

Personalizing or Reminding?

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Personalizing or Reminding? How to Better Incentivize Savings among Underbanked Individuals*

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Abstract

We use a randomized control trial to evaluate why text message (SMS) reminders may have worked in previous studies to incentivize savings. In a sample of underbanked Paraguayans, we replicate the previous findings that personalized SMS reminders have short-lived impact on savings targets and amounts, and that savings is mostly done through informal mechanisms. However, we find that either receiving generic SMSs or personalized reminders on their own help little to encourage savings behavior (and sometimes even reduce it) and only their combination plays a positive role. Salience appears to be particularly detrimental for more sophisticated individuals. Personalization reduces savings for individuals who are shown to have not been able to save previously. Combining both salience and personalization appears to be key, particularly for individuals who are more financially experienced, suggesting that simple SMS reminders alone are unlikely to be successful for this group.

JEL Codes: D14, D91, O16

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1 Introduction

Saving money appears to be difficult for most people but particularly for the poor, leaving them more vulnerable to economic shocks. While income restrictions may limit the capacity of any individual to undertake a savings plan, other barriers, mostly psychological or behavioral, have been highlighted as potentially equally relevant in explaining low savings levels. [Ashraf et al. \(2006\)](#) show that individuals who are offered an opportunity to use a commitment savings product, increase on average their savings balances despite not being specifically compensated for this restriction and receiving the same interest rate as they would in a regular account, which suggests that individuals lack commitment. Subsequent studies have extensively explored the role that simpler mechanisms than commitment devices, such as text messages (SMS reminders), can play in stimulating savings. In a study in Chile, [Kast et al. \(2018\)](#) find that SMS commitment reminders increase both savings balances and the number of deposits. [Karlan et al. \(2016\)](#), through a study developed in Peru, Bolivia and the Philippines, find that receiving SMS reminders about one's previously specified commitments to save increases the likelihood of meeting such commitments, and weaker but suggestive evidence that reminders also increase savings amounts.

While "salience", or bringing savings "top of mind" through SMS reminders, has been argued to be the key reason behind the effectiveness of these cited interventions, they did much more than send SMS reminders. For example, some interventions helped individuals set a specific savings goal and crafted personalized messages based on this goal. It thus remains unclear what exact features of the intervention are delivering the positive results and what exact behavioral barriers are being solved or reduced. Is it just that SMS reminders bring savings "top of mind" for recipients? Is it the fact that the messages are personalized to reflect individual savings goals? Or is it a combination of both?

We propose to better understand the behavioral biases that may lead to under savings and identify the specific mechanisms that may have worked in previous studies. To do so, we use tablet-based interventions for poor to middle income individuals in Paraguay through a randomized control trial.¹ We can check if our results replicate those of previous studies and at the same time distinguish the effects between "salience" and the "personalization" of the information. The experiment is based on the scheme shown in Table 1.

Theoretically, if individuals do not save for unforeseen future events because of present-bias, reminding them that savings is important could help them uphold their past savings commitments. However, the SMS reminders may also alter perceived social norms (one's perception of

¹For the purpose of another project, the sampling frame was selected to be representative of clients of a bank that serves this market in Paraguay, although both samples have little overlap.

Table 1. Experimental Framework

	Not Personalized (no savings goal)	Personalized (with savings goal)
Not Salient (no SMS)	No SMS reminders and no savings goal.	Asked to state a monthly savings goal, but no SMS reminders.
Salient (with SMS)	Generic SMS reminders on the importance of saving, without any personalized information.	Monthly SMS reminders of savings goal.

how much others are saving) or generate a psychological response known as “reactance” by inducing the individual to feel that their capacity to freely make savings decisions has been limited (Cagala et al., 2019; Dillard and Shen, 2005). Following the research on “rational inattention” (Caplin and Dean, 2015; Caplin et al., 2020), SMS reminders may also force individuals to more carefully consider a decision and this increased attention could be welfare-decreasing for them, although estimates suggest that there is a limit to that argument (Bronchetti et al., 2020).

Similarly, while goal-setting may help intertemporally inconsistent individuals stick to their objectives more easily, studies have emphasized that goals that are far in the future may not help present-biased individuals and may need to be task-based so as to be realized in a limited time horizon (Clark et al., 2020). Studies have shown that the difficulty in following through with one’s plans may be more relevant than lack of attention (Bronchetti et al., 2015).

The combination of both reminders and goal-setting may reinforce each other if the difficulty of goal-setting can be offset through reminding. The challenges of reactance or social norms could also be undone by personalization since individuals would not be sent generic messages about savings but rather messages that reflect their self-defined goals. Adding personalization could also improve the diminished present-bias effect of reminders, and reminders could make it easier for people to stick to savings goals.

We aimed to contrast the role of salience versus individualized information and their combination in interventions that incentivize savings.² We first designed a face-to-face baseline survey that included eliciting a very detailed budget balance and a detail of unforeseen expenses interviewees had faced during the previous year. This tablet-based intervention was common to all participants in the study. At the end of the survey, participants were randomly assigned to one of 4 groups (the tablet assignment was linked to a random number between 0 and 1). A first group, serving as control, received no more interventions. A second group used the information collected in the baseline survey to help participants set a savings goal but without further inter-

²This RCT was registered in the AEA registry as AEARCTR-0002080. No pre-analysis plan was registered but the main outcomes were stated as savings objectives and behavior. Our informed consent also mentioned the possibility to link individual’s information to administrative database which will come from the Paraguayan Credit Bureau.

action. The third group was simply sent SMSs reminding them of the importance of savings but did not help participants set up a savings goal nor reminded them of that goal through SMSs. The final group combined both salience and personalization whereby they were helped to set up a savings goal and then reminded of that goal through SMSs.

The timing of the intervention was as follows. A detailed tablet-based income and consumption survey was applied to 4,028 poor to middle-income individuals between the months of July and December of 2016. The entire sample is part of what is considered the “Gran Asunción” or the metropolitan area of the capital of Paraguay. Those who shared their phone number and were assigned to receive SMSs, received them for 6 months following the first intervention. The endline survey was also conducted face-to-face using tablets from August to November of 2017, between 8 to 16 months after the first intervention (which meant that at the time of the survey, between 2-10 months had passed since the receipt of the last SMS). We managed to find 2,853 cases in the endline, for a response rate of around 70 percent. We find attrition to be balanced between treatments although correlated with some individuals’ characteristics.

We then measure the impact of “salience” and “personalization” as well as the combination of both, using an Intention-to-Treat framework. We measure impacts on savings objectives as well as amount saved between the baseline and the endline survey. We also explore measures of how individuals saved, and whether we see a difference in their purchases, budget balances, credit behavior, exposures to shocks, and reported well-being. Finally, we also obtained data about the participants regarding their credit behavior from the Credit Bureau of Paraguay to study whether these self-reported savings behaviors also modified their relationship with the credit market.

We find evidence that personalization on its own does little to increase savings, even decreasing savings on average. Similarly, the impact of salience on its own is relatively muted. We find, however, that combining personalization and salience significantly and positively increases the impact of both interventions, leading to higher savings intentions (in both propensity and amount) and higher savings amounts among participants while receiving the SMSs. Similar results are obtained for credit. Other financial outcomes (balance budget, self-reported financial well-being, etc.) were not statistically significantly affected by any of our interventions. We find relatively limited evidence that the impact we had on savings was long-lasting as savings amounts at the moment of the endline survey were not significantly higher in groups that received either “salience” or “personalization” or a combination of both compared to the control group. These results appear to be confirmed in the Credit Bureau information where we observe that salience increases the amount of unpaid debt in the system while the interaction with personalization decreases it.

Our evidence shows that the participants perceived the interventions in the ways we anticipated. Those who were assigned to receive personalized SMSs were more likely to remember

receiving SMSs about a given savings goal than those who were only assigned to the “salience” treatment. On the other hand, the baseline survey appears to have been experienced differentially between groups despite the fact that it was designed to be almost identical for all treatment arms. For example, being assigned to salience marginally raised the probability of remembering being told to save 10 percent of their income, which was never specifically mentioned during the survey process. Finally, we find that the treatments did not differentially affect how or where participants saved, when they did so.

We find limited evidence that how personalization and salience are combined matters. In one treatment arm, individuals were guided to set a tangible savings objective (e.g., a cell phone) instead of a simple number. In the other, we used the variability of income faced by individuals in the previous year to encourage precautionary savings. We find no evidence that the way in which personalization and salience were combined impacted savings behavior, although our statistical power is a bit reduced in this case. We nevertheless found that the take-up of the tangible savings objective treatment was significantly lower than that of the other alternatives suggesting that individuals may not be saving for a particular purchase. Thus, our results appear to indicate that it is combining personalization and salience that matters, more than the exact way in which this is done.

Heterogeneity analysis suggests that salience seems to have a negative impact for individuals that may have been more sophisticated in terms of financial literacy. This suggests that simply reminding people who already understand the importance of saving, may even backfire. This is related to studies that have found “psychological reactance” (Dillard and Shen, 2005) as in the case of people being reminded of academic honesty actually increasing plagiarism (Cagala et al., 2019). Combining reminders with personalization significantly decreases this penalty thus suggesting that sophisticated individuals may particularly benefit from the combination of interventions. Personalization seems to decrease savings intentions and behaviors for those who were shown their previous incapacity to save during the baseline exercise. This may thus be welfare-enhancing if personalization helps individuals realize their real savings capacity through the intervention.

Overall, our results suggest that reminders to bring savings “top of mind” without an effort to personalize the message appropriately may not be useful in changing savings behavior. This is very different than the result obtained by Cunha et al. (2017) in the context of communicating to parents about the importance of attending school. This may be because savings is a more complicated decision than school attendance and that there may be very different “optimal” decisions for each participant. However, offering personalized information requires effort. We argue that our digital baseline survey, implemented on a tablet, is an approach that could be widely adopted by banks, financial institutions, education organizations, and even governments,

thus offering a policy application for our intervention.

This paper is part of a large literature base that has tried to foster higher savings. Traditional mechanisms to increase savings have focused on improving access. [Dupas et al. \(2018\)](#) find that lowering the cost of savings accounts increases uptake. However, the authors are unable to find evidence that savings amounts increased. Other studies have had similar results, although [Kast and Pomeranz \(2014\)](#) find positive impacts on increased savings amounts by improving access. [Gertler et al. \(2018\)](#) find that prize-linked savings accounts increase savings on the extensive margin by inducing new savers to open accounts. However, they also find that only a minority of new account openers remain active users in the long-term (presumably those for whom the benefit of saving in a formal account is higher than anticipated). This paper relates to the literature on the role of salience. We use reminders about savings to enhance the salience of savings plans. [Karlan et al. \(2016\)](#) show that SMS reminders that include individualized information about pre-specified savings goals are useful at increasing savings plans and potentially also the amount of savings. On the other hand, [Banerjee et al. \(2018\)](#) and [Attanasio et al. \(2019\)](#) find no evidence that SMSs can increase savings, including showing some evidence of decreased savings when comparing groups receiving SMS reminders. By unpacking the ingredients of SMSs, we may be able to explain why they work in some contexts and not in others.

