Vaccines at Work:
Experimental Evidence from a Firm Campaign

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Abstract

Health campaigns in firms could be a cost-effective approach to reduce sickness absence and to mitigate negative economic consequences of ill-health among employees. Low participation rates, however, may prevent firms from realizing such private economic benefits. Moreover, employees may overestimate the effects of the campaign and engage in risky behaviors that could be detrimental to their health, thereby reducing the potential benefits of the intervention. We ran a natural field experiment with a bank in Ecuador, where we employed a randomized encouragement design by experimentally manipulating incentives to participate in a campaign to get vaccinated against influenza. This allows us to study the determinants of on-site vaccination and the consequences of increased participation in a firm campaign for employees, thereby informing about the private incentives for firms to run such interventions. Using rich administrative records merged with employee survey data, we find strong evidence that opportunity costs to participate in the campaign and peer behavior in the firm matter to increase vaccination take-up. Contrary to the firm's expectation, increased participation in the campaign did not imply reduced sickness absence during the flu season. As we observe no relevant health benefits or externalities via co-worker vaccination, our comprehensive analyses indicate that the campaign most likely was not economically beneficial for the firm. Finally, we find evidence consistent with the notion that a vaccination campaign can influence the behavior of employees concerning their health, which could limit the economic promise of such interventions.

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

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1. Introduction

Company management is interested in reducing employee sickness and absence since it is costly. Employees on sick leave commonly receive compensation while not providing any productive benefit to the firm. A possible way to reduce sickness absence of employees is to leverage health campaigns. In particular, preventive health campaigns to vaccinate against influenza (the flu) could be a low-cost way to achieve this goal. This aligns with recommendations of public health institutions like the Centers for Disease Control and Prevention (CDC) that encourage employers to offer flu vaccines at the workplace (CDC, 2022). In the United States alone, the flu is associated with an economic burden of approximately $34.7 billion annually (Rothman, 2017) and 16 million days of lost productivity (Molinari et al., 2007). Hence, flu vaccination seems to be a promising approach to tackle influenza's economic consequences for firms.

However, individual behavior can counter the potential benefits of health interventions, especially in the context of firm campaigns to get vaccinated. According to public health institutions, vaccination rates are usually below recommended levels, ranging from 2% to 70% in European countries (Mereckiene, 2015) and 38.5% among adults in the United States during the 2017–2018 flu season (Srivastav et al., 2018). Therefore, it is essential to understand how to affect participation in a vaccination campaign, especially in populations with low take-up rates, like working-age adults. Further, adopting protective technologies may induce individuals to undertake riskier behaviors. In a firm, employees may overestimate the vaccine's protection and engage in risky behaviors such as delaying doctor visits when feeling sick or reducing other measures to prevent illnesses. Such forms of moral hazard could counter the benefits of health campaigns.

We present the first comprehensive study of the determinants and consequences of employees participating in a firm-sponsored flu vaccination campaign. In cooperation with a bank in Ecuador, we modified incentives for employees to take part in the company's 2017 vaccination campaign, thereby creating exogenous variation in campaign participation. First, we introduced subsidies by selecting an income threshold at which the vaccine price for employees changed. Second, due to capacity constraints the campaign ranged from Wednesday to Saturday, allowing us to randomize the day each employee was assigned for vaccination. Thereby, we could manipulate the opportunity costs of participating in the campaign, as employees either had to arrange their weekend schedules to get to the office or did not incur such extra costs during the workweek. Third, we varied the content of the invitation emails to appeal to altruistic or selfish motives via information nudges.

In a next step, we exploit the experimental variation to study the consequences of employee campaign participation. First, we analyze whether exogenously caused changes in peer behavior affect the co-workers' propensity to vaccinate. Second, we study the implications of campaign participation on employees' health and sickness absence, informing us of the campaign's economic benefit. Third, we explore the behavioral implications of the campaign to gauge the possibility of moral hazard behaviors when adopting medical technology.

Our experimental study yields credible evidence on the *determinants* of employees participating in a firm campaign within an authentic work context. At the same time, we address several challenges when studying the *consequences* of a firm campaign that aims to reduce sickness absence by vaccinating employees against the flu. The first challenge is to provide causal evidence. To the best of our knowledge, no natural field experiment has yet been conducted to identify the implications of flu vaccination among employees. Reviews of the medical research point out many limitations of the available evidence, while documenting rather modest health benefits of flu vaccination, with no clear evidence for sickness absence (Demicheli et al., 2018; Osterholm et al., 2012; Østerhus, 2015). Despite identification problems like the "healthy vaccine recipient effect" that could bias non-experimental evidence, observational studies are often preferred by medical researchers due to ethical concerns, which also explains why randomized controlled trials (RCTs) with clean placebos are rare (Baxter et al., 2010; Loeb et al., 2010; Sanson-Fisher et al., 2007). We present a methodological alternative to circumvent the dilemma of withholding a potentially effective treatment. In our view, a random encouragement design (Bjorvatn et al., 2020; List et al., 2017) represents an innovative approach to address ethical concerns by exploiting variation in vaccination generated through modified incentives to take part in a firm campaign.

The second challenge we address is capturing the behavioral effects of increasing firm-vaccination rates through the company's health campaign. While medical research usually focuses on direct effects only, firms and policymakers are mainly interested in the total health impacts, encompassing both medical effects and behavioral responses that could indirectly affect health. Subjects in medical RCTs usually know they are part of an experiment, but do not know if they have received a specific type of vaccine, eliminating any variation in behavior to vaccination across experimental conditions. In contrast, our random encouragement design introduces no uncertainty in treatment, allowing us to study the total health impact and potential behavioral effects of the firm campaign. To identify the latter, we use non-flu-related health outcomes, since the flu vaccine

promises to only affect one particular disease.

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

While these design features are crucial in determining campaign impacts, randomly incentivizing employees to participate has some challenges and limitations. First, we have no control over the effects of modifying incentives for participation. Second, even if our modifications substantially change participation, the exogenous variation may not suffice to identify minor effects in each outcome with relatively low incidence. However, as long as we observe a substantial exogenous increase in the share of employees getting a flu shot, which is all a successful vaccination campaign can do, we can study the overall health and economic impacts of such an increase. In the end, implications like reduced absenteeism are of prime interest to researchers and firm managers that organize and pay for such campaigns, hoping for a return on their investment.

To conduct our analyses, we utilize data from various sources. We were granted access to the campaign records and the bank's administrative data on employee health. In addition to sickness absence, we observe medical diagnoses, allowing us to distinguish flu from non-flu-related sickness. Employee surveys conducted before and after the campaign complement the administrative data and enable inspections of relevant mechanisms. In all these datasets, we identify each employee and merge the data with the treatment assignment from the campaign.

On the determinants of campaign participation we find the following. Neither the vaccine price nor information on the altruistic or personal benefits of vaccination did affect participation. Conversely, assigning employees to vaccinate during the workweek, increased take-up by about ten percentage points and, hence, roughly doubled compared to Saturday appointments. It implies that reducing opportunity costs has a strong effect on take-up for working adults. Thanks to this exogenous increase in employee vaccination, we can study the impact of peer participation in the

campaign on individual participation. Due to randomization at the employee level, by chance, some units had more employees assigned to the workweek than others; hence, these employees could be more encouraged to get the vaccine than those in other units. Our analysis reveals clear evidence of positive peer effects on the probability of individual take-up.

On the consequences of the firm campaign, we investigate whether participation in the campaign increases employee health, thereby potentially reducing sickness absence. If influenza is a major driver of employee health and the vaccine is effective, encouraging employees to vaccinate could reduce absence from work. However, the results do not show meaningful health benefits, and a back-of-the-envelope cost-benefit analysis documents that the campaign most likely did not generate any significant economic benefit to compensate the firm for its investment.

There are several potential explanations for the absence of an effect on employee health. We might underestimate health benefits because of externalities. However, our analyses show that changes in peer vaccination do not affect individual sickness or absence, implying that health spillovers did not matter. As another explanation, the vaccine may have been medically ineffective, which we cannot inspect deeper. Alternatively, the flu may not be a major driver of illness in a firm with a working-age population, rendering any effort to tackle the disease as pointless from an economic perspective. Finally, increased firm campaign participation to prevent illness may trigger riskier and, thus, health-threatening behavior among employees that overestimate the vaccine's protection. Such a behavioral response could be a general mechanism to limit the success of health interventions. Our setting allows us to study this issue.

