantly higher demand.

[10] The post-intervention survey included a question on vaccination during the flu season with three answer categories: vaccinated at the campaign, vaccinated outside the campaign and no vaccination. According to the responses, 59 employees stated that they got vaccinated outside the campaign. While actually none of those individuals received a vaccination during the bank's campaign, according to the medical records, this could be due to the question wording, which was not limited to vaccination against the flu. Hence, employees could be reporting other type of vaccinations, so that it is not clear whether or not those employees received a flu shot outside the company. Furthermore, there is some evidence for misreporting on vaccination. 18 individuals stated that they participated in the vaccination campaign, but they did not according to the records. Meanwhile, one individual who actually was vaccinated stated no vaccination. As it may be well-known among employees, the company cannot verify claims on vaccination outside the campaign. While this ultimately is true for us as well, we conducted some checks to gauge possible implications for our empirical investigation. First, we run a robustness check by excluding those 19 individuals misremembering whether they were vaccinated from the analysis. The results do not change when we do so. Second, the same holds when we exclude individuals claiming vaccination outside the campaign. Finally, we test whether these self-reported vaccinations differ significantly according to treatment status. This is not the case.

the workweek and did not receive any information nudge.

Table 2 presents the effects of the different treatments on take-up rates. Column 1 shows the baseline results of the effects 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* group. This effect is approximately 46% of the take-up in Quito for the *Control* and is statistically significant at the 1% level. Hence, minimizing opportunity costs associated with vaccination helps increase take-up.

Conversely, we find that emphasizing either the altruistic or the selfish benefits of vaccination did not affect take-up. The coefficients in both cases are close to zero, negative, and statistically insignificant. It is plausible that supplying a sentence of additional information is not sufficient to further increase take-up, given the substantial effect of reducing opportunity costs.[11] One interpretation of these results is that information would have to be highly salient to accrue an effect on vaccine take-up rates in a company such as this.

---- Table 2 about here ---

Columns 2–4 of Table 2 show the results from several robustness checks. Specifically, Column 2 shows that controlling for vaccine price, income, tenure, division in the company, gender, age, and education level does not affect the estimates.[12] Column 3 shows the result when we exclude cases of non-compliance.[13] In this subsample, assigning employees to *Saturday* decreased take-up by 6.7 percentage points, significant at the 5% level. We cannot reject that this estimate is statistically the same as the baseline result. The estimates of the effects of the information treatments are practically the same as the main estimates. Column 4 addresses the fact that only employees who

[11] The post-intervention survey asked whether the employees recalled the altruistic and selfish information statements. Table A2 shows that neither employees assigned to the *Altruistic* treatment nor those assigned to the *Selfish* treatment remembered their respective statements better than the *Control* group. Information spillovers could be another issue here, but this is unlikely given that our design involved providing information directly to the treated individuals via email. We also do not think that he email is too long to read since the email contains a prominently placed image and not a lot of text (see Figure A3 and Figure A4 for the altruistic and selfish treatment).

[12] A simple Bonferroni-adjustment results in a p-value of 0.03 for the Saturday assignment. While we may not be sufficiently powered to detect small coverage changes due to the nudges, we have sufficient power to detect differences across the workweek and the weekend.

[13] We identified in the campaign records 12 employees assigned to a day during the workweek who were actually vaccinated on the *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 the Saturday and were unable to send employees home without being vaccinated if they showed up on that day. In contrast, no employees who were assigned to *Saturday* got vaccinated during the workweek.

work in the bank's headquarters in Quito were assigned to be vaccinated on a *Saturday*.[14] In this subsample, assigning employees to *Saturday* decreased take-up by almost nine percentage points (51% of the control group take-up), significant at the 1% level. This result is slightly larger than the baseline result, but we cannot reject that they are statistically the same. Both information treatments again display small, negative, and statistically insignificant effects.

Lastly, in Column 5, we inspect whether assignment to different days in the week affected take-up differentially. We use the fact that vaccination days were randomly assigned to employees in the Quito subsample and regress our indicator of vaccine take-up on dummies for each assigned day (*Wednesday*, *Thursday*, *Friday*, and *Saturday*).[15] These estimates show that the take-up rates for *Thursday* and *Friday* are not statistically different from that for *Wednesday*, while the effect of *Saturday* is substantially larger in magnitude and very close to the baseline estimate in Column 1.[16] These results do not support time-inconsistent preferences that would induce procrastination as the mechanism behind the *Saturday* effect, and they are consistent with increasing opportunity costs.

4.2 Further Evidence on Opportunity Costs

We analyze heterogeneous treatment effects across subgroups of our study population by focusing on differences across gender, distance to work, and employees with and without children. This may yield further evidence that opportunity costs are driving the difference in take-up rates between employees assigned to be vaccinated on a day during the workweek and those for *Saturday*. Note that the information treatments have no differential effect across subgroups. The estimates are small and statistically insignificant (see Table A3).

--- Figure 1 about here ---

Figure 1 first shows that assignment to *Saturday* reduces take-up by 8.8 percentage points for men and 6.7 percentage points for women. The difference is not statistically significant. Second, those employees who live further away than the median are slightly less likely to have been vaccinated

[14] The estimates are almost identical for the Quito subsample if we include control variables. For employees in Quito, we can also control for the distance between the bank and their homes to capture transportation costs.

[15] Of the bank's employees in Quito, after excluding the call center, 23.4% were assigned to vaccinate on *Wednesday*, 26.7% to *Thursday*, 26.5% to *Friday*, and 23.4% to *Saturday*.

[16] While the effect of assignment to *Friday* is not significant, it is 44% 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 six-hour workday rather than an eight-hour workday like the other weekdays.

(-9.7 pp.) when assigned to *Saturday* than those who live closer to the bank (-8.3 pp.), but this difference is not statistically significant.[17] This result implies that additional travel costs, as captured by the distance to work, are not the main factor driving the difference in take-up rates between employees assigned to the workweek and *Saturday*. Third, we consider differences between the effects for employees with and without children, as having children may imply higher opportunity costs at the weekend due to increased family obligations. We find that assignment to *Saturday* decreased take-up by 10.6 percentage points for employees with children, while the effect is much smaller (5.3 percentage points) and insignificant for employees without children. Although the difference between these two effects is not statistically significant, this picture is consistent with the idea that opportunity costs increase for individuals assigned to *Saturday.*

Finally, to further disentangle transportation costs from opportunity costs, we run two regressions, including interaction terms between *Saturday* and distance to the bank and between *Saturday* and vaccination price groups, respectively (Table A4). The results show that an additional mile in travel distance (i.e., transportation cost) does not have a differential effect on the likelihood of getting vaccinated on a weekday compared to *Saturday*. Additionally, there are no statistically significant differences between the high-price, workweek vaccination group and the low-price, *Saturday* vaccination group, which is inconsistent with the idea that heterogeneous transportation costs drive the main results in Table 2.

In conclusion, several pieces of evidence support that the difference in take-up rates between employees assigned a day during the workweek and *Saturday* corresponds with a change in the opportunity costs of vaccination. In the rest of our analyses, we use only this variation in take-up resulting from lower opportunity costs as an instrument.

4.3 Peer Effects on Vaccination Take-Up

Peer effects may play an important role in vaccination behavior by increasing or decreasing take-up rates. On the one hand, if more individuals get vaccinated, the prevalence of the disease may decrease, making it less likely for others to get sick. Thus, if there are costs involved in getting vaccinated, it may be optimal for some people not to do so if their peers decide to vaccinate. Theoretically, this free-rider problem can result in a Nash equilibrium, where nobody takes the

[17] The median distance that employees live away from work is 6.5 km. Note that distance to work was calculated based on employees' home addresses using a geo-location service.

vaccine (Chen & Toxvaerd, 2014). On the other hand, peer vaccination may increase the probability of individual take-up. Such positive peer effects in vaccination could occur, for example, because individuals care about how others perceive them (Karing 2018). As a result, co-workers may imitate the health care behavior of their peers to conform to perceived social norms.

Since all treatments are orthogonal by design, we focus on the exogenous variation in take-up created by assigning individuals to get vaccinated during the workweek to estimate peer effects in vaccination. The bank's work units define the social groups of employees that work together and with whom they are in close contact. We model the effect of the proportion of peers excluding individual i in work unit j who take the vaccine on employee i's decision as:

$$Takeup_{i,jc} = \gamma_c + \beta_1 Prop.Takeup_{-i,jc} + \beta_2 X_{ic} + \beta_3 \bar{X}_{-i,jc} + \pi_3 Workweek_{i,c} + u_{i,jc} \ , \qquad (2)$$

where $Prop.Takeup_{-i,jc}$ corresponds to the proportion of peers assigned to get vaccinated on the workweek for i in unit j, $Workweek_{ic}$ is the assignment to vaccinate on the workweek for individual i, and $\bar{X}_{-i,jc}$ are the average observable characteristics of peers j. Manski (1993) shows that if we estimate equation (2) by ordinary least squares (OLS), then self-selection, common environmental factors, and reflection will confound the true peer effects β_1 and β_3. However, in our design, employees are randomly assigned to vaccinate on the workweek independent of their unit. This creates exogenous variation across units that affects the proportion of peers who get vaccinated independently of employee i's decision to vaccinate because, by chance, some units have more employees assigned to the workweek than other units. We can average equation (2) across unit j, leaving out individual i, to obtain the following first stage equation:

$$Prop.Takeup_{-i,jc} = \frac{\gamma_c}{1-\beta_1} + \frac{\beta_2+\beta_3}{1-\beta_1}\bar{X}_{-i,jc} + \frac{\pi_3}{1-\beta_1} Prop.Workweek_{-i,jc} + \frac{\bar{u}_{i,jc}}{1-\beta_1} \ , \qquad (3)$$

where the proportion of peers in unit j who get vaccinated is a function of the proportion of peers *randomly assigned* to be vaccinated during the workweek ($Prop.Workweek_{-i,jc}$). Random assignment of both individuals and peers within work units implies that $Prop.Workweek_{-i,jc}$ is uncorrelated with both $\bar{X}_{-i,jc}$ and $\bar{u}_{i,jc}$. Hence, the reduced form equation is as follows:

$$Takeup_{i,jc} = \left(\frac{\gamma_c}{1-\beta_1}\right) + \left(\frac{\beta_1\beta_2+\beta_3}{1-\beta_1}\right)\bar{X}_{-i,jc} + \beta_2 X_{i,c} + \frac{\beta_1\pi_3}{1-\beta_1} Prop.Workweek_{-i,jc} + \pi_3 Workweek_{i,c} + \tilde{u}_{i,jc} \qquad (4)$$

In our design, the exclusion restriction holds because the proportion of peers vaccinated during the workweek 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) and (4) to obtain an instrumental variable (IV) estimate of the effect of the proportion of vaccinated peers on an individual employee's take-up. Variation across units from the proportion of peers assigned to the workweek and variation within the unit from individual assignment to the workweek allow us to identify both the individual treatment effect and the peer effect, as noted in equation (4). The error term in equation (4) includes both the individual error from equation (2) and the average error from equation (3), so we cluster the standard errors at the unit level.