Our paper also relates to the literature that highlights problems that lead to people to underestimate the benefits of saving. These problems can be reduced by setting a specific target of future consumption that makes savings tangible (e.g., [Akbas et al., 2016](#); [Stango and Zinman, 2009](#)). We find no difference between this treatment and our combined salience and personalization treatment.

Our paper is well-placed within the literature on the role of financial education. We can think of our budget analysis exercise completed at baseline as a very personalized type of financial education. In general, this literature has found that financial education is not very attractive nor largely effective. [Goldberg \(2014\)](#) reviews a set of existing studies and argues that there is very limited effect of financial literacy interventions on savings rates. In particular, two studies for Indonesia, [Cole et al. \(2011\)](#) and [Carpena et al. \(2011\)](#) both show no impact of interventions. [Seshan and Yang \(2014\)](#) found that financial literacy training increased savings for those with initially low levels of savings. Our results suggest that we may need to personalize this information (as in [Fuentes et al., 2018](#)) and combine it with reminders for it to be effective. Personalization could reduce attempts to adhere to non-feasible savings plans, reducing frustration and distress. [Attanasio et al. \(2019\)](#) find that a tablet-based intervention, which was personalized and much more intensive than ours, increased financial literacy and savings.

In addition, the treatment that seeks to incentivize precautionary savings also highlights the role of saving as insurance against unexpected fluctuations in income and expenditure (e.g., [Kast](#)

and Pomeranz, 2014). The literature has emphasized that poor households are more likely to experience short-term variations in income and have difficulties facing unforeseen costs (Collins et al., 2009). We think we are the first study to try to incentivize savings by providing personalized information to individuals regarding their history with these shocks and making these more salient. This is related to the “availability heuristic”: something that comes to mind more easily is implied to be more important psychologically.

Finally, our paper joins a series of recent studies that have highlighted the limitations of behavioral interventions to incentivize positive behavior. Damgaard and Gravert (2018) show that nudges to incentivize charitable donations may backfire and increase the likelihood of participants unsubscribing from the charity’s mailing list. Similarly, Carrera et al. (2018) show that goal-setting for physical exercise may not have any impact on the level of physical activity undertaken. Our experiment suggests that while nudges and personalized goal-setting may independently not change behavior, their combination appears to be more successful in increasing savings.

The rest of the paper is organized as follows. The next section presents the experimental details and Section 3 presents the data and the empirical strategy employed. Section 4 presents the results while the last section concludes.

2 Experimental Design

2.1 Treatment Details

This section details the experimental design we followed. A detailed income and consumption survey was applied to 4,028 poor to middle-income individuals between the months of July and December of 2016 through a tablet-based interface. To help individuals construct a “balance” for their monthly budget in order to measure their capacity to save, our survey asked careful questions about income and expenses. It first asked each respondent what their income was last month (if last month was considered “abnormal”, it asked the interviewee to consider a “normal” month). It included a detailed consideration of all income sources required to cover the participant’s needs and those of their dependents. It suggested as sources of income: wages, other variable income, remittances, family transfers, government transfers, earnings from business, etc. Once all income sources were entered, the sum was presented to the respondent to verify if there were any mistakes. If the respondent found the sum not to correspond to their aggregate income, they were then allowed to go back and change their initial answers. A similar procedure was followed for expenses. We provided respondents with 14 expense categories (housing, food, education, health, transportation, home phone, cell phone, Internet and Cable,

gas, electricity, heating, credit payments, entertainment, other). For each category, the respondent was asked to estimate their expenses in the last month, considering only expenses that were covered by their income. After answering each category, the tablet would sum all expenses and ask the respondent if the sum corresponded to a proper estimate of aggregate expenses. If the respondent disagreed, they could go back and readjust their answers. Once that process was completed, the tablet would calculate the individual's balance as income minus expenses and would request confirmation once more. The interviewees had many opportunities to revisit incoherent answers. In addition to this detailed budget information, details regarding individual and household savings were collected.

The survey also attempted to measure exposure to economic shocks. Specifically, it asked the participants to detail in which of the past 12 months they had faced difficulties covering unexpected expenses or experienced drops in income. We carefully trained surveyors to understand that once-a-year expected types of payments (e.g., taxes) are not unforeseen expenses and that they should instead focus on eliciting information on accidents, health shocks, death-related events, etc. If the individual answered a positive number of months, the tablet would then ask if it was because of drops in income, unexpected expenses, or both. If drops in income were identified, we asked for how many months this was a problem and the monthly average amount of "lost" income. If unexpected expenses were mentioned, we asked for the type of shock, the approximate amount, and the number of times this event had occurred. We then asked if the individual had ever thought of saving to face that type of unexpected expense.

After answering the baseline survey, the interface randomly assigned the individual to one of six groups (the tablet assignment was linked to a random number between 0 and 1 and ranges of 0.166 were assigned to each treatment). Each of the treatment groups received, through an interaction with the tablet interface, different types of information to help the participant in thinking about his or her optimal level of savings. In some treatments, the information provided to the participant used their responses to the baseline survey as inputs for initial suggestions. A member of the survey team guided the subject through the assigned treatment. We now explain each treatment one by one, focusing on the first four since they constitute the basis of our analysis.

Table 2 shows the detail of what each experimental group experienced. In our control group each participant received a short message on the tablet screen reminding them of the importance of savings. Specifically, the screen stated, in Spanish: "Remember that saving is useful to improve your quality of life. It's cheaper than asking for credit and you acquire more security to face unexpected expenses." That group would then finish the intervention and would never be contacted again until the endline.

In our salient treatment group, the participant received exactly the same message as in the

Table 2. Experimental Design

	Not Personalized (no savings goal)	Personalized (with savings goal)
Not Salient (no SMS)	<p>During interview: No savings goal elicited.</p> <p>After interview: No more interaction.</p>	<p>During interview: "To get motivated to save, it may be important to have a savings goal. According to the survey, your monthly income is ... and your monthly expenses are ... which implies that you have a monthly balance of ... Knowing this, what do you think could be your monthly savings goal?"</p> <p>After interview: No more interaction.</p>
Salient (with SMS)	<p>During interview: "To help you remember this, we can send you a monthly message via SMS during the next 6 months. Would you like to receive this message?"</p> <p>After interview: Received the following SMS for 6 months "Remember to save! With savings, you can buy without asking for credit and you acquire more security to face unexpected expenses."</p>	<p>During interview: "To get motivated to save, it may be important to have a savings goal. According to the survey, your monthly income is ... and your monthly expenses are ... which implies that you have a monthly balance of ... Knowing this, what do you think could be your monthly savings goal?"</p> <p>"To help you remember this, we can send you a monthly message via SMS during the next 6 months. Would you like to receive this message?"</p> <p>After interview: Received the following SMS for 6 months "Remember to save! According to your income and expenses, you had established a monthly saving goal of Gs..."</p>

control group at the end of the interview. But the next screen stated "To help you remember this, we can send you a monthly message via SMS during the next 6 months. Would you like to receive this message?" If the participant agreed, their phone number was then solicited. If they accepted, the following SMS was sent to them every month for 6 months: "Remember to save! With savings, you can buy without asking for credit and you acquire more security to face unexpected expenses."

In the personalized treatment group, the participant received exactly the same message as in control group at the end of the interview. But the next screen stated "To get motivated to save, it may be important to have a savings goal. According to the survey, your monthly income is ... and your monthly expenses are ... which implies that you have a monthly balance of ... Knowing this, what do you think could be your monthly savings goal?" The information was taken from the survey of income and expenses, as detailed above. Once the person specified a goal, the screen

would then show that savings amount again and remind them that this would be their monthly savings goal.

Finally, in our combined salient and personalization treatment group, participants received the same message as the other groups on the tablet. Afterwards, the tablet prompted the participant to accept or reject the SMS using the same phrasing as in the salient treatment group. If the participant accepted to receive a message, the following SMS would be sent to them every month for 6 months: "Remember to save! According to your income and expenses, you had established a monthly saving goal of Gs...". In the case where the participant had been unable to specify a savings goal but agreed to receive the SMS, we sent the same generic message as was sent to the salient treatment group.

Individuals assigned to these four groups will constitute our main sample of analysis since the comparison between each group is straightforward. Nevertheless, we also implemented two other ways in which salience and personalization were combined. In a fifth group, denoted the "tangible goal", the tablet instead stated, "To get motivated to save, it may be important to have a savings goal. These are some examples of goods or services that you may want to purchase [Photos]. Which good or service would you like to purchase with your savings? Name the good or service...What is the monthly amount you could save in the next 6 months to achieve your objective of purchasing a ... If you were to purchase this ... using credit, you would need to face a monthly payment of ... for 6 months. Instead, if you decide to save, you would only need to put aside ... monthly for 6 months." The tablet displayed images examples of goals that could be set, including a vacation, a motorcycle, a cell phone, a laptop and a word for "others" to help interviewees better understand what they were being asked to do. These examples were obtained from two sessions of focus groups we had previously conducted with a similar population. The credit calculations assumed that buying these items with credit would be 40 percent more expensive than using savings, a rate that was common in our baseline survey for this population. The participant was then asked if they wanted to be reminded of the message through SMS. Those who agreed and were able to pick a savings objective received the following message: "Remember to save! In your objective to purchase a ... you had established a monthly savings goal of Gs...". The participants who were unable to specify a good or service they wished to save for but agreed to receive the SMS were sent the same generic message as the salient treatment group.

In the sixth group, which we will refer to as "precautionary savings", the individuals were reminded of the amounts they faced as unexpected shocks the year before. This was calculated by multiplying the number of months where the person faced a drop in income times the average monthly lost income plus the sum of all unexpected expenses the household faced. Specifically, the tablet led the individual through the following exercise: "Savings allows you to face unex-

pected expenses and unexpected falls in income to cover your expenses without needing credit. According to the survey, you faced unexpected shocks of ...Gs last year. It is possible that in the next 12 months, you may face similar unexpected shocks as those you faced last year. What would be the amount required to cover these unexpected shocks? If you saved ... for 6 months you could cover half of these unexpected shocks. Knowing this, what is the monthly amount you could save during these 6 months with the objective of covering half of these unexpected shocks? To face these unexpected shocks using credit, you would need to pay a monthly installment of ... during 6 months. If instead, you saved, you would need to save monthly ... for 6 months." Again, the credit cost was calculated as being 40 percent more expensive than using savings. After this, the participants were invited to sign up for the SMS message reminding them how much they had committed to save. Those who agreed and set up a savings goal received the following message: "Remember to save! To cover your unexpected expenses and possible unforeseen shocks, you had established a monthly saving goal of Gs..." Those who were unable to set such a goal but agreed to receive the SMS were sent the same generic message as the salient treatment group.

