Getting to the Top of Mind: How Reminders Increase Saving

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ABSTRACT.   We provide evidence from field experiments with three different banks, that reminder messages increase commitment attainment for clients who recently opened commitment savings accounts. Messages that mention both savings goals and financial incentives are particularly effective, while other content variations such as gain versus loss framing do not have significantly different effects. Nor do we find evidence that receiving additional late reminders has an additive effect. These empirical results do not map neatly into existing models, so we provide a simple model where limited attention to exceptional expenses can generate under-saving that is in turn mitigated by reminders.

I. Introduction

Consumption, savings and borrowing behavior is sometimes difficult to reconcile with traditional models of intertemporal choice. Calibrations of U.S. data suggest that extremely high short-term discount rates are necessary to explain observed borrowing patterns (Laibson, Repetto and Tobacman 2007). Voluntary commitment devices help increase savings (Ashraf, Karlan and Yin 2006b; Benartzi and Thaler 2004). Default options have large effects on retirement savings decisions (Madrian and Shea 2001; Beshears et al 2008). In the developing world, there is evidence of persistent borrowing at high daily rates for predictable expenses (Ananth, Karlan and Mullainathan 2007) even though several studies have found that expanding access to savings accounts improves various outcomes, including income-generation (Dupas and Robinson 2013; Karlan, Ratan, and Zinman 2014). These patterns are often explained by models that emphasize time inconsistency and self-control problems (Laibson 1997; O'Donoghue and Rabin 1999; Fudenberg and Levine 2006; Banerjee and Mullainathan 2009). In such models, people can exhibit both impatience and patience, depending on the horizon or good of choice.

We provide evidence, from three field experiments and a simple theoretical model, suggesting that a different consumer psychology – limited attention – plays an important role in saving behavior. This approach is similar to Akerlof (1991), which emphasizes salience rather than costly self-control as a driver of procrastination, and to Bordalo, Gennaioli, and Shleifer (2013), which emphasizes salience as a driver of consumer choice.

Our experiments suggest that monthly reminders, sent by three different banks in Bolivia, Peru, and the Philippines, help clients meet their savings goals, on average and pooling across sites, compared to a no-reminder control group. Cross-site differences in setting and non-randomized features motivate estimating site-specific treatment effects as well. These results do not rule out identical effects of getting reminders, and for the most part the point estimates are similar across sites, but the cross-site comparisons are statistically imprecise. These findings are novel empirical field evidence on the influence of reminders on savings at a particular bank, although we lack outcome data on the household to speak about aggregate household savings, or more holistic measures of financial condition.[1]

Our experiment also generates results on the effects of two reminder design elements that have received less scrutiny in prior work: timing and content. The content variations suggest that many

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[1]Kast et al 2012 test messages that encourage saving with feedback and peer pressure or information, on a sample of microcredit borrowers. Cadena and Schoar 2011 and Karlan, Morten, and Zinman 2014 test reminders for loan repayment. Stango and Zinman 2014, and Zwane et al 2011, find that survey content serves as reminders to avoid bank overdrafts and to take-up insurance products. This work builds on a large body of evidence from clinical trials that reminders improve patient behavior across a variety of domains from increasing exercise (Calzolari and Nardotto 2012) to quitting smoking (Free et al 2011) to using sunscreen (Armstrong et al 2009) to adhering with kidney transplant protocols (Miloh et al 2009). See van Dulmen et al (2007) and Krishna et al (2009) for reviews of evidence on the impacts of reminders on clinical adherence.

reminders are actually not effective (although we caution that our null results are imprecisely esti-
mated), and that the most effective ones are those that remind people of both financial incentives
and savings goals; it may be the case that a savings reminder is effective if (and only if) it brings
multiple motivations for saving to the top of mind. On the other hand, other aspects of reminder
content (e.g., loss or gain framing), and the timing variations we tested, do not have significant
effects.[2]

Although the full pattern of our empirical results suggests some role for imperfectly rational
attention in savings behavior, we do not think they are fully explained by any extant theory. Our
empirical results do not square easily with existing models of attention and salience, which focus on
various forms of rational and quasi-rational (behavioral) inattention to prices or product attributes.[3]

So we develop a simple theory where limited attention to something other than prices or product
attributes can distort intertemporal allocations. Our theory illustrates that reminder effects on saving
are consistent with consumers being relatively inattentive to future "exceptional"(infrequent, and
often relatively large) expenses (Sussman and Alter 2013), and that reminders can increase saving
by making these future expenses more salient: bringing them to "top of mind." Our theoretical
approach is complementary with the model of salience put forward by Bordalo, Gennaioli, and
Shleifer (2013).

However, our theory does not generate sharp predictions on our timing or content variations. Nor
does our theory account for potential important interactions between limited attention and limited
self-control, or between reminders and other aspects of heterogeneity across people or settings. Hence
our findings further motivate work on interactions between limited attention and limited self-control
(Taubinsky 2013; Ericson 2014), and on consideration sets, and the psychology of incentives (Alba
et al 1991; Kamenica 2012).

Our sample includes only clients who had recently opened a commitment or goal-based savings
accounts: clients either made a plan to save a "commitment amount" by a "commitment end-date," or
to making regular deposits of an amount they specified until a commitment end-date. In some cases,
clients explicitly disclosed the specific expenditure they were saving for to the bank. Plan adherence
was incentivized by commitment (illiquidity until goal amount reached with the Philippines bank)
and/or by a bonus (higher yield in Peru, higher yield and free life insurance in Bolivia, and higher
yield in the Philippines for a random subset).

This sample has an advantage and disadvantage. It helps by allowing us to construct messages
that plausibly *remind* the client about her intent to save for a goal, as opposed to providing new
information and/or persuasion. We caution that this distinction is not crystal clear, as message

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[2]O'Keefe and Jensen (2009) find differential effects of loss vs. gain framing on disease detection behaviors in their
meta-analysis.

[3]Models of rational inattention include Sims 1998 and 2003; Mankiw and Reis 2002; Ball, et al 2005; and Reis 2006.
Models of behavioral inattention and salience include Gabaix and Laibson 2006; Chetty, Looney, and Kroft (2009);
Koszegi and Szeidl 2013; Bordalo, Gennaioli, and Shleifer 2013.

content is difficult to cleanly categorize, whether in advertising or other forms of communication (Bagwell 2007; DellaVigna and Gentzkow 2010). Nevertheless, our sample does offer reassurance that messages such as "don't forget your deposit this month!"or "... reach your savings goal of [client's specific future expense]!"are relevant because everyone in the sample will have recently made a specific goal or plan about their savings. There is some disadvantage to generalizability because we don't know whether our results would hold for people without a clear savings plan and/or goal.

All told, our results provide a potential novel microfoundation for mental accounting (Thaler 1990): instead of, or in addition to, offering a weak counter to temptation, mental labels provide a strong association between today's saving(s) and specific future events, increasing the probability that individuals attend to those events when choosing consumption, and thereby improving smoothing. Our results also suggest that many pro-savings treatments can be reinterpreted as operating through attention instead of, or in addition to, through self-control with large transaction costs for undoing non-binding commitments; e.g., opt-out default (Choi et al 2004); prepaid fertilizer (Duflo, Kremer, and Robinson 2011) or deposit collection (Ashraf, Karlan and Yin 2006a). Indeed, our model generates undersaving without any role for (time-varying) impatience or commitment. Our model also suggests that overborrowing may occur in part because debt can be "salience-advantaged"relative to saving; e.g., when debt is available "on-demand,"at the moment when an exceptional spending opportunity arises (unexpectedly, due to limited attention) and is (momentarily) at the "top of mind."

The paper proceeds as follows. Section II details the settings and design of our field experiments. Section III presents the results. Section IV details a simple theoretical model that helps interpret our empirical results. Section V concludes.

II. Experimental Design

Here we describe the setting, design, and implementation for three field experiments designed to test the hypothesis that limited attention plays a role in under-saving. The three experiments were implemented by three different banks in three different country "sites:" Bolivia, Peru, and the Philippines. The sample for each experiment is comprised of new commitment savings account holders. What is meant by "commitment savings account" differs in each of the three sites, and will be detailed below. After opening the account, the bank randomly assigned reminder treatments: reminder or not, and then content and timing within the reminder group. Banks did not mention or advertise reminders prior to random assignment, nor did they create random assignments for non-takers, and hence we conduct our analysis on account-openers only. In two of three sites - Bolivia and Philippines - the reminders were not even announced at account-opening; the bank just started sending them at no charge to clients. Each bank also had its personnel collect some "baseline"data prior to making the product offer.

