Gambling for Retirement: The Economics of Savings Lotteries*

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Abstract

In a nationwide field experiment with Colombia's public pension fund, we study the effects of savings lotteries that offer the chance to win a prize if households meet a minimum savings threshold during a qualification period. We find that lotteries significantly increase savings during the qualification period, but this effect is short-lived: savings decline by nearly the same amount afterward, offsetting most of the intended policy impact. Following subjects up to 4 year post-intervention, we find that lotteries fail to generate a habit to save. Lotteries are also regressive, do not target present-biased individuals, and unintentionally lead some households to forgo eligibility for life and disability insurance. Model-based estimates suggest that workers misperceive the winning odds of lotteries and that lotteries reduce welfare. We leverage a second large field experiment that randomizes deterministic savings subsidies ("matches") and show that they are more cost-effective in increasing savings than lotteries and that they induce post-treatment habit formation.

Keywords: Retirement Savings, Prize-Linked Savings, Lotteries, Household Finance, Development Economics, Field Experiment

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1 Introduction

Since Pigou (1920), economists have studied the use of taxation to optimize resource allocations in society. Taxes and subsidies steer behavior by providing deterministic incentives that operate through people's price elasticities. One alternative to subsidies is the use of *probabilistic* incentives—"Pigouvian lotteries"—that reward socially desirable behavior with the chance to win a prize. While Pigouvian lotteries have not yet received much attention among academics, these incentives are ubiquitous in real world policies. Lotteries have been used to incentivize vaccinations, energy savings, public transport use, tax reporting, or retirement savings.[1] This paper studies the latter in the context of a large-scale field experiment with the Colombian government.

The case for the use of savings lotteries includes a number of arguments that lack rigorous empirical evidence. One argument is that temporary lottery incentives may change people's long-run *habits*, so that the targeted behavior persists beyond the intervention period. Another argument is that of *"Paternalistic Targeting:"* people who undersave due to self-control problems and myopia may also be those who are attracted by gambling. It has been plausibly argued that the lower part of the wealth distribution would be particularly responsive to lotteries (Kearney et al., 2010), which has been shown to be the segment of the population with lower self-control (Lockwood, 2020). Finally, lotteries could be more *cost-effective* than traditional Pigouvian subsidies ("matches") as they pay only the winner instead of everybody who saves.

We investigate these and other aspects in two nation-wide natural field experiments with the Colombian public pension fund, *Colpensiones*, and observe subjects for 6 years. The pension fund invests contributions made by informal workers into a conservative inflation-protected portfolio that represents the only way for most of these workers to access capital markets. The economic frictions that these savings vehicles try to address are household myopia regarding future retirement needs (Beshears et al., 2018; Gomes et al., 2021) and the fiscal externality from entering retirement without adequate savings, which results in publicly funded support.

Field Experiment using Lotteries In the first experiment, the pension fund randomly offered households the chance to win a prize of size X if the household deposited at least Y Colombian

[1] For example, Sweden rewarded law-abiding drivers with entries into a cash prize draw funded by speeders' fines (Schultz, 2010), Palo Alto's energy-savings raffle used lotteries to encourage energy conservation (Browne, 2016), and Indian Railways' Lucky Yatra campaign turned train ticket purchases into lottery tickets (Team Impact, 202). Other examples of such lotteries include Norway's recycling lottery (Norsk Tipping, 2021), Ohio's "Vax-a-Million" vaccine lottery (Walkey et al., 2021), Singapore's INSINC transit lottery (Moleskis, 2023), and Hong Kong's vaccine lottery (Agence France-Presse, 2021). For savings lotteries see, for instance, Guillén and Tschoegl (2002), as well as Gertler et al. (2023) for an overview of savings lottery incentives globally. Savings lotteries also became legal in the United States in 2014 under the American Savings Promotion Act (United States Congress, 2014).

Pesos (COP) in their illiquid retirement account, during some qualification period T. The experiment randomized $X \in \{\text{Cash, House, House+Cash}\}$, either offering the household the chance to win a small cash prize, a house, or both. To qualify for the Cash lottery, households had $T = 2$ weeks to save $Y =$ 55,000 COP (= 17 USD in 2019, 15% of biweekly income). For the House lottery, the deadline was $T = 6$ months with a savings threshold of $Y =$ 500,000 COP (= 153 USD, 12% of biannual income). Subjects in the Cash lottery treatment were offered in total 18 consecutive cash lotteries (every two weeks); subjects in the House lottery received two lottery offers (once a semester). Finally, the third treatment group received both biweekly Cash lottery offers and biannual House lottery offers. Control subjects received no offers.

We document the following main results. Lotteries change behavior: The probability for savings to increase above the qualification threshold increases by 8.6% for the Cash lottery and 15% for the House lottery. Locally, effects of the lotteries are even stronger: The probability for savings to bunch at *exactly* the qualification threshold increases by 600% and 40% for the Cash and House lottery, respectively. Combining both lotteries (House+Cash) has intermediate effects. At first sight, it appears that the policy causes its intended effect. However, by observing savings dynamics, we find that the treatment effect on savings is *negative* directly after the lottery deadline has passed. This means that households strategically shifted their savings from the future into the qualification period in order to meet the lottery threshold. These savings are then missing after the intervention, which eliminates much of the total effect of the lottery on savings. The overall effect on savings (counting both qualification and post-qualification period) is *zero* for the Cash lottery and only 2.2% for House lottery.

We observe further that the lotteries interfere with other retirement savings incentives. As a hybrid social security system, Colpensiones also offers informal workers free disability and life insurance if they make at least 6 deposits throughout the year. The lotteries cause households to bundle savings into the lottery qualification periods and to save *less frequently*, which disqualifies them from insurance coverage. This behavior is likely a mistake rather than a rational choice because rational workers would simply deposit more frequently during the lottery period, thereby qualifying for both the lottery and the life insurance (i.e., there is no economic tradeoff). We also show through survey evidence and revealed preference measures that the welfare loss from forgoing insurance is likely large, exceeding the benefits of the lottery. Our evidence suggests that Pigouvian lotteries can crowd out attention to other retirement policies.

We then follow households for 4 years post-intervention and find that treated subjects do not increase savings in the long-run. This is important because the primary motive of savings lotteries is often not to increase savings *once* but rather to establish a habit of saving more permanently.

We can further rule out that lotteries induce a gambling addiction to future savings lotteries.

Specifically, we do not find that treated households are more inclined to respond to future (non-randomized) lotteries that Colpensiones uses until this day. This rejects the hypothesis that these lotteries cause addictive behavior in the spirit of Becker and Murphy (1988) —a natural concern with policies that use gambling to change behavior.[2]

To understand how well-targeted lotteries are, we obtain data on stated time preferences, as well as administrative data proxies for wealth. Results suggest that impatient workers have no larger treatment effects than patient workers. In addition, lotteries tend to be regressive as wealthier workers are more likely to qualify.

Field Experiment using Matches In the second experiment, we ask whether deterministic match incentives provide a more favorable outcomes than lotteries. Three years after the first experiment, Colpensiones randomly matched M COP to every savings account that saved any positive amount within 8 weeks. We find that these incentives increase weekly savings by on average 33–49% depending on the match size. Different from lotteries, households do not respond strategically by pre-poning savings. In addition, we find positive habit formation as post-treatment savings increase by 23–31%. We estimate that moderate matches of $M = 2,500$ COP are *7.5 times* more cost-effective in increasing savings than lotteries. Thus, although matches pay *everyone* who saves while lotteries only pay a few winners, matches are more cost effective.

Our final exercise estimates household primitives and welfare. We show theoretically that household's subjective odds to win the lottery can be empirically recovered by comparing the savings response to lotteries with the response to matches. Assuming CARA preferences, we find that households underestimate the winning probability of cash lotteries (small prizes) while overestimating the odds of house lotteries (large prizes). This is consistent with models of probability weighting (Kahneman and Tversky, 1979) and selective attention (Barberis, 2013; Bordalo et al., 2012; Kőszegi and Szeidl, 2013) where large prizes cause subjective odds that are overly optimistic.[3]

The effects of lotteries on household welfare and social welfare turn out to be negative, mostly because households lose valuable life and disability insurance. From a policy perspective, our results imply that lotteries should be designed in a way not to interfere with other existing incentives that are tied to specific savings behavior. Further, they should likely include few but large prizes, and be more directly targeted to present biased workers.

[2] A caveat is that we cannot rule out whether savings lotteries affect demand for other lotteries we do not observe (e.g. gambling in casinos).

[3] The comparison between deterministic and stochastic incentives also connects to recent evidence on *intrinsic* preferences for randomization, where individuals value stochastic choice itself beyond its expected utility (Agranov and Ortoleva, 2022).

Literature Our paper ties into a small strand of work that discusses lotteries as tools for public policy. Early research has studied how lotteries can be used to boost charity contributions in fundraising (Landry et al., 2006; Lange et al., 2007a,b) or attract savers to invest into UK Premium Bonds that have a lottery-like element (Tufano, 2008). Other complementary work has studied the effect of prize-linked savings account of private banks on demand for new bank deposits and savings (Cole et al., 2022; Cookson, 2018; Gertler et al., 2023). We mainly differ from this work i) by studying public retirement policy instead of private marketing strategies, together with a quantification of welfare, ii) documenting unintended consequences and long-term dynamics of Pigouvian lotteries, and iii) by providing a comparison to deterministic savings subsidies from a complementary field experiment.[4] Existing work has shown that gambling is used to meet anticipated expenditures (Herskowitz, 2021) and that savings accounts with a lottery feature can substitute for gambling (Cole et al., 2022; Cookson, 2018). While our results show that savings lotteries are not addictive, it may well be possible that they reduce demand for outside option gambling, which we do not observe. Atalay et al. (2014) and Filiz-Ozbay et al. (2015) study savings lotteries in laboratory experiments, finding substantially larger effects than our field experiment. We complement this evidence by showing that these policies may yield much more conservative outcomes once they are taken to a real-world setting. Lockwood et al. (2024) evaluate state-run lotteries, which are used to raise tax revenue. This is conceptually different from the corrective ("Pigouvian") lotteries we study, whose purpose is to incentivize behavior that governments consider socially desirable.[5]

We also add to recent studies showing that retirement savings policies may backfire. Important work has shown that automatic enrollment of workers into retirement plans boost savings temporarily but that these effects fade away in the long-run as worker switch employers (Choukhmane, 2019). Choukhmane and Palmer (2024) find that increasing minimum savings contributions reduces savings on other savings accounts. Similarly, Medina (2021) shows that reminders about credit card fees reduced late payments on credit cards but increased overdraft fees on deposit accounts. We find a new dimension along which paternalistically motivated policies can backfire, such as by reducing future savings and reducing insurance coverage.

Our evidence further helps understanding behavioral public policy in developing countries

[4]The only field experimental evidence we are aware of is Gertler et al. (2023) who study how banks' advertisement of lotteries through posters and loud-speaker cars affects openings of deposit accounts. We improve upon this important work by observing *everybody* who is treated rather than the subset of people who select into opening an account. This also advances our ability to understand long-term effects on savings since our households could not select into having a savings account based on potential unobservables that may correlate with savings behavior. Another difference to this prior work is that our experiment can speak to the comparison with matches, to household's subjective winning odds, and to gambling addiction to savings lotteries.

[5]Relatedly, Kuhn et al. (2011) investigate how state-run lotteries affect behavior of the winner and the social circle around them.

(Kremer et al., 2019) by speaking to psychological components of lotteries, including habit formation, addiction, and subjective beliefs.

In the following, Section 2 describes the experiment and Section 3 the main results. In Section 4, we present results from a second, complementary, field experiment on deterministic savings matches. Section 5 uses the combination of the lottery and match experiment to structurally estimate households' subjective odds of the lotteries. We conclude in Section 6.

2 Lottery Experiment

2.1 *Partner Institution*

In response to the exclusion of informal workers earning below the minimum wage from the formal pension system, *Beneficios Económicos Periódicos* (BEPS) was created by the Colombian government in 2015. Administered by Colpensiones, BEPS is a hybrid social security scheme that offers a retirement savings option for informal workers who make up 60% of the workforce.[6] To save, BEPS participants make deposits with a defined minimum amount ($5,000 COP) up to an annual limit which was set at $1.6 million COP in 2019 ($489 USD). The contributions are invested in a inflation-protected equity portfolio. At retirement age, bimonthly annuities are paid out to participants based on the total portfolio value as well as a one-time government match of 20% on their cumulative contributions (Villa and Benitez, 2023).

Fiscal externalities The fiscal externality of undersaving in Colombia arises because individuals who reach retirement without qualifying for a contributory pension or without having accumulated enough savings in the BEPS system become eligible for the non-contributory old-age program *Colombia Mayor*. This program provides a modest social assistance transfer of about 80,000 to 100,000 COP per month (roughly USD 20–25), which corresponds to around 60% of the extreme poverty line. Although small, these transfers represent a fiscal burden since they are funded from general revenues, and they currently reach close to two million older Colombians. Thus, every peso of private undersaving increases the likelihood of public support through *Colombia Mayor*, generating a fiscal externality that justifies Pigouvian incentives to save. This motivates Pigouvian incentives above and beyond concerns about household myopia and present bias.

[6] In addition to BEPS, there is a non-contributory cash transfer scheme for workers that have failed to save enough to earn the right to a pension. The transfers are low (60% of the extreme poverty line) and are targeted towards the extremely poor. The scheme covers roughly 2 million beneficiaries (25% of those older than 65) and is the "least generous non-contributory pension systems among emerging market economies" (ILO, 2021).

2.2 *Experimental Details*

To encourage savings by BEPS account holders, Colpensiones implemented a prize-linked savings program in April of 2019. The program involved two types of lotteries: i) small "cash prizes", raffled twice a month, and ii) large "house prizes" twice a year.

Cash Lotteries A total of 18 cash lotteries took place every two weeks on Wednesday between April 24 and December 18. Each lottery consisted of 200 cash prizes with a value of $100,000 COP ($31 USD). To be eligible for a ticket, account holders were required to save $55,000 COP ($17 USD) over two weeks, ending on the Sunday prior to the lottery. They could earn earn a ticket for each $55,000 saved, which corresponds to 7.7% of the average monthly informal wage.

House Lotteries The large house lotteries each consisted of one prize worth a maximum amount of $102,000,000 COP ($31,140 USD in 2019). The prize could be applied toward the purchase of a house, all associated fees, furniture, and related equipment. The house lotteries took place twice in the year, on July 3, 2019 and January 15, 2020. To be eligible for the first lottery, clients had to save at least $500,000 COP ($153 USD) in the first semester of the year (January 1 to June 30). Similarly, to be eligible for the second lottery, clients had to save at least $500,000 COP during the second semester of the year (July 1 to December 31, 70% of the average monthly informal wage).

Treatment Assignment Households were randomly offered one of three lotteries: i) the Cash lottery (N = 14,201), ii) the House lottery (N = 96,337), and iii) both lotteries in combination (N = 14,201). Subjects were informed about the respective lottery via bi-weekly SMS text messages on the second and fourth Thursday of each month (see Figure A1 for English translation of these messages). A fourth group received only savings reminders and served as the control group (N = 96,270).

Technically, the pension fund randomized the text messages rather than the lotteries. This means that the raffle of a lottery always drew among all households who qualified for that lottery, independent of the treatment they were assigned to. However, because the text messages were the only way that Colpensiones informed households about a lottery, this randomization is almost as-if the lotteries were directly randomized. A potential caveat are spillover effects: households that were assigned to one lottery might have learned about another lottery from other subjects. We show in Appendix D, that in this case, our estimates correspond to a lower bound of the true treatment effect: If control subjects learn about the lottery this leads to an upward bias in control group savings, thereby attenuating the estimated treatment effect. However, further below, we provide empirical evidence against treatment spillovers: Households in one treatment

group (e.g., Cash lottery) bunch exactly at the qualification threshold of the lottery they have been assigned to (e.g., $55,000 COP), but never at thresholds of other treatment groups (e.g., $500,000 COP).

Timeline Figure 1 displays the timeline of the lotteries. The first message was sent on April 11, and subjects had two weeks to save at least $55,000 COP between April 8 and April 21 to be eligible for the first Cash lottery. They received the second message on April 25, informing them about another Cash lottery with the same conditions.[7] In total, there were 18 such cash lotteries and the final Cash lottery eligibility period ended on December 15. Subjects were not informed at any time that these lotteries would be repeated, but they likely learned over time that the same offer would be made to them again two weeks from now.

Subjects randomized into the House lottery received the first message on April 11 and had to save $500,000 COP until June 30th to qualify for the first House lottery. To qualify for the second House lottery, they had to save the same amount between July 1st and December 31st.

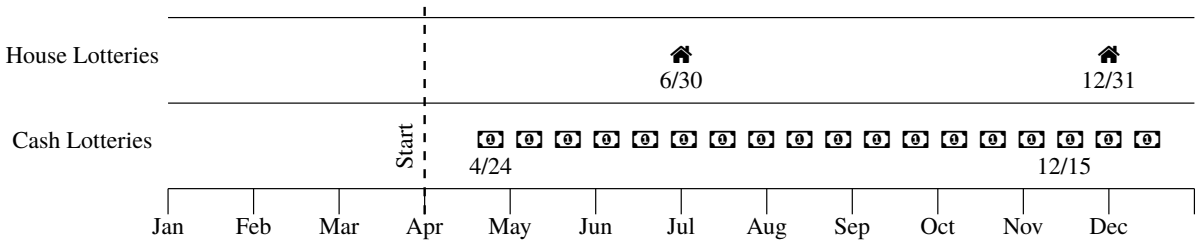
Note that in the first House lottery, subjects were only informed roughly 3 months prior to the deadline, while in the second House lottery they were informed 6 months before.[8] However, even during the first lottery, subjects were informed that their entire cumulated savings of 6 months (starting 1st of January 2019) were eligible to meet the threshold. Thus, while the two house lotteries differ in terms of when subjects were informed, they both use 6 months of savings to determine whether subjects qualified.

