Journal of Economic Behavior and Organization 222 (2024) 25–63

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Research Paper

Personalizing or reminding? How to better incentivize savings among underbanked individuals ☆

Viviane Azevedo [a], Jeanne Lafortune [b,c,*], Liliana Olarte [d,1], José Tessada [b,*]

[a] IDB Invest, United States of America
[b] Pontificia Universidad Catolica de Chile, Chile
[c] J-PAL, Chile
[d] IFC, United States of America

ARTICLE INFO

Dataset link: https://github.com/jeannelafortune/Paraguay.experiment

JEL classification:
D14
D91
O16

ABSTRACT

We use a large-scale randomized control trial to evaluate information interventions to motivate savings in a mostly unbanked population. We find positive impacts of personalized SMSs on savings during the messaging period and larger than existing studies. However, they fade away after messaging stops, like in other experiments. Compared to the existing literature, we can better identify the sources of that effect. Personalized SMSs are not more likely to be received or more memorable. Highlighting a specific monetary goal appears to be more effective than tangibility. Also, salience or personalization separately do little to increase the intensive margin of savings. Heterogeneous effects are consistent with limited attention co-existing with other behavioral biases; for groups where reactance and frustration play a role, those are overcome only when personalized SMSs are employed.

1. Introduction

Evidence suggests saving money is 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 explored the role mechanisms simpler than commitment devices, such as text messages (SMS reminders), can play in stimulating savings. Despite the prevalent presumption

☆ This study was approved by the IRB of Pontificia Universidad Catolica de Chile. We thank Constanza Palacios and Angélica Eguiguren as well as the entire team of E+E for great field implementation. We thank Bernardo de Moura, Katia Everke and Rubén Jara for excellent research assistance. We are grateful to Jorge Agüero, Irani Arraíz and participants at the First LACEA BRAIN Annual Conference, PACDEV 2019, IPA Financial Inclusion Conference 2019, Ridge May Forum Impact Evaluation Network, LACEA/LAMES 2019 conference, Behavioral Workshop 2019 at Universidad Los Andes, Chile. Tessada acknowledges funding from ANID FONDECYT Regular 1191933 and Lafortune from ANID FONDECYT Regular 1210772. The opinions expressed in this publication reflect the views of its authors and not necessarily those of the Inter-American Development Bank Group, the World Bank Group, their respective Board of Directors, or the countries they represent. The usual disclaimers apply. No potential conflict of interest was reported by the authors.

* Corresponding authors.
E-mail addresses: vivianea@iadb.org (V. Azevedo), jlafortune@uc.cl (J. Lafortune), nlolarte@ifc.org (L. Olarte), jtessada@uc.cl (J. Tessada).

[1] This study was initiated while the author worked at IDB Invest.

https://doi.org/10.1016/j.jebo.2024.04.005
Received 22 March 2023; Received in revised form 27 February 2024; Accepted 3 April 2024

Available online 17 April 2024
0167-2681/© 2024 Elsevier B.V. All rights reserved.

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.

that SMSs may not be "strong" enough to change savings behavior, a few existing studies have found significant effects. 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 show weaker but suggestive evidence that reminders also increase savings amounts. Why are SMS likely to increase savings and can they do so in an environment where the participants have limited links with the financial sector and have not signed up for a savings product? We study this in the context of Paraguay, a country where financial services are limited.

While SMS savings intervention may be effective because of "salience", i.e. bringing savings "top of mind" through SMS reminders, they also tend to be multifaceted. Many interventions help individuals set a specific savings goal (or are linked to a financial product that has such an intrinsic objective) and craft personalized messages based on this goal. In many contexts, SMSs are also sent at the moment where an account or a new financial product was being opened. Furthermore, not all SMS studies on savings have found positive impacts. Banerjee et al. (2018) find that sending monthly text messages that remind individuals of their motivation to save as expressed in the initial assessment did not lead to increased savings. Similarly, Attanasio et al. (2019) find that sending non-personalized SMSs did not lead to increased savings, and in some cases, decreased the probability to save and lowered savings in bank accounts. By unpacking the ingredients of SMSs, we may be able to explain why they work in some contexts and not in others. What exact features of the SMS interventions are delivering the positive results and what exact behavioral barriers are being solved or reduced? Is it just that SMS reminders bring savings to the "top of mind" for recipients? Or are they helping at solving a number of behavioral biases simultaneously, including limited attention?

We propose to better understand the behavioral biases that may lead to under savings and identify the specific mechanisms that may lead to SMS influencing savings for a population that has not registered for a financial product with a bank or similar institution. To do so, we use tablet-based interventions for poor to middle income individuals in Paraguay through a randomized control trial.[2] We can check if our base results replicate those of previous studies and at the same time distinguish the effects between "salience" and the "personalization" of the information and also about the way these may be combined. The experiment is based on the scheme shown in Table 1.

Furthermore, we are able to see if effects are long-lasting depending on the type of intervention a person receives. We focus on the role of SMS alone, without the intermediation of a financial institution.

Theoretically, if individuals do not save for unforeseeable future events because of present-bias, reminding them that saving is important could help them uphold their past savings commitments (as in Akbas et al., 2016). This is the hypothesis presented by Karlan et al. (2016) to explain why they observed positive impacts of personalized SMS. However, the SMS reminders may also alter perceived social norms (one's perception of 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., 2024; Dillard and Shen, 2005). Following the research on "rational inattention" (Caplin and Dean, 2015; Caplin et al., 2020; Maćkowiak et al., 2023), 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., 2023).

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; Stango and Zinman, 2009). 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). Another paper that used an aspiration intervention (McKenzie et al., 2022) also finds that increasing micro-entrepreneurs' aspirations did not lead them to save more and actually decreased their borrowing, maybe because it increased their frustration. Most of the experiments on SMS use a sample that has set-up a goal previously. By isolating the impact of goal-setting on its own, we can check whether savings may be impeded by this difficulty or whether there may be frustration by participants.

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 make reminders more efficient at reducing present-bias, and adding reminders to goal-setting could make it easier for people to stick to savings goals.

[2] For 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. In our sample, only around 15 percent had a savings account in a financial institution and amongst those who had asked for a loan in the last year, only 9 to 13 percent had done so to a bank or bank-like institution, the rest using informal sources.

We aim to contrast the role of salience versus individualized information and their combination in interventions that incentivize savings.[3] We first designed a face-to-face baseline survey that included eliciting a very detailed budget balance and a list 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 a group. 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 interaction. 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 fourth group combined both salience and personalization whereby they were helped to set up a savings goal and then reminded of that goal through SMSs. We also included two different alternatives about how salience and personalization could be combined. In a fifth group, participants were asked to choose a tangible goal to save towards (e.g., a TV), and in the sixth group, we used the variability of income faced by individuals in the previous year to encourage them to set a savings goal to cover unexpected shocks. Individuals in both, the sixth and the fifth groups, received personalized SMS reminders.

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 monthly messages 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 2,925 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.[4] We first measure the impact on savings behavior during the period of messaging. We then look at savings objectives as well as actual savings at the moment of the endline survey. We also explore measures of income, spending, budget balances, credit behavior, exposures to shocks, and reported well-being. While many previous experiments on savings partnered with financial institutions and measured savings within that entity, we use self-reported savings. Given that only about 16% of the adult population in Latin America and the Caribbean report saving through a financial institution (Cavallo et al., 2016), only 6% do so in Paraguay (Demirgüç-Kunt et al., 2017), which is similar to what we find in our sample, we may capture a more relevant fraction of savings through this method, despite potential challenges linked to self-reporting. In addition, central data agglomerators, like credit bureaus in the country, do not capture savings information from several institutions. Finally, we also obtained data about the participants regarding their credit behavior from the credit bureau in Paraguay to study whether these self-reported savings behaviors also modified their relationship with the credit market.

We study in detail the role of behavioral biases through 3 different strategies. First, our empirical design allows us to test whether salience or personalization on their own play a role. We also can contrast three different ways in which personalized SMSs can be constructed. Secondly, we asked participants to report what they remembered of the messaging and of the baseline visit to study what may be the channels that make personalized SMSs more effective. Finally, we also explore heterogeneity by baseline behaviors to study whether some biases are more or less present in certain groups.

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 amounts among participants while receiving the SMSs and higher savings intentions (in both propensity and amount). The magnitudes we obtain are larger than previous studies which could be due to the way we combined salience and personalization or to the difference in our population. Changes in savings habits were not long-lasting as savings decisions 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. Other financial outcomes (balance budget, self-reported financial well-being, etc.) were not statistically significantly affected by any of our interventions. 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.

Why would only combining salience and personalization increases savings? We first find differences between the way personalized SMSs work for short-run savings. Providing a monetary goal (whether as a generic objective or in the context of precautionary savings) increases the probability to save and the amounts saved during the messaging compared to control while those assigned to the tangible goal did not observe a statistically significant difference compared to that control.[5] This appears to be mostly driven by the fact that most participants had difficulty setting a goal and as such, received a generic SMS. This would first potentially explain why our results are larger than those of Karlan et al. (2016) who reminded participants of non-monetary goals. It also seems to suggest that it is not the lack of tangibility that difficult savings in this group.

Another hypothesis would be that personalizing the SMS simply makes it more memorable or more visible and does not solve any other bias than limited attention. However, we find that sending a personalized SMS does not increase the likelihood that the

[3] This 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.

[4] We can use the ITT since take-up of messaging was similar between the salience only and the combined treatments.

[5] However, we must note that we cannot reject the hypothesis that all 3 groups had similar results, due to low statistical power.

participant remembers receiving it compared to a generic one. It does make that participant remember receiving a message with a particular amount which suggests that the channel through which personalized SMSs operate is really by reminding the individual of their specific goal. We also find that being assigned to the personalized SMS group makes individuals remember more being told about their own savings potential, that savings were important and that savings is better than credit at the moment of the baseline. This would suggest that indicating a particular amount in SMS can make the initial budgeting exercise and the conclusion drawn from it more remarkable to the participants.

Finally, we draw on heterogeneity analysis to measure whether lack of attention is also mixed with other behavioral biases that make personalized SMSs more suitable. We suggest 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., 2024). Combining reminders with personalization significantly decreases this penalty thus suggesting that personalized SMSs, compared to generic ones, do not lead individuals to think that nobody else than them saves or that their capacity to decide has been taken away since it mentions the goal they elected themselves. 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" may not be useful in changing savings behavior without personalization. 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. Offering personalized information requires effort but a digital baseline survey, like ours, could be widely adopted by financial institutions, education organizations, and even governments, thus offering a policy application for our intervention.

This paper is part of a large literature that has tried to foster higher savings. Traditional mechanisms to increase savings have focused on improving access and measured savings in that same vehicle. 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. Dupas and Robinson (2013) and Prina (2015) both find that access to savings accounts increase savings. Other studies like Pomeranz and Kast (2024) find positive impacts on increased savings amounts by improving access. Gertler et al. (2021) 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, excluding the channel of access.

Our paper contributes to the relatively small literature that has tried to understand why reminders may increase savings. Karlan et al. (2016) find that SMSs that mention both savings goals and financial incentives (e.g. specifying the additional interest rate gains for making all the deposits planned) are more effective than SMSs that just show the latter, at least on meeting savings commitments, though not at increasing savings amounts. While this indicates that some SMSs are more useful than others, the goals they mentioned are either a tangible goal (in Peru) which is our weakest treatment or a non-personalized without mention of specific amount in Bolivia. Furthermore, the authors argue that given their confidence intervals, their results are too imprecise to reach a definitive conclusion on the content variation of the SMSs. While these results begin to elucidate the importance of SMSs content and personalization, they are unable to separately identify the impact of only salience or only personalization and their combination.

Our paper also complements the small strand of literature on the effectiveness of savings commitment devices, particularly 'soft' commitments. Within the first group, Karlan et al. (2016) and Beshears et al. (2020) find that products that restrict access to deposits or that impose withdrawal penalties may succeed in increasing savings. In the latter group, Gargano and Rossi (2024) find that individuals who set savings goals increase their deposits, though only a minority meet the actual goal. Authors also find that the specificity of the goal (e.g., saving for a car) is less successful than a generic savings goal. In our experiment, we find that goal setting alone was ineffective at increasing savings, though similar to Gargano and Rossi (2024) we also find that tangible goals did not impact savings behavior.

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 are in line with evidence that personalization might help in improving financial education and planning interventions (as in Attanasio et al., 2019; Fuentes et al., 2024). However, we also find that the impact of personalization alone might be muted if not combined with reminders.

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. The treatment units were approached in their home by the survey team. The research team emphasized that this was a study from the Inter-American Development Bank at baseline. This was identified as a respected institution by our sample. We did not ask them whether they were familiar with SMS but our pilot showed all participants appear to be very familiar with the technology.

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. Given that some individuals lived under their parents' roofs and paid no housing expenses while others were head of the household, we asked the respondent to think about the income they used to satisfy their needs and those of their dependents if they had. 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 individual expenses in the last month, considering only expenses that were covered by their own 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. This was piloted with focus groups and was found to be understood by participants. 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 with equal probability.[6] 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 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 the 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

[6] Because of the use of an interface and a immediate randomization, we could not stratify the randomization.

Table 2
Experimental Design.

Treatment group	# Participants	Message during interview (Personalization)	Message via SMS (Salience) for next 6 months
(1) Control group	712	*No savings goal elicited.*	*No more interaction.*
(2) Salience only	671	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? *(No savings goal elicited)*	Remember to save! With savings, you can buy without asking for credit and you acquire more security to face unexpected expenses.
(3) Personalization only	639	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?	*No more interaction.*
(4) Basic sal. and per.	675	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? 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?	Remember to save! According to your income and expenses, you had established a monthly saving goal of Gs...
(5) Tangible goal	644	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.	Remember to save! In your objective to purchase a ... you had established a monthly savings goal of Gs...
(6) Precautionary savings	587	Savings allows you to face unexpected 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.	Remember to save! To cover your unexpected expenses and possible unforeseen shocks, you had established a monthly saving goal of Gs...

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.

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. 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) and that savings can reduce the exposition to shocks (Pomeranz and Kast, 2024). The amount of unexpected shocks 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 unexpected 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.

Individuals who agreed to receive a SMS would receive one SMS per month. The messages were sent in the first week of the month and were sent on different days of the week. SMS did not include any other text than the one we detailed implying that people were not reminded of why they were receiving the messages. Those messages were coming from the same phone number every month. In our focus group, this group of participants appeared very familiar with SMS.

2.2. Take-up

Our interventions required the participation of individuals invited to the survey. Appendix Table A.1 shows information regarding take-up. First, individuals who were asked to set a goal (groups 3 through 6) needed to be able to do so. In the two treatments that involved the budgeting exercise following the setting of a savings goal, 80.4 percent and 78.2 percent of the individuals, respectively, set up a savings goal. This fraction falls to 54.3 percent for the tangible savings group. It is almost the same as the first two groups (76.7 percent) for the precautionary savings group. This is a first indication that the budgeting exercise may have helped individuals to set up a monetary goal more easily than thinking of a tangible objective. In focus groups, individuals also found that in general, they were not savings for something so clear like going on a trip or paying their children tuition but something a little bit less concrete like "eventually buying a house" or "making improvements to my house" which may have been more difficult to quantify in terms of money. It may indicate that making savings more tangible may not be the main behavioral bias faced by our participants. It could thus be that since individuals who were assigned to the tangible group had more difficulty setting up a goal that this would be a reason why this group would experience fewer benefits from the intervention.