--- Table 3 about here ---

Table 3 presents the main results for peer effects on vaccination. The first stage estimate in Column 1 indicates that a ten-percentage-point increase in the proportion of peers assigned to the workweek increased the proportion of peers vaccinated by 3.1 percentage points. The effective F-statistic of Montiel Olea and Pflueger (2013) is 16.48; therefore, we can reject the null of weak instruments for a threshold of 20%, which suggests that the instrument is relevant. The estimates in Columns 2–4 show that peer vaccination positively affects individual take-up and that not accounting for endogeneity biases the effect downwards. The IV estimate in Column 4 indicates that a ten-percentage-point increase in the proportion of peers getting vaccinated increases take-up by 7.9 percentage points.[18] In further analyses, we attempt to shed more light on potential mechanisms

[18] Results in Table 3 are robust to controlling for individual workweek assignment. Note that the effect of own assignment to the workweek is within the confidence intervals of the estimates in Table 2, suggesting that spillovers from peers do not affect identifying the effect of individual treatment on take-up. Furthermore, as seen in Table A5 (Panel A), the results are robust to controlling for the total number of employees in the unit, considering that smaller units may have larger proportions. Results are also robust when we control for the mean age and gender of the peers. For another check, we change the definition of the instrument. By considering the timeline of events and defining our instrumental variable as a cumulative proportion of cases separately for each individual, we avoid considering future vaccinations of coworkers. As seen in Table A5 (Panel B), peer effects remain significantly positive when using only variation in peers assigned to vaccinate on the same day or before. Note that we do not have information to investigate order effects within each day. Finally, since some employees in a unit are in different regions, we adjusted our overall peer instrument to a within the region and between region peer instrument. We found no significant difference between the two. This is inconsistent with the idea of free-riding if we assume that the potential health benefits of coworker vaccination can only be relevant for coworkers at the same location.

behind the peer effects on individual take-up.[19] In our interpretation of the available evidence, the positive peer effects most likely result from individuals feeling pressured to follow behavior that they deem socially acceptable.

5. Analysis of the Effects of Vaccination on Sickness, Absence, and Behavior

In this section, we exploit random assignment to a vaccination appointment in the workweek as an instrument to study whether flu vaccination improves health and reduces sickness absence. To shed light on one of the potential mechanisms underlying the impact of flu vaccines, we use the same approach also to explore whether vaccination affects health-related behaviors.

5.1 Effects of Flu Vaccination on Health and Sickness Absence

Flu vaccines may affect health through multiple avenues, both direct and indirect. First and foremost, getting vaccinated could have a direct medical effect on health. Furthermore, as indicated by the results in the previous section, if a person gets vaccinated, the likelihood that their peers will also get vaccinated increases, which may decrease the transmission rate of the disease and positively affect health. As the proportion of vaccinated peers within the 142 units in the firm varies substantially (between 0% and 100%), this could indirectly affect health outcomes.[20] Ideally, we could estimate the effect of vaccine take-up on health-related outcomes (Y_{ijc}) such as sick days through these two channels as follows:

$$Y_{ijc} = \alpha + \gamma_c + \theta Takeup_{ijc} + \delta Prop.Takeup_{jc} + \nu_{ijc} \tag{5}$$

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[19] In a first analysis, we use data from a post-intervention survey questionnaire on beliefs and knowledge of vaccines. Reduced-form analyses reveal no significant effects on responses to any of the questions, as shown in Table A6. This suggests that peer behavior neither affected beliefs nor supplied new information about the vaccine. The survey evidence also speaks against the idea that employees were happy or upset that some coworkers had the chance to vaccinate during work hours while others did not. In particular, a reduced talking activity could imply that employees are upset when assigned to the weekend. However, we do not find a significant reduction in the propensity to talk with coworkers when the proportion of coworkers with workweek assignment increases. In further analyses, we estimate an expanded model with an interaction between individual workweek assignment and peer workweek assignment. This reveals no significant result that may inform us about mechanisms. In contrast, by including a unit-size interaction in our main model, a separate analysis reveals that units with small unit sizes drive the effects. This aligns with the idea that pressure to conform to peer behavior is stronger in smaller groups. Finally, we estimate whether different subgroups of peers affect individual vaccination decisions differently based on the idea that certain groups are more likely to create feelings of belonging than others. For instance, individuals may have a particular incentive to follow the behavior of peers of the same gender, which could be seen as a relevant peer group. In line with this, we observe that the behaviors of the same gender group seem more relevant to individuals' actions than peers of a different gender.

[20] Figure A10 displays the number of employees by unit. Note that the CDC and the WHO indicate that vaccination rates over 75% grant herd immunity.

However, vaccination take-up and the proportion of vaccinated peers 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 (5) by OLS would be biased downward. This speaks for instrumenting the following variables: i) take-up with an indicator of assignment to vaccination during the workweek, and ii) the proportion of vaccinated peers in the unit with the proportion of peers assigned to the workweek. We thus identify effects on health by exploiting variation across units from the proportion of peers assigned to the workweek and variation within the unit from individual assignment to the workweek. The unadjusted first stage equations have F-statistics of 6.6 and 8.9, respectively, implying that the IV estimates of equation (5) may have a problem of finite sample bias.[21] Thus, we focus on the valid reduced form estimates of regressing the health outcomes on the instruments. Also, the reduced form estimates capture the effect of the intervention, which is the relevant estimate for organizations considering health campaigns and the potential benefits of increasing participation.

--- Table 4 about here ---

Table 4 Panel A presents the effects of flu vaccination on the probability of being diagnosed sick for any reason between November 2017 and February 2018. The OLS results in Column 1 suggest no significant link between getting vaccinated and the probability of being diagnosed with an illness. The reduced form results in Column 2 confirm that getting vaccinated does not affect the probability of being diagnosed as sick. Being randomly assigned to a day in the workweek (which increases vaccination take-up) decreases the probability of sickness by 1.7 percentage points (3.5% of the baseline), which is insignificant at conventional levels. Additionally, the estimates in Columns 1 and 2 indicate that the proportion of vaccinated peers does not affect the probability of being diagnosed with an illness. This implies that underestimating individual health benefits due to vaccination is not an issue in the absence of any significant externalities from peer to peer, be it via exogenously encouraged take-up or health spillovers.[22]

[21] The results of Montiel Olea and Pflueger (2013) only apply in cases with one endogenous variable.

[22] In additional analyses, we inspect deeper whether externalities could be relevant for health. In particular, we run interaction analyses by exploiting the randomness in individual treatment assignments and treatment assignments of peers in the work unit. The interaction between these variables turns out to be insignificant, which means that higher chances of peer effects do not change our estimate of the individual treatment effect. In general, we do not observe any evidence that work units with large shares of workweek assignments have better health. However, they should in case there were significant health benefits of take-up, be it in the form of individual health benefits, externalities, or

Table 4 Panel B shows the effects of flu vaccination on the probability of having a sick day. The OLS correlation suggests that vaccination decreases the probability of having a sick day by 4.1 percentage points, but this effect is insignificant. The reduced form estimates in Column 2 imply that getting vaccinated does not affect the probability of having a sick day. Being randomly assigned to a day in the workweek (which increases vaccination take-up) even increases the probability of having a sick day slightly by 1.3 percentage points (5% of the baseline), which is insignificant at conventional levels. Furthermore, the evidence shows that the proportion of vaccinated peers does not affect the probability of sickness absence. From the firm's perspective, these results suggest that the investment in the health campaign was not worthwhile.[23]

To provide a more precise answer to the question of whether the vaccination campaign could have been economically beneficial for the company carrying out the intervention, we perform a back-of-the-envelope calculation and determine the net benefit of the campaign using our evidence on sickness absence.[24] As can be seen in Table A11, we first calculate statistics for the Saturday (control) group, such as the average daily wage and sick day probability. We find that the average wage paid for a sick day incidence by the firm is $16.45 in the Saturday group. We then use the main treatment effect (of 1.3 pp.) for the assignment to the workweek from the previous paragraph, to calculate a worst-case, average-case, and best-case scenario where the worst- and best-case are the upper and lower confidence intervals of our workweek treatment effect. When comparing the mean sick day incidence of those three scenarios relative to Saturday, we find a wage loss due to sickness incidence from being offered vaccination during the workweek that ranges from $5.00 to -$3.51. In each case, we also have to account for the costs due to the vaccine subsidies that the

both. Note that our setting would enable us to learn more about different forms of externalities, if those had occurred, by adjusting the definition of our peer vaccination instrument. However, our findings on individual and peer vaccination remain the same when we define the proportion of peers differently and use the location-adjusted version of the instrument by focusing only on co-workers of the same unit at the same location.

[23] Our findings on the effects of vaccination on health and absence are robust to various checks and deeper analyses. In particular, the results remain the same if we take out the proportion of peers and estimate only the individual effect of vaccination. Furthermore, we come to the same conclusions based on analyzing the number of sick days as the dependent variable. Note that some of the diagnoses include severe illnesses such as cancer, meaning that many recorded sick days are not related to the flu. Excluding two outliers with more than 100 sick days, the coefficient of the reduced form is insignificantly positive, in line with our finding in Table 4 Panel B on the probability of having a sick day. Finally, all the results in this section are robust when only the Quito subsample is used.