To analyze potential behavioral effects, we exploit the incidence of a national health emergency in January 2018. That month, the Ecuadorian government encouraged people to visit their doctor if they felt any flu symptoms via a large-scale media outreach. Our idea is to leverage medical diagnoses on non-flu respiratory illnesses, which share symptoms with the flu, as a measure of changes in behavior during the emergency, since increased participation in a flu vaccination campaign cannot medically affect the probability of such diagnoses. However, those employees who feel more protected may refuse to visit a doctor and, thus, get diagnosed with fewer mild non-flu respiratory diseases. We indeed find a negative effect of increased campaign participation on the chance of being diagnosed with a non-flu respiratory disease in January, with no effect in other months, suggesting that some employees thought flu-like symptoms indicate just a minor respiratory illness without the need to heed the government's advice. This mechanism is supported by another

result showing that workweek assignment for vaccination decreased visits to the bank's on-site doctor in January, with no effect in other months. Finally, by analyzing the employee surveys, we obtain evidence that the workweek assignment to the campaign had a negative effect on health-related behavior in employees. This behavioral effect interacts with beliefs on vaccine effectiveness, supporting the idea that individuals who feel more protected engage more in riskier practices.

With this study, we contribute to our understanding of the determinants and the consequences of employee participation in firm campaigns. There is a growing and managerially relevant literature on firm-based initiatives and interventions that alter, among others, internal idea crowdsourcing (Blasco et al. 2019), corporate social responsibility (List & Momeni 2021), and psychological safety within employee teams (Castro et al. 2022). We focus on how a firm campaign can improve the take-up of vaccines as a medical technology and thereby contribute to this literature by employing a unique setup that provides insights into campaign participation determinants such as opportunity costs, prices, and nudges. As a particular contribution, our paper provides robust evidence on peer effects in the context of participation in a firm campaign using a credible identification strategy.[1] Similarly, we add to a better understanding of the consequences of firm campaigns on employee sickness and absenteeism and the managerial incentives to run those campaigns in the first place. So far, the evidence in this context is often correlational, especially when it comes to the implications of flu vaccine programs in firms (Samad et al. 2006). Hence, there is a lack of credible evidence on the determinants (Royer et al., 2015; Carrera et al., 2020) and, even more so, on the consequences (Handel & Kolstad, 2017; Gubler et al., 2018) of health campaigns in organizations. As an exception, Jones et al. (2019) experimentally demonstrate the ineffectiveness of a university wellness program to improve employee health.[2]

[1] We thereby also contribute to the broader research on peer effects in vaccination (Bouckaert et al., 2020; Geoffard & Philipson, 1997; Rao et al., 2017; Sato & Takasaki 2018b) by documenting how co-workers influence participation in firm campaigns. Co-workers as a peer group have received a lot of attention in research about peer effects on performance (Chan and Pierce, 2014; Cornelissen et al., 2017; Falk & Ichino, 2006; Mas & Moretti, 2009; Tan & Netessine, 2019) but far less attention in our context. On the determinants of vaccination, research mainly discusses how individual take-up is affected by laws, information, education, age, health status, health behavior, and lifestyle (Bradford & Mandich, 2015; Chang, 2018; Godinho et al., 2016; Maurer, 2009; Milkman et al., 2021; Milkman et al., 2022; Oster, 2018; Sato & Takasaki 2018a; Schaller et al., 2019; Schmitz & Wuebker, 2011). For a field experiment on how prompts affect flu vaccination among employees in a company context, see Milkman et al. (2011).

[2] While we investigate employees in a firm, others study on-site health interventions with school children (Belot et al., 2016; Just & Price, 2013; List & Samek, 2015). We further contribute to the research on public health interventions (Bütikofer & Salvanes, 2020; Cawley, 2010) and, more broadly, to the research on the impacts of medical technologies (Alam & Wolff, 2016; Bütikofer et al., 2020; Duflo et al., 2019; Jeon & Pohl, 2019). Given our focus on vaccination, we also add to research 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). There is also a growing number of

We hope to encourage other researchers to use our methodological approach to obtain causal estimates of economically and managerially relevant health outcomes, such as sickness absence (Bütikofer & Skira, 2018; De Paola et al., 2014; Kampkötter & Marggraf, 2015; Pichler, 2015; Van Den Berg et al., 2019; Ziebarth & Karlsson, 2010). Further, by providing experimental evidence for changes in behavior due to the vaccination campaign, we contribute to the research on health-related behaviors (Belot et al., 2020; Schneider & Sutter, 2020; Sutter et al., 2013), especially risky health behaviors (Arni et al., 2021; Cawley & Ruhm, 2011). Few studies have considered the possibility of medical technologies unintentionally causing moral hazard (Prasad & Jena, 2014; Richens et al., 2000), while papers in economics exploring moral hazard in the context of health interventions present mixed results (Doleac & Mukjerjee, 2018; Einav et al. 2013; Klick & Stratmann, 2007; Margolis et al., 2014; Moghtaderi & Dor, 2021). Our study therefore informs theoretical discussions about risk compensation for vaccinations (Auld, 2003; Talamàs and Vohra, 2020) and offers an alternative explanation of why firm campaigns to lower sickness may not always be as successful as expected in preventing employee absenteeism.

2. Experimental Design

This section describes the experiment that we conducted in cooperation with a bank in Ecuador. The bank focuses on consumer credit and is one of the largest credit card issuers in the country. Its headquarters is in Quito (the capital of Ecuador), and it has six branches across the country with over 1,300 full-time employees distributed across 31 divisions within 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.[3] In 2017, the bank decided to extend its annual campaign to all its employees and allowed us to experimentally modify incentives to take part in the campaign without the employees being aware of this modification.[4] To investigate how to increase campaign participation and its consequences, we implemented three interventions: we

quasi-experimental studies on flu vaccines that overall provide mixed evidence, as average health effects are often small or insignificant, while positive effects are found in years when the vaccine matched well with the prevalent flu viruses (Anderson et al., 2020; Carrera et al., 2021; Van Ourti & Bouckaert, 2020; Ward, 2014; White 2021). As unique features of our approach, we do not rely on assumptions, such as randomness in vaccine match quality, while being able to address issues of medical RCTs, including the problem of scrutiny and potential behavioral implications.

[3] 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%.

[4] We thus 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).

randomized assignments for on-site vaccination across weekdays, we implemented information nudges by varying the email content to invite employees to get vaccinated, and we changed the vaccine's price using income-dependent subsidies.

Given that health insurance does not cover flu vaccination, the bank decided to provide the vaccine for free to areas of the business that had participated in campaigns in previous years and to partially subsidize it for new participants. Since the company opposed randomized subsidies, we used information on employees' income to allocate this subsidy. An income threshold for the subsidy was chosen to maximize the sample size while passing density and covariate smoothness checks before the intervention. Employees who earned less than $750 per month would pay $4.95 to get vaccinated, while those who earned more than $750 would pay $7.49 (the total vaccine price was $9.99). Each employee was informed about the applicable price of the vaccine in their invitation email. This email included basic information about the campaign and informed employees that the payment for the vaccine would be deducted directly from their paychecks if they opted to get vaccinated. The email also contained the assigned day and time for their vaccination. Figure A1 shows an example of an invitation to receive the low-price flu shot on a Thursday morning.

To examine the effects of opportunity costs and information, we randomly assigned all employees into one of four groups that are orthogonal to each other.[5] First, we are taking advantage of an organizational bottleneck: due to limited capacity, not all employees could be invited to get vaccinated during the workweek (Wednesday, Thursday, or Friday). Therefore, our first treatment increased the opportunity costs of participation by randomly assigning employees to get vaccinated on a *Saturday* (see Figure A2 for an example invitation). Based on data from the employees' magnetic swipe cards used to enter company buildings, we know that 96.6% of the employees do not work regularly on Saturdays. Hence, most individuals in the *Saturday* group had to arrange their schedules in order to travel to the bank for vaccination, while employees with workweek assignment were allowed to take time off their duties to get vaccinated. Nevertheless, one may also argue 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.

