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. Lotteries boost savings temporarily during the qualification period and cause large bunching at the qualification threshold. However, savings fall by almost the same amount in the post-treatment period, erasing most of the intended policy effect. Lotteries further crowd out attention to other financial incentives, causing households to forgo life and disability insurance. Following households for 4 years post-intervention, we find that lotteries induce no positive long-run effects on savings. We leverage a second field experiment that randomizes deterministic savings subsidies ("matches") and show that they are more cost-effective in increasing savings than lotteries. Finally, we estimate subjective winning probabilities of lotteries and find evidence of probability weighting. Our results illustrate how savings policies can backfire along multiple dimensions that often remain unobserved.

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

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1 Introduction

For over a century, economists have studied the use of taxation to optimize resource allocations in society (Pigou, 1920). Taxes and subsidies provide deterministic incentives that operate through people's price elasticities. One alternative to subsidies is the use of *probabilistic* incentives—"Pigouvian lotteries"—that reward desirable behavior with the chance to win a prize. These policies may not just strain the government budget less than subsidies, making them particularly attractive for countries with tight resource constraints. They may also target a different segment of the population: individuals who are unresponsive to deterministic incentives but attracted to upside risk and gambling. It has been plausibly argued that especially the lower part of the wealth distribution would be responsive to lotteries (Herskowitz, 2021). While there have been advocates calling for research on Pigouvian lotteries, there is strikingly little evidence on how they change allocations and welfare (Lange et al., 2007).

In contrast, policymakers and banks have made ubiquitous use of lotteries, mostly in order to encourage savings. Throughout Latin America and other developing countries, governments and banks offer households the chance to win a prize if they save a minimum amount or open a deposit account.[1] The economic frictions that these savings vehicles try to address are inadequate access to capital markets and household myopia regarding future retirement needs (Gomes et al., 2021). In this paper, we study a nation-wide natural field experiment by the Colombian public pension fund that randomized savings lotteries. We obtain data on over 200,000 informal workers and observe their retirement contributions for a period of 6 years. The pension fund invests contributions into a conservative inflation-protected portfolio and represents the only way for these workers to access capital markets. The data allows us to answer i) whether lotteries increase savings,

[1] See, for instance, Guillén and Tschoegl (2002). PLS accounts also became legal in the United States in 2014 under the American Savings Promotion Act (United States Congress, 2014).

ii) whether they cause a habit to save post-intervention, iii) whether they are addictive, and iv) how they interact with the demand for other financial assets, in particular life and disability insurance. In addition, we obtain data on a second nationwide field experiment that randomized *deterministic* savings subsidies ("matches"). This auxiliary experiment allows us to study v) under which conditions lotteries can be more cost-effective than deterministic incentives.

In the experiment, Colombia's public pension fund "Colpensiones" randomly offered households the chance to win a prize of size X if the household deposited at least Y Colombian 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 to save Y =55,000 COP (= 17 USD in 2019) within $T = 2$ weeks. For the House lottery, the savings threshold was Y =500,000 COP (= 153 USD) with a deadline of $T = 6$ months. 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% for the Cash lottery and 0.3% 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 strategic intertemporal substitution by households eliminates most of the benefit of the

policy. Specifically, households pre-pone some of their future savings into the qualification period in order to meet the lottery threshold. As a consequence, savings *decrease* for treated households after the qualification period has passed. The overall effect on savings (counting both qualification and post-qualification period) is *zero* for the Cash lottery and 2.2% for House lottery.

We observe further unintended effects of the policy. As a hybrid social security system, Colpensiones also offers households free disability and life insurance if they make at least 6 deposits throughout the year. The lotteries cause households to save larger amounts but *less frequently*, which disqualifies some households for insurance coverage. During the experiment in 2019, the lotteries reduce the probability to qualify for insurance by 0.5%. One year after the intervention, the probability to qualify is even 1% lower in the treatment groups because the lotteries induced a habit to deposit less frequently. We 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 policies—a phenomenon that has also been documented for Pigouvian subsidies in other contexts (Rodemeier and Löschel, 2025).

We then follow households for 4 years post-intervention. During these years, Colpensiones ran additional savings lotteries that were not randomized. We do not find that treated households are more inclined to respond to these lotteries than control households. This rejects the hypothesis that these lotteries cause addictive behavior—a natural concern with policies that use gambling to change behavior. Models of addiction (Becker and Murphy, 1988) would predict that treated households are more likely to gamble again during future lotteries, which is not the case. This evidence is helpful for policy debates around whether Pigouvian lotteries cause a gambling addiction and should be prohibited

(see e.g., National Conference of State Legislatures, 2019).[2] On a perhaps less optimistic note, we do not see that treated subjects increase savings in general post-intervention. This is important because the primary motive of savings lotteries is likely not to increase savings *once* but rather to establish a habit of saving more permanently.

Would deterministic incentives provide a more favorable outcome? To answer this question, we obtain access to a second experiment in 2022 in which 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.

As our last empirical exercise, we try to understand whether households' beliefs about the odds to win the lottery are roughly in line with the true winning odds. The true winning probability is equal to the number of prizes raffled divided by the number of households qualifying for the lottery. In order to have accurate beliefs about the odds, households need to form beliefs about how many other households participate in the lottery. Identifying these subjective probabilities is difficult to answer with choice data from the field and requires us to take a more structural approach. We show theoretically that subjective probabilities can be empirically recovered by comparing the savings response to lotteries with the response to matches, as long as we parameterize a utility function. Assuming CARA preferences, we find that households underestimate the winning prob-

[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).

ability 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.

