Incentives to Vaccinate

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Abstract

Whether monetary incentives to change behavior work and how they should be structured are fundamental economic questions. We overcome typical data limitations in a large-scale field experiment on vaccination ($N = 5,324$) with a unique combination of administrative and survey data. We find that guaranteed incentives of $20 increase uptake by 13 percentage points in the short run and 9 in the long run. Guaranteed incentives are more effective than lottery-based, prosocial, or individually-targeted incentives, though all boost vaccinations. There are no unintended consequences on future vaccination or heterogeneities based on vaccination attitudes and incentivized economic preferences. Further, administrative data on relatives shows substantial positive spillovers. Our findings demonstrate the great potential of incentives for improving public health and provide guidance on their design.

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1 Introduction

Whether and how to use monetary incentive policies to change behavior remains a fundamental topic for policy-makers and academics. Given that a large share of the public and private disease burden is linked to modifiable unhealthy behaviors (WHO, 2023), economists and public health experts have long debated the potential of payments to promote healthy behavior and internalize externalities. For example, incentives have been considered to encourage exercising (e.g., Charness and Gneezy, 2009; Milkman et al., 2021*a*; Carrera et al., 2022), weight loss (Volpp et al., 2008), smoking cessation (e.g., Volpp et al., 2009), blood donation (Lacetera, Macis and Slonim, 2013; Goette and Stutzer, 2020) and vaccination (e.g., Banerjee et al., 2010; Alsan, Garrick and Graziani, 2019; Campos-Mercade et al., 2021).

While some progress has been made in understanding the effects of monetary incentives on health behaviors, as reviewed in the next section, considerable disagreement remains about the very fundamentals of when and how incentives work: The potential effectiveness of incentives, whether incentives backfire for certain individuals, what types of incentives work best, and whether incentives have unintended consequences such as negative spillover or long-run effects (for reviews, see Gneezy, Meier and Rey-Biel, 2011; Bowles and Polanía-Reyes, 2012; Kamenica, 2012).

Given the ongoing debate on the effectiveness of incentives, it is perhaps not surprising that during the recent COVID-19 pandemic, there was no consensus on whether incentives would help or hurt immunization.[1] Although incentives had been used for vaccinations prior to the pandemic, the lack of clear evidence regarding their effects and optimal implementation led to a variety of conflicting policy strategies.[2] Strategies included lottery-based incentives, in-kind or prosocial incentives, guaranteed monetary incentives, and not offering any incentives at all (see, e.g., Thirumurthy et al., 2022, who review the wide range of incentive programs put forth by 24 US states).

[1]In 2020 and 2021, as governments and organizations urgently sought ways to increase vaccination rates, numerous academic and media articles debated the use of incentives to encourage vaccination. Some experts argued that incentives would likely be effective and their benefits would outweigh potential risks (Savulescu, 2021; Oza, 2021; Persad et al., 2021), and proposals to pay people to take the vaccine were endorsed by many journalists, policy-makers such as Joe Biden, and renowned researchers, including Greg Mankiw, Steven Levitt, and Paul Romer. However, many others cautioned against the use of incentives, suggesting that they could theoretically reduce vaccination uptake (e.g., Jecker, 2021; Largent and Miller, 2021; Volpp, Loewenstein and Buttenheim, 2021).

[2]For example, some academics argued that lottery incentives may be more effective than guaranteed incentives because people tend to overweight small probabilities (e.g., Milkman et al., 2022), while others argued that lottery incentives would be less effective (e.g., Brewer et al., 2022).

A major obstacle to a better understanding of the impacts of incentives in general, and on health behaviors and vaccination specifically, is limited data. Encompassing causal analyses require exogenous variation in incentives, rich survey and administrative data, and sufficient statistical power to assess treatment effects across different subgroups (Harrison and List, 2004; Athey et al., 2019; Haushofer and Metcalf, 2020; List, 2020; DellaVigna and Linos, 2022; Saccardo et al., 2023; Campos-Mercade et al., 2023). For instance, given the concerns related to potential unintended consequences of incentives, it is crucial not only to obtain these data immediately following incentivization but also in the long run and for future vaccinations. Additionally, assessing concerns that incentives might affect certain people differently—such as deterring hesitant people from vaccinating, or encouraging particularly selfish or risk-seeking people—requires detailed surveys, including data on vaccination attitudes and accurate measures of economic preferences. Such extensive data is essential to guide discussions on whether to offer incentives, how they should be designed, and who should be targeted with specific incentives.

In this paper, we comprehensively assess the impact of incentives to vaccinate. We conducted a large, pre-registered randomized controlled trial that incentivized a general population sample in Sweden ($N = 5,324$) to receive a booster dose (the third dose) of a COVID-19 vaccine. We timed the intervention to coincide with the start of booster dose rollout. We divided our sample into a control group and four treatment groups that each received a particular type of incentive if they got the booster dose within 30 days of participating in the experiment. We study the incentive types most frequently considered by academics and policy makers: a guaranteed incentive of $20 (about SEK 200), a lottery incentive with an expected value of $20, and a donation to charity of $20 (Mellström and Johannesson, 2008; Lacetera and Macis, 2010; Imas, 2014; Tonin and Vlassopoulos, 2015; DellaVigna and Pope, 2018; Fabbri, Nicola Barbieri and Bigoni, 2019; Duch et al., 2023*a*). Moreover, we include a novel intervention that allows participants to choose from any of the three incentive types.

Through our collaboration with the Public Health Agency of Sweden and the Tax Agency of Sweden, we create a unique link between rich experimental data with complete population-wide administrative vaccination records for all participants, as well as for their married partners, parents, and children. The resulting data has several key advantages: First, rather than relying on intentions or self-reported vaccination behavior, as much of the previous research does, we can identify with certainty whether each participant vaccinated. Second, the

vaccination records capture the timing of each participant's vaccination decisions, including any subsequent doses, for up to 22 months following the intervention, allowing us to examine both the long-run effects and the potential unintended consequences of incentives for future vaccination. Third, our experimental data allows us to study heterogeneous treatment effects with detailed information on participant characteristics, including incentivized measures of risk and social preferences and attitudes surrounding vaccination. Fourth, data on married partners, parents, and children enables us to evaluate spillover effects of the interventions. Last, a common drawback of experiments changing health behavior is attrition (Saccardo et al., 2023). Given that the administrative records cover all vaccinations and the whole population—and not just, e.g., locations that were exposed to the intervention or data from only specific providers—we have no attrition for the outcome measurements across time and place.

We document six main findings. First, we find that all incentive schemes have large impacts on vaccination uptake, with guaranteed incentives being the most effective. In the control condition, 32% of participants took the vaccine within 30 days of the experiment. In comparison, 45% of individuals in the guaranteed incentives condition vaccinated within 30 days ($p < 0.001$), 41% in the lottery condition ($p < 0.001$), and 37% in the donation condition ($p = 0.028$).

The effect of guaranteed incentives conceptually replicates the results of Campos-Mercade et al. (2021), where, in a separate experiment, we explored the short-run impact of a guaranteed incentive treatment in Sweden. In that study, a guaranteed incentive of ∼$24 led to a 4 percentage point increase in first-dose uptake of a COVID-19 vaccine within 30 days, starting from a 72% baseline. Thus, our first finding not only replicates the positive short-run effect of guaranteed incentives observed in Campos-Mercade et al. (2021)—revealing an effect three times greater—but also expands it by comparing this effect with those of other types of incentives.

Our second main finding is that incentives not only accelerate vaccination uptake but also substantially increase long-run vaccination rates. We observe vaccination behavior for nearly two years after our initial experiment and find persistent effects. For instance, participants in the guaranteed incentives condition were not only 13 percentage points more likely to get vaccinated within 30 days compared to those in the control group, but also showed a 9 percentage points higher uptake 22 months after the intervention. We observe similar results

for the lottery and choice conditions and conclude that incentives are effective not only in accelerating vaccination decisions, but also in sustainably increasing overall uptake.

Our third main finding documents heterogeneous treatment effects based on rich experimental data and then examines the potential for targeting using machine learning. To examine concerns that incentives may backfire for certain groups of people, such as vaccine hesitant and prosocial individuals, we test for heterogeneities using exhaustive data on socio-demographics, vaccination attitudes, as well as incentivized measures of risk and social preferences. Based on OLS specifications, we find some evidence for heterogeneous treatment effects. In particular, the donation incentive is more effective among the most prosocial individuals than among the most selfish individuals. However, contrary to common concerns in the literature, none of the incentive types reduce vaccination rates, not even among the most prosocial individuals. We do not find substantial heterogeneous treatment effects depending on risk-preferences,[3] vaccination attitudes, or socio-demographics such as gender, age, income, or education. Instead, the results show that the different incentives raise vaccination rates irrespective of people's background, preferences, and vaccine attitudes.

Next, we study the potential of targeting incentives to certain groups allowing for complex interactions in characteristics. While guaranteed incentives are the most effective overall at increasing vaccinations and the OLS results discussed above do not indicate large heterogenities, certain groups might still react more strongly to the lottery or charitable donation conditions. If such groups existed, a more effective program could target each group with the incentive scheme that is most effective for that specific group. Although we find some evidence for heterogeneous treatment effects for social preferences, they are insufficient to effectively target treatments. The reason is that the guaranteed incentives condition is particularly effective for a wide variety of groups, even the most prosocial participants. Using machine learning to target different groups of individuals with certain incentive schemes—allowing for more flexible interactions between sociodemographics, vaccination attitudes, and preferences—confirms the finding (following Knaus, 2022; Athey and Wager, 2021; Zhou, Athey and Wager, 2023). In sum, the guaranteed incentives prove to be the most effective across different participant groups.

[3]We tailored our measures of risk and social preferences to the lottery and donation treatments and preregistered them as the two main dimensions of heterogeneity. Surprisingly, we do not find statistically significant interaction effects between the lottery incentives and risk preferences

We also report the results of a novel intervention, where participants can choose which of the three vaccination incentives they would like to receive. This intervention may be particularly effective if participants have unobservable preferences and attitudes that allow them to sort into interventions more effectively than we researchers can assign them based on observable characteristics. We find that participants indeed choose different incentive schemes (42% guaranteed, 28% lottery, 30% donation) and that the choice strongly depends on their social and risk preferences, which could indicate such sorting. Yet, vaccination uptake after 30 days in the choice condition is statistically indistinguishable from the uptake in the guaranteed condition (43% vs. 45%). Within the choice condition, the uptake after the choice of each incentive is similar to uptake in the randomly assigned conditions. This set of findings further highlights that heterogeneity in reactions to incentives is too small to target different types of individuals with specific incentive schemes.

Our fourth main finding is that we can reject even small unintended consequences of incentives on future adherence to vaccination schedules. A large body of research warns that financial incentives could crowd out prosocial or intrinsic motivation (Titmuss, 1970; Deci, 1971; Gneezy and Rustichini, 2000; Fehr and Rockenbach, 2003; Mellström and Johannesson, 2008; Sandel, 2012; Royer, Stehr and Sydnor, 2015; Largent and Miller, 2021; Esteves-Sorenson and Broce, 2022; Karing, Finetti and Kuloszewski, 2024) and increase the risk perceptions of the incentivized behavior (Frey and Oberholzer-Gee, 1997; Bénabou and Tirole, 2003; Ellingsen and Johannesson, 2008; Volpp and Cannuscio, 2021), leading to a reduction of the incentivized behavior in the future when no payments are offered.[4] To study unintended consequences in the long-run, we measure an additional vaccination decision: fourth-dose uptake.[5] To this end, we use administrative data for vaccination behavior up to 22 months after the experiment. If financial incentives crowd-out future motivation or increase risk perceptions associated with the vaccine, they might result in fewer incentivized individuals taking the fourth dose compared to the control group. However, we find no evidence of negative (or positive) treatment effects on fourth-dose uptake; we can reject even small unintended consequences.

[4]Monetary incentives may also have positive consequences for future health behavior, for example through habit formation or by emphasizing the social importance of vaccination (Charness and Gneezy, 2009; Royer, Stehr and Sydnor, 2015; Schneider et al., 2023).

[5]Note that fourth-dose uptake is a novel decision participants have to take, as there were no restrictions in place if one did not receive a fourth-dose, no automatic follow-up appointments, and no automatic reminders based on third-dose uptake.

Our fifth main finding is that offering financial incentives has sizable positive spillover effects on partners who were not incentivized. In principle, spillover effects could be negative, for instance if family members decide to delay vaccination hoping to receive similar incentives in the future, or positive, such as when encouraging one person to vaccinate motivates their family members to also get vaccinated. We combine our experimental data with data from the Tax Agency of Sweden to identify family members, and then measure vaccination uptake of family members in data provided by the Public Health Agency of Sweden. We uncover that vaccination behavior is often coordinated with partners. For example, 30% of our experiment participants received the first dose of the COVID-19 vaccine on the same day as their partners. We then directly test for the spillover effects of our treatment conditions on the partners' decision to vaccinate and find a substantial positive spillover effect. Incentives, when pooled across all conditions, lead to a 7 percentage points increase in vaccination uptake among the partners of the participants. This effect size is about half as large as the direct effects of the incentives on the participants themselves. In contrast to partners, we find little evidence that vaccination behavior is temporally coordinated with parents or children and, similarly, do not find evidence of spillover effects on these groups.

Our sixth and concluding finding highlights the importance of using administrative data to assess the impacts of incentives and identifies a new, well-performing surrogate outcome (Athey et al., 2019; Anderer, Bastani and Silberholz, 2021) when administrative data on vaccination is lacking. Most research examining interventions to foster vaccination uptake relies on self-reported vaccination intentions (for recent meta studies, see Batteux et al., 2022; Huang, Huang and Yu, 2023; Khazanov et al., 2023; Ruggeri et al., 2024). This research assumes that impacts on intentions are a reliable predictor of impacts on actual vaccination behavior, which may not be true.[6] We speak to this large and rapidly growing body of work by collecting data on intentions and examining whether intention effects are a reliable proxy for actual effects on behavior. In contrast to the large behavioral changes that we observe, we find no evidence that incentives impact intentions to vaccinate.[7] If anything, and at odds with actual behavior, data on intentions suggests that the donation incentives backfire. In

[6]On the one hand, it is possible that incentives and experimenter demand effects lead people to report lofty intentions but have very little effect on actual behavior, resulting in an upward bias when using intentions as the outcome. On the other hand, it is also possible that incentives do not change intentions, but rather narrow the intention-behavior gap, resulting in a downward bias when using intentions as the outcome.

[7]Note that we do find that intentions are highly correlated with actual uptake. Providing such correlations is the standard approach to validate self-reported measures (e.g., Fehr and Rockenbach, 2003; Falk et al., 2018). However, our study shows that this approach is problematic when using self-reported items as outcome

sum, relying on intentions when providing policy advice could be problematic and lead to costly policy mistakes.

In contrast to intentions, we find that a behavioral proxy—whether participants click on a link for more information on vaccination and to schedule an appointment—is more reliable. All incentive conditions statistically significantly increase the likelihood of clicking on the link, and the relative ranking of these effects closely mirrors the ranking of their impact on actual vaccination uptake.[8] We conclude that researchers should be wary of results from studies that are based solely on intentions. Instead, in the absence of administrative data, researchers should consider outcomes more closely linked to actual behaviors, such as clicking on an appointment link.

In conclusion, our results show that guaranteed incentives are highly effective in promoting COVID-19 vaccination. The context is comparable to that of other important vaccines—such as flu, measles, or human papillomavirus—where the population is also urged to take multiple doses. These findings highlight the potential of incentives to improve public health, as a large share of the burden of disease comes from vaccination-preventable diseases (The Lancet, 2024; Li et al., 2021): 16 million lives could be saved by vaccination in the next 6 years alone (Shattock et al., 2024).

Beyond the vaccination context, this paper offers valuable behavioral insights into the effects of incentives on behavior. Our experiment indicates that: i) modest guaranteed incentives can be very effective in the long run and outperform other common incentive schemes, even across very diverse groups, ii) incentives can generate positive spillover effects, a benefit often overlooked in policy implementation, iii) boosting efficiency of incentives by carefully targeting specific types of incentives to certain individuals is challenging, even with detailed measures of preferences and personality or with self-selection into different incentive schemes, and iv) surrogate outcomes must be carefully selected, with a focus on behavioral measures.

This paper proceeds as follows. In Section 2, we summarize and discuss related literature. In Section 3, we document the experimental design and data. Sections 4 to 9 present our findings. We conclude in Section 10.

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measures to proxy for treatment effects. The reason is that the treatment can impact the mapping between self-reported and actual behaviors.

[8]While this behavioral proxy performs better than intentions, it is still far from ideal, as it does not provide insights into baseline vaccination levels or the exact sizes of the treatment effects.

2 Related literature on incentives and health behavior change

This paper adds to a large and rapidly growing literature on using incentives to change health behaviors. Examples include studies on encouraging exercising (Charness and Gneezy, 2009; Acland and Levy, 2015; Royer, Stehr and Sydnor, 2015; Carrera et al., 2020; Milkman et al., 2021*a*), smoking cessation (Volpp et al., 2006, 2009; Halpern et al., 2018; Berlin et al., 2021), weight loss (Volpp et al., 2008), blood donation (Mellström and Johannesson, 2008; Lacetera, Macis and Slonim, 2013, 2014; Goette and Stutzer, 2020), and healthy eating (List and Samek, 2015; Dolan, Galizzi and Navarro-Martinez, 2015; Belot, James and Nolen, 2016; Angelucci et al., 2019).[9] For instance, Charness and Gneezy (2009) investigate the impact of incentives on short and long-run gym attendance among university students, while Lacetera, Macis and Slonim (2014) study how offering gift cards influenced blood donation rates among past donors (see also Goette and Stutzer, 2020).

Part of this literature focuses on incentivizing vaccination behavior (e.g., Banerjee et al., 2010; Bronchetti, Huffman and Magenheim, 2015; Mantzari, Vogt and Marteau, 2015; Gibson et al., 2017; Alsan, Garrick and Graziani, 2019; Campos-Mercade et al., 2021; Chang et al., 2023; Duch et al., 2023*b*; Schneider et al., 2023; Serra-Garcia and Szech, 2023).[10] For example, in one of the earliest interventions incentivizing vaccination, Banerjee et al. (2010) studied the effects of offering small non-monetary incentives, including raw lentils and metal plates, on childhood immunisation in India. The study most closely related to the present paper is our previous experiment reported in Campos-Mercade et al. (2021), where we examined the

[9]Other examples include health screening (Stone et al., 2002; Lieberman et al., 2019; Jones, Molitor and Reif, 2024), sleeping (Avery, Giuntella and Jiao, 2019; Giuntella, Saccardo and Sadoff, 2024), organ donation (Becker and Elias, 2007), medication adherence (Barankay et al., 2020), and clinical trial participation (Halpern et al., 2011).

[10]Early research found that small incentives for childhood immunization and modest-sized incentives for flu and HPV vaccinations could be effective (Banerjee et al., 2010; Bronchetti, Huffman and Magenheim, 2015; Mantzari, Vogt and Marteau, 2015; Gibson et al., 2017; Alsan, Garrick and Graziani, 2019). More recently, the evaluations of various incentive interventions on vaccination have been more mixed. Research has documented a short-term positive impact of incentives on a first dose of the COVID-19 vaccine within relatively pro-vaccination samples (Campos-Mercade et al., 2021; Duch et al., 2023*b*). Conversely, Jacobson et al. (2022) and Chang et al. (2023) observed no statistically significant effect of incentives in more vaccine-hesitant groups. The results on the effects of the US state incentive programs on vaccination uptake are also mixed. While Barber and West (2022) and Cohn et al. (2022) report positive effects, Milkman et al. (2022) and Thirumurthy et al. (2022) found minimal effects, if any, of various city and state incentive programs. Common limitations of previous work include the lack of comprehensive administrative data on the full scope of vaccinations, the restriction to short-term effect analyses, the absence of individual-level data for examining treatment heterogeneities such as differences in vaccine hesitancy, the unavailability of measures for future vaccination uptake to assess potential adverse outcomes, and the lack of access to individually-linked data to investigate potential spillover effects.

impact of a ∼$24 guaranteed incentive on first-dose COVID-19 vaccination rates among a representative sample of the Swedish population.[11]

Campos-Mercade et al. (2021) also illustrates some of the key data limitations researchers typically face when studying the effects of incentives on health behaviors. Aside from the present paper, Campos-Mercade et al. (2021) offers one of the most comprehensive datasets, being the only other study based on a broadly representative sample with nation-wide administrative registries on individual behavior. However, despite the quality of the data, the study faces important limitations: it offered only one type of incentive, guaranteed incentives, and could only analyze effects within a 30-day window. This means the study was unable to test impacts beyond the very short run, identify potential spillover effects, or compare different types of incentives. Other common limitations include lacking survey data to understand whether incentives backfire for certain groups and complete data on behavior.

The limitations of previous studies leave important questions unanswered and raise concerns about external validity (Harrison and List, 2004; List, 2020), scalability (Al-Ubaydli, List and Suskind, 2017; DellaVigna and Linos, 2022), and the choice, accuracy, and comprehensiveness of outcomes for an accurate understanding of the overall effects (Athey et al., 2019). Consequently, despite recent advances in data quality, there is still much disagreement about the effectiveness of incentives for vaccination and health behaviors more generally as it remains unclear whether they backfire for some groups of people, are effective in the long-run, or have unintended consequences.