We then ask if the characteristics of individuals who were able to set up a goal differed by their assignment status. We find, in Appendix Table A.2, that individuals who were able to set up a goal were more likely to have higher income, balance and savings in the baseline. These differences were mostly nullified when asked to set a goal and offered the SMS. While this could imply that the group that was assigned to receive a personalized SMS has more take-up for some characteristics than in the group where only personalization was at play, controlling for these characteristics does not appear to influence the results we present below and we thus think that this is not the cause of the patterns we present.

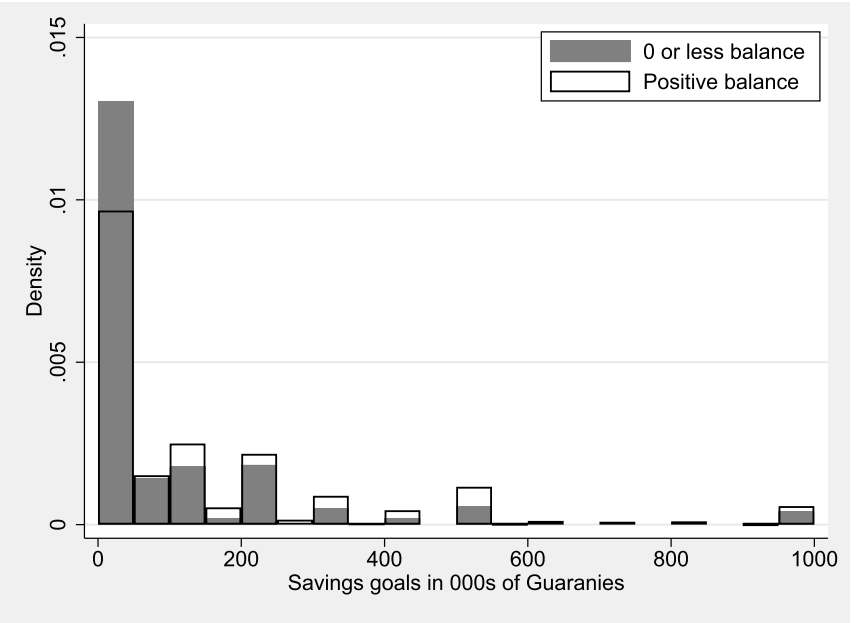
Groups that were assigned to receive messages also had to accept to receive them. The group that had the highest rate of take-up was the one which received the generic message at 74.2 percent. The rates for the other groups are slightly lower in magnitudes (71.3 for SxP, 67.4 for the tangible goal and 70.3 for the precautionary savings) although the difference is not statistically significant. This suggests that it is unlikely that take-up of messages would explain differences across treatment groups.

Appendix Table A.2 explore whether there were differences in those who accepted to receive SMSs depending on the treatment they were assigned to. It details that individuals who accepted the SMSs were similar in all treatment arms. They suggest, however, that those that agreed to receive messages were younger and were more likely to have had financial problems in the last year. They also were more likely to be financially literate.

Finally, individuals who agreed to receive SMS also had to be connected and be able to receive the messages they were supposed to receive. We only faced 11 cases where we were unable to deliver the SMS leading all groups to have more than 99 percent probability of having received at least one message. Out of all the messages that one was supposed to receive, at least 97 percent of them were received. This leads us to believe that is unlikely to be a reason why some individuals may not benefit from the intervention.

2.3. 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



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. $N = 393$ for those with negative balance and $N = 1,608$ for those with positive balance.

Fig. 1. Savings Target by Initial Budget Balance.

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 and attrition rates. We updated the power calculations as detailed in Appendix Table A.3. We found that 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.

2.4. 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. 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)[7] 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. Fig. 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 savings objective. Among participants who did set a goal, the average savings objectives were 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 who were able to set a goal are electing as an objective something that is about a third of their balances.[8] 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.

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

[8] Given that individuals who were able to set a positive goal had higher average balances, this is why 315,000 or 360,000 Gs represents a third of their balances.

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- 1998[9] we were able to confirm that the neighborhoods where we sampled more intensively were populations that had more poverty. It is important to note that although our sample was constructed to have similar characteristics as clients of a formal bank, this population was mostly unbanked (only 10 percent had a formal savings account) and none had signed up for a financial product as has been the case for most other SMS studies.

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 4,028 individuals between the months of July and December of 2016. 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, 50 percent head of household and average years of education were only 6.7. Average reported monthly earnings were around Gs 2.581.000 (USD 460 using market exchange rate or USD 1,045 adjusting for PPP[10]) Their reported average expenses amount to Gs 1.825.000 (USD 320), with an average monthly balance of Gs 757.000 (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: 38 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.

Since our baseline survey was in part conducted during December which may be a special month because of holiday spending, we checked whether there is seasonality in the baseline data in the top 2 panels of Appendix Table A.4. We find, contrary to our hypothesis that income and expenses are larger in the months prior to December than in that particular month. Balances are also higher in October and November than in December. However, once we condition on the controls included in our regressions, we find no evidence of a statistically significant difference between months. This would suggest that more than seasonality, there were differences in the characteristics of the surveyed individuals depending on the month of the survey. Furthermore, we see in Appendix Table A.4 that we are, in general, balanced across treatment arms between each month. Finally, we also run all of our analysis using as controls the months of the baseline survey.

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. This is information that was typically not collected in other SMS experiments and it allows us to study more carefully the channels at play.

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

[9] The latest information we found available.

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

Table 3
Balance.

	C. Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)	Joint F-test	Agg. Effect ($\sum_{i=1}^{3}\beta_i$)
Age	30.80	-0.60	-0.85	0.93	0.77	-0.51
	(10.74)	(0.57)	(0.58)	(0.74)		(0.47)
Women	0.61	-0.01	0.00	-0.00	0.07	-0.00
	(0.49)	(0.03)	(0.03)	(0.03)		(0.02)
Years of education	6.71	0.12	0.04	-0.06	0.43	0.10
	(2.50)	(0.13)	(0.13)	(0.17)		(0.11)
Single	0.54	-0.01	0.00	-0.01	0.17	-0.01
	(0.50)	(0.03)	(0.03)	(0.04)		(0.02)
Head of household	0.50	0.01	-0.01	-0.01	0.21	-0.01
	(0.50)	(0.03)	(0.03)	(0.04)		(0.02)
Worked last year	0.93	0.01	-0.00	-0.01	0.49	0.01
	(0.26)	(0.01)	(0.01)	(0.02)		(0.01)
Total ind. income ('000s)	2,581	147	44.61	-62.33	0.30	129
	(3,035)	(189)	(173)	(259)		(165)
Total ind. expenses ('000s)	1,825	160	-97.80	-51.49	1.71	10.41
	(1,696)	(124)	(85.87)	(142)		(75.22)
Account balance (thousands)	757	11.94	142	-35.88	0.56	118
	(2,421)	(127)	(137)	(196)		(141)
Saves	0.31	-0.00	-0.00	-0.02	0.43	-0.02
	(0.46)	(0.02)	(0.03)	(0.03)		(0.02)
Individual savings (log)	3.72	0.02	-0.01	-0.13	0.17	-0.12
	(5.59)	(0.30)	(0.31)	(0.40)		(0.24)
Had fin. prob. last year...	0.38	0.04	-0.02	-0.01	1.56	0.00
	(0.49)	(0.03)	(0.03)	(0.03)		(0.02)
...because of fall in income	0.26	0.04*	-0.01	-0.04	1.98	-0.00
	(0.44)	(0.02)	(0.02)	(0.03)		(0.02)
Index of well-being	-0.00	0.01	0.02	-0.04	0.25	-0.01
	(0.79)	(0.04)	(0.04)	(0.06)		(0.03)
F-test:		0.76	0.40	0.34		

Table presents the mean in baseline of the variable and the coefficients of regression equation (1) using as outcome variables each baseline characteristic. The fifth column shows the joint F-test of significance for the three β and the last column presents the sum of each β and standard errors calculated using the delta-method. Robust standard errors are presented in parentheses. $N = 4,028$ for all variables except for "Individual savings (log)" ($N = 4,008$ because 20 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? * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The following variables present statistically significant differences between Salience and Personalization: "total ind. income" (5%), "had finan. problems last year" (5%), and "because of fall in income" (5%). For the rest of the variables, Salience and Personalization are not statistically different at usual significance levels.

who had a debt registered before the baseline although our results are similar when including them. This is because we cannot get information on new defaults for those individuals.

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:

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

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 (which involves a personalized SMS) compared to what would have been obtained by having a goal setting session without a reminder or a reminder (generic since no information on goals would be possible) without a goal-setting session, that is to say the impact of personalization and of salience on their own. To obtain the impact of the combined treatment compared to the control, we will simply add all βs 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.[11] In our main specification, we combine all three versions of the personalized and salient treatment. We show, in Appendix B (Figs. B.1–B.3, Tables B.1–B.6), the results when we restrict our sample to the four most comparable treatments. We also will discuss the difference between the three types of personalization of the SMSs.

Because we will wish to study the possible channels through which salience, personalization or their combination could or not impact savings, we will also estimate these heterogenously. In that case, we estimate only one equation where we add to Equation (1) a dummy for being in the group and then we interact each treatment dummy with an indicator variable for belonging to group 1 or 2, excluding the main effect.

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 probability of having faced a financial problem last year because of a fall in income that is slightly higher for those assigned to the salient treatment compared to control.[12] We cannot reject the hypothesis that the coefficients are jointly zero for any of the treatments. We also cannot reject the hypothesis that all three coefficients are 0 for all outcomes, except individual expenses where it is marginally so. Overall, this suggests that our randomization appears to have generated groups of similar ex-ante individuals to be assigned to each treatment.

The last three types of messages are combined in the above table. We see, in Appendix Table A.5, that the sample is also balanced between the six groups individually. The joint-tests of significance are always unable to reject the hypothesis that all coefficients are 0. We see two marginally statistically significant coefficients whereby individuals in the tangible group have slightly more years of education than the control group and the precautionary savings group a slightly lower probability of having saved the previous year. Those two variables are included as control variables in the estimations.

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 4,028 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.[13] 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 2,925 individuals, implying an attrition rate of about 27 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. We find evidence that those who were less finally literate and those who had taken a loan in the previous year were more likely to respond to our endline survey: in one case, this would lead us to underestimate the impact of the intervention compared to the full sample while in the other, it would provide an overestimate. It is thus difficult to determine exactly what would be the impact of the combined intervention on the full sample. However, our estimate will remain unbiased for that sample as long as our treatments are not correlated with attrition. Appendix Table A.6 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. Finally, we also show that our sample continues to be balanced even taking into account attrition, as shown in Appendix Table A.7 when we combine all salience and personalization groups or when we treat them separately, as in Appendix Table A.8.

4. Results

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

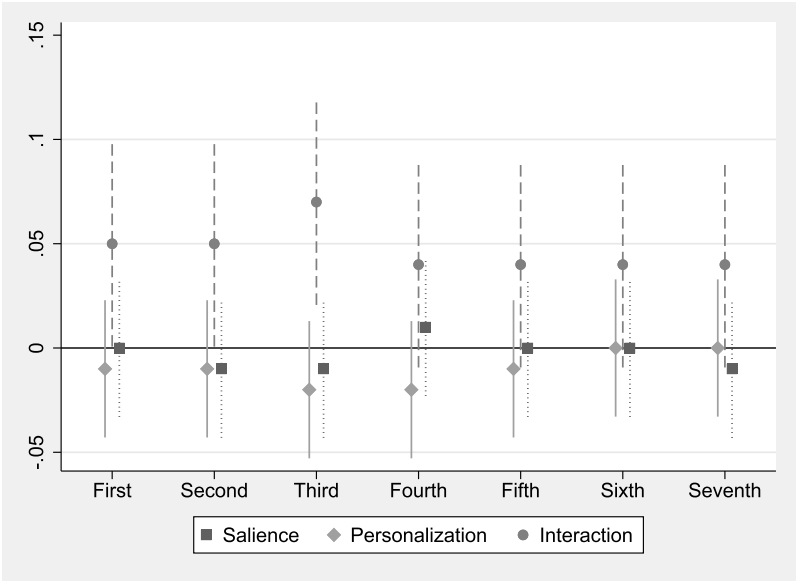
4.1. Savings during messaging

We first look at whether our intervention influenced savings during the receipt of SMSs. In the endline survey, we asked whether individuals saved (any amount) during the seven months after the baseline survey. This would thus correspond to the months where they were receiving the SMSs if they were assigned to do so. We first display these results graphically in Fig. 2 where the coefficients of β_1 (as squares), β_2 (as diamonds), β_3 (as circles) for each month after the baseline survey (and thus during the six months of the messaging). Personalization and salience on their own do not influence the savings behavior right after the baseline survey. Coefficients for personalization and salience on their own are small, very often negative and not statistically significant. But participants assigned to combined treatments have a statistically significantly larger probability of saving during almost all months than those assigned to only personalization or salience as shown by the fact that the interaction coefficient is positive and significant. We observe that this group was about 5% more likely to save in each month than those who were only assigned to either personalization or salience on their own. This is a bit lower than the effect reported by Pomeranz and Kast (2024). This finding is summarized in the first row of Table 4. In that table, coefficients β_1, β_2 and β_3 are presented in the second, third and fourth columns

[11] We explored the possibility of allowing standard errors to be correlated across individuals living in the same neighborhood. Results are almost identical to the ones presented here.

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

[13] We 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.



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. $N = 2,925$.

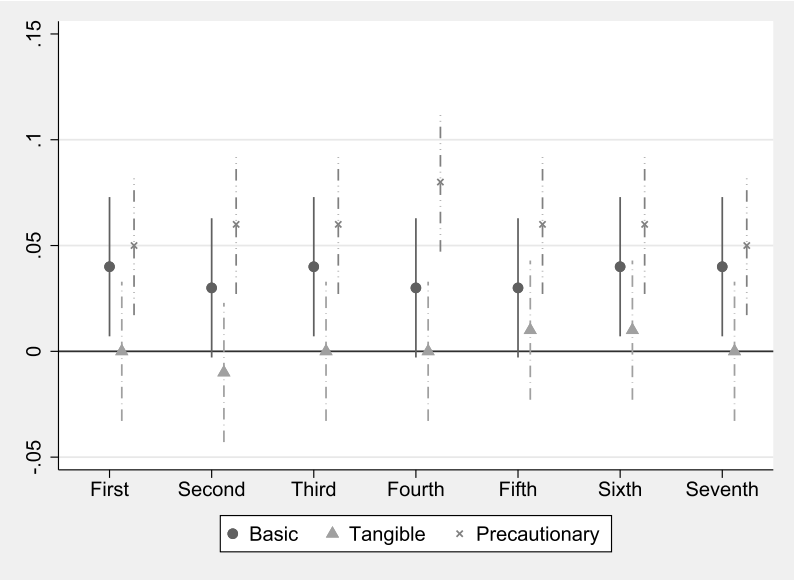
Fig. 2. Impact of Salience, Personalization and Their Combination on Whether Individuals Saved During Each Month of Intervention.

respectively while the sum of the three is shown in the fifth. In that table, we observe that the combined group saved in 0.3 more months compared to the control group and this difference is statistically significant. On the extensive margin during the period of the SMSs, we thus see that what matters is combining personalization and salience. Since our estimation equation assumes that the added benefit of personalization to the group that experiences salience is the same as the added benefit of salience to the group that receives personalization, we relax this assumption by estimating the effect of the combined treatment compared to that of only personalization and only salience in a restricted sample in Appendix Table A.10 and find very similar results. For that reason, we employ the main estimation equation for the rest of the outcomes.