2.2 Power Calculations

We initially performed power calculations to determine a target sample size. We assumed, as is traditional, a power of 0.8 and a significance of 5 percent. We also assumed that baseline data would have a correlation with the endline outcome data of about 0.4 because we felt that many of our outcomes would be relatively stable over time without our intervention although we had no panel data to verify this assumption. Since our treatment is individual and our subjects are unlikely to interact with each other in a meaningful way, we also assume that there is no intracluster correlations. At that point, we calculated that we would be able to detect effects as small as 0.12 standard deviations in any outcome when comparing each treatment to the control group or with each other if we had at least 667 observations per group. However, this assumed full compliance and no attrition.

After the baseline, we updated our power calculations using actual compliance rates. We found that individuals who were assigned to the salient treatments accepted the invitation to receive SMS at rates above 70 percent and that more than 75 percent of those who were assigned to the personalization treatments were able to set a goal. Once individuals agreed to receive the SMS, we also needed to have the technological conditions to send the message (active phone line, mostly). We only faced 11 cases where we were unable to deliver the SMS out of 10,000 messages, something that leads us to believe that this is unlikely to be a strong reason why some individuals may not benefit from the intervention. In our additional personalization and salience treatments, the precautionary savings intervention had a similar level of compliance. However, for the tangible goal treatment, only 54 percent of participants selected a savings objective based

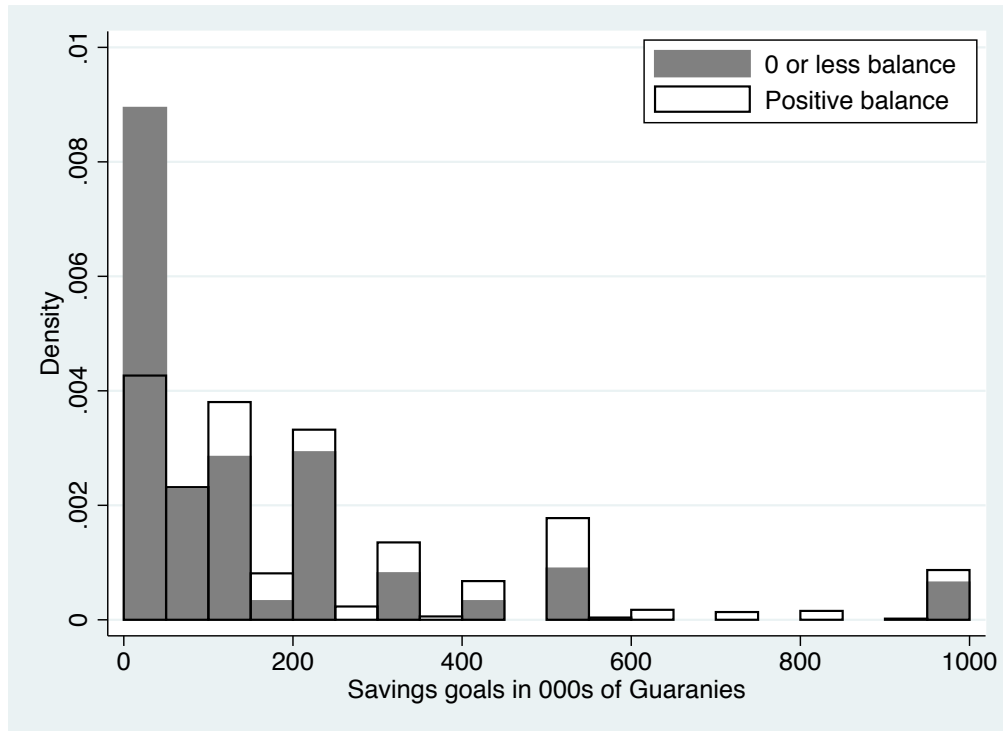
on a tangible good/service and only 41 percent took up the full treatment, namely picking a savings objective and receiving an SMS. We updated the power calculations as detailed in Appendix Table A1. We found that the correlation between baseline and endline outcomes was even larger than anticipated, leading us to face relatively low minimum detectable effects, often below 0.1, for outcomes where we have baseline information. However, for outcomes that had no baseline information, our experiment will allow us to detect effects larger than 0.2 to 0.3 standard deviations which is still within range of what has been detected in this literature.

2.3 Responses of Participants at Baseline

Our interventions required a careful exercise by the respondent to set a savings goal in two of our treatment arms (personalization and the combined treatment). Since this was a challenging exercise for most individuals, it is important to explain how they appear to have set goals depending on the type of message they received. In the personalization only treatment, 80 percent of all participants were able to set a goal, a number very similar to the share of participants in the combined personalization and salient treatment that did (78 percent). How coherent are the savings goals established by our participants? Overall, our average individual reported a positive monthly balance of 630,000 Gs (approx. USD 109)³ in our baseline sample. This hides a large variance with some individuals reporting negative balances while others reported extremely large positive balances. We also find that individuals are somehow rational in the decision of whether or not to set a savings goal. Figure 1 shows that individuals who reported a positive balance in the baseline set significantly larger goals than those who revealed a negative balance. This suggests that income constraints appear to play a strong role in the ability of individuals to set savings goals. Personalized information may thus be relevant for individuals to set a goal. Among participants who did set a goal, the range of savings objectives were from 315,000 Gs (approx. USD 55) for our personalized goal without SMS group and 360,000 Gs (approx. USD 62) for our personalized goal with SMS group. This suggests that on average individuals are electing as an objective something that is about a third of their balances. However, there is considerable variance: we find that between a quarter and a third of those who set a goal propose something that is larger than their balance.

³Employing the exchange rate for 12/2016 as reported by the Central Bank of Paraguay (5,766.93 PYG/USD).

Figure 1. Savings Target by Initial Budget Balance



Notes: This graph presents the histogram of savings targets from the personalization only group and the combined personalization and salient group. Dark-colored bars are for individuals who had a 0 or negative budget balance at baseline while white bars are for individuals who had a positive budget balance at baseline.

3 Data and empirical analysis

3.1 Data

Our target population was selected to mimic the clients of a formal bank in Paraguay that specializes in middle-low and low-income clients and with whom we had partnered for another study focused on credit (Azevedo et al., 2020). We used the original distribution of these clients as a guide to determine our sampling frame. We asked the survey firm to find individuals to fit quotas by neighborhood, gender, age, and occupation to match as closely as possible the distribution of the pool of applicants of the bank's product. For neighborhoods where we had both poverty rates in 1997- 1998⁴ we were able to confirm that the neighborhoods where we sampled more intensively were populations that had more poverty.

Employed sampling quotas were:

1. Cities: Asunción (20%), Areguá (4%), Capiatá (12%), Fernando de la Mora (6%), Itaguá (7%), Augusto Saldivar (1%), Lambaré (8%), Limpio (6%), Luque (13%), Mariano Roque Alonso (4%), Ñemby (5%), San Antonio (1%), Villa Elisa (4%), Ypane (1%) and San Lorenzo (9%)
2. Gender: 61 % women
3. Age: 18-25 (42%), 26-35 (38%), 36-45 (11%), 46-55 (5%), 56-65 (3%), over 65 (1%).
4. Occupations: public employee (1.5%), private employee (42.8%), self-employed or business owner (39.8%), domestic workers (9.5%), unemployed and others (5.2%).

A baseline survey was applied to 2,697 individuals between the months of July and December of 2016.⁵ In addition to the income, expenses, and unforeseen economic shock questions we detailed in the previous section, the survey included measures of labor supply, credit market interactions, psychological well-being and the relationship with the particular bank we had partnered with. On average, surveyed individuals were 30 years old, 61 percent women, 54 percent single, 49 percent head of household and average years of education were only 6.8. Average reported monthly earnings were around Gs 2.642.000 (USD 460 using market exchange rate or USD 1,070 adjusting for PPP⁶). As a point of comparison, the "base of the pyramid" is defined by international organizations as households who have less than 10 USD PPP per day per capita.

⁴The latest information we found available.

⁵We focus on our main sample, which includes our four main treatment groups. Our full sample (4,028), includes the two additional personalization and salient treatment groups which have very similar characteristics as those reported for our main sample.

⁶Employing the PPP conversion factor for 2016 reported by the World Bank Group (2,469.579 PYG/USD PPP).

We do not have household size in this setting but assuming household sizes of at least 3 people (with 1 earner), this would indicate that a significant size of our sample would be included as being part of the “base of the pyramid”. Their reported average expenses amount to Gs 1.866.000 (USD 320), with an average monthly balance of Gs 782.000 (USD 130). Interestingly, a third of individuals save and they report saving a monthly amount of Gs 137.000 (USD 20) which is a sixth of their monthly household balance. This suggests, as has been discussed in the literature, that much of the lack of savings seems to come from expenses that are not even registered by the individual. Financial shocks are common: 39 percent state having had financial problems in the previous year and this was mostly due to unexpected drops in income. A summary of other measures of financial and economic well-being is included in Table 3.

We then conducted an endline survey to measure how our interventions influenced savings behavior and attitude. This survey was conducted about one year after the baseline, between August and November of 2017 by the same survey firm as the baseline and using the same type of tablet. The survey included the same income and expenses module as the baseline, the same calculations of economic shocks, information regarding labor supply, credit, assets and psychological well-being. Finally, the last section details savings intentions, behaviors, and outcomes. We also included a module to understand how much interviewees remembered from the baseline survey exercise and for those treated, how much they remembered of the specific activities of their respective treatment arms.

Finally, in June of 2020, we obtained information on all study participants from Equifax, the Credit Bureau of Paraguay. From this data set, we could obtain the district of election in 2020, the requests to the Credit Bureau between 2017 and 2020 with the date and the entity making the request, the credit score of the participant in 2020, whether they had any outstanding debt in 2020 and the amount of debt they had in the system. We were not able to obtain more detailed information about the debt (by entity) but were provided the maximum number of days since the person had been registered as having unpaid debt. We exclude individuals who had a debt registered before the baseline although our results are similar when including them.

