When the message hurts:
The unintended impacts of nudges on saving*

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April 5, 2022

Abstract

We implement a field experiment in Romania to elucidate how informational nudges and goal setting impact saving. We find no evidence that text message reminders, either in the form of a general reminder or information about the savings goals of peers, encourage saving. Further, both types of messages discourage saving for participants who set a goal, particularly among high goal setters. We posit that informational nudges unintentionally increase the salience of unrealistic goals and engender boomerang effects that discourage high goal setters from saving. Among participants who received no messages, those who set goals save more, suggesting a tradeoff between commitment devices and informational nudges in this context.

Keywords: Saving, nudges, commitment devices, reminders, peer information, goal setting, boomerang effects, middle-income, Romania
JEL Codes: O16, D14, C93

1 Introduction

Financial inclusion is a concern of governments, international organizations, and commercial institutions alike. On a macro level, financial development stimulates economic growth (Jayaratne and Strahan, 1996; Black and Strahan, 2002; Levine, 2005) and reduces income inequality (Beck et al., 2007). On a micro level, financial access is critical for poverty alleviation (Burgess and Pande, 2005; Honohan, 2004), and economic welfare more generally (Allen et al., 2016).

While financial inclusion embodies several concepts, such as banking, access to credit, and risk management, interventions aiming to promote financial inclusion emphasize saving as a crucial first step, noting its role in mitigating shocks and facilitating investment (Demirgüç-Kunt et al., 2020). Often, a central feature in the design of such interventions is the use of informational nudges, like messaging, and commitment devices, such as goal setting, that are intended to incentivize saving behavior. Evaluations of these interventions, however, show mixed results.[1] Recent experiments in low-income settings have shown a positive impact of messaging and goals on saving, where messaging takes different forms, from general reminders about one's goal to peer information (Karlan et al., 2016; Kast et al., 2018; Rodriguez and Saavedra, 2019). In high-income settings the results are more mixed and suggest that nudges may sometimes discourage saving (Beshears et al., 2015; Grinstein-Weiss et al., 2017; Raue et al., 2020; Dur et al., 2021).

Nudges can be powerful interventions, but occasionally they may fail to achieve their objectives, and in some cases even backfire. Understanding the potential heterogeneity of treatment effects, and in particular, how nudges interact with other incentives, is thus key to the successful design of such interventions (Beshears and Kosowsky, 2020).

We design a randomized intervention to elucidate how informational nudges, including general reminders and peer information, interact with goal setting to impact saving behav-

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[1]See Steinert et al. (2018) and Duvendack and Mader (2020) for recent surveys.

ior. General reminders are thought to increase saving by addressing limited attention and promoting salience.[2] While there are several theoretical reasons why peer information should also encourage saving, from belief updating to social benchmarking,[3] in some settings peer information has been shown to discourage saving (Beshears et al., 2015). Moreover, while goal setting theoretically helps to overcome self-control problems, and is a standard feature in most interventions aiming to promote saving, goal setting can sometimes interact with nudges in discouraging ways. For example, if peer information leads to a "boomerang effect" (Clee and Wicklund, 1980), such as oppositional reactions or negative belief updating, messaging may reduce saving among people whose goals are inconsistent with their peers.[4] General reminders may also unintentionally decrease saving among those who set unrealistic goals by increasing the salience of said goals.[5]

Our randomized intervention specifically tests whether distinct types of messaging – general reminders versus peer information – have a differential impact on saving, and further, whether these messages interact with goal setting in unintended ways. We implement our experiment among a random sample of 500 farmers in Romania, which is classified by the World Bank as an upper-middle income country, with a per capita GDP of $12,400 USD in 2018 at the time of our experiment. Financial inclusion levels in Romania are some of the lowest in Europe and Central Asia, where just 14 percent of adults formally save. We focus explicitly on farmers, as they represent almost a quarter of the labor force and a significant portion of the unbanked in Romania, suggesting that the potential gains from financial inclusion are large among this group.

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[2]Karlan et al. (2016) develop a theoretical model where limited attention to exceptional expenses generates undersaving, which is mitigated by reminders. Akerlof (1991) emphasizes the role of salience rather than costly self-control as a driver of procrastination. Bordalo et al. (2013) develop a model of salience as a driver of consumer choice.

[3]See Schultz et al. (2007) for a discussion of peer information in psychology. SeeBreza and Chandrasekhar (2019) for a discussion of peer information in financial decision-making.

[4]See Beshears et al. (2015) for oppositional reactions in savings; see Schultz et al. (2007) for negative belief updating in the context of energy consumption.

[5]See Harding and Hsiaw (2014) in the context of energy conservation; see Hsiaw (2013) for the underlying theory of unrealistic goal-setting and self-control.

At the beginning of the experiment, each participant was presented with an offer to join our home-savings bank and informed that after three months, the research team would revisit to observe the total accumulated savings and pay one percent interest on the balance. At baseline, all participants were asked whether they would like to set a savings goal and if so, how much they wished to save over the following months. In addition, participants were randomized into one of three groups. In the first group, participants received a general text message reminder to save approximately half-way through the study period. In a second group, participants received the general reminder and were also informed of the average savings goals of their peers. In a third, control group, participants received no text messages.

We find that on average, 17 percent of participants saved in the experimental account, accumulating 488 Lei ($122 USD) over the three-month period.[6] We verify that engagement with the experimental account is unrelated to the participant's (mis)trust in strangers or to possible hoarding behavior prior to follow-up. Thus, relative to low baseline financial engagement, in which 27 percent of participants reported owning a formal savings account with an average balance of 700 Lei ($175 USD), engagement with the experimental account is qualitatively meaningful.

We then estimate the impact of messaging on saving behavior. First, we examine the effect of receiving any message – reminder or peer – on the propensity to save and find no significant difference between treatment and control. We then estimate the separate effect of reminders versus peer information and find that while both types of messaging appear to reduce the propensity to save, the difference is neither statistically significantly different from the control group nor between the two treatment groups.

Next, we examine heterogeneity by whether the participant set a savings goal. While the option to set a goal was given to all participants, the decision to set a goal was elicited prior to random treatment assignment, allowing for causal inference of the heterogeneous treatment effect. In addition, the proportion of those setting a goal (30 percent of the

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[6]Unconditional on saving. At the time of the experiment, 1 Lei was approximately $0.25 USD.

sample) is statistically equivalent across treatment and control, and is balanced across a host of observable baseline characteristics, suggesting exogenous variation in goal setting. In the control group with no messaging, those who set a goal were 15 percentage points more likely to save in the experimental account and saved 125 percent more on average than those who did not set a goal. However, in the treatment group, any messaging – reminder or peer – reduced the propensity to save for those who set a goal by 21 percentage points. When we disaggregate the treatment into reminder versus peer, we find that both types of messaging reduce the propensity to save equally. Tests for selection on observables (Oster, 2016) suggest that the results are not driven by potentially confounding unobservable factors. Further, when we examine the effect of messaging on those who did not set a goal, we find that while the coefficients are positive, they are qualitatively small and statistically insignificant, suggesting that on its own, messaging has no impact on saving.

These findings suggest important implications for the design of savings interventions in similar settings. While encouraging customers to set goals can promote saving, reminding customers to save can reverse the gains of goal setting. Further, in the absence of goal setting, reminders may not hurt the propensity to save, but they also may not increase it. Therefore, adopting simple commitment devices, such as goal setting, and foregoing informational nudges, may be the more effective way to promote financial inclusion in similar settings.

We then attempt to elucidate why messaging negatively interacts with goal setting in our sample by comparing those who set high goals (i.e., above the mean savings goals of their peers) with those who set low or no goals. Here we find that for people who set high goals, messaging reduces the propensity to save by 23 percentage points. Furthermore, general reminders and peer information have a statistically equivalent impact on reducing savings among high goal setters.

We posit that messaging negatively interacts with high goal setting for two reasons. First, for the group that received peer information, high-goal setters appear to update their

beliefs downward, closer to that of their peers, exhibiting behavior that is consistent with a boomerang effect (Clee and Wicklund, 1980), such as negative belief updating (Schultz et al., 2007). Second, for the group that received general reminders, we argue that the reminder to save increased the salience of unrealistic savings goals among high goal setters. Hsiaw (2013) shows theoretically how unrealistic goals can discourage agents from exercising self-control, while Harding and Hsiaw (2014) show in the context of energy conservation that unrealistic goal setters were less able to conserve energy consumption. Our findings are consistent with this phenomenon, suggesting that increasing the salience of unrealistic goals can also disincentivize financial savings.