Products, marketing, and some reminder features varied across sites, as detailed below and

summarized in Tables 1 and 2. This variation motivates our analysis of site-specific as well as pooled treatment effects. Table 1 provides a summary of account features and non-randomized reminder features across the three country settings. Tables 2a-2c detail the randomized reminder features - content and timing - and resulting cells and cell sizes.

Experiment 1: First Valley Bank, Western Mindanao, Philippines

First Valley Bank (FVB), a for-profit bank operating in Western Mindanao, Philippines, worked with us to randomize reminders as part of the rollout of its new Gihandom (Dream) Savings product.[4] Between April and August 2007, bank marketing employees conducted door-to-door marketing visits in rural and small urban areas and offered 10,056 individuals the opportunity to open a Gihandom account. As part of this marketing visit, the bank employee also conducted a brief five to ten minute survey. Bank staff used personal digital accessories (PDAs) for the baseline survey and random assignment to treatments. Of the 10,056 offers, 2,314 (23%) opened an account.

Gihandom allows a client to set her own commitment amount (US$50 or above) and commitment end-date (from three months to two years after opening).[5] Except in hardship cases, clients can then access funds only once both commitments - amount and date - have been met.[6] Once the client opens the account with a minimum deposit of US$2.50, there is no fixed deposit schedule to fulfill. The client receives a savings lockbox and is encouraged at sign-up to make small deposits on a daily basis. When the client desires, the client goes to the bank to deposit the money in the lockbox (e.g., when it is full). Clients also can, and do, go to the bank and make normal deposit, without the lockbox. We do not have data on whether they client had the lockbox with them for the deposit.

Among clients with a cell phone (66% of those who opened accounts), the bank randomly and independently assigned some clients to receive "regular "and/or "late "text message reminders to come to the bank to make a deposit each month.[7] The bank sent the late reminder only if the client did not make any deposit in a given month. Reminders were further randomized to gain or loss frame language with respect to "making your dream come true."A client assigned to receive both regular and late reminders got the same frame on all messages.

Table 2a shows the different reminder scripts and cell sizes. Table 3 Panel A shows balance checks on the randomizations.

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[4]The bank also randomly assigned offers to a) saving yield (1.5% APY, its normal rate; 3.0%; or 1.5%, + 1.5% reward for meeting commitment; b) whether clients were given offers for an individual account only, a joint account only, or the choice of individual or joint account. None of these variations significantly affected takeup or savings balances (Karlan and Zinman 2014). Nevertheless we control for these variations when estimating reminder effects.

[5]Clients assigned to the reward interest who meet their commitment (i.e., have at least their committed amount in the account as of their end-date) get the interest applied retroactively.

[6]Only about 1% of accounts do hardship withdrawals. The Gihandom account offers a stricter commitment than the one in Ashraf, Karlan and Yin (2006b), where funds can be withdrawn after *either* the commitment amount or end-date are reached.

[7]A fourth group was randomly assigned to deposit collection service. The deposit collection was not widely used, and thus the bank stopped providing the service. We include controls for individuals who were originally assigned to receive deposit collection.

Experiment 2: Bank of Ica, Ica, Peru

In Peru, the Government-owned bank Caja de Ica worked with us to randomize reminders as part of the rollout of its new product Plan Ahorro ("Saving Plan"). The bank marketed the product on television and radio in the Ica metropolitan area (urban and rural), and clients signed up over the course of several months . When opening an account, Plan Ahorro clients selected a commitment end-date (between 6 months and 12 months post-opening), a minimum commitment amount to deposit each month, and a goal (specific expenditure) label from 14 pre-established categories. A table of these savings goals is shown in Appendix Table 1; note that the most generic goal ("Emergency") is far-and-away the most common one. Clients were required to make each planned deposit within ten days of each monthly due date in order to meet their commitment. Commitment compliance was rewarded with an annualized interest rate of 8% per annum rather than the normal 4% per annum.

As at our other sites, the bank randomly assigned reminders to clients after they signed up for the product. The bank sent letters because low cell phone prevalence made text messages impractical. As in the Philippines, the bank did independent randomizations for regular and/or late reminders that were assigned to the same gain or loss frame. The bank sent regular reminders with a target client-receipt date seven days before the due date for that month's scheduled deposit. The bank also randomly assigned regular-reminder clients to have their reminders signed by either the bank, or the client herself, with her signature recorded at account-opening. As in the Philippines, the bank sent a late reminder only if the client was late (i.e., if they had not made a deposit three days after their scheduled deposit date).[8] All late-reminder letters were signed by the bank.

Table 2c shows the different reminder scripts and cell sizes. Table 3 Panel A shows balance checks on the randomizations.

The bank implemented two additional treatments designed to increase the salience of the client's specific expenditure goal. One treatment randomly assigned some in the reminder group to get a letter that focused on their particular goal (in addition to containing the boilerplate reminder content, see Table 2b). Another treatment independently and randomly assigned the gift clients received upon opening the account: a jigsaw puzzle of their goal, a photo of their goal, or a pen. Those in the jigsaw puzzle group received a piece of the puzzle after each deposit.[9]

Experiment 3: Ecofuturo Bank, Bolivia

Ecofuturo, a for-profit bank in Bolivia, worked with us to implement a text message reminder program for its established product Ecoaguinaldo. "Aguinaldo" is the year-end bonus, equal to one month's pay, that employers are required to pay salaried employees in Bolivia. Ecofuturo markets

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[8]Clients assigned to receive late reminders were randomly assigned to receive their late reminder if (a) they were late for any scheduled deposit, (b) they were late for any of the first four scheduled deposits, and c) they were late for any of the fifth or later scheduled deposits. These treatments had imprecisely estimated and statistically insignificant effects on savings.

[9]Most goal pictures are self-explanatory. Individual saving for an "emergency,"got a picture of puzzle of a hospital emergency room. Individuals saving for "other,"got a picture of puzzle of the "Plan Ahorro"savings account logo.

Ecoaguinaldo as a product designed to help its clients, many of whom are self-employed, save up all year for their own year-end payout. The product is marketed for three months between January and March on television and radio in urban areas of Bolivia close to Ecofuturo's branches. Clients who opened an account during January-March 2008 were brought into the study and eligible to make savings deposits until the December 2008 commitment end-date.

At sign-up, clients chose a monthly minimum deposit amount (with a floor of US$1.41). Clients making all of their committed monthly deposits received a bonus interest rate of 6% for their first ten months following enrollment in the program (compared to a regular interest rate of 3%) as well as free life and accident insurance.[10] Clients missing one deposit, or withdrawing money before the payout date, forfeited the higher interest rate and had their insurance policies canceled.

As in the Philippines, clients with a cell phone were randomly assigned to receive text message reminders or not. In Bolivia the experimental design called for everyone assigned to get a reminder to get one every month, in advance of their scheduled deposit; i.e., all reminders were supposed to be "regular," and unlike the other two sites the bank did not randomly assign variation in regular versus late reminders. But the bank deviated from this design in two ways. First, it did not start sending reminders until several months after account opening (which took place January-March 2008), beginning in May.[11] Second, the bank switched from a "regular" to "late" reminder rule starting in August: from August-November, clients were only sent a reminder if they had not yet made a deposit that month. As a consequence most clients received fewer reminders than prescribed by the design. (We use the random assignments in our analysis, rather than actual treatment status, to avoid bias from the endogeneity of not yet having made a deposit.) Interestingly, the cross-country comparisons in Table 4 suggest that lower-frequency messaging does not seem to reduce the potency of reminders. This suggests that it is the initial reminders that matter (consistent with "tuning out" or habituation over time), although we lack the random variation required to test that hypothesis.

Every reminder mentioned the savings goal, broadly speaking: "Aguinaldo," the year-end bonus (note that this goal is somewhat more generic than at the Peru site, where many clients indicated a more specific future goal like school fees). Besides the boilerplate script content was randomized 2x2: (gain or loss frame) x (mention insurance incentive or not). The latter treatment parallels the Peru design in the sense that it produces some reminders that mention both a financial incentive and a savings goal (in this case, the "Aguinaldo" or year-end bonus).

Table 2c shows the different reminder scripts and cell sizes. Table 3 Panel A shows balance checks on the randomizations.

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[10]Ecofuturo paid the monthly premiums of $0.13 and $0.32 on policies that paid $214 in the event of death and $285 in the event of debilitating injury.