3.2 Empirical Strategy

We estimate the causal impact of our interventions using an ITT framework. In particular, since our goal is to disentangle the role of salience and personalization, we chose to combine all the data and estimate one single regression of the form:

$$y_{it} = \beta_1 \text{Salient}_i + \beta_2 \text{Personalized}_i + \beta_3 \text{Salient}_i * \text{Personalized}_i + \alpha X_i + \delta y_{it-1} + \varepsilon_{it} \quad (1)$$

Table 3. Balance

	Mean	Salience	Person.	Both	Agg. Effect
Age	30.23	-0.54 (0.40)	-0.22 (0.40)	0.30 (0.56)	-0.45 (0.40)
Women	0.61	-0.01 (0.02)	0.01 (0.02)	0.00 (0.04)	-0.00 (0.02)
Years of education	6.77	0.11 (0.12)	0.06 (0.12)	-0.11 (0.17)	0.06 (0.12)
Share single	0.54	-0.01 (0.03)	-0.01 (0.03)	0.03 (0.04)	0.01 (0.03)
Head of household	0.49	0.01 (0.02)	0.00 (0.02)	-0.01 (0.03)	0.00 (0.02)
Worked last year	0.94	0.01 (0.01)	-0.01 (0.01)	0.01 (0.02)	0.00 (0.01)
Total ind. income (thousands)	2,642	-19.64 (125)	147 (132)	-231 (174)	-103 (120)
Total ind. expenses (thousands)	1,866	70.12 (82.20)	-115* (68.88)	131 (107)	86.55 (69.91)
Account balance (thousands)	782	24.73 (125)	132 (132)	-246 (175)	-89.98 (121)
Saves	0.31	0.01 (0.02)	-0.01 (0.02)	-0.01 (0.03)	-0.00 (0.02)
Individual savings (thousands)	137	9.91 (26.54)	24.79 (44.31)	85.72 (139)	120 (89.47)
Had financial problems last year...	0.39	0.01 (0.02)	-0.01 (0.02)	-0.00 (0.03)	0.00 (0.02)
...because of fall in income	0.26	0.01 (0.02)	-0.00 (0.02)	-0.03 (0.03)	-0.02 (0.02)
Index of well-being	0.00	-0.00 (0.04)	0.03 (0.04)	-0.04 (0.06)	-0.01 (0.04)

Table presents the mean in baseline of the variable and the coefficients of regression equation (1) using as outcome variables each baseline characteristic. The last column presents the sum of each β and standard errors calculated using the delta-method. Robust standard errors are presented in paranthesis. N=2,697 for all variables except for "Individual savings (thousands)" (N=2,679 because 18 individuals said they had saved but did not recall the exact amount). Index of well-being is made up from yes or no answers to 4 questions: Do you lose sleep? Do you feel tense? Do you feel nervous? Do you feel things collapsing? * p<0.1, ** p<0.05, *** p<0.01.

where y_{it} is the outcome of individual i measured in the endline. $Salient_i$ takes the value of one if individual i was randomly assigned to receive SMS while $Personalized_i$ is a dummy that takes the value of one if the individual was randomly assigned to set up a personalized savings goal. Note that those assigned to receive a salient and personalized treatment will have both dummies equal to one. This implies that β_3 measures the additional impact of combining

personalization and salience above and beyond each of the individual treatments. To obtain the impact of the treated compared to the control, we will simply add all β s and obtain the standard errors through the delta method. We also control for X_i , baseline characteristics of the surveyed individuals (gender, age, educational attainment, marital status, occupations, district, month of baseline survey, risk aversion, financial literacy, number of household members in the household, log baseline income, log baseline expenditures, log baseline balance, log baseline individual and household savings and the number of months with difficulties as reported in the baseline), y_{it-1} is the baseline value of the outcome (when it was measured). Standard errors are allowed to be heteroscedastic.

This is an appropriate strategy if our randomization was properly conducted. We show, in Table 3, that we have no differences in baseline characteristics by the type of treatment that individuals received except for the individual expenses that are slightly lower for those assigned to the personalized treatments.⁷ Overall, this suggests that our randomization appears to have generated groups of similar ex-ante individuals to be assigned to each treatment.

Nevertheless, the sample with which we base our analysis is slightly different than that mentioned above because of attrition. We were unable to find all of the 2,697 individuals in our original sample. The main issue was that many individuals had moved to other areas of Paraguay or even out of the country, making it impossible for us to find these respondents.⁸ Other reasons involve the incapacity of finding the respondent at their house despite multiple visits and calls or the unwillingness of participants to respond to the endline survey. Overall, we managed to reach 1,963 individuals, meaning we had an attrition rate of about 28 percent. We find that individuals who were found in the endline were older, more likely to be males, to have fewer years of education, to be in a relationship, to be a business owner, not to pay social security and to live in a slightly larger household. This will affect for whom our estimate is valid. However, our estimate will remain unbiased for that sample as long as our treatments are not correlated with attrition. Table A2 shows that there is no statistically significant different attrition between those assigned to personalization or salience treatments compared to the control group. We also find that those assigned to the personalization and salience treatment were no more likely to be found than those assigned to only one of the two types of treatments. However, we find that those assigned to both treatments were slightly more likely to drop out than those assigned to the control group. We will thus explore whether the results depend on this differential attrition rate in our robustness section.

⁷To do this, we estimate equation (1) using as our outcome variables baseline characteristics.

⁸We confirm this by using Credit Bureau data where we find that those who dropped out were 11 percent more likely to be registered to vote in a different district than the one we had identified as their home at baseline.

4 Results

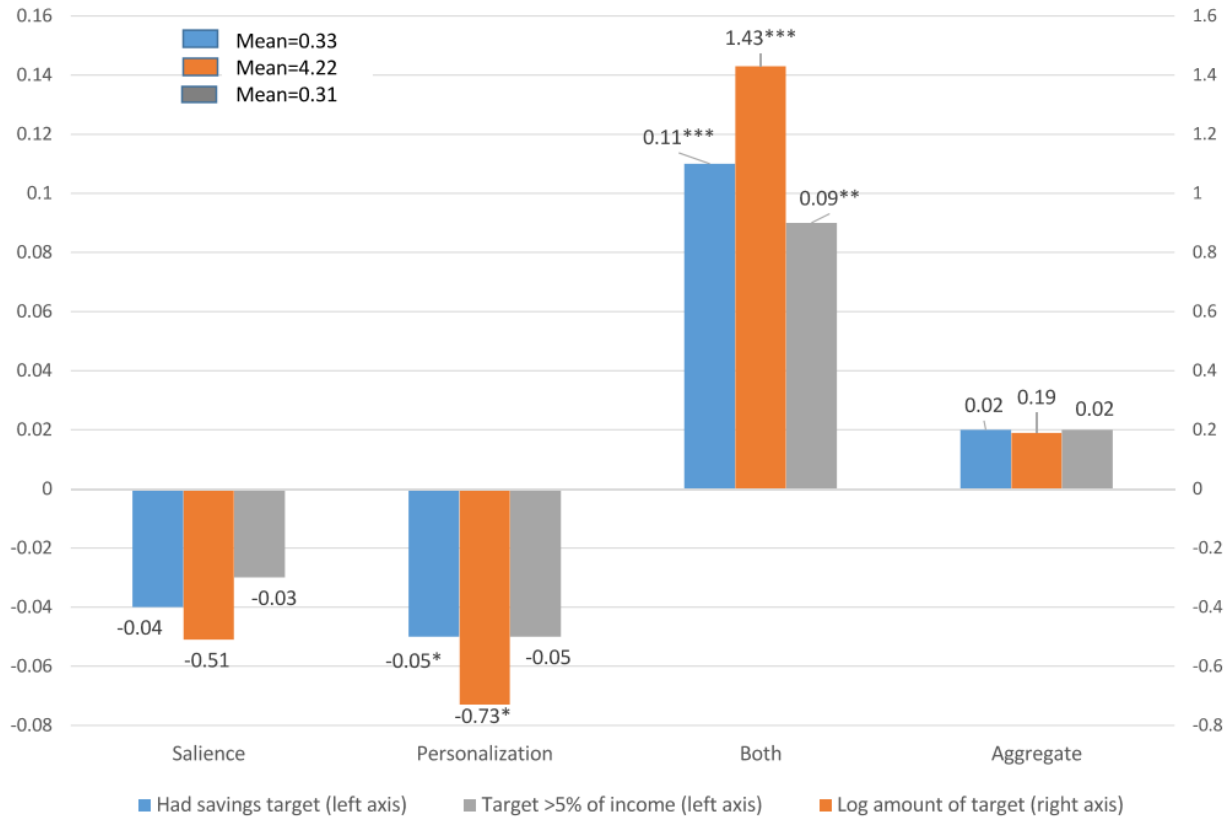
Having described our empirical strategy and our data, we now turn to exploring the results of the interventions we conducted.

4.1 Savings

Our intervention aimed at altering savings behavior and thus we start by exploring the impact we had on savings. Since most previous studies found significant impacts on savings intentions, we elicited savings targets from participants at endline to first check whether our treatments also affected savings objectives. Figure 2 shows the coefficients of β_1 , β_2 , β_3 and their sum on three different outcomes. First, we measure whether the individual had a savings target the month before the endline survey. The second outcome is whether that savings target represented more than 5 percent of their baseline income. The last outcome is the log amount of the savings target (where those who did not set a target are replaced by the log of 1). Results show that salience on its own does not alter goal setting in any way. Similarly, personalization alone was unable to increase savings targets; it actually significantly reduced the amount of the target set. We hypothesize that this may be because personalization shows individuals their real capacity to save and thus forces some people to be more “rational” in their goal-setting. However, combining salience and personalization has a very strong impact on the capacity of individuals to set a target and on the amount of the target compared to only receiving salience or personalization. It increases the probability of setting a savings objective by 11 percentage points, increases the probability that this savings target is at least 5 percent of baseline income by 9 percentage points and increases the amount pledged by 143 percent. The aggregate impact of our combined interventions compared to the control is not statistically significant but remains positive, increasing the probability of setting an objective by 2 percentage points and the amount of that target by 19 percent. We thus conclude that savings intentions appear to be more strongly reinforced by the combination of salience and personalization.