While many of our results point to pitfalls, we establish a number of opportunities and pathways on how to design better lotteries. First, lotteries can be cost-effective as long as the lottery prize is large. While our cash lotteries always proved ineffective, our house lotteries increased savings by 2 COP per every COP invested by the pension fund (although these estimates are noisy). Second, there is no evidence that lotteries are addictive. Our evidence can be held against common anti-gambling arguments that generate political opposition to these tools. Third, if lotteries are implemented, they should be designed in a way not to interfere with other existing incentives. In our case, the fact that both lotteries and insurance coverage were tied to a specific savings behavior caused cognitive interference.

Literature Our paper ties into a small strand of work that discusses lotteries as tools for public policy. Early research has studied lotteries to finance public goods (Lange et al., 2007) and UK Premium Bonds that have a lottery-like element (Tufano, 2008). Following 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 documenting unintended consequences and long-term dynamics, ii) by studying public retirement policy instead of private marketing strategies, and iii) by providing a comparison to deterministic savings subsidies from a complementary field experiment.[3] Existing work has shown that

[3]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

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.

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. These forms of substitution from other accounts are mitigated in our setting where informal workers do not have access to commercial banks and credit cards. Yet, we find other dimensions along which retirement savings policies can distort choice, in particular the demand for important life and disability insurance.

Our evidence further helps understanding behavioral public policy in developing countries (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.

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.

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 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.[4] To save, BEPS participants make deposits with a defined minimum amount ($5,000 COP) with 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, the portfolio value is paid out to participants in terms of bimonthly annuities. They also receive a one-time government match of 20% on their cumulative contributions (Villa and Benitez, 2023).

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.

[4]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).

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 ($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 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 biweekly 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, since the text messages were the only way households were informed about a lottery, this randomiza-

tion 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 C, 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.[5] 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.

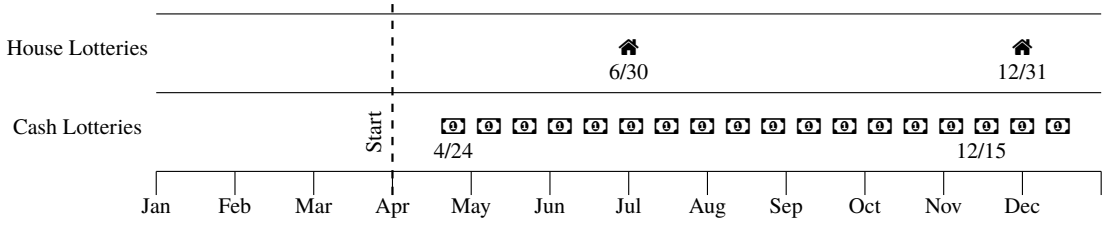
Note that in the first House lottery, subjects were only informed roughly 3 months prior to the deadline, while in the second House lottery they were informed 6 months

[5]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.

before.[6] 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.

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 A2 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

[6]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.

with more females 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 rewards savings above some threshold $\underline{s}$ but not below. This should increase both i) the probability that savings are above $\underline{s}$ and ii) the density at $\underline{s}$. To test for this, we construct an indicator variable $y_{it} \in \{0, 1\}$ that for subject i. In our first empirical specification, $y_{it} = \mathbf{1}(s_{it} = \underline{s})$, in our second specification we use $y_{it} = \mathbf{1}(s_{it} \geq \underline{s})$. In both specifications, we regress outcome y_{it} of subject i on treatment vector τ_i:

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

λ_t are month fixed effects, ϵ_{it} is the residual. We run this regression i) for biweekly savings and set $\underline{s}$ =\$55,000, and ii) for biannual savings and set $\underline{s}$ =\$500,000.

Treatment coefficients are shown in Figure 2. In Panel a, we plot 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 is a large relative increase of 600% compared to Control. The House+Cash group shows a similar but slightly smaller treatment effect. Consistent with correct randomization, subjects in the House treatment did not bunch at the Cash threshold.

The House lottery increases the probability of biannual savings to bunch at the 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.

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 percentage points (or 11% relative to control). The impact of the Cash lottery in isolation is smaller, around 0.14pp or 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 is not due to issues with treatment assignment, but happens mechanically: Even subjects that save for the House lottery are more likely to qualify for the Cash lottery, simply because they increased their savings. This is an important difference to 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.[7]

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

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

effect of the Cash treatment.

Result 1: *Lotteries cause savings to bunch 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, we obtain a different picture once we study treatment effect dynamics. Figure 3 shows the monthly treatment effect on *savings* (measured in 1,000 COP) over time, pooling all three treatment groups for illustrative purposes. We provide effects for each subgroup further below.