We build on and contribute to this literature by overcoming data limitations curtailing previous work, thereby studying the effects of incentives on a key health behavior in the most comprehensive way to date. We study the effects of differently structured incentives using a pre-registered RCT with access to a broadly representative sample, complete individual-level administrative data to examine short and long-run effects, rich survey data to study heterogeneous treatment effects, individually linked family members to study spillovers, and a highly relevant, carefully controlled setting. Based on the wealth of data and the extensive hetero-

[11]Closely related to the literature on incentives for health behaviors, an emerging literature examines the impact of how "nudges"—subtle changes in the choice structure or provision of information with no material change in incentives—affect health behaviors (see, e.g., Stutzer, Goette and Zehnder, 2011; Patel, Volpp and Asch, 2018; Milkman et al., 2021*b*; DellaVigna and Linos, 2022). In the context of vaccination, specific powerful messages (Milkman et al., 2021*b*), reminders (Milkman et al., 2011; Dai et al., 2021; Chang et al., 2023) and communication doctors' consensus (Bartoš et al., 2022) have been found to work, although nudges may at times be less effective than modest monetary incentives (Bronchetti, Huffman and Magenheim, 2015; Campos-Mercade et al., 2021).

geneity analyses—including machine learning analyses able to identify diverse groups—we believe our study provides strong evidence that incentives can be effective for everyone, also in the long-run.

We also contribute to a second strand of literature studying the effectiveness of different types of incentives (Gneezy and Rustichini, 2000; Bandiera, Barankay and Rasul, 2005; Mellström and Johannesson, 2008; Lacetera and Macis, 2010; Halpern et al., 2011; Gneezy and Rey-Biel, 2014; Imas, 2014; Tonin and Vlassopoulos, 2015; DellaVigna and Pope, 2018; Milkman et al., 2021*a*; Englmaier et al., 2024), often in the lab and in the context of effort provision. Previous work in this area focuses on understanding which kind of incentives are more effective on average.[12]

We move beyond studying average responses to incentives by studying, in the field, whether incentives can be more efficiently deployed by targeting them based on individual socio-demographics, attitudes, and preferences. To this end, we use incentivized and tailored measures of risk and social preferences which are closely related to the treatments. To assess the potential of targeting in a disciplined way, we use machine learning, exploiting in addition the many measures we collected on attitudes toward vaccination and personality. To further study the potential of targeting, we allow for unconstrained heterogeneity by studying whether self-selection, with a new treatment that allows people to choose their own incentives (see also Woerner et al., 2021 for a related intervention), can improve efficiency.

Finally, our machine learning approach relates to a nascent and rapidly growing literature using machine learning to examine heterogeneous treatment effects (Knaus, Lechner and Strittmatter, 2020; Chernozhukov et al., 2021; Wallace, 2023; Buyalskaya et al., 2023) and, exploiting the uncovered heterogeneities, to target treatment assignment (Cagala et al., 2021; Opitz et al., 2022; Athey et al., 2022). Our paper is among the first using machine learning in combination with economic preferences assessing the potential of targeting specific policies to certain people.

[12]One notable exception is Andreoni et al. (2023), who estimate time discounting among government health workers in Pakistan and use these estimates to design individually-tailored incentives. Their findings indicate that these tailored contracts outperform other conventional policy alternatives.

3 Experimental design, data and estimation

We carried out a pre-registered randomized controlled trial (RCT) involving a general population sample of Swedish residents. The trial took place in February 2022, coinciding with the initiation of booster vaccinations for most of the population in Sweden. By December 2023, we linked the RCT data with records from the Public Health Agency of Sweden and the Tax Agency of Sweden, with complete administrative vaccination records of the trial participants and their family members. The study protocols were approved by the Swedish Ethical Review Authority (Etikprövningsmyndigheten), and informed consent about the data collection was secured from all participants during the enrollment process.

3.1 Overview

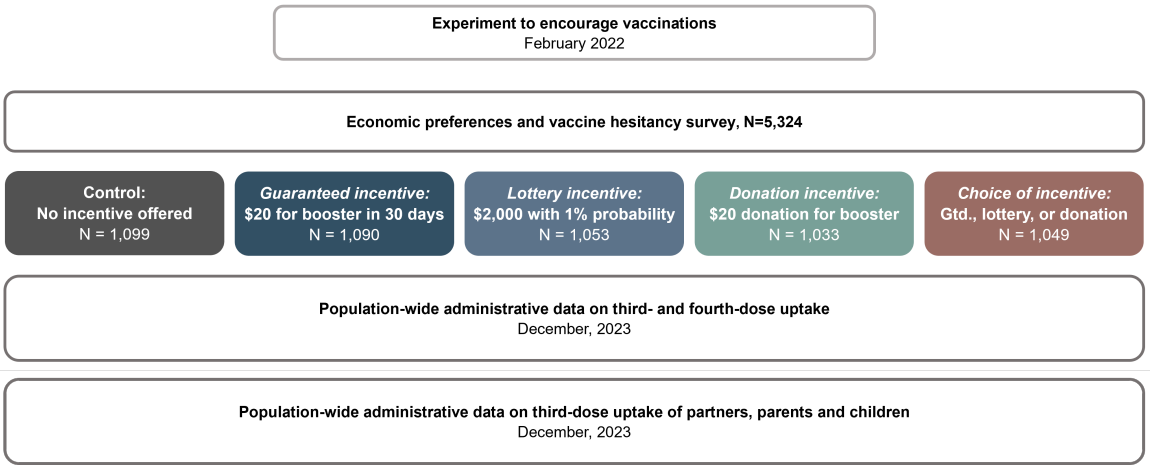
Figure 1 summarizes the experimental design. We ran the field experiment with 5,324 participants aged 18 to 64, recruited from a broadly representative online panel actively recruited by the company Norstat.[13] These age groups had already been recommended to take two doses of a COVID-19 vaccine in 2021 and at the time of the trial were recommended to take a first booster dose (their third dose). We carried out the experiment right as the booster dose became available, in February 2022. For the age groups of our participants, only the Pfizer-BioNTech and Moderna vaccines were used, which were offered free of charge.[14] Individuals could easily sign-up online or by phone for a third-dose appointment at their local vaccination center with typically no waiting time.[15]

[13]The company guarantees representativity in terms of age, gender, and region of residence. However, we find the sample to also mimic the general population in terms of the main measures gathered in the survey and from the registries, such as income, education, and vaccination rate.

[14]While the initial distribution of the first dose varied regionally, by July 2021, the vaccine was accessible to all adults. Individuals were advised to receive their second dose 3-4 weeks following the first. Additionally, a booster was recommended at least 90 days after the second dose. Consequently, everyone in the age groups we examined had the opportunity to receive their second dose well before getting the booster in February 2022. In practice, very few participants received their second dose less than 90 days prior to the experiment. Including these participants does not alter the results.

[15]The Swedish government implemented very few restrictions for people who had taken fewer than two vaccine doses and no restrictions for people who did not get a third dose. Restrictions for people with less than two doses only applied for indoor public gatherings and events, where people were required to show a vaccination certificate if there were more than 50 participants. Accordingly, our experiment shows the impacts of incentives for vaccination doses which are free, do not impact individual movement restrictions, and are administered in the context of essentially endemic disease. These are similar conditions as for other key vaccines such as the flu, measles, tuberculosis, human papillomavirus (HPV), and (potential) malaria vaccines.

Figure 1: Experimental design



Note: The figure summarizes the experimental design with the corresponding treatment groups and the linked experimental, survey, and population-wide administrative data. Using the participants' social security number, the Public Health Agency of Sweden matched the survey data with population-wide Swedish administrative records for vaccinations including for family members. As it is not possible to opt out of or delete records in the vaccination registry, the administrative records include the date of each COVID-19 vaccination of each Swedish resident. The main pre-registered preference measures were incentivized.

In a pre-screening questionnaire administered by Norstat, we allowed participation of only people who had taken the two initial doses and who were therefore recommended to take a booster dose.[16] After the pre-screening and consent form, we gathered participants' social security number (personnummer) and measured their socio-demographics, economic preferences, personality traits, and attitudes towards COVID-19 vaccines. We then randomly allocated them to either the control group or one of several incentive conditions for getting the booster dose of a COVID-19 vaccine within 30 days.

Using the participants' social security number, the Public Health Agency of Sweden matched the survey data with population-wide Swedish administrative records for vaccinations, which allows us to examine whether and when each participant got vaccinated. As the Public Health Agency matched the data in December 2023, we can examine long-run effects of incentives up to 660 days after RCT participation. The data includes the entire COVID-19 vaccination history, including fourth-dose uptake, allowing us to also study the long-run impacts of incentives on uptake of future vaccination when no financial incentives were offered anymore. Stating the social security number is common in Sweden and we detailed in the

[16] At the time, about 80% of the population aged 18 to 64 had taken the two initial doses of the COVID-19 vaccine. A few individuals enrolled even though they had not yet taken two doses or already had received a third dose. Our analysis does not include these participants, but all results are equivalent when including these groups (Appendix Tables B.6 and B.7).

consent form how the social security number will remain completely anonymous when linked to other data, even to us researchers.[17]

Finally, the Tax Agency of Sweden identified all registered or married partners, parents, and children of the study participants. We then also received registry data on third-dose vaccination uptake for these family members from the Public Health Agency. This data allows us to study spillover effects of incentives on family members who did not receive any incentive to vaccinate.

3.2 Experimental conditions and survey design

The survey consisted of four parts: consent form, main questionnaire on economic preferences and attitudes, experimental condition, and a final part to gather surrogate outcomes. See the complete questionnaire translated to English, including all questions and all interventions, in Appendix Section G.

Main questionnaire on economic preferences and attitudes. First, we elicited incentivized prosociality and risk preference measures using a dictator game and an investment game (based on Gneezy and Potters, 1997). In the dictator game, participants received an endowment of $20 and were asked how much of it to donate to the charity Save the Children. In the investment game, participants received an endowment of $20 and were asked how many lottery tickets they wanted to buy, where each lottery ticket costed $1 and had a 1 percent chance of paying $100 and a 99 percent chance of paying nothing. To incentivize these questions, for each game we randomly drew ten participants and implemented their choice.[18]

[17]The first two screens of the online experiment were hosted by the survey provider and asked participants about their social security number. The participants were then forwarded to the actual experiment hosted by us, with a newly constructed ID. We did not have access to the dataset containing the social security numbers, and the survey company did not have access to our dataset. Instead, the survey provider directly sent the dataset consisting of the social security number and the constructed ID to the Public Health Agency of Sweden. We then sent the experimental data that included the constructed ID to the Public Health Agency. The public health agency then linked the two datasets, linked this data to registry data, and then deleted the social security number and the newly constructed ID. We then received a completely anonymized dataset. The reason why we opted for this procedure was to guarantee that the social security number would never be stored together with the experimental data or the vaccination data, and to increase participants' trust when providing their social security number. Note that people in Sweden are used to providing their social security number on many occasions, including most online transactions and memberships.

[18]We tailored the preference measures to align with the interventions. For instance, as described below, the donation condition offered a $20 donation to Save the Children. We measured prosociality by offering participants the option to donate up to $20 to the same charity, guaranteeing a close match between preferences and treatments.

In addition, we elicited secondary preference measures using survey items. We measured altruism, reciprocity, trust, patience, and risk-affinity using experimentally validated measures from the Global Preference Survey (Falk et al., 2018). Moreover, we measured proxies for the tendency to procrastinate, self- and social-image concerns in the prosocial domain and extrinsic motivation, taken from the psychology literature (Tuckman, 1991; Amabile et al., 1994; Plant and Devine, 1998; Aquino and Reed II, 2002), as well as adherence to social norms and importance of autonomy using two questions from the Schwartz Value Survey. We proceeded by collecting attitudes toward the COVID-19 vaccine to capture vaccine hesitancy (worries about side effects, safety perceptions, knowledge about side effects), COVID-19 history (whether participants ever tested positive, were in a risk group, and worried about getting COVID-19) and socio-demographics.

Experimental conditions. After completing the main questionnaire, participants were randomly allocated to one of five experimental conditions. In the control condition, as in all other experimental conditions, we encouraged participants to vaccinate within 30 days of participating in the survey ("We would like to encourage you to get the third dose of a COVID-19 vaccine as soon as it becomes available to you, ideally within 30 days of taking part in this survey").

In addition to the message that was also part of the control condition, the four incentive conditions included rewards conditional on vaccination within 30 days after participating in the survey. In the guaranteed incentive condition, we offered participants the equivalent of $20 in Swedish kronor (SEK 200). In the lottery incentive condition, we offered participants a lottery ticket with a 1% chance of winning $2000 (the expected value is $20). In the donation incentive condition, we offered participants to make a donation of $20 to the charity Save the Children on their behalf. Finally, in the choice condition, we offered people the free choice of any of the three incentive schemes (guaranteed, lottery, or donation incentive). For example, if a participant chose the guaranteed incentive, she would receive $20 conditional on vaccination within the 30-day window.

We informed participants that we would verify their vaccination status and timing using administrative data and provide the promised reward if they got vaccinated within the 30-day window.[19] Additionally, we included a small text box in the survey where one of the

[19]Because the third dose was only recommended and available three months after the second dose, in all conditions, including the control group, we clarified that "if it has not yet been three months since you took

authors signed a guarantee that participants would receive the reward if they were vaccinated within the specified time frame.[20] Participants were also informed that we would match the experimental data with vaccination registries to facilitate the payments as soon as possible, but no later than July 2022. The payment was made using "Norstat cash," a system familiar to participants that allows them to redeem their earnings for vouchers valid at most major online and in-person retail stores in Sweden.

Surrogate outcomes: Intentions and clicking on an appointment link. After the intervention, we asked all participants about their intentions to get vaccinated. Participants were asked whether they thought that they would get a third dose of a COVID-19 vaccine within one month (Intention Vaccinate). At the very end of the questionnaire, we provided participants with a link to a website of their regional health authorities where they could receive information about how to sign up for a vaccination appointment. We recorded whether participants clicked on the link (Appointment Link Click). As these outcomes were measured after the assignment to the incentives conditions, they could be immediately affected by the condition.

3.3 Administrative vaccination records

We linked the experimental data with administrative data from national COVID-19 vaccination registers comprising all residents of Sweden. As it is not possible to opt out of or

the second dose, then we mean 30 days after the third dose becomes available to you." In the incentive conditions, we told participants that if it had been less than three months since the second dose, to pay the incentive we would instead consider the 30-day window after the third dose became available to them. For most participants this corresponds to the 30-day window after the trial. However, for 415 participants it had been less than three months since they received the second dose when they participated in the trial. Because the third dose was only recommended and available three months after the second dose, the 30-day incentive window for these participants started after the trial. For these participants, we consider the 30-day window that started three months after they got the second dose. This is a slight deviation from the pre-registration plan, where we made the mistake of considering the 30-day window after the trial for all participants, which was inconsistent with the experimental design. However, in Appendix Table B.5, we show that the results do not change if we instead follow this pre-registered approach or if we exclude participants who received the second dose less than three months before the study.

[20]One potential concern is that the share of participants that finish the survey differs across treatment conditions, for example, because the different conditions may imply different intellectual effort. However, 99% of the participants who encountered the treatments finished the survey and we find no meaningful differences across the treatments, indicating no meaningful treatment differences in sample selection. An advantage is that we asked for the social security number at the very beginning of the study. Hence, we can also match to administrative data all participants who dropped out when they encountered the treatments. In our main specification, we include all participants who encountered one of the conditions, even if they did not finish the study after that, to avoid any potential bias.

delete records in the vaccination registry, the administrative records include the date of each COVID-19 vaccination of each Swedish resident.

In the administrative data, we see whether and when each participant got vaccinated. The Public Health Agency of Sweden linked our trial data at the individual-level with the administrative data on December 19, 2023. As the trial ended on February 8, 2022, we observe for each participant whether and when they got a third COVID-19 vaccine shot within more than 22 months after trial participation. Our pre-registered main outcome variable corresponds to a dummy variable on whether participants got the third dose within the 30-day window (Third-dose uptake within 30 days). We similarly construct third-dose vaccination uptake within 60, 90, 180, 360, and 660 days after the 30-day window started. The data also allows us to measure fourth-dose uptake up to December 2023.

In collaboration with the Swedish Tax Agency, we used each participant's social security number to identify all children, parents, and registered and married partners of the trial participants based on Swedish registry data. The Swedish Tax Agency sent this data to the Public Health Agency, which matched it with vaccination records. This data includes the complete COVID-19 vaccination history of all family members, including the date of each dose of a COVID-19 vaccine the person received until December 2023. Moreover, the data includes sociodemographic information on age, gender, region and relationship to the trial participant (parent, child or partner). The Public Health Agency also matched this data with our experimental data. However, due to privacy regulations, they only added limited information about the related trial participant: treatment assignment, age, gender, region, and the vaccination history. This data allows us to study spillover effects of incentives on vaccination uptake within 30 days of family members who were not incentivized.

3.4 Data collection, exclusion criteria, and sample

The participants were recruited from a general population panel in Sweden by the survey company Norstat. Norstat actively recruits people via phone calls to create a representative panel in terms of age, region, and gender. In our case, we asked the company to recruit participants between the ages of 18 to 65. Participants were asked to fill out an online survey, and responses were collected between January 27, 2022 and February 8, 2022.

Participants were excluded from the trial if they had not yet received two doses of a COVID-19 vaccine, already received a third dose, or were not recommended to take a third

dose by the Public Health Agency of Sweden at the time of the trial. To do so, in the consent form we asked participants whether they had not yet received two doses of a COVID-19 vaccine, had already received a third dose, were pregnant, had previously experienced an allergic reaction that required hospital care, or had ever experienced a severe allergic reaction after they got a vaccine. Those who answered affirmatively in one of these questions were excluded from the experiment.

We obtained responses from 6,579 individuals to the online survey. We excluded 197 participants from the analysis that discontinued the survey before encountering the screen with one of the treatment or control conditions. Another 69 individuals saw the experimental intervention but did not complete the questionnaire, which we keep in the dataset to avoid bias. We excluded 195 participants who answered the survey more than once and were exposed to different treatment conditions. We further excluded 612 participants who according to the administrative records had already received a third dose when participating in the survey and 232 observations from participants who had not yet received the second dose of a COVID-19 vaccine at the time of the trial. Finally, we excluded 19 individuals that at the time of the trial were not yet 18 years old. Our final and main analysis sample includes 5,324 participants. Appendix Tables B.6 and B.7 show that our results do not change when using different sample inclusion criteria.

Descriptive statistics of the main trial sample are presented in Appendix Tables A.1 and A.2. In comparison with the Swedish population, our sample is broadly representative with respect to age, gender and region. However, we have an overrepresentation of individuals aged 26 to 35 years and an underrepresentation of individuals aged 46 and older (see Appendix Table A.2). In Appendix Table B.1, we show that results do not change when using sampling weights to adjust for the misrepresentation. In addition, we find that participants' socio-demographics are comparable across experimental conditions (see Appendix Table A.3).

For the dataset of family members, we focus on family members who were recommended to take the third dose of the COVID-19 vaccine within the 30-day window of the study. Specifically, these family members had not yet taken the third dose at the time of the study and received the second dose early enough to have the third dose available to them. The final dataset then includes 1,170 observations from partners, 714 from children and 1,331 from parents. Descriptive statistics of the partners, parents and children sample are presented

in Appendix Tables A.4, A.6, and A.5. Appendix Table A.7 shows family members' sociodemographics were also comparable across experimental conditions.

3.5 Estimation and pre-registration

We pre-registered our analyses in detail in the AEA RCT Registry (AEARCTR-0008906). To estimate treatment effects on vaccination uptake, we pre-registered using ordinary least squares (OLS) regressions exactly as follows:

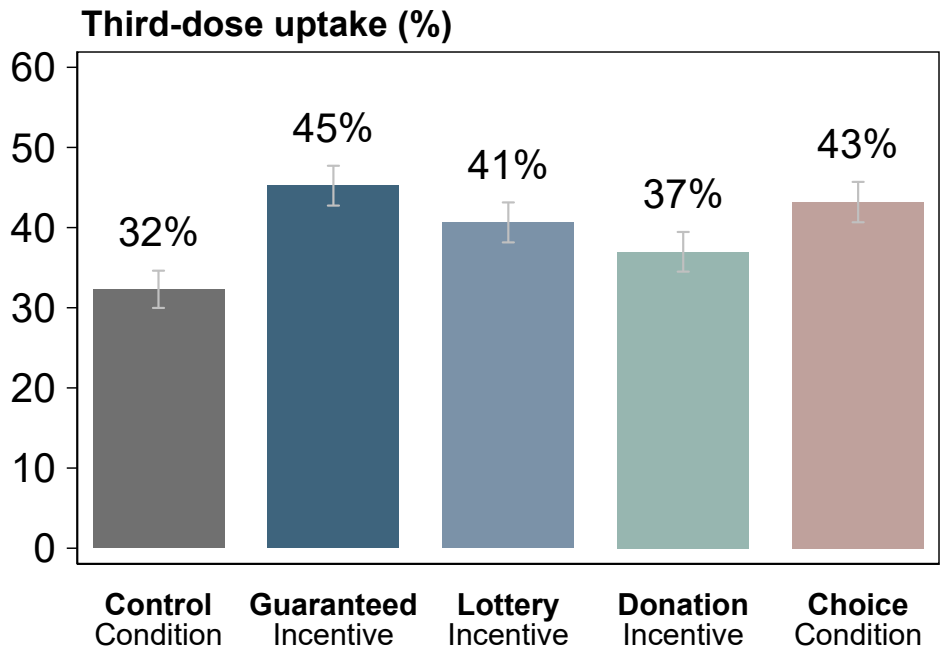
$$\begin{aligned}\text{Vaccination Outcome}_i = \beta_0 + \beta_1 \mathbb{1}(\text{“guaranteed”})_i + \beta_2 \mathbb{1}(\text{“lottery”})_i + \beta_3 \mathbb{1}(\text{“donation”})_i \\ + \beta_4 \mathbb{1}(\text{“choice”})_i + \beta_5 X_i + \epsilon_i\end{aligned}$$

where $VaccinationOutcome_i$ is the outcome of interest for participant i and $\mathbb{1}(\text{“treatment”})_i$ is a dummy variable capturing whether the participant was assigned to the specified treatment condition. The impact of each treatment relative to the control condition is captured by the corresponding β coefficient. X_i is a vector of pre-registered control variables consisting of gender, age groups in 5-year brackets, region, interactions between each age group and region, being in an at-risk group for COVID-19, civil status, having children in the household, employment status, education, parents' place of birth, and income. Finally, ϵ_i is an individual specific error robust to heteroscedasticity. We use an equivalent approach to identify spillover effects on family members, but cluster standard errors at the trial participant level. For family members, we only have sociodemographic information about gender, age and region, as well as information on vaccination history, for which we control in the same way as for participants.