Fig. 3 compares the probability of saving in each month of individuals assigned to each of the three personalized and salient treatment versus that of the control. It documents heterogeneity within the results shown above. We observe that while in our basic combined treatment, effects are similar in size to the one of the interaction term presented in the previous figure, the effect of being assigned to the precautionary savings treatment is larger, although we cannot distinguish statistically between the two. Both interventions statistically significantly increase the probability of having saved during the period of SMS receipt compared to the control. On the other hand, the only message that did not include a monetary amount, the tangible goal intervention, show no difference in the probability of saving compared to the control. Given that the tangible goal is closer to the experiment of Karlan et al. (2016) in Peru, this could indicate that our combined salience and personalization could provide larger effects than those presented in this paper.

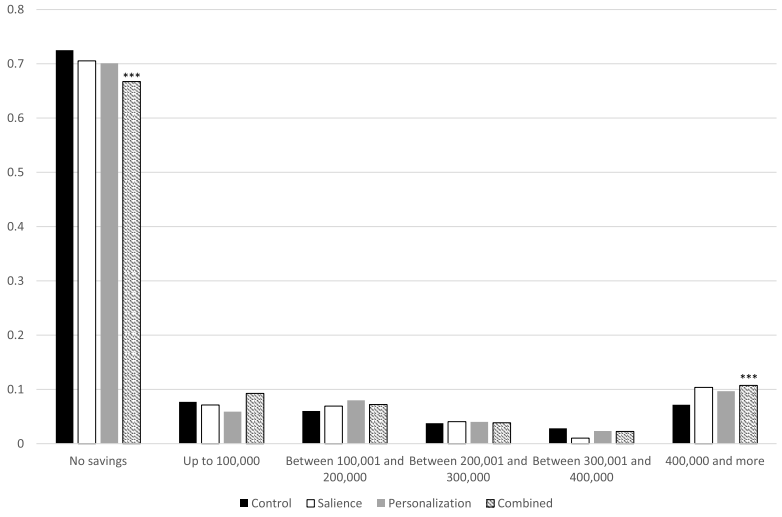
We next show graphically the impact on the amount saved. Fig. 4 shows that the fraction of participants that did not save at all was largest in the control group. The groups assigned to receive either only personalization or only salience have about 2 percent fewer participants who do not save than the control. This is statistically significantly lower for the combined group, matching the above results. Once we look at the behavior of those who did save, we observe a pattern that is a bit less clear. While the combined treatment group was more likely to save small and large (statistically significantly so in that case) amounts, what happens for amounts in the middle of the distribution is less obvious.

When we use more continuous outcomes, as represented by the amount saved over the period and what fraction of baseline income it represented, the difference between salience, personalization and their combination is less clear. In the second and third row of Table 4, we observe that only when comparing the combined treatment to the control do we find a statistically significant increase in savings. Magnitudes are not small as we find that 5 percent more of the group exposed to both personalization and salience saved more than 5 percent of their baseline income than those in the control group. This is a fifth of the value for the control group. The amount saved over the 6 months of the intervention are 84 percent higher in the combined treatment group than in the control. This is much larger than what was estimated by Karlan et al. (2016), at around 6 percent and often not significant on average and close to 15 percent for SMSs that combined goal reminder and financial incentives. It is relatively similar to that of Pomeranz and Kast (2024) and Rodríguez and Saavedra (2019). However, in contrast to the extensive margin, we find that salience on its own, personalization on its own and the added benefit of combining both contributes in similar magnitudes to the added savings. Our intervention thus increased the savings stock of participants compared to the control group while they were receiving SMSs, when those were personalized. This was due to the fact that salience, personalization and their combination each played an



Notes: This graph presents the coefficients α_4 (circles), α_5 (triangles), and α_6 (crosses) of the following regression: $y_{it} = \alpha_1 + \alpha_2 SO_i + \alpha_3 PO_i + \alpha_4 SP_i + \alpha_5 TG_i + \alpha_6 PS_i + \gamma X_i + \delta y_{it-1} + \epsilon_{it}$, where SO is a dummy that indicates "Salience only"; PO is a dummy that indicates "Personalization only"; SP is a dummy that indicates "Salience and personalization"; TG is a dummy that indicates "Tangible goal"; PS is a dummy that indicates "Precautionary savings"; and the rest of the variables are defined as in equation (1). The outcome is a dummy for whether the individual saved in each month following the baseline. Therefore, circles represent the difference between personalized SMS and control, triangles that between the tangible group and control and crosses that between precautionary savings and control. 95 percent confidence intervals are also presented. $N = 2,925$.

Fig. 3. Impact of Combined Treatments on Whether Individuals Saved During Each Month of Intervention (As Compared to the Control Group).



Notes: This graph presents the distribution of participants by saving amount ranges during the intervention for each of the treatments. $N = 2,909$. The stars above bars indicate if each treatment differs statistically to the control: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Fig. 4. Impact on Amount Saved During the Intervention.

additive role. In the top panel of Appendix Table A.9, we show that these conclusions are unaffected when excluding controls from the regressions. When comparing each of the salient and personalized treatments separately (see Appendix Table A.11), we observe that as before, only the combined treatment and the precautionary savings one increase savings compared to the control while the tangible goal does not. There is too much noise between each treatment for us to be able to say that they are statistically different

Table 4
Impact on Savings During the Intervention Period.

	C. Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)	Agg. Effect ($\sum_{i=1}^{3}\beta_i$)
Num of months saved	1.10	-0.02	-0.07	0.32*	0.23**
		(0.13)	(0.13)	(0.17)	(0.11)
Amount saved (log)	3.34	0.25	0.42	0.17	0.84***
		(0.34)	(0.35)	(0.45)	(0.28)
Saved > 5pct of baseline income	0.23	0.02	0.04	-0.01	0.05***
		(0.03)	(0.03)	(0.03)	(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 parentheses. 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. $N = 2,925$ except for 16 observations with missing amount saved. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are not statistically different at usual significance levels for any variable.

between themselves but it seems again that the tangible goal, because of its low take-up, did not modify savings behavior during the months where the messages were sent.

How did the amounts saved during the intervention compare to participants' stated goals at baseline and that were reminded to them through SMS when assigned to that group? We asked participants to compare their savings during the period to their stated goal using 6 categories. This limits us to look only at the group that had to set a goal. Almost two-third of the respondents declared being significantly short of their objective while only 7 percent declared having saved the same amount. Another 12 percent saved a bit less and 11 percent a bit more than their goal, while 7 percent saved significantly more than their target. In Appendix Table A.12, we found that combining personalization with salience helped individuals save an amount similar to their goal. This suggests that combining both personalization and salience may have made saving that exact amount more desirable. However, it does not help us in understand why salience or personalization on their own did not help to increase the probability of saving during the intervention.

To understand better what types of biases could be playing a role, we asked individuals how they had saved. In the control group, 14 percent declared having spent less, 8 percent had increased their income and 4 percent had reduced the cost of their credit. We find that the combined treatment increased their savings mostly by spending less compared to the control and not by reducing credit costs. However, we see little difference across salience, personalization and their combination. Those results are presented in Appendix Table A.13. This would indicate that it is not because each treatment made individuals think of reducing their expenses more than others that we see a positive effect of personalizing SMSs.

Another hypothesis would be that personalized SMSs are just more likely to be read and thus they solve better the problem of low attention. The top panel of Table 5 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 5. 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 and that the difference between those who were assigned to the combined treatment and those who had only salience is not really a difference in "salience".

The second panel of Table 5 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 by the generation of an actual goal and a reminder.

Table 5
Impact of Treatment Assignment on Treatment Perception.

	C. Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)	Agg. Effect ($\sum_{i=1}^{3} \beta_i$)
Individual remembers receiving SMS...					
... regarding savings	0.12	0.12***	0.02	0.00	0.14***
		(0.02)	(0.02)	(0.03)	(0.02)
... urging savings in general	0.03	0.07***	0.02	-0.03*	0.05***
		(0.02)	(0.01)	(0.02)	(0.01)
... urging saving a specific amount	0.01	0.01	0.01	0.03***	0.05***
		(0.01)	(0.01)	(0.01)	(0.01)
Individual remembers...					
... being surveyed last year	0.93	-0.02	0.00	0.01	-0.01
		(0.02)	(0.02)	(0.02)	(0.01)
... being told saving is important	0.54	-0.02	-0.04	0.08**	0.02
		(0.03)	(0.03)	(0.04)	(0.03)
... being told about savings potential	0.43	0.02	0.03	0.02	0.08***
		(0.03)	(0.03)	(0.04)	(0.03)
... a recommendation to save 10pct of income	0.46	0.06*	-0.00	0.01	0.07***
		(0.03)	(0.03)	(0.04)	(0.03)
... hearing that credit is more expensive than saving	0.59	-0.04	-0.08***	0.08**	-0.04*
		(0.03)	(0.03)	(0.04)	(0.02)

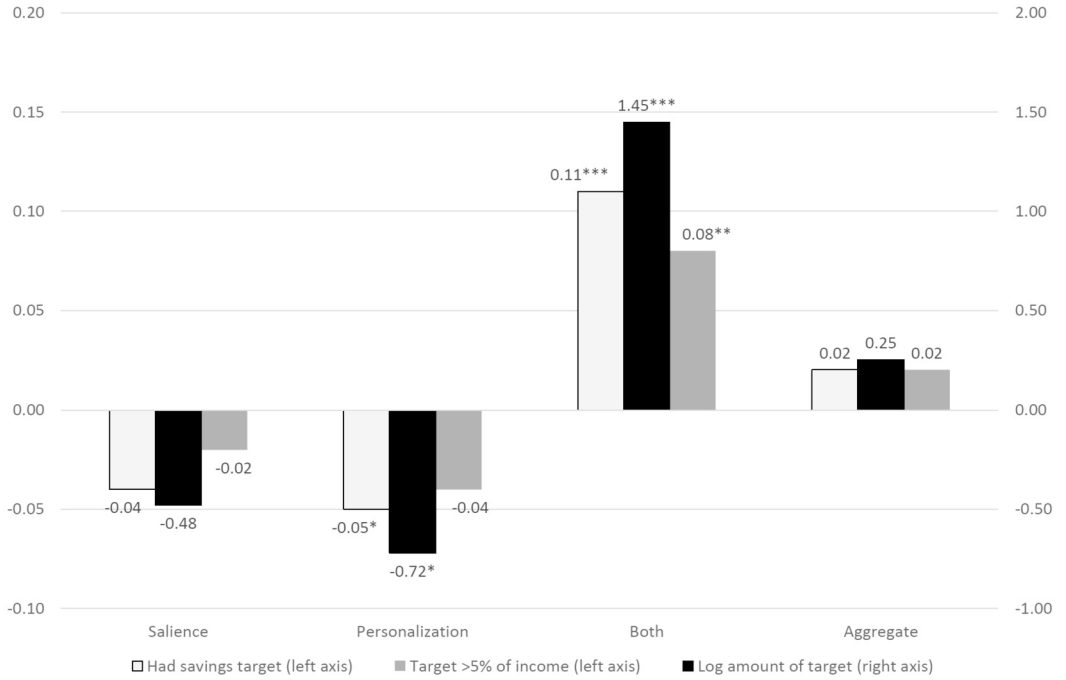
Table presents the mean in baseline of the variable and the coefficients of regression equation (1) using as outcome variables each variable mentioned. The last column presents the sum of each β and standard errors calculated using the delta-method. Robust standard errors are presented in parentheses. 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. $N = 2,925$. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The following variables present statistically significant differences between Salience and Personalization: "regarding savings" (1%), "urging savings in general" (1%), and "a recommendation to save 10pct of income" (10%). For the rest of the variables, Salience and Personalization are not statistically different at usual significance levels.

4.2. Savings post messaging

What happened when the SMSs were not longer being sent? We first start by exploring the impact the intervention had on savings' intentions at endline, about 6 months after the groups assigned to receive SMSs stop receiving them. This has been studied in most other savings studies as indicative of willingness to save. Fig. 5 shows results for 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. The coefficients presented in the figure correspond to the difference between those who received the salient treatment and the control: they are small, negative and not significant. Similarly, personalization alone was unable to increase savings targets; we actually observe a negative difference between those who received a personalized treatment and the control. We see that personalization actually significantly reduced the probability of setting a target and 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 8 percentage points and increases the amount pledged by 145 percent compared with treatments that only included salience and personalization on their own. The aggregate impact of our combined intervention 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 25 percent. We thus conclude that savings intentions appear to be more strongly reinforced by the combination of salience and personalization as were the extensive margin of savings during the intervention. When comparing across the type of combination, we find that the three personalized and salient treatments had similar differences compared to the control group, with the precautionary savings treatment being larger in magnitude but not statistically significantly so, as shown in Appendix Table A.11. Thus, we continue to see that the combination of personalization and salience plays a special role in the setting of savings objectives that is not seen when only personalization or only salience are employed. This suggests that the behavioral biases that may have been solved through our intervention continue to play a role in the setting of savings intentions.

Do these intentions translate into actual savings? We next measure monthly actual (flow) savings at endline in Table 6. 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 in the month previous to the endline survey.[14] The coefficients are small and do not show a clear pattern in terms

[14] As discussed in the robustness question, we also asked about whether the individual had saved over the last 6 months instead of last month. Given that we have no other information regarding the savings over the last six months in addition to this yes/no variation, we do not exploit it but the results are similar.



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 white), whether their target was more than 5 percent of their income (in grey) and the log amount of the target (in black). The "Aggregate" columns are the sum of each coefficients. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. $N = 2,925$ for had a savings target and $N = 2,915$ for the amount outcomes.

Fig. 5. Impact of Treatments on Savings Target in Endline.

of signs. The following two rows check whether this absence of impact on savings flow at endline 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 once SMS reminders are no longer sent, individuals are unable to translate positive budget balance 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. In Appendix Table A.11, we report that the way personalization and salience were combined does not appear to differ in terms of results. 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 (flow) once the intervention has stopped. Our results are aligned with other literature that finds effects of savings interventions to be short lived or diminishing over time (e.g., Attanasio et al., 2019; Kast et al., 2018) except for Rodríguez and Saavedra (2019) who find that SMSs had positive effects on youth savings even after the messaging stopped.

