Priming the pump:
Can paying interest upfront increase savings?

Peter P. Carroll[*] Flora Myamba[†] Daniel L. Nielson[‡] Joseph Price[§]
Philip Roessler[¶] Wayne Aaron Sandholtz[||]

August 16, 2023[**]

Abstract

We experimentally test whether people are more likely to reach a savings target when interest is paid as a conditional upfront bonus, rather than after the savings target has been reached. We partner with a mobile network operator (MNO) and an NGO which provides cash transfers to women in rural Tanzania. We invited participants to keep at least 10% of their cash transfers in their mobile wallet until a certain date, 5 months later. We randomized whether doing so would let them keep an "upfront" bonus provided at the start, earn an equivalent sum of "interest" at the end, or receive no bonus but only "info" about saving. We measure outcomes using daily mobile money wallet balances provided by the MNO. The "upfront" group were more likely to reach the savings target (net of the bonus), not just by the end but also throughout the study period. They were less likely to withdraw the entire cash transfer immediately, and more likely to make deposits during the study period. The traditional "interest" treatment, despite offering an above-market interest rate, had no effect on meeting the target or on deposits and withdrawals. Neither treatment had any effect on noisy average mobile wallet balances, but the "upfront" group carried higher median net mobile wallet balances one month into the program. However, after regulatory disputes prevented th NGO from making an expected cash transfer during the study period, many participants withdrew all money – including the upfront bonus – such that the upfront group's median net balance at the study's end was negative, and lower than the other groups'. We show that the design of financial products can affect savings behavior.

Keywords:
JEL Codes:

[*]Department of Political Science, University of Michigan, Ann Arbor, MI, USA
[†]Women and Social Protection in Tanzania, Dar es Salaam, Tanzania
[‡]Department of Government, University of Texas at Austin, Austin, TX, USA
[§]Department of Economics, Brigham Young University, Provo, UT, USA
[¶]Department of Government, William & Mary, Williamsburg, VA, USA
[||]Department of Economics, Nova School of Business and Economics, Carcavelos, Portugal

[**]We are grateful for our implementing partner Innovations for Poverty Action Tanzania, especially Rachel Steinacher, Rachel Jones, Justus Mugango, and Robert Mwandumbya. We thank Lore Vandewalle and Victoire Girard for helpful comments. This project was funded by the Innovations for Poverty Action Financial Inclusion Program, Financial Services for the Poor Initiative. We thank the participants of the Evidence in Governance and Politics (EGAP) 2017 meeting for their helpful comments during the design stage of this project. Pre-registration materials available at https://osf.io/z263e.

1 Introduction

Many individuals save less than they would like. This is true among financially literate individuals in rich countries (Choi, Laibson, Madrian, & Metrick, 2004; Madrian & Shea, 2001). It is also true among the world's poor, who tend to be saddled with additional stressors that affect decision-making (Mani, Mullainathan, Shafir, & Zhao, 2013; Banerjee & Duflo, 2007; Collins, Morduch, Rutherford, & Ruthven, 2009). Undersaving can reduce welfare by making it difficult to smooth consumption, respond to negative shocks, or make profitable investments (Karlan, Ratan, & Zinman, 2014). Existing evidence suggests that while many individuals may simply be too poor to save, the design of financial products can affect their take-up, in the developed world and the developing world (Thaler & Benartzi, 2004; Dupas, Karlan, Robinson, & Ubfal, 2018).

Interest is typically paid after savings have been salted away for a while – but what if prospective savers were instead paid an equivalent bonus upfront, conditional on saving a certain amount of principal for a given time? This might make interest feel more salient, changing people's savings behavior (Finkelstein, 2009; Cabral & Hoxby, 2012). The bestowal of an upfront bonus which can be lost might motivate greater savings effort through the endowment effect (Kahneman, Knetsch, & Thaler, 1990).

Upfront conditional bonuses have been found to affect effort in contexts as diverse as education and manufacturing (Levitt, List, Neckermann, & Sadoff, 2016; Hossain & List, 2012).

Conditional upfront bonuses can produce unexpected results, however, when there is more than one dimension of effort. Pierce, Rees-Jones, and Blank (2022) provided car salespeople with performance bonuses upfront, with the threat of clawback upon failure to reach a sales target. It changed sales behavior, but backfired for the company because workers tended to give up on meeting the target for more lucrative cars and focused instead on meeting the more accessible target for lower-value cars. Because upfront bonuses function essentially as an interest-free loan, miscalibrations of this kind can be costly.

In this paper, we experimentally evaluate a novel savings product which pays interest as a conditional upfront bonus in rural Tanzania. Tanzania is an important place in which to learn more about the drivers of savings and financial inclusion: at the time of the study, less than half of Tanzanians above age 15 had a bank account or mobile money account, and less than a fifth of Tanzanians under 55 report that they are saving for retirement (World Bank, 2017; FinScope, 2017).

We partner with an NGO which was providing quarterly cash transfers of 70,000 Tanzanian shillings ($\approx$ \$32 USD) via mobile money to women in the Shinyanga region of rural Tanzania. We were able

to obtain daily mobile wallet balance data from the mobile network operator (MNO) through which the mobile money cash transfers were administered. This created an attractive environment for testing a savings intervention; the cash transfers created a predictable and uniform influx of principal, and the administrative data on mobile wallet balances provided unusually clean measures of financial activity and savings outcomes through the mobile wallet. Some research on cash transfers has found that recipients save and/or invest part of the transfers (Ravallion & Chen, 2005; Gertler, Martinez, & Rubio-Codina, 2012; Handa, Natali, Seidenfeld, Tembo, & Davis, 2018). Other work has found no impact of cash transfers on savings (Baird, McIntosh, & Özler, 2019). But requiring recipients to save part of their cash transfers has been shown to raise educational attainment, suggesting that encouragements to save cash transfers can lead to productive investments (Barrera-Osorio, Linden, & Saavedra, 2019).

We randomized 371 participants into three savings incentives groups; each group received a separate training at the start of the program. We invited all groups to keep at least 10% of each cash transfer they received during the study in their mobile wallet until a certain date, five months away. The "upfront" group received an immediate bonus of 7000 TZS, which they were told they would lose if they failed to meet the savings threshold by the target date. The "interest" group were told they would receive 7000 TZS at the end of the study if they met the savings threshold by that date. The "info" group were encouraged but not incentivized to meet the target by the end of the period. All participants were sent text messages throughout the study period reminding them of the details of their incentive and encouraging them to save.[1]

The effective interest rate of these saving schemes – both of the upfront and the traditional variety – were designed to be much higher than market interest rates in order to enable a clear comparison of the timing of the payment. Participants received a bonus (or interest) payment of 7000 TZS in exchange for saving 7000 TZS for roughly five months. This translates into a 240% nominal annual interest rate, or 236.2% real interest rate after accounting for 3.8% inflation (National Bureau of Statistics, 2018). Assuming quarterly compounding interest, this corresponds to an Annual Percent Yield of 540%.

We measured participants' savings responses using administrative data on mobile wallet balances furnished by the MNO.[2] This data consists of daily balance data for all participants in the experiment, for some (but not all) of the days of the experimental period. Our primary outcome variable is the

[1] Figure C.5 shows one of the training materials used to explain the upfront bonus scheme.

[2] MNOs were long prohibited from offering interest in many jurisdictions. Since mobile money is the only interaction many people have with the formal financial system, this made it difficult to examine the role of interest in how the very poor make savings decisions in the developing world. By distributing profits in a way comparable to paying interest, our MNO partner found a way to circumvent this restriction (?, ?).

mobile wallet balance on 24 Sept 2018, the set deadline for participants to meet their savings goals and receive the interest payment or keep the bonus. We define savings balances net of the upfront bonus. This administrative data allows us to confirm that cash transfers were received in most women's mobile wallets, as were the upfront bonus payments we sent to the "upfront" treatment group. Participants received one transfer of 70,000 TZS shortly after the experiment began, in April of 2018. They were meant to receive another one in July of 2018, but due to problems with the NGO this transfer never arrived.

Our main result is that the upfront account increased the likelihood that participants reached the savings target. By the end of the study period, the upfront group was 14 percentage points more likely to have at least 7000 TZS in their account (net of the bonus), from a base of 19% in the info-only group. (The interest group was statistically indistinguishable from the info group.) This difference appeared one day after the April cash transfer and persisted throughout the study period. Participants in the upfront group were less than half as likely to withdraw the entire value of their cash transfer within one day. They made more frequent deposits during the study period – and withdrew more after it ended, showing that they understood the terms of the program.

Although the upfront account caused movement around the savings threshold, it did not consistently increase overall savings balances. It seems to have done so initially: by one month after the transfer, the median net balance was 6000 TZS higher in the upfront group than in the other two groups. (Average net balance was also somewhat higher, but not statistically significantly so.) But in July, an anticipated cash transfer from the NGO failed to materialize. This may have sparked mistrust in the promise of bonuses to be paid at the end of the study period; many participants withdrew all their money, including the upfront bonus for those who had received it. By the end of the study, the median net balance in the upfront group had turned negative, and was 6600 TZS *lower* than in the other groups. (Again, the average was also somewhat lower, but not statistically significant.) We show evidence that the positive effects of the upfront account on the saving threshold was also driven mostly by participants who had received all expected cash transfers from the NGO in the past, suggesting that trust in the financial institution is an important mediator of effects. This is consistent with prior work demonstrating the role of trust in accumulating savings (Bachas, Gertler, Higgins, & Seira, 2021).

