COVER LETTER



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National Bureau of Economic Research (NBER)

April 18, 2022

Dear Selection Committee,
Dear Drs. Raymond Fisman, Will Dobbie,

as a first-generation student and Hispanic, I write to you to express my interest in the Review of Economics and Statistics Fellowship for the NBER Summer Institute.

This fellowship is of extraordinary interest to me to engage in effective research in economics by receiving high-quality exposure to novel research from leading experts in the field via the attendance to the NBER Summer Institute. I am an economist with a PhD from Texas A&M University who has worked as a postdoctoral scholar at the Stanford Center for Population Health prior to switching to the Laboratory for Innovation Science at Harvard.

I am studying individual and peer behavior from different backgrounds, institutions, and cultures with an important focal area of Latin America. The overarching aim of my research is to advance our understanding of causes and consequences of problematic individual choices and conditions that affect health, wellbeing, and the labor market.

In a field experiment, I investigate the determinants affecting decisions to vaccinate and moral hazard implications within a bank located in the Ecuadorian capital Quito and beyond. I find that a main determinant for individual vaccination is peer vaccination, which is likely driven by passive peer pressure via social norms. In a work-in-progress I move from studying passive to active peer pressure by testing an economic theory in partnership with a loan collection company in Ecuador.

To understand different cultures and institutions, I am further conducting research in- and outside the United States. For example, in Germany I am studying television its impact on wellbeing and the labor supply as well as the question how to overcome peer pressure to drink. In Australia, I am studying the unintended consequences of supplementary private health insurance on financial well-being. In the United States, I leverage laboratory test thresholds to understand racial disparities in medication prescriptions.

I am very interested in the following NBER sessions: Development Economics, Health Care, Health Economics, Public Economics and Labor Studies.

Thank you for your time and consideration.

Sincerely,

Manuel Hoffmann

MANUEL HOFFMANN

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EMPLOYMENT

Harvard University, Harvard Business School, Faculty of Arts and Sciences, Postdoc *2021*
Princeton University, The Mamdouha S. Bobst Center for Peace and Justice, Visiting Postdoc *2020-2021*
Stanford University, College of Medicine, Center for Population Health Sciences, Joint Postdoc
University of Heidelberg, Heidelberg Institute of Global Health, Joint Postdoc

RESEARCH INTERESTS

Behavioral Economics, Finance, Innovation, Labor, Development, and Health

EDUCATION

Ph.D. in Economics, Texas A&M University *2020*
European Master in Labour Studies, London School of Economics and University of Trier *2015*
M.Sc. in Survey Statistics, University of Trier
M.Sc. in Economic Analysis and Measurement, University of Trier *2014*
M.Sc. in Business Administration, University of Trier (*top of my class*)
- Study Abroad: Texas A&M International University, Laredo, TX
B.Sc. in Business Administration, University of Trier *2011*
- Study Abroad: Bloomsburg University of Pennsylvania

WORKING PAPERS

Vaccines at Work (with Roberto Mosquera and Adrian Chadi)
Influenza imposes substantial costs in terms of human lives and productivity losses. Vaccination could be a cost-effective way to reduce these costs, but low take-up rates, particularly of working adults, and vaccination unintentionally causing moral hazard may decrease its benefits. We ran a natural field experiment with employees of a large bank in Ecuador where we experimentally manipulated incentives to participate in a flu vaccination campaign. We find that reducing opportunity costs of vaccination doubled take-up. Coworker peer take-up also increased individual take-up significantly. Contrary to the company''s expectations, the effect of vaccination on health outcomes was ineffective with no measurable health externalities. Using administrative records on sickness diagnoses and employee surveys, we find evidence consistent with vaccination causing moral hazard, which could decrease its effectiveness.

Private Health Insurance under Universal Health Care
While the purchase of private health insurance is clearly beneficial in the absence of public insurance, it is difficult to evaluate individual costs and benefits when baseline coverage exists for everyone. I study the consequences of being privately insured when universal health care is the baseline. The problem of selection is approached through a regression kink design in conjunction with a policy implemented in Australia in the year 2000, punishing agents for delaying the purchase of private health until later in life. By investigating the benefits with an administrative tax panel, this study reveals potential hidden costs and unintended consequences of private health insurance under universal health care.

Television, Health and Happiness (with Adrian Chadi)
Watching television is the most time-consuming human activity besides work but its role for individual well-being is unclear. Negative consequences portrayed in the literature raise the question whether this popular activity constitutes an economic good or bad and is a prime example of irrational behavior reducing individual health and happiness. We are the first to comprehensively address this question by exploiting a large-scale natural experiment in West Germany, where households in a few geographically restricted areas received commercial television via terrestrial frequencies. Rich panel data allow us to determine how signal availability over time changes individual time use and well-being. Contrary to previous research, we find no health impact when television consumption increases. For life satisfaction, we even find positive effects. Additional data supports the notion that television is not an economic bad and that non-experimental evidence may be driven by negative selection.

Overcoming Peer Pressure to Drink (with Vanessa Mertins, Bernd Leisen)
Fairness Preferences and the Pandemic (with Catherine Eckel, Yinjunjie Zhang, Raisa Sara)

PUBLICATION

The Pro-Russian Conflict and its Impact on Stock Returns in Russia and the Ukraine,
Journal of International Economics and Economic Policy with Matthias Neuenkirch (2015).

WORK IN PROGRESS

- Active Peer Pressure: A Loan Repayment Field Experiment (Roberto Mosquera and Daniel Stephenson)
- Competition and Collaboration under Open Innovation (Frank Nagle and Aastha Gupta)
- TV and the Labor Market, Two Natural Experiments from Germany (Adrian Chadi and Sven Hartmann)
- Surrogate Indices, Government Policies, and Health (Susan Athey, Alice Milivinti, Rebecca Hasdell)

RESEARCH AFFILIATION

Harvard University, Laboratory for Innovation Science at Harvard	*2021-now*
Stanford University, The Center for Population Health Sciences, Researcher	
Australian National University, Tax and Transfer Policy Institute (TTPI), Research Affiliate	
University of Heidelberg, Heidelberg Institute of Global Health	
U.S. Veterans Affairs, Palo Alto, Researcher	
Victoria University of Wellington, Research Associate	*2019-now*
European Commission, Behavioral Insights Team, Nudge Unit, Expert Consultant	*2020*
Duisburg-Essen University, CINCH, Health Economics Fellow	
University of Melbourne, Melbourne Institute, Visiting Researcher	*2018*
German Institute for Economic Research (DIW), Visiting Researcher	
Texas A&M University, Center for Field Experiments and Design (Profs. Petrie & Castillo)	*2018-2020*
Texas A&M University, Economic Research Laboratory (Prof. Eckel)	*2015-2020*
University of Trier, Experimental Economics Laboratory (Prof. Mertins)	*2013-2015*
University of Trier, Institute for Labor Law and Industrial Relations in the EU (Prof. Goerke)	*2013-2015*

HONORS, AWARDS AND GRANTS

Stanford Spectrum NIH Grant with Susan Athey, Alice Milivinti, Rebecca Hasdell ($20,000, Co-PI)	*2021*
Harvard Research Funding ($3,000)	
Royal Economic Society Conference Travel Grant (£500)	
Texas A&M Graduate Assistantship	*2015-2020*
Public Choice Society Graduate Student Fellowship ($700 - Covid-19 cancellation)	*2020*
European Economic Job Market Travel Grant ($200)	
Princeton University & New York University Abu Dhabi, WESSI Workshop ($2,000)	*2019*
SEA Graduate Session Nomination (Funded, incl. Membership)	
Texas A&M University, 3 Minute Thesis Competition, Finalist	
Texas A&M Student Research Week, 2nd Prize for Graduate Oral Presentation ($150)	
Bush Foundation, Summertime for Advancement in Research (Star) Award	*2018*
Texas A&M Student Research Week, 1st Prize for Graduate Oral Presentation ($300)	
Texas A&M SAAER, 1st Prize for Economics and 2nd Prize for Lightening Round ($1,650)	
London School of Economics, Erasmus Plus scholarship and Tuition Waiver,	*2015*
Texas A&M International University, 1st Prize LBV Conference	*2013*
Texas A&M University Kingsville, 2nd Prize Javelina Research Symposium	
Texas A&M Int. University, DAAD Promos and ISEP scholarship, Tuition Waiver	
University of Trier (Dr. Rieger), Financial Economics Experiment (€1,000)	
Bloomsburg University, PA, DAAD Promos Scholarship, Tuition Waiver	
Recommendation for scholarship at the "Studienstiftung des Deutschen Volkes"	*2010*
Aristotle University, Thessaloniki, Greece, Erasmus Scholarship	*2008*
National and Kapodistrian University, Athens, Greece, Language Scholarship	*2007*

SERVICE

Session Organizer and Chair, "Peers, Health, and Labor" SEA 2021; Session Chair, "Economics of Drugs", SEA 2020; Discussant at SEA 2019, WEAI 2019, SEA 2020, SEEDEC 2021, Ethics Reviewer for Experiments at Oxford University 2021-2022

PRESENTATIONS (INCL. INVITED, UPCOMING, AND COVID-19 CANCELLATIONS)

ESA Special Meeting, University of Chicago, UCEMA (JILAEE) *2022*

University of Chicago, John List Experimental Group, Online, Allied Social Science Association (ASSA), *2021*
Online Presentation (2), Royal Economic Society virtual conference, 8th IZA Workshop on
Environment, Health, and Labor markets, IZA Workshop: Economics of Time Use and Mental Health:
Stress, Sleep, Fatigue and Employment, Australian National University (ANU) seminar, Western
Economic Association International (WEAI) ESA sessions,North American Meeting, Economic Science
Association (ESA), Tucson, Arizona, Southern Economic Association (SEA), Houston, Texas,
Public Choice Society (PCS), Savannah, Georgia, European Association of Labour Economists (EALE)
conference, Padua, Italy, virtual presentation, Laboratory for Innovation Science at Harvard,
Online Seminar (2), Symposium on Experimental Economics in Developing Countries (SEEDEC),
Covid-19 Cancellations: Colloquium on Personnel Economics (COPE), Aarhus University, Denmark

Southern Economic Association (SEA), Georgia, Center for Experimental Economics, *2020*
University of Oxford, Online Presentation, Allied Social Science Association (ASSA), San Diego,
Virtual Society of Labor Economists (SOLE), Berlin, Germany, University of Alberta, Canada,
Seminar Presentation, Boston College, for Retirement Research, Online Presentation, CINCH
Duisburg-Essen University, Germany, Online Presentation (3),
Covid-19 Cancellations: Public Choice Society (PCS), Newport Beach, California; Royal Economic
Society (RES), Belfast, United Kingdom, Student Research Week, Texas A&M University, Colloquium
on Personnel Economics (COPE), University College London, Unintended Consequences of Regulation,
Cambridge University, United Kingdom; Spring Meeting of Young Economists 2020, Bologna, Italy

Allied Social Science Association (ASSA), Atlanta; Winter Institute (WESSI), New York *2019*
University, Florence; Southern Economic Association (SEA), Fort Lauderdale; Association for
Public Policy Analysis and Management (APPAM), Denver; Western Economic Association
International (WEAI), San Francisco; McMaster University, Canada; Public, Labor, Industrial
Organization (PLIO) Conference, Texas A&M University; Applied Economics Research Symposium,
and Student Research Week, Texas A&M University, Organization (PLIO) Conference, Texas A&M
University; Applied Economics Research Symposium, and Student Research Week,
Texas A&M University and Student Research Week, Texas A&M University

Allied Social Science Association (ASSA), Philadelphia; Advances with Field Experiments (AFE), *2018*
Boston; Southern Economic Association (SEA) Washington DC; Economic Science Association (ESA),
World Meeting, Berlin, Germany; Economic Science Association (ESA), North-American Meeting,
Antigua, Guatemala; Graduate School of Economics (GSE) Summer Forum, Barcelona, Spain; University
of Melbourne, Australia; University of California, Irvine; TExAS Conference, San Antonio; Symposium
for Agricultural and Applied Economics Research, and Student Research Week, Texas A&M University