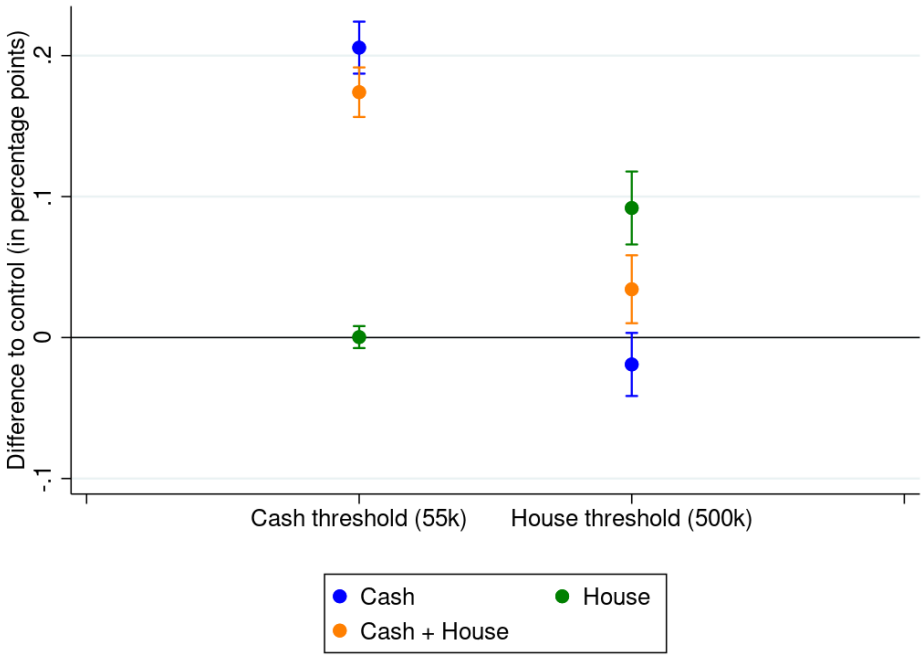
The plot starts in January 2019. The first red bar is April 11th where subjects received the first lottery offers. The second red bar is the end of the first and the start of the second House lottery (July 1). The last bar is December 31st when the qualification period for the second House lottery ends.

In line with the timing of the treatments, we do not observe a differential savings response between treatment groups and control prior to April 11th.[8] Upon receiving the offer, monthly savings increase immediately as visualized by the first positive spike. While the response is immediate, the economic magnitude is small, constituting an increase of 500 COP ($\approx$ 0.15 USD in 2019) on average. Importantly, after the first qualification period for the House lottery has been reached, the treatment effect becomes statistically significantly *negative*. This indicates that subjects strategically preponed 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 become slightly positive before December. However, the positive effects are again followed by a negative slump in savings at the

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

Figure 2: Bunching at and above Qualification Threshold

(a) Savings = Threshold



(b) Savings $\geq$ Threshold

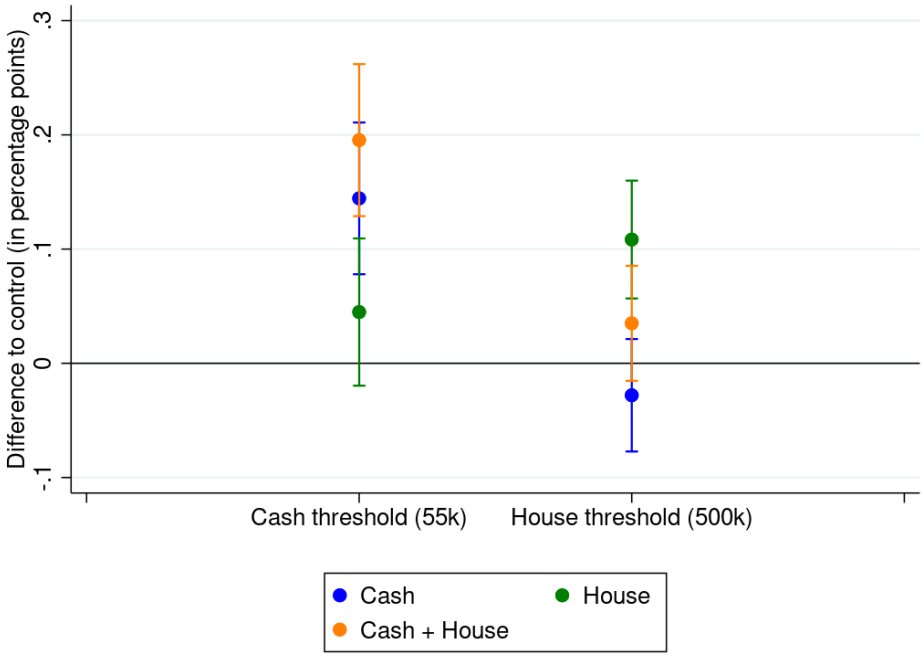
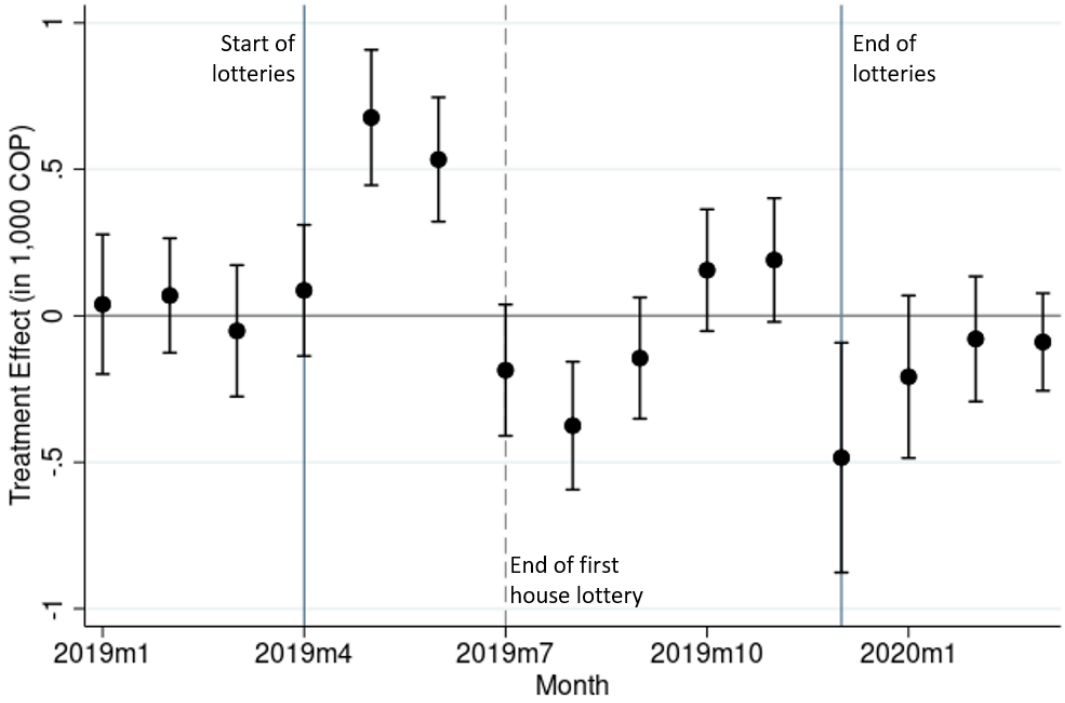