As a final exercise, we examine how our randomized messages interact with important behavioral and institutional factors that are known to influence savings, including present-bias, trust in financial institutions, and geographic proximity to banks. Several studies reveal that time preferences play an important role in determining savings behavior.[7] We focus specifically on present-bias, which is a form of time-inconsistent preferences that have been shown to reduce savings in low-income settings (Ashraf et al., 2006; Tanaka et al., 2010; Bauer et al., 2012; Dupas and Robinson, 2013; Giné et al., 2018). Trust is also an important determinant of financial behavior, particularly among low-income households (Agnew et al., 2012), while trust in financial institutions has been shown to impact preferences for formal savings, particularly in Central and Eastern Europe (Stix, 2013). Finally, geographic proximity is a significant predictor of bank account ownership and nonbank financial transactions in both high and low-income settings (Goodstein and Rhine, 2017; Dupas et al., 2018). In our sample, we find that while proximity to bank is uncorrelated with experimental savings, present-biased participants are more likely to save in the experimental account, as well as those with more trust in financial institutions. Importantly, however, there is no meaningful interaction with the messaging treatments along these dimensions, suggesting that informational nudges neither attenuate nor exacerbate underlying behavioral and institutional

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[7]See, e.g., Choi and Han (2018) and Epper et al. (2020).

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factors that are known to influence savings.

Our findings relate to several literatures. First, we contribute to the work on messaging and financial behavior.[8] Specifically in regard to savings, Karlan et al. (2016) test the impact of messages that highlight savings goals versus financial incentives and find that both types increase saving. Messages that are framed in terms of a new beginning have also been shown to increase retirement savings (Beshears et al., 2021). Messages that include peer information, however, are not always effective. While several studies have documented peer effects in financial decision-making,[9] recent field experiments on peer messaging show that information about peer behavior (Raue et al., 2020) and peer pressure (Kast et al., 2018) may increase saving in some settings and discourage saving in others (Beshears et al., 2015). Furthermore, even when messaging promotes saving, it remains unclear whether peer messaging is more effective than general reminders to save.

Our experimental design teases out the relative importance of general reminders versus peer information and finds that, at least in our setting, there is no differential impact between the two approaches, but that both types of messages negatively interact with goal setting to discourage saving. Our study is one of the first to examine this heterogeneity, suggesting an important trade-off between goal setting and informational nudges in upper-middle income settings. This finding relates directly to the literature on goal setting, where a rich theoretical literature establishes the role of goals in managing self-control problems,[10] and in particular, how unrealistic goals can lead to sub-optimal choices (Hsiaw, 2013). Our findings are consistent with empirical work on goal setting and energy conservation, which shows that consumers who choose realistic goals to reduce energy consumption save substantially more (Harding and Hsiaw, 2014).

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[8]See, e.g., Karlan and Zinman (2014) and Cadena and Schoar (2011) who test the impact of reminders on loan repayment.

[9]See for example Duflo and Saez (2003) on 401(k) participation in the US and Lieber and Skimmyhorn (2018) on charitable giving in the US.

[10]See, among others, Koch and Nafziger (2011); Suvorov and van de Ven (2008); Loewenstein (2007); Locke and Latham (2002); Heath et al. (1999); Latham and Locke (1991); Bandura (1989); Kahneman and Tversky (1979).

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We further contribute to the consumer psychology literature on boomerang effects (Clee and Wicklund, 1980; Ringold, 2002), where the provision of information about the behavior of one's peers can lead to unintended consequences in the treatment group. Our results relate most closely to boomerang effects observed by Beshears et al. (2015), who find that providing peer information reduces 401(k) participation in the US. The specific boomerang effect documented there is an oppositional reaction, in which participants shift their behavior away from their peers upon realizing that their peers were saving more. In our setting, the observed boomerang effect is more consistent with negative belief updating, in which participants shift their behavior downward, toward their peers, upon learning that their own behavior is less common than previously believed.[11]

Finally, most of the research on financial inclusion interventions comes from high income countries, like the US, or low-income settings in Africa, Southeast Asia, or Latin America. We know far less about the nuances of interventions in upper-middle income countries, such as Romania. Yet, some of the largest potential gains from financial inclusion stand to be made in these places, where financial inclusion is low, but incomes are relatively high. Per capita income in Romania is among the highest in Eastern Europe and Central Asia, but financial inclusion is among the lowest.[12] On average, 58 percent of adults have a bank account and the share of adults formally saving is 14 percent – the lowest share in the European Union.[13] Financial inclusion in Romania is still negatively affected by low confidence in financial institutions, increased preference for cash, and poor financial education,[14] such that the scope for successful interventions is large. Our findings suggest that the design of

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[11]Boomerang effects are common in other contexts, too. See, for example, Schultz et al. (2007) in regard to energy consumption, Bhargava and Manoli (2015) in regard to the Earned Income Tax Credit in the US, Fellner et al. (2013) on tax compliance, Ashraf et al. (2014) on the student performance in community health worker training, Bursztyn et al. (2020) on preferences for female labor supply, and Robinson et al. (2021) in the context of awards to motivate student attendance.

[12]According to the World Bank, in 2017 globally about 1.7 billion adults remain unbanked and there were 116 million unbanked adults in in Europe and Central Asia. The majority live in Romania, the Russian Federation, Turkey, Uzbekistan, and Ukraine.

[13]IMF Global Findex database, 2017.

[14]National Bank of Romania - Financial Stability Report, December 2019.

such interventions may be context specific. We test some of the most popular features of recent interventions and find that what is successful in low-income settings is not always suitable in higher-income settings. In particular, our findings are consistent with work in the US, which shows that informational nudges can actually harm incentives to save, especially when they increase the salience of unrealistic goals or lead to unintended boomerang effects.

The rest of this article proceeds as follows. Section 2 details the setting and design of our field experiment. Section 3 presents the summary statistics. Section 4 details the empirical results. Section 5 concludes.

2 Experimental design

We conducted our experiment over five months, from November 2017 to March 2018, among a random sample of farmers living in Suceava county, a rural county in northeast Romania. We focus specifically on farmers for several reasons. First, agriculture is one of the primary sectors in the Romanian economy, with over 23 percent of the labor force employed in agricultural activities – the highest in all of Europe (Eurostat, 2017). In December 2017, monthly average net earnings in agriculture (1844 Lei = $461 USD) were one of the lowest in the economy, around 30 percent lower than the average (2629 Lei = $657 USD) according to the Romanian National Institute for Statistics, making farmers some of the poorest households in the country. Second, farmers represent a significant portion of the unbanked in Romania; in our sample, only 27 percent of farmers had a formal savings account at baseline, compared to 58 percent in the national average, and of those who had an account, only 20 percent made deposits on a regular basis.

This is likely due to the fact that farmers are often paid in cash and at irregular intervals. Specific statistics on financial inclusion of farmers are unavailable. However among the general population, nearly 60 percent of the unbanked in Romania are from the poorest 40 percent of households and about 3 million unbanked adults in Romania receive payments, such as wages, social benefits, or pensions, in cash (Demirgüc-Kunt et al., 2019). The

irregular nature of agricultural income, in combination with a poorly developed banking sector,[15] especially in rural areas, make it difficult for farmers to interface with automatic banking services that have been shown to incentivize savings in other contexts (Madrian and Shea, 2001; Taler and Benartzi, 2004; Grinstein-Weiss et al., 2017; Roll et al., 2020), necessitating a different approach to financial inclusion in this setting.

To construct our experimental sample, we implemented a stratified random sampling from a list, provided by the Agency of Payments and Intervention for Agriculture (APIA), of all farmers in Suceava county receiving support from the European Agricultural Guarantee Fund.[16] We first selected a random sample of localities (called comuna) to ensure balance across particular geographic and historical factors. We restricted selection to comuna that are within 15 km of the historical border between the former Habsburg and Ottoman empires, given recent evidence suggesting that institutional legacies impact savings in this region (Walker, 2020; Grosjean, 2011). From a total of 41 comuna that met our geographic criteria, we randomly selected 27 for the experiment: 9 comuna in former Habsburg regions, 9 comuna in former Ottoman regions, and 9 comuna split by the historical border. Within each comuna, we then randomly selected 20 farmers to invite to the experiment, for a total target of 540 participants.[17]

Our experimental design consisted of several components. We first administered a baseline survey to each of the selected participants privately in their homes to collect information on basic demographic characteristics, risk and time preferences, engagement with various financial services, farm attributes, and trust in financial institutions. Time preferences were measured using a multiple price list (MPL), which asked the participant to decide whether they would prefer to receive a lower sum of cash in a near period or a larger amount in a

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[15]Financial intermediation (measured as the ratio of total private sector credit to GDP) was 25.6% of GDP in 2018, roughly three times below the EU average.

[16]APIA operates under the Ministry of Agriculture and Rural Development based on Law 1/2004 and since January 2007 has been administering support measures financed by the European Agricultural Guarantee Fund (EAGF) (http://www.apia.org.ro/).

[17]Comuna correspond to the NUTS5 administrative level in Romania and have an average population of 3,388 inhabitants (Romanian National Institute for Statistics). There are 114 total comuna in our sample county.

later period. We use these questions to measure the extent to which the participant demonstrates hyperbolic discounting (or present bias).[18] We elicited risk preferences using the method developed by Eckel and Grossman (2008). This method is a simple way of measuring risk preferences and has been widely used in the literature, especially in populations where participants have lower math abilities (Charness et al., 2013).