Symposium for Agricultural and Applied Economics Research, Texas A&M University Economic *2017*
Science Association (ESA), North-American Meeting, Richmond, Virginia

Workshop on Self-Control, Self-Regulation and Education, Aarhus University, Denmark *2013*

PARTICIPATION AT CONFERENCES, WORKSHOP, SEMINARS AND SUMMER SCHOOLS

ASSA 2022 Virtual Annual Meeting *2022*

Behavioral Game Theory Workshop from University of East Anglia, NeuroPsychoEconomics Conference *2021*
Choice-Process Data Workshop, Post conference, ESA, Tucson

Virtual Market Design Seminar, Virtual Advances with Field Experiments, Essen Health Economics *2020*
Seminar Series, HEaLth and Pandemics Econ Working Group, MiddExLab Virtual Seminar

International Week in Vechta, University of Vechta, Germany; Experimetrics Workshop by Peter *2018*
Moffat and Open Source for Open Science Workshop, Texas A&M University

Experimental Economics Workshop IFREE, Chapman University, California; Technion Summer School *2017*
on Decisions, Laws, and the Big Data Revolution, Haifa, Israel

Workshop on Sickness Absenteeism and Presenteeism at the IAAEU, Trier, Germany; eFrame Summer *2013*
School of Trier and Pisa, University of Trier

TEACHING EXPERIENCE

Texas A&M University, Econometrics (3), Experimental Economics (2), Economic Development, Teaching Assistant (TA), Tutoring Lab, CIRTL Teaching Certificate	*2015-2020*
Texas A&M University, Empirical Development Economics, Program Evaluation (2), Guest Lecturer	*2017-2019*
Texas A&M University, Games and Economic Behavior, Instructor	*2017*
University of Vechta, International Human Service Management, Instructor	*2015*
University of Vechta, Game Theory, Behavioral Health Economics, Guest Lecturer	*2018-2021*
University of Trier, Business I, Business II, Human Resource Management, TA	*2010-2014*

REFEREE ACTIVITIES

Health Economics, Economic Inquiry, Journal of Economic Behavior and Organization, Review of Economic Design, Journal of Behavioral Public Administration, China Economic Review, Health Economics, Review, 2018 Spring Meeting of Young Economists, Spain (7 papers), 2021 Spring Meeting of Young Economists, Italy (4 papers), 2022 Spring Meeting of Young Economists, France (5 papers)

PROFESSIONAL MEMBERSHIPS

Royal Economic Society (RES), American Economic Association (AEA), Economic Science Association (ESA), Southern Economic Association (SEA),Western Economic Association International (WEAI), Society for Experimental Finance (SEF), Association for Public Policy Analysis and Management (APPAM), European Society for Population Economics (ESPE)

LANGUAGE AND TECHNICAL SKILLS

English (Fluent), German (Native), Spanish (Intermediate), and Greek (Elementary)
Stata, R, Python, Matlab/GNU Octave, SPSS, ArcGIS, PHP, Latex, Markdown, Gimp, Photoshop, Github, AWS

REFERENCES

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Vaccines at Work

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

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Abstract

Influenza vaccination could be a cost-effective approach for reducing costs in terms of both human lives and productivity, but low take-up rates and vaccination unintentionally causing moral hazard may decrease its benefits. We ran a natural field experiment in cooperation with a bank in Ecuador where we experimentally manipulated incentives to participate in a health intervention, allowing us to determine the personal consequences of being randomly encouraged to get vaccinated using administrative firm data. In the first stage, we found strong evidence that opportunity costs and peers matter to increase vaccination demand. In the second stage, contrary to the company's expectation, vaccination did not reduce sickness absence during the flu season. Getting vaccinated was ineffective, with no measurable health externalities from co-worker vaccination. We ruled out meaningful individual health effects in relation to several thresholds of expected vaccine effectiveness. Using a dataset of administrative records on medical diagnoses and employee surveys, we found evidence consistent with vaccination causing moral hazard, which could decrease the effectiveness of vaccination.

JEL Classification: D90, I12, J01, N36

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

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1. Introduction

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

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

For this research, we exploited the background of a company's vaccination campaign to present the first comprehensive study of both the determinants and consequences of flu vaccination. In cooperation with a bank in Ecuador that provides annual vaccination campaigns to improve its employees' health, we implemented a natural field experiment by randomizing incentives to get a flu shot.[2] Our design introduced three modifications to the bank's 2017 vaccination campaign to

[1] Public health institutions recommend that every individual over six months of age should be vaccinated against the flu. However, flu vaccination rates in European countries range from 2% to 70% (Mereckiene, 2015), and only 38.5% of adults 18 and older were immunized in the United States during the 2017–2018 flu season (Srivastav et al., 2018).
[2] We follow the definition of a natural field experiment by studying behavior in an environment where subjects make their decisions naturally without knowing that they are participants in an experiment (Harrison & List, 2004).

create exogenous variation in the vaccination take-up rate. First, we introduced income-dependent subsidies and selected an income threshold at which the price of the vaccine for employees would change. Second, due to capacity constraints, it was necessary to assign employees to be vaccinated either during the workweek or on a Saturday. By randomizing the assignment of employees for vaccination, we could manipulate the opportunity costs of vaccination. Employees would incur additional transportation costs and would need to arrange their weekend schedules to attend a vaccination appointment on a Saturday, while assigning employees to a time during the workweek minimized their opportunity costs because the bank allowed them to take time off their duties to get vaccinated at the firm's location. Third, we varied the content of the invitation emails to appeal to altruistic or selfish motives.

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

Our design overcomes several challenges that arise when studying the causal effects of vaccination on health-related outcomes. The first challenge is to identify the causal effect of getting vaccinated. While the medical literature documents modest positive health effects of flu vaccination, many of the existing studies could be affected by selection and other biases (Demicheli et al., 2014; Demicheli et al., 2018; Jefferson et al., 2010; Osterholm et al., 2012; Østerhus, 2015). For instance, researchers describe the problem of a "healthy vaccine recipient effect" that could bias observational studies. If healthier individuals are more likely to get vaccinated, this positive selection bias could lead to an overestimation of the health effects of vaccination. Nevertheless, observational studies without randomization of vaccination are often preferred because of ethical concerns regarding randomized controlled trials (RCTs) with placebos in the context of health (Baxter et al., 2010; Sanson-Fisher et al., 2007). For the same reason, RCTs are often conducted using other types of vaccines instead of clean placebos to provide potential health benefits for experimental participants in the control group (Loeb et al., 2010). We present a methodological alternative that addresses ethical concerns by using the exogenous variation in

vaccination rates generated through the manipulation of incentives to take part in the campaign. To study the impact of vaccination on health-related outcomes, we thus employ a random encouragement design (Bjorvatn et al., 2015; List et al., 2017). This is an innovative approach in the context of preventive medical technologies that circumvents the ethical dilemma of withholding a potentially effective medical treatment while allowing for gathering causal evidence.

The second challenge we address is capturing the total effect of vaccination. Public health institutions and companies are interested in the total effect of health interventions, encompassing both medical and behavioral responses. However, medical research on vaccines generally focuses solely on the medical effects and does not consider changes in behavior that may affect health. In RCTs, participants know that they are in an experiment, but they do not know whether they have received a specific type of vaccine or not. This eliminates the possibility of identifying changes in behavior when comparing experimental conditions. In contrast, our random encouragement design introduces no uncertainty in treatment, thus capturing both the behavioral and medical effects of getting vaccinated. This allows us to explore whether vaccination induces individuals to adopt riskier behaviors.

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

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

Next, we exploit the exogenous variation created by randomly assigning employees to get vaccinated during the workweek to study the consequences of vaccination. First, we study the effect of peer vaccination on individual take-up. For this purpose, we make use of exogenous variation in the proportion of peers who get vaccinated. In our setup, co-workers who work together directly every day define the relevant social groups. Given randomization at the employee level, by chance, some units have more employees assigned to the workweek than other units; hence, they are more encouraged to get the vaccine than those in other units. According to our findings, when the proportion of peers getting vaccinated increases by ten percentage points, take-up rates increase by 6.2 percentage points. More in-depth analysis of the mechanisms behind these peer effects suggests that peers getting vaccinated does not change an individual's information about or beliefs toward vaccination; rather, it seems that employees may get vaccinated to follow behaviors that they deem socially acceptable.

Second, we investigate an aspect of the consequences of vaccine take-up that is highly relevant for policymakers and firms that run vaccination campaigns: whether flu vaccination is effective in improving working adults' health, thereby potentially reducing sickness-related absence. If flu vaccination decreases flu cases, we could expect that offering employees the opportunity to get vaccinated during the workweek should reduce the number of flu cases and, accordingly, absence from work. However, our estimates show no evidence that exogenously triggered vaccination decreased sickness in general or sickness-related absence. Furthermore, the data from the medical records indicate that the probability of contracting the flu did not change as a result of participation in the vaccination campaign. The confidence intervals rule out effects that correspond to meaningful thresholds of an effective vaccine based on CDC figures.

There are several potential explanations for the lack of evidence for health improvements due to vaccination. It could be that positive health spillovers from vaccinated to unvaccinated individuals explain the underestimation of health benefits. However, our results show that peer vaccination does not affect the probability of being diagnosed with an illness or taking a sick day, which is consistent with unit vaccination rates being below herd-immunity levels. It is also possible that the flu vaccine administered was medically ineffective, which our design cannot rule out. Finally, independent of the vaccine's medical effectiveness, employees may have adopted riskier and thus health-threatening behavior, which could have mitigated the immunity benefit of the vaccine.

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

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

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

As the final test of moral hazard, we investigate whether individuals report abandoning practices that are believed to be effective in improving health, regardless of their actual medical effectiveness. We find that assigning employees for vaccination during the workweek decreased the frequency of individuals reporting to engage in practices that are culturally believed to help prevent the flu and other respiratory diseases. Moreover, these effects are driven by individuals who believe the vaccine is beneficial for flu prevention, which supports the idea of vaccinated individuals feeling protected and engaging in riskier practices.

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

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

and changing beliefs are the mechanisms through which non-hierarchical peers affect the adoption of medical technologies.

Our study contributes to this literature in several ways. First, we employ a unique setup that allows for variation in different types of cost. We manipulate the opportunity costs of vaccination directly by varying the day of vaccination, which affects the next best alternative for different participants. Thus, we directly test the effect of changing these costs, which can be different from the effects of compensating through reward incentives.[4] We find that reducing opportunity costs has a substantial effect on vaccination of working-age adults who are not constrained by income and who live in locations where access to vaccines is not an issue, as is the case in most major cities in both developing and developed countries. The estimates are of similar magnitude to those of previous studies focusing only on vulnerable populations, which implies that opportunity costs are an important factor for any population. In contrast, a small change in the vaccine's price did not increase the take-up rate, suggesting that financial incentives must be substantial in order to be effective. Additionally, in line with the findings of previous studies, information nudges proved ineffective (Bronchetti et al., 2015; Godinho et al., 2016). Second, we study how the adoption of medical technologies can be affected by a peer group that has received little attention in this research area so far: co-workers.[5] Most working adults share at least half of their waking time with their co-workers. Unlike friends and other non-hierarchical peers, employees cannot choose the individuals they are going to spend time with during work hours after they are hired. We document that this peer group can have a significant positive influence on the adoption of preventive medical technologies such as vaccines, which is inconsistent with the theoretical concept of free-riding in this context. Our evidence suggests that the primary mechanism for peer effects in vaccine take-up is the employee conforming to the norms of their workgroup, which indicates how policymakers could influence the adoption of preventive medical technologies in working adults.

Furthermore, our study contributes to the literature on the consequences of medical technologies (Alam & Wolff, 2016; Bütikofer & Skira, 2018; Duflo et al., 2019) as well as the literature of on-site health interventions (Belot et al., 2016; Just & Price, 2013; List & Samek, 2015) and the broader literature on public health interventions (Bütikofer & Salvanes, 2020; Cawley, 2010). Our

[4] Starting with the seminal work of Kahneman and Tversky (1979), many behavioral economics studies find that losses are treated differently than gains.