The firm allowed us to further implement two information nudges in the population of employees

[5] 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 did not have a company email address.

assigned to get vaccinated during the workweek. To determine the impact of our information treatments, we distinguish between three groups of employees with a workweek assignment, including a *Control* group without an information nudge (see Figure A1 for an example). The first nudge highlighted the social benefits of flu immunization (*Altruistic*). In addition to the information provided to the *Control* group, the email included the following wording: "Getting vaccinated also protects people around you, including those who are more vulnerable to serious flu illness, like infants, young children, the elderly, and people with serious health conditions that cannot get vaccinated" (see Figure A3). The second nudge highlighted the individual benefits of flu immunization (*Selfish*). In addition to the information provided to the *Control* group, the email included the following wording: "Vaccination can significantly reduce your risk of getting sick, according to health officials from the World Health Organization and numerous scientific studies" (see Figure A4). Note that we kept these two additional messages as unobtrusive as possible to prevent confounding the effect of information with salience or other behavioral factors.

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 of the experiment. For them, the campaign was just a regular activity organized by the HR department. Employees are used to receiving emails from HR; according to the HR manager, they typically read them carefully. A reminder was sent out using the same email account a week later. The campaign ran from November 8 to November 11, 2017, at locations within the bank's offices in each branch. The bank hired an external medical team to supply and inject the vaccines. Finally, the bank conducted a post-intervention survey during March and April 2018.[6]

3. Data from the Firm Campaign

This section describes the data used in our empirical analyses. First, we were granted access to the firm's administrative records about its employees, including gender, age, education level, children, tenure, income, medical diagnoses, and sick days. The records also provide information about the employee's job and work unit, i.e., the position within the bank's organizational structure. The work

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

units within the firm were established more than two decades ago. Second, we collected campaign participation information in the form of 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 knowledge and beliefs about vaccination, habits related to health, relationships with co-workers, opinions about the campaign, and work motivation.

--- Table 1 about here ---

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

The administrative data include information on medical diagnoses (sickness incidence) and sick days (sickness absence) obtained from two sources: on-site doctors and medical certificates from external doctors (72 different physicians in total). It is important to note that Ecuadorian law establishes that employees must present a medical certificate to receive a sick day.[7] Consequently, the on-site doctors report every patient's visit to HR, including the diagnosis (the type of disease), whether they granted sick days or not, and the number of sick days granted. Similarly, if an employee takes time off work to go to an outside doctor, they must present a medical certificate to HR that indicates the diagnosis and the number of sick days granted (if any). Hence, in addition to sickness absence, we also observe milder health problems when employees were diagnosed with an illness, but the doctor did not consider the condition severe enough to grant a sick day. Between January and early November 2017 (before the intervention), two out of three employees had some illness, and 37% had at least one sick day (see Table 1).

The doctors diagnose their patients using a combination of a physical examination, blood tests,

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

and culture tests. The procedures undertaken are recorded in individual medical records, to which we do not have access. Diagnoses that name the patient's illness as "flu" provide us with a narrow definition of flu-related sickness. If flu cases occur with complications, the data report the complication as the diagnosis and do not mention the flu explicitly. To address this issue, we use an extended definition of flu-related sickness, which includes diagnoses that indicate complications caused by the flu, according to a third-party physician (who also provided us with an even broader definition for additional analyses). Any other respiratory disease not classified by this doctor as flu is listed as a non-flu respiratory disease. A second physician has verified these measures distinguishing flu- from non-flu-related illness which provides further confidence in those measures.

Table 1 also shows evidence on the balance of treatment assignment. Columns 2–5 present the mean employee characteristics across the four groups; all characteristics have almost identical means across all groups. Column 6 shows the p-value of a joint significance test of differences of means for each characteristic. We cannot reject the null hypothesis that the means are the same across groups, implying that the randomization was successful. Using the Kruskal–Wallis rank test yields the same result. Finally, we test whether participation rates in the pre- and post-intervention surveys are different across treatments; no statistically significant difference is detected.[8]

4. Determinants of Participation in the Firm Campaign

This section studies how monetary and non-monetary determinants affect working adults' decisions to participate in a firm campaign to get vaccinated against the flu. Specifically, we consider the effect of opportunity costs, information nudges, prices, 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.[9] We conclude that this price change may be too small to affect firm campaign participation.

For each treatment, the last row in Table 1 presents the rate of employees participating in the campaign. The *Control* group shows a rate of 22%, the *Altruistic* treatment shows a rate of 17%, and the *Selfish* treatment shows a rate of 19%. Comparing the three groups suggests that the information nudges were insufficient to increase take-up. In contrast, being assigned to get

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

[9] Figure A9 shows no visible discontinuity across the threshold. Regression discontinuity estimates also indicate no significant decrease in take-up due to a higher vaccine price, which is robust to a variety of checks (see Table A1).

vaccinated during the workweek substantially increased participation in the campaign, as take-up on average more than doubled compared to the *Saturday* treatment with a take-up rate of 8%.[10]

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

By running regression analyses, we now inspect the robustness of the initial findings based on our descriptive comparison of treatment differences in campaign participation. To do so, 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 participating in the campaign by getting vaccinated. $Saturday_{ic}$, $Altruism_{ic}$, and $Selfish_{ic}$ are dummy variables that indicate treatment assignment. We estimate the effect of the 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. In our regression analyses, we control for whether the branch was in Quito or not (γ_c) to account for differences in the implementation of the vaccination day assignment across branches.[11] Since employees outside of Quito were exogenously assigned to the workweek, one could also argue against controlling for Quito fixed effects, given that all the non-Quito employees were lucky to

[10] While our randomized intervention allows us to identify a substantial boost in campaign participation, one may wonder whether the campaign's impact on the number of vaccinated employees may differ due to the option to get flu shots outside of the company. Hypothetically, if the campaign was successful in getting employees vaccinated but at the expense of outside vaccinations, this could play a role in the discussion of the benefit of the campaign (Section 5) and hence may even serve as an alternative explanation why such health campaigns are not beneficial for companies. To learn more about this, we use data from the post-intervention survey, which included a question on vaccination during the flu season with three answer categories: vaccinated at the firm campaign, vaccinated outside the campaign, and no vaccination. According to their responses, 59 employees got vaccinated outside the campaign. While none of them participated in the campaign, according to the records, the question wording was not limited to flu vaccination and, hence, employees could be reporting other types of vaccinations as well. Furthermore, there is some evidence for misreporting on vaccination, as 18 employees stated that they participated in the campaign, but they did not according to the records. Meanwhile, one employee who actually was vaccinated stated no vaccination. With these issues in mind, we conducted some checks using the information from the survey. First, we run a robustness check by excluding those 19 employees misremembering whether they were vaccinated from the analysis. The results do not change when we do so. Second, the same holds when we exclude employees claiming vaccination outside the campaign. Finally, we test whether these self-reported vaccinations differ significantly according to treatment status. This is not the case.

[11] At the time of our study, 18 percent of the bank's employees worked outside of Quito. Depending on the location (see Figure A5), they could get vaccinated in a single day. In practice, branches in the coastal areas were randomly assigned for vaccinations to be carried out on a Wednesday, and branches in the highlands were assigned to Thursday. Consequently, we notice a substantially higher participation rate of 31 percent outside of Quito compared to 13 percent in Quito where employees were randomly assigned to Wednesday, Thursday, Friday, or Saturday.

have received the reduced opportunity costs treatment. Hence, the following regression results can be considered a conservative estimate in this regard.

Table 2 presents the effects of the different treatments on campaign take-up. Column 1 shows that assigning employees to *Saturday* decreased take-up by 7.9 percentage points compared to the workweek *Control* group, which is statistically significant at the 1% level. Hence, minimizing opportunity costs associated with vaccination clearly helps increase campaign participation, given that the treatment effect is similarly large as the average take-up in the Saturday group (see Table 1), which points to a relative increase of roughly 100%. 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 campaign participation, given the substantial effect of reducing opportunity costs.[12]

---- Table 2 about here ---

Columns 2–4 of Table 2 show the results of further robustness checks. Specifically, column 2 shows that controlling for vaccine price, income, tenure, division in the company, gender, age, and education level does not affect the estimates. Column 3 shows that the main findings are robust when we exclude cases of non-compliance.[13] Column 4 complements our analysis by restricting the sample to employees in Quito. In this subsample, assigning employees to *Saturday* decreased take-up by almost nine percentage points, which is significant at the 1% level and, vice versa, confirms an increase in participation by more than 100% due to lowering opportunity costs.