We also show that the upfront bonus increased savings most among those who were already least financially stressed. Positive effects on meeting the savings threshold were concentrated among participants with relatively high average pre-treatment mobile wallet balances, and among those who did not

withdraw the full value of their prior cash transfer in one day.[3] We do not find evidence that treatment differed by whether participants controlled their own phone, nor by their prior level of mobile money usage.

Our results suggest that the design of financial products can encourage saving even among very poor populations. Previous literature has raised the possibility that some people are simply too poor to save (Dupas et al., 2018). Many of the women in our sample came from vulnerable populations living very near subsistence. And indeed, many simply withdrew all or most of their balance (including the bonus) as soon as possible. However, the upfront bonus account motivated a significant portion of them to keep a certain amount of money in their accounts until a given date – even when the speed with which they withdrew after that date suggests some amount of sacrifice was involved in leaving the money there. This is consistent with previous work finding that financial products can add value for new savers through channels other than average assets as measured by researchers (Prina, 2015).

Our main contribution is to the literature on designing savings products informed by insights in behavioral economics. An extensive experimental and quasi-experimental literature studies attempts to create savings products aimed at overcoming present bias through default options or commitment devices (Thaler & Benartzi, 2004; Ashraf, Karlan, & Yin, 2006; Giné, Karlan, & Zinman, 2010; Brune, Chyn, & Kerwin, 2021). These papers tend to focus on present bias in the *cost* of saving – i.e., the disproportionate weight given to forgone consumption today. But present bias may also affect the *benefit* side of saving: interest. If present bias causes the relative overweighting of today's consumption, then a savings account which moved the salience of future interest payments to the present could potentially motivate present-biased individuals to save more.

The rest of this paper continues as follows. Section 2 describes the context and the data we use. Section 3 lays out the research design and details the implementation. Section 4 presents our results. Section 5 concludes.

2 Context and Data

Saving was somewhat rare among Tanzanian households in 2017. The fraction of Tanzanians above age 15 who had a bank account or mobile money account in 2017 was 47% according to the World Bank (World

[3]Because some of our outcomes of interest rely on account balance averages, account dormancy poses a measurement risk. We assess the possible impact of account dormancy on treatment effects in section 4.5.3, finding that any effects we find are not artifacts of inflated account balances due to dormant accounts.

Bank, 2017). This shows that mobile money at the time was not unknown, but was far from universal. A nationally representative survey in 2017 illuminates the financial landscape for ordinary Tanzanians at the time (FinScope, 2017). 41% depended primarily on farming or fishing as their main income-generating activity. When faced with an unexpected expense, only 19% of Tanzanians reported that they would use savings to cope. Similarly, of Tanzanians under age 55, only 18% report that they are saving for retirement.

Our study participants were recruited from among enrollees of the USAID-funded, Jhpiego-implemented Sauti Project in rural Tanzania. Sauti works with adolescent girls and young women with the aim of buffering the population from risky behavior and reduce HIV/AIDS and other sexually transmissible infections (USAID, 2019). (All participants in our study were 18 years of age and older.) Sauti included a quarterly cash-transfer program which disbursed cash transfers of 70,000 TZS (≈$32 USD) each, starting in May of 2017. Sauti participants were recruited into the Sauti program in two batches; these batches were then maintained as distinct groups in the scheduling of cash transfers (we also stratified treatment by enrollment batch). To facilitate the cash transfers, as part of the Sauti program each participant received a basic mobile phone handset and a SIM card from one of Tanzania's major mobile network operators.

The provision of mobile phones, as well as the cash transfer schedule, made this an attractive context in which to study the structure of savings incentives.

We began recruiting participants in our savings study from among Sauti enrollees in March of 2018. At this time, the NGO had already sent three cash transfers to Sauti participants. In March of 2018 we carried out extensive piloting of the savings training meetings.[4] In April, we recruited and trained the 371 participants who form our sample here, all from the district of Shinyanga. Working with our partner NGO, we sent invitations via text message to recent enrollees in the Sauti program, inviting them to participate in a savings training program. Participants were randomized into treatment arms prior to sending the invitations, but the invitation to the savings training meeting was identical for each experimental arm. These participants were enrolled into the savings study, and received the savings training which constituted the treatment, shortly before receiving a fourth cash transfer in April of 2018. We recruited an additional 342 participants in July to augment the sample prior to the final expected Sauti cash transfer in July 2018. However, regulatory disputes prevented the NGO from making the July cash transfer, meaning our July enrollees had nothing from which to save. We therefore exclude them from analysis, focusing on the participants recruited in April and how much they saved of the NGO's April cash transfer.

[4]Initially, we had planned to incorporate a cohort of March enrollees into the study. However, due to an implementation problem the randomization protocol was not followed as intended, necessitating the exclusion of this group from the analysis.

2.1 Outcome data

To assess the effect of the treatments on savings behavior, we use administrative data from our partner Mobile Network Operator (MNO) that documents activity in the mobile money wallets of participants. Our primary outcome, as indicated in the pre-registration of our study, is participants' savings level at the end of the program, i.e., whether their mobile wallets contained at least 10% of the April cash transfer (net of the upfront bonus) on 24 September 2018.[5] We obtained consent from eligible study participants to allow for the research team to gain access to data recorded by the MNO about their mobile phone and mobile money use. The MNO provided daily balance amounts for a total of 313 days between 1 April 2017 and 30 September 2018, including 41 days during the treatment period of 15 April 2018 to 24 September 2018.[6] This allows us to observe the dates and amounts of participants' deposits and withdrawals before, during, and (briefly) after the study period. As the mobile wallet balance data is at the daily level, we define deposits and withdrawals as days with a higher or lower balance than the previous day in the data.

Figure 1 shows the daily mobile wallet balances for cash transfer recipients in our study. This graph illustrates the dynamic we found to be common: many (but not all) participants withdraw nearly their entire balance as soon as possible after receiving a transfer. This shows up on the graph as sharp balance spikes on the days in which the NGO made cash transfers.

Figure 2 shows a histogram of the participants' average daily mobile wallet balances in the period prior to treatment. The data are highly skewed, with most users carrying very small balances from day to day, and a small number of users maintaining relatively large balances. The median daily mobile balance, averaged over all days prior to treatment, was 1818 TZS — less than 1 USD. The top decile carried between 10,000 and 48,000 TZS. We show results on both mean and median mobile wallet balances.

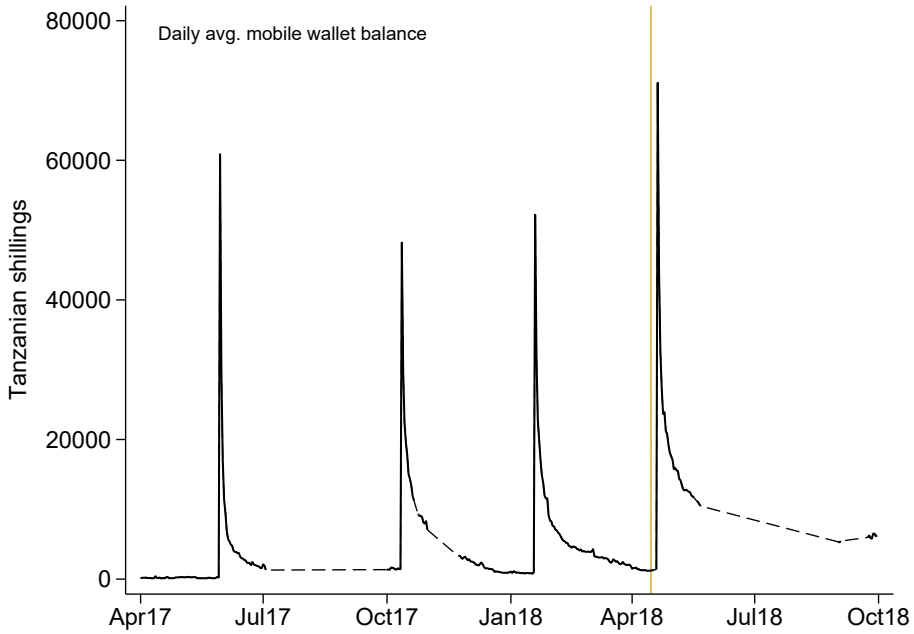
3 Experimental Design

We randomly assigned participants in the savings study to one of three experimental conditions, consisting of different incentives for saving. These incentives were explained in training meetings, held separately for each treatment group, on 15 April 2018.

[5] Pre-registration materials available at https://osf.io/z263e.