[11]In May the bank began sending reminders to a random subset of those assigned to the reminder group. In June and July the bank sent reminders to everyone in the reminder group.

III. Results

OVERALL EFFECT OF REMINDERS ON SAVING

The first test of our attention treatment uses data from all three experiments to identify the effect of getting any reminder.

$$Y_i = \alpha + \beta R_i + \gamma Z_i + \varepsilon_i \tag{1}$$

We measure Y, savings by client i, in two different ways (Table 3 Panel C contains summary statistics on these two variables). One is an indicator for "Met Commitment:" whether the client complied with the features of her commitment savings account.[12] We define this based on the account terms at each site: making each regularly scheduled deposit in Bolivia, making each deposit within 10 days of scheduled deposit date in Peru, and reaching the commitment amount by the commitment end-date in the Philippines. The binary nature of Met Commitment yields precise estimates of treatment effects, in contrast with measures of savings balances, which are highly variable and skewed. Note however that since Met Commitment has a timing element, a saver could still save substantial amounts even when failing to meet her commitment, by missing at least one scheduled deposit or making a late deposit. The share of individuals who exceed their savings commitments is 3% in Peru, 20% in the Philippines and 43% in Bolivia. We show estimated treatment effects on Met Commitment in Columns 3 and 4 of Tables 4-6.

Our second measure of savings is the log of (1+Amount Saved) in the commitment account, summing all deposits made during the commitment period.[13] This measure yields less precise estimates of treatment effects due to its high variance; the skewness of savings balances also creates a functional form issue. Figure 1 provides a boxplot of the level Amount Saved (in USD) for the reminder and no-reminder groups with and without outliers. We show estimated treatment effects on log(1+Amount Saved) in Columns 1 and 2 of Tables 4-6. In Appendix Table 2 we estimate alternative functional form specifications. For completeness, we include an estimation of the reminders' effect on the level of quantity saved, which is highly subject to the outliers that can be seen in Figure 1. We also estimate an alternative log specification as the log of (0.01+ Amount Saved) and a specification using the inverse hyperbolic sine of the level of quantity saved as suggested by (Burbidge et al 1988). Our results are similar for both log specifications and for the inverse hyperbolic sine specification.

R is an indicator that equals 1 if the bank randomly assigned the client to receive any reminder, with no-reminder the omitted category. Z is a vector of randomization conditions, other treatment assignments, and country fixed effects. We also show robustness to including X, a vector of the client's baseline characteristics. We report OLS estimates in the main tables; the results are robust

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[12]In each of the three countries, a small subset of individuals (457 total, with 69 in Peru, 276 in Bolivia and 112 in the Philippines) opened multiple savings accounts. These individuals were incorrectly assigned to different reminder treatments for each account. Because these individuals are more likely to have been assigned to receive a reminder for at least one of their accounts, we drop these observations.

[13]2,560 clients had made no deposits by the end of the study and therefore had zero balances.

to using probit for Met Commitment (results available upon request).

Table 4 Panel A presents OLS estimates of equation (1). In this pooled sample, clients assigned to receive a monthly reminder are 3.2 percentage points more likely to meet their commitment (SE = 0.9pp). This represents a 5.4% increase over the control group likelihood of 0.55. As expected, the results on amount saved are less precise. We estimate that log(1+Amount Saved) is 5.9 or 6.1 percent higher in the reminder group (SE=0.037 in both specifications), with p-values of 0.11 and 0.10.

The potential for cross-site heterogeneity - in banks, clients, products, and/or reminder features - motivates estimation of site-specific treatment effects. We do this by estimating equation (1) separately for each of the three sites. The drawback of this strategy is reduced sample size and power.

Table 4 Panel B presents the site-specific OLS estimates. Comparing results of the average effect of reminders across countries, we note three results. First, the only statistically significant effect comes from Bolivia, where the sample size is the largest. Second, the lack of statistical significance seems, to be due to low power rather than evident differences across sites: each of the 12 point estimates are positive, and within-outcome the point estimates look fairly similar. Third, and in keeping with the eyeball test, although the Bolivia results are statistically significant on their own, they are not statistically significantly different than the other sites. P-values on the cross-site comparisons range from 0.11 to 0.33 for Met Commitment, and from 0.24 to 0.74 for log(1+Amount Saved). Note however that these comparisons are imprecisely estimated, particularly for Amount Saved, due to large standard errors on the individual point estimates.

CONTENT VARIATION

Table 5 presents results from tests of our three key content variations. We again estimate the following specifications with and without controls for baseline characteristics. Panel A shows OLS results from pooled across all three sites and estimating:

$$Y_i = \alpha + \beta_1 G_i + \beta_2 L_i + \gamma Z_i + \varepsilon_i \tag{2}$$

where G_i indicates that the individual was assigned to receive gain-framed reminders and L_i indicates loss-framed. No-reminder is again the omitted category. The four point estimates are uniformly higher for loss-framed, but none of the tests for difference between gain- vs. loss-frame is statistically significant (the p-values range from 0.18 to 0.45).

Panel B shows OLS estimates from estimating, on the Peru sample:

$$Y_i = \alpha + \beta_1 IC_i + \beta_2 SE_i + \gamma Z_i + \varepsilon_i \tag{3}$$

where IC_i indicates that the individual was assigned reminders that mentioned the interest rate

incentive only, and SE_i indicates reminders that mentioned the specific expenditure goal as well (see Table 2b for scripts). No-reminder is again the omitted category. We find no differential effects on Met Commitment: the p-value on the estimated difference between Incentive vs. Incentive+Goal is 0.42 or 0.43, depending on specification. But the results on log(1+Amount Saved) suggest that the Incentive+Goal reminder was more effective, increasing savings by 10 to 11 percent relative to both the control group and the Incentive reminder group, with p-values indicating high confidence that both differences are statistically significant. Why do we find effects on Amount Saved but not Met Commitment? In Bolivia and Peru individuals committed to making minimum monthly deposits by specific dates and in the Philippines individuals committed to restricting access to their funds until a specific date and deposit amount were reached. Therefore meeting the commitment is only possible if and if all monthly deposits were made. This makes it possible to save a large amount but not "meet the commitment," for example by missing one deposit.

Another possibility is simply imprecision: e.g., the point estimates for Met Commitment on the incentive+goal reminder are positive, comparable in magnitude in percentage terms to their point estimates on log(1+Amount Saved), and nearly statistically significant (p-values of 0.17 and 0.21).[14]

Panel C estimates the (roughly) analogous regression on the Bolivia sample:

$$Y_i = \alpha + \beta_1 GL_i + \beta_2 IC_i + \gamma Z_i + \varepsilon_i \tag{4}$$

where GL_i indicates that the individual was assigned to reminders that mentioned only the savings goal (Aguinaldo, the year-end bonus), and IC_i indicates reminders that mention the life insurance incentive as well (see Table 2c for scripts). No-reminder is the omitted category per usual. As in Peru, we find some evidence - stronger in Bolivia - that messages mentioning both a savings goal and financial incentive are more effective. Here we do see effects on Met Commitment, with the Goal+Incentive reminder inducing several percentage point higher success rates, at 99% confidence levels, than either the control group or Goal reminder. We also estimate 10 or 11% higher savings amounts in the Goal+Incentive group, with p-values of 0.04 and 0.03 when comparing to the control group, and 0.07 and 0.10 when comparing to the Goal reminder group.

We stop short of inferring that only reminders mentioning both a goal and incentive are effective at changing saving behavior, as the confidence intervals on our statistical null results are imprecise. But the evidence from Peru and Bolivia does allow us to reasonably infer that reminders mentioning

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[14]While there might be a concern that the combination of saving large amounts but not meeting commitment would indicate sub-optimal over-saving, we do not think this is the case. Individuals may have set strategically low monthly savings totals to ensure that they would be able to save the minimum amount each month. Despite setting a small minimum monthly deposit amounts, many individuals may have hoped to be able to save more than that minimum. Because we do not have data on individuals' ideal savings goals independent of the targets chosen for the commitment product, we cannot determine whether individuals in our sample are "over-saving." We see some suggestive evidence for strategic selection of goal amounts: 49% of individuals in Bolivia saving more than their goal had chosen the minimum monthly deposit amount.

both are effective relative to the control group, and relatively effective compared to mentioning only an incentive or a goal.