We construct several measures of socio-economic status, which should correspond to the propensity to save. Farm area is a proxy for land wealth. In addition, we count the total pieces of farm equipment owned, including: tractor, trailer, hand plow, truck, cultivator, chemical applicator, mechanical seeder, thresher, tractor plow, and bale press. We construct an index of consumer durables ownership, where participants are assigned one point for each of the following items they own: refrigerator, freezer, automatic washing machine, audio equipment, computer, cell phone, television, bicycle, motorcycle, moped, and car. Finally, we ask the respondent to report the type of saving methods they use, as well as total value of all formal and informal savings at baseline.

At the end of the survey, all participants were presented with an offer to join our experimental savings bank and given a small box to deposit their savings into over the study period. The enumerator informed the participant that our team would return in three months to conduct a follow-up interview and pay one percent interest on the total savings accumulated in their experimental account. To incentivize saving in the experimental account, we specifically chose an interest rate that was slightly higher than the average bank interest rate at the time of the study, but not so high so as to set unrealistic incentives.[19] At the time the savings boxes were administered, but prior to treatment, participants were also asked whether they would like to set a savings goal for their experimental account, and if so, how much they wished to save.

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[18]See, e.g., Frederick et al. (2002) Cohen et al. (2020) for a review of literature on time discounting and preferences. See Ashraf et al. (2006); Tanaka et al. (2010); Bauer et al. (2012); Dupas and Robinson (2013); Giné et al. (2018) for applications to savings

[19]According to National Bank of Romania (www.bnr.ro/Statistics-report-1124.aspx) the average interest rates for Lei denominated deposits in November 2017 was 0.92%, suggesting that our experimental home savings bank was slightly more profitable than saving in a formal bank.

We designed the experimental account as a savings box to be kept at home for several reasons. First, given the rural nature of our experimental setting, simply offering participants the option to open an account with an existing bank is not feasible. 70 percent of participants do not have a bank in their village and would have to travel on average more than five km to the nearest village to access a physical bank. Given that only 58 percent of these participants own a car, and in the absence of accessible public transportation, the transaction costs involved with traveling to a physical bank are not trivial. Moreover, previous studies suggest that indirect transaction costs, such as travel time and distance to the nearest bank, are a significant barrier to saving.[20] Second, while access to banks is balanced across treatment and control, financial under-development presents a logistical barrier to saving that we overcome by bringing the experimental bank directly to participants, ensuring that the transaction costs of saving are equal for all subjects. Third, many banks have account opening fees and minimum deposit requirements, which have been shown to inhibit financial engagement in other contexts (Dupas and Robinson, 2013).

Saving at home is not uncommon in our sample. Of the 40 percent of participants who report saving informally (i.e., outside of a bank), 52 percent save cash at home. Other common methods include illiquid agricultural assets, such as animals (57 percent) and grain inventory (37 percent). In this context, our home savings bank is consistent with the preferences of its potential customers, while at the same time providing aspects of formal savings, such as interest, liquidity, and observability of balances.

Nonetheless, one may be concerned that the design of the savings boxes may over or understate incentives to save. For instance, participants may decide to put all of their cash in the box on the day that the survey team arrives or pool cash from friends and family members to reap the interest rather than save on a regular basis. While we cannot directly observe this behavior, we did ask respondents during the follow-up survey how frequently they deposited in the experimental account, as well as the average amount deposited. Conditional on saving,

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[20]See, e.g., Goodstein and Rhine (2017) in the US and Dupas et al. (2018) in sub-Saharan Africa.

94 percent of respondents reported saving on a weekly (44 percent) or monthly basis (50 percent), while the average deposit amount was between 100 and 500 Lei. This is consistent with the savings amounts counted by enumerators, where the median total savings was 1100 Lei conditional on saving. This amount is in line with average incomes of farmers (around 23,000 Lei annual), particularly considering that our study was conducted right after the harvest when incomes are highest.

Alternatively, one may think that the home savings incentives were insufficient for farmers who may accumulate wealth by investing in their farms rather than saving. During our study period, however, estimates from the European Union indicate that the return on assets for Romanian farmers was negative, suggesting that saving is a superior investment to farm assets.[21] Moreover, given the risky nature of agriculture and the absence of formalized insurance markets for farming in Romania, saving is a primary method for risk-smoothing among our sample.

One may further wonder whether participants' (mis)trust in the research team could impact engagement with the experimental account. If participants did not believe that the enumerators would return to pay interest on the balance, this would diminish incentives to save. In the survey, we asked participants to report how much they trusted people they met for the first time: (i) completely, (ii) somewhat, (iii) not very much, and (iv) not at all. We create a dummy variable equal to one if the participant reported (i) or (ii), and zero otherwise, and correlate this measure of trust with saving in the experimental account (Appendix Table 1). There is no correlation between trust in strangers and engagement with the experimental account.

To capture the impact of messaging on savings, we randomized participants, within each comuna, into one of three groups. Participants in the control group received only the savings box. In a second group, participants received the savings box and were also sent one text message after eight weeks to remind them to save. We call this the reminder treatment. The

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[21]Source: https://ec.europa.eu/info/sites/default/files/food-farming-fisheries/farming/documents/agricultural-capital-land-value_en.pdf.

reminder message stated: "Thank you for participating in this research. This is a friendly reminder to save. We will be visiting you sometime in March to pay your accumulated interest."

In a third group, participants received the box and text message reminder, but in addition to being reminded to save, they also received information about the average savings goal of all participants in the study. We call this the peer treatment. The peer message stated: "Thank you for participating in this research. This is a friendly reminder to save. On average, participants in this study expect to save 900 Lei. We will be visiting you sometime in March to pay your accumulated interest."[22]

At the time the savings boxes were administered, participants were given no information regarding the messaging treatments.

We hired a local survey company to administer the baseline and follow-up surveys, conduct the time and risk elicitation tasks, send the text message reminders, and count and pay interest on the savings at the end of the experimental period. Baseline surveys were administered in late November and early December 2017, just after the harvest and at a time when farmers had been recently paid for their crop yields. Follow-up surveys and interest payments were completed in early March 2018.

3   Summary statistics

Of the 540 randomly selected farmers who were invited to the experiment, 503 participated in the baseline survey and were given savings boxes. Of the 503 baseline participants, 412 completed the follow-up and presented their experimental savings accounts for interest payments. Appendix Table 2 conducts a series of t-tests to compare the baseline characteristics of participants who completed the follow-up with those who did not. The exercise reveals that attrition was balanced on almost every dimension, with the exception that people who

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[22]92 percent of participants own a mobile phone and there is no systematic difference between treatment and control with regard to mobile phone ownership. In the event the participant did not own a mobile phone, they were asked to provide a proxy who could be contacted for the follow-up. For participants in the treatment group, the proxy would have received the reminder text messages.

had a formal savings account at baseline and who reported a higher total balance in that account were less likely to complete the follow-up. Importantly, however, attrition does not vary by assignment to the messaging treatment or control group.

Next, we verify that the randomization in the follow-up sample is balanced across important baseline characteristics that could impact saving behavior. Here, we group the reminder and peer treatment into one category and compare them to the control group that received no reminders. The exercise in Table 1 reveals that treatment is balanced across a host of baseline characteristics, including age, gender, household composition, education, farm size, ownership of consumer durables, time and risk preferences, baseline savings, and financial access. In Appendix Table 4, we separate the treatment groups into reminder and peer and find that baseline covariates are balanced over the disaggregated treatment groups, as well.

Of the farmers who completed the follow-up, 17 percent saved in the experimental account, and accumulated 488 Lei ($122 USD) on average (unconditional) over the three-month period. While the take-up rate may seem low at first glance, it is notable that at baseline, 27 percent of the sample reports saving in a formal account. Of those who had a formal account, the average account balance at the time of the baseline survey was 700 Lei ($175 USD).[23] Hence, relative to these baseline figures, the level of engagement with the experimental account is qualitatively meaningful.

Our aim is to understand how different forms of messaging impact savings and whether these messages interact with goal setting in ways that may help or hinder savings. We begin by plotting the average savings across the treatment and control groups to visually examine differences in means. Panel (a) of Figure 1 plots the mean likelihood that the participant saved in the experimental bank account, separated by control and treatment group. Panel

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[23]One may wonder whether participants who owned a formal savings account at baseline were more or less likely to engage with the experimental account. While this relationship is irrelevant for the internal validity of the experimental intervention, since baseline savings is balanced across treatment and control, it may be relevant for external validity. Appendix Table 3 provides weak evidence that if anything, participants who owned a formal savings account at baseline were more likely to save in the experimental account and that there is no significant interaction with our messaging treatment. Given the higher attrition rate among account owners, however, the net effect is likely zero, as columns (5)-(10) suggest. We nonetheless control for baseline account ownership in our robustness specifications.