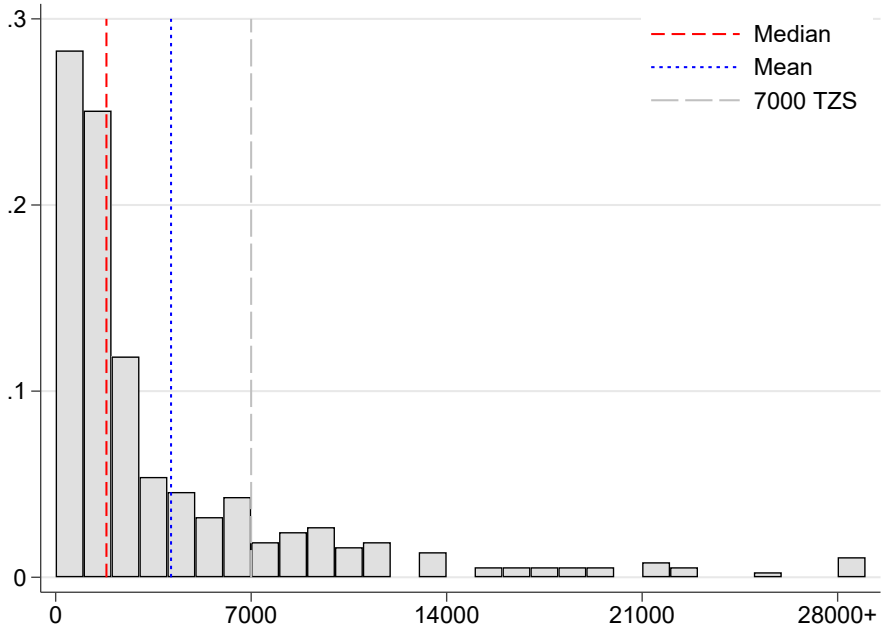
[6] See Appendix Section B.1 for more details on which dates do and don't have mobile wallet data available.

Figure 1: Average daily mobile wallet balances



Dashed lines show periods of missing data. Vertical sand-colored line shows the date in which participants were enrolled into the treatment.

Figure 2: Histogram: Pre-treatment average daily wallet balance (TZS)



Histograms of individuals' average unadjusted mobile wallet balances during the period from 1 Apr 2017 to 14 Apr 2018. Right tail collapsed above 28000 TZS.

3.1 Experimental Conditions

- **Upfront bonus savings account treatment** (n=103): Participants were told they would be provided with a front-loaded benefit of 7,000 Tanzanian Shillings (≈$3.15 USD) deposited into the participants' mobile wallets shortly after the training meeting. (The upfront bonus arrived in participants' accounts five days after the training, on 25 April 2018). They were told they could keep the upfront bonus if and only if they saved 10 percent of each quarterly cash transfer they received over the course of the study (5 months). We explained, for example, that participants who received two 70,000 Tsh cash transfers during the study would get to keep upfront bonus if on 24 September 2018 their mobile wallet balance were at least 21,000 TZS (10% or 7,000 TZS in savings from each of the two cash transfers that happened during the study, plus the 7,000 TZS from the upfront bonus). If their mobile wallet balance was below 21,000 TZS at the end of the study, they would lose the 7,000 TZS bonus (but not any other amount they had saved). As it turned out, these participants only received one cash transfer during the study period, meaning they had to have at least 14,000 TZS in their account at the end of the study to qualify to keep the bonus. Periodic text messages reminded participants of the incentive scheme.[7]

- **Traditional interest savings account treatment** (n=140): Participants were told they would receive a lump sum of 7,000 TZS in interest at the end of the study if they saved at least 10 percent of each cash transfer they received during the period. That is, if they received two cash transfers during the period, and had at least 14,000 TZS in their account on 24 September 2018, they would receive an extra 7,000 TZS as interest. Periodic text messages reminded participants of the incentive scheme.

- **Savings information only** (n=128): These participants were not eligible for any interest but also received a training, and were encouraged to save at least 10% of their cash transfers. They also received regular text messages which paralleled those of the treatments, to remind them of importance of savings.

3.2 Randomization

Participants were randomized into one of the three treatment arms upon enrollment into the study. Randomization was stratified by four variables: Sauti enrollment batch, meeting location, cost to travel to the meeting location, and pre-treatment mobile money usage. The Sauti enrollment batch refers to dummies

[7] See Appendix Figure C.5 for the poster used in training the upfront bonus condition.

for the two different "batches" in which women were originally enrolled into the Sauti program (of which our study participants were a subset). The meeting location variable takes 8 different values, referring to villages and neighborhoods where the training meetings took place. The travel cost variable refers to the four different amounts offered to the women as travel compensation for attending the training meetings, depending on how far from the location they lived. The mobile money usage variable refers to three categories of pre-treatment mobile money usage frequency, based on MNO data, during the 46 days from January 1, 2018 through February 15, 2018. These categories correspond roughly to those with no activity, those who quickly withdrew cash transfers and did not use their account otherwise, and those who showed some use beyond withdrawing their cash transfers.[8]

3.3 Randomization balance

Table 1 tests whether the main pre-treatment variables we consider are different across treatment groups. We show participants' age, the average daily mobile wallet balance in the pre-treatment period, the average number of balance changes prior to treatment, a dummy for whether participants reported their phone to be in their possession and working, and a dummy for whether a phone call from the NGO to the registered number resulted in speaking with the participant.

We do not observe more significant differences than would be expected from chance.

3.4 Implementation

Because of the regulatory disputes which prohibited the NGO from making the cash transfer intended for July of 2018, participants received only one of two scheduled transfers. The NGO did not make us aware of this missed transfer until after the end of the study period, so we were unable to send any corrective or explanatory message. Because of this unforeseen challenge to study participants, we decided after the study ended to let *all* upfront group members keep the bonus, and we provided the 7000 TZS to *all* interest group members, regardless of whether they reached savings targets. Participants did not know they would enjoy this unconditional windfall at the end of the study – nor did we – so we consider participants to have made savings decisions based on the expectation that the bonus and interest would be conditional on saving 10% of any cash transfers they received.

[8] See figures of mobile wallet patterns in Appendix B.2.

Table 1: Balance

Variable	(1) Info only Mean (SE)	(2) Interest Mean (SE)	(3) Upfront Mean (SE)	(1)-(2) Pairwise t-test Diff	(1)-(3) Pairwise t-test Diff	(2)-(3) Pairwise t-test Diff
Age	20.938 (0.156)	20.971 (0.142)	21.243 (0.176)	-0.034	-0.305	-0.271
Avg. daily MM balance, pre	4796.101 (651.323)	3449.419 (441.168)	4234.028 (541.281)	1346.683*	562.073	-784.610
Avg. prior balance $\geq$ 7000	0.211 (0.036)	0.121 (0.028)	0.194 (0.039)	0.090**	0.017	-0.073*
Avg. num. balance changes / mo, pre	1.585 (0.095)	1.702 (0.130)	1.432 (0.080)	-0.117	0.153	0.270
Received May 2017 transfer	0.867 (0.030)	0.871 (0.028)	0.767 (0.042)	-0.004	0.100**	0.104**
Received Oct 2017 transfer	0.602 (0.043)	0.636 (0.041)	0.592 (0.049)	-0.034	0.009	0.043
Received Jan 2018 transfer	0.648 (0.042)	0.721 (0.038)	0.689 (0.046)	-0.073	-0.041	0.032
Withdrew Jan'18 transfer in 1 day | rec.	0.365 (0.034)	0.404 (0.036)	0.339 (0.039)	-0.038	0.026	0.065
Received Apr 2018 transfer	0.859 (0.031)	0.879 (0.028)	0.854 (0.035)	-0.019	0.005	0.024
Reached by phone for survey	0.836 (0.033)	0.836 (0.031)	0.864 (0.034)	0.000	-0.028	-0.028
SIM in possession & working | survey	0.896 (0.026)	0.954 (0.016)	0.962 (0.017)	-0.058*	-0.066**	-0.008
Phone in possession & working | survey	0.659 (0.039)	0.748 (0.033)	0.788 (0.037)	-0.090*	-0.130**	-0.040
Number of observations	128	140	103	268	231	243

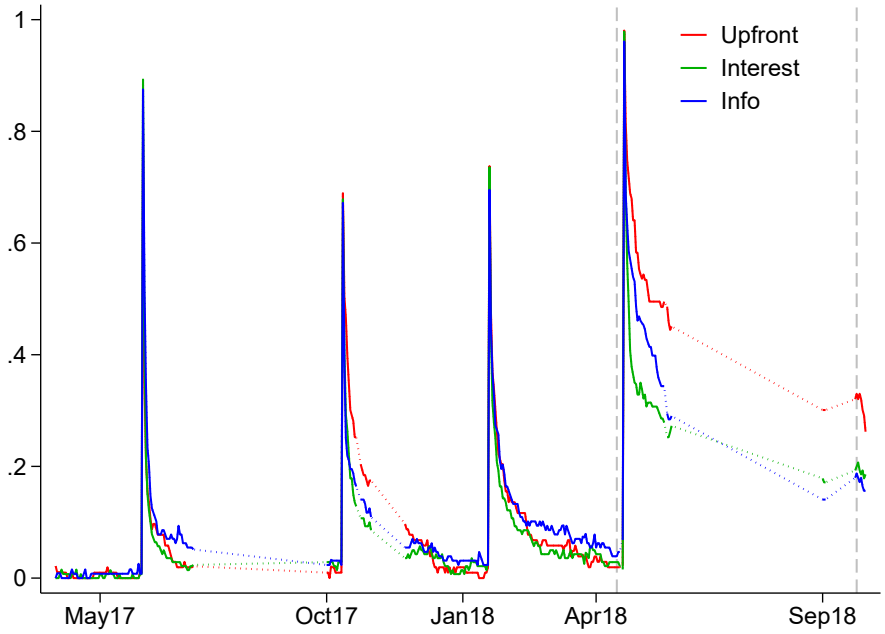
Data on withdrawal of Jan'18 transfer missing for those who didn't receive it. Data on SIM and phone possession missing for those who didn't take the survey. Missing data for these variables replaced with the global mean. Consistent with the pre-analysis plan, and with all specifications elsewhere in the paper, all t-tests in this table include fixed effects for the categorical variables we used as randomization strata (number of distinct values): Meeting place (8), batch (2), Mobile money usage category (4), category of travel cost for reaching the training (4). All missing values in balance variables are treated as the mean. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

4 Results

4.1 Meeting the savings target

Figure 3 illustrates our main finding: the fraction of participants meeting the 7000-shilling saving threshold was dramatically higher in the upfront group during the treatment period.

