Vaccines at Work

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Abstract

How can firms reduce employee absenteeism? Health campaigns to get vaccinated could be a cost-effective approach to improve health and reduce sickness-related absence in order to address the negative economic consequences of influenza for firms. Low vaccination rates among employees, however, may reduce these benefits. Moreover, vaccinated individuals could overestimate their level of protection against sickness and engage in behaviors detrimental to their health which then may reduce the benefits of such campaigns. We ran a natural field experiment in cooperation with a bank in Ecuador, where we employed a randomized encouragement design by experimentally manipulating incentives to participate in a health campaign to get vaccinated against the flu. This allows us to study the determinants of vaccination take-up and to understand the consequences for the employees when being randomly encouraged to get vaccinated. We find strong evidence that opportunity costs and peers in the company matter to increase vaccination demand. Contrary to the company's expectation, the health campaign did neither reduce sickness absence nor the incidence of ill-health during the flu season. Using a dataset of administrative records on medical diagnoses and employee surveys, we find evidence consistent with vaccination causing individuals to engage in more risky behaviors concerning their health, which could decrease the effectiveness of the intervention.

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1. Introduction

Company managers have an interest in reducing employee sickness and absence since it is costly to the firm. When employees are on sick leave, they commonly receive compensation while not providing any productive benefit to the firm. A possible way to reduce sickness-related absence of employees is by leveraging health campaigns. In particular preventive health campaigns to get vaccinated against the flu could be a low-cost way with possible externalities to achieve this goal.

The Centers for Disease Control and Prevention (CDC) encourages employers to offer vaccinations against the flu at the workplace (CDC, 2022) given that seasonal influenza (the flu) is oftentimes linked to substantial health problems in the population. 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). In the United States, the flu is associated with an economic burden of approximately $34.7 billion per year, mostly due to loss of life and foregone work (Rothman, 2017), and 16 million days of lost productivity (Molinari et al., 2007). Hence, flu vaccination seems a potentially promising approach to reducing the incidence of the disease and its costs in form of employee absenteeism.

However, individual behavior can counter the potential benefits of vaccination in two ways. First, according to the World Bank, the 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 rates, particularly among working adults (the group least likely to get the vaccine). Second, economic theory and empirical evidence suggest that the adoption of protective technologies may induce individuals to undertake riskier behaviors. Vaccinated individuals may overestimate the protection that the vaccine grants and engage in risky behaviors such as waiting longer before visiting the doctor when they are feeling sick and taking fewer protective measures to prevent illnesses, be it flu-related or not. Such forms of moral hazard could counter the benefits of adopting a preventive medical technology like the flu vaccine.

For this research, we exploited the background of a company's vaccination campaign to present the first comprehensive study of both the determinants and consequences of flu vaccination. In

[1] Public health institutions recommend that every individual over six months of age should be vaccinated against the flu. However, flu vaccination rates in European countries range 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).

cooperation with a bank in Ecuador that provides annual vaccination campaigns to improve its employees' health, we implemented a natural field experiment by randomizing incentives to get a flu shot.[2] Our design introduced three modifications to the bank's 2017 vaccination campaign to create exogenous variation in the vaccination take-up rate. First, we introduced income-dependent subsidies and selected an income threshold at which the price of the vaccine for employees would change. Second, due to capacity constraints, it was necessary to assign employees to be vaccinated either during the workweek or on a Saturday. By randomizing the assignment of employees for vaccination, we could manipulate the opportunity costs of vaccination. Employees would incur additional transportation costs and would need to arrange their weekend schedules to attend a vaccination appointment on a Saturday, while assigning employees to a time during the workweek minimized their opportunity costs because the bank allowed them to take time off their duties to get vaccinated at the firm's location. Third, we varied the content of the invitation emails to appeal to altruistic or selfish motives.

The exogenous variation in vaccination rates generated by these modifications of the campaign allows us to study the consequences of employees 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 both individual vaccination and peer vaccination on employees' health and sickness-related absence. Third, we explore the behavioral implications of getting vaccinated to gauge the possibility of employees engaging in moral hazard behaviors 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 existing studies could be affected by selection and other biases (Demicheli et al., 2014; Demicheli et al., 2018; Jefferson et al., 2010; Osterholm et al., 2012; Østerhus, 2015). 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, this positive selection bias could lead to an overestimation of the health effects of vaccination. Nevertheless, observational studies without randomization of vaccination are often preferred because of ethical concerns regarding randomized controlled trials (RCTs) with placebos

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

in the context of health (Baxter et al., 2010; Sanson-Fisher et al., 2007). For the same reason, RCTs are often conducted using other types of vaccines instead of 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 rates 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., 2020; List et al., 2017). This is an innovative approach in the context of preventive medical technologies to circumvent the ethical dilemma of withholding a potentially effective medical treatment while allowing for gathering causal evidence.

The second challenge we address is capturing the total effect of vaccination. Public health institutions and companies are interested in the total effect of health interventions, encompassing both medical and behavioral responses. However, medical research on vaccines generally focuses solely on the medical effects and does not consider changes in behavior that may affect health. In RCTs, participants know that they are in an experiment, but they do not know whether they have 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, thus capturing both the behavioral and medical effects of getting vaccinated. This allows us to explore whether vaccination induces individuals to adopt riskier behaviors.

The bank's data also allow us to address a third challenge. There is a potential to underestimate the effectiveness of a vaccine due to externalities. Vaccinated peers could encourage co-workers to also get vaccinated, which then may improve their health if the vaccine prevents them from getting sick. Furthermore, peer vaccination could indirectly affect health, even in the absence of individual vaccination, if there are positive health spillovers from the vaccinated to the unvaccinated (White, 2021). While this idea of reduced disease transmission is unlikely to play a role in our setting with flu vaccination rates in Ecuador fluctuating around 2% (ENSANUT, 2012), we are able to empirically assess the role of such externalities by using exogenous variation in peer vaccination across work units.

For a comprehensive analysis of health-related outcomes, we utilize the access granted to the bank's administrative data and merge these with information on treatment assignment at the individual level. The data include detailed medical diagnoses for each employee so that illnesses, including flu diagnoses, and the resulting sick days can be identified. 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.

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

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

Second, we investigate an aspect of the consequences of vaccine take-up that is highly relevant for policymakers and firms that run vaccination campaigns: whether flu vaccination is effective in improving working adults' health, thereby potentially reducing sickness-related absence. If flu vaccination decreases flu cases, we could expect that offering employees the opportunity to get vaccinated during the workweek should reduce the number of flu cases and, accordingly, absence from work. However, our estimates show no evidence that exogenously triggered vaccination decreased sickness in general or sickness-related absence. Furthermore, the data from the medical records indicate that the probability of contracting the flu did not change as a result of 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 for the lack of evidence for health improvements due to vaccination. It could be that we underestimate health benefits because of externalities. However, our results consistently show that peer vaccination does not affect the probability of being diagnosed with an illness or taking a sick day, which implies that health was neither affected by increased take-up through peer effects nor did any health spillovers from the vaccinated to the unvaccinated occur in our setting with overall low vaccination rates. As another explanation, employees may have adopted riskier and thus health-threatening behavior, which could have mitigated the immunity benefit of the vaccine.

We provide evidence from several behavioral tests that is consistent with the notion of individuals adopting riskier behaviors when they have been vaccinated. Vaccinated individuals could overestimate the protection provided by 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 an individual cannot distinguish them unless they visit the doctor. In January 2018, as a result of a significant increase of flu cases nationwide, the Ecuadorian government launched a widespread media campaign encouraging people to visit their doctor if they felt any flu-related symptom whatsoever. If vaccinated individuals felt protected, they would have been less likely to follow the government's recommendation 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 find that assigning individuals to be vaccinated during 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. In line with the idea that vaccinated individuals feel more protected, they might think flu-like symptoms indicate a minor respiratory illness and not heed the government's advice. Second, by the same logic, we explore whether vaccination affected the likelihood of going to the bank's doctor at the on-site health center, assuming that vaccinated employees were less likely to visit the doctor when the government launched its media campaign due to feeling more protected. Our findings confirm that being assigned a vaccination during the workweek decreased the probability of an employee visiting the on-site doctor by 8.6 percentage points in January 2018, with no effect in other months. Finally, by exploring survey data, we find that employees assigned for vaccination during the workweek are more likely to report abandoning practices that are believed to help preventing sickness. Moreover, this is driven by individuals who believe the vaccine is beneficial for flu prevention, which supports the idea of vaccinated individuals feeling protected and engaging in riskier practices.

Our findings contribute to two strands of literature. The first is the literature on the determinants of take-up rates of vaccination and other medical technologies. Previous studies in this area mainly discuss how vaccination take-up rates are affected by laws, information, education, age, health status, health behavior, and lifestyle (Bradford & Mandich, 2015; Chang, 2018; Godinho et al., 2016; Maurer, 2009; Oster, 2018; Schaller et al., 2019; Schmitz & Wuebker, 2011). To date, few studies have considered how compensating for the opportunity costs of vaccination affects vaccine take-up rates among children and vulnerable groups in rural areas in developing countries (Banerjee et al., 2010; Sato & Takasaki, 2018a) or populations with limited income (Bronchetti et al., 2015) by providing in-kind transfers.[3] With regard to 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 friendship groups or neighbors finds mixed results (Bouckaert et al., 2020; Chen & Toxvaerd, 2014; Geoffard & Philipson, 1997; Kremer & Miguel, 2007; Rao et al., 2017; Sato & Takasaki, 2018b).

Our study contributes to this literature in several ways. First, we employ a unique setup that allows for variation in different types of cost. Our results indicate 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 is the case in most major cities in both developing and developed countries. The estimates are of similar magnitude to those of previous studies focusing 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

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

not increase the take-up rate, suggesting that financial incentives must be substantial in order to be effective. Additionally, while some studies find that information nudges can be effective to change health behavior in general (for a recent review, see Siddique et al., 2020), we find them to be ineffective, in line with other studies on vaccine take-up (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.[4] Unlike friends and other non-hierarchical peers, employees cannot choose the individuals they are going to spend time with during work hours after they are hired. We document that this peer group can have a significant positive influence on the adoption of preventive medical technologies such as vaccines.