We next explore other financial outcomes in Table 7. 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 to the "top of mind". Interestingly, this increased use of credit did not translate into higher rates of default, which is rare in our sample. In panel B, we find no evidence that our interventions significantly affected the number of months where households faced unexpected financial shocks nor the 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 rather obtained through the Credit Bureau of Paraguay (Equifax). We present these results in Panel D of 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, nor on the credit score of participants or on the probability of having unpaid debt. We find that the amount of outstanding debt is significantly higher for those who were assigned to the salience 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

Table 6
Impact on Savings at Endline.

	C. Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)	Agg. Effect ($\sum_{i=1}^{3} \beta_i$)
Any savings last month	0.36	-0.01	0.01	0.02	0.02
		(0.03)	(0.03)	(0.04)	(0.02)
Amount saved (log) in last month	4.40	-0.08	0.25	0.11	0.27
		(0.35)	(0.37)	(0.47)	(0.29)
Saved > 5pct of baseline income	0.29	-0.00	0.03	-0.02	0.02
		(0.03)	(0.03)	(0.04)	(0.02)
Positive budget balance (dummy)	0.69	0.05*	0.02	-0.03	0.04
		(0.03)	(0.03)	(0.04)	(0.02)
Budget balance (log)	5.71	1.34*	0.50	-1.07	0.77
		(0.69)	(0.71)	(0.90)	(0.57)
Expenses on necessities	12.85	-0.30	-0.37	0.53*	-0.14
		(0.22)	(0.22)	(0.29)	(0.17)
Discretional expenses	5.88	0.59	0.11	-0.44	0.26
		(0.36)	(0.37)	(0.48)	(0.29)

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 parentheses. 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. $N = 2,925$ except for amount saved in last month ($N = 2,881$), expenses on necessities ($N = 2,869$) and discretional expenses ($N = 2,900$). * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are not statistically different at usual significance levels for any variable.

Table 7
Impact on Other Financial Outcomes.

	C. Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)	Agg. Effect ($\sum_{i=1}^{3} \beta_i$)
	Panel A: Credit market				
Requested credit	0.40	-0.05*	-0.04	0.05	-0.04
		(0.03)	(0.03)	(0.04)	(0.02)
Total amount approved ('000s)	2,522	-784	-1,064**	896*	-951**
		(490)	(462)	(536)	(452)
Defaulted	0.02	-0.00	0.00	-0.00	-0.00
		(0.01)	(0.01)	(0.01)	(0.01)
	Panel B: Financial resilience				
No. of months with financial problems	1.45	0.14	-0.05	0.01	0.10
		(0.15)	(0.14)	(0.19)	(0.12)
Thought of using savings next time?	0.33	-0.04	-0.02	0.02	-0.04*
		(0.03)	(0.03)	(0.04)	(0.02)
	Panel C: Financial well-being				
Index of well-being	-0.01	0.04	-0.00	-0.05	-0.01
		(0.04)	(0.04)	(0.06)	(0.03)
Satisfaction with financial situation (1-10)	6.32	-0.05	-0.06	0.19	0.08
		(0.14)	(0.14)	(0.18)	(0.11)
	Panel D: Outcomes from Credit Bureau				
Requests per 100 days	0.78	-0.03	-0.01	0.04	0.01
		(0.05)	(0.05)	(0.07)	(0.04)
Credit score	36.92	0.79	1.01	-0.68	1.13
		(2.70)	(2.77)	(3.55)	(2.21)
Probability of having unpaid debt	0.41	0.00	0.01	-0.01	0.00
		(0.03)	(0.03)	(0.04)	(0.02)
Total unpaid debt ('000s Gs)	2,361	1,740**	398	-1,066	1,072
		(882)	(679)	(1,277)	(757)

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 parentheses. 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. $N = 2,925$ except for total amount approved ($N = 2,871$) and for Credit Bureau outcomes (N = 2,862). 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? * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are not statistically different at usual significance levels for any variable.

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 different 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.[15]

4.3. Robustness

We first explore whether attrition may explain our results.[16] 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 A.6. To address these concerns, we estimate coefficient bounds using a strategy first introduced by Lee (2009). Appendix Table A.16 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 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 an 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 salience, we would be worried that those individuals were primed to report savings according to their stated goal. In Appendix Table A.17, we show that there is no difference between the probability of reporting savings (when asked for a numerical value) exactly equal to one's goal set at baseline 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 two similar (although not identical) questions regarding savings before and after this section. Appendix Table A.18 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 substantial 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.

5. Heterogeneity

We next explore whether some groups responded more or less to the personalized SMSs as a way to distinguish why the combination of personalization and salience could be particularly important. We use two broad groups of variables. The first, presented in Table 8, generate groups based on who could, before our intervention, have better understood that saving was a good decision. To measure this, we use financial literacy and whether the individual had saved before the intervention. This allows us to test how these interventions function depending on the likelihood of "reactance". The second division, shown in Table 9, is based on individuals who were more or less coherent in their budgeting decisions since these could benefit more from goal-setting. To measure coherence, we first use differences between savings and balance as a sign of individuals who cannot translate budgetary surplus into savings. Secondly, we use loan-taking because this is (aside from loans to finance the purchase of a durable good) likely to stem from an incapacity to pay for all expenses using current income and thus a sign of budgeting incoherence.

We first divide the sample by financial sophistication, as shown in Table 8.[17] 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 8, 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 although all coefficients are positive, but has a very negative impact on those who were more literate, and statistically significantly so 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.

[15] Results not presented but available upon request.

[16] Since attrition between the salience only and the personalization only groups and the control was very similar, the bounds produced by the analysis are quite tight as shown in Appendix Tables A.14 and A.15 and do not suggest that our main estimate are in any way driven by differential attrition.

[17] The heterogeneity could be affected by differences in take-up which we explored in the bottom panel of Appendix Table A.2. Those more financially literate were more likely to accept SMS but not differentially when the SMS was personalized suggesting that this will not be a source of confusion for the analysis. For savings at baseline, we find a difference in the capacity to set up a goal but this is not impacting the results as will be shown below.

Table 8
Impact on Savings by Financial Sophistication at Baseline.

		Not sophisticated				Sophisticated			
		Sal. (β_1)	Per. (β_2)	SxP (β_3)	Agg. ($\sum_{i=1}^{3}\beta_i$)	Sal. (β_1)	Per. (β_2)	SxP (β_3)	Agg. ($\sum_{i=1}^{3}\beta_i$)
Panel A: By financial literacy									
Savings during messaging	# Months	0.07	-0.10	0.17	0.14	-0.13	-0.04	0.51*	0.34*
		(0.17)	(0.17)	(0.22)	(0.14)	(0.21)	(0.20)	(0.27)	(0.17)
	Log amount	0.27	0.17	0.04	0.49	0.20	0.71	0.34	1.25***
		(0.46)	(0.46)	(0.60)	(0.37)	(0.52)	(0.53)	(0.69)	(0.42)
	Has one	0.01	-0.05	0.09*	0.04	-0.09**	-0.05	0.15**	0.00
		(0.04)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.06)	(0.04)
Target post messaging	Log amount	0.07	-0.65	1.07*	0.49	-1.16**	-0.79	1.92**	-0.03
		(0.50)	(0.49)	(0.65)	(0.41)	(0.58)	(0.58)	(0.75)	(0.47)
	> 5% inc.	0.00	-0.05	0.06	0.02	-0.06	-0.04	0.10*	0.01
		(0.04)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.06)	(0.03)
Savings at endline	Log amount	0.38	0.64	-0.44	0.58	-0.63	-0.23	0.78	-0.09
		(0.47)	(0.49)	(0.63)	(0.39)	(0.53)	(0.55)	(0.71)	(0.43)
	Discr. exp.	0.71	0.08	-0.07	0.71*	0.46	0.16	-0.89	-0.27
		(0.48)	(0.49)	(0.64)	(0.39)	(0.55)	(0.56)	(0.71)	(0.45)
Panel B: By baseline savings behavior									
Savings during messaging	# Months	0.06	-0.09	0.18	0.15	-0.20	-0.07	0.67*	0.40*
		(0.14)	(0.14)	(0.18)	(0.12)	(0.28)	(0.30)	(0.38)	(0.24)
	Log amount	0.58	0.34	-0.20	0.71***	-0.47	0.56	1.01	1.11*
		(0.39)	(0.38)	(0.51)	(0.31)	(0.69)	(0.73)	(0.92)	(0.58)
	Has one	0.03	-0.04	0.06	0.04	-0.17***	-0.08	0.23***	-0.02
		(0.03)	(0.03)	(0.04)	(0.03)	(0.05)	(0.06)	(0.07)	(0.05)
Target post messaging	Log amount	0.34	-0.60	0.69	0.43	-2.22***	-0.95	3.03***	-0.15
		(0.45)	(0.43)	(0.57)	(0.36)	(0.69)	(0.73)	(0.93)	(0.59)
	> 5% inc.	0.03	-0.04	0.03	0.02	-0.14***	-0.05	0.20***	0.00
		(0.03)	(0.03)	(0.04)	(0.03)	(0.05)	(0.06)	(0.07)	(0.04)
Savings at endline	Log amount	-0.03	0.49	-0.07	0.39	-0.20	-0.31	0.52	0.02
		(0.41)	(0.42)	(0.54)	(0.33)	(0.69)	(0.71)	(0.92)	(0.57)
	Discr. exp.	0.12	0.12	-0.25	-0.01	1.57**	0.05	-0.76	0.86
		(0.44)	(0.45)	(0.57)	(0.35)	(0.65)	(0.66)	(0.85)	(0.54)

Table presents the coefficients of regression equation (1) using as outcome variables each variable mentioned, 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 parentheses. 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. $N = 1,589$ for the first four columns of the first panel, $N = 2,034$ for the second panel, $N = 1,336$ for the last four columns of the first panel, $N = 891$ for the second. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are not statistically different at usual significance level for any variable in Panel A. In Panel B, the following variables present statistically significant differences between Salience and Personalization: "had a savings target last month" (at 5% on no sophisticated and at 10% on sophisticated), "amount of savings target (log)" (at 5% on not sophisticated and at 10% on sophisticated), "target > 5pct of baseline income" (at 5% on not sophisticated and no difference on sophisticated), and "discr. exp." (no difference on not sophisticated and at 5% on sophisticated).

Our results also indicate that the interaction of salience and personalization is particularly important for those who were more financially literate: coefficients are more positive for that group than for the non-finally sophisticated individuals and a larger fraction of them are statistically significant. This suggests that combining salience and personalization helps more those who were more sophisticated before our intervention as compared to the simple separate interventions. This would imply that "reactance" is significantly reduced when one is reminded of their own savings goal. Financially literate individuals would benefit more from the combined treatment compared to the generic SMS because they are more negatively affected by reactance. For savings during messaging, this leads to higher aggregate effects of the combined treatment for the financially sophisticated. For target setting post-intervention, the combined bonus is not large enough to compensate the negative impact of salience on its own.

We next look at heterogeneity by savings at baseline. The first four columns of Panel B of Table 8 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, something that is not visible for those who had not saved at baseline. It is also statistically significantly different from 0 in the case of setting a target post-messaging. Once more, the coefficients of the combination of treatment compared to salience or personalization only are larger in magnitudes and only statistically significant for those who were saving at baseline as compared to others. This emphasizes that just priming individuals to save may be detrimental, particularly 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

Table 9
Impact on Savings by Budget Management Coherence.

		Less coherent				More coherent			
		Sal. (β_1)	Per. (β_2)	SxP (β_3)	Agg. ($\sum_{i=1}^{3}\beta_i$)	Sal. (β_1)	Per. (β_2)	SxP (β_3)	Agg. ($\sum_{i=1}^{3}\beta_i$)
Panel A: By discrepancy between savings and budget balance									
Savings during messaging	# Months	-0.05	-0.21	0.46*	0.20	0.01	0.07	0.18	0.27*
		(0.20)	(0.19)	(0.25)	(0.17)	(0.17)	(0.18)	(0.24)	(0.15)
	Log amount	0.02	0.06	0.63	0.71*	0.46	0.78	-0.29	0.96***
		(0.51)	(0.50)	(0.66)	(0.42)	(0.46)	(0.48)	(0.63)	(0.37)
	Has one	-0.06	-0.08*	0.18***	0.03	-0.01	-0.03	0.05	0.01
		(0.04)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.05)	(0.03)
Target post messaging	Log amount	-0.88	-1.00*	2.25***	0.37	-0.11	-0.43	0.67	0.13
		(0.55)	(0.52)	(0.70)	(0.45)	(0.53)	(0.53)	(0.70)	(0.43)
	> 5% of inc.	-0.06	-0.06*	0.14***	0.01	0.01	-0.02	0.03	0.02
		(0.04)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.05)	(0.03)
Savings at endline	Log amount	-0.26	-0.22	0.72	0.25	0.09	0.74	-0.53	0.30
		(0.52)	(0.53)	(0.68)	(0.43)	(0.48)	(0.51)	(0.65)	(0.39)
	Discr. exp.	1.30**	0.70	-1.23*	0.76*	-0.09	-0.47	0.35	-0.22
		(0.53)	(0.53)	(0.69)	(0.43)	(0.49)	(0.51)	(0.65)	(0.40)
Panel B: Took loan in year before baseline.									
Savings during messaging	# Months	0.29	-0.17	0.04	0.16	-0.12	-0.04	0.42**	0.26**
		(0.29)	(0.27)	(0.36)	(0.22)	(0.15)	(0.15)	(0.19)	(0.13)
	Log amount	0.75	0.57	-0.73	0.59	0.08	0.37	0.48	0.93***
		(0.69)	(0.70)	(0.92)	(0.54)	(0.40)	(0.40)	(0.52)	(0.32)
	Has one	-0.06	-0.15***	0.18**	-0.03	-0.03	-0.02	0.09**	0.04
		(0.06)	(0.06)	(0.07)	(0.05)	(0.03)	(0.03)	(0.04)	(0.03)
Target post messaging	Log amount	-0.67	-2.01***	2.36**	-0.32	-0.40	-0.26	1.12**	0.47
		(0.75)	(0.71)	(0.96)	(0.60)	(0.44)	(0.43)	(0.57)	(0.36)
	> 5% of inc.	-0.02	-0.12**	0.13*	-0.01	-0.02	-0.02	0.06	0.02
		(0.06)	(0.05)	(0.07)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)
Savings at endline	Log amount	0.01	0.17	-0.00	0.18	-0.12	0.26	0.16	0.30
		(0.69)	(0.70)	(0.92)	(0.55)	(0.41)	(0.43)	(0.55)	(0.34)
	Discr. exp.	0.67	0.62	-0.74	0.55	0.56	-0.07	-0.34	0.15
		(0.70)	(0.72)	(0.93)	(0.57)	(0.42)	(0.43)	(0.55)	(0.35)

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 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 parentheses. 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. $N = 1,441$ for the first four columns of the first panel, $N = 781$ for the second panel, $N = 1,484$ for the last four columns of the first panel, $N = 2,144$ for the second. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Salience and Personalization are not statistically different at usual significance levels for any variable in Panel A. In Panel B, the following variables present statistically significant differences between Salience and Personalization (only on less coherent): "target amount (log)" (10%), and "target > 5% of inc." (10%).

doing something that they are told they should do. Aggregate effects, however, are not that different but stem from the combination of different forces.