Our pre-registration plan focused on our main analysis, which only included the analysis of the impact of the different incentive conditions on vaccination uptake within 30 days. We also pre-registered the study of heterogeneous treatment effects based on prosociality and risk preferences and the corresponding machine learning analysis. We did not pre-register the analysis of long-run effects, negative unintended consequences (fourth-dose uptake), and spillovers effects on family members, since there were uncertainties about whether and when this data would be shared with us. Hence, these analyses should be read as more exploratory. However, in analyzing this data we use the pre-registered specifications whenever possible.

4 Impact on vaccination in the short run

Figure 2: Incentives increase third-dose uptake within 30 days



Note: This figure displays the proportion of participants in the different conditions that got the third dose of a COVID-19 vaccine within the 30-day window in which the incentives were active ($N = 5,324$). The figure is based on survey data from the trial linked to Swedish administrative records on vaccination.

Our main outcome variable is whether participants got the third dose within 30 days according to the administrative records. Figure 2 displays the main results based on raw differences between treatment groups. The results show that guaranteed incentives increased vaccination uptake the most, followed by the choice condition and the lottery incentive. In the guaranteed condition, vaccination uptake within 30 days increased from 32.3% in the control group to 45.2% in the incentives condition—a relative increase of more than 40%.

Table 1, column (1), shows the corresponding regression estimates, using the pre-registered specification and controls. The regression results confirm the pattern in the raw data: Guaranteed incentives increased vaccination uptake by 13.02 percentage points (pp) ($p < 0.001$), lottery incentives by 8.45 pp ($p < 0.001$), the donation incentive by 4.66 pp ($p = 0.028$), and the choice condition by 10.90 pp ($p < 0.001$).

While the pre-registered main analysis focuses on the comparison between each of the incentive conditions and the control condition, we can also compare the impacts of the different incentive conditions. The guaranteed condition had the largest effect. Third-dose uptake after 30 days in the guaranteed condition is statistically significantly higher than

Table 1: Impact on short- and long-run third-dose uptake

Dependent Variable	Booster uptake within					
	30 days	60 days	90 days	180 days	360 days	660 days
	(1)	(2)	(3)	(4)	(5)	(6)
Guaranteed Condition	13.02***	9.95***	8.25***	7.30***	8.44***	8.58***
	(2.11)	(2.16)	(2.17)	(2.16)	(2.14)	(2.14)
Lottery Condition	8.45***	6.45***	6.73***	5.74***	7.26***	7.18***
	(2.12)	(2.17)	(2.18)	(2.16)	(2.14)	(2.14)
Donation Condition	4.66**	1.84	1.66	0.44	0.84	0.61
	(2.12)	(2.19)	(2.21)	(2.20)	(2.20)	(2.20)
Choice Condition	10.90***	9.01***	8.48***	6.72***	7.16***	6.83***
	(2.13)	(2.18)	(2.20)	(2.18)	(2.16)	(2.16)
Controls	yes	yes	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of short- and long-run third-dose COVID-19 vaccination uptake on indicators for the experimental conditions using the pre-registered set of controls. "Third-dose uptake X days" measures the proportion of participants who received the third dose within X days after the start of the incentive window. Heteroscedasticity robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

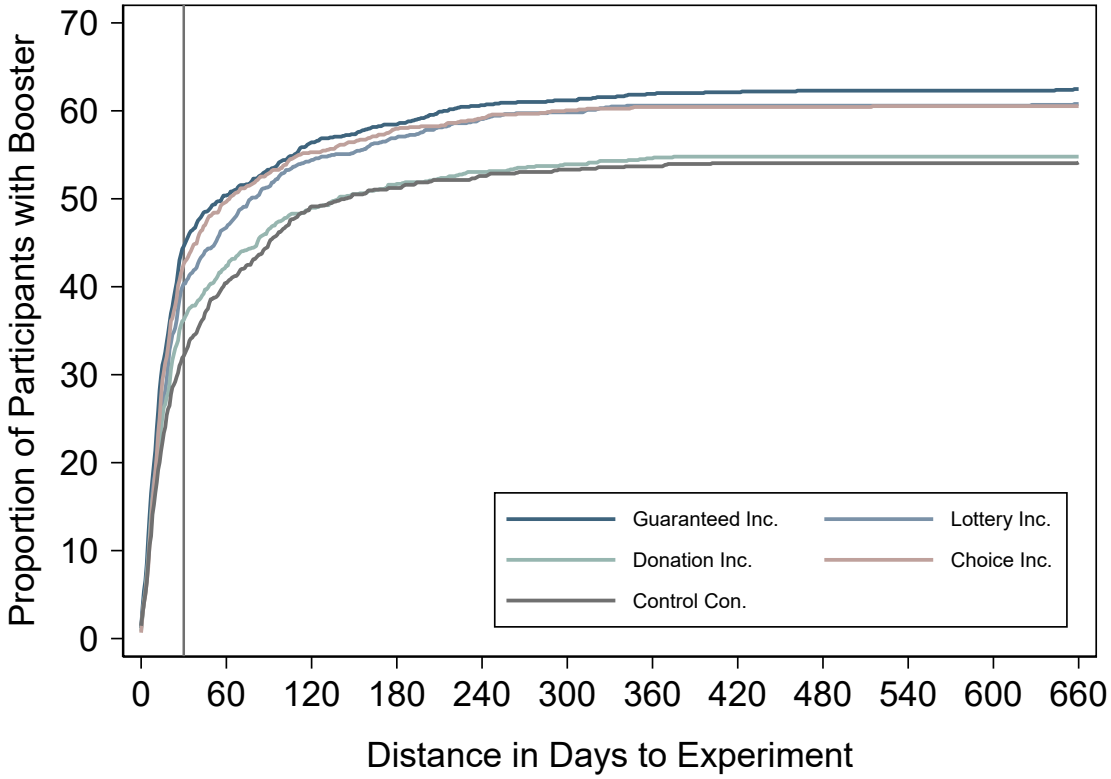
in the lottery ($p = 0.034$) and the donation conditions ($p < 0.001$), but not than in the choice condition ($p = 0.327$).[21] In the short-run, all incentives substantially boost vaccination uptake, with guaranteed incentives and offering a choice of incentive being the most effective.

5 Impact on vaccination in the long run

Table 1, in columns (2) to (6), provides results on long-run vaccination rates. We find that incentives not only accelerated vaccination uptake but increased vaccination rates in the long run. While the effects sizes get smaller in absolute terms—implying that part of the increase is due to an acceleration of vaccine uptake—they remain large and statistically significant for the guaranteed, lottery, and choice conditions. Even after 22 months, uptake remains 8.58 pp higher in the guaranteed condition ($p < 0.001$), 7.18 pp higher in the lottery condition ($p = 0.001$) and 6.83 pp higher in the choice condition ($p = 0.002$). In the donation condition

[21] We also find higher third-dose vaccination rates of the lottery vs. the donation condition ($p = 0.082$) and the choice vs. donation condition ($p = 0.004$), but no statistically significant difference between the lottery and the choice condition ($p = 0.260$).

Figure 3: Incentives increase third-dose uptake in the long-run



Note: The figure shows the development of the proportion of participants who got the third dose of a COVID-19 vaccine over time in the different treatment conditions (Kaplan-Meyer curves). The figure is based on survey data from the trial linked to Swedish administrative records on vaccination where which includes the date of when each participant got vaccinated ($N = 5,324$).

the estimated treatment effect after 22 months is only 0.61 pp, which is not statistically different from zero ($p = 0.781$).

Figure 3 shows vaccination rates relative to trial participation in all treatment groups (Kaplan-Meyer curves). The figure further illustrates a persistent increase of vaccination rates in the guaranteed, lottery and choice condition, but not in the donation condition. There is an initial increase in vaccination rates in the donation condition, but the control condition catches up within 2 months after trial participation. For the other treatments, initial increases translate to persistent differences in vaccination rates with the guaranteed condition dominating the other conditions throughout.

We find consistent treatment effects on days to vaccinate (the number of days that it takes for the participant to get the third dose after filling out the survey) using a Tobit regression. We estimate that, in comparison to the control condition, the guaranteed incentives, lottery, donation, and choice conditions reduced days to vaccinate by 92.1 ($p < 0.001$), 73.7 ($p = 0.001$), 3.6 ($p = 0.875$) and 71.8 ($p = 0.002$), respectively (Appendix Table B.2).

Taken together, these results show that the guaranteed incentives are particularly effective in both accelerating and permanently increasing vaccination uptake. The lottery condition and the choice condition are also effective, particularly in the long run, while the donation condition is less effective in the short run and do not lead to an increase in vaccination rates in the long run. These results are robust to a battery of robustness checks, such as including different sets of control variables, using different sample inclusion criteria, using sample weights, and using logistic regressions (Appendix Section B).

6 Behavioral targeting: Using preference heterogeneity to increase effectiveness

The previous section shows that guaranteed incentives are, on average, particularly effective in increasing vaccination rates. In this section, we first study whether the impact of interventions varies across different groups. Examining heterogeneous treatment effects allow us to address concerns raised in the literature that incentives backfire for certain groups of people, particularly the vaccine hesitant and prosocial individuals. We then study whether heterogeneity based on preference measures, socio-demographics, and vaccination attitudes, can be used to target policies in a way that increases overall vaccination rates beyond the vaccination rates in the guaranteed condition. Identifying such differential responses to various incentive types is important for policy design. If some incentive schemes would be more effective for specific groups of people, then the efficiency of health policies could be increased by targeting certain incentive schemes to the most responsive groups. We finally study the choice condition to explore how people self-select into the different incentives schemes and the impacts of self-selection on uptake.

6.1 Heterogeneous treatment effects

In this section, we discuss heterogeneous treatment effects. We expected the donation condition to be particularly effective among prosocial individuals and the lottery condition to be particularly effective among risk-taking individuals. We pre-registered the two variables as the main variables of interest for the heterogeneity analyses.

We find substantial preference heterogeneity (see Appendix Figure C.1 for the distributions). For instance, while 38.6% donated the maximum amount to Save the Children in the dictator game, 17.6% of participants chose not to share any funds with the charity. Similarly, in the investment game, 28.2% opted to invest the maximum amount into risk lottery tickets, whereas 36.9% decided not to invest any funds. Thus, there exists considerable heterogeneity in motives to act, which could potentially be used to target policies.

Table 2 examines heterogeneous treatment effects based on prosociality and risk-taking. We use OLS and interact indicator variables for the incentive conditions with the preference measures, as pre-registered. We do not find that the lottery condition is more effective for risk-taking individuals. However, we do see that the donation condition is more effective among prosocial individuals relative to the less prosocial individuals, both in the short- and long-run. The estimates from column (1) suggest that the treatment effects of the donation condition on third-dose uptake within 30 days for a maximally prosocial individual (prosociality = 200) is 9.41 pp ($p = 0.004$), whereas for a minimally prosocial individual (prosociality = 0), it is -2.19 pp ($p = 0.543$). Prosociality has no impact on the effectiveness of the guaranteed, lottery, and choice conditions. Appendix Tables C.1 to C.8 provide estimates of heterogenous treatment effects for other preferences measures, vaccine attitudes, COVID-19 history, and socio-demographics. We find that there are no substantial heterogeneous treatment effects for these other characteristics.

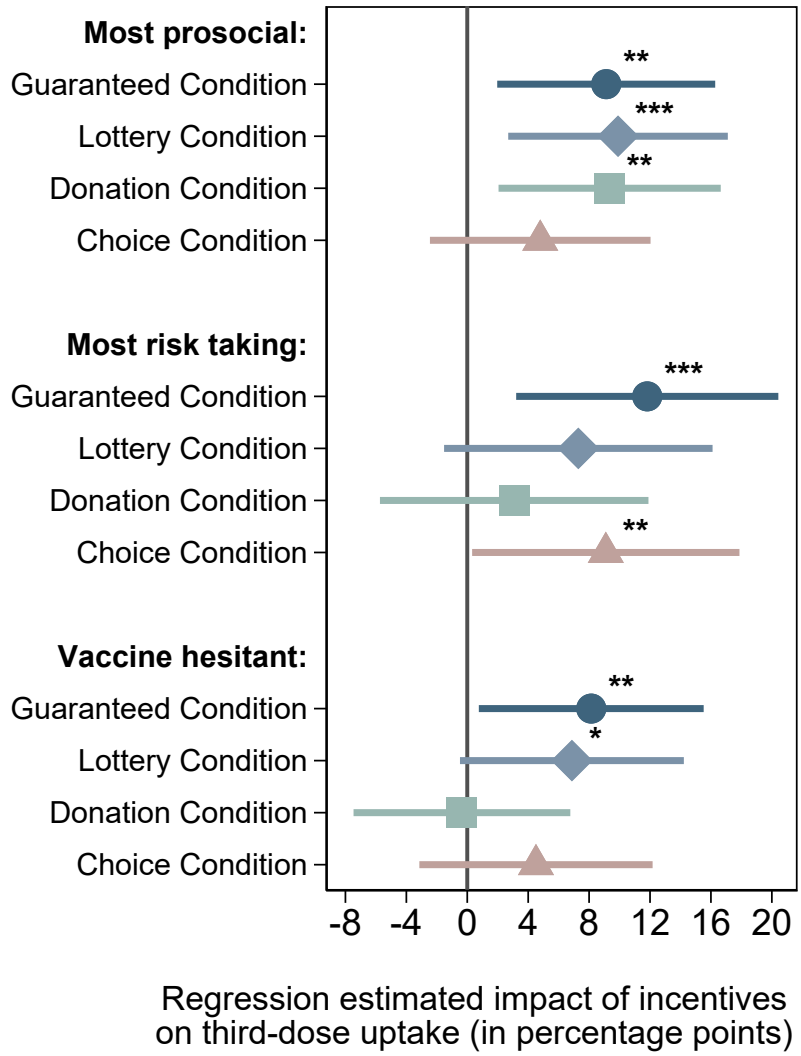
Figure 4 gives the estimated treatment effects when we restrict the sample to the most prosocial, risk-taking, and vaccine hesitant individuals. The figure shows that, contrary to concerns raised in the literature, none of the incentive types reduce vaccination rates among the most prosocial, most risk taking, or most vaccine hesitant individuals. We conclude that incentives have the potential to raise vaccination rates irrespective of people's economic preferences, vaccine attitudes, and socio-demographics.

Table 2: Heterogeneous treatment effects depending on prosociality and risk taking

Dependent Variable	Third-dose uptake within 30 days		Third-dose uptake within 660 days	
	(1)	(2)	(3)	(4)
Guaranteed Condition x Prosociality	-0.015		0.014	
	(0.028)		(0.028)	
Lottery Condition x Prosociality	0.025		0.031	
	(0.027)		(0.027)	
Donation Condition x Prosociality	**0.058****		**0.066****	
	(0.027)		**(0.028)**	
Choice Condition x Prosociality	-0.054**		-0.032	
	(0.028)		(0.028)	
Guaranteed Condition x Risk taking		-0.003		0.012
		(0.026)		(0.026)
Lottery Condition x Risk taking		**0.024**		**0.019**
		(0.026)		**(0.026)**
Donation Condition x Risk taking		-0.010		0.005
		(0.026)		(0.027)
Choice Condition x Risk taking		-0.026		-0.025
		(0.026)		(0.026)
Guaranteed Condition	12.944***	12.979***	8.354***	8.499***
	(2.120)	(2.120)	(2.140)	(2.143)
Lottery Condition	8.263***	8.306***	6.967***	7.058***
	(2.122)	(2.125)	(2.140)	(2.145)
Donation Condition	4.421**	4.655**	0.334	0.587
	(2.116)	(2.128)	(2.188)	(2.201)
Choice Condition	10.749***	10.824***	6.678***	6.769***
	(2.130)	(2.131)	(2.162)	(2.163)
Controls	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days and within 660 days (the end of the follow-up period) on indicators for the experimental conditions. We de-meaned prosociality and risk taking (mean prosociality = 114, mean risk taking = 84) such that Guaranteed Condition, Lottery Condition, Donation Condition and Choice Condition give estimated treatment effects for participants with mean prosociality and risk preferences. Heteroscedasticity robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 4: Impacts on prosocial, risk taking and vaccine hesitant individuals



Note: This figure displays the coefficient estimates and corresponding 95% confidence intervals from OLS regression on third-dose uptake within the 30-day window in which the incentives were active using the pre-registered set of controls. The figure is based on survey data from the trial linked to Swedish administrative records on vaccination. "Most prosocial" includes all participants that, in the dictator game, donated the entire endowment of SEK 200 to Save the Children (Prosociality=200, $N = 2,056$, 38.6% of the sample). "Most risk taking" includes all participants that, in the investment game used the entire endowment of SEK 200 to buy lottery tickets (Risk taking=200, $N = 1,501$, 28.2% of the sample). "Vaccine hesitant" includes all participants that agree with the statement that "I am worried about the side effects from COVID-19 vaccines." ($N = 1,510$, 28.4% of the sample). $*p < 0.10, **p < 0.05, ***p < 0.01$

6.2 Using machine learning to target policies

The heterogeneity analyses based on the pre-registered main interactions suggest that targeting specific incentives to particular groups may not substantially increase vaccinations. For example, while the donation condition is more effective among prosocial individuals, the estimates in column (1) of Table 2 predicts an even larger treatment effect for these individuals if they were allocated to the guaranteed condition. Figure 4 shows that the guaranteed con-

dition is indeed particularly effective even among the most prosocial and most risk-taking individuals.[22] However, potential heterogeneities in interactions between variables or non-linearities might go unnoticed with simple interactions. Next, we allow for more flexibility using tree-based policy learning based on causal forest estimates.

We aim to answer the following question: Based on everything we know about individuals, can we increase vaccination uptake by assigning the most responsive individuals to specific incentive schemes? Causal forests allow us to estimate highly non-linear heterogeneities. Policy trees then allow us to study whether the resulting heterogeneities are substantial enough for targeting specific incentive schemes to particular groups with policy learning (Knaus, 2022; Athey and Wager, 2021; Zhou, Athey and Wager, 2023). One benefit of causal forests is their use of out-of-sample estimation, allowing for the assessment of heterogeneous treatment effects derived from multidimensional, non-linear interactions with consistent standard errors.

Our findings are in line with the OLS analyses: Heterogeneities, if present, are not large enough to allow effectively targeting incentive policies to well-defined groups. Accordingly, we cannot increase overall vaccination uptake in comparison to assigning everyone to the guaranteed condition. This holds even when we use all information from socio-demographics, preferences, and variables capturing vaccination attitudes and risk-group status.

The policy trees assign policies maximizing uptake based on heterogeneous treatment effects. We estimate the policy trees using the algorithm developed by Wager and Athey (2018) as implemented in Knaus (2022). Using policy trees of depth two with five-fold cross-validation and all variables for targeting increases the vaccination rate by only 0.4 percentage point within 30 days relative to the guaranteed condition ($SE = 1.2, p = 0.71$). The only treatment that shows overall statistically significant heterogeneity in a causal forest vs. the guaranteed incentive treatment is the donation treatment ($p < 0.01$), the other arms do not show any statistically significant overall heterogeneity vs. the guaranteed incentive (lottery $p = 0.77$, choice $p = 0.89$, p-values from omnibus tests). As the linear regression results have already shown, prosociality plays a substantial role for explaining heterogeneity in treatment

[22]One potential explanation for our inability to effectively increase vaccination uptake by considering diverse motivations is that our elicited preferences may not accurately capture prosociality and risk preferences, or they may be subject to too much noise. To partially address this concern, we tailored the preference measures to align with the interventions. For instance, in the donation condition, receiving a vaccine would result in a SEK 200 donation to Save the Children, and we measured prosociality by offering participants the option to donate up to SEK 200 to the same charity. Furthermore, the choice condition further addresses this concern because it is not based on preferences elicited by the experimenter (see section 6.3).

effects for this treatment. Otherwise, there is limited heterogeneity that can be explained by covariates consistent with the results from policy learning.

Hence, while preference variables play some role in explaining heterogeneous treatment effects, they do not allow to target incentives in a way that would substantially increase overall vaccination rates in the short- or long-run (for more details, see Appendix Section D). In sum, there is no evidence that observable factors explain significant heterogeneity in treatment effects that would allow to effectively target policy interventions in comparison to simply assigning everyone to the guaranteed condition.

6.3 Self-targeting: Letting people choose the incentive they like

The choice condition allows individuals to sort into incentive schemes based on their preferences, potentially leveraging preference heterogeneity beyond what we measure. Assuming that individuals have a good understanding of their own preferences, giving people a choice could address imperfect measurement of preferences and functional form limitations of statistical models.