Furthermore, our study contributes to the literature on the consequences of medical technologies (Alam & Wolff, 2016; Bütikofer et al., 2020; Duflo et al., 2019; Jeon & Pohl, 2019) as well as the literature of on-site health interventions (Milkman et al., 2011; Belot et al., 2016; Just & Price, 2013; List & Samek, 2015; Jones et al., 2021) and the broader literature on public health interventions (Bütikofer & Salvanes, 2020; Cawley, 2010). Our findings on the health effects of vaccine take-up add to an ongoing discussion that predominantly takes place in the medical literature, with some recent exceptions in the economics literature (Ager et al., 2017; Carpenter & Lawler, 2019; Lawler, 2017). With regard to flu vaccines, studies based on quasi-experimental approaches do not show a clear picture as the average health effects are insignificant, while positive effects can be found by focusing on years when the flu vaccine matched well with the prevalent flu viruses (Ward, 2014; Anderson et al., 2020; Carrera et al., 2021; White 2021). Very few medical studies to date have considered the possibility of medical technologies unintentionally causing moral hazard (Prasad & Jena, 2014; Richens et al., 2000), while the few existing papers in economics exploring moral hazard in the context of medical interventions present mixed results (Doleac & Mukjerjee, 2018; Klick & Stratmann, 2007; Margolis et al., 2014; Moghtaderi & Dor, 2021).[5]

The present study contributes to this literature in several ways. First, we employ a novel design

[4] In contrast, there is a large body of research regarding the influence of co-workers on the productivity of their peers in the workplace (Bandiera et al. 2010; Herbst and Mas, 2015; Mas and Moretti, 2009).

[5] There is a large body of literature concerning whether the adoption of safety devices leads individuals to adopt riskier practices (Auld, 2003; Cohen & Einav, 2003; Klick & Stratmann, 2007; Peltzman, 1975, 2011; Prasad & Jena, 2014; Richens et al., 2000; Talamàs & Vohra, 2020) and a large number of studies investigating moral hazard in insurance, e.g. Einav et al. (2013) and Einav & Finkelstein (2018).

for public health and medical interventions, allowing us to circumvent measurement and ethical problems. In contrast to previous studies in the economics literature on vaccines, our experimental approach does not rely on assumptions, such as randomness in vaccine match quality. At the same time, our natural field experiment also addresses potential issues of RCTs as employed in medical research, including the problem of scrutiny and potential behavioral implications. Thanks to our setting, we can also inspect the possibility of externalities by using plausibly random variation in co-worker vaccination across work units, which indicates that an adult-working age population may not be subject to health spillovers as compared to high-risk groups included in other research (White, 2021). We hope to encourage other researchers to use our methodological approach to obtain causal estimates in the context of health and related outcomes, such as sickness absence (Bütikofer & Skira, 2018; Pichler 2015; Ziebarth & Karlsson, 2010). Second, based on experimental variation, we provide behavioral evidence that getting vaccinated induces individuals to feel protected and thus to forgo preventive practices. This result is consistent with the hypothesis of preventive medical technologies causing moral hazard and with the theoretical model of Talamàs and Vohra (2020). Finally, by showing that preventive medical technologies can trigger riskier behaviors, we offer an explanation as to why health interventions may not always be as successful as expected in improving health outcomes. This finding implies that firms and policymakers should consider behavioral implications when promoting the adoption of preventive medical technologies.

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

2. Experimental Design

We conducted the field experiment in cooperation with a bank in Ecuador. The selected bank focuses on consumer credit and is one of the largest credit card issuers in the country. Its headquarters are in Quito (the capital of Ecuador), and it has six branches across the country with over 1,300 employees in total, distributed across 31 divisions with 142 work units. The bank had previously run small vaccination campaigns; these involved only some employees in crowded areas and were run in the bank's offices during the workweek.[6] In 2017, the bank decided to extend its annual campaign to all its employees, and it allowed us to experimentally modify the campaign to investigate the effects of vaccination and how to increase take-up rates. Our encouragement design requires strong variation in take-up in order to study peer effects and the effects of vaccination on health. 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 get vaccinated. All interventions are orthogonal to each other, allowing us to evaluate them independently.

Given that health insurance does not cover the flu vacciation, the bank decided to provide the vaccine for free to areas of the business that had participated in campaigns in previous years and to partially subsidize it for new participants. Since the company opposes randomized subsidies,

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

we used information on employees' income to allocate this subsidy. The average monthly income in the company is $1,766 and the threshold for the subsidy was chosen to maximize the sample size while passing the density and covariate smoothness checks prior to the intervention. Employees who earned less than $750 per month would pay $4.95 to get vaccinated, while those who earned more than $750 would pay $7.49 (the full price of the vaccine is $9.99). Each employee was informed about the applicable price of the vaccine in their invitation email. This email included basic information about the campaign and informed employees that the payment for the vaccine would be deducted directly from their paycheck if they opted to get vaccinated. The email also contained information on the assigned day and time for their vaccination. Figure A1 shows an example of an invitation to receive a low-price flu shot on a Thursday morning.

To examine the effects of opportunity costs and information, we randomly assigned all employees into one of four groups.[7] 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 at random for each employee.

The first treatment increased the opportunity costs of vaccination by assigning employees to get vaccinated on a *Saturday*. The bank's employees usually do not work during the weekend, so individuals in this group would incur extra transportation costs and have to arrange their schedules to travel to the bank and get vaccinated.[8] Otherwise, this group received the same information as the Control group (see Figure A2). This treatment was only applied in Quito because 82% of the bank's employees work in Quito; all the other branches are substantially smaller, and their employees could all get vaccinated in a single day, which was not possible in the capital.[9] Also, keep in mind that being able to get vaccinated on a Saturday was, despite higher opportunity costs, still a unique opportunity since vaccination during the weekend is not commonly available in Ecuador.

We also implemented two information nudges. We kept the additional messages as unobtrusive

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

[8] Based on data from the employees' magnetic swipe cards that they use to enter company buildings, only 0.4% of the bank's employees work regularly on Saturdays.

[9] Branches in the coastal areas were assigned for vaccinations to be carried out on a Wednesday, and branches in the highlands were assigned to Thursday.

as possible to prevent confounding the effect of information with salience or other behavioral factors. The first nudge highlighted the social benefits of flu immunization (*Altruistic*). In addition to the information provided to the Control group, the email included the following wording: “Getting vaccinated also protects people around you, including those who are more vulnerable to serious flu illness, like infants, young children, the elderly and people with serious health conditions that cannot get vaccinated” (see Figure A3). The second nudge highlighted the individual benefits of flu immunization (*Selfish*). In addition to the information provided to the Control group, the email included the following wording: “Vaccination can significantly reduce your risk of getting sick, according to 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; WHO FluNet, 2021). The bank ran a pre-intervention survey from October 25 to October 29, 2017. The human resources (HR) department sent the intervention emails on November 1, 2017, using its official email account. The employees were not aware that this study was taking place. For them, the campaign was just a regular activity organized by the HR department. Employees are used to receiving emails from HR and, according to the HR manager, they typically read these emails carefully. A reminder was sent out using the same email account a week later. The vaccination campaign ran from November 8 to November 11, 2017, at locations within the bank’s offices in each branch. The bank hired an external medical team to supply and inject the vaccines. Finally, the bank conducted a post-intervention survey during March and April 2018.[10]

3. Data

This section describes the data used in our analyses for assessing how monetary and non-monetary determinants can affect take-up rates and the effects of flu vaccination. First, we were granted access to the firm’s administrative records about its employees, which include information on gender, age, education level, children, tenure, and income as well as medical diagnoses and

[10] 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.

sick days. The records also provide information about the employee's job and their work unit, i.e., their position within the bank's organizational structure. Work units have been predetermined by the company, which were established more than two decades ago. Second, we collected vaccination take-up data from the bank's campaign records. Third, we gathered data from the pre- and post-intervention surveys. These surveys asked employees about their previous illnesses and general health, knowledge and beliefs about vaccination and the flu vaccine, habits related to health, relationships with co-workers, opinions about the campaign, motivation, organizational attachment and work satisfaction, and risk and time preferences.

--- Table 1 about here ---

Table 1 presents the mean characteristics of the bank's employees (Column 1). On average, the employees earn a total monthly income of $1,766. As a reference, in 2017, the national average total monthly income in Ecuador was $479, which implies that the bank's employees are in the three highest deciles of Ecuadorian income distribution (ENEMDU, 2017). The average length of employment with the bank is more than seven years, and the average age of the employees is around 36 years. The company employs roughly the same numbers of men and women, and more than 90% of its employees have at least some college education, which is close to education levels in developed countries. 50 percent of the employees have children. The average distance that employees live away from work is 7.58 km, and a work unit consists of approximately 29 employees. Almost 50 percent of the employees completed the pre-intervention survey, representing a high completion rate compared to previous surveys from HR. The completion rate decreased to 36% for the post-intervention survey.

The administrative data include medical diagnoses and sick days as two measures of health. These measures come from two sources: on-site doctors, and medical certificates from external doctors (72 different physicians in total). It is important to note that Ecuadorian law establishes that employees must present a medical certificate to receive a sick day.[11] Consequently, the on-site doctors report every patient's visit to HR, including the diagnosis (the type of disease), whether they granted sick days or not, and the number of sick days granted. Furthermore, by law, if an

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

employee takes time off work to go to an outside doctor, they have to present a medical certificate to HR that indicates the diagnosis and the number of sick days granted (if any). Hence, in addition to sick days, we can also observe instances of employees being diagnosed with an illness but having no sick days granted for cases where a doctor did not consider their condition severe enough. Thus, sick days are a measure of more severe illness. Between January and early November 2017 (before the intervention), two out of three employees had some kind of illness, and 37% had at least one sick day (see Table 1).

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

Table 1 also shows evidence on the balance of treatment assignment. Columns 2–5 present the mean employee characteristics across the four groups; all variables have almost identical means across all groups. For each characteristic, Column 6 shows the p-value of a joint significance test of differences of means. We cannot reject the null hypothesis that the means are the same across the four treatments, which suggests that our randomization was successful. A Kruskal–Wallis rank test produces the same result. Finally, we test whether participation rates in the pre- and post-intervention surveys are different across treatments; no statistically significant difference is detected.[12]

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

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 in detail the effect of opportunity costs, information nudges, and peers on take-up rates. We do not find any effect of the \$2.48 price difference from the income-dependent vaccine subsidy on vaccination take-up rates.[13] We thus conclude that this price change may be too small to induce changes in take-up behavior.

The last row in Table 1 presents the flu immunization take-up rates for the different treatments during the campaign. The *Control* group shows a take-up rate of 22%, the *Altruistic* treatment shows a take-up rate of 17%, and the *Selfish* treatment shows a take-up rate of 19%. A comparison across the three groups thus 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 the *Saturday* treatment (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 effects of opportunity costs, altruistic information, and selfish information on vaccination take-up for employee *i* in city *c* using the following equation:

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

where $Takeup_{ic}$ is an indicator of getting vaccinated. We control for whether the branch was in Quito or not (γ_c) to account for differences in implementation of the vaccination day assignment

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

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

across branches (as discussed in Section 2). $Saturday_{ic}$, $Altruism_{ic}$, and $Selfish_{ic}$ are dummy variables that indicate treatment assignment. Thus, we estimate the effect of the different treatments relative to those individuals in the *Control* group who were assigned to vaccination on a day during the workweek and did not receive any information nudge.