We next turn to the savings during the receipt of SMSs. In the endline survey, we asked whether individuals saved during the seven months following the interview. We first display these results graphically in Figure 3. Results show a very similar pattern as the one we presented above. Personalization and salience on their own do not influence the savings behavior right after the initial survey. But participants assigned to the combined treatment have a statistically significantly larger probability of saving during almost all months than those assigned to only personalization or salience. This finding is summarized in the first row of Table 4 where we see that the combined group now has a statistically significantly larger number of months where savings occurred compared to the control group. For the amount saved and the comparison of

Figure 2. Impact of Treatments on Savings Target In Endline

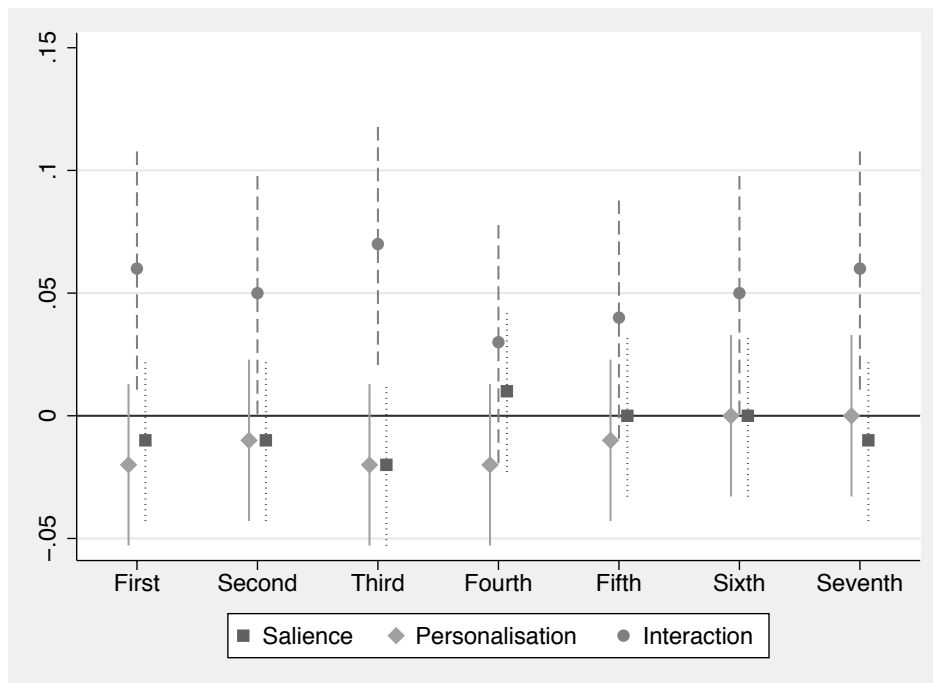


Notes: This graph presents each of the coefficients of regression equation (1) using as outcomes a dummy for whether the individual had a savings target (in blue), whether their target was more than 5 percent of their income (in grey) and the log amount of the target (in orange). The “Aggregate” columns are the sum of each coefficients. * p<0.1, ** p<0.05, *** p<0.01.

that amount to baseline income (second and third rows) we find that saliency and personalization combined was the only intervention that significantly resulted in a positive impact. However, we now lose the statistical power to contrast the combination of personalization and saliency to each of these characteristics on their own.

How did actual savings amounts compare to participants’ stated goals at baseline? Almost 80 percent of the respondents declared not having saved an amount close to the savings target. Another 12 percent saved an amount similar to their savings goal, while less than 10 percent saved more than their objective. While not reported here, we found that combining personalization with saliency helped individuals save an amount similar to their goal. This suggests that combining both personalization and saliency is particularly effective at raising savings during the intervention period and at increasing savings intentions at endline.

Figure 3. Impact of Treatments on Savings during the Intervention



Notes: This graph presents each of the coefficients of regression equation (1) using as outcomes a dummy for whether the individual saved in each month following the baseline. Squares represent β_1 , diamonds β_2 and circles β_3 . 95 percent confidence intervals are also presented.

We next measure actual savings at endline in the bottom panel of Table 4. We find that the interventions did not change the probability that someone saved, the amount saved, or the probability of saving more than 5 percent of income. The coefficients are small and do not show a clear pattern in terms of signs. The following two rows check whether this absence of impact on savings is because individuals were unable to change their income and expenses as a way to modify their savings. We find that individuals who received the salient treatment only were able to have a more positive balance at endline (due to higher income) although this did not translate into larger savings. This suggests that SMS reminders are unlikely to work because they bring savings to the “top of mind” since they were unable to translate budget surpluses into savings. There appears to be some lasting impact on how expenditures are made. We see that personalization and salience on their own raise discretionary expenses and decrease expenses on necessities. Their combination, on the other hand, nullifies the change even though the impact is not significant. Thus, it appears our intervention continues to modify expenditures in a way that would help savings, but too noisily for us to detect an influence on savings in the long-run.

Our next sets of results look at the way individuals saved, as shown in Table 5. We find that most of the results we documented below appear to stem from individuals spending less rather than acquiring new sources of income. We do not see large differences in how the saving was done between those exposed to salience or personalization only and those that had both interventions combined. Nevertheless, we find that when comparing those who had a salience and personalization treatment to the control group, there is a statistically significant impact on spending less and reducing credit costs, consistent with our results on savings and credit use below. The bottom panel of Table 5 shows that the increase in savings was mostly done informally (without a formal bank account), particularly when the person was assigned to the combined treatment. We find no evidence that any of our interventions influenced savings in a bank or a cooperative.

We next explore other financial outcomes in Table 6. The first panel looks at credit behavior. We find something similar to what we found for savings behavior. Personalization and salience on their own reduce the use of credit, in some cases significantly. The combination of both, on the other hand, reverses that response, leading to slightly more use of credit than either intervention on its own. This is a bit surprising since we usually think of credit use as negative savings but this may be due to the fact that credit can be used as a commitment device to save. It could also be due to the fact that personalization shows individuals that they should rely less on credit given their limited means, but once combined with salience, this effect is nullified since salience brings consumption needs “top of mind”. Interestingly, this increased use of credit did not translate into higher rates of default. In panel B, we find no evidence that our interventions significantly affected the number of months where households faced unexpected financial shocks nor the

Table 4. Impact on Actual Savings

	Mean	Saliency	Person.	Both	Agg.
	Savings during intervention period				
Num of months saved	1.12	-0.03 (0.13)	-0.08 (0.13)	0.37** (0.19)	0.26* (0.14)
Amount saved (log)	3.71	0.24 (0.34)	0.40 (0.35)	0.38 (0.50)	1.02*** (0.35)
Saved > 5pct of baseline income	0.26	0.02 (0.03)	0.04 (0.03)	0.01 (0.04)	0.08*** (0.03)
	Savings at endline				
Saved (dummy)	0.37	-0.01 (0.03)	0.01 (0.03)	0.02 (0.04)	0.02 (0.03)
Amount saved (log)	4.49	-0.08 (0.36)	0.25 (0.37)	0.09 (0.52)	0.26 (0.36)
Saved > 5pct of baseline income	0.30	-0.00 (0.03)	0.03 (0.03)	-0.01 (0.04)	0.02 (0.03)
Positive balance (dummy)	0.71	0.05* (0.03)	0.02 (0.03)	-0.03 (0.04)	0.04 (0.03)
Budget balance (log)	6.40	1.43** (0.69)	0.53 (0.71)	-1.25 (1.00)	0.71 (0.71)
Expenses on necessities	12.58	-0.23 (0.23)	-0.32 (0.23)	0.49 (0.33)	-0.06 (0.22)
Discretionary expenses	6.05	0.60* (0.36)	0.11 (0.37)	-0.65 (0.53)	0.07 (0.37)

Table presents the mean in baseline of the variable and the coefficients of regression equation (1) using as outcome variables each variable mentioned. The last column presents the sum of each β and standard errors calculated using the delta-method. Robust standard errors are presented in paranthesis. Controls for the baseline variable of the outcome and for gender, age, educational attainment, marital status, occupations, district, month of baseline survey, risk aversion, financial literacy, number of household members in the household, log baseline income, log baseline expenditures, log baseline balance, log baseline individual and household savings and the number of months with difficulties as reported in the baseline are included. N=1,963 except for those with missing observations due to answers "I don't know": 4 for the amount of target, 11 for amount saved during the intervention and 44 for amount saved last month. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

reliance on savings. In the bottom panel, we use an index of well-being and a self-reported satisfaction with their financial situation. We find no evidence that our intervention changed financial well-being. The coefficients in the last outcome are consistent with the rest of our results but too noisy to be significant. Thus the temporary increase in savings does not appear to have been accompanied by changes in other financial outcomes.

Our only longer-term outcomes (with information from 2020) were not self-reported but

Table 5. Impact on Savings Behavior

	Mean	Saliency	Personalization	Both	Agg. Effect
How did you save during the intervention?					
Spent less	0.17	0.02 (0.02)	0.02 (0.02)	0.02 (0.03)	0.06*** (0.02)
Increased income	0.09	0.01 (0.02)	-0.00 (0.02)	0.02 (0.03)	0.03 (0.02)
Reduced credit costs	0.03	-0.02 (0.01)	-0.00 (0.01)	-0.00 (0.02)	-0.02** (0.01)
Where did you keep your savings?					
At home	0.21	-0.00 (0.02)	0.01 (0.03)	0.06 (0.04)	0.07*** (0.03)
With a friend	0.02	0.00 (0.01)	0.01 (0.01)	-0.01 (0.01)	0.00 (0.01)
In a bank/cooperative	0.07	0.01 (0.02)	0.01 (0.02)	-0.02 (0.02)	0.00 (0.02)

Table presents the mean in baseline of the variable and the coefficients of regression equation (1) using as outcome variables each variable mentioned. The last column presents the sum of each β and standard errors calculated using the delta-method. Robust standard errors are presented in paranthesis. Controls for the baseline variable of the outcome and for gender, age, educational attainment, marital status, occupations, district, month of baseline survey, risk aversion, financial literacy, number of household members in the household, log baseline income, log baseline expenditures, log baseline balance, log baseline individual and household savings and the number of months with difficulties as reported in the baseline are included. N=1,963. * p<0.1, ** p<0.05, *** p<0.01.

rather obtained through the Credit Bureau of Paraguay (Equifax). We present these results in Table 7. We find no evidence of long-term impact in terms of contacts with the financial system as measured by the number of information requests made by financial intermediaries to the Credit Bureau. We also see no change in the credit score of the participants. Finally, while we observe no difference in the probability of having unpaid debt, we do find that the amount of outstanding debt is significantly higher for those who were assigned to the saliency only treatment and that this effect is counteracted when this was combined with personalization. This would thus be consistent with our self-reported results whereby savings were higher for the combined treatment leading them to be able to have lower unpaid debt. While this may be considered a very long-term outcome, we explored the stickiness of indebtedness for a sample of individuals in Paraguay for which we requested the data in 2017 and 2020. We found that unpaid debt in 2017 is an excellent predictor of unpaid debt in 2020 which could explain why a short-term impact on savings could still impact debt levels 3 years later.⁹

⁹Results not presented but available upon request.