[24] In our analysis, we do not consider potential benefits due to other mechanisms, such as morale and productivity. Given that we do not find effects on illness and sick days that are first-order outcomes, it is likely that the campaign did not affect productivity or morale. Indeed, Table A10 presents estimates on self-reported productivity and the duration of the working day as measured by the employees' magnetic card swipes for entering and exiting the bank. Neither of those outcomes are significantly altered by the workweek relative to the weekend assignment.

firm has provided. As a result, we find that there may have been a small benefit for the firm of roughly $3 in the best-case scenario for each additional employee incentivized to get the flu shot.[25] However, this analysis does not take into account the vaccine subsidies paid to employees who got the vaccine in any case, including those without the incentive of a workweek invitation. Moreover, there are opportunity costs for the company, for example due to the loss of working time for other projects that the HR department could have devoted their time to. We therefore conclude that even in the best-case scenario for this flu season the campaign has resulted in negligible benefits if any.

To complete our analysis of health-related outcomes, we also exploit data on medical diagnoses to estimate the effect of vaccination on the probability of being diagnosed with the flu. This discussion can be found in Appendix A1. According to the results, getting vaccinated was ineffective in decreasing the probability of having the flu, which also is true for the proportion of vaccinated peers (see Table A7). To evaluate whether the estimates rule out meaningful effects of vaccination, we then use the flu-related sickness information to implement an equivalence test based on two one-sided hypothesis tests (Hartman & Hidalgo, 2018; King et al., 2000; Lakens, 2017; Rainey, 2014). The results imply that we can safely rule out meaningful health benefits of the flu vaccination based on public health figures provided to policymakers (see Figure A12).

In summary, our results suggest that vaccinating employees against the flu is ineffective in improving health and therefore not economically beneficial. A simple explanation could be that the flu vaccine was medically ineffective. As discussed in the previous literature and Appendix A1, even health institutions in support of vaccination acknowledge that the quality of the flu vaccine can vary substantially across different years. At first glance, this interpretation of a medically ineffective vaccine aligns with our evidence showing no health improvements for employees, including flu-specific illness. However, two alternative explanations are compatible with a medically effective vaccine. First, the problem of flu-related sickness may be too rare in a healthy working-age population to detect any significant health effects of increased vaccination rates. Second, vaccination could also indirectly affect health outcomes, besides a possible medical effect, if employees change their behavior. Vaccinated individuals could overestimate the

[25] As an extension of our cost-benefit analysis, we can also consider the number of sick days. When we scale our results from Table A11 by the mean number of sick days, we determine a net benefit of roughly $15 in the best-case scenario and net costs of more than $30 in the worst-case scenario. Given the above-mentioned problem of outliers in the number of sick days, we could also make use of the median as an alternative to the average number of sick days. This leads to the same results as in Table A11, given that the median in the control group is exactly one sick day.

vaccine's protection and engage in riskier behaviors; for example, they may avoid going to the doctor or wait longer than unvaccinated individuals to do so when they feel flu-like symptoms. In addition, vaccinated individuals may take fewer protective measures; for example, they may wash their hands less frequently. These changes in behavior could affect health in general and increase the chances of getting the flu. While any of such behavioral responses could be very relevant for policy-makers considering health campaigns, we also conduct the following analysis on employee behavior to learn more about a possible explanation for the lacking effectiveness of the flu vaccine.

5.2 Behavioral Effects of Getting Vaccinated

To explore the behavioral effects of flu vaccination, we first inspect whether vaccinated individuals react differently than unvaccinated individuals when flu-like symptoms appear. An important factor here is that non-flu respiratory diseases have symptoms like 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 they contract or show symptoms of a respiratory disease. For example, suppose vaccinated employees felt more protected. In that case, they might have been less likely to go to the doctor when they felt flu-like symptoms, thus decreasing the probability of being diagnosed with a non-flu disease. In particular, this would concern mild illnesses where it is up to the individual to decide whether to consult a doctor.

To implement this test, we utilize the richness of the data on medical diagnoses to identify cases of non-flu respiratory illnesses and exploit a policy intervention of the Ecuadorian government that occurred in our investigation period. In January 2018, Ecuador experienced a significant increment in flu cases nationwide (Direccion Nacional de Vigilancia Epidemiologica, 2018). As a result, the Ecuadorian government launched a massive media campaign asking people 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 respiratory diagnoses among vaccinated employees in that month.

--- Figure 2 about here ---

We estimate the reduced form effects of vaccination by month during our investigation period.

Figure 2 presents the effects of being assigned to a vaccination appointment during the workweek on flu and non-flu respiratory diagnoses. In line with cross-section estimates (see Table A7), being assigned a flu shot during the workweek does not affect the probability of being diagnosed with the flu in any month. For non-flu respiratory diagnoses, we do not expect to find any effects in the absence of changes in behavior. However, this only applies to November, December, and February. In January, when the government encouraged individuals to go to the doctor, being assigned a vaccination appointment during the workweek decreased the probability of being diagnosed with a non-flu respiratory disease by 7.2 percentage points.[26] This result suggests that employees assigned an appointment during the workweek, who were more likely to get vaccinated, felt more protected and thus were less likely to visit the doctor when they felt flu-like symptoms. These estimates are consistent with the hypothesis of riskier behavior among vaccinated individuals, as these employees appear to have thought they were protected against the flu.

We can also investigate whether vaccination affects the likelihood of visiting the doctor at the on-site health center. The bank's health center is convenient for its employees because they do not have to ask for time off to go to the doctor; they can take a few minutes off work to visit the health center. Before the intervention, the on-site doctors accounted for 77 percent of all cases of diagnosed sickness. If vaccinated employees felt more protected, they may have been less likely to visit these doctors when the government launched its media campaign.

--- Figure 3 about here ---

Figure 3 presents the effects of assigning employees a flu shot during the workweek on the probability of visiting the on-site doctor, broken down by month. We find no significant effect in November, December, or February. In January, a workweek assignment for vaccination decreased the probability of going to the on-site doctor by 8.6 percentage points (21% of the baseline). This

[26] Comparing the significance levels for each month in Figure 2 for flu vs. non-flu, we find that we cannot reject the null that the effects are the same for all months except for January, where we find a significant difference between flu and non-flu effects of 6.8 percentage points (p-value = 0.016). In one of our additional robustness checks, we confirm a significant January effect when controlling for individual fixed effects using a month-based difference-in-difference approach (Figure A11). We also estimate the effect of assignment to an appointment during the workweek on non-flu diagnoses, collapsing the data of the four months to a cross-section. In this specification, being assigned to the workweek decreases the probability of being diagnosed with a non-flu respiratory disease by 7.7 percentage points, almost identical to the effect in January. Finally, the results are similar when we use the more severe measure of sick days, which reveals a less precisely estimated effect for January, in line with the idea that the unvaccinated are more likely to go to the doctor in the presence of predominantly mild flu symptoms.

finding indicates that vaccinated individuals feel protected and are willing to take risks concerning their health, unlike unvaccinated individuals who prefer having a check-up at the doctor. While it is debatable if employees with mild flu-like symptoms should ignore the government's advice by not going to the doctor, the critical point for our discussion is that such a behavior can be seen as risky and hence consistent with moral hazard.

--- Table 5 about here ---

To learn more about health-related behaviors, we analyzed data from the post-intervention survey where the employees were asked to report how often they: (i) exercise, (ii) take nutritional supplements, (iii) use an umbrella when it rains, and (iv) wash their hands. Table 5 shows the effects of assigning employees to vaccination during the workweek on these outcomes. This does not seem to affect how often employees wash their hands (1.2% of the baseline), which could be because almost all employees reported that they wash their hands regularly. Assigning employees weekday appointments shows a negative but statistically insignificant effect on how often the employees exercise (4.9% of the baseline) and how often they take nutritional supplements (19.5% of the baseline). The effect on how often employees carry an umbrella is statistically significant, decreasing the frequency of carrying an umbrella by 1.22 points (17.6% of the baseline) on a Likert scale where one means "never" and ten means "all the time."[27]

While this indicates that vaccinated individuals were more willing to be engaged in riskier behaviors concerning their health, it is interesting to note that many people, including Ecuadorians, believe that carrying an umbrella could help to prevent the flu or other respiratory illnesses. Psychology research shows that cultures across the world associate the fact that the flu virus survives longer in a cold and wet environment with the belief that individuals catch the flu by getting wet or cold (Au et al., 2008; Baer et al., 1999; Helman, 1978; Sigelman et al., 1993).[28] To link this result more closely to the issue of vaccines, we investigate heterogeneous effects across individuals' beliefs on the effectiveness of vaccination using the pre-intervention survey. We find that the umbrella effect is driven by employees who strongly believe vaccines are effective (Table

[27] This effect is significant at the 5% level (p-value = 0.012) and robust to adjusting for multiple comparisons following Anderson (2008). As can be seen in Table 5, we also obtain a significant result if we use an index by summing up all four scores and dividing the sum by four.

[28] Also, since Quito is on the Equator Line, there are no marked seasons in the year. Temperatures can fluctuate between the upper forties (°F) and the lower eighties (°F) in one day. There are no accurate forecasts for rainfall.

A9). This conforms to the idea that vaccinated individuals feel protected and therefore neglect measures that they believe help prevent respiratory illnesses.

In summary, our evidence on riskier behaviors regarding health indicates a potential concern for policymakers regarding the overall effects of medical interventions, given that any health risk due to behavioral changes could threaten an intervention's success. Note that our evidence does not clearly support the idea of behavioral changes that could increase the chances of getting the flu. While using an umbrella might help to avoid a cold by not getting soaking wet on a rainy day, it does not yield protection against infectious disease, independent of what the people in our setting may believe. Visiting the doctor might also not help in this respect, even though our findings could also be interpreted as potentially indicative of other riskier behaviors which may indeed be relevant for the probability of getting the flu.[29]

6. Conclusions

Individual behavior may threaten the success of health interventions in multiple ways. First and foremost, individuals can decide not to participate in an intervention. In this paper, we find that a small price change and information nudges that appeal to either the selfish or altruistic benefits of vaccination did not induce a change in behavior. In contrast, it appears that reducing opportunity costs could have a substantial effect on participation in a vaccination campaign for working-age employees. Additionally, peers positively influence vaccination rates in the workplace.