[5] In contrast, there is a large body of research regarding the influence of co-workers on the productivity of their peers in the workplace (Bandiera et al. 2010; Herbst and Mas, 2014; Mas and Moretti, 2009).

findings on the health effects of vaccine take-up add to an ongoing discussion that predominantly takes place in the medical literature, with a few recent exceptions in the economics literature (Ager et al., 2017; Carpenter & Lawler, 2019; Lawler, 2017). With regard to flu vaccines, Ward (2014) finds that flu vaccination increases sickness absences in years when the flu vaccine matches poorly with the prevalent flu viruses, while it has no effect in years when the vaccine matches well. The difference between these two results, which would control for moral hazard, points to the medical benefits of the vaccine. In another vaccination study, Anderson et al. (2020) find no effect of vaccination on hospitalization and mortality among an elderly population using a regression discontinuity design. Very few medical studies to date have considered the possibility of medical technologies unintentionally causing moral hazard (Prasad & Jena, 2014; Richens et al., 2000), while the few existing papers in economics exploring moral hazard in the context of medical interventions present mixed results (Doleac & Mukjerjee, 2018; Klick & Stratmann, 2007; Margolis et al., 2014; Moghtaderi & Dor, 2016).[6]

The present study contributes to this literature in three ways. First, we employ a novel design for public health and medical interventions, allowing us to circumvent measurement and ethical problems. We hope to encourage other researchers to use the same methodology to obtain causal estimates in the field of health. Second, with our evidence on the impact of flu vaccination on sickness-related absence, we contribute to the research on the determinants of this workplace outcome (Bütikofer & Skira, 2018; Pichler 2015; Ziebarth & Karlsson, 2010). Third, based on experimental variation, we provide behavioral evidence that getting vaccinated induces individuals to feel protected and thus to forgo preventive practices such as visiting their doctor when they experience symptoms of an illness. This result is consistent with the hypothesis of preventive medical technologies causing moral hazard and with the theoretical model of Talamàs et al. (2020). Finally, by showing that preventive medical technologies can unintentionally cause moral hazard, we offer an explanation as to why health interventions may not always be as successful as expected in improving health outcomes. This finding implies that firms and policymakers should consider moral hazard when promoting the adoption of preventive medical technologies.

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

2. Experimental Design

We conducted the field experiment in cooperation with a bank in Ecuador. The selected bank focuses on consumer credit and is one of the largest credit card issuers in the country. Its headquarters are in Quito (the capital of Ecuador), and it has six branches across the country with over 1,300 employees in total, distributed across 31 divisions with 142 working 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.[7] In 2017, the bank decided to extend its annual campaign to all its employees, and it allowed us to experimentally modify the campaign to investigate the effects of vaccination and how to increase take-up rates. We implemented three interventions: we changed the vaccine's price for some employees using income-dependent subsidies, we randomized assignments for on-site vaccinations across weekdays, and we implemented information nudges by varying the content of the emails used to invite employees to get vaccinated.

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

To examine the effects of opportunity costs and information, we randomly assigned all employees into one of four groups.[8] First, employees assigned to the control group (*Control*) were

[7] 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%.

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

invited to get vaccinated during the workweek (Wednesday, Thursday, or Friday) and were allowed to take time off their duties to get vaccinated. The specific day was selected at random for each employee.

The first treatment increased the opportunity costs of vaccination by assigning employees to get vaccinated on a *Saturday*. The bank's employees usually do not work during the weekend, so individuals in this group would incur extra transportation costs and have to arrange their schedules to travel to the bank and get vaccinated.[9] Otherwise, this group received the same information as the Control group (see Figure A2). This treatment was only applied in Quito because 82% of the bank's employees work in Quito; all the other branches are substantially smaller, and their employees could all get vaccinated in a single day, which was not possible in the capital.[10]

We also implemented two information nudges. We kept the additional messages as unobtrusive as possible to prevent confounding the effect of information with salience or other behavioral factors. The first nudge highlighted the social benefits of flu immunization (*Altruistic*). In addition to the information provided to the Control group, the email included the following wording: "Getting vaccinated also protects people around you, including those who are more vulnerable to serious flu illness, like infants, young children, the elderly and people with serious health conditions that cannot get vaccinated" (see Figure A3). The second nudge highlighted the individual benefits of flu immunization (*Selfish*). In addition to the information provided to the Control group, the email included the following wording: "Vaccination can significantly reduce your risk of getting sick, according to both health officials from the World Health Organization and numerous scientific studies" (see Figure A4). Employees in these two treatments were assigned to get vaccinated during the workweek, while the specific day was selected randomly.

Our intervention targeted the Ecuadorian flu season, which usually covers the period from November to the end of February (Ropero, 2011). The bank ran a pre-intervention survey from October 25 to October 29, 2017. The human resources (HR) department sent the intervention emails on November 1, 2017, using its official email account. The employees were not aware that this study was taking place. For them, the campaign was just a regular activity organized by the HR department. Employees are used to receiving emails from HR and, according to the HR

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

[10] 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.

manager, they typically read these emails carefully. A reminder was sent out using the same email account a week later. The vaccination campaign ran from November 8 to November 11, 2017, at locations within the bank's offices in each branch. The bank hired an external medical team to supply and inject the vaccines. Finally, the bank conducted a post-intervention survey during March and April 2018.[11]

3. Data

This section describes the data used in our analyses for assessing how monetary and non-monetary determinants can affect take-up rates and the effects of flu vaccination. First, we were granted access to the firm's administrative records about its employees, which include information on gender, age, education level, children, tenure, and income as well as medical diagnoses and sick days. The records also provide information about the employee's job and their work unit, i.e., their position within the bank's organizational structure. Work units have been predetermined by the company, which were established more than two decades ago. Second, we collected vaccination take-up data from the bank's campaign records. Third, we gathered data from the pre- and post-intervention surveys. These surveys asked employees about their previous illnesses and general health, knowledge and beliefs about vaccination and the flu vaccine, habits related to health, relationships with co-workers, opinions about the campaign, motivation, organizational attachment and work satisfaction, and risk and time preferences.

--- Table 1 about here ---

Table 1 presents the mean characteristics of the bank's employees (Column 1). On average, the employees earn a total monthly income of $1,760. As a reference, in 2017, the national average total monthly income in Ecuador was $479, which implies that the bank's employees are in the three highest deciles of Ecuadorian income distribution (ENEMDU, 2017). The average length of employment with the bank is more than seven years, and the average age of the employees is around 36 years. The company employs roughly the same numbers of men and women, and more

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

than 90% of its employees have at least some college education, which is close to education levels in developed countries. 50 percent of the employees have children. The average distance that employees live away from work is 7.58 km, and a work unit consists of approximately 29 employees. Almost 50 percent of the employees completed the pre-intervention survey, representing a high completion rate compared to previous surveys from HR. The completion rate decreased to 36% for the post-intervention survey.

The administrative data include medical diagnoses and sick days as two measures of health. These measures come from two sources: on-site doctors, and medical certificates from external doctors (72 different physicians in total). It is important to note that Ecuadorian law establishes that employees must present a medical certificate to receive a sick day.[12] Consequently, the on-site doctors report every patient's visit to HR, including the diagnosis (the type of disease), whether they granted sick days or not, and the number of sick days granted. Furthermore, by law, if an employee takes time off work to go to an outside doctor, they have to present a medical certificate to HR that indicates the diagnosis and the number of sick days granted (if any). Hence, in addition to sick days, we can also observe instances of employees being diagnosed with an illness but having no sick days granted for cases where a doctor did not consider their condition severe enough. Thus, sick days are a measure of more severe illness. Between January and early November 2017 (before the intervention), two out of three employees had some kind of illness, and 37% had at least one sick day (see Table 1).

The doctors diagnose their patients using a combination of a physical examination, blood tests, and culture tests. The specific procedures undertaken are recorded in individual medical records, to which we do not have access. Diagnoses that name the patient's illness as "flu" provide us with the narrowest definition of flu-related sickness. If flu cases present with complications, the data report the complication as the diagnosis and thus does not mention the flu explicitly. To address this issue, we implement an extended definition of flu-related sickness, which includes diagnoses that could likely indicate complications caused by flu, according to a third-party physician. We focus on this measure in our empirical analysis and check the robustness of the results by employing the narrowest definition and an even broader definition, provided by the same third-party physician. Any other respiratory disease not classified by this doctor as flu is by definition

[12] 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.

listed as a non-flu respiratory disease. A second physician has verified these measures to ensure confidence regarding the distinction between flu-related and non-flu-related health problems.

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

4. Analysis of Vaccination Take-Up

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

The last row in Table 1 presents the flu immunization take-up rates for the different treatments during the campaign. The *Control* group shows a take-up rate of 22%, the *Altruistic* treatment shows a take-up rate of 17%, and the *Selfish* treatment shows a take-up rate of 19%. A comparison across the three groups thus suggests that the information treatments were not sufficient to increase take-up. In contrast, being assigned to get vaccinated during the workweek increases take-up by 14 percentage points in contrast to the *Saturday* treatment (112%).[14] We extend the analysis of these effects in the next section.

[13] Figure A9 shows no visible discontinuity across the threshold. Regression discontinuity estimates also indicate no significant change in take-up at the cutoff, which is robust to different bandwidths (see Table A1).

[14] In the post-intervention survey, 59 employees reported that they got vaccinated outside the campaign. Vaccination outside the campaign is not significantly different according to treatment status. No employees who stated they got vaccinated externally received a vaccination during the bank's campaign. For one individual who stated that they did not get vaccinated, our records indicated that they did get vaccinated. Meanwhile, 18 individuals stated they got vaccinated during the campaign but did not according to the records. If we exclude those 19 individuals as misremembering whether they were vaccinated, the estimates do not change. We also note that, between November 2017 and February 2018, 20 treated employees left their jobs at the bank. Attrition is not affected by treatment assignment.

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

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

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

where $Takeup_{ic}$ is an indicator of getting vaccinated. We include Quito fixed effects γ_c to account for differences in implementation of the vaccination day assignment across branches (as discussed in Section 2). $Saturday_{ic}$, $Altruism_{ic}$, and $Selfish_{ic}$ are dummy variables that indicate treatment assignment. Thus, we estimate the effect of the different treatments relative to those individuals in the *Control* group who were assigned to vaccination on a day during the workweek and did not receive any information nudge.

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

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

---- Table 2 about here ---

[15] The post-intervention survey asked whether the employees recalled the altruistic and selfish information statements. Appendix Table A2 shows that neither employees assigned to the *Altruistic* treatment nor those assigned to the *Selfish* treatment remembered their respective statements better than the *Control* group. Information spillovers could be another issue here, but this is unlikely given that our design involved providing information directly to the treated individuals via email.

Columns 2–4 of Table 2 show the robustness of the results to the inclusion of controls, to the use of a restricted sample, and to controlling for non-compliance. Specifically, Column 2 shows that controlling for vaccine price, income, tenure, division in the company, gender, age, and education level does not affect the estimates. Column 3 addresses the fact that only employees who work in the bank's headquarters in Quito were assigned to be vaccinated on a *Saturday*.[16] In this subsample, assigning employees to *Saturday* decreased take-up by almost nine percentage points (51% of the control group take-up), significant at the 1% level. This result is slightly larger than the main result, but we cannot reject that they are statistically the same. Both information treatments display small, negative, and statistically insignificant effects. Column 4 shows the effect of controlling for non-compliance.[17] In this subsample, assigning employees to *Saturday* decreased take-up by 6.7 percentage points, significant at the 5% level. We cannot reject that this estimate is statistically the same as the baseline result. The estimates of the effects of the information treatments are practically the same as the main estimates.