Table 1: Balance of baseline covariates by treatment and control

| | Treatment | | Control | | Difference |
	N	Mean	N	Mean	P-value
Age	330	51.27	167	53.00	0.157
Female	334	0.21	169	0.23	0.587
Post-secondary school	334	0.39	169	0.36	0.582
Household size	333	3.96	169	3.78	0.273
Children in household	333	0.68	169	0.78	0.284
Adults in household	333	2.74	169	2.55	0.163
Work off farm	326	0.31	168	0.27	0.408
Farm size (Ha)	334	11.53	167	12.08	0.766
Farm equip	334	2.39	169	2.14	0.376
Durables	334	6.59	169	6.56	0.875
σ risk choice	334	2.71	168	2.72	0.990
Discount rate	334	94.94	169	95.11	0.365
Present bias	334	0.06	169	0.05	0.764
Set goal	334	0.31	169	0.27	0.441
High goal	334	0.26	169	0.24	0.612
Saves in cash	334	0.19	169	0.20	0.676
Formal savings account	332	0.25	169	0.30	0.217
Formal savings (Lei)	328	169.28	168	214.14	0.199
Informal savings (Lei)	321	282.40	164	269.36	0.741
No bank in village	334	0.69	169	0.70	0.774

Durables is the total number of consumer durables that the participant owns from the following list: refrigerator, freezer, automatic washing machine, audio equipment, computer, cell phone, television, bicycle, motorcycle, moped, and car. *Farm equip* is the total pieces of farm equipment owned.

(b) disaggregates treatment into the separate reminder and peer groups. In both panels, the treatment group has a lower likelihood of saving than the control group, but the difference does not appear to be statistically significant at the 90% CI.

Next, we examine average savings across participants who set a savings goal and those who did not, and whether goal setting interacts with the treatment in heterogeneous ways. In panel (a) of Figure 2, we see that participants who set a goal have a higher likelihood of saving in the experimental account, but this difference is not statistically significant at the 90% CI. In panel (b), we examine heterogeneity by random treatment assignment and find that in the control group of participants who received no messages, those who set a goal saved significantly more than those who did not set a goal. However, for participants who received any kind of message (reminder or peer), those who set a goal were no more likely

Figure 1: Experimental savings and reminders

(a) Any reminder

(b) Specific reminders
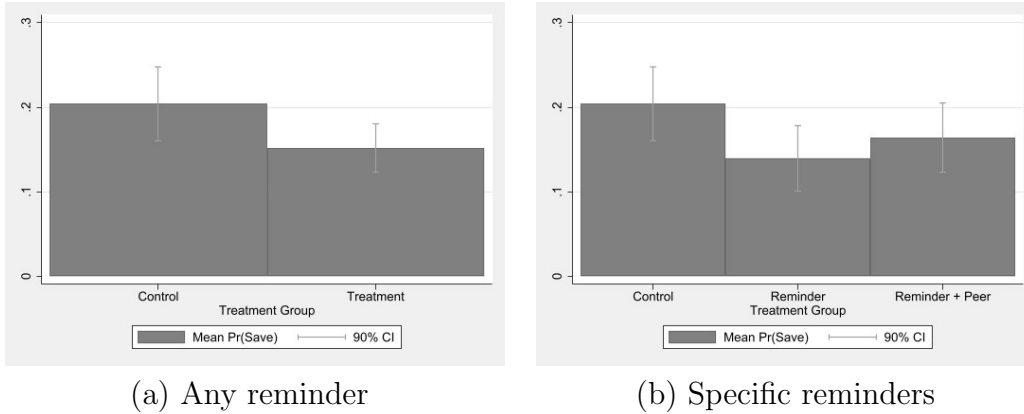
Figure 2: Experimental savings, goals, and reminders

(a) Any savings goal

(b) Savings goal, by treatment
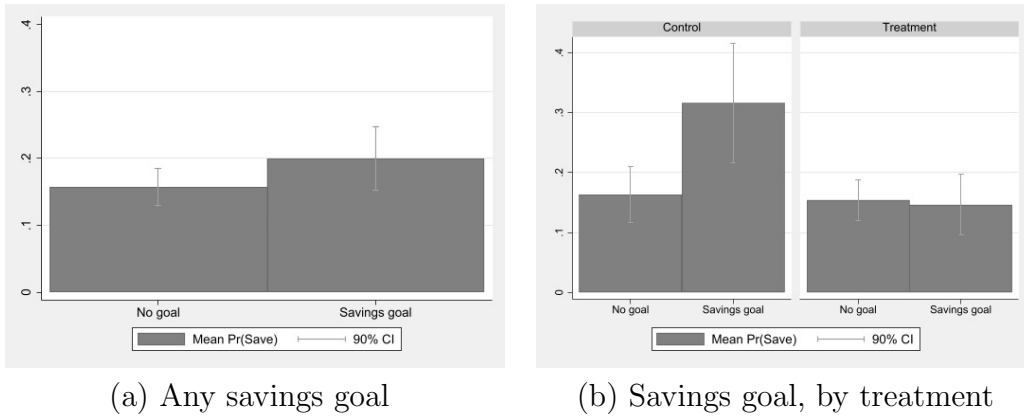
to save than those who did not. In fact, the figure suggests that messaging actually reduced the propensity to save among participants who set a goal.

4 Results

4.1 Messaging

We first estimate the average treatment effect of messaging on saving behavior with the following equation:

$$Y_i = \alpha + \beta_1 T_i + \mu_c + \varepsilon_c \tag{1}$$

where T_i is treatment status. We include comuna fixed effects, μ_c, to control for unobserv-

Table 2: The impact of messages on saving behavior

| | (1) | (2) | (3) | (4) |
	Pr(Save)		IHS(Amount saved)	
Any message	-0.039		-0.281	
	(0.050)		(0.364)	
Reminder		-0.050		-0.335
		(0.054)		(0.384)
Peer		-0.028		-0.231
		(0.055)		(0.419)
Mean savings (control):	0.20	0.20	442	442
Observations	412	412	412	412
R^2	0.109	0.110	0.117	0.117

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. All estimates include *comuna* fixed effects and cluster standard errors at the *comuna* level in parenthesis.

able geographic and institutional factors, and cluster standard errors at the comuna level, ε_c. Given that treatment was randomized within comuna, this empirical strategy identifies within-comuna variation in the treatment effect, independent of observable and unobservable comuna-level confounds that could impact individual saving behavior.

We estimate the impact of messaging on the probability of saving, as well as total amount saved in the experimental account at the time of the follow-up, including zero savings. We take the inverse hyperbolic sine of the account balance to address skew in the data and interpret effect sizes in terms of percent changes.[24] We estimate the effect of any messaging treatment, as well as the separate impact of the reminder versus peer treatment to elucidate which treatment is potentially more important.

The results in Table 2 confirm that the messaging had no impact on saving. The coefficients are negative, but statistically insignificant at the 90% confidence interval.

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[24]The inverse hyperbolic sine approximates the natural log function, but is defined at zero. Given that some participants saved nothing, we prefer to use the inverse hyperbolic sine rather than add a trivially small number to 0 and then take the natural log.

4.2 Goal Setting

Next we examine whether the messaging had a differential impact on saving behavior for people who set a savings goal. For participant i living in comuna c, we examine the impact of the messaging treatment, T_i, and setting a goal, G_i, on saving in the experimental account, Y_i. We again include comuna fixed effectx, μ_c, and cluster standard errors at the comuna level. For robustness, in some specifications we also control for individual level characteristics, X_i, including: age, female, post-secondary education, risk tolerance, consumer durables index, works off-farm, and whether the participant had a formal savings account at baseline. Our estimating equation is the following:

$$Y_i = \alpha + \beta_1 T_i + \beta_2 G_i + \beta_3 G_i * T_i + \beta_4 X_i + \mu_c + \varepsilon_c \tag{2}$$

Since goals were elicited prior to treatment, and are orthogonal to the treatment, we can recover causal estimates of the heterogeneous treatment effects. Moreover, note from Table 1 that setting a savings goal is balanced across treatment and control groups, while Appendix Table 5 shows that goal setting is balanced across a host of observable covariates.

As further validation, in our specifications that include controls, we also conduct an exercise to elucidate the extent to which the results are driven by selection on unobservables (Oster, 2016) and report delta coeffiecients (δ) at the bottom of the table. At the core of the test is a conjecture about the covariance between the omitted variable and the treatment variable – in our case, the interaction between the randomized treatment and the decision to set a goal. One commonly made assumption is that the covariance between the omitted and treatment variable is equivalent to that between the observables and the treatment variable. This is known as the proportional selection assumption and implies a coefficient of proportionality (δ) equal to 1. We calculate the coefficient of proportionality that would overturn our results and present it in the bottom row of our main table.[25] Intuitively, a higher

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[25]Based on (Oster, 2016) recommendation, we assume an Rmax that is the lesser of 1.3 times the R-

absolute value δ coefficient implies that the results are unlikely to be driven by selection on unobservables, where a value of $|\delta| \geq 2$ is a rule-of-thumb threshold (Oster, 2016).