Figure 3: Aggregate Treatment Effect on Savings



end of 2019, just after the end of the last Cash lottery on Dec 15. As we show in Section 3.2, the first positive spike in savings in May and June is mostly driven by the first House lottery, while the second smaller spike in October and November is driven by the Cash lotteries.

Table A3 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 Panel C and B of the table, this effect is entirely concentrated around active savers (those that saved a positive amount in the previous year). There is

no effect on inactive savers.

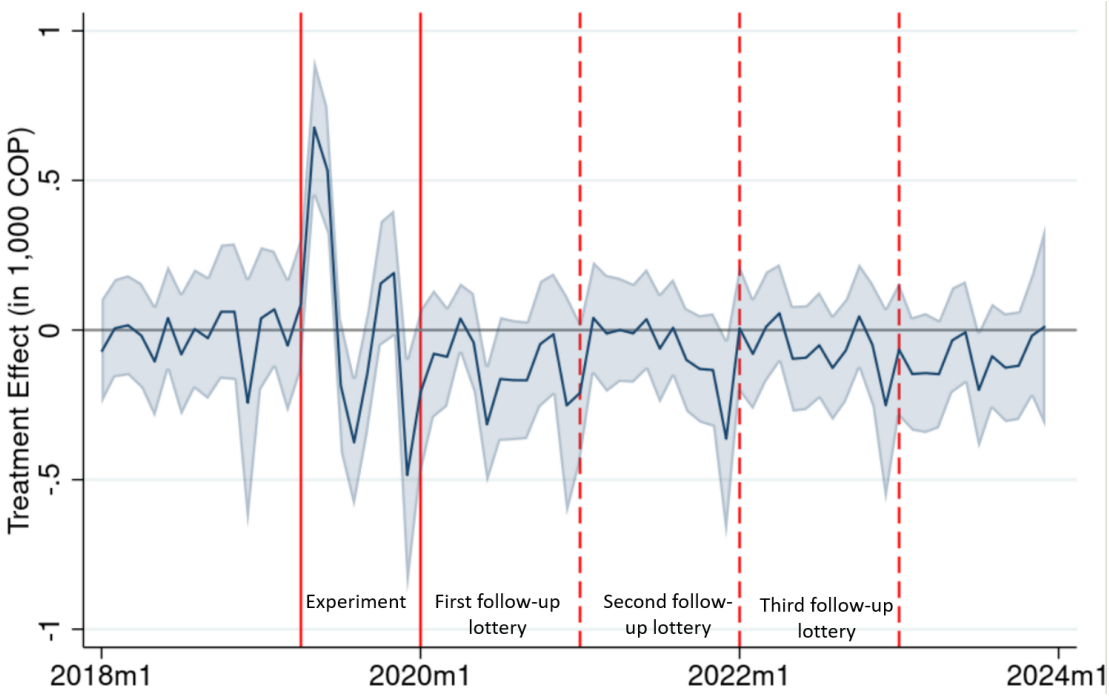
Result 2: *Lotteries cause savings to increase during the qualification period but to almost equally decrease afterwards. The overall effect on savings ranges from zero to 2.2%.*

One may argue that the primary objective of the policy is not to induce one-time savings but rather to establish a habit of saving, long after the lottery has expired. To study this question, we obtained additional administrative data from BEPS that allows us to follow subjects behavior 4 years post-treatment. 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. We plot the time series of treatment effects of our experimental intervention in Figure 3, 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).

If our intervention induced a habit to save more, 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.

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 cause addiction. If lotteries caused addiction, this would mean that being randomly exposed to a lottery once causes households to be more inclined to respond to future lotteries (Becker and Murphy, 1988). This is not the case: different from our experimental time period, there is no positive spike in savings around the (dashed) future savings deadlines.

