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**Working Paper**

# How to Help the Poor to Save a Bit: Evidence from a Field Experiment in Kenya

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IZA DP No. 10024

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June 2016

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

# How to Help the Poor to Save a Bit: Evidence from a Field Experiment in Kenya

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# ABSTRACT

## How to Help the Poor to Save a Bit: Evidence from a Field Experiment in Kenya[1]

Worldwide, the majority of workers hold jobs in the informal sector that do not provide access to social insurance programs. We partnered with a savings product provider in Kenya to test the extent to which behavioral interventions and financial incentives can increase the saving rate through a voluntary pension program for informal workers with low and irregular income. Our experiment lasted for six months and included a total of twelve conditions. The control condition received weekly reminders and balance reporting via text messages. The treatment conditions received in addition one of the following interventions: (1) reminder text messages framed as if they came from the participant's kid (2) a golden colored coin with numbers for each week of the trial, on which participants were asked to keep track of their weekly deposits (3) a match of weekly savings: The match was either 10% or 20% up to a certain amount per week. The match was either deposited at the end of each week or the highest possible match was deposited at the start of each week and was adjusted at the end. Among these interventions, by far the most effective was the coin: Those in the coin condition saved on average the highest amount and more than twice as those in the control condition. We hypothesize that being a tangible track-keeping object; the coin made subjects remember to save more often. Our results support the line of literature suggesting that saving decisions involve psychological aspects and that policy makers and product designers should take these influences into account.

JEL Classification: G21, B49, D03

Keywords: savings, field experiment, behavioral economics

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## 1. Introduction

How do people decide how much to consume today versus how much to save for future consumption? The answer to this question is central for many important economic analyses as well as government policies. Savings behavior observed across many situations exhibit numerous inconsistencies with standard models of inter-temporal choice. For example, factors such as patience and risk tolerance, which should in principle explain differences in observed retirement savings, fail to do so based on the U.S. data (Bernheim, Skinner and Weinberg, 2001). Changing the offer for (401)K plan participation from opt-out by default to opt-in by default influences the number of employees who sign up and their total savings (Madrian and Shea, 2001). On the other hand it is recognized since Strotz (1955) that psychological factors such as self-control problems play a major role in savings decisions, and that people exhibit dynamic inconsistencies even in simple inter-temporal decisions related to saving (Frederick et al., 2001). The psychological view that emerges from this line of research has very different implications for the factors that influence the savings behavior and ultimately for how institutions and incentive mechanisms should be designed.

If individuals suffer from self-control problems, are impulsive and have a shortsighted vision – the question of "how to get people make better saving decisions" becomes an important economic problem. One approach to study this question is through field experiments, by varying the features of an existing savings product. By testing whether certain interventions increases saving rates, we gain insight about the underlying decision making process, and propose alternative approaches and mechanisms. For example, Ashraf, Karlan and Yin (2006) demonstrated that some individuals voluntarily take up financial commitment devices, which limit their access to their own savings for a certain time period even when these plans offer no direct benefit -- suggesting that self-control is an issue for some people, and that some of these individuals are sophisticated enough to take binding measures against it (O'Donoghue and Rabin, 1999). In other research it has been shown that even simply announcing a savings goal and then regularly reporting to peers the levels of achieved savings increase saving rates, suggesting that even non-

binding commitments can be effective when dealing with self-control problems (Kast, Meier and Pomeranz, 2012).

Having an accurate model for what motivates people to save more becomes particularly important when policy makers want to encourage their citizens to save more. For example developing countries of Sub-Saharan Africa are looking for ways to increase domestic savings in hopes of achieving a higher rate of economic growth and job creation[2]. As another example, recently, some of the federal institutions in the United States started to adopt policies to encourage low-income families to save more of their regular income, as well as their tax refunds.[3] With the importance of such saving programs, the question is what kinds of interventions would be most cost effective? Would these be based on the standard economic model? Should they be based on some behavioral findings and if so, on which ones?

Currently one of the most common interventions to encourage low-income individuals to save more is financial literacy education. The underlying assumption for such financial literacy programs is that the major barrier that is preventing low-income individuals from saving is their lack of knowledge about the benefits of savings and the way saving products work. Even though it is true that low-income individuals score low on financial literacy tests, recent research suggests that the effectiveness of financial literacy programs is negligible (Fernandes, Lynch and Netemeyer, 2014). Another common intervention to encourage higher saving rates is the golden standard of economics: Providing incentives, in the form of matching contributions.[4] Undeniably, a match creates a motivation for saving. For example, Duflo et al. (2006) reports that during tax-time in the United States, a 20 percent match for IRA contributions increased the take up rate from 3 to 8 percent in a sample of middle-and low-income families in St. Louis. However, since matching can

[2] See the discussion paper "Incentivizing non-retirement savings" released by the National Treasury of South Africa, retrieved at www.treasury.gov.za.

[3] See "Assets for Independence Act" legislation at http://www.acf.hhs.gov/programs/ocs/resource/afi-legislation-0

[4] Matches are given sometimes given with certain pre-conditions, such as not withdrawing deposits for a certain timeframe, or meeting the pre-set savings goals. See instructions on Individual Development Accounts at http://cfed.org/programs/idas/.

be prohibitively costly, it is possible that psychological interventions might be cheaper and hence more practical.

One effective psychological intervention is sending messages regularly to remind saving. The idea behind reminder messages is that people have busy lives and cannot pay attention to all of their future expenditures in the present, leading them to under-save (Karlan et al., 2012). Thus, by making the relevant information regarding future needs salient, a reminder message can help to make a better decision regarding saving. Though this intervention is promising given its simplicity and low cost, it is also clear that in many real world problems people do not always respond to mere information. We illustrate this idea by two examples. First, in the charity context, it is widely documented that people donate more to a charity when they see the picture of a particular child who needs help, compared to when they receive the information demonstrating the severity of the problem (Slovic, 2007). Armed with this knowledge, charities often use specific cues to appeal potential donors' emotions while conveying information about why they should donate. Another example is about exercising. While there is abundant information about the benefits of exercising, people usually fail to follow through even though they think that they should exercise. However recent popularity of wearable activity trackers suggest that the design of how relevant information is brought to mind can matter a lot for directing behavior. While there are many features that make activity trackers popular, we speculate that two of them are particularly important: First the wearable parts of the trackers are appealing in design. Second, they set small daily, weekly goals and give nonmonetary rewards such as badges when users meet them. With these two features, users are constantly reminded about their tracking device, and rewarded for their on-going achievements.

In this study, we designed two psychological interventions using similar approaches to the ones explained above and we tested their effect on saving behavior in a field experiment in Kenya. The first of these interventions is sending regular reminder text messages to the savers framed as if sent by their kids; and asking them to save for their future. Even though the savings product we used was an individual savings product, we hypothesize that reminding individuals of their own kids through the text message

framing could appeal to their emotions, and thus motivate them to save more. The second intervention aims to create a tangible and appealing representation of savings to serve as a reminder and to provide small rewards for deposits and thus create a sense of accomplishment among savers, boosting their motivation. To this end, we designed a metal coin, which looks like gold to represent savings (see Figure 1). The coin displays a picture representing continuous savings on the front and resulting increase in wealth on the back. The coin also has numbers for each week of the six months period of our experiment, so that users can mark each number in a specific way to keep track of the weeks in which they saved and in which they did not. The idea behind the coin is that savings, particularly small deposits towards long-term savings are abstract. It is very hard to make sense of what each deposit means for the final goal, as well as the final result of missing a deposit.