We present the results in Table 3. In column (1) we estimate the impact of setting a savings goal and find no evidence that people who set a goal saved more. While the coefficient is positive, it is statistically insignificant. In column (2) we investigate whether the messaging treatment had a differential effect on the likelihood of saving for people who set a savings goal versus those who did not. The results confirm the picture in Figure 2 that messaging reduces the propensity to save among goal setters. The coefficient on savings goal suggests that for people in the control group who received no messaging, those who set a goal were 15 percentage points more likely to save in the experimental account than those who did not set a goal. However, the coefficient on the interaction between setting a goal and treatment suggests that messaging completely reverses this effect, decreasing the probability of saving by 21 percentage points for those who set a goal.

In columns (3) and (4) we disaggregate the treatment into whether the participant received the simple reminder or peer information. Here, we find that both messages decrease the likelihood of saving for people who set goals. We cannot reject the null hypothesis that the coefficients on *Reminder x Goal* and *Peer x Goal* are statistically equivalent, suggesting that both types of messaging reduce the likelihood of saving among goal setters by approximately 20 percentage points.[26]

In columns (5)–(8) we replicate the estimation using the inverse hyperbolic sine of total savings and obtain similar results. Here, however, we find that the peer reminders, in particular, reduced the total amount of savings for people who set savings goals. The coefficient suggests that peer messaging closed the savings gap between goal and non-goal

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squared achieved by the full regression specification and 1. The Rmax is an estimate of the R-squared that would be achieved in the case where we were able to include all the key unobservables.

[26]Given the size of our sample, at 0.8 power and a significance level of 10%, the minimum detectable effect is 14.5 percentage points for the treatment x savings goal interaction. In the specifications that disaggregate the treatment, the minimum detectable effect is 18.9 percentage points for the reminder x savings goal interaction and 19.7 percentage points for the peer x savings goal, suggesting that these empirical specifications are sufficiently powered.

setters by 160 percent. Intuitively, this means that in the control group, goal setters saved more than non-goal setters, while in the peer message group, goal setters saved less than non-goal setters.

It is worth acknowledging that in columns (4) and (8) the absolute value of the δ coefficients for goal, as well as for both interaction effects are sufficiently large, suggesting that the results are not driven by selection on unobservables.

Note that in the specifications that interact goal setting with treatment, the coefficient on treatment represents the effect of messaging for those who did not set a goal. Here, we find that while the coefficient is positive, it is insignificant for any message, as well as for each of the reminder and peer messages. This result has important implications for interventions that both encourage goal setting and utilize messaging, in that there appears to be a trade-off between the two strategies. While encouraging customers to set goals can promote savings, nudging customers to save can reverse these gains, particularly when these nudges include peer information. In the absence of goal setting, nudges may not deter the will to save, but they also may not increase it. Thus, encouraging goal setting alone may be the more effective way to promote saving in this environment.

Next, we aim to understand why reminders reduced savings for goal-setters. We create a dummy variable equal to one if the savings goal set by the participant was above the mean savings goal of the sample and zero if below, where participants who set no goal are assigned a value of zero. We re-estimate equation (2) and present the results in Table 4. In column (1), we find that people who set a high savings goal were no more likely to save than those who set a lower goal or no goal. In column (2) we interact treatment with whether the participant set a high goal and find that in the control group that received no messages, participants who set a high goal were 17 percentage points more likely to save than those who set a low goal or no goal. However, for high goal setters who received messages, the likelihood of saving is 23 percentage points lower. In columns (3) and (4) we disaggregate the treatment into reminder vs. peer and find that both messages reduced the likelihood of

Table 3: The impact of messages and savings goals

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Save in experimental account)				IHS(Amount saved in experimental account)			
Set goal	0.013	0.154**	0.154**	0.139*	0.231	1.252**	1.249**	1.117*
	(0.047)	(0.071)	(0.072)	(0.073)	(0.379)	(0.602)	(0.607)	(0.619)
Any message		0.023				0.161		
		(0.051)				(0.404)		
Set goal × Any message		-0.210***				-1.517**		
		(0.073)				(0.651)		
Reminder			0.010	0.021			0.062	0.103
			(0.058)	(0.059)			(0.447)	(0.451)
Peer			0.037	0.049			0.254	0.301
			(0.056)	(0.055)			(0.449)	(0.451)
Set goal × Reminder			-0.207*	-0.185*			-1.388	-1.194
			(0.103)	(0.099)			(0.900)	(0.860)
Set goal × Peer			-0.214***	-0.210***			-1.637**	-1.596**
			(0.073)	(0.071)			(0.642)	(0.634)
Additional controls:	No	No	No	Yes	No	No	No	Yes
Mean savings (control):	0.20	0.20	0.20	0.20	442	442	442	442
Observations	412	412	412	397	412	412	412	397
R^2	0.107	0.123	0.124	0.145	0.116	0.129	0.130	0.151
δ Goal				20.237				5.774
δ Goal x Reminder				-5.455				-3.491
δ Goal x Peer				-2.718				-2.555

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. All estimates include *comuna* fixed effects and cluster standard errors at the *comuna* level in parenthesis. Additional controls include: age, female, post-secondary education, standard deviation of risk game choice, index of consumer durables, works off-farm, and whether the respondent currently has a savings account. The coefficients δ are calculated using the *psacalc* function in Stata assuming an R-max = $1.3 * R^2$ and represent the proportional degree of selection on unobservables needed to overturn the estimated effects.

saving for high goal setters by roughly 20 to 25 percentage points.[27] The absolute value of the δ coefficients for high goal, as well as for both interaction effects are sufficiently large, suggesting that the results are not driven by selection on unobservables. Further, we cannot reject null hypothesis that the *Reminder x High goal* and *Peer x High goal* coefficients are equal, suggesting that both types of messaging demotivate high goal setters from saving.

We replicate this analysis in columns (5)-(8) using the inverse hyperbolic sine of the total savings balance and obtain similar results. While we cannot reject the null hypothesis that the reminder and peer effects are the same, only the coefficient on *Peer x High goal* is

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[27]Given the size of our sample, at 0.8 power and a significance level of 10%, the minimum detectable effect is 15.7 percentage points for the treatment x high goal interaction. In the specifications that disaggregate the treatment, the minimum detectable effect is 20.2 percentage points for the reminder x high goal interaction and 19.4 percentage points for the peer x high goal, suggesting that these empirical specifications are sufficiently powered.

Table 4: Impact of messages by size of savings goal

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Save in experimental account)				IHS(Amount saved in experimental account)			
High goal	0.009	0.166**	0.166**	0.143*	0.211	1.374*	1.376*	1.186*
	(0.043)	(0.079)	(0.080)	(0.078)	(0.336)	(0.671)	(0.679)	(0.664)
Any message		0.019				0.140		
		(0.047)				(0.370)		
High goal × Any message		-0.229**				-1.705**		
		(0.085)				(0.737)		
Reminder			0.012	0.024			0.096	0.156
			(0.054)	(0.055)			(0.417)	(0.425)
Peer			0.027	0.040			0.186	0.244
			(0.052)	(0.051)			(0.416)	(0.414)
High goal × Reminder			-0.252**	-0.232*			-1.777	-1.617
			(0.121)	(0.116)			(1.067)	(1.030)
High goal × Peer			-0.213**	-0.209**			-1.659**	-1.633**
			(0.083)	(0.079)			(0.719)	(0.693)
Additional controls:	No	No	No	Yes	No	No	No	Yes
Mean savings (control):	0.20	0.20	0.20	0.20	442	442	442	442
Observations	412	412	412	397	412	412	412	397
R^2	0.107	0.124	0.125	0.146	0.116	0.131	0.131	0.152
δ Goal				6.110				3.822
δ Goal x Reminder				-4.880				-3.408
δ Goal x Peer				-2.137				-2.037

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. High savings goal equal to 1 if the participant's savings goal exceeded the mean savings goal in the sample (900 Lei). All estimates include *comuna* fixed effects and cluster standard errors at the *comuna* level in parenthesis. Additional controls include: age, female, post-secondary education, standard deviation of risk game choice, index of consumer durables, works off-farm, and whether the respondent currently has a savings account. The coefficients δ are calculated using the *psacalc* function in Stata assuming an R-max = $1.3 * R^2$ and represent the proportional degree of selection on unobservables needed to overturn the estimated effects.

statistically significant at conventional levels of inference. We also find in column (8) that again the absolute value of the δ coefficients for high goal, as well as both interaction effects are sufficiently large, suggesting that the results are not driven by selection on unobservables.