Lastly, in Column 5, we inspect whether assignment to different days in the week affected take-

[12] While our evidence on insignificant effects of nudges aligns with other research on vaccine take-up (Bronchetti et al., 2015; Godinho et al., 2016), one may wonder if information treatments have to be highly salient to accrue an additional effect on take-up rates in a company such as this. 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* nor to the *Selfish* treatment remembered their respective statements better than the *Control* group. Note, however, that there was a months-long distance between the actual firm campaign participation and the post-survey. Another issue could be information spillovers: this is unlikely since the information was provided directly to the treated employees via email. Finally, we do not think that the email was too long to read since the email contained a prominently placed image with little text (see Figure A3 and Figure A4 for the altruistic and selfish treatment).

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

up differentially. Using the Quito sample, we regress our indicator of take-up on dummies for each assigned day (*Wednesday*, *Thursday*, *Friday*) while excluding *Saturday* as the baseline in this analysis. The estimates show that the take-up rates for *Wednesday* and *Thursday* are 8 pp. larger relative to the Saturday baseline of 8 percent. Notably, Friday is not statistically different from *Saturday*, albeit close to the ten percent level.[14] These results are consistent with reduced opportunity costs during the workweek relative to Saturday.

To further understand opportunity costs as an underlying driver of campaign participation, we inspect possible heterogeneity in treatment effects across subgroups of our study population by focusing on differences across gender, distance to work, and employees with and without children (see Table A3). For the information treatments, we find no differential effects across subgroups, as all the estimates are small and statistically insignificant. For the assignment to *Saturday*, we find similar treatment effects for men and women, and, hence, no effect heterogeneity along the lines of gender. For those employees who live further away than the median, we also find a similar treatment effect compared to those who live closer to the bank. This implies that additional travel costs, as captured by the distance to work, are not the main factor driving the difference in take-up rates between employees assigned to the workweek and *Saturday*. Finally, we consider differences between the effects for employees with and without children, as having children may imply higher opportunity costs at the weekend due to increased family obligations. We find that assignment to *Saturday* reduces campaign participation of employees with children by roughly twice as much as for employees without children. Although the difference between these two effects is not statistically significant, this picture is consistent with the idea that opportunity costs increase for individuals assigned to *Saturday*.[15]

In conclusion, several pieces of evidence support that the difference in campaign participation

[14] 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*. Note that being assigned to *Friday* can increase the opportunity cost of campaign participation because it is only a six-hour workday rather than an eight-hour workday like the other weekdays. This supports the idea that *Friday* assignment provides us with an opportunity to enlarge the "high-opportunity cost" treatment group for additional analyses of the consequences of exogenously increased take-up.

[15] Note that the treatment effects in Table A3 become stronger when we exclude Quito fixed effects from the analysis, but this does not change the conclusions about effect heterogeneity. Note also that there are ways to further disentangle transportation costs from opportunity costs in additional regressions. First, we add an interaction term between *Saturday* and distance to the bank. The results show that an additional km in travel distance (i.e., transportation cost) does not have a differential effect on the likelihood of campaign participation on a weekday compared to *Saturday*. Second, we add interaction terms between *Saturday* and vaccination price groups, which reveals no statistically significant differences between the high-price, workweek vaccination group and the low-price, *Saturday* vaccination group. These results are inconsistent with the idea that heterogeneous transportation costs drive our results.

between employees assigned a day during the workweek and *Saturday* corresponds to a change in the opportunity costs of vaccination. In the rest of our analyses, we exploit this variation in campaign participation resulting from lower opportunity costs due to workweek assignment.

4.2 Peer Effects on Participation in the Firm Campaign

Peer effects may play an important role when deciding whether to participate in a firm campaign to get vaccinated. On the one hand, if more co-workers get a flu shot, the prevalence of the disease may decrease, making it less likely for others to get sick. Thus, if there are costs involved in getting vaccinated, it may be optimal for some employees not to do so if their peers decide to vaccinate. Theoretically, this free-rider problem can result in a Nash equilibrium, where nobody takes the vaccine (Chen & Toxvaerd, 2014). On the other hand, peer vaccination may increase the probability of individual campaign participation. Such positive peer effects in vaccination could occur, for example, because individuals care about how others perceive them (Karing 2018). As a result, co-workers may imitate the behavior of their peers to conform to perceived social norms.

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

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

where $Prop.Takeup_{-i,jc}$ corresponds to the proportion of peers assigned to get vaccinated on the workweek for i in unit j, $Workweek_{ic}$ is the assignment to vaccinate on the workweek for individual i, and $\bar{X}_{-i,jc}$ are the average observable characteristics of peers j. Manski (1993) shows that if we estimate equation (2) by ordinary least squares (OLS), then self-selection, common environmental factors, and reflection will confound the true peer effect β_1. However, in our design, employees are randomly assigned to participate in the firm campaign during the workweek independent of their unit. This creates exogenous variation across units that affects the proportion of peers who get vaccinated independently of employee i's decision to vaccinate since, 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:

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

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:

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

In our design, the exclusion restriction holds because the proportion of peers vaccinated during the workweek is the only channel through which the proportion of peers assigned to the workweek can affect the individual's vaccination decision. Hence, we can combine the estimates from equations (3) and (4) to obtain an instrumental variable (IV) estimate of the effect of the proportion of vaccinated peers on an individual's campaign take-up. Variation across units from the proportion of peers assigned to the workweek and variation within the unit from individual assignment to the workweek allow us to identify both the individual employee 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 ---

Table 3 presents the main results for peer effects on participation in the firm campaign as measured by vaccination take-up. 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 who participated in the campaign 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 coworker participation positively affects individual participation 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 coworkers getting vaccinated increases take-up by 7.9 percentage points. In additional analyses, we find that the results do not change qualitatively when

we do not control for Quito fixed effects or only use the Quito subsample.[16]

Using the available data, we conduct further analyses to shed more light on potential mechanisms behind the peer effects on participation in the campaign.[17] In our interpretation of the available evidence, the positive peer effects most likely result from individuals feeling pressured to follow behavior that they deem socially acceptable.

5. Analysis of Health, Sickness Absence, and Health-Related Behaviors

In this section, we exploit random assignment to a vaccination appointment during the workweek to study whether or not increased participation in the firm campaign reduces sickness absence by improving health. Workweek assignment thereby identifies the effect of a successful intervention to improve vaccination in a company workforce and serves as the relevant estimate for organizations evaluating the success of health campaigns and the economic benefits of increasing participation. To shed more light on the potential mechanisms underlying the impact of the firm campaign, we employ the same approach using random workweek assignment to explore changes in health-related behaviors (in Section 5.2).

[16] Results in Table 3 are robust to controlling for individual workweek assignment. Note that the effect of own assignment to the workweek is within the confidence intervals of the estimates in Table 2, suggesting that spillovers from peers do not affect identifying the effect of individual treatment on take-up. Furthermore, as seen in Table A4 (Panel A), the results are robust to controlling for the total number of employees in the unit, considering that smaller units may have larger proportions. Results are also robust when we control for the mean age and gender of the peers. For another check, we change the definition of the instrument. By considering the timeline of events and defining our instrumental variable as a cumulative proportion of cases separately for each individual, we avoid considering future vaccinations of coworkers. As seen in Table A4 (Panel B), peer effects remain significantly positive when using only variation in peers assigned to vaccinate on the same day or before. Note that we do not have information to investigate order effects within each day. Finally, since some employees in a unit are in different locations, we adjusted our overall peer instrument to a within location and a between location peer instrument. We found no significant difference between the two. This is inconsistent with the idea of free riding if we assume that the potential health benefits of coworker vaccination can only be relevant for coworkers at the same location.

[17] In a first analysis, we use data from a post-intervention survey questionnaire on beliefs and knowledge of vaccines. Reduced-form analyses reveal no significant effects on responses to any of the questions, as shown in Table A5. This suggests that peer behavior neither affected beliefs nor supplied new information about the vaccine. The survey evidence also speaks against the idea that employees were happy or upset that some coworkers had the chance to vaccinate during work hours while others did not. In particular, a reduced talking activity could imply that employees are upset when assigned to the weekend. However, we do not find a significant reduction in the propensity to talk with coworkers when the proportion of coworkers with workweek assignment increases. In further analyses, we estimate an expanded model with an interaction between individual workweek assignment and peer workweek assignment. This reveals no significant result that may inform us about mechanisms. In contrast, by including a unit-size interaction in our main model, a separate analysis reveals that units with small unit sizes drive the effects. This aligns with the idea that pressure to conform to peer behavior is stronger in smaller groups. Finally, we estimate whether different subgroups of peers affect individual vaccination decisions differently based on the idea that certain groups are more likely to create feelings of belonging than others. For instance, individuals may have a particular incentive to follow the behavior of peers of the same gender, which could be seen as a relevant peer group. In line with this, we observe that the behaviors of the same gender group seem more relevant to individuals' actions than peers of a different gender.