On top of testing the effectiveness of these two psychological interventions to motivate low-income individuals save a bit more, the second aim of this study is to compare the effectiveness of the psychological interventions to the effectiveness of providing financial incentives. A policy issue of crucial importance is having a good understanding about the effect size of financial incentives as well as psychological interventions and their combinations. Since the setting of financial products exhibit very particular details, it is very hard to compare the effects of psychological interventions measured with one study in a particular country with a particular product to the effect size of financial incentives measured in a different country with a different product. The difficulty of comparing the effectiveness across studies makes it very difficult to understand the relative effects of psychological interventions and financial incentives in general. By testing many types of interventions within one study, we also aim to contribute to this line of literature.[5]

To test the effectiveness of these two approaches, appealing to emotions by reminding one's kids, representing savings with a tangible track-keeping object and to compare these interventions to financial incentives, we partnered with the administrators of a newly developed savings product in Kenya, Mbao Savings Plan, which resembles a

[5] In a loan repayment context, Cadena and Shoar (2013) find that sending reminder text messages three days earlier than the loan repayment deadlines reduces late payments as much as a promised interest reduction of 25% for the next loan if all deadlines are met for the loan repayment.

regular bank account with a commitment device, that is, a three year restriction on withdrawals after registering.

Our experiment lasted for six months and included a total of twelve conditions, one baseline, one control and ten treatment conditions. The baseline condition continued to receive no regular communication from the savings product as it was the standard procedure used by Mbao. The control condition received text messages twice every week, one on the first day of the week, asking participants to save during the week, and one on the last day, reporting their weekly savings and final savings balance. The treatment conditions differed in one of the following ways from the control condition: (a) The *kids* treatment received the text message on the first day of the week framed as if it came from the participant's kid. (b) The *coin* treatment received the "gold coin" at the start of the intervention and each week, on top of the regular report message, participants received an additional text message, asking them to scratch around that week's number with a knife if they saved that week and below the number if they did not. (c) The *ex-post match 10%* treatment received a 10 percent match for weekly savings up to 100 Shillings per week, deposited in accounts on the last day of the week and reflected in the balance report. The first message of the week in this treatment was also modified to remind about the match participants would receive if they saved that week. (d) The *pre-match 10%* treatment received the highest possible match with the same 10% rate and 100 Shillings weekly cap, that is 10 Shillings deposited in accounts on the first day of each week and the reminder text message of the week informed participants about this and reminded them that they would lose it if they did not save at all. This treatment was thus exactly the same as the *ex-post match 10%* treatment in terms of incentives and the two only differed in the framing of first weekly reminder text message. (e) The *ex-post match 20%* and *pre-match 20%* treatments were the same as the *ex-post match 10%* and *pre-match 10%*, but offered a 20% match rate up to 100 Shillings a week, thus higher financial incentives to save. (f) Four additional treatments involved a combination of *ex-post match 10%* and *pre-match 10%* treatments with either *kids* treatment or with the *coin* treatment. These four treatments allow us to compare the effect of combining a financial incentive with a psychological intervention to increasing the financial incentive 100 percent.

We tested the interventions explained above on a pre-selected sample of 2,400 Mbao customers. We reached out to these customers via mobile phone at the beginning of the experiment and they accepted a six months "promotional service" from Mbao. Each individual from this sample was offered one randomly preselected intervention as "a promotional service". In addition, we offered the product to 1567 individuals in the slums in Nairobi, offering each of them one randomly selected intervention as a "promotional service". We collected savings data for six months from the 2400 individuals in the existing customers sample and the 1464 individuals who signed up for Mbao from the new customers sample.

Our first general finding is that the coin was by far the most effective intervention in increasing savings. In the existing customers sample, those in the control condition saved on average 674 Shillings at the end of six months whereas those in the coin condition saved more than twice on average, 1,461 Shillings. Average savings in the treatments that involved a combination of the coin and *ex-post match 10%* and *pre-match 10%*, were 1,118 Shillings and 1,156 Shillings respectively. The difference between all three conditions that involved the coin and the control condition is statistically and economically significant. Second, we find that only sending weekly reminder text messages increased the fraction of participants in the existing customers sample who saved a positive amount in the six months of the experiment from 0.48 in the No Intervention condition to 0.73 in the Reminder condition. Furthermore, average savings in the Reminder condition were 674 Shillings whereas they were less than half in the No Intervention condition, only 266 Shillings. Together, these two results confirm the findings of previous research on the effect of reminders as well as support our hypothesis that further psychological interventions can increase its effect significantly.

Our third finding is that in the existing customers sample the text message from kids treatment and all treatments that involved financial incentives in the form of matching slightly increased savings compared to the control[6], however the increases are not statistically significant in any of these treatments. Going back to the policy question we posed earlier, our results provide evidence that a psychological intervention, in our study

[6] With the exception of *ex-post match 10%* treatment, in which average savings were slightly lower than the control.

the coin, can be more effective than financial incentives, and thus imply that policy makers and product designers should consider such interventions as viable alternatives to financial incentives whenever appropriate. Obviously the effectiveness of financial incentives can always be increased with higher matching amounts, however it is important to note that in our study, if we matched the actual savings of the coin condition participants from the existing customers sample at 20% with a 100 Shillings weekly cap, it would cost us 139 Shillings per person, which is worth about $1.5. On the other hand, the coins cost us only about $0.6 per person.[7] Thus, the coin was not only more effective than matching savings at 20% level, but also cost less.

Finally, we find that in the new customers sample, the majority of the participants, about 85 percent, who registered for Mbao at the beginning of the experiment did not save at all in the next six months. Average savings per saver in the six main treatments we employed in this sample were not significantly different from average savings in the control. We speculate that either these interventions were not strong enough to substantially change the behavior of a sample with little interest in saving to begin with, or that a low trust level in such a newly developed financial product suppressed any effect we could observe in this sample.

The rest of this paper is organized as follows: Section 2 gives background information about the mobile money system in Kenya and our partner institution. Section 3 explains the experimental setup. Section 4 explains the implementation. Section 5 describes our data. Section 6 reports the results and section 7 discusses the implications for policy and future research.

## 2. Background on mobile money and partner institution

Kenya's financial system experienced a major transformation when the first mobile phone-based financial system in the country was launched in 2007, called M-Pesa.[8] It is a service offered by the leading mobile operator in the country, Safaricom, a subsidiary of

[7] The coins were produced by a supplier from Alibaba.com and the cost per person mentioned here includes the shipping cost, which was about the same as the total cost of the coins.

[8] M stands for mobile and Pesa means money in Swahili. For detailed information on the MPesa system see Jack and Suri (2014).