As a robustness exercise, in Table 5, we replicate the previous estimation, restricting the sample to only participants who set a goal. Again, we find that high goal setters save less when they receive any form of messaging, but now there is weak evidence that reasonable goal setters save more with messaging.

The finding that messaging demotivates high goal setters from saving is consistent with results found in other contexts. In the US, Beshears et al. (2015) randomly provided employees with information about the 401(k) savings behavior of their peers and found that it reduced the likelihood of enrolling in a 401(k) plan. In particular, they find that em-

Table 5: Impact of messaging by size of goal (Goal Setters, only)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Save in experimental account)				IHS(Amount saved in experimental account)			
High goal	0.023	0.334**	0.367***	0.324**	0.292	2.886**	3.124**	2.741**
	(0.097)	(0.125)	(0.127)	(0.125)	(0.806)	(1.189)	(1.202)	(1.022)
Any message		0.220				1.976		
		(0.138)				(1.301)		
High goal × Any message		-0.424**				-3.540**		
		(0.168)				(1.464)		
Reminder			0.383*	0.528**			3.265*	4.382**
			(0.203)	(0.248)			(1.749)	(2.064)
Peer			0.096	0.366*			0.988	3.182*
			(0.097)	(0.196)			(0.990)	(1.725)
High goal × Reminder			-0.631**	-0.737**			-5.059**	-5.863**
			(0.265)	(0.303)			(2.214)	(2.496)
High goal × Peer			-0.270*	-0.556**			-2.396*	-4.702**
			(0.147)	(0.233)			(1.315)	(2.013)
Additional controls:	No	No	No	Yes	No	No	No	Yes
Observations	120	120	120	116	120	120	120	116

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. Sample restricted to participants who set a goal. All estimates include *comuna* fixed effects and cluster standard errors at the *comuna* level in parenthesis. Additional controls include: age, female, post-secondary education, standard deviation of risk game choice, index of consumer durables, works off-farm, and whether the respondent currently has a savings account.

ployees with a zero percent contribution rate at baseline are less likely to save when they see that a higher fraction of their peers are participating in the plan, suggesting a type of "boomerang effect" known as an oppositional reaction. Our results suggest a boomerang effect, but one that is more consistent with negative belief updating (see for example the literature on energy consumption (Schultz et al., 2007) or norms about female labor supply (Bursztyn et al., 2020)). When participants learn that their savings goal is higher than their peers' goals, they adjust their behavior downward, closer to that of their peers. What is interesting in our context, however, is that general reminders to save have the same impact as peer information, suggesting that while negative belief updating is occurring, it is also possible that simply reminding high goal setters to save can be de-motivating.

This could be due to the fact that their goals are unrealistic. Appendix Figure 1 shows the distribution of savings goals for those who set goals, which suggests that indeed some of the stated goals were probably unrealistic. Appendix Figure 2, however, shows that the size of goal is positively correlated with the participant's savings capacity, suggesting that while

some are unrealistic, the stated goals are not purely noise.[28] On average, high goal setters expected to save 5000 Lei ($1250 USD) over the three-month period. This would require a deposit of over 1600 Lei ($400 USD) each month, which is around seven percent of the average annual income of a farmer in Romania. Even in the presence of a windfall, perhaps after the harvest when our study takes place, 5000 Lei represents 22.6 percent of the average annual income for farmers, suggesting that while not entirely irrational, some of the high savings goals were unrealistic.

Hsiaw (2013) shows theoretically that goals must be sufficiently realistic for reference-dependent agents to counteract impatience; when unrealistic goals are set, the incentive to wait longer to obtain the higher net payoff is weaker, leading agents with unrealistic goals to stop too early. In our case, the pure reminder to save most likely increased the salience of unrealistic goals, thereby reducing the likelihood of saving among unrealistic goal setters. In Appendix Table 7, we consider alternate definitions of unrealistic goals, including whether the stated savings goal was above the comuna average or greater than the participant's existing savings at baseline, and find again that messaging hinders savings for high goal setters.

4.3 Patience, Trust, and Financial Access

In this section we explore whether certain behavioral and institutional factors that have been shown to influence savings interact with our experimental treatment in ways that promote or hinder savings. We focus on three key factors. First, we explore the role of time preferences – specifically, present bias. Present bias theoretically and empirically reduces savings, but it has been shown that this behavior can be mitigated with the use of commitment savings products.[29] While our experimental account is not a commitment

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[28]We proxy for savings capacity with two different varialbes: (i) the total value of savings at baseline, and (ii) the size of the respondent's farm, both which should be positive correlated with income and wealth.

[29]A commitment savings product requires the customer to commit to a time period in which they must refrain from making withdrawals and in some cases attain a minimum balance before accessing their savings. Commitment products have been effective in promoting savings in lesser-developed contexts. See for example work by Ashraf et al. (2006) in the Philippines, Karlan et al. (2016) in the Philippines, Peru and Bolivia,

product per se, it does provide present biased people a method of saving that is absent of transaction costs, thereby reducing some of the cognitive hurdles for present biased people. Moreover, for the participant, knowing that the research team would return to observe the amount in the experimental account and pay interest on the balance could have instilled a form of social commitment through an implicit deadline.[30]

Next, we examine trust in financial institutions. Trust has been shown to impact formal savings, particularly in Central, Eastern, and Southeastern Europe, where recent work suggests that preferences to save in cash at home cannot be fully explained by whether people are banked or unbanked, and that mistrust in banks is a salient factor in cash preferences (Stix, 2013). Finally, we study the role of financial access, since geographic proximity has been shown to impact financial transactions in wealthy countries like the US (Goodstein and Rhine, 2017), while in less developed economies, distance to the nearest bank is a constraint on experimental savings (Dupas et al., 2018). Furthermore, it has been shown that the unbanked gain the most from expanding financial access (Bruhn and Love, 2014; Burgess and Pande, 2005).

We begin by re-estimating equation (2), substituting savings goals with our behavioral and institutional parameters. First, we examine present bias, then financial trust, and financial access. Again, we estimate the impact of the messaging treatment and its interaction with a given parameter on the likelihood of saving in the experimental account, as well as the inverse hyperbolic sine of the total amount saved. We present the findings in Table 6.

We start with present bias in Panel A. In column (1) we find evidence that present-biased participants are more likely to save in in the experimental account. This is consistent with the idea that providing a savings device with zero transaction costs and implicit commitment can overcome present biased behavior. In column (2) we interact present bias with our treatment indicator and find that messaging does not have a differential impact on savings

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and Dupas and Robinson (2013) in Kenya, among others.

[30]Information about the three-month follow-up and interest payment may have implicitly created an external or internally imposed deadline, which is a form of pre-commitment that can overcome present-bias (Ariely and Wertenbroch, 2002).

Table 6: Impact of messages by present bias, trust, and financial access

	(1)	(2)	(3)	(4)	(5)	(6)
		Pr(Save)			IHS(Amount saved)	
Panel A: Present bias						
Present bias	0.148*	0.171	0.162	1.385*	1.563	1.487
	(0.085)	(0.160)	(0.164)	(0.717)	(1.322)	(1.342)
Any message		-0.036	-0.032		-0.258	-0.248
		(0.047)	(0.046)		(0.336)	(0.329)
Present bias × Any message		-0.037	-0.049		-0.283	-0.374
		(0.207)	(0.205)		(1.743)	(1.716)
Observations	412	412	405	412	412	405
Panel B: Trust in financial institutions						
High trust	0.074	0.103*	0.073	0.736*	0.904*	0.652
	(0.044)	(0.060)	(0.057)	(0.377)	(0.488)	(0.465)
Any message		-0.020	-0.027		-0.147	-0.237
		(0.047)	(0.047)		(0.331)	(0.331)
High trust × Any message		-0.053	-0.025		-0.312	-0.039
		(0.078)	(0.080)		(0.604)	(0.627)
Observations	410	410	403	410	410	403
Panel C: Financial acess						
No bank	0.042	-0.025	-0.001	0.241	-0.347	-0.203
	(0.054)	(0.094)	(0.100)	(0.466)	(0.748)	(0.787)
Any message		-0.099	-0.082		-0.817	-0.735
		(0.086)	(0.089)		(0.660)	(0.677)
No bank × Any message		0.088	0.066		0.770	0.649
		(0.092)	(0.095)		(0.695)	(0.715)
Additional controls:	No	No	Yes	No	No	Yes
Observations	412	412	405	412	412	405

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. All estimates include *comuna* fixed effects and cluster standard errors at the *comuna* level in parenthesis. Additional controls include: age, female, post-secondary education, standard deviation of risk game choice, discount rate, index of consumer durables, and whether the respondent currently has a savings account.

for present biased people, even when including the full set of controls in column (3). The coefficient on the interaction term is negative, but statistically insignificant and qualitatively small. We replicate the estimation in columns (4)-(6) using the inverse hyperbolic sine of the account balance and obtain similar results. Present biased participants save more in the experimental account, but messaging does not have a differential impact on savings.