5.1 Health and Sickness Absence

In a first step, we examine the raw treatment effects of the firm campaign on health outcomes during the flu season, i.e. in the time period from November 2017 to February 2018. We focus on the propensity of sickness incidence, i.e. being diagnosed sick, and sickness absence, measured by whether or not the employee was granted a sick day. From the perspective of the company, sickness absence can be seen as the economically more relevant indicator of a successful health campaign since incidences of sickness without absence from work are usually minor health problems.

--- Figure 1 ---

Figure 1 shows that the likelihood of sickness incidence for employees with a workweek assignment declines by 4.3 percentage points (8% of the baseline), which is statistically insignificant at conventional levels. Hence, the probability of being sick during the flu season does not clearly differ from the probability that we observe for the employees with a Saturday assignment, which is about 50 percent. When turning to sickness absence, we note that there is a 27 percent likelihood for the average employee to have at least one sick day granted when assigned to Saturday. Interestingly, this outcome variable does not decrease at all, but instead we observe a slight increase by 1.0 percentage points (4% of the baseline) when being assigned to the workweek. Again, the average treatment effect is not statistically significant at any meaningful level.

In the next step, we check the robustness of these initial findings by running regression analyses, where we estimate the effects of the firm campaign on both health outcomes and, among other things, control for Quito fixed effects. As shown in the previous section, if a person gets vaccinated, the likelihood that their peers will also get vaccinated increases, which may improve the health of co-workers and decrease the transmission rate of the disease. Since not only the number of employees varies substantially across the company's 142 units (see Figure A10), but also the proportion of vaccinated peers (with vaccination rates between 0% and 100%), this could indirectly affect health outcomes. Hence, by regressing the health outcomes on assignment to the workweek and the proportion of peers assigned to the workweek, we also address the role of possible externalities due to participation in the health campaign.

--- Table 4 about here ---

Table 4 shows estimates of the consequences of participation in the firm health campaign. Panel A presents the regression results from our analysis of the probability of being diagnosed sick. During our analysis, we also compare the effects of increased participation in the health campaign (Column 2), based on random workweek assignment, to simple correlations when we focus on actual vaccinations (Column 1). This shows that there is no significant correlation between getting vaccinated and the probability of being diagnosed with an illness. Our main results confirm that the campaign to get employees vaccinated had no effect the probability of being diagnosed as sick. Being randomly assigned to a day in the workweek decreases the probability of sickness by 1.7 percentage points, which is insignificant at conventional levels. Regarding peer effects, the estimates in Columns 1 and 2 indicate that the proportion of vaccinated peers does not affect the probability of being diagnosed with an illness. This implies that underestimating individual health benefits due to vaccination is not an issue in the absence of any significant externalities from peer to peer, be it via exogenously encouraged take-up or health spillovers.[18]

Table 4 Panel B shows the results for the probability of having a sick day. The correlation analysis in Column 1 suggests that vaccination decreases the probability of having a sick day by roughly four percentage points, which is insignificant at conventional levels. The main results in Column 2 imply that campaign participation does not affect sickness absence. Being randomly assigned to a day in the workweek increases the probability of having a sick day slightly by 1.3 percentage point, which is insignificant as well. Furthermore, the evidence shows that the proportion of vaccinated peers does not affect the probability of sickness absence. From a firm and managerial perspective, these results suggest that the investment in the health campaign was not worthwhile.[19]

[18] Our findings on the effects of the firm campaign on health outcomes are robust to various checks and deeper analyses. First, the results remain the same if we remove the proportion of peers and estimate only the individual workweek effect. Second, all the results in this section are robust when only the Quito subsample is used. Third, when we compare the effects across work units, we do not find that units with large shares of workweek assignments have better health, although they should in case there were significant health benefits of the campaign (be it in the form of individual health benefits, externalities, or both). Fourth, our findings remain the same when we re-define the proportion of peers and use the location-adjusted version of the workweek instrument by focusing only on co-workers of the same unit at the same location (see footnote 16). Finally, our setting potentially lends itself to study the health effects of campaign participation via two-stage least squares (2SLS) analyses using instrumental variables based on workweek assignment. F-statistics of 6.6 (individual vaccination) and 8.9 (peer vaccination) are below the common threshold for IV relevance when we focus on a specification which includes Quito FE. In addition to excluding Quito FE, we can test a specification where we enlarge the high opportunity cost treatment group by comparing an extended weekend (Friday, Saturday) to a shortened workweek (Wednesday and Thursday). All of our 2SLS analyses lead to the same results as presented in this section.

[19] We come to the same conclusions when we analyze the number of sick days as the dependent variable. Note in this context that some of the diagnoses include severe illnesses such as cancer, meaning that many recorded sick days are not related to the flu. When we exclude two outliers with more than 100 sick days from the analysis, we find an insignificantly positive effect of the workweek assignment, in line with the result in Table 4 Panel B.

To provide a more precise answer to the question of whether the firm campaign could have been economically beneficial for the company carrying out the intervention, we perform a back-of-the-envelope calculation and determine the net benefit of the campaign using our evidence on sickness absence. As can be seen in Table A6, we first calculate statistics, such as the average daily wage and sick day probability, for employees with a Saturday appointment who serve as the control group in this analysis.[20] We find that the average wage paid for a sick day incidence by the firm is $16.45 in the Saturday group. We then use the main treatment effect (of 1.3 pp.) for the assignment to the workweek from the previous paragraph, to calculate a worst-case, average-case, and best-case scenario where the worst- and best-case are the upper and lower confidence intervals of our workweek effect. When comparing the mean sick day incidence of those three scenarios relative to Saturday, we find a wage loss due to sickness incidence from being offered campaign participation during the workweek that ranges from $5.00 to -$3.51. As a result, we find that there may have been a small benefit for the firm of roughly $3 in the best-case scenario for each additional employee incentivized to get the flu shot.[21] However, once we take into account the vaccine subsidies paid to employees who got the vaccine in any case, including those without the incentive of a workweek invitation, we find that the firm campaign costs outweigh the benefits in each scenario. Moreover, there are opportunity costs for the firm, for example due to the loss of working time for other projects that the HR department could have devoted their time to. We therefore conclude that even in the best-case scenario for this flu season the campaign has not resulted in measurable benefits.

To complete our analysis of health outcomes, we exploit data on medical diagnoses to investigate possible impacts on the probability of being diagnosed with the flu. This discussion can be found in Appendix A. According to the results (see Tables A8 and A9), there is no evidence of changes in the probability of having the flu, which also is true for the proportion of vaccinated peers. To further assess to what extent we can rule out meaningful health effects, we use the flu sickness information to implement an equivalence test based on two one-sided hypothesis tests (Hartman & Hidalgo,

[20] In our analysis, we do not consider potential benefits due to other mechanisms, such as morale and productivity. Given that we do not find effects on illness and sick days that are first-order outcomes, it is likely that the campaign did not affect productivity or morale. Indeed, Table A7 presents estimates on self-reported productivity and the duration of the working day as measured by the employees' magnetic card swipes for entering and exiting the bank. Neither of those outcomes are significantly altered by the workweek relative to the weekend assignment.

[21] As an extension of our cost-benefit analysis, we can also consider the number of sick days. When we scale our results from Table A6 by the mean number of sick days, the range of potential benefits and costs increases when comparing best-case and worst-case scenarios. Given the above-mentioned problem of outliers in the number of sick days, we could also make use of the median as an alternative to the average number of sick days. This leads to the same results as in Table A6 since the median in the control group is exactly one sick day.

2018; King et al., 2000; Lakens, 2017; Rainey, 2014). By considering public health figures provided to policymakers, this analysis adds further support to the conclusion of lacking health benefits due to the firm campaign (see Figure A12).