Figure 3: Fraction with net mobile wallet balance $\geq$ 7000 TZS, daily



Dotted lines at 15 April 2018 and 24 September 2018; the date of treatment and the end of the study.
Mobile wallet balances for upfront group are net of the upfront bonus.

All treatment arms received training and encouragement to save beginning in April 2018; this may have something to do with why the fraction of all treatment groups above the savings target rose significantly during the study period. Also notable is the sharp dip in the upfront group in the one week for which we observe mobile wallet balances after the end of the study.

Table 2 shows regression coefficients for this result. The Upfront treatment caused a higher proportion of participants to carry a net balance of 7000 TZS or above, all throughout the experimental period. This difference fell moderately in the days after the end of the program.

Figure 4 illustrates that this effect was driven by bunching above the savings threshold. This shows the histogram for gross mobile wallet balances (*not* net of the upfront bonus) at the end of the study period, 24 September 2018. The upfront group has fewer participants with a zero balance, but this is not surprising,

Table 2: Effect on keeping a balance over the savings target after . . .

	1 day	1 week	1 month	4 months	5 months (end of program)	5 mos + 1 wk
Upfront	0.132**	0.111*	0.151**	0.156***	0.139**	0.103*
	(0.059)	(0.065)	(0.063)	(0.052)	(0.056)	(0.053)
Interest	0.005	-0.183***	-0.066	0.038	0.012	0.031
	(0.054)	(0.060)	(0.058)	(0.048)	(0.051)	(0.049)
p: interest $\neq$ upfront	0.028	0.000	0.001	0.022	0.021	0.162
DV Mean	0.688	0.531	0.344	0.141	0.188	0.156

N = 371. Observation is at the individual level. Treatment happened on 15 Apr 2018. Cash transfer happened on 20 Apr. Outcomes correspond to dummies for having a net mobile wallet balance above 7000 TZS on 21 Apr, 28 Apr, 17 May, 1 Sept, 24 Sept, 30 Sept 2018. Omitted treatment category is the savings-info-only treatment arm. All regressions include fixed effects for batch, meeting place, travel cost category, and pre-treatment mobile money usage category. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

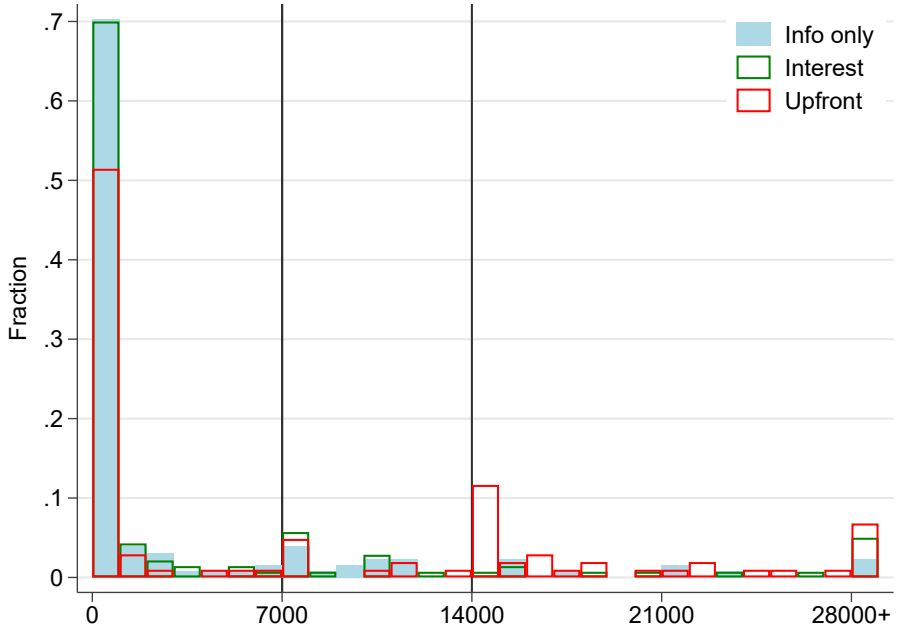
since this group also had an extra 7000 shillings provided to them up front. A small amount of bunching at 7000 shillings is visible for all groups; this was the target mentioned in the information-only treatment, and the amount participants in the traditional interest group were required to reach to qualify for the interest. There is a much larger spike at 14,000 shillings, only for the upfront group – this is the amount they were required to have in their account to qualify to keep the upfront bonus (the 7000 bonus itself plus 10% of the 70,000-shilling transfer they received).

4.2 Savings balances over time

Table 3 shows how median and average mobile wallet balances evolved over time (net of the upfront bonus). We find no significant difference in balances immediately after the transfer. After one week, the interest group's balances had fallen relative to the others'. By one month after the transfer – May 2018 – the upfront group had higher median balances than either of the other groups. Mobile wallet balance data is not available again until the beginning of September, after the NGO had already failed to make the July cash transfer. By this point – and by the end of the program at the end of September – median balances were lowest in the upfront group. This is because many participants in all groups withdrew all or nearly all of their available balance. In the case of the upfront group, this means their *net* balance went negative.

Figure 5 illustrates this graphically. The upfront group maintained a higher average for a month after the treatment and the April transfer, then fell below the other groups after the July transfer failed to materialize and many users drew down their entire accounts including the upfront bonus. Appendix

Figure 4: Distributions of mobile wallet balance, by treatment arm



Histograms of unadjusted mobile wallet balances on 24 Sept 2018 by treatment arm. Right tail collapsed above 28000 TZS.

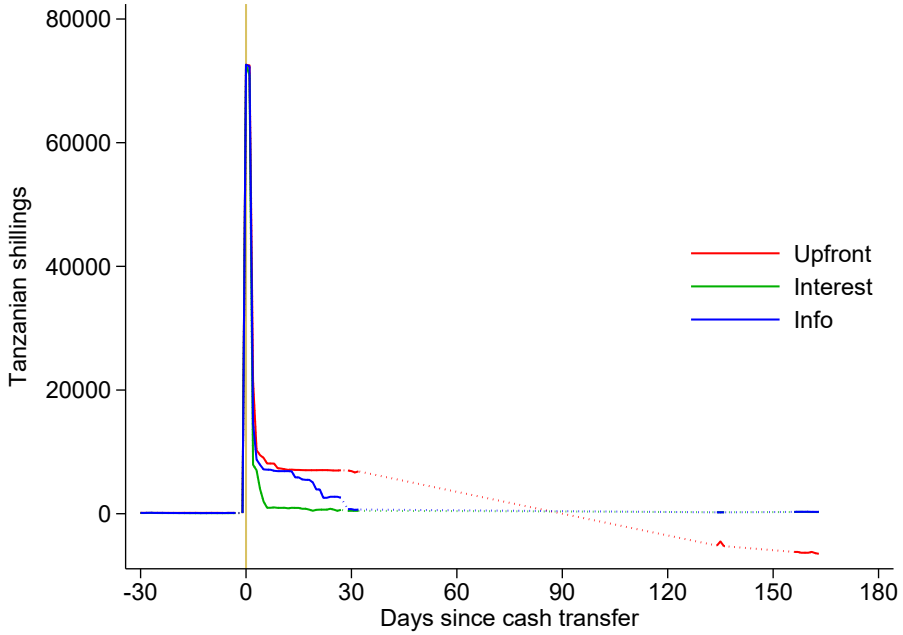
Table 3: Effect on net mobile wallet balance (TZS) after . . .