Timing Variation

Table 6 considers the timing (and conditionality) variations. Panel A estimates whether assignment to a late reminder has any marginal effect on savings behavior above and beyond getting a regular reminder (no-reminder is the omitted category per usual). We find no evidence that it does, although the null results are typically imprecisely estimated, especially for treatment effects on log(1+Amount Saved). Part of the imprecision is due to the lack of random variation in regular vs. late in Bolivia, leading us to restrict this sample to Peru and Philippines.

Panel B considers the gift treatment in Peru, which also has a timing/conditionality element. Recall that pens and goal photos were gifted at account-opening, while goal puzzle were gifted piecemeal, after each deposit. Here the pen gift is the omitted category. Neither of the goal-focused gifts changed saving behavior relative to the pen, although per usual our null are imprecisely estimated, especially when estimating treatment effects on log(1+ Amount Saved). Nor do we find any evidence of differential effects from the photo or puzzle, with p-values ranging from 0.55 to 0.77, although again we emphasize that these differences are imprecisely estimated (albeit less so for the Met Commitment treatment effect estimates).

It strikes us a noteworthy that, as with the reminder content tests in Table 5, simply making the savings goal salient with a puzzle or photo does not seem to be sufficient to change savings behavior. Perhaps puzzles or photos that depicted both the goal and the financial incentive would have been more effective, as seems to be the case with the reminder messages in Table 5.

We also note that the tests in Table 6 do not exhaust all timing or conditionality variations worth testing. For example, Ericson (2014) suggests that regular reminders may vary in effectiveness depending on how long they arrive before the task deadline (the task being making a deposit in our case).

Alternative/Complementary Explanations

Perhaps reminders impact saving because they are a signal from the bank that saving is important, that the bank values the client's relationship, and or that the bank is trustworthy? While the signaling interpretation is clearly consistent with the average effect of the messages (Table 4 Panel A), it is only consistent with the differential impact of messages that mention both financial incentives and goal if these messages are relatively effective signaling devices. Nor does the signaling explanation readily explain why certain messages are more effective than gifts like the photo, puzzle, or pen. Peru also has some random variation in who wrote the regularly-timed reminders - clients, at the time they signed up for the account; or the bank - and we find no evidence that bank-written reminders are more effective.[15]

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[15]Results available upon request. In fact the point estimates on client-written are uniformly larger than the point estimates on bank-written (no regular reminder is the omitted category), although our estimates of the differences between the two are imprecise due to the small sample limited to clients from Peru and dropping those assigned to the late reminder.

Another explanation for the effectiveness of reminders would be a two-part argument: individuals do not have consistently time-inconsistent preferences as traditionally modeled (Laibson 1997; O'Donoghue and Rabin 1999), but are rather stochastically quasi-hyperbolic, with reminders reducing present-bias in a given time period. This mechanism is not so different from our model below, where the reminder operates on expectations rather than preferences. Empirically, we do not see any evidence that reminders are more effective when individuals make time-inconsistent hypothetical choices in a baseline survey, although the time-inconsistency measure is only available in the Philippines and the estimates are very imprecise (results available upon request).

Lastly, Soman and Zhao (2011) argue that savings messages are more effective when they trigger an implementation mindset instead of a deliberative mindset. It could be that reminders of both goals + financial incentives are relatively effective at spurring individuals to action. We note however that every reminder message tested in this study is action-oriented (along the lines of: "make your deposit"). So we speculate that differential effectiveness is more likely due to some messages making (more of) the benefits of saving more salient than other messages.

Cost Effectiveness of Reminders

The variable cost of sending direct mail reminders is nontrivial (almost a dollar in the Peru context). Given our estimated treatment effect (a 6% increase in bank balances) and the small average balances ($100 or less), mailing reminders is not cost-effective for banks under reasonable assumptions about rates of return on deposited funds. Indeed the one bank here that experimented with mailing reminders discontinued them after the study.

However, sending reminders by text message has near zero marginal cost. And indeed Ecofuturo in Bolivia has continued sending the reminders. Direct mail costs and the recent emergence of low-cost text messaging may help explain why most banks have not (yet) offered reminders to save, although casual observation suggests that banks are adopting this technology with a steep upward trend.

IV. Model

In order to provide intuition for how reminders might generate increases in savings, we consider a model of limited attention in the context of lifetime consumption. Our purpose is merely to illustrate how limited attention to certain types of future expenses can distort intertemporal choices, and how reminders can mitigate any distortion. The theory does not generate any testable predictions regarding reminder design elements; i.e., it is silent on *which* reminders will be (most) effective. Our theory focuses on the effects of limited attention to what Sussman and Alter (2013) label ("exceptional ") expenses. These expenses are exceptional in their relative infrequency and large size, but not necessarily in their stochasticity: exceptional expenses can be, in principle at least, perfectly forecastable. A common example for the subjects in our field experiments would be school fees; a common example for the U.S. would be car registration fees. In our model individuals face two

kinds of consumption opportunities each period: an exceptional expense which occurs with certainty but can differ in each period (it could be school fees in one period, a night out in the next, etc.), and ("ordinary ") consumption. We make two key assumptions: 1) ordinary consumption is "top of mind:" there are no foresight problems;[16] 2) exceptional expenses are not top of mind: individuals fail to anticipate some of these future expenditure needs/opportunities, and underestimate how much they will end up spending on them. This assumption is related to research in psychology that illustrates how many individuals are subject to a planning fallacy, where they tend to systematically underestimate how long it will take to complete certain tasks (Buehler et al 2012). Sussman and Alter (2013) present survey evidence in support of both of these assumptions (see also Ulkumen et al 2008). Our consumer chooses consumption to maximize her lifetime utility given the future expenditure opportunities she "attends "to, i.e., that she does not, prospectively, forget.[17]

A simple example illustrates our model. Suppose you hear that your favorite singer is coming to town three months from now, with (near-)certainty. This is an exceptional spending opportunity. You decide to go, and mark the concert date in your calendar. Tickets will not go on sale until the night of the show. Ideally, if your utility is concave with respect to consumption, you would finance the ticket by smoothing the expenditure shock over your lifetime – including some saving over the next three months. But if you are inattentive as in our model, you may sometimes forget your concert plan and choose to consume instead of save. Then, when the day of the concert arrives, you face the inferior options of reducing current consumption, forgoing the concert, or financing it disproportionately through debt (e.g., by charging it to a credit card or incurring a checking account overdraft fee and thus reducing future consumption even more in order to cover the financing costs). This sort of attentional failure may or may not be consequential in isolation. But lifetime consumption allocations are the result of countless such decisions, and small distortions can add up (and compound).

Formally, we model individual consumption over a finite horizon period with $3 \leq T < \infty$. In each period, individuals receive constant income y. We assume for simplicity that individuals do not discount the future so that the discount rate $\delta = 1$. As such we do not model potential interactions between inattention and present-bias, as this is a major contribution in its own right (Ericson 2014).

In our simple model, individuals derive utility from ordinary consumption and from exceptional expenditure opportunities. The utility from ordinary consumption is represented by the function

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[16]Our model also requires that individuals are more likely to forget future expenditures than future income, and we assume perfect forecasting of income. Prior work on income forecasting has theorized that anticipatory utility might lead consumers to *over*estimate their income (Brunnermeier, Papakonstantinou, and Parker 2008); we are not aware of any theory or evidence on the *under*estimation of income.

[17]Our setup is related to Mullainathan (2002), where individuals fail to remember information that predicts future income, and to Schwartzstein (forthcoming) where individuals may mistakenly only attend to information that they consider relevant for a prediction task (see Hanna et al 2013 for empirical evidence in support of this theory) . It is also closely related to Holman and Zaidi (2010), which focuses on prospective memory errors in the form of failing to remember to follow-through on tasks and Taubinsky (2013), which provides a psychologically-grounded model of how tasks get to the top of mind.

u which is increasing and concave in consumption. Individuals face one exceptional expenditure opportunity in every period. This opportunity can differ in composition across time (e.g., medical one period, car registration the next), but always has a unit cost of 1. So "forecasting" a lumpy expenditure opportunity is equivalent to "remembering" prospectively that one will arise (Ericson, 2011). Individuals who make the exceptional expenditure receive additive utility $\bar{u}$. The purchase decision is represented by the binary variable x_t, where $x_t = 1$ if individuals buy and $x_t = 0$ otherwise.