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 A.19 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 since being reminded of a larger goal could improve savings intentions and savings behavior and we have shown that the savings goal was larger when the initial balance was positive in Fig. 1.[18] This is interesting because if we think that previous studies had a self-selected sample that had signed up for a particular savings program and those were more likely to be more sophisticated or had more desire to save, this suggests that we should have found even smaller effects than the ones they present. This would imply that it is more the way we personalized the SMSs that explain our larger effects, at least in terms of amounts.

We may also hypothesize that if personalized SMSs make savings more salient, we should observe a larger effect amongst those who had difficulty sticking to a savings objective previously.[19] Table 9 divides individuals who had more coherent budget manage-

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

[19] Once more, we verify whether compliance differed by our categories for heterogeneity. In Appendix Table A.2, we show that those who had savings similar to balance had an easier time setting a goal which would mean that it should be easier to see an impact of that treatment, which is the opposite of what is observed. Those who took a loan are not differentially able to set a goal.

ment and those who struggle. 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. All coefficients except for that of amount saved during the messaging are more negative for that group than for the group with more coherent answers at baseline. 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 be more helpful than simply setting a goal for 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. However, it is not the case that the combined treatment compared to the control generates better outcomes for the less coherent group highlighting that the positive effect of the interaction cannot fully undo the negative impacts of the goal-setting session for those.

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 clarify the reasons why previous studies have found positive impacts of personalized SMSs. In line with Karlan et al. (2016)'s conclusions: if there are combinations of behavioral biases, we may need to think carefully about the type of interventions that are best to be implemented and simple ones may backfire. The results are consistent with a story where salience on its own negatively influences the savings behavior of individuals who are more financially sophisticated, because of reactance. Personalization appears to have lowered savings goals and behavior for groups told they were previously unable to translate their budgetary surplus into savings and for those who had to rely on credit, generating frustration in that group. Finally, we observe a consistent pattern where the combination of salience and personalization is in part able to reverse these problems but not necessarily enough to make the combined treatment impactful. 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".

6. Conclusions

In this paper, we conducted an experiment to disentangle behavioral biases that may deter underbanked individuals from savings. Despite the difference in the selection of participants, we find that personalized SMSs increase savings while messages were sent as they did in other studies and with even larger magnitudes than Karlan et al. (2016). This is not the case for generic SMSs and much less so when emphasizing a tangible goal as in previous studies instead of a monetary amount. We discard that the benefit of personalizing SMSs is due to higher take-up or because it is more salient or memorable to participants.

We are one of the first studies to look at the impact of personalized goal-setting on its own and find that it does little to increase the extensive margin of 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 on its own plays a limited role and seems to have a negative impact for more sophisticated individuals. We find that combining personalization and salience is able to counter the negative impacts of reactance and frustration suggesting that they do something more than simply reducing limited attention.

Our findings are relevant when thinking about designing savings products for the poor. It appears that the behavioral biases that are limiting savings go beyond limited attention. 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. The fact that our results were obtained in an unbanked population imply that this is something that could be conceived by agents other than financial institutions.

It also looks like an intervention only focused on salience may be able to generate less "optimal" savings since that generic SMS, on their own, have a stronger impact on individuals who had limited financial knowledge and negative balances in 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.

Declaration of competing interest

Tessada acknowledges funding from ANID FONDECYT Regular 1191933. Lafortune acknowledges funding from ANID FONDECYT Regular 1210772. The opinions expressed in this publication reflect the views of its authors and not necessarily those of the Inter-American Development Bank Group, the World Bank Group, their respective Board of Directors, or the countries they represent. No potential conflict of interest was reported by the authors.

Data availability

Anonymized data is available at https://github.com/jeannelafortune/Paraguay.experiment.

Appendix A. Additional tables and figures

This section presents additional tables and figures.

Table A.1
Take-up Rates.

	SO	PO	SP	TG	PS	SP+TG+PS
Did the individual have a goal?	-	0.804	0.782	0.543	0.767	0.699
	-	(0.397)	(0.413)	(0.499)	(0.423)	(0.459)
Did the individual accept SMS?	0.742	-	0.713	0.674	0.703	0.697
	(0.438)	-	(0.453)	(0.469)	(0.457)	(0.460)
Did the ind. get at least one message?	99.4%	-	99.4%	99.5%	99.6%	99.5%
Percent of target messages received	97.4%	-	97.5%	96.7%	99.6%	97.0%

SO = Salience only; PO = Personalization only; SP = Salience and personalization; TG = Tangible goal; and PS = Precautionary savings. Standard deviations are presented on parenthesis. The last row displays the percentage of received messages based on the target, which corresponds to the product of the number of individuals who accepted to receive messages and the months that the intervention lasted. Because we do not have that information at an individual level, we cannot provide standard deviations.

Table A.2
Take-up and baseline characteristics.

	Accepts sms (sms = 1)			Has a goal (goal = 1)		
	C. Mean	sms	sms*SxP	C. Mean	goal	goal*SxP
	Panel A: Baseline characteristics					
Age	30.86	-0.89	-0.30	29.95	-0.00	0.41
		(0.92)	(1.07)		(1.10)	(1.21)
Share of women	0.62	-0.02	0.05	0.63	-0.02	-0.01
		(0.04)	(0.05)		(0.05)	(0.05)
Years of education	7.14	-0.42**	0.43*	6.31	0.54**	-0.20
		(0.21)	(0.24)		(0.24)	(0.27)
Share single	0.55	-0.02	-0.01	0.51	0.04	-0.05
		(0.04)	(0.05)		(0.05)	(0.06)
Head of household	0.53	-0.04	0.06	0.47	0.02	-0.03
		(0.04)	(0.05)		(0.05)	(0.06)
Worked last year	0.97	-0.03*	0.05**	0.90	0.04	-0.04
		(0.02)	(0.02)		(0.03)	(0.03)
Total ind. income (thousands)	2,852	-167	-272	1,975	809***	-516
		(334)	(492)		(244)	(422)
Total ind. expenses (thousands)	2,025	-53.88	89.94	1,640	108	113
		(225)	(245)		(143)	(163)
Account balance (thousands)	827	-79.39	-395	335	701***	-630*
		(214)	(397)		(183)	(375)
Individual savings (log)	3.51	0.31	0.08	0.92	3.47***	-1.64***
		(0.49)	(0.56)		(0.40)	(0.47)
Had financial problems last year...	0.24	0.24***	-0.06	0.37	-0.01	0.12**
		(0.04)	(0.05)		(0.05)	(0.05)
...because of fall in income	0.16	0.11***	-0.08*	0.22	0.04	0.11**
		(0.03)	(0.04)		(0.04)	(0.05)
Index of well-being	-0.11	0.17***	-0.06	0.13	-0.14	0.08
		(0.07)	(0.08)		(0.09)	(0.09)
	Panel B: Financial characteristics					
Financial literacy	0.35	0.14***	-0.05	0.42	0.06	0.03
		(0.04)	(0.05)		(0.05)	(0.05)
Positive savings at baseline	0.30	0.02	0.02	0.07	0.30***	-0.15***
		(0.04)	(0.05)		(0.03)	(0.04)
Savings similar to budget balance	0.52	-0.05	0.06	0.38	0.17***	-0.04
		(0.04)	(0.05)		(0.05)	(0.05)
Took loan in year before baseline	0.30	-0.07*	0.09*	0.19	0.07	0.00
		(0.04)	(0.05)		(0.04)	(0.05)
Positive balance	0.77	0.00	0.01	0.70	0.16***	-0.06
		(0.04)	(0.04)		(0.04)	(0.05)

Table presents the mean in baseline of the indicated variables and the coefficients obtained by regressing the these characteristics on a binary variable that indicates whether the person accepts to receive messages. Financial characteristics are defined as in Table 8, Table 9 and Table A.19. $N = 2,676$ in sms regression and $N = 2,645$ in goal regression.

Table A.3
Minimum Detectable Effects by Outcome.

	Salience		Personalization		SxP	
Outcomes	N	MDE	N	MDE	N	MDE
	Panel A: Savings during intervention					
Num of months saved	1023	0.236	1006	0.220	1958	0.243
Savings during intervention (log)	1016	0.237	1001	0.220	1950	0.244
Saved > 5pct of baseline income	1016	0.237	1001	0.220	1950	0.244
	Panel B: Savings at endline					
Saved (dummy)	1007	0.229	990	0.213	1928	0.236
Amount saved (log)	1007	0.228	990	0.213	1928	0.235
Positive balance (dummy)	1023	0.236	1006	0.220	1958	0.243
Budget balance (log)	1023	0.236	1006	0.220	1958	0.243
Expenses on household items	1007	0.232	987	0.217	1917	0.240
Expenses on fun, clothes, and co.	1018	0.231	998	0.216	1940	0.239
	Panel C: Other outcomes					
N months with financial problems	1023	0.236	1006	0.220	1958	0.243
Thought of using savings next time?	1023	0.234	1006	0.218	1958	0.240
Index of well being	1023	0.208	1006	0.193	1958	0.214
Satisfaction with financial situation	1023	0.224	1006	0.209	1958	0.231
Requested credit	1023	0.229	1006	0.214	1958	0.236
Total amount requested (thousands)	1020	0.227	1002	0.212	1950	0.234
Total amount approved (thousands)	1002	0.229	990	0.213	1919	0.236
Defaulted	1023	0.236	1006	0.220	1958	0.243

Table A.4
Seasonality.

	C. Mean	Jul	Aug	Sep	Oct	Nov	Joint F-test
	Panel A: Baseline info-Regressions without controls						
Bas. balance ('000s)	840	-9.14	83.20	105	387***	416***	5.73***
		(100)	(90.89)	(122)	(95.04)	(113)	
Bas. income ('000s)	2,677	985***	791***	549**	1,380***	996***	36.09***
		(176)	(133)	(213)	(130)	(132)	
Bas. expenses ('000s)	1,841	994***	708***	443***	1,008***	579***	46.77***
		(134)	(92.37)	(150)	(74.96)	(61.50)	
	Panel B: Baseline info-Regressions with controls						
Bas. balance ('000s)	840	-114	-197	-176	-5.81	-86.11	0.73
		(130)	(157)	(164)	(138)	(76.50)	
Bas. income ('000s)	2,677	136	-158	-194	97.14	-159*	1.38
		(199)	(199)	(264)	(171)	(94.62)	
Bas. expenses ('000s)	1,841	244*	36.15	-10.48	125	-76.62	2.45**
		(131)	(107)	(159)	(82.21)	(53.20)	
	Panel C: Assignment-Regressions without controls						
Salience only	0.17	-0.04	-0.01	0.01	-0.00	-0.02	0.80
		(0.03)	(0.03)	(0.06)	(0.02)	(0.02)	
Person. only	0.16	0.05	0.02	-0.01	-0.01	0.02	1.16
		(0.03)	(0.03)	(0.05)	(0.02)	(0.02)	
Salience and Perso.	0.50	-0.09**	-0.02	-0.06	0.01	0.02	2.26**
		(0.04)	(0.04)	(0.07)	(0.03)	(0.03)	
	Panel D: Assignment-Regressions with controls						
Salience only	0.17	-0.05	-0.02	0.00	-0.00	-0.03	0.74
		(0.03)	(0.03)	(0.06)	(0.03)	(0.02)	
Person. only	0.16	0.08**	0.04	0.02	0.02	0.03	0.92
		(0.04)	(0.03)	(0.05)	(0.03)	(0.02)	
Salience and Perso.	0.50	-0.09*	-0.04	-0.06	-0.00	0.03	2.28**
		(0.05)	(0.04)	(0.08)	(0.04)	(0.03)	

Table presents the mean of each indicated variable and the coefficients obtained by regressing these variables over the baseline months (reference month is December). Controls 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 in the second and fourth panel. $N = 4,028$. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.5
Disaggregated Balance.

	C. Mean	SO	PO	SP	TG	PS	Joint F-test
Age	30.80	-0.60	-0.85	-0.86	-0.28	-0.39	0.71
	(10.74)	(0.57)	(0.58)	(0.56)	(0.59)	(0.57)	
Women	0.61	-0.01	0.00	-0.01	-0.02	0.01	0.30
	(0.49)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Years of education	6.71	0.12	0.04	0.07	0.23*	0.01	0.83
	(2.50)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	
Single	0.54	-0.01	0.00	0.01	-0.01	-0.03	0.49
	(0.50)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Head of household	0.50	0.01	-0.01	-0.01	0.01	-0.02	0.40
	(0.50)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Worked last year	0.93	0.01	-0.00	0.02	-0.00	0.01	0.80
	(0.26)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
Total ind. income (thousands)	2,581	147	44.61	54.81	289	51.98	0.23
	(3,035)	(189)	(173)	(175)	(340)	(168)	
Total ind. expenses (thousands)	1,825	160	-97.80	97.95	-33.92	-34.04	1.35
	(1,696)	(124)	(85.89)	(107)	(88.86)	(85.20)	
Account balance (thousands)	757	11.94	142	-43.15	322	86.02	0.68
	(2,421)	(127)	(137)	(122)	(320)	(131)	
Saves	0.31	-0.00	-0.00	-0.00	-0.00	-0.05*	1.11
	(0.46)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	
Individual savings (log)	3.72	0.02	-0.01	0.03	0.09	-0.47	0.95
	(5.59)	(0.30)	(0.31)	(0.30)	(0.31)	(0.29)	
Had finan. problems last year...	0.38	0.04	-0.02	0.01	0.00	-0.00	0.98
	(0.49)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
...because of fall in income	0.26	0.04*	-0.01	-0.01	-0.01	0.00	1.28
	(0.44)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	
Index of well-being	-0.00	0.01	0.02	-0.01	0.01	-0.03	0.30
	(0.79)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	
F-test:		0.76	0.40	0.44	0.45	0.69	

Table presents the mean in baseline of the variable and the coefficients obtained by regressing these characteristics on each treatment. The seventh column shows the joint F-test of significance for the five β. Robust standard errors are presented in parentheses. $N = 4,028$ for all variables except for individual savings (log) ($N = 4,008$ because 20 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? * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.6
Attrition Test.

	C. Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)	Agg. Effect ($\sum_{i=1}^{3} \beta_i$)
Panel A: Without controls					
Attrition (dummy)	0.25	0.01	0.00	0.02	0.03*
		(0.02)	(0.02)	(0.03)	(0.02)
Panel B: With controls					
Attrition (dummy)	0.25	0.01	0.00	0.03	0.04**
		(0.02)	(0.02)	(0.03)	(0.02)

Table presents the control mean 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 parentheses. Controls for gender, age, educational attainment, labor force, occupations, and district are included in Panel B. $N = 4,028$. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are not statistically different at usual significance levels on both panels.

Table A.7
Balance Conditional on Answering the Endline.