Vodaphone. M-Pesa is in principle a checking account linked to the mobile phone number. In a country where less than 20 percent of the adult population has access to bank accounts whereas access to a mobile phone even among the residents of rural areas is 90 percent, M-Pesa has filled a major gap in the economy: Safe money transfer at a low cost. Anyone who has a mobile phone account and a national ID number can sign up for the M-Pesa account at a cost as much as 100 shillings (approx. $1.1). Once the account balance is positive, one can transfer e-money to another phone by sending a text message, receive e-money from another phone and cash-out the e-money in a certified M-Pesa agent shop. Depositing and receiving money is free, however there is a transaction cost associated with each transfer on the sender, following a step function. There is also a fixed cost for withdrawing money. Each M-Pesa account has a private PIN number required to be entered with each transaction, providing the security of accounts. Only after four years of its launch, in 2011, nearly 70 percent of the adult population (*14 million*) adopted M-Pesa; making it the most widely adopted mobile money system in the world (Jack and Suri, 2014). Partly, this success was possible by the quick spread of the *agents* network, small booths converting cash to e-money and vice versa. The total number of M-Pesa agents in the country reached 28,000 in 2011, making it easy for anyone to exchange e-money for cash at any time. M-Pesa accounts do not pay interest and do not lend money, thus it does not completely substitute a bank account. Government regulations restrict each individual account balance to be at most 50,000 shillings (approximately $555).

Given the high adoption of M-Pesa, many financial institutions started to integrate with it. Currently, many banks allow their customers to withdraw M-Pesa funds from ATMs and insurance companies accept payments through M-Pesa. For this project, we partnered with the administrators of a savings product, which is fully integrated with the M-Pesa system. This savings product, called Mbao, resembles a regular savings account with a few twists we will explain below.

Mbao was launched in late 2010 by Eagle Africa, an insurance company based in Nairobi and a large private bank, Kenya Commercial Bank (KCB hereafter) with the support of the government body responsible for regulating retirement plans (Retirement Benefits

Authority) and the national association of the informal sector workers (Jua Kali). The aim of the product was to provide informal sector workers formal access to savings[9] and to be a substitute for formal pension plans such workers do not have access to. The product was designed for this specific aim in mind: It does not offer a fixed interest rate like a regular savings account, instead funds are invested by KCB which serves as the funds manager, and returns for each account are calculated at the end of each year. It offers a binding contract to the members, encouraging long-term savings: Withdrawing funds before three years bears a cost and foregoing the returns of the year in which the withdrawal is made. Because its target group is low-middle income workers with irregular income stream, it does not impose a required minimum deposit either monthly or yearly. Registering is easy, one just provides his national ID number, fills out a one-page form and pays 100 shillings fee to sign up[10]. Since the product operates on M-Pesa system deposits are made by sending a text message, thereby moving the specified fund from the M-Pesa account to the Mbao account. Regular M-Pesa transfer fee applies and is deducted form the M-Pesa balance, and one immediately gets an update via text message about the new Mbao balance as well as the remaining M-Pesa balance. Mbao balance is checked via text message as well, with a regular text message cost. There are no regular balance updates other than upon a new deposit, thus checking the balance is costly. By the time we partnered with the administrators, the product customers were not getting any regular information or encouragement messages. With this operation model, it was very easy that saving slipped away from minds of the majority of customers unless they were very motivated.

According to the data the administrators shared with us, there were 35,204 unique accounts in the Mbao database in June 2013, of which only 11,041 were active in the six months prior to analysis. This low rate of active accounts is not surprising as the main channel of growing the customer base was arranging social (e.g. concerts), or community service events (e.g. free medical exams) and inviting the audience to sign up for free. Since signing up is so easy, even those with little or no interest could have done it because of reciprocity or social norm concerns. Prior to our experiment, we only had

[9] Informal sector made up 90 percent of all the businesses in the country in 2005 according to www.ilo.org.
[10] It is also offered for free at promotional events.

access to the unique identifiers for each account and their savings data. Not surprisingly given the zero communication policy of the product, more than half of the active accounts were only active in at most two weeks in the preceding six months of our initial analysis. We implemented our experiment on a selected sample from active accounts, to measure the effect of the interventions on a population with a revealed interest to save but low actual savings. To measure the effects on the interventions on new customers, we also offered the product to the residents of the slums in Nairobi. The following sections explain the experimental design, implementation and the timeline.

## 3. Design

Our experiment involved 10 treatment conditions, one control condition and one baseline condition, adding up to 12 conditions in total as shown in Table 1. Six treatments were main interventions and four treatments were a combination of two main interventions. We employed the full set of conditions with the existing customers sample and only the control and main treatment conditions with the new customers sample. The text of the weekly messages sent to each condition is summarized in Table 2. Baseline, control and main intervention treatments are explained below:

*No intervention (baseline):* As mentioned before, the customers did not get any regular message from the Mbao administrators before our experiment. Those in the *no reminder* condition received during the experiment just the same service as what they received before: No communication about their savings account.

*Reminder (control):* Those in this condition received two text messages every week, one reminder message on the first day of the week and one report message on the last day of each week. The reminder message read as: "Please don't forget to save for your future this week with MBAO PENSION PLAN." and the report message was: "Between *day1* and *day7* you saved Ksh *X* with MBAO for *account no.* Your account balance is Ksh *Y*." The reason for choosing this particular setup for the control condition was that some of the treatments required at least one message each week either on the first day or on the last day of the week. To avoid any bias that could result from sending messages on different times, we decided to include two messages in all conditions and only vary the content across conditions.

*Coin:* The coin treatment provided individuals a gold-colored coin, with 4 cm diameter and 0.6 pounds weight (approximately equal to the weight of 5 US Quarters). The coin was designed to look like real gold coins to represent savings. To intensify this feeling its surface displays a picture representing savings on the front and an increase in wealth on the back. The coin has engraved numbers from 1 to 24, representing each week of the six months intervention period. Those in this condition were told to think of this coin as something to represent their savings and were asked to keep it in a safe place and were told that they would use it to keep track of their weekly savings. During the experimental period, they received the same reminder message on the first day of the week as those in the control and the same report message at the end of each week. However, immediately after the report message, they received an additional message asking them to scratch *around* the corresponding week number if they saved that week and *below* if they did not.

*Kids:* This treatment differed from the control only in the reminder message. Those in this condition received the reminder message as "Hi *daddy/mommy*. Please deposit as much as you can this week to MBAO PENSION PLAN for our future. Thank you for saving. *Name*". The name of the kid was obtained from participants when they signed up for the experiment and they knew that the message was coming from the Mbao administrators. Those who did not have kids were asked to give the name of someone they would like to receive the message from and instead of mommy (daddy), their text message mentioned their name.