Finally, subjects in the Cash+House treatment received the combined house and cash messages.

[7]The lottery periods began on Mondays and messages were sent on the following Thursday. The drawings to determine the winners were performed on the first Wednesday following the end of the eligibility period. For example, the eligibility period for the second Cash lottery was between April 22 and May 5, subjects received the SMS on April 25, and the drawing took place on May 8.

[8]This is simply because Colpensiones decided to implement the policy after the first quarter of the year but wanted to split the two lotteries into semesters because they planned to keep this schedule for future lotteries.

Figure 1: Experiment Timeline in 2019



Notes: Figure presents the timeline of the lottery drawings and experiment implementation. Treated households were offered the first lottery in April 2019. The icons indicate the dates of the lottery drawings for the respective lotteries. In the Cash lottery group, each two weeks a new lottery would be offered with a two week qualification deadline. The last Cash lottery ended on Dec 15. In the House lottery group, there were only two lotteries; one with a savings deadline at June 30, and a second one with a deadline at December 31, 2019. Subjects were never informed that one lottery would follow another.

Data BEPS provides us with a rich dataset that details each deposit that households made with the pension fund. Every deposit includes a time stamp, the amount deposited, and a household ID. We observe deposits for 6 years, starting in January 2018 and ending in January 2024. Table A4 in the Appendix reports summary statistics across treatment groups. In total, we observe 221,009 accounts in our data. In the year prior to the experiment, 2018, the average account holder saved COP 96,000 ($\approx$ 35 USD in 2018). Around 70% of accounts saved at least once a year. 70% of savers are female, which is in line with more women working in the informal sector. The average age is 50 years, which is only 7 and 12 years below the official retirement age for women (57) and men (62) in Colombia, respectively. Because BEPS was introduced in 2015, the average age of the account is only around 2 years. We also observe each account holder's welfare index, called SISBEN, that is assigned by the Colombian government to identify beneficiaries of social assistance programs. The composite welfare index is mostly composed using information on income, health, and access to basic services (e.g., electricity) and ranges from 0 to 100 (from poorest to richest). The average SISBEN score of account holders in our sample is 31.5, which the index labels as "moderate poverty."

All observables are balanced across groups, confirming successful randomization.

3 Results

Savings lotteries are a nonlinear savings incentive that only rewards savings above threshold $\underline{s}$. This incentive should increase i) the density at $\underline{s}$ and ii) the probability that savings are above

$\underline{s}$.[9] To test for this, we construct an indicator variable $y_{it} \in \{0, 1\}$ for subject i. In both of our empirical specifications, we regress outcome y_{it} of subject i on treatment vector τ_i:

$$y_{it} = \alpha + \beta\tau_i + \lambda_t + \epsilon_{it} \tag{1}$$

In our first specification, $y_{it} = \mathbf{1}(s_{it} = \underline{s})$ and in our second specification we use $y_{it} = \mathbf{1}(s_{it} \geq \underline{s})$. λ_t are month fixed effects, ϵ_{it} is the residual. We run this regression for biweekly savings and set $\underline{s}$ =\$55,000 and for biannual savings and set $\underline{s}$ =\$500,000.

Figure 2 presents estimates estimates of our treatment effects. Panel a, displays differences in the probability to bunch exactly at the threshold. In any 2-week period, the probability to adjust savings equal to the Cash lottery threshold increases by 0.2 percentage points for the Cash lottery. This effect is a large relative increase of 600% compared to the Control group. The House+Cash group shows a slightly smaller treatment effect. As expected under randomization, subjects in the House treatment did not bunch at the Cash threshold. The last of bunching in the house lottery is consistent with subjects in the house lottery not learning about the cash lottery.

The House lottery increases the probability of biannual savings to bunch at the higher savings requirement of 500k COP by around 0.1%, which is an increase of 40% relative to Control. Subjects in the Cash group do not bunch at this threshold more frequently than Control, again confirming internal validity of the estimates. Bunching in House+Cash is half the effect than in House. Thus, bunching is attenuated at both thresholds in House+Cash relative to the respective individual lottery treatments. One explanation is that the two prizes compete for attention so that adding the Cash prizes to the House Lottery makes the latter less effective (Rodemeier and Löschel, 2025).

In Panel b, we look at differences in the probability to qualify for the two lotteries. The House+Cash lottery has the largest impact on the probability to meet the Cash threshold, increasing it by around 0.2 pp (11% relative to Control). The impact of the Cash lottery in isolation is smaller, around 0.14 pp (8% relative to Control). We further see that even subjects in the House lottery are more likely to pass the threshold of the Cash lottery. This effect is mechanical because saving for the house lottery makes one more likely to qualify for a Cash lottery. This effect differs importantly from Panel a, where only households in the Cash treatment had an incentive to *exactly* bunch at the Cash threshold, and only House treatment subjects had an incentive to bunch at the and the higher threshold.[10]

Turning to the probability to qualify for the House lottery, we find an increase of 0.1pp for

[9] In a frictionless world, a rational model would only predict (i) but with lumpy savings workers may not precisely target $\underline{s}$ and instead choose more diffused savings above $\underline{s}$.

[10] Thus, Panel a serves as a check of treatment assignment, while Panel b is more relevant for economic interpretation.

the House lottery ($p < 0.01$), which is 0.3% compared to control. House+Cash increase the likelihood by 0.04pp but this effect is not statistically significant. There is no effect of the Cash treatment.

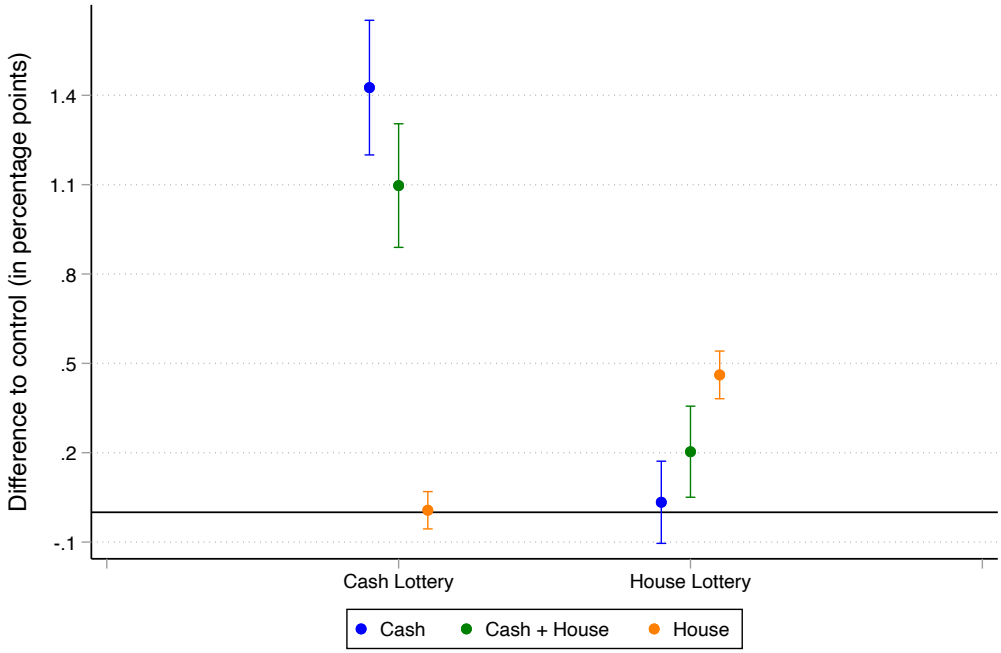
Result 1: *Lotteries cause savings to bunch at and above the qualification threshold.*

Overall, these results indicate that households bunch above the lottery thresholds and, at first sight, seem to suggest that lotteries had the intended effect by increasing savings. However, the treatment effect dynamics reveal a different story. Figure A3 shows the treatment effects on *savings* in percent over time.

The plots start in January 2019. The first blue bar is April 11th where subjects received the first lottery offers. The last blue bar is December 31st when the qualification period for the second House lottery ends. For the House and House+Cash lotteries, we plot monthly savings effects. The second dashed bar is the end of the first and the start of the second House lottery (July 1). For the cash lottery, we report weekly savings effects since there is 18 biweekly cash lotteries. Each shaded beige bar indicates the week in which a cash lottery ended.

Figure 2: Bunching at and above Qualification Threshold

(a) Savings = Threshold



(b) Savings $\geq$ Threshold

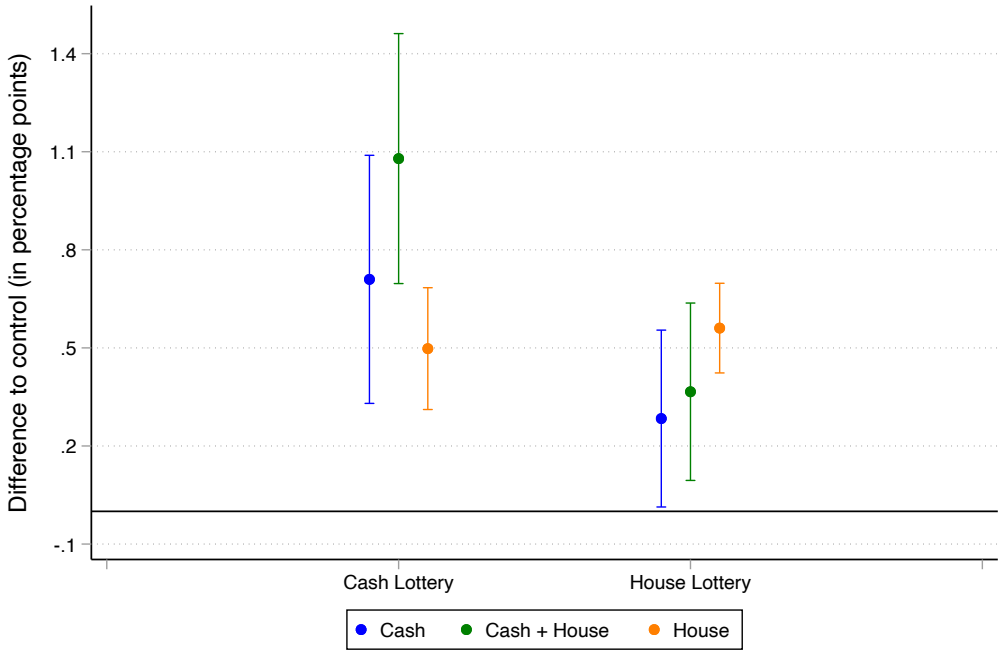
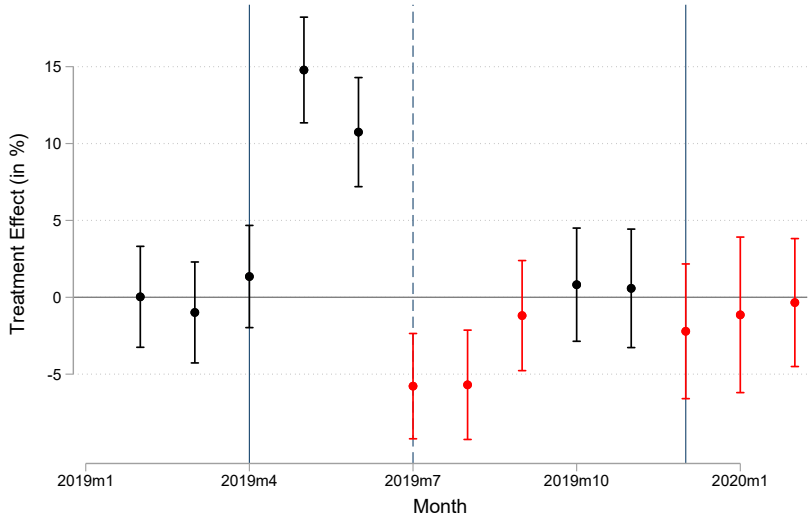
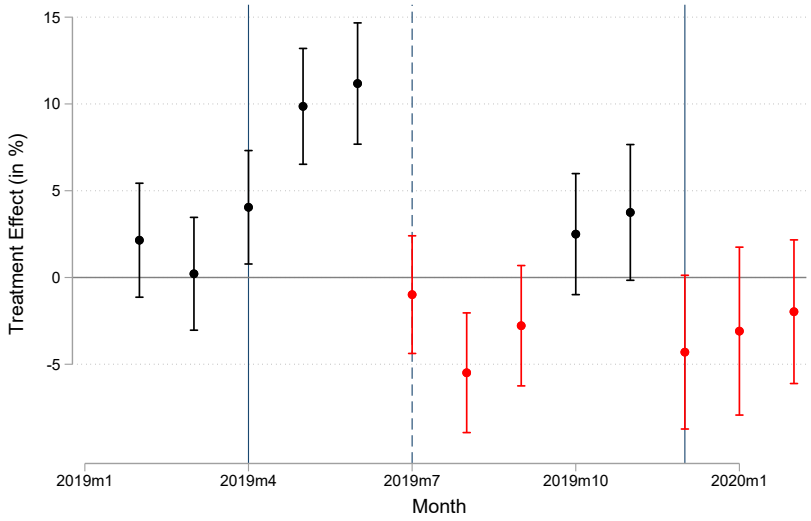


Figure 3: Aggregate Treatment Effects on Savings

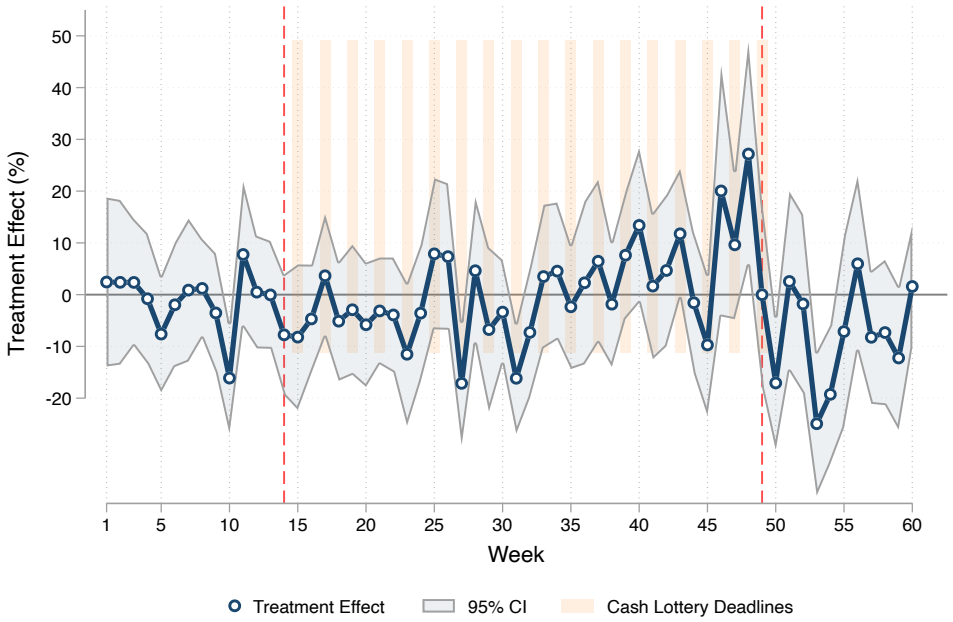
(a) House



(b) House+Cash



(c) Cash



We do not observe a differential savings response between treatment groups and Control in any of the plots prior to April 11th, in line with successful randomization.[11] In Panel A, monthly savings increase immediately upon receiving the offer, as visualized by the first positive spike. The effect is sizeable in relative terms as monthly savings increase by up to 15%. Importantly, after the first qualification period for the House lottery has been reached, the treatment effect becomes statistically significantly *negative*, around -5%. This indicates that subjects *strategically* brought forward savings from future periods into the qualification period in order to meet the threshold. These savings are then missing in future periods, which causes a drop in savings once the lottery has been raffled. Over time, savings recover and the treatment effect remains around zero until the rest of the year. The second house lottery does not seem to induce any additional savings response.

In Panel B, we plot the effects for the House+Cash lottery. We observe a similar, albeit slightly lower effect on savings during the first qualification period of the house lottery. The spike in savings is again followed by a slump of negative treatment effects. Around October, the patterns substantially diverge from what we saw in Panel A. Treatment effects now become positive, again, and hover around 3-4%, statistically significant at the 10%-level. The positive effects are again followed by a negative slump in savings at the very end of 2019, just after the end of the last Cash lottery on Dec 15 (but before the end of the second House lottery on Dec 31). This suggests that the positive treatment effects that started in October are likely driven by the Cash lotteries rather than by the second House lottery.

Panel C confirms this by illustrating that the effects of the Cash lottery really gain most traction around October. Towards the end of the last cash lotteries, treatment effects are as large as 25% relative to control. Treatment effects are noisy throughout the time series because we are plotting fine-grained weekly savings. In Figure A1, we show more statistically robust results by plotting monthly effects for the Cash lotteries as well.

Treatment effects again become *negative* after the last cash lottery has passed, indicating that even for this treatment subjects have strategically moved savings from the future into the qualification periods. The negative dip starts immediately after the deadline of the last Cash lottery and still before the deadline of the second House lottery, which further confirms our evidence from Figure 6 that subjects in the Cash treatment remained unaware of the House lotteries.

Table A6 in the Appendix reports effects on the total amount saved during various time intervals. For the entire experiment, from April to the end of December, both the Cash and the House+Cash lotteries have precisely estimated null effects on aggregate savings. The House lottery in isolation increased savings by around 2.2% ($p < 0.05$). Importantly, as we show in

[11] This is also true for 2018, see Figure 4.

Panels b and c of the table, this effect is entirely concentrated around subjects who saved a positive amount in the previous year. There is no effect on inactive subjects (those who did not save a positive amount in the previous year).

