Vaccines at Work: Experimental Evidence from a Firm Campaign

Manuel Hoffmann[1] Roberto Mosquera[2] Adrian Chadi[3]

This version: May 31, 2025

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. We discuss potential explanations for this result and present evidence consistent with the notion that a vaccination campaign can influence the behavior of employees concerning their health.

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

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

---

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

[1] University of California, Irvine, Paul Merage School of Business, 4293 Pereira Dr, Irvine, CA 92697, United States.
[2] Carrera de Economía, FACEA, Universidad de las Américas (UDLA), Via a Nayon, Quito, 170124, Ecuador.
[3] University of Southampton, Department of Economics, Highfield Campus, Southampton, SO17 1BJ, UK.

1. Introduction

Company management is interested in reducing employee sickness and absence since it is costly. Employees on sick leave commonly receive compensation without providing any productive benefit to the firm. While this has been of great concern for companies in the time before COVID, the recent pandemic has brought even more attention to the issue of respiratory diseases that induce employees to call in sick. A possible way to reduce absenteeism is to leverage health campaigns. In this context, preventive health interventions are particularly interesting, such as campaigns where employees are encouraged to vaccinate against influenza (the flu), one of the most common respiratory diseases. From an economic perspective, it is important to find out whether or not such campaigns could be a low-cost way to achieve this goal of reducing sick leave among employees. This idea certainly 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), given that in the United States (US) 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). According to the latest CDC statistics (2024a; 2024b), influenza incidence is at pre-pandemic levels, suggesting a high and stable economic burden, consistent with the idea of encouraging flu vaccination as a promising approach to tackle the 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 31.1% among adults in the US during the 2017–2018 flu season, without noticeable changes in recent statistics (ECDC, 2024; CDC, 2024c). 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, for example, postpone doctor visits when they feel sick or reduce 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, the campaign lasted from Wednesday to Saturday due to capacity constraints, 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 who were invited for the weekend had to change their plans to get to the bank, while other employees did not incur such extra costs. Third, we varied the content of the invitation emails to appeal to altruistic or selfish motives via information nudges.

Thanks to these modifications, we create a research context where we can exploit experimental variation in vaccination at the employee level to study its consequences, including how peer vaccination affects individual take-up. Most importantly, we can study the implications of campaign participation on employees' health and sickness absence, informing us of the campaign's economic benefit. We also discuss explanations for our main finding and explore the campaign's behavioral implications to gauge the possibility of moral hazard behaviors when adopting medical technology.

Based on this research setup, 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 participate in a firm campaign.[1]

[1] While we present a way to design an experiment within the ethical constraints inherent in research involving the medical treatment of human subjects, we have further addressed the ethical dimension of our research by obtaining IRB approval. As part of this approval process at a US university, an Ecuadorian expert provided an additional ethics review.

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, which includes possible 'placebo effects' when individuals think they get better regardless of the actual medical effect of the treatment. 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 firm campaign's total health impact and potential behavioral effects. To identify the latter, we use non-flu-related health outcomes, since the flu vaccine promises only to 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 who 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. First, 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. Second, while the low-opportunity costs treatment effectively increased on-site vaccinations, it could be that employees in the Saturday treatment were more likely to get vaccinated elsewhere. Given the available information in our setting, this is unlikely to explain the similar health outcomes across the two groups. As a third explanation, the vaccine may have been medically ineffective, which we cannot rule out based on the available data. Fourth, 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 pointless from an economic perspective. In light of the available data, this appears to be a plausible explanation that could considerably limit the prospects of such health interventions in

general. Finally, increased firm campaign participation may trigger riskier and, thus, health-threatening behavior among employees who overestimate the vaccine's protection. Since such a behavioral response would be of interest to both researchers and management of companies running health interventions, we exploit the fact that our setting allows us to explore 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. While participation in a flu vaccination campaign cannot medically affect the probability of such diagnoses, participants 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 indicated 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, data from employee surveys show a negative effect of workweek assignment on health-related behavior, which interacts with beliefs on vaccine effectiveness, further supporting the idea that employees who feel more protected engage more in riskier practices.

With this study, we contribute to our understanding of the determinants and 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. In particular, our paper shows how the scheduling of appointments across days matters strongly for campaign participation, which we exploit in a second step to provide causal evidence on peer effects in a firm context using random variation for our identification strategy.[2]

[2] We thereby contribute to the research on peer effects in vaccination that is largely quasi-experimental (Bouckaert et

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 regarding 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 a university wellness program's ineffectiveness in improving employee health.[3]

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). In this regard, some studies have considered the role of vaccines (Auld, 2003; Moghtaderi & Dor, 2021; Talamàs and Vohra, 2020), while there is a larger literature in economics on moral hazard in the context of health (Einav et al. 2013; Klick & Stratmann, 2007; Margolis et al., 2014). Notably, recent research on vaccines shows evidence of similar behavioral phenomena in the context of the COVID pandemic (Andersson et al. 2021).

---

al., 2020; Geoffard & Philipson, 1997; Rao et al., 2017), except for Sato & Takasaki (2019a) who use experimental variation in tetanus vaccination among Nigerian women. Co-workers as a peer group have received a lot of attention in research on performance (Chan et al., 2014; Cornelissen et al., 2017; Falk & Ichino, 2006; Mas & Moretti, 2009; Tan & Netessine, 2019), in contrast to our research context on employee health. Note that there is broader research on the determinants of individual vaccinations, with a focus on laws, information, education, age, health status, health behavior, and lifestyle (Bradford & Mandich, 2015; Chang, 2018; Maurer, 2009; Milkman et al., 2021; Milkman et al., 2022; Oster, 2018; Sato & Takasaki 2019b; Schaller et al., 2019; Schmitz & Wuebker, 2011). For field experiments on how to affect flu vaccination among employees in a company context, see Milkman et al. (2011) and Nowalk et al. (2010).

[3] 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 vaccines, we also add to research that predominantly takes place in the medical literature, with some exceptions in the economics literature (Ager et al., 2017; Carpenter & Lawler, 2019; Lawler, 2017). There is also a growing number of 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.

Accordingly, our findings may also be informative in light of the recently developing field of research on COVID vaccinations, where many controlled interventions have produced evidence on the determinants of take-up (see Hoffmann et al. 2023 for a literature review).[4] There are certainly some differences between COVID vaccines and flu shots, which could explain why we find rather strong effects of incentives to get vaccinated. For example, regarding 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. Still, we believe that opportunity costs and peer effects may also be relevant in many other contexts outside of firms like the one studied in our paper on flu vaccines, thereby informing the discussion on the determinants of participation in COVID vaccination campaigns. Regarding the health consequences of such campaigns, it is an open question to what extent there are differences between influenza and COVID (Freedman et al. 2022). In this context, we hope to contribute with our experimental research to a larger debate, which for COVID vaccination has also just begun (Gandhi et al. 2024, Girma and Paton 2024a, 2024b).

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 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.[5] 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.[6] To investigate how to increase campaign participation and its consequences, we implemented three interventions: randomized assignments for on-site vaccination across weekdays, implemented information nudges by varying the email content to invite employees to get vaccinated, and changed the vaccine's price

[4] Examples of experiments on the determinants of COVID vaccination are Campos-Mercade et al. (2021), Dai et al. (2021), Jacobson et al. (2022), Milkman et al. (2024), Rabb et al. (2022), Reddinger et al. (2022), Saccardo et al. (2023), and Schneider et al. (2023). For quasi-experimental work, see, e.g., Giulietti et al. (2023) and Karaivanov et al. (2022).
[5] 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%.
[6] 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).

using income-dependent subsidies. In the following, we focus on the most important aspects of our design (for more information on the process behind our experimental study, see Appendix A1).

To find out if monetary considerations affect campaign participation, we allocated the vaccine subsidy by choosing an income threshold, thereby maximizing the sample size while passing density and covariate smoothness checks. Employees who earned less than $750 per month would pay $4.95 to get vaccinated, and 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.[7] First, we exploited an organizational bottleneck: due to limited capacity, not all employees could be invited to get vaccinated during the workweek (Wednesday, Thursday, or Friday). Thus, 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% do not work regularly on Saturdays. Hence, in order to get vaccinated on the weekend, which is not normally possible in Ecuador, most individuals in the *Saturday* group had to arrange their schedules to travel to the bank for vaccination. In contrast, employees with workweek assignment were allowed to take time off their duties to get vaccinated.

To determine the impact of the information nudges, we separate the employees with a workweek assignment into three groups, including a *Control* group without a 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 the

[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 did not have a company email address.

flu vaccine (*Selfish*). In addition to the information provided to the *Control* group, the email included the following: "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).

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.[8]

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.[9] Figure A9 illustrates the distribution of employee numbers across the firm's work units, which were established more than two decades prior to our study. 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 in Column 1 (see Table A1 for

[8] Figure A5 displays the geographic locations of the banks' branches. Figure A6 depicts the timeline. Figure A7 shows the flu vaccine used, and Figure A8 shows an individual getting vaccinated during the campaign.
[9] For further analyses, we can also identify individuals in leading positions ('supervisors'). In addition, we can use information about functional areas within the bank to distinguish between 'front office' and 'back office' employees.

further statistics). 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). Note that Ecuadorian law establishes that employees must present a medical certificate to receive a sick day.[10] Consequently, the on-site doctors report every patient's visit to HR, including the diagnosis (the type of disease), whether they granted sick days and the number of sick days. Similarly, if employees take time off work to see an external 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 were sick and 37% had at least one sick day (see Table 1).

The doctors diagnose their patients using a combination of a physical examination, blood tests, and culture tests. The procedures undertaken are recorded in individual medical records, to which we do not have access. Diagnoses that name the patient's illness as "flu" provide us with a narrow definition of flu-related sickness. If flu cases occur with complications, the data report the complication as the diagnosis and do not mention the flu explicitly. As the main definition of flu-related sickness, we thus include cases of diagnoses that indicate complications caused by the flu, according to a third-party physician (who also provided us with a broader definition for additional analyses). Any other respiratory disease not classified as flu is considered as a "non-flu" respiratory disease, as shown in Table 1.[11]

[10] 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.

[11] To classify cases of respiratory illness, we provided the physician with sickness type descriptions from the medical

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.[12]

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

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

records. A second physician also received the sickness type descriptions as well as the other physician's classification of flu- and non-flu-related illness for review. It turned out that the physicians strongly agreed in their assessments. Regarding our main definition of flu-related sickness, the second evaluation led to seven additional influenza cases. Regarding the broader definition of flu-related sickness, there was no disagreement between the two physicians.

[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. 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.

[13] Figure A10 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 A2).

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. As in all of our analyses focused on the impact of individually assigned treatments, we report heteroscedastic robust standard errors at the employee level. In our regression analyses, we also 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.[14] 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 have received the reduced opportunity costs treatment. Hence, the following regression results can be considered a conservative estimate.

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 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.[15]

[14] 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.

[15] While there are studies showing that nudges can work (List et al., 2023), our evidence aligns with other research on vaccine take-up (Bronchetti et al., 2015; Godinho et al., 2016). As a possible factor, information treatments have to be highly salient to accrue an additional effect on take-up rates in such a firm context. The post-intervention survey asked whether the employees recalled the altruistic and selfish information statements. Table A3 shows that neither employees

---- 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 a few cases of employees showing up later than when they were assigned (see Table A4 for details). While the medical team in charge of the campaign complied with the request to enforce the day assigned to each employee at the beginning, they did not send employees away without being vaccinated if they showed up on less busy days. 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-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.[16] 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 A5). 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

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).

[16] 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.

gender. For those employees who live further away than the median distance to work, we also find a similar treatment effect compared to those who live closer to the bank. This implies that additional travel costs are not 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*.[17]

In conclusion, several pieces of evidence support that the difference in campaign participation between employees assigned a day during the workweek and *Saturday* corresponds to a change in the opportunity costs of vaccination.[18] While our main finding may seem quite intuitive, it identifies credible and strong variation in campaign participation, which we exploit below by using random workweek assignment as a trigger for increased individual vaccine take-up.

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 2024). As a result, co-

[17] Note that the treatment effects in Table A5 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.

[18] Another possible driver of vaccine take-up could be the concern about getting ill. In this spirit, we inspect front office employees who have more physical contact with customers, and thus might be expecting a particular benefit from getting vaccinated. Indeed, the increase in vaccinations is larger among front-office employees compared to back-office employees due to workweek assignment; yet, this interaction effect is not statistically significant across specifications.

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 consider the fact that the number of employees per unit varies across the firm (see Figure A9) when 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 who get vaccinated 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:

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

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

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

In our design, the exclusion restriction holds because the proportion of peers vaccinated during the workweek is the only channel through which the proportion of peers assigned to the workweek can affect the individual's vaccination decision. Hence, we can combine the estimates from equations

(3) and (4) to obtain an instrumental variable (IV) estimate of the effect of the proportion of vaccinated peers on an individual'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 work unit level.

--- Table 3 about here ---

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 co-worker 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 co-workers 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.[19]

To understand what is behind the peer effects on campaign participation, we first use data from the employee surveys, which, however, do not reveal evidence of a specific mechanism in this context.[20] In contrast, by including a unit-size interaction in our main estimation model, a separate

[19] Results in Table 3 are robust to controlling for individual workweek assignment. 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 A6 (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 further checks, 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 co-workers. As seen in Table A6 (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, we find no significant differences between different peer instruments when we consider that some employees in the same unit are in different locations, allowing us to define a within location and a between location peer instrument.