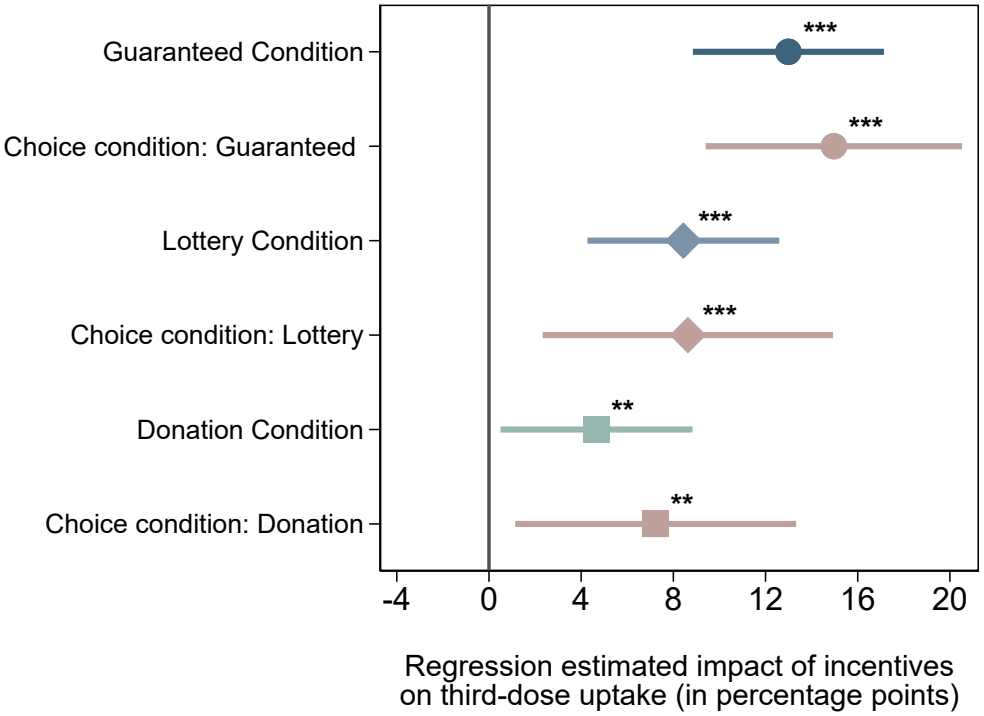
We find substantial diversity in choices: 42.3% of participants select the guaranteed payment, 28.0% opt for the lottery payment, and 29.6% choose the donation (see Appendix Figure C.2). Additionally, we find that prosociality and risk-taking predict the choices very well (see Appendix Figures C.3 and C.4). Although these results suggest that there is preference heterogeneity and that individuals are able to self-select based on their preference, we do not find self-selection to increase vaccination uptake relative to the guaranteed condition (see Figure 2 and Table 1).

When combining the data from the guaranteed, lottery, and donation schemes, and then comparing booster dose uptake within 30 days in the pooled data with that in the choice condition, we estimate an increase of 1.65 percentage points in the choice condition. However, this increase is not statistically significant (OLS regression, $N = 4,225$, $p = 0.356$). When we weight the pooled data to reflect the distribution of choices in the choice condition, the estimated increase in vaccination uptake is 1.17 percentage points ($SE = 0.65, p = 0.513$).

Figure 5 presents booster dose uptake within 30 days in the choice condition conditional on the choice of incentive scheme (relative to the control condition).[23] These estimates should

[23]Appendix Figure C.5 replicates Figure 5 for third-dose uptake after 660 days.

Figure 5: Third-dose uptake within 30 days conditional on choice in choice condition



Note: This figure displays the coefficient estimates and corresponding 95% confidence intervals from OLS regression on third-dose uptake within the 30-day window in which the incentives were active using the pre-registered set of controls. The figure is based on survey data from the trial linked to Swedish administrative records on vaccination. $*p < 0.10, **p < 0.05, ***p < 0.01$

be interpreted with caution as the non-random assignment of the incentive scheme means the observed effects are not purely causal. The figure also provides a comparison to the uptake in the corresponding condition where incentives were assigned exogenously. The figure illustrates that vaccination uptake of individuals self-selecting into a condition closely resembles vaccination uptake of those randomly allocated to the same condition. We find no significant difference in third-dose uptake between the Guaranteed condition and choosing the guaranteed incentives in the choice condition ($p = 0.493$), the Lottery condition and choosing the lottery incentive in the choice condition ($p = 0.952$), and the Donation condition and choosing the donation incentive in the choice condition ($p = 0.414$). Therefore, the donation incentives also do not appear to be particularly effective among individuals who choose them.

We conclude that guaranteed incentives dominate lottery and prosocial incentives, even for the most prosocial and risk-taking individuals. Both machine learning and the choice condition confirm that we cannot gain much from assigning specific types of people to different incentive schemes.

7 Negative unintended consequences: Impact on future vaccinations

A major concern with monetary incentives is that even when effective, they may have negative unintended consequences ultimately reducing long-run healthy behaviors. On the other hand, monetary incentives may also have positive impacts on future health behavior, for example through habit formation or by emphasizing the social importance of vaccination. Our experiment allows us to test for the existence of such negative or positive unintended consequences by looking at long-run fourth-dose uptake.[24]

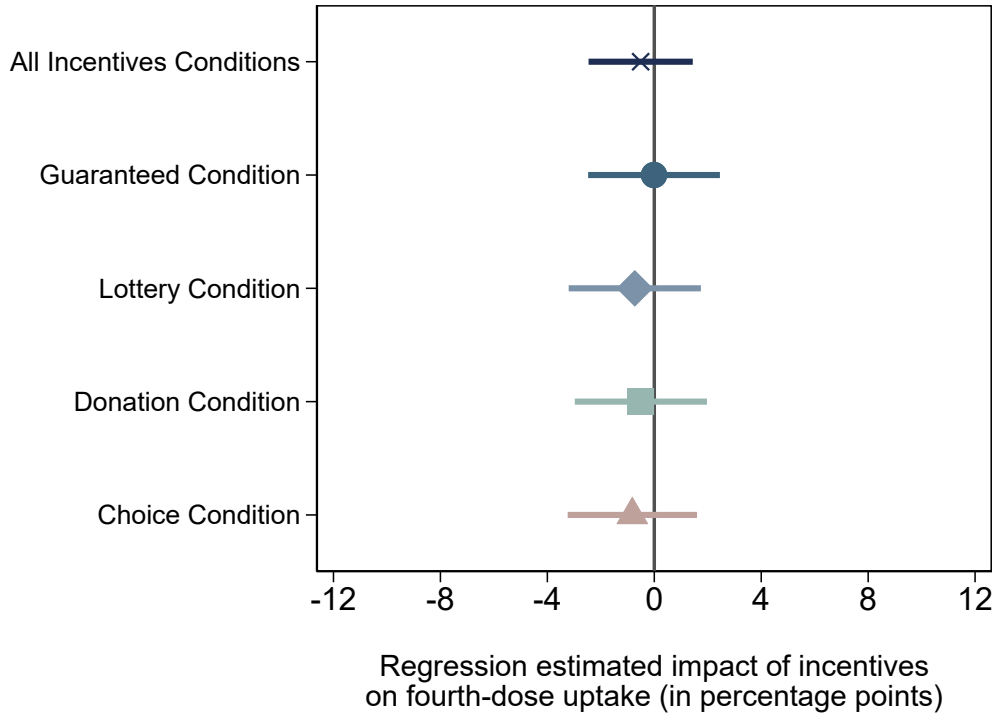
Importantly, taking a fourth-dose is a novel decision participants take after taking a third dose. In the general population, there were no restrictions in place if one did not receive a fourth dose, no automatic follow-up appointments, and no automatic reminders based on third-dose uptake. This is consistent with the large variance in the time between third and fourth dose uptake in the control groups, with the distance in days between the two doses in the control group ranging from 90 to 660 days ($avg. = 305$, $sd = 142$).

Nearly two years after the intervention, 9.5% of control group participants received a fourth dose. Accordingly, there is scope for the incentive conditions to affect vaccination rates. Figure 6 gives the treatment effects of the different conditions on fourth-dose vaccination rates (Fourth-dose uptake): The figure shows no consequences, positive or negative, on fourth-dose uptake. The estimated treatment effects in the guaranteed, lottery, donation, and choice conditions are -0.01 pp ($p = 0.99, 95\% - CI : [-2.47, 2.46]$), -0.73 pp ($p = 0.56, 95\% - CI : [-3.20, 1.74]$), -0.50 pp ($p = 0.69, 95\% - CI : [-2.97, 1.97]$) and -0.82 pp ($p = 0.506, 95\% - CI : [-3.24, 1.60]$), respectively. When we pool the data from all four incentives condition, the estimated treatment effect is -0.51 pp ($p = 0.608, 95\% - CI : [-2.46, 1.44]$).[25] Note

[24]Only a limited number of studies have explored the unintended consequences of vaccination incentives. One exception is Schneider et al. (2023), which follows up on the experimental participants in Campos-Mercade et al. (2021) to investigate whether incentives had negative unintended consequences. This paper mainly focuses on survey measures, including self-reported third-dose uptake, moral values, and risk perceptions, and finds no evidence of negative unintended consequences. Similarly, Bronchetti, Huffman and Magenheim (2015) found that flu vaccine incentives positively affected the intention to receive further doses, and Mantzari, Vogt and Marteau (2015) found that incentives for the HPV vaccine did not adversely impact vaccine attitudes and knowledge. Unlike these studies, which rely on survey measures, the present paper uses registry data on future booster dose uptake. This allows us to better assess whether incentives backfired in the long run and influenced future non-incentivized vaccination decisions up to almost two years after the intervention.

[25]Given that our intervention had long-run effects on uptake of the third dose, the fact that there were no treatment effects on fourth-dose uptake means that the probability of receiving the fourth dose, conditional

Figure 6: No impact on fourth-dose uptake



Note: This figure displays the coefficient estimates and corresponding 95% confidence intervals from OLS regression on fourth-dose uptake using the pre-registered set of controls. The figure is based on survey data from the trial linked to Swedish administrative records on vaccination. "Fourth-dose uptake" indicates the proportion of participants who got the fourth-dose by December 2023 ($N = 5,324$). "All incentives conditions" denotes the estimate when the guaranteed, lottery, donation and choice conditions are pooled. $*p < 0.10, **p < 0.05, ***p < 0.01$

that our estimates are rather precise, allowing us to reject substantial negative (or positive) consequences.[26]

Appendix Tables E.1 and E.2 show that the treatments also had no statistically significant impact on the timing of getting the fourth dose, neither on fourth-dose vaccination rates at different points in time, nor on the time when people got the fourth dose. Appendix Figure E.1 gives the proportion of participants who got the fourth dose in the different treatment conditions (Kaplan-Meyer curves).

We conclude that financial incentives had no substantial impacts on adherence to vaccination schedules in the long-run. This finding alleviates worries in the literature that incentives could have large negative unintended consequences. At the same time, there is no evidence for

on having received the third dose, is lower in the incentive conditions. While these point estimates are indeed negative as expected, they are not statistically significantly different from zero.

[26]For example, the standard deviation of fourth-dose uptake is 28.7. Hence, the threshold for a small effect size according to Cohen's d would be 5.74 pp. None of the 95% confidence intervals crosses this threshold.

the alternative theories that incentives may have positive consequences on future vaccination behavior.

8 Behavioral spillovers of incentives on family members

In this section, we examine potential behavioral spillovers from offering financial incentives to family members who were not exposed to incentives. Specifically, we consider booster dose uptake among partners, parents, and children of trial participants.

One reason for expecting spillover effects to family members is the sharing of vaccination experiences within the family. However, given that our sample consists of trial participants who have already received two doses of the COVID-19 vaccine, and thus have prior vaccination experience, we expect this factor to play a minor role in our context. A more important reason for potential spillovers seems the coordination among family members to schedule vaccinations together. Indeed, we see that 30.2% of trial participants received the first dose on the same day as their partners. However, the percentage was much lower for coordination with parents (2.6%) and children (1.4%), likely due to differing vaccination schedules across age groups.[27]

Table 3, in columns (1) and (2), gives the estimated treatment effects on third-dose uptake within 30 days for partners. Participants whose partners were in the guaranteed, lottery, donation and choice condition are 7.63 pp ($p = 0.093, 95\% - CI : [-1.28, 16.54]$), 8.13 pp ($p = 0.084, 95\% - CI : [-1.10, 17.36]$), 6.24 pp ($p = 0.188, 95\% - CI : [-3.05, 15.53]$) and 6.21 pp ($p = 0.181, 95\% - CI : [-2.90, 15.33]$) more likely to take the third dose, respectively. Pooling data from all four incentive conditions, third-dose uptake is 7.05 pp higher than in the control condition ($p = 0.048, 95\% - CI : [0.054, 14.04]$). Although rather imprecisely estimated, the spillover effects on partners are large with around half the size of the direct effects. Appendix Table F.1 confirms the robustness of these findings to different sets of controls.

[27] We see a similar pattern when we look at second and third-dose uptake (see Appendix Tables A.4, A.6, and A.5). We also note that we do not find that children and parents coordinate on different doses. For example, only 1.68% of RCT participants got the second dose the same day that one of their children got the first dose and only 2.1% of RCT participants got the first dose the same day one of their parents got the second dose. An additional reason why we see little coordination between parents and children is that they live in different areas. While most of the trial participants live in the same area as their partners (93.4%), far fewer lived in the same area as their parents (44.7%) or children (60.6%).

We also find that vaccination rates among partners of the incentivized participants remain higher in the long-run—for example, pooling the data from the incentives condition, vaccination rates after 660 days are 3.50 pp higher than in the control condition ($p = 0.356$)—but are no longer statistically significantly different from zero (see Appendix Table F.2). Therefore, a sizable part of the impact on vaccination uptake likely comes from accelerating vaccination.

Table 3 indicates no statistically significant effects on third-dose vaccination uptake of children and parents. Appendix Tables F.3 and F.4 also show no impact on long-run vaccination rates. One likely explanation for the different findings for partners vs. children and parents is that study participants coordinated getting the third dose only with their partners, but not with their children and parents.

Table 3: Impact on third-dose uptake of family members

Dependent Variable	Third dose within 30 days of					
	Partners		Parents		Children	
	(1)	(2)	(3)	(4)	(5)	(6)
All Incentives Conditions	7.05**		-4.05		-0.29	
	(3.56)		(3.86)		(3.91)	
Guaranteed Condition		7.63*		-2.90		-5.48
		(4.54)		(4.66)		(4.83)
Lottery Condition		8.13*		-5.22		3.74
		(4.70)		(4.96)		(5.12)
Donation Condition		6.24		-4.06		-3.35
		(4.73)		(4.85)		(5.16)
Choice Condition		6.21		-4.28		3.58
		(4.65)		(4.83)		(5.42)
Controls	yes	yes	yes	yes	yes	yes
Observations	1,170	1,170	1,331	1,331	714	714

Note: The table focuses on the partners, parents and children of the trial participants. The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days of the trial on indicators for the experimental conditions. Controls include age, gender, date of second dose and region. Standard errors are clustered at the trial participant level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

9 Surrogate outcomes: Intentions to vaccinate and clicks on an appointment link

In addition to data on actual vaccination uptake, we also collected information on vaccination intentions. Many studies examining the effectiveness of incentives and other interventions on health behaviors do not have access to administrative data on actual behavior but rely only on self-reported intentions (for recent meta studies, see Batteux et al., 2022; Huang, Huang and Yu, 2023; Khazanov et al., 2023). This work rests on two related assumptions: first, that vaccination intentions accurately reflect actual vaccination uptake, and second, that they indicate the treatment effects of interventions. Our unique combination of survey and administrative data allows us to test both of these assumptions.

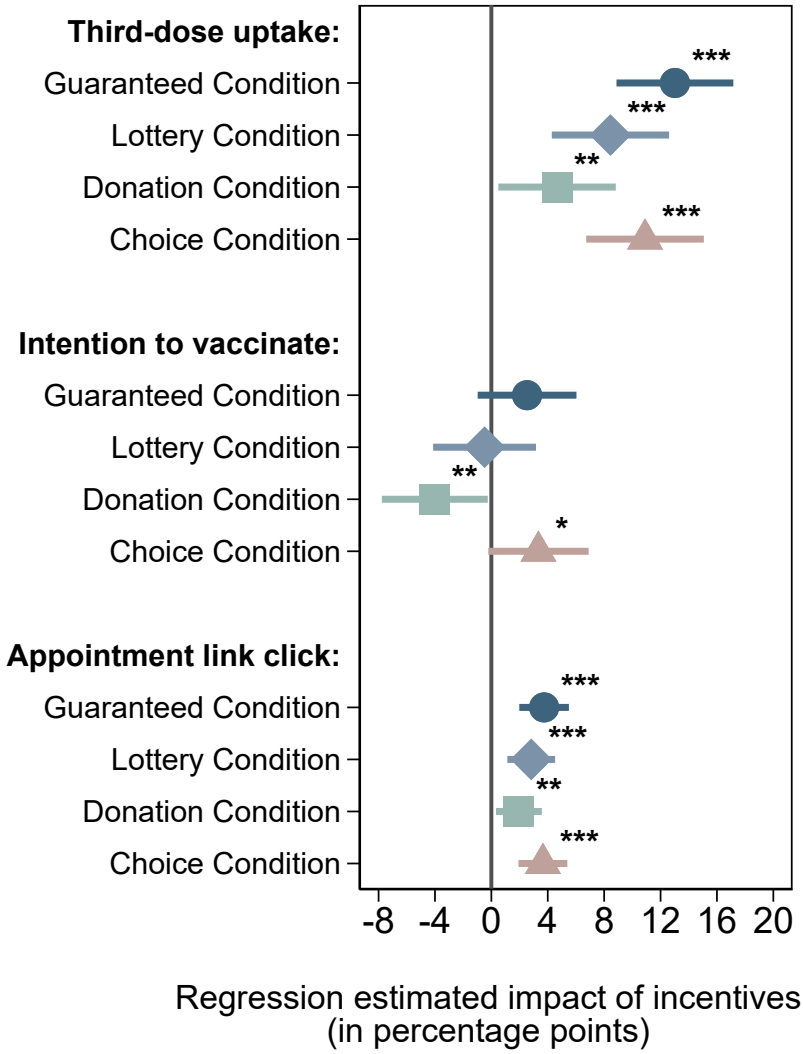
First, our analysis confirms that vaccination intentions do predict actual vaccination uptake. For instance, participants in the control condition who express an intention to get vaccinated within 30 days of vaccine availability are 34.7 percentage points more likely to actually vaccinate within that timeframe than participants who report no intention to vaccinate ($p < 0.001, 95\% - CI : [30.4, 39.1]$).

However, when we compare treatment effects on vaccination intentions and treatment effects on vaccination uptake, we find large differences, as depicted in Figure 7 and Table 4. Despite observing large treatment effects on vaccination uptake in the guaranteed and lottery conditions, focusing on intentions would suggest only small and not statistically significant effects of 2.5 pp ($p = 0.157, 95\% - CI : [-0.1, 6.0]$) and -0.5 pp ($p = 0.796, 95\% - CI : [-4.1, 3.2]$), respectively. For the donation condition, we would even conclude that incentives decrease vaccination uptake by 4 pp ($p = 0.036, 95\% - CI : [-7.8, -0.25]$). Thus, the treatment effects on intentions are far from reflecting the treatment effects on actual vaccination uptake.

This discrepancy may seem puzzling, given the high correlation between intentions and uptake. What explains this pattern is that the intervention changed the relationship between intention and actual uptake; individuals in the incentives condition who intended to vaccinate were more likely to follow through with this plan. To study this this, we again consider the difference in vaccination uptake between individuals that intend to vaccinate and individuals that do not intend to vaccinate. Compared to the control group (34.7 pp), this difference is statistically significantly higher in the guaranteed condition (difference = 52.7 pp, p-value

from comparison with control group < 0.001), lottery (42.9 pp, $p = 0.015$), and choice (49.4 pp, $p < 0.001$) conditions. The difference is also higher in the donation condition, although not statistically significantly (39.1 pp, $p = 0.196$). These findings suggest that incentives prompted participants who intended to vaccinate but would not have followed through, thereby reducing the intention-behavior gap (Sheeran, 2002; Milkman et al., 2011; Rogers et al., 2015). Overall, these findings indicate that vaccination intentions cannot reliably be used to study treatment effects, even when strongly correlated with actual vaccination uptake.

Figure 7: Treatment effects on actual uptake and intentions



Note: This figure displays the coefficient estimates and corresponding 95% confidence intervals from OLS regression on different outcomes using the pre-registered set of controls. The figure is based on survey data from the trial linked to Swedish administrative records on vaccination. "Third-dose uptake" considers third-dose uptake within the 30-day window in which the incentives were active as outcome. "Intention vaccinate" considers intentions to vaccinate as outcome measure (response to the question "Do you think you will get an additional shot of a COVID-19 vaccine within the first month after the vaccine becomes available to you?", collected as No/Yes and coded as 0/100). "Appointment link click" considers whether the participant clicked the link to get information on how to make a vaccination appointment as outcome measure (0/1, coded as 0/100). $*p < 0.10, **p < 0.05, ***p < 0.01$

We collected another secondary measure capturing actual behavior: whether, at the end of the survey, participants clicked on a link to get information on how they can book an appointment to get the vaccine. Unlike vaccination intentions, this measure captures a costly behavior (albeit with low effort costs). Figure 7 and Table 4 suggest that this behavioral outcome is more reliable in qualitatively capturing actual treatment effects. While the treatment effects on clicking on the link are smaller than those on actual vaccination uptake, the treatment effects are statistically different from zero and result in the same ranking of treatment conditions in terms of effectiveness as when we consider vaccination uptake. Taken together, this evidence indicates that researchers should be very careful when extrapolating from intentions to actual treatment effects and that costly behavioral measures may be the preferred alternative in the absence of administrative data.

Table 4: Impact on vaccination intentions and appointment link click

Dependent Variable	Intention to Vaccinate	Appointment Link Click
	(1)	(2)
Guaranteed Condition	2.53	3.74***
	(1.79)	(0.90)
Lottery Condition	-0.48	2.83***
	(1.86)	(0.86)
Donation Condition	-4.01**	1.95**
	(1.92)	(0.83)
Choice Condition	3.34*	3.66***
	(1.82)	(0.88)
Controls	yes	yes
Observations	5,314	5,314

Note: The table shows coefficient estimates from linear regressions of intentions to vaccinate and appointment link click on indicators for the experimental conditions using the pre-registered set of controls. "Intention vaccinate" considers intentions to vaccinate as outcome measure (response to the question "Do you think you will get an additional shot of a COVID-19 vaccine within the first month after the vaccine becomes available to you?", collected as No/Yes and coded as 0/100). "Appointment link click" considers whether the participant clicked the link to get information on how to make a vaccination appointment as outcome measure (0/1, coded as 0/100). Heteroscedasticity robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

10 Discussion and conclusion

If effective, incentives could be a powerful tool for policy-makers to encourage health behaviors that benefit society and reduce the high burden of disease linked to harmful behaviors (WHO, 2023). While some progress has been made in understanding the effects of incentive schemes on health behaviors, doubts about their effectiveness and worries about unintended consequences persist because of data limitations. Our approach of combining experimental, survey, and administrative data demonstrates a valuable way of overcoming common data limitations curtailing empirical research and allows us to comprehensively evaluate the impact of different incentive policies.