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



Thus, Figure 4 is informative as it can rule out both i) habit formation in savings (no positive *shift* in post-treatment savings), and ii) an induced gambling addiction for savings lotteries (no spike around threshold).

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

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

3.1 *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 each subject i and year t, we construct a qualification dummy y_{it} that is equal to one if either i) 6 deposits have been made in this year, or ii) 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.

In 2018, there is no effect, which again confirms succesful treatment randomization. During the treatment year, 2019, the lotteries cause a statistically significant drop of –0.5 percentage points 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 (although they save 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.

How important is this loss in insurance for the saving poor? We argue that the welfare loss is likely sizeable. First, 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, we illustrate that insurance is important to household also by revealed preference. 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 also a violation of a model with fully rational agents 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 rather 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.*

3.2 *Heterogeneity by Lottery Prize*

Figure 7 reports the time series of treatment effects for each lottery prize separately. The house prize causes the largest temporary increase during the first 4 months of treatment,

peaking at an increase in savings of 1.2%. After that, the effect becomes negative immediately, reaching almost -0.5%. During the entire 6 months of the second house prize lotter, we do not observe a positive effect. This is likely because households preponed all of their liquid savings into the first period and deposited them with the pension fund to qualify for the first house prize. This pattern highlights again that strategic intertemporal substitution eliminates a large part of the positive treatment effect of the lottery.

The Cash lottery induces no effect on average savings for the first 5 months but causes a slight spike of around 0.25% during the last treatment months. This effect is not significant. It is also fully compensated for one month after the end of the last Cash lottery, where we observe a large negative drop in savings of –0.6% ($p < 0.01$). This implies again that households mostly shift around liquid savings intertemporally instead of increasing total savings.

Panel c) illustrates treatment effects from offering households both prizes. Effects are a combination of Panel a) and b). During the first four months, savings increase by less than 1%, indicating that adding the cash prize to the house prize slightly *reduces* savings. One explanation is that the two prizes compete for attention (Rodemeier and Löschel, 2025) so that the probability to pay attention to the house prize is lower in Panel c. Since cash prizes are less attractive, the total savings response is lower than if all households paid attention to the house prize alone.

None of the treatments induce any long-term habit formation during the four years post-treatment, nor any addictive behavior towards future savings lotteries.

Figure 5: Eligibility for Life & Disability Insurance

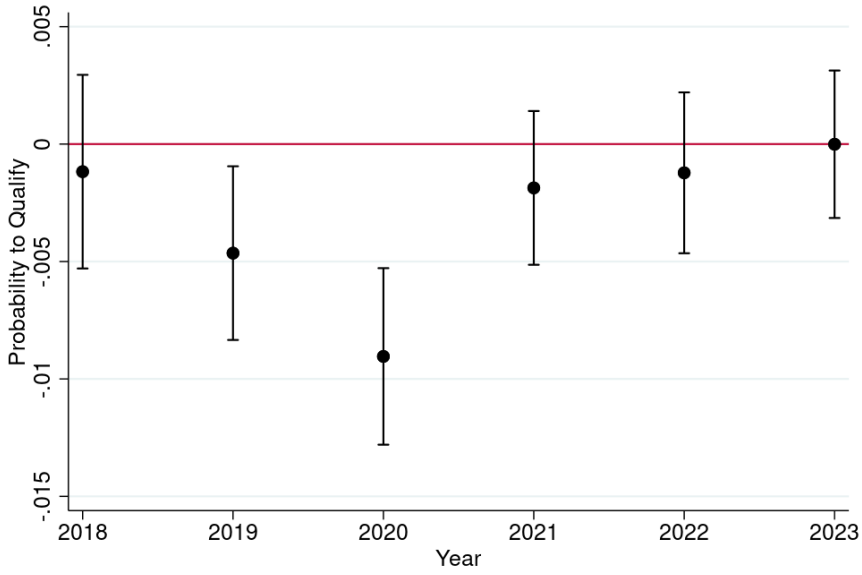
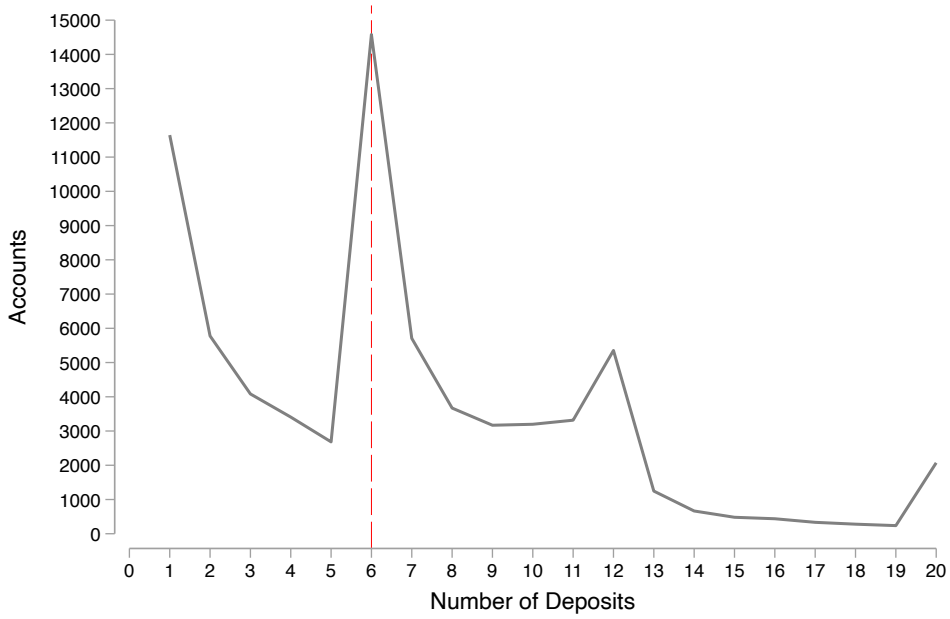