To maximize lifetime utility, individuals choose current period ordinary consumption c_t and whether to make the exceptional expenditure x_t.

$$\sum_{t=1}^{T} \left(u(c_t) + x_t \bar{u} \right)$$

subject to:

$$w_{t+1} = w_t + y - c_t - x_t \text{ for all } t$$
$$w_1 = 0 \text{ and } w_{T+1} = 0$$

where w_t represents the wealth at the start of a period. We assume that individuals start and end with zero wealth. Individuals may save or borrow, and borrowed money must be repaid by life's end. For simplicity, we assume that there is no interest charged on loans or earned on savings.

Full Attention Benchmark

Fully "attentive" individuals correctly forecast all future exceptional expenditure opportunities from the first period, and, given concavity with respect to ordinary consumption, will optimize by smoothing: $c_t = c^*$ in all periods. Backward induction provides the intuition that consumption in each period will be a function of the number of number of exceptional expenditures financed.

$$c = y - \left(\sum_{t=1}^{T} x_t \right) / T$$

We denote $k_t = \sum_{\tau=t+1}^{T} x_\tau$ as the total number of future exceptional expenditures to be financed after t. In the first period, individuals will optimally chose to satisfy $k_1^* + x_1^*$ lifetime expenditures such that:

$$u' \left(y - \frac{k_1^* + x_1^*}{T} \right) \leq \bar{u} < u' \left(y - \frac{(k_1^* + x_1^* + 1)}{T} \right)$$

That is, individuals will finance exceptional expenditures until they are indifferent between the utility from financing an additional one and the marginal utility from ordinary consumption (assuming

individuals are not constrained by their lifetime income constraint $k_1^* + x_1^* \leq Ty$). Perfectly attentive individuals will smooth their consumption by consuming $c_t = y - \frac{k_1^* + x_1^*}{T}$ in each period. For fully attentive individuals, consumption and savings plans will never deviate from the optimal plan made in the first period.

Inattention to Future Exceptional Expenditures

In practice, individuals may not be fully attentive to all exceptional expenditure opportunities (Sussman and Alter 2013). We assume that inattentive individuals attend perfectly to ordinary consumption and current-period lumpy expenditure opportunities, but only attend to future exceptional expenditure opportunities with some probability $\theta \in [0, 1)$.

An inattentive individual then chooses her current-period ordinary consumption c_t, and whether to make the current-period exceptional expense x_t, in order maximize her lifetime utility, as she perceives it in that time period (i.e., considering only those exceptional expense opportunities she takes into account in the current period).

$$u(c_t) + x_t \bar{u} + \sum_{\tau = t+1}^{T} \left(\theta \left[u(c_\tau) + x_\tau \bar{u} \right] + (1 - \theta) u(c_\tau) \right)$$

subject to:

$$
\begin{aligned}
w_{t+1} &= w_t + y - c_t - x_t \text{ for all } t \\
w_0 &= 0 \text{ and } w_{T+1} = 0
\end{aligned}
$$

We assume that individuals are unaware of their inattention to exceptional expenditures: people believe they are optimizing utility as they would in the rational model, but they actually underforecast the number of expenditure opportunities. While not all individuals will be fully naive about their inattention, our model provides a framework for considering those individuals who are either unaware of their inattention (Koehler, White, and John (2010) and Ericson (2011) find evidence of substantial naivete), overly optimistic about their ability to perfectly forecast, or otherwise unable to provide themselves with reminders as (cost-)effectively as a third-party could.

Inattentive individuals reoptimize their savings plan in every period depending on the realization of $\widetilde{K}_t$ which is a random variable representing the number of future exceptional spending opportunities an individual attends to in period t. $\widetilde{K}_t$ has expected value $E(\widetilde{K}_t) = \theta(T - t)$. Instead of saving the same fixed amount in each period, individuals will consider both their current period wealth and the set of future expenditures they attend to when choosing consumption. Because individuals may forecast a different number of future exceptional expenses in different periods, they must recalibrate their savings plans as they are faced with unanticipated exceptional spending opportunities. In doing

this, individuals will either forgo some exceptional expenditures (illustrated in example 1) or curtail consumption (illustrated in example 2), depending on how much utility individuals get from each exceptional expenditure ($\bar{u}$).

As with fully attentive individuals, concave utility implies that inattentive individuals will plan to smooth consumption so that $\widetilde{c}_t^* = \widetilde{c}_\tau^*$ for all t, τ. By backward induction, we can see that consumption for the inattentive individual will be:

$$\widetilde{c}_t = y + \left[\widetilde{w}_t - \widetilde{x}_t - \theta \left(\sum_{\tau=t+1}^{T} \widetilde{x}_\tau \right) \right] / (T - t + 1)$$

We denote the number of future exceptional expenses an individual plans to make in period t as: $\widetilde{k}_t = \theta \sum_{\tau=t+1}^{T} \widetilde{x}_\tau$. Individuals will choose whether to make the current expenditure ($\widetilde{x}_t^*$) and plan to make future expenditures ($\widetilde{k}_t^* = \theta \sum_{\tau=t+1}^{T} \widetilde{x}_\tau^*$):

$$u'\left(y + \frac{\widetilde{w}_t - (\widetilde{k}_t^* + \widetilde{x}_t^*)}{T - t + 1} \right) \leq \bar{u} < u'\left(y + \frac{\widetilde{w}_t - (\widetilde{k}_t^* + \widetilde{x}_t^* + 1)}{T - t + 1} \right)$$

Provided they are not constrained by their lifetime budget constraint in period t ($\widetilde{w}_t + \widetilde{k}_t^* + \widetilde{x}_t^* \leq [T - t + 1]y$), individuals will finance exceptional expenses until they are indifferent between the utility from financing an additional one and the marginal utility from ordinary consumption.[18]

SAVINGS AND INATTENTION

The inattentive consumer's savings in period t is:

$$\widetilde{s}_t^* = \frac{\widetilde{k}_t^* + \widetilde{x}_t^*}{T - t + 1} - \widetilde{x}_t^*$$

We can now compare expected savings for inattentive and attentive individuals. Individuals can only plan to purchase weakly less than the future exceptional spending opportunities they foresee: $\widetilde{k}_t^* \leq \widetilde{K}_t$. Recall that in our full attention benchmark, the consumer simply spends her income in each period, for a constant savings rate of zero. In contrast the inattentive consumer borrows in some period(s):

$$E(\widetilde{s}_t^*) = \frac{E(\widetilde{k}^*) + x_t^*}{T - t + 1} - x_t^* \leq \frac{[\theta(T - t) + 1]}{T - t + 1} - 1 < 0 = s_t^* \tag{5}$$

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[18]When individuals are budget constrained they will plan to finance fewer expenditures than k_t^* and the intuition of our main result remains the same. Although the model is agnostic about *when* expenditures will be financed, we make the assumption that as long as individuals still gain positive marginal utility from financing exceptional expenditures, they will finance the one in the current period first. That is, as long as $\widetilde{k}_t^* + \widetilde{x}_t^* \geq 1$, $\widetilde{x}_t^* = 1$.

So the inattentive consumer has a negative expected savings rate that is lower than the full-attention consumer's.

For simplicity, we have assumed that exceptional spending opportunities occur with certainty. Our model would easily generalize to allow for uncertain expenditures, provided that the subjective probability that an expenditure will appear in an individual's forecast of future expenditures is always lower than the objective probability that an expenditure will occur. However, if exceptional spending opportunities are sufficiently uncertain, it could be that individuals actually subjectively overforecast expenditures. Therefore, our model and predictions probably best apply to individuals saving for predictable expenses. Evidence suggests that individuals undersave and overborrow even when facing predictable expenditures (Ananth, Karlan, Mullainathan 2007). For example, Choi, Laibson, Madrian and Metrick (2004) present survey evidence suggesting that two-thirds of individuals feel that they undersave for the predictable expenditure of retirement.

EXAMPLE

Consider an example in three periods, $T = 3$. Suppose that fully attentive individuals prefer to finance all 3 exceptional expenditure opportunities. That is, suppose that

$$3u\left(y - 1\right) + 3\bar{u} > 3u\left(y - \frac{2}{3}\right) + 2\bar{u}$$

Now suppose that an inattentive consumer fails to attend to just one of the future exceptional spending opportunities. Because she (incorrectly) forecasts only 2 instead of 3, she borrows to finance part of the expenditure she faces in period 1. In period 2, when faced with an unplanned exceptional expense, she realizes her mistake. She then has two options: cut consumption to finance both remaining exceptional expenditures, or forgo one. She will finance an unexpected expenditure if and only if financing the expenditure affords greater utility than maintaining higher ordinary consumption. That is, she will finance the expenditure if and only if:

$$u\left(y - \frac{2}{3}\right) + 2u\left(y - \frac{7}{6}\right) + 3\bar{u} > 3u\left(y - \frac{2}{3}\right) + 2\bar{u}$$

Inattention in the first period ultimately reduces lifetime utility, whether through lower ordinary consumption or through making fewer total exceptional expenditures.