	1 day		1 week		1 month		4 months		5 months (end)	
	qreg	OLS	qreg	OLS	qreg	OLS	qreg	OLS	qreg	OLS
Upfront	-6	-5	551	4397	6028***	1711	-6073***	-2229	-6583***	-3369
	(7011)	(4918)	(1629)	(3972)	(441)	(3366)	(1601)	(1643)	(311)	(2208)
Interest	-542	-2394	-6167***	-7397**	-161	-232	-4	278	-35	34
	(6580)	(4537)	(1418)	(3664)	(230)	(3106)	(68)	(1516)	(71)	(2037)
p: interest ≠ upfront	0.938	0.621	0.000	0.003	0.000	0.557	0.000	0.121	0.000	0.117
N	371	371	371	371	371	371	371	371	371	371
DV Mean		44887		20571		9411		3899		5182
DV Median	70020		7124		879		263		326	

N = 371. Observation is at the individual level. Treatment happened on 15 Apr 2018. Cash transfer happened on 20 Apr. Outcomes correspond to mobile wallet balances on 21 Apr, 28 Apr, 17 May, 1 Sept, 24 Sept 2018. Omitted treatment category is the savings-info-only treatment arm. Odd columns use median regression with heteroskedasticity-robust standard errors from Stata package qreg2 by J.M.C. Santos Silva. Even columns use OLS. All regressions include fixed effects for batch, meeting place, travel cost category, and pre-treatment mobile money usage category. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A.1 shows the same thing for means.

Figure 5: Net daily median balance by treatment group



Median daily mobile wallet balances by treatment group, net of the 7000-shilling upfront bonus. Cash transfer occurred on 20 April 2018; upfront bonus was sent on 25 April.

4.3 Deposits and withdrawals

How did participants in the upfront group reach the savings target? Did they more successfully avoid making withdrawals, supplement the cash transfers with deposits of their own, or both? We test this by looking at changes to daily mobile wallet balances. We refer to any day-to-day reduction as a "withdrawal" and any day-to-day increase as a "deposit" (we use these terms loosely; these balances changes could also be due to purchases or transfers from other mobile money users, but our data do not permit us to reliably distinguish these actions). These measures of deposits and withdrawals exclude the days on which the cash transfer or the upfront bonus were disbursed, so any effect can not be mechanical.

Table 4 shows that the difference is driven not by fewer withdrawals, but by more deposits.

During the period of the study – that is, between the date of treatment and the target savings date – participants in the upfront group were more likely to make at least one deposit (from an already high base of 89%), and made 33% more deposits in this period (.62 on a base of 1.9). They did not, however, deposit a statistically distinguishably larger amount of money (consistent with the null results on overall balance). Their withdrawal behavior during this period also looked like that of the other groups.

Table 4: Effect on balance changes

	Any deposit	Num. deposits	Amt. deposited	Any withdrawal	Num. withdrawals	Amt. withdrawn
			Panel A: Study period			
Upfront	0.101***	0.620***	1515.639	0.014	0.356	-989.474
	(0.032)	(0.233)	(3573.795)	(0.015)	(0.390)	(3776.109)
Interest	0.045	0.188	181.115	0.016	0.232	2002.687
	(0.030)	(0.215)	(3297.237)	(0.014)	(0.360)	(3483.895)
p: interest $\neq$ upfront	0.081	0.060	0.704	0.924	0.746	0.420
DV Mean	0.891	1.867	10479.273	0.977	3.578	72387.742
			Panel B: Post-study period			
Upfront	0.064	0.062	876.179	0.174***	0.166*	1750.845*
	(0.039)	(0.051)	(1144.005)	(0.053)	(0.088)	(948.882)
Interest	0.038	0.029	273.487	0.019	-0.057	279.087
	(0.036)	(0.047)	(1055.476)	(0.049)	(0.081)	(875.453)
p: interest $\neq$ upfront	0.501	0.512	0.592	0.003	0.010	0.115
DV Mean	0.063	0.086	1455.625	0.156	0.273	1173.297

N = 371. Observation is at the individual level. Treatment date: 15 Apr 2018. Cash transfer date: 20 Apr. Study period: 15 Apr-24 Sept (163 days; mobile wallet data available for 41 of these days). Post-study period: 25-30 Sept (6 days; mobile wallet data available for all days). Outcomes do not include the day of the cash transfer, nor the day of the bonus payment for upfront group. Omitted treatment category is the Info Only group. All regressions include fixed effects for batch, meeting place, travel cost category, and pre-treatment mobile money usage category. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In the period *after* the study – beyond the target savings date – the upfront group were twice as likely to withdraw, and withdrew more money (though the estimate is a bit imprecise). This suggests that many participants were acutely aware of the details of the program and were careful to comply with the requirements in order to keep the bonus. The fact that withdrawals happened immediately after the program end date is indicative of participants' attention to the program's details and the high value to these participants of getting their money out of the account.

4.4 Effects on speed of cash transfer withdrawal

The raw mobile balance data make clear that many participants emptied their accounts as quickly as possible. Table 5 tests whether either treatment affected the speed of withdrawals. We define a complete withdrawal around the time of a transfer by measuring how long it takes for the account to fall below the balance it showed the day before the transfer – therefore this measure is only defined for individuals who we can verify received a transfer.

Table 5: Effect on how quickly mobile account returned to pre-cash-transfer balance

	19 Jan 2018 transfer (pre-treatment)				20 Apr 2018 transfer (post-treatment)			
		Withdrawn after . . .				Withdrawn after . . .		
	Received	1 day	1 week	2 weeks	Received	1 day	1 week	2 weeks
Upfront	0.045	-0.041	-0.003	0.001	-0.002	-0.164***	-0.186***	-0.127*
	(0.060)	(0.078)	(0.070)	(0.064)	(0.045)	(0.058)	(0.069)	(0.071)
Interest	0.070	0.062	0.085	0.055	0.020	-0.036	0.160**	0.086
	(0.055)	(0.071)	(0.064)	(0.059)	(0.042)	(0.053)	(0.063)	(0.065)
p: interest $\neq$ upfront	0.680	0.169	0.194	0.376	0.611	0.025	0.000	0.002
N	371	255	255	255	371	321	321	321
Ctrl mean	0.648	0.361	0.699	0.783	0.859	0.273	0.418	0.500

Observation is at the individual level. Omitted treatment category is the info-only treatment arm. All regressions include fixed effects for batch, meeting place, travel cost category, and pre-treatment mobile money usage category. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The four columns on the left refer to the last cash transfer which happened prior to the treatment, in January 2018. The means on the last row of the table show how common quick withdrawal is: 36% of January transfer recipients had withdrawn the full value of the transfer after one day. After two weeks, the figure was 78%. Treatment had no effect on the likelihood of receiving the January transfer, nor on the likelihood of withdrawing it all within one day, within one week, or within two weeks.

The four columns on the right refer to the cash transfer which happened shortly after treatment, in April 2018. Neither treatment affected the likelihood participants received the April transfer. But the Upfront treatment dramatically reduced the likelihood of withdrawing the full value of the transfer, beginning one day afterward and persisting for at least two weeks. The Upfront group was 60% less likely than the Info group to withdraw the full value after one day. It also seems that the Interest group was considerably more likely to withdraw the full value, though only at one week.

4.5 Mechanisms

What drives the effects we find? We consider a number of potential mechanisms: trust in the financial institution, opportunity cost of saving, phone access, and familiarity with mobile money.

4.5.1 Trust in financial institution

The payment of interest – and the potential clawback of bonuses – are promises about future actions, which people may or may not believe. As previously mentioned, the partner NGO was prevented from sending a promised cash transfer to all participants in July of 2018, and this coincided with a drop in average and median mobile wallet balances (though we are unable to speak to the causality of this coincidence). However, not all previous transfers were received by all participants either. Because we know the dates of the transfers, we can impute which participants did or did not receive the expected transfers by checking whether their balance increased by the expected amount on that day. The fraction of the sample which had received 0, 1, 2, and all 3 of the prior transfers was 7, 16, 32, and 45 respectively.

Participants who received more of the promised transfers in the past may have been more likely to believe that the promises of interest and/or clawback would also be honored, and therefore have been more likely to strive to meet the savings threshold. We test this by interacting treatment variables with dummies for whether the participant had received one, two, or all three cash transfers prior to the treatment.

Table 6 suggests that more successful transfers in the past predicted a higher treatment effect of the Upfront account on meeting the savings threshold. The coefficient on treatment in the absence of any previous successful transfer is negative, but the interaction term with one and two transfers received are positive – though none of these are significant. The interaction term on "3 transfers received" – which describes nearly half the sample – is significant and huge. This suggests that trust in the financial institution plays an important role in people's decisions whether to alter their saving and spending patterns in response to changes to saving incentives.

4.5.2 Opportunity cost of saving

For participants who are very poor, saving an extra 7000 TZS could imply giving up multiple meals. The prevalence of immediate withdrawals in all treatment groups, despite the high effective interest rate, suggests that many participants had pressing needs for the money. If some participants are simply too poor to save, we would expect the positive effects of the treatment to be driven by participants with a less immediate need for cash. Conversely, if the lack of saving is driven by reduced mental bandwidth due to poverty, it's possible the Upfront bonus account could create a larger shock to the salience of interest for poorer participants.