[20] 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 A7, which

analysis reveals that units with small sizes drive the effects. This aligns with the idea that pressure to conform to peer behavior is stronger in smaller groups, which is consistent with research on the effects of team size (Chadi and Homolka, 2023). In the context of health-related behavior, it may also be that face-to-face interactions in smaller groups are more intense, so employees may, thus, feel particular pressure to get vaccinated to protect others. 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. In our interpretation of the available evidence, we conclude that the positive peer effects most likely result from individuals feeling pressured by their peers 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. In addition, we discuss explanations for our main result (Section 5.2), and employ the same approach using random workweek assignment to explore changes in health-related behaviors (Section 5.3). As described in more detail in Appendix A1, we follow our ex-ante plans closely by focusing on health outcomes, while our analysis of behavioral effects is to some extent exploratory.

5.1 Health and Sickness Absence

In a first step, we examine the raw treatment effects of the firm campaign on health outcomes during

suggests that peer behavior neither affected beliefs nor supplied new information about the vaccine. The survey evidence in Table A7 also speaks against the idea that employees were happy or upset that some co-workers had the chance to vaccinate during work hours while others did not, as we do not find significant changes in the propensity to talk with co-workers when the proportion of co-workers with workweek assignment increases. In further analyses, we modify our estimation model by adding an interaction between individual workweek assignment and peer workweek assignment. This also reveals no significant result that may inform us about mechanisms. By restricting the sample to units with one supervisor and at least two subordinates, we also analyze the role of the supervisor acting as a possible role model during the campaign. However, we find no such effect for the employee's likelihood of vaccination. This underscores that co-workers at the same hierarchical level are most relevant for peer effects to occur.

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 about here ---

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 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 the number of employees varies substantially across the company's 142 units (see Figure A9) and the proportion of vaccinated peers per unit (with vaccination rates between 0% and 100%), this could indirectly affect health outcomes. We consider such health externalities by regressing the health outcomes not only on assignment to the workweek but also on the proportion of peers assigned to the workweek. As a result of considering peer effects in our analysis, we cluster the standard errors at the work unit level.

--- 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 had no siginificant effect on health, as the workweek assignment reduced 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.[21]

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 points, 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.[22]

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-

[21] 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 19). 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.

[22] 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.

envelope calculation and determine the net benefit of the campaign using our evidence on sickness absence. As can be seen in Table A8, 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.[23] 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.[24] 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 to which the HR department could have devoted their time. 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 A2. According to the results (see Tables A10 and A11), there is no evidence of changes in the probability of having the flu, which is also 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 data and public health figures to implement an equivalence test, which adds further support to the conclusion of lacking health benefits due to the firm campaign (see Figure A12).

[23] 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 A9 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.

[24] As an extension of our cost-benefit analysis, we can also consider the number of sick days. When we scale our results from Table A8 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 A8 since the median in the control group is exactly one sick day.

5.2 Interpretation

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 firm campaigns may be successful in affecting on-site behavior but at the expense of behavior outside the company. In our case, employees with a Saturday appointment could have chosen to get vaccinated elsewhere, while those invited during the workweek did not do so. However, given that the people in our setting with nationwide vaccination rates close to zero (see Section 1) rarely choose to get a flu shot on their own if not encouraged by someone to do so, this explanation unlikely plays a role in the context studied here. This conclusion also aligns with our analysis of the data from the employee survey.[25]

Since there is also no evidence of positive health externalities (see Section 5.1) that explain why bank employees are similarly healthy in all treatment groups, there must be another reason for our main result. An obvious candidate is the idea that the flu vaccine was medically ineffective. As discussed in previous literature and in Appendix A2, even health institutions that support 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. Since this result is robust even when we vary the definition of influenza diagnoses (Table A11), we ultimately cannot rule out this explanation. However, two further 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 significant health benefits from increased vaccination rates. A standard workforce targeted by such a campaign may have only mild flu symptoms and a low risk of severe complications. In this context, a survey we conducted in 2024 (see Appendix B for details) shows that around 4 in 10 respondents say that their employer recommends a flu vaccination as part of health management; a share that is

[25] In the post-intervention survey, we included a question on vaccination during the flu season where we asked about vaccinations at the firm campaign, vaccinations outside the campaign, and no vaccination. We were intentionally vague to not reveal our intent in this survey, and so the question wording was not limited to flu vaccination, which means that employees could also be reporting other types of vaccinations. According to the data, 60 employees got vaccinated outside the campaign. None of them participated in the campaign. 18 employees stated that they participated in the campaign but did not according to the records. One employee who took part in the campaign stated no vaccination. With these issues in mind, we conducted additional checks of our main results regarding the effects of the workweek treatment on vaccination as well as health outcomes. First, when we exclude the 19 employees misremembering whether they were vaccinated from the analysis, the results do not change. Second, the same holds when we exclude employees claiming vaccination outside the campaign. Finally, we test whether outside vaccinations differ significantly according to treatment status. This is not the case, which speaks against the idea that our experimental manipulation affected the propensity of getting a flu shot elsewhere, for example due to higher awareness regarding the issue of flu vaccination.

similar across different age groups and in terms of self-assessed health status. This suggests that firm policies to increase flu vaccination target relatively healthy prime-age populations, which aligns with our experimental dataset, as discussed in Section 3. In fact, Table 1 shows that many employees get diagnosed sick over the year, but their health problems are often not severe enough to get a sick day granted. Another factor is that many of the diagnoses are unrelated to influenza, even during the flu season, as our dataset includes a relatively large share of 'non-flu' respiratory diseases that may have flu-like symptoms. As a result, the actual health problem of the flu may be perceived as larger than it is in working-age populations. All this speaks to the idea of exaggerated expectations regarding the economic benefits of campaigns to increase flu vaccination in prime-age employees.

Second, vaccination could also indirectly affect health outcomes, besides possible medical effects, if employees change their behavior. Vaccinated individuals could overestimate the vaccine's protection and, for example, take fewer protective measures or change their health-related habits in general. In the next section, we therefore explore the impact of the firm campaign on employee behavior to learn more about another possible explanation for its lack of effectiveness in reducing sickness absence and about possible changes in risk-taking, given that such behavioral responses are of interest both from a researcher's perspective and for decision-makers such as HR managers.

5.3 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. We thereby exploit the idea 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. 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 who 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 2 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 A10), 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.[26] These results are consistent with the idea of riskier behavior among employees who may have thought they were more likely to be 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 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 3 presents the effects of assigning employees a flu shot during the workweek on the probability of visiting the on-site doctor, broken down by month. We find no significant effect in November, December, or February. In January, a workweek assignment for vaccination decreased

[26] Comparing the significance levels for each month in Figure 2 for flu vs. non-flu, we find that we cannot reject the null that the effects are the same for all months except for January, where we find a significant difference between flu and non-flu effects of 6.8 percentage points (p-value = 0.016). In one of our additional robustness checks, we confirm a significant January effect when controlling for employee fixed effects using a month-based difference-in-difference approach (Figure A11). 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.

the probability of going to the on-site doctor by 8.6 percentage points, which supports the idea that the campaign participants felt more protected, 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 necessarily benefit from going to the doctor during flu season, the critical point for our discussion is that such 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 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. When we decompose the measures, we find that increased participation in the firm campaign does not affect how often employees wash their hands, which could be because almost all employees reported that they wash their hands regularly. We find a negative but statistically insignificant effect on how often the employees exercise and take nutritional supplements. The effect on how often employees carry an umbrella is statistically significant at the 5% level and robust to adjust for multiple comparisons following Anderson (2008), decreasing the frequency of carrying an umbrella by 1.2 points.

While this supports the idea that employees differ in their willingness to engage in risky health behaviors, it is interesting to note that many people, including Ecuadorians, believe that carrying an umbrella could help prevent the flu or other respiratory illnesses.[27] 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 pre-intervention survey. We find that the umbrella effect is driven by employees who strongly believe vaccines are effective (Table A12). This conforms to the idea that employees think they can neglect measures that they believe help prevent respiratory

[27] 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.

illnesses because they expect an effective firm campaign to provide protection against sickness.

In summary, our evidence on riskier behaviors regarding health raises concerns for company management and policymakers, given that any health risk due to behavioral changes could threaten an intervention's success. Note that our evidence suggests moral hazard, but it does not 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.[28]

6. Conclusions

The private benefits of firm health campaigns are ex-ante unclear. Individual behavior of employees 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 did not induce a change in employee behavior. In contrast, both opportunity costs and peer behavior can have a substantial effect on employee participation in a health campaign for working-age adults. For health benefits, the exogenous increase in vaccination did not significantly affect any of our main outcomes, despite evidence of psychological impacts, suggesting a possible placebo effect where employees feel protected and thus feel better, regardless of the actual medical effect. While we cannot rule out that the vaccine was medically ineffective, an important factor in explaining the ineffectiveness of such firm campaigns could be that the flu is not necessarily a major health issue in a typical prime-age company workforce. Additionally, we have some evidence of employees adopting riskier health behaviors, which can also limit the effectiveness of firm campaigns and health interventions in general. In any case, based on our cost-benefit analysis, we

[28] 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 an employee is vaccinated, a doctor might be more likely to misdiagnose flu-like symptoms as a non-flu illness. However, our results show that employees who were more likely vaccinated due to workweek assignment were less likely diagnosed with a non-flu respiratory disease. Third, the data on medical diagnoses correspond to sick employees who visited the on-site doctor or an external doctor during work hours, while those who saw an external doctor outside work hours and were not granted a sick day are coded as healthy. However, due to random treatment assignment, this measurement error is unlikely to affect our results.

conclude that the vaccination 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 (2020). 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. In these countries, working conditions in large companies are similar. Also, the flu season in Ecuador has matched those in the US and Europe since 2015 (WHO FluNet, 2021), and the flu vaccines used in Ecuador also match those used in these countries. 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, in terms of scaling our insights, 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 for health, we do not expect substantial changes in the results conditional on having a similar context.

In this context, one may wonder how our experimental study informs current and evolving management challenges in the post-COVID era. A possible reason to expect changes in companies' health policies could be increased absenteeism observed in some countries after the pandemic. To learn more about this and possible implications for health management, we ran a large survey in 2024 with respondents from Germany, a country experiencing increases in sick leave that point to a decline in employee health. Our survey (see Appendix B for details) reveals that nearly half of the respondents have noticed an overall increase in health issues among employees compared to before the pandemic. Yet, it is unclear whether this explains the rise in absenteeism, as employees with mild health issues now seem to call in sick more often, according to a similar number of respondents. With respect to health management implications, the survey data do not indicate substantial changes in the respondents' companies. If anything, they report that more employers recommend that their employees vaccinate against the flu as part of their workplace health management. In summary, the survey suggests that our research appears to be at least as relevant today as before COVID.

In terms of practical implications, our idea of employing a randomized encouragement design is helpful to circumvent ethical dilemmas when studying the consequences of health interventions to

identify both potential economic benefits and behavioral changes in an unbiased manner. As an important lesson learned from our investigation, we inform policymakers at the company level as well as at the public level about cost-effective measures that can improve participation in health campaigns. Finally, our paper shows that there may not be sufficient private incentives for firms to implement 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.

References

Ager, P., Worm Hansen, C., & Sandholt Jensen, P. (2017). Fertility and early-life mortality: Evidence from smallpox vaccination in Sweden. *Journal of the European Economic Association*, 16(2), 487-521.

Alam, S. A., & Wolff, H. (2016). Do pesticide sellers make farmers sick? Health, information, and adoption of technology in Bangladesh. *Journal of Agricultural and Resource Economics*, 62-80.

Anderson, M. L. (2008). Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association*, 103(48a4), 1481-1495.

Anderson, M. L., Dobkin, C., & Gorry, D. (2020). The Effect of Influenza Vaccination for the Elderly on Hospitalization and Mortality: An Observational Study With a Regression Discontinuity Design. *Annals of Internal Medicine*, 172(7), 445-452.

Andersson, O., Campos-Mercade, P., Meier, A. N., & Wengström, E. (2021). Anticipation of COVID-19 vaccines reduces willingness to socially distance. *Journal of Health Economics*, 80, 102530.

Arni, P., Dragone, D., Goette, L., & Ziebarth, N. R. (2021). Biased health perceptions and risky health behaviors—theory and evidence. *Journal of Health Economics*, 76, 102425.

Au, T. K. F., Chan, C. K., Chan, T. K., Cheung, M. W., Ho, J. Y., & Ip, G. W. (2008). Folkbiology meets microbiology: A study of conceptual and behavioral change. *Cognitive Psychology*, 57(1), 1-19.

Auld, M. C. (2003). Choices, beliefs, and infectious disease dynamics. *Journal of Health Economics*, 22(3), 361-377.

Baer, R. D., Weller, S. C., Pachter, L., Trotter, R., de Alba Garcia, J. G., Glazer, M., Klein, R., Deitrick, L., Baker, D.F., Brown, L., Khan-Gordon, K., Martin, S.R., Nichols, J., Ruggiero, J. (1999). Cross-cultural perspectives on the common cold: Data from five populations. *Human Organization*, 251-260.

Banerjee, A. V., Duflo, E., Glennerster, R., & Kothari, D. (2010). Improving immunization coverage in rural India: clustered randomized controlled evaluation of immunization campaigns with and without incentives. *BMJ*, 340, c2220.