	C. Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)	Joint F-test	Agg. Effect ($\sum_{i=1}^{3} \beta_i$)
Age	31.16	-0.84	-0.19	0.78	0.61	-0.24
	(10.91)	(0.66)	(0.69)	(0.88)		(0.55)
Women	0.63	-0.01	-0.01	0.00	0.13	-0.01
	(0.48)	(0.03)	(0.03)	(0.04)		(0.02)
Years of education	6.61	0.23	-0.00	-0.16	1.01	0.07
	(2.49)	(0.15)	(0.15)	(0.20)		(0.13)
Single	0.53	-0.02	-0.01	-0.00	0.42	-0.03
	(0.50)	(0.03)	(0.03)	(0.04)		(0.03)
Head of household	0.49	0.01	0.02	-0.01	0.11	0.01
	(0.50)	(0.03)	(0.03)	(0.04)		(0.03)
Worked last year	0.93	0.01	-0.01	-0.00	0.64	0.01
	(0.26)	(0.02)	(0.02)	(0.02)		(0.01)
Total ind. income ('000s)	2,650	48.63	-78.83	49.26	0.13	19.05
	(3,331)	(217)	(203)	(303)		(213)
Total ind. expenses ('000s)	1,789	93.85	-103	49.63	1.39	40.95
	(1,528)	(112)	(93.33)	(139)		(83.09)
Account balance ('000s)	861	-8.63	23.69	-36.96	0.03	-21.90
	(2,731)	(167)	(161)	(245)		(185)
Saves	0.31	0.01	-0.01	-0.02	0.37	-0.01
	(0.46)	(0.03)	(0.03)	(0.04)		(0.02)
Individual savings	3.71	0.19	0.01	-0.32	0.39	-0.12
	(5.62)	(0.36)	(0.36)	(0.47)		(0.29)
Had finan. prob. last year...	0.38	0.04	-0.01	-0.01	1.40	0.02
	(0.49)	(0.03)	(0.03)	(0.04)		(0.02)
...because of fall in income	0.26	0.04	-0.02	-0.02	1.48	0.01
	(0.44)	(0.03)	(0.03)	(0.04)		(0.02)
Index of well-being	-0.01	0.02	0.04	-0.05	0.23	0.01
	(0.79)	(0.05)	(0.05)	(0.07)		(0.04)
F-test:		1.48	0.50	0.51		

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 parentheses. $N = 2,925$ for all variables except for Individual savings (log) ($N = 2,916$ because 9 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? * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The following variables present statistically significant differences between Salience and Personalization: "total ind. expenses" (10%), "had finan. problems last year" (10%), and "because of fall in income" (5%). For the rest of the variables, Salience and Personalization are not statistically different at usual significance levels.

Table A.8
Disaggregated Balance Conditional on Answering the Endline.

	C. Mean	SO	PO	SP	TG	PS	Joint F-test
Age	30.87	-0.84	-0.19	-0.81	0.29	-0.22	0.71
	(10.72)	(0.66)	(0.69)	(0.67)	(0.70)	(0.68)	
Women	0.62	-0.01	-0.01	-0.01	-0.03	-0.01	0.30
	(0.49)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Years of education	6.68	0.23	-0.00	0.10	0.14	-0.03	0.83
	(2.42)	(0.15)	(0.15)	(0.15)	(0.16)	(0.15)	
Single	0.51	-0.02	-0.01	-0.01	-0.04	-0.04	0.49
	(0.50)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Head of household	0.50	0.01	0.02	0.00	0.03	0.01	0.40
	(0.50)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Worked last year	0.93	0.01	-0.01	0.03*	-0.01	0.00	0.80
	(0.25)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	
Total ind. income ('000s)	2,654	48.63	-78.83	-55.49	303	-186	0.23
	(4,760)	(217)	(203)	(210)	(454)	(183)	
Total ind. expenses ('000s)	1,808	93.85	-103	191	-22.26	-40.85	1.35
	(1,785)	(112)	(93.37)	(133)	(99.36)	(92.97)	
Account balance ('000s)	853	-8.63	23.69	-247*	325	-145	0.68
	(4,179)	(167)	(161)	(139)	(432)	(139)	
Saves	0.30	0.01	-0.01	-0.00	0.01	-0.04	1.11
	(0.46)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Individual savings (log)	3.67	0.22	0.03	-0.03	0.15	-0.41	0.95
	(5.62)	(0.35)	(0.36)	(0.36)	(0.36)	(0.35)	
Had finan. prob. last year...	0.39	0.04	-0.01	0.04	0.02	0.01	0.98
	(0.49)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
...because of fall in income	0.26	0.04	-0.02	0.00	-0.00	0.02	1.28
	(0.44)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Index of well-being	0.01	0.02	0.04	-0.00	0.04	0.01	0.30
	(0.81)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	
F-test:		1.48	0.50	1.13	0.85	0.99	

Table presents the mean in baseline of the variable and the coefficients obtained by regressing these characteristics on each treatment. The seventh column shows the joint F-test of significance for the five β. Robust standard errors are presented in parentheses. $N = 2,925$ for all variables except for individual savings (log) ($N = 2,916$ because 9 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? * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9
Effects on Savings (without baseline controls).

	C. Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)	Agg. Effect ($\sum_{i=1}^{3}\beta_i$)
	Panel A: Savings during intervention period				
Num of months saved	1.10	-0.03	-0.08	0.32*	0.21*
		(0.14)	(0.14)	(0.18)	(0.12)
Amount saved (SR)	3.34	0.25	0.35	0.15	0.75***
		(0.35)	(0.36)	(0.47)	(0.29)
Saved > 5pct of baseline income	0.23	0.02	0.04	-0.01	0.04**
		(0.03)	(0.03)	(0.04)	(0.02)
	Panel B: Savings target at endline				
Had a savings target last month	0.34	-0.03	-0.05*	0.10***	0.02
		(0.03)	(0.03)	(0.04)	(0.02)
Amount of savings target (log)	4.41	-0.34	-0.71*	1.32***	0.27
		(0.38)	(0.38)	(0.49)	(0.31)
Target > 5pct of baseline income	0.32	-0.02	-0.05	0.07*	0.01
		(0.03)	(0.03)	(0.04)	(0.02)
	Panel C: Actual savings at endline				
Saved (dummy)	0.36	-0.01	0.01	0.02	0.02
		(0.03)	(0.03)	(0.04)	(0.02)
Amount saved (LR)	4.40	-0.13	0.23	0.15	0.25
		(0.36)	(0.37)	(0.47)	(0.29)
Saved > 5pct of baseline income	0.29	-0.00	0.03	-0.02	0.01
		(0.03)	(0.03)	(0.04)	(0.02)
Positive balance (dummy)	0.69	0.05	0.03	-0.04	0.04*
		(0.03)	(0.03)	(0.04)	(0.02)
Budget balance (log)	5.71	1.30*	0.79	-1.26	0.83
		(0.69)	(0.71)	(0.90)	(0.58)
Expenses on household items	12.85	-0.33	-0.40*	0.61**	-0.12
		(0.22)	(0.23)	(0.30)	(0.17)
Discretional expenses	5.88	0.74**	0.08	-0.50	0.32
		(0.38)	(0.38)	(0.49)	(0.31)

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 parentheses. Same sample sizes as estimations with controls. $N = 2,925$, except for "amount saved" on Panel A (2,909), Panel B (2,915), and Panel C (2,881), "expenses on household items" (2,869), and "discretional expenses" (2,900). * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are statistically different at usual significance levels only on "discretional expenses" (10%).

Table A.10
Comparing Salience and Personalization Versus Interaction.

	Salience			Personalization		
	C. Mean	SxP	Obser.	C. Mean	SxP	Obser.
Num of months saved	1.07	0.24**	1,919	1.01	0.29***	1,902
		(0.11)			(0.11)	
Amount saved (SR)	3.59	0.61**	1,908	3.69	0.38	1,893
		(0.30)			(0.30)	
Saved > 5pct of baseline income	0.25	0.03	1,908	0.27	0.01	1,893
		(0.02)			(0.02)	

In the first three columns, the sample is restricted to individuals assigned to salience, whether they are assigned to personalization or not. The dependent variables are the specified outcomes, while the independent variable is a dummy indicating individual assignment to personalization. C. Mean indicates the mean outcomes for those assigned to salience without personalization. In the second three columns, the sample is restricted to individuals assigned to personalization, whether they are assigned to salience or not. The dependent variables are the specified outcomes, while the independent variable is a dummy indicating individual assignment to salience. C. Mean indicates the mean outcomes for those assigned to personalization without salience.

Table A.11
Comparing the Way Personalization and Salience are Combined.

	Versus control			Versus SP	
	SP	TG	PS	TG	PS
	Panel A: Savings during intervention period				
Num of months saved	0.26*	0.02	0.42***	-0.23	0.18
	(0.14)	(0.14)	(0.14)	(0.14)	(0.15)
Amount saved (SR)	1.02***	0.44	1.05***	-0.61	0.02
	(0.35)	(0.35)	(0.35)	(0.38)	(0.37)
Saved > 5pct of baseline income	0.08***	0.04	0.05*	-0.04	-0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
	Panel B: Savings target				
Had a savings target last month	0.02	0.02	0.03	-0.00	0.01
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Amount of savings target (log)	0.19	0.24	0.31	-0.00	0.10
	(0.39)	(0.39)	(0.38)	(0.41)	(0.40)
Target > 5pct of baseline income	0.02	0.01	0.01	-0.01	-0.01
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
	Panel C: Savings at endline				
Saved (dummy)	0.02	0.01	0.04	-0.01	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Amount saved (LR)	0.23	0.11	0.47	-0.10	0.27
	(0.36)	(0.36)	(0.36)	(0.38)	(0.38)
Saved > 5pct of baseline income	0.01	0.01	0.02	0.00	0.01
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Positive balance (dummy)	0.03	0.04	0.03	0.01	0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Budget balance (log)	0.59	1.05	0.67	0.42	0.09
	(0.71)	(0.70)	(0.70)	(0.72)	(0.71)
Expenses on household items	-0.11	-0.08	-0.21	0.08	-0.07
	(0.21)	(0.21)	(0.22)	(0.22)	(0.23)
Discretional expenses	0.03	0.33	0.40	0.31	0.39
	(0.37)	(0.36)	(0.37)	(0.38)	(0.38)

The first and the third columns present the comparison of being assigned to the different types of salience+personalization compared to the control. Dummies for salience only and personalization only are included but not shown here. $N = 2,925$ except those with missing observations due to people answering "I don't know": 10 people for the amount of the target, 16 people for amount saved during the intervention and 44 for amount saved last month. In panel A, we can reject the hypothesis that tangible goal and precautionary savings are equal at 1 percent. The fourth and the fifth column present the comparison of being assigned to the tangible goal or the precautionary savings goal compared to the personalized and salient treatment. $N = 1,427$ 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. Robust standard errors are presented in parentheses. 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. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.12
Impact on Savings Compared to Stated Goal.

	No savings	Much less than goal	Bit less than goal	Same as goal	Bit more than goal	Much more than goal
SxP	-0.05	-0.00	0.01	0.04**	-0.00	0.00
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
M. control	0.49	0.14	0.12	0.07	0.11	0.07

Table presents the coefficient of the interaction between being assigned to the salience and the personalization treatment on a dummy indicating the difference between the savings target and the actual savings during the intervention. The sample for this regression is limited to those assigned to personalization or the combined treatment. The mean of the outcome for those in the personalization group only is also included. Robust standard errors are presented in parentheses. 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. $N = 1,112$. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.14
Bounding "Salience Only" Effects Due to Attrition.

	Salience only effect	Upper bound	Lower bound
	Panel A: Savings during intervention		
Num of months saved	-0.01	-0.01	-0.02
	(0.13)	(0.13)	(0.13)
Amount saved (SR)	0.27	0.28	0.26
	(0.34)	(0.34)	(0.34)
Saved > 5pct of baseline income	0.02	0.03	0.02
	(0.03)	(0.03)	(0.03)
	Panel B: Savings target		
Had a savings target last month	-0.04	-0.04	-0.04
	(0.03)	(0.03)	(0.03)
Amount of savings target (log)	-0.48	-0.48	-0.50
	(0.38)	(0.38)	(0.38)
Target > 5pct of baseline income	-0.02	-0.02	-0.02
	(0.03)	(0.03)	(0.03)
	Panel C: Savings at endline		
Saved (dummy)	-0.01	-0.01	-0.01
	(0.03)	(0.03)	(0.03)
Amount saved (LR)	-0.07	-0.06	-0.12
	(0.35)	(0.35)	(0.35)
Saved > 5pct of baseline income	0.00	0.00	-0.00
	(0.03)	(0.03)	(0.03)
Positive balance (dummy)	0.05*	0.05*	0.05*
	(0.03)	(0.03)	(0.03)
Budget balance (log)	1.35**	1.35**	1.33*
	(0.69)	(0.69)	(0.69)
Expenses on household items	-0.31	-0.26	-0.32
	(0.22)	(0.22)	(0.22)
Discr. expenses	0.53	0.53	0.51
	(0.36)	(0.36)	(0.36)

This table reports the salience (only) effect using the full sample (first column) and excluding the 1 percent highest (respectively lowest) reported outcome values amongst those assigned to the control group (second column, respectively third column).

Table A.13
Impact on Savings Behavior.

	C. Mean	Salience (β_1)	Personalization (β_2)	SxP (β_3)	Agg. Effect ($\sum_{i=1}^{3} \beta_i$)
	How did you save during the intervention				
Spent less	0.14	0.02	0.02	0.02	0.06***
		(0.02)	(0.02)	(0.03)	(0.02)
Increased income	0.08	0.01	-0.00	0.01	0.02
		(0.02)	(0.02)	(0.02)	(0.01)
Reduced credit costs	0.04	-0.02*	-0.00	-0.01	-0.03***
		(0.01)	(0.01)	(0.01)	(0.01)

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 parentheses. 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. $N = 2,925$. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are not statistically different at usual significance levels for any variable.

Table A.15
Bounding "Personalization Only" Effects Due to Attrition.

	Person. only effect	Upper bound	Lower bound
	Panel A: Savings during intervention		
Num of months saved	-0.08	-0.04	-0.09
	(0.13)	(0.13)	(0.13)
Amount saved (SR)	0.39	0.48	0.34
	(0.34)	(0.34)	(0.35)
Saved > 5pct of baseline income	0.04	0.04*	0.04
	(0.03)	(0.03)	(0.03)
	Panel B: Savings target		
Had a savings target last month	-0.05*	-0.05*	-0.06*
	(0.03)	(0.03)	(0.03)
Amount of savings target (log)	-0.72*	-0.63*	-0.75**
	(0.37)	(0.37)	(0.37)
Target > 5pct of baseline income	-0.04	-0.04	-0.05*
	(0.03)	(0.03)	(0.03)
	Panel C: Savings at endline		
Saved (dummy)	0.01	0.01	0.01
	(0.03)	(0.03)	(0.03)
Amount saved (LR)	0.24	0.26	0.23
	(0.37)	(0.37)	(0.37)
Saved > 5pct of baseline income	0.03	0.04	0.03
	(0.03)	(0.03)	(0.03)
Positive balance (dummy)	0.02	0.02	0.01
	(0.03)	(0.03)	(0.03)
Budget balance (log)	0.51	0.58	0.16
	(0.71)	(0.71)	(0.70)
Expenses on household items	-0.38*	-0.35	-0.47**
	(0.23)	(0.23)	(0.22)
Discr. expenses	0.16	0.26	0.08
	(0.37)	(0.37)	(0.38)

This table reports the personalization (only) effect using the full sample (first column) and excluding the 0.4 percent highest (respectively lowest) reported outcome values amongst those assigned to the control group (second column, respectively third column).