Regarding the health benefits of the intervention, the flu vaccination did not significantly affect any of our outcomes. While we cannot rule out that the flu vaccine was medically ineffective or that the flu was no major health problem in the company workforce, we have provided evidence that employees adopted riskier health behaviors after vaccination. Such behavioral effects arguably

[29] There are a few alternative interpretations unrelated to moral hazard. First, if doctors misdiagnose the flu, some diagnosed non-flu cases could have been flu cases. However, as our data include diagnoses from 72 different doctors from different health centers and hospitals, it is unlikely that there is a systematic misdiagnosis issue. Additionally, the results are robust to using a broader definition of flu-related illness. Second, if a doctor learns that a person showing flu-like symptoms is vaccinated, they might be more likely to misdiagnose those symptoms as a non-flu respiratory disease. However, the results in Figure 2 show that employees assigned to appointments during the workweek, who were more likely to get vaccinated, had a lower rate of diagnosis of non-flu respiratory diseases than those assigned to the weekend. Third, the data on medical diagnoses correspond to employees who visited the on-site doctor or an external doctor during work hours. Sick individuals who saw an external doctor outside work hours and were not granted a sick day are coded as healthy. However, this measurement error can bias the flu and non-flu estimates only if it is correlated with the random assignment to vaccination during the workweek, which is unlikely. Similarly, adverse selection or related phenomena cannot be an issue in our study using random variation in take-up.

constitute a critical pathway by which individual behavior can limit the effectiveness of health interventions in general. For the company, we found that the vaccination campaign was not economically beneficial.

Our study presents multiple practical implications for firm health campaigns. From a research perspective, employing a randomized encouragement design is helpful to circumvent any ethical dilemma when studying the consequences of interventions relevant to people's health. It allows for studying both potential health benefits and behavioral changes in an unbiased way. A potential presence of moral hazard in health-related behavior implies that firms and policymakers should consider this phenomenon in designing interventions such as vaccination campaigns. A promising mechanism to mitigate this issue could be to increase awareness of the importance of other protective measures so that people will not rely only on the protection potentially provided by medical technology. This policy conclusion also informs the discussion on similar behavioral phenomena in the context of the COVID pandemic (Andersson et al. 2021).

Another lesson learned from our investigation is how to encourage increased participation in health interventions. In this paper, we have identified two cost-effective measures that can increase vaccination take-up rates in a workplace context where monetary factors do not seem to play a significant role in individuals' willingness to participate in a health campaign. Decreasing opportunity costs is one option that may drastically increase participation, supporting the use of mobile campaigns available on busy days and in locations where people usually congregate. In addition, since we have found that peer behavior has an important effect on vaccination take-up rate, employers can increase participation in health campaigns by implementing mechanisms to incentivize groups of employees. Small rewards for an entire unit when its members participate in the intervention could significantly affect participation rates. Evaluating the role of such peer incentives in health-related contexts is a promising area for future research.

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Table 1 Summary Statistics

	Full Sample	Control	Altruistic	Selfish	Saturday	F-test (p-value)
	(1)	(2)	(3)	(4)	(5)	(6)
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
Prop. Having Children	0.52	0.52	0.53	0.55	0.48	0.640
Distance to Work (km)	7.58	7.32	7.70	7.78	7.51	0.797
Work Unit Size (#)	29.3	27.9	31.2	29.7	28.4	0.567
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	0.67	0.67	0.64	0.67	0.835
Granted a Sick Day	0.36	0.37	0.34	0.36	0.35	0.797
Diagnosed Flu Sick	0.11	0.09	0.13	0.13	0.10	0.348
Diagnosed Non-Flu Sick	0.36	0.37	0.34	0.36	0.35	0.819
Vaccination Take-up	0.17	0.22	0.17	0.19	0.08	0.070
N	1,164	344	294	310	216	

Notes: This table 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.

Table 2 Effects of Treatments on Vaccination Take-Up

	Baseline	With Controls	Quito Sample	Non-Compliance	Day of Week Effects
	(1)	(2)	(3)	(4)	(5)
Altruistic Information	-0.0260	-0.0209	-0.0262	-0.0493	
	(0.0310)	(0.0303)	(0.0306)	(0.0332)	
Selfish Information	-0.0032	-0.0011	-0.0103	-0.013	
	(0.0314)	(0.0316)	(0.0308)	(0.0339)	
Thursday					0.0002
					(0.0346)
Friday					-0.0356
					(0.0331)
Saturday	-0.0789***	-0.0791***	-0.0671**	-0.0898***	-0.0818***
	(0.0301)	(0.0304)	(0.0298)	(0.0313)	(0.0315)
Average take-up in the base group	0.1732		0.1623	0.1732	0.1651
N	1,164	1,164	1,152	929	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. Specifications 1-3 and 5 control for Quito 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. In Column 3, we exclude 12 individuals who were assigned to vaccinate during the workweek but went to vaccinate on Saturday. Column 4 presents the estimates using only employees in Quito. In Column 5, we test for different effects across the days of the week using only data from Quito. Using clustered standard errors at the work unit level (142 clusters) yields similar standard errors with no loss of statistical significance. For columns 1-4, we define the base group as the control group in Quito while it is the employees assigned to Wednesday in Quito in column 5.

Table 3 Effect of Peer Vaccination on Individual Take-up

	First Stage	**Reduced Form**	**OLS**	**2SLS**
	(1)	(2)	(3)	(4)
Proportion of Peers:				
Assigned to the Workweek	0.0031***	0.0024***		
	(0.0007)	(0.0008)		
Vaccinated			0.0051***	0.0079***
			(0.0007)	(0.0017)
F-value	16.481			
N	1,138	1,138	1,138	1,138

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

Notes: Standard errors (clustered at the unit level) are in parentheses. The outcome in Column 1 is the proportion of peers who got vaccinated, and the outcome in columns 2-4 is an indicator of individual vaccination. 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. We measure the proportion of peers vaccinated and the proportion of peers assigned to the workweek in percentage points. We define peers as all employees who work in the same unit. All specifications control for Quito fixed effects and individual assignments to the workweek. 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 vaccinated. Column 4 presents 2SLS estimates of the effect of a change in the proportion of peers vaccinated. The first stage F-Stat is based on the Montiel Olea-Pflueger F-value.

Table 4 Effects of Vaccination on Sickness and Sick Days

	OLS	Reduced Form
	(1)	(2)
Panel A: Sickness		
Assigned to the workweek		-0.0166
		(0.0358)
Prop. peers assigned to the workweek		-0.0005
		(0.0011)
Vaccinated	-0.0068	
	(0.0324)	
Prop. peers vaccinated	0.0000	
	(0.0009)	
Average for unvaccinated in Quito (p.p.)	0.47	
N	1,120	
Panel B: Sick Days		
Assigned to the workweek		0.0123
		(0.0361)
Prop. peers assigned to the workweek		-0.0000
		(0.0010)
Vaccinated	-0.0407	
	(0.0298)	
Prop. peers vaccinated	0.0004	
	(0.0009)	
Average for unvaccinated in Quito (p.p.)	0.28	
N	1,120	

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

Notes: Standard errors (clustered at the unit level) are in parentheses. This table presents the effects on the probability of being diagnosed sick with any illness and the probability of having a sick days granted for any illness. The estimates include only units with two or more employees. All specifications control for Quito fixed effects. Column 1 presents OLS estimates. Column 2 presents the reduced form estimates.

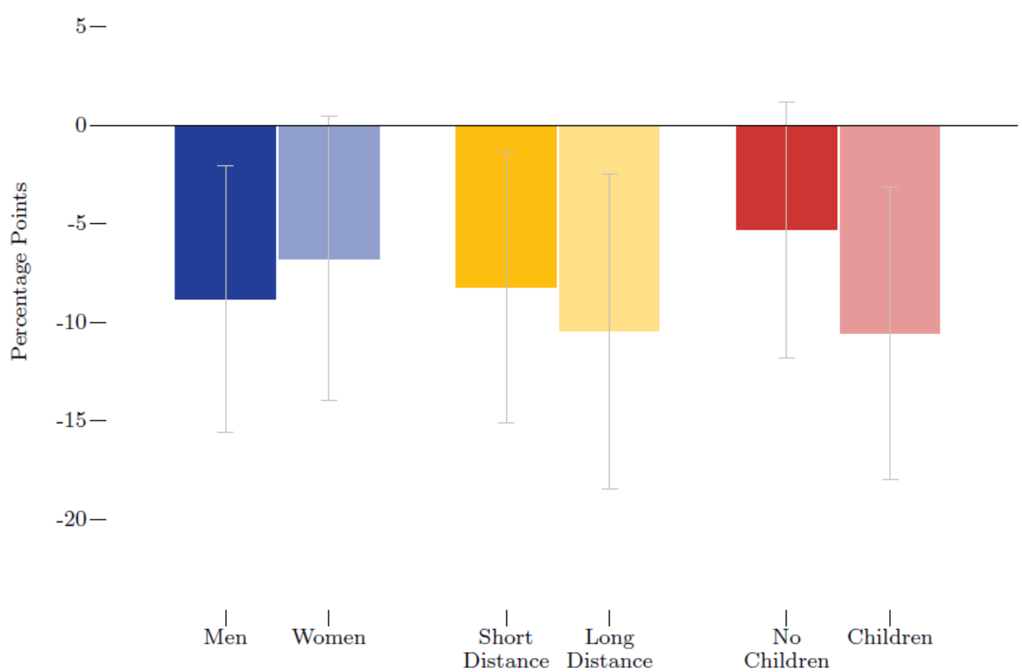
Table 5 Reduced Form Estimates on Health-Related Habits

	Baseline	Coefficient	N
	(1)	(2)	(3)
How often do you exercise	5.93	-0.3145	358
		(0.4026)	
How often do you take dietary supplements	3.18	-0.6212	358
		(0.4376)	
How often do you carry an umbrella when it rains	6.85	-1.2070**	358
		(0.4861)	
How often do you wash your hands	9.25	0.1086	358
		(0.1835)	
Health-related habits index	6.33	-0.5056**	358
		(0.2215)	