Result 2: *Lotteries increase savings during the qualification period but cause a negative effect on savings afterwards. The overall effect on savings ranges from zero to 2.2%.*

3.1 *Long-Run Effects*

Even if the policy did not substantially induce one-time savings, it may still be beneficial if it established a habit of savings. We study whether the treatments induced a habit of savings using additional administrative data from BEPS that follows subjects behavior 4 years post-treatment.[12] We plot the time series of treatment effects of our experimental interventions in Figure 4, starting in January 2018 and following households until January 2024. The red solid lines represent start and end of our experiment, while the dashed lines show the deadlines of the lotteries in future years (always Dec 31st).

Under habit formation, we would observe positive treatment coefficients after January 2020. Instead, we are able to rule out even small positive treatment effects. If anything, coefficients are mostly negative. Interestingly, coefficients are most negative and last for the longest for the Cash Lottery. This can be explained by the previous observation that the Cash lottery effects started to become largest towards the end of 2019. Subjects seemed to have financed much of this increase by moving savings out of the beginning of 2020 into the end of 2019.

Result 3: *Savings lotteries do not cause habit formation.*

We also do not observe that household who were treated during our experiment respond differently around the deadline of lotteries in future years. This is helpful in understanding whether savings lotteries are addictive. If lotteries caused lottery-addiction, randomly exposing subjects to a lottery once would cause households to be more inclined to respond to future lotteries (Becker and Murphy, 1988). We find no evidence of this: different from our experimental time period, there is no positive spike in savings around the (dashed) future savings deadlines.

Thus, the evidence in Figure 4 rules out both habit formation in savings (no positive *shift* in post-treatment savings), and an induced gambling addiction for savings lotteries (no spike

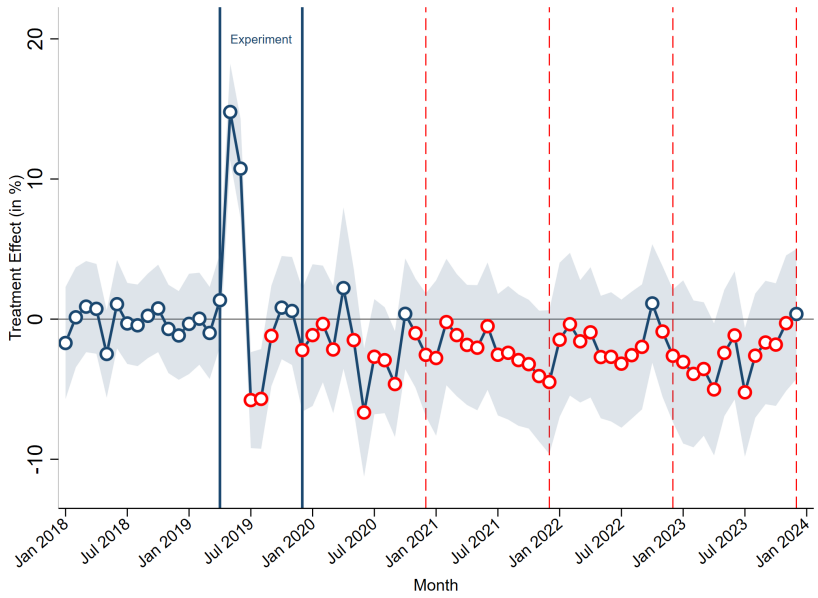
[12] During these years, BEPS also ran savings lotteries but they were not randomized and had different thresholds and prizes. Qualification periods for the lotteries were also different: they always started on January 1st and had a deadline of Dec 31st.

around threshold).

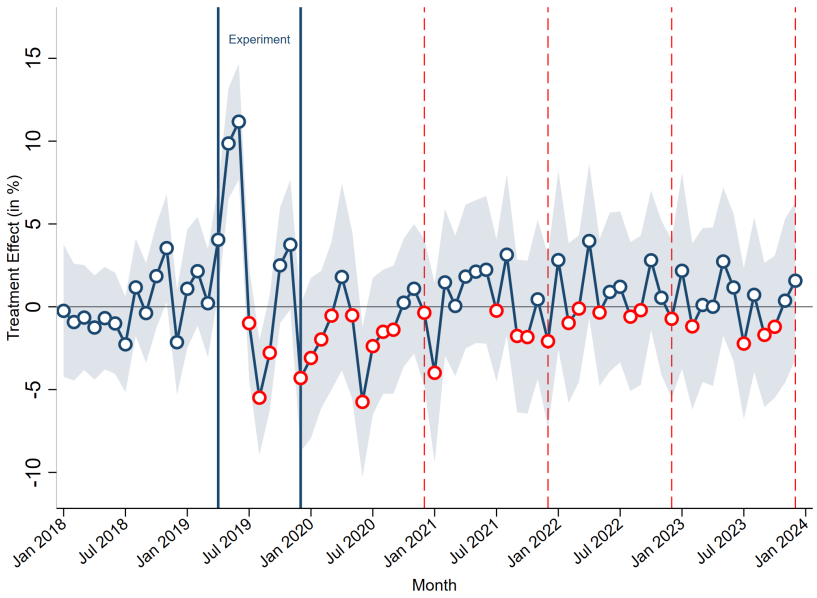
Result 4: *Savings lotteries do not cause a gambling addiction.*

Figure 4: Long-Run Effects and Responses to Follow-up Lotteries

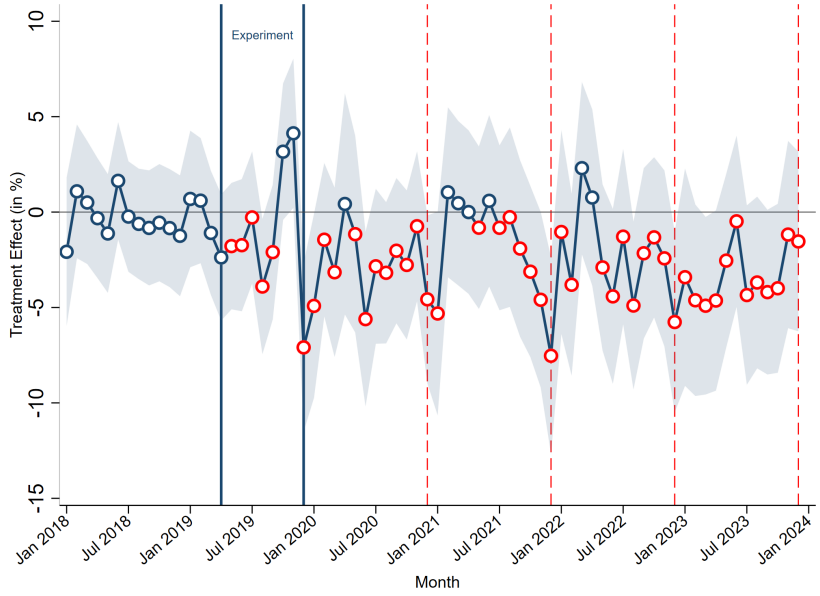
(a) House



(b) House+Cash



(c) Cash



3.2 *Effects on other Financial Assets*

As previously discussed, BEPS also offers access to free basic life and disability insurance. To qualify, account holders are required to make at least six deposits in year, or save the target minimum yearly contribution. The target is equal to six days of a legal monthly minimum salary, which was $150,000 COP and $157,000 COP in 2018 and 2019, respectively. Coverage is provided for a full calendar year conditional on qualification in the prior year and includes physical disability, chronic illness, life insurance and a grant to cover funeral services.

A savings promotion like a lottery has the potential to expand important insurance coverage among the poor. This could be true even if subjects do not increase aggregate savings but simply deposit more frequently to meet the threshold of 6 deposits per year.

In Figure 5, we compute the cumulative probability to qualify for life & disability insurance in the following year. For illustrative purposes, we pool across all treatments, but effects look similar across each of the lotteries. For each subject i and year t, we construct a qualification dummy y_{it} that is equal to one if either 6 deposits have been made in this year, or the total amount of savings is equal or above COP 157,000. At least one of the two requirements has to be met to receive insurance coverage in the following year. We then regress y_{it} on the treatment in each year, separately, and plot the treatment coefficients in the figure.

As expected, there is no effect in 2018, prior to treatment assignment. During the treatment year, 2019, the lotteries cause a statistically significant drop of 0.5 pp in the share of households that qualify for insurance. We explore this pattern in the data and find that the negative effect on insurance qualification is entirely driven by subjects reducing their number of deposits due to the lotteries, despite saving slightly more in total. Strikingly, the negative effect on lottery qualification persists even during 2020 and is even twice as a large as during 2019. This can be explained either by subjects having preponed savings into 2019, which are then missing in 2020, or by subjects depositing less frequently by habituation.

The welfare loss from losing this insurance is likely substantial. According to a study by Colpensiones, the insurance is highly valued by account holders, with 61% of survey respondents reporting it as their *primary* motivation for saving with Colpensiones (Colpensiones, 2023). Second, Figure 6 displays the empirical distribution of accounts by the total number of deposits in 2018 for participants that made at least one deposit. The distribution clearly shows that account holders strategically change their behavior in response to the insurance incentives. There is a substantial increase in the number of participants that made deposits at the benefits threshold (6 deposits). Only 4% and 8% of participants made five and seven deposits, respectively, while 20% of participants made exactly six deposits.

A reduction in insurance qualification is inconsistent with a model with fully rational agents

Figure 5: Eligibility for Life & Disability Insurance

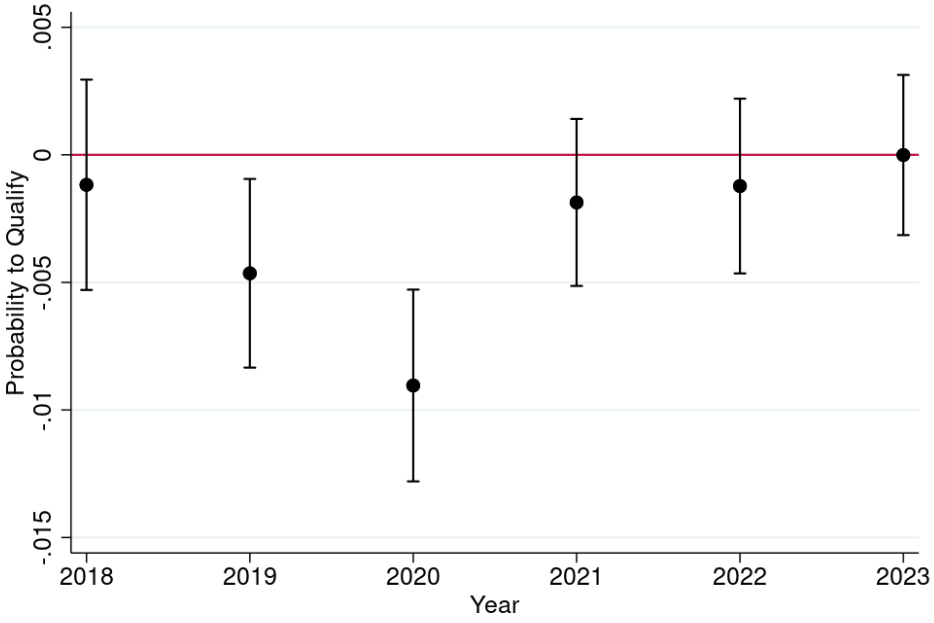
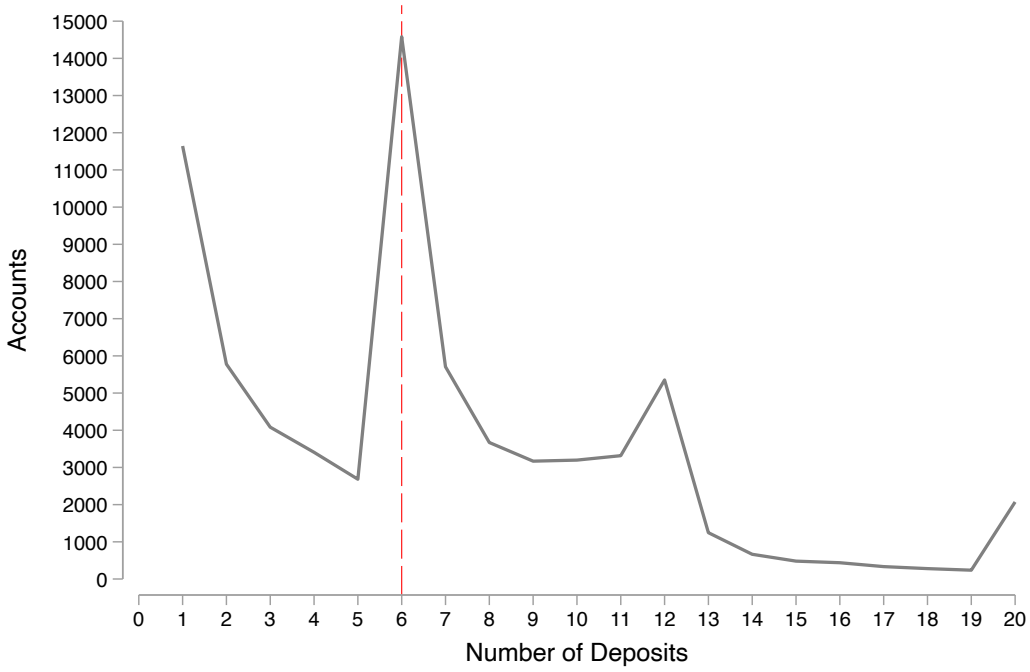


Figure 6: Bunching at the Insurance Threshold Prior to the Experiment



Notes: This figure displays a histogram of the total number of deposits made 2018. The red vertical line indicates the qualifying number of transactions (6) for the BEPS insurance. Figure includes all participants that made at least one deposit in 2018 and that had an account that was opened prior to January 1, 2018.

and no optimization frictions. This is because qualifying for both a lottery and the insurance is not mutually exclusive, so there is no trade-off. A household that saved four times to qualify for the first House lottery, could have just as well saved smaller amounts six times—thereby qualifying for both the lottery and the insurance. The negative effect on insurance qualification suggests that the lottery promotions crowd out attention to other benefits, causing households to forget about the insurance. This insight highlights the importance of observing household behavior beyond pure savings responses. Retirement savings policies may have side effects on other choice domains that can have large welfare costs.

Result 5: *Lotteries cause households to lose life and disability insurance. This effect persists post-treatment.*

3.3 *Distributional Effects*

In this section, we study the distributional consequences of the lottery. Specifically, for each treatment indicator τ_x with $x \in \{\text{House}, \text{Cash}, \text{House+Cash}\}$, we estimate the flexible specification:

$$\begin{aligned}\text{savings}_i = \beta_0 + \beta_1 \tau_{x,i} + \beta_2 \text{SISBEN}_i + \beta_3 \text{SISBEN}_i^2 \\ + \beta_4(\tau_{x,i} \times \text{SISBEN}_i) + \beta_5(\tau_{x,i} \times \text{SISBEN}_i^2) + \varepsilon_i,\end{aligned}$$

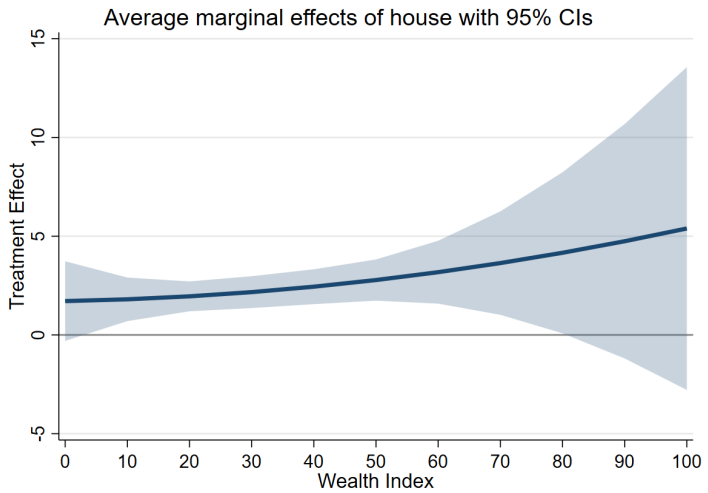
where savings_i denotes the amount saved by individual i. The variable SISBEN_i is the SISBEN III score, a proxy for socioeconomic status that ranges from 0 (poorest) to 100 (least vulnerable). We include interactions between the treatment indicator and both linear and quadratic terms in the SISBEN score to allow for flexible heterogeneity in the treatment effect across the wealth distribution. Robust standard errors are used throughout. To interpret the heterogeneity, we compute the marginal effect of each treatment on savings as a function of the SISBEN score,

$$\left.\frac{\partial\, \text{savings}_i}{\partial\, \tau_{x,i}}\right|_{\text{SISBEN}_i = s}, \tag{2}$$

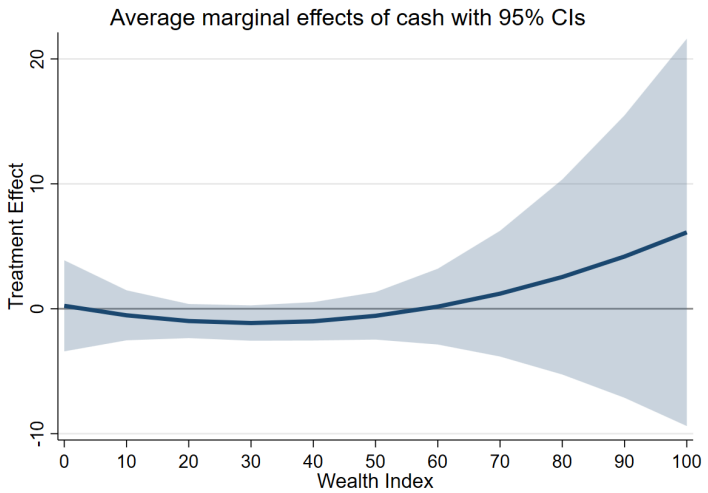
for values $s \in \{0, 10, 20, \ldots, 100\}$. We plot these marginal effects along with 95% confidence intervals to visualize how the impact of the lottery incentive varies across the distribution of household wealth.