Baxter, R., Lee, J., & Fireman, B. (2010). Evidence of bias in studies of influenza vaccine effectiveness in elderly patients. *Journal of Infectious Diseases*, 201(2), 186-189.

Blasco, A., Jung, O. S., Lakhani, K. R., & Menietti, M. (2019). Incentives for public goods inside organizations: Field experimental evidence. *Journal of Economic Behavior & Organization*, 160, 214-229.

Belot, M., James, J., & Nolen, P. (2016). Incentives and children's dietary choices: A field experiment in

primary schools. *Journal of Health Economics*, 50, 213-229.
Belot, M., James, J., & Spiteri, J. (2020). Facilitating healthy dietary habits: An experiment with a low income population. *European Economic Review*, 129, 103550.
Bouckaert, N., Gielen, A. C., & Van Ourti, T. (2020). It runs in the family–Influenza vaccination and spillover effects. *Journal of Health Economics*, 74, 102386.
Bjorvatn, K., Cappelen, A. W., Sekei, L. H., Sørensen, E. Ø., & Tungodden, B. (2020). Teaching through television: Experimental evidence on entrepreneurship education in Tanzania. *Management Science*, 66(6), 2308-2325.
Bradford, W. D., & Mandich, A. (2015). Some state vaccination laws contribute to greater exemption rates and disease outbreaks in the United States. *Health Affairs*, 34(8), 1383-1390.
Bronchetti, E. T., Huffman, D. B., & Magenheim, E. (2015). Attention, intentions, and follow-through in preventive health behavior: Field experimental evidence on flu vaccination. *Journal of Economic Behavior & Organization*, 116, 270-291.
Bütikofer, A., Cronin, C. J., & Skira, M. M. (2020). Employment effects of healthcare policy: Evidence from the 2007 FDA black box warning on antidepressants. *Journal of Health Economics*, *73*, 102348.
Bütikofer, A., & Salvanes, K. G. (2020). Disease Control and Inequality Reduction: Evidence from a Tuberculosis Testing and Vaccination Program. *Review of Economic Studies*, *87*(5), 2087-2125.
Bütikofer, A., & M. M. Skira (2018). Missing Work Is a Pain - The Effect of Cox-2 Inhibitors on Sickness Absence and Disability Pension Receipt. *Journal of Human Resources*, 53(1), 71-122.
Campos-Mercade, P., Meier, A. N., Schneider, F. H., Meier, S., Pope, D., & Wengström, E. (2021). Monetary incentives increase COVID-19 vaccinations. *Science*, 374(6569), 879-882.
Carpenter, C. S., & Lawler, E. C. (2019). Direct and spillover effects of middle school vaccination requirements. *American Economic Journal: Economic Policy*, 11(1), 95-125.
Carrera, M., Lawler, E. C., & White, C. (2021). Population mortality and laws encouraging influenza vaccination for hospital workers. *Annals of Internal Medicine*, 174(4), 444-452.
Carrera, M., Royer, H., Stehr, M., & Sydnor, J. (2020). The structure of health incentives: Evidence from a field experiment. *Management Science*, 66(5), 1890-1908.
Castro, S., Englmaier, F., & Guadalupe, M. (2022). Fostering psychological safety in teams: Evidence from an RCT. Available at SSRN.
Cawley, J. (2010). The economics of childhood obesity. *Health Affairs*, 29(3), 364-371.
Cawley, J., & Ruhm, C. J. (2011). The economics of risky health behaviors. In Handbook of health economics (Vol. 2, pp. 95-199). Elsevier.
CDC (2022). Promoting Vaccination in the Workplace. Accessed August 16, 2022. Source: https://www.cdc.gov/flu/business/promoting-vaccines-workplace.htm.
CDC (2024a). Flu Disease Burden: Past Seasons. Accessed December 15, 2024. Source: https://www.cdc.gov/flu-burden/php/data-vis/past-seasons.html
CDC (2024b). Preliminary Estimated Flu Disease Burden 2023–2024 Flu Season. Accessed December 15, 2024. Source: https://www.cdc.gov/flu-burden/php/data-vis/2023-2024.html
CDC (2024c). FluVaxView Interactive! Accessed December 15, 2024. Source: https://www.cdc.gov/fluvaxview/interactive/index.html
Chadi, A., & Homolka, K. (2023). Under (peer) pressure: Experimental evidence on team size and task performance. *Managerial and Decision Economics*, 44(7), 3769-3786
Chan, T. Y., Li, J., & Pierce, L. (2014). Compensation and peer effects in competing sales teams. *Management Science*, 60(8), 1965-1984.
Chang, L. V. (2018). Information, education, and health behaviors: Evidence from the MMR vaccine autism

controversy. *Health Economics*, 27(7), 1043-1062.
Chen, F., & Toxvaerd, F. (2014). The Economics of Vaccination. *Journal of Theoretical Biology*, 363, 105-117.
Cornelissen, T., Dustmann, C., & Schönberg, U. (2017). Peer effects in the workplace. *American Economic Review*, 107(2), 425-56.
Czibor, E., Jimenez-Gomez, D., & List, J. A. (2019). The dozen things experimental economists should do (more of). *Southern Economic Journal*, 86(2), 371-432.
Dai, H., Saccardo, S., Han, M. A., Roh, L., Raja, N., Vangala, S., Modi, H., Pandya, S., Sloyan, M. & Croymans, D. M. (2021). Behavioural nudges increase COVID-19 vaccinations. *Nature*, 597(7876), 404-409.
De Paola, M., Scoppa, V., & Pupo, V. (2014). Absenteeism in the Italian public sector: The effects of changes in sick leave policy. *Journal of Labor Economics*, 32(2), 337-360.
Demicheli, V., Demicheli, V., Jefferson, T., Ferroni, E., Rivetti, A., & Di Pietrantonj, C. (2018). Vaccines for preventing influenza in healthy adults. *Cochrane database of systematic reviews* (2).
Direccion Nacional Epidemiologica (2018). Influenza Actualización Epidemiológica SE 47,2017 – SE 44 2018, Ministerio de Salud Publica del Ecuador, Accessed December 31, 2018. Source: https://www.salud.gob.ec/actualizacion-epidemiologica-2018-influenza.
Duflo, E., Dupas, P., Ginn, T., Barasa, G. M., Baraza, M., Pouliquen, V., & Sharma, V. (2019). HIV prevention among youth: A randomized controlled trial of voluntary counseling and testing for HIV and male condom distribution in rural Kenya. *PloS One*, *14*(7), e0219535.
ECDC (2024). Survey report on national seasonal influenza vaccination recommendations and coverage rates in EU/EEA countries. Accessed December 15, 2024. Source: https://www.ecdc.europa.eu/en/publications-data/survey-report-national-seasonal-influenza-vaccination-recommendations
Einav, L., Finkelstein, A., Ryan, S. P., Schrimpf, P., & Cullen, M. R. (2013). Selection on moral hazard in health insurance. *American Economic Review*, 103(1), 178-219.
ENSANUT (2012). Encuesta Nacional de Salud y Nutrición de Ecuador.
ENEMDU (2017). Encuesta Nacional de Empleo, Desempleo y Subempleo.
Falk, A., & Ichino, A. (2006). Clean evidence on peer effects. *Journal of Labor Economics*, 24(1), 39-57.
Freedman, S., Sacks, D., Simon, K., & Wing, C. (2022). Direct and indirect effects of vaccines: Evidence from COVID-19. NBER working paper 30550.
Gandhi, A., Larkin, I., McGarry, B., Wen, K., Yu, H., Berry, S., Mor, V., Syme, M., & White, E. (2024). The Health and Employment Effects of Employer Vaccination Mandates. NBER working paper 33072.
Geoffard, P. Y., & Philipson, T. (1997). Disease Eradication: Private versus Public Vaccination. *American Economic Review*, 87(1), 222-230.
Girma, S., & Paton, D. (2024a). COVID-19 vaccines as a condition of employment: Impact on uptake, staffing, and mortality in elderly care homes. *Management Science*, 70(5), 2882-2899.
Girma, S., & Paton, D. (2024b). Using double-debiased machine learning to estimate the impact of Covid-19 vaccination on mortality and staff absences in elderly care homes. *European Economic Review*, 170, 104882.
Giulietti, C., Vlassopoulos, M., & Zenou, Y. (2023). When reality bites: Local deaths and vaccine take-up. *European Economic Review*, 156, 104463.
Godinho, C. A., Yardley, L., Marcu, A., Mowbray, F., Beard, E., & Michie, S. (2016). Increasing the intent to receive a pandemic influenza vaccination: Testing the impact of theory-based messages. *Preventive Medicine*, 89, 104-111.
Gubler, T., Larkin, I., & Pierce, L. (2018). Doing well by making well: The impact of corporate wellness

programs on employee productivity. *Management Science*, 64(11), 4967-4987.
Handel, B., & Kolstad, J. (2017). Wearable technologies and health behaviors: New data and new methods to understand population health. *American Economic Review*, 107(5), 481-485.
Harrison, G. W., & J. A. List. (2004). Field experiments. *Journal of Economic Literature*, 42(4), 1009-1055.
Helman, C. G. (1978). Feed a cold, starve a fever"—folk models of infection in an English suburban community, and their relation to medical treatment. *Culture, Medicine and Psychiatry*, 2(2), 107-137.
Hoffmann, M., Baggio M., & Krawczyk, M.W. (2023). Vaccination demand and acceptance: a literature review of key behavioural insights. Technical report, European Commission.
Jacobson, M., Chang, T. Y., Shah, M., Pramanik, R., & Shah, S. B. (2022). Can financial incentives and other nudges increase COVID-19 vaccinations among the vaccine hesitant? A randomized trial. *Vaccine*, 40(43), 6235-6242.
Jeon, S. H., & Pohl, R. V. (2019). Medical innovation, education, and labor market outcomes of cancer patients. *Journal of Health Economics*, *68*, 102228.
Jones, D., Molitor, D., & Reif, J. (2019). What do workplace wellness programs do? Evidence from the Illinois workplace wellness study. *Quarterly Journal of Economics*, 134(4), 1747-1791.
Just, D. R., & Price, J. (2013). Using incentives to encourage healthy eating in children. *Journal of Human Resources*, 48(4), 855-872.
Kampkötter, P., & Marggraf, K. (2015). Do employees reciprocate to intra-firm trainings? An analysis of absenteeism and turnover rates. *The International Journal of Human Resource Management*, 26(22), 2888-2907.
Karaivanov, A., Kim, D., Lu, S. E., & Shigeoka, H. (2022). COVID-19 vaccination mandates and vaccine uptake. *Nature Human Behaviour*, 6(12), 1615-1624.
Karing, A. (2024). Social signaling and childhood immunization: A field experiment in Sierra Leone. *Quarterly Journal of Economics*, 139(4), 2083-2133.
Klick, J., & Stratmann, T. (2007). Diabetes treatments and moral hazard. *Journal of Law and Economics*, 50(3), 519-538.
Lawler, E. C. (2017). Effectiveness of vaccination recommendations versus mandates: Evidence from the hepatitis A vaccine. *Journal of Health Economics*, 52, 45-62.
List, J. A. (2020). Non est disputandum de generalizability? A glimpse into the external validity trial. NBER working paper 27535.
List, J. A., Metcalfe, R. D., Price, M. K., & Rundhammer, F. (2017). Harnessing Policy Complementarities to Conserve Energy: Evidence from a Natural Field Experiment. NBER working paper 23355.
List, J. A., & Momeni, F. (2021). When corporate social responsibility backfires: Evidence from a natural field experiment. *Management Science*, 67(1), 8-21.
List, J. A., Rodemeier, M., Roy, S., & Sun, G. K. (2023). Judging nudging: Understanding the welfare effects of nudges versus taxes. NBER working paper 31152.
List, J. A., & Samek, A. S. (2015). The behavioralist as nutritionist: leveraging behavioral economics to improve child food choice and consumption. *Journal of Health Economics*, 39, 135-146.
Loeb, M., Russell, M. L., Moss, L., Fonseca, K., Fox, J., Earn, D. J., Aoki, F., Horsman, G., Van Caeseele, P., Chokani, K., Vooght, M., Babiuk, L., Webby, R., & Walter, S.D (2010). Effect of influenza vaccination of children on infection rates in Hutterite communities: a randomized trial. *JAMA*, 303(10), 943-950.
Manski, C. F. (1993). Identification of endogenous social effects: The reflection problem. *Review of Economic Studies*, 60(3), 531-542.
Margolis, J., Hockenberry, J., Grossman, M., & Chou, S. Y. (2014). Moral hazard and less invasive medical treatment for coronary artery disease: The case of cigarette smoking. NBER working paper 20373.

Mas, A., & Moretti, E. (2009). Peers at work. *American Economic Review*, 99(1), 112-45.

Maurer, J. (2009). Who has a clue to preventing the flu? Unraveling supply and demand effects on the take-up of influenza vaccinations. *Journal of Health Economics*, 28(3), 704-717.

Mereckiene, J. (2015). ECDC Technical Report. Seasonal influenza vaccination in Europe Overview of vaccination recommendations and coverage rates in the EU Member States for the 2012–13 influenza season. European Centre for Disease Prevention and Control.

Milkman, K. L., Beshears, J., Choi, J. J., Laibson, D., & Madrian, B. C. (2011). Using implementation intentions prompts to enhance influenza vaccination rates. *Proceedings of the National Academy of Sciences*, 108(26), 10415-10420.