Turning to the role of trust in financial institutions in Panel B, participants in the baseline survey were asked how much confidence they have in financial institutions: (i) a great deal, (ii) quite a lot, (iii) not very much, (iv) none at all. We create a dummy variable equal to 1 for participants who selected (i) or (ii) and zero otherwise. 30 percent of participants reported high trust in financial institutions by this measure. We then correlate this measure of trust with saving in the experimental account. The exercise suggests that participants with higher trust in financial institutions are more likely to engage with the experimental account, but the relationship is statistically weak. Moreover, there is no differential impact of the messaging treatment for high versus low trust participants. The coefficient on the interaction term is negative, but statistically insignificant and qualitatively small.

Finally, we examine the role of financial access in Panel C. Here, there is no evidence that participants who live in a village without a bank engage more or less with the experimental account. The coefficients in columns (1) and (4) are positive, but insignificant. The interaction with the treatment is also positive, but again statistically insignificant. We replicate these estimations in Appendix Table 8, separating the treatment into reminder versus peer, and obtain similar results. In regard to financial access, however, there is weak evidence that the peer treatment increases savings for participants who do not have a bank in their village, but decreases savings for participants with financial access. This could indicate a trade-off for messaging in places that are banked versus unbanked, suggesting that peer messaging is helpful in places that are unbanked and harmful in places that have better financial access. However, the statistical relationship is too weak to make this claim with confidence.

5   Conclusion

We design a randomized intervention in an upper-middle income setting to elucidate how informational nudges impact saving and whether these nudges interact with commitment devices in unintended ways. We focus specifically on messaging and goal setting, two standard features of financial inclusion interventions in other contexts, and find that while

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there is no average effect of messaging on savings, it discourages saving among participants who set goals. This effect is driven by participants who set unrealistically high goals, suggesting that messaging unintentionally increases the salience of unrealistic goals and causes high goal setters to adjust their behavior downward in a pattern that is consistent with negative belief updating. While goal setting does promote saving, this is only true in the absence of messaging.

These findings are important for the design of savings interventions in upper-middle income countries, where there is a dearth of evidence relative to higher or lower income settings. Our results suggest that, on their own, informational nudges have no impact on saving. While simple commitment devices like goal setting can encourage saving, these goals ought to be realistic. Further, and more importantly, reminding goal setting customers to save or providing them with information about the savings goals of their peers appears to fully reverse the gains of goal setting, suggesting a tradeoff between informational nudges and commitment devices in this context.

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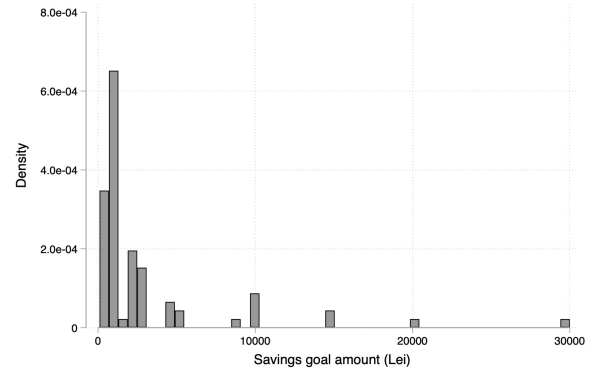
Appendices

A   Additional Figures and Tables

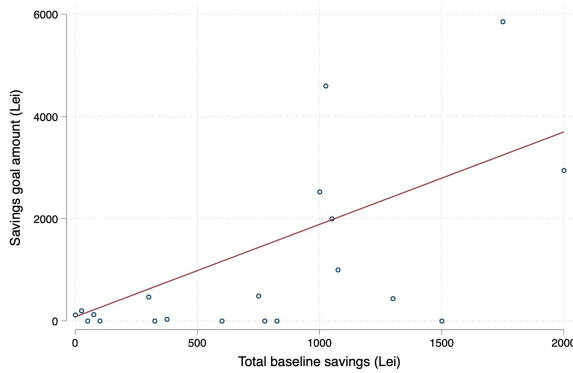Appendix Figure 1: Distribution of savings goals
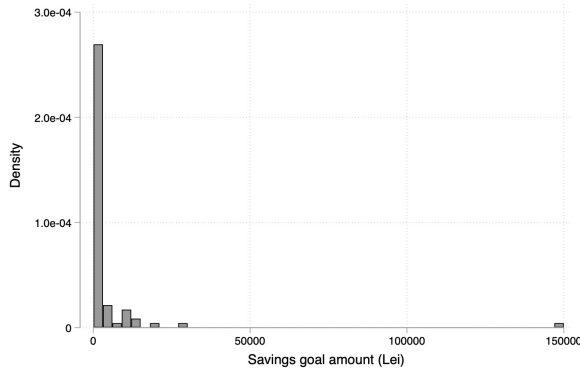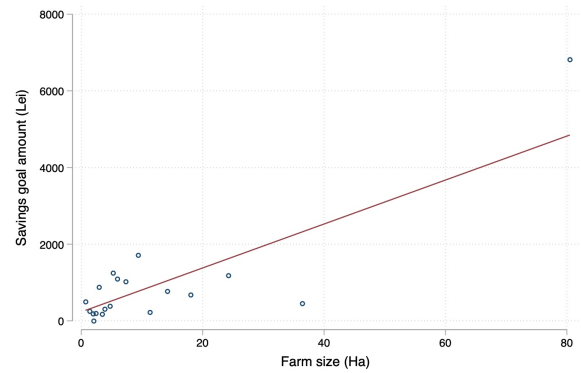

(a) All

(b) Outlier removed

Appendix Figure 2: Savings goal amount and baseline saving capacity

(a) Baseline savings

(b) Farm size

Appendix Table 1: Trust in strangers and engagement with the experimental account

	(1)	(2)	(3)	(4)
	Pr(Save)		IHS(Amount saved)	
Trust people you meet for the first time	0.024	0.016	0.010	-0.045
	(0.044)	(0.045)	(0.343)	(0.336)
Additional controls:	No	Yes	No	Yes
Observations	412	405	412	405

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. All estimates include *comuna* fixed effects and cluster standard errors at the *comuna* level in parenthesis. Additional controls include: age, female, post-secondary education, standard deviation of risk game choice, index of consumer durables, works off-farm, and whether the respondent currently has a savings account.

Appendix Table 2: Balance of covariates by attrition between baseline and follow-up

	No follow-up		Follow-up		Difference
	N	Mean	N	Mean	P-value
Control	91	0.30	412	0.34	0.382
Reminder	91	0.34	412	0.33	0.847
Peer	91	0.36	412	0.33	0.494
Age	91	51.56	406	51.92	0.812
Female	91	0.25	412	0.21	0.357
Post-secondary school	91	0.40	412	0.37	0.698
Household size	91	3.84	411	3.91	0.697
Children in household	91	0.58	411	0.74	0.193
Adults in household	91	2.76	411	2.66	0.542
Work off farm	91	0.29	403	0.30	0.785
IHS(Farm size Ha)	91	2.33	410	2.50	0.158
Farm equip	91	2.40	412	2.29	0.750
Durables	91	6.70	412	6.56	0.536
σ risk choice	90	2.72	412	2.71	0.997
Discount rate	91	94.85	412	95.03	0.402
Present bias	91	0.03	412	0.06	0.265
Set goal	91	0.31	412	0.29	0.756
Saves in cash	91	0.21	412	0.19	0.631
Formal savings account	91	0.35	410	0.25	0.045
Formal savings (Lei)	91	243.96	405	171.11	0.088
Informal savings (Lei)	89	237.64	396	287.06	0.305
No bank in village	91	0.70	412	0.69	0.864

Appendix Table 3: Baseline savings and engagement with the experimental account

DV: Pr(Save)	Excluding attritors				Including attritors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Savings account	0.092*	0.101*	0.096	0.112	0.051	0.055	0.062	0.075
	(0.048)	(0.050)	(0.083)	(0.076)	(0.045)	(0.047)	(0.084)	(0.079)
Any message			-0.029	-0.013			-0.028	-0.014
			(0.057)	(0.053)			(0.052)	(0.047)
Savings account × Any message			-0.010	-0.036			-0.019	-0.041
			(0.106)	(0.100)			(0.100)	(0.091)
Additional controls:	No	Yes	No	Yes	No	Yes	No	Yes

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. All estimates include comuna fixed effects and cluster standard errors at the comuna level in parenthesis. Additional controls include: age, female, post-secondary education, standard deviation of risk game choice, present bias, index of consumer durables, and whether the participant works off-farm. Columns (5)-(10) assume that all attirtors did not save in the experimental account.