Table 6. Impact on other Financial Outcomes

	Mean	Salienc	Person.	Both	Agg. Effect
Panel A: Credit market					
Requested credit	0.36	-0.05*	-0.04	0.06	-0.03
		(0.03)	(0.03)	(0.04)	(0.03)
Total amount approved ('000s)	1,917	-752	-1,015**	1,107*	-660
		(492)	(473)	(585)	(509)
Defaulted	0.02	-0.00	-0.00	0.00	-0.00
		(0.01)	(0.01)	(0.01)	(0.01)
Panel B: Financial resilience					
N. of months with financial problems	1.50	0.14	-0.06	-0.01	0.07
		(0.15)	(0.14)	(0.21)	(0.15)
Thought of using savings next time?	0.31	-0.04	-0.03	0.03	-0.03
		(0.03)	(0.03)	(0.04)	(0.03)
Panel C: Financial well-being					
Index of well-being	0.01	0.04	-0.01	-0.03	0.00
		(0.04)	(0.04)	(0.06)	(0.04)
Satisfaction with financial situation (1-10)	6.32	-0.03	-0.05	0.21	0.13
		(0.14)	(0.14)	(0.20)	(0.14)

Table presents the mean in baseline of the variable and the coefficients of regression equation (1) using as outcome variables each variable mentioned. The last column presents the sum of each β and standard errors calculated using the delta-method. Robust standard errors are presented in paranthesis. Controls for the baseline variable of the outcome and for gender, age, educational attainment, marital status, occupations, district, month of baseline survey, risk aversion, financial literacy, number of household members in the household, log baseline income, log baseline expenditures, log baseline balance, log baseline individual and household savings and the number of months with difficulties as reported in the baseline are included. N=1963 except for "Total amount requested": 1,956 and "Total amount approved": 1,931. Index of well-being is made up from yes or no answers to 4 questions: Do you lose sleep? Do you feel tense? Do you feel nervous? Do you feel things collapsing? * p<0.1, ** p<0.05, *** p<0.01.

4.2 Robustness

We first explore whether attrition may explain our results. We focus on the aggregate effect of the combined treatment compared to the control group since this is the only group for which attrition was shown to be statistically different to that of the control group, as shown in Appendix Table A2. To address these concerns, we estimate coefficient bounds using a strategy first introduced by Lee (2009). Appendix Table A3 shows bounds calculated where we eliminated either the top or the bottom 7 percent of the control group since this was the estimated difference in attrition. The table suggests that our results are not driven by differential attrition. Almost all of the outcomes where we had previously found a statistically significant difference with respect

Table 7. Impact on Credit Bureau outcomes

	N	Mean	Saliency	Person.	Both	Agg.
Requests per 100 days	1935	0.78	-0.03 (0.05)	-0.00 (0.05)	0.07 (0.08)	0.04 (0.05)
Credit score	1935	38.74	0.70 (2.73)	0.81 (2.80)	1.44 (3.92)	2.95 (2.76)
Probability of having unpaid debt	1790	0.38	0.00 (0.03)	0.01 (0.03)	-0.01 (0.05)	0.00 (0.03)
Total unpaid debt ('000s Gs)	1790	2,900	1,872* (979)	544 (675)	-2,068* (1,187)	348 (582)

Table presents the mean in baseline of the variable and the coefficients of regression equation (1) using as outcome variables each variable mentioned. The last column presents the sum of each β and standard errors calculated using the delta-method. Robust standard errors are presented in paranthesis. Controls for the baseline variable of the outcome and for gender, age, educational attainment, marital status, occupations, district, month of baseline survey, risk aversion, financial literacy, number of household members in the household, log baseline income, log baseline expenditures, log baseline balance, log baseline individual and household savings and the number of months with difficulties as reported in the baseline are included. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

to the control group also show results that are statistically different from zero for both bounds.

We next explore whether our results may be driven by participants trying to please surveyors in their responses. While it is always possible that participants answer questions in a way to please the surveyor instead of providing their truthful answer, we do not a priori have a hypothesis why this would be different between each treatment group. We may hypothesize that if anything, salient treatment participants would be more likely to want to report higher savings since they had been “prompted” by the SMS to think that the surveyors like savings. Given that the only treatment where we observe a positive impact on savings is the one that combines personalization and saliency, we would be worried that those individuals were primed to report savings according to their stated goal. In Appendix Table A4, we show that there is no difference between the probability of reporting savings exactly equal to one’s goal between those assigned to the personalization treatment and those assigned to the combined treatment. This suggests that our results are unlikely to be driven by a desire of participants to please the surveyor.

Finally, we consider the fact that some of our savings questions were asked at different moments in the survey. Since a section of our survey asked participants what they remembered about the SMSs they received during the intervention, we were concerned that respondents may have answered differently to savings questions posed before and after this section. Fortunately, we asked individuals a very similar question regarding savings before and after this section. Appendix Table A5 contrasts the responses to both questions. We find no evidence that reported savings were larger when answered after the section on the intervention. The table shows sub-

stantial differences between the two answers but not in a pattern that suggests that individuals increased their reported savings when potentially “primed” by the questions regarding the intervention.

4.3 Channels

Why would the combination of salience and personalization impact savings? Maybe only the personalized SMS were actually read by participants. We first check if participants’ perception of their treatment group was as it was designed. The top panel of Table 8 evaluates how assignment to a given treatment influences what individuals remember about the SMSs they may have received. First of all, very few participants remember having received an SMS about savings in the last 6 months. Only about 25 percent of those who were assigned to receive SMSs remembered receiving them at endline (but so did 12 percent of those NOT assigned to receive them). Being assigned to such treatment raised the probability of remembering receiving the SMSs by about 12 percent, as can be seen in the first line of the top panel of Table 8. Being assigned to just personalization does not have an effect on participants remembering SMSs about savings, as would be expected. More importantly, the combination of personalization and salience does not increase the probability of participants remembering receiving SMSs about savings. This suggests that the impact of the combined treatment was not because it made SMSs more memorable. The next two lines of the panel show the probability that someone remembered the main content of the messages. Participants were able to correctly identify the unique message targeted to their group. Those in the salience only group were more likely to remember receiving a message urging savings in general, a likelihood that decreased for those who received the combination of personalization and salience. On the other hand, those assigned to the combined treatment were 5 percent more likely to remember receiving SMSs urging them to save a specific amount but that was not the case for those assigned to the salience treatment alone. Overall, these results imply that participants interpreted the SMS in the way we intended.

The second panel of Table 8 looks at how participants remembered the baseline exercise. We show that assignment may have led participants to interpret the baseline differentially despite the fact that 99 percent of the intervention was identical across treatments. Being assigned to salience only slightly raises the probability of participants remembering being told to save 10 percent of their income (which was never said). This suggests that salience can make participants think that savings is a rule that they need to follow, without emphasizing savings capacity. Those assigned to personalization were much less likely to remember comparing credit to savings (which was only mentioned to those assigned to personalization treatments). Finally, we see that the interaction between salience and personalization increased the probability of participants remembering many of the messages they actually received at baseline, and this is significant for being told that

savings is important. Thus, there is a chance that part of the effect of combining personalization and salience may be because in the minds of participants, the baseline experience emphasized the importance of savings.

Table 8. Impact of Treatment Assignment on Treatment Perception

	Mean	Salience	Person.	Both
Individual remembers receiving SMS...				
... regarding savings	0.19	0.12*** (0.02)	0.01 (0.02)	0.01 (0.04)
... urging savings in general	0.06	0.07*** (0.02)	0.02 (0.01)	-0.04* (0.02)
... urging saving a specific amount	0.03	0.00 (0.01)	0.01 (0.01)	0.05*** (0.02)
Individual remembers...				
... being surveyed last year	0.93	-0.02 (0.02)	0.00 (0.02)	0.00 (0.02)
... being told saving is important	0.55	-0.02 (0.03)	-0.04 (0.03)	0.12*** (0.04)
... being told about savings potential	0.47	0.02 (0.03)	0.03 (0.03)	0.07 (0.05)
... being told to save 10pct of income	0.49	0.06* (0.03)	0.00 (0.03)	0.03 (0.05)
... comparing credit to savings	0.54	-0.04 (0.03)	-0.09*** (0.03)	0.05 (0.04)

Table presents the mean in baseline of the variable and the coefficients of regression equation (1) using as outcome variables each variable mentioned. The last column presents the sum of each β and standard errors calculated using the delta-method. Robust standard errors are presented in paranthesis. Controls for the baseline variable of the outcome and for gender, age, educational attainment, marital status, occupations, district, month of baseline survey, risk aversion, financial literacy, number of household members in the household, log baseline income, log baseline expenditures, log baseline balance, log baseline individual and household savings and the number of months with difficulties as reported in the baseline are included. N=1963. * p<0.1, ** p<0.05, *** p<0.01.

Next, we ask whether the way personalization and salience are combined matters. To do so, we compare our combined treatment to the two additional treatments we had included that involved both SMS reminders and personalization (the tangible goal and precautionary savings interventions). We present these results in Appendix Table A6. We restrict our sample only to these three treatments and the excluded category is our combined treatment. We find no evidence that the three treatments were statistically significantly different from each other. In general, the tangible goal treatment shows negative differences but the results are too noisy for us to reject

the hypothesis that its impact was the same as the reference group. Overall, our results suggest that what matters is combining personalization and salience, not exactly how this was done.

5 Heterogeneity

We then explore who is responding more to which treatment to shed light on the channels. We first divide the sample by financial sophistication, as shown in Table 9. Our first hypothesis was that personalization would be particularly useful for individuals who are less financially literate. We measured financial literacy by asking three typical questions: value of time, inflation and compound interest. We coded participants as financially literate if they answered at least two out of our three questions correctly, which includes about 45 percent of the sample. In Panel A of Table 9, we distinguish the impact that salience, personalization, and their interaction had on individuals who were financially literate versus those who were not. We restrict ourselves to a few of our main outcomes. We find no evidence that personalization helped individuals who were less literate: the effects have the same sign and are not statistically significant for either type of participant. We find that salience on its own has no effect on those who were not literate and has a very negative impact on those who were more literate, at least in terms of setting savings targets. This would be consistent with the hypothesis of “reactance” since those individuals likely already knew savings is a good thing and the reminder may have led them to feel that their right to choose to save was restricted. It could have also altered their perception of social norms, giving them the sense that most individuals do not save. However, we find that the interaction of salience and personalization is particularly important for those who were more financially literate. This suggests that combining salience and personalization particularly helps those who were more sophisticated before our intervention. This would imply that “reactance” is significantly reduced when one is reminded of their own savings goal. In that case, being reminded of one’s own intention is very different than being given general advice. This would be more the case for literate individuals who understand the importance of savings from the beginning.

We next define financial sophistication as having positive savings at baseline. The first four columns of Panel B of Table 9 show the results for individuals who were unable to save at baseline while the last four focus on the third who had positive savings. We find a very similar pattern to the one of the top panel. Personalization does not seem to differentially impact either group, but salience on its own has a negative impact on those who saved at baseline, an effect that is counteracted for those who combined salience with personalization. This emphasizes that just priming individuals to save may be detrimental, in particular for individuals who were already motivated to save on their own. Once this is combined with a personalized goal, savings are less

negatively impacted as the individual may be motivated to increase their levels of savings and not just reminded that they were doing something that they are told they should do.