REMINDERS

In models with costly self-control, individuals decide to consume more in the current period while fully accounting for all future expenditures. Time-inconsistency arises from changing valuations, not from changing perceptions of the opportunity set. Hence reminders should have no impact on consumption decisions. In contrast, if time-inconsistency arises from under-forecasting future needs, reminders can increase savings.

We model reminders as an exogenous increase in the probability that individuals attend to future exceptional expenditure opportunities: $\theta_r > \theta_{nr}$. In turn reminders affect the expected number of future expenditure opportunities attended to in a current period:

$$E_{nr}(\widetilde{K}_t) = \theta_{nr}(T - t)$$
$$E_r(\widetilde{K}_t) = \theta_r(T - t)$$

with $E_r(\widetilde{K}_t) > E_{nr}(\widetilde{K}_t)$. As before, individuals only plan to make exceptional expenses they foresee, and the optimal number of planned expenditures in future periods will be less than or equal to the number of expenditures attended to: $\widetilde{k}_t^* \leq \widetilde{K}_t$. Therefore the expected number of exceptional expenses an individual plans to make is higher in the presence of reminders: $E_r(\widetilde{k}_t^*) > E_{nr}(\widetilde{k}_t^*)$. From equation (1) we can then see that expected savings will be increasing in θ, and hence higher after a reminder is introduced: $E_r(\widetilde{s}_t^*) > E_{nr}(\widetilde{s}_t^*)$.

We suspect that models of temptation would only make the prediction that reminders increase savings rates under the additional assumption that reminders affect preferences or the cost of self-control. Although this might be a natural assumption to make for reminders or other exogenous, Pavlovian stimuli for *current consumption* (Laibson 2001), there is little if any psychological or neurological evidence that such stimuli can change the marginal utility of *saving* (or future consumption).

V. Conclusion

We provide evidence that limited attention places a role in savings behavior.

Empirically, we test the effect of reminders on savings in field experiments with three banks in Peru, Bolivia and the Philippines. Our sample is comprised of people who have recently opened a commitment savings account and made a plan to save. We find evidence that getting reminders increase the likelihood of meeting one's commitment to save, and weaker but suggestive evidence that reminders increase savings amounts as well. We also test several reminder design elements, principally content and timing. We do not find evidence the early or late reminders are differentially effective. Nor do we find evidence that gain- or loss-framed content is differentially effective. We do find evidence that messages featuring both a savings goal (namely, a future expense) and a financial incentive are particularly effective. Our evidence is consistent with the hypothesis that these messages are the only ones that change behavior, although we caution against making this inference given that our null results are imprecisely estimated.

Theoretically, we show that a particular form of limited attention - to exceptional expenses a la Sussman and Alter (2013) - is sufficient to generate the result that reminders change savings behavior. A richer model would generate testable predictions about what sorts of reminders should work, when, and for whom. For example, it might be the case that a model that incorporates both

limited attention and time-inconsistent preferences (or some other source of present bias) would predict different or null effects of (certain) reminders, in which case our experimental design could provide a test of pure attention compared to models where attention interacts with preferences (see Ericson 2014). We think this is a promising and plausible direction for future research, in part because of recent progress in modeling other types of interactions between preference dynamics and behavioral factors (Acland and Levy 2013; Taubinsky 2013).

A closely related issue is measuring a broader set of outcomes that might be affected by interventions designed to correct limited attention. If limited attention takes particular forms, or if consumers have additional behavioral biases, then attention-getting treatments could have countervailing and even perverse effects. Reminders from one bank may crowd-out savings in other instruments, or even induce (expensive) borrowing to offset lost consumption, thereby reducing savings on net.

The interaction between different types of limited attention is another important line of inquiry. Our model focuses on inattention to future exceptional spending opportunities, while assuming that individuals attend perfectly to income. If individuals are also inattentive to income, this could mitigate the welfare losses in our model. Indeed some individuals may actually cultivate some inattention to their income as a way to save more. For example, individuals may set up automatic savings deposits, or take fewer tax exemptions than permitted, as a means of decreasing the amount of disposable income that is salient.

We also speculate that the frequency and source of attention shocks plays an important role. Attention may interact with habit formation; e.g., if reminders to save induce consumers to adopt pro-savings routines.[19] Conversely, some consumers may eventually "tune out" repeated reminders. These dynamics suggest that reminders or other attention shocks may be particularly effective when they focus on inducing a one-time change with "sticky" consequences (e.g., 401k enrollment, fertilizer prepayment, or automatic payment of annual car registration fees).

Ultimately, the welfare implications of limited attention depend not only on how a consumer responds to a given attention shock, but also on how shocks are generated (endogenously) and interact. For example, only one of the banks that implemented reminders in this study has continued reminding its clients to save. Might the market under-supply reminders, particularly if consumers are naive about their limited attention? Do lenders exploit attentional failures by providing financing "on-demand," that is tied to specific expenditures (as is common in durables financing), and comes with built-in reminders (e.g., required monthly payments)? Understanding the market for attention is critical.

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[19]Unfortunately we lack data that would useful for studying habits and other dynamics; e.g., high-frequency data on savings behavior during the experiment, and savings post-experiment.

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Table 1. Account and Non-Randomized Reminder Features

Site	New or existing product offered by bank?	Bank	Marketing	Commitment Period	Commitment Amount*Frequency	Additional goal elicitation at baseline?	Commitment reward***	Commitment penalty	Frequency of reminders	Timing and content of reminders	Reminders delivered by	Reminders announced at account opening?***
Philippines	New	First Valley, for-profit MFI	Door-to-door in rural and small urban areas	Chosen by accountholder: between 3 months and 24 months	Chosen by accountholder: total amount to save by end of period	None	Preferential interest rate in randomly assigned cases	Withdrawal prohibited	Monthly, starting in the first month after account opening	See Table 2a	SMS	No
Peru	New	Caja de Ica, government-owned	TV and radio in the Ica region (urban and rural)	Chosen by accountholder: monthly term between 6 months and 12 months	Chosen by accountholder: minimum deposit to make each month	Specific expenditure	Preferential interest rate	None	Monthly, starting in the first month after account opening	See Table 2b	Letter	Yes
Bolivia	Existing	Ecofuturo, for-profit MFI	TV and radio in urban markets	All commitments end in December, so varies with time of takeup	Chosen by accountholder: minimum deposit to make each month	All accounts labeled with the goal of "Aguinaldo" (a year-end bonus of one month's pay)	Preferential interest rate (for 10 months) and free insurance	Interest differential forfeited, insurance cancelled	Monthly, starting several months after account opening*	See Table 2c	SMS	No

* Study-eligible accounts were opened Jan-March 2008, with all commitment periods ending in December 2008. Bank started sending reminders to a random subset of those assigned to get a reminder in May, and then to the rest of the reminder group starting in June. In June and July, everyone assigned to get a reminder was sent one in advance of their scheduled deposit date. From August-November only those who had not yet made a deposit were sent a reminder.

** "At account opening" = post-marketing and post-signup, but before the first reminder sent.

*** Philippines: bank randomly assigned offers to: 1.5% APY, its normal rate; 3.0%; or 1.5% + 1.5% reward for meeting commitment. These variations did not affect takeup or savings balances; nevertheless we control for them when estimating reminder effects. Peru: 8% APY vs. 4% APY, applied retroactively. Bolivia: 6% APY vs. 3% APY, + insurance monthly premiums of $0.13 and $0.32 for coverage of $214 in the event of death and $285 in the event of debilitating injury.

Table 2a. Treatment Summary for the Phillipines

Timing	Goal mentioned?	Frame	Sample assigned	Full Message
Regular only	"your dream"	Gain	163	Frequent deposit into the Gihandom Savings account will make your dream come true. A reminder from 1st Valley Bank.
		Loss	187	If you don't frequently deposit into your Gihandom Savings account your dream will not come true. A reminder from 1st Valley Bank.
Late and Regular	"your savings goal", "your dream"	Gain	397	You didn't deposit in the 1st Valley Gihandom account for 30 days. Don't forget to deposit, so you can reach your savings goal, make your dream come true!
		Loss	410	You didn't deposit in the 1st Valley Gihandom account for 30 days. If you forget to deposit, you cannot reach your savings goal and make your dream come true!