Milkman, K. L., Patel, M. S., Gandhi, L., Graci, H. N., Gromet, D. M., Ho, H., ... & Duckworth, A. L. (2021). A megastudy of text-based nudges encouraging patients to get vaccinated at an upcoming doctor's appointment. *Proceedings of the National Academy of Sciences*, 118(20), e2101165118.

Milkman, K. L., Gandhi, L., Patel, M. S., Graci, H. N., Gromet, D. M., Ho, H., ... & Duckworth, A. L. (2022). A 680,000-person megastudy of nudges to encourage vaccination in pharmacies. *Proceedings of the National Academy of Sciences*, 119(6), e2115126119.

Milkman, K. L., Ellis, S. F., Gromet, D. M., Jung, Y., Luscher, A. S., ... & Duckworth, A. L. (2024). Megastudy shows that reminders boost vaccination but adding free rides does not. *Nature*, 631, 179-188.

Moghtaderi, A., & Dor, A. (2021). Immunization and moral hazard: The HPV vaccine and uptake of cancer screening. *Medical Care Research and Review*, 78(2), 125-137.

Molinari, N. A. M., Ortega-Sanchez, I. R., Messonnier, M. L., Thompson, W. W., Wortley, P. M., Weintraub, E., & Bridges, C. B. (2007). The annual impact of seasonal influenza in the US: measuring disease burden and costs. *Vaccine*, 25(27), 5086-5096.

Montiel Olea, J. L., & Pflueger, C. (2013). A robust test for weak instruments. *Journal of Business & Economic Statistics*, 31(3), 358-369.

Nowalk, M.P., Lin, C. J., Toback, S. L., Rousculp, M. D., Eby, C., Raymund, M., & Zimmerman, R. K. (2010). Improving influenza vaccination rates in the workplace: a randomized trial. *American journal of preventive medicine*, 38(3), 237-246.

Oster, E. (2018). Does disease cause vaccination? Disease outbreaks and vaccination response. *Journal of Health Economics*, 57, 90-101.

Osterholm, M. T., Kelley, N. S., Sommer, A., & Belongia, E. A. (2012). Efficacy and effectiveness of influenza vaccines: a systematic review and meta-analysis. *Lancet Infectious Diseases*, 12(1), 36-44.

Østerhus, Sven Frederick. (2015). Influenza vaccination: a summary of Cochrane reviews. *European Journal of Clinical Microbiology & Infectious Diseases*, 34(2), 205-213.

Pichler, S. (2015). Sickness absence, moral hazard, and the business cycle. *Health economics*, *24*(6), 692-710.

Rabb, N., Swindal, M., Glick, D., Bowers, J., Tomasulo, A., Oyelami, Z., Wilson, K.H., & Yokum, D. (2022). Evidence from a statewide vaccination RCT shows the limits of nudges. *Nature*, 604(7904), E1-E7.

Rao, N., Kremer, M., Mobius, M., & Rosenblat, T. (2017). Social Networks and Vaccination Decisions. Mimeo.

Reddinger, J. L., Levine, D., & Charness, G. (2022). Can targeted messages reduce COVID-19 vaccination hesitancy? A randomized trial. *Preventive medicine reports*, 29, 101903.

Riener, G., Schneider, S. O., & Wagner, V. (2020). Addressing validity and generalizability concerns in field experiments. MPI Collective Goods Discussion Paper, (2020/16).

Ropero, A. M. (2011). Pan American Health Organization. Vaccination against seasonal and pandemic influenza. Buenos Aires.

Rothman, T. (2017). The Cost of Influenza Disease Burden in U.S Population. *International Journal of*

Economics and Management Sciences, 6:443.

Royer, H., Stehr, M., & Sydnor, J. (2015). Incentives, commitments, and habit formation in exercise: evidence from a field experiment with workers at a fortune-500 company. *American Economic Journal: Applied Economics*, 7(3), 51-84.

Samad, A. H., Usul, M. H., Zakaria, D., Ismail, R., Tasset-Tisseau, A., Baron-Papillon, F., & Follet, A. (2006). Workplace vaccination against influenza in Malaysia: does the employer benefit?. *Journal of Occupational Health*, 48(1), 1-10.

Sanson-Fisher, R. W., Bonevski, B., Green, L. W., & D'Este, C. (2007). Limitations of the randomized controlled trial in evaluating population-based health interventions. *American Journal of Preventive Medicine*, 33(2), 155-161.

Saccardo, S., Dai, H., Han, M., Vangala, S., Hoo, J., & Fujimoto, J. (2023). Field Tested: Assessing the Transferability of Behavioral Interventions. Mimeo.

Sato, R., & Takasaki, Y. (2019a). Peer effects on vaccination behavior: experimental evidence from rural Nigeria. *Economic Development and Cultural Change*, 68(1), 93-129.

Sato, R., & Takasaki, Y. (2019b). Psychic vs. Economic Barriers to Vaccine Take-up: Evidence from a Field Experiment in Nigeria. *World Bank Economic Review*, 33(3), 622-642.

Schaller, J., Schulkind, L., & Shapiro, T. (2019). Disease outbreaks, healthcare utilization, and on-time immunization in the first year of life. *Journal of Health Economics*, *67*, 102212.

Schmitz, H., & Wübker, A. (2011). What determines influenza vaccination take-up of elderly Europeans? *Health Economics*, 20(11), 1281-1297.

Schneider, F. H., Campos-Mercade, P., Meier, S., Pope, D., Wengström, E., & Meier, A. N. (2023). Financial incentives for vaccination do not have negative unintended consequences. *Nature*, 613(7944), 526-533.

Schneider, S. O., & Sutter, M. (2020). Higher order risk preferences: New experimental measures, determinants and field behavior. MPI Collective Goods Discussion Paper, (2020/22).

Sigelman, C., Maddock, A., Epstein, J., & Carpenter, W. (1993). Age differences in understandings of disease causality: AIDS, colds, and cancer. *Child Development*, 64(1), 272-284.

Sutter, M., Kocher, M. G., Glätzle-Rützler, D., & Trautmann, S. T. (2013). Impatience and uncertainty: Experimental decisions predict adolescents' field behavior. *American Economic Review*, 103(1), 510-531.

Talamàs, E., & Vohra, R. (2020). Free and perfectly safe but only partially effective vaccines can harm everyone. *Games and Economic Behavior*, 122, 277-289.

Tan, T. F., & Netessine, S. (2019). When you work with a superman, will you also fly? An empirical study of the impact of coworkers on performance. *Management Science*, 65(8), 3495-3517.

Van Den Berg, G. J., Hofmann, B., & Uhlendorff, A. (2019). Evaluating vacancy referrals and the roles of sanctions and sickness absence. *Economic Journal*, 129(624), 3292-3322.

Van Ourti, T., & Bouckaert, N. (2020). The Dutch influenza vaccination policy and medication use, outpatient visits, hospitalization and mortality at age 65. *European Journal of Public Health*, *30*(2), 275-280.

Ward, C. J. (2014). Influenza vaccination campaigns: is an ounce of prevention worth a pound of cure? *American Economic Journal: Applied Economics*, 6(1), 38-72.

White, C. (2021). Measuring Social and Externality Benefits of Influenza Vaccination. *Journal of Human Resources*, 56(3), 749-785.

WHO FluNet (2021). FluNet, World Health Organization, https://www.who.int/tools/flunet.

Ziebarth, N. R., & Karlsson, M. (2010). A natural experiment on sick pay cuts, sickness absence, and labor costs. *Journal of Public Economics*, 94(11-12), 1108-1122.

Table 1 Average Employee Characteristics across Treatments of 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	0.37	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
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. This also applies to flu- and non-flu-related sickness, both of which refer only to respiratory diseases.

Table 2 Regression Analysis of Treatment Effects on Participation in the Firm Campaign

	Baseline	With Controls	No Late-Comers	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 late-comers who were assigned to vaccinate earlier in the workweek (on Wednesday or Thursday) but went to vaccinate later (on Friday or 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. 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. The sample used for this analysis is therefore restricted to the bank's 116 units with at least two employees and thereby differs from the main sample. Note that the results of this analysis hold when using a sample that is further restricted to at least three employees. 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	

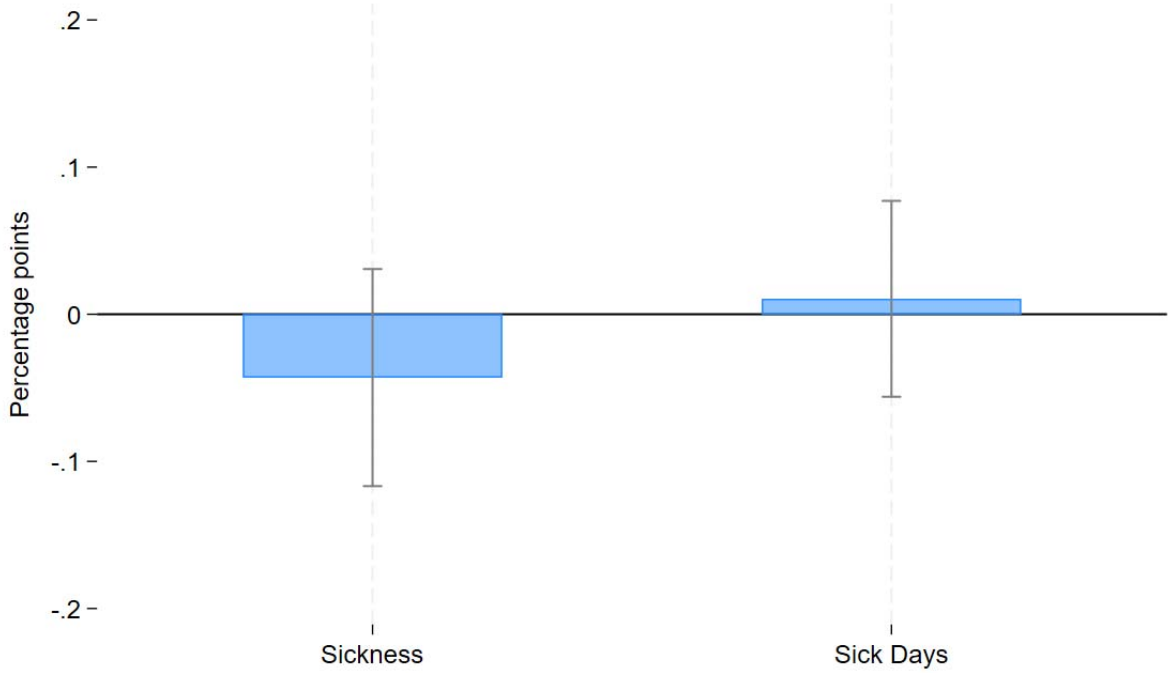
Note: This table presents estimates of the effects on the probability of being diagnosed sick with any illness and the probability of having a sick day granted for any illness. The estimates include only units with two or more employees. All specifications control for Quito fixed effects. Column 1 presents 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 (1)	Coefficient (2)	N (3)
Health-related habits index	6.30	-0.5205** (0.2215)	359
How often do you exercise	5.93	-0.3100 (0.4025)	359
How often do you take dietary supplements	3.18	-0.6238 (0.4374)	359
How often do you carry an umbrella when it rains	6.85	-1.2280** (0.4855)	359
How often do you wash your hands	9.25	0.0799 (0.1832)	359

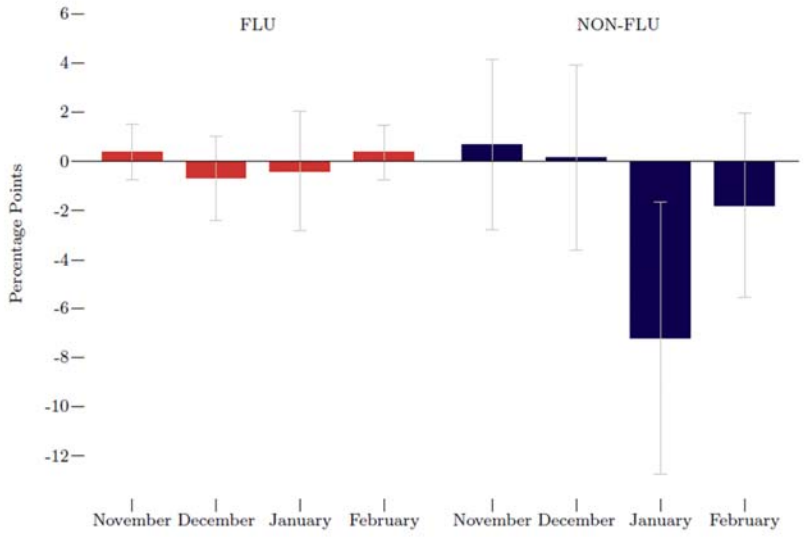
Note: This table presents the estimates of the effect of being assigned to the workweek on four health-related habits and activities. All responses are on a scale from 1 ("never") to 10 ("all the time"). The health-related habits index is the sum of all four scores divided by four. All specifications 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