Using a field experiment in a large and broadly representative sample of the Swedish population, rich survey data, and nationwide vaccination and population registries, we study the effects of different types of incentives for vaccination. We find large and lasting effects of guaranteed incentives on vaccination uptake. Providing lottery incentives is also effective, but less so than guaranteed incentives. Donation incentives, however, are substantially less effective and only accelerate vaccination without affecting long-run vaccination levels.

Our rich survey data allows us to study whether incentives affect different groups of people differently, and whether economic preferences matter in understanding who reacts to which incentive type. By using machine learning methods and a novel treatment that allows participants to choose their own incentive, we are able to study to what extent there is scope for individually targeting incentives to increase effectiveness. We find that although there are some heterogeneous treatment effects, neither machine learning techniques nor allowing people to self-select into an incentive scheme increases overall vaccination uptake. The guaranteed condition outperforms the other incentive types across different groups, rendering the heterogeneities insufficient to effectively target treatments.

Another important aspect of our data is that it captures both intentions to vaccinate and actual vaccination uptake, enabling us to determine whether treatment effects on intentions serve as a reliable indicator of treatment effects on actual uptake. This speaks to a large literature which uses data on intentions—rather than behavior—to study the effects of interventions on health behaviors, as well as an emerging literature on surrogate outcomes (Athey et al., 2019). We find that treatment effects on intentions are highly inaccurate proxies of treatment effects on behavior. This is very problematic given the large body of research that relies solely on intentions. We also offer a possible alternative for studies that do not have

access to administrative data: We find that a behavioral outcome more closely linked to actual uptake—whether participants click on a link to get information about how to schedule an appointment—does much better as a proxy to study treatment effects on actual behavior.

While using intention data is problematic when studying treatment effects on behavior, it does provide some insights into potential channels through which incentives work. Intentions do not capture our treatment effects because we find that participants who end up reacting to incentives are those who already intended to vaccinate in the first place. This indicates that incentives are particularly effective at reducing the intention-behavior gap (Sheeran, 2002; Milkman et al., 2011; Rogers et al., 2015), rather than persuading individuals who do not intend to vaccinate to change their minds.

Our data also allows us to study the spillover of incentives to closely related persons at the individual level. A primary concern in the incentives literature is the potential for such incentives to inadvertently discourage the behavior of others. For instance, peers without incentives might delay taking a vaccine in anticipation of future rewards or might interpret the use of incentives as a negative signal on the safety of the vaccine (e.g, Bénabou and Tirole, 2011; Ellingsen and Johannesson, 2008; Angelucci et al., 2019; Volpp and Cannuscio, 2021). This is a difficult question to study, as it requires rich data on who is close to the treated participants as well as their behavior. By combining administrative registries from different institutions, we track down the relatives of each of our participants and reliably study their behavior. We find a clear connection between people's vaccination behavior and vaccination uptake of their partners as well as positive spillovers of incentives on partners, which we estimate to be about half the direct effect of the incentives.

As is common with empirical research, a key limitation of our paper is that the results are drawn from one particular vaccination context: COVID-19 booster dose uptake in Sweden. However, we believe that the results are likely to generalize to other vaccination contexts, including settings with recurrent vaccinations in other high-income countries. First, Sweden is comparable in terms of vaccination rates, levels of inequality, and economic preferences with other Western countries (Falk et al., 2018; WGI, 2023; Mathieu et al., 2020). Second, the COVID-19 booster dose is now periodically recommended in most countries, similar to other vaccines with recurrent or multiple doses such as flu, measles, and human papillomavirus. This suggests that the mechanisms influencing individuals' decisions to take the COVID-19 booster are similar to those for other vaccines. Finally, our rich dataset allows us to

study the effects of incentives across a wide range of groups, varying in vaccine hesitancy, economic preferences, trust, personality, and sociodemographics such as age, gender, income, and education. Although the distribution of these groups may differ across countries, the consistent effectiveness of incentives across all groups suggests a generalizable pattern in response to incentives. Therefore, we believe that the effectiveness of incentives in promoting recurrent vaccinations might be similar in other contexts.

Our experiment presents a clear conclusion: Guaranteed incentives can significantly boost long-term vaccination rates without notable negative unintended consequences. As governments and organizations scramble for strategies to increase vaccination rates, our findings indicate that modest guaranteed monetary incentives for everyone could be an effective approach.

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Incentives to Vaccinate

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Appendix

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A Summary and sample statistics

Table A.1: Summary statistics

Variable	Mean	SD	Min.	Max.	N
Behaviors and Intentions					
Third dose within 30 days	39.65	48.92	0	100	5,324
Third dose within 60 days	46.32	49.87	0	100	5,324
Third dose within 90 days	50.02	50.00	0	100	5,324
Third dose within 180 days	55.39	49.71	0	100	5,324
Third dose within 360 days	58.25	49.32	0	100	5,324
Third dose within 660 days	58.56	49.27	0	100	5,324
Third-dose by December 19, 2023	58.72	49.24	0	100	5,324
Days Between Dose 3 Vaccination and Survey	45.68	75.67	0	682	3,126
Appointment Link Click	0.05	0.22	0	1	5,324
Intention vaccinate	0.77	0.42	0	1	5,314
Fourth-dose by December 19, 2023	9.07	28.72	0	100	5,324
Treatment Assignment					
Guaranteed Condition	0.20	0.40	0	1	5,324
Lottery Condition	0.20	0.40	0	1	5,324
Donation Condition	0.19	0.40	0	1	5,324
Choice Condition	0.20	0.40	0	1	5,324
All Incentives Conditions	0.79	0.40	0	1	5,324
Control condition	0.21	0.40	0	1	5,324
Sociodemographics					
Age	33.36	10.16	17	64	5,324
Female	0.54	0.50	0	1	5,324
Single	0.25	0.43	0	1	5,324
Sarbo	0.06	0.23	0	1	5,324
Couple	0.40	0.49	0	1	5,324
Married	0.26	0.44	0	1	5,324
Other Civil Status	0.03	0.16	0	1	5,324
Has Children	0.46	0.50	0	1	5,324
Elementary School or Lower	0.04	0.19	0	1	5,324
High-school	0.34	0.47	0	1	5,324
Professional Training	0.13	0.33	0	1	5,324
In College	0.09	0.28	0	1	5,324
College Degree	0.40	0.49	0	1	5,324
PhD	0.01	0.09	0	1	5,324
Employed	0.76	0.43	0	1	5,324
Unemployed	0.03	0.17	0	1	5,324
In College	0.16	0.37	0	1	5,324
Retired	0.01	0.10	0	1	5,324
Other Professional Situation	0.04	0.20	0	1	5,324
Mother from Sweden	0.94	0.24	0	1	5,324
Mother from Rest of Europe	0.00	0.05	0	1	5,324
Mother from North America	0.01	0.09	0	1	5,324
Mother from South America	0.01	0.07	0	1	5,324
Mother from Africa	0.03	0.16	0	1	5,324
Mother from the Middle-east	0.02	0.14	0	1	5,324
Mother from the Rest of Asia	0.00	0.02	0	1	5,324
Father from Sweden	0.93	0.25	0	1	5,324
Father from Rest of Europe	0.00	0.05	0	1	5,324
Father from North America	0.01	0.10	0	1	5,324
Father from South America	0.01	0.08	0	1	5,324
Father from Africa	0.03	0.17	0	1	5,324
Father from the Middle-east	0.01	0.12	0	1	5,324
Father from the Rest of Asia	0.00	0.03	0	1	5,324
Income 0-5000kr	0.04	0.20	0	1	5,324
Income 5001-10000kr	0.05	0.22	0	1	5,324
Income 10001-15000kr	0.13	0.33	0	1	5,324
Income 15001-20000kr	0.12	0.33	0	1	5,324
Income 20001-25000kr	0.21	0.40	0	1	5,324
Income 25001-30000kr	0.20	0.40	0	1	5,324
Income 30001-35000kr	0.13	0.33	0	1	5,324
Income 35001-40000kr	0.06	0.24	0	1	5,324
Income 35001-40000kr	0.03	0.16	0	1	5,324
Income 45001-50000kr	0.01	0.12	0	1	5,324
Income 50000kr-55000kr	0.01	0.08	0	1	5,324
Income more than 55000kr	0.01	0.10	0	1	5,324
Economic Preferences					
Prosociality	114.48	78.33	0	200	5,324
Risk taking	84.24	83.14	0	200	5,324
Altruism survey	8.31	2.54	1	11	5,324
Risk taking survey	6.79	1.94	1	11	5,324
Time discounting survey	8.00	1.75	1	11	5,324
Trust survey	6.45	2.39	1	11	5,324
Reciprocity survey	9.82	1.44	1	11	5,324
Self-image concerns survey	8.65	1.82	1	11	5,324
Social-image concerns survey	5.47	2.82	1	11	5,324
Importance autonomy survey	9.04	1.65	2	11	5,324
Extrinsic motivation survey	7.55	2.37	1	11	5,324
Procrastination survey	6.77	2.71	1	11	5,324
Norm-following survey	7.09	2.32	1	11	5,324
COVID-19 Related					
Ever tested positive for COVID-19	0.55	0.50	0	1	5,324
In a risk group for COVID-19	0.08	0.28	0	1	5,324
COVID-19 vaccines are safe	4.13	0.97	1	5	5,324
Diseases can be triggered by vaccinations	1.95	1.02	1	5	5,324
Worried about side-effects	2.43	1.29	1	5	5,324
Worried about needles	2.48	1.28	1	5	5,324

Table A.2: RCT sample and the Swedish population

Variable	N	Mean	SD	Mean
18 to 25 years	5,324	0.18	0.39	0.16
26 to 35 years	5,324	0.45	0.50	0.24
36 to 45 years	5,324	0.22	0.41	0.21
46 to 55 years	5,324	0.10	0.30	0.22
56 to 65 years	5,324	0.05	0.22	0.18
Female	5,324	0.54	0.50	0.50
Region Stockholm	5,324	0.26	0.44	0.23
Region Östra Mellansverige	5,324	0.15	0.36	0.17
Region Småland med öarna	5,324	0.07	0.26	0.08
Region Sydsverige	5,324	0.15	0.36	0.15
Region Västsverige	5,324	0.22	0.42	0.20
Region Norra Mellansverige	5,324	0.06	0.25	0.08
Region Mellersta Norrland	5,324	0.03	0.18	0.04
Region Övre Norrland	5,324	0.05	0.21	0.05

Note: Comparison of the trial data on age, gender and region. For age, we consider the Swedish age distribution in between 18 and 65.

Table A.3: Balance checks

Dependent Variable	Age	Female	Single	Has childr.	College	Income	Immigr.	Unempl.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Guaranteed Condition	-0.08*	0.05	-0.02	-0.01	-0.02	0.01	0.04	-0.04
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Lottery Condition	-0.02	0.01	0.03	0.02	-0.01	0.02	0.03	0.03
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Donation Condition	-0.05	-0.06	-0.06	-0.07	-0.05	0.01	0.03	0.00
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Choice Condition	-0.05	-0.08*	-0.02	0.05	-0.05	0.03	0.04	0.07
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)
N	5,324	5,324	5,324	5,324	5,324	5,324	5,324	5,324

Note: Results from an OLS regression in which we explain each sociodemographic variable with each of the four different conditions. All sociodemographic variables are standardized. Heteroscedasticity robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Summary statistics partners

Variable	Mean	SD	Min.	Max.	N
Behaviors					
Third-dose within 30 days	0.34	0.47	0	1	1,170
Third-dose within 60 days	0.42	0.49	0	1	1,170
Third-dose within 90 days	0.47	0.50	0	1	1,170
Third-dose within 180 days	0.54	0.50	0	1	1,170
Third-dose within 360 days	0.57	0.49	0	1	1,170
Third-dose within 660 days	0.58	0.49	0	1	1,170
Same day: first-dose	0.30	0.46	0	1	1,170
Same day: second-dose	0.28	0.45	0	1	1,170
Same day: third-dose	0.24	0.43	0	1	828
Treatment Assignment					
Guaranteed Condition	0.20	0.40	0	1	1,170
Lottery Condition	0.19	0.39	0	1	1,170
Donation Condition	0.20	0.40	0	1	1,170
Choice Condition	0.20	0.40	0	1	1,170
All incentives conditions	0.79	0.41	0	1	1,170
Control Condition	0.21	0.41	0	1	1,170
Sociodemographics					
Age	40.67	8.66	22	76	1,170
Female	0.43	0.50	0	1	1,170

Note: "Same day" measures whether the person got the first, second, and third dose of the COVID-19 vaccine at the same time as the trial participant.

Table A.5: Summary statistics children

Variable	Mean	SD	Min.	Max.	N
Behaviors					
Third-dose within 30 days	0.22	0.41	0	1	714
Third-dose within 60 days	0.26	0.44	0	1	714
Third-dose within 90 days	0.29	0.45	0	1	714
Third-dose within 180 days	0.34	0.47	0	1	714
Third-dose within 360 days	0.36	0.48	0	1	714
Third-dose within 660 days	0.36	0.48	0	1	714
Same day: first-dose	0.01	0.12	0	1	714
Same day: second-dose	0.03	0.16	0	1	714
Same day: third-dose	0.02	0.15	0	1	514
Treatment Assignment					
Guaranteed Condition	0.18	0.38	0	1	714
Lottery Condition	0.21	0.41	0	1	714
Donation Condition	0.19	0.40	0	1	714
Choice Condition	0.18	0.39	0	1	714
All incentives conditions	0.76	0.43	0	1	714
Control Condition	0.24	0.43	0	1	714
Sociodemographics					
Age	25.12	6.01	16	46	714
Female	0.46	0.50	0	1	714

Note: "Same day" measures whether the person got the first, second, and third dose of the COVID-19 vaccine at the same time as the trial participant.

Table A.6: Summary statistics parents

Variable	Mean	SD	Min.	Max.	N
Behaviors					
Third-dose within 30 days	0.36	0.48	0	1	1,331
Third-dose within 60 days	0.44	0.50	0	1	1,331
Third-dose within 90 days	0.49	0.50	0	1	1,331
Third-dose within 180 days	0.56	0.50	0	1	1,331
Third-dose within 360 days	0.58	0.49	0	1	1,331
Third-dose within 660 days	0.59	0.49	0	1	1,331
Same day: first-dose	0.03	0.16	0	1	1,331
Same day: second-dose	0.02	0.15	0	1	1,331
Same day: third-dose	0.03	0.17	0	1	1,016
Treatment Assignment					
Guaranteed Condition	0.24	0.43	0	1	1,331
Lottery Condition	0.18	0.38	0	1	1,331
Donation Condition	0.19	0.39	0	1	1,331
Choice Condition	0.18	0.39	0	1	1,331
All incentives conditions	0.79	0.40	0	1	1,331
Control Condition	0.21	0.40	0	1	1,331
Sociodemographics					
Age	58.96	7.85	36	92	1,331
Female	0.53	0.50	0	1	1,331

Note: "Same day" measures whether the person got the first, second, and third dose of the COVID-19 vaccine at the same time as the trial participant.

Table A.7: Balance checks for family members

Dependent Variable	Age			Female		
	Partners	Parents	Children	Partners	Parents	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Guaranteed Condition	-0.08	-0.04	-0.08	0.03	-0.04	0.01
	(0.05)	(0.05)	(0.06)	(0.09)	(0.07)	(0.13)
Lottery Condition	-0.09	-0.10*	-0.02	-0.04	0.04	-0.06
	(0.05)	(0.05)	(0.06)	(0.09)	(0.07)	(0.12)
Donation Condition	-0.04	-0.10**	0.01	0.05	-0.01	0.00
	(0.06)	(0.05)	(0.05)	(0.09)	(0.07)	(0.12)
Choice Condition	-0.04	-0.06	-0.02	0.06	-0.04	0.02
	(0.05)	(0.05)	(0.06)	(0.09)	(0.07)	(0.13)
Controls	yes	yes	yes	yes	yes	yes
Observations	1,170	1,331	714	1,170	1,331	714

Note: Results from an OLS regression in which we explain each sociodemographic variable with each of the four different conditions. All sociodemographic variables are standardized. Heteroscedasticity robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Robustness of main results

Table B.1: Impact on short- and long-run third-dose uptake with sample weights

Dependent Variable	Booster uptake within			
	30 Days	660 days	30 Days	660 days
	Reweighted age		Reweighted gender	
	(1)	(2)	(3)	(4)
Guaranteed Condition	12.88***	9.20***	13.19***	8.82***
	(2.56)	(2.57)	(2.12)	(2.14)
Lottery Condition	8.69***	7.61***	8.83***	7.54***
	(2.60)	(2.63)	(2.13)	(2.15)
Donation Condition	4.76*	1.57	4.83**	0.71
	(2.61)	(2.71)	(2.13)	(2.21)
Choice Condition	11.88***	6.90***	11.11***	7.14***
	(2.62)	(2.65)	(2.13)	(2.17)
Controls	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of short- and long-run third-dose COVID-19 vaccination uptake on indicators for the experimental conditions using the pre-registered set of controls. "Third dose uptake X days" measures the proportion of participants who received the third dose within X days after the start of the incentive window. We have an overrepresentation of individuals aged 26 to 35 years and an underrepresentation of individuals ages 46 or older as well as an overrepresenation of women. In this table, we use sampling weights to adjust for the misrepresentation. Heteroscedasticity robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Impact on days to vaccinate

Dependent Variable	Days to Vaccination
	(1)
Guaranteed Condition	-92.13***
	(22.31)
Lottery Condition	-73.65***
	(22.56)
Donation Condition	-3.61
	(22.90)
Choice Condition	-71.77***
	(22.63)
Controls	yes
Observations	5,324

Note: The table shows coefficient estimates from regressions on the number of days that it takes for the participant to get the third dose after filling out the survey. We use a Tobit regression to account for the fact that the data is censored. Heteroscedasticity robust standard errors are shown in parentheses. We use the pre-registered controls: gender, age, region, interactions between age and region, being in an at-risk group for COVID-19, civil status, having children in the household, employment status, education, parents' place of birth, and income. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Impact on short-and long-run third-dose uptake, different sets of controls

Dependent Variable	Booster within 30 Days			Booster within 660 days		
	(1)	(2)	(3)	(4)	(5)	(6)
Guaranteed Condition	12.93***	12.78***	13.02***	8.34***	8.21***	8.58***
	(2.07)	(2.12)	(2.11)	(2.10)	(2.14)	(2.14)
Lottery Condition	8.34***	8.09***	8.45***	6.73***	6.71***	7.18***
	(2.07)	(2.13)	(2.12)	(2.13)	(2.16)	(2.14)
Donation Condition	4.68**	4.27**	4.66**	0.65	-0.16	0.61
	(2.06)	(2.12)	(2.12)	(2.16)	(2.20)	(2.20)
Choice Condition	10.88***	10.42***	10.90***	6.39***	5.82***	6.83***
	(2.08)	(2.13)	(2.13)	(2.13)	(2.18)	(2.16)
Sociodemographic controls	no	no	yes	no	no	yes
Age x Region FE	no	yes	yes	no	yes	yes
Observations	5,324	5,324	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days and within 660 days (the end of the follow-up period) on indicators for the experimental conditions. We include different sets of controls. Heteroscedasticity robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: Impact on short-and long-run third-dose uptake, logit

Dependent Variable	Booster uptake within					
	30 days	60 days	90 days	180 days	360 days	660 days
	(1)	(2)	(3)	(4)	(5)	(6)
Guaranteed Condition	0.58***	0.43***	0.36***	0.32***	0.37***	0.38***
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Lottery Condition	0.38***	0.28***	0.29***	0.25***	0.32***	0.32***
	(0.10)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Donation Condition	0.21**	0.08	0.07	0.02	0.04	0.03
	(0.10)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Choice Condition	0.49***	0.39***	0.36***	0.29***	0.31***	0.30***
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Controls	yes	yes	yes	yes	yes	yes
Observations	5,270	5,261	5,265	5,271	5,258	5,263

Note: The table shows coefficient estimates from logit regressions of short- and long-run third-dose COVID-19 vaccination uptake on indicators for the experimental conditions using the pre-registered set of controls. "Third dose uptake X days" measures the proportion of participants who received the third dose within X days after the start of the incentive window. Some observations are dropped during estimation because the region and age interactions predict the vaccination uptake perfectly in smaller regions. Heteroscedasticity robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Impact on short- and long-run vaccination uptake, different incentive windows

Dependent Variable	Third-dose uptake 30 Days		Third-dose uptake 660 days	
	(1)	(2)	(3)	(4)
Guaranteed Condition	12.54***	12.99***	8.59***	7.91***
	(2.11)	(2.23)	(2.14)	(2.20)
Lottery Condition	8.44***	8.30***	7.08***	6.30***
	(2.12)	(2.24)	(2.14)	(2.21)
Donation Condition	4.02*	4.04*	0.60	-0.11
	(2.12)	(2.25)	(2.20)	(2.28)
Choice Condition	10.58***	11.07***	6.82***	6.61***
	(2.12)	(2.25)	(2.16)	(2.23)
Controls	yes	yes	yes	yes
Different incentive window	not recode	drop	not recode	drop
Observations	5,324	4,918	5,324	4,918

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days and within 660 days (the end of the follow-up period) on indicators for the experimental conditions. In the specifications in the paper, we consider whether participants got the third dose within 30 days (or 660 days) after the incentive 30-day window became active. For most participants, this corresponds to the 30-day window after the trial. However, for 415 participants, it had not been three months since the second dose when they participated in the trial. Because the third dose was only recommended and available 3 months after the second dose, the 30-day incentive window for these participants started after the trial. In the main analysis, we consider the actual incentive window. Here, we give results when we always take 30 days after the trial (as we initially pre-registered), see columns "Different incentive window = not recode" or if we drop participants that got the second dose within 3 months before the trial "Different incentive window = drop". Heteroscedasticity robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Impact on short-run third-dose uptake, robustness to different sample inclusion criteria