Lastly, in Column 5, we check whether assignment to different days in the week affected take-up differentially. We use the fact that vaccination days were randomly assigned, and we regress our indicator of vaccination take-up on dummies for each assigned day (*Wednesday*, *Thursday*, *Friday,* and *Saturday*) using Quito's subsample.[18] These estimates show that the take-up rates for *Thursday* and *Friday* are not statistically different from that for *Wednesday*, while the effect of *Saturday* is substantially larger in magnitude and very close to the baseline estimate in Column 1.[19] These results do not support time-inconsistent preferences that would induce procrastination as the mechanism behind the *Saturday* effect, and they are consistent with increasing opportunity costs.[20]

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

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

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

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

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

4.2 Further Evidence on Opportunity Costs

We analyze heterogeneous treatment effects across different subgroups of our study population, which may yield further evidence that opportunity costs are driving the difference in take-up rates between employees assigned to be vaccinated on a day during the workweek and those for *Saturday*.[21] We focus on differences across gender, distance to work, and employees with and without children.[22] Figure 1 shows that assignment to *Saturday* has more substantial effects for men than for women, although the difference is not statistically significant.

--- Figure 1 about here ---

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

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

Finally, to further disentangle transaction costs from opportunity costs, we run two regressions, including interaction terms between *Saturday* and distance to the bank and between *Saturday* and vaccination price groups, respectively (Appendix Table A4). The results show that an additional mile in travel distance (i.e., transaction cost) does not have a differential effect on the likelihood

[21] We find that the information treatments have no differential effect across subgroups. These estimates are small and statistically insignificant. See Appendix Table A3.
[22] Distance to work was calculated based on employees' home addresses using a geo-location service.

of getting vaccinated on a weekday compared to *Saturday*. Additionally, there are no statistically significant differences between the high-price, workweek vaccination group and the low-price, *Saturday* vaccination group, which is inconsistent with the idea that heterogeneous transaction costs are driving the main results in Table 2.

In conclusion, several pieces of evidence suggest that the difference in take-up rates between employees assigned to the a day during the workweek and to *Saturday* corresponds with a change in the opportunity costs of vaccination. Since most bank activities occur during the workweek, one could argue that the *Saturday* treatment may have increased the opportunity costs to get vaccinated to a particularly large extent, even though the magnitude of the effect is similar to that in other settings (Banerjee et al., 2010; Bronchetti et al., 2015).[23] In the rest of our analyses, we use only this variation in take-up resulting from lower opportunity costs as an instrument.

4.3 Peer Effects on Vaccination Take-Up

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

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

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

We model the effect of the proportion of peers excluding individual i in 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 $Prop.Takeup_{-i,jc}$ is the proportion of peers in unit j who get vaccinated and $\bar{X}_{-i,jc}$ are the average observable characteristics of peers j. For each individual, $Prop.Takeup_{-i,jc}$ corresponds to the proportion of peers assigned to get vaccinated before or on the same day as i. Manski (1993) shows that, if we estimate equation (2) by ordinary least squares (OLS), then self-selection, common environmental factors, and reflection will confound the true peer effects β_1 and β_3. However, in our design, employees are randomly assigned to vaccinate on a day during the workweek independently of their unit. This creates a form of exogenous variation that affects the proportion of peers who get vaccinated independently of employee *i*'s decision to get vaccinated because, by chance, some units have more employees assigned to the workweek than other units. We can average equation (2) across unit j, 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 ($Prop.Workweek_{-i,jc}$). For each individual, $Prop.Workweek_{-i,jc}$ corresponds to the proportion of peers assigned to the same weekday or a weekday preceding that to which i was assigned. By taking the timeline of events into account and defining this variable as a cumulative proportion of cases separately for each individual, we avoid considering future vaccinations of co-workers when analyzing peer effects. Random assignment of both individuals and peers within work units implies that $Prop.Workweek_{-i,jc}$ is uncorrelated with both $\bar{X}_{-i,jc}$ and $\bar{u}_{i,jc}$. Hence, the reduced-form equation is as follows:

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

In our design, the exclusion restriction holds because the proportion of peers who got vaccinated 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 variables (IV) estimate of the effect of the proportion of vaccinated peers on an individual employee's take-up. The error term in equation (4) includes both the individual error from equation (2) and the average error from equation (3), so we cluster the standard errors at the unit level.

--- Table 3 about here ---

Panel A in Table 3 presents the main results. The first-stage estimate in Column 1 indicates that a ten-percentage-point increase in the proportion of peers assigned to the workweek increased the proportion of peers getting vaccinated by 3.1 percentage points. The effective F-statistic of Montiel Olea and Pflueger (2013) is 22.87; therefore, we can reject the null of weak instruments for a threshold of 20%, which suggests that the instrument is relevant. The estimates in Columns 2–4 show that peer vaccination has a positive effect on individual take-up and that not accounting for endogeneity biases the effect upward. The IV estimate in Column 4 indicates that a ten-percentage-point increase in the proportion of peers getting vaccinated increases take-up by 6.2 percentage points. The results are robust to controlling for the total number of employees in the unit and for the mean age and gender of the peers (Appendix Table A5).[24]

Two potential mechanisms behind the positive peer effect on individual take-up are peers changing personal beliefs about vaccination and individuals following behavior that they deem socially acceptable. To disentangle these potential channels, we first explore whether peers might be changing personal beliefs. To measure this, we use the responses to the post-intervention survey questions on beliefs and knowledge of flu vaccines and interactions with co-workers related to vaccination. Even though answering the post-intervention survey is not correlated with treatment

[24] Mechanically, smaller units may have larger proportions. We also control for the proportion of peers in the unit who hold some level of managerial position. The point estimates are not affected by including this control variable. Including a unit-size interaction in our main model reveals that the effects are driven by units with small unit sizes. We note that the employee sheets used to assign vaccinations contained employees listed in a randomized manner; therefore, we are not able to investigate order effects within each day.

assignment (Table 1), the first stage loses precision due to the smaller sample size in the survey. We focus on reduced-form analyses to prevent issues with finite sample bias in the IV estimate. Table A6 shows the results on a set of 12 outcomes, indicating that there is no significant effect on any of the questions. Moreover, the point estimates are small compared to the baselines, which suggests that peer behavior neither affected beliefs nor supplied new information about the vaccine.

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

--- Table 4 about here ---

First, we consider whether managers influence an individual's vaccination decision. If individuals react to their managers' behavior, this could be in response to an order and not because of the unit's norms. Columns 1 and 2 in Table 4 show that individuals do not follow the behavior of peers in managerial positions; rather, they follow the behavior of peers not in those positions, i.e., those who constitute the majority of the working unit,[25] with a ten-percentage-point increase in the proportion of vaccinated peers not in managerial positions increasing take-up by 1.7 percentage points. Second, Columns 3 and 4 in Table 4 show that individuals do not react to the

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

behavior of peers of similar age (within a three-year bandwidth), but that they do react to peers of different ages from themselves, with a ten-percentage-point increase in the proportion of peers of different ages getting vaccinated increasing take-up by 5.5 percentage points. Given the large age dispersion within the units (Appendix Figure A10), it is unlikely that age groups can create feelings of belonging. Hence, these results are in line with the idea that individuals follow the behavior of the working unit. Third, we test whether employees follow behavior they deem to be socially acceptable based on their gender. Column 5 shows that a ten-percentage-point increase in the proportion of peers of the same gender who get vaccinated results in a 5.7-percentage-point increase in take-up. This effect is almost identical to the main estimate and is driven by men. Column 6 shows that the effect of peers of a different gender is 54% smaller and is not significant. These results indicate that the behaviors of their gender groups influence individuals' actions, which is consistent with previous research.

Together, these findings suggest that the estimated peer effects are a consequence of individuals following behavior that they deem socially acceptable.

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

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

5.1 Effects of Flu Vaccination on Health and Absence

Flu vaccines may affect health through multiple avenues, both direct and indirect. First and foremost, getting vaccinated could have a direct effect on health by increasing immunity against four strands of the flu virus. Furthermore, as indicated by the results in the previous section, if a person gets vaccinated, the likelihood that their peers will also get vaccinated increases. This effect would imply that an employee's peers are more protected against the flu, which may decrease the transmission rate of the disease. Thus, positive peer effects on vaccination take-up could create an indirect channel through which getting vaccinated might have a positive effect on health. The proportion of vaccinated peers within the 142 units in the firm varies substantially (between 0%

and 67%), which could play an indirect role in health outcomes.[26] Ideally, we could estimate the effect of flu immunization on health-related outcomes (Y_{ijc}) such as medical diagnoses and sick days through these two channels as follows:

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

However, vaccination take-up rates and the proportion of peers who get vaccinated are potentially endogenous. For example, individuals with healthier lifestyles could be more likely to vaccinate and less likely to need a sick day, so the estimates of equation (5) by OLS would be biased downward. This speaks for instrumenting the following variables: i) take-up with an indicator of assignment to vaccination during the workweek, and ii) the proportion of vaccinated peers in the unit with the proportion of peers assigned to the workweek. The unadjusted first-stage equations have F-statistics of 6.6 and 8.9, respectively, implying that the IV estimates of equation (5) may have a problem of finite sample bias.[27] Thus, we focus on the valid reduced-form estimates of regressing the health outcomes on the instruments.

--- Table 5 about here ---

Table 5 presents the effects of getting the flu vaccination on the probability of being diagnosed sick for any reason between November 2017 and February 2018. The OLS estimate in Column 1 suggests that getting vaccinated decreases the probability of being diagnosed with an illness by 0.7 percentage points (1.4% of the baseline); however, the effect is insignificant. The reduced-form estimates in Column 2 imply that getting vaccinated does not affect the probability of being diagnosed as sick. Being randomly assigned to a day in the workweek (which increases vaccination take-up) decreases the probability of sickness by 1.5 percentage points (3.4% of the baseline), which is insignificant at conventional levels. Additionally, the results in Columns 1 and 2 indicate that the proportion of vaccinated peers does not affect the probability of being diagnosed with an illness.

[26] Figure A11 displays the number of employees by unit. The CDC and WHO indicate that vaccination rates over 75% grant herd immunity.
[27] The results of Montiel Olea and Pflueger (2013) only apply in cases with one endogenous variable.

--- Table 6 about here ---

Table 6 shows the effects of flu vaccination on the probability of having a sick day. The OLS correlation suggests that vaccination decreases the probability of having a sick day by 4.1 percentage points, but this effect is not significant. Conversely, the reduced-form estimates in Column 2 imply that getting vaccinated does not affect the probability of having a sick day. Being randomly assigned to a day in the workweek (which increases vaccination take-up) increases the probability of having a sick day by 1.3 percentage points (5% of the baseline), which is insignificant at conventional levels. From the firm's overall perspective, these results suggest that the investment in the health campaign was not worthwhile.[28]

--- Table 7 about here ---

There are many diseases over which the flu vaccine has no immunity benefit. Hence, we exploit the data on medical diagnoses and estimate the effect of vaccination on the probability of being diagnosed with the flu (Table 7). The OLS estimates in Column 1 suggest that getting vaccinated decreases the probability of being diagnosed with the flu. However, the reduced-form estimate in Column 2 indicates that being assigned to the workweek increased the probability of being diagnosed with the flu by 0.4 percentage points (9% of the baseline), which is not significant at conventional levels. This result further suggests that getting vaccinated was ineffective in decreasing the probability of having the flu. Furthermore, the estimates in Columns 1 and 2 show that the proportion of vaccinated peers does not affect the probability of being diagnosed with the flu, which suggests that vaccination rates are too low to provide herd immunity. Thus, we disregard the proportion of vaccinated peers in the following analyses.[29]

[28] We reach the same conclusion based on findings for the number of sick days. Note that the diagnoses include severe illnesses such as cancer, meaning that a large number of the recorded sick days are not related to the flu. If we exclude outliers with more than 100 sick days, the coefficient of the reduced form is insignificantly positive, in line with our finding in Table 6 on the probability of having a sick day. It is also noted that our results for sickness and sick days do not change if we take out the proportion of peers and estimate only the individual effect of vaccination.

[29] As can be seen in Appendix Table A7, the main result is robust to the inclusion of controls (gender, age, tenure, and income) and to using a broader and narrower definition of flu-related illness. We check the main result by performing a bounding exercise (Appendix Table A8), in which we consider the possible role of individuals being vaccinated externally to the firm in our results (see Section 5.3 for details).