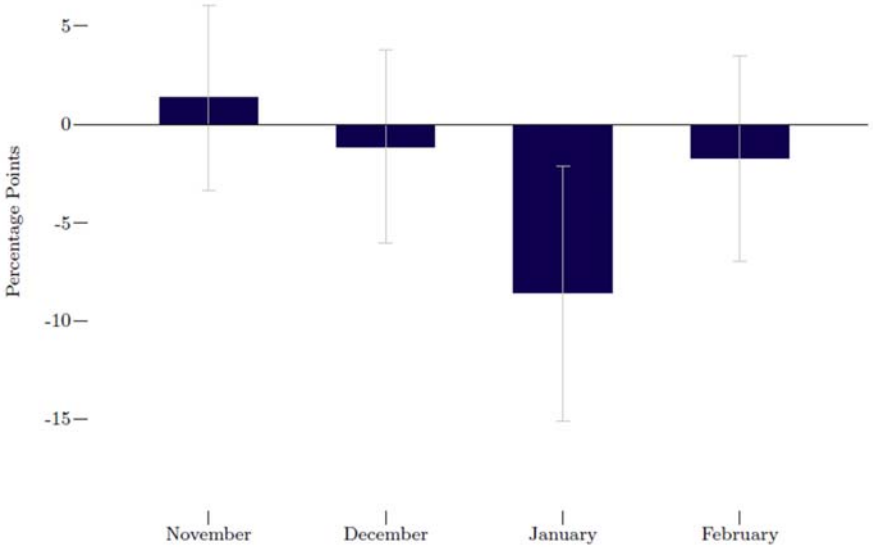
Note: This figure presents the average treatment effect of assignment to the workweek relative to Saturday on the probability of sickness incidence (for any illness) and sickness absence (i.e. having a sick day granted for any illness). The figure presents 95% heteroscedastic robust confidence intervals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

Figure 2 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 3 Monthly Visits to the On-Site Doctor



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

Vaccines at Work:
Experimental Evidence from a Firm Campaign

Manuel Hoffmann Roberto Mosquera Adrian Chadi

Online Appendices

Online Appendix A

Table A1 Summary Statistics of Employee Characteristics from the Firm Campaign

Panel A. Basic Statistics

	Mean	Median	SD	Minimum	Maximum
Monthly Income ($)	1,766	1,366.10	1,550.62	318.32	17,387.60
Company Tenure (years)	7.95	6	6.68	0	40
Prop. Women	0.49	0	0.50	0	1
Age (year)	36.6	35	8.37	23	80
Prop. College Education	0.91	1	0.28	0	1
Prop. Having Children	0.52	1	0.50	0	1
Distance to Work (km)	7.62	6.68	5.39	0.17	65.90
Work Unit Size (#)	29.30	18	36.46	1	142
Pre Survey Participation	0.48	0	0.50	0	1
Post Survey Participation	0.36	0	0.48	0	1
Diagnosed Sick	0.66	1	0.47	0	1
Granted a Sick Day	0.37	0	0.48	0	1
Diagnosed Flu Sick	0.11	0	0.32	0	1
Diagnosed Non-Flu Sick	0.36	0	0.48	0	1
Vaccination Take-up	0.17	0	0.38	0	1

Panel B. Distribution of Continuous Variables

	P10	P25	P50	P75	P90
Monthly Income ($)	634.67	816.85	1,366.10	2,150	3,383.33
Company Tenure (years)	1	3	6	12	16
Age (year)	27	30	35	41	48
Distance to Work (km)	1.92	3.85	6.68	10.48	14.08
Work Unit Size (#)	4	7	18	30	128

Note: This table provides summary statistics in the form of the mean, median, standard deviation (SD), the minimum and maximum for the full sample of bank employees in Panel A. For the continuous variables shown in Panel A, we additionally provide distributional statistics in Panel B via percentiles (from P10 to P90). 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. This also applies to flu-related and non-flu-related sickness, both of which refer only to respiratory diseases.

Table A2 Regression Discontinuity Effects of the Vaccine Price on Campaign Participation

	Baseline	With Controls	Quito Sample	No Late-Comers
	(1)	(2)	(3)	(4)
Monthly earnings above $750	0.0590	0.1738	0.0655	0.0400
	(0.0730)	(0.1533)	(0.0786)	(0.0722)
N	608	608	461	604

Note: Robust standard errors in parentheses. This table presents the local average treatment effects of a 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. All specifications control for Quito fixed effects. Column 1 presents our main estimates without adding additional controls. In Column 2, we test the robustness of the main estimates controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level. Column 3 presents the estimates using only employees in Quito. In Column 4, we exclude late-comers who were assigned to vaccinate earlier in the workweek (on Wednesday or Thursday) but went to vaccinate later (on Friday or Saturday). Reducing the bandwidth in steps of $50 to $150 does not change the results. * p<0.1 ** p<0.05 *** p<0.01

Table A3 Recall Information Statements

	Heard Altruistic Statement	Heard Selfish Statement
	(1)	(2)
Altruistic Information	-1.5205	-8.6702**
	(4.9361)	(4.1577)
Selfish Information	-3.8382	-0.0535
	(4.9514)	(4.0233)
Saturday	-3.9681	-2.8515
	(6.2202)	(5.0108)
Baseline	69.47	76.71
N	378	378

Note: 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. * p<0.1 ** p<0.05 *** p<0.01

Table A4 Vaccination Assignment Day and Actual Day of Campaign Participation

	Employees	Did not vaccinate	Vaccinated on:			
			Wednesday	Thursday	Friday	Saturday
Assigned to Wednesday	403	326	73		1	3
Assigned to Thursday	298	224		69	1	4
Assigned to Friday	247	215			29	3
Assigned to Saturday	216	198				18
Total	1,164	963	73	69	31	28

Note: This table shows for all 1,164 employees when they were assigned to vaccinate during the campaign and when they got vaccinated (if so). Of the bank's 929 employees in Quito, after excluding the call center, 23.5% were assigned to vaccinate on Wednesday, 26.7% to Thursday, 26.6% to Friday, and 23.3% to Saturday. The four days refer to the intervention period from November 8 to 11, 2017.

Table A5 Heterogeneous Treatment Effects on Campaign Participation

	Men	**Women**	**Short Distance**	**Long Distance**	**No Children**	**Children**
	(1)	(2)	(3)	(4)	(5)	(6)
Altruistic Information	-0.0017	-0.0508	-0.0545	-0.0429	-0.0163	-0.0368
	(0.0452)	(0.0429)	(0.0445)	(0.0515)	(0.0421)	(0.0454)
Selfish Information	0.0098	-0.0166	-0.0085	-0.0201	0.0188	-0.0253
	(0.0439)	(0.0451)	(0.0458)	(0.0526)	(0.0435)	(0.0452)
Saturday	-0.0883**	-0.0677	-0.0825**	-0.0972**	-0.0531	-0.1056**
	(0.0413)	(0.0441)	(0.0420)	(0.0485)	(0.0396)	(0.0453)
N	593	571	445	449	556	608

Note: This table presents the effect of the different treatments on vaccination take-up for different subgroups in the study's population. Long distance is defined as employees living further than the median sample distance of 6.5 km. All specifications control for Quito fixed effects. Robust standard errors in parentheses.
* p<0.1 ** p<0.05 *** p<0.01

Table A6 Peer Effects Robustness and Alternative Definitions

		Additional control variables:	
	Baseline	Unit Size	Peer Characteristics
	(1)	(2)	(3)
A.			
Baseline: Workweek Assignment	0.0025***	0.0024***	0.0021**
	(0.0008)	(0.0008)	(0.0009)
N	1,138	1,138	1,138
B.			
Before-or-Same Day Assignment	0.0017**	0.0019**	0.0016**
	(0.0008)	(0.0007)	(0.0008)
N	1,138	1,138	1,138

Note: This table presents reduced form results of the proportion of peers assigned to vaccinate during the workweek on individual vaccination across different definitions of the instrument for peer vaccination and different sets of control variables. Panel A presents the results using the baseline definition of the instrumental variable, which is all employees who work in the same unit and were assigned to vaccination in the workweek while controlling for individual assignments to the workweek. Panel B presents the results when switching to an instrument using only exogenous variation in peers who were working in the same unit and were assigned to the same day or before to get vaccinated while controlling for individual assignments to the day of the week. Column 1 shows the baseline results where we control for Quito fixed effects. Column 2 shows the results when we control for the number of employees in each unit. Column 3 shows the results when we control for the number of employees in each unit and peers' age and gender. We always measure the proportion of peers in percentage points. Standard errors (clustered at the unit level) are in parentheses. * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table A7 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)	0.0015	3.74	378
	(0.0024)		
Talked with coworkers about getting vaccinated (pp)	-0.0008	0.56	360
	(0.0013)		
Went with coworkers to get vaccinated (pp)	0.0006	0.13	360
	(0.0008)		
Probability of Getting Healthy Without the Vaccine (0-100)	-0.0655	44.25	367
	(0.0474)		
Probability of Getting Healthy With the Vaccine (0-100)	0.0282	56.48	367
	(0.0524)		
Informed about the Flu (0-100)	-0.0295	69.80	372
	(0.0541)		
Informed about the Flu Vaccine (0-100)	-0.0638	63.70	372
	(0.0556)		
Afraid of the Flu (0-100)	-0.0394	37.20	372
	(0.0693)		
Afraid of the Flu Vaccine (0-100)	-0.0219	24.66	372
	(0.0705)		
Would Get Vaccinated out of the Workplace (pp)	0.0001	0.61	367
	(0.0012)		
Coworkers Convinced me to get Vaccinated (0-100)	0.0374	20.60	360
	(0.0733)		
I Convinced my Coworkers to get Vaccinated (0-100)	0.0732	28.37	360
	(0.0678)		

Note: 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. Standard errors (clustered at the unit level) are in parentheses. * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table A8 Cost-Benefit Back-of-the-Envelope Analysis

Average Statistics for Saturday (Control Group)

Daily wage	\$60.13
Sick day incidence	0.2736
Wage paid for sick day incidence	\$16.45
Vaccine subsidy	\$5.50

Cost-Benefit Analysis	**Worst Case**	**Average Case**	**Best Case**
Sick day incidence relative to Saturday	0.0830	0.0123	-0.0584
Average sick day incidence	0.3567	0.2859	0.2152
Wage paid for sick day incidence	\$21.45	\$17.19	\$12.94
Wages paid relative to Saturday	-\$5.00	-\$0.74	\$3.51
Wages paid relative to Saturday – vaccine subsidy	-\$10.50	-\$6.24	-\$1.99

Note: The table provides a back-of-the-envelope calculation for the costs and benefits of the firm campaign. Panel A shows control group statistics for the average employee. The mean daily wage has been calculated based on the monthly fixed wage divided by 22 workdays. The wage paid for sick day incidence is calculated based on the mean daily wage multiplied by the mean sick day incidence. The mean subsidy arises from the subsidies that the firm provided for individuals during this campaign to reduce the vaccine price. The total price of the vaccine (\$10.00) also includes the cost of the medical team of two nurses administering the vaccine, which means that the subsidies represent the relevant cost for the firm. Panel B shows the calculations for the cost-benefit analysis. The sick day incidence relative to Saturday is the treatment effect from Table 4 Panel B with the worst and best case being the 95% lower and upper confidence interval.

Table A9 Analysis of Employee 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	0.1731	0.1534	-0.0958	-0.2368	-0.1410
	(0.1356)	(0.1718)	(0.1962)	(0.2930)	(0.3617)
N	343	343	403	403	403

Note: This table presents estimates of the effect of the assignment to the workweek on self-reported measures productivity and duration of the workday. The post-intervention survey collects these self-reported measures on a scale from 0 to 10. The swipe card information corresponds to January and is measured in hours. Robust standard errors in parentheses. * p<0.1 ** p<0.05 *** p<0.01

Table A10 Analysis of Flu Diagnoses

	Correlation	**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)	
N	1,120	

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

Table A11 Robustness Check for Different Definitions of Flu Diagnoses

	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	-0.0151
	(0.0156)	(0.0160)	(0.0196)
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	1,145	1,145	1,145

Note: This table presents robustness checks of the effects of being assigned to the workweek on the probability of being diagnosed sick because of the flu using different definitions. All specifications control for Quito fixed effects. Panel B additionally considers control variables for the vaccine's price, income, tenure, division in the company, gender, age, education level. Robust standard errors in parentheses. Using clustered standard errors at the work unit level yields similar standard errors with no change in statistical significance. * p<0.1 ** p<0.05 *** p<0.01

Table A12 Heterogeneous Campaign 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	-1.2280**	359
		(0.4855)	
	B. Vaccine Effective		
How often do you carry an umbrella when it rains	7.13	-1.6098***	257
		(0.5639)	
	C. Vaccine Ineffective		
How often do you carry an umbrella when it rains	6.06	-0.2292	102
		(0.9615)	

Note: This table presents estimates of the 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. Robust standard errors in parentheses. Using clustered standard errors at the work unit level yields similar standard errors with no loss of statistical significance. * p<0.1 ** p<0.05 *** p<0.01

Figure A1 Firm Campaign Treatment Message: Control

Note: 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!