Dependent Variable	Vaccination Uptake 30 Days				
	(1)	(2)	(3)	(4)	(5)
Guaranteed Condition	13.02***	13.05***	12.38***	12.93***	12.30***
	(2.11)	(2.12)	(2.05)	(2.11)	(2.05)
Lottery Condition	8.45***	8.44***	8.78***	8.37***	8.71***
	(2.12)	(2.12)	(2.07)	(2.12)	(2.07)
Donation Condition	4.66**	4.75**	3.71*	4.64**	3.71*
	(2.12)	(2.13)	(2.04)	(2.12)	(2.07)
Choice Condition	10.90***	10.97***	10.66***	10.95***	10.72***
	(2.13)	(2.13)	(2.06)	(2.13)	(2.07)
Controls	yes	yes	yes	yes	yes
Exlude no 2nd dose	yes	yes	no	yes	no
Exclude <18	yes	yes	yes	no	no
Exlude not finished	no	yes	no	no	no
Observations	5,324	5,314	5,555	5,343	5,574

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days on indicators for the experimental conditions. The table provides results for different sample inclusion criteria. Specification (1) is our main specification that includes individuals that went through the experimental intervention but did not finish the rest of the questionnaire to avoid any biases (Exclude not finished = no), but excludes participants that did not receive the 2nd dose of a COVID-19 vaccine at the time of the trial (Exclude no 2nd dose = yes) or were not yet 18 years old at the time of the trial (Exclude <18 = yes). Specification (2) - (5) build on this specification. In specification (2), we exclude participants that did not finish the questionnaire (potentially introducing a bias), specifications (3) and (5) include participants that did not receive the 2nd dose of a COVID-19 vaccine at the time of the trial, and specification (4) and (5) include participants that were not yet 18 years old. Controls as pre-registered. Heteroscedasticity robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.7: Impact on long-run third-dose uptake, robustness to different sample inclusion criteria

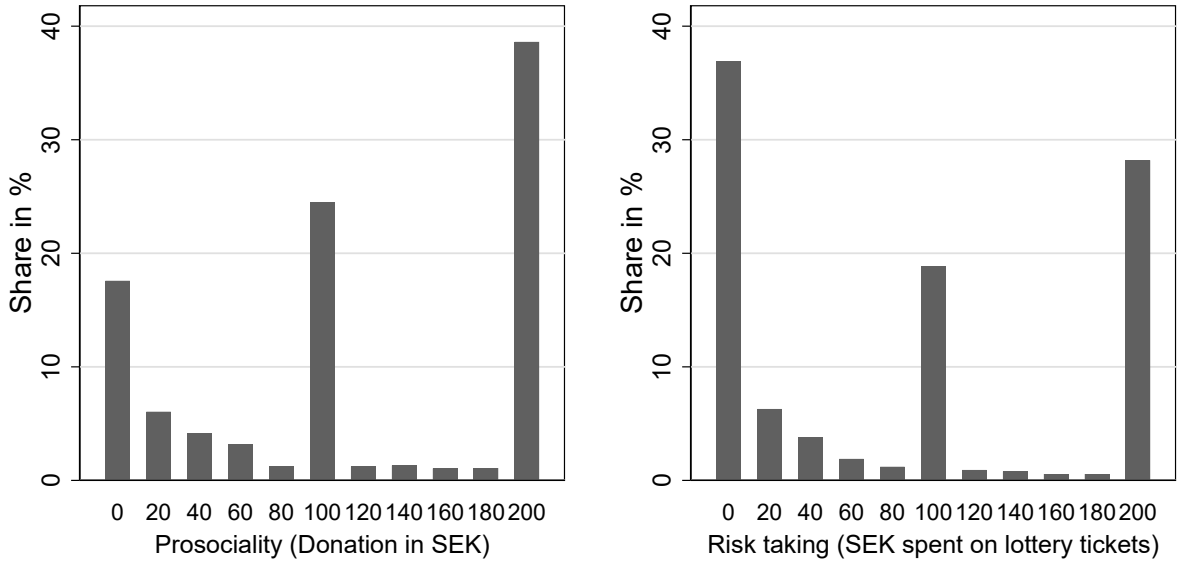
Dependent Variable	Vaccination Uptake 660 Days				
	(1)	(2)	(3)	(4)	(5)
Guaranteed Condition	8.58***	8.54***	7.96***	8.57***	7.96***
	(2.14)	(2.14)	(2.11)	(2.14)	(2.08)
Lottery Condition	7.18***	7.16***	7.71***	7.14***	7.68***
	(2.14)	(2.14)	(2.11)	(2.14)	(2.10)
Donation Condition	0.61	0.65	-0.54	0.56	-0.57
	(2.20)	(2.20)	(2.15)	(2.20)	(2.10)
Choice Condition	6.83***	6.87***	6.72***	6.83***	6.73***
	(2.16)	(2.16)	(2.12)	(2.16)	(2.10)
Controls	yes	yes	yes	yes	yes
Exlude no 2nd dose	yes	yes	no	yes	no
Exclude <18	yes	yes	yes	no	no
Exlude not finished	no	yes	no	no	no
Observations	5,324	5,314	5,555	5,343	5,574

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 660 days on indicators for the experimental conditions. The table provides results for different sample inclusion criteria. Specification (1) is our main specification that includes individuals that went through the experimental intervention but did not finish the rest of the questionnaire to avoid any biases (Exclude not finished = no), but excludes participants that did not receive the 2nd dose of a COVID-19 vaccine at the time of the trial (Exclude no 2nd dose = yes) or were not yet 18 years old at the time of the trial (Exclude <18 = yes). Specification (2) - (5) build on this specification. In specification (2), we exclude participants that did not finish the questionnaire (potentially introducing a bias), specifications (3) and (5) include participants that did not receive the 2nd dose of a COVID-19 vaccine at the time of the trial, and specification (4) and (5) include participants that were not yet 18 years old. Controls as pre-registered. Heteroscedasticity robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C Behavioral targeting: Using preference heterogeneity to increase incentive effectiveness

C.1 Heterogeneities

Figure C.1: Distributions of prosociality and risk taking



Note: The figures show the distribution of prosociality (how much of the initial SEK 200 endowement participants donate to "Save the Children") and risk taking (how much of the initial SEK 200 endowement participants buy lottery 2 tickets for SEK 20 with a 1% chance of winning SEK 2,000).

Table C.1: Heterogeneous treatment effects: survey measures of altruism, reciprocity, trust

Dependent Variable	Third-dose uptake within					
	30 Days			660 days		
Var	Altruism	Reciprocity	Trust	Altruism	Reciprocity	Trust
	(1)	(2)	(3)	(4)	(5)	(6)
Guaranteed Condition x Var	-1.656**	0.457	-2.353***	-1.048	0.132	-2.335***
	(0.818)	(1.476)	(0.880)	(0.840)	(1.524)	(0.901)
Lottery Condition x Var	-0.468	0.623	-0.867	-1.053	1.861	-1.273
	(0.812)	(1.481)	(0.878)	(0.838)	(1.496)	(0.902)
Donation Condition x Var	0.412	0.277	0.069	-0.017	1.515	-0.240
	(0.811)	(1.382)	(0.880)	(0.852)	(1.446)	(0.923)
Choice Condition x Var	-1.166	-1.464	-1.034	-1.555*	-0.504	-1.081
	(0.816)	(1.481)	(0.899)	(0.837)	(1.498)	(0.922)
Guaranteed Condition	13.101***	12.970***	13.200***	8.516***	8.682***	8.814***
	(2.116)	(2.118)	(2.110)	(2.135)	(2.140)	(2.136)
Lottery Condition	8.406***	8.446***	8.635***	7.141***	7.193***	7.410***
	(2.120)	(2.123)	(2.122)	(2.135)	(2.140)	(2.140)
Donation Condition	4.778**	4.691**	4.772**	0.735	0.666	0.761
	(2.123)	(2.130)	(2.121)	(2.191)	(2.201)	(2.196)
Choice Condition	10.872***	10.949***	11.067***	6.781***	6.909***	7.042***
	(2.129)	(2.129)	(2.126)	(2.156)	(2.159)	(2.159)
Controls	yes	yes	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days on indicators for the experimental conditions, preference measures, and the interactions of treatment indicators and preference measures. We de-meaned all preference measures. Heteroscedasticity robust standard errors are shown in parentheses.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Heterogeneous treatment effects: survey measures of norm-following, self-image, social-image

Dependent Variable	Third-dose uptake within					
	30 Days			660 days		
Var	Norm-following (1)	Self-image (2)	Social-image (3)	Norm-following (4)	Self-image (5)	Social-image (6)
Guaranteed Condition x Var	-1.068	-2.295**	-0.399	-1.732*	-2.057*	-0.103
	(0.915)	(1.136)	(0.737)	(0.927)	(1.208)	(0.753)
Lottery Condition x Var	-0.877	-1.426	0.344	-1.929**	-1.201	-0.119
	(0.921)	(1.105)	(0.762)	(0.930)	(1.171)	(0.777)
Donation Condition x Var	-1.399	-1.381	0.295	-2.267**	-0.959	-0.598
	(0.913)	(1.128)	(0.758)	(0.933)	(1.201)	(0.781)
Choice Condition x Var	-0.996	-2.530**	0.835	-1.935**	-1.991*	0.682
	(0.934)	(1.133)	(0.757)	(0.962)	(1.207)	(0.770)
Guaranteed Condition	12.905***	12.981***	13.031***	8.397***	8.536***	8.513***
	(2.114)	(2.114)	(2.116)	(2.131)	(2.136)	(2.137)
Lottery Condition	8.394***	8.356***	8.411***	7.079***	7.080***	7.119***
	(2.119)	(2.120)	(2.123)	(2.132)	(2.137)	(2.141)
Donation Condition	4.574**	4.657**	4.646**	0.474	0.655	0.578
	(2.124)	(2.125)	(2.126)	(2.189)	(2.196)	(2.199)
Choice Condition	10.758***	10.859***	10.918***	6.626***	6.778***	6.879***
	(2.127)	(2.127)	(2.128)	(2.151)	(2.158)	(2.157)
Controls	yes	yes	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days on indicators for the experimental conditions, preference measures, and the interactions of treatment indicators and preference measures. We de-meaned all preference measures. Heteroscedasticity robust standard errors are shown in parentheses.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: Heterogeneous treatment effects: survey measures of risk taking, time discounting, procrastination

Dependent Variable	Third-dose uptake within					
	30 Days			660 days		
Var	Risk taking survey (1)	Time discounting (2)	Procrastination (3)	Risk taking survey (4)	Time discounting (5)	Procrastination (6)
Guaranteed Condition x Var	-0.311	0.065	-1.433*	0.181	-0.674	-0.583
	(1.098)	(1.168)	(0.771)	(1.122)	(1.213)	(0.779)
Lottery Condition x Var	0.894	0.721	-0.801	0.870	-0.055	-0.009
	(1.100)	(1.194)	(0.788)	(1.122)	(1.224)	(0.782)
Donation Condition x Var	0.903	2.024*	-0.793	0.244	0.794	0.731
	(1.122)	(1.187)	(0.790)	(1.167)	(1.257)	(0.807)
Choice Condition x Var	-0.606	-1.021	1.302*	-0.322	-1.328	0.875
	(1.106)	(1.184)	(0.768)	(1.109)	(1.235)	(0.787)
Guaranteed Condition	12.867***	13.057***	13.093***	8.526***	8.617***	8.680***
	(2.115)	(2.114)	(2.113)	(2.139)	(2.136)	(2.135)
Lottery Condition	8.432***	8.431***	8.406***	7.176***	7.160***	7.190***
	(2.123)	(2.121)	(2.121)	(2.141)	(2.139)	(2.141)
Donation Condition	4.618**	4.688**	4.656**	0.609	0.639	0.638
	(2.126)	(2.122)	(2.126)	(2.199)	(2.196)	(2.199)
Choice Condition	10.980***	10.942***	10.846***	6.882***	6.859***	6.843***
	(2.130)	(2.131)	(2.126)	(2.162)	(2.160)	(2.160)
Controls	yes	yes	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days on indicators for the experimental conditions, preference measures, and the interactions of treatment indicators and preference measures. We de-meaned all preference measures. Heteroscedasticity robust standard errors are shown in parentheses.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.4: Heterogeneous treatment effects: survey measures of importance of autonomy, extrinsic motivation

Dependent Variable	Third-dose uptake within			
	30 Days		660 days	
Var	Autonomy importance	Extrinsic motivation	Autonomy importance	Extrinsic motivation
	(1)	(2)	(3)	(4)
Guaranteed Condition x Var	-2.402*	-0.745	-0.803	-1.397
	(1.257)	(0.884)	(1.296)	(0.911)
Lottery Condition x Var	-0.327	0.599	0.700	0.125
	(1.280)	(0.912)	(1.300)	(0.923)
Donation Condition x Var	0.742	0.649	1.151	-0.229
	(1.292)	(0.891)	(1.349)	(0.933)
Choice Condition x Var	-2.954**	-0.420	-1.693	-0.304
	(1.299)	(0.915)	(1.315)	(0.934)
Guaranteed Condition	13.081***	12.873***	8.639***	8.386***
	(2.111)	(2.110)	(2.130)	(2.131)
Lottery Condition	8.423***	8.453***	7.146***	7.156***
	(2.122)	(2.120)	(2.139)	(2.140)
Donation Condition	4.727**	4.676**	0.608	0.638
	(2.126)	(2.123)	(2.199)	(2.195)
Choice Condition	11.160***	11.069***	7.094***	6.952***
	(2.125)	(2.131)	(2.151)	(2.160)
Controls	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days on indicators for the experimental conditions, preference measures, and the interactions of treatment indicators and preference measures. We de-meaned all preference measures. Heteroscedasticity robust standard errors are shown in parentheses.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: Heterogeneous treatment effects: COVID-19 history and vaccination attitudes

Dependent Variable	Third-dose uptake within					
	30 Days			660 days		
Var	Ever tested positive COVID-19 (1)	Risk group COVID-19 (2)	COVID-19 vacc. are safe (3)	Ever tested positive COVID-19 (4)	Risk group COVID-19 (5)	COVID-19 vacc. are safe (6)
Guaranteed Condition x Var	-7.329*	-8.986	3.783**	-2.480	-9.633	2.800
	(4.228)	(7.905)	(1.924)	(4.305)	(7.823)	(1.980)
Lottery Condition x Var	-2.367	-9.907	0.196	0.105	-9.054	-0.883
	(4.267)	(7.768)	(2.019)	(4.328)	(8.007)	(2.088)
Donation Condition x Var	0.893	-9.945	2.910	1.149	-12.910	-0.470
	(4.233)	(7.930)	(1.988)	(4.387)	(8.047)	(2.101)
Choice Condition x Var	-6.506	-6.331	1.375	-1.726	-13.336*	-0.659
	(4.263)	(7.900)	(2.035)	(4.338)	(8.019)	(2.146)
Guaranteed Condition	16.878***	13.748***	12.922***	9.974***	9.340***	8.480***
	(3.120)	(2.197)	(2.047)	(3.162)	(2.226)	(2.011)
Lottery Condition	9.812***	9.227***	8.803***	7.035**	7.876***	7.713***
	(3.199)	(2.214)	(2.075)	(3.230)	(2.230)	(2.042)
Donation Condition	4.192	5.442**	5.148**	0.055	1.615	1.246
	(3.122)	(2.209)	(2.063)	(3.246)	(2.289)	(2.091)
Choice Condition	14.686***	11.389***	10.558***	7.738**	7.885***	6.427***
	(3.215)	(2.210)	(2.077)	(3.254)	(2.243)	(2.076)
Controls	yes	yes	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days on indicators for the experimental conditions, preference measures, and the interactions of treatment indicators and preference measures. We de-meaned "COVID-19 vacc. are safe". Heteroscedasticity robust standard errors are shown in parentheses.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.6: Heterogeneous treatment effects: vaccination attitudes

Dependent Variable	Third-dose uptake within					
	30 Days			660 days		
Var	Vaccination can trigger diseases	Worried side-effects	Worried needles	Vaccination can trigger diseases	Worried side-effects	Worried needles
	(1)	(2)	(3)	(4)	(5)	(6)
Guaranteed Condition x Var	-3.804*	-3.367**	-1.623	-4.214**	-0.723	-3.410**
	(1.979)	(1.541)	(1.651)	(2.056)	(1.571)	(1.651)
Lottery Condition x Var	-0.296	-1.248	-5.160***	-2.008	0.177	-6.237***
	(2.034)	(1.578)	(1.631)	(2.093)	(1.586)	(1.642)
Donation Condition x Var	-0.524	-2.271	-2.434	-2.473	-0.632	-3.597**
	(2.041)	(1.543)	(1.651)	(2.128)	(1.606)	(1.682)
Choice Condition x Var	-0.948	-2.161	-2.021	-3.010	-0.225	-3.514**
	(2.012)	(1.564)	(1.682)	(2.086)	(1.610)	(1.678)
Guaranteed Condition	12.715***	13.042***	12.838***	8.224***	8.759***	8.426***
	(2.102)	(2.055)	(2.095)	(2.120)	(2.060)	(2.116)
Lottery Condition	8.386***	9.165***	8.402***	7.077***	8.184***	7.120***
	(2.115)	(2.080)	(2.112)	(2.129)	(2.069)	(2.124)
Donation Condition	4.628**	5.013**	4.456**	0.573	1.163	0.376
	(2.116)	(2.065)	(2.109)	(2.180)	(2.112)	(2.178)
Choice Condition	10.810***	11.481***	11.294***	6.712***	7.650***	7.240***
	(2.124)	(2.082)	(2.122)	(2.147)	(2.095)	(2.139)
Controls	yes	yes	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days on indicators for the experimental conditions, preference measures, and the interactions of treatment indicators and preference measures. We de-meaned all measures. Heteroscedasticity robust standard errors are shown in parentheses.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.7: Heterogeneous treatment effects: age, gender

Dependent Variable	Third-dose uptake within 30 Days		660 days	
Var	Age	Female	Age	Female
	(1)	(2)	(3)	(4)
Guaranteed Condition x Var	-0.070	-4.724	0.174	-6.153
	(0.208)	(4.254)	(0.212)	(4.323)
Lottery Condition x Var	-0.085	-10.097**	0.115	-8.678**
	(0.206)	(4.289)	(0.211)	(4.329)
Donation Condition x Var	-0.057	-4.900	0.159	-2.854
	(0.206)	(4.266)	(0.217)	(4.411)
Choice Condition x Var	0.059	-5.815	-0.035	-7.445*
	(0.213)	(4.250)	(0.219)	(4.324)
Guaranteed Condition	13.030***	15.613***	8.621***	11.999***
	(2.117)	(3.134)	(2.137)	(3.230)
Lottery Condition	8.456***	14.002***	7.146***	11.951***
	(2.123)	(3.177)	(2.142)	(3.211)
Donation Condition	4.673**	7.359**	0.620	2.245
	(2.127)	(3.095)	(2.199)	(3.250)
Choice Condition	10.947***	14.043***	6.793***	10.810***
	(2.130)	(3.070)	(2.161)	(3.164)
Controls	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days on indicators for the experimental conditions, preference measures, and the interactions of treatment indicators and preference measures. We demeaned age. Heteroscedasticity robust standard errors are shown in parentheses.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

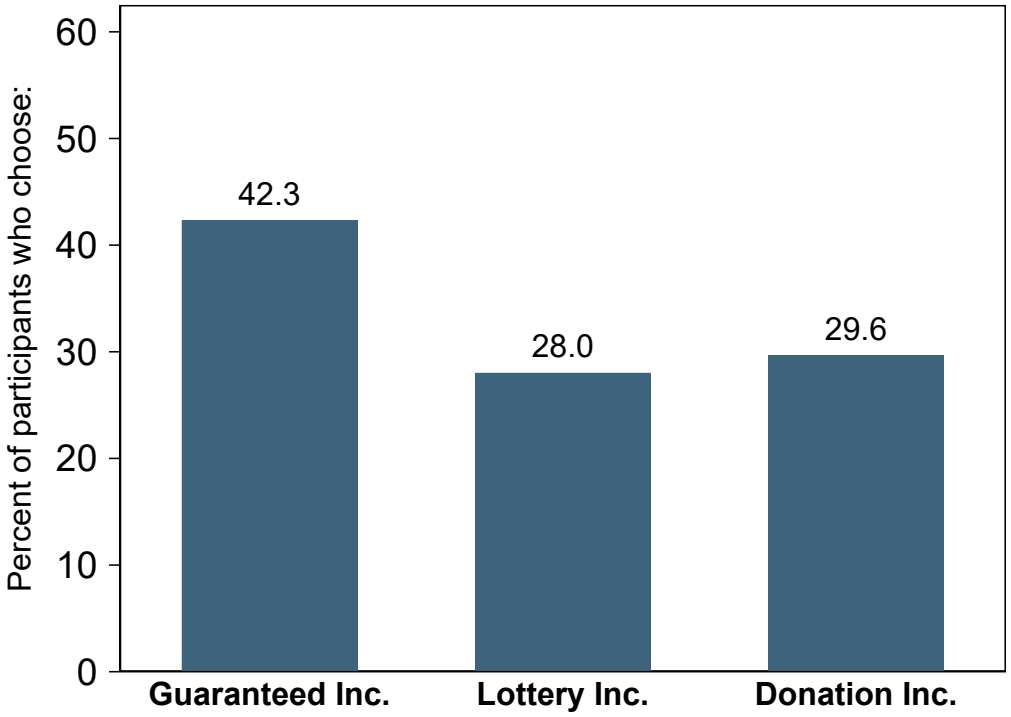
Table C.8: Heterogeneous treatment effects: income, education, and children