*Ex-post match (10% and 20%):* Those in the 10% ex-post match condition received 10 percent extra for their weekly savings up to 100 shillings. We implemented the cap to discourage gaming the system by those who can afford to save more, as well as because this is a common procedure for the match implementations in real world. We chose the cap as 100 shillings ($1.1) because it is low enough that the majority could afford saving that much each week, yet it is still much higher than the average savings per week of the previous year that it would be high enough even if it functioned as an anchor. The reminder message on the first day of the week read: "Save at least Ksh100 with MBAO PENSION PLAN this week to receive Ksh10 extra. If you don't save, you won't receive any extra money". The report message included only an extra sentence "You received x shillings extra from Mbao" on top of the control report message. The 20% ex-post match

treatment was exactly the same as the 10% ex-post match treatment except that the match rate was 20 percent up to 100 shillings.

*Pre-match (10% and 20%):* The pre-match treatments provided the same financial incentives as the ex-post match treatments. The difference was that the highest possible match was deposited in the accounts on the first day of the week and the reminder message provided this information with the following text: "MBAO PENSION PLAN deposited Ksh10 in your account. Save at least Ksh100 this week to keep it. If you don't save, MBAO will take it back." The report message was the same as the report message of the ex-post match conditions.

## 4. Timeline and implementation

The experiment lasted from October 2013 to October 2014. Between October 2013 and January 2013, we collected baseline survey data from all participants. Within a month after the baseline survey data was collected, each participant started to receive the intervention; and their saving data was collected for the next six months. At the end of six months we contacted all participants via cell phone and collected endline survey data. Endline survey data collection lasted from April to October in 2014. Even though baseline survey data from both existing and new customers samples were collected simultaneously, randomization was done separately within each sample and the procedure was slightly different for each of the two samples, as we explain below:

*Existing customers:* From the 11,041 customers in the database of the institution who were active in the six preceding months of June 2013, we selected the 4,596, who saved on average less than 250 Shillings in the weeks they were active. The reason for selecting such a specific group was to avoid including a distinct group of customers with higher income than the rest and potentially employed in formal sector.[11] This was also consistent with our goal of testing our interventions on individuals with low to middle income.

We randomly assigned each customer from the pre-selected sample to one of twelve conditions as explained earlier. Starting in October 2013, we called the customers in the

[11] We had been informed that the employees of the bank and insurance company were invited to join the plan in the early stages of development.

preselected group on their cell phones. After the introduction we informed them that they were randomly selected to receive a promotional service from the institution for 24 weeks and that we were conducting a research among customers to improve the product. After we explained the intervention they were randomly assigned to, we obtained their consent for research and collected baseline survey data. For those in the Coin condition, we obtained the location of their residence during the introductory call, arranged a particular day to deliver the coin to them and called again to confirm the delivery. During the second call we explained once again in detail how they were going to use the coin. Each customer was compensated for their time for the baseline survey with 150 Shillings, which was sent to them electronically via M-Pesa. To exclude those with formal jobs we first asked the customers in the preselected sample whether they were contributing to the national social security, which is compulsory for the formal sector employees. If they answered yes, we explained them that the research did not target formal sector employees. With this setup, we only collected baseline survey data from those who were willing to receive the promotional service from the institution.

Given that all interventions provided customers weekly messages including a weekly balance report, which is a considerable service by itself, we do not suspect that different interventions posed a selection bias problem.[12] In fact, the number of customers who accepted to participate in each condition was very similar around 210.

*New Customers:* We also offered the savings product to the residents of the major slums in Nairobi: Kibera, Makina and Mathare. In each slum, we opened a booth with Mbao fliers on the street and invited people to the booth and explained the product features. Then we informed potential customers that they are eligible to receive "a promotional service" as well if they register. In order to ensure fairness, we explained that there are seven different promotions (based on six treatments and one control condition) and that they would randomly select their promotion by choosing one of the seven cards with different shapes handed out to them. Based on the card they picked, we explained to them

[12] With the exception of No Intervention condition. But given that those who accepted to participate in the No Intervention condition could only be more motivated to save, this would only cause the potential underestimation of the effect of sending reminder messages compared to no intervention. And it does not pose any problem for estimating the effect of the treatment conditions compared to the control condition.

the relevant intervention and asked if they wanted to sign up. Signing up was free and it only required them to fill out the sign up form with their national identification number and their mobile phone number. If they signed up we also collected baseline survey data from them and compensated them for their time with 150 Shillings.

## 5. Data and Descriptive Statistics

We report the results using three datasets: The baseline and endline survey data we collected[13] and the transaction level deposits data.

Of the 4,596 members we pre-selected for the existing members sample, those we could reach[14], who did not contribute to the national social security and who accepted the promotional service were 2,643. 8 individuals out of this group changed their mind during the survey and stated that they were not interested in the promotion anymore. In the remaining sample, from 45 individuals we could not collect any baseline survey data. We drop these observations. Finally, 190 customers who accepted to participate in the study faced an issue with the institution within the first 2 weeks after they started to receive the intervention. In order to eliminate the potential effects of this problem on savings behavior, we drop these observations as well, noting that including them does not change the results. We report the survey and savings data of the 2400 remaining members who participated in the study.

In the slums, we offered the savings product to a total of 1567 individuals. Of these, 89 did not register, and 14 registered but did not want the promotion they were offered. We surveyed all individuals we offered the product to, however we report the results from the 1464 individuals who registered and accepted the promotion they were offered. We note that the high rate of the individuals who registered for the product might be due to the 150 Shillings we paid to those who took the baseline survey. Even though we only told them about the payment after we asked whether they wanted to register or not, it is possible that word had spread in the community and they already knew about the

[13] We didn't receive the endline survey data yet, but it will be included in the paper very soon.

[14] It is common that people change their phone numbers often, thus some phone numbers we called were not active anymore or belonged to another person.

payment. As we will describe later, results form this sample reveal that only a small fraction of those who registered actually saved with the product, suggesting that many of those enrolled had no intention to save using Mbao.

Table 1 shows summary statistics for six demographic variables and two variables related to income, separate for the two samples. Key differences between the two samples were that (1) a larger fraction of the new customers sample consisted of female (0.44 versus 0.34), new customers sample was about five years younger than the existing customers sample and (3) a lower fraction of the new customers were married compared to the existing customers sample (0.50 versus 0.71). Table 2 and Table 3 show the summary statistics of the same variables for each condition separately and reports the p-value of an F-test after regressing the outcome variable on dummies for each condition, omitting the control condition (Reminder). None of the variables were significantly different across conditions except for the household size, confirming that our randomization was done properly.

## 6. Results

Before we present the results of the experiment, we present the results of a "prediction study" in which we collected online survey data from 119 individuals recruited on Amazon M-Turk in exchange for 25 cents. The participants in this study were US residents of average age 34.8 and 85% had some college degree. In this study we described the interventions and asked participants to predict the effectiveness of each treatment. On average participants predicted that the pre-match 20% and ex-post match 20% would yield the highest savings, followed by similar average ranking of pre-match 105 and ex-post match 10%, followed by *kids* treatment. The treatment predicted to be least effective was the *coin* treatment. Below we present the results of the real experiment and contrast them with these intuitions.

Given the differences between the two samples, we analyze the effects of treatments separately for each sample. Overall, the response to the interventions was quite different in each sample: Among the existing customers sample seventy-three percent of the

sample saved during the experimental period, however only 15.3 % of the new customers sample saved any amount.