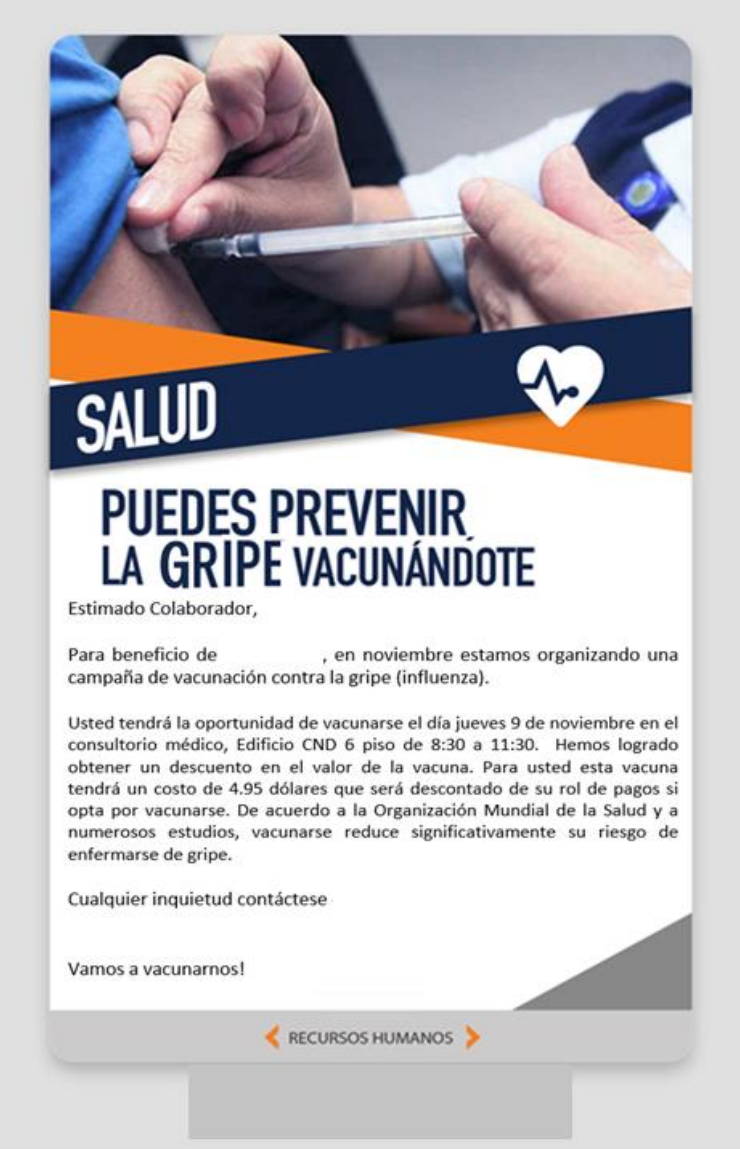
Figure A2 Firm Campaign Treatment Message: Opportunity Cost (Saturday)

Note: 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 Firm Campaign Treatment Message: Altruism

Note: 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 Firm Campaign Treatment Message: Selfish



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

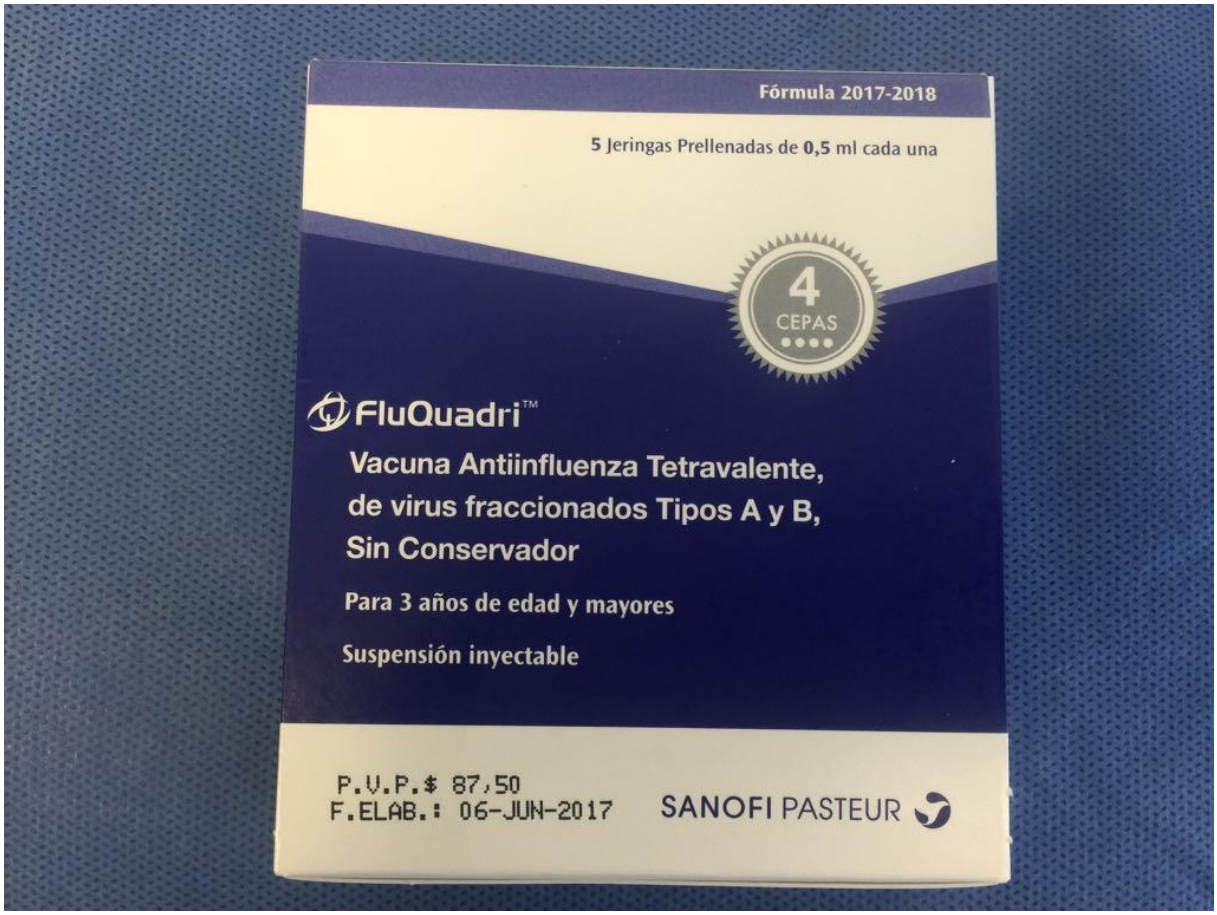
Figure A5 Locations of the Bank in Ecuador

Note: The map contains the locations of the bank in Ecuador (orange), where the firm campaign was implemented.

Figure A6 Timeline of the Firm Campaign

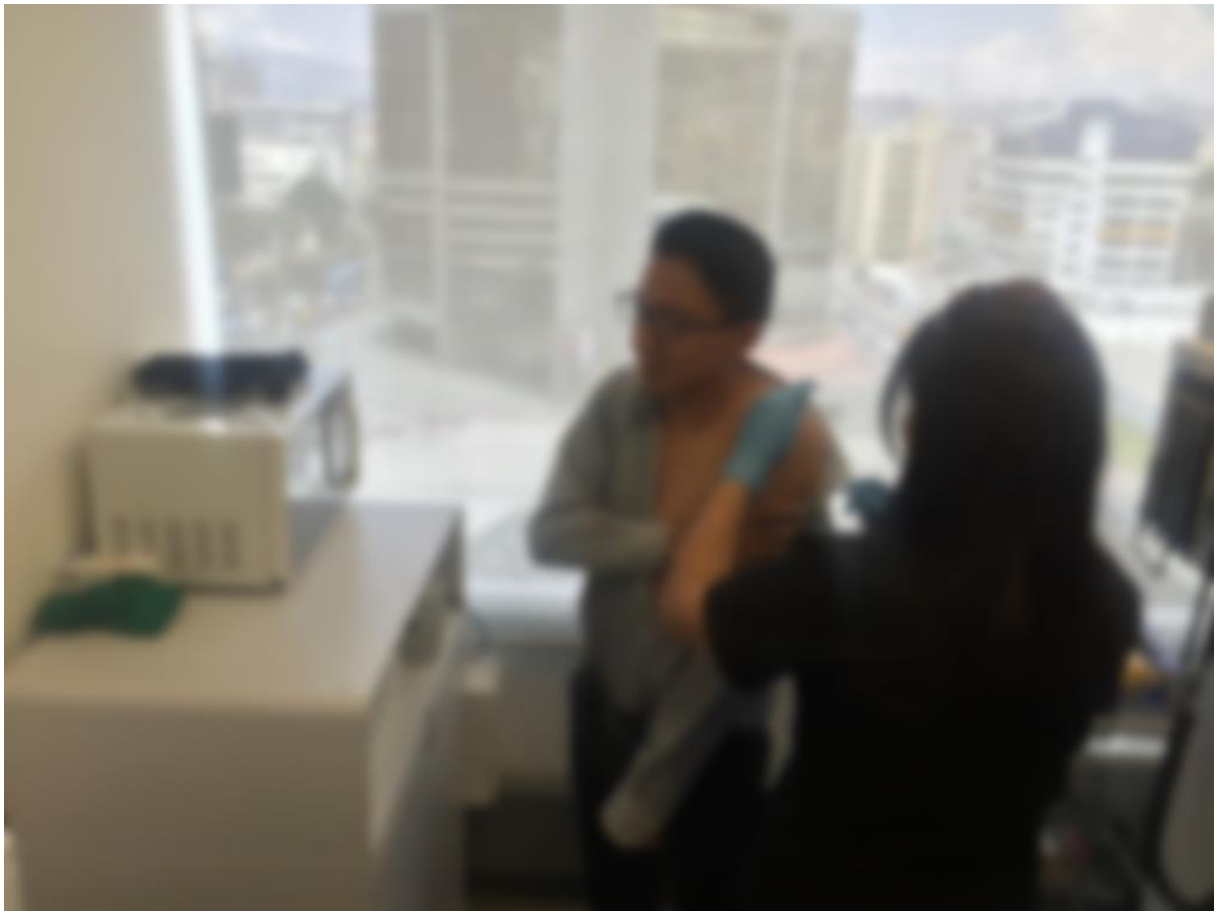
Note: The bank sent out the pre-intervention survey on October 18. The bank sent emails with the different treatments on November 1 using the Human Resources department mail account. It sent a reminder email on November 7. The firm campaign took place between November 8 and November 11. The post-treatment period (Ecuadorian flu season) went from November 13 to March 1. The bank sent the post-intervention survey out during March and April 2018.

Figure A7 The Firm Campaign's Influenza Vaccine



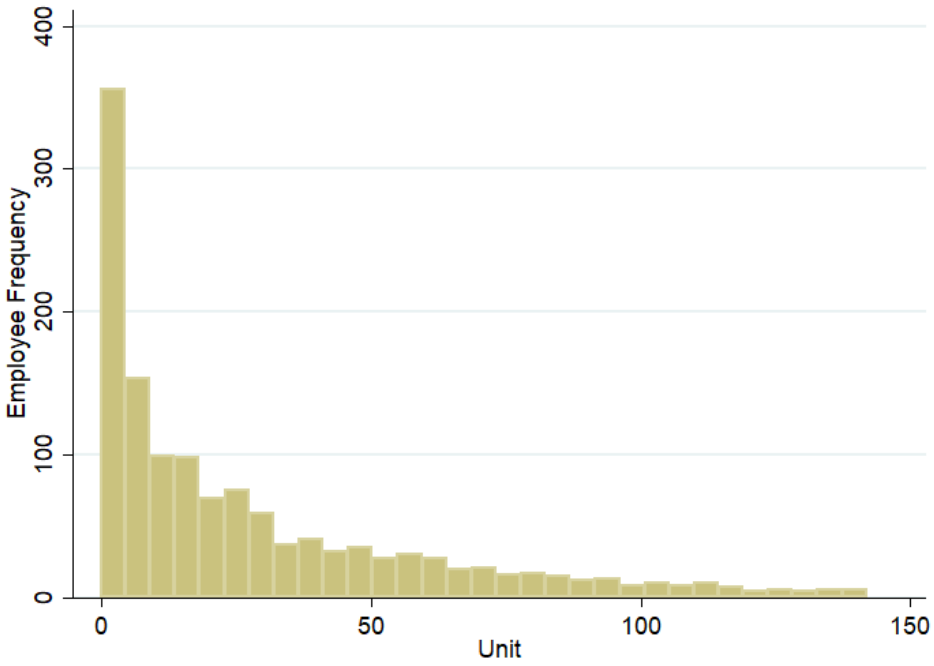
Note: The above package contains the influenza vaccine used in the firm campaign. According to the manufacturer, this vaccine protects against four strands of the flu, two from type A and two from type B.