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

Conversely, we find that emphasizing either the altruistic or the selfish benefits of vaccination did not affect take-up. The coefficients in both cases are close to zero, negative, and statistically insignificant. It is plausible that supplying a sentence of additional information is not sufficient to further increase take-up, given the substantial effect of reducing opportunity costs.[15] One interpretation of these results is that information would have to be highly salient to accrue an effect on vaccine take-up rates in a company 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 vaccine price, income, tenure, division in the company, gender, age, and education level does not affect the estimates.[16] Column 3 addresses the fact that only employees who work in the bank's headquarters in Quito were assigned to be vaccinated on a *Saturday*.[17] 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

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

[16] A simple Bonferroni-adjustment results in a p-value of 0.03 for the Saturday assignment.

[17] The estimates are almost identical for the Quito sample if we include controls. For Quito, we also control for distance between the bank and the participants' homes to capture transportation costs.

than the main result, but we cannot reject that they are statistically the same as confidence intervals overlap. Both information treatments display small, negative, and statistically insignificant effects. Column 4 shows the effect of controlling for non-compliance.[18] In this subsample, assigning employees to *Saturday* decreased take-up by 6.7 percentage points, significant at the 5% level. We cannot reject that this estimate is statistically the same as the baseline result. The estimates of the effects of the information treatments are practically the same as the main estimates.

Lastly, in Column 5, we check whether assignment to different days in the week affected take-up differentially. We use the fact that vaccination days were randomly assigned, and we regress our indicator of vaccination take-up on dummies for each assigned day (*Wednesday*, *Thursday*, *Friday*, and *Saturday*) using Quito's subsample.[19] These estimates show that the take-up rates for *Thursday* and *Friday* are not statistically different from that for *Wednesday*, while the effect of *Saturday* is substantially larger in magnitude and very close to the baseline estimate in Column 1.[20] These results do not support time-inconsistent preferences that would induce procrastination as the mechanism behind the *Saturday* effect, and they are consistent with increasing opportunity costs.[21]

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 rates between employees assigned to be vaccinated on a day during the workweek and those for *Saturday*.[22] We focus on differences across gender, distance to work, and employees with and without children.[23] Figure 1 shows that assignment to *Saturday* reduces take-up by 8.8 percentage

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[18] We identified in the campaign records 12 employees assigned to a day during the workweek who were actually vaccinated on the *Saturday*. The bank asked the medical team in charge of the vaccination campaign to enforce the day assigned to each employee, but they failed to enforce this requirement on the Saturday and were unable to send employees home without being vaccinated if they showed up on that day. In contrast, no employees who were assigned to *Saturday* got vaccinated during the workweek.

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

[20] While the effect of assignment to *Friday* is not significant, it is 44% of the effect of *Saturday* and two orders of magnitude larger than the effect of *Thursday*. Being assigned to *Friday* can slightly increase the opportunity cost of vaccination because it is only a six-hour workday rather than an eight-hour workday like the other weekdays.

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

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

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

points for men and 6.7 percentage points for women, although the difference is not statistically significant.

--- Figure 1 about here ---

Distance to work reflects the transportation costs that an individual regularly incurs. The median distance that employees live away from work is 6.5 km. Figure 1 shows that those who live further away than the median are slightly less likely to have been vaccinated (-9.7 pp.) when assigned to *Saturday* than those who live closer to the bank (-8.3 pp.), but this difference is not statistically significant. This result is consistent with the additional travel costs, but the magnitude suggests that these costs are not the main factor driving the difference in take-up rates between employees assigned to the workweek and to *Saturday*.

We also consider differences between the effects for employees with and without children. Having children may imply higher opportunity costs at the weekend due to 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.*

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

In conclusion, several pieces of evidence suggest that the difference in take-up rates between employees assigned to the a day during the workweek and to *Saturday* corresponds with a change in the opportunity costs of vaccination. Since most bank activities occur during the workweek, one could argue that the *Saturday* treatment may have increased the opportunity costs to get vaccinated to a particularly large extent, even though the magnitude of the effect is similar to that in other settings (Banerjee et al., 2010; Bronchetti et al., 2015).[24] In the rest of our analyses, we use only this variation in take-up resulting from lower opportunity costs as an instrument.

4.3 Peer Effects on Vaccination Take-Up

Peer effects may play an important role in vaccination behavior by either increasing or decreasing take-up rates. On the one hand, if more individuals get vaccinated, the prevalence of the disease may decrease, thus making it less likely for others to get sick. Thus, if there are costs involved in getting vaccinated, it may be optimal for some people not to do so if their peers decide to get vaccinated. Theoretically, this free-rider problem can result in a Nash equilibrium, where nobody takes the vaccine (Chen & Toxvaerd, 2014). On the other hand, peer vaccination may increase the probability of individual take-up. Such positive peer effects in vaccination could occur, for example, because individuals care about how they are perceived by others (Karing 2018). As a result, co-workers may imitate the health care behavior of their peers to conform with perceived social norms.

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

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

where $Prop.Takeup_{-i,jc}$ corresponds to the proportion of peers assigned to get vaccinated on the workweek for i in unit j, $Workweek_{ic}$ is the assignment to vaccinate on the workweek for individual i, and $\bar{X}_{-i,jc}$ are the average observable characteristics of peers j. Manski (1993) shows that if we estimate equation (2) by ordinary least squares (OLS), then self-selection, common

[24] Given that vaccination rates in Ecuador are very low, external vaccination generally does not seem to be as convenient as in-company vaccination. One could argue that offering slots on a Saturday provided an opportunity to get vaccinated that did not exist otherwise, since physicians typically have office hours only during the workweek. Consequently, an assignment to get the vaccine during working time on a weekday might have lowered opportunity costs even further.

environmental factors, and reflection will confound the true peer effects β_1 and β_3. However, in our design, employees are randomly assigned to vaccinate on the workweek independent of their unit. This creates exogenous variation across units that affects the proportion of peers who get vaccinated independently of employee i's decision to 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, leaving out individual i, to obtain the following first stage equation:

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

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

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

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

--- Table 3 about here ---

Table 3 presents the main results on peer effects in vaccination. The first stage estimate in Column 1 indicates that a ten-percentage-point increase in the proportion of peers assigned to the workweek increased the proportion of peers getting vaccinated by 3.1 percentage points. The effective F-statistic of Montiel Olea and Pflueger (2013) is 16.48; therefore, we can reject the null of weak instruments for a threshold of 20%, which suggests that the instrument is relevant. The estimates in Columns 2–4 show that peer vaccination 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-point increase in the proportion of peers getting vaccinated increases take-up by 7.9 percentage points.[25] In further analyses, we attempt to shed more light on potential mechanisms that could be behind the peer effects on individual take-up.[26] In our interpretation of the available evidence, the positive peer effects most likely are a consequence of individuals feeling pressured to follow behavior that they deem socially acceptable.

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

In this section, we exploit random assignment to a vaccination appointment in the workweek as an instrument to study whether flu vaccination improved health, and thereby reduced sickness

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

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

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 whether getting vaccinated can induce health-threatening behaviors.

5.1 Effects of Flu Vaccination on Health and Absence

Flu vaccines may affect health through multiple avenues, both 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. Furthermore, as indicated by the results in the previous section, if a person gets vaccinated, the likelihood that their peers will also get vaccinated increases. 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 100%), which could play an indirect 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 follows:

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

However, vaccination take-up rates 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 downward. This speaks for instrumenting the following variables: i) take-up with an indicator of assignment to vaccination during the workweek, and ii) the proportion of vaccinated peers in the unit with the proportion of peers assigned to the workweek. Again, we exploit variation across units from the proportion of peers assigned to the workweek and variation within unit from individual assignment to the workweek to identify both the effect of individual take-up and the peer effect on health. The unadjusted first stage equations have F-statistics of 6.6 and 8.9, respectively, implying that the IV estimates of equation (5) may have a problem of finite sample

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

bias.[28] Thus, we focus on the valid reduced form estimates of regressing the health outcomes on the instruments.

--- Table 4 about here ---

Table 4 presents the effects of getting the 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 decreases the probability of being diagnosed with an illness by 0.7 percentage points (1.4% of the baseline); however, the effect is insignificant. The reduced form estimates in Column 2 imply that getting vaccinated does not affect the probability of being diagnosed as sick. Being randomly assigned to a day in the workweek (which increases vaccination take-up) decreases the probability of sickness by 1.7 percentage points (3.5% of the baseline), which is insignificant at conventional levels. Additionally, the results in Columns 1 and 2 indicate that the proportion of vaccinated peers does not affect the probability of being diagnosed with an illness. This implies that underestimation of individual health benefits due to vaccination is not an issue in the absence of any significant externalities from peer to peer, be it via exogenously encouraged take-up or via health spillovers.[29]

--- Table 5 about here ---

Table 5 shows the effects of flu vaccination on the probability of having a sick day. The OLS correlation suggests that vaccination decreases the probability of having a sick day by 4.1 percentage points, but this effect is not significant. Conversely, the reduced form estimates in Column 2 imply that getting vaccinated does not affect the probability of having a sick day. Being randomly assigned to a day in the workweek (which increases vaccination take-up) increases the

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

[29] In additional analyses, we inspect deeper whether externalities could be relevant for health. In particular, we run interaction analyses by exploiting the randomness in both individual treatment assignment and treatment assignment of peers in the work unit. The interaction between these variables turns out to be insignificant, which means that higher chances of peer effects do not change our estimate of the individual treatment effect. In general, we do not observe any evidence that work units with large shares of workweek assignments have better health, although they should in case there were significant health benefits of take-up, be it in form of individual health benefits, externalities or both. Note that our setting would in principle enable us to learn more about different forms of externalities, if those had occurred, by adjusting the definition of our peer vaccination instrument. However, our findings on both individual vaccination and peer vaccination do not change when we define the proportion of peers differently and use the location-adjusted version of the instrument by focusing only on co-workers of the same unit at the same location.

probability of having a sick day by 1.3 percentage points (5% of the baseline), which is insignificant at conventional levels. From the firm's overall perspective, these results suggest that the investment in the health campaign was not worthwhile.[30]

--- Table 6 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. The OLS estimates in Column 1 of Table 6 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), which is not significant at conventional levels. This result further suggests that getting vaccinated was ineffective in decreasing the probability of having the flu. Furthermore, the estimates in Columns 1 and 2 show that the proportion of vaccinated peers does not affect the probability of being diagnosed with the flu, which suggests that vaccination rates are too low to provide herd immunity. Thus, we disregard the proportion of vaccinated peers in the following analyses.[31]

To evaluate whether the estimates rule out meaningful effects of vaccination, we implement an equivalence test based on two one-sided hypothesis tests (Hartman & Hidalgo, 2018; King et al., 2000; Lakens, 2017; Rainey, 2014). The equivalence test has two parts. First, we must define what constitutes a meaningful effect of vaccination. This value comprises two thresholds to evaluate whether the estimates rule out meaningful effects. To define this value, we use flu vaccine effectiveness estimates from CDC data (CDC, 2019). While these estimates come from observational studies on flu hospitalizations and might be biased, they constitute the criteria that

[30] Our findings on the health implications of vaccination are robust to a variety of checks and deeper analyses. In particular, our results for sickness and absence do not change if we take out the proportion of peers and estimate only the individual effect of vaccination. Furthermore, we come to the same conclusions based on analyzing the number of sick days as the dependent variable. Note that some of the diagnoses include severe illnesses such as cancer, meaning that a large number of the recorded sick days are 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 in Table 5 on the probability of having a sick day. Finally, all the results in this section are robust when only the Quito sample is used.