Figure 7: Heterogeneity by Wealth Index

(a) House



(b) Cash



(c) House + Cash

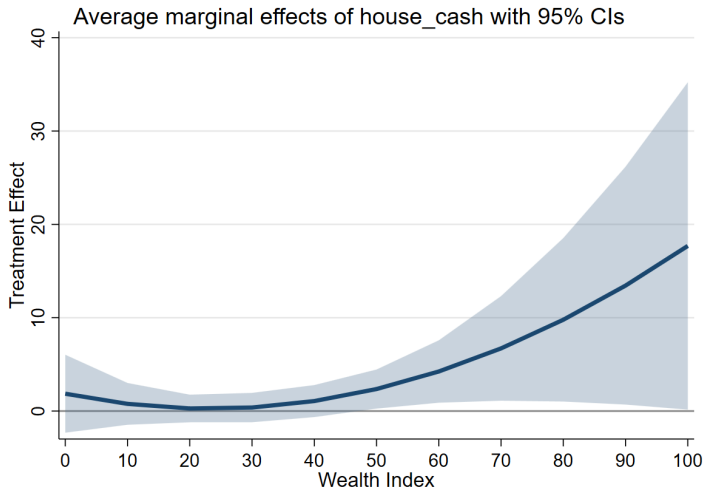
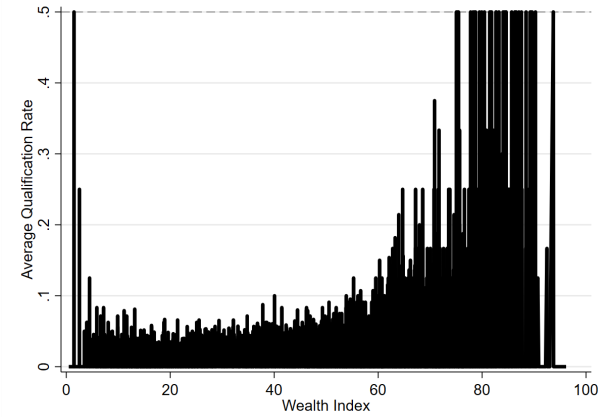


Figure 8: Qualification for Lottery Prize by Wealth Index

(a) House



(b) Cash

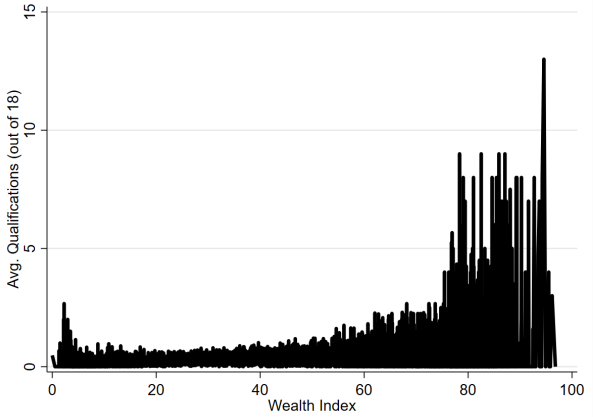


Figure 7 plots results for the first 4 months, which correspond to the qualification period of the first House lottery. The general picture that emerges from the three panels is that lotteries tend to be have larger treatment effects for wealthier workers. The results are most precisely estimated in Panel A, the estimated effect of the house lottery on savings rises from approximately 2,000 COP at the lowest SISBEN scores to 5,000 COP at the highest score. The Cash and House+Cash lotteries in Panel B and C, respectively, have similarly low treatment effects at the bottom of the wealth index distribution, but even higher positive treatment effects at the top than the House lottery. While point estimates are less precisely estimated, the common pattern across the three Panels suggests that lotteries mostly change behavior of less vulnerable workers.

This heterogeneity is consistent with wealthier individuals facing fewer liquidity constraints. If lotteries are generally meant to benefit workers, then the current results imply that lotteries are regressive.

To further illustrate this, Figure 8 plots the average number of qualifications for a lottery prize across the wealth index. In both panels, the average qualification rate sharply increases with income. In Panel a, we find that only 10% of workers with a wealth index below 60 qualify for a house lottery, while *half* of all workers with most values above 75 qualify. Panel b shows a similar pattern for the cash lotteries: Among workers with a wealth index below 60, the average qualification rate is mostly around 2%, but goes up to around 10% for index values above 75.[13]

[13]We should note that all of the workers in our sample are relatively poor, so that it may well be beneficial to transfer lottery prizes to workers that have an even higher SISBEN score. With this in mind, we conclude that lotteries mostly benefit the less vulnerable workers within our sample.

3.4 *Targeting Present-Biased Savers*

Even if lotteries have little aggregate effect, they may increase welfare if they target the right workers. In the context of pension policies, the right workers to target are especially those who make "present biased" (time-inconsistent) choices and excessively discount future payoffs (Choukhmane and Palmer, 2024). We show in Appendix E.5 more formally that the correlation between lottery treatment effects and time inconsistency is an important statistic for welfare. For now, we take this insight as given, and provide reduced-form evidence as to whether these two variables correlate.

We follow a common approach in the literature and connect survey proxies of present bias with our administrative savings data (e.g., Lockwood et al., 2024). A common finding in the literature is that these stated preferences predict incentivized choices (e.g., Falk et al., 2022) and our analysis confirms this insight, as we discuss below. The World Bank has implemented a representative survey in Colombia in 2012, called the "STEP Skills Measurement Household Survey." Besides many questions on labor market outcomes, wealth, and family composition, the survey also elicits stated risk and time preferences.

For time preferences, the survey uses a very coarse version of the stair-case method. Respondents first choose between receiving 2,160,000 pesos today versus 3,240,000 pesos in one year (a 50% return). Those who choose the immediate payment are then asked if they would wait for 4,320,000 pesos (100% return), while those who choose to wait are asked if they would still wait for only 2,592,000 pesos (20% return). This design allows us to bracket each respondent's discount rate into one of four intervals: below 20%, between 20% and 50%, between 50% and 100%, or above 100%. The survey also asks those with discount rates above 100% to state the minimum amount they would accept in one year instead of the immediate payment, which avoids censoring discount rates of very impatient individuals. A caveat of the survey is that it only allows us to compute a single discount rate. While any discount rate could in principle reflect rational exponential discounting, the literature has found that high discount rates strongly predict present-biased behavior (e.g., Toubia et al., 2013).[14]

The survey further elicits personality traits that have natural connections to self-control problems and myopia. The relevant questions are: *"Do you finish whatever you begin?", "Do you*

[14]Estimating present bias directly requires observing both a near-term tradeoff (e.g., today vs. one year) and a tradeoff between two future points in time (e.g., in one year vs. in two years). Since we do not observe the latter choice, we interprete high discount factors from the first choice as evidence of present bias. In our main analysis, we classify individuals above the median discount factor of 160% as impatient—i.e. those subjects who are only indifferent between the two payments if the later payment is 2.6 times larger than the payment today. While we view this as a conservative definition of present bias, we obtain similar results to the ones discussed in this section when we restrict present bias to subjects with much higher discount factors than the median. Further, questions that ask for a qualitative self evaluation of personal characteristics (e.g., enjoying relaxation more than hard work) correlate intuitively with high discount rates and yield similar results regarding the targeting properties of lotteries.

work very hard? For example, do you keep working when others stop to take a break?", "Do you prefer relaxation more than hard work?", "Do you enjoy working on things that take a very long time?", and *"Do you think about how the things you do will affect you in the future?"* Respondents answer on a four-point scale from *"almost always"* to *"almost never."* These questions capture different dimensions of self-control, persistence, and forward-looking behavior that correlate strongly with the discount rates elicited in the sequential bracketing approach.

For risk preferences, the survey employs a similar staircase method but using lottery choices. Respondents first choose between a certain payment of 216,000 pesos versus a 50-50 lottery paying either 0 or 648,000 pesos. Depending on their choice, they face a second lottery with either higher or lower expected value, allowing us to classify respondents into four risk preference categories ranging from risk-loving to highly risk-averse.

We merge responses to observable characteristics that are available both in the World Bank Survey and in our administrative data. Specifically, we limit the World Bank Survey data to informal workers and match observations by department, age, and gender. Using this procedure, we are able to assign the stated preference data to 117,036 workers in our experiment.[15]

In Table A4 in the Appendix, we show that these stated preferences strongly predict behavior in the field. Workers who are very risk-averse are more likely to save in general. This is consistent with theory as higher risk aversion motivates more pecuniary savings against future adverse shocks. Workers who are more impatient save substantially less than more patient workers. These results confirm the finding in the literature that stated time and risk preferences meaningfully capture consequential behavior in the field (Falk et al., 2022).

For illustrative purposes, Figure 9, splits the sample into participants with a below- and above-median discount rate and label the groups "patient" and "impatient". In Panel a, we find that the House lottery had *smaller* treatment effects on impatient workers. These differences are especially large and statistically significant during the qualification period of the first house lottery (up until July). While the first House lottery caused both groups to slightly increase savings, the lottery does not target those workers whose behavior is more affected by impatience. For the Cash and House+Cash lotteries, we find no statistically significant difference between the two groups.

Potential concerns one could have about Figure 9 is that we pool time preferences into two coarse categories, which may ignore important nuances, and high impatience may to some extent reflect a high rational discount rate rather than present bias. To complement our analysis, in

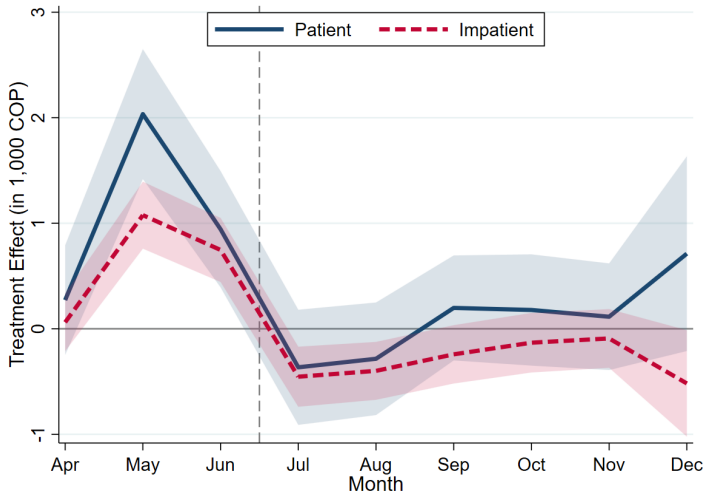
[15] Since the World Bank survey covers less areas than our administrative data, we cannot assign survey responses to every worker in the experiment. While this may mitigate external validity, we find that whether survey responses are missing does not correlate with treatment assignment. All heterogenous treatment effect coefficients have a casual interpretation. We further do not find that missing survey responses correlate with other observable characteristics, suggesting that our results may generalize to our larger sample.

Appendix A2, we report treatment effects for the entire range of time preferences in our sample and find very similar patterns to those reported here. In Table A4, we also study treatment effects of people with personality traits that are indicative of present bias, *while holding discount rates fixed*. That is, we study the interaction of myopic personality trades above and beyond the role of (high but potentially rational) discount rates. While these traits strongly correlate with savings in intuitive ways even when controlling for discount rates, they still do not generally interact with lottery treatment effects. Across all these different specifications, the evidence seems to agree that lotteries are generally not advantageously targeted towards more present-biased workers.

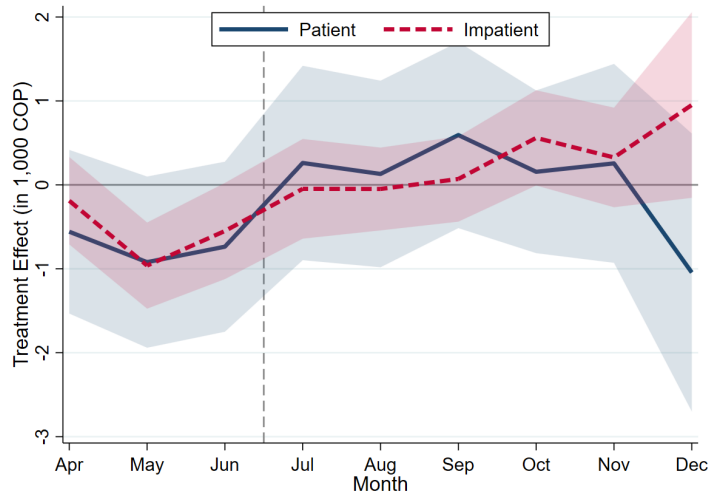
Result 6: *Lotteries do not target present-biased workers.*

Figure 9: Heterogeneity by Impatience

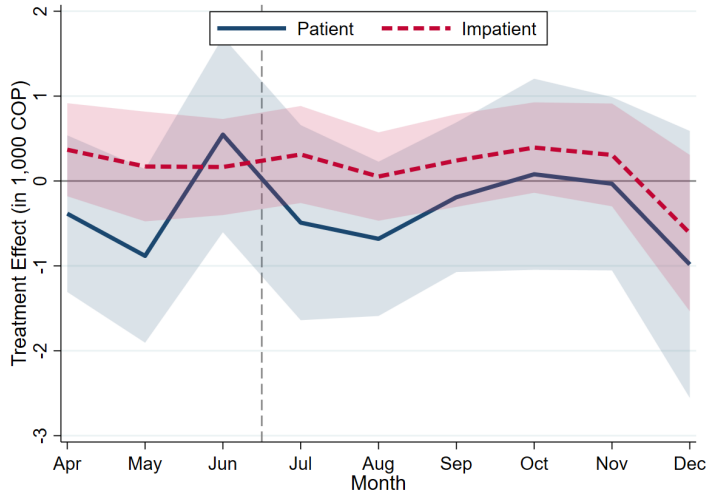
(a) House



(b) Cash



(c) House + Cash



4 Comparison to Deterministic Incentives: Field Experiment on Savings Matches

Lotteries are a non-traditional policy tool as they offer stochastic rather than deterministic benefits to saving households. It is, therefore, interesting to study how they compare to policies that subsidize savings for every household. To answer this question, we obtain data from a second nation-wide field experiment by Colpensiones that randomized savings matches across households, three years after our lottery experiment. A particularly beneficial feature of the data is that we obtain data for those households in the matches experiment that were also part of the lottery experiment. This allows us to make some inferences about how the same household responds to stochastic versus deterministic incentives.[16]

The experiment ran from February until December 2022. Trained pension fund employees called households registered in BEPS administrative records and explained the incentive to them. Participants were randomly assigned to receive a savings match $m \in \{2{,}500, 5{,}000, 10{,}000\}$ COP if they saved a positive amount within 2 months after the call. The matched amount was added to their retirement account. A fourth group was assigned to receive no match and served as the control group. To allow for comparability, participants in the control group also received a call, but were only provided with general information about the BEPS program, including its benefits, eligibility criteria, and how they could voluntarily contribute to their savings. No mention of the counterpart incentive was made.

Table 1 reports treatment effects on i) the probability to save any positive amount during the qualification period, on ii) total weekly savings (in % to control), and iii) on post-treatment savings (in % to control), i.e., when the qualification period has expired. We use simple OLS regressions regressing each outcome on the vector of treatment dummies. For column 2, we cluster standard errors on the household level. We consider the two months period in which subjects were incentivized to save in order to qualify for the match.

A \$2,500 match increases the savings probability by 13 percentage points ($p < 0.01$), which is a large relative treatment effect of 54% compared to control. Increasing the matching amount to 5,000 and 10,000 has only a small incremental effect of 1 and 2 percentage points. Turning to column 2, a \$2,500 match raises savings by 38%, a substantially larger amount than any of the lotteries. The \$5,000 match has a slightly lower effect than the \$2,500 match, but these differences are not statistically significant. We refrain from interpreting this difference extensively since the match did not incentivize the amount saved but the probability to save any positive amount. Finally, the COP 10,000 match increases savings by 49% relative to control.

Column 3 shows that the positive effect on savings persists post-treatment. Post-treatment

[16] A limitation of this comparison is that the experiments happened at different points in time. However, we do not need to assume that savings *behavior* is the same between the two years, but only that time trends do not change treatment *effects* on behavior.

savings increase anywhere between 24% and 31% relative to control. These effects are large and highlight that matches, different from lotteries, induce a habit to save.