### 6.1 Treatment effects in the existing customers sample

We could not deliver the coin to a total of 47 participants in the three conditions involving it. 11 participants in the "Coin", 24 participants in the "Pre-match 10% & Coin" and 12 participants in the "Ex-post match 10% & Coin" treatment did not receive the coin. To avoid selection bias, we follow the intent-to-treat approach and report general results by keeping these individuals in the sample. For the *kids* condition, summary statistics from Table 1 shows that 81 percent of this sample actually had kids. We report the results including all observations form this treatment in the analysis as well.

We first present the results by looking at the total savings per person during the experimental period. There was one outlier in the data with a particularly higher total savings than the rest of the sample.[15] This observation belonged to the No Intervention condition. We drop this observation reporting the results in this section because it creates a very high noise for and masks the underlying pattern of the No Intervention condition.

Figure 2 summarizes the main results from this section and shows the graph of average savings during the experimental period in each condition. There is a very clear pattern in this graph: First, average savings in the No Intervention condition is significantly lower than the Control (Reminder) condition. Second, in all treatments excluding the three that involve the coin, average savings are very close to the average savings in the control (Reminder) and the difference between each of these treatments and the control is not statistically significant. Third, those who only received the coin saved on average the most among all conditions, 1461 Shillings, where average savings in the control condition was only 674 Shillings. This implies that on average, the coin more than doubled average savings. Finally, average savings in all three treatments, which involved the coin, and *only* average savings in these treatments, were significantly higher than the

[15] This observation was 20 standard deviations higher than the mean. The next highest total savings per person was 13 standard deviations higher than the mean.

average savings in the control. It is interesting that in the two treatments combining ex-post and pre-match interventions at 10% level with the coin, the average savings were 1,118 and 1,156 Shillings respectively, both of which is lower than the average savings in the coin treatment. Thus, over the coin there was no additional benefit of adding financial incentives to the intervention. While it is interesting that combining the coin with financial incentives lowered the average savings, we hypothesize that this is caused by the 100 Shillings-weekly cap imposed on the match. Such an unintended anchoring effect is indeed a potential problem in every matching program (See Madrian, 2012 for a discussion) and it is particularly interesting that we observe such a negative effect of the match program in our sample, even though we paid particular attention not to be a victim of it when we designed the experiment. We further investigate this later by looking at individual savings patterns in the three coin treatments separately.

In summary, the most important result for this sample is that giving a tangible coin to the participants and asking them to keep track of their weekly savings influenced their savings behavior strongly, resulting in statistically and economically significant increase in savings.

To further analyze this data, in Table 6 we report the results of the OLS regression with the following model:

$$S_i = \alpha + \beta_{ci} Condition\ dummy_{ci} + \varepsilon_i$$

$S_i$ is the outcome variable for individual $i$. $Condition\ dummy_c$ where $c \in \{1,11\}$ is a dummy variable for each condition, omitting the control condition (Reminder). The outcome variable in specification 4 is a dummy taking the value one if a participant saved a positive amount during the experimental period and zero if he did not save at all. In all the other specifications, the outcome variable is total savings per person during the experimental period. Specification (1) conveys the same information as in the Figure 2, while specifications (2) and (3) take the effect of the outliers into account with the winsorized samples at top 1% and top 5% respectively. The main result of this part is that the sharp increase in average savings caused by the Coin treatment is not driven by

outliers. Specification (4) reveals that about 73% of those in control condition saved a positive amount during the experimental period. The share of those who saved positive was about 7% higher in the two Pre-match conditions, which were marginally significant, but almost the same in all other conditions except for the No Intervention. In No Intervention condition, the fraction of savers was 25 percent less than the control, highlighting the importance of reminders on savings behavior. The effect in this sample is particularly higher than the effect from Karlan et al. (2012) which might be due to the fact that the reminder condition in our experiment did not only remind to save but also provided free balance reports. Finally, the last three specifications look at the effects of the treatments on savings conditional on saving a positive amount. Since the fraction of savers was not substantially different among conditions, as expected, the results form these specifications follow the pattern observed in the first three specifications.

### 6.2 Treatment effects in the new customers sample

Because there is no such a clear pattern of results in the new customers sample, we only report the OLS regressions following the same estimation strategy we used for the existing customers sample in Table 7. In the first specification we see that the average savings in the control condition is 80.7 Shillings, quite lower than the average savings we observed in the existing customers sample. The main reason for this result is that the majority of this sample, 85 percent of those who registered for Mbao, did not save at all in the six months period. Even though the average savings in the *coin* treatment is the highest compared to the control, the difference is not statistically significant and the second and third specifications imply that it is driven by outliers. Average savings in the ex-post match 10% treatment is 75 Shillings higher than the average savings in the control treatment, which is largely driven by the higher fraction of participants saving in this treatment. An interesting result from these estimations is that those in the *kids* treatment saved on average less than those in the control. But given the very low share of savers it is pre-mature to interpret these results.

We hypothesize that the low share of savers in this sample can be due to our payment of 150 Shillings to those who took the baseline survey and the possibility that those who

registered were not interested in saving with Mbao. If our hypothesis holds, the results from this sample imply that none of the interventions had the needed power of significantly increasing the interest in this product. However, it is important to note, that it is also possible that the lack of interest in the Mbao saving plan could extend beyond a general lack of interest (and an interest only in the 150 Shillings) to a deeper lack of trust in slums in such financial product.

## 7. Discussion

In this study, we implemented two psychological interventions along with small financial incentives in an attempt to increase savings rates on a sample of existing and new customers of a newly developed savings product in Kenya, called Mbao. In general we find that among those who had an initial interest in saving with the product, the coin treatment led to an increase in average savings by more than 100% compared to the control, thus being the most effective intervention among the many interventions we tested. Furthermore, in line with previous research concerning the effectiveness of reminder messages, we also find evidence that reminders are effective in increasing savings. We found that sending regular reminder and balance report messages increased average savings about 100% compared to not sending any messages. In contrast, the small financial incentives we provided did not seem to have a big effect on savings rate in the existing customer sample. We speculate that the ineffectiveness of these financial incentives is because for some customers the matching financial incentives offered were not high enough to change behavior while for other customers they were ineffective because of the weekly cap we imposed on matching.

Our results have theoretical as well as practical implications and call for future research to better understand the role of tangible reminders on saving rates, among the poor and more generally. On the theoretical side, the fact that providing the coin was more effective than simply reminding to save implies that the design of the way information is presented can have a significant effect on behavior. Previous research has focused on the effects of framing the language on behavior, however our finding suggests that there is more to modify the information presentation to influence behavior. In particular, it

suggests that in the context of savings, a tangible representation of overall behavior (coin representing overall savings) and small activities (scratches representing weekly deposits) can motivate a behavior with immediate costs and delayed gratification.

Practically, our study suggests that policy makers and product designers could benefit from this type of low-cost and practical interventions to target positive behavior change. The results further suggest that the common-sense assumptions about the effectiveness of policies might fail strongly and emphasizes the benefits of using controlled experiments to guide policy decisions. The authors in fact speculate that a lot of policies impacting the lives of millions are based on gut feelings of the few people in charge, and that by experimentation many of such policies could be improved.