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

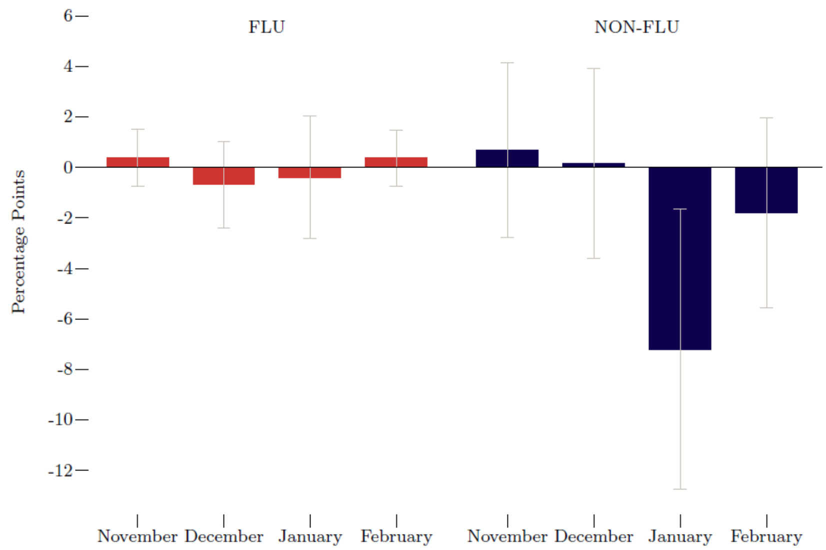
Notes: Robust standard errors in parentheses. This table presents the intent-to-treat effects of being assigned to the workweek on four daily habits and activities related to health and preventing the flu. All responses are on a scale from 1 ("never") to 10 ("all the time"). The health-related habits index is the sum of all four scores divided by four. All specifications control for Quito fixed effects. Column 2 presents the reduced form estimates. Column 3 presents the number of individuals who answered the survey question.

Figure 1 Heterogeneous Effects of Assignment to Vaccination on Saturday on Take-up



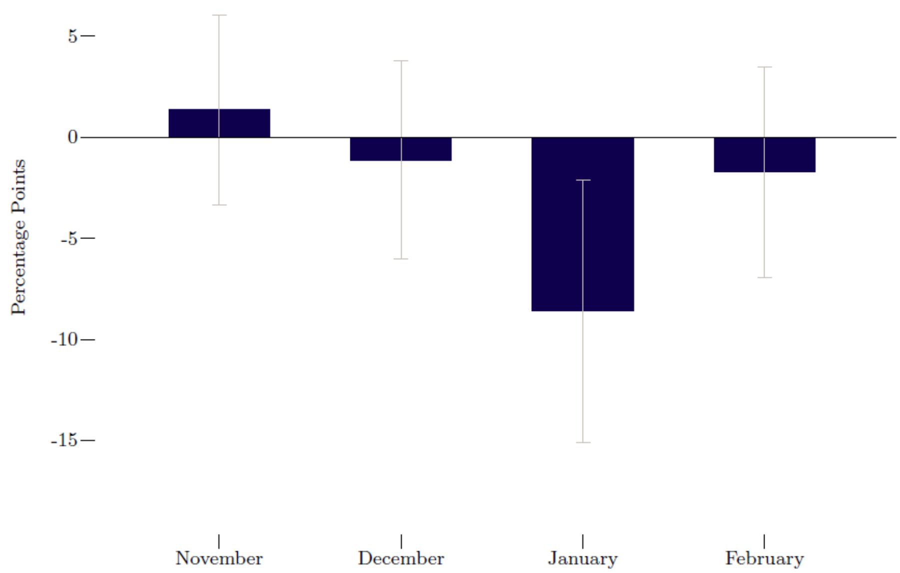
Notes: This figure presents the intent-to-treat effect of assignment to Saturday on vaccination take-up for different subgroups in the sample. All specifications control for city fixed effects. The figure presents each subgroup's point estimate and the 90% heteroscedastic robust confidence interval.

Figure 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 illustration presents the effect of assignment to vaccination on the workweek on flu diagnoses, and the right illustration presents the effect of assignment to vaccination on the workweek on non-flu respiratory diagnoses. The figure presents the point estimate and the 95% heteroscedastic robust confidence interval. November includes cases of diagnosed sickness detected since November 12, after the vaccination campaign.

Figure 3 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% heteroscedastic robust confidence interval. November includes sick days granted since November 12, after the vaccination campaign.

Online Appendix

Appendix A1: Additional Analyses of Flu-Related Sickness Outcomes

In this appendix, we describe our analyses of data on medical diagnoses to estimate the effect of vaccination on the probability of being diagnosed with the flu. OLS results in Column 1 of Table A7 suggest that getting vaccinated goes along with a lower probability of being diagnosed with the flu. However, the reduced form estimate in Column 2 shows that being assigned to the workweek increased the probability of being diagnosed with the flu by 0.4 percentage points, which is not significant at conventional levels. This result suggests that getting vaccinated was ineffective in decreasing the probability of having the flu. Furthermore, the estimates in Columns 1 and 2 show that the proportion of vaccinated peers does not affect the probability of being diagnosed with the flu.

As can be seen in Table A8, the main result for individual vaccine take-up is robust to the inclusion of controls (gender, age, tenure, and income) and to use of a broader and narrower definition of flu-related illness. In another check, we again find no significant impacts when controlling for individual fixed effects using a month-based difference-in-difference approach (Figure A11).

To evaluate whether the estimates rule out meaningful effects of vaccination, we implement an equivalence test based on two one-sided hypothesis tests (Hartman & Hidalgo, 2018; King et al., 2000; Lakens, 2017; Rainey, 2014). The equivalence test has two parts. First, we must define what constitutes a meaningful effect of vaccination. This value comprises two thresholds to evaluate whether the estimates rule out meaningful effects. To define this value, we use flu vaccine effectiveness estimates from CDC data (CDC, 2019). While these estimates come from observational studies on flu hospitalizations and might be biased, they constitute the criteria that policymakers use to evaluate the vaccine's effectiveness. Since our experimental design guarantees that getting vaccinated is the only channel through which assignment to the workweek in the campaign affects health outcomes, we can use reduced form estimates to evaluate whether vaccination has a meaningful effect. In the following exercise, we focus on the probability of being granted a sick day due to having the flu.

The CDC provides data regarding the percentage effectiveness of the vaccine. However, we require percentage point changes for the equivalence test. These percentage changes come from

CDC cross-tabulations on the number of individuals vaccinated and not vaccinated and the number of individuals getting sick with the flu and not getting sick with the flu. The CDC further adjusts these estimates to control for demographic characteristics that affect natural immunity to the flu and thus result in larger estimates; therefore, the reported percentages are a conservative lower bound of the CDC estimates. When comparing our data on flu-related sick days with the CDC hospitalizations, we argue that our estimate comparisons are conservative since a vaccine that is ineffective for the less severe measure should not be effective for the more severe measure. According to the CDC, for working adults, the 2013–2014 vaccine had the highest effectiveness (reducing hospitalizations by 16 percentage points), the 2014–2015 vaccine had the lowest effectiveness (reducing hospitalizations by only 2.2 percentage points), and the 2017–2018 vaccine's effectiveness (the period of the campaign for the current research) fell in between those estimates (reducing hospitalizations by 8.4 percentage points). To compare these values with the reduced form estimates, we multiply the CDC effectiveness estimates by the smallest effect of assignment to the workweek on take-up reported in Table 2, i.e., the most conservative estimate of the first stage (6.7 percentage points). This calculation yields reduced form reference values of −1.1 percentage points, −0.1 percentage points, and −0.6 percentage points, respectively.

In a second step, we test whether the reduced form effect is smaller than each reference value (−1.1, −0.1, −0.6) and higher than the absolute values of the reference values (1.1, 0.1, 0.6). This is equivalent to comparing both the reference values and their respective absolute values with the 90-percent confidence interval of the estimated effect (Lakens, 2017; Rainey, 2014). If the 90-percent confidence interval lies between the reference and its absolute value, then the estimated effects are consistent only with meaningless effects. If the confidence interval falls outside this value range, we cannot rule out meaningful effects in the direction in which the confidence interval overlaps the boundary.

Figure A12 presents the comparisons. We can reject the CDC's vaccine effectiveness estimate for the best season (2013–2014) and that for our campaign season (2017–2018). The estimated effect is consistent with the vaccine's effectiveness of 2014–2015 (the season with the lowest effectiveness). These results imply that we can rule out meaningful health benefits of the flu vaccination based on public health figures provided to policymakers.

Appendix A2: Additional Tables and Figures

Table A1 Regression Discontinuity Effects of Higher Price on Vaccination Take-Up

	Baseline	**With Controls**	**Non-Compliance**	**Quito Sample**
	(1)	(2)	(4)	(3)
Monthly earnings above $750	0.0590 (0.0730)	0.1738 (0.1533)	0.0400 (0.0722)	0.0655 (0.0786)
N	608	608	604	461

* 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 $4.95. 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. In Column 4, we exclude 12 individuals who were assigned to vaccinate during the workweek but went to vaccinate on Saturday. Reducing the bandwidth in steps of $50 to $150 does not change the results.

Table A2 Recall Information Statements

	Heard Altruistic Statement	**Heard Selfish Statement**
	(1)	(2)
Altruistic Information	-1.2079 (4.9521)	-8.4337** (4.1692)
Selfish Information	-3.8421 (4.9557)	-0.0181 (4.0281)
Saturday	-3.5966 (6.2362)	-2.5732 (5.0237)
Baseline	69.09	76.43
N	377	377

* 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 A3 Heterogeneous Treatment Effects on Vaccination Take-up

	Men	Women	Short Distance	Long Distance	No Children	Children
	(1)	(2)	(3)	(4)	(5)	(6)
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 A4 Vaccination Take-Up Analysis of Transportation Costs

	Saturday by Distance (1)	**Saturday by Payment** (2)
Distance	0.0028 (0.0024)	
Saturday	-0.0605 (0.0373)	-0.1224 (0.0751)
Saturday interacted with Distance	-0.0012 (0.0041)	
$4.95 payment		-0.0177 (0.0444)
$7.49 payment		-0.0813** (0.0408)
Saturday interacted with $4.95 payment		0.0043 (0.0866)
Saturday interacted with $7.49 payment		0.0217 (0.0800)
N	895	1,164

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

Notes: Robust standard errors in parentheses. This table presents the results from regressions with vaccine take-up as the dependent variable. It shows interactions between the assignment to Saturday with the distance between the bank and the participants' homes (column one) and vaccine payment (column two). The reference group regarding vaccine payments is a $0 payment.

Table A5 Peer Effects Robustness and Alternative Definitions

		Additional control variables:	
	Baseline	Unit Size	Peer Characteristics
	(1)	(2)	(3)
A.			
Baseline: Workweek Assignment	0.2454***	0.2380***	0.2145**
	(0.0844)	(0.0848)	(0.0861)
N	1,138	1,138	1,138
B.			
Before-or-Same Day Assignment	0.1741**	0.1880**	0.1598**
	(0.0755)	(0.0725)	(0.0754)
N	1,138	1,138	1,138

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

Notes: This table presents reduced form results of the proportion of peers assigned to vaccinate during the workweek on individual vaccination across different definitions of the instrument for peer vaccination and different sets of control variables. Panel A presents the results using the baseline definition of the instrumental variable, which is all employees who work in the same unit and were assigned to vaccination in the workweek while controlling for individual assignments to the workweek. Panel B presents the results when switching to an instrument using only exogenous variation in peers who were working in the same unit and were assigned to the same day or before to get vaccinated while controlling for individual assignments to the day of the week Column 1 shows the baseline results where we control for Quito fixed effects. Column 2 shows the results when we additionally control for the number of employees in each unit. Column 3 shows the results when we additionally control for the number of employees in each unit and peers' age and gender. We always measure the proportion of peers in percentage points. Standard errors (clustered at the unit level) are in parentheses.

Table A6 Potential Mechanisms for Peer Effects

	Effect of Prop. of Peers Assigned to the Workweek on	Baseline	N
	(1)	(2)	(3)
Beliefs about the Flu, its Vaccine, and Interactions with Coworkers			
Vaccines Effective to Improve Health (1-5)	0.0016	3.74	378
	(0.0024)		
Talked with coworkers about getting vaccinated (pp)	-0.0008	0.56	359
	(0.0013)		
Went with coworkers to get vaccinated (pp)	0.0006	0.13	359
	(0.0008)		
Probability of Getting Healthy Without the Vaccine (0-100)	-0.0627	44.25	366
	(0.0473)		
Probability of Getting Healthy With the Vaccine (0-100)	0.0273	56.48	366
	(0.0524)		
Informed about the Flu (0-100)	-0.0302	69.80	371
	(0.0541)		
Informed about the Flu Vaccine (0-100)	-0.0634	63.70	371
	(0.0556)		
Afraid of the Flu (0-100)	-0.0376	37.20	371
	(0.0692)		
Afraid of the Flu Vaccine (0-100)	-0.0243	24.66	371
	(0.0708)		
Would Get Vaccinated out of the Workplace (pp)	0.0000	0.61	366
	(0.0012)		
Coworkers Convinced me to get Vaccinated (0-100)	0.0403	20.60	359
	(0.0739)		
I Convinced my Coworkers to get Vaccinated (0-100)	0.0778	28.37	359
	(0.0686)		

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

Notes: Standard errors (clustered at the unit level) are 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. We measure 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 specifications control for Quito fixed effects and individual assignments to the workweek. Column 1 presents estimates, Column 2, the baseline value for each outcome, and Column 3, the sample size.

Table A7 Effects of Vaccination on Flu Diagnoses

	OLS	Reduced Form
	(1)	(2)
Assigned to the workweek		0.0045
		(0.0155)
Prop. peers assigned to the workweek		-0.0003
		(0.0006)
Vaccinated	-0.0254*	
	(0.0151)	
Prop. peers vaccinated	-0.0001	
	(0.0004)	
Average for unvaccinated in Quito (p.p.)	0.0457	
N	1,120	

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

Notes: Standard errors (clustered at the unit level) are in parentheses. This table presents the effects of flu vaccination on the probability of being diagnosed sick because of the flu. All specifications control for Quito fixed effects. Column 1 presents OLS estimates. Column 2 presents the reduced form estimates. The sample includes only units with two or more employees.

Table A8 Robustness Check for Reduced Form Effects of Vaccination on the Flu

	Narrowest Definition of Flu	Main Definition of Flu	Broadest Definition of Flu
	(1)	(2)	(3)
A. Baseline specification			
Assigned to the workweek	-0.0054	0.0032	-0.0118
	(0.0156)	(0.0160)	(0.0191)
N	1,148	1,148	1,148
B. Additional control variables			
Assigned to the workweek	-0.0051	0.0040	-0.0115
	(0.0157)	(0.0161)	(0.0192)
N	1145	1145	1145

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

Notes: Robust standard errors in parentheses. This table presents robustness checks of the effects of being assigned to the workweek on the probability of being diagnosed sick because of the flu using different definitions. All specifications control for Quito fixed effects. Panel B additionally considers control variables for the vaccine's price, income, tenure, division in the company, gender, age, education level, and income.

Table A9 Heterogeneous Effects on Using an Umbrella

	Baseline	Coefficient	N
	(1)	(2)	(3)
	A. Overall		
How often do you carry an umbrella when it rains	6.85	-1.2190**	358
		(0.4856)	
	B. Vaccine Effective		
How often do you carry an umbrella when it rains	7.13	-1.5793***	256
		(0.5651)	
	C. Vaccine Ineffective		
How often do you carry an umbrella when it rains	6.06	-0.2292	102
		(0.9615)	

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

Notes: Robust standard errors in parentheses. This table presents the intent-to-treat effect of being assigned to the workweek on instances of carrying an umbrella and heterogeneity with beliefs of vaccine effectiveness splitting beliefs at the median on a Likert-scale of 8/10. Responses are on a scale from 1 (“never”) to 10 (“all the time”). Column 2 presents the reduced form estimates. Column 3 presents the number of individuals who answered the survey.

Table A10 Reduced Form Effects on Productivity

	Post-Survey		Swipe-Cards		
	General Productivity	Productivity Post-Intervention	Entry to Work	Exit from Work	Duration at Work
	(1)	(2)	(3)	(4)	(5)
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	343	343	403	403	403

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

Notes: Robust standard errors in parentheses. This table presents the intent-to-treat 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.

Table A11: Cost-Benefit Back-of-the-Envelope Analysis

Average Statistics for Saturday (Control Group)			
Daily wage			$60.13
Sick day incidence			0.2736
Wage paid for sick day incidence			$16.45
Vaccine subsidy			$5.50
Cost-Benefit Analysis	**Worst Case**	**Average Case**	**Best Case**
Sick day incidence relative to Saturday	0.0830	0.0123	-0.0584
Average sick day incidence	0.3567	0.2859	0.2152
Wage paid for sick day incidence	$21.45	$17.19	$12.94
Benefit in wages paid relative to Saturday	-$5.00	-$0.74	$3.51

Note: The table provides a back-of-the-envelope calculation for the costs and benefits of the firm campaign. Panel A shows control group statistics for the average employee. The mean daily wage has been calculated based on the monthly fixed wage divided by 22 workdays. The wage paid for sick day incidence is calculated based on the mean daily wage multiplied by the mean sick day incidence. The mean subsidy arises from the subsidies that the firms provided for individuals during this campaign. Panel B shows the calculations for the cost-benefit analysis. The sick day incidence relative to Saturday is the treatment effect from Table 4 Panel B with the worst and best case being the 95% lower and upper confidence interval.

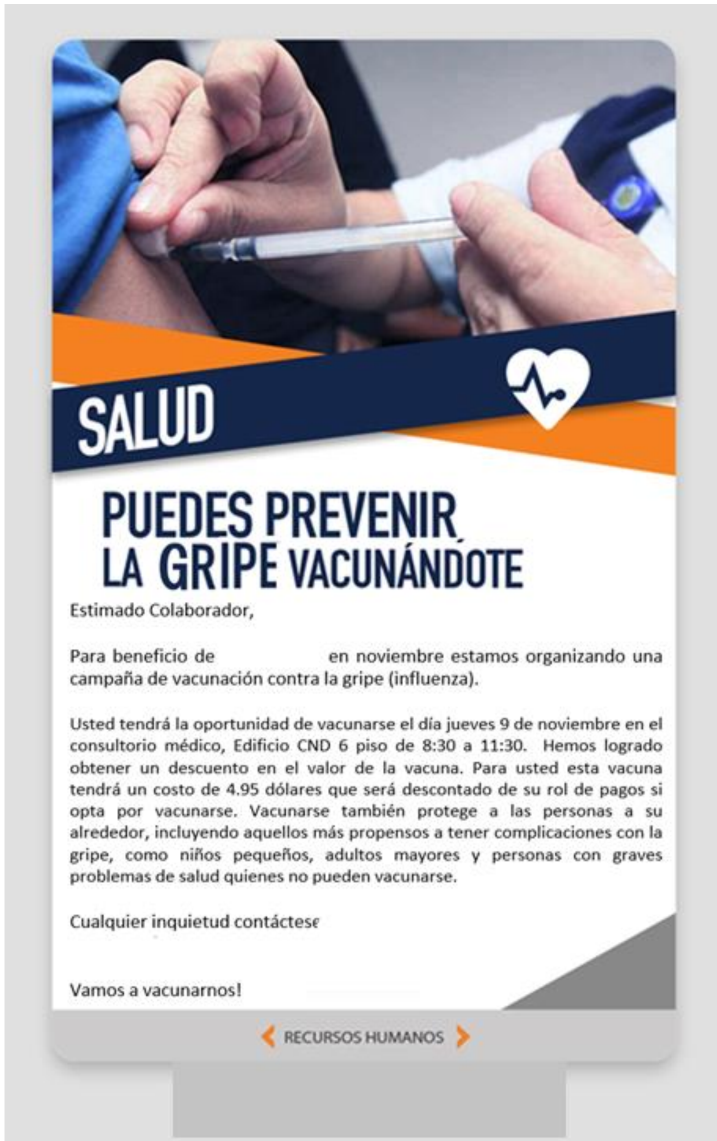
Figure A1 Treatment Message: Control

Notes: The above image portrays the email sent to the control group. Translation: Dear Employee, we are 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, which will be deducted from your payroll if you choose to get vaccinated. If you have questions, please contact _____. Let's get vaccinated!

Figure A2 Treatment Message: Opportunity Cost (Saturday)

Notes: The above image portrays the email sent to the "*Saturday*" treatment group. Translation: Dear Employee, we are 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, which will be deducted from your payroll if you choose to get vaccinated. If you have questions, please contact _____. Let's get vaccinated!

Figure A3 Treatment Message: Altruism



Notes: The above image portrays the email sent to the "Altruistic Treatment" group. Translation: Dear Employee, we are 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, which 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!

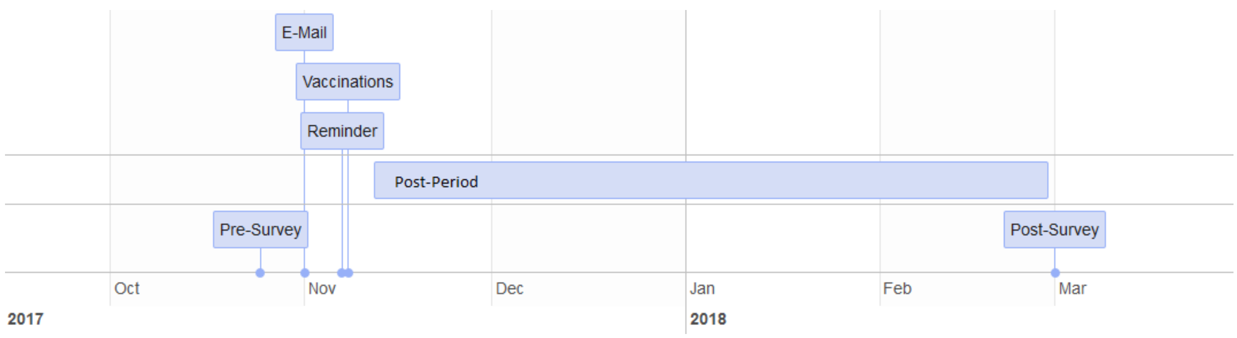
Figure A4 Treatment Message: Selfish

Notes: The above image portrays the email sent to the "Selfish Treatment" group. Translation: Dear Employee, we are 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, which will be deducted from your payroll if you choose to get vaccinated. Vaccination can significantly reduce your risk of getting sick, according to health officials from the World Health Organization and numerous scientific studies. If you have questions, please contact _____. Let's get vaccinated!

Figure A5 Locations of the Bank in Ecuador

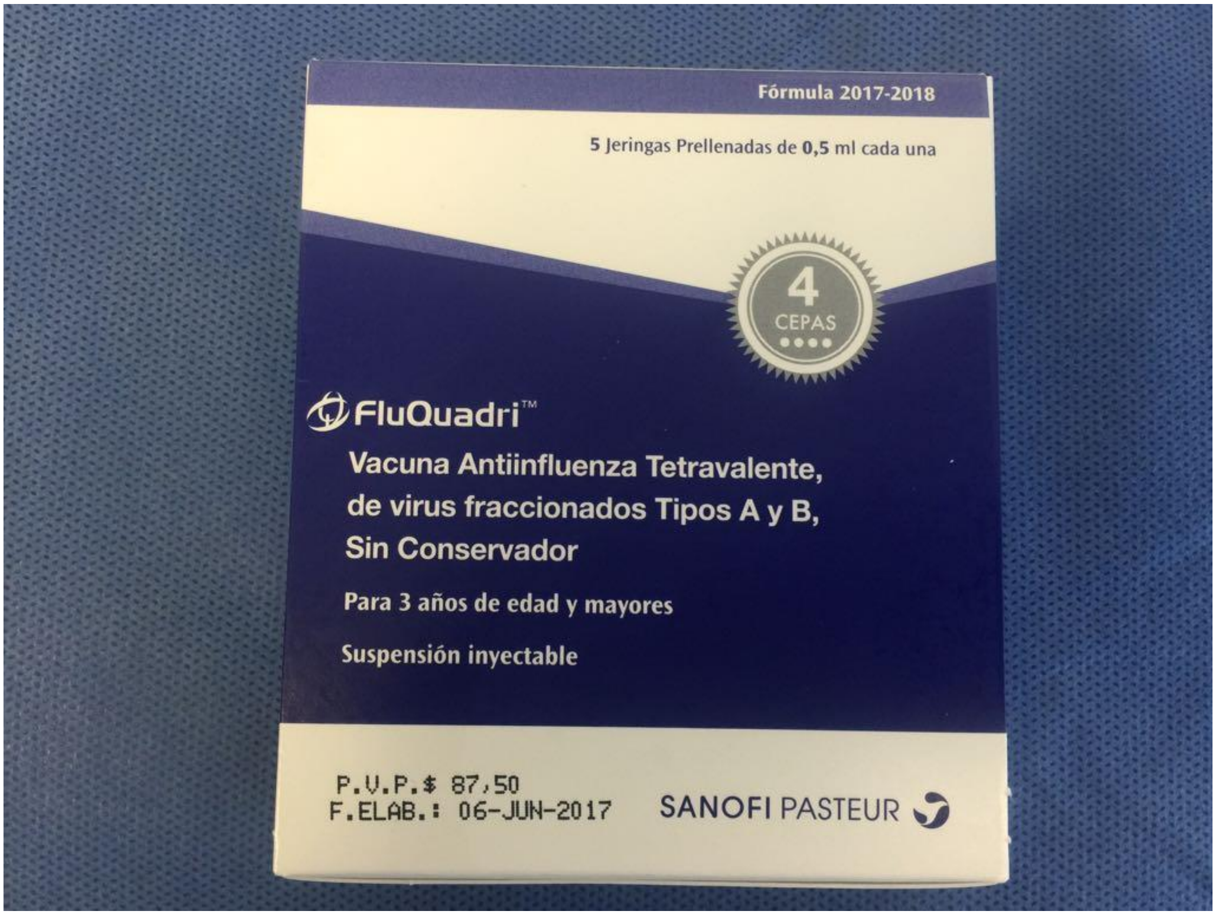
Notes: The map contains the locations of the bank in Ecuador (orange), where we implemented our intervention.

Figure A6 Timeline of Experiment Implementation



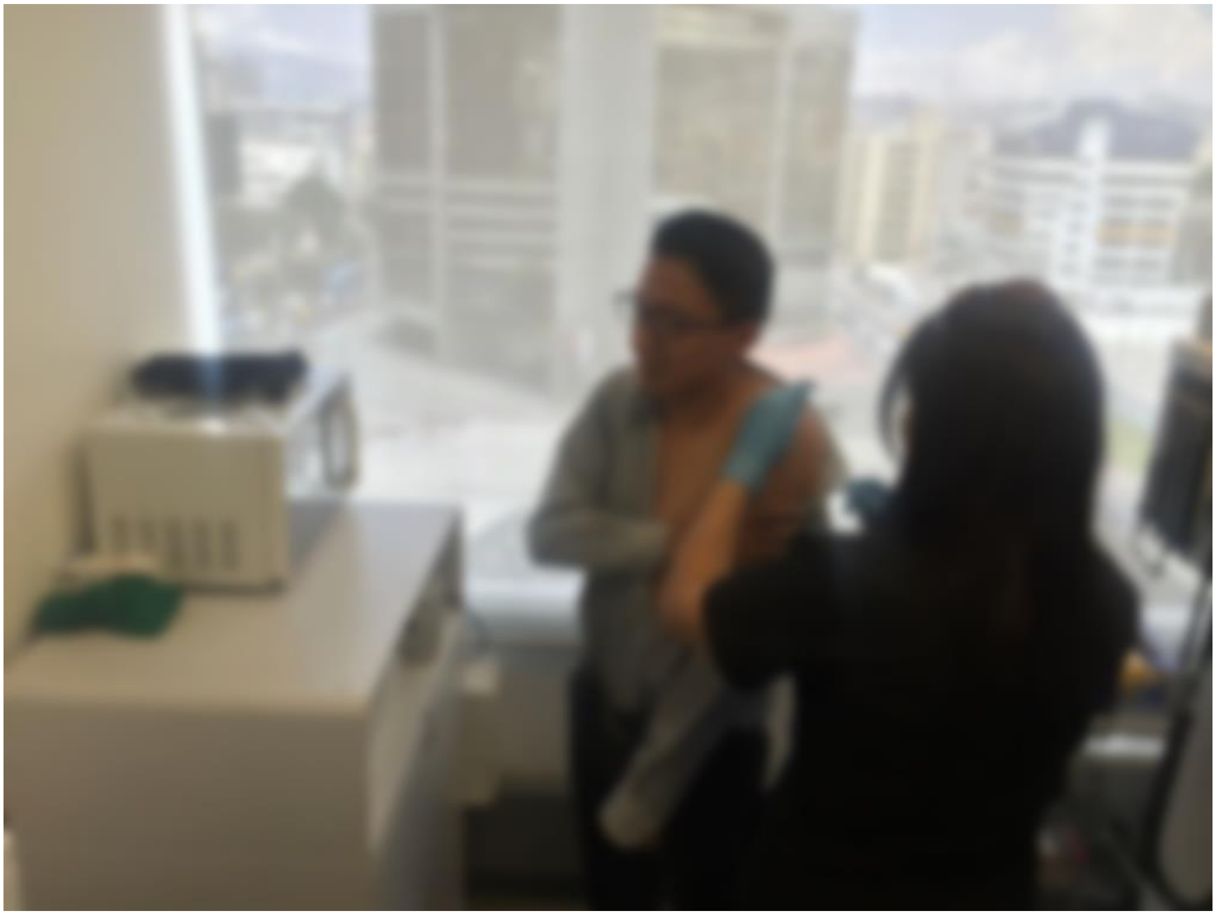
Notes: The bank sent the pre-intervention survey on October 18. The bank sent emails with the different treatments on November 1 using the Human Resources Department mailing account. Furthermore, it sent a reminder on November 7. The vaccination campaign took place between November 8 and November 11. 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 A7 Vaccination Campaign: Influenza Vaccine



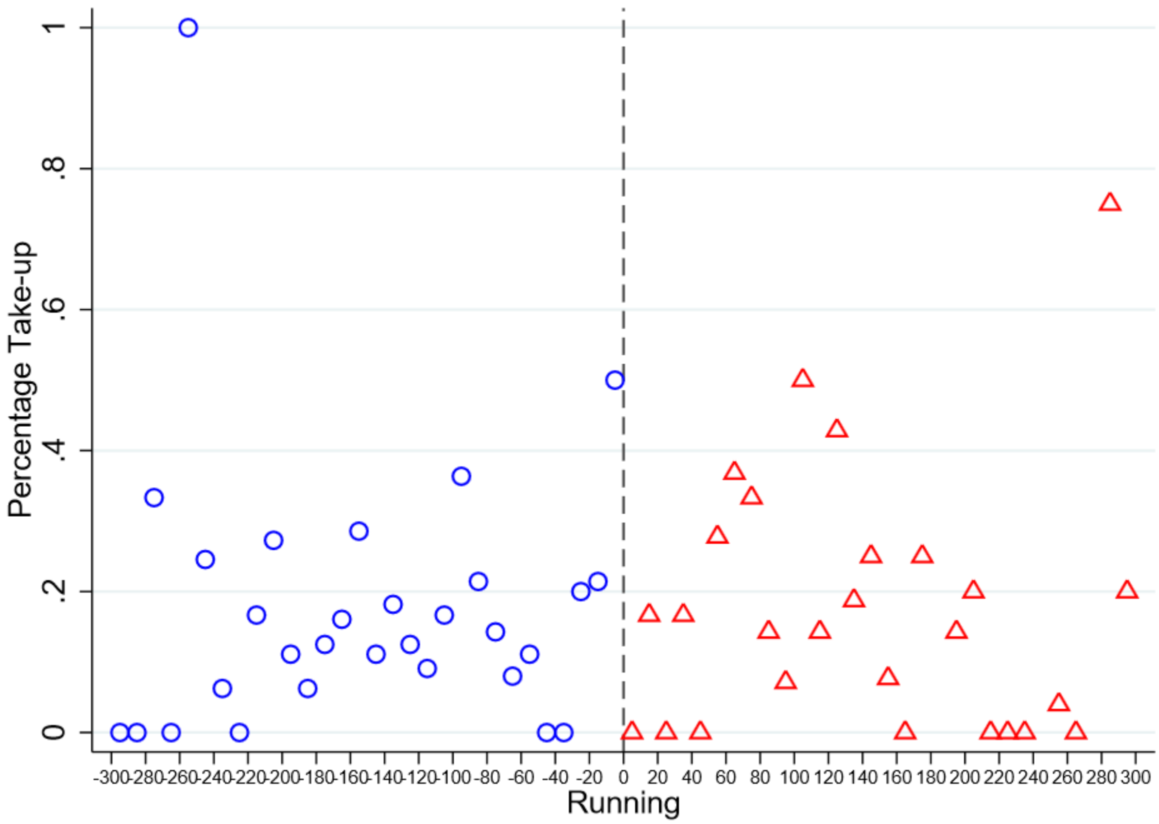
Notes: The above package contains the influenza vaccine used in the campaign. This vaccine protects against four strands of the flu, two from type A and two from type B.

Figure A8 Vaccination Campaign: Flu Shot in Action



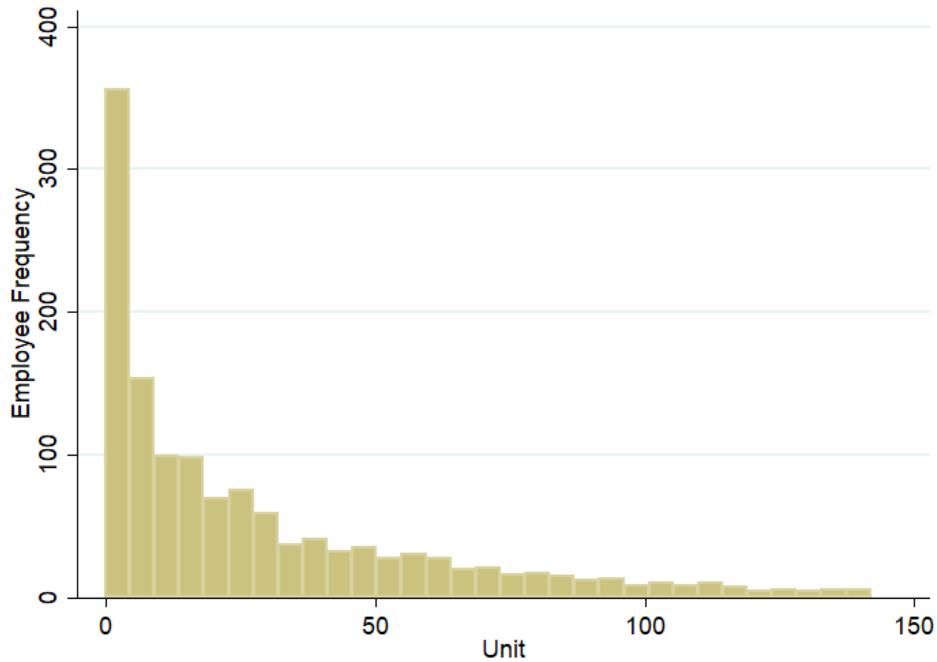
Notes: Immunization at the firm.

Figure A9 Vaccination 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 $4.95.

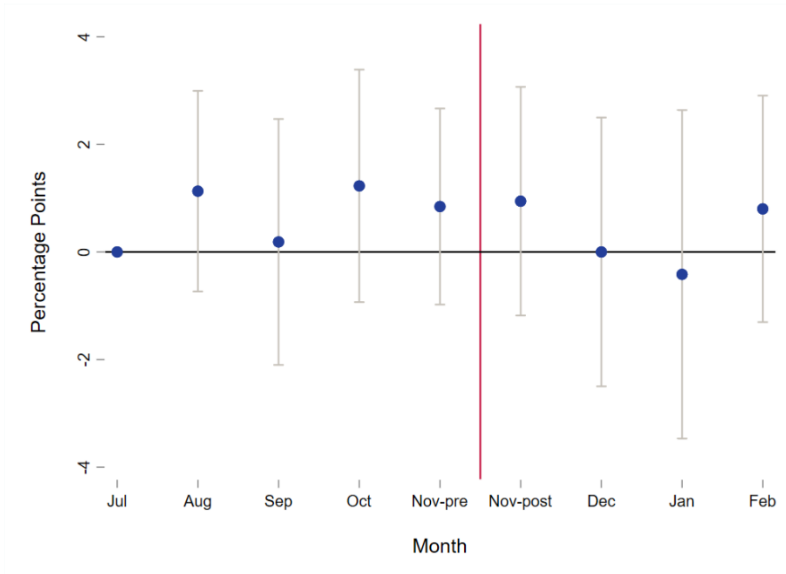
Figure A10 Frequency Distribution of Employees in Units



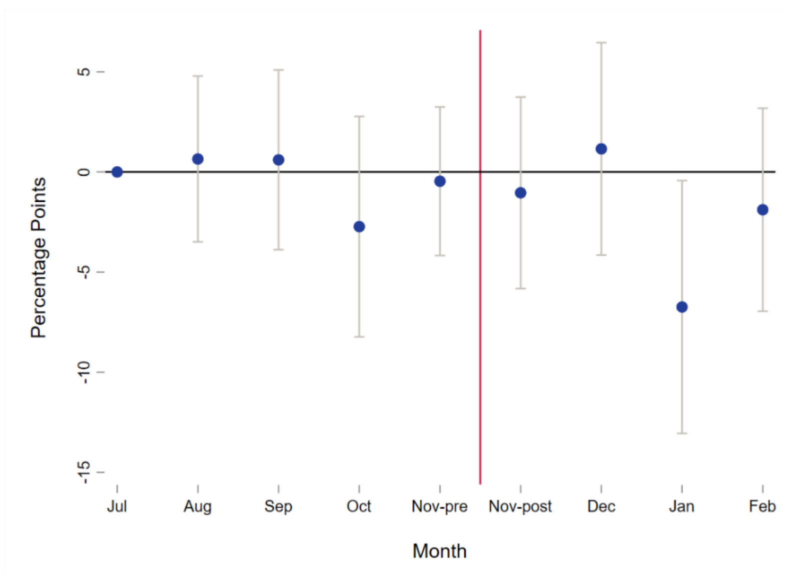
Notes: This figure presents the number of employees in each of the 142 units.

Figure A11 Difference in Difference Effect of Vaccination on Diagnosed Sickness

(a) Flu Diagnoses

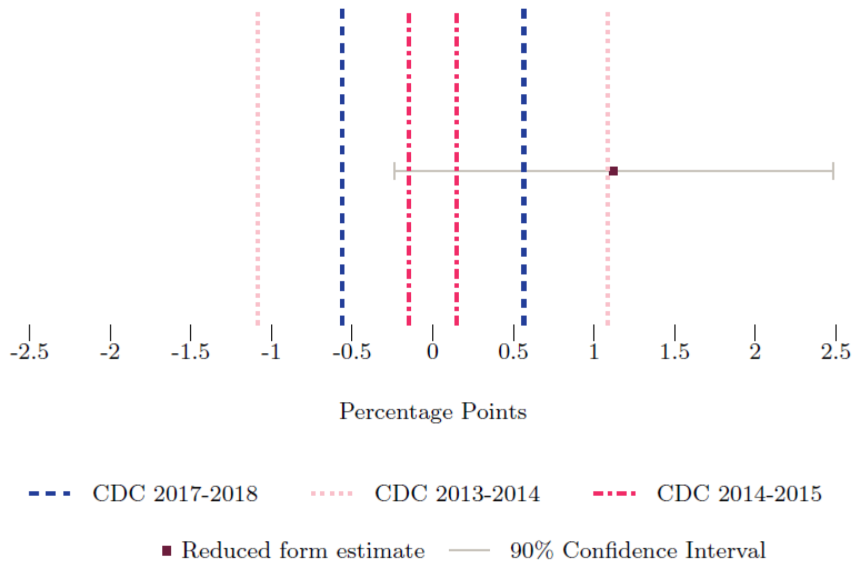


(b) Non-flu Diagnoses



Notes: This figure presents difference-in-differences estimates of the effect of assignment to the workweek on flu and non-flu diagnoses. Estimates control for individual fixed effects.

Figure A12 Equivalence Test for the Effectiveness of Vaccination



Notes: This figure presents the reduced form estimate of the effect of assignment to the workweek on flu incidence to adjusted CDC estimates of the effectiveness of the flu vaccine for 2013-2014, 2014-2015, and 2017-2018 seasons.