[31] As can be seen in Table A7, 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. We check the main result by performing a bounding exercise (Table A8), in which we consider the possible role of individuals being vaccinated externally to the firm in our results (see Section 5.3 for details). The results also hold when we check the probability of being granted a sick day due to having the flu as an outcome variable.

policymakers use to evaluate the vaccine's effectiveness. Since our experimental design guarantees that getting vaccinated is the only channel through which assignment to the workweek in the campaign affects health outcomes, we can use reduced form estimates to evaluate whether vaccination has a meaningful effect on the probability of being granted a sick day due to having the flu.[32] When comparing our sick days with the CDC hospitalizations we argue that our estimate comparisons are conservative since a vaccine that is not effective for the less severe measure should not be effective for the more severe measure either. According to the CDC, for working adults, the 2013–2014 vaccine had the highest effectiveness (reducing hospitalizations by 16 percentage points), the 2014–2015 vaccine had the lowest effectiveness (reducing hospitalizations by only 2.2 percentage points), and the 2017–2018 vaccine's effectiveness (the period of the campaign for the current research) fell in between those estimates (reducing hospitalizations by 8.4 percentage points). To compare these values with the reduced form estimates, we multiply the CDC effectiveness estimates by the smallest effect of assignment to the workweek on take-up reported in Table 2, i.e., the most conservative estimate of the first stage (6.7 percentage points). This calculation yields reduced form reference values of −1.1 percentage points, −0.1 percentage points, and −0.6 percentage points, respectively.

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

--- Figure 2 about here ---

[32] The CDC provides data regarding the percentage effectiveness of the vaccine. However, we require percentage point changes for the equivalence test. These percentage changes come from CDC cross-tabulations on the number of individuals vaccinated and not vaccinated and the number of individuals getting sick with the flu and not getting sick with the flu. The CDC further adjusts these estimates to control for demographic characteristics that affect natural immunity to the flu and thus result in larger estimates; therefore, the reported percentages are a conservative lower bound of the CDC estimates.

Figure 2 presents the comparisons. We can reject the CDC's vaccine effectiveness estimate for the best season (2013–2014) and that for our campaign season (2017–2018). The estimated effect is consistent with the vaccine's effectiveness of 2014–2015 (the season with the lowest effectiveness). These results imply that we can 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 rates. We study this potential issue in the next section.

5.2 Behavioral Effects of Getting Vaccinated

The previous results imply that vaccinating employees against the flu appears to be ineffective to improve health. A simple explanation could be that the flu vaccine was medically ineffective. As discussed in the previous section, even health institutions in support of vaccination acknowledge that the quality of the flu vaccine can vary substantially across different years. At first glance, this interpretation of a medically ineffective vaccine aligns with our evidence showing no health improvements for employees including flu-specific illness. However, there are two alternative explanations that are compatible with a medically effective vaccine. First, the problem of flu-related sickness may be too rare in a healthy working-age population to detect any significant health effects of increased vaccination rates. Second, vaccination could also indirectly affect health outcomes, besides a possible medical effect, if employees change their behavior. Vaccinated individuals could overestimate the protection of the vaccine and engage in riskier behaviors; for example, they may avoid going to the doctor, or may wait longer than unvaccinated individuals to do so, when they feel flu-like symptoms. In addition, vaccinated individuals may take fewer protective measures; for example, they may wash their hands less frequently. These changes in behavior could affect health in general and also increase chances of getting the flu. While any of such behavioral responses could be very relevant for policy-makers considering health campaigns, we also conduct the following analysis on employee behavior to learn more about a possible explanation for the lacking effectiveness of the flu vaccine.

To explore the behavioral effects of flu vaccination, we first inspect whether having been vaccinated induces different reactions than being unvaccinated when flu-like symptoms appear. An important factor here is that non-flu respiratory diseases have symptoms like the flu, but the vaccine does not provide any immunity benefit to prevent them. Thus, flu vaccination should not affect the probability of being diagnosed with a non-flu disease, so any effect on this probability would imply a change in how individuals react when they contract or show symptoms of a respiratory disease. For example, if vaccinated employees felt more protected, they might have been less likely to go to the doctor when they felt flu-like symptoms, thus decreasing the probability of being diagnosed with a non-flu disease. In particular, this would concern cases of mild illnesses where it is up to the individual to decide whether to consult a doctor or not.

To implement this test, we utilize the richness of the data on medical diagnoses to identify cases of non-flu respiratory illnesses, and we exploit a policy intervention of the Ecuadorian government that occurred 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 people to go to the doctor if they felt any flu symptoms. If vaccinated individuals felt protected, we argue that they may not have followed the government's recommendation, resulting in fewer visits to the doctor and fewer non-flu respiratory diagnoses among vaccinated employees in that month.

--- Figure 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 a vaccination appointment during the workweek on flu and non-flu respiratory diagnoses. As with the cross-section estimates in Table 6, employees being assigned appointments during the workweek does not affect the probability of being diagnosed with the flu in any month. The 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 was specifically encouraging individuals to go to the doctor, being assigned a vaccination appointment during the workweek decreased the probability of being diagnosed with a non-flu respiratory disease by 7.2 percentage points.[33] This result is robust when controlling for individual fixed effects (Figure A11).

We also estimate the effect of assignment to an appointment during the workweek on non-flu diagnoses, collapsing the data of the four months to a cross-section. In this specification, being assigned to the workweek decreases the probability of being diagnosed with a non-flu respiratory disease by 7.7 percentage points (Table A8), which is almost identical to the effect in January. This result suggests that employees assigned an appointment during the workweek, who were more likely to get vaccinated, felt more protected and thus were less likely to visit the doctor when they felt flu-like symptoms. These estimates are consistent with the hypothesis of riskier behavior among vaccinated individuals, as these employees appear to have thought that they were protected against the flu.

--- Figure 4 about here ---

We can also investigate whether vaccination affects the likelihood of visiting the doctor at the on-site health center. The bank's on-site health center is a convenient facility for its employees because they do not have to ask for time off to go to the doctor; they can just take a few minutes of their working time to visit the health center. Before the intervention, the on-site doctors accounted 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 presents the effects of assigning employees to vaccination appointments during the workweek on the probability of visiting the on-site doctor, broken down by month. There was no significant effect in November, December, or 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 indicates that vaccinated individuals feel protected and hence are willing to take risks concerning their health, in contrast to unvaccinated individuals who rather prefer having a check-up at the doctor. While it is debatable whether it is actually a problem if employees with mild flu-like symptoms ignore the government's advice by

[33] Comparing the significance levels for each month in Figure 3 for flu vs. non-flu, we find that we cannot reject the null that the effects are the same for all months except for January, where we find a significant difference between flu and non-flu effects of 6.8 percentage points (p-value = 0.016). The results are qualitatively the same when we use the more severe measure of sick days.

not going the doctor, the important point for our discussion is that such a behavior is generally risky and hence consistent with moral hazard.

--- Table 7 about here ---

To learn more about health-relevant behaviors, we analyze data from the post-intervention survey where the employees were asked to report how often they: (i) exercise, (ii) take nutritional supplements, (iii) use an umbrella when it rains, and (iv) wash their hands. Table 7 shows the effects of assigning employees to a vaccination appointment during the workweek on these outcomes. This factor does not seem to affect how often employees wash their hands (1.2% of the baseline), which could be due to the fact that almost all employees reported that they wash their hands regularly. Assigning employees weekday appointments shows 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 umbrella is statistically significant, decreasing the frequency of carrying an umbrella by 1.22 points (17.6% of the baseline) on a Likert scale where one means "never" and ten means "all the time."[34] While this indicates that vaccinated individuals were more willing to be engaged in riskier behaviors concerning their health, it is interesting to note that many people, including Ecuadorians, believe that carrying an umbrella could help to prevent the flu or other respiratory illnesses. Psychology research shows that cultures across the world associate the fact that the flu virus survives longer in a cold and wet environment with the belief that individuals catch the flu by getting wet or cold (Au et al., 2008; Baer et al., 1999; Helman, 1978; Sigelman et al., 1993).[35]

Finally, 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 effective in preventing the flu (Table A9). These findings again suggest that vaccinated individuals feel protected and therefore neglect measures that they believe to be helpful for preventing respiratory illnesses.

In summary, the results on riskier behaviors regarding health indicates a potential concern for

[34] This effect is significant at the 5% level (p-value = 0.012) and robust to adjusting for multiple comparisons following Anderson (2008). As can be seen in Table 7, we also obtain a significant result if we use an index by summing up all four scores and dividing the sum by four.

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

policy-makers regarding the overall effects of medical interventions, given that any health risk due to behavioral changes could pose a threat to an intervention's success. Having said that, our evidence does not clearly support the idea of behavioral changes that could actually increase the chances of getting the flu. While using an umbrella might help to avoid a cold by not getting soaked wet on a rainy day, it does not yield protection against an infectious disease, independent of what the people in our setting may believe. Visiting the doctor might also not help in this respect, even though our findings could also be interpreted as potentially indicative for other riskier behaviors which may indeed be relevant for the probability of getting the flu. In the following, we discuss alternative explanations for the evidence on behavioral implications of getting vaccinated.