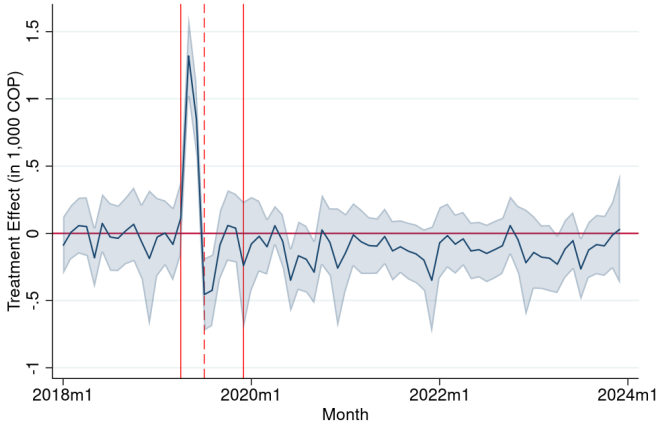
Figure 6: Bunching at the Insurance Threshold Prior to the Experiment



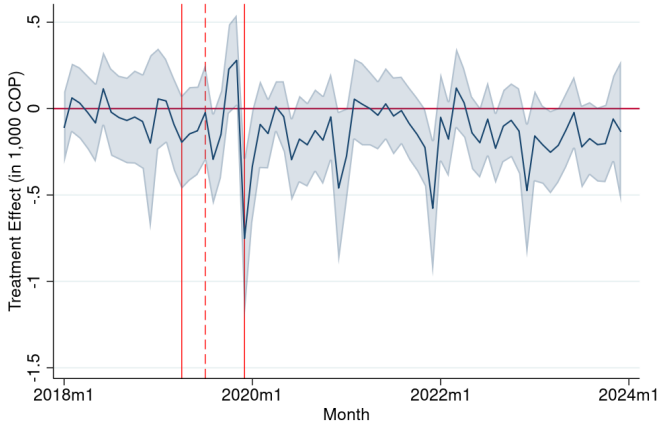
Notes: This figure displays a histogram of the total number of deposits made 2018. The red vertical line indicates the qualifying number of transactions (6) for the BEPS insurance. Figure includes all participants that made at least one deposit in 2018 and that had an account that was opened prior to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

Figure 7: Heterogeneity by Lottery Prize

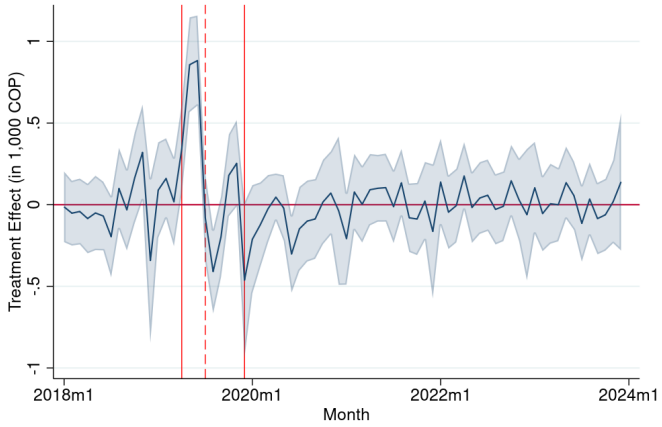
(a) House



(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.[9]

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.

[9] 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.

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 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.

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 8 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