Table 1: Effects of Savings Matches

	(1) Saving Probability in Qualification Period	(2) Weekly Savings (in % to Control)	(3) Post-Treatment Savings (in % to Control)
Match amount			
+2,500	0.13***	0.38***	0.25**
	(0.015)	(0.13)	(0.097)
+5,000	0.14***	0.33***	0.24**
	(0.016)	(0.12)	(0.097)
+10,000	0.15***	0.49***	0.31***
	(0.015)	(0.13)	(0.098)
Constant	0.24***		
	(0.012)		
Observations	7995	69225	280524
Adjusted R^2	0.012	0.000	0.001

Notes: This table reports treatment effects of savings matches on i) the probability to save and ii) the amount saved relative to control. The first two columns report effects on the entire sample of the experiment. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

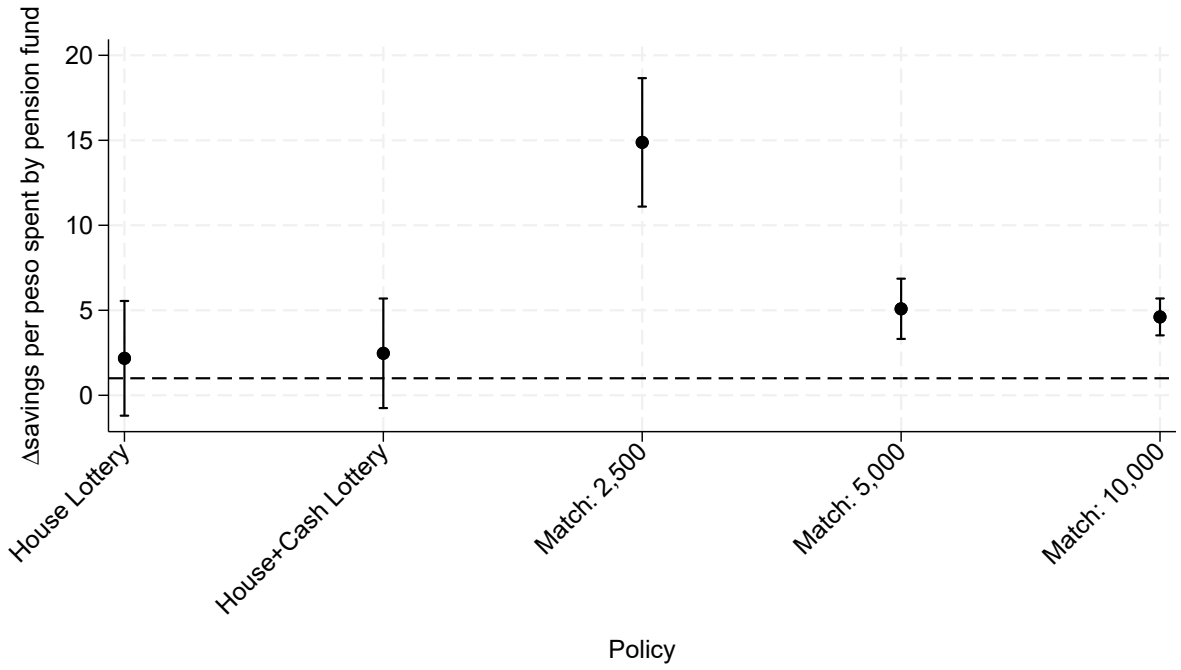
We compute the cost effectiveness of each of those matches and compare it to the lotteries. We define cost effectiveness as the change in savings for each COP spent by Colpensiones on the intervention. Figure 10 presents results. Since the Cash lottery did not increase aggregate savings, we exclude it from the comparison. Both House and House+Cash treatments increase savings by around 2 COP per COP invested by the pension fund. We cannot rule out that these effects are statistically different from zero.

The match coefficients are at least twice as large as the ones of the lotteries and always significantly different from zero. The lowest match of 2,500 COP provides particularly promising results: For every COP invested by Colpensiones, household savings increase by a striking *15 COP*. This is 7.5 times more cost effective than lotteries. Increasing the generosity of matches to 5,000 or 10,000 COP reduces cost effectiveness to a ratio of 5:1 because the incremental increase in savings is low.

Even though lotteries are only paid to a few winners, while matches are paid to everyone who qualifies, the deterministic incentives prove more cost effective. Since savings are concave in matches, especially low match levels have a substantial cost advantage over lotteries.

Result 7: *Matches are more cost-effective than Lotteries and induce long-term behavioral change.*

Figure 10: Cost Effectiveness: Lotteries versus Matches



5 Mental Probability Representations

In this Section, we show how to recover subjective winning probabilities of lotteries by comparing the savings responses to lotteries with the responses to matches. Since our primary source of identification will come from the *immediate* savings response to the treatments, we do not explicitly model future income shocks and random fluctuations in assets and wealth. In the first period, the household earns income y_0 and chooses illiquid savings s_0, yielding contemporaneous utility $u_0(y_0 - s_0)$. Savings earn a gross return of $R = 1 + r$ at retirement, T time periods away from today: $u_T(y_T + \sum_{s=0}^{T-1} s_t \cdot R^{T-t})$. In the presence of a savings lottery, savings in period 1 increase the probability to win the lottery in period 2. Specifically, the households earns lottery tickets $\tau = s_0/p$ where p is the required number of savings for one ticket; in the experiment $p \in \{55,000, 500,000\}$. The lottery prize is $x \in \{100,000; 102,000,000\}$. If the household wins the lottery, her contamporaneous utility in period 1 is $u_1(y_1 - s_1 + x)$ and $u(y - s_1)$ otherwise.

In the experiment, the winning probability of subject i among J participants is $\omega(\tau_i) = \frac{\tau_i}{\sum_j \tau_j}$, so each additional ticket increases both the numerator and denominator. A subject earns a lottery ticket for every p in savings s_0, meaning $\tau = \frac{s_0}{p}$. Given that the number of tickets chosen empirically is small and the number of participants is large, ω is approximately linear in τ_i,

$\omega(\tau_i) = \frac{\tau_i}{\sum_j \tau_j} \approx \pi\tau_i$, with π being the winning probability per ticket. This also allows us to assume that the impact of each ticket on the overall winning probability is negigble and that households do not strategically interact with each other when choosing how much to save.

Let $\hat{\pi}$ be the households *subjective* winning probability, which could differ from π. Further denote δ as the exponential discount factor. In addition, we allow for the possibility that households are present-focused with $\beta \in (0, 1]$. Any $\beta < 1$ implies that households discount future utility quasi-hyperbolically due to a preference for isntant gratification (Laibson, 1997). The household chooses savings to maximize lifetime utility:

$$\max_{\boldsymbol{s}} \left\{ u_0(y - s_0) + \beta \left(\delta \cdot \mathbf{E}_{\omega(\hat{\pi}, s_0)}[u_1(y_1, s_1, x)] + \delta^2 V_{t+2}(\mathbf{s}) \right) \right\} \tag{3}$$

where $\mathbf{E}_{\Phi(\hat{\pi}, s_0)}[u_1(y_1, x)] = u_1(y_1 - s_1 + x)\hat{\pi}\frac{s_0}{p} + u_1(y_1 - s_1)\left(1 - \hat{\pi}\frac{s_0}{p}\right)$ and $V = \sum_{t=2}^{T-1} \delta^{t-2} u(y_t - s_t) + \delta^{T-2} u_T(y_T + \sum_{t=0}^{T-1} s_t R^{T-t})$. Let s_0 be savings at t_0 without the lottery and s_0^l savings with the lottery. Then, the first-order condition in the presence of a lottery is

$$\underbrace{-u_0'(y - s_0^l)}_{\text{cost of consumption shifting}} + \underbrace{\beta\delta \cdot \frac{\hat{\pi}}{p}\left[u_1(y - s_1^l + x) - u_1(y - s_1^l)\right]}_{\text{perceived lottery gain}} + \beta \cdot \underbrace{\delta^T \cdot R^T \cdot u_T'(w_T^l)}_{\text{savings benefit at retirement}} = 0, \tag{4}$$

while in the absence of the lottery it is

$$-u_0'(y - s_0) + \beta(\delta R)^T \cdot u_T'(w_T^0) = 0. \tag{5}$$

In the experiment we observe the treatment effect of the lottery $\Delta s = s_0^l - s_0$, which is implicitly given by equating the two FOCs above. This treatment effect can be expressed as a function of the subjective winning probability $\hat{\pi}$, which we wish to recover with the data. We parameterize utility as CARA, $u(y) = -\frac{e^{-\gamma y}}{\gamma}$, with γ measuring absolute risk aversion.[17] Equating the two FOCs above and using first-order Taylor expansions yields: [18]

$$\Delta s \approx \hat{\pi} \cdot \frac{\frac{\delta}{p}\left[e^{-\gamma(y - s_1^l)} - e^{-\gamma(y - s_1^l + x)}\right]/\gamma}{\gamma e^{-\gamma(y - s_0^0)} + \gamma\delta^T R^{2T} e^{-\gamma(w_T^0 + s_0 R^T)}}. \tag{6}$$

[17] With CARA, the respective FOCs become $e^{-\gamma(y - s_0^l)} + \delta \cdot \frac{\hat{\pi}}{p}\left[-e^{-\gamma(y - s_1^l + x)} + e^{-\gamma(y - s_1^l)}\right]/\gamma + \delta^T \cdot R^T \cdot e^{-\gamma w_T^l} = 0$ and $e^{-\gamma(y - s_0)} + \delta^T \cdot R^T \cdot e^{-\gamma w_T^0} = 0$.

[18] Using a first-order Taylor expansion, we approximate $e^{-\gamma(y_0 - s_0^l)} \approx e^{-\gamma(y_0 - s_0^0)} + \gamma e^{-\gamma(y_0 - s_0^0)}\Delta s$ and $e^{-\gamma(w_T^l + s_0^l R^T)} \approx e^{-\gamma(w_T^0 + s_0 R^T)} - \gamma R^T e^{-\gamma(w_T^0 + s_0 R^T)}\Delta s$. Substituting this into the difference in the FOCs and factoring out Δs yields equation 6.

which gives us a moment condition to estimate $\hat{\pi}$ empirically. Thus, knowledge of the treatment effect together with estimates of y, s_0, δ, and γ recover $\hat{\pi}$. While we do not directly observe y, we observe each household's poverty score (SISBEN), which is known to strongly predict income. We transform this distribution into an income distribution of the same shape and with annual mean equal to COP 8,400,000, which we is the average annual income of informal workers. The counterfactual savings without a lottery, s_0, is obviously not observed for treatment subjects. However, we observe subjects savings during that same time period one year before when no lottery was present. We use this as a proxy for s_0 but we first subtract a general time trend in savings that we compute from the difference in savings of control group subjects during the same time period. Since savings are generally small relative to income, measurement error in s_0 will have a negligible effect on the estimates. We assume $\delta = 0.99$ in line with the literature (Laibson et al., 2024) but our results are not very sensitivity to alternative values. The true value of β is irrelevant for identification of $\hat{\pi}$.

Finally, we estimate the curvature of the utility function, γ, from our complementary experiment on savings matches for the same individuals that are part of the lottery experiment. This experiment incentivized subjects to increase their probability to save any positive amount during the qualification period. Let m denote the size of the match and $s_0(m)$ the chosen savings during the qualification period. As we prove in the Appendix, observing how the probability to save changes in response to different match levels identifies γ. Specifically,

$$\frac{\partial ln(\frac{\partial \Pr(s_0(m)>0)}{\partial m})}{\partial m} \approx -\gamma \cdot (w_T'(m) + 1) \tag{7}$$

The LHS measures how the increase in the savings probability changes as we raise the match level, which is what we observe empirically. Intuitively, the curvature of this "extensive margin savings function" is directly pinned down by the curvature of the utility function. We estimate a value of $\gamma = .0006137$ per 1,000 COP, which translates to around $.00184$ per USD.[19] This falls well into the range of estimates in the literature.

With these primitives at hand, we proceed to estimate $\hat{\pi}$ from the data. Let $D_i \in \{0, 1\}$ be treatment assignment to the lottery. Then, we let savings be $s_i = \alpha + \hat{\pi}\beta_i D_i + \epsilon_i$, with $\beta_i = \frac{\frac{\delta}{p}\left[e^{-\gamma(y_i - s_{1i}^l)} - e^{-\gamma(y_i - s_{1i}^l + x)}\right]/\gamma}{\gamma e^{-\gamma(y_i - s_{0i}^0)} - \gamma\delta^T R^{2T_i} e^{-\gamma(w_{Ti}^0 + s_{0i}R^{T_i})}}$. Since we know the age of the subject, we can compute

[19] Since we do not observe people's total wealth, we cannot directly translate this into a Constant Absolute Risk Aversion (CRRA) parameter. A common calibration for CRRA in the macro and finance literature is a value of 2, which we would obtain if worker's wealth is around slightly less than half a year of average wage. While this is not an unreasonable wealth level for many of these poor workers, the total value of assets they hold is likely somewhat higher. Consequently the CRRA value is likely above 2 and workers are fairly risk averse.

retirement T_i for each subject individually. We estimate $(\alpha, \hat{\pi})$ by solving

$$\min_{(\alpha,\hat{\pi})} \mathbf{E}[s_i - \alpha - \hat{\pi}\beta_i D_i] \tag{8}$$

and approximate the efficient weight matrix via a two-step GMM estimator.

Result 8: *Subjects under-estimate the odds of small-prize lotteries and over-estimate the odds of large-prize lotteries.*

5.1 *Estimation Results*

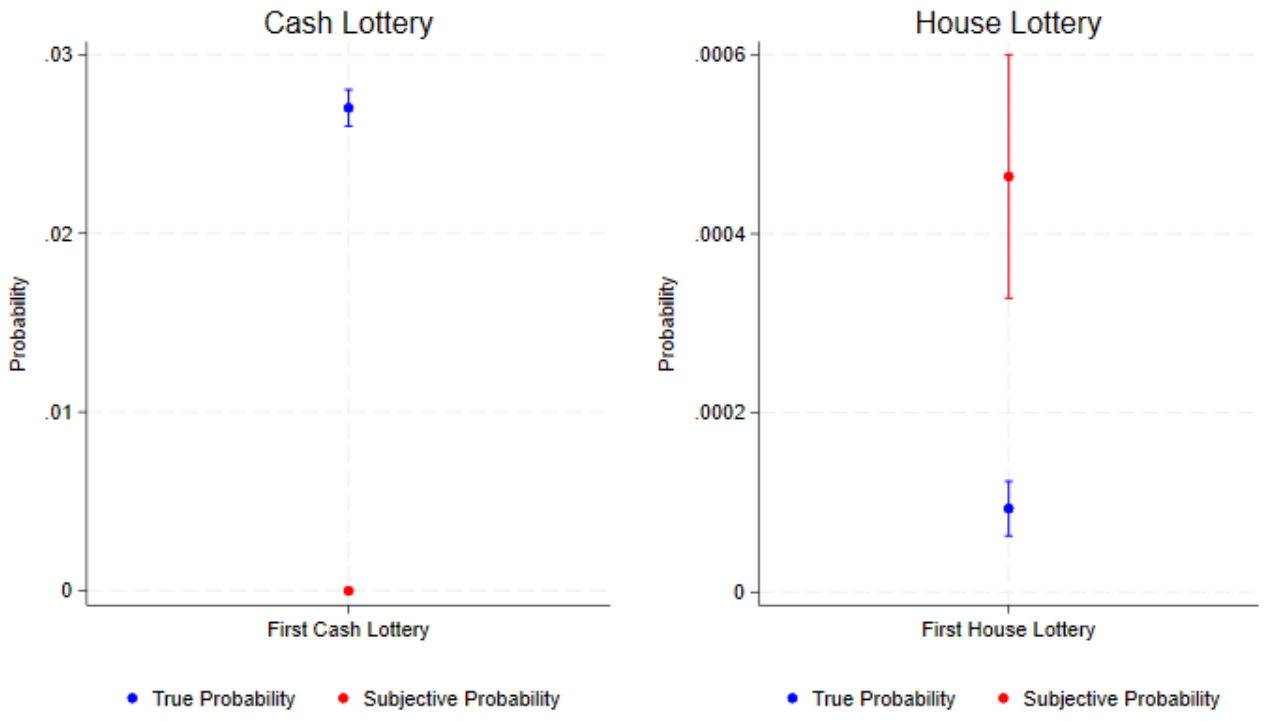
We estimate equations 8 for the first House lottery and the first Cash lottery, separately. Focusing on the first interaction with a lottery is the cleanest analysis because the savings response is not yet confounded by responses to prior lotteries. During follow-up lotteries, subjects might already be substantially liquidity constrained because they pre-poned some of their liquid savings into the prior period. Our model does not consider repeated interactions with lotteries because it would require us to make assumptions on how much of their liquid wealth subjects substitute from the future into the present. Focusing on the first House and Cash lottery allows us to avoid making these assumptions.

Figure 11 shows the subjective winning probability, $\hat{\pi}$, for the Cash lottery (Panel A) and the House lottery (Panel B). Red dots are the point estimates and the bars represent 95% confidence intervals. In blue we show the true ex-post winning probability per ticket, π, that we compute by dividing the number of prizes by the number of total lottery tickets that were gathered by subjects during the qualification period.

The true winning probability for the Cash lottery is fairly large, amounting to around 2.7%. The lack of a savings response to the lottery implies a subjective winning probability of zero. Thus, only inframarginal households that would have reached the savings threshold even without the lottery experienced a utility gain in expectation. We observe the opposite pattern for the House lottery: The true probability is .01% while the subjective probability is .04%. The difference is highly statistically significant, $p < 0.01$. The stark difference between Panel A and B suggests that households underestimate the odds when prizes are low and overestimate them when prizes are high.

This is consistent with models of probability weighting (Kahneman and Tversky, 1979) and selective attention to attributes (Bordalo et al., 2012; Kőszegi and Szeidl, 2013). If the attribute of an option (such as the prize) increases, it becomes particularly salient and attracts attention. Since attention is limited, the increase in contrast of one attribute interferes with attention to another attribute (such as the odds). As a result, households focus too much on the odds (and

Figure 11: Subjective Versus True Winning Probabilities



neglect the prize) when the prize is low, and focus too much on winning when the prize is high (neglecting the fact that they may lose). This aligns with recent evidence on selective attention to features of price promotions (Gennaioli et al., 2025).

These findings are important because they imply that lotteries induce suboptimal savings responses if households otherwise optimize.

5.2 *Welfare*

In this Section, we discuss potential welfare implications of the lottery intervention. Since we do not observe any long-run effects of the interventions, the lotteries do not affect the fiscal externality associated with undersaving. In particular, there is no evidence that the interventions change the share of individuals who will eventually qualify for the non-contributory *Colombia Mayor* program. As a consequence, we abstract from this channel and focus on the prevalent behavioral frictions in our welfare computations. We allow the social planner to be paternalistic, meaning that she believes workers' behavior may not maximize their own utility. From the social planner's perspective, normative utility under the lottery is:

$$W = u_0(y - s_0(\beta, \hat{\pi})) + \delta \cdot \left[\pi \frac{s_0(\beta, \hat{\pi})}{p} u_1(y_1 - s_1 + x) + \left(1 - \pi \frac{s_0(\beta, \hat{\pi})}{p}\right) u_1(y_1 - s_1) \right] + \sum_{t=2}^{T-1} \delta^t u(y_t - s_t(\beta, \hat{\pi})) + \delta^T u_T(w_T) + \mathcal{L}(\hat{\pi}) \quad (9)$$

where $\mathcal{L}$ represents the welfare from insurance coverage and $\theta(\hat{\pi})$ is the uptake of insurance coverage by the worker. This uptake can be crowded out the more workers become aware of the lottery (i.e., $\hat{\pi}$).