Dependent Variable	Third-dose uptake within					
	30 Days			660 days		
Var	Income	Education	Children	Income	Education	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Guaranteed Condition x Var	-3.176	1.092	-5.521	-1.305	0.751	-4.154
	(1.953)	(1.507)	(4.216)	(1.969)	(1.507)	(4.286)
Lottery Condition x Var	-2.114	1.627	-5.506	0.115	2.481	-3.951
	(2.021)	(1.520)	(4.235)	(2.020)	(1.521)	(4.285)
Donation Condition x Var	-1.814	0.299	-2.134	-0.857	0.983	-3.412
	(2.042)	(1.518)	(4.288)	(2.113)	(1.564)	(4.432)
Choice Condition x Var	-5.589***	1.859	-7.463*	-3.682*	3.224**	-6.159
	(1.945)	(1.505)	(4.244)	(1.981)	(1.519)	(4.311)
Guaranteed Condition	12.983***	12.965***	15.594***	8.560***	8.510***	10.522***
	(2.114)	(2.115)	(2.883)	(2.136)	(2.139)	(2.920)
Lottery Condition	8.420***	8.353***	11.023***	7.151***	7.032***	9.024***
	(2.121)	(2.121)	(2.913)	(2.140)	(2.146)	(2.944)
Donation Condition	4.642**	4.605**	5.729**	0.599	0.531	2.213
	(2.124)	(2.125)	(2.876)	(2.197)	(2.200)	(2.975)
Choice Condition	10.953***	10.887***	14.465***	6.887***	6.827***	9.773***
	(2.125)	(2.130)	(2.949)	(2.158)	(2.160)	(2.984)
Controls	yes	yes	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake within 30 days on indicators for the experimental conditions, preference measures, and the interactions of treatment indicators and preference measures. We de-meaned income and education. Heteroscedasticity robust standard errors are shown in parentheses.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

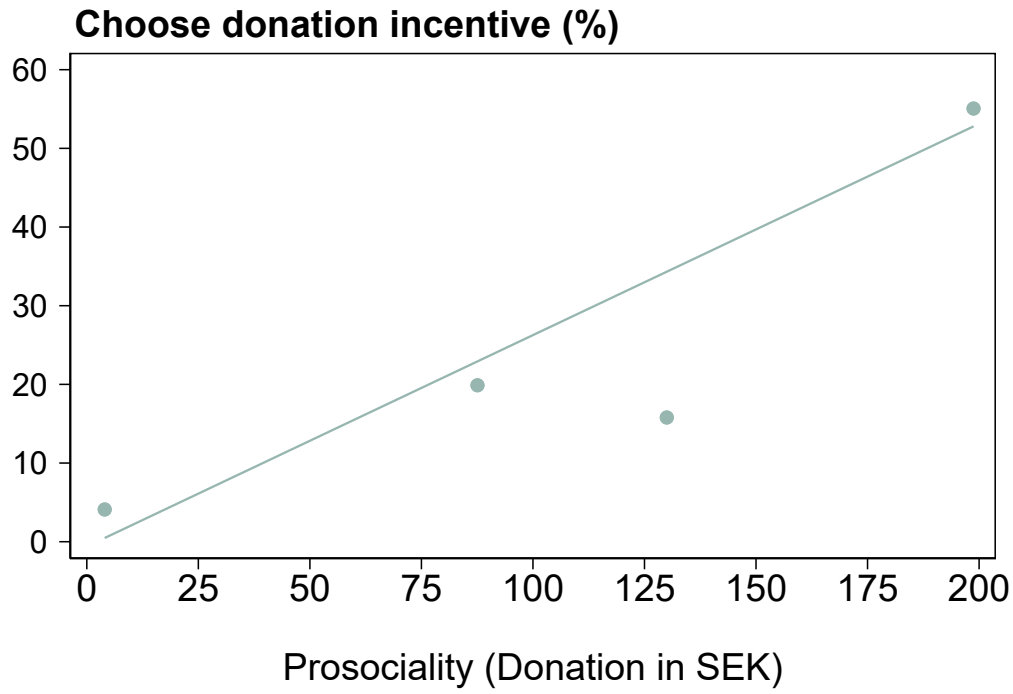
C.2 Choice condition

Figure C.2: Proportion of participants in the choice condition choosing each incentive



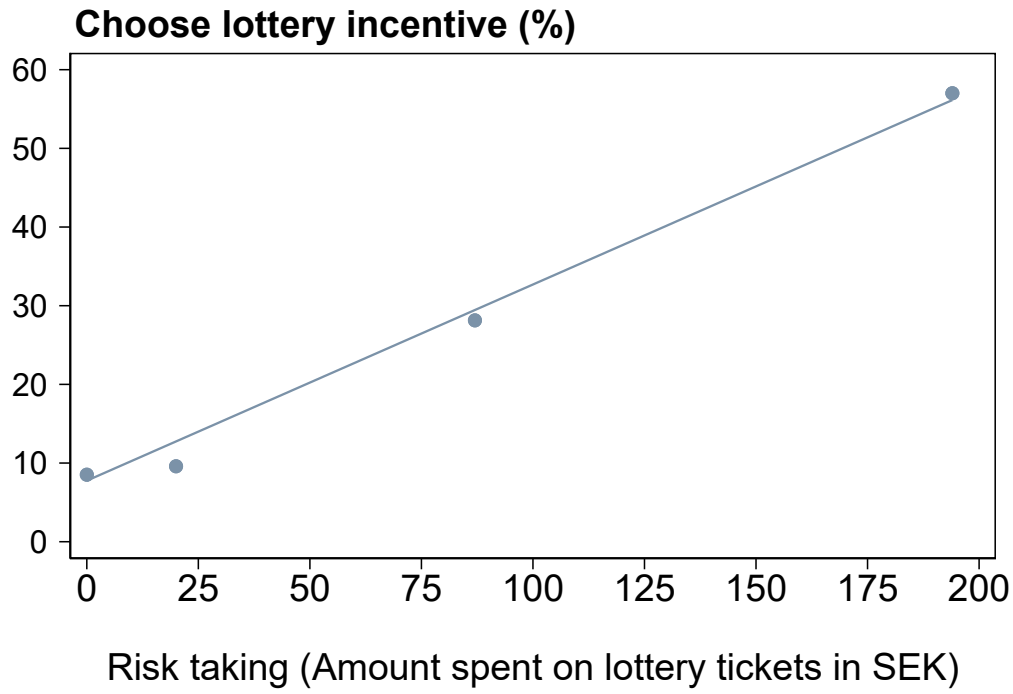
Note: The figure shows the distribution of choices in the choice condition.

Figure C.3: Prosociality and probability of choosing donation condition



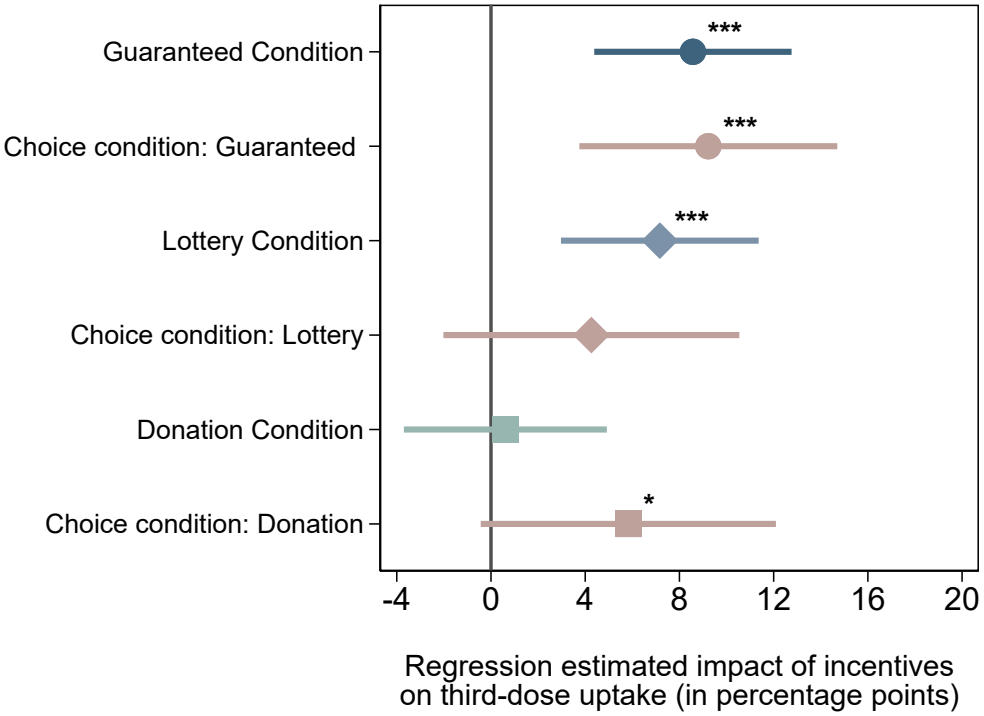
Note: Binned scatter plot between probability of choosing the donation incentive in the Choice condition and prosociality. Using a linear probability model, we estimate the probability of choosing the donation incentives increases by 53.7 pp ($N = 1m049, p < 0.001$) when we move from minimal prosociality (prosociality=0) to maximal prosociality (prosociality=200).

Figure C.4: Risk taking and probability of choosing lottery condition



Note: Binned scatter plot between probability of choosing the lottery incentive in the Choice condition and risk taking. Using a linear probability model, we estimate the probability of choosing the lottery incentives increases by 49.9 pp ($N = 1,049, p < 0.001$) when we move from minimal risk taking (Risk taking=0) to maximal risk taking (Risk taking=200).

Figure C.5: Third-dose uptake within 660 days conditional on the choice in the choice condition



Note: This figure displays the coefficient estimates and corresponding 95% confidence intervals from OLS regression on third-dose uptake within the 660-day after the start of the window in which the incentives were active using the pre-registered set of controls. The figure is based on survey data from the trial linked to Swedish administrative records on vaccination.

D Using machine learning to target policies: Policy learning

Table D.1 shows the key results from policy learning using policy trees with depth 2 for different policy choice sets and using differing sets of independent variables. For the main results, we only allow policy trees to choose among the incentive treatment conditions since in practice it will be difficult to use incentives nationwide only for some, but not for all individuals.[28] The policy trees assign policies maximizing uptake based on heterogeneous treatment effects estimated from causal forests with the independent variables indicated. With depth-2 policy trees the algorithm assigns policies to 4 mutually exclusive groups based on two variable splits.[29] The idea is simple: If heterogeneities as estimated by causal forests are large enough, policy-learning should be able to identify groups based on a few variable splits which will be most effectively targeted by one or the other incentive.

We estimate the policy trees using the algorithm developed by Wager and Athey (2018) as implemented in Knaus (2022). First, we use a causal forest to estimate treatment effect heterogeneity. Then, we estimate policy trees for targeting in four fifths of the sample. Last, we estimate the effectiveness of policy targeting in the remaining one fifth of the sample to avoid overfitting. To maximize power, we use 5-fold cross-validation as implemented by Knaus (2022) and average the corresponding uptake (shown in the column "Targeting").

Table D.1 shows the corresponding results. Independent variables used refers to the variable groups used for the causal forests and policy trees. We use all socio-demographics, preference variables, and variables related to COVID-19 in the most comprehensive specification.[30] Targeting gives the overall uptake if targeting was applied. Gtd. uptake states the uptake as estimated by causal forests if all received the guaranteed incentive. The Difference is the estimated difference between the two and shown with standard errors and p-value. The share of people assigned to the different policies is based on a policy tree fit to the full sample and illustrates how often a policy is assigned across all groups. Most important variables indicate the variables which were most often used for splitting the underlying causal forest, giving an impression of the relative importance of variables for heterogeneity of treatment

[28] The results are equivalent when we allow policy trees to choose from all treatments as well as the control based on all variables and with the goal of maximizing uptake within 30 days: Uptake in this case is 43.6% with a difference to the guaranteed incentive of -1.2 ($se = 1.2, p = 0.3$).

[29] We also obtain similar estimates using depth 3 policy trees with the choice set of fixed, lottery, and social as well as using all independent variables for the causal forest estimates.

[30] We treat income and age as continuous variables, as well as all preference and COVID-19-related variables which are non-binary.

Table D.1: Policy learning

Policy learning in cross-validated trees with depth 2			**Uptake with and without targeting**					Share of people assigned (full sample)				Most important variables		
Policy learning choice set	Dependent variable	Independent variables used	Targeting	Gtd. uptake	**Difference**	**SE**	**p**	Gtd.	Lottery	Don.	Choice	1	2	3
Guranteed, lottery, donation	Vaccinated within 30 days	Sociodem	44.3	45.3	**-1**	**1**	**0.32**	84	12	4	-	Age	Income	Region mother
		Sociodem & prefs	43.0	45.4	**-2.4**	**1.4**	**0.07**	64	20	16	-	Falk altruism	Donation	Extrinsic motivation
		Sociodem & COVID attitudes & prefs.	45.4	44.9	**0.4**	**1.2**	**0.71**	68	32	0	-	Donation	Falk altruism	Trust
Guranteed, lottery, donation, choice		Sociodem	43	45.4	**-2.4**	**1.1**	**0.02**	82	13	0	5	Age	Income	Location
		Sociodem & prefs	41.9	45.3	**-3.4**	**3.4**	**0.05**	17	28	14	42	Procrastination	Falk altruism	Donation
		Sociodem & COVID attitudes & prefs.	44.6	45	**-0.4**	**1.5**	**0.84**	35	24	0	42	Falk altruism	Procrastination	Donation
Guranteed, lottery, donation	Vaccinated within 660 days	Sociodem	61.2	62.5	**-1.3**	**1.5**	**0.38**	63	27	10	-	Age	Procrastination	College
		Sociodem & prefs	61.2	62.5	**-1.3**	**1.3**	**0.3**	64	36	0	-	Procrastination	Trust	Extrinsic motivation
		Sociodem & COVID attitudes & prefs.	60	62.1	**-2.1**	**1.3**	**0.11**	46	54	0	-	Procrastination	Trust	Age
Guranteed, lottery, donation, choice		Sociodem	61.5	62.5	**-1**	**1.5**	**0.52**	63	12	0	25	Age	Income	College
		Sociodem & prefs	61.4	62.5	**-1.1**	**1.6**	**0.49**	53	24	0	23	Extrinsic motivation	Procrastination	Trust
		Sociodem & COVID attitudes & prefs.	62.3	62.2	**0.1**	**1.6**	**0.93**	44	9	0	47	Procrastination	Extrinsic motivation	Norm-following

Note: Results from 5-fold cross-validated policy trees of depth 2 (Wager and Athey, 2018; Knaus, 2022), comparing the overall uptake using policy learning to uptake with giving everyone a guaranteed incentive. Policy trees are either fit with all treatment arms or only the fixed, lottery, and social treatment arms. To compute the share of people assigned we fit a policy tree to the full sample without sample splitting. The variable importance is based on how often a certain variable is used for splits in the generalized random forests underlying the full-sample policy tree, we show the three most important variables. Sociodemographics includes gender, age, income as well as dummies for region, civil status, whether the respondents has children, educational levels and occupation. Preferences includes risk and social preferences (incentivized), as well as unincentivized responses on altruism, risk attitudes, patience, reciprocity, self-image, social image, autonomy, extrinsic motivation, trust, procrastination, and norm-following. COVID-19 attitudes include perceived safety, likelihood of triggering disease, worries about side-effects, worries about needles, whether they respondent has tested positive and whether the respondent is in an at-risk group. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

effects. In line with the OLS results, altruism is most often used for variable splits in the causal forests for vaccination within 30 days (for the long-run, procrastination and trust are more often used).

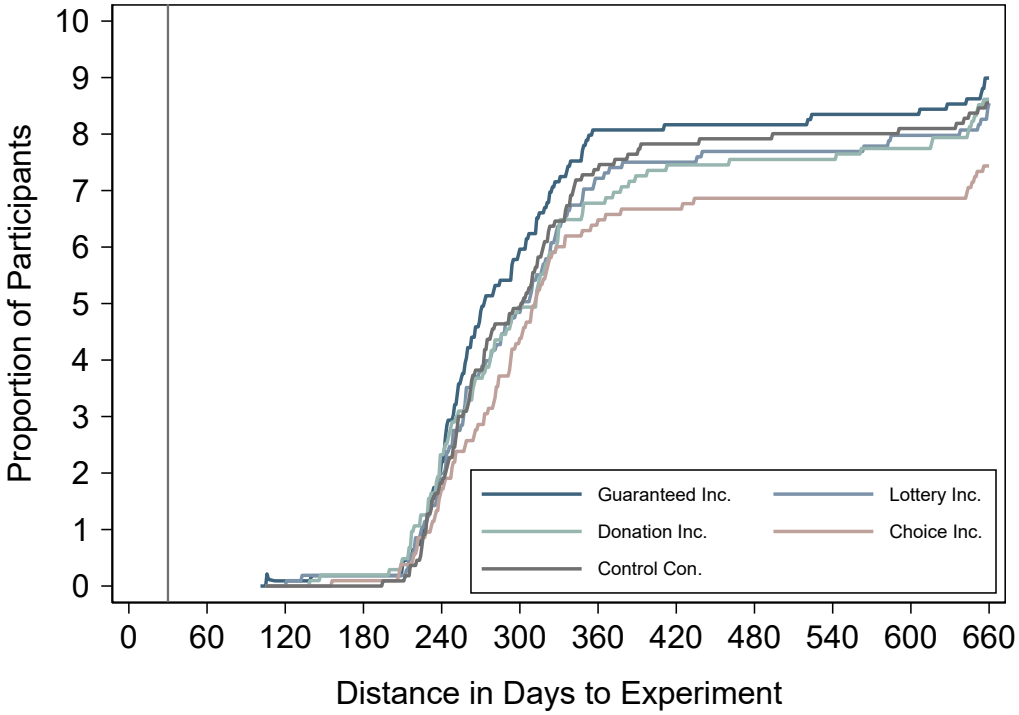
Overall, the results show that exploiting heterogeneities in a very flexible manner to target policies to a few groups does not increase uptake statistically significantly. The results from the policy trees indicate that overall uptake is higher when most people would be assigned guaranteed incentives and when all information on socio-demographics, preferences, and COVID-19 variables is used. In general, the trees assign policies in line with our main results on effectiveness: guaranteed incentives are most often assigned, followed by lottery if choice is not available or choice otherwise. The donation incentive is assigned much less than the other incentives. The intuition for why policy learning sometimes leads to an estimated decrease in uptake is that it can only split on two variables and needs to relatively inflexibly

assign treatments to individuals with marginally higher treatment effects of a policy, which then out-of-sample means choosing too often a dominated policy (here the lottery or social incentive). However, even when we assign incentive policies not based on a tree, but on the heterogenous treatment effects from a causal forest (assigning everyone to either guaranteed, lottery, or donation based on where an individual has the largest effect), does not lead to a statistically significant improvement in vaccination uptake.[31]

[31]We estimate uptake by estimating a causal forest, then assign each individual to the policy with the biggest effect and calculate the corresponding uptake. In this case, 91% of individuals get assigned to the guaranteed incentive and 9% to the lottery incentive, whereas only 0.2% get assigned to the social incentive.

E Negative unintended consequences: Impact on long-run vaccinations

Figure E.1: Proportion of participants who got fourth-dose per day after the trial



Note: The figure shows the development of the proportion of participants who got the fourth dose of a COVID-19 vaccine over time in the different treatment conditions (Kaplan-Meier curves). The figure is based on survey data from the trial linked to Swedish administrative records on vaccination, which includes the date of when each participant got vaccinated (N=5,324).