Table 6: By prior transfers received: trust

	Ending balance $\geq$ 7000 TZS	Ending balance (mean)	Withdraw Apr transfer in 1 day	Num. deposits
Upfront	-0.203	-2670.662	-0.266	0.636
	(0.185)	(7378.674)	(0.202)	(0.780)
Interest	-0.277	-4159.194	0.226	0.334
	(0.219)	(8742.426)	(0.235)	(0.925)
1 transfer received	-0.241	-4622.358	0.171	-0.463
	(0.167)	(6648.771)	(0.178)	(0.703)
2 transfers received	-0.060	440.045	-0.151	0.186
	(0.143)	(5697.055)	(0.152)	(0.602)
3 transfers received	-0.153	878.720	-0.233	0.206
	(0.142)	(5658.182)	(0.149)	(0.598)
Upfront $\times$ 1 transfer received	0.375	-158.868	-0.082	0.416
	(0.234)	(9313.968)	(0.250)	(0.985)
Upfront $\times$ 2 transfers received	0.313	-3206.398	0.160	-0.264
	(0.213)	(8469.839)	(0.227)	(0.896)
Upfront $\times$ 3 transfers received	0.455**	1145.233	0.119	0.042
	(0.203)	(8096.892)	(0.217)	(0.856)
Interest $\times$ 1 transfer received	0.349	15153.882	-0.277	1.126
	(0.259)	(10308.215)	(0.275)	(1.090)
Interest $\times$ 2 transfers received	0.258	1100.806	-0.410	-0.345
	(0.237)	(9456.225)	(0.253)	(1.000)
Interest $\times$ 3 transfers received	0.349	3138.590	-0.210	-0.434
	(0.232)	(9259.724)	(0.247)	(0.979)
DV Mean	0.188	5182.469	0.273	1.867
N	371	371	321	371

Observation is at the individual level. Omitted treatment category is the savings-info-only treatment arm. Ending balances are net of the upfront bonus. Number of prior transfers received means how many of the three previous cash transfers showed up in the individual's mobile wallet (May 2017, Oct 2017, Jan 2018). The percentage of the sample which had received 0, 1, 2, and all 3 of the prior transfers was 7, 16, 32, and 45 respectively. All regressions include fixed effects for batch, meeting place, travel cost category, and pre-treatment mobile money usage category. p-value is for the test of equality of the interactions of interest.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We present two complementary (though crude) measures of individual-level poverty. One is the speed with which participants withdrew the last transfer before the treatment: we assume that quicker withdrawal implies a more urgent need for the cash, meaning the opportunity cost of saving would be high.[9] Our other measure is related: average daily mobile money balance prior to treatment. Our two measures of opportunity cost of saving complement each other, but not perfectly. The correlation between the

[9]Surprisingly, the pairwise correlations across transfer dates between propensity to immediately withdraw are quite low, as seen in A.1.

dummies for "Immediate withdrawal of January transfer" and "Low average mobile money balance" is 0.56.

Table 7 interacts treatment dummies with a dummy for whether the participant withdrew all of her January 2018 transfer in one day. Of the 255 participants who received a transfer in January 2018, 37% withdrew it all by the next day. These participants were no more likely than the Info group to meet the savings target by the end of the study – the entire treatment effect was concentrated among participants who had not withdrawn immediately in January. (Curiously, this heterogeneity does not appear for other outcomes, including the speed of withdrawal of the April transfer.) This suggests that participants whose need for immediate cash was too acute were unwilling to sacrifice today's consumption for a possible future benefit.

Table 7: By prior speed of withdrawal

	Ending balance $\geq$ 7000 TZS	Ending balance (mean)	Withdraw Apr transfer in 1 day	Num. deposits
Upfront	0.322***	-366.714	-0.111	0.600**
	(0.085)	(3663.229)	(0.078)	(0.297)
Interest	0.100	425.747	-0.094	-0.253
	(0.080)	(3475.824)	(0.076)	(0.282)
Quick withdraw, pre	0.061	-91.567	0.033	-0.327
	(0.098)	(4251.551)	(0.092)	(0.345)
Upfront $\times$ Quick withdraw, pre	-0.386***	-10057.474	-0.031	0.043
	(0.145)	(6281.295)	(0.136)	(0.509)
Interest $\times$ Quick withdraw, pre	-0.125	-3628.853	0.164	0.637
	(0.130)	(5624.349)	(0.122)	(0.456)
DV Mean	0.181	6387.012	0.213	1.880
N	255	255	227	255

Observation is at the individual level. Omitted treatment category is the savings-info-only treatment arm. Ending balances are net of the upfront bonus. Quick withdraw indicator takes a value of one if the individual's account balance fell to or below its level of its pre-transfer balance one day after the Jan 2018 cash transfer (defined for participants who received the January 2018 transfer). Fraction of the sample with 'Quick withdraw, pre' == 1: 0.373. All regressions include fixed effects for batch, meeting place, travel cost category, and pre-treatment mobile money usage category. p-value is for the test of equality of the interactions of interest.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8 interacts treatment with a dummy for whether the participant had an average pre-treatment daily balance below the median. (The median value for participants' average daily mobile money balance in the pre-treatment period was 1819 TZS.) Similarly to the previous table, we see that the positive effects of the upfront account are concentrated solely in the group with a relatively high pre-treatment average

mobile wallet balance.

Table 8: By prior mobile money balance

	Ending balance $\geq$ 7000 TZS	Ending balance (mean)	Withdraw Apr transfer in 1 day	Num. deposits
Upfront	0.267***	-1074.409	-0.205***	0.580*
	(0.075)	(3009.329)	(0.076)	(0.319)
Interest	0.029	548.951	-0.117	-0.106
	(0.073)	(2940.097)	(0.077)	(0.311)
Low bal., pre	0.008	-1474.430	0.073	-0.048
	(0.074)	(2962.862)	(0.077)	(0.314)
Upfront $\times$ Low bal., pre	-0.288***	-5304.115	0.116	0.089
	(0.110)	(4415.343)	(0.116)	(0.467)
Interest $\times$ Low bal., pre	-0.029	-703.104	0.126	0.532
	(0.102)	(4078.117)	(0.106)	(0.432)
DV Mean	0.188	5182.469	0.273	1.867
N	371	371	321	371

Observation is at the individual level. Omitted treatment category is the savings-info-only treatment arm. Ending balances are net of the upfront bonus. Low prior balance indicator for individuals with below-median average daily mobile wallet balance prior to the treatment. Fraction of the sample with 'Low bal., pre' == 1: 0.501. All regressions include fixed effects for batch, meeting place, travel cost category, and pre-treatment mobile money usage category. p-value is for the test of equality of the interactions of interest.* p $<$ 0.10, ** p $<$ 0.05, *** p $<$ 0.01

This suggests that for people near subsistence, saving may simply not be rational, and that, at least for the savings account innovation tested here, the incentive design fails to deter consumption today. In the absence of simply increasing incomes, efforts to motivate saving are probably more likely to be useful for people who are far enough from subsistence to think about saving.

4.5.3 Phone access

Because the savings in question here is in an account mediated by a mobile phone, access to a phone may play an important role. Prior research has found that phone churn is a serious challenge to mobile phone related interventions (Roessler et al., 2018). A worry in our context is that the valuable asset of the phone might be stolen, or commandeered by other family members, and thus prevent participants from using their accounts. Another worry is that not having access to one's phone may lead in practice to account dormancy. Because our primary outcome is closely related to account activity, this dormancy has important ramifications for our analysis. Cash transfers were sent to program participants regardless of

their phone status. Dormant accounts accumulate unused funds, which leads to higher account balances, compared to those who had account access with continued possession of a working phone. If the effects we measure were driven by account dormancy, we would expect to see them concentrated among participants with phone access.

Our partner NGO conducted a survey on the phone ownership status of participants in Nov-Dec 2017, a few months before our study. That survey found that, of those they were able to reach for the survey, 80.8% of the NGO's overall sample was in working possession of their program phone. Figure A.2 shows the full responses to the phone survey among those reached, in both the NGO's larger sample and the smaller sample which were originally recruited into our study.

To test for whether effects are driven by those who report having access to their phones, we interact treatment with a dummy for phone ownership (conditional on being reached for the survey). Table 9 shows the results. Across most of our main specifications and outcome measurements, neither the upfront nor the interest treatment interact with phone ownership in a statistically significant way (though puzzlingly the interaction term is negative for the number of deposits). This suggests to us that any treatment effects we measure are not merely artefacts of account dormancy. Table A.2 repeats this exercise, interacting treatment with a dummy for whether the participant was reached for the phone survey, finding broadly similar results.

4.5.4 Familiarity with mobile money

Because these accounts are implemented through mobile money, participants' comfort with the mobile money system might mediate their savings behavior. The NGO provided phones and SIM cards to all participants at the start of the cash transfer program, but most participants were new to mobile technology (USAID, 2019).

We test whether effects are driven by participants who exhibited a relatively high usage of mobile money prior to treatment. We measure usage of mobile money by the number of balance changes in the pre-treatment period, and create a dummy for whether a participant is above the median.

Table 10 shows the results. We do not find strong evidence that the main effects we report in the paper were driven by people with high ex-ante mobile money usage (although it appears that the interest treatment caused higher average ending balances among high pre-treatment mobile money users).