Table 1: Effects of Savings Matches

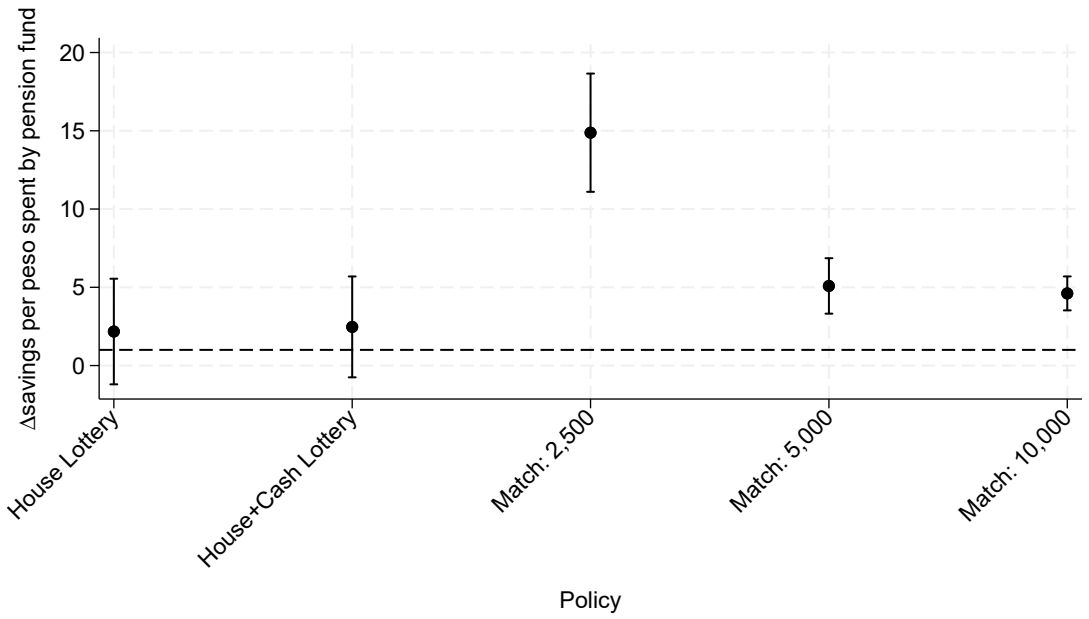
	(1) Saving Probability in Qualification Period	(2) Weekly Savings (in % to Control)	(3) Post-Treatment Savings (in % to Control)
Match amount			
+2,500	0.13***	0.38***	0.25**
	(0.015)	(0.13)	(0.097)
+5,000	0.14***	0.33***	0.24**
	(0.016)	(0.12)	(0.097)
+10,000	0.15***	0.49***	0.31***
	(0.015)	(0.13)	(0.098)
Constant (Control)	0.24***		
	(0.012)		
Observations	7,996	69,233	280,559
Adjusted R^2	0.011	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.

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.

Figure 8: 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:

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

where $\mathbf{E}_{\Phi(\hat{\pi}, s_0)}[u_1(y_1, x)] = u_1(y_1 - s_1 + x)\hat{\pi}\frac{s}{p} + u_1(y_1 - s_1)\left(1 - \hat{\pi}\frac{s}{p}\right)$ 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

$$\underbrace{-u_0'(y - s_0^l)}_{\text{cost of consumption shifting}} + \underbrace{\beta\delta \cdot \frac{\hat{\pi}}{p}\left[u_1(y - s_1^l + x) - u_1(y - s_1^l)\right]}_{\text{perceived lottery gain}} + \beta \cdot \underbrace{\delta^T \cdot R^T \cdot u_T'(w_T^l)}_{\text{savings benefit at retirement}} = 0, \tag{3}$$

while in the absence of the lottery it is

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

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.Equating the two FOCs above and using first-order Taylor expansions yields:

$$\Delta s \approx \hat{\pi} \cdot \frac{\frac{\delta}{p}\left[e^{-\gamma(y-s_1^l)} - e^{-\gamma(y-s_1^l+x)}\right]/\gamma}{\gamma e^{-\gamma(y-s_0^0)} - \gamma\delta^T R^{2T} e^{-\gamma(w_T^0+s_0 R^T)}}. \tag{4}$$

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 strongly predicts 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.

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. Specifcially,

$$\frac{\partial ln(\frac{\partial \Pr(s_0(m)>0)}{\partial m})}{\partial m} \approx -\gamma \cdot (w_T'(m)+1) \tag{5}$$

The RHS 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 $\gamma = .0002$ per 1,000 COP, which translates to around $.0008$ per USD. 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 retirement T_i for each subject individually. We estimate $(\alpha, \hat{\pi})$ by solving

$$\min_{(\alpha,\hat{\pi})} \mathbf{E}[s_i - \alpha - \hat{\pi}\beta_i D_i] \tag{6}$$

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

5.1 *Estimation Results*

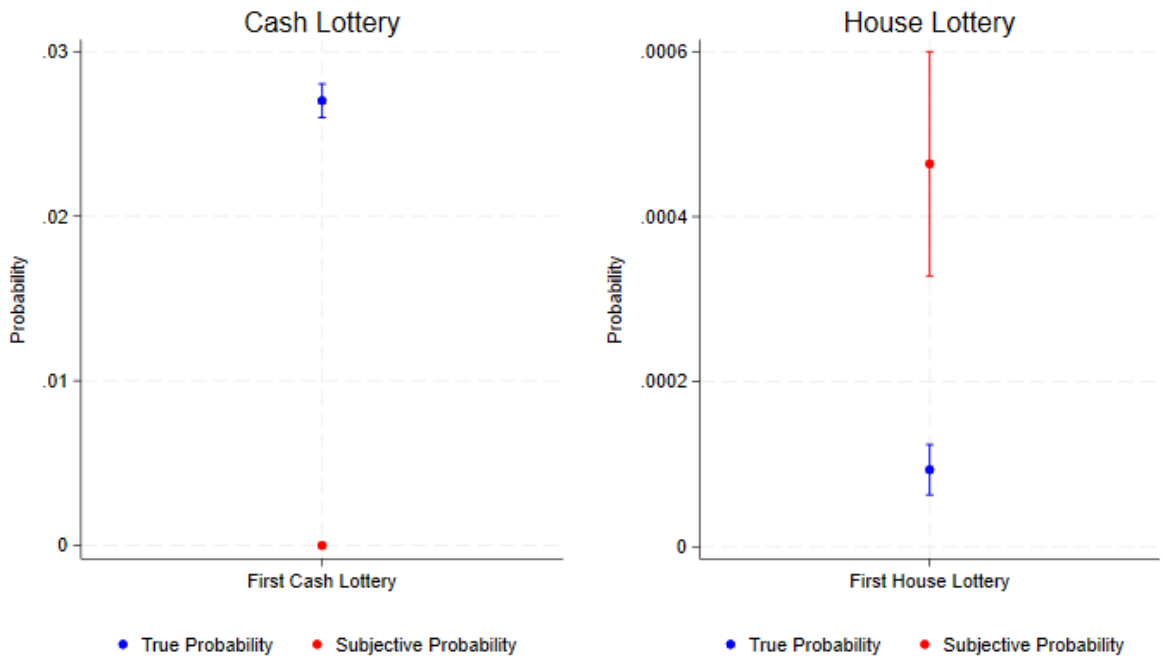
We estimate equations 6 for the first House lottery and the first Cash lottery, separately. Focusing on the first interaction with a lottery is the cleanest analysis because the savings response is not yet confounded by responses to prior lotteries. During follow-up lotteries, subjects might already 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 9 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