Figure A8 Firm Campaign: Flu Shot in Action



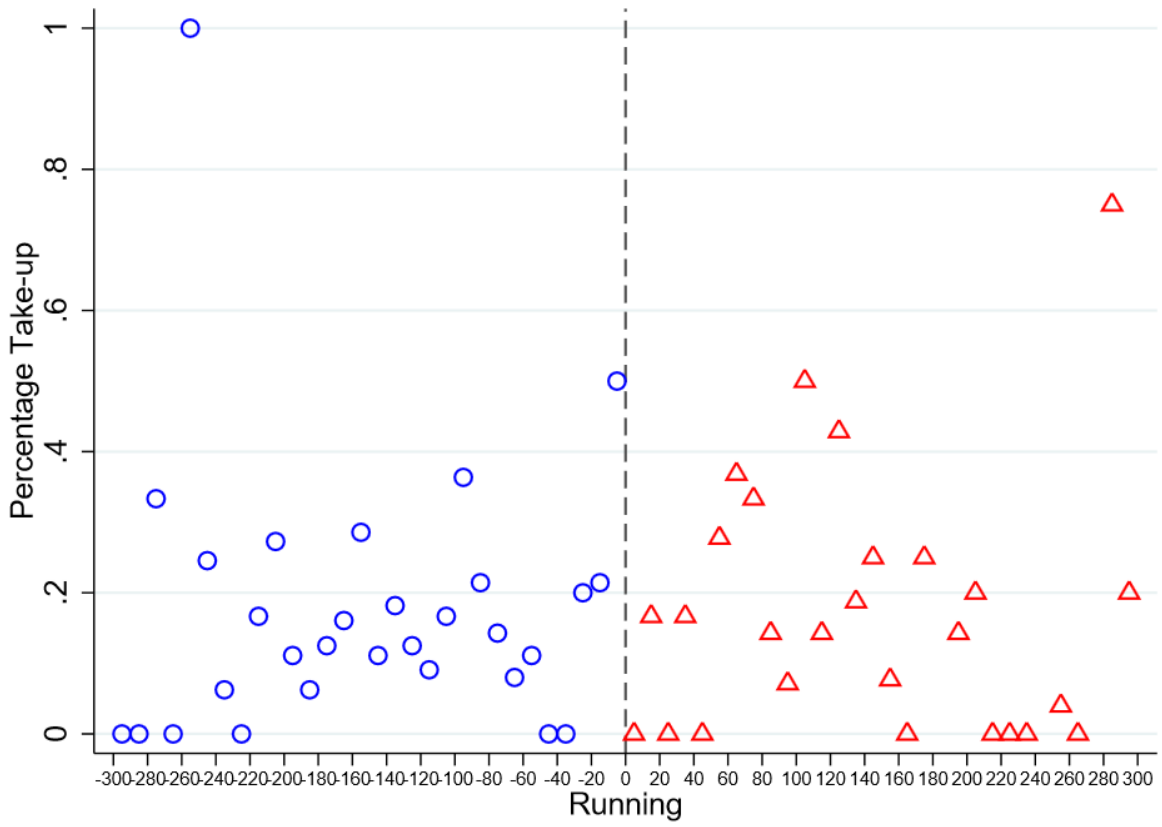
Note: Participation in the campaign at the firm.

Figure A9 Frequency Distribution of Employees in Units



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

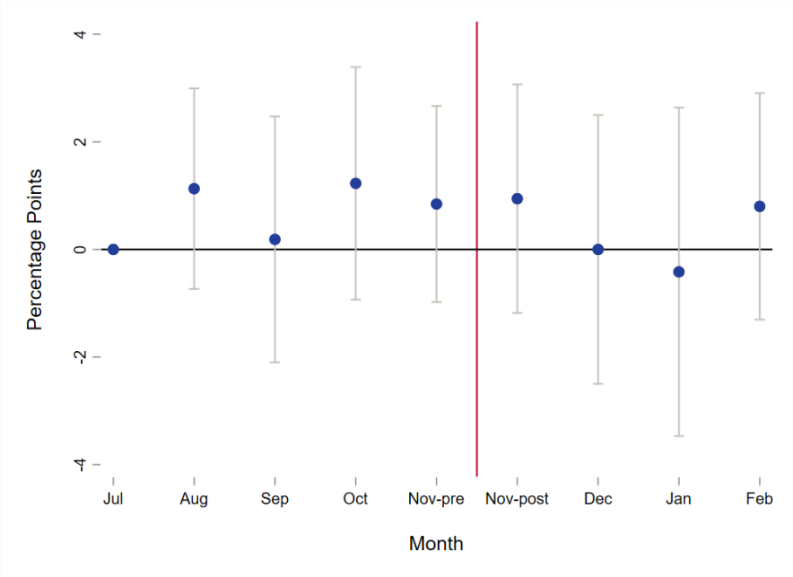
Figure A10 Firm Campaign Participation around $750 Wage Threshold



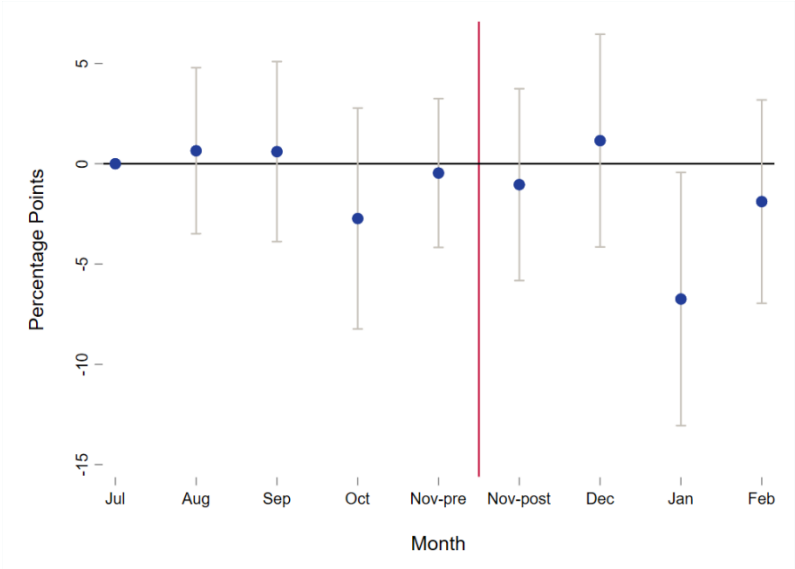
Note: This figure presents the evolution of firm campaign participation around the $750 threshold with a bin size of $10. Individuals who earn more than $750 paid $7.49 for the vaccine, while employees whose wage is below this threshold paid $4.95.

Figure A11 Difference in Difference Analysis of Sickness Incidence

(a) Flu Diagnoses

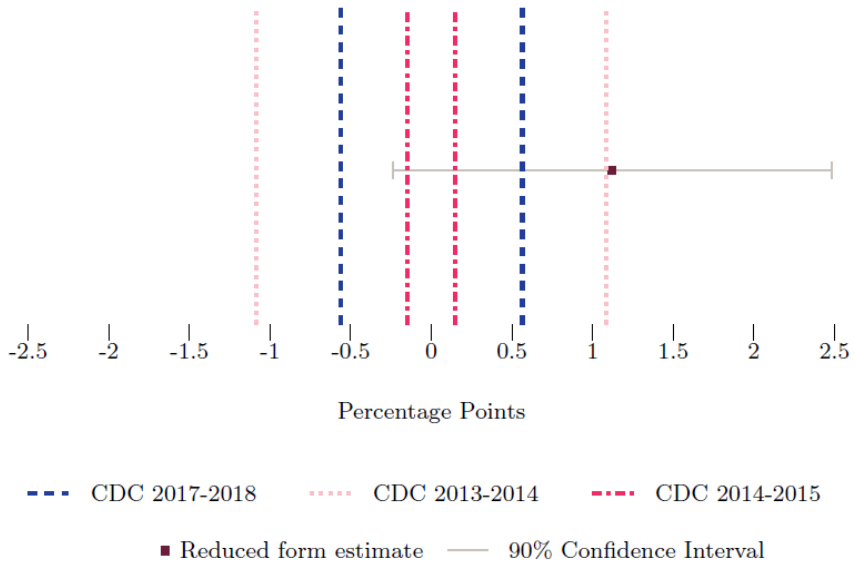


(b) Non-flu Diagnoses



Note: This figure presents difference-in-differences estimates of the effect of assignment to the workweek on flu and non-flu diagnoses incidence. Estimates control for individual fixed effects. 95% heteroscedastic robust confidence intervals are shown.

Figure A12 Equivalence Test for the Effectiveness of the Firm Campaign



Note: This figure presents the estimate of the effect of assignment to the workweek on flu-related sickness absenteeism to adjusted CDC estimates of the effectiveness of the flu vaccine for 2013-2014, 2014-2015, and 2017-2018 seasons.

Appendix A1: Background Information on the Study Design

This appendix chapter describes how our study was conceived and executed. We provide additional details on the background of the experimental intervention and our ex-ante plan, including ideas that we do not discuss in the main body of the paper. In this context, we also point out aspects of our analysis that are more exploratory in nature, which may be of interest given that in 2017, at the time of our intervention, we did not register the design and empirical analysis before starting the data collection (although we registered our study later).

After initiating our cooperation with the Ecuadorian bank where we ran the intervention, we designed our study in 2017. Thanks to this cooperation, we could implement a random encouragement design to study the consequences of a firm-wide vaccination campaign as a novel contribution to the economic analysis of health interventions. For that purpose, it was important to detect significant determinants of vaccination modifiable by us to create exogenous variation in vaccine take-up. A central part of our research objectives has therefore always been the discussion and analysis of what motivates people to get vaccinated, especially since understanding the determinants of vaccination is another important and policy-relevant research aim.

In our pre-intervention discussions with the bank, we identified several potentially relevant determinants of flu vaccination that could enable us to study how campaigns to increase vaccine take-up may or may not affect health outcomes in a firm context. First, we discovered an organizational bottleneck concerning the scheduling of vaccinations, as several days, including Saturday, were necessary to offer an appointment for the entire 1000+ employee workforce. We expected this could make campaign participation more or less attractive for employees whose opportunity costs of vaccination may differ depending on the day of the week. Second, inspired by the fact that vaccination encouragement emails are a routine aspect of our work lives, we exploited that many employees were offered an appointment during the workweek to establish additional within-workweek information treatments in the form of two information nudges, thereby mimicking an intervention often used by employers to encourage employees to vaccinate. To prevent possible contamination across treatments and confounding the effect of information with salience or other behavioral factors, we kept these additional email messages as unobtrusive as possible. Finally, we found out about a third opportunity to induce exogenous variation: Given that health insurance does not cover flu vaccination, the bank was willing to subsidize the vaccine

for its employees. Contrary to the assignment day (modification 1) and email content (modification 2), the firm did not allow us to randomize subsidies across employees, but we could set the wage threshold at which the subsidy of the vaccine changed. This created a textbook case of a regression discontinuity design (RDD) to better understand whether employees' vaccination decisions are sensitive to minor changes in the price of a medical product.[1]

Regarding the determinants of campaign participation, we follow our ex-ante plan closely in the paper as we describe all modifications of the campaign to vary employee vaccination, including those that did not affect behavior significantly. Accordingly, we include a discussion of the RDD approach and the original experimental design with its modification of the email content. What we have kept out of the paper is an even more detailed analysis of why people do (not) get vaccinated. In this context, we enriched our employee surveys with questions on pro-sociality, risk, and time preferences, which we initially thought could inform a discussion of the mechanisms underlying vaccination decisions, especially if we were unable to find significant determinants of vaccination.

Instead, we focus more in the paper on the consequences of campaign participation where we follow our plan closely by exploiting our rich data on health outcomes. This is straightforward as employee health and especially sickness absence are of great relevance from the standpoint of the researcher and economically aligned with the firm interest in recouping a return on its investment. Moreover, we provide a discussion of explanations for the ineffectiveness of the campaign, which contrasted expectations of the firm that paid for it, to enrich the value of our contribution and inform policymakers interested in the outcomes of health campaigns. Part of the discussion is an analysis based on data we collected on purpose, such as survey data on health-related behaviors. Other analyses, such as the one based on doctor visits during the national emergency in early 2018, are exploratory, as we rely on events and information that we could not anticipate. In this part of the paper, we also benefit from using the aggregate share of workweek assignments at the business unit level as a trigger for peer vaccination. This idea came up during our work on the project after we found out that the workweek assignment indeed strongly affected vaccination. By analyzing the effects of peer vaccination at the unit level, we can discuss a possible role of health externalities as a potential explanation for the lack of differences in health between treatment groups and separately complement our analysis of the determinants of campaign participation.

[1] In this analysis, we focus on employees for whom the vaccine subsidy varied depending on their wage. This excludes employees who were targeted by the previous year's campaign and for whom the bank decided to pay a full subsidy.

Appendix A2: Additional Analyses of Flu-Related Sickness Outcomes

In this appendix, we describe our analyses of data on medical diagnoses to learn more about possible effects of participation in the firm campaign. A correlation analysis presented in Column 1 of Table A8 suggests that individual vaccination goes along with a lower probability of being diagnosed with the flu. However, the analysis in Column 2 based on our main approach reveals a positive effect of being assigned to the workweek on the probability of being diagnosed with the flu, which is close to zero and insignificant. This suggests that the campaign was ineffective in decreasing the probability of having the flu. Furthermore, the estimates in Columns 1 and 2 show no effects of the proportion of vaccinated peers on the probability of being diagnosed with the flu.

As can be seen in Table A9, our main finding is robust to the use of a broader and a narrower definition of flu-related illness as well as to the inclusion of control variables (gender, age, tenure, and income). In another check, we again find no significant impacts when controlling for individual fixed effects using a month-based difference-in-difference approach (Figure A11).

To evaluate whether we can rule out meaningful effects of the firm vaccination campaign on flu-related illness, 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). These estimates come from observational studies on flu hospitalizations and, hence, might be biased because of endogeneity issues. Also, hospitalizations almost exclusively occur to the very old and are less likely for the average working-age person. While the CDC numbers may thus be seen as an overestimate, they constitute the criteria that policymakers use to assess vaccine effectiveness.

In the following exercise, we compare the CDC data with our evidence from the firm campaign by focusing on the probability of being granted a sick day due to the flu. While the CDC provides data about the percentage effectiveness of the vaccine, 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 consider demographic characteristics that affect natural immunity to the flu and thus result in larger estimates; therefore,

the reported cross-tabulation percentages are a conservative lower bound of the CDC estimates. 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 our estimates from the firm campaign, we multiply the CDC effectiveness values by the relevant first stage of the workweek assignment effect on vaccine take-up. This calculation yields reference values of −1.1 percentage points, −0.1 percentage point, and −0.6 percentage points, respectively.