In summary, our results show that the campaign to increase flu vaccination among employees was ineffective in improving health and, therefore, it was not economically beneficial. A simple explanation could be that the flu vaccine was medically ineffective. As discussed in the previous literature and Appendix A, even health institutions in support of vaccination acknowledge that the quality of the flu vaccine can vary substantially across flu seasons. At first glance, this interpretation of a medically ineffective vaccine aligns with our evidence showing no health improvements for employees, including flu-specific illness. However, two alternative explanations are compatible with a medically effective vaccine. First, the problem of flu sickness may be too rare in a healthy working-age population to create any significant health benefit 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 vaccine's protection and engage in riskier behaviors; for example, they may avoid going to the doctor or wait longer than unvaccinated individuals to do so when they feel flu-like symptoms. In addition, vaccinated individuals may take fewer protective measures. In the next section, we therefore explore the impact of the firm vaccination campaign on employee behavior, also to learn more about a possible explanation for its lack of effectiveness in reducing sickness absence.

5.2 Behavioral Effects

To explore possible implications of the firm campaign on health-related behaviors, we first inspect whether employees with a higher chance of campaign participation (due to workweek assignment) reacted differently than employees with a lower chance (due to Saturday assignment) as a result of flu-like symptoms. An important factor here is that non-flu respiratory diseases have symptoms like the flu, but the vaccine does not promise immunity to prevent them. Thus, flu vaccination should not affect the probability of being diagnosed with a non-flu disease, so any effect on this probability would imply a change in how individuals react when they contract or show symptoms of a respiratory disease. For example, suppose vaccinated employees feel more protected. In that case, they might be less likely to go to the doctor when they felt flu-like symptoms, thus decreasing the probability of being diagnosed with a non-flu disease. In particular, this would concern mild

illnesses where it is up to the individual to decide whether to consult a doctor.

To implement this test, we utilize the richness of the data on medical diagnoses to identify cases of non-flu respiratory illnesses and exploit a national emergency that occurred during our investigation period. In January 2018, Ecuador experienced a significant increment in flu cases nationwide (Direccion Nacional Epidemiologica, 2018). As a result, the Ecuadorian government launched a massive media initiative asking people to go to the doctor if they felt any flu symptoms. If employees that participated in the firm campaign were less concerned about the flu, we argue that they may not have followed the government's recommendation, resulting in fewer visits to the doctor and fewer non-flu respiratory diagnoses in that month.

--- Figure 2 about here ---

Figure 1 presents the effects of being assigned to a campaign vaccination appointment during the workweek on flu and non-flu respiratory diagnoses for each month of the investigation period. In line with cross-section estimates (see Table A8), being assigned a flu shot during the workweek does not affect the probability of being diagnosed with the flu. For non-flu respiratory diagnoses, we do not expect to find any effects in the absence of changes in behavior. However, this only applies to November, December, and February. In January, when the government encouraged individuals to go to the doctor, being assigned a vaccination appointment during the workweek decreased the probability of being diagnosed with a non-flu respiratory disease.[22] These results are consistent with the idea of riskier behavior among employees who may have thought they were more likely protected against the flu thanks to the firm vaccination campaign.

By investigating possible implications for the likelihood of employees visiting the doctor at the on-site health center, we can further inspect the idea that employees felt more protected during the health emergency. The bank's health center is convenient for its employees because they do not

[22] Comparing the significance levels for each month in Figure 1 for flu vs. non-flu, we find that we cannot reject the null that the effects are the same for all months except for January, where we find a significant difference between flu and non-flu effects of 6.8 percentage points (p-value = 0.016). In one of our additional robustness checks, we confirm a significant January effect when controlling for employee fixed effects using a month-based difference-in-difference approach (Figure A11). We also estimate the effect of assignment to an appointment during the workweek on non-flu diagnoses, collapsing the data of the four months to a cross-section. In this specification, being assigned to the workweek decreases the probability of being diagnosed with a non-flu respiratory disease by 7.7 percentage points, almost identical to the effect in January. Finally, the results are similar when we use the more severe measure of sick days, which reveals a less precisely estimated effect for January, in line with the idea that the unvaccinated are more likely to go to the doctor in the presence of predominantly mild flu symptoms.

have to ask for time off to go to the doctor; they can take a few minutes off work for a visit. Before the firm campaign, the on-site doctors accounted for 77 percent of all cases of diagnosed sickness.

--- Figure 3 about here ---

Figure 2 presents the effects of assigning employees a flu shot during the workweek on the probability of visiting the on-site doctor, broken down by month. We find no significant effect in November, December, or February. In January, a workweek assignment for vaccination decreased the probability of going to the on-site doctor by 8.6 percentage points (21% of the baseline). This finding further supports the idea that the participants of the campaign felt more protected and were willing to take risks concerning their health, unlike unvaccinated employees who preferred having a check-up at the doctor in January. While it is debatable if employees with mild flu-like symptoms should ignore the government's advice by not going to the doctor, the critical point for our discussion is that such a behavior could be seen as risky and, hence, consistent with moral hazard.

--- Table 5 about here ---

To learn more about health-related 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 on a Likert scale where one means "never" and ten means "all the time." We combine these four measures by averaging over them to obtain an index of health-related habits. Table 5 shows the effects of assigning employees to vaccination during the workweek on these outcomes. We find significant reductions in health-related habits for employees assigned to the workweek by 0.5 points (7.9% of the baseline). When we decompose the measures, we find that participation in the firm health campaign does not seem to affect how often employees wash their hands (1.2% of the baseline), which could be because almost all employees reported that they wash their hands regularly. Assigning employees weekday appointments shows a negative but statistically insignificant effect on how often the employees exercise (4.9% of the baseline) and take nutritional supplements (19.5% of the baseline). The effect on how often employees carry an umbrella is statistically significant at the 5% level and robust to adjusting for multiple comparisons following Anderson (2008), decreasing the frequency of carrying an umbrella by 1.22 points (17.6% of the baseline).

While this provides additional support for the idea that employees differ in their willingness to engage in risky 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.[23] To link our result more closely to the issue of vaccines, we investigate heterogeneous effects across individual beliefs on the effectiveness of vaccination using the firm campaign pre-intervention survey. We find that the umbrella effect is driven by employees who strongly believe vaccines are effective (Table A10). This conforms to the idea that employees think they can neglect measures that they believe help prevent respiratory illnesses, because they expect an effective firm campaign to provide protection against sickness.

In summary, our evidence on riskier behaviors regarding health indicates a potential concern for company management and policymakers for the overall effects of health interventions, given that any health risk due to behavioral changes could threaten an intervention success. Note that our evidence suggests moral hazard, but it does not clearly confirm the idea that behavioral changes increase chances of getting the flu. While using an umbrella might help to avoid a cold by not getting soaking wet on a rainy day, it does not yield protection against infectious diseases, independent of what the people in our setting may believe. Visiting the doctor might also not help in this respect, even though our findings can be interpreted as indicative of other riskier behaviors which may indeed be relevant for the probability of getting the flu.[24]

6. Conclusions

The private benefits of firm health campaigns are ex-ante unclear. Individual behavior of employees

[23] Psychology research shows that cultures across the world associate a higher prevalence of influenza in a cold and wet environment with the belief that individuals are more likely to catch the flu by getting wet or cold (Au et al., 2008; Baer et al., 1999; Helman, 1978; Sigelman et al., 1993). This may play a particular role in our context of a country located on the Equator Line. In Ecuador, there are no marked seasons in the year, and temperatures can fluctuate between the upper forties (°F) and the lower eighties (°F) in one day. Also, there are no accurate forecasts for rainfall.

[24] There are a few alternative interpretations unrelated to moral hazard. First, if doctors misdiagnose the flu, some diagnosed non-flu cases could have been flu cases. However, as our data include diagnoses from 72 different doctors from different health centers and hospitals, it is unlikely that there is a systematic misdiagnosis issue. Additionally, the results are robust to using a broader definition of flu-related illness. Second, if a doctor learns that a person showing flu-like symptoms is vaccinated, they might be more likely to misdiagnose those symptoms as a non-flu respiratory disease. However, the results in Figure 1 show that employees assigned to appointments during the workweek, who were more likely to get vaccinated, had a lower rate of diagnosis of non-flu respiratory diseases than those assigned to the weekend. Third, the data on medical diagnoses correspond to employees who visited the on-site doctor or an external doctor during work hours. Sick employees who saw an external doctor outside work hours and were not granted a sick day are coded as healthy. However, this measurement error can bias the flu and non-flu estimates only if it is correlated with the random assignment to a workweek appointment, which is unlikely. Similarly, adverse selection or related phenomena cannot be an issue in our study using random variation in campaign participation.

may threaten the success of such health interventions in multiple ways. First and foremost, individuals can decide not to participate in an intervention. By exploiting the opportunity to modify a firm campaign to improve employee health, we find that a small price change and information nudges that appeal to either the selfish or altruistic motives did not induce a change in employee behavior. In contrast, it appears that reducing opportunity costs could have a substantial effect on employee participation in a health campaign for working-age adults. Additionally, peer behavior can positively influence participation in the firm campaign. For health benefits, the exogenous increase in vaccination did not significantly affect any of our outcomes. While we cannot rule out that the vaccine was medically ineffective or that the flu was no major health issue in the company workforce, we have evidence of employees adopting riskier health behaviors. This arguably constitutes an important pathway by which individual behavior can limit the effectiveness of firm campaigns and health interventions in general. Based on our cost-benefit analysis, we conclude that the campaign was not economically beneficial for the firm.