5.3 Other Interpretations of the Results on the Behavioral Effects of Vaccination

After providing several pieces of evidence on changes in individual behavior due to flu vaccination, we briefly discuss some alternative interpretations of these findings, not related to moral hazard. Misdiagnoses is one potential competing explanation. If doctors are not able to distinguish the flu from other non-flu respiratory diseases, then some of the diagnosed non-flu cases could have actually been flu cases. However, as our data include diagnoses from 72 different doctors from different health centers and hospitals, it is unlikely that there is a systematic issue of misdiagnosis. Additionally, the results are robust to using a broader definition of flu-related illness. Finally, misdiagnoses would not explain why vaccinated individuals reported being less likely to carry an umbrella.

We could also suggest that doctors may have misdiagnosed conditional on whether an individual had been vaccinated or not. If a doctor learned that a person who is showing flu-like symptoms has been vaccinated, they 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 appointments during the workweek, who were more likely to get vaccinated, had a lower rate of diagnosis of non-flu respiratory diseases than those assigned to the weekend.

Another potential concern is the fact that the data on medical diagnoses used in this study correspond with employees who visited the on-site doctor or an external doctor during working hours, while those who saw an external doctor outside working hours and who were diagnosed sick but were not granted a sick day are coded as healthy. This measurement error will not bias the flu and non-flu estimates as long as it is uncorrelated with the assignment to vaccination during the workweek. However, if employees assigned to get vaccinated during the workweek are more likely to consult an external doctor outside working hours, then this would overestimate the effect on non-flu respiratory diagnoses. To address this potential concern, we bound the effect (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 a working day) to obtain a lower bound. The effect of being assigned to a day during the workweek on the probability of being diagnosed with a non-flu respiratory illness is always negative and bounded between 5.6 and 10.3 percentage points (Table A8).

A final alternative to the interpretation of behavioral changes indicative of moral hazard is the idea of adverse selection. Accordingly, 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 rates. 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 and with 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 an intervention. In this paper, we find that a small price change, as well as information nudges that appeal to either the selfish or altruistic benefits of vaccination, did not induce a change in behavior. In contrast, it appears that reducing opportunity costs could have a substantial effect on participation in a vaccination campaign for working-age employees. Additionally, peers were identified as an important factor influencing vaccination rates in the workplace. Regarding the health benefits of the intervention, the 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 have presented evidence consistent with individuals adopting riskier health behaviors after getting vaccinated. Riskier behaviors may constitute a second pathway by 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, e.g., morale and productivity.[36] Our calculation suggests that the net benefit of the campaign was negative regarding sick days. In the best-case scenario, the treatment may have resulted in a net gain of $0.17 regarding gains in work attendance during the flu season, which is not sufficient to compensate the bank for its costs that include vaccine subsidies of $2.57, $5.05, and $9.99 per vaccine.[37]

Our study presents multiple practical implications for health campaigns. 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 for studying both potential health benefits and behavioral changes in an unbiased way. A potential presence of moral hazard in health-related behavior implies that firms and policymakers should consider this phenomenon in the design of interventions such as vaccination campaigns. A promising mechanism to mitigate this issue could be campaigns to increase awareness of the importance of other protective measures, so that people will not rely only on the protection potentially provided by medical technology. This conclusion also informs the discussion on similar behavioral phenomena in the context of the COVID pandemic (Andersson et al. 2021).

Another lesson learned from our investigation is how to encourage increased participation in health interventions. In this paper, we have identified two cost-effective measures that can increase vaccination take-up rates in a workplace context where monetary factors do not seem to play a significant role in individuals' willingness to participate in a health campaign. Decreasing

[36] A channel pertaining to company morale is the perception of individuals that the company cares more about their health when assigned to the workweek, which leads them to behave differently. However, we cannot find evidence for that channel using data on organizational perceptions from our participants' post-intervention survey responses. Table A10 presents imprecise estimates on self-reported productivity and the duration of the working day (as measured by the employees' magnetic card swipes for entering and exiting the bank). Albeit statistically insignificant, the point estimates suggest that assigning employees to get vaccinated during the workweek increased their perception of their productivity, while it decreased the duration of their working day 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, in comparison with some employees being able to finish their tasks and leave earlier than others.

[37] The estimate's confidence interval implies that, at most, assigning employees to be vaccinated during 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 working days in a month (22), and multiply this value by 0.005.

opportunity costs is one option that may drastically increase participation, which supports the use of mobile campaigns that are available on busy days and in locations where people usually congregate. In addition, since we have found that peer behavior has an important effect on vaccination take-up rate, employers can increase participation in health campaigns by implementing mechanisms to incentivize groups of employees. Small rewards for an entire unit when its members take part in the intervention 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)
	(1)	(2)	(3)	(4)	(5)	(6)
Monthly Income ($)	1,766	1,860	1,701	1,681	1,827	0.316
Company Tenure (years)	7.9	8.3	7.7	8.1	7.5	0.761
Prop. Women	0.49	0.51	0.52	0.46	0.47	0.497
Age (year)	36.6	37.2	36.4	36.6	35.7	0.553
Prop. College Education	0.91	0.92	0.91	0.90	0.93	0.759
Prop. Having Children	0.52	0.52	0.53	0.55	0.48	0.640
Distance to Work (km)	7.58	7.32	7.70	7.78	7.51	0.797
Work Unit Size (#)	29.3	27.9	31.2	29.7	28.4	0.567
Pre Survey Participation	0.48	0.50	0.50	0.47	0.40	0.171
Post Survey Participation	0.36	0.36	0.38	0.33	0.35	0.519
Diagnosed Sick	0.66	0.67	0.67	0.64	0.67	0.835
Granted a Sick Day	0.37	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
	(1)	(2)	(3)	(4)	(5)
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	1,164	1,164	929	1,152	929

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

Notes: Robust standard errors in parentheses. This table presents OLS estimates of the effect of the different treatments on vaccination take-up. All specifications control for 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
	(1)	(2)	(3)	(4)
Proportion of Peers:				
Assigned to the Workweek	0.0031***	0.0024***		
	(0.0007)	(0.0008)		
Vaccinated			0.0051***	0.0079***
			(0.0007)	(0.0017)
F-value	16.481			
N	1,138	1,138	1,138	1,138

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

Notes: Standard errors clustered at the unit level in parentheses. The outcome in Column 1 is the proportion of peers who got vaccinated and the outcome in columns 2-4 is an indicator of individual vaccination. The bank has 116 units with more than one employee. This table presents the effect of peers' vaccination take-up on the individual's vaccination decision. We measure the proportion of peers vaccinated and the proportion of peers assigned to the workweek in percentage points. We define peers as all employees who work in the same unit. All specifications control for Quito fixed effects and individual assignments to the workweek. Column 1 presents the results for the first stage. Column 2 displays the results of the reduced form. Column 3 presents OLS estimates of the effect of a change in the proportion of peers that get vaccinated. Column 4 presents 2SLS estimates of the effect of a change in the proportion of peers that get vaccinated. The first stage F-Stat is based on the Montiel Olea-Pflueger F-value.

Table 4 Effects of Vaccination on Overall Sickness

	OLS	Reduced Form
	(1)	(2)
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	1,120	

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the effects 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 5 Effects of Vaccination on Overall Sick Days

	OLS	Reduced Form
	(1)	(2)
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	1,120	

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the 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 6 Effects of Vaccination on Flu Diagnoses

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

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

Notes: Standard errors clustered at the unit level 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.

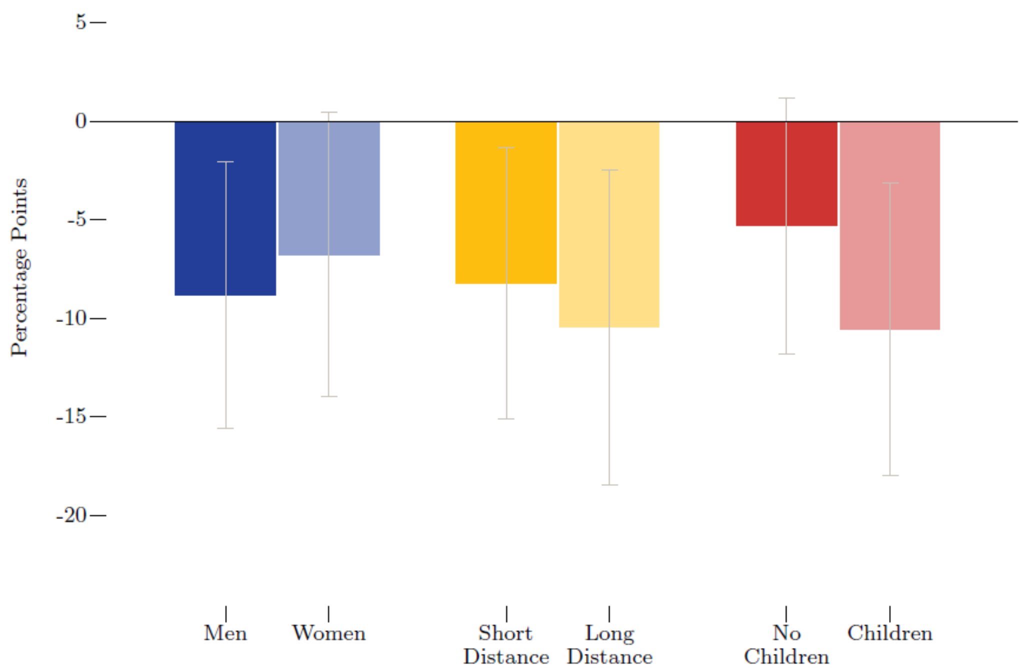
Table 7 Reduced Form Estimates on Health-Related Habits

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

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

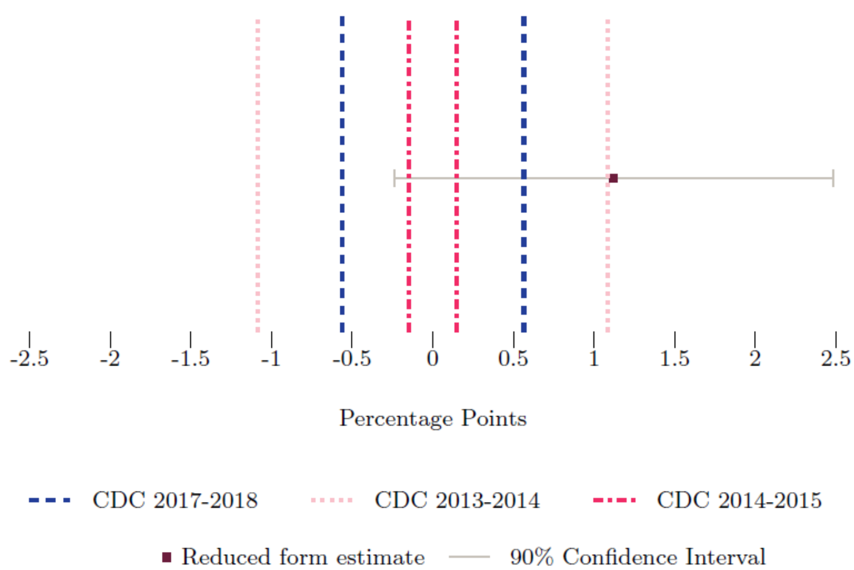
Notes: Robust standard errors in parentheses. This table presents the intent-to-treat effects of being assigned to the workweek on four daily habits and activities related to health and preventing the flu. All responses are on a scale from 1 (“never”) to 10 (“all the time”). The health-related habits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