Table E.1: Impact on short-and long-run fourth-dose uptake

Dependent Variable	4th dose uptake within					
	120 days	180 days	240 days	300 days	360 days	660 days
	(1)	(2)	(3)	(4)	(5)	(6)
Guaranteed Condition	0.09	0.18	0.47	1.08	0.71	0.65
	(0.09)	(0.13)	(0.62)	(0.97)	(1.14)	(1.21)
Lottery Condition	0.00	0.05	0.05	-0.16	-0.16	0.02
	(0.01)	(0.07)	(0.59)	(0.95)	(1.13)	(1.22)
Donation Condition	0.00	0.22	0.49	-0.00	-0.72	0.04
	(0.01)	(0.14)	(0.63)	(0.95)	(1.11)	(1.22)
Choice Condition	0.01	0.04	-0.02	-0.19	-0.49	-0.57
	(0.01)	(0.11)	(0.58)	(0.91)	(1.10)	(1.18)
Controls	yes	yes	yes	yes	yes	yes
Observations	5,324	5,324	5,324	5,324	5,324	5,324

Note: The table shows coefficient estimates from linear regressions of fourth-dose COVID-19 vaccination uptake using the pre-registered set of controls. Heteroscedasticity robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E.2: Impacts on other measures of fourth-dose uptake

Dependent Variable	Days to fourth-dose	Days to fourth-dose	Days to fourth-dose	Distance fourth- and third-dose
	(1)	(2)	(3)	(4)
Guaranteed Condition	-22.54	13.69	21.57	-40.34*
	(48.36)	(46.96)	(46.68)	(24.26)
Lottery Condition	-14.95	7.97	15.34	-38.22
	(48.88)	(47.51)	(47.22)	(27.17)
Donation Condition	-10.70	-13.15	-5.84	-5.61
	(49.43)	(48.77)	(48.46)	(26.30)
Choice Condition	24.65	50.93	56.95	-34.57
	(50.15)	(48.89)	(48.59)	(27.31)
Controls	yes	yes	yes	yes
Observations	5,324	3,071	3,111	483

Note: The table shows coefficient estimates from regressions on other measures related to fourth-dose uptake. Column (1) to (3) consider the date when participants got the fourth dose and show coefficient estimates from Tobit regressions to account for the fact that the data is censored. Column (4) shows coefficient estimates from linear regressions of the distance in days between receiving the third and fourth dose of a COVID-19 vaccine on each of the treatment conditions. Specification (1) includes the entire sample, specification (2) only includes participants that got the third dose within 10 months after the trial (allowing us to study fourth dose uptake for at least one year after the trial), specification (3) only includes participants that got the third dose within 16 months after the trial (allowing us to study fourth dose uptake for at least 6 months after the trial). Heteroscedasticity robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

F Behavioral spillovers of incentives on family members

Table F.1: Impact on third-dose uptake of family members, different sets of controls

Dependent Variable	Third dose within 30 days of								
	Partners			Parents			Children		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All Incentives Conditions	6.30*	6.87**	7.05**	-1.63	-2.44	-4.05	-0.65	0.35	-0.29
	(3.30)	(3.30)	(3.56)	(3.59)	(3.59)	(3.86)	(3.68)	(3.60)	(3.91)
Controls	no	yes	yes	no	yes	yes	no	yes	yes
Observations	1,170	1,170	1,170	1,331	1,331	1,331	714	714	714

Note: The table shows coefficient estimates from linear regressions of third-dose COVID-19 vaccination uptake of family members either within 30 days of the trial or up to the end of the follow-up period on indicators for the experimental conditions. Controls include age, gender, date of second dose, and region. Standard errors are clustered at the trial participant level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F.2: Impact on shorter and longer-run vaccination uptake of partners

Dependent Variable	Vaccination uptake within											
	30 Days		60 Days		90 Days		180 Days		360 Days		660 Days	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
All Incentives Conditions	7.05**		5.03		3.12		2.03		2.93		3.50	
	(3.56)		(3.79)		(3.82)		(3.83)		(3.78)		(3.78)	
Guaranteed Condition		7.63*		4.47		0.91		0.18		0.86		1.40
		(4.54)		(4.81)		(4.84)		(4.91)		(4.84)		(4.83)
Lottery Condition		8.13*		4.68		5.05		5.25		6.75		7.79
		(4.70)		(4.92)		(4.99)		(4.93)		(4.80)		(4.78)
Donation Condition		6.24		4.96		1.48		-0.66		-0.14		0.07
		(4.73)		(4.94)		(5.01)		(5.03)		(4.99)		(5.00)
Choice Condition		6.21		5.98		5.12		3.46		4.36		4.87
		(4.65)		(4.85)		(4.86)		(4.86)		(4.78)		(4.78)
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	1,170	1,170	1,170	1,170	1,170	1,170	1,170	1,170	1,170	1,170	1,170	1,170

Note: The table shows coefficient estimates from linear regressions of short- and long-run third-dose COVID-19 vaccination uptake of partners on indicators for the experimental conditions. Controls include age, gender, date of second dose, and region. Standard errors are clustered at the trial participant level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F.3: Impact on shorter and longer-run vaccination uptake of parents

Dependent Variable	Vaccination uptake within											
	30 Days		60 Days		90 Days		180 Days		360 Days		660 Days	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
All Incentives Conditions	-4.05		-5.27		-5.09		-2.44		-3.10		-2.53	
	(3.86)		(3.97)		(3.98)		(4.06)		(4.01)		(4.01)	
Guaranteed Condition		-2.90		-5.94		-4.52		-2.91		-4.76		-3.73
		(4.66)		(4.83)		(4.83)		(4.88)		(4.84)		(4.83)
Lottery Condition		-5.22		-5.58		-7.79		-5.53		-6.02		-5.75
		(4.96)		(5.00)		(5.08)		(5.28)		(5.24)		(5.24)
Donation Condition		-4.06		-4.30		-4.15		-0.67		-1.58		-1.60
		(4.85)		(5.07)		(5.08)		(5.15)		(5.11)		(5.12)
Choice Condition		-4.28		-5.24		-4.08		-0.70		0.28		1.24
		(4.83)		(5.07)		(5.13)		(5.19)		(5.15)		(5.14)
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	1,331	1,331	1,331	1,331	1,331	1,331	1,331	1,331	1,331	1,331	1,331	1,331

Note: The table shows coefficient estimates from linear regressions of short- and long-run third-dose COVID-19 vaccination uptake of parents on indicators for the experimental conditions. Controls include age, gender, date of second dose, and region. Standard errors are clustered at the trial participant level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F.4: Impact on shorter and longer-run vaccination uptake of children

Dependent Variable	Vaccination uptake within											
	30 Days		60 Days		90 Days		180 Days		360 Days		660 Days	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
All Incentives Conditions	-0.29		-2.04		-2.70		-1.56		-0.85		-0.84	
	(3.91)		(4.50)		(4.59)		(4.67)		(4.75)		(4.75)	
Guaranteed Condition		-5.48		-6.04		-6.20		-4.38		-2.72		-2.89
		(4.83)		(5.46)		(5.66)		(6.11)		(6.18)		(6.17)
Lottery Condition		3.74		-0.50		0.92		0.44		2.03		2.53
		(5.12)		(5.55)		(5.79)		(5.83)		(6.17)		(6.14)
Donation Condition		-3.35		-6.69		-10.02*		-8.66		-8.22		-8.54
		(5.16)		(5.75)		(6.05)		(6.49)		(6.51)		(6.54)
Choice Condition		3.58		4.60		3.98		5.84		5.05		5.07
		(5.42)		(6.06)		(6.14)		(6.20)		(6.31)		(6.31)
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	714	714	714	714	714	714	714	714	714	714	714	714

Note: The table shows coefficient estimates from linear regressions of short- and long-run third-dose COVID-19 vaccination uptake of children on indicators for the experimental conditions. Controls include age, gender, date of second dose, and region. Standard errors are clustered at the trial participant level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

G Instructions

CONSENT FORM

Information for Research Participants

We would like to ask if you would participate in a research project. This document provides information about the project and what participation entails.

What is the project and why are you being asked to participate? The project studies attitudes and behaviors related to vaccination against the coronavirus (COVID-19). By participating, you contribute to research in this area. You have been contacted through Norstat to ensure a broad sample of participants from various age groups and locations in Sweden.

The research sponsor is Lund University. The application has been approved by the Ethical Review Authority (diary number 2021-06669-01).

How does the project proceed? This study involves answering a series of questions. Your answers will be linked to vaccination records from the national vaccination registry. To determine if you are suitable to participate, we need you to answer the following:

- I have received a second dose of the COVID-19 vaccine.
- I have not received (or booked an appointment for) a third dose of the COVID-19 vaccine.

[ ] Yes, both are correct
[ ] No

Possible Consequences and Risks of Participation Your participation does not involve any risks.

What happens to my data? The project will collect and register information about you and your opinions. Your responses will be protected so unauthorized persons cannot access them, ensured by an encrypted link between your personal data and the survey responses. Results will be presented in research reports containing only summary statistics.

By participating, you help us better understand decision-making around COVID-19 vaccines. Your survey responses will be linked to data from the national vaccination registry. For this purpose, you will be asked to provide your personal identification number on a separate webpage. Your personal identification number will be kept confidential and researchers will not have access to it.

Click here for more information about the procedure that ensures the confidentiality of your data.

Data Management Neither researchers, Enkätfabriken, Folkhälsomyndigheten, nor any other person will have access to both your personal identification number and your survey responses.

Matching between survey and vaccination data will occur three times: once in spring 2022, once in summer 2022, and once in spring 2023. By the end of 2023, Enkätfabriken will delete

your personal identification number. The data will then be fully anonymized by deleting the anonymized ID number. Anonymized data may be shared with other researchers for future research evaluation.

Your responses and results will be handled so unauthorized persons cannot access them.

The data controller for your personal data is Lund University. According to the EU's General Data Protection Regulation (GDPR), you have the right to access your data handled in the project, correct any errors, request data deletion, or limit the processing of your personal data. However, the right to deletion and limitation does not apply when the data is necessary for the current research.

If you want to access the data, contact Erik Wengström at the Department of Economics, Box 7080, 220 07 Lund. Phone: 046 222 0123, email: erik.wengstrom@nek.lu.se. The Data Protection Officer at Lund University, Kristin Asgermyr, can be reached at 046-222 0000 or via email at dataskyddsombud@lu.se. If you are dissatisfied with the processing of your personal data, you have the right to file a complaint with the Swedish Authority for Privacy Protection.

How do I get information about the project's results? Research reports can be ordered from the responsible researcher (see below). Full reports typically take more than a year to complete. Aggregated information about other participants' decisions is available upon request. No participant will be forced to take part in the results.

Insurance and Compensation There is no special insurance linked to the study as participation does not involve any apparent risks. Upon completion of the survey, you will receive 15 kronor. Additionally, some randomly selected participants will be offered further compensation. The compensation is taxable.

Participation is Voluntary Your participation is voluntary, and you can withdraw at any time without providing a reason, and without affecting your future care or treatment. To withdraw, contact the project leader (see below).

Project Leader The study is led by Professor Erik Wengström, Department of Economics at Lund University. Address: Department of Economics, Box 7080, 220 07 Lund. Email: erik.wengstrom@nek.lu.se. Phone: 046 222 0123.

Want to Save This Information? If you wish to save this information, please print the page as a PDF file and save it on your computer. Alternatively, you can access this information again after the study at the following webpage: Research Study.

Consent to Participate in the Study I have received information about the study and contact details for the responsible researcher to ask questions. I have had the opportunity to save the information.

Location and Date
Name

I consent to participate in the study.

Personal Identification Number Please provide your personal identification number:

Note:
Remember that your personal identification number will be treated confidentially and no one (neither researchers, the survey company, Folkhälsomyndigheten, nor anyone else) will link your personal identification number to your survey responses.
Your personal identification number will be saved by Norstat, the company responsible for survey recruitment, while the survey responses will be saved by researchers. Thus, neither Norstat nor the researchers will be able to link your survey responses to your personal identification number.
Click here for more information about the procedure ensuring the confidentiality of the data we collect.

EXPERIMENTAL INSTRUCTIONS

Survey Preferences

We will now ask a series of questions about your willingness to act in certain ways. Please indicate your response on a scale from 0 to 10 where 0 means "completely unwilling" and 10 means "very willing." You can also use any number between 0 and 10 to indicate where you fall on the scale.

0 = Completely unwilling
1
2
3
4
5
6
7
8
9
10 = Very willing

- How willing are you to donate to charity without expecting anything in return?
- In general, how willing are you to take risks?
- How willing are you to give up something that benefits you today for a better benefit in the future?

How well do the following statements describe you as a person? Answer on a scale from 0 to 10 where 0 means "does not describe me at all" and 10 means "describes me perfectly." You can also use any number between 0 and 10 to indicate where you fall on the scale.

0 = Does not describe me at all
1
2
3

4
5
6
7
8
9
10 = Describes me perfectly

- When someone does me a favor, I am willing to do something in return.
- Being generous is an important part of my self-esteem.
- I try to appear generous to avoid disapproval from others.
- It is important for me to make my own decisions about what I do.
- I am strongly motivated by the money I can earn.
- I assume people only have the best intentions.
- I tend to procrastinate even if I know it would be better to do things right away.
- It is important for me to always behave correctly and avoid doing things people would say are wrong.

Donation

Now the computer will randomly select 10 participants and give them 200 SEK. If you are one of the 10 participants, you have the opportunity to donate some of the money to Save the Children. How much of the 200 SEK do you want to donate to Save the Children? Save the Children is a non-governmental organization that aims to improve children's living conditions. Think carefully about your decision. If you are selected as one of the 10 participants, we will donate the amount you choose to Save the Children and pay you the remaining amount of the 200 SEK.

- How much do you want to donate to Save the Children? (we will pay you the rest)
  0 SEK
  20 SEK
  40 SEK
  60 SEK
  80 SEK
  100 SEK
  120 SEK
  140 SEK
  160 SEK
  180 SEK
  200 SEK

Lottery

Now the computer will randomly select 10 participants and give them 200 SEK. If you are one of the 10 participants, you have the opportunity to buy lottery tickets with the money. How many lottery tickets do you want to buy with this money? Each ticket costs 10 SEK. If you buy one ticket, you have a 1% chance of winning 1000 SEK, and for each additional ticket you buy, the prize increases by 1000 SEK. The money you do not spend on tickets you get to keep. For example:

- If you do not buy any tickets, you keep 200 SEK.
- If you buy 10 tickets, you have a 1% chance of winning 10,000 SEK (and you keep 100 SEK).
- If you buy 20 tickets, you have a 1% chance of winning 20,000 SEK.

Think carefully about your decision. If you are selected as one of the 10 participants, we will pay you the amount you did not spend on tickets. We will also conduct the random draw based on the number of tickets you bought, and if you win, we will pay out the corresponding prize to you.

- How many tickets do you want to buy? (we will pay you the money you do not use)
  No tickets (you get 200 SEK)
  2 tickets (you get 180 SEK and have a 1% chance of winning 2,000 SEK)
  4 tickets (you get 160 SEK and have a 1% chance of winning 4,000 SEK)
  6 tickets (you get 140 SEK and have a 1% chance of winning 6,000 SEK)
  8 tickets (you get 120 SEK and have a 1% chance of winning 8,000 SEK)
  10 tickets (you get 100 SEK and have a 1% chance of winning 10,000 SEK)
  12 tickets (you get 80 SEK and have a 1% chance of winning 12,000 SEK)
  14 tickets (you get 60 SEK and have a 1% chance of winning 14,000 SEK)
  16 tickets (you get 40 SEK and have a 1% chance of winning 16,000 SEK)
  18 tickets (you get 20 SEK and have a 1% chance of winning 18,000 SEK)
  20 tickets (you get 0 SEK and have a 1% chance of winning 20,000 SEK)

COVID-19

We will now ask some questions about COVID-19 and vaccinations.

- Have you ever tested positive for COVID-19?
  [ ] No
  [ ] Yes
  [ ] Don’t know
- Are you in a risk group for COVID-19?
  [ ] No
  [ ] Yes
  [ ] Don’t know

To what extent do you agree with the following statements?

0 = Strongly disagree
1
2
3
4
5
6
7
8
9
10 = Strongly agree

- COVID-19 vaccines are generally safe.
- Vaccines can lead to diseases such as autism, multiple sclerosis, and diabetes.
- I am worried about the side effects of COVID-19 vaccines.
- I am worried about getting sick with COVID-19.

Demographic Information

- What year were you born? (select from dropdown) ▼ 2005 ... <1955
- Do you identify as a woman or a man?
  [ ] Woman
  [ ] Man
  [ ] Neither man nor woman
- Which description best fits you?
  [ ] Single
  [ ] In a relationship but living apart
  [ ] Living with partner
  [ ] Married or registered partner
  [ ] Other, namely _____
- How many children (under 18 years) live in your household?
  [ ] No children
  [ ] 1 child
  [ ] 2 children
  [ ] 3 children
  [ ] 4 children
  [ ] 5 or more children
- What is your main occupation?
  [ ] Working
  [ ] Unemployed
  [ ] Student
  [ ] Retired
  [ ] Other, namely _____
- What is your highest level of education?
  [ ] Primary education or lower
  [ ] Secondary education or folk high school education
  [ ] Post-secondary education (e.g., qualified vocational education)
  [ ] Ongoing higher education
  [ ] Higher education
  [ ] Research education
- In which region do you live? (select from dropdown) ▼ Blekinge ... Other
- Where was your mother born?
  [ ] Sweden
  [ ] Another European country
  [ ] North America
  [ ] South America
  [ ] Africa
  [ ] Middle East
  [ ] Other parts of Asia
  [ ] Oceania
- Where was your father born?
  [ ] Sweden

[ ] Another European country
[ ] North America
[ ] South America
[ ] Africa
[ ] Middle East
[ ] Other parts of Asia
[ ] Oceania

- What is your total monthly income after tax, including any benefits? Also include student loans if you are a student. Answer even if you are not completely sure.
  [ ] 0 - 5,000 SEK after tax
  [ ] 5,001 - 10,000 SEK after tax
  [ ] 10,001 - 15,000 SEK after tax
  [ ] 15,001 - 20,000 SEK after tax
  [ ] 20,001 - 25,000 SEK after tax
  [ ] 25,001 - 30,000 SEK after tax
  [ ] 30,001 - 35,000 SEK after tax
  [ ] 35,001 - 40,000 SEK after tax
  [ ] 40,001 - 45,000 SEK after tax
  [ ] 45,001 - 50,000 SEK after tax
  [ ] 50,001 - 55,000 SEK after tax
  [ ] More than 55,001 SEK after tax

Treatments:

Control group:

We want to encourage you to take a third dose of the COVID-19 vaccine (booster) as soon as it is offered to you, preferably within 30 days after participating in this survey. If it has not been 3 months since you took the second dose, we mean 30 days after the third dose becomes available to you.

Guaranteed incentives:

We want to encourage you to take a third dose of the COVID-19 vaccine (booster) as soon as it is offered to you, preferably within 30 days after participating in this survey. To encourage you to get a third dose, we offer you 200 SEK if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey. We will pay you as soon as possible, but no later than July. Using vaccination data from Folkhälsomyndigheten, we will check if you have been vaccinated within 30 days after participating in this survey. If you have been vaccinated, you will receive 200 SEK.

- I understand that if I take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, I will receive 200 SEK.
  [ ] Yes
  [ ] No
- I understand that if I do NOT take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, I will NOT receive 200 SEK.
  [ ] Yes
  [ ] No

Lottery incentives:

We want to encourage you to take a third dose of the COVID-19 vaccine (booster) as soon as it is offered to you, preferably within 30 days after participating in this survey. To encourage you to get a third dose, we offer you a lottery ticket that gives you a 1% chance of winning 20,000 SEK if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey. We will pay you as soon as possible, but no later than July. Using vaccination data from Folkhälsomyndigheten, we will check if you have been vaccinated within 30 days after participating in this survey. If you have been vaccinated, you will have a 1% chance of winning 20,000 SEK.

- I understand that if I take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, I will have a 1% chance of winning 20,000 SEK.
  [ ] Yes
  [ ] No
- I understand that if I do NOT take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, I will NOT have a 1% chance of winning 20,000 SEK.
  [ ] Yes
  [ ] No

Donation incentives:

We want to encourage you to take a third dose of the COVID-19 vaccine (booster) as soon as it is offered to you, preferably within 30 days after participating in this survey. To encourage you to get a third dose, we will donate 200 SEK to Save the Children if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey. We will send you a receipt as soon as we have donated the money, no later than July. Using vaccination data from Folkhälsomyndigheten, we will check if you have been vaccinated within 30 days after participating in this survey. If you have been vaccinated, we will donate 200 SEK to Save the Children.

- I understand that if I take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, 200 SEK will be donated to Save the Children.
  [ ] Yes
  [ ] No
- I understand that if I do NOT take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, 200 SEK will NOT be donated to Save the Children.
  [ ] Yes
  [ ] No

Choice incentives:

We want to encourage you to take a third dose of the COVID-19 vaccine (booster) as soon as it is offered to you, preferably within 30 days after participating in this survey. To encourage you to get a third dose, we offer you a choice of the following rewards:

- 20,000 SEK with a 1% chance. We offer you a lottery ticket that gives you a 1% chance of winning 20,000 SEK if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey.
- 200 SEK to Save the Children. We will donate 200 SEK to Save the Children if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey.
- 200 SEK. We offer you 200 SEK if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey.

Which reward do you choose? Choose carefully, as your choice will determine your actual reward.
[ ] 20,000 SEK with a 1% chance
[ ] 200 SEK to Save the Children
[ ] 200 SEK

Guaranteed incentives confirmation:

You chose to receive 200 SEK. We offer you 200 SEK if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey. We will pay you as soon as possible, but no later than July. Using vaccination data from Folkhälsomyndigheten, we will check if you have been vaccinated within 30 days after participating in this survey. If you have been vaccinated, you will receive 200 SEK.

- I understand that if I take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, I will receive 200 SEK.
  [ ] Yes
  [ ] No
- I understand that if I do NOT take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, I will NOT receive 200 SEK.
  [ ] Yes
  [ ] No

Lottery incentives confirmation:

You chose to receive a 1% chance of winning 20,000 SEK. We offer you a lottery ticket that gives you a 1% chance of winning 20,000 SEK if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey. We will pay you as soon as possible, but no later than July. Using vaccination data from Folkhälsomyndigheten, we will check if you have been vaccinated within 30 days after participating in this survey. If you have been vaccinated, you will have a 1% chance of winning 20,000 SEK.

- I understand that if I take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, I will have a 1% chance of winning 20,000 SEK.
  [ ] Yes
  [ ] No
- I understand that if I do NOT take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, I will NOT have a 1% chance of winning 20,000 SEK.
  [ ] Yes
  [ ] No

Donation incentives confirmation:

You chose to donate 200 SEK to Save the Children. We will donate 200 SEK to Save the Children if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey. We will send you a receipt as soon as we have donated the money, no later than July. Using vaccination data from Folkhälsomyndigheten, we will check if you have been vaccinated within 30 days after participating in this survey. If you have been vaccinated, we will donate 200 SEK to Save the Children.

- I understand that if I take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, 200 SEK will be donated to Save the Children.
  [ ] Yes
  [ ] No
- I understand that if I do NOT take a third dose of the COVID-19 vaccine within 30 days after participating in this survey, 200 SEK will NOT be donated to Save the Children.
  [ ] Yes
  [ ] No

Intentions

Do you think you will take a third dose of the COVID-19 vaccine within 30 days after participating in this survey?
[ ] Yes
[ ] No

Thank You

We encourage you again to take a third dose of the COVID-19 vaccine (booster) as soon as it is offered to you, preferably within 30 days after participating in this survey.

[Remember that we pay 200 SEK if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey.]

[Remember that we offer a 1% chance of winning 20,000 SEK if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey.]

[Remember that we will donate 200 SEK to Save the Children if you take a third dose of the COVID-19 vaccine within 30 days after participating in this survey.]

Click here for information on how to book a third dose of the COVID-19 vaccine in your region (the link opens in a new window). Remember to submit your responses even if you click the link above!

Thank you for participating in our study! If you have any questions, contact the responsible researcher, Professor Erik Wengström, Department of Economics at Lund University. Address: Department of Economics, Box 7080, 220 07 Lund. Email: erik.wengstrom@nek.lu.se. Phone: 046 222 0123.

Click the arrow below to submit your responses.