Table 9: By possession of phone

	Ending balance $\geq$ 7000 TZS	Ending balance (mean)	Withdraw Apr transfer in 1 day	Num. deposits
Upfront	0.053	-4128.804	-0.254**	1.129**
	(0.122)	(5054.298)	(0.125)	(0.479)
Interest	0.075	-166.525	-0.045	0.400
	(0.105)	(4339.072)	(0.106)	(0.411)
Phone in possession and working	0.065	1684.027	-0.111	0.852**
	(0.086)	(3554.939)	(0.087)	(0.337)
Upfront $\times$ Phone in possession and working	0.061	-615.881	0.170	-0.921*
	(0.142)	(5878.339)	(0.146)	(0.557)
Interest $\times$ Phone in possession and working	-0.088	-756.700	0.095	-0.569
	(0.125)	(5179.270)	(0.127)	(0.491)
DV Mean	0.196	5916.009	0.242	1.963
N	313	313	269	313

Observation is at the individual level. Omitted treatment category is the savings-info-only treatment arm. Ending balances are net of the upfront bonus. Phone possession variable defined only for those who were able to be surveyed. Fraction of the sample with 'Phone in possession and working' == 1: 0.728. All regressions include fixed effects for batch, meeting place, travel cost category, and pre-treatment mobile money usage category. p-value is for the test of equality of the interactions of interest.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: By prior mobile money usage

	Ending balance $\geq$ 7000 TZS	Ending balance (mean)	Withdraw Apr transfer in 1 day	Num. deposits
Upfront	0.140*	-5851.857*	-0.135	0.604*
	(0.080)	(3167.098)	(0.085)	(0.336)
Interest	-0.009	-4287.544	-0.069	0.164
	(0.075)	(2945.243)	(0.076)	(0.312)
High MM use	-0.046	-5773.049*	-0.040	0.148
	(0.080)	(3141.121)	(0.083)	(0.333)
Upfront $\times$ High MM use	-0.003	4691.006	-0.055	0.032
	(0.112)	(4422.212)	(0.117)	(0.469)
Interest $\times$ High MM use	0.039	8189.026**	0.066	0.055
	(0.103)	(4080.306)	(0.107)	(0.433)
DV Mean	0.188	5182.469	0.273	1.867
N	371	371	321	371

Observation is at the individual level. Omitted treatment category is the savings-info-only treatment arm. Ending balances are net of the upfront bonus. High mobile money use indicator for individuals with above-median number of mobile money balance changes prior to the treatment. Fraction of the sample with 'High MM use' == 1: 0.518. All regressions include fixed effects for batch, meeting place, travel cost category, and pre-treatment mobile money usage category. p-value is for the test of equality of the interactions of interest.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5 Conclusion

This paper tests a novel savings account, designed to incentivize savings by providing an upfront benefit, conditional on meeting a savings target by a certain date – rather than providing the benefit only upon the attainment of the target.

Development economists have long been puzzled by the nonexistence of savings coupled with the existence of borrowing. There are many explanations for this phenomenon: low financial literacy, pressure from family and friends, mistrust of financial institutions, lack of adequate savings technology. Mobile money accounts appear to have the potential to solve at least some of these problems, perhaps increasing the likelihood that the unbanked can move into the formal financial sector and save money to use on unexpected expenses, rather than relying on costly borrowing.

At the same time, a large literature has shown that behavioral economists' insights on predictable human deviations from the predictions of expected utility models can be harnessed to nudge people into saving more money – in many cases, this is an outcome that the savers are happy with. However, most of this literature focuses on subjects in the developed world, where financial systems are ubiquitous and most people are far from subsistence.

We chose a poor and largely unbanked sample because we hoped to find evidence of a mechanism that could allow financial institutions to offer interest-bearing accounts to those previously ignored by the formal financial system. Finding ways to allow vulnerable populations to meet their expenditures without costly borrowing has the potential to increase welfare.

We found that the upfront bonus account dramatically increased the fraction of participants who met the savings threshold by the target date – although it did not increase average net balances, and in fact decreased median net balances (when subtracting the upfront bonus). Large numbers of participants simply withdrew all money possible from their mobile accounts, although the upfront account caused participants to delay withdrawals. The upfront account also motivated more frequent deposits. The more traditional interest account, with rates high above market rates, did not increase any measure of saving.

This finding shows that the design of financial products can affect their use, even by the poor in a context where the formal financial sector consists of mobile banking. It is also congruent with previous work that finds that some people may be too poor to save (Dupas et al., 2018) – we find that all positive effects on saving are concentrated among those who are least financially constrained. We also show that trust in the financial institution is vital, an important lesson as many unbanked people in the poor world

move warily into the formal financial system for the first time.

References

Ashraf, N., Karlan, D., & Yin, W. (2006). Tying odysseus to the mast: Evidence from a commitment savings product in the philippines. *The Quarterly Journal of Economics*, *121*(2), 635–672.

Bachas, P., Gertler, P., Higgins, S., & Seira, E. (2021). How Debit Cards Enable the Poor to Save More. *Journal of Finance*, *76*(4), 1913–1957. doi: 10.1111/jofi.13021

Baird, S., McIntosh, C., & Özler, B. (2019). When the money runs out: Do cash transfers have sustained effects on human capital accumulation? *Journal of Development Economics*, *140*(March), 169–185. doi: 10.1016/j.jdeveco.2019.04.004

Banerjee, A. V., & Duflo, E. (2007). The economic lives of the poor. *Journal of economic perspectives*, *21*(1), 141–168.

Barrera-Osorio, F., Linden, L. L., & Saavedra, J. E. (2019). Medium- and long-term educational consequences of alternative conditional cash transfer designs: Experimental evidence from Colombia. *American Economic Journal: Applied Economics*, *11*(3), 54–91. doi: 10.1257/app.20170008

Brune, L., Chyn, E., & Kerwin, J. (2021). Pay Me Later: Savings Constraints and the Demand for Deferred Payments. *American Economic Review*, *111*(7), 2179–2212. doi: 10.2139/ssrn.3822087

Cabral, M., & Hoxby, C. (2012). *The hated property tax: salience, tax rates, and tax revolts* (Tech. Rep.). National Bureau of Economic Research.

Choi, J. J., Laibson, D., Madrian, B. C., & Metrick, A. (2004). For better or for worse: Default effects and 401 (k) savings behavior. In *Perspectives on the economics of aging* (pp. 81–126). University of Chicago Press.

Collins, D., Morduch, J., Rutherford, S., & Ruthven, O. (2009). *Portfolios of the poor*. Princeton University Press.

Dupas, P., Karlan, D., Robinson, J., & Ubfal, D. (2018). Banking the unbanked? Evidence from three countries. *American Economic Journal: Applied Economics*, *10*(2), 257–297. doi: 10.1257/app.20160597

Finkelstein, A. (2009). E-ztax: Tax salience and tax rates. *The Quarterly Journal of Economics*, *124*(3), 969–1010.

FinScope. (2017). FinScope Tanzania – Insights that Drive Innovation. *Household survey*.

Gertler, P. J., Martinez, S. W., & Rubio-Codina, M. (2012). Investing cash transfers to raise long-term living standards. *American Economic Journal: Applied Economics*, *4*(1), 164–192. doi: 10.1257/app.4.1.164

Giné, X., Karlan, D., & Zinman, J. (2010). Put your money where your butt is: a commitment contract for smoking cessation. *American Economic Journal: Applied Economics*, *2*(4), 213–35.

Handa, S., Natali, L., Seidenfeld, D., Tembo, G., & Davis, B. (2018). Can unconditional cash transfers raise long-term living standards? Evidence from Zambia. *Journal of Development Economics*, *133*(January), 42–65. Retrieved from http://dx.doi.org/10.1016/j.jdeveco.2018.01.008 doi: 10.1016/j.jdeveco.2018.01.008

Hossain, T., & List, J. A. (2012). The behavioralist visits the factory: Increasing productivity using simple framing manipulations. *Management Science*, *58*(12), 2151–2167. doi: 10.1287/mnsc.1120.1544

Kahneman, D., Knetsch, J. L., & Thaler, R. H. (1990). Experimental tests of the endowment effect and the coase theorem. *Journal of political Economy*, *98*(6), 1325–1348.

Karlan, D., Ratan, A. L., & Zinman, J. (2014). Savings by and for the Poor: A research review and agenda. *Review of Income and Wealth*, *60*(1), 36–78. doi: 10.1111/roiw.12101

Levitt, S. D., List, J. A., Neckermann, S., & Sadoff, S. (2016). The behavioralist goes to school: Leveraging behavioral economics to improve educational performance. *American Economic Journal: Economic Policy*, *8*(4), 183–219.

Madrian, B. C., & Shea, D. F. (2001). The power of suggestion: Inertia in 401 (k) participation and savings behavior. *The Quarterly journal of economics*, *116*(4), 1149–1187.

Mani, A., Mullainathan, S., Shafir, E., & Zhao, J. (2013). Poverty impedes cognitive function. *science*, *341*(6149), 976–980.

National Bureau of Statistics. (2018). *Press release: National consumer price index (ncpi) for april, 2018.* 18 Kivukoni Road, P.O. Box 796, Dar es Salaam, Tanzania. Retrieved from https://web.archive.org/web/20191114003645/https://www.nbs.go.tz/nbs/takwimu/cpi/CPI_April_2018.pdf (Ref: NBS/S.40/21/536)

Pierce, L., Rees-Jones, A., & Blank, C. (2022). The Negative Consequences of Loss-Framed Performance Incentives. *NBER Working Paper*.