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



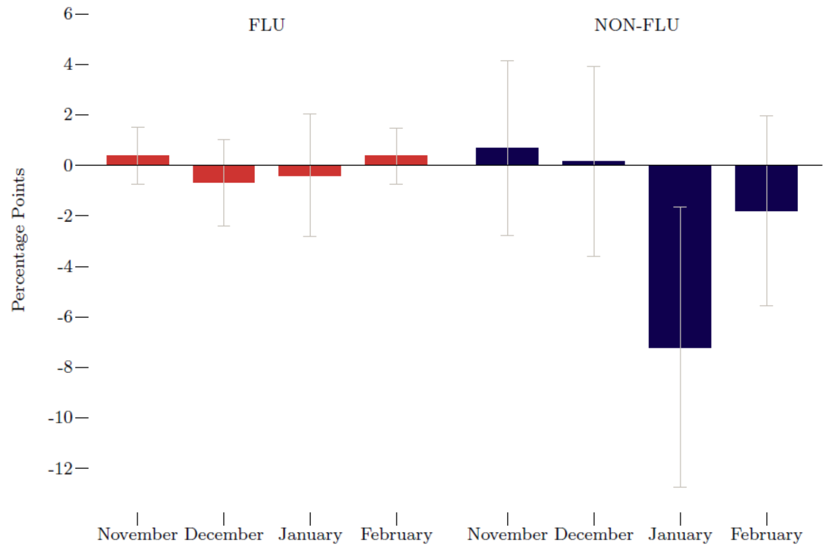
Notes: This figure presents the intent-to-treat effect of assignment to Saturday on vaccination take-up for different subgroups in the sample. All specifications control for city fixed effects. The figure presents the point estimate and the 90% heteroscedastic robust confidence interval for each subgroup.

Figure 2 Equivalence Test for the Effectiveness of Vaccination



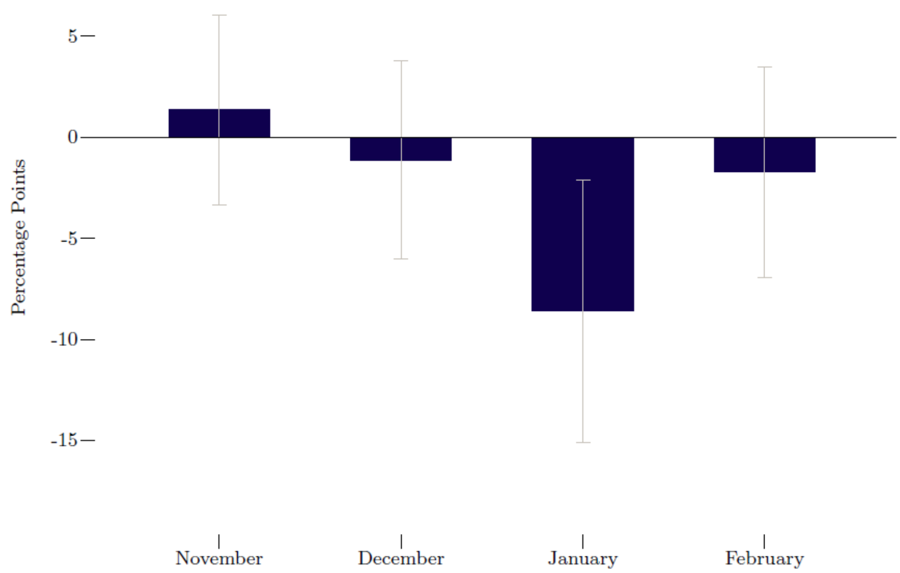
Notes: This figure presents the reduced form estimate of the effect of assignment to the workweek to adjusted CDC estimates of the effectiveness of the flu vaccine for 2013-2014, 2014-2015, and 2017-2018 seasons.

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

Figure 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 (1)	With Controls (2)	Quito Sample (3)	Non-Compliance (4)
Monthly earnings above $750	0.0590 (0.0730)	0.1738 (0.1533)	0.0655 (0.0786)	0.0400 (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 $4.95. There is no visible discontinuity across the threshold — all specifications control for city fixed effects. Column 1 presents our main estimates without adding additional controls. In Column 2, we test the robustness of the main estimates controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level. Column 3 presents the estimates using only employees in Quito, 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 (1)	Heard Selfish Statement (2)
Altruistic Information	-1.2079 (4.9521)	-8.4337** (4.1692)
Selfish Information	-3.8421 (4.9557)	-0.0181 (4.0281)
Saturday	-3.5966 (6.2362)	-2.5732 (5.0237)
Baseline	69.09	76.43
N	377	377

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

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

Table A3 Heterogeneous Treatment Effects on Vaccination Take-up

	Men	**Women**	**Short Distance**	**Long Distance**	**No Children**	**Children**
	(1)	(2)	(3)	(4)	(5)	(6)
Altruistic Information	-0.0017	-0.0508	-0.0564	-0.0477	-0.0163	-0.0368
	(0.0452)	(0.0429)	(0.0441)	(0.0521)	(0.0421)	(0.0454)
Selfish Information	0.0098	-0.0166	-0.0074	-0.0291	0.0188	-0.0253
	(0.0439)	(0.0451)	(0.0460)	(0.0527)	(0.0435)	(0.0452)
Saturday	-0.0883**	-0.0677	-0.0825**	-0.1047**	-0.0531	-0.1056**
	(0.0413)	(0.0441)	(0.0420)	(0.0488)	(0.0396)	(0.0453)
N	593	571	446	449	556	608

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

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

Table A4 Vaccination Take-Up Analysis of Transaction Costs

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

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

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

Table A5 Peer Effects Robustness and Alternative Definitions

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

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

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

Table A6 Potential Mechanisms for Peer Effects

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

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the reduced form effect of peers assigned to the workweek on a series of outcomes identified by the row headers. The measurement unit of each outcome is in parentheses next to the outcome's name. We measure the proportion of peers assigned to the workweek in percentage points. Thus, the estimates represent the effect of a one percentage point change in the proportion of peers. We define peers as all employees who work in the same unit. All specifications control for Quito fixed effects and individual assignments to the workweek. Column 1 presents estimates, Column 2, the baseline value for each outcome, and Column 3, the sample size.

Table A7 Robustness Check for Reduced Form Effects of Vaccination on the Flu

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

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

Notes: Robust standard errors in parentheses. This table presents robustness checks of the effects of being assigned to the workweek on the probability of being diagnosed sick because of the flu using different definitions 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 A8 Bounds

	Diagnosed with Flu			Diagnosed with Non-flu		
	Main	Lower Bound	Upper Bound	Main	Lower Bound	Upper Bound
	(1)	(2)	(3)	(4)	(5)	(6)
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 A9 Heterogeneous Effects on Using an Umbrella

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

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

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

Table A10 Reduced Form Effects on Productivity

	Post-Survey		Swipe-Cards		
	General Productivity	**Productivity Post-Intervention**	**Entry to Work**	**Exit from Work**	**Duration at Work**
	(1)	(2)	(3)	(4)	(5)
Assigned to the workweek	0.1684	0.1534	-0.1492	-0.4879	-0.3387
	(0.1357)	(0.1718)	(0.1945)	(0.3487)	(0.4004)
N	343	343	403	403	403

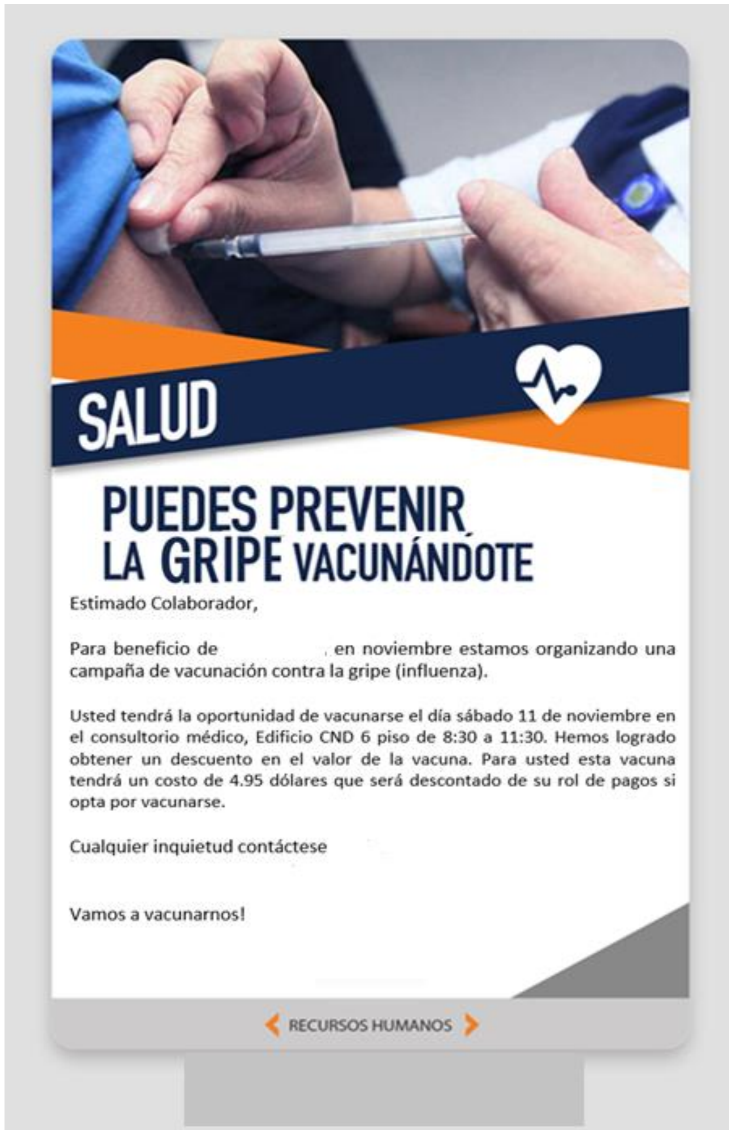
* p<0.1 ** p<0.05 *** p<0.01

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)