Table A.16
Bounding Aggregate Effects Due to Attrition.

	Agg. Effect	Upper bound	Lower bound
	Panel A: Savings during intervention		
Num of months saved	0.23**	0.49***	0.17
	(0.11)	(0.10)	(0.11)
Amount saved (SR)	0.84***	1.09***	0.71***
	(0.28)	(0.27)	(0.28)
Saved > 5pct of baseline income	0.05***	0.08***	0.04**
	(0.02)	(0.02)	(0.02)
	Panel B: Savings target		
Had a savings target last month	0.02	0.06***	0.01
	(0.02)	(0.02)	(0.02)
Amount of savings target (log)	0.25	0.66**	0.02
	(0.31)	(0.31)	(0.32)
Target > 5pct of baseline income	0.02	0.05**	0.00
	(0.02)	(0.02)	(0.02)
	Panel C: Savings at endline		
Saved (dummy)	0.02	0.04*	0.01
	(0.02)	(0.02)	(0.02)
Amount saved (LR)	0.27	0.51*	0.11
	(0.29)	(0.29)	(0.30)
Saved > 5pct of baseline income	0.02	0.04	0.01
	(0.02)	(0.02)	(0.02)
Positive balance (dummy)	0.04	0.05**	0.00
	(0.02)	(0.02)	(0.02)
Budget balance (log)	0.77	1.02*	-0.32
	(0.57)	(0.58)	(0.56)
Expenses on household items	-0.16	-0.06	-0.58***
	(0.17)	(0.18)	(0.14)
Disc. expenses	0.21	0.54*	-0.03
	(0.30)	(0.30)	(0.30)

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 4 percent highest (respectively lowest) reported outcome values amongst those assigned to the control group (second column, respectively third column).

Table A.17
Testing for Pleasing Surveyor.

	Personalization		Salience*Personalization		Difference	
Treatment	Prob.	N	Prob.	N	Prob.	$\|z\|$-value
Goal = Savings	0.199	468	0.228	645	-0.029	1.273
	(0.019)		(0.017)		(0.025)	

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 A.18
Testing for Framing.

Saved during past 6 months (measured after questions about intervention)	Saved last month (measured before questions about intervention)		
	No	Yes	Total
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 chi2(1) = 1.15, which implies we cannot reject the null hypothesis of symmetric table.

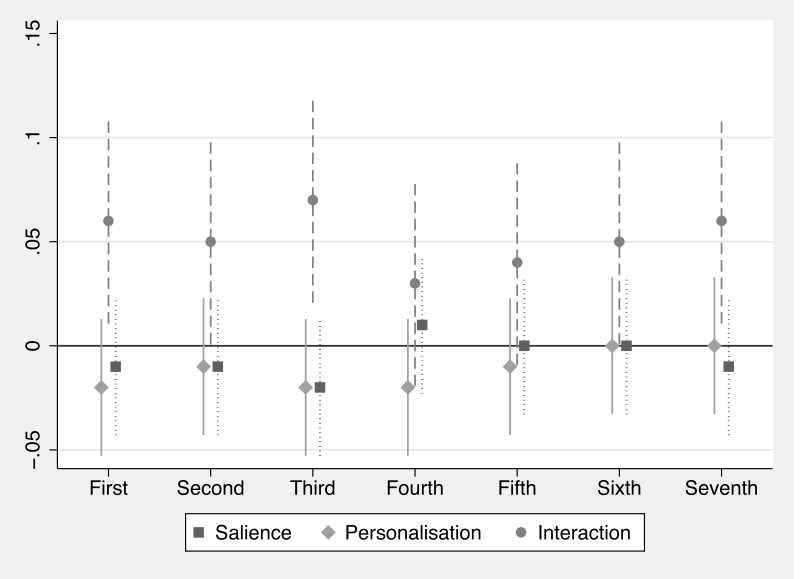
Table A.19
Impact on Savings by Budget Balance at Baseline.

		Negative balance				Positive balance			
		Sal. (β_1)	Per. (β_2)	SxP (β_3)	Agg. ($\sum_{i=1}^{3}\beta_i$)	Sal. (β_1)	Per. (β_2)	SxP (β_3)	Agg. ($\sum_{i=1}^{3}\beta_i$)
Savings	# Months	-0.14	-0.09	0.26	0.03	0.02	-0.06	0.32*	0.29**
during		(0.27)	(0.31)	(0.38)	(0.24)	(0.15)	(0.15)	(0.19)	(0.12)
messaging	Log amount	0.63	0.48	-0.17	0.94*	0.15	0.40	0.26	0.80***
		(0.69)	(0.71)	(0.94)	(0.55)	(0.40)	(0.39)	(0.52)	(0.32)
	Has one	0.02	-0.06	0.01	-0.02	-0.05	-0.05	0.14***	0.04
		(0.06)	(0.07)	(0.09)	(0.05)	(0.03)	(0.03)	(0.04)	(0.03)
Target post	Log amount	0.23	-0.85	0.26	-0.36	-0.67	-0.68*	1.77***	0.41
messaging		(0.82)	(0.86)	(1.11)	(0.66)	(0.43)	(0.41)	(0.55)	(0.35)
	> 5% of inc.	0.06	-0.04	-0.03	-0.01	-0.05	-0.04	0.11***	0.02
		(0.06)	(0.06)	(0.08)	(0.05)	(0.03)	(0.03)	(0.04)	(0.03)
	Log amount	0.44	0.13	-0.40	0.18	-0.22	0.27	0.23	0.28
Savings		(0.70)	(0.74)	(0.95)	(0.57)	(0.41)	(0.42)	(0.54)	(0.33)
at endline	Disc. exp.	0.11	-0.46	0.65	0.31	0.72*	0.24	-0.72	0.24
		(0.73)	(0.81)	(1.02)	(0.61)	(0.41)	(0.42)	(0.54)	(0.34)

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 parentheses. 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. $N = 605$ for the first four columns of the first panel, $N = 2,320$ for the second panel. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are not statistically different at usual significance levels for any variable.

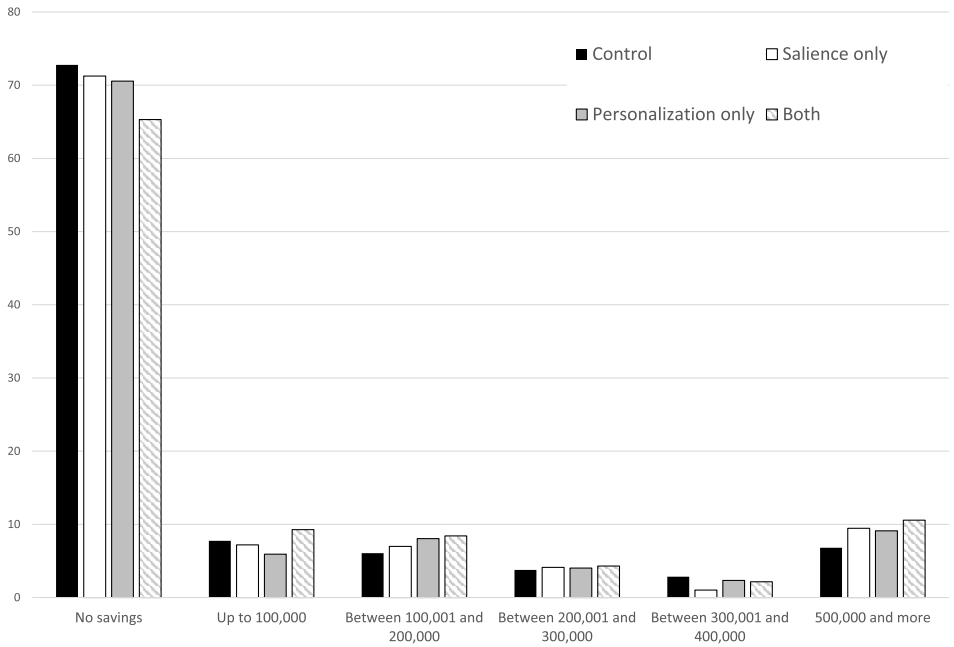
Appendix B. Results when eliminating the last 2 treatments

This section includes the main results of the paper when focusing only on the four more related treatments.



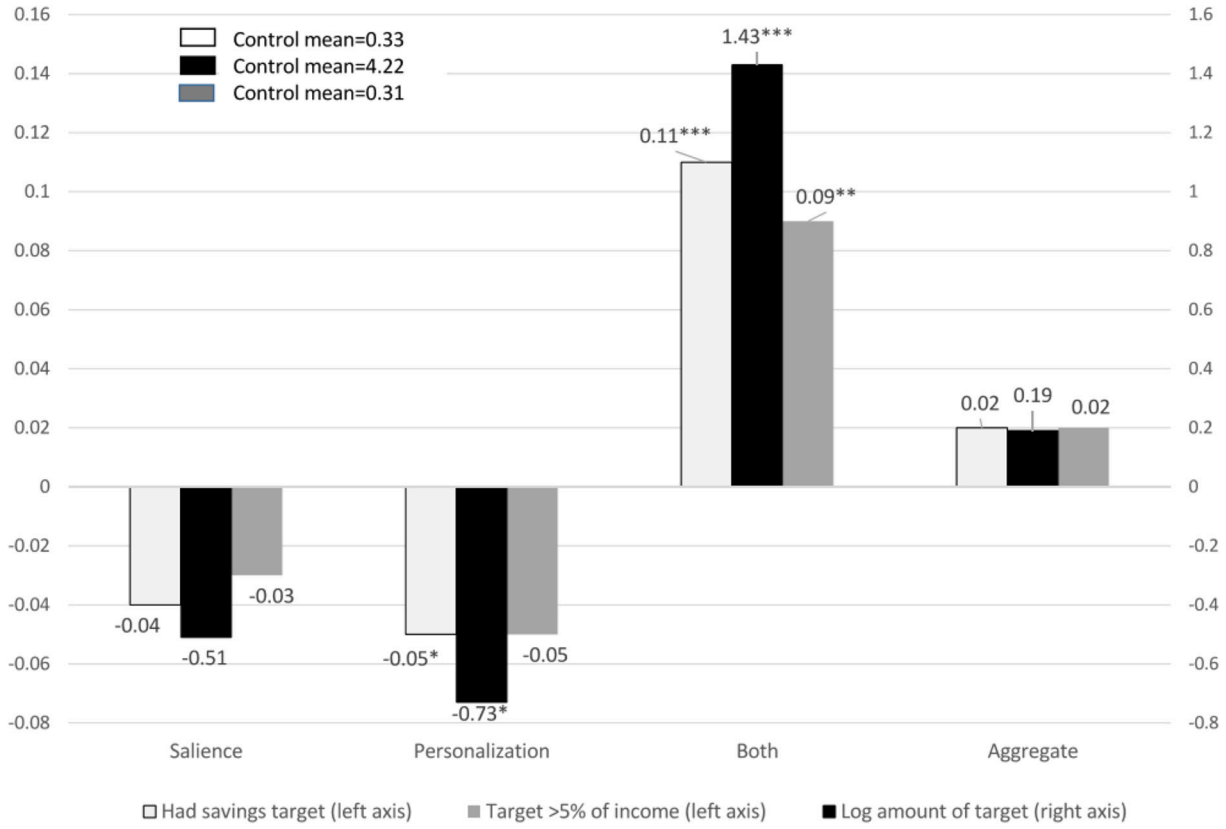
Notes: This graph presents each of the coefficients of regression equation (1) using as outcomes a dummy for whether the individual saved in each month following the baseline. Squares represent β_1, diamonds β_2 and circles β_3. 95 percent confidence intervals are also presented. $N = 1,963$.

Fig. B.1. Impact of Treatments on Whether Individuals Saved During Each Month of Intervention.



Notes: This graph presents the distribution of participants by saving amount ranges during the intervention for each of the treatments. $N = 1,952$.

Fig. B.2. Impact on Amount Saved During the Intervention.



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 white), whether their target was more than 5 percent of their income (in grey) and the log amount of the target (in black). The "Aggregate" columns are the sum of each coefficients. * p<0.1, ** p<0.05, *** p<0.01. N=1,963 for had a savings target and $N = 1,959$ for the amount outcomes.

Fig. B.3. Impact of Treatments on Savings Target In Endline (only T1-T4).

Table B.1
Impact on Savings During the Intervention Period.

	Control Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)	Agg. Effect ($\sum_{i=1}^{3} \beta_i$)
Num of months saved	1.10	-0.03	-0.08	0.37**	0.26*
		(0.13)	(0.13)	(0.19)	(0.14)
Monthly amount saved (log)	3.34	0.24	0.40	0.38	1.02***
		(0.34)	(0.35)	(0.50)	(0.35)
Saved > 5pct of baseline income	0.23	0.02	0.04	0.01	0.08***
		(0.03)	(0.03)	(0.04)	(0.03)

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 parentheses. 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 11 observations with missing amount saved. * p<0.1, ** p<0.05, *** p<0.01. Salience and Personalization are not statistically different at usual significance levels for any variable.

Table B.2
Impact of Treatment Assignment on Treatment Perception (only T1-T4).

	Control Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)
Individual remembers receiving SMS...				
... regarding savings	0.12	0.12***	0.01	0.01
		(0.02)	(0.02)	(0.04)
... urging savings in general	0.03	0.07***	0.02	-0.04*
		(0.02)	(0.01)	(0.02)
... urging saving a specific amount	0.01	0.00	0.01	0.05***
		(0.01)	(0.01)	(0.02)
Individual remembers...				
... being surveyed last year	0.93	-0.02	0.00	0.00
		(0.02)	(0.02)	(0.02)
... being told saving is important	0.54	-0.02	-0.04	0.12***
		(0.03)	(0.03)	(0.04)
... being told about savings potential	0.43	0.02	0.03	0.07
		(0.03)	(0.03)	(0.05)
... being told to save 10pct of income	0.46	0.06*	0.00	0.03
		(0.03)	(0.03)	(0.05)
... comparing credit to savings	0.59	-0.04	-0.09***	0.05
		(0.03)	(0.03)	(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 parentheses. 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. $N = 1,963$. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The following variables present statistically significant differences between Salience and Personalization: "regarding savings" (1%), "urging savings in general" (1%), and "a recommendation to save 10pct of income" (10%). For the rest of the variables, Salience and Personalization are not statistically different at usual significance levels.

Table B.3
Impact on Savings at Endline.