Normative welfare deviates from worker's perceived welfare for three reasons. First, the social planner views time inconsistent behavior as a mistake and, consequently, evaluates welfare with $\beta = 1$. Second, the social planner uses the objectively correct winning probability π to weight the respective payout, not the potentially incorrect $\hat{\pi}$. Present focus and the subjective winning probability enter the social welfare function only through workers' *behavior*, as captured by $s_t(\beta, \hat{\pi})$. Third, we assume the worker partially ignores life insurance qualification when responding to the lottery, which the social planner considers a mistake. These tensions between normative welfare and agents' behavior is a common specification in the behavioral welfare economics literature (e.g., Bernheim and Taubinsky, 2019; Choukhmane, 2019).

The experimental intervention in this paper are messages that make workers aware of the lottery, i.e., it changes $\hat{\pi}$ by $\Delta\hat{\pi}$. This results in treatment effects on savings τ_t at time $t \in \{0, 1, ...T-1\}$. In the experiment, we shift beliefs from zero (unawareness prior to the message) to some value $\bar{\pi}$, i.e., $\Delta\hat{\pi} = \bar{\pi} - 0$. Empirically, we found that savings drop immediately following the qualification period (i.e., $\tau_1 < 0$ while there are no further effects on long-run savings ($\tau_t = 0$ for $t = 2, ..., T-1$). Therefore, the only welfare-relevant periods are 0, 1 and T. As we prove in Appendix E, a first-order approximation of the welfare effect of the lottery can be obtained by: [20]

[20] We prefer a first-order approximation of welfare effects over an estimation of a fully specified life-cycle model because it allows us to make considerably less assumptions. For a well-estimated life-cycle model we would require information on long-run asset holdings outside of the BEPS system, which is not available in our data. Other factors such as income shocks and liquidity constraints would become much more relevant in a lifecycle model, while they should have less import on our analysis that relies on savings in period 0, 1, and then consumption only again at retirement. While we could merge some publicly available wealth statistics to our sample on some coarse observables (e.g. workers' region), we are worried that this approach results in considerable measurement error and potential intransparency as to what drives our results. Our first-order approximation allows us to mitigate these issues and is a common approach in empirical public finance.

$$\Delta W \approx \underbrace{\rho}_{\text{expected prize}} + \tau_0 \left[\underbrace{\frac{\delta \Delta u_1}{p}(\pi - \beta\hat{\pi})}_{\text{Belief distortion}} + \underbrace{\delta^T u_T' R(1-\beta)}_{\text{Under-saving Correction}} \right] + \underbrace{\tau_1 \delta^{T-1} u_T' R^{T-1}(1-\beta)}_{\text{strategic substitution}} - \underbrace{\Delta\mathcal{L}}_{\text{insurance loss}} \tag{10}$$

The first term $\rho = \pi \cdot \frac{s_0}{p} \cdot \frac{\delta}{\gamma}\left[e^{-\gamma c_1} - e^{-\gamma(c_1+x)}\right]$ denotes the first-order gain from the lottery prize. The second term is the welfare change due to changing misperception workers have about the lottery. If workers were fully rational ($\beta = 1$ and $\pi = \hat{\pi}$) then this term would be zero by the envelope theorem, as small deviations from the optimum have no first-order welfare effects.[21] The following terms, represents the change in welfare due to a reduction in savings in the post-lottery period as workers pre-poned their savings into period 0. Again, this welfare term would be zero if workers behaved time-consistent. $\beta = 1$. With time-inconsistent workers, $0 \leq \beta < 1$, welfare effect of the lottery depends on how it changes future savings. The last term is the welfare loss from forgoing insurance coverage.

Calibrations Following Laibson et al. (2024), we calibrate $\delta = 0.99$ and $\beta = 0.5$. We show the sensitivity to these assumptions in Appendix E.

We identify $\Delta\mathcal{L}(\hat{\pi})$ directly from the data. Under CARA utility, an individual with annual income y faces mortality risk q and would receive death benefit B if insured.[22] The WTP for insurance, denoted P^*, solves:

$$(1-q)u(y-P^*) + qu(B) = (1-q)u(y) + qu(0) \tag{11}$$

Since the insurance is provided at zero premium, the welfare value equals P^*. Using our estimated CARA parameter and a mortality probability of 0.2% among poor workers in Colombia, we find $P^* \approx 1,000,000$ COP (around 312 USD in 2019 or 12% of annual income). This is a considerable but reasonable willingness to pay given the benefits in case of death and the high mortality rate.

The lottery causes a reduction in insurance qualification probability after the intervention

[21] In principle, the social planner could eliminate this behavioral wedge through "optimal deception": Design an information campaign such that workers believe the winning chance to be $\hat{\pi} = \frac{\pi - \frac{\delta^T u_T' R(1-\beta)p}{\delta \Delta u_1}}{\beta}$. We do not explore this possibility further in this paper.

[22] This benefit would go to the family of the deceased and we assume that the family has the same utility function and that the worker values the family's utility equally to her own utility. We could also assume that the worker values the family's utility by some multiplicative altruism parameter α (i.e., a "discount factor") and take parameter values from the literature (which happen to be above zero but below 1). Empirically, this alternative specification turns out to have little impact on our conclusion.

period, in 2020 and 2021.[23] Let θ_t denote the reduction in qualification probability in year t following the lottery. Empirically, we find that for the cash lottery, $\theta_1 = 0.00741$ (i.e., 0.741% reduction in year 2020) and $\theta_2 = 0.01324$ (1.324% reduction in year 2021). For the house lottery, the reductions are smaller: $\theta_1 = 0.00346$ and $\theta_2 = 0.00824$. The expected welfare loss from reduced insurance access is:

$$\Delta\mathcal{L} = \sum_{t=1}^{2} \delta^t \theta_t P^*. \tag{12}$$

With these parameter values at hand, we estimate equation 10 by a GMM estimator

Heterogeneity So far, we assumed a representative agent framework. This simplification makes results more transparent but it lacks some realism. In Appendix E.5, we show how to estimate welfare effects while allowing for heterogeneity in all primitives $(\gamma, \hat{\pi}, \delta, \beta)$—all of which may covary with each other, which implies specific targeting properties of the lottery. Using our auxiliary survey, we are already showed that the lotteries have no advantageous targeting properties, as they do not target present-biased workers. Therefore, allowing for heterogeneity in the model will yield weakly lower welfare effects as in the representative agent framework. To enhance clarity and transparency of the results, we present here the representative agent model and refer the interested reader to the appendix for the heterogenous agent model. Our main conclusions do not change when we add heterogeneity to the model.

5.3 *Results*

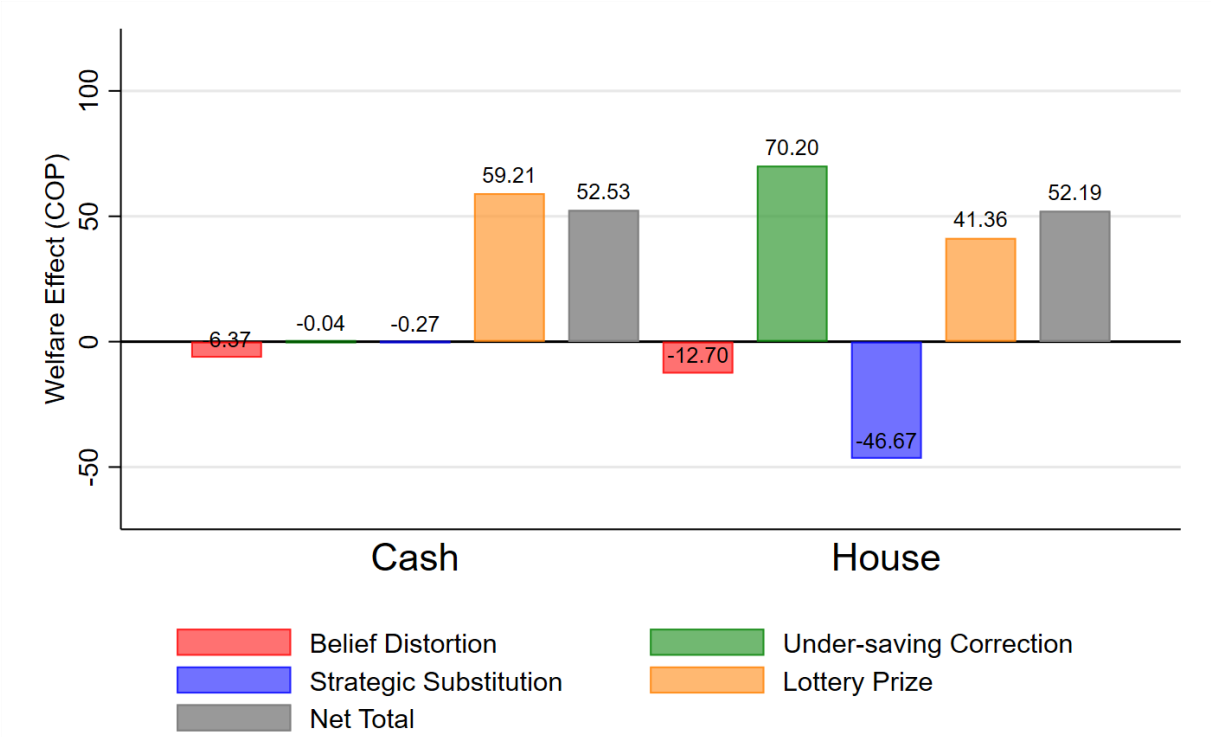
Panel A of Figure 12 shows the direct welfare effects of the lottery, ignoring the change in life insurance coverage. We plot the change in welfare in COP due to i) the expected transfer from the lottery prize, ii) the belief distortion, iii) the correction in under-savings due to present bias, iv) strategic subsitution, and v) the net total of these three effects.

The quantification illustrates nicely that the direct welfare effects of the lottery are essentially zero. The net welfare effect of the Cash lottery amounts to a reduction of 53 COP, which is less than 0.02 USD. Most of this slightly positive effect is simply driven by the fact that a lottery offers an expected windfall gain from the lottery prize. Since we are only looking at welfare of the recipients, we are ignoring how the lottery prize is financed. Including financing costs

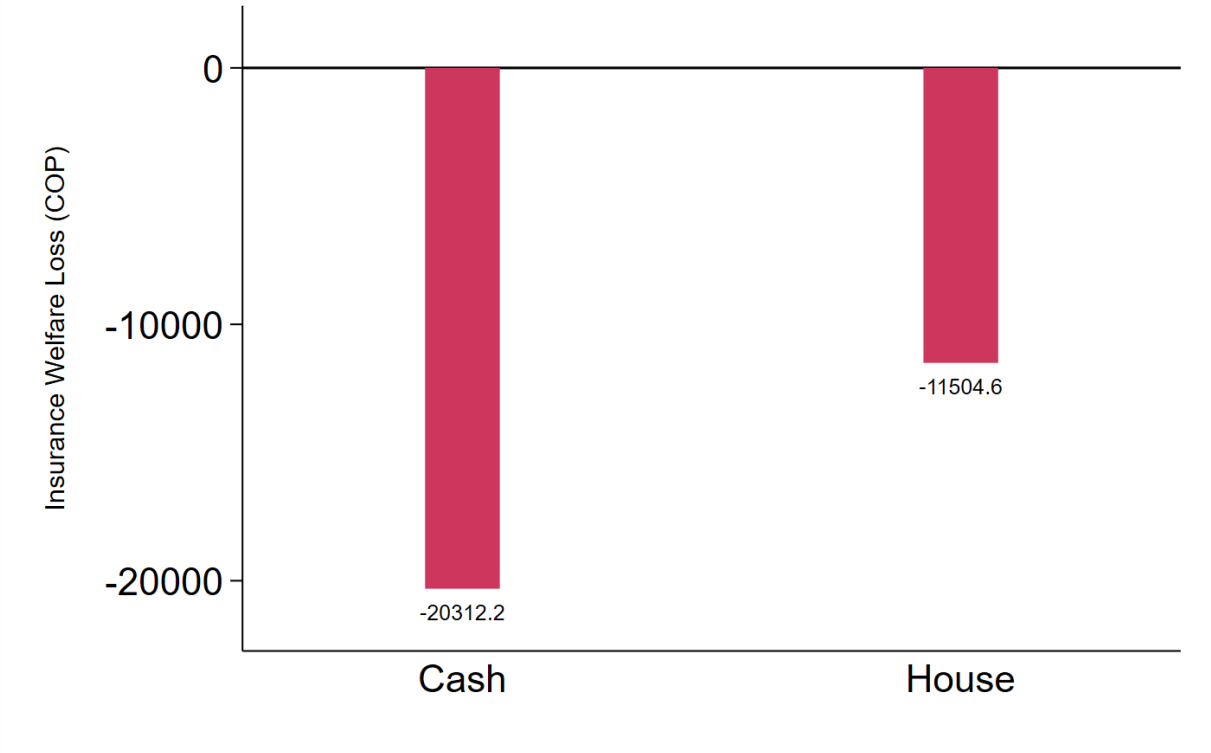
[23] We interpret this reduction in the qualification probability as workers *forgetting* (rather than purposefully forgoing) to qualify for life insurance. This interpretation is welfare-relevant because it implies workers make a mistake rather than a rational tradeoff. This interpretation seems most reasonable to us, given i) the large willingness to pay for the life insurance, ii) our earlier survey evidence showing that the majority of participants states the life insurance as the main motivation to save into the BEPS account, and iii) strong bunching and the qualification threshold in Figure 6.

Figure 12: Welfare Effects

(a) Direct Effects



(b) Indirect Effects due to Reduced Life Insurance Coverage



would mitigate the welfare effect further. For the House lottery, welfare increases by a similar amount of 52 COP. Here, the benefits of the lottery are both the lottery prize (41 COP) and the correction in under-savings (70.20 COP), who outweigh the negative welfare effects arising from an over-estimation of the winning probability (-14.15 COP) and the strategic substitution from the post-lottery period (-46.67 COP).

Turning to Panel B, we find that the most important effect of the lottery is the loss in life insurance coverage. This effect is largest for the Cash lottery, causing a reduction in welfare by 20,312 COP. The welfare loss from the House lottery is around half the size, 11,504 COP. These magnitudes are 3% and 1% of monthly income, respectively, and, therefore, non-negligible. It is also important to note that these are the ex-ante welfare effects on the *average* worker. Since lotteries only caused a reduction in the life insurance qualification by up to 1%, looking at average welfare effects masks how much the 1% of workers who will be hit by the insurance loss will suffer. To put this in perspective, the annual value of life insurance (willingness to pay) is 1 million COP. For a worker who loses coverage for two years, the present value of this loss is $1,000,000 \times (0.99 + 0.99^2) = 1,970,100$ COP. This represents approximately 5.4% of annual income. Therefore, while the average welfare loss is 20,312 COP for cash lottery participants (affecting them with probability 2.06%), a worker who actually loses insurance coverage suffers a welfare loss of nearly 2 million COP. This highlights that the lottery creates a small risk of an enormous welfare loss for individual workers. If these are particularly vulnerable workers that society assigns higher welfare weights to, then the negative welfare implications of the lotteries could be further amplified.

Result 9: *Lotteries reduce welfare by crowding out life insurance coverage. Without this crowd-out effect, the welfare effect becomes effectively zero.*

6 Conclusion

This paper offers empirical evidence on Pigouvian lotteries that incentivize retirement savings, documents unintended side effects, and compares these policies to deterministic savings subsidies.

Lotteries fail along three central dimensions of the policy rationale. First, they do not meaningfully increase savings. Workers strategically substitute savings from future periods into the lottery qualification period, which erases most of the observed short-run benefits. The policies also do no build lasting saving habits as post-intervention effects are zero for at least four years. Second, lotteries interfere with other retirement incentives, causing workers to forget to qualify for life insurance coverage. Third, they do not target the households most likely to suffer from

present bias. Instead, they benefit relatively wealthier workers who are more likely to save even without the policy.

By contrast, deterministic incentives in the form of savings matches substantially and permanently increase savings. We estimate that matches can be over 7 times more cost effective than lotteries.

While our results are not particularly favorable to Pigouvian lotteries, our conclusions are limited to the specific designs we study. Given the scarcity of evidence, it is too early to discard corrective lotteries from the public finance toolbox. Our findings instead raise the question of whether alternative designs could mitigate the drawbacks we document. More promising lotteries may involve larger but less frequent prizes, closer integration with existing social insurance incentives, and more direct targeting of present-biased workers. Given their widespread use in practice, corrective lotteries warrant careful evaluation to determine whether alternative calibrations can complement traditional incentives.

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A Additional Figures

Table A1: Treatment Messages Translated into English

	Second Thursday	Fourth Thursday
Control N = 96,270	Save with Colpensiones BEPS and protect your old age. Call now at [redacted]	Do you want to build a more secure future? Save in the Colpensiones BEPS program. Tel [redacted]
House (Pooled) N = 96,337	Earn with Colpensiones BEPS. Save $500k by [DATE] and participate in the drawing for a home. Call [redacted]	Want to win with BEPS-Colpensiones? Accumulate savings of $500k by [DATE] for the drawing of a home. Call [redacted]
House + Cash prize N = 14,201	Earn with Colpensiones BEPS. By saving $55k every two weeks you can earn $100k in cash. Accumulate $500k by [DATE] for housing lottery. Call [redacted]	Want to win with BEPS-Colpensiones? Save $55k every two weeks for $100k in cash and accumulate $500k by [DATE] for housing lottery. call [redacted]
Cash prize N = 14,201	Earn with BEPS-Colpensiones. If you save $55k every two weeks, you can earn $100k in cash. Call [redacted]	Want to win with BEPS-Colpensiones? Save $55k every two weeks for the $100k cash drawings. Call [redacted]

Notes: This table shows English translations of the text messages that were sent in Spanish to households. The date presented in the table refers to the date of the nearest House lottery.