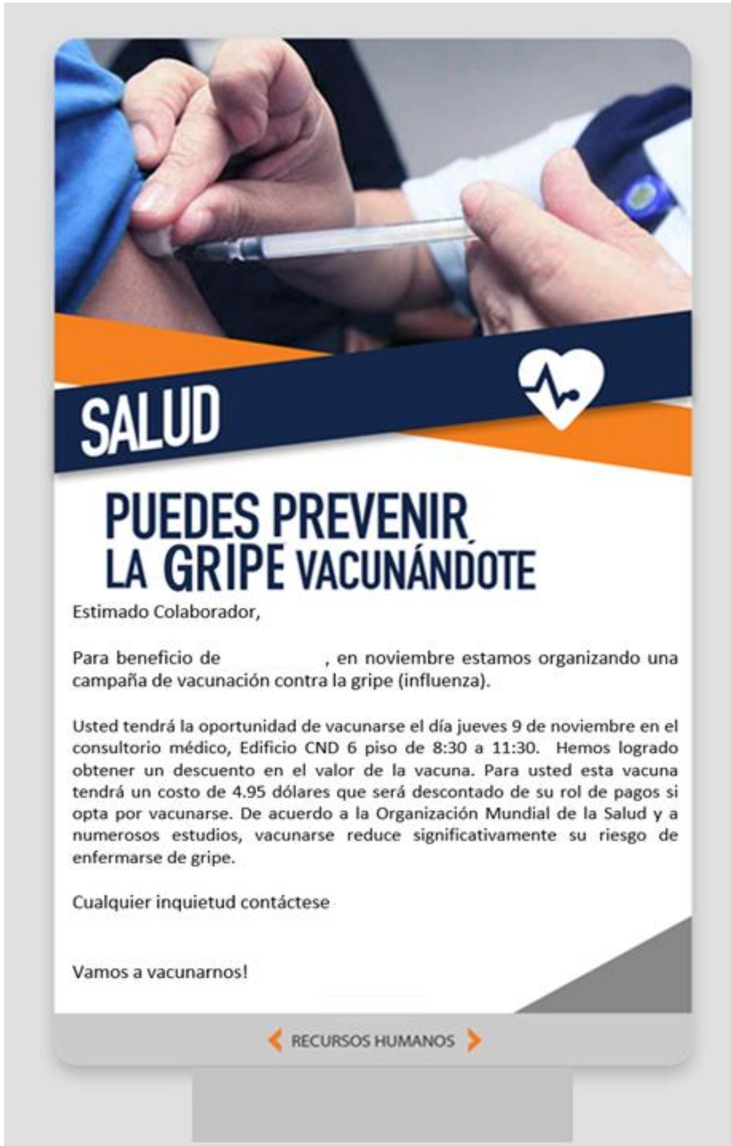


Notes: The above image portrays the email sent to the "*Saturday*" treatment group. Translation: Dear Employee, we are running an influenza vaccination campaign in November. You are eligible for a flu shot on Saturday, November 11, from 8:30 to 11:30. We obtain a discount on the vaccine's price. For 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

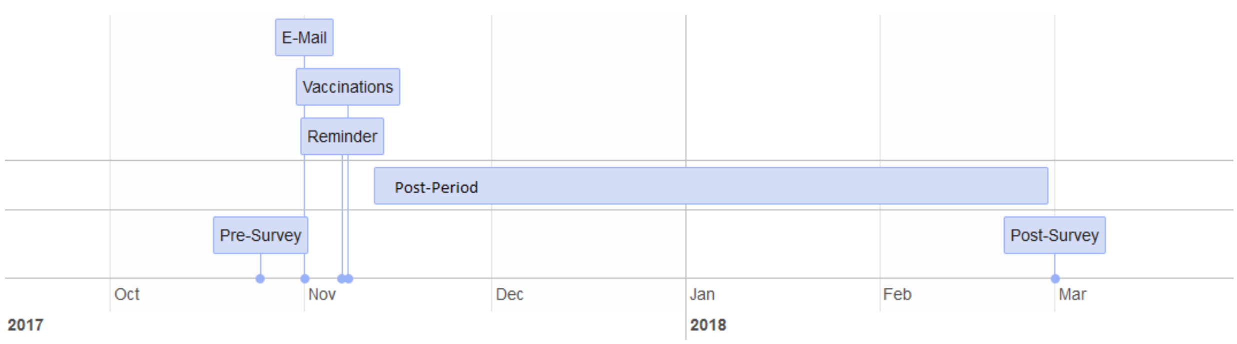


Notes: The above image portrays the email sent to the "Selfish Treatment" group. Translation: Dear Employee, we are running an influenza vaccination campaign in November. You are eligible for a flu shot on Thursday, November 9, from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, 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

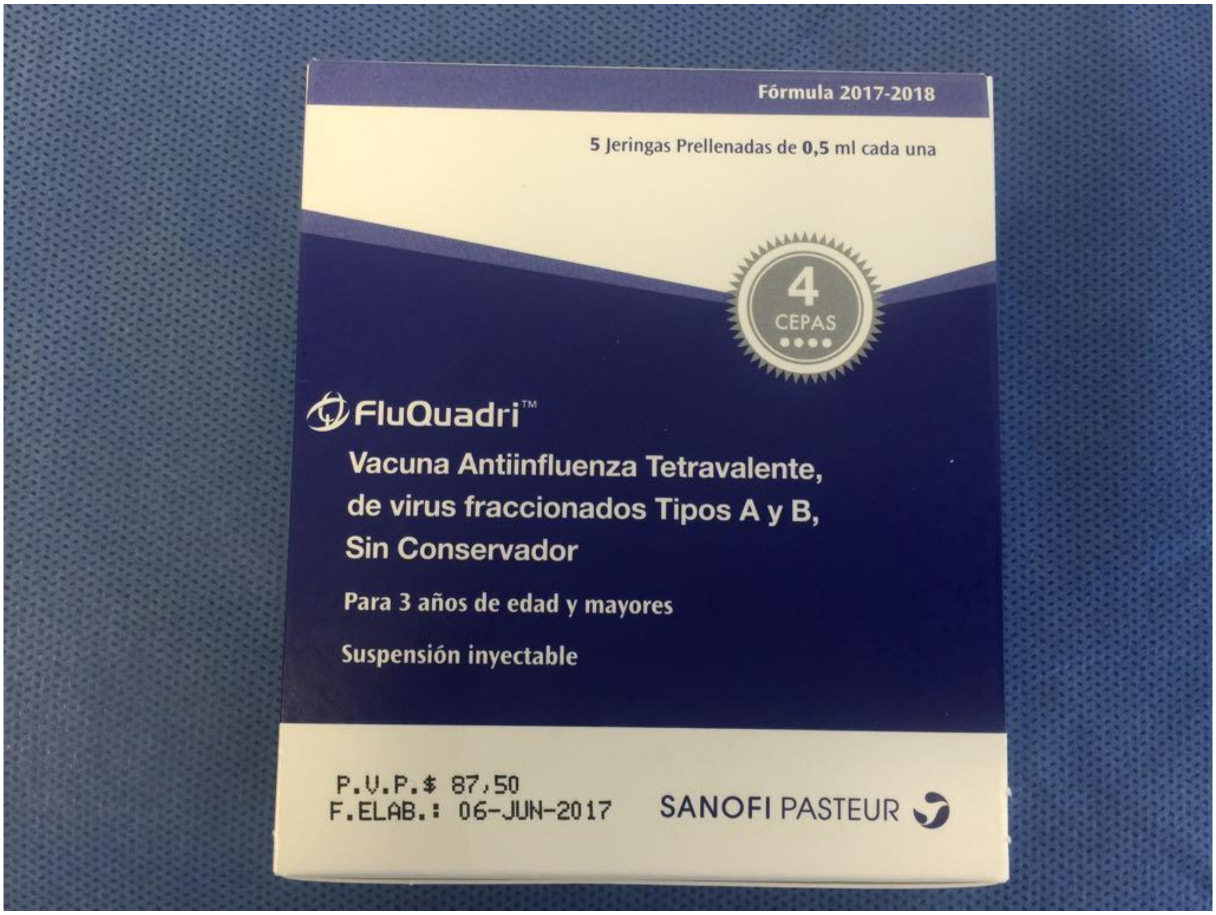
Notes: The map contains the locations of the bank in Ecuador (orange), where we implemented our intervention.