To evaluate whether the estimates rule out meaningful effects of vaccination, we implement an equivalence test based on two one-sided hypothesis tests (Hartman & Hidalgo, 2018; King et al., 2000; Lakens, 2017; Rainey, 2014). The equivalence test has two parts. First, we must define what constitutes a meaningful effect of vaccination. This value comprises two thresholds to evaluate whether the estimates rule out meaningful effects. To define this value, we use flu vaccine effectiveness estimates from CDC data. While these estimates come from observational studies on flu hospitalizations and might be biased, they constitute the criteria that policymakers use to evaluate the vaccine's effectiveness. Since our experimental design guarantees that getting vaccinated is the only channel through which assignment to the workweek in the campaign affects health outcomes, we can use reduced-form estimates to evaluate whether vaccination has a meaningful effect on the probability of being granted a sick day due to having the flu.[30] According to the CDC, for working adults, the 2013–2014 vaccine had the highest effectiveness (reducing hospitalizations by 16 percentage points), the 2014–2015 vaccine had the lowest effectiveness (reducing hospitalizations by only 2.2 percentage points), and the 2017–2018 vaccine's effectiveness (the period of the campaign for the current research) fell in between those estimates (reducing hospitalizations by 8.4 percentage points). To compare these values with the reduced-form estimates, we multiply the CDC effectiveness estimates by the smallest effect of assignment to the workweek on take-up reported in Table 2, i.e., the most conservative estimate of the first stage (6.7 percentage points). This calculation yields reduced-form reference values of −1.1 percentage points, −0.1 percentage points, and −0.6 percentage points, respectively.

In a second step, we test whether the reduced-form effect is smaller than each reference value (−1.1, −0.1, −0.6) and higher than the absolute values of the reference values (1.1, 0.1, 0.6). This is equivalent to comparing both the reference values and their respective absolute values with the 90-percent confidence interval of the estimated effect (Lakens, 2017; Rainey, 2014). If the 90-percent confidence interval lies between the reference and its absolute value, then the estimated effects are consistent only with meaningless effects. If the confidence interval falls outside this

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

value range, we cannot rule out meaningful effects in the direction in which the confidence interval overlaps the boundary.

--- Figure 2 about here ---

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

5.2 Can Getting Vaccinated Cause Moral Hazard?

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

Vaccinated individuals could overestimate the protection of the vaccine and engage in riskier behaviors; for example, they may avoid going to the doctor, or may wait longer than unvaccinated individuals to do so, when they feel flu-like symptoms. In addition, vaccinated individuals may take fewer protective measures; for example, they may wash their hands less frequently. These changes in behavior would expose individuals more to strands of the flu virus that the vaccine does not cover as well as to other diseases that share transmission mechanisms with the flu.

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

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

--- Figure 3 about here ---

We estimate the reduced-form effects of vaccination by month during our investigation period. Figure 3 presents the effects of being assigned to a vaccination appointment during the workweek on flu and non-flu respiratory diagnoses. As with the cross-section estimates in Table 7, employees being assigned appointments during the workweek does not affect the probability of being diagnosed with the flu in any month. The estimates are smaller than 0.7 percentage points in magnitude and insignificant at conventional levels. These results further confirm that the vaccination campaign was ineffective. Regarding non-flu diagnoses, if vaccination did not induce individuals to feel more protected, we would expect to find no effect on the probability of being diagnosed with a non-flu respiratory disease. This is true in November, December, and February.

However, in January, when the government was specifically encouraging individuals to go to the doctor, being assigned a vaccination appointment during the workweek decreased the probability of being diagnosed with a non-flu respiratory disease by 7.2 percentage points.[31] This result is robust when controlling for individual fixed effects (Appendix Figure A12).

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

--- Figure 4 about here ---

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

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

--- Table 8 about here ---

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

Table 8 shows the effects of assigning employees to a vaccination appointment during the workweek on these outcomes. This factor does not affect how often employees wash their hands (1.2% of the baseline), which is not surprising since almost all employees reported that they wash their hands regularly. Assigning employees weekday appointments shows a negative but statistically insignificant effect on how often employees exercise (4.9% of the baseline) and how often they take nutritional supplements (19.5% of the baseline). The effect on how often employees carry an umbrella is statistically significant, decreasing the frequency of carrying an umbrella by 1.22 points (17.6% of the baseline) on a Likert scale where one means "never" and ten means "all the time."[33] We can also investigate heterogeneous effects across individuals' beliefs on the effectiveness of the vaccine using the pre-intervention survey. We find that the effect is driven by individuals who believe the vaccine is very effective in preventing the flu (Appendix Table A9). These findings thus suggest that vaccinated individuals feel protected and therefore neglect other measures that they may believe to be helpful for preventing respiratory illnesses.

5.3 Other Interpretations of the Results on Moral Hazard

In the previous section, we provided several pieces of evidence supporting the idea that flu vaccination causes a moral hazard issue. Here, we discuss whether other interpretations of these

[32] Also, since Quito is on the Equator Line, there are no marked seasons in the year. Temperatures can fluctuate between the upper forties (°F) and the lower eighties (°F) in one day. There are no accurate forecasts for rainfall.
[33] This effect is significant at the 5% level (p-value = 0.012) and robust to adjusting for multiple comparisons following Anderson (2008).

findings, not related to moral hazard, could explain the results. Misdiagnoses is one potential competing explanation. If doctors are not able to distinguish the flu from other non-flu respiratory diseases, then some of the diagnosed non-flu cases could have actually been flu cases. However, as our data include diagnoses from 72 different doctors from different health centers and hospitals, it is unlikely that there is a systematic issue of misdiagnosis. Additionally, the results are robust to using a broader definition of flu-related illness. Finally, misdiagnoses would not explain why vaccinated individuals reported being less likely to carry an umbrella, which is culturally recognized as a protective measure against the flu.

We could also suggest that doctors may have misdiagnosed conditional on whether an individual had been vaccinated or not. If a doctor learned that a person who is showing flu-like symptoms has been vaccinated, they might be more likely to misdiagnose those symptoms as a non-flu respiratory disease. However, the results in Figure 3 show that employees assigned to appointments during the workweek, who were more likely to get vaccinated, had a lower rate of diagnosis of non-flu respiratory diseases than those assigned to the weekend.

Another potential concern is the fact that the data on medical diagnoses used in this study correspond with employees who visited the on-site doctor or an external doctor during working hours, while those who saw an external doctor outside working hours and who were diagnosed sick but were not granted a sick day are coded as healthy. This measurement error will not bias the flu and non-flu estimates as long as it is uncorrelated with the assignment to vaccination during the workweek. However, if employees assigned to get vaccinated during the workweek are more likely to consult an external doctor outside working hours, then this would overestimate the effect on non-flu respiratory diagnoses. To address this potential concern, we bound the effect (Lee, 2009). First, we calculate the treatment–control difference in the proportion of healthy individuals. Then, we trim this difference from the control group (assigned to vaccination on *Saturday*) to obtain an upper bound, and we trim this difference from the treatment group (assigned to vaccination on a working day) to obtain a lower bound. The effect of being assigned to a day during the workweek on the probability of being diagnosed with a non-flu respiratory illness is always negative and bounded between 5.6 and 10.3 percentage points (Appendix Table A8).

A final alternative to moral hazard is the idea of adverse selection, which would suggest that employees with higher risk tolerance regarding health are more likely to get vaccinated and to

engage in risky health behavior. However, adverse selection cannot be a driver of our results because we use an exogenous source of variation on take-up rates. The marginal individual who gets vaccinated is a person who would not have gotten vaccinated if assigned to *Saturday*. This variation is uncorrelated with the underlying risk preferences and with other traits of employees that could determine adverse selection.

6. Conclusions

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

To answer the question of whether the vaccination campaign was economically beneficial for the company carrying out this health intervention, we can perform a back-of-the-envelope calculation of the net benefit of this campaign. This analysis has the limitation that we are not able to fully quantify all of the possible effects that vaccination may have on outcomes relevant to the bank, e.g., morale and productivity.[34] Our calculation suggests that the net benefit of the campaign

[34] A channel pertaining to company morale is the perception of individuals that the company cares more about their health when assigned to the workweek, which leads them to behave differently. However, we cannot find evidence for that channel using data on organizational perceptions from our participants' post-intervention survey responses. Appendix Table A10 presents imprecise estimates on self-reported productivity and the duration of the working day (as measured by the employees' magnetic card swipes for entering and exiting the bank). Albeit statistically insignificant, the point estimates suggest that assigning employees to get vaccinated during the workweek increased their perception of their productivity, while it decreased the duration of their working day by about a third of an hour. Given that the bank pays a fixed salary, this could suggest an increase in productivity. However, in the absence of more precise measures of productivity, we cautiously conclude from this analysis that there is no sizable productivity premium. One could argue that, from the perspective of a company, sick days have higher economic relevance, given that they often go along with re-assignment of tasks, in comparison with some employees being able to finish their tasks and leave earlier than others.

was negative regarding sick days. In the best-case scenario, the treatment may have resulted in a net gain of $0.17 regarding gains in work attendance during the flu season, which is not sufficient to compensate the bank for its costs that include vaccine subsidies of $2.57, $5.05, and $9.99 per vaccine.[35]

Our study presents multiple practical implications for health interventions. From a research perspective, it is useful to employ a randomized encouragement design to circumvent any ethical dilemma when studying the consequences of interventions relevant to people's health. It allows for studying both potential health benefits and behavioral changes in an unbiased way. The presence of moral hazard in health-related behavior implies that firms and policymakers should consider this phenomenon in the design of interventions such as vaccination campaigns. A promising mechanism to mitigate this issue could be campaigns to increase awareness of the importance of other protective measures against respiratory viruses and bacteria so that people will not rely only on the protection potentially provided by medical technology.

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

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

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Table 1 Summary Statistics

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

Notes: This table characterizes the mean employee of the bank, where we implemented our intervention. We present statistics for the full sample and the four treatment groups. The last column presents the p-value of a joint significance test to check whether there are significant differences across the treatment groups. The proportion of employees diagnosed sick or granted a sick day corresponds to the period between January 1 and November 7, 2017, before the vaccination campaign.

Table 2 Effects of Treatments on Vaccination Take-Up

	Baseline	With Controls	Quito Sample	Non-Compliance	Day of Week Effects
Altruistic Information	-0.0260	-0.0209	-0.0493	-0.0262	
	(0.0310)	(0.0303)	(0.0332)	(0.0306)	
Selfish Information	-0.0032	-0.0011	-0.013	-0.0103	
	(0.0314)	(0.0316)	(0.0339)	(0.0308)	
Thursday					0.0002
					(0.0346)
Friday					-0.0356
					(0.0331)
Saturday	-0.0789***	-0.0791***	-0.0898***	-0.0671**	-0.0818***
	(0.0301)	(0.0304)	(0.0313)	(0.0298)	(0.0315)
Average take-up base group in Quito		0.1732		0.1623	0.1651
N	1164	1164	929	1152	929

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

Notes: Robust standard errors in parentheses. This table presents OLS estimates of the effect of the different treatments on vaccination take-up. All specifications control for Quito fixed effects. Column 1 presents our main estimates from equation (1) without adding additional controls. In Column 2, we test the robustness of the main estimates controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level. Column 3 presents the estimates using only employees in Quito, the city where we implemented our four treatments. In Column 4, we exclude 12 individuals who were assigned to vaccinate in the workweek but went to vaccinate on Saturday. In Column 5, we test for different effects across the different days of the week using only data from Quito that has all the treatments. Using clustered standard errors at the work unit level (142 clusters) yields similar standard errors with no loss of statistical significance. For Columns 1-3, we define the base group as the Control group in Quito. For Column 4, it is the same group but adjusting the sample for non-compliance, and for Column 5, it is the take-up rate on Wednesday in Quito.