In light of recent discussions on the generalizability of experimental results from the field (Czibor et al., 2019; Riener et al., 2020), we assess the external validity of our findings by discussing four relevant conditions, as suggested by List (2022). First, in terms of selection, our study population from a large bank is more educated and has a higher income than Ecuador's general population, which speaks to the work context in developed and developing nations in the Western world. Since 2015, the flu season in Ecuador has matched those in the United States and Europe (WHO FluNet, 2021), and the flu vaccines used in Ecuador also match those used in the United States and Europe. Working conditions in large companies are similar across these countries. Therefore, the results are relevant for managers and policymakers when discussing how to increase participation in health campaigns within companies. Second, in terms of attrition, we have perfect compliance in delivering the experimental treatments, and most of our outcomes are measured with administrative data from the firm. Third, considering the naturalness of the choice task, setting, and time frame, we use a natural field experiment. Thus, individuals are engaged in a natural and familiar task, unaware of the intervention, and not placed on artificial margins. Finally, the magnitude of the effect of opportunity costs on vaccine take-up is comparable to results in other research that varies costs in different settings (Banerjee et al., 2010), and in terms of scaling our insights, we do not expect a substantial change with respect to the health results conditional on having a similar context.

Our study has multiple practical implications. From a research perspective, employing a

randomized encouragement design is helpful to circumvent ethical dilemmas when studying the consequences of interventions relevant to people's health and absenteeism. It allows for identifying both potential health benefits and behavioral changes in an unbiased way to inform policymakers and firms that could consider important phenomena like moral hazard in health-related behavior when designing interventions such as preventive health campaigns. A promising mechanism to mitigate this issue could be to increase awareness of the importance of other protective measures so that people will not rely only on the protection potentially provided by medical technology. This policy conclusion also informs the discussion on similar behavioral phenomena in the context of the COVID pandemic (Andersson et al. 2021).

Having said that, our findings may also be seen as informative in light of the recent research on how to affect COVID vaccinations, where a large number of controlled interventions has produced mixed evidence on the determinants of vaccine take-up.[25] There are certainly some differences between COVID vaccines and flu shots, which could explain why we find strong effects of incentives on the decision to get vaccinated. For example, for attitudes towards vaccination, individuals may be less stubborn in their decision-making and hence more responsive to incentives when not facing a controversial and politically loaded issue. Nonetheless, we believe that opportunity costs and peer effects may also be relevant in many other contexts outside of companies like the one investigated in our study on the topic of flu vaccines.

Hence, as another important lesson learned from our investigation, we inform policymakers at the company level as well as at the public level about how to encourage participation in health interventions. By having identified two cost-effective measures that can increase vaccine take-up in a workplace context, we hope to inspire policies to improve participation in health campaigns. For example, based on our finding about the importance of opportunity costs, one may think of using mobile campaigns available on busy days and in locations where people usually congregate. Our findings on the role of peer behavior suggest that campaign organizers could increase participation by implementing mechanisms to incentivize whole groups of individuals.

Finally, our paper shows that there may not be sufficient private incentives for firms to implement

[25] Examples of experimental papers on the determinants of COVID vaccine take-up come from Campos-Mercade et al. (2021), Dai et al. (2021), Jacobson et al. (2022), Rabb et al. (2022), Reddinger et al. (2022), Saccardo et al. (2023) and Schneider et al. (2023). For quasi-experimental research on COVID vaccines, see for example, Girma & Paton (forthcoming), Giulietti et al. (2023), and Karaivanov et al. (2022). Hoffmann et al. (2023) provide a literature review of the behavioral research on vaccines, including COVID vaccines.

health interventions, even if the approach of a campaign to get employees vaccinated against the flu seems promising at first. While this means that public policymakers interested in a healthy population cannot solely rely on the management of private firms to achieve this goal, we argue that more rigorous research based on credible designs could help bring these interests together by showing which approach is most effective from a management perspective as well.

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Table 1 Summary Statistics from the Firm Campaign

	Full Sample	Control	Altruistic	Selfish	Saturday	F-test (p-value)
	(1)	(2)	(3)	(4)	(5)	(6)
Monthly Income ($)	1,766	1,860	1,701	1,681	1,827	0.316
Company Tenure (years)	7.9	8.3	7.7	8.1	7.5	0.761
Prop. Women	0.49	0.51	0.52	0.46	0.47	0.497
Age (year)	36.6	37.2	36.4	36.6	35.7	0.553
Prop. College Education	0.91	0.92	0.91	0.90	0.93	0.759
Prop. Having Children	0.52	0.52	0.53	0.55	0.48	0.640
Distance to Work (km)	7.58	7.32	7.70	7.78	7.51	0.797
Work Unit Size (#)	29.3	27.9	31.2	29.7	28.4	0.567
Pre Survey Participation	0.48	0.50	0.50	0.47	0.40	0.171
Post Survey Participation	0.36	0.36	0.38	0.33	0.35	0.519
Diagnosed Sick	0.66	0.67	0.67	0.64	0.67	0.835
Granted a Sick Day	0.36	0.37	0.34	0.36	0.35	0.797
Diagnosed Flu Sick	0.11	0.09	0.13	0.13	0.10	0.348
Diagnosed Non-Flu Sick	0.36	0.37	0.34	0.36	0.35	0.819
Vaccination Take-up	0.17	0.22	0.17	0.19	0.08	0.070
N	1,164	344	294	310	216	

Note: 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. Distance to work was calculated based on employees' home addresses using a geo-location service and is only available for employees in Quito. 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 Regression Analysis of Treatment Effects on Participation in the Firm Campaign

	Baseline	With Controls	Non-Compliance	Quito Sample	Day of Week Effects
	(1)	(2)	(3)	(4)	(5)
Altruistic Information	-0.0260	-0.0209	-0.0262	-0.0493	
	(0.0310)	(0.0303)	(0.0306)	(0.0332)	
Selfish Information	-0.0032	-0.0011	-0.0103	-0.013	
	(0.0314)	(0.0316)	(0.0308)	(0.0339)	
Wednesday					0.0818***
					(0.0315)
Thursday					0.0820***
					(0.0302)
Friday					0.0462
					(0.0285)
Saturday	-0.0789***	-0.0791***	-0.0671**	-0.0898***	
	(0.0301)	(0.0304)	(0.0298)	(0.0313)	
N	1,164	1,164	1,152	929	929

Note: This table presents OLS estimates of the effect of the different treatments on vaccination take-up. Specifications 1, 2, and 3 control for Quito fixed effects. Column 1 presents our main estimates from equation (1) without adding additional controls. In Column 2, we test the robustness of the main estimates controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level. In Column 3, we exclude 12 individuals who were assigned to vaccinate during the workweek but went to vaccinate on Saturday. Column 4 presents the estimates using only employees in Quito. In Column 5, we test for different effects across the days of the week using only data from Quito with Saturday as the baseline. Using clustered standard errors at the work unit level (142 clusters) yields similar standard errors with no loss of statistical significance. Robust standard errors in parentheses. * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table 3 Effect of Peer Vaccination on Individual Participation in the Campaign

	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

Note: The outcome in Column 1 is the proportion of peers who got vaccinated, and the outcome in columns 2-4 is an indicator of individual vaccination. The bank has 116 units with more than one employee. This table presents the effect of peers' vaccination take-up on the individual's vaccination decision. We measure the proportion of peers vaccinated and the proportion of peers assigned to the workweek in percentage points. We define peers as all employees who work in the same unit. All specifications control for Quito fixed effects and individual assignments to the workweek. Column 1 presents the results for the first stage. Column 2 displays the results of the reduced form. Column 3 presents OLS estimates of the effect of a change in the proportion of peers vaccinated. Column 4 presents two-stage least square (2SLS) estimates of the effect of a change in the proportion of peers vaccinated. The first stage F-Stat is based on the Montiel Olea-Pflueger F-value. Standard errors (clustered at the unit level) are in parentheses. * p<0.1 ** p<0.05 *** p<0.01