Figure A6 Timeline of Experiment Implementation



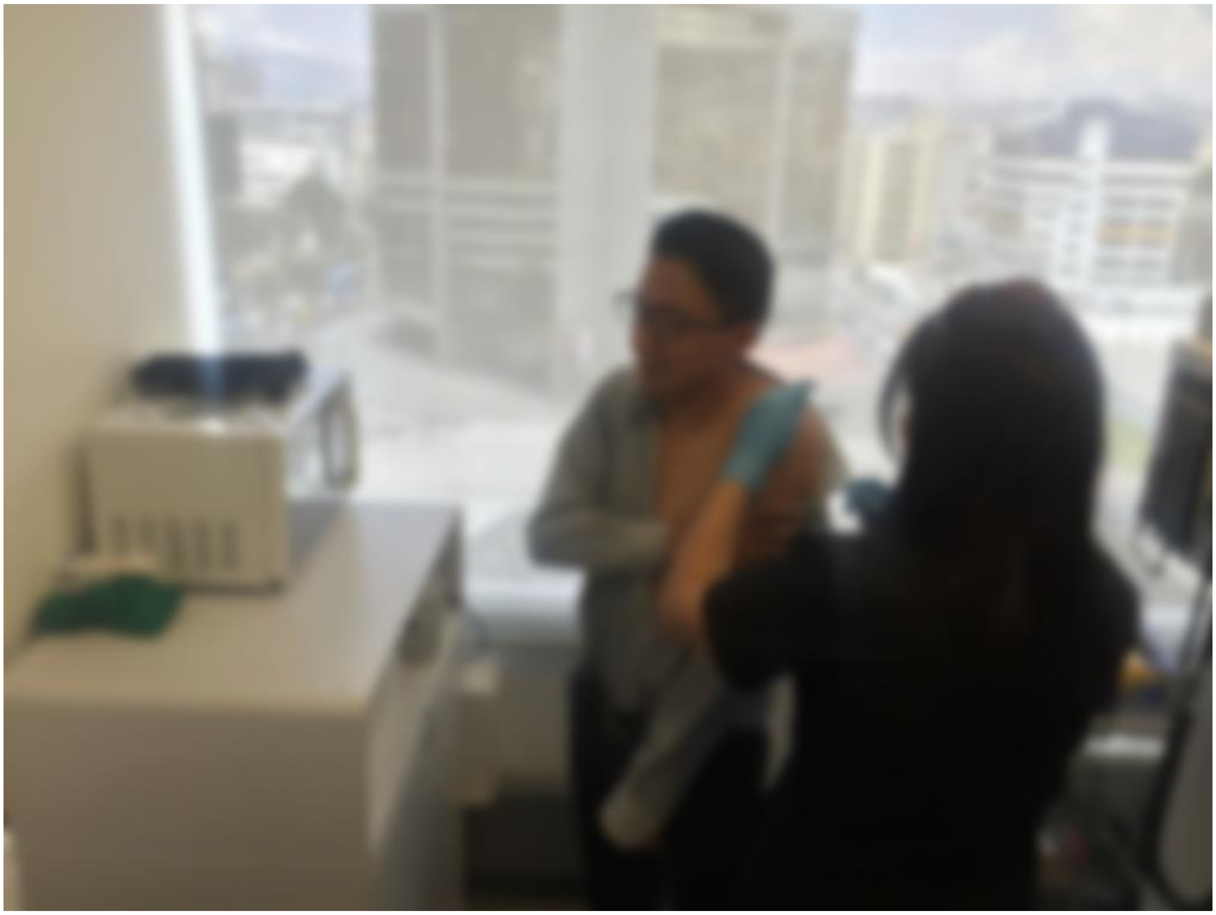
Notes: The bank sent the pre-intervention survey on October 18. The bank sent emails with the different treatments on November 1 using the Human Resources Department mailing account. Furthermore, it sent a reminder on November 7. The vaccination campaign took place between November 8 and November 11.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



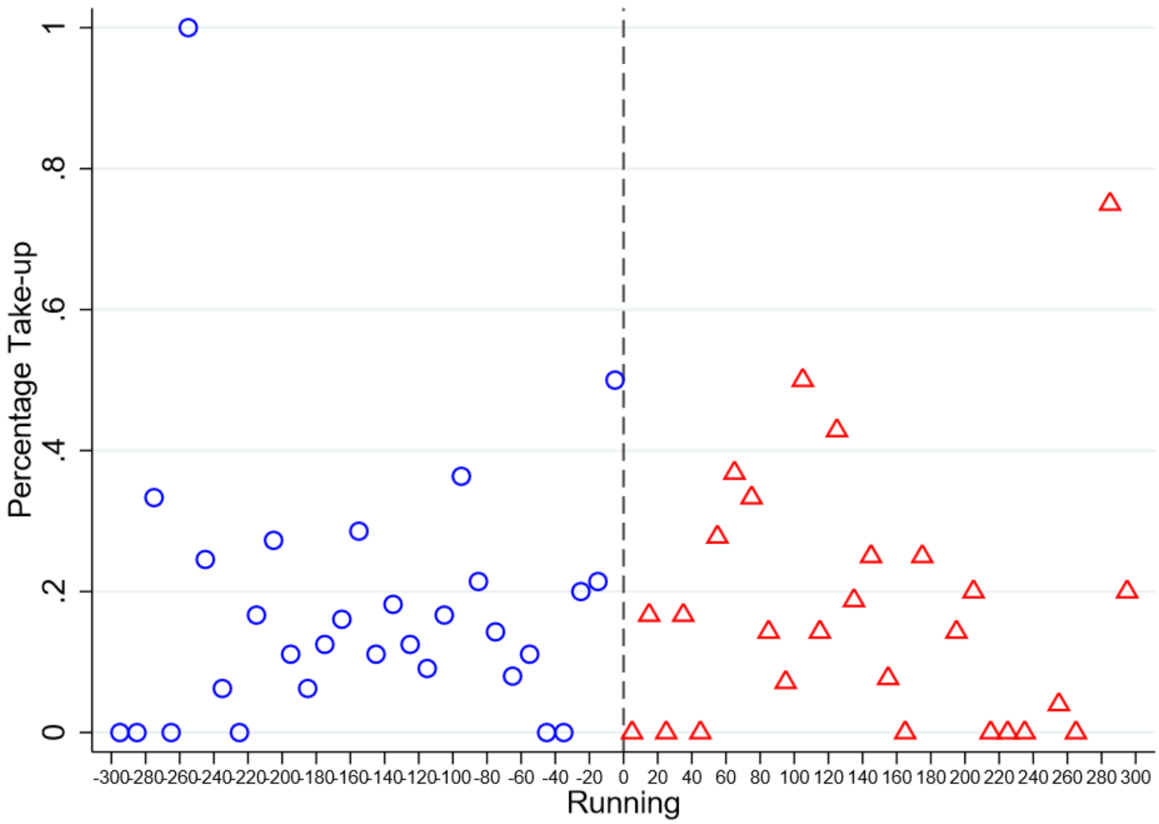
Notes: The above package contains the influenza vaccine used in the campaign. This vaccine protects against four strands of the flu, two from type A and two from type B.

Figure A8 Vaccination Campaign: Flu Shot in Action



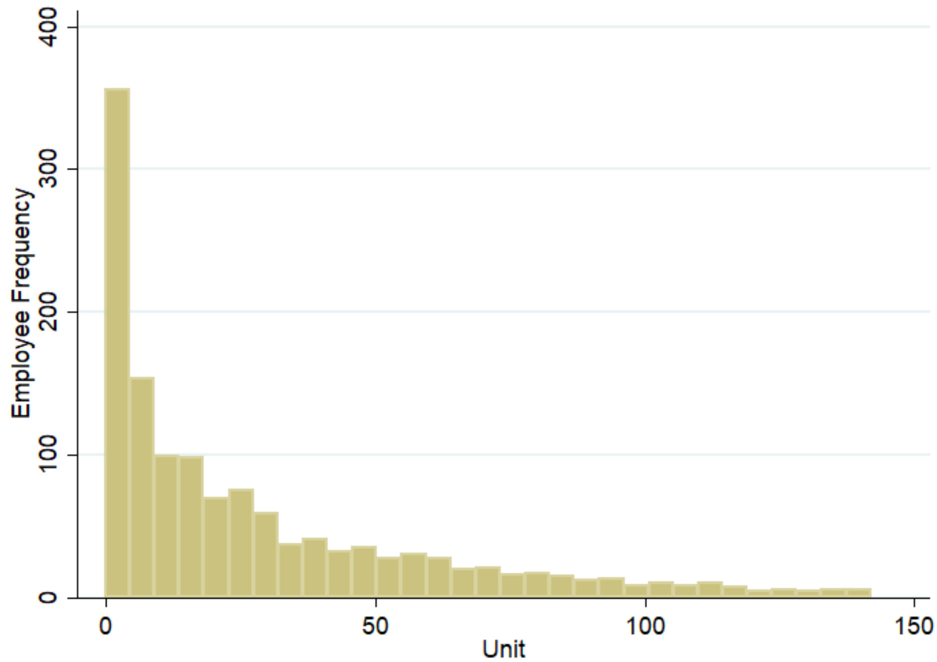
Notes: Immunization at the firm.

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 $4.95.

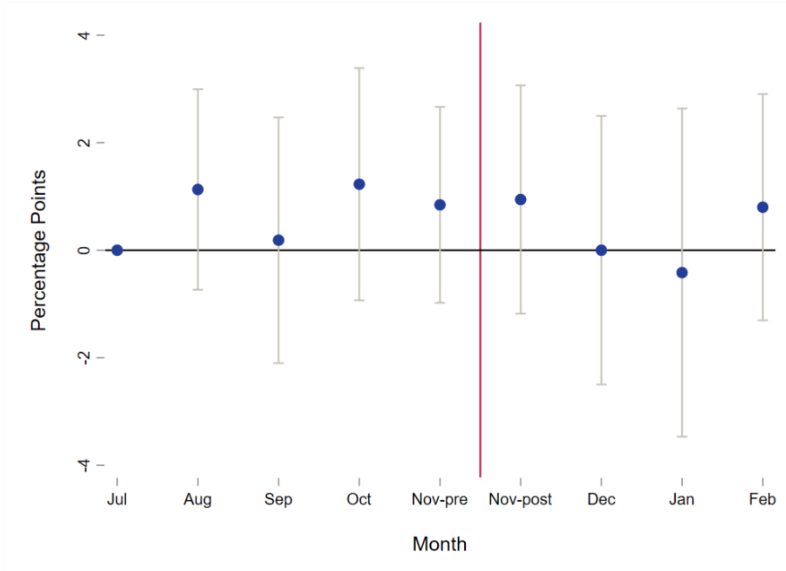
Figure A10 Frequency Distribution of Employees in Units



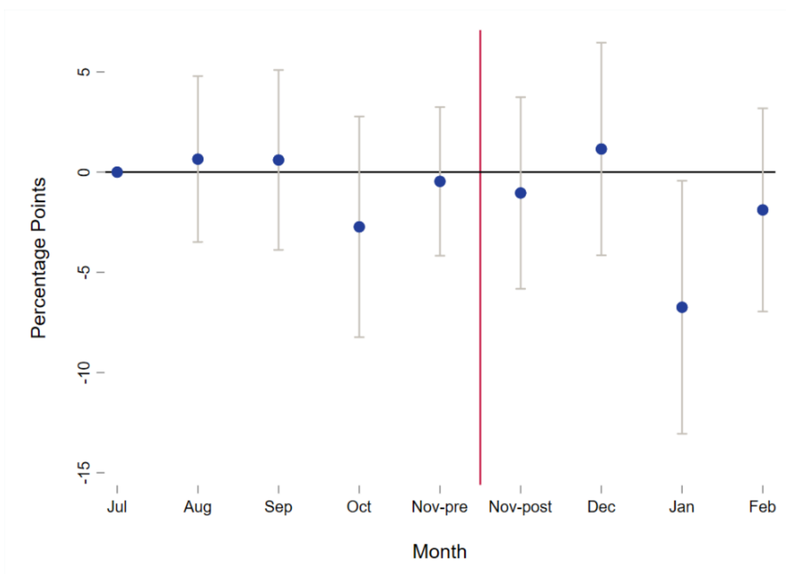
Notes: This figure presents the number of employees in each of the 142 units.

Figure A11 Difference in Difference Estimates of the Effect of Vaccination on Diagnosed Sickness

(a) Flu Diagnoses



(b) Non-flu Diagnoses



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