Table 3 Effect of Peer Vaccination on Individual Take-up

	First Stage	Reduced Form	OLS	2SLS
Proportion of Peers:				
Assigned to the Workweek	0.2079***	0.0013***		
	(0.0434)	(0.0006)		
Vaccinated			0.9917***	0.6248***
			(0.0744)	(0.2158)
Montiel Olea-Pflueger F-value	22.869			
N	1138	1138	1138	1138

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

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

Table 4 Heterogeneous Peer Effects on Individual Take-up

	Proportion of Peers Vaccinated:					
	In Managerial Positions	Not in Managerial Positions	Similar Age	Different Age	Same Gender	Different Gender
First stage	0.2073***	0.2080***	0.0630***	0.2571***	0.1914***	0.1774***
	(0.0326)	(0.0265)	(0.0167)	(0.0408)	(0.0612)	(0.0305)
	[40.4701]	[61.4662]	[14.2110]	[39.7137]	[9.7657]	[33.6441]
Reduced form	0.0032	0.0369	-0.0306	0.1493**	0.1073	0. 0465
	(0.0039)	(0.0279)	(0.0560)	(0.0602)	(0.0680)	(0.0672)
2SLS	0.0152	0.1773	-0.4777	0.5490***	0.5673**	0.2622
	(0.0193)	(0.1224)	(0.9082)	(0.2109)	(0.2355)	(0.33587)
N	982	1082	1138	1138	1138	1138

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

Notes: Standard errors clustered at the unit level in parentheses, first stage F-values in brackets. The bank has 116 units with more than one employee. This table presents the heterogeneous effects of different types of peer vaccination take-up on the individual's vaccination decision. The column headers identify the type of peer, for example, in the first column, we present the effect of the proportion of peers of the same gender who got vaccinated on individual vaccination. We measure the proportion of peers vaccinated and the proportion of peers assigned to the workweek in percentage points. Thus, the estimates represent the effect of a one percentage point change in the proportion of peers. The first row presents the first stage results; for example, the fifth cell indicates that if the proportion of peers of the same gender assigned to the workweek increased by one percentage point, then the proportion of peers of the same gender who got vaccinated increases by 0.22 percentage points. The second row presents reduced form results. For example, for the fifth column, if the proportion of peers of the same gender assigned to the workweek increased by one percentage point, then individual take-up increases by 0.17 percentage points. The third row presents 2SLS estimates. For example, for the fifth column, if the proportion of peers of the same gender assigned who got vaccinated increases by one percentage point, then individual take-up increases by 0.60 percentage points. All specifications control for Quito fixed effects and individual assignments to the workweek. Sample size changes because not everybody has a peer in the same category. For example, if a manager has no manager peers in her unit, then that observation is dropped.

Table 5 Effects of Vaccination on Overall Sickness

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

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

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

Table 6 Effects of Vaccination on Overall Sick Days

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

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

Notes: Standard errors clustered at the unit level in parentheses. This table presents the effect on the probability of having a sick day. The estimates include only units with two or more employees. All specifications control for Quito fixed effects. Column 1 presents OLS estimates. Column 2 presents the reduced form estimates.

Table 7 Effects of Vaccination on Flu Diagnoses

	OLS	Reduced Form
Assigned to the workweek		0.0045
		(0.0155)
Prop. peers assigned to the workweek		-0.0003
		(0.0006)
Vaccinated	-0.0254*	
	(0.0151)	
Prop. peers vaccinated	-0.0001	
	(0.0004)	
Average for unvaccinated in Quito (p.p.)		0.0457
N		1120

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

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

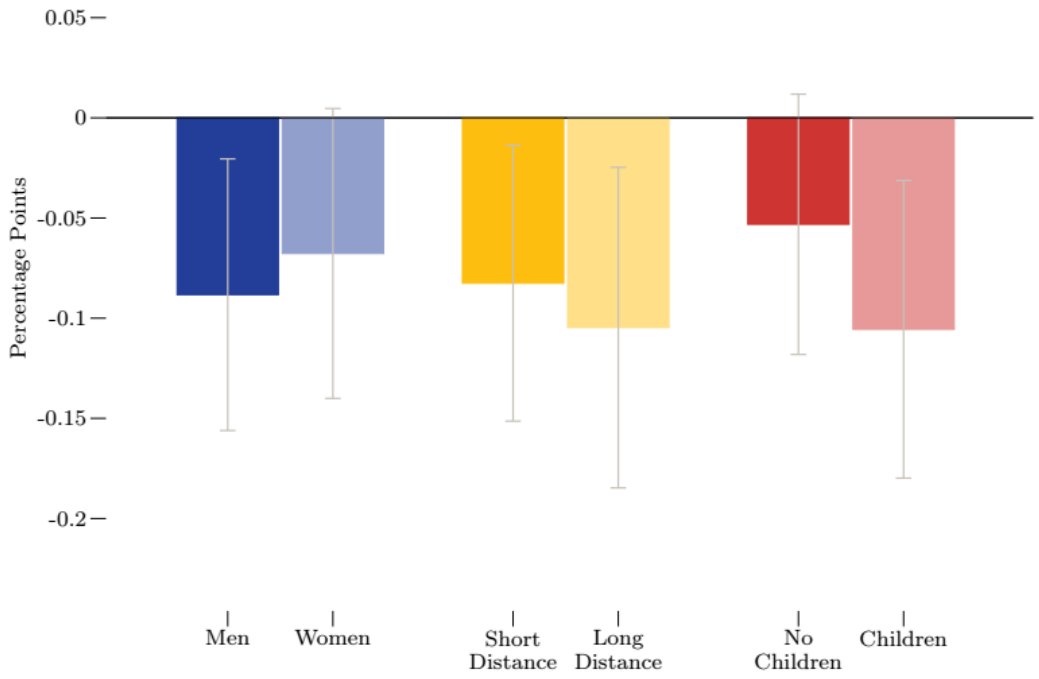
Table 8 Reduced Form Estimates on Health-Related Habits

	Baseline	Coefficient	N
Responses on a scale from 1 ("never") to 10 ("all the time")			
How often do you exercise	5.93	-0.3145	358
		(0.4026)	
How often do you take dietary supplements	3.18	-0.6212	358
		(0.4376)	
How often do you carry an umbrella when it rains	6.85	-1.2070**	358
		(0.4861)	
How often do you wash your hands	9.25	0.1086	358
		(0.1835)	

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

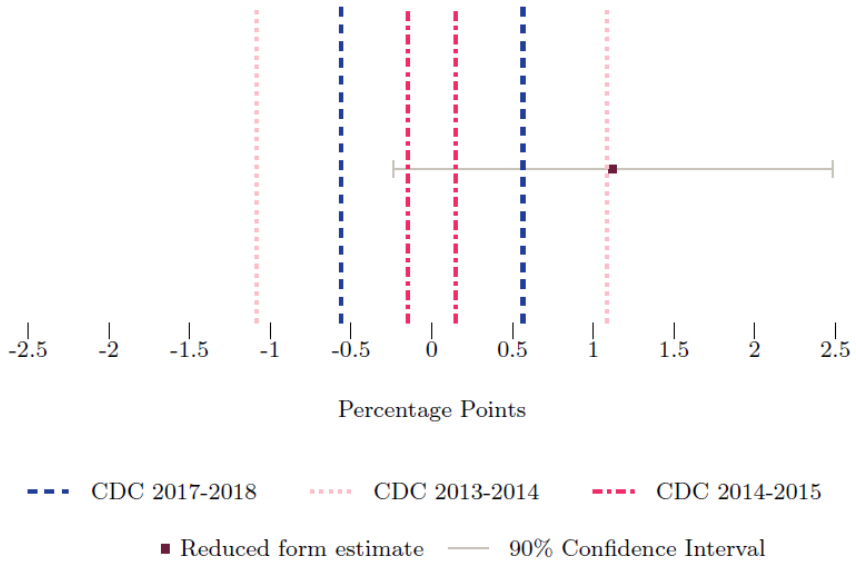
Notes: Robust standard errors in parentheses. This table presents the reduced form effects of being assigned to the workweek on four daily habits and activities related to health and preventing the flu. All specifications control for Quito fixed effects. Column 2 presents the reduced form estimates. Column 3 presents the number of individuals who answered the survey question.

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



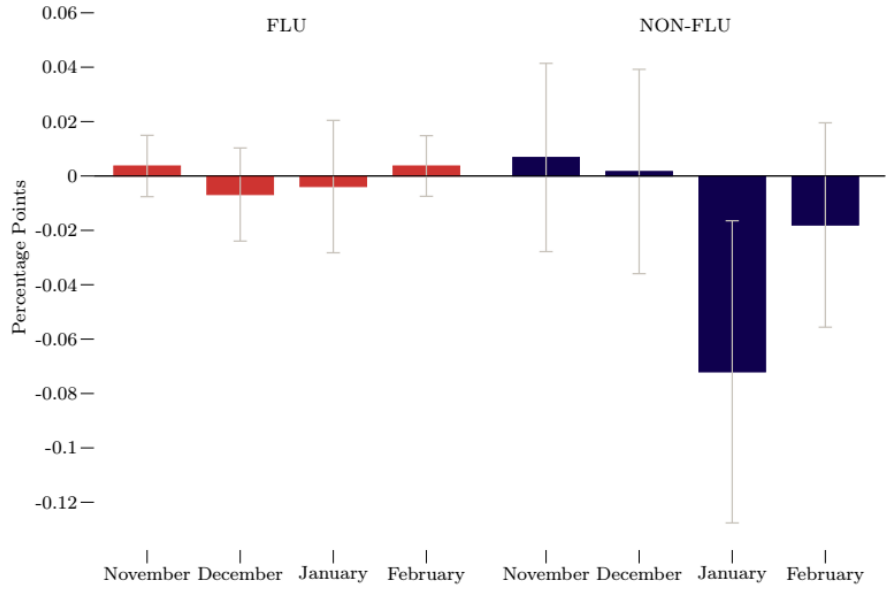
Notes: This figure presents the intent-to-treat effect of assignment to Saturday on vaccination take-up for different subgroups in the sample. All specifications control for city fixed effects. The figure presents the point estimate and the 90% heteroscedastic robust confidence interval for each subgroup.

Figure 2 Equivalence Test for the Effectiveness of Vaccination



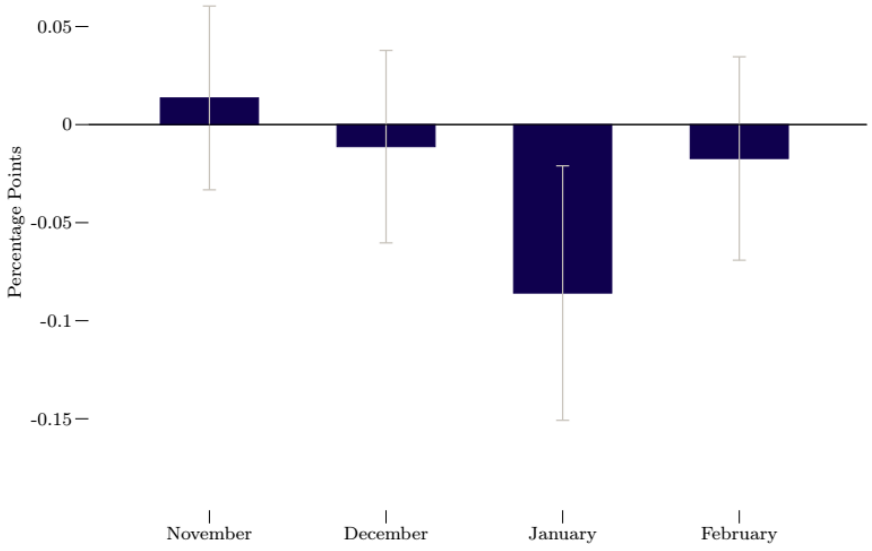
Notes: This figure presents the reduced-form estimate of the effect of assignment to the workweek to adjusted CDC estimates of the effectiveness of the flu vaccine for 2013-2014, 2014-2015, and 2017-2018 seasons.