Finally, it would be interesting to explore how to improve the coin we used in this study. For example, in a world where near field payments such as Apple Pay becomes more prevalent, could we present a virtual coin displayed on a smartphone to influence savings behavior of low-middle income individuals in the developed world? As the world moves to electronic payment systems, the questions of how to represent money and savings become more open and more important, and understanding how to do it right can be one of the keys to a better financial future.

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TABLES

**Table 1: Design of the experiment and the sample size**

| Existing Customers | N | New Customers | N |
|---|---|---|---|
| 1. No Intervention | 124 | 1. Reminder (Control) | 203 |
| 2. Reminder (Control) | 204 | 2. Coin | 197 |
| 3. Coin | 206 | 3. Kids | 201 |
| 4. Kids | 207 | 4. Ex-post match 10% | 214 |
| 5. Ex-post match 10% | 208 | 5. Ex-post match 20% | 214 |
| 6. Ex-post matching 20% | 211 | 6. Pre-match 10% | 223 |
| 7. Pre-match 10% | 201 | 7. Pre-match 20% | 212 |
| 8. Pre-match 20% | 210 | | |
| 9. Ex-post match 10% & Kids | 209 | | |
| 10. Pre- match 10% & Kids | 207 | | |
| 11. Ex-post match 10% & Coin | 200 | | |
| 12. Pre- match 10% & Coin | 213 | | |
| Total Sample Size: | 2400 | Total Sample Size: | 1464 |

**Table 2: Text Messages**

| Conditions | Reminder Message | | Report Message |
|---|---|---|---|
| Reminder (Control) | Please don't forget to save for your future this week with MBAO PENSION PLAN. | | Between <begin> and <end> you saved Ksh<save> with MBAO for account <id>. Your account balance is Ksh<balance>. |
| Coin | same as control | message 1: | same as control |
| | | *message 2: if saved* | You saved with MBAO PENSION PLAN this week. Please scratch AROUND <week> on your gold coin. |
| | | *message 2: if not saved* | You didn't save with MBAO PENSION PLAN this week. Please scratch BELOW <week> on your gold coin. |
| Kids | Hi <daddy>, Please deposit as much as you can this week to MBAO PENSION PLAN for our future! Thank you for saving. <Jane> | | same as control |
| Ex-post match 10% | Save at least Ksh100 with MBAO PENSION PLAN this week to receive Ksh10 extra. If you don't save, you won't receive any extra money. | | Between <begin> and <end> you saved Ksh<save> with MBAO for account <id>. You received <save/10> extra from MBAO. Your account balance is Ksh<balance>. |
| Pre-match 10% | MBAO PENSION PLAN deposited Ksh10 in your account. Save at least Ksh100 this week to keep it. If you don't save, MBAO will take it back. | | Between <begin> and <end> you saved Ksh<save> with MBAO for account <id>. You received <save/10> extra from MBAO. Your account balance is Ksh<balance>. |

**Table 3: Summary statistics for key variables in each sample**

| | Existing Customers | New Customers |
|---|---|---|
| Female | 0.342 | 0.443 |
| | (0.010) | (0.013) |
| | [2400] | [1464] |
| Age | 36.510 | 31.372 |
| | (0.194) | (0.259) |
| | [2351] | [1464] |
| Years of education | 10.703 | 10.072 |
| | (0.061) | (0.079) |
| | [2370] | [1463] |
| Married | 0.716 | 0.502 |
| | (0.009) | (0.013) |
| | [2376] | [1464] |
| Have children | 0.819 | 0.696 |
| | (0.008) | (0.012) |
| | [2375] | [1464] |
| Householdsize | 3.874 | 4.280 |
| | (0.041) | (0.094) |
| | [2371] | [1018] |
| Renter | 0.830 | 0.870 |
| | (0.008) | (0.009) |
| | [2373] | [1464] |
| Irregular income | 0.887 | 0.833 |
| | (0.007) | (0.011) |
| | [2219] | [1121] |

*Notes:* For each variable, standard errors are presented in the parantheses and the number of observations are presented in brackets.

## Table 4: Summary statistics for key variables in existing customers sample

| | All sample | No Intervention | Reminder | Coin | Kids | Pre-match 10% | Pre-match 20% | Post-match 10% | Post-match 20% | Pre-match 10% & Kids | Pre-match 10% & Coin | Post-match 10% & Kids | Post-match 10% & Coin | p-value |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Female | 0.342 | 0.363 | 0.392 | 0.364 | 0.343 | 0.308 | 0.338 | 0.356 | 0.322 | 0.329 | 0.305 | 0.364 | 0.325 | 0.829 |
| | (0.010) | (0.043) | (0.034) | (0.034) | (0.033) | (0.033) | (0.033) | (0.033) | (0.032) | (0.033) | (0.032) | (0.033) | (0.033) | |
| | [2400] | | | | | | | | | | | | | |
| Age | 36.510 | 37.439 | 36.915 | 36.722 | 36.920 | 37.327 | 35.800 | 35.351 | 36.137 | 36.468 | 35.948 | 36.780 | 36.755 | 0.625 |
| | (0.194) | (0.897) | (0.651) | (0.656) | (0.663) | (0.693) | (0.664) | (0.663) | (0.634) | (0.651) | (0.635) | (0.635) | (0.713) | |
| | [2351] | | | | | | | | | | | | | |
| Years of education | 10.703 | 10.672 | 10.545 | 11.020 | 10.581 | 10.625 | 10.816 | 10.788 | 10.641 | 10.704 | 10.769 | 10.820 | 10.426 | 0.890 |
| | (0.061) | (0.238) | (0.228) | (0.209) | (0.223) | (0.226) | (0.186) | (0.187) | (0.223) | (0.199) | (0.181) | (0.215) | (0.227) | |
| | [2370] | | | | | | | | | | | | | |
| Married | 0.716 | 0.734 | 0.729 | 0.699 | 0.672 | 0.725 | 0.734 | 0.663 | 0.758 | 0.689 | 0.744 | 0.716 | 0.735 | 0.588 |
| | (0.009) | (0.040) | (0.031) | (0.032) | (0.033) | (0.032) | (0.031) | (0.033) | (0.030) | (0.032) | (0.030) | (0.032) | (0.032) | |
| | [2376] | | | | | | | | | | | | | |
| Have children | 0.819 | 0.863 | 0.833 | 0.825 | 0.863 | 0.815 | 0.778 | 0.793 | 0.815 | 0.806 | 0.782 | 0.859 | 0.816 | 0.303 |
| | (0.008) | (0.031) | (0.026) | (0.027) | (0.024) | (0.028) | (0.029) | (0.028) | (0.027) | (0.028) | (0.028) | (0.024) | (0.028) | |
| | [2375] | | | | | | | | | | | | | |
| Householdsize | 3.874 | 3.734 | 4.192 | 3.888 | 4.186 | 4.065 | 3.922 | 3.812 | 3.941 | 3.568 | 3.814 | 3.778 | 3.526 | 0.010 |
| | (0.041) | (0.178) | (0.144) | (0.141) | (0.141) | (0.144) | (0.144) | (0.142) | (0.139) | (0.130) | (0.133) | (0.133) | (0.143) | |
| | [2371] | | | | | | | | | | | | | |
| Renter | 0.830 | 0.831 | 0.768 | 0.811 | 0.813 | 0.865 | 0.807 | 0.812 | 0.855 | 0.840 | 0.852 | 0.824 | 0.888 | 0.120 |
| | (0.008) | (0.034) | (0.030) | (0.027) | (0.027) | (0.024) | (0.028) | (0.027) | (0.025) | (0.026) | (0.025) | (0.027) | (0.023) | |
| | [2373] | | | | | | | | | | | | | |
| Irregular income | 0.887 | 0.904 | 0.899 | 0.882 | 0.881 | 0.867 | 0.897 | 0.858 | 0.886 | 0.918 | 0.874 | 0.878 | 0.910 | 0.818 |
| | (0.007) | (0.028) | (0.022) | (0.024) | (0.023) | (0.025) | (0.022) | (0.025) | (0.023) | (0.020) | (0.024) | (0.024) | (0.021) | |
| | [2219] | | | | | | | | | | | | | |