	Control Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)	Agg. Effect ($\sum_{i=1}^{3} \beta_i$)
Any savings last month	0.36	-0.01	0.01	0.02	0.02
		(0.03)	(0.03)	(0.04)	(0.03)
Amount saved (log) in last month	4.40	-0.08	0.25	0.09	0.26
		(0.36)	(0.37)	(0.52)	(0.36)
Saved > 5pct of baseline income	0.29	-0.00	0.03	-0.01	0.02
		(0.03)	(0.03)	(0.04)	(0.03)
Positive budget balance (dummy)	0.69	0.05*	0.02	-0.03	0.04
		(0.03)	(0.03)	(0.04)	(0.03)
Budget balance (log)	5.71	1.43**	0.53	-1.25	0.71
		(0.69)	(0.71)	(1.00)	(0.71)
Expenses on necessities	12.85	-0.23	-0.32	0.49	-0.06
		(0.23)	(0.23)	(0.33)	(0.22)
Discretional expenses	5.88	0.60*	0.11	-0.65	0.07
		(0.36)	(0.37)	(0.53)	(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 parentheses. 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 44 with missing observations for amount saved last month. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are not statistically different at usual significance levels for any variable.

Table B.4
Impact on Other Financial Outcomes.

	Control Mean	Salience (β_1)	Person. (β_2)	SxP (β_3)	Agg. Effect ($\sum_{i=1}^{3} \beta_i$)
	Panel A: Credit market				
Requested credit	0.40	-0.05*	-0.04	0.06	-0.03
		(0.03)	(0.03)	(0.04)	(0.03)
Total amount approved ('000s)	2,522	-752	-1,015**	1,107*	-660
		(502)	(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	1.45	0.14	-0.06	-0.01	0.07
		(0.15)	(0.14)	(0.21)	(0.15)
Thought of using savings next time?	0.33	-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)
	Panel D: Credit Bureau outcomes				
Requests per 100 days	0.78	-0.03	-0.00	0.07	0.04
		(0.05)	(0.05)	(0.08)	(0.05)
Credit score	36.92	0.70	0.81	1.44	2.95
		(2.73)	(2.80)	(3.92)	(2.76)
Probability of having unpaid debt	0.41	0.00	0.01	-0.01	0.00
		(0.03)	(0.03)	(0.05)	(0.03)
Total unpaid debt ('000s Gs)	2,361	1,680*	544	-1,449	778
		(893)	(638)	(1,135)	(616)

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 parentheses. 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 "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? * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are not statistically different at usual significance levels for any variable.

Table B.5
Impact on Savings by Financial Sophistication at Baseline.

		Not sophisticated				Sophisticated			
		Sal. (β_1)	Per. (β_2)	SxP (β_3)	Agg. ($\sum_{i=1}^{3}\beta_i$)	Sal. (β_1)	Per. (β_2)	SxP (β_3)	Agg. ($\sum_{i=1}^{3}\beta_i$)
Panel A: By financial literacy									
Savings during messaging	# Months	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)
	Log amount	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)
	Has one	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 post messaging	Log amount	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)
	> 5% 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)
Savings at endline	Log amount	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									
Savings during messaging	# Months	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)
	Log amount	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)
	Has one	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 post messaging	Log amount	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)
	> 5% 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)
Savings at endline	Log amount	-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 parentheses. 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. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are not statistically different at usual significance level for any variable in Panel A. In Panel B, the following variables present statistically significant differences between Salience and Personalization: “had a savings target last month” (at 10% on no sophisticated and no difference on sophisticated), “amount of savings target (log)” (at 10% on not sophisticated and at 10% on sophisticated), “target > 5pct of baseline income” (at 10% on not sophisticated and no difference on sophisticated), and “discr. exp.” (no difference on not sophisticated and at 5% on sophisticated).

Table B.6
Impact on Savings by Budget Management Coherence.

		Less coherent				More coherent			
		Sal. (β_1)	Per. (β_2)	SxP (β_3)	Agg. ($\sum_{i=1}^{3}\beta_i$)	Sal. (β_1)	Per. (β_2)	SxP (β_3)	Agg. ($\sum_{i=1}^{3}\beta_i$)
Panel A: By discrepancy between savings and budget balance									
Savings during messaging	# Months	-0.06	-0.21	0.48*	0.22	0.00	0.04	0.25	0.30
		(0.20)	(0.19)	(0.28)	(0.21)	(0.17)	(0.18)	(0.26)	(0.19)
	Log amount	0.01	0.11	0.83	0.95*	0.46	0.70	-0.07	1.09**
		(0.51)	(0.50)	(0.74)	(0.53)	(0.46)	(0.48)	(0.69)	(0.47)
	Has one	-0.07	-0.07*	0.15**	0.01	-0.01	-0.04	0.07	0.03
		(0.04)	(0.04)	(0.06)	(0.04)	(0.04)	(0.04)	(0.06)	(0.04)
Target post messaging	Log amount	-0.89	-0.95*	2.00**	0.17	-0.13	-0.51	0.86	0.23
		(0.55)	(0.52)	(0.78)	(0.57)	(0.53)	(0.54)	(0.77)	(0.54)
	> 5% inc.	-0.07	-0.06	0.13**	-0.00	0.01	-0.03	0.05	0.04
		(0.04)	(0.04)	(0.06)	(0.04)	(0.04)	(0.04)	(0.06)	(0.04)
Savings at endline	Log amount	-0.24	-0.15	0.76	0.37	0.10	0.67	-0.58	0.19
		(0.52)	(0.53)	(0.77)	(0.55)	(0.49)	(0.51)	(0.71)	(0.49)
	Discr. exp.	1.33**	0.72	-1.27*	0.79	-0.15	-0.47	0.00	-0.62
		(0.54)	(0.54)	(0.77)	(0.54)	(0.49)	(0.52)	(0.73)	(0.51)
Panel B: By whether took a loan year before baseline									
Savings during messaging	# Months	0.30	-0.14	-0.05	0.11	-0.14	-0.06	0.52**	0.32**
		(0.29)	(0.26)	(0.39)	(0.27)	(0.15)	(0.15)	(0.22)	(0.16)
	Log amount	0.86	0.68	-0.68	0.86	0.02	0.31	0.75	1.08***
		(0.69)	(0.70)	(1.01)	(0.67)	(0.40)	(0.40)	(0.59)	(0.41)
	Has one	-0.06	-0.15***	0.12	-0.09	-0.03	-0.02	0.11**	0.06*
		(0.06)	(0.06)	(0.08)	(0.06)	(0.03)	(0.03)	(0.05)	(0.04)
Target post messaging	Log amount	-0.68	-2.00***	1.60	-1.08	-0.44	-0.27	1.39**	0.68
		(0.76)	(0.71)	(1.06)	(0.75)	(0.44)	(0.44)	(0.64)	(0.46)
	> 5% inc.	-0.01	-0.12**	0.09	-0.05	-0.03	-0.02	0.09*	0.04
		(0.06)	(0.05)	(0.08)	(0.06)	(0.03)	(0.03)	(0.05)	(0.03)
Savings at endline	Log amount	0.05	0.23	-0.35	-0.06	-0.13	0.24	0.28	0.39
		(0.69)	(0.71)	(1.02)	(0.68)	(0.42)	(0.43)	(0.61)	(0.43)
	Discr. exp.	0.72	0.64	-0.95	0.40	0.46	-0.01	-0.60	-0.15
		(0.71)	(0.72)	(1.04)	(0.73)	(0.43)	(0.44)	(0.62)	(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 parentheses. 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. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Salience and Personalization are not statistically different at usual significance levels for any variable in Panel A. In Panel B, the following variables present statistically significant differences between Salience and Personalization (only on less coherent): "target amount (log)" (10%), and "target > 5% of inc." (5%).

References

Akbas, Merve, Ariely, Dan, Robalino, David A., Weber, Michael, 2016. How to Help Poor Informal Workers to Save a Bit: Evidence from a Field Experiment in Kenya. IZA Discussion Papers 10024. Institute for the Study of Labor (IZA).

Ashraf, Nava, Dean, Karlan, Yin, Wesley, 2006. Tying Odysseus to the mast: evidence from a commitment savings product in the Philippines. Q. J. Econ. 121 (2), 635–672.

Attanasio, Orazio, Bird, Matthew, Cardona-Sosa, Lina, Lavado, Pablo, 2019. Freeing Financial Education via Tablets: Experimental Evidence from Colombia. Supplementary Material. Working Paper 25929. National Bureau of Economic Research.

Azevedo, Viviane, Lafortune, Jeanne, Olarte, Liliana, Tessada, José, 2020. Does Formal Credit Lead to Financial Inclusion or Distress? Results Using an Algorithm-based Loan among Underserved Clients in Paraguay. Unpublished manuscript.

Banerjee, Abhijit, Martinez, Claudia, Puentes, Esteban, 2018. Incentives to save. Unpublished Mimeo.

Beshears, John, Choi, James J., Harris, Christopher, Laibson, David, Madrian, Brigitte C., Sakong, Jung, 2020. Which early withdrawal penalty attracts the most deposits to a commitment savings account? J. Public Econ. 183, 104144.

Bronchetti, Erin T., Kessler, Judd B., Magenheim, Ellen B., Taubinsky, Dmitry, Zwick, Eric, 2023. Is attention produced optimally? Theory and evidence from experiments with bandwidth enhancements. Econometrica 91 (2), 669–707.

Bronchetti, Erin Todd, Huffman, David B., Magenheim, Ellen, 2015. Attention, intentions, and follow-through in preventive health behavior: field experimental evidence on flu vaccination. J. Econ. Behav. Organ. 116, 270–291.

Cagala, Tobias, Glogowsky, Ulrich, Rincke, Johannes, 2024. Detecting and preventing cheating in exams: evidence from a field experiment. J. Hum. Resour. 59 (1), 210–241.
Caplin, Andrew, Dean, Mark, 2015. Revealed preference, rational inattention, and costly information acquisition. Am. Econ. Rev. 105 (7), 2183–2203.
Caplin, Andrew, Csaba, Dániel, Leahy, John, Nov, Oded, 2020. Rational inattention, competitive supply, and psychometrics*. Q. J. Econ. 135 (3), 1681–1724.
Carpena, Fenella, Cole, Shawn, Shapiro, Jeremy, Zia, Bilal, 2011. Unpacking the causal chain of financial literacy. Policy Research Working Paper Series 5798. The World Bank.
Carrera, Mariana, Royer, Heather, Stehr, Mark, Sydnor, Justin, Taubinsky, Dmitry, 2018. The limits of simple implementation intentions: evidence from a field experiment on making plans to exercise. J. Health Econ. 62, 95–104.
Cavallo, Eduardo, Frisancho, Verónica, Karver, Jonathan, 2016. The state of saving in Latin America and the Caribbean. In: Cavallo, Eduardo, Serebrisky, Tomás (Eds.), SAVING FOR DEVELOPMENT: How Latin America and the Caribbean Can Save More and Better, IDB Publications (Book Chapters). Inter-American Development Bank, pp. 21–43. Chapter 2.
Clark, Damon, Gill, David, Prowse, Victoria, Rush, Mark, 2020. Using goals to motivate college students: theory and evidence from field experiments. Rev. Econ. Stat. 102 (4), 648–663.
Cole, Shawn, Sampson, Thomas, Zia, Bilal, 2011. Prices or knowledge? What drives demand for financial services in emerging markets? J. Finance 66 (6), 1933–1967.
Collins, Daryl, Morduch, Jonathan, Rutherford, Stuart, Ruthven, Orlanda, 2009. Portfolios of the Poor: How the World's Poor Live on $2 a Day. Princeton University Press.
Cunha, Nina, Lichand, Guilherme, Madeira, Ricardo, Bettinger, Eric, 2017. What is It About Communicating with Parents? Technical Report. Stanford University. Unpublished Mimeo.
Damgaard, Mette Trier, Gravert, Christina, 2018. The hidden costs of nudging: experimental evidence from reminders in fundraising. J. Public Econ. 157, 15–26.
Demirgüç-Kunt, Asli, Klapper, Leora, Sunger, Dorothe, Ansar, Saniya, Hess, Jake, 2017. Global Financial Inclusion (Global Findex) Database. Technical Report. World Bank.
Dillard, James, Shen, Lijiang, 2005. On the nature of reactance and its role in persuasive health communication. Commun. Monogr. 06 (72), 144–168.
Dupas, Pascaline, Robinson, Jonathan, 2013. Why don't the poor save more? Evidence from health savings experiments. Am. Econ. Rev. 103 (4), 1138–1171.
Dupas, Pascaline, Karlan, Dean, Robinson, Jonathan, Ubfal, Diego, 2018. Banking the unbanked? Evidence from three countries. Am. Econ. J. Appl. Econ. 10 (2), 257–297.
Fuentes, Olga, Lafortune, Jeanne, Riutort, Julio, Tessada, José, Villatoro, Félix, 2024. Personalized information as a tool to improve pension savings: results from a randomized control trial in Chile. Econ. Dev. Cult. Change 72 (2), 725–770.
Gargano, Antonio, Rossi, Alberto, 2024. Goal setting and saving in the FinTech era. J. Finance. https://doi.org/10.1111/jofi.13339.
Gertler, Paul, Higgins, Sean, Scott, Aisling, Seira, Enrique, 2021. The Long-Term Effects of Temporary Incentives to Save: Evidence from a Prize-Linked Savings Field Experiment. Unpublished Working Paper.
Goldberg, Jessica, 2014. Products and policies to promote saving in developing countries. IZA World Labor, 74.
Karlan, Dean, McConnell, Margaret, Mullainathan, Sendhil, Zinman, Jonathan, 2016. Getting to the top of mind: how reminders increase saving. Manag. Sci. 62 (12), 3393–3411.
Kast, Felipe, Meier, Stephan, Pomeranz, Dina, 2018. Saving more in groups: field experimental evidence from Chile. J. Dev. Econ. 133, 275–294.
Lee, David S., 2009. Training, wages, and sample selection: estimating sharp bounds on treatment effects. Rev. Econ. Stud. 76 (3), 1071–1102.
Maćkowiak, Bartosz, Matějka, Filip, Wiederholt, Mirko, 2023. Rational inattention: a review. J. Econ. Lit. 61 (1), 226–273.
McKenzie, David, Mohpal, Aakash, Yang, Dean, 2022. Aspirations and financial decisions: experimental evidence from the Philippines. J. Dev. Econ. 156, 102846.
Pomeranz, Dina, Kast, Felipe, 2024. Savings accounts to borrow less: experimental evidence from Chile. J. Hum. Resour. 59 (1), 70–108.
Prina, Silvia, 2015. Banking the poor via savings accounts: evidence from a field experiment. J. Dev. Econ. 115, 16–31.
Rodríguez, Catherine, Saavedra, Juan E., 2019. The persistent effects of youth savings reminders: experimental evidence from text-message campaigns in Colombia. J. Dev. Econ. 139, 135–156.
Seshan, Ganesh, Yang, Dean, 2014. Motivating migrants: a field experiment on financial decision-making in transnational households. J. Dev. Econ. 108 (C), 119–127.
Stango, Victor, Zinman, Jonathan, 2009. Exponential growth bias and household finance. J. Finance 64 (6), 2807–2849.