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



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

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



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

Online Appendix

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

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

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

Notes: Robust standard errors in parentheses. This table presents the local average treatment effects of a small price change on vaccination take-up. We report the normalized coefficient at a wage of $750 and a bandwidth of $300. Individuals who earn more than $750 paid $7.49 for the vaccine, while employees whose wage is below this threshold paid $4.95. There is no visible discontinuity across the threshold — all specifications control for city fixed effects. Column 1 presents our main estimates without adding additional controls. In Column 2, we test the robustness of the main estimates controlling for the vaccine's price, income, tenure, division in the company, gender, age, and education level. Column 3 presents the estimates using only employees in Quito, the city where we implemented our four treatments. In Column 4, we exclude 12 individuals who were assigned to vaccinate in the workweek but went to vaccinate on Saturday. Reducing the bandwidth in steps of $50 to $150 does not change the results.

Table A2 Recall Information Statements

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

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

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

Table A3 Heterogeneous Treatment Effects on Vaccination Take-up

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

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

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

Table A4 Vaccination Take-Up Analysis of Transaction Costs

	Saturday by Distance	Saturday by Payment
Distance	0.0028	
	(0.0024)	
Saturday	-0.0605	-0.1224
	(0.0373)	(0.0751)
Saturday interacted with Distance	-0.0012	
	(0.0041)	
$5 payment		-0.0177
		(0.0444)
$7 payment		-0.0813**
		(0.0408)
Saturday interacted with $5 payment		0.0043
		(0.0866)
Saturday interacted with $7 payment		0.0217
		(0.0800)
N	895	1164

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

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

Table A5 Robustness Check on Peer Effects Estimates

	Unit Size	Peer Characteristics
A. Main Effect		
Proportion of peers:		
Vaccinated	0.6371***	0.5736***
	(0.2038)	(0.2157)
N	1138	1138
B. Heterogeneous Effects		
Proportion of peers:		
Same Gender Vaccinated	0.6423***	0.6033***
	(0.2007)	(0.2140)
Different Gender Vaccinated	0.1743	0.0975
	(0.3796)	(0.3821)

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

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

Table A6 Potential Mechanisms for Peer Effects

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

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

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

Table A7 Robustness Check on Effects of Vaccination on the Flu

	Narrowest Definition of Flu	Main Definition of Flu	Broadest Definition of Flu
	Reduced Form	Reduced Form	Reduced Form
a. Baseline specification			
Assigned to the workweek	-0.0054	0.0032	-0.0118
	(0.0156)	(0.0160)	(0.0191)
N	1148	1148	1148
b. Additional control variables			
Assigned to the workweek	-0.0051	0.0040	-0.0115
	(0.0157)	(0.0161)	(0.0192)
N	1145	1145	1145

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

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

Table A8 Bounds

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

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

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

Table A9 Heterogeneous Effects on Using an Umbrella

	Baseline	Coefficient	N
Responses on a scale from 1 ("never") to 10 ("all the time")			
	A. Overall		
How often do you carry an umbrella when it rains	6.85	-1.2190**	358
		(0.4856)	
	B. Vaccine Effective		
How often do you carry an umbrella when it rains	7.13	-1.5793***	256
		(0.5651)	
	C. Vaccine Ineffective		
How often do you carry an umbrella when it rains	6.06	-0.2292	102
		(0.9615)	

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

Notes: Robust standard errors in parentheses. This table presents the intent-to-treat effect of being assigned to the workweek on instances of carrying an umbrella and heterogeneity with beliefs of vaccine effectiveness splitting beliefs at the median on a Likert-scale of 8/10. Column 2 presents the reduced form estimates. Column 3 presents the number of individuals who answered the survey.

Table A10 Reduced Form Effects on Productivity

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

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

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

Figure A1 Treatment Message: Control



Notes: The above image portrays the email sent to the control group. Translation: Dear Employee, we are running an influenza vaccination campaign in November. You are eligible for a flu shot on Thursday, November 9, from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, the price is $4.95, which will be deducted from your payroll if you choose to get vaccinated. If you have questions, please contact _____. Let's get vaccinated!

Figure A2 Treatment Message: Opportunity Cost (Saturday)



Notes: The above image portrays the email sent to the "*Saturday*" treatment group. Translation: Dear Employee, we are running an influenza vaccination campaign in November. You are eligible for a flu shot on Saturday, November 11, from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, the price is $4.95, which will be deducted from your payroll if you choose to get vaccinated. If you have questions, please contact _____. Let's get vaccinated!

Figure A3 Treatment Message: Altruism



Notes: The above image portrays the email sent to the "Altruistic Treatment" group. Translation: Dear Employee, we are running an influenza vaccination campaign in November. You are eligible for a flu shot on Thursday, November 9, from 8:30 to 11:30. We obtain a discount on the vaccine's price. For you, the price is $4.95, which will be deducted from your payroll if you choose to get vaccinated. Getting vaccinated yourself also protects people around you, including those who are more vulnerable to severe flu illness, like infants, young children, the elderly and people with dangerous health conditions that cannot get vaccinated If you have questions, please contact_____. Let's get vaccinated!

Figure A4 Treatment Message: Selfish



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

Figure A5 Locations of the Bank in Ecuador



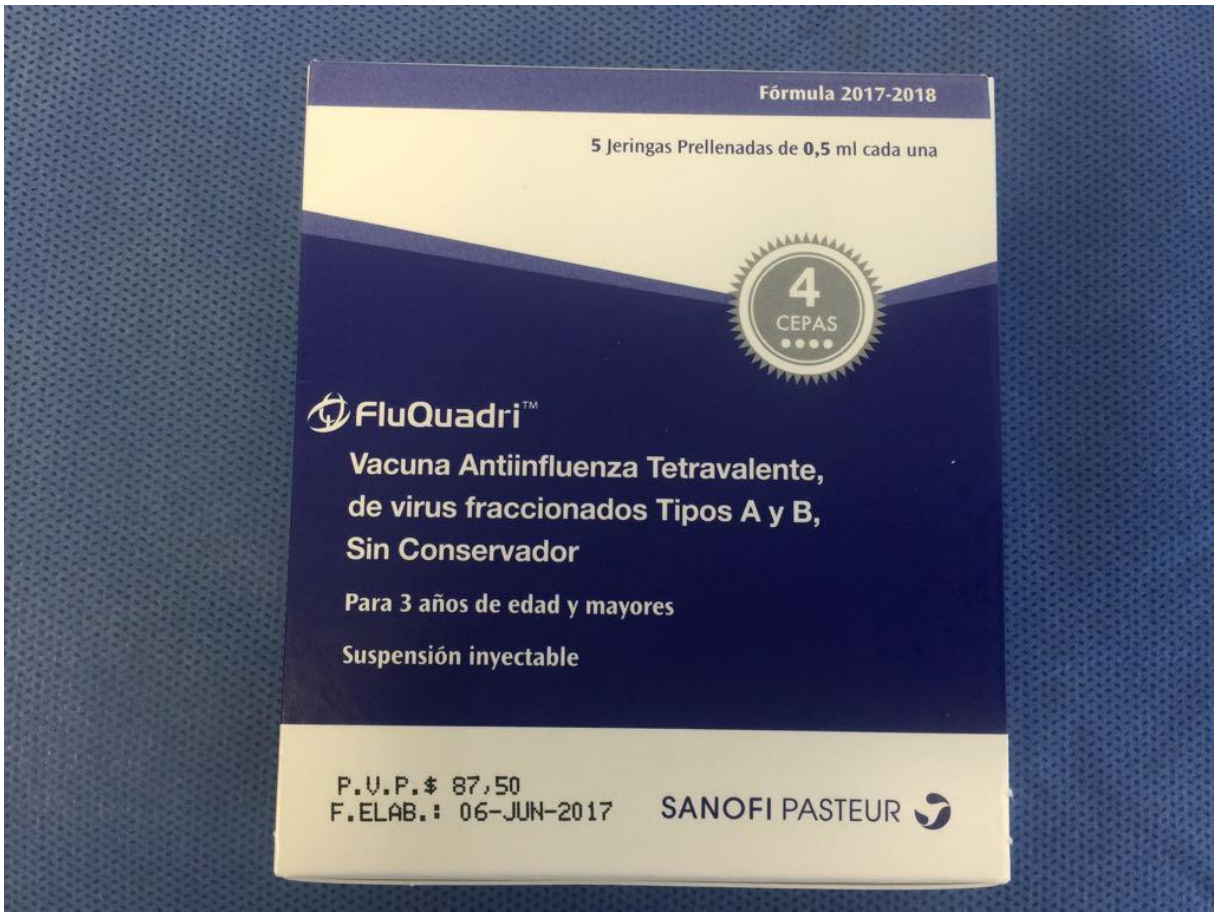
Notes: The map contains the locations of the bank in Ecuador (orange), where we implemented our intervention.

Figure A6 Timeline of Experiment Implementation



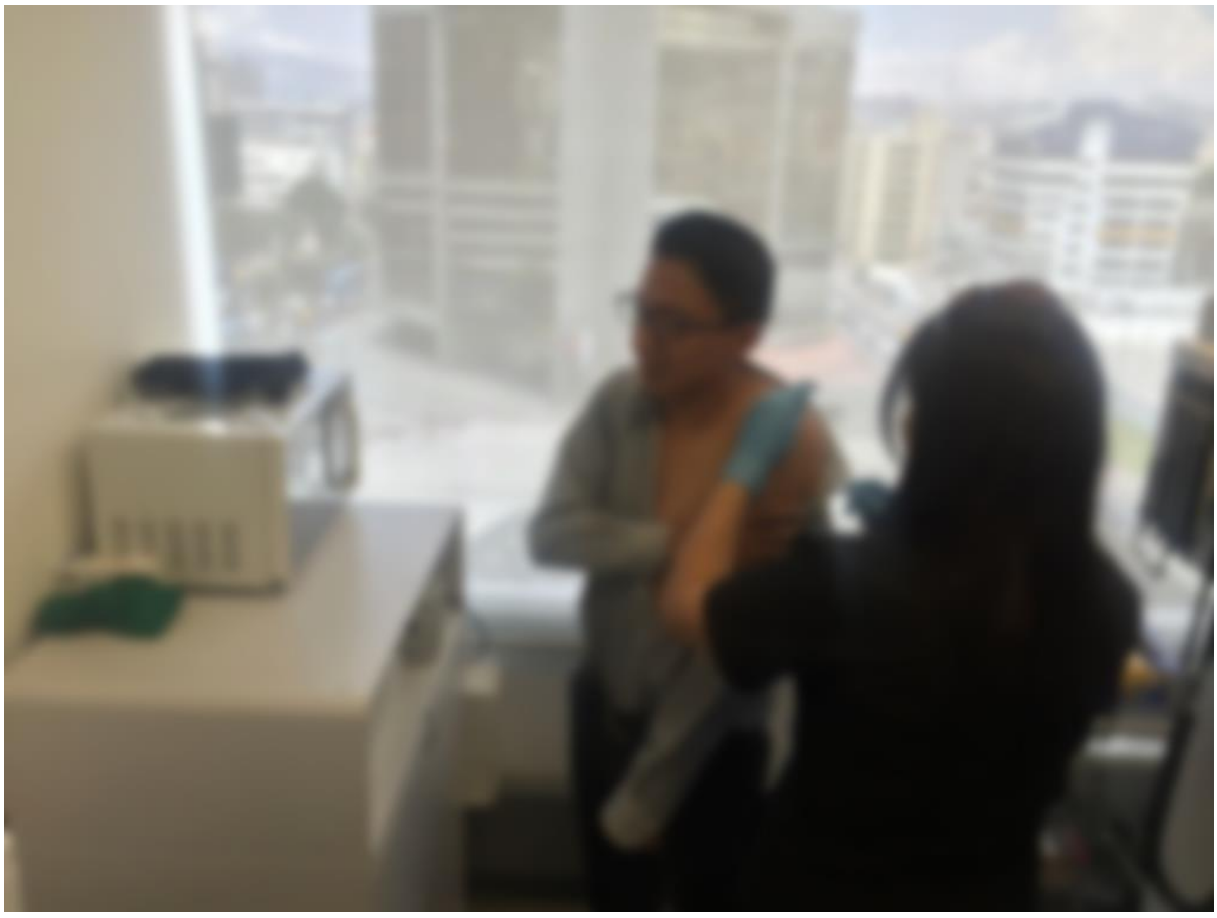
Notes: The bank sent the pre-intervention survey on October 18. The bank sent emails with the different treatments on November 1 using the Human Resources Department mailing account. Furthermore, it sent a reminder on November 7. The vaccination campaign took place between November 8 and November 11. The post-treatment period (Ecuadorian flu season) went from November 13 to March 1. The bank sent the post-intervention survey during March and April 2018.