Notes: For each variabl, standard errors are presented in parantheses. In the first column, total number of observations for each variable is presented in brackets. The last column reports the p-value of the F-statistic from regressing the variable of interest on condition dummies.

**Table 5: Summary statistics for key variables in new customers sample**

| | All sample | Reminder | Coin | Kids | Pre-match | | Post-match | | p-value |
|---|---|---|---|---|---|---|---|---|---|
| | | | | | 10% | 20% | 10% | 20% | |
| Female | 0.443 | 0.458 | 0.416 | 0.483 | 0.426 | 0.458 | 0.449 | 0.411 | 0.760 |
| | (0.013) | (0.035) | (0.035) | (0.035) | (0.033) | (0.034) | (0.034) | (0.034) | |
| | [1464] | | | | | | | | |
| Age | 31.372 | 32.296 | 30.279 | 30.592 | 31.704 | 31.014 | 32.879 | 30.734 | 0.089 |
| | (0.259) | (0.731) | (0.663) | (0.662) | (0.672) | (0.657) | (0.762) | (0.633) | |
| | [1464] | | | | | | | | |
| Years of education | 10.072 | 9.813 | 10.548 | 10.294 | 9.991 | 9.873 | 10.023 | 10.005 | 0.189 |
| | (0.079) | (0.229) | (0.215) | (0.197) | (0.193) | (0.204) | (0.198) | (0.218) | |
| | [1463] | | | | | | | | |
| Married | 0.502 | 0.517 | 0.437 | 0.547 | 0.538 | 0.524 | 0.505 | 0.444 | 0.137 |
| | (0.013) | (0.035) | (0.035) | (0.035) | (0.033) | (0.034) | (0.034) | (0.034) | |
| | [1464] | | | | | | | | |
| Have children | 0.696 | 0.724 | 0.629 | 0.701 | 0.704 | 0.689 | 0.720 | 0.701 | 0.509 |
| | (0.012) | (0.031) | (0.034) | (0.032) | (0.031) | (0.032) | (0.031) | (0.031) | |
| | [1464] | | | | | | | | |
| Householdsize | 4.280 | 4.449 | 4.137 | 4.057 | 4.433 | 4.786 | 4.344 | 3.727 | 0.021 |
| | (0.094) | (0.211) | (0.221) | (0.174) | (0.183) | (0.467) | (0.187) | (0.155) | |
| | [1018] | | | | | | | | |
| Renter | 0.870 | 0.862 | 0.878 | 0.856 | 0.892 | 0.854 | 0.911 | 0.832 | 0.195 |
| | (0.009) | (0.024) | (0.023) | (0.025) | (0.021) | (0.024) | (0.019) | (0.026) | |
| | [1464] | | | | | | | | |
| Irregular income | 0.833 | 0.832 | 0.811 | 0.825 | 0.856 | 0.843 | 0.826 | 0.835 | 0.961 |
| | (0.011) | (0.030) | (0.032) | (0.031) | (0.027) | (0.029) | (0.029) | (0.029) | |
| | [1121] | | | | | | | | |

Notes: For each variable, standard errors are presented in parantheses. In the first column, total number of observations for each variable is presented in brackets. The last column reports the p-value of the F-statistic from regressing the variable of interest on condition dummies.

**Table 6: Regressions with existing customers sample**

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | Savings during trial | | | Fraction of savers | Savings during trial | | |
| Conditions | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Coin | 787.2*** | 527.4*** | 389.0*** | 0.046 | 970.8*** | 636.3*** | 460.4*** |
| | (211.3) | (129.7) | (100.2) | (0.043) | (261.8) | (154.8) | (116.6) |
| Kids | 162.9 | 109.0 | 47.92 | 0.018 | 185.9 | 115.3 | 36.73 |
| | (129.6) | (112.1) | (89.07) | (0.043) | (161.8) | (138.3) | (107.6) |
| Pre-match 10% | 195.4 | 82.59 | 55.04 | 0.081* | 161.5 | 22.41 | -7.260 |
| | (171.5) | (104.9) | (85.82) | (0.042) | (208.7) | (125.9) | (101.4) |
| Ex-post match 10% | -52.26 | -63.51 | -80.49 | -0.053 | -12.50 | -28.75 | -56.70 |
| | (104.2) | (100.5) | (83.64) | (0.045) | (134.8) | (129.1) | (104.4) |
| Pre-match 20% | 122.2 | 119.0 | 136.3 | 0.079* | 61.57 | 57.65 | 83.50 |
| | (97.69) | (96.75) | (86.95) | (0.041) | (116.2) | (115.1) | (101.9) |
| Ex-post match 20% | 82.39 | 69.95 | 50.71 | 0.014 | 99.55 | 82.94 | 57.81 |
| | (107.1) | (103.1) | (87.66) | (0.043) | (132.7) | (127.3) | (106.1) |
| Pre-match 10%& Kids | 213.0* | 203.8* | 180.5** | 0.062 | 202.2 | 190.7 | 164.9 |
| | (109.4) | (106.7) | (92.02) | (0.042) | (131.1) | (127.6) | (108.4) |
| Ex-post match 10% & Kids | 147.2 | 61.64 | 39.10 | 0.030 | 162.1 | 50.36 | 22.56 |
| | (138.1) | (103.6) | (87.03) | (0.043) | (173.5) | (127.0) | (104.8) |
| Pre-match 10% & Coin | 482.9*** | 397.8*** | 311.9*** | 0.044 | 582.9*** | 473.1*** | 364.3*** |
| | (143.2) | (117.9) | (93.56) | (0.042) | (174.8) | (141.3) | (109.0) |
| Ex-post match 10% & Coin | 444.3*** | 376.9*** | 325.1*** | 0.035 | 541.7*** | 454.2*** | 388.9*** |
| | (147.2) | (117.1) | (98.66) | (0.043) | (180.8) | (140.2) | (115.6) |
| No Intervention | -407.7*** | -407.7*** | -371.2*** | -0.299*** | -364.7*** | -364.7*** | -315.4** |
| | (89.71) | (89.71) | (83.28) | (0.055) | (133.4) | (133.4) | (126.2) |
| Constant | 674.0*** | 674.0*** | 637.4*** | 0.730*** | 910.5*** | 910.5*** | 861.1*** |
| | (71.21) | (71.21) | (62.92) | (0.031) | (88.52) | (88.52) | (77.16) |
| Observations | 2,399 | 2,399 | 2,399 | 2,399 | 1,810 | 1,810 | 1,810 |
| R-squared | 0.026 | 0.033 | 0.037 | 0.035 | 0.028 | 0.035 | 0.038 |
| Winsorized | | 1% | 5% | | | 1% | 5% |
| Among savers | | | | | Yes | Yes | Yes |