Prina, S. (2015). Banking the poor via savings accounts: Evidence from a field experiment. *Journal of Development Economics*, *115*, 16–31. Retrieved from http://dx.doi.org/10.1016/j.jdeveco.2015.01.004 doi: 10.1016/j.jdeveco.2015.01.004

Ravallion, M., & Chen, S. (2005). Hidden impact? Household saving in response to a poor-area develop-

ment project. *Journal of Public Economics*, *89*(11-12), 2183–2204. doi: 10.1016/j.jpubeco.2004.12.003

Roessler, P., Myamba, F., Carroll, P., Jahari, C., Kilama, B., & Nielson, D. L. (2018). Mobile-phone ownership increases poor women's household consumption: A field experiment in tanzania. In *Proceedings of the evidence in governance and politics meeting, nairobi, kenya* (pp. 1–15).

Thaler, R. H., & Benartzi, S. (2004). Save more tomorrow™: Using behavioral economics to increase employee saving. *Journal of political Economy*, *112*(S1), S164–S187.

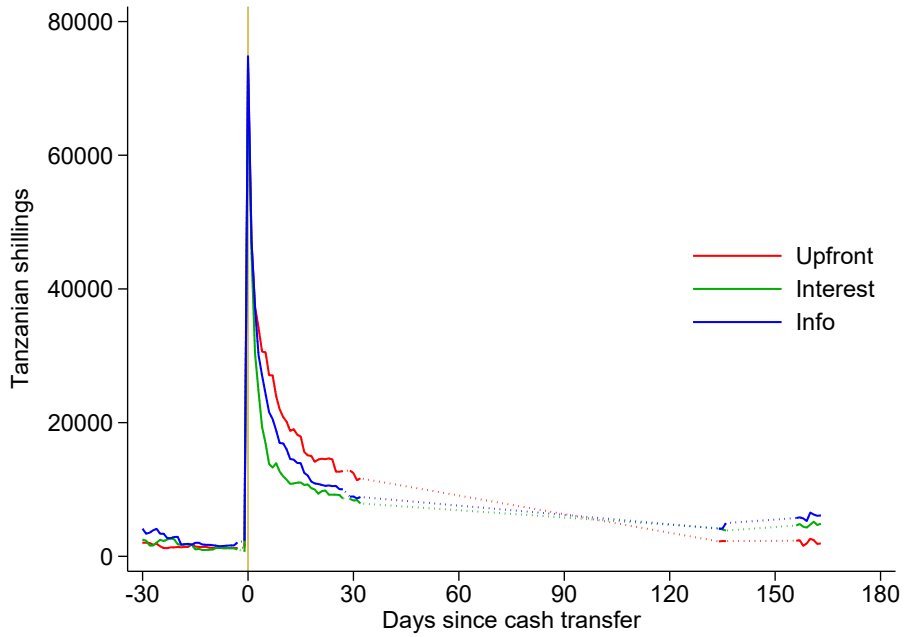
USAID. (2019, March). *USAID Tanzania: Sauti Project* (Tech. Rep.). Retrieved from https://www.usaid.gov/sites/default/files/documents/1860/03.27.2019_Sauti.pdf

World Bank. (2017). *Account ownership at a financial institution or with a mobile-money-service provider (% of population ages 15+) - Tanzania* . (data retrieved from World Development Indicators, https://data.worldbank.org/indicator/FX.OWN.TOTL.ZS?locations=TZ)

Appendix

A Extra tables and figures

Figure A.1: Net daily average balance by treatment group



Notes.

Table A.1: Complete 1-day withdrawal of cash transfer, pairwise correlations

	May '17	Oct '17	Jan '18	Apr '18
May '17	1.0000			
Oct '17	0.0119	1.0000		
Jan '18	0.1569	0.2567	1.0000	
Apr '18	0.1157	0.1229	0.1258	1.0000

Figure A.3 plots coefficients and 95% confidence intervals from quantile regressions from the 10th to the 90th percentile of the mobile wallet balances (net of bonus) at the end of the study period. It shows that a large subset of upfront participants withdrew their entire balance including the upfront bonus, leaving them with a "negative" balance relative to the placebo group who could go no lower than 0. But effects on the right side of the distribution, while noisier, suggest that the upfront treatment increased balances in this part of the distribution. The effect of the interest account relative to the placebo is zero

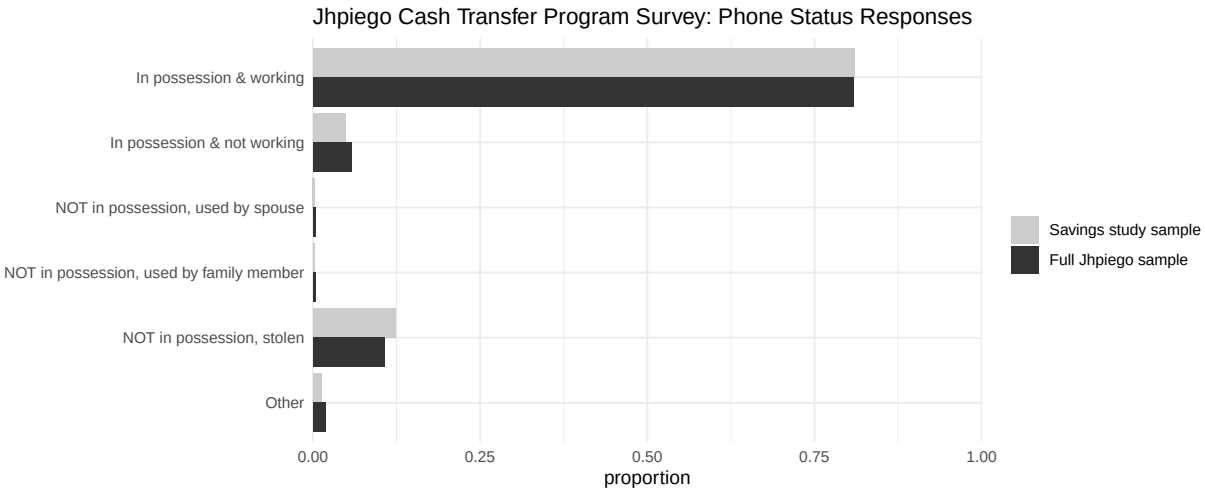


Figure A.2: Jhpiego program mobile phone churn

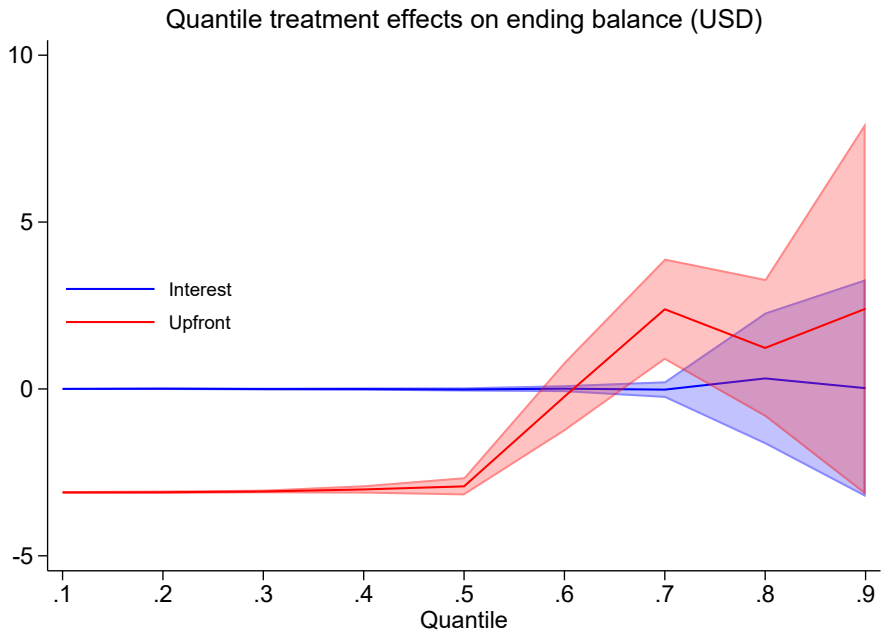
Table A.2: By whether they were reached for the survey

	Ending balance $\geq$ 7000 TZS	Ending balance (mean)	Withdraw Apr transfer in 1 day	Num. deposits
Upfront	0.301**	1984.393	-0.272*	1.009*
	(0.145)	(5776.793)	(0.144)	(0.610)
Interest	-0.008	3054.535	-0.267**	0.820
	(0.127)	(5041.045)	(0.132)	(0.532)
Responded to phone survey	0.071	4878.544	-0.162	0.473
	(0.101)	(4006.521)	(0.103)	(0.423)
Upfront $\times$ Responded to phone survey	-0.190	-6366.334	0.132	-0.467
	(0.157)	(6261.791)	(0.157)	(0.661)
Interest $\times$ Responded to phone survey	0.024	-3609.425	0.277*	-0.755
	(0.139)	(5512.743)	(0.144)	(0.582)
DV Mean	0.188	5182.469	0.273	1.867
N	371	371	321	371

Observation is at the individual level. Omitted treatment category is the savings-info-only treