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DISCUSSION PAPER SERIES

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ABSTRACT

Vaccines at Work*

Influenza vaccination could be a cost-effective way to reduce costs in terms of human lives and productivity losses, but low take-up rates and vaccination unintentionally causing moral hazard may decrease its benefits. We ran a natural field experiment in cooperation with a bank in Ecuador, where we modified its vaccination campaign. Experimentally manipulating incentives to participate in this health intervention allows us to study peer effects with organizational data and to determine the personal consequences of being randomly encouraged to get vaccinated. We find that assigning employees to get vaccinated during the workweek roughly doubled take-up compared to employees assigned to the weekend, which indicates that reducing opportunity costs plays an important role in increasing vaccination rates. Coworker take-up also increased individual take-up significantly and is driven by social norms. Contrary to the company's expectation, vaccination did not reduce sickness absence during the flu season. Getting vaccinated was ineffective with no measurable health externalities from coworker vaccination. We rule out meaningful individual health effects when considering several thresholds of expected vaccine effectiveness. Using a dataset of administrative records on medical diagnoses and employee surveys, we find evidence consistent with vaccination causing moral hazard, which could decrease the effectiveness of vaccination.

JEL Classification: D90, I12, J01, N36

Keywords: health intervention, flu vaccination, sickness-related absence, field experiment, random encouragement design, moral hazard, technology adoption

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1. Introduction

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

However, individual behavior can counter the potential benefits of vaccination in two ways. First, according to the World Bank, the Center for Disease Control (CDC), and other public health institutions, vaccination rates in most countries of the world are substantially below recommended levels.[1] Therefore, it is essential to understand the factors that affect vaccination take-up, particularly of working adults who are the least likely to get the vaccine. Second, economic theory and empirical evidence suggest that the adoption of protective technologies may induce individuals to behave riskier. Vaccinated individuals may overestimate the protection that the vaccine grants and engage in risky behaviors like waiting longer before going to the doctor when feeling sick and taking fewer protective measures to prevent illnesses. Thus, moral hazard could counter the benefits of adopting a preventive medical technology like the flu vaccine.

In this paper, we study the causes and consequences of vaccination: how economic factors affect working adults' decision to vaccinate, the effects of vaccination on health and whether flu vaccination can cause moral hazard. In cooperation with a bank in Ecuador that provides annual vaccination campaigns to improve its employees' health, we implement a natural field experiment by randomizing incentives to get a flu shot.[2]

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

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

Our design allows us to analyze how monetary and non-monetary determinants – price, opportunity costs, information, and peers – affect working adults' decision to vaccinate. We introduced three modifications to the bank's 2017 vaccination campaign to influence take-up. First, implementing a company policy of income-dependent subsidies, we selected an income threshold at which the vaccine's price for employees changed. Second, due to capacity constraints, employees had to be assigned to vaccinate during the workweek or on Saturday. By randomizing the assignment of employees for vaccination, we could manipulate the opportunity costs of vaccination because employees would need to incur additional transportation costs and arrange their weekend schedules to get vaccinated on a Saturday. In contrast, assigning employees to the workweek minimizes their opportunity costs because the bank allows them to take time off their duties to get vaccinated at the firm's location. Third, we varied the content of the invitation emails by appealing to altruistic or selfish motives.

In the next step, we use the exogenous variation in vaccination generated by these modifications of the campaign to study the consequences of getting a flu shot. First, we analyze the effects of peer vaccination on the propensity for a co-worker to also get vaccinated. Second, we study the impact of individual vaccination as well as peer vaccination on employees' health and sickness-related absence. Third, we analyze how getting vaccinated affects the behavior of employees in order to discuss the possibility of moral hazard when adopting medical technology.

Our design overcomes several challenges that arise when studying the causal effects of vaccination on health-related outcomes. The first challenge is to identify the causal effect of getting vaccinated. While the medical literature documents modest positive health effects of flu vaccination, many of the studies could be affected by selection and other biases (Jefferson et al., 2010; Osterholm et al., 2012; Demicheli et al., 2014; and Østerhus, 2015; Demicheli et al., 2018). For instance, researchers describe the problem of a "healthy vaccine recipient effect" that could bias observational studies. If healthier individuals are more likely to get vaccinated, such a positive selection bias could lead to an overestimation of the health effects. Nevertheless, observational studies without randomization of vaccination are often preferred because of ethical concerns regarding randomized controlled trials with placebos in the context of health (Sanson-Fisher et al., 2007; Baxter et al., 2010). For the same reason, RCTs are often conducted using other types of vaccines instead of clean 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 generated through the manipulation of incentives to take part in the campaign. To study the impact of vaccination on health-related outcomes, we thus employ a random encouragement design (Bjorvatn et al. 2015, List et al., 2017). This is an innovative idea in the context of preventive medical technologies circumventing the ethical dilemma of withholding a potentially effective medical treatment while allowing for causal evidence.

The second challenge we overcome is capturing the total effect of vaccination. Public health institutions and companies are interested in the total effect of health interventions that includes medical and behavioral responses. However, medical research on vaccines focuses solely on the medical effects, without considering changes in behavior that may affect health. In randomized controlled trials, participants know that they are in an experiment but do not know if they received a specific type of vaccine or not. This eliminates the possibility of identifying changes in behavior when comparing experimental conditions. In contrast, our random encouragement design introduces no uncertainty in treatment, capturing both the behavioral and medical effects of getting vaccinated. This allows us to explore if vaccination induces individuals to adopt riskier behaviors.

The bank's data allows us to address a third challenge. We may underestimate the medical effectiveness of the vaccine because of positive health-spillovers from the vaccinated to the unvaccinated (White, 2018). The empirical setting attenuates this concern as flu vaccination rates in Ecuador fluctuate around 2% (ENSANUT, 2012). To assess the role of spillovers directly, we estimate the effect of peer vaccination on health outcomes. For a comprehensive analysis of health-related outcomes, we have access to the bank's administrative data that we merge with information on treatment assignment at the individual level. The data include detailed medical diagnoses for each employee so that we can identify illnesses, including flu diagnoses, and the resulting sick days. We can also distinguish flu-related sickness from non-flu-related sickness, which allows us to study the behavioral effects of vaccination. Finally, employee surveys before and after the vaccination campaign complement the administrative data and allow inspection of mechanisms for the effects on employee health and behavior.

We find the following results. First, a $2.48 change in the vaccine's price did not affect take-up. Second, decreasing opportunity costs by assigning employees to get vaccinated during the workweek increased take-up by slightly more than ten percentage points, which constitutes an increase compared to Saturday of roughly 100 percent. Thus, reducing opportunity costs has a remarkably strong effect on take-up for working adults. Third, we find no effect from providing information on altruistic or personal benefits of vaccination. The coefficients are close to zero, negative, and statistically insignificant.

Next, we study the effect of peer vaccination on individual take-up. For this purpose, we can exploit exogenous variation in the proportion of peers who get vaccinated. In our setup, coworkers that work directly together every day define the relevant social groups. Given randomization at the employee level, by chance, some units have more employees assigned to the workweek than other units, and hence, are encouraged to get the vaccine more than in other units. We find that when the proportion of peers that get vaccinated increases by ten percentage points, take-up increases by 7.9 percentage points. More in-depth analyses of the mechanisms behind these peer effects suggest that peers are not changing information or beliefs about vaccination, but instead, employees follow behaviors that they deem socially acceptable by conforming to the peer prescriptions of their working group.

As a question of high relevance for policy-makers and firms that run vaccination campaigns, we then investigate the consequences of vaccine take-up by examining if flu vaccination is effective in improving working adults' health, thereby potentially lowering sickness-related absence. If flu vaccination decreases flu cases, we expect that offering employees the opportunity to get vaccinated during the workweek would reduce the number of flu cases and absence from work. However, the estimates show no evidence that exogenously triggered vaccination decreased sickness in general or sickness-related absence. By using the data from the medical records, we also cannot find that the probability of getting the flu changed due to participation in the vaccination campaign. The confidence intervals rule out effects that correspond to meaningful thresholds of an effective vaccine based on CDC figures.

There are several potential explanations why we cannot find evidence for health improvements due to vaccination. First, it could be that positive health-spillovers from vaccinated to unvaccinated individuals explain why we underestimate health benefits. However, the results show that peer vaccination does not affect the probability of being diagnosed sick or having a sick day, which is consistent with unit vaccination rates being below herd immunity levels. Second, it could be that the flu vaccine was medically ineffective, which our design cannot rule out. Third, independent of the vaccine's medical effectiveness, employees could adopt riskier and thus health-threatening behavior that could mitigate the vaccine's immunity benefit.

We provide evidence from several behavioral tests consistent with the notion of individuals adopting riskier behaviors when they get vaccinated. Vaccinated individuals could overestimate the protection of the vaccine and avoid going to the doctor when they have flu-like symptoms. We test this hypothesis by investigating the effects of vaccination on non-flu respiratory illness during a national health emergency. The flu vaccine does not provide immunity against non-flu respiratory illnesses, so flu vaccination should not affect the probability of being diagnosed with these diseases. However, flu and non-flu respiratory diseases share symptoms, so a person cannot distinguish them unless she goes to the doctor. In January 2018, as a result of a significant increment of flu cases nationwide, the Ecuadorian government launched a massive media campaign asking the population to go to the doctor if they felt *any* flu-related symptom. If vaccinated individuals felt protected, they would have been less likely to follow the government's calling when feeling flu-like symptoms, resulting in fewer visits to the doctor and fewer diagnoses of non-flu respiratory diseases that share symptoms with the flu in that month compared to unvaccinated employees.

First, we check if vaccination affects the likelihood of being diagnosed with a non-flu respiratory disease. If vaccinated individuals feel more protected, they might think flu-like symptoms correspond to a minor respiratory illness, and not heed the government's calling, which would reduce non-flu diagnoses. Consistent with this hypothesis, the results show that assigning individuals to the workweek decreased the likelihood of being diagnosed with a non-flu respiratory disease by 7.2 percentage points during January, with no effect in other months.

Second, by the same logic, we check if vaccination affects the likelihood of going to the bank's doctor at the on-site health center. This health center is a convenient feature for the employees because they can visit the doctor during work hours without asking for permission and without charge. Before our intervention, on-site doctors accounted for 77 percent of all diagnosed sickness cases. Vaccinated individuals may have been less likely to visit these doctors when the government launched its media campaign because they felt more protected. In January 2018, we find that being assigned to the workweek for vaccination decreased the probability of going to the onsite doctor by 8.6 percentage points, with no effect in other months.

As the final test of moral hazard, we check if individuals report abandoning practices believed to be effective in improving health, regardless of their actual medical effectiveness. We find that assigning employees for vaccination on the workweek decreased the frequency of individuals reporting to engage in practices culturally believed to help prevent the flu and other respiratory diseases. Moreover, these effects are driven by individuals who believe the vaccine is beneficial to prevent the flu, 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 of vaccines and other medical technologies. Previous studies mainly discuss how vaccination take-up is affected by laws, information, education, age, health status, health behavior, and lifestyle (Maurer, 2009; Schmitz and Wuebker, 2011; Godinho et al., 2016; Bradford and Mandich, 2015; Chang, 2018; Oster, 2018). The few studies focusing on economic aspects have considered how compensating the opportunity costs of vaccination affects vaccine take-up of children and vulnerable groups in rural areas in developing countries (Banerjee et al., 2010; Sato and Takasaki, 2018a) or populations with limited income (Bronchetti et al., 2015) by providing in-kind transfers.[3] For the effect of peers on the adoption of medical technologies, the theoretical literature predicts free-riding on vaccination benefits due to herd immunity, but empirical research based on non-hierarchical peer networks such as friends or neighbors finds mixed results (Geoffard and Philipson, 1997; Kremer and Miguel, 2007; Chen and Toxvaerd, 2014; Rao et al., 2017; Sato and Takasaki, 2018b). Rao et al. (2017) find that providing information and changing beliefs is the mechanism through which non-hierarchical peers affect the adoption of medical technologies.

Our study contributes to this literature in several ways. First, we employ a unique setup that allows for variation in different types of costs. We manipulate the opportunity costs of vaccination directly by changing the day of vaccination, which changes the next best alternative participants face. Thus, we directly test the effect of changing these costs, which can be different from

[3] Economic theory identifies both monetary and opportunity costs as a relevant component in the decision to adopt medical technologies like vaccination (Brito et al., 1991; Geoffard and Philipson, 1997; Kremer and Miguel, 2007; Chen and Toxvaerd, 2014).

compensating them.[4] We find that reducing opportunity costs has a substantial effect on vaccination of working-age adults who are not constrained by income and who live in locations where access to vaccines is not an issue, as in most major cities in both developing and developed countries. The estimates are of similar magnitude as previous studies that focused only on vulnerable populations, which implies that opportunity costs are an important factor for any population. In contrast, a small change in the vaccine's price did not raise take-up suggesting that financial incentives have to be substantial in order to be effective. Also, information nudges were ineffective, similar to those in previous studies (Bronchetti et al., 2015; Godinho et al. 2016). Second, we study how the adoption of medical technologies can be affected by a peer group that has received little attention in this research area so far: co-workers. [5] Most working adults share at least half of their awake time with their coworkers. Unlike friends and other non-hierarchical peers, employees cannot choose the individuals they are going to work with after they are hired. We document that this peer group can have a significant positive influence on the adoption of preventive medical technologies like vaccines, which is inconsistent with the theoretical concept of free-riding in this context. Our evidence indicates that the primary mechanism for peer effects in vaccine take-up is the employee conforming to the norms of their workgroup. This result provides a new policy lever to influence the adoption of preventive medical technologies in working adults.

Our study contributes to the literature on the consequences of medical technologies as well as the literature of on-site health interventions (Just and Price, 2013; List and Samek, 2015; Belot et al., 2016), and the broader literature on public health interventions (Evers et al., 1998; Cawley, 2010; Bütikofer and Salvanes, forthcoming). Our findings on the health effects of vaccine take-up add to an ongoing discussion that predominantly takes place in the medical literature, with a few recent exceptions in the economics literature (Ager et al. 2017, Lawler 2017, Carpenter and Lawler 2019). In a study on flu vaccines, Ward (2014) finds that flu vaccination increased sickness absences in years when the flu vaccine had a bad match with the prevalent flu viruses, and it had no effect in years when the vaccine had a good match. The difference between these two results, which would control for moral hazard, points to the medical benefits of the vaccine. Very few

[4] Behavioral economics studies have found that losses are treated differently than gains since the seminal theory of Kahneman and Tversky (1979).

[5] In contrast, there is a large body of research about the effects of co-workers on the productivity of their peers at the workplace (Mas and Moretti, 2009; Herbst and Mas, 2014).

medical studies consider the possibility of medical technologies unintentionally causing moral hazard (Richens et al., 2000; Prasad and Jena, 2014) while the few papers in economics that study moral hazard in the context of medical interventions find mixed results (Klick and Stratmann, 2007; Margolis et al., 2014; Moghtaderi and Dor, 2016; Doleac and Mukjerjee, 2018).[6]

We contribute to this literature in three ways. First, we employ a novel design for public health and medical interventions, allowing us to circumvent measurement and ethical problems. We hope to encourage other researchers to use the same methodology in the field of health to obtain causal estimates. Second, with our evidence on the consequences of flu vaccination for sickness-related absence, we contribute to the research on the determinants of this particular workplace outcome (Ziebarth and Karlsson, 2010; Bütikofer and Skira, 2018). Third, we provide behavioral evidence based on experimental variation that getting vaccinated induces individuals to feel protected and to forgo preventive practices like going to the doctor in the presence of illness symptoms. This result is consistent with preventive medical technologies causing moral hazard and with the theoretical model of Talamàs et al. (2018). Finally, by showing that preventive medical technologies can unintentionally cause moral hazard, we offer an explanation of why health interventions may not always be as successful in improving health. This finding implies that firms and policymakers should consider moral hazard when promoting the adoption of preventive medical technologies.

2. Experimental Design

We ran the field experiment in cooperation with a bank in Ecuador. This bank focuses on consumer credit and is one of the largest credit card issuers in the country. Its headquarters are in Quito, Ecuador's capital, and it has six branches across the country with over 1,300 employees, distributed in 31 divisions with 142 working units. The bank had run small vaccination campaigns in the past. These campaigns included only some employees in crowded areas and ran during the

[6] There is a large literature studying whether the adoption of safety devices lead individuals to adopt riskier practices (Peltzman, 1975; Richens et al., 2000; Auld, 2003; Cohen and Einav, 2003; Klick and Stratmann, 2007; Peltzman, 2011; Prasad and Jena, 2014; and Talamàs et al., 2018) and also a large literature that studies moral hazard in insurance, e.g. see Einav et al. (2013) and Einav and Finkelstein (2018).

workweek in the bank's offices.[7] In 2017, the bank decided to extend its annual campaign to all its employees and allowed us to experimentally modify it to investigate how to increase take-up and the effects of vaccination. We implemented three interventions: we changed the vaccine's price for some employees using income-dependent subsidies, we randomized assignments for on-site vaccinations across weekdays, and we implemented information nudges by varying the content of the emails used to invite employees to vaccinate.

The bank decided to provide the vaccine for free to areas that participated in campaigns in previous years and to partially subsidize it for new participants. Since the company opposed the randomizing subsidies, we used information on employees' income to allocate this subsidy. Employees who earned less than $750 per month would pay $4.95 to get vaccinated, while those who earned more than $750 would pay $7.43. Note that the vaccine's full price is $9.99. Employees were informed about the vaccine's price in their invitation email. This email included basic information about the campaign and informed employees that the payment for the vaccine was directly deducted from their paycheck if they opted to get vaccinated. The email also contained information on the assigned day and time. Appendix Figure A1 shows an example of an invitation to a low-price flu shot on Thursday morning.

To examine the effects of opportunity costs and information, we randomly assigned all employees into one of four groups.[8] First, employees assigned to the control group (*Control*) were invited to get vaccinated during the workweek (Wednesday, Thursday, or Friday) and were allowed to take time off their duties to get vaccinated. The specific day was selected randomly for each employee.

The first treatment increased the opportunity costs of vaccination by assigning employees to get vaccinated on *Saturday*. The employees usually do not work during the weekend, so they would incur extra transportation costs and have to arrange their schedules to go to the bank and get vaccinated.[9] Otherwise, this group received the same information as the Control (see Figure A2).

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

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

[9] Based on data from the employees' magnetic swipe cards to enter the bank, only 0.4% of the employees work regularly on Saturdays.

This treatment was only applied in Quito because all the other branches are substantially smaller (82% of the employees work in Quito), and their employees could get vaccinated in a single day, which was not possible in Quito.[10]

We also implemented two information nudges. We kept the additional messages as unobtrusive as possible to prevent confounding the effect of information with salience or other behavioral factors. The first nudge highlights the social benefits of flu immunization (*Altruistic Treatment*). In addition to the information provided to the control group, the email included the phrase: "Getting vaccinated also protects people around you, including those who are more vulnerable to serious flu illness, like infants, young children, the elderly and people with serious health conditions that cannot get vaccinated" (see Figure A3). The second nudge highlights the individual benefits of flu immunization (*Selfish Treatment*). In addition to the information provided to the control group, the email included the phrase: "Vaccination can significantly reduce your risk of getting sick, according to both health officials from the World Health Organization and numerous scientific studies" (see Figure 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). The bank ran a pre-intervention survey from October 25 to October 29, 2017. Human Resources sent the intervention emails on November 1, 2017, using its official email account. Employees were not aware that this study was taking place. For them, the campaign was just a regular activity organized by the Human Resources department. Employees are used to receiving emails from Human Resources, and according to the Human Resources manager, they typically read these emails carefully. The bank sent out a reminder using the same email account a week later. The vaccination campaign ran from November 8 to November 11, 2017, at locations within the bank's offices in each branch. The bank hired an external medical team to supply and inject the vaccines. Finally, the bank conducted a post-survey during March and April 2018.[11]

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

[11] 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.

3. Data

This section describes the data used in our analyses for assessing how monetary and non-monetary determinants can affect take-up and the effects of flu vaccination. First, we have access to the firm's administrative records about its employees: gender, age, education level, and dependents; job and its position within the bank's organizational structure; tenure and income; and medical diagnoses and sick days. Second, we collected vaccination take-up data from the bank's campaign records. Third, we use data from pre- and post-intervention surveys. These surveys asked employees about previous illnesses and general health, knowledge, and beliefs about vaccination and the flu vaccine, habits related to health, relations with coworkers, opinions about the campaign, motivation, organizational attachment and work satisfaction, and risk and time preferences.

--- Table 1 about here ---

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

The administrative data include two measures of health: medical diagnoses and sick days. These measures come from two sources: onsite doctors and medical certificates from outside 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.[12] Consequently, the onsite doctors report every visit they receive to Human Resources. The doctors report the diagnosis (the type of disease),

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

whether they granted a sick day or not, and the number of sick days granted. Also, by law, if an employee takes time off work to go to an outside doctor, then she has to present a medical certificate to Human Resources that indicates the diagnosis and number of sick days granted, if any. Hence, in addition to sick days, we can also observe employees being diagnosed sick with no sick days granted for cases where a doctor did not consider the illness severe enough. Thus, sick days are a measure of more severe illness. From January to early November 2017, before the intervention, two out of three employees were sick from any disease, and 37% had at least one sick day (see Table 1).

Doctors diagnose their patients using a combination of a physical examination, blood tests and culture tests. The specific procedure is part of individual medical records to which we do not have access. Diagnoses that name the ‘flu’ as the reason for being diagnosed sick provide us with the narrowest definition of flu-related sickness. If flu cases presented complications, then the data reports the complication as the diagnosis and does not mention the flu explicitly. To address this issue, we have an extended definition of flu-related sickness, which includes diagnoses that could likely be complications due to the flu, according to a third-party physician. We focus on this measure in our empirical analysis and check the robustness of the results by employing the narrowest definition and an even broader definition, also provided by this physician. Any other respiratory disease that was not classified by this doctor as flu is by definition listed as a non-flu respiratory disease. A second physician verified these measures to be sure about the distinction between flu-related and non-flu-related health problems.

Table 1 also shows evidence on the balance of treatment assignment. Columns 2 to 5 present the mean employee characteristics across the four groups. All variables have almost identical means across groups. For each characteristic, Column 6 shows the p-value of a joint significance test of differences of means. We cannot reject the null hypothesis that the means are the same across the four treatments, which suggests that our randomization was successful. The Kruskal-Wallis rank test shows the same result. Finally, we test whether participating in the pre and post surveys is different across treatments. We find no statistically significant difference.

4. Analysis of Vaccination Take-up

In this section, we study how monetary and non-monetary determinants affect working adults' decision to vaccinate. Specifically, we consider the effect of opportunity costs, information nudges, and peers on take-up in detail. We do not find any effect of the $2.48 price difference on vaccination take-up from the income-dependent vaccine subsidy.[13] We conclude that such price change may be too small to induce changes in take-up behavior.

The last row in Table 1 presents the flu immunization take-up rates for the different treatments during the campaign. The *Control* group has a take-up rate of 22%, the *Altruistic* treatment has a take-up of 17%, and the *Selfish* treatment has a take-up of 19%. Comparing across the three groups suggests that the information treatments were not sufficient to increase take-up. In contrast, being assigned to get vaccinated during the workweek increases take-up by 14 percentage points in contrast to *Saturday* (112%).[14] 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 effect of opportunity costs, altruistic information, and selfish information on vaccination take-up for employee *i* in city *c* using the following equation:

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

where $Takeup_{ic}$ is an indicator of getting vaccinated. We include Quito fixed effects γ_c to account for differences in 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

[13] Figure A9 shows no visible discontinuity across the threshold. Regression discontinuity estimates also do not indicate any significant change in take-up at the cutoff which is robust to different bandwidths (see Table A1).

[14] In the post-intervention survey 59 employees report that they got vaccinated outside the campaign. Vaccination outside the campaign is not significantly different by treatment status. All employees who stated they got vaccinated outside, did not get vaccinated during the campaign. One individual stated not getting vaccinated, even though the person did get vaccinated according to our records. 18 individuals stated they got vaccinated during the campaign but did not. If we exclude those 19 individuals, which misremember vaccinations, the estimates do not change. Also note that between November 2017 and February 2018, 20 treated employees quit the bank. Attrition is not affected by treatment assignment.

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

Table 2 presents the effects of the different treatments on take-up. Column 1 shows the baseline results of the effect of opportunity costs and information on vaccination take-up. The estimates indicate that assigning employees to *Saturday* decreased take-up by 7.9 percentage points compared to the *Control*. This effect is approximately 46% of the take-up in Quito for the *Control* and is statistically significant at the 1% level. Hence, minimizing the opportunity costs associated with vaccination is a useful measure to increase take-up.

Conversely, we find that emphasizing either the altruistic or the selfish benefits of vaccination does not affect take-up. The coefficients are close to zero, negative, and statistically insignificant. It is plausible that supplying a sentence of additional information is not enough to further increase take-up, given that reducing opportunity costs has a substantial effect on it.[15] One interpretation of these results is that information would have to be very salient to accrue an effect on vaccine take-up in a company context such as this.

---- Table 2 about here ---

Columns 2-4 of Table 2 show the robustness of the results to the inclusion of controls, to the use of a restricted sample, and to controlling for non-compliance. Specifically, Column 2 shows that controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level does not affect the estimates. Column 3 addresses the fact that only employees who work in the bank's headquarters in Quito were assigned to vaccinate on *Saturday*. In this subsample, assigning employees to *Saturday* decreased take-up by almost nine percentage points (51% of the control group take-up), significant at the 1% level. This result is slightly larger than the main result, but we cannot reject that they are statistically the same. Both information treatments have small, negative, and statistically insignificant effects. Column 4 shows the effect of controlling for non-compliance.[16] In this subsample, assigning employees to *Saturday* decreased

[15] The post intervention survey asks if the employee recalls the altruistic and selfish information statements. Appendix Table A2 shows that neither employees assigned to the *Altruistic* treatment nor those assigned to the *Selfish* treatment remember their respective statements better than the control. Another issue could be spillovers of information, but this is unlikely given that our design provides information directly to the treated individuals via email.

[16] We identified in the campaign records 12 employees assigned to the workweek who vaccinated on *Saturday*. The bank asked the medical team in charge of the vaccination campaign to enforce the day assigned to each employee, but

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 effect of the information treatments are practically the same as the main estimates.

Lastly, in Column 5, we check whether assignment to different days in the week affects take-up differentially. We exploit the fact that vaccination days are randomly assigned, and we regress our indicator of vaccination take-up on dummies for the assigned day (*Wednesday*, *Thursday*, *Friday,* or *Saturday*), using Quito's subsample.[17] These estimates show that take-up on *Thursday* and *Friday* is not statistically different from take-up on *Wednesday*, while the effect of *Saturday* is substantially larger in magnitude and very close to the baseline estimate in Column 1.[18] These results do not support time-inconsistent preferences that would induce procrastination as the mechanism behind the *Saturday* effect and are consistent with increasing opportunity costs.[19]

4.2 Further Evidence on Opportunity Costs

We analyze heterogeneous treatment effects across different subgroups of our study population, which may yield further evidence that opportunity costs are driving the difference in take-up between being assigned to vaccinate on the workweek and Saturday.[20] We focus on differences across gender, distance to work, and employees with and without children.[21] Figure 1 shows that assignment to *Saturday* has more substantial effects for men than for women, although the difference is not statistically significant.

--- Figure 1 about here ---

they failed to enforce this requirement on Saturday and were unable to send employees back home if they showed up that day. In contrast, nobody of those assigned to *Saturday* got vaccinated during the workweek.

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

[18] 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 6-hour workday instead of an 8-hour workday.

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

[20] We find that the information treatments have no differential effect across subgroups. These estimates are small and statistically insignificant. See Appendix Table A3.

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

Distance to work reflects the transportation costs that an individual regularly incurs. The median employee lives 6.5 km away from work. Figure 1 shows that those who live further away than the median are slightly less likely to get vaccinated when they were assigned to *Saturday* than those who live closer to the bank, but this difference is not statistically significant. This result is consistent with increasing travel costs, but the magnitude suggests that travel costs are not the main factor driving the difference in take-up between employees assigned to the workweek and *Saturday*.

Finally, we consider differences in the effect between employees with and without children. Having children may imply higher opportunity costs at the weekend by increased family obligations. Figure 1 shows that assignment to *Saturday* decreased take-up by 10.6 percentage points for employees with children, while the effect is smaller (5.3 percentage points) and insignificant for employees without children. Although the difference between these two effects is not significant, its magnitude is consistent with the idea that opportunity costs increase for individuals assigned to *Saturday.*

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

4.3 Peer Effects on Vaccination Take-up

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

The exogenous variation in take-up created by assigning individuals to get vaccinated in the workweek allows us to estimate peer effects in vaccination. The bank's units define the social groups of employees that work directly together. Thus, we can identify the effect of social groups with whom adults share a large portion of their daily time on vaccine take-up. We will also use this approach to analyze peer effects in health caused by vaccinated peers below (Section 5).

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

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

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

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

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

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

In our design, the exclusion restriction holds because the proportion of peers that got vaccinated is the only channel through which the proportion of peers assigned to the workweek can affect the individual's vaccination decision. Hence, we can combine the estimates from equations (3) and (4) to obtain an IV estimate of the effect of the proportion vaccinated peers on the employee's take-up. The error term in equation (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 ---

Panel A in Table 3 presents the main results. The first stage estimate in Column 1 indicates that a ten-percentage-point increase in the proportion of peers assigned to the workweek increased the proportion of peers that get vaccinated by 3.1 percentage points. The effective F-statistic of Montiel Oleas and Pflueger is 16.48, so 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 has a positive effect on individual take-up and that not accounting for endogeneity biases the effect downwards. The IV estimate in Column 4 indicates that a ten percentage points increase in the proportion of peers that get vaccinated increased take-up by 7.9 percentage points. The results are robust to controlling for the total number of employees in the unit and mean age and gender of the peers (Appendix Table A4).[22]

--- Table 4 about here ---

Two potential mechanisms behind the positive peer effect on individual take-up are peers changing personal beliefs about vaccination or individuals following behavior that they deem socially acceptable. To disentangle these potential channels, we first explore if peers might be changing personal beliefs. We exploit the post-intervention survey questions on beliefs and knowledge of flu vaccines and interactions with coworkers related to vaccination. Even though answering the post-intervention survey is not correlated with treatment assignment (Table 1), the first stage loses precision due to the smaller sample size in the survey. We focus on reduced-form analyses to prevent issues with finite sample bias in the IV estimate. Panel A in Table 4 shows the results on a set of 12 outcomes. Of these outcomes, the proportion of peers assigned to the

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

workweek had a negative and significant effect only on talking with coworkers about vaccination.[23] Employees assigned to the workweek are less likely to mention this to their coworkers because usually, events organized by the bank take place during the workweek.[24] There is no significant effect on any of the questions regarding information or beliefs about the vaccine or on questions that measure how much coworkers influenced the vaccination decision. Moreover, the point estimates are small compared to the baselines, which suggests that peer behavior did neither affect beliefs nor supplied new information about the vaccine.

--- Table 5 about here ---

As another mechanism of the positive peer effects on vaccination take-up, we test if employees following behavior that they deem socially acceptable. Akerlof and Kranton (2000) show that identity-related behavior, i.e., conforming with the prescriptions or norms of a group, can rationalize a series of behaviors by bolstering a sense of belonging or preventing a loss of image in the group. This creates a channel through which an individual's actions can cause responses from others. To test this mechanism, we estimate how the behavior of different subsets of peers affects individual vaccination, following the idea that groups create feelings of belonging that could affect behavior (Akerlof and Kranton, 2000; Hoffman et al., 1996; Perkins, 2002). For instance, Akerlof and Kranton (2000) argue that as every person is assigned a gender, individuals have an incentive to follow the prescriptions of their gender to reaffirm their own identity. A similar behavioral response can appear for other groups to which the individual has a sense of belonging. However, to which group individuals react is an empirical question that depends on the context. Table 5 presents these results.

First, we study if managers influence an individual's vaccination decision. If individuals react to their managers' behavior, this could be in response to an order and not because of the unit's norms. Columns 1 and 2 in Table 5 show that individuals do not follow the behavior of peers in managerial positions, but follow the behavior of peers not in those positions, those who constitute

[23] This effect is robust to adjusting for the false discovery rate as in Anderson (2008).
[24] Additionally, an employee who learns she is in a unit with a large proportion of employees assigned to Saturday might feel lucky that she was assigned to the workweek and get vaccinated. This would bias downwards the estimate of the effect of the proportion of vaccinated peers on take-up in Table 3.

the majority of the working unit.[25] A ten percentage point increase in the proportion of vaccinated peers not in managerial positions increased take-up by 8.9 percentage points.

Second, Columns 3 and 4 in Table 5 show that individuals do not react to the behavior of peers of similar age (within a three-year bandwidth) but react to peers of different ages. A ten percentage point increase in the proportion of peers of different ages who get vaccinated increased take-up by 5.3 percentage points. Given the large age dispersion within the units (Appendix Figure A10), it is unlikely that age groups can create feelings of belonging. Hence, these results are in line with the idea that individuals follow the behavior of the working unit.

Third, we test whether employees follow behavior they deem socially acceptable from their gender. Column 5 shows that a ten-percentage point increase in the proportion of peers of the same gender who get vaccinated increased take-up by 7.6 percentage points. This effect is almost identical to the main estimate and is driven by men. Column 6 shows that the effect of peers of a different gender is 37% smaller and is not significant. These results indicate that the behavior of gender groups influence individual actions, which is consistent with previous research.

Finally, we check if extrinsically motivated employees are more likely to be affected by peer behavior. Intuitively, intrinsically motivated individuals like doing their job and are more likely to work hard. These employees do not need extrinsic incentives from their employers to be motivated to comply. In contrast, the extrinsically motivated employees respond to external stimuli from their surrounding environment, which implies that they should be more likely to follow peer behavior. The pre-intervention survey has questions to determine if employees are intrinsically or extrinsically motivated.[26] Panel B in Table 4 shows the reduced form effect of the proportion of peers assigned to the workweek on these subgroups. For extrinsically motivated employees, a ten-percentage point increase in the proportion of peers assigned to the workweek would increase take-up by five percentage points, while intrinsically motivated employees' take-up would increase by only 2.2 percentage points. Together, these pieces of evidence indicate that the estimated peer effects are a consequence of individuals conforming to the norms of their workgroup.

[25] Managerial positions include supervisors, line bosses, assistant managers, and managers.

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

5. Analysis of the Effects of Vaccination on Health and Risky Behavior

In this section, we exploit random assignment to get vaccinated in the workweek as an instrument to study if flu vaccination improves health and thereby reduced sickness absence in our intervention. In order to shed light on one of the potential mechanisms underlying these results, we use the same approach to explore if getting vaccinated can induce health-threatening behaviors, i.e., moral hazard.

5.1 Effects of Flu Vaccination on Health and Absence

Flu vaccines may affect health through multiple avenues, direct and indirect. First and foremost, getting vaccinated could have a direct effect on health by increasing immunity against four strands of the flu virus. Besides, the results in the previous section show that if a person gets vaccinated, the likelihood that her peers get vaccinated increases. This effect would imply that an employee's peers are more protected against the flu, which may decrease the transmission rate of the disease. Thus, positive peer effects on vaccination take-up could create an indirect channel through which getting vaccinated might have a positive effect on health. The proportion of vaccinated peers within the 142 units in the firm varies substantially between 0 and 67%, which indirectly could play a role in health outcomes.[27] Ideally, we could estimate the effect of flu immunization on health-related outcomes (Y_{ijc}), such as medical diagnoses and sick days, through these two channels as:

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

However, vaccination take-up and the proportion of peers who get vaccinated are potentially endogenous. For example, individuals with healthier lifestyles could be more likely to vaccinate and less likely to need a sick day, so the estimates of equation (5) by OLS would be biased downwards. This speaks for instrumenting 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

[27] Figure A11 displays the number of employees by unit. The CDC and WHO indicate that vaccination rates over 75% grant herd immunity.

proportion of peers assigned to the workweek. The unadjusted first stage equations have F-statistics of 6.6 and 8.9, respectively, implying that IV estimates of equation (5) may have a problem of finite sample bias.[28] Thus, we focus on the valid reduced form estimates of regressing the health outcomes on the instruments.

--- Table 6 about here ---

Table 6 presents the effects of flu vaccination on the probability of being diagnosed sick for any reason between November 2017 and February 2018. The OLS estimate in Column 1 suggests that getting vaccinated decreased the probability of being diagnosed as sick by 0.7 percentage points (1.4% of the baseline), although the effect is insignificant. The reduced form estimates in Column 2 imply that getting vaccinated did not affect the probability of being diagnosed sick. Being randomly assigned to the workweek – which increases vaccination take-up – decreased the probability of sickness by 1.5 percentage points (3.4% of the baseline), which is insignificant at conventional levels. Additionally, the results in columns 1 and 2 indicate that the proportion of vaccinated peers does not affect the probability of being diagnosed sick.

--- Table 7 about here ---

Table 7 shows the effects of flu vaccination on the probability of having a sick day. The OLS correlation suggests that vaccination decreased the probability of having a sick day by 4.1 percentage points, but this effect is not significant. Conversely, the reduced form estimates in Column 2 imply that getting vaccinated did not affect the probability of having a sick day. Being randomly assigned to the workweek, which increases vaccination take-up, increased the probability of having a sick day by 1.3 percentage points (5% of the baseline), which is insignificant at conventional levels. From an overall perspective of the firm, the results suggest that the investment in the health campaign was not worthwhile.[29]

[28] The results of Montiel Oleas and Pflueger (2013) only apply for the case of one endogenous variable.

[29] We reach the same conclusion based on findings for the number of sick days. Note that sick diagnoses include severe illnesses, such as cancer, which leads to large numbers of sick days not related to the flu. If we exclude outliers with more than 100 sick days, the coefficient of the reduced-form is insignificantly positive, in line with our finding

--- Table 8 about here ---

There are many diseases over which the flu vaccine has no immunity benefit. Hence, we exploit the data on medical diagnoses and estimate the effect of vaccination on the probability of being diagnosed with the flu (Table 8). The OLS estimates in Column 1 suggest that getting vaccinated decreases the probability of being diagnosed with the flu. However, the reduced form estimate in Column 2 indicates that being assigned to the workweek increased the probability of being diagnosed with the flu by 0.4 percentage points (9% of the baseline), not significant at conventional levels. This result further suggests that getting vaccinated was ineffective in decreasing the probability of having the flu. Also, the estimates in columns 1 and 2 show that the proportion of vaccinated peers do not affect the probability of being diagnosed with the flu, which suggests that vaccination rates are too low to provide herd immunity. Thus, we drop the proportion of vaccinated peers in the following analyses.[30]

To evaluate if the estimates rule out meaningful effects of vaccination, we implement an equivalence test based on two one-sided hypothesis tests (King et al., 2000; Rainey, 2014; Lakens, 2017; Hartman and Hidalgo, 2018). The equivalence test has two parts. First, we have to define what constitutes a meaningful effect of vaccination. This value defines two thresholds to evaluate if the estimates rule out meaningful effects. To define this value, we use flu vaccine effectiveness estimates from CDC data. While these estimates come from observational studies on flu hospitalizations and might be biased, they constitute the criteria 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 if vaccination has a meaningful effect on the probability of being granted a sick day because of the flu.[31]According to the CDC, the 2013-2014

in Table 7on the probability of having a sick day or not. Note also that our results for sickness and sick days do not change if we take out the proportion of peers and estimate only the individual effect of vaccination.

[30] As can be seen in Appendix Table A5, the main result is robust to the inclusion of controls (gender, age, tenure and income) and to using a broader and narrower definition of flu-related illness. Also note that we check the main result by performing a bounding exercise (Appendix Table A6), in which we consider a possible role of vaccinations outside of the firm for our results (see Section 5.3 for details).

[31] The CDC provides 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 who got sick with the flu or not. The CDC further adjusts

vaccine had the highest effectiveness (it reduces hospitalizations by 16 percentage points), the 2014-2015 vaccine had the lowest effectiveness (it reduces hospitalizations by 2.2 percentage points), and the 2017-2018 vaccine's effectiveness during time of the campaign was in between those estimates (it reduces hospitalizations by 8.4 percentage points) for working adults. 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, 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 if the reduced form effect is smaller than each reference value (-1.1, -0.1, -0.6) and higher than the absolute value of the reference values (1.1, 0.1, 0.6). This is equivalent to comparing both the reference values and their respective absolute values to the 90 percent confidence interval of the estimated effect (Rainey, 2014; Lakens, 2017). 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 goes over one of these boundaries, it means we cannot rule out meaningful effects in the direction in which the confidence interval overlaps the boundary.

--- Figure 2 about here ---

Figure 2 presents the comparisons. We can reject the CDC's effectiveness estimate for the best season (2013-2014) and the CDC's estimate for our campaign season (2017-2018). The estimated effect is consistent with the effectiveness of 2014-2015, the worst season for which there are data available. These results imply that we can safely rule out meaningful health benefits of the flu vaccination based on public health figures provided to policymakers from this intervention. However, the confidence interval in Figure 2 does not rule out potentially large positive values, which would suggest that getting vaccinated might increase illness. We study this potential issue in the next section.

these estimates controlling for demographic characteristics that affect natural immunity to the flu resulting in larger estimates, so the reported percentages are a conservative lower bound of the CDC estimates.

5.2 Can Getting Vaccinated Cause Moral Hazard?

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

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

To explore if flu vaccination may cause moral hazard, we test if getting vaccinated induces different reactions than being unvaccinated when flu-like symptoms appear. The idea 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 being diagnosed sick with respiratory disease. In particular, if vaccinated employees felt more protected, they might have been less likely to go to the doctor when they felt flu-like symptoms, decreasing the probability of being diagnosed with a non-flu disease. In particular, this would concern cases of mild illnesses where it is up to the individual to decide to go to a doctor or not.

To implement this test, we exploit the richness of the data on medical diagnoses that allows us to identify cases of non-flu respiratory illnesses. We exploit a policy intervention of the Ecuadorian government that happened in our investigation period. In January 2018, Ecuador experienced a significant increment of flu cases nationwide (Direccion Nacional de Vigilancia Epidemiologica, 2018). As a result, the Ecuadorian government launched a massive media campaign asking the population to go to the doctor if they felt any flu symptoms. If vaccinated individuals felt protected, we argue that they may not have followed the government's recommendation, resulting in fewer visits to the doctor and fewer non-flu respiratory diagnoses in that month.

--- Figure 3 about here ---

We estimate the reduced-form effects of vaccination by month during our investigation period. Figure 3 presents the effects of being assigned to the workweek on flu and non-flu respiratory diagnoses. As with the cross-section estimates in Table 8, assigning employees to the workweek does not affect the probability of being diagnosed with the flu in any month. The point estimates are smaller than 0.7 percentage points in magnitude and insignificant at conventional levels. These results further confirm that the vaccination campaign was ineffective. Regarding non-flu diagnoses, if vaccination did not induce individuals to feel more protected, we would expect to find no effect on the probability of being diagnosed with a non-flu respiratory disease. This is true in November, December, and February. However, in January, when the government asked individuals to go to the doctor, being assigned to the workweek decreased the probability of being diagnosed with a non-flu respiratory disease by 7.2 percentage points.

We also estimate the effect of assignment to 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 decreased the probability of being diagnosed with a non-flu respiratory disease by 7.7 percentage points (Appendix Table A5), almost identical to the effect in January. This result suggests that employees assigned to the workweek, who were more likely to get vaccinated, felt protected, and went less to the doctor when they felt flu-like symptoms. These estimates are consistent with the hypothesis of riskier behavior among vaccinated individuals, as they appeared to think that they are protected against the flu.

--- Figure 4 about here ---

We can also investigate if vaccination affects the likelihood of going to the doctor at the on-site health center. The bank's on-site health center is a convenient feature for its employees because they do not have to ask for time off to go to the doctor as they can take a few minutes of their work time to go to the health center. Before the intervention, the on-site doctors account for 77 percent of all cases of diagnosed sickness. If vaccinated individuals felt more protected, they may have been less likely to visit these doctors when the government launched its media campaign. Figure 4**Fehler! Verweisquelle konnte nicht gefunden werden.** presents the effects of assigning employees to the workweek on the probability of going to the on-site doctor by month. There was no significant effect in November, December, and February. In January, being assigned to the workweek for vaccination decreased the probability of going to the onsite doctor by 8.6 percentage points (21% of the baseline). This finding is consistent with moral hazard.

--- Table 9 about here ---

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

Table 9 shows the effects of assigning employees to the workweek on these outcomes. Assigning employees to the workweek did not affect how often employees wash their hands (1.2% of the baseline), which is not surprising since almost all employees report that they wash their hands regularly. Assigning employees to the workweek had a negative but statistically insignificant effect on how often 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

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

umbrella is statistically significant. Being assigned to the workweek decreases 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 "all the time."[33] We can also investigate heterogeneous effects across individuals' beliefs on the effectiveness of the vaccine using the pre-intervention survey. We find that the effect is driven by individuals who believe the vaccine is very useful in preventing the flu (Appendix Table A7). Thus, this result suggests that vaccinated individuals feel protected, so they neglect other measures that they believe to be helpful in order to prevent respiratory illnesses.

5.3 Other Interpretations of the Results on Moral Hazard

In the previous section, we provide several pieces of evidence supporting the idea that flu vaccination caused a moral hazard problem. In the following, we discuss whether other interpretations of these findings not related to moral hazard could explain the results. Misdiagnoses could be a competing explanation. If doctors are not able to distinguish the flu from other non-flu respiratory diseases, then some of the non-flu cases could have been flu cases. However, as we observe diagnoses from 72 different doctors from different health centers and hospitals, it is unlikely that doctors systematically misdiagnosed the flu. Also, the results are robust to using a broader definition of flu-related illness. Finally, misdiagnoses do not explain why vaccinated individuals report being less likely to carry an umbrella as a cultural protective measure against the flu.

We could also think that doctors misdiagnose conditional on whether a person got vaccinated or not. When a doctor learns that a person who shows flu-like symptoms got vaccinated, the doctor might be more likely to misdiagnose those symptoms as a non-flu respiratory disease. However, the results in Figure 3 show that employees assigned to the workweek, who are more likely to get vaccinated, were diagnosed less with non-flu respiratory diseases.

Another potential concern is the fact that the data on medical diagnoses correspond to employees who went to the onsite doctor or an external doctor during working hours, while employees who went to an external doctor outside working hours, who were diagnosed sick but were not granted a sick day, are coded as healthy. This measurement error will not bias the flu and

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

non-flu estimates as long as it is uncorrelated with the assignment to the workweek. However, if employees assigned to get vaccinated during the workweek are more likely to go to an external doctor after work, then this would overestimate the effect on non-flu respiratory diagnoses. We bound the effect to address this potential concern (Lee, 2009). First, we calculate the treatment-control difference in the proportion of healthy individuals. Then, we trim this difference from the control group (assigned to vaccination on Saturday) to obtain an upper bound, and we trim this difference from the treatment group (assigned to vaccination on the workweek) to obtain a lower bound. The effect of being assigned to the workweek on the probability of being diagnosed with a non-flu respiratory is always negative and bounded between 5.4 and 9.8 percentage points (Appendix Table A6).

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

6. Conclusions

Individual behavior may threaten the success of health interventions in multiple ways. First and foremost, individuals can decide not to participate. In this paper, we find that a small price change, as well as information nudges that appeal to the selfish or altruistic benefits of vaccination, does not induce a change in behavior. In contrast, reducing opportunity costs has a substantial effect on participation in a vaccination campaign for working-age employees. Additionally, peers are an important factor that influences vaccination in the workplace. Regarding the health benefits of the intervention, flu vaccination did not have a significant effect on any of our outcomes. While we cannot rule out that the flu vaccine was medically ineffective, we find evidence consistent with moral hazard, i.e., individuals adopting riskier behaviors after getting vaccinated. Moral hazard constitutes a second way through which individual behavior can limit the effectiveness of health interventions.

To answer the question of whether the vaccination campaign was economically beneficial for the company carrying out this health intervention, we can perform a back-of-the-envelope calculation of the net benefit of this campaign. This analysis has the limitation that we are not able to fully quantify all of the possible effects that vaccination may have on outcomes relevant to the bank, like morale and productivity.[34] Our calculation suggests that the net benefit of the campaign was negative regarding sick days. In the best-case scenario, the treatment may result in a net gain of \$0.17 regarding gains in work attendance during the flu season, which is not enough to compensate the bank for its costs that include vaccine subsidies of \$2.57, \$5.05 and \$9.99 per vaccine.[35]

Our study allows us to draw multiple practical implications for health interventions. From a research perspective, it is useful to employ a randomized encouragement design to circumvent any ethical dilemma when studying the consequences of interventions relevant to people's health. It allows not only the study of potential health benefits and but also of behavioral changes in an unbiased way. The presence of moral hazard in health-related behavior implies that firms and policymakers should consider this phenomenon in the design of interventions like vaccination campaigns. A promising mechanism to mitigate it could be to increase awareness that the proposed measure, such as flu vaccination, does not guarantee a 100% protection against illnesses. It might be necessary to remind individuals to continue making use of other protective measures against respiratory viruses and bacteria, instead of letting them rely only on the protection potentially provided by medical technology.

[34] A channel pertaining to company morale is the perception of individuals that the company cares more about their health when assigned to the work-week which leads them to behave differently. However, we cannot find evidence for that channel using data on organizational perceptions from our post-survey. Appendix Table A8 presents imprecise estimates on self-reported productivity and the duration of the workday measured by the employees' magnetic cards swipes to enter and exit the bank. Albeit statistically insignificant, the point estimates suggest that assigning employees to get vaccinated in the workweek increased their perception on their productivity, while decreased the duration of their workday by about a third of an hour. Given that the bank pays a fixed salary, this could suggest an increase in productivity. However, in the absence of more precise measures of productivity, we cautiously conclude from this analysis that there is no sizable productivity premium. One could argue that from the perspective of a company, sick days have higher economic relevance, given that they often go along with re-assignment of tasks, compared to when some employees are able to finish tasks and leave earlier than others.

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

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

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Table 1 Summary Statistics

	Full Sample	Control	Altruistic	Selfish	Saturday	F-test (p-value)
Monthly Income ($)	1,766	1,860	1,701	1,681	1,827	0.316
Company Tenure (years)	7.9	8.3	7.7	8.1	7.5	0.761
Prop. Women	0.49	0.51	0.52	0.46	0.47	0.497
Age (year)	36.6	37.2	36.4	36.6	35.7	0.553
Prop. College Education	0.91	0.92	0.91	0.90	0.93	0.759
Pre Survey Participation	0.48	0.50	0.50	0.47	0.40	0.171
Post Survey Participation	0.36	0.36	0.38	0.33	0.35	0.519
Diagnosed Sick	0.66	0.67	0.67	0.64	0.67	0.835
Granted a Sick Day	0.37	0.37	0.40	0.37	0.34	0.797
Diagnosed Flu Sick	0.11	0.09	0.13	0.13	0.10	0.348
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
Altruistic Information	-0.0260	-0.0209	-0.0493	-0.0262	
	(0.0310)	(0.0303)	(0.0332)	(0.0306)	
Selfish Information	-0.0032	-0.0011	-0.013	-0.0103	
	(0.0314)	(0.0316)	(0.0339)	(0.0308)	
Thursday					0.0002
					(0.0346)
Friday					-0.0356
					(0.0331)
Saturday	-0.0789***	-0.0791***	-0.0898***	-0.0671**	-0.0818***
	(0.0301)	(0.0304)	(0.0313)	(0.0298)	(0.0315)
Average take-up base group in Quito		0.1732		0.1623	0.1651
N	1164	1164	929	1152	929

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

Notes: Robust standard errors in parentheses. This table presents OLS estimates of the effect of the different treatments on vaccination take-up. All specifications control for 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. Column 3 presents the estimates using only employees in Quito, the city where we implemented our four treatments. In Column 4, we exclude 12 individuals who were assigned to vaccinate in the workweek but went to vaccinate on Saturday. In Column 5, we test for different effects across the different days of the week using only data from Quito that has all the treatments. Using clustered standard errors at the work unit level (142 clusters) yields similar standard errors with no loss of statistical significance. For Columns 1-3, we define the base group as the Control group in Quito. For Column 4, it is the same group but adjusting the sample for non-compliance, and for Column 5, it is the take-up rate on Wednesday in Quito.

Table 3 Effect of Peer Vaccination on Individual Take-up

	First Stage	Reduced Form	OLS	2SLS
Proportion of Peers:				
Assigned to the Workweek	0.3106***	0.2454***		
	(0.0765)	(0.0844)		
Vaccinated			0.5116***	0.7900***
			(0.0748)	(0.1777)
F-value	16.481			
N	1138	1138	1138	1138

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

Notes: Standard errors clustered at the unit level in parentheses. The bank has 116 units with more than one employee. This table presents the effect of peers' vaccination take-up on the individual's vaccination decision. We measure the proportion of peers vaccinated and the proportion of peers assigned to the workweek in percentage points. Thus, the estimates represent the effect of a one percentage point change in the proportion of peers, and the reported coefficients measure percentage point changes. 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 that get vaccinated. Column 4 presents 2SLS estimates of the effect of a change in the proportion of peers that get vaccinated.

Table 4 Potential Mechanisms for Peer Effects

	Effect of Prop. of Peers Assigned to the Workweek on	Baseline	N
a. Beliefs about the Flu, its Vaccine, and Interactions with Coworkers			
Vaccines Effective to Improve Health (1-5)	-0.0015	3.74	378
	(0.0049)		
Talked with coworkers about getting vaccinated (pp)	-0.0065***	0.56	359
	(0.0021)		
Went with coworkers to get vaccinated (pp)	0.0010	0.13	359
	(0.0014)		
Probability of Getting Healthy Without the Vaccine (0-100)	-0.0054	44.25	366
	(0.0712)		
Probability of Getting Healthy With the Vaccine (0-100)	0.0300	56.48	366
	(0.0911)		
Informed about the Flu (0-100)	0.0104	69.80	371
	(0.0721)		
Informed about the Flu Vaccine (0-100)	0.0112	63.70	371
	(0.0970)		
Afraid of the Flu (0-100)	0.0457	37.20	371
	(0.1231)		
Afraid of the Flu Vaccine (0-100)	0.0935	24.66	371
	(0.1178)		
Would Get Vaccinated out of the Workplace (pp)	-0.0024	0.61	366
	(0.0020)		
Coworkers Convinced me to get Vaccinated (0-100)	0.0399	20.60	359
	(0.1221)		
I Convinced my Coworkers to get Vaccinated (0-100)	-0.0537	28.37	359
	(0.1329)		
b. Heterogeneous Effects for Extrinsically and Intrinsically Motivated Individuals			
Vaccination of Extrinsically Motivated Individuals (pp)	0.4961***	0.13	247
	(0.1271)		
Vaccination of Intrinsically Motivated Individuals (pp)	0.2240	0.16	262
	(0.1672)		

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the reduced form effect of peers assigned to the workweek on a series of outcomes identified by the row headers. The measurement unit of each outcome is in parentheses next to the outcome's name. 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 5 Heterogeneous Peer Effects on Individual Take-up

	Proportion of Peers Vaccinated:					
	In Managerial Positions	Not in Managerial Positions	Similar Age	Different Age	Same Gender	Different Gender
First stage	0.1710*	0.3058***	0.0859***	0.2504***	0.2222***	0.2136***
	(0.0885)	(0.0789)	(0.0225)	(0.0472)	(0.0570)	(0.0696)
	[3.7302]	[15.0272]	[14.6127]	[28.1757]	[15.1879]	[9.4195]
Reduced form	0.0184	0.2746***	-0.0084	0.1336**	0.1700***	0.1026*
	(0.0525)	(0.0749)	(0.0606)	(0.0611)	(0.0635)	(0.0607)
2SLS	0.1075	0.8928***	-0.0974	0.5298***	0.7603***	0.4780*
	(0.2904)	(0.1552)	(0.7153)	(0.2030)	(0.1906)	(0.2473)
N	982	1082	1138	1138	1101	1030

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

Notes: Standard errors clustered at the unit level in parentheses, first stage F-values in brackets. The bank has 116 units with more than one employee. This table presents the heterogeneous effects of different types of peer vaccination take-up on the individual's vaccination decision. The column headers identify the type of peer, for example, in the first column, we present the effect of the proportion of peers of the same gender who got vaccinated on individual vaccination. We measure the proportion of peers vaccinated and the proportion of peers assigned to the workweek in percentage points. Thus, the estimates represent the effect of a one percentage point change in the proportion of peers. The first row presents the first stage results; for example, the fifth cell indicates that if the proportion of peers of the same gender assigned to the workweek increased by one percentage point, then the proportion of peers of the same gender who got vaccinated increases by 0.22 percentage points. The second row presents reduced form results. For example, for the fifth column, if the proportion of peers of the same gender assigned to the workweek increased by one percentage point, then individual take-up increases by 0.17 percentage points. The third row presents 2SLS estimates. For example, for the fifth column, if the proportion of peers of the same gender assigned who got vaccinated increases by one percentage point then individual take-up increases by 0.76 percentage points. All specifications control for Quito fixed effects and individual assignments to the workweek. Sample size changes because not everybody has a peer in the same category. For example, if a manager has no manager peers in her unit, then that observation is dropped.

Table 6 Effects of Vaccination on Overall Sickness

	OLS	Reduced Form
Assigned to the workweek		-0.0166
		(0.0358)
Prop. peers assigned to the workweek		-0.00048
		(0.00110)
Vaccinated	-0.0068	
	(0.0324)	
Prop. peers vaccinated	0.00003	
	(0.00094)	
Average for unvaccinated in Quito (p.p.)	0.47	
N	1120	

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the effects on the probability of being diagnosed sick in general. 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 7 Effects of Vaccination on Overall Sick Days

	OLS	Reduced Form
Assigned to the workweek		0.0123
		(0.0361)
Prop. peers assigned to the workweek		-0.00006
		(0.00101)
Vaccinated	-0.0407	
	(0.0298)	
Prop. peers vaccinated	0.00042	
	(0.00094)	
Average for unvaccinated in Quito (p.p.)	0.2808	
N	1120	

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the effect on the probability of having a sick day. 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 8 Effects of Vaccination on Flu Diagnoses

	OLS	Reduced Form
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	1120	

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the effects of flu vaccination on the probability of being diagnosed sick 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.

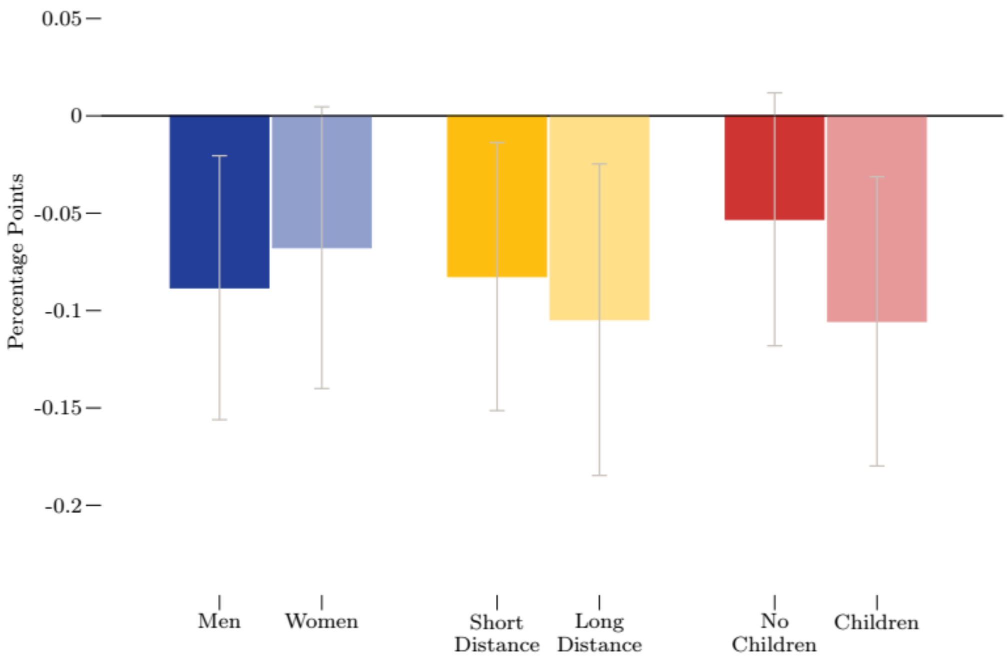
Table 9 Reduced Form Estimates on Health-Related Habits

	Baseline	Coefficient	N
Responses on a scale from 1 (“never”) to 10 (“all the time”)			
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)	

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

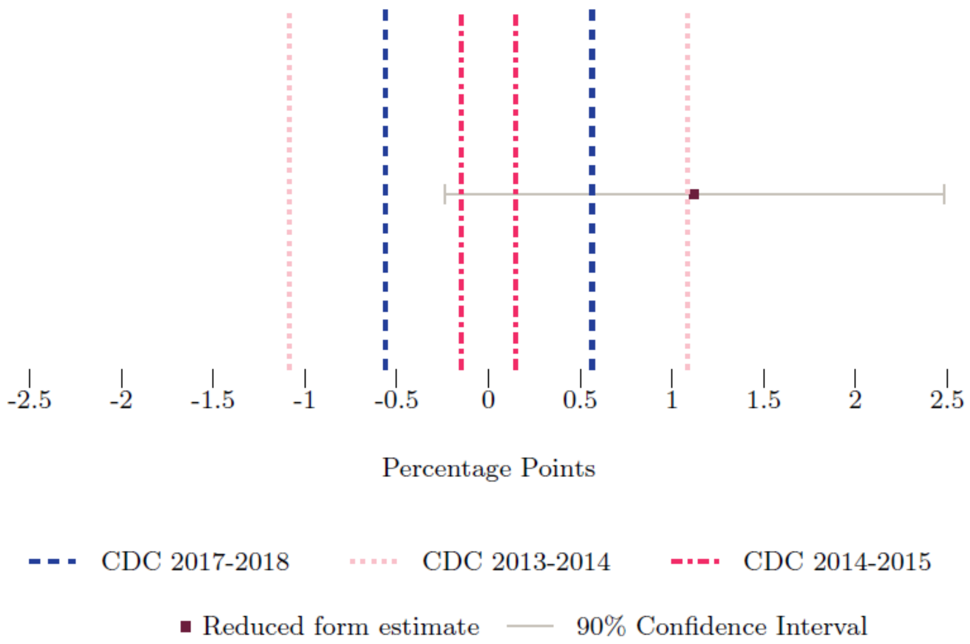
Notes: Robust standard errors in parentheses. This table presents the reduced form effects of being assigned to the workweek on four daily habits and activities related to health and preventing the flu. All specifications control for Quito fixed effects. Column 2 presents the reduced form estimates. 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



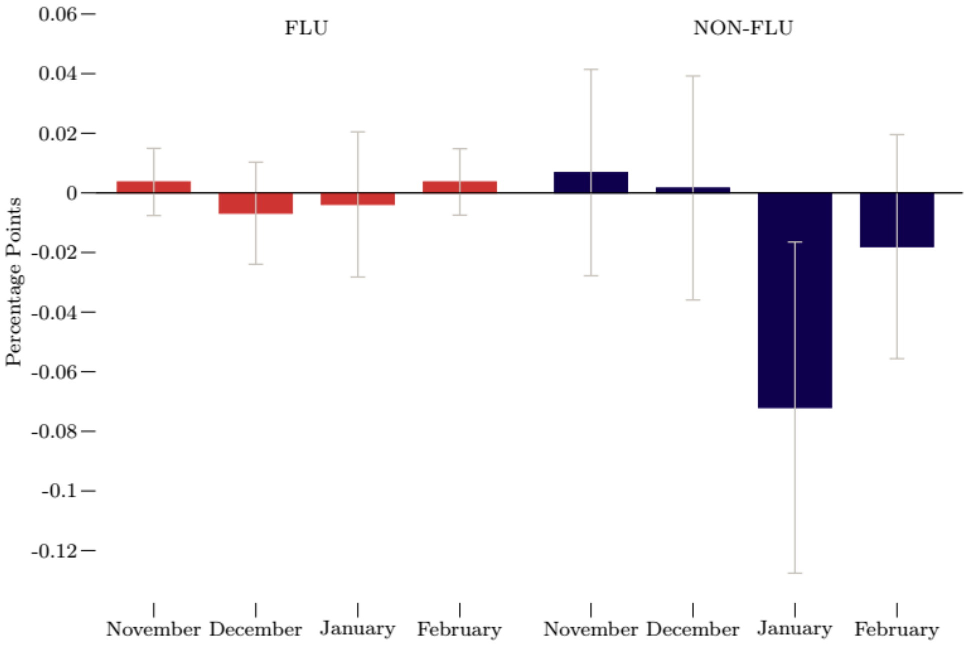
Notes: This figure presents the intent-to-treat effect of assignment to Saturday on vaccination take-up for different subgroups in the sample. All specifications control for city fixed effects. The figure presents the point estimate and the 90% heteroscedastic robust confidence interval for each subgroup.

Figure 2 Equivalence Test for the Effectiveness of Vaccination



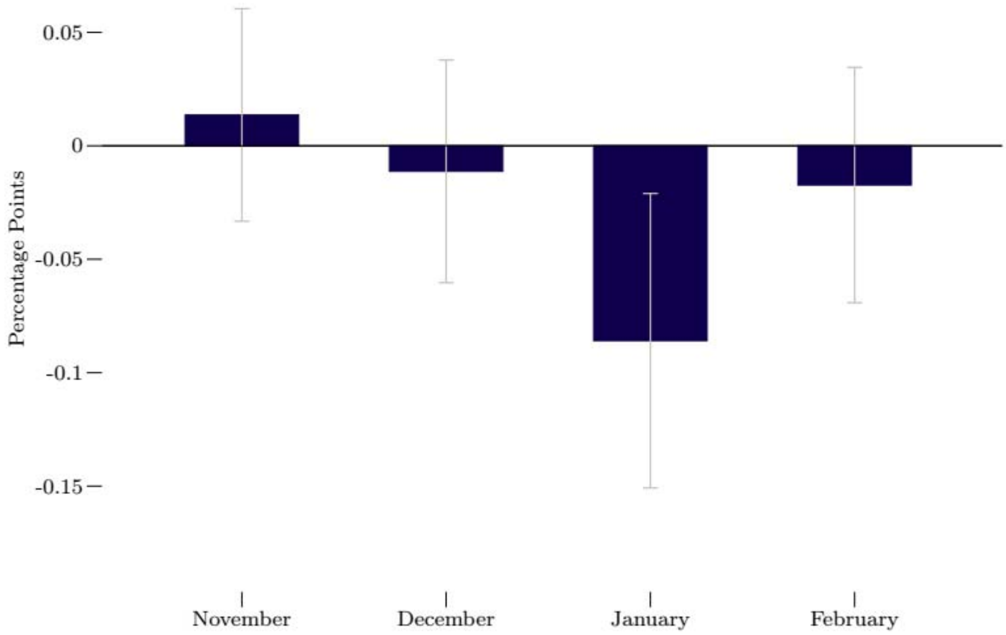
Notes: This figure presents the reduced-form estimate of the effect of assignment to the workweek to adjusted CDC estimates of the effectiveness of the flu vaccine for 2013-2014, 2014-2015, and 2017-2018 seasons.

Figure 3 Reduced Form Estimates of the Effect of Vaccination on Diagnosed Sickness



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

Figure 4 Reduced Form Estimates on the Probability of Going to the Onsite Doctor



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

Online Appendix

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

	Baseline	With Controls	Quito Sample	Non-Compliance
Threshold	0.0590	0.1738	0.0655	0.0400
	(0.0730)	(0.1533)	(0.0786)	(0.0722)
N	608	608	461	604

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

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

Table A2 Recall Information Statements

	Heard Altruistic Statement	Heard Selfish Statement
Altruistic Information	-1.2079	-8.4337**
	(4.9521)	(4.1692)
Selfish Information	-3.8421	-0.0181
	(4.9557)	(4.0281)
Saturday	-3.5966	-2.5732
	(6.2362)	(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
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 Robustness Check on Peer Effects Estimates

	Unit Size	Peer Characteristics
A. Main Effect		
Proportion of peers:		
Vaccinated	0.7852***	0.7102***
	(0.1836)	(0.1888)
N	1138	1138
B. Heterogeneous Effects		
Proportion of peers:		
Same Gender Vaccinated	0.7538***	0.7242***
	(0.1979)	(0.2031)
Different Gender Vaccinated	0.4779**	0.4302*
	(0.02408)	(0.2457)

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

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

Table A5 Robustness Check on Effects of Vaccination on the Flu

	Narrowest Definition of Flu	Main Definition of Flu	Broadest Definition of Flu
	Reduced Form	Reduced Form	Reduced Form
	a. Baseline specification		
Assigned to the workweek	-0.0054	0.0032	-0.0118
	(0.0156)	(0.0160)	(0.0191)
N	1148	1148	1148
	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 of the flu. 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, and education level, and income.

Table A6 Bounds

	Diagnosed with Flu			Diagnosed with Non-flu		
	Main	Lower Bound	Upper Bound	Main	Lower Bound	Upper Bound
Assigned to the workweek	0.0032	0.0050	0.0024	-0.0777**	-0.1028***	-0.0562
	(0.0160)	(0.0161)	(0.0161)	(0.0363)	(0.0379)	(0.0368)
N	913	898	858	913	898	858

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

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

Table A7 Heterogeneous Effects on Using an Umbrella

	Baseline	Coefficient	N
Responses on a scale from 1 (“never”) to 10 (“all the time”)			
	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. Column 2 presents the reduced form estimates. Column 3 presents the number of individuals who answered the survey.

Table A8 Reduced Form Effects on Productivity

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

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!

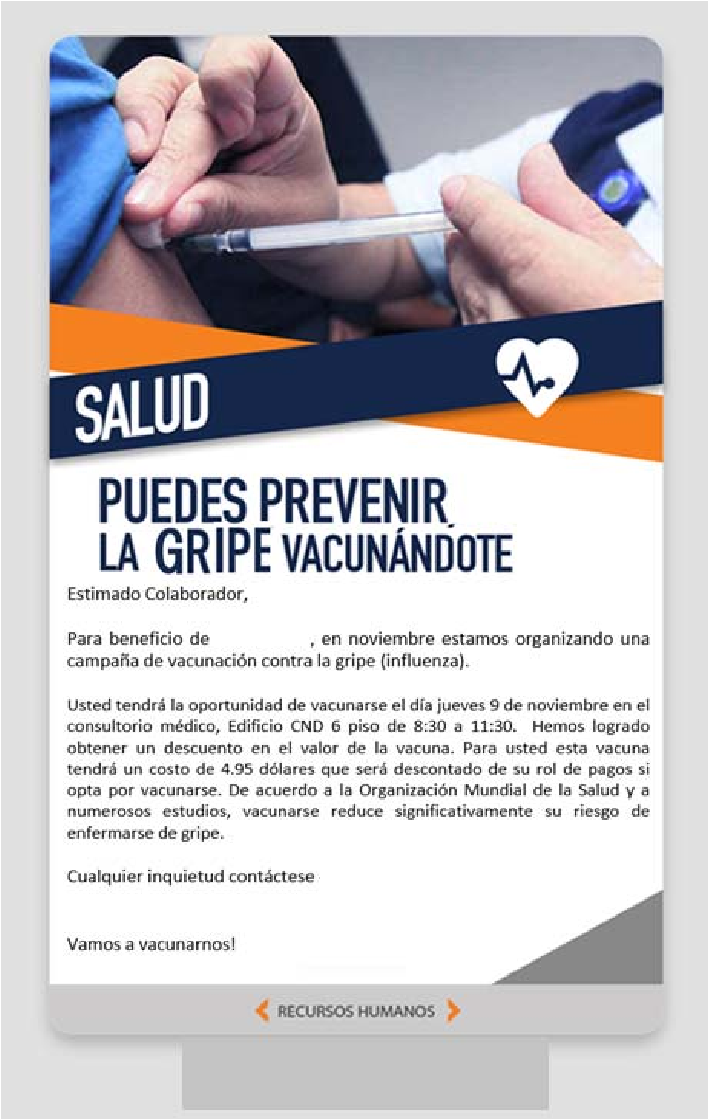
Figure A2 Treatment Message: Opportunity Cost (Saturday)

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 both health officials from the World Health Organization and numerous scientific studies. If you have questions, please contact _____. Let’s get vaccinated!

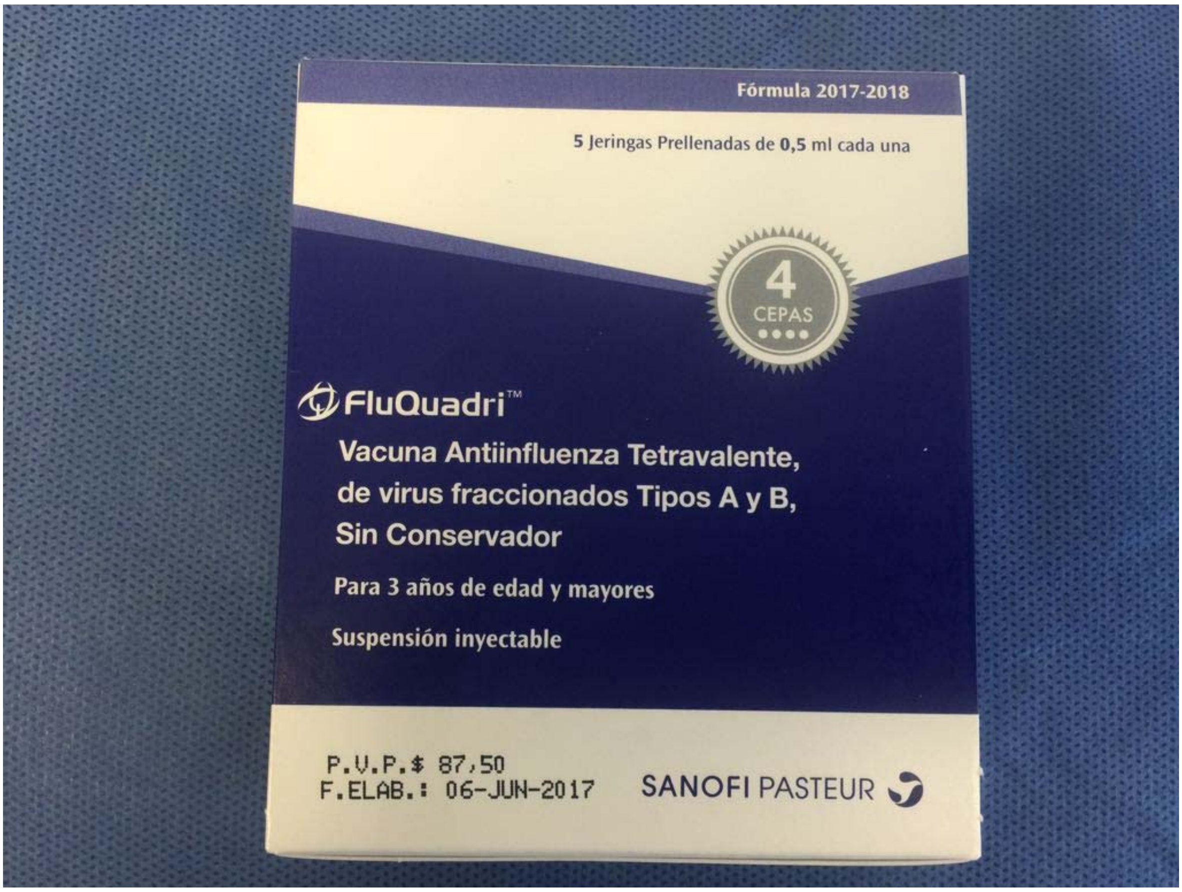
Figure A5 Locations of the Bank in Ecuador

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

Figure A6 Timeline of Experiment Implementation

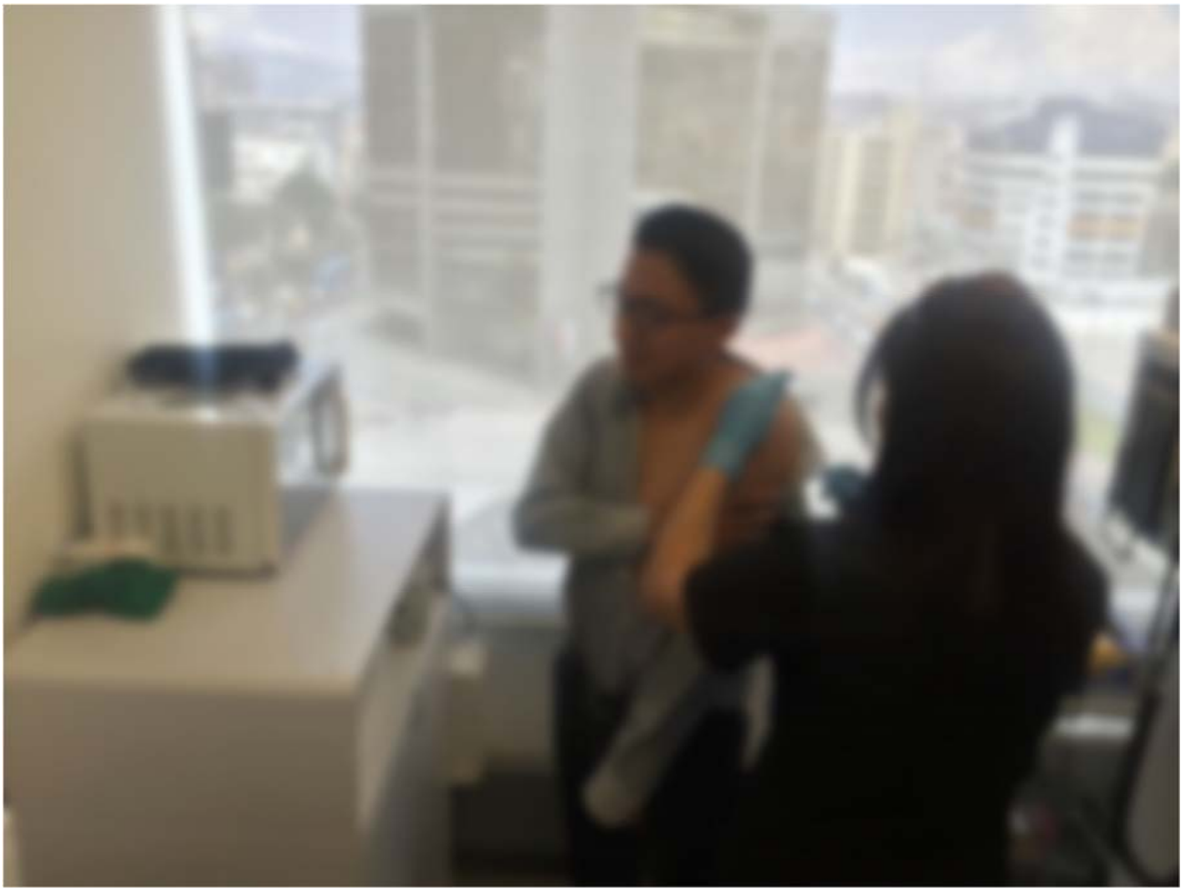
Notes: The bank sent the pre-intervention survey on October 18. The bank sent emails with the different treatments on November 1 using the Human Resources Department mailing account. Furthermore, it sent a reminder on November 7. The vaccination campaign took place between November 8 and November 11.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



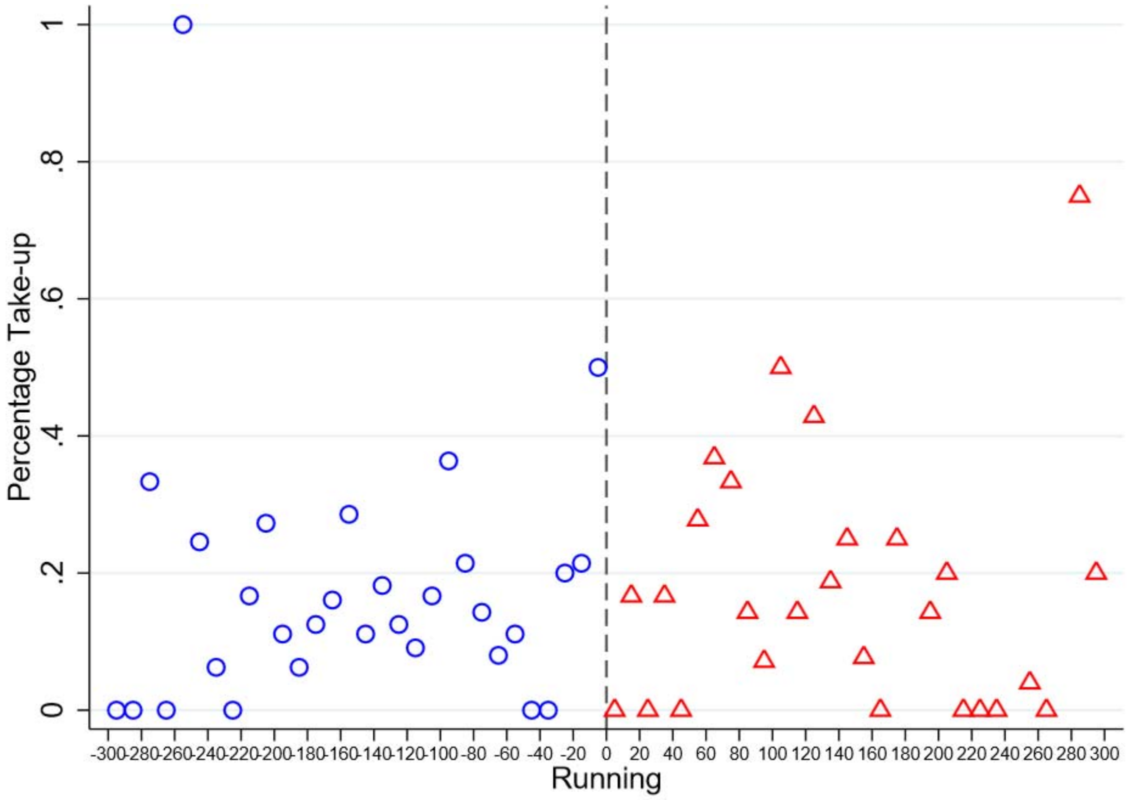
Notes: The above package contains the influenza vaccine used in the campaign. This vaccine protects against four strands of the flu, two from type A and two from type B.

Figure A8 Vaccination Campaign: Flu Shot in Action



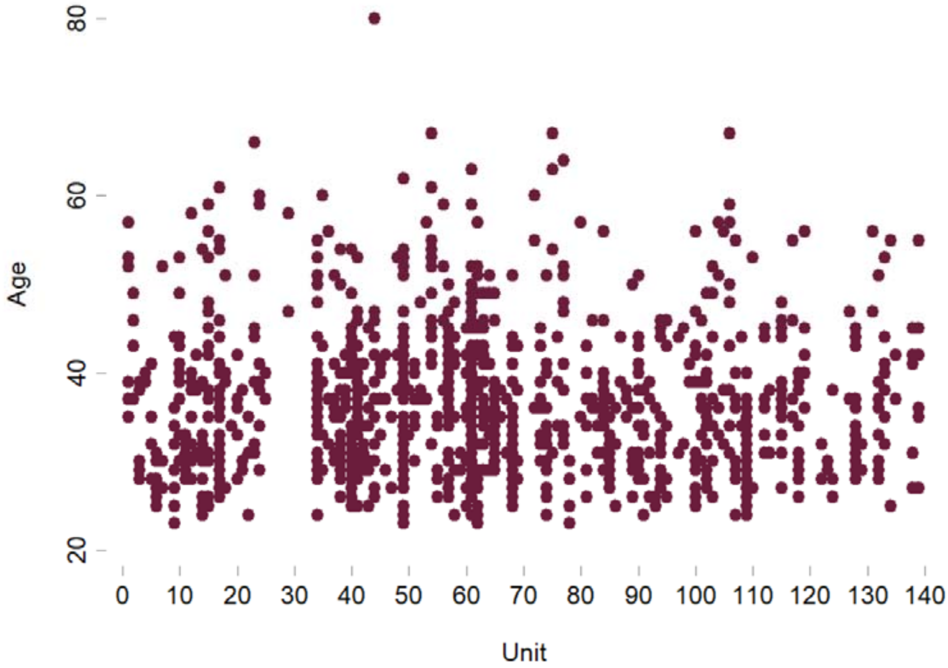
Notes: Immunization at the firm.

Figure A9 Vaccination Take-up around $750 Wage Threshold



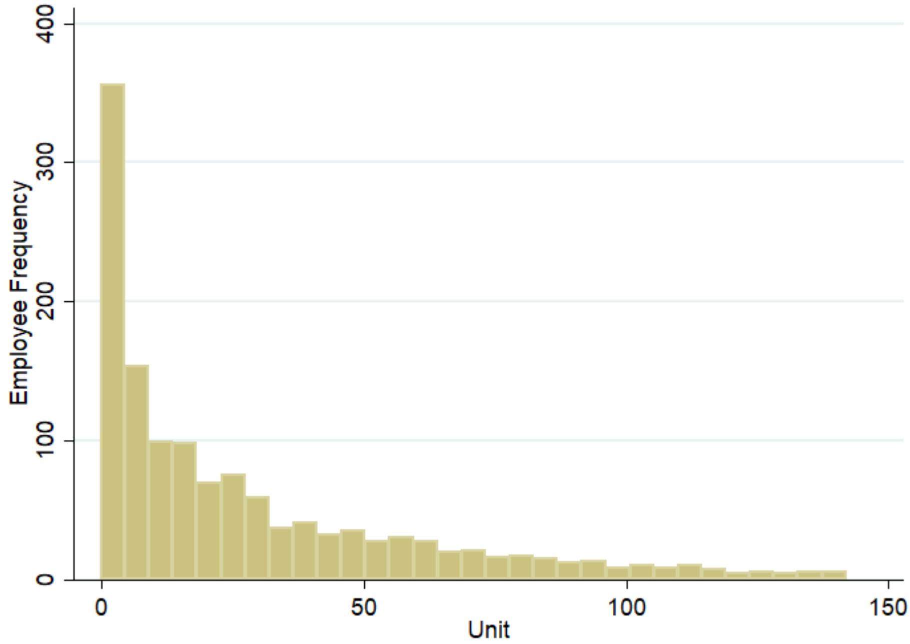
Notes: This figure presents the evolution of vaccine take-up around the $750 threshold with a bin size of $10. Individuals who earn more than $750 paid $7.49 for the vaccine, while employees whose wage is below this threshold paid $4.99.

Figure A10 Distribution of Employees in Units



Notes: This figure presents the distribution of age within each of the company's working units.

Figure A11 Distribution of Employees in Units



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