Figure A1: Aggregate Treatment Effects on Savings in Cash Treatment

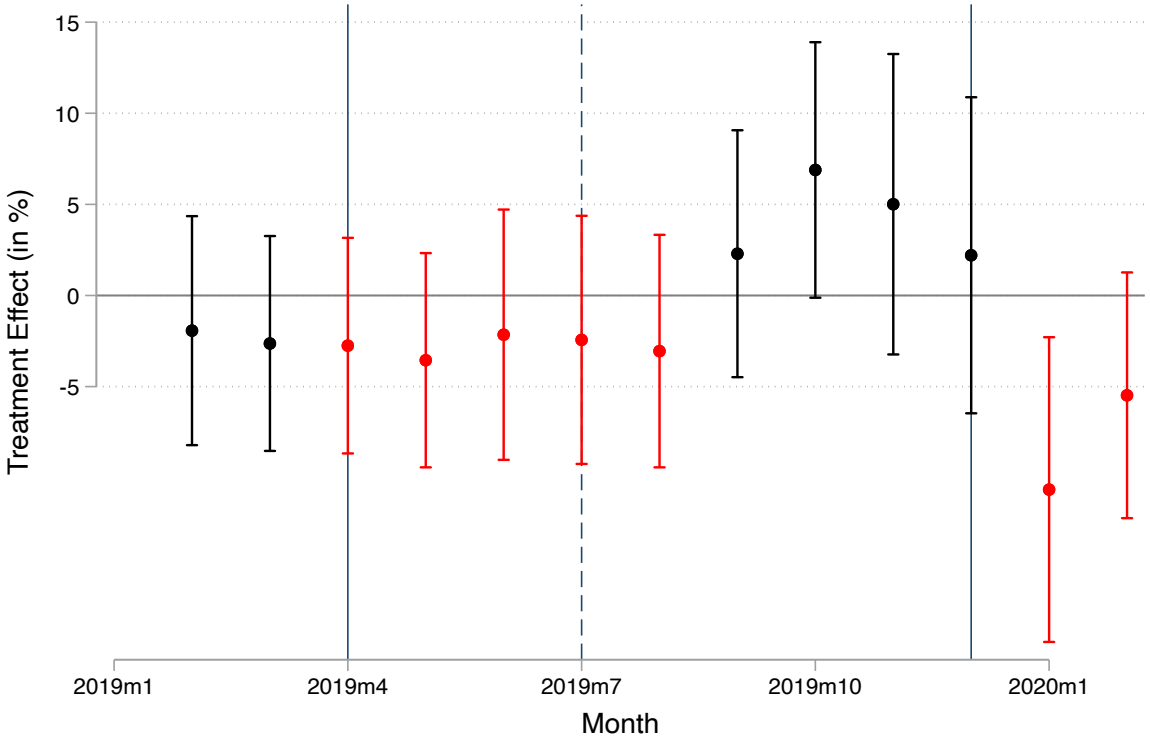
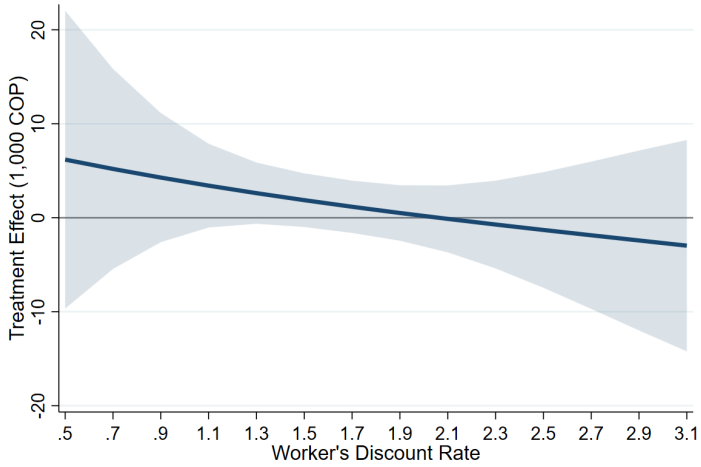
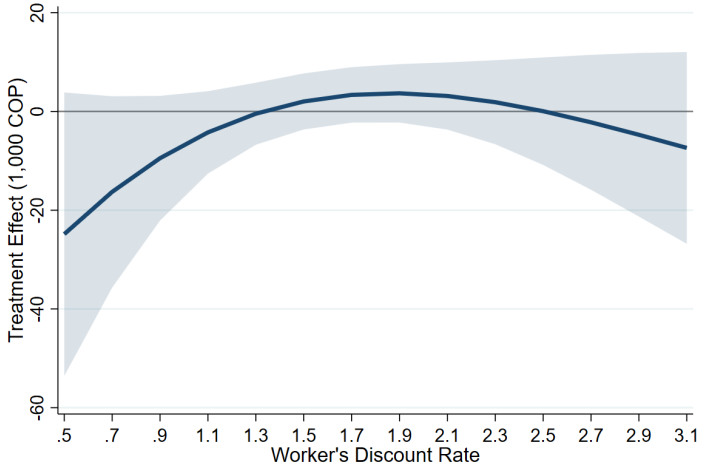


Figure A2: Treatment Effect Heterogeneity by Impatience

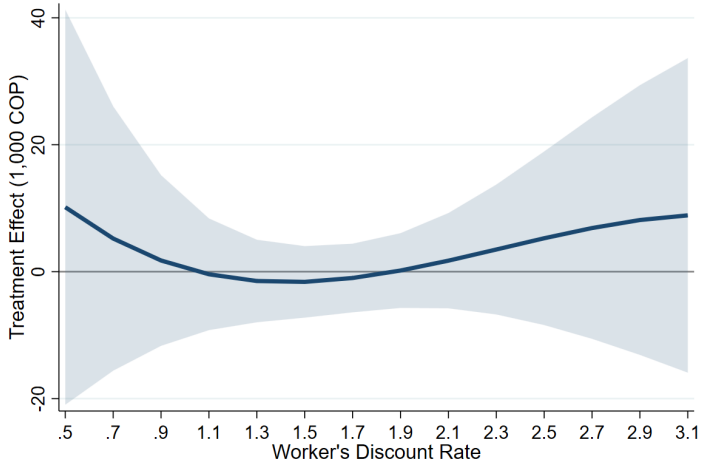
(a) House



(b) Cash



(c) House + Cash



B Additional Tables

Table A2: Sample Characteristics and Treatment Balance

		Treatment Arm				
	All	Control	House	House + Cash	Cash	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
a. Baseline Savings:						
2018 Savings (1000s Pesos)	96.52	96.80	96.49	95.79	95.53	0.86
	(0.41)	(0.63)	(0.63)	(1.62)	(1.62)	
2019 Savings (1000s Pesos)	23.32	23.33	23.26	23.87	23.11	0.75
	(0.14)	(0.21)	(0.21)	(0.56)	(0.55)	
2018 Positive Savings (%)	68.00	67.98	67.93	68.44	68.19	0.63
	(0.10)	(0.15)	(0.15)	(0.39)	(0.39)	
2019 Positive Savings (%)	30.11	30.09	30.00	30.74	30.30	0.32
	(0.10)	(0.15)	(0.15)	(0.39)	(0.39)	
2018 Deposits	3.88	3.88	3.86	3.86	3.92	0.66
	(0.01)	(0.02)	(0.02)	(0.04)	(0.05)	
2019 Deposits	0.78	0.78	0.78	0.78	0.79	0.60
	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	
$\geq$ Six Deposits in 2018 (%)	35.88	35.95	35.79	35.69	36.11	0.77
	(0.10)	(0.15)	(0.15)	(0.40)	(0.40)	
$\geq$ Six Deposits in 2019 (%)	2.08	2.09	2.06	1.94	2.27	0.26
	(0.03)	(0.05)	(0.05)	(0.12)	(0.12)	
b. Characteristics:						
Female (%)	0.70	0.70	0.70	0.70	0.70	0.84
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
Average Age (Years)	50.64	50.64	50.66	50.50	50.60	0.37
	(0.02)	(0.03)	(0.03)	(0.09)	(0.09)	
Sisben	31.50	31.47	31.52	31.48	31.53	0.93
	(0.04)	(0.06)	(0.06)	(0.15)	(0.15)	
Age of Account (Years)	1.87	1.87	1.87	1.85	1.87	0.26
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	
Active	0.50	0.50	0.50	0.50	0.50	0.61
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
Observations	221,009	96,270	96,337	14,201	14,201	

Notes: This table includes the mean of each variable by treatment group. Savings variables are calculated for the full year in 2018 and for the pre-experimental period (January - March) in 2019. Standard errors are presented below the mean in parentheses. Column 6 displays the *p*-value from a Wald test evaluating whether all values are equal across groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table A3: Heterogeneity by Impatience & Myopic Personality Traits

	Impatience	Finish Tasks	Prefer Relaxing	Works Hard
House	1.504	1.401	1.534	1.494
	(1.221)	(1.218)	(1.220)	(1.221)
Cash	0.804	0.864	1.055	0.832
	(2.426)	(2.422)	(2.430)	(2.424)
House+Cash	-0.360	-0.106	-0.344	-0.317
	(2.412)	(2.416)	(2.412)	(2.414)
Impatience	-15.53***	-22.21***	-14.20***	-18.08***
	(1.814)	(1.241)	(1.227)	(1.241)
Risk Aversion	29.72***	33.28***	26.77***	31.64***
	(1.968)	(1.944)	(1.996)	(1.979)
Finish What Begin		51.66***		
		(3.996)		
Prefer Relaxing			-20.67***	
			(2.167)	
Work Hard				4.538*
				(2.650)
House × Impatience	-3.122			
	(2.511)			
House × Finish What Begin		8.368		
		(5.581)		
House × Prefer Relaxing			1.756	
			(2.995)	
House × Work Hard				2.556
				(3.601)
Cash × Impatience	1.663			
	(4.552)			
Cash × Finish What Begin		-10.68		
		(11.32)		
Cash × Prefer Relaxing			-13.20**	
			(6.163)	
Cash × Work Hard				2.432
				(6.866)
House+Cash × Impatience	-0.842			
	(4.914)			
House+Cash × Finish What Begin		10.85		
		(10.50)		
House+Cash × Prefer Relaxing			6.900	
			(5.675)	
House+Cash × Work Hard				2.648
				(6.481)
Observations	117036	117036	117036	117036
R-squared	0.003	0.007	0.004	0.003

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table includes the mean of each variable by treatment group. Savings variables are calculated for the full year in 2018 and for the pre-experimental period (January - March) in 2019. Standard errors are presented below the mean in parentheses. Column 6 displays the *p*-value from a Wald test evaluating whether all values are equal across groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table A4: Heterogeneity by Risk Aversion & Impatience

	Low Risk Averse		Highly Risk Averse	
	(1) Patient	(2) Impatient	(3) Patient	(4) Impatient
Treatment Effects				
House	3.33***	1.86**	3.14***	1.97**
	(0.95)	(0.87)	(1.03)	(0.89)
Cash	−1.21	−2.22	0.02	0.90
	(1.72)	(1.49)	(1.86)	(1.71)
House+Cash	−0.92	1.92	3.16	0.11
	(1.79)	(1.75)	(2.05)	(1.72)
Control Mean	31.41***	21.17***	25.24***	26.34***
	(0.63)	(0.58)	(0.66)	(0.60)
Observations	34,785	24,931	24,327	32,993
R-squared	0.000	0.000	0.000	0.000

Dependent variable: Savings in first lottery period (thousands COP)

Robust standard errors in parentheses

Subgroups defined by median split of risk aversion and impatience measures

Notes: This table includes the mean of each variable by treatment group. Savings variables are calculated for the full year in 2018 and for the pre-experimental period (January - March) in 2019. Standard errors are presented below the mean in parentheses. Column 6 displays the *p*-value from a Wald test evaluating whether all values are equal across groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table A5: Treatment Effects on Savings

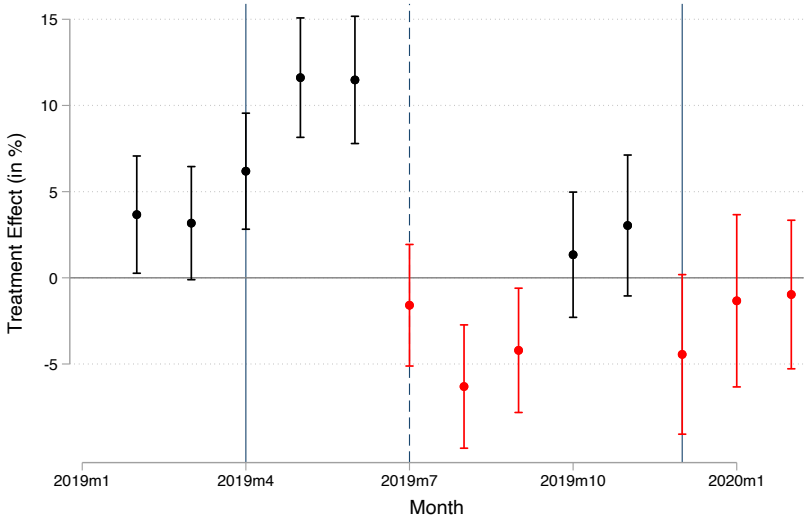
	Savings		Insurance
	(1) Levels	(2) Poisson	(3) (Percent)
Panel A: All participants			
House	1.624***	0.0221**	-0.242
	(0.533)	(0.0101)	(0.149)
Cash	0.626	0.00157	-0.757***
	(1.058)	(0.0209)	(0.292)
House + Cash	1.199	0.0127	-0.995***
	(1.068)	(0.0205)	(0.289)
Observations	221009	221009	221009
Mean dep. var.	69.54	132.9	26.42
Panel B: Active			
House	3.472***	0.0230**	-0.521*
	(1.000)	(0.00921)	(0.274)
Cash	1.713	0.00739	-1.473***
	(2.008)	(0.0189)	(0.539)
House + Cash	2.619	0.0151	-1.903***
	(2.020)	(0.0189)	(0.534)
Observations	109791	109791	109791
Mean dep. var.	132.9	132.9	50.82
Panel C: Inactive			
House	-0.154	-0.0134	-0.0344
	(0.377)	(0.0522)	(0.0972)
Cash	-0.737	-0.0888	-0.187
	(0.663)	(0.105)	(0.185)
House + Cash	-0.408	-0.0484	-0.297*
	(0.672)	(0.101)	(0.180)
Observations	111218	111218	111218
Mean dep. var.	7.345	7.345	2.456

Notes: This table reports treatment effects on savings and the likelihood to qualify for life insurance. Savings is measured over the experimental period (April 8 through December 31, 2019). Treatment effects on savings are estimated in levels and using Poisson regressions. Insurance is a binary variable indicating whether an account qualified for life and disability insurance in 2019. Panel A considers all accounts. Panel B considers all accounts that saved any amount in the six months prior to the experiment. Panel C considers accounts that did not save in the si months prior to the experiment. All columns control for the baseline value of the outcome variable, age, gender, social support (SISBEN), and account age, as well as region fixed effects. Standard errors are clustered on the participant level. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

C Additional Treatments

Figure A3: Aggregate Treatment Effects on Savings

(a) House+Cash



(b) Cash

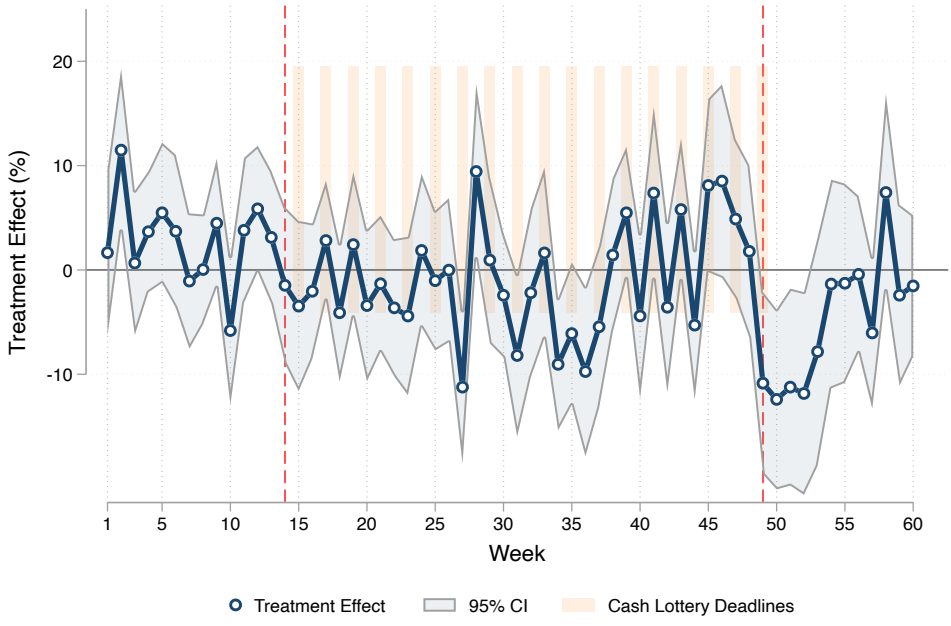


Table A6: Treatment Effects on Savings

	Savings		Insurance
	(1) Levels	(2) Poisson	(3) (Percent)
Panel A: All participants			
House	1.608***	0.0223**	-0.243
	(0.534)	(0.00976)	(0.149)
Cash	0.613	0.00233	-0.761***
	(1.059)	(0.0201)	(0.292)
Cash (July)	-2.425***	-0.0370***	0.0823
	(0.555)	(0.0105)	(0.156)
House + Cash	1.238	0.0137	-0.989***
	(1.069)	(0.0198)	(0.289)
House + Cash (July)	0.428	0.00408	0.646***
	(0.560)	(0.0104)	(0.156)
Observations	386332	386332	386332
Mean dep. var.	69.54	132.9	26.42
Panel B: Active			
House	3.431***	0.0233***	-0.525*
	(1.001)	(0.00893)	(0.274)
Cash	1.667	0.00786	-1.482***
	(2.009)	(0.0183)	(0.539)
Cash (July)	-4.172***	-0.0357***	0.0705
	(1.042)	(0.00959)	(0.287)
House + Cash	2.706	0.0165	-1.892***
	(2.022)	(0.0183)	(0.534)
House + Cash (July)	0.826	0.00369	1.144***
	(1.049)	(0.00951)	(0.287)
Observations	192095	192095	192095
Mean dep. var.	132.9	132.9	50.82
Panel C: Inactive			
House	-0.153	-0.0133	-0.0350
	(0.377)	(0.0522)	(0.0972)
Cash	-0.744	-0.0845	-0.186
	(0.663)	(0.105)	(0.185)
Cash (July)	-0.624	-0.0789	0.00638
	(0.384)	(0.0557)	(0.101)
House + Cash	-0.418	-0.0531	-0.294
	(0.671)	(0.100)	(0.180)
House + Cash (July)	0.0118	0.0195	0.117
	(0.394)	(0.0547)	(0.103)
Observations	194237	194237	194237
Mean dep. var.	7.345	7.345	2.456

Notes: This table reports treatment effects on savings and the likelihood to qualify for life insurance. Savings is measured over the experimental period (April 8 through December 31, 2019). Treatment effects on savings are estimated in levels and using Poisson regressions. Insurance is a binary variable indicating whether an account qualified for life and disability insurance in 2019. Panel A considers all accounts. Panel B considers all accounts that saved any amount in the six months prior to the experiment. Panel C considers accounts that did not save in the si months prior to the experiment. All columns control for the baseline value of the outcome variable, age, gender, social support (SISBEN), and account age, as well as region fixed effects. Standard errors are clustered on the participant level. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

D Identification in Case of Treatment Spillovers

We formally demonstrate that our observed treatment effects provide a lower bound of the true treatment if control subjects learn about the lotteries through other channels. This leads to an upward bias in control group participation, thereby attenuating the estimated treatment effect.