The above results could be simply driven by the fact that individuals who were able to save at baseline were in a better financial situation at that moment. However, the results presented in Appendix Table A7 suggest otherwise. When we separate our sample between those who had a positive and a negative balance at baseline, we find much more muted evidence that salience may have been more detrimental for those who had a positive balance. However, we continue to find that the combination of salience and personalization mostly helped those who had a positive balance at baseline. This is logical if the savings goal was made based on the balance, being reminded of a larger goal could improve savings intentions and savings behavior.¹⁰

Table 10 divides individuals who had more coherent budget management and those who were struggling. We start by dividing the sample between those who saved a similar amount as their budget balance and those who saved less than that balance. We see little impact of salience here, but personalization, on its own, negatively impacts individuals with large discrepancies between their savings and their budget balance. This suggests that being shown that in the past you have been unable to translate budgetary surpluses into savings reduces your savings behavior. Thus, far from helping individuals see that they could do more with their resources, personalized goal-setting appears to highlight people's difficulty with saving and make them less likely to persevere in their savings objectives. This is partially undone for those whose personalization was combined with salience showing that bringing savings to the "top of mind" with a specific goal may particularly help individuals who would otherwise be discouraged. This pattern appears to continue even in the long-run for that group, although only significantly so for discretionary expenditures.

In Panel B, we contrast the impact on participants who took out a loan the year before to those who did not at baseline. Those who took out a loan last year can be thought of as those who saved negatively or whose budgetary capacity was particularly limited. Again we find that it is for these individuals that personalization played a negative role. This suggests that highlighting the difficulty that they would have faced had they not been able to access credit the previous year would discourage them from engaging in a savings plan. While the combination of salience and personalization helped both groups in similar magnitudes, the impact is only statistically significant for those who had not received credit in the previous year. Furthermore, the positive impact of the combination on short-term outcomes is only visible for those who had not taken out a loan.

Overall, we find that these results are consistent with a story where salience on its own neg-

¹⁰A similar pattern was found in Clark et al. (2020) who found students who set more ambitious goals were more likely to follow-through.

actively influences the savings behavior of individuals who are more financially sophisticated, indicating that one should be careful when bringing savings to the top of people's minds. Personalization appears to have lowered savings objectives and behavior for groups that were told that they were not capable in the past to translate their budgetary surplus into savings and for those who had to rely on credit. This suggests that savings may not be limited by the knowledge of one's balance but more by beliefs that one will be unable to achieve savings objectives. Finally, we observe a consistent pattern where the combination of salience and personalization is in part able to revert these problems for individuals who are more financially sophisticated, those who did not have to rely on credit, and those who were shown that they had the capacity to save but who had been unable to do so. This provides evidence that the population that needs to be reminded of a given savings objective is far from being universal but rather specific to individuals who can benefit from such "nudge".

Table 9. Impact on Savings by Financial Sophistication at Baseline

	Not sophisticated				Sophisticated			
	Sal.	Per.	SxP	Agg.	Sal.	Per.	SxP	Agg.
Panel A: By financial literacy								
Has a savings target	0.00	-0.05	0.07	0.02	-0.09**	-0.06	0.16**	0.02
	(0.04)	(0.04)	(0.06)	(0.04)	(0.04)	(0.04)	(0.06)	(0.05)
Target amount (log)	0.02	-0.67	0.90	0.24	-1.17**	-0.81	2.12**	0.14
	(0.51)	(0.49)	(0.73)	(0.52)	(0.58)	(0.58)	(0.83)	(0.58)
Target > 5% of inc.	-0.00	-0.05	0.06	0.01	-0.06	-0.05	0.13**	0.03
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.06)	(0.04)
# Months saved	0.05	-0.12	0.08	0.01	-0.13	-0.03	0.71**	0.54***
	(0.17)	(0.17)	(0.24)	(0.17)	(0.21)	(0.21)	(0.30)	(0.22)
Amount saved (SR)	0.27	0.17	0.14	0.57	0.19	0.68	0.67	1.54***
	(0.46)	(0.46)	(0.67)	(0.47)	(0.52)	(0.53)	(0.77)	(0.52)
Amount saved (LR)	0.39	0.70	-0.76	0.33	-0.65	-0.32	1.16	0.19
	(0.48)	(0.49)	(0.69)	(0.49)	(0.54)	(0.55)	(0.80)	(0.55)
Discr. exp.	0.75	0.15	-0.07	0.83*	0.45	0.07	-1.32*	-0.80
	(0.49)	(0.50)	(0.71)	(0.50)	(0.55)	(0.56)	(0.78)	(0.55)
Panel B: By Savings behavior at baseline								
Has a savings target	0.03	-0.04	0.03	0.02	-0.17***	-0.08	0.26***	0.01
	(0.04)	(0.03)	(0.05)	(0.04)	(0.05)	(0.06)	(0.08)	(0.06)
Target amount (log)	0.31	-0.57	0.41	0.15	-2.25***	-0.98	3.46***	0.23
	(0.45)	(0.43)	(0.64)	(0.45)	(0.70)	(0.74)	(1.05)	(0.76)
Target > 5% of inc.	0.03	-0.04	0.01	0.00	-0.14***	-0.06	0.25***	0.05
	(0.03)	(0.03)	(0.05)	(0.03)	(0.05)	(0.06)	(0.08)	(0.06)
# Months saved	0.04	-0.09	0.10	0.05	-0.19	-0.11	1.02**	0.72***
	(0.14)	(0.14)	(0.20)	(0.15)	(0.29)	(0.30)	(0.42)	(0.31)
Amount saved (SR)	0.55	0.36	-0.21	0.71*	-0.45	0.48	1.68*	1.72***
	(0.39)	(0.38)	(0.57)	(0.40)	(0.69)	(0.73)	(1.02)	(0.72)
Amount saved (LR)	-0.01	0.52	-0.18	0.33	-0.22	-0.36	0.72	0.13
	(0.41)	(0.43)	(0.61)	(0.42)	(0.70)	(0.72)	(1.02)	(0.72)
Discr. exp.	0.19	0.11	-0.52	-0.22	1.48**	0.04	-0.79	0.73
	(0.44)	(0.45)	(0.64)	(0.44)	(0.65)	(0.67)	(0.94)	(0.67)

Table presents the coefficients of regression equation (1) using as outcome variables each variable mentioned and the sum of each β and standard errors calculated using the delta-method. The first four columns correspond to individuals who are not financially literate (in the first panel) or who did not save at baseline (second panel). The opposite is true for the last four columns. Robust standard errors are presented in paranthesis. Controls for the baseline variable of the outcome and for gender, age, educational attainment, marital status, occupations, district, month of baseline survey, risk aversion, financial literacy, number of household members in the household, log baseline income, log baseline expenditures, log baseline balance, log baseline individual and household savings and the number of months with difficulties as reported in the baseline are included. N=1,070 for the first four columns of the first panel, N=1,353 for the second panel, N=893 for the last four columns of the first panel, N=610 for the second. * p<0.1, ** p<0.05, *** p<0.01.

Table 10. Impact on Savings by Budget Management Coherence

	Less coherent				More coherent			
	Sal.	Per.	SxP	Agg.	Sal.	Per.	SxP	Agg.
Panel A: By discrepancy between savings and budget balance								
Has a savings target	-0.07 (0.04)	-0.07* (0.04)	0.15** (0.06)	0.01 (0.04)	-0.01 (0.04)	-0.04 (0.04)	0.07 (0.06)	0.03 (0.04)
Target amount (log)	-0.89 (0.55)	-0.95* (0.52)	2.00** (0.78)	0.17 (0.57)	-0.13 (0.53)	-0.51 (0.54)	0.86 (0.77)	0.23 (0.54)
Target > 5% of inc.	-0.07 (0.04)	-0.06 (0.04)	0.13** (0.06)	-0.00 (0.04)	0.01 (0.04)	-0.03 (0.04)	0.05 (0.06)	0.04 (0.04)
# Months saved	-0.06 (0.20)	-0.21 (0.19)	0.48* (0.28)	0.22 (0.21)	0.00 (0.17)	0.04 (0.18)	0.25 (0.26)	0.30 (0.19)
Amount saved (SR)	0.01 (0.51)	0.11 (0.50)	0.83 (0.74)	0.95* (0.53)	0.46 (0.46)	0.70 (0.48)	-0.07 (0.69)	1.09** (0.47)
Amount saved (LR)	-0.24 (0.52)	-0.15 (0.53)	0.76 (0.77)	0.37 (0.55)	0.10 (0.49)	0.67 (0.51)	-0.58 (0.71)	0.19 (0.49)
Discr. exp.	1.33** (0.54)	0.72 (0.54)	-1.27* (0.77)	0.79 (0.54)	-0.15 (0.49)	-0.47 (0.52)	0.00 (0.73)	-0.62 (0.51)
Panel B: By whether took a loan year before baseline								
Has a savings target	-0.06 (0.06)	-0.15*** (0.06)	0.12 (0.08)	-0.09 (0.06)	-0.03 (0.03)	-0.02 (0.03)	0.11** (0.05)	0.06* (0.04)
Target amount (log)	-0.68 (0.76)	-2.00*** (0.71)	1.60 (1.06)	-1.08 (0.75)	-0.44 (0.44)	-0.27 (0.44)	1.39** (0.64)	0.68 (0.46)
Target > 5% of inc.	-0.01 (0.06)	-0.12** (0.05)	0.09 (0.08)	-0.05 (0.06)	-0.03 (0.03)	-0.02 (0.03)	0.09* (0.05)	0.04 (0.03)
# Months saved	0.30 (0.29)	-0.14 (0.26)	-0.05 (0.39)	0.11 (0.27)	-0.14 (0.15)	-0.06 (0.15)	0.52** (0.22)	0.32** (0.16)
Amount saved (SR)	0.86 (0.69)	0.68 (0.70)	-0.68 (1.01)	0.86 (0.67)	0.02 (0.40)	0.31 (0.40)	0.75 (0.59)	1.08*** (0.41)
Amount saved (LR)	0.05 (0.69)	0.23 (0.71)	-0.35 (1.02)	-0.06 (0.68)	-0.13 (0.42)	0.24 (0.43)	0.28 (0.61)	0.39 (0.43)
Discr. exp.	0.72 (0.71)	0.64 (0.72)	-0.95 (1.04)	0.40 (0.73)	0.46 (0.43)	-0.01 (0.44)	-0.60 (0.62)	-0.15 (0.44)

Table presents the coefficients of regression equation (1) using as outcome variables each variable mentioned and the sum of each β and standard errors calculated using the delta-method. The first four columns correspond to individuals who are large discrepancy between their budget balance and savings (in the first panel) or those who had taken a loan in the year previous to the baseline (second panel). The opposite is true for the last four columns. Robust standard errors are presented in paranthesis. Controls for the baseline variable of the outcome and for gender, age, educational attainment, marital status, occupations, district, month of baseline survey, risk aversion, financial literacy, number of household members in the household, log baseline income, log baseline expenditures, log baseline balance, log baseline individual and household savings and the number of months with difficulties as reported in the baseline are included. N=967 for the first four columns of the first panel, N=527 for the second panel, N=996 for the last four columns of the first panel, N=1,436 for the second. * p<0.1, ** p<0.05, *** p<0.01.