Table 2b. Treatment Summary for Peru

Timing	Timing portion of message	Financial incentive mentioned?	Goal mentioned?	Frame	Sample assigned	Full Message
Regular Only	We would like to remind you that your next Plan Ahorro deposit should be made on [Date].	Yes	No	Gain	94	*Regular* + If you make all of your deposits you will receive a total of [Amount] in additional interest rate incentive!
			No	Loss	93	*Regular* + If you miss a payment you will lose a total of [Amount] in additional interest rate incentive!
			Yes, client's specific expenditure goal	Gain	80	*Regular* + If you make all of your deposits you will receive a total of [Amount] in additional interest rate incentive that you will be able to use toward your savings goal of [Goal]!
			Yes, client's specific expenditure goal	Loss	75	*Regular* + If you miss a payment you will lose a total of [Amount] in additional interest rate incentive that you will be able to use toward your savings goal of [Goal]!
Regular and Late	We would like to remind you that your Plan Ahorro deposit should have been made on [Date]. If you would like to continue with Plan Ahorro please make your deposit as soon as possible.	Yes	No	Gain	827	*Late* + If you make all of your deposits you will receive a total of [Amount] in additional interest rate incentive!
			No	Loss	752	*Late* + If you miss a payment you will lose a total of [Amount] in additional interest rate incentive!
			Yes, client's specific expenditure goal	Gain	255	*Late* + If you make all of your deposits you will receive a total of [Amount] in additional interest rate incentive that you will be able to use toward your savings goal of [Goal]!
			Yes, client's specific expenditure goal	Loss	260	*Late* + If you miss a payment you will lose a total of [Amount] in additional interest rate incentive that you will be able to use toward your savings goal of [Goal]!

Table 2c. Treatment Summary for Bolivia

Timing	Financial incentive mentioned?	Goal mentioned?	Frame	Sample assigned	Full Message
Regular May-July, then late August-November*	Yes	"(Eco)aguinaldo" = one-month salary bonus	Gain	1173	Ecofuturo reminds you: Maintain your Ecoaguinaldo life insurance! Don't forget your deposit this month! You will keep your insurance by making all of your deposits on time.
			Loss	1173	Ecofuturo reminds you: Maintain your Ecoaguinaldo life insurance! Don't forget your deposit this month! You will lose your insurance if you don't make all of your deposits on time.
	No		Gain	1177	Ecofuturo reminds you: Your Ecoaguinaldo is within reach! Don't forget your deposit this month! You will be one step closer to your savings goal.
			Loss	1177	Ecofuturo reminds you: Don't fail to reach your Ecoaguinaldo! Don't forget your deposit this month! If you don't make your deposit you increase the chance of not reaching your savings goal.

Study-eligible accounts were opened Jan-March 2008, with all commitment periods ending in December 2008. Bank started sending reminders to a random subset of those assigned to get a reminder in May, and then to the rest of the reminder group starting in June. In June and July, everyone assigned to get a reminder was sent one in advance of their scheduled deposit date. From August-November only those who had not yet made a deposit were sent a reminder.

Table 3: Descriptive Statistics and Orthogonality Verification
Mean, Standard Deviation

	Pooled Sample			Peru			Bolivia			Philippines		
	Mean	Obs	P-value from F-Test from regression of characteristic on treatments	Mean	Obs	P-value from F-Test from regression of characteristic on treatments	Mean	Obs	P-value from F-Test from regression of characteristic on treatments	Mean	Obs	P-value from F-Test from regression of characteristic on treatments
Panel A: Demographic Characteristics												
Female, Proportion	0.642 (0.479)	14006	0.592	0.708 (0.455)	2835	0.928	0.594 (0.491)	9650	0.295	0.826 (0.379)	1521	0.069
Age	34.775 (12.257)	13827	0.643	38.201 (13.709)	2657	0.449	34.191 (11.929)	9652	0.823	32.491 (10.337)	1518	0.823
Completed High School, Proportion	0.436 (0.496)	14016	0.082	0.752 (0.432)	2844	0.025	0.305 (0.460)	9652	0.892	0.674 (0.469)	1520	0.957
Wealthy	0.721 (0.448)	4365	0.567	0.693 (0.461)	2844	0.564				0.773 (0.419)	1521	0.876
Married, Proportion	0.207 (0.405)	11159	0.994				0.140 (0.347)	9638	0.951	0.633 (0.482)	1521	0.996
Has Saved Formally Before	0.195 (0.396)	11173	0.816				0.154 (0.361)	9652	0.978	0.454 (0.498)	1521	0.288
Weekly Income	37.645 (55.842)	1521	0.286							37.645 (55.842)	1521	0.286
Did Not Save As Much As I Want = 1	0.728 (0.445)	1480	0.848							0.728 (0.445)	1480	0.848
Spent Before I Saved = 1	0.251 (0.434)	1480	0.374							0.251 (0.434)	1480	0.374
Panel B: Savings Balance												
Amount Saved by Goal Date	0.551 (0.497)	14017		0.688 (0.463)	2844		0.564 (0.496)	9652		0.214 (0.410)	1521	
Proportion meeting commitment	86.751 (197.877)	14017		105.915 (193.580)	2844		90.205 (210.974)	9652		29.000 (65.217)	1521	

Variables in Panel A measured using baseline survey or application data. Income is based on the previous seven days. Wealthy is based on index constructed from measures of income, homeownership, water source, roof matrials, and water source.

Table 4: Estimates of the effect of getting any reminder (vs. no reminder)

Savings measure on LHS:	Log (1+Amount Saved)		1=Met Commitment	
	(1)	(2)	(3)	(4)
Panel A: Pooled Sample				
Pooled Sample	0.059	0.061*	0.032***	0.032***
	(0.037)	(0.037)	(0.009)	(0.009)
Baseline Controls	No	Yes	No	Yes
Mean of Dep Var	3.129	3.129	0.553	0.553
N	13,560	13,560	13,560	13,560
Panel B: Countries				
Peru (n = 2,775)	0.033	0.023	0.038	0.034
	(0.059)	(0.060)	(0.027)	(0.027)
Bolivia (n = 9,376)	0.058	0.057	0.033***	0.032***
	(0.043)	(0.042)	(0.010)	(0.010)
Philippines (n = 1,409)	0.115	0.159	0.015	0.020
	(0.099)	(0.098)	(0.029)	(0.028)
Baseline Controls	No	Yes	No	Yes
Mean of Dep Var	3.129	3.129	0.553	0.553
N	13,560	13,560	13,560	13,560
P-value from F-test of Peru = Bolivia	0.74	0.64	0.33	0.30
P-value from F-test of Peru = Philippines	0.48	0.24	0.16	0.11
P-value from F-test of Bolivia = Philippines	0.59	0.34	0.23	0.12

* p<0.10, ** p<0.05, *** p<0.01. OLS, with Huber-White standard errors in parentheses. "Amount saved" is the total amount of deposits made from account opening through the end of the commitment period. "Met commitment" is adhering to the term of the commitment: making all of the required deposits in Peru or Bolivia, saving the goal amount by the end of the commitment period in the Philippines. All regressions include controls for marketing offers in the Philippines (interest rate, joint/single account, deposit collection), number of accounts per individual and country fixed effects. Baseline controls include the full set household demographics listed in Table 1.
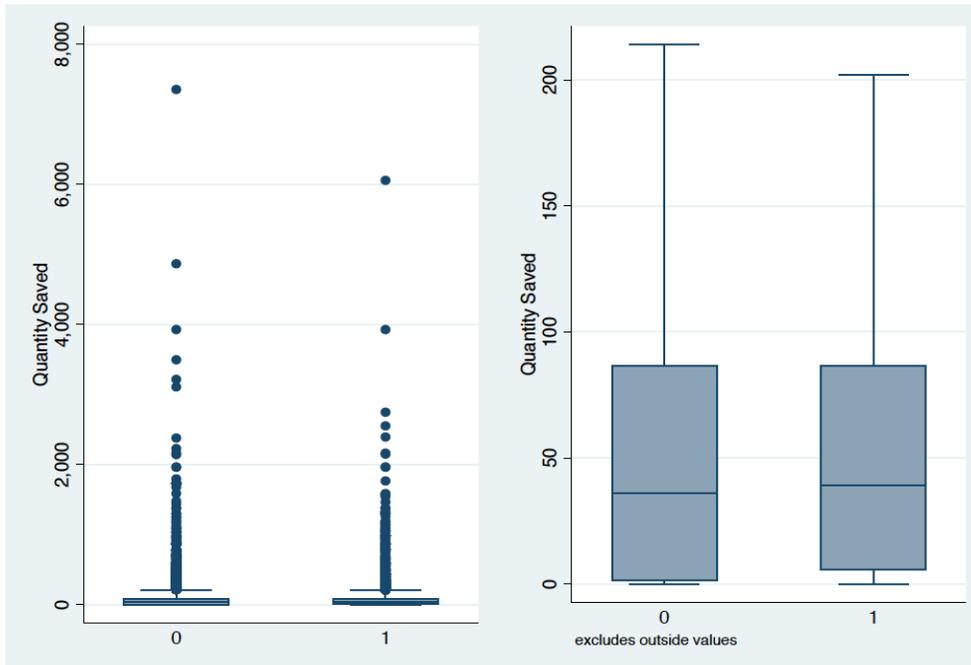
Figure 1: Amount Saved