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

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

References

CDC (2019). Past Seasons Vaccine Effectiveness Estimates. Center for Disease Control. Accessed July 28, 2019. Source: https://www.cdc.gov/flu/vaccines-work/past-seasons-estimates.html.

Hartman, E., & Hidalgo, F. D. (2018). An equivalence approach to balance and placebo tests. *American Journal of Political Science*, 62(4), 1000-1013.

King, G., Tomz, M., & Wittenberg, J. (2000). Making the Most of Statistical Analyses: Improving Interpretation and Presentation. *American Journal of Political Science*, 347-361.

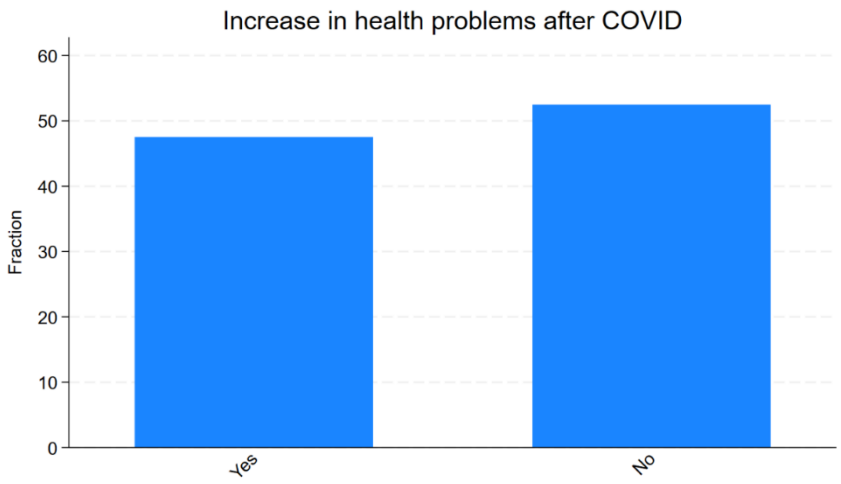
Lakens, D. (2017). Equivalence tests: a practical primer for t tests, correlations, and meta-analyses. *Social Psychological and Personality Science*, 8(4), 355-362.

Rainey, C. (2014). Arguing for a negligible effect. *American Journal of Political Science*, 58(4), 1083-1091.

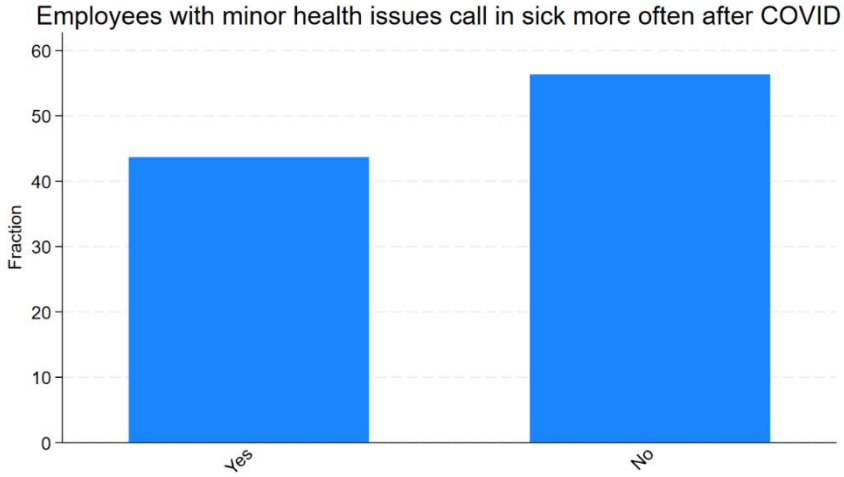
Online Appendix B

Figure B1. Survey Questions on Health and Sickness Absence

Panel A

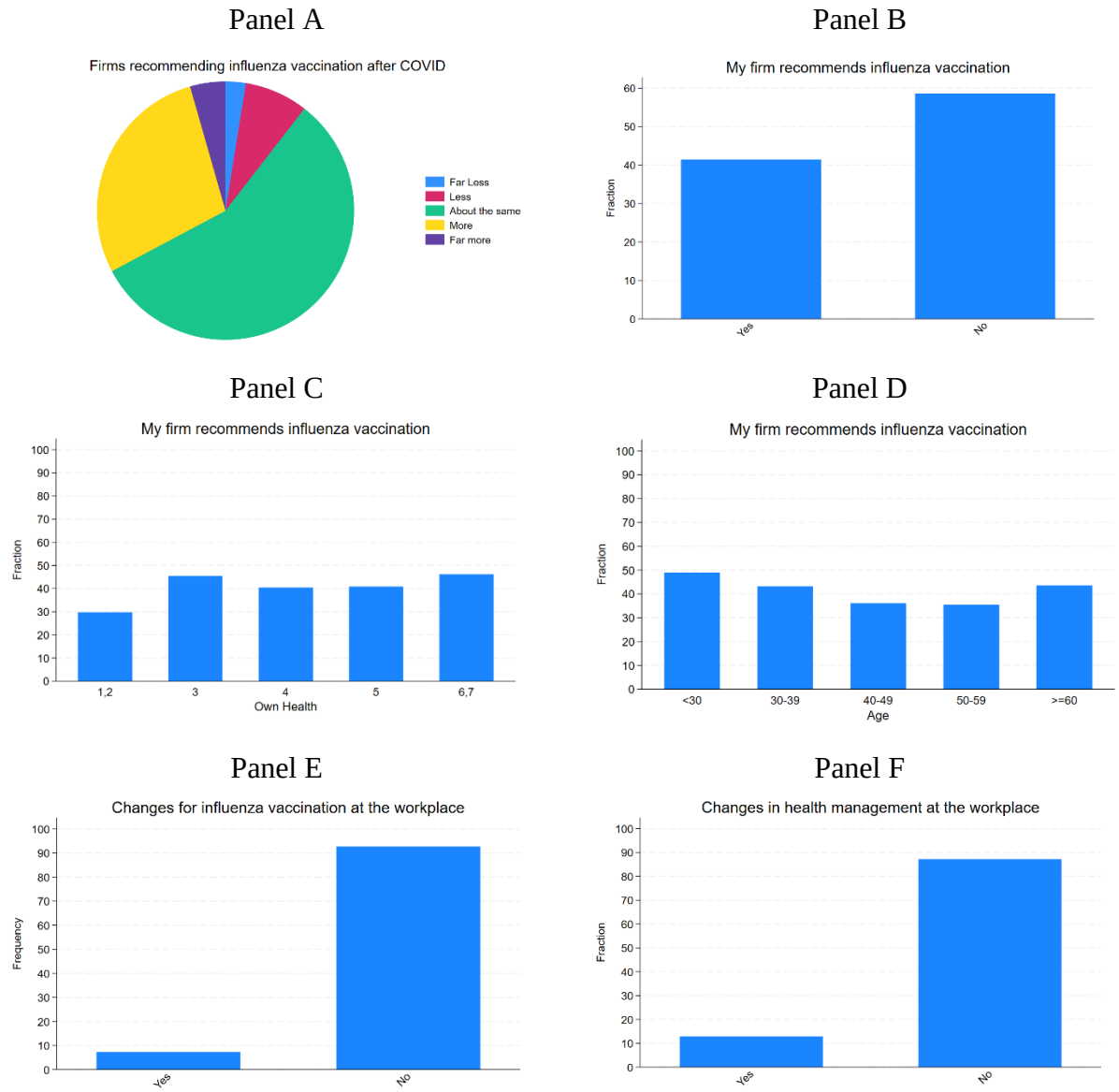


Panel B



Note: This figure presents the fraction of individual responses to questions 1 and 2 in our survey on changes in employee health and health management challenges in the post-COVID era. The question for Panel A is "Do you think that employees in 2024 have more health problems than before the COVID pandemic?" The question for Panel B is "Do you think that employees with minor health issues call in sick more often in 2024 than before the COVID pandemic?"

Figure B2. Survey Questions on Workplace Health Management



Note: This figure presents the fraction of individual responses to questions 3 to 6 in our survey on changes in employee health and health management challenges in the post-COVID era. The question for Panel A is "Do you think that compared to the time before the COVID pandemic, more or fewer companies today recommend that their employees get a flu vaccination as part of their health management?" The question for Panel B is "Does your employer recommend getting a flu vaccination?" In Panels C and D, we show the share of "Yes" answers to the question in Panel B (regarding the employer's recommendation to get a flu vaccination) across respondents' health status and age categories. To capture their own health status, the respondents indicate whether they are "healthy" on a scale from 1 ("does not apply at all") to 7 ("does apply completely"). The question for Panel E is "Has your employer's workplace health management regarding flu vaccinations changed compared to the time before the COVID pandemic?" The question for Panel F is "Has your employer's workplace health management generally changed compared to the time before the COVID pandemic?"

Appendix B: Workplace Health Management Survey (2024)

This appendix examines the extent to which firms may face new health management challenges in the post-COVID era and the role of flu vaccinations in this context. A possible reason to expect such changes are increases in employee absenteeism observed in some countries in the wake of the pandemic. One example is the country of Germany where absence rates increased from around 11 to 15 sick leave days in 2022, the year the country lifted most of its pandemic restrictions. Germany's federal statistical office provides the official statistics and refers to "major waves of influenza and colds" as possible explanation for the high level of sickness absence. More recent findings based on analyses of healthcare insurance data show that absence rates in Germany remain high since then and media reports point to a role of respiratory problems in this context.[2]

For managers and policymakers, such statistics seem to further underscore how important policies and managerial considerations today are to prevent diseases in the working-age population. On the one hand, one could argue that companies now have even stronger motives to think of ways to address ill-health in employees, especially in relation to respiratory problems, so that evidence-based analyses of economic costs and benefits of health campaigns such as in our paper appear even more relevant than before. On the other hand, one could ask if there are changes in the role of health interventions, especially flu vaccination campaigns, as one of the most common policies preferred by companies to tackle potential health problems in their workforces.

To empirically assess the possibility of new health management challenges for companies, we conducted a survey with individuals from Germany, a country with a widely reported rise in sickness absence post-COVID. We conceived a module with several survey questions that we integrated into an ongoing survey project conducted online in the summer of 2024. Our sample includes 1,113 quasi-representative observations which were drawn from the adult population of Germany along the criteria of gender, age, and educational levels.

We designed the survey questions to address two major issues. First, it is an open question whether there was an increase in health problems that could have led to the reported jump in absenteeism after the pandemic. While this phenomenon could be driven by increases in actual

[2] The internet sources can be found here: https://www.destatis.de/EN/Themes/Labour/Labour-Market/Quality-Employment/Dimension2/2_3_StaffSickLeave.html (website of Germany's federal statistical office about sick leave), https://www.spiegel.de/wirtschaft/service/arbeitsmarkt-und-krankenstand-deutschland-kaempft-mit-anhaltend-hohen-fehlzeiten-a-a962ac9b-75d5-4d93-838c-5487b59a1f08 (example of news coverage in the German media) and https://www.ft.com/content/8e7bc450-7dc7-45c2-82ed-99ab2a8c4952 (example of international news coverage).

health problems, it could also be due to behavioral changes, such as employees calling in sick more often when they have mild health problems, similar to the discussion in our paper on employee behavior. Second, assuming that there are noticeable changes in sickness absence due to changes in employee health, we asked questions to identify possible implications for health management in companies. In particular, we were interested in whether there are changes in the role of firm-wide health policies today, compared to before 2020, also with respect to flu vaccination.

As the first result from our survey, Figure B1 Panel A shows that slightly more than half of the respondents (52%) do not agree that employees are sick more often today compared to before COVID. This is interesting given the above-mentioned and widely reported statistics on sick leave, which suggest increased health problems among working-age people in the wake of the pandemic.[3] Second, Figure B1 Panel B shows that a relatively large share of respondents (43%) believe that employees call in sick more often today if they have mild health problems.[4] Arguably, this behavioral finding can help explain the increase in absenteeism reported by German health institutions if some employees may not be sick more often but make different behavioral choices.

In Figure B2, we illustrate the evidence from our questions on the health management in a company context, with a focus on flu vaccination. Panel A provides little evidence of perceived changes in the likelihood of employers recommending flu vaccines to their employees as part of their health management, which, if anything, seems to play a bigger role nowadays as it did before COVID. According to Panel B, the share of employers actually recommending flu vaccination to their workforces is quite substantial (40%), considering that there are millions of different companies in Germany.[5] Notably, this share is quite similar across different groups of people in terms of self-assessed health and age (Panels C and D). Finally, we asked questions about perceived changes in the health management of firms. This idea receives almost no support, as the vast majority (93%) do not report changes in the health management concerning flu vaccinations at work (Panel E), and only a small share of respondents (12%) report that workplace health management has generally changed compared to the time before the COVID pandemic (Panel F).

[3] When we inspect the text comments by respondents who do agree with the idea of increased health problems, we obtain no clear picture. Their responses refer to many different things, including a weakened immune system, which might be consistent with the idea of increased susceptibility towards respiratory diseases.
[4] As an example of a textbox comment to explain the increase in shirking behavior, a respondent stated that "everything is blamed on COVID" nowadays. Another respondent argued that it is "easier to get sick leave" post-COVID.
[5] We can also look at textbox comments by those respondents who reported that their employer does not provide a recommendation. Accordingly, some of the reasons given are employer indifference, lack of trust, and cost concerns.