6 Conclusions

In this paper, we conducted an experiment to disentangle the role played by SMS reminders in altering savings behavior among underbanked individuals. We aimed to determine what is most effective: receiving generic SMS reminders to bring savings "top of mind" (salience), personalized reminders, or a combination of both? We find that personalization on its own seems to do little to increase savings. It may even decrease savings on average, particularly for individuals who were shown during the baseline exercise that they were unable to save in the past despite having a budgetary surplus. Salience seems to have a negative impact for more sophisticated individuals. This may explain why recent studies such as [Banerjee et al. \(2018\)](#) find that SMS reminders have negative impacts on savings rates in more financially developed markets such as Chile. We find that combining personalization and salience is key to increasing both savings target and short-run savings, particularly for those more sophisticated individuals. Overall, impacts are short-lived and through informal savings mechanisms.

Our findings are relevant when thinking about designing savings products for the poor. It appears that interventions may need to combine low-cost reminders with some way of collecting the information required to personalize the messages. This may raise the cost of such interventions but appears to significantly increase their effectiveness.

It also looks like an intervention only focused on salience may be able to generate less "optimal" savings in this group, as such intervention had a stronger impact on individuals who had limited financial knowledge and negative balances at the time of the baseline survey. This suggests that a widespread SMS strategy may actually lead some households who would rather not save to increase their efforts. This is an area that should be explored further.

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A Appendix

Table A1. Minimum detectable effects by outcome

Variables	Salience		Personalization		SxP	
	N	MDE	N	MDE	N	MDE
Num of months saved	1023	0.236	1006	0.220	996	0.291
Amount saved (log)	1007	0.068	990	0.064	981	0.086
Saved > 5pct of baseline income	1016	0.237	1001	0.220	993	0.292
Saved (dummy)	1007	0.066	990	0.061	981	0.081
Positive balance (dummy)	1023	0.236	1006	0.220	996	0.291
Budget balance (log)	1023	0.236	1006	0.220	996	0.291
Expenses on necessities	1007	0.052	987	0.048	973	0.064
Discretionary expenses	1018	0.051	998	0.048	988	0.064
Requested credit	1023	0.057	1006	0.053	996	0.070
Total amount approved (thousands)	1002	0.067	990	0.062	979	0.083
Defaulted	1023	0.002	1006	0.002	996	0.003
N. of months with financial problems	1023	0.016	1006	0.015	996	0.020
Thought of using savings next time?	1023	0.036	1006	0.034	996	0.045
Index of well being	1023	0.113	1006	0.105	996	0.139
Satisfaction with financial situation (1-10)	1023	0.074	1006	0.069	996	0.092

Table A2. Attrition test

	Mean	Salience	Person.	Both	Agg.
Attrition (dummy)	0.27	0.01	0.00	0.05	0.07***
		(0.02)	(0.02)	(0.03)	(0.02)

Table presents the mean in baseline of attrition and the coefficients of regression equation (1) using as outcome variables attrition in the endline. The last column presents the sum of each β and standard errors calculated using the delta-method. Robust standard errors are presented in paranthesis. Controls for gender, age, educational attainment, labor force, occupations and district are included. N=2,697. * p<0.1, ** p<0.05, *** p<0.01.

Table A3. Bounding aggregate effects due to attrition

	Agg. Effect	Upper bound	Lower bound
		Savings target	
Had a savings target last month	0.02 (0.03)	0.06** (0.03)	-0.01 (0.03)
Amount of savings target (log)	0.19 (0.39)	0.92*** (0.39)	-0.07 (0.40)
Target > 5pct of baseline income	0.02 (0.03)	0.07*** (0.03)	-0.00 (0.03)
		Savings during intervention	
Num of months saved	0.26* (0.14)	0.72*** (0.12)	0.19 (0.14)
Amount saved (log)	1.02*** (0.35)	1.48*** (0.35)	0.77** (0.36)
Saved > 5pct of baseline income	0.08*** (0.03)	0.11*** (0.03)	0.06** (0.03)
		Savings at endline	
Saved (dummy)	0.02 (0.03)	0.07** (0.03)	0.00 (0.03)
Amount saved (log)	0.26 (0.36)	0.66* (0.36)	-0.06 (0.38)
Saved > 5pct of baseline income	0.02 (0.03)	0.06** (0.03)	0.00 (0.03)
Positive balance (dummy)	0.04 (0.03)	0.07** (0.03)	-0.02 (0.03)
Budget balance (log)	0.71 (0.71)	1.17 (0.72)	-0.78 (0.69)
Expenses on necessities	-0.10 (0.21)	0.04 (0.22)	-0.85*** (0.16)
Discr. expenses	-0.02 (0.38)	0.53 (0.38)	-0.41 (0.38)

This table reports the sum of the three β with standard errors calculated using the delta-method using the full sample (first column) and excluding the 7 percent highest (respectively lowest) reported outcome values amongst those assigned to the control group (second column, respectively third column).

Table A4. Testing for pleasing surveyor

Treatment	Personalization		Salience*Personalization		Difference	
	Prob.	N	Prob.	N	Prob.	z -value
Goal=Savings	.199 (.018)	468	.233 (.020)	459	-.034 (.027)	1.273

Table presents the number of individuals assigned to either personalized treatment (first two columns) and personalized and salient treatment (third and fourth columns) who had provided a goal and who reported the amount of savings in the endline as well as the probability that they reported saving exactly their goal amount. The last two columns report the difference in probability and the z-value of that test, which indicates we cannot reject both groups had the same probability of responding a savings amount that was the same as the goal.

Table A5. Testing for framing

Saved during past 6 months (measured after questions about intervention)	Saved last month (measured before questions about intervention)		Total
	No	Yes	
No	1,526	260	1,785
Yes	285	810	1,095
Total	1,811	1,070	2,881

Table presents the frequency of responses to two different measures of savings. McNemar's $\chi^2(1) = 1.15$, which implies we cannot reject the null hypothesis of symmetric table.

Table A6. Comparing the way personalization and salience are combined

	Tangible goal	Precautionary savings
		Savings target
Had a savings target last month	-0.00 (0.03)	0.01 (0.03)
Amount of savings target (log)	-0.00 (0.41)	0.10 (0.40)
Target > 5pct of baseline income	-0.01 (0.03)	-0.01 (0.03)
		Savings during intervention period
Num of months saved	-0.23 (0.14)	0.18 (0.15)
Amount saved (log)	-0.61 (0.38)	0.02 (0.37)
Saved > 5pct of baseline income	-0.04 (0.03)	-0.03 (0.03)
		Savings at endline
Saved (dummy)	-0.01 (0.03)	0.02 (0.03)
Amount saved (log)	-0.10 (0.38)	0.27 (0.38)
Saved > 5pct of baseline income	0.00 (0.03)	0.01 (0.03)
Positive balance (dummy)	0.01 (0.03)	0.00 (0.03)
Budget balance (log)	0.42 (0.72)	0.09 (0.71)
Expenses on necessities	0.08 (0.22)	-0.07 (0.23)
Discretionary expenses	0.31 (0.38)	0.39 (0.38)

Table presents the comparison of being assigned to the tangible goal or the precautionary savings goal compared to the personalized and salient treatment. Robust standard errors are presented in parenthesis. Controls for the baseline variable of the outcome and for gender, age, educational attainment, marital status, occupations, district, month of baseline survey, risk aversion, financial literacy, number of household members in the household, log baseline income, log baseline expenditures, log baseline balance, log baseline individual and household savings and the number of months with difficulties as reported in the baseline are included. N=1,427 except those with missing observations due to people answering "I don't know": 6 people for the amount of the target, 6 people for amount saved during the intervention and 21 for amount saved last month.

Table A7. Impact on savings by budget balance at baseline

	Negative balance				Positive balance			
	Sal.	Per.	SxP	Agg.	Sal.	Per.	SxP	Agg.
Has a savings target	0.01 (0.06)	-0.07 (0.07)	0.05 (0.10)	-0.00 (0.07)	-0.05 (0.03)	-0.05 (0.03)	0.13*** (0.05)	0.03 (0.03)
Target amount (log)	0.18 (0.82)	-1.05 (0.86)	0.72 (1.22)	-0.16 (0.84)	-0.68 (0.43)	-0.67 (0.42)	1.66*** (0.62)	0.31 (0.44)
Target > 5% of inc.	0.06 (0.06)	-0.06 (0.06)	0.02 (0.09)	0.02 (0.06)	-0.05 (0.03)	-0.05 (0.03)	0.11** (0.05)	0.02 (0.03)
# Months saved	-0.13 (0.27)	-0.11 (0.31)	0.28 (0.40)	0.04 (0.28)	0.00 (0.15)	-0.07 (0.15)	0.39* (0.22)	0.32** (0.16)
Amount saved (SR)	0.68 (0.68)	0.45 (0.71)	0.32 (1.05)	1.45** (0.72)	0.13 (0.40)	0.38 (0.40)	0.41 (0.58)	0.92** (0.40)
Amount saved (LR)	0.49 (0.71)	0.01 (0.76)	-0.41 (1.06)	0.09 (0.72)	-0.21 (0.41)	0.29 (0.42)	0.26 (0.60)	0.33 (0.42)
Discr. exp.	0.05 (0.74)	-0.53 (0.81)	0.56 (1.12)	0.08 (0.77)	0.72* (0.42)	0.28 (0.42)	-0.94 (0.60)	0.05 (0.42)

Table presents the coefficients of regression equation (1) using as outcome variables each variable mentioned and the sum of each β and standard errors calculated using the delta-method. The first four columns correspond to individuals who had a negative balance in the baseline. The opposite is true for the last four columns. Robust standard errors are presented in paranthesis. Controls for the baseline variable of the outcome and for gender, age, educational attainment, marital status, occupations, district, month of baseline survey, risk aversion, financial literacy, number of household members in the household, log baseline income, log baseline expenditures, log baseline balance, log baseline individual and household savings and the number of months with difficulties as reported in the baseline are included. N=403 for the first four columns of the first panel, N=1,560 for the second panel.* p<0.1, ** p<0.05, *** p<0.01.