Figure 9: Subjective Versus True Winning Probabilities



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 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.

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 have small positive treatment effects on savings but cause a large number of important unintended consequences. Households compensate increased savings during the lottery period by reducing savings in future periods. Some households further lose access to disability and life insurance by focusing too much on the lottery promotion, forgetting about other financial incentives that are tied to regular savings. In addition, structural estimates suggest that savings responses are suboptimal as lottery prizes distort subjective beliefs about the odds of winning.

In terms of long-run dynamics, we are able to rule out that lotteries established a permanent increase in savings up to 4 years post-treatment. We also do not observe that treated households are more inclined to respond to future lotteries, indicating that savings lotteries do not induce a gambling addiction.

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

While our results are not particularly favorable to lotteries as a policy, our conclusions are limited to the specific lotteries we tested. Given the scarcity of studies on lotteries, it is too early to discard this policy from the public finance toolbox. We encourage future research to explore additional dimensions that determine the success of lotteries in various areas of society.

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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.

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: Treatment Effects on Savings

	Qualification		Savings		Insurance
	(1) House	(2) Cash	(3) Levels	(4) Poisson	(5) (Percent)
Panel A: All participants					
House	0.567***	0.475***	1.628***	0.0221**	-0.242
	(0.0646)	(0.0950)	(0.533)	(0.0101)	(0.149)
Cash	0.266**	0.714***	0.625	0.00157	-0.757***
	(0.128)	(0.195)	(1.058)	(0.0209)	(0.292)
House + Cash	0.284**	1.113***	1.205	0.0127	-0.995***
	(0.127)	(0.197)	(1.068)	(0.0205)	(0.289)
Observations	221009	221009	221009	221009	221009
Mean dep. var.	3.735	8.214	69.54	132.9	26.42
Panel B: Active					
House	1.102***	0.895***	3.480***	0.0230**	-0.521*
	(0.124)	(0.178)	(1.000)	(0.00921)	(0.274)
Cash	0.551**	1.418***	1.703	0.00739	-1.473***
	(0.247)	(0.366)	(2.007)	(0.0189)	(0.539)
House + Cash	0.569**	1.886***	2.631	0.0151	-1.903***
	(0.245)	(0.365)	(2.020)	(0.0189)	(0.534)
Observations	109791	109791	109791	109791	109791
Mean dep. var.	7.187	15.46	132.9	132.9	50.82
Panel C: Inactive					
House	0.0459	0.0553	-0.153	-0.0134	-0.0344
	(0.0372)	(0.0657)	(0.377)	(0.0522)	(0.0972)
Cash	-0.0194	-0.0195	-0.733	-0.0888	-0.187
	(0.0690)	(0.129)	(0.663)	(0.105)	(0.185)
House + Cash	-0.00728	0.308**	-0.409	-0.0484	-0.297*
	(0.0673)	(0.141)	(0.672)	(0.101)	(0.180)
Observations	111218	111218	111218	111218	111218
Mean dep. var.	0.346	1.099	7.345	7.345	2.456

Notes: This table reports treatment effects on savings for different time intervals. Columns (2) and (4) include winsorized values of the outcome variable at the 95th percentile. 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 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:

$$Y_i = D_i Y_i(1) + (1 - D_i) Y_i(0), \tag{7}$$

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

The average treatment effect (ATE) is given by:

$$\tau = \mathbb{E}[Y_i(1) - Y_i(0)]. \tag{8}$$

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:

$$Y_i(0) = S_i Y_i(1) + (1 - S_i) Y_i(0^*), \tag{9}$$

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

Taking expectations, we obtain:

$$\mathbb{E}[Y_i(0)] = p_S \mathbb{E}[Y_i(1)] + (1 - p_S) \mathbb{E}[Y_i(0^*)], \tag{10}$$

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:

$$\hat{\tau} = \mathbb{E}[Y_i(1)] - \mathbb{E}[Y_i(0)]. \tag{11}$$

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

$$\hat{\tau} = \mathbb{E}[Y_i(1)] - \left(p_S \mathbb{E}[Y_i(1)] + (1 - p_S)\mathbb{E}[Y_i(0^*)]\right). \tag{12}$$

Rearranging terms:

$$\hat{\tau} = (1 - p_S)\left(\mathbb{E}[Y_i(1)] - \mathbb{E}[Y_i(0^*)]\right) = (1 - p_S)\tau. \tag{13}$$

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

$$\hat{\tau} \leq \tau. \tag{14}$$

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.