Table 5: Reminder Content (no reminder is omitted category)

Savings Measure on LHS:	Log (1 + Amount Saved)		1 = Met Commitment	
	(1)	(2)	(3)	(4)
Panel A: Loss vs. Gain				
Gain-Framed Reminder	0.043	0.047	0.025**	0.025**
(no reminder is omitted category)	(0.041)	(0.041)	(0.011)	(0.010)
Loss-Framed Reminder	0.075*	0.075*	0.039***	0.038***
	(0.041)	(0.040)	(0.011)	(0.010)
P-value test of Gain = Loss	0.39	0.45	0.18	0.20
Baseline Controls	No	Yes	No	Yes
Mean of Dependent Variable	3.129	3.129	0.553	0.553
N	13,560	13,560	13,560	13,560
Panel B: Incentive (and Goal): Peru only				
Financial Incentive Reminder	-0.005	-0.009	0.033	0.031
(no reminder is omitted category)	(0.061)	(0.061)	(0.028)	(0.028)
Financial Incentive + Goal Reminder	0.134**	0.130*	0.050	0.047
	(0.067)	(0.068)	(0.031)	(0.031)
P-value test of Incentive = Incentive + Goal	0.001	0.001	0.42	0.43
Baseline Controls	No	Yes	No	Yes
Mean of Dependent Variable	4.099	4.099	0.691	0.691
N	2,775	2,775	2,775	2,775
Panel C: Goal (and Incentive): Bolivia only				
Goal Reminder	0.008	0.005	0.012	0.011
(no reminder is omitted category)	(0.053)	(0.052)	(0.013)	(0.012)
Financial Incentive + Goal Reminder	0.108**	0.112**	0.053***	0.054***
	(0.052)	(0.051)	(0.013)	(0.012)
P-value test of Goal = Incentive + Goal	0.10	0.07	0.004	0.003
Baseline Controls	No	Yes	No	Yes
Mean of Dependent Variable	2.983	2.983	0.564	0.564
N	9,376	9,376	9,376	9,376

* p<0.10, ** p<0.05, *** p<0.01. OLS, with Huber-White standard errors in parentheses. "Amount saved" is the total amount of deposits made from account opening through the end of the commitment period. "Met commitment" is adhering to the term of the commitment: making all of the required deposits in Peru or Bolivia, saving the goal amount by the end of the commitment period in the Philippines. Financial incentive in Peru is a higher interest rate. Goal in Peru is the client-specific plan for what would be bought with the saved amount, elicited at baseline. Goal in Bolivia is generic: "aguinaldo", or saving to give oneself the traditional Bolivian year-end bonus (for those working in the formal sector) of one month's pay. Financial incentive in Bolivia is free life insurance. All regressions include controls for marketing offers in the Philippines (interest rate, joint/single account, deposit collection), number of accounts per individual, and country fixed effects (when relevant). Omitted variable in all regressions is "no reminder."

Table 6: Reminder Timing

Savings measure on LHS:	Log (1+Amount saved)		1=Met Commitment	
	(1)	(2)	(3)	(4)
Panel A: Late *vs.* Regular (Peru and Philippines only)				
Regular Reminder	0.063	0.076	0.031	0.033
(no reminder is omitted category)	(0.067)	(0.068)	(0.025)	(0.026)
Interaction: Any Reminder * Late Reminder	0.003	-0.001	-0.003	-0.005
	(0.050)	(0.049)	(0.019)	(0.019)
Baseline Controls	No	Yes	No	Yes
Mean of Dependent Variable	3.457	3.457	0.528	0.528
N	4,184	4,184	4,184	4,184
Panel B: Gifts (Peru only, includes no reminder message group)				
Puzzle of Goal	-0.044	-0.039	0.004	-0.002
(pen is omitted category)	(0.047)	(0.048)	(0.023)	(0.023)
Photo of Goal	-0.031	-0.024	0.010	0.010
	(0.044)	(0.044)	(0.021)	(0.021)
P-value of test for Puzzle of Goal = Photo of Goal	0.75	0.72	0.77	0.55
Baseline Controls	No	Yes	No	Yes
Mean of Dependent Variable	4.099	4.099	0.691	0.691
N	2,775	2,775	2,775	2,775

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. OLS, Huber-White standard errors in parentheses. "Amount saved" is the total amount of deposits made from account opening through the end of the commitment period. "Met commitment" is adhering to the term of the commitment: making all of the required deposits in Peru or Bolivia, saving the goal amount by the end of the commitment period in the Philippines. All regressions include controls for number of accounts per individual, and country fixed effects (when relevant). Omitted variable in Panel A is "no reminder." Omitted variable in Panel B is receiving a pen.

Appendix Table 1: Savings Goal Expenditures in Peru

Savings Goal	Frequency	Percentage of Sample
1. Purchase equipment and tools	15	0.51
2. Buy merchandise	19	0.64
3. Purchase a moto taxi	21	0.71
4. Purchase land	31	1.04
5. Purchase vehicle(s)	22	0.74
6. Purchase housing	45	1.52
7. Education	458	15.43
8. Emergency/Contingency	1294	43.60
9. Purchase household equipment	105	3.54
10. Social and family events	78	2.63
11. Starting a business	97	3.27
12. Improve business	27	0.91
13. Improve housing	101	3.40
14. Other	655	22.07

Appendix 2: Robustness Checks for Specification of Quantity Saved, Pooled and by Country

Savings measure on LHS:	Amount Saved		Log(0.01 + Amount Saved)		Inverse Hyperbolic Sine of Amount Saved	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pooled Sample						
Pooled Sample	-5.546	-5.458	0.117*	0.120*	0.070*	0.071*
	(3.985)	(3.975)	(0.069)	(0.069)	(0.042)	(0.042)
Baseline Controls	No	Yes	No	Yes	No	Yes
Mean of Dep Var	85.328	85.328	2.213	2.213	3.648	3.648
N	13,560	13,560	13,560	13,560	13,560	13,560
Panel B: Countries						
Peru (n = 2,775)	-21.057	-22.208	0.035	0.023	0.035	0.024
	(16.542)	(16.589)	(0.061)	(0.062)	(0.061)	(0.061)
Bolivia (n = 9,376)	-4.297	-4.439	0.128	0.127	0.069	0.068
	(4.325)	(4.326)	(0.083)	(0.081)	(0.049)	(0.048)
Philippines (n = 1,409)	3.107	5.385	0.110	0.169	0.130	0.178*
	(4.333)	(4.498)	(0.111)	(0.111)	(0.108)	(0.108)
Baseline Controls	No	Yes	No	Yes	No	Yes
Mean of Dep Var	85.328	85.328	2.213	2.213	3.648	3.648
N	13,560	13,560	13,560	13,560	13,560	13,560
P-value from F-test of Peru = Bolivia	0.33	0.30	0.36	0.31	0.66	0.57
P-value from F-test of Peru = Philippines	0.16	0.11	0.55	0.25	0.45	0.22
P-value from F-test of Bolivia = Philippines	0.23	0.12	0.90	0.76	0.61	0.35

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. Huber-White standard errors in parentheses. Reminder messages sent by text message in Bolivia and Philippines. Reminder message sent by mail in Peru. All regressions include controls for marketing offers in the Philippines (interest rate, joint/single account, deposit collection), number of accounts per individual and country fixed effects. Baseline controls include the full set household demographics listed in Table 1.