Table 4 Regression Analysis of Employee Health and Absenteeism

	Correlation analysis	Reduced form analysis
	(1)	(2)
Panel A: Sickness Incidence		
Assigned to the workweek		-0.0166
		(0.0358)
Prop. peers assigned to the workweek		-0.0005
		(0.0011)
Vaccinated	-0.0068	
	(0.0324)	
Prop. peers vaccinated	0.0000	
	(0.0009)	
N	1,120	
Panel B: Sickness Absence		
Assigned to the workweek		0.0123
		(0.0361)
Prop. peers assigned to the workweek		-0.0000
		(0.0010)
Vaccinated	-0.0407	
	(0.0298)	
Prop. peers vaccinated	0.0004	
	(0.0009)	
N	1,120	

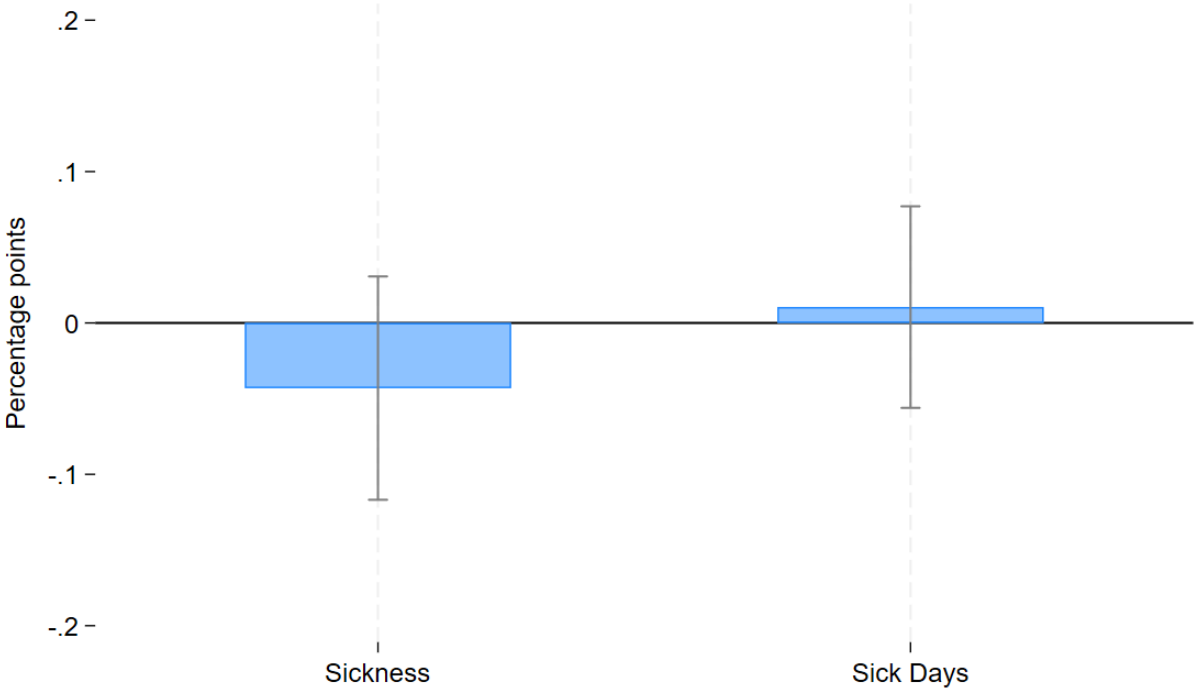
Note: This table presents estimates of the effects on the probability of being diagnosed sick with any illness and the probability of having a sick day granted for any illness. The estimates include only units with two or more employees. All specifications control for Quito fixed effects. Column 1 presents the results from analyses using information on campaign participation. Column 2 presents the results from analyses using random workweek assignment to get vaccinated. Standard errors (clustered at the unit level) are in parentheses. * p<0.1 ** p<0.05 *** p<0.01

Table 5 Health-Related Habits

	Baseline	Coefficient	N
	(1)	(2)	(3)
Health-related habits index	6.33	-0.5056**	358
		(0.2215)	
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)	

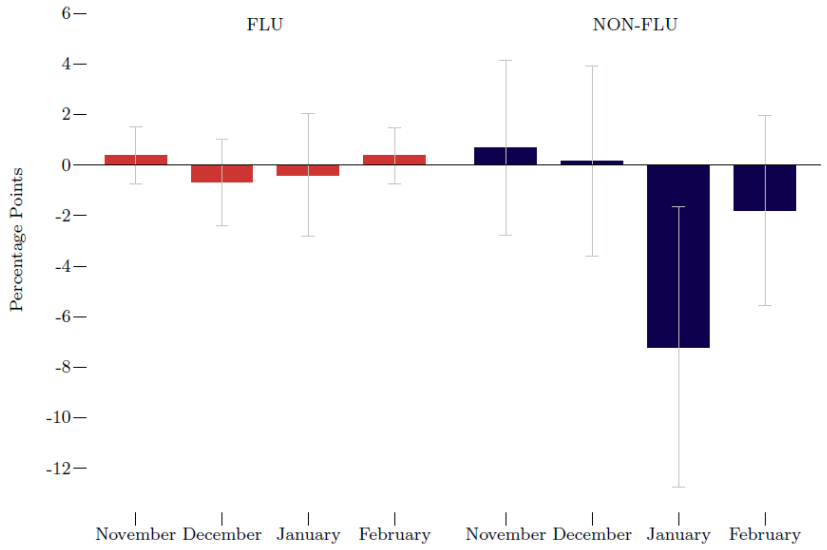
Note: This table presents the estimates of the effect of being assigned to the workweek on four health-related habits and activities. All responses are on a scale from 1 ("never") to 10 ("all the time"). The health-related habits index is the sum of all four scores divided by four. All specifications control for Quito fixed effects. Robust standard errors in parentheses. * p<0.1 ** p<0.05 *** p<0.01

Figure 1 Probability of Employee Sickness and Absenteeism



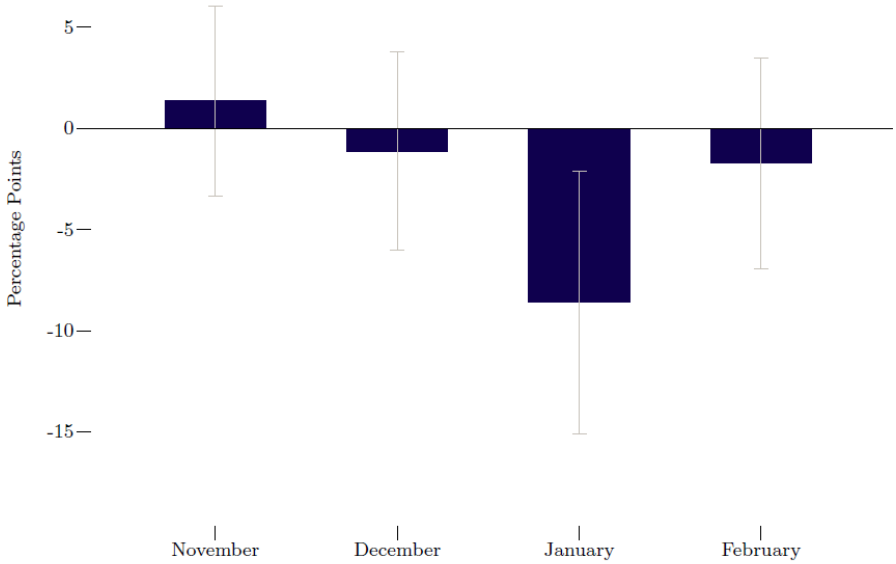
Note: This figure presents the average treatment effect of assignment to the workweek relative to Saturday on the probability of sickness incidence (for any illness) and sickness absence (i.e. having a sick day granted for any illness). The figure presents 95% confidence intervals around the averages.

Figure 1 Monthly Sickness Incidence



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

Figure 2 Monthly Visits to the Onsite Doctor



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