Appendix Table 4: Balance of covariates by treatment group

	Age (1)	Female (2)	Educ (3)	HH Size (4)	Off farm (5)	Farm size (6)	Farm equip (7)	Durables (8)	Risk (9)	Discount (10)	Present (11)	Cash (12)	Save (13)	No bank (14)
Reminder	-1.439	-0.017	0.050	0.125	0.026	-0.024	0.152	0.117	-0.119	-0.094	0.013	-0.018	-0.049	-0.022
	(2.092)	(0.043)	(0.058)	(0.224)	(0.064)	(0.128)	(0.325)	(0.287)	(0.222)	(0.174)	(0.032)	(0.035)	(0.042)	(0.035)
Peer	-1.780	-0.007	0.042	0.261	0.091*	0.030	0.264	0.236	-0.018	-0.127	0.000	-0.016	-0.049	-0.005
	(1.290)	(0.037)	(0.048)	(0.250)	(0.051)	(0.110)	(0.323)	(0.230)	(0.288)	(0.132)	(0.026)	(0.033)	(0.045)	(0.022)
Observations	497	503	503	502	494	501	503	503	502	503	503	503	501	503

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. Standard errors clustered at the comuna level in parenthesis. Excluded group is the control group.

Appendix Table 5: Balance of covariates for setting a savings goal

	Age (1)	Female (2)	Educ (3)	HH Size (4)	Off farm (5)	Farm size (6)	Farm equip (7)	Durables (8)	Risk (9)	Discount (10)	Present (11)	Cash (12)	Save (13)	No bank (14)
Set goal	0.555	0.032	0.075	-0.294	0.174**	-0.218*	-0.228	0.084	-0.094	0.158	0.035	0.087	0.137**	-0.039
	(1.782)	(0.044)	(0.047)	(0.176)	(0.070)	(0.111)	(0.345)	(0.197)	(0.276)	(0.255)	(0.029)	(0.051)	(0.056)	(0.038)
Observations	497	503	503	502	494	501	503	503	502	503	503	503	501	503

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. Standard errors clustered at the comuna level in parenthesis.

Appendix Table 6: Balance of covariates for present bias, trust, and financial access

	Age (1)	Female (2)	Educ (3)	HH Size (4)	Off farm (5)	Farm size (6)	Farm equip (7)	Durables (8)	Risk (9)	Discount (10)	Present (11)	Cash (12)	Save (13)	No bank (14)
Panel A: Present bias														
Present bias	-4.000	0.036	0.096	0.169	-0.022	0.356	0.648	0.387*	0.317	0.206	1.000	0.041	-0.051	-0.058
	(2.797)	(0.082)	(0.089)	(0.422)	(0.083)	(0.230)	(0.678)	(0.207)	(0.560)	(0.214)	(.)	(0.086)	(0.075)	(0.058)
Observations	497	503	503	502	494	501	503	503	502	503	503	503	501	503
Panel B: Trust in financial institutions														
Trust	-2.655**	0.026	-0.009	-0.044	0.014	0.177	0.037	0.401*	0.403**	-0.192	0.008	0.041	0.042	0.010
	(1.263)	(0.040)	(0.049)	(0.136)	(0.063)	(0.139)	(0.287)	(0.209)	(0.176)	(0.197)	(0.025)	(0.047)	(0.044)	(0.021)
Observations	494	500	500	499	491	498	500	500	499	500	500	500	498	500
Panel C: Financial access														
No bank in village	-2.349	0.008	-0.065	0.440	-0.091*	-0.159	0.197	0.054	0.175	-0.257	-0.042	-0.058	-0.050	1.000***
	(1.522)	(0.047)	(0.091)	(0.269)	(0.049)	(0.216)	(0.680)	(0.196)	(0.342)	(0.210)	(0.041)	(0.049)	(0.106)	(0.000)
Observations	497	503	503	502	494	501	503	503	502	503	503	503	501	503

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. Standard errors clustered at the comuna level in parenthesis

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Appendix Table 7: Impact of messaging by alternate definitions of unrealistic goals

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Save in experimental account)			IHS(Amount in experimental account)		
Panel A: Goal higher than comuna savings goal						
High goal (Comuna goal)=1	0.232***	0.232**	0.217**	1.870**	1.867**	1.714**
	(0.083)	(0.084)	(0.088)	(0.706)	(0.711)	(0.748)
Any message	0.025			0.174		
	(0.047)			(0.375)		
High goal (Comuna goal)=1 × Any message	-0.284***			-2.095**		
	(0.090)			(0.805)		
Reminder		0.015	0.028		0.102	0.155
		(0.054)	(0.055)		(0.415)	(0.425)
Peer		0.035	0.049		0.240	0.294
		(0.053)	(0.052)		(0.421)	(0.430)
High goal (Comuna goal)=1 × Reminder		-0.288**	-0.267**		-2.021*	-1.814*
		(0.119)	(0.118)		(1.045)	(1.032)
High goal (Comuna goal)=1 × Peer		-0.280***	-0.275***		-2.163***	-2.096**
		(0.086)	(0.087)		(0.772)	(0.785)
Panel B: Goal higher than mean baseline savings						
High goal (Baseline savings)=1	0.138*	0.138*	0.125*	1.140*	1.137*	1.023*
	(0.071)	(0.072)	(0.069)	(0.607)	(0.613)	(0.590)
Any message	0.013			0.090		
	(0.048)			(0.385)		
High goal (Baseline savings)=1 × Any message	-0.189**			-1.371*		
	(0.082)			(0.719)		
Reminder		0.001	0.013		-0.002	0.053
		(0.056)	(0.058)		(0.430)	(0.443)
Peer		0.026	0.040		0.176	0.232
		(0.054)	(0.053)		(0.429)	(0.432)
High goal (Baseline savings)=1 × Reminder		-0.189	-0.170		-1.252	-1.096
		(0.116)	(0.111)		(1.004)	(0.960)
High goal (Baseline savings)=1 × Peer		-0.192**	-0.191**		-1.480**	-1.465**
		(0.076)	(0.070)		(0.663)	(0.617)
Additional controls:	No	No	Yes	No	No	Yes
Observations	412	412	397	412	412	397

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. All estimates include *comuna* fixed effects and cluster standard errors at the *comuna* level in parenthesis. Additional controls include: age, female, post-secondary education, standard deviation of risk game choice, index of consumer durables, works off-farm, and whether the respondent currently has a savings account.

Appendix Table 8: Impact of reminder v. peer, by present bias, trust, and financial access

	(1)	(2)	(3)	(4)	(5)	(6)
		Pr(Save)			IHS(Amount saved)	
Panel A: Present bias						
Present bias	0.148*	0.171	0.162	1.385*	1.562	1.487
	(0.085)	(0.161)	(0.165)	(0.717)	(1.332)	(1.351)
Reminder		-0.043	-0.038		-0.279	-0.258
		(0.055)	(0.054)		(0.381)	(0.366)
Peer		-0.030	-0.027		-0.241	-0.241
		(0.049)	(0.049)		(0.367)	(0.370)
Present bias × Reminder		-0.111	-0.131		-0.803	-0.956
		(0.188)	(0.191)		(1.591)	(1.602)
Present bias × Peer		0.031	0.026		0.194	0.159
		(0.259)	(0.252)		(2.164)	(2.101)
Observations	412	412	405	412	412	405
Panel B: Trust in financial institutions						
High trust	0.074	0.102	0.073	0.736*	0.899*	0.650
	(0.044)	(0.061)	(0.057)	(0.377)	(0.493)	(0.465)
Reminder		-0.023	-0.028		-0.124	-0.202
		(0.059)	(0.060)		(0.407)	(0.409)
Peer		-0.020	-0.028		-0.192	-0.281
		(0.048)	(0.047)		(0.341)	(0.336)
High trust × Reminder		-0.100	-0.077		-0.690	-0.429
		(0.077)	(0.081)		(0.620)	(0.671)
High trust × Peer		-0.005	0.022		0.066	0.314
		(0.122)	(0.115)		(0.965)	(0.913)
Observations	410	410	403	410	410	403
Panel C: Financial acess						
No bank	0.042	-0.023	0.004	0.241	-0.320	-0.157
	(0.054)	(0.094)	(0.099)	(0.466)	(0.740)	(0.776)
Reminder		-0.063	-0.034		-0.479	-0.266
		(0.093)	(0.095)		(0.713)	(0.732)
Peer		-0.134	-0.129		-1.139	-1.192
		(0.090)	(0.090)		(0.720)	(0.706)
No bank × Reminder		0.017	-0.023		0.190	-0.138
		(0.113)	(0.118)		(0.840)	(0.875)
No bank × Peer		0.154	0.146		1.314*	1.350*
		(0.092)	(0.093)		(0.726)	(0.717)
Additional controls:	No	No	Yes	No	No	Yes
Observations	412	412	405	412	412	405

* p< 0.10, ** p<0.05, *** p < 0.01. OLS estimates. All estimates include *comuna* fixed effects and cluster standard errors at the *comuna* level in parenthesis. Additional controls include: age, female, post-secondary education, standard deviation of risk game choice, discount rate, index of consumer durables, and whether the respondent currently has a savings account.