Figure A7 Vaccination Campaign: Influenza Vaccine



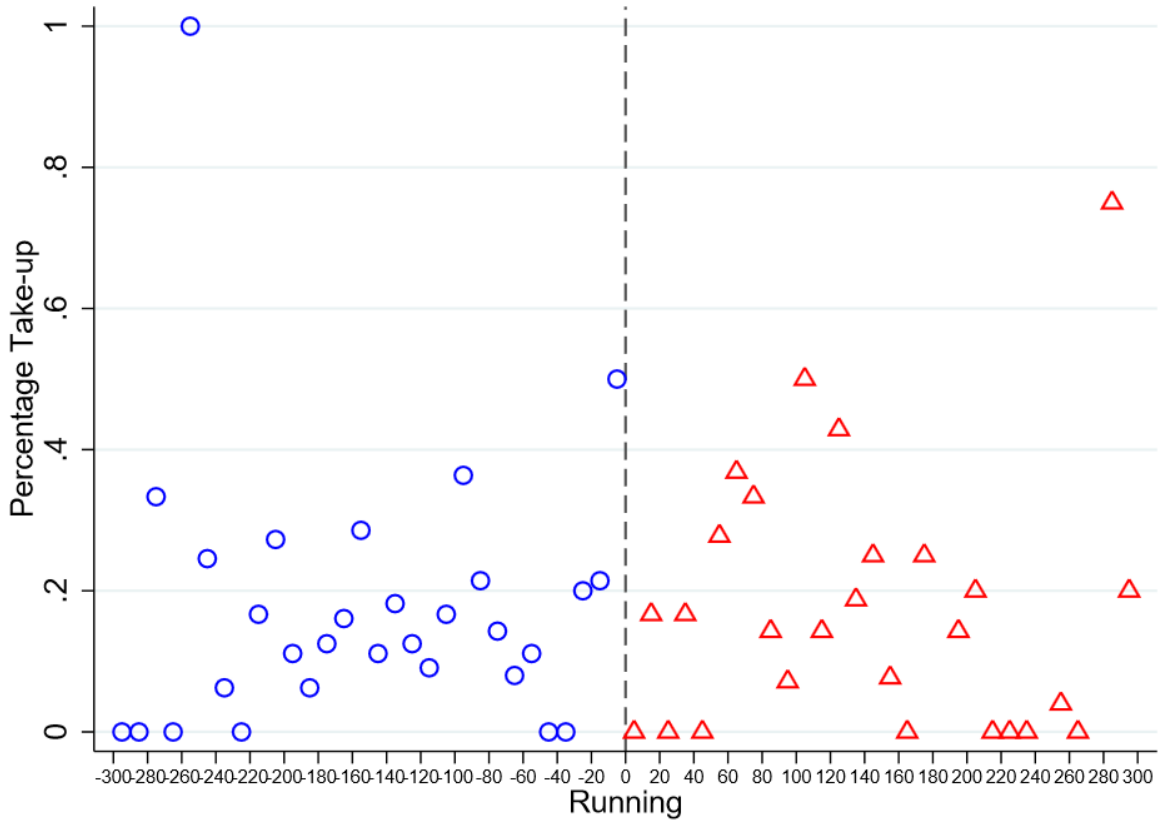
Notes: The above package contains the influenza vaccine used in the campaign. This vaccine protects against four strands of the flu, two from type A and two from type B.

Figure A8 Vaccination Campaign: Flu Shot in Action



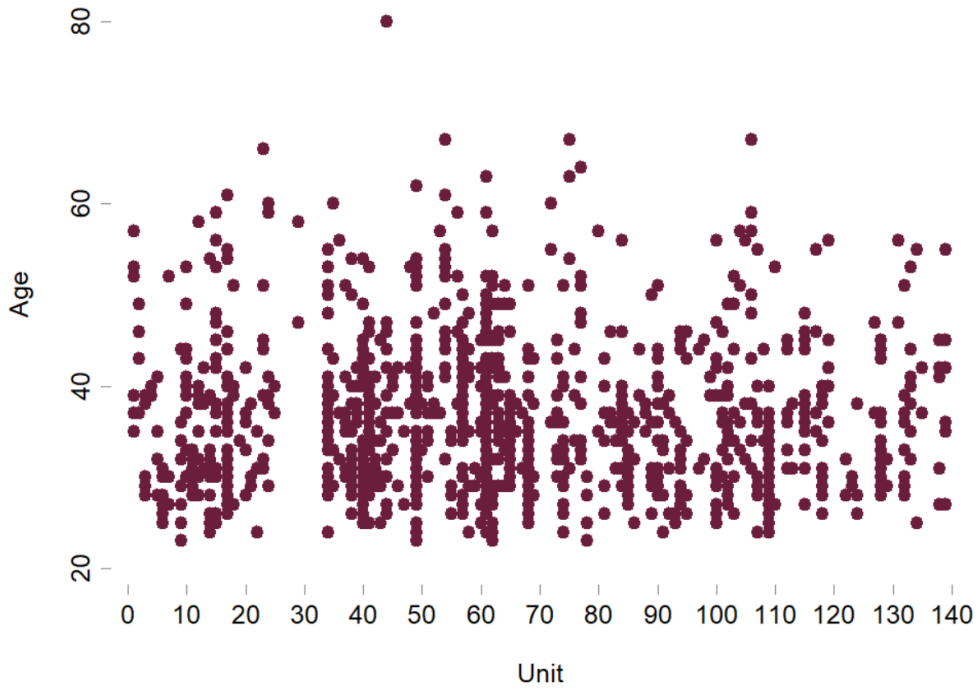
Notes: Immunization at the firm.

Figure A9 Vaccination Take-up around $750 Wage Threshold



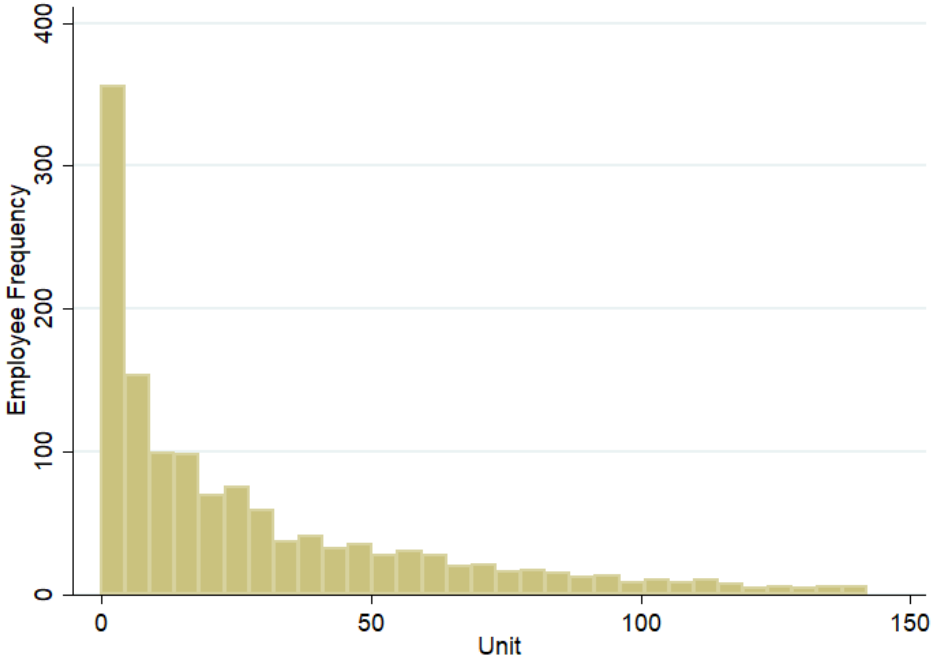
Notes: This figure presents the evolution of vaccine take-up around the $750 threshold with a bin size of $10. Individuals who earn more than $750 paid $7.49 for the vaccine, while employees whose wage is below this threshold paid $4.95.

Figure A10 Age Distribution of Employees in Units



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

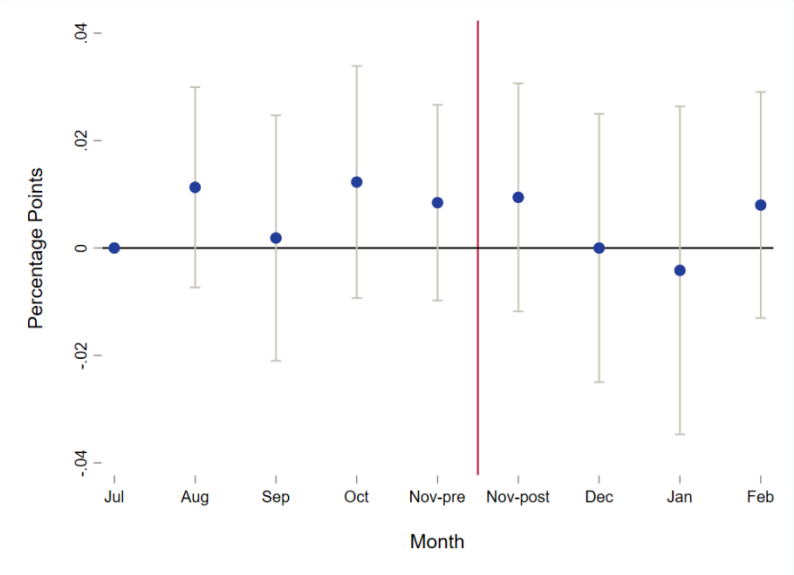
Figure A11 Frequency Distribution of Employees in Units



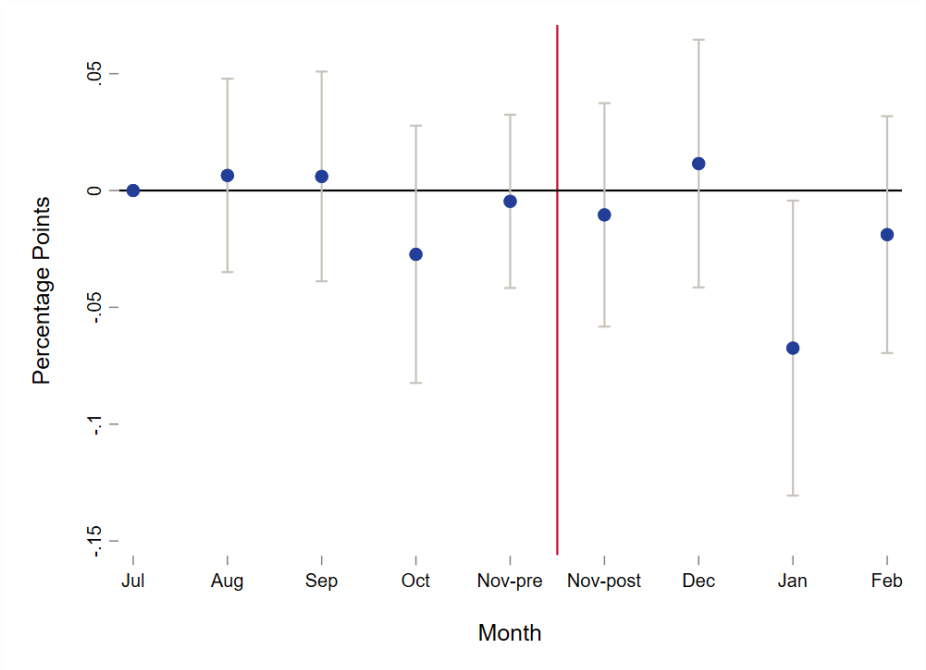
Notes: This figure presents the number of employees in each of the 142 units.

Figure A12 Difference in Difference Estimates of the Effect of Vaccination on Diagnosed Sickness

(a) Flu Diagnoses



(b) Non-flu Diagnoses



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