Let $Y_i(1)$ denote the potential outcome for individual i if they are treated (i.e., receive information about the lottery via SMS), and let $Y_i(0)$ denote the potential outcome if they are not treated. The observed outcome is:

$$Y_i = D_i Y_i(1) + (1 - D_i) Y_i(0), \tag{13}$$

where $D_i = 1$ indicates treatment and $D_i = 0$ indicates control.

The average treatment effect (ATE) is given by:

$$\tau = \mathbb{E}[Y_i(1) - Y_i(0)]. \tag{14}$$

However, in the presence of spillovers, some control individuals ($D_i = 0$) may learn about the lotteries through other means, leading them to participate despite not receiving the SMS. Let S_i be an indicator for such spillovers, with $S_i = 1$ if individual i in the control group learns about the lottery through other channels and $S_i = 0$ otherwise. The observed control group outcome can be decomposed as:

$$Y_i(0) = S_i Y_i(1) + (1 - S_i) Y_i(0^*), \tag{15}$$

where $Y_i(0^*)$ is the true counterfactual outcome if i were not exposed to the lottery in any way.

Taking expectations, we obtain:

$$\mathbb{E}[Y_i(0)] = p_S \mathbb{E}[Y_i(1)] + (1 - p_S) \mathbb{E}[Y_i(0^*)], \tag{16}$$

where $p_S = \mathbb{P}(S_i = 1)$ is the probability of spillover.

The estimated treatment effect, $\hat{\tau}$, compares the treated group to the observed control group:

$$\hat{\tau} = \mathbb{E}[Y_i(1)] - \mathbb{E}[Y_i(0)]. \tag{17}$$

Substituting the expression for $\mathbb{E}[Y_i(0)]$, we obtain:

$$\hat{\tau} = \mathbb{E}[Y_i(1)] - \left(p_S \mathbb{E}[Y_i(1)] + (1 - p_S) \mathbb{E}[Y_i(0^*)]\right). \tag{18}$$

Rearranging terms:

$$\hat{\tau} = (1 - p_S)\left(\mathbb{E}[Y_i(1)] - \mathbb{E}[Y_i(0^*)]\right) = (1 - p_S)\tau. \tag{19}$$

Since $p_S \geq 0$, it follows that:

$$\hat{\tau} \leq \tau. \tag{20}$$

Thus, the estimated treatment effect is attenuated due to spillovers and constitutes a lower bound on the true treatment effect. This formalizes our claim that if control subjects learn about the lotteries through other channels, our observed treatment effects underestimate the true impact of the program. We next show that this is unlikely to be the case.

E Mathematical Appendix

E.1 *Proof of Optimal Savings Response to Lottery*

Proposition 1 (Savings Response to Lottery)**.** *Under CARA preferences, the treatment effect of a lottery with subjective winning probability $\hat{\pi}$ on first-period savings is approximately:*

$$\Delta s \approx \hat{\pi} \cdot \frac{\frac{\delta}{p}\left[e^{-\gamma(y-s_1^l)} - e^{-\gamma(y-s_1^l+x)}\right]/\gamma}{\gamma e^{-\gamma(y-s_0^0)} - \gamma\delta^T R^{2T} e^{-\gamma(w_T^0 + s_0 R^T)}} \tag{21}$$

where p is the savings required per lottery ticket and x is the prize.

Proof. The household's optimization problem with lottery is:

$$\max_{\mathbf{s}} \left\{u_0(y - s_0) + \beta\left(\delta \cdot \mathbb{E}_{\omega(\hat{\pi}, s_0)}[u_1(y_1, s_1, x)] + \delta^2 V_{t+2}(\mathbf{s})\right)\right\} \tag{22}$$

where the expected utility in period 1 is:

$$\mathbb{E}_{\omega(\hat{\pi}, s_0)}[u_1] = u_1(y_1 - s_1 + x)\hat{\pi}\frac{s_0}{p} + u_1(y_1 - s_1)\left(1 - \hat{\pi}\frac{s_0}{p}\right) \tag{23}$$

The first-order condition with lottery is:

$$-u_0'(y - s_0^l) + \beta\delta \cdot \frac{\hat{\pi}}{p}[u_1(y - s_1^l + x) - u_1(y - s_1^l)] + \beta\delta^T R^T u_T'(w_T^l) = 0 \tag{24}$$

Without lottery:

$$-u_0'(y - s_0) + \beta\delta^T R^T u_T'(w_T^0) = 0 \tag{25}$$

Under CARA utility, $u'(c) = e^{-\gamma c}$. Taking a first-order Taylor expansion around s_0^0:

$$e^{-\gamma(y_0 - s_0^l)} \approx e^{-\gamma(y_0 - s_0^0)} + \gamma e^{-\gamma(y_0 - s_0^0)} \Delta s \tag{26}$$

$$e^{-\gamma(w_T^l + s_0^l R^T)} \approx e^{-\gamma(w_T^0 + s_0 R^T)} - \gamma R^T e^{-\gamma(w_T^0 + s_0 R^T)} \Delta s \tag{27}$$

Substituting into the difference of FOCs and solving for Δs yields the result. □

E.2 *Proof of Welfare Effects*

Proposition 2 (Welfare Decomposition)**.** *The first-order welfare effect of informing households about a lottery (changing $\hat{\pi}$ from 0 to $\bar{\pi}$) is:*

$$\frac{\partial W}{\partial \hat{\pi}} \Delta \hat{\pi} = \tau_0 \cdot \left(\frac{\delta \Delta u_1}{p} (\pi - \beta \hat{\pi}) + \delta^T u_T' R (1 - \beta) \right) + \sum_{t=1}^{T-1} \tau_t \cdot \delta^T u_T' R^{T-t} (1 - \beta) \tag{28}$$

where $\tau_t = \frac{\partial s_t}{\partial \hat{\pi}} \Delta \hat{\pi}$ is the treatment effect on savings in period t.

Proof. Social welfare under the lottery is:

$$W(\pi, \mathbf{s}(\beta, \hat{\pi})) = u_0(y - s_0(\beta, \hat{\pi})) + \delta \cdot \mathbb{E}_{\omega(\pi, s_0)}[u_1(y_1, s_1, x)] + \sum_{t=2}^{T-1} \delta^t u(y_t - s_t) + \delta^T u_T(w_T) \tag{29}$$

Taking the total derivative:

$$\frac{dW}{d\hat{\pi}} = \sum_{t=0}^{T-1} \frac{\partial W}{\partial s_t} \frac{\partial s_t}{\partial \hat{\pi}} \tag{30}$$

For $t = 0$:

$$\frac{\partial W}{\partial s_0} = -u_0'(y - s_0) + \delta \cdot \frac{\pi}{p} [u_1(y - s_1 + x) - u_1(y - s_1)] + \delta^T u_T' R^T \tag{31}$$

The household's FOC implies:

$$u_0'(y - s_0) = \beta \delta \cdot \frac{\hat{\pi}}{p} [u_1(y - s_1 + x) - u_1(y - s_1)] + \beta \delta^T u_T' R^T \tag{32}$$

Substituting:

$$\frac{\partial W}{\partial s_0} = \frac{\delta \Delta u_1}{p} (\pi - \beta \hat{\pi}) + \delta^T u_T' R (1 - \beta) \tag{33}$$

For $t \geq 1$:

$$\frac{\partial W}{\partial s_t} = -\delta^t u'(y_t - s_t) + \delta^T u'_T R^{T-t} \tag{34}$$

Using the household's Euler equation: $u'(y_t - s_t) = \beta\delta^{T-t} R^{T-t} u'_T$, we get:

$$\frac{\partial W}{\partial s_t} = \delta^T u'_T R^{T-t}(1-\beta) \tag{35}$$

Combining all periods yields the result. □

E.3 *Welfare Effects of the Lottery Itself*

The direct welfare effect from the expected prize is:

$$\Delta W^{\text{prize}} = \pi \cdot \frac{s_0}{p} \cdot \frac{\delta}{\gamma}[e^{-\gamma c_1} - e^{-\gamma(c_1+x)}] \tag{36}$$

Proof. A household saving s_0 obtains s_0/p lottery tickets. Expected utility with the lottery is:

$$\mathbb{E}[U^{\text{lottery}}] = u_0(y_0 - s_0) + \delta[\pi \cdot \frac{s_0}{p} \cdot u_1(c_1 + x) + (1 - \pi \cdot \frac{s_0}{p}) \cdot u_1(c_1)] + V_2 \tag{37}$$

Without the lottery:

$$\mathbb{E}[U^{\text{no lottery}}] = u_0(y_0 - s_0) + \delta u_1(c_1) + V_2 \tag{38}$$

The difference yields:

$$\Delta W^{\text{prize}} = \delta\pi \cdot \frac{s_0}{p} \cdot [u_1(c_1 + x) - u_1(c_1)] \tag{39}$$

Under CARA utility:

$$u_1(c_1 + x) - u_1(c_1) = -\frac{1}{\gamma}e^{-\gamma(c_1+x)} + \frac{1}{\gamma}e^{-\gamma c_1} = \frac{1}{\gamma}[e^{-\gamma c_1} - e^{-\gamma(c_1+x)}] \tag{40}$$

Therefore:

$$\Delta W^{\text{prize}} = \pi \cdot \frac{s_0}{p} \cdot \frac{\delta}{\gamma}[e^{-\gamma c_1} - e^{-\gamma(c_1+x)}] \tag{41}$$

□

E.4 *Identification of Risk Aversion from Matching Experiment*

Proposition 3 (CARA Parameter Identification)**.** *Given a matching experiment where households receive match m for any positive savings, the CARA parameter γ is identified by:*

$$\frac{\partial \ln\left(\frac{\partial \Pr(s_0(m)>0)}{\partial m}\right)}{\partial m} \approx -\gamma \cdot (w_T'(m)+1) \tag{42}$$

Proof. Let V_m denote the value of saving positive amount with match m, and V_0 the value of not saving. The household saves if:

$$V_m - V_0 \geq \epsilon \tag{43}$$

where ϵ is an unobserved preference shock with CDF $F(\cdot)$.

The probability of saving is:

$$S(m) = \Pr(s_0 > 0) = F(V_m - V_0) \tag{44}$$

The marginal value of the match at retirement is:

$$\frac{\partial V_m}{\partial m} = \delta^T u_T'(w_T(m)+m) = \delta^T e^{-\gamma(w_T(m)+m)} \tag{45}$$

Therefore:

$$\frac{\partial S(m)}{\partial m} = \delta^T e^{-\gamma(w_T(m)+m)} f(V_m - V_0) \tag{46}$$

Taking logs:

$$\ln\left(\frac{\partial S(m)}{\partial m}\right) = \ln(\delta^T) - \gamma(w_T(m)+m) + \ln(f(V_m - V_0)) \tag{47}$$

Differentiating with respect to m:

$$\frac{\partial \ln\left(\frac{\partial S(m)}{\partial m}\right)}{\partial m} = -\gamma(w_T'(m)+1) + \frac{f'(V_m - V_0)}{f(V_m - V_0)} \frac{\partial (V_m - V_0)}{\partial m} \approx -\gamma(w_T'(m)+1) \tag{48}$$

where the approximation requires that $F(\cdot)$ is locally linear—a common requirement in the sufficient statistics literature (Chetty, 2009). □

E.5 *Heterogeneous Preferences and Welfare*

Proposition 4 (Welfare with Heterogeneity)**.** *With heterogeneous preferences* $(\gamma_i, \beta_i, \hat{\pi}_i) \sim F(\gamma, \beta, \hat{\pi})$*, the average welfare effect is:*

$$\begin{aligned}\mathbb{E}\left[\frac{dW_i}{d\hat{\pi}_i}\right] = &\underbrace{\mathbb{E}\left[\frac{\partial s_{0i}}{\partial \hat{\pi}_i}\right] \cdot \mathbb{E}[\Omega_i] + \mathbb{E}\left[\frac{\partial s_{1i}}{\partial \hat{\pi}_i}\right] \cdot \mathbb{E}[\Psi_i]}_{\textit{Homogeneous benchmark}} \\ &+ \underbrace{\Lambda_1 \cdot \textit{Var}(\gamma_i) + \Lambda_2 \cdot \textit{Cov}(\gamma_i, \beta_i) + \Lambda_3 \cdot \textit{Cov}(\gamma_i, \hat{\pi}_i)}_{\textit{Risk aversion interactions}} \\ &+ \underbrace{\Lambda_4 \cdot \textit{Var}(\hat{\pi}_i) + \Lambda_5 \cdot \textit{Cov}(\beta_i, \hat{\pi}_i)}_{\textit{Belief interactions}} \end{aligned} \tag{49}$$

where Λ_k *are functions of second derivatives evaluated at population means.*

Proof. Using the identity $\mathbb{E}[XY] = \mathbb{E}[X]\mathbb{E}[Y] + \text{Cov}(X, Y)$ and second-order Taylor expansions around population means $(\bar{\gamma}, \bar{\beta}, \bar{\hat{\pi}})$:

$$\frac{\partial s_{0i}}{\partial \hat{\pi}_i} \approx \left.\frac{\partial s_0}{\partial \hat{\pi}}\right|_{(\bar{\gamma}, \bar{\hat{\pi}})} + \left.\frac{\partial^2 s_0}{\partial \hat{\pi} \partial \gamma}\right| (\gamma_i - \bar{\gamma}) + \left.\frac{\partial^2 s_0}{\partial \hat{\pi}^2}\right| (\hat{\pi}_i - \bar{\hat{\pi}}) \tag{50}$$

$$\Omega_i \approx \bar{\Omega} + \left.\frac{\partial \Omega}{\partial \gamma}\right| (\gamma_i - \bar{\gamma}) + \left.\frac{\partial \Omega}{\partial \beta}\right| (\beta_i - \bar{\beta}) + \left.\frac{\partial \Omega}{\partial \hat{\pi}}\right| (\hat{\pi}_i - \bar{\hat{\pi}}) \tag{51}$$

Computing the covariance:

$$\text{Cov}\left(\frac{\partial s_{0i}}{\partial \hat{\pi}_i}, \Omega_i\right) = \mathbb{E}\left[\left(\frac{\partial s_{0i}}{\partial \hat{\pi}_i} - \mathbb{E}\left[\frac{\partial s_{0i}}{\partial \hat{\pi}_i}\right]\right)(\Omega_i - \mathbb{E}[\Omega_i])\right] \tag{52}$$

Substituting the Taylor expansions and collecting terms yields the decomposition with:

$$\Lambda_1 = \left.\frac{\partial^2 s_0}{\partial \hat{\pi} \partial \gamma}\right| \cdot \left.\frac{\partial \Omega}{\partial \gamma}\right| + \left.\frac{\partial^2 s_1}{\partial \hat{\pi} \partial \gamma}\right| \cdot \left.\frac{\partial \Psi}{\partial \gamma}\right| \tag{53}$$

$$\Lambda_2 = \left.\frac{\partial^2 s_0}{\partial \hat{\pi} \partial \gamma}\right| \cdot \left.\frac{\partial \Omega}{\partial \beta}\right| + \left.\frac{\partial^2 s_1}{\partial \hat{\pi} \partial \gamma}\right| \cdot \left.\frac{\partial \Psi}{\partial \beta}\right| \tag{54}$$

and analogously for Λ_3, Λ_4, and Λ_5. □