Notes: All monetary values are in Kenyan Shillings. 1 USD = 90.3 Kenyan Shllings approximately. Robust standard errors are in parentheses. Significance levels are *** p<0.01, ** p<0.05, * p<0.1

**Table 7: Regressions with new customers sample**

| | Savings during trial | | | Fraction of savers | Savings during trial | | |
|---|---|---|---|---|---|---|---|
| Conditions | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Coin | 104.9 | 17.81 | 9.323 | 0.025 | 516.0 | 30.08 | 10.38 |
| | (69.31) | (29.68) | (13.55) | (0.037) | (356.2) | (137.1) | (45.28) |
| Kids | -35.82 | -32.88 | -19.54* | -0.028 | -167.6 | -150.5 | -88.88* |
| | (26.45) | (23.88) | (11.28) | (0.034) | (154.2) | (136.3) | (49.64) |
| Pre-match 10% | 6.075 | 0.665 | -3.835 | -0.000 | 40.68 | 5.157 | -24.68 |
| | (32.20) | (27.24) | (12.43) | (0.035) | (171.7) | (138.2) | (48.30) |
| Ex-post match 10% | 75.44* | 59.36* | 25.28* | 0.095** | 101.9 | 48.15 | -12.83 |
| | (41.04) | (32.03) | (13.94) | (0.039) | (163.4) | (123.4) | (41.93) |
| Pre-match 20% | -0.153 | -6.923 | -4.655 | -0.006 | 22.39 | -25.19 | -18.55 |
| | (32.81) | (25.63) | (12.46) | (0.035) | (182.6) | (129.2) | (48.37) |
| Ex-post match 20% | 8.603 | -9.276 | -4.245 | -0.003 | 68.76 | -51.42 | -22.02 |
| | (36.73) | (25.77) | (12.34) | (0.035) | (208.2) | (131.3) | (46.74) |
| Constant | 80.72*** | 76.29*** | 45.76*** | 0.153*** | 528.6*** | 499.5*** | 299.7*** |
| | (21.18) | (18.68) | (9.067) | (0.025) | (108.9) | (91.17) | (32.95) |
| Observations | 1,464 | 1,464 | 1,464 | 1,464 | 241 | 241 | 241 |
| R-squared | 0.008 | 0.008 | 0.010 | 0.010 | 0.031 | 0.011 | 0.019 |
| Winsorized | | 1% | 5% | | | 1% | 5% |
| Among savers | | | | | Yes | Yes | Yes |

Notes: All monetary values are in Kenyan Shillings. 1 USD = 90.3 Kenyan Shllings approximately.
Robust standard errors are in parentheses. Significance levels are *** p<0.01, ** p<0.05, * p<0.1

FIGURES

**Figure 1:**

Design of the coin

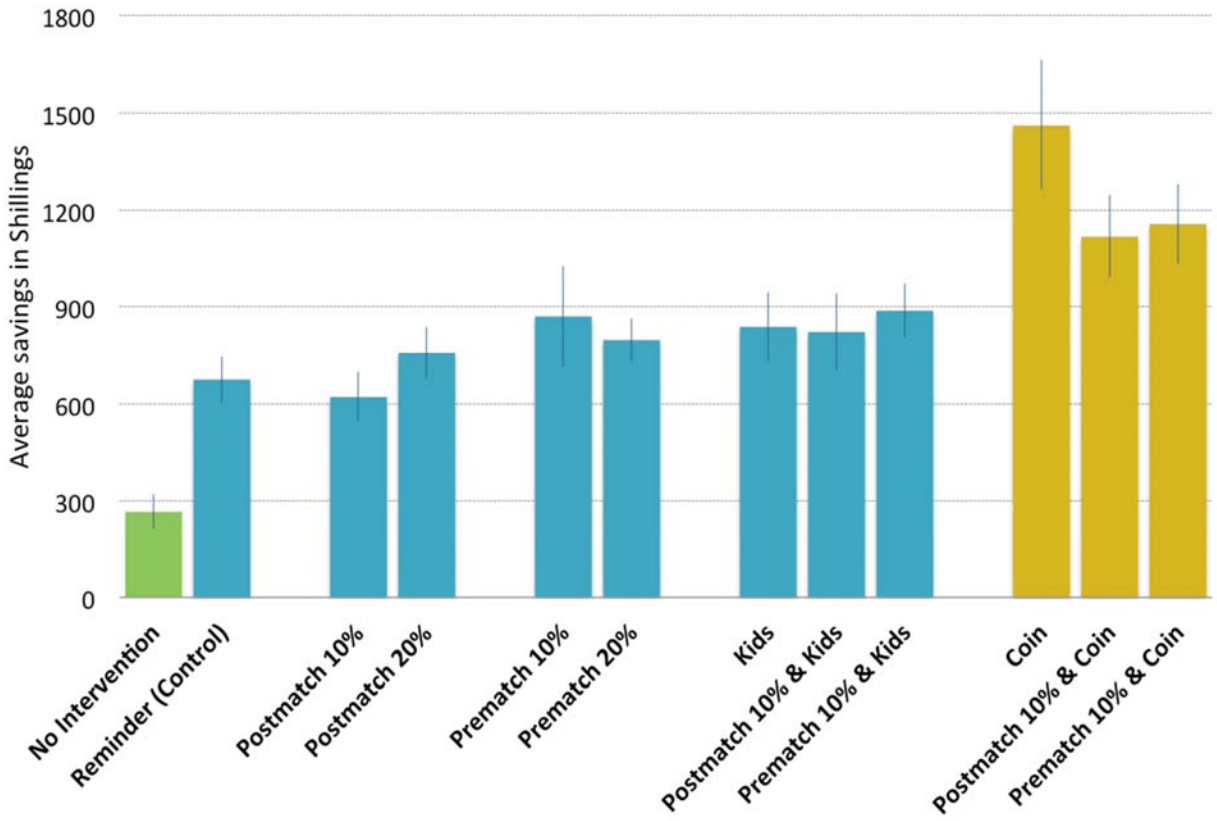


**Figure 2:**

Average savings (calculated as the sum of deposits during 24-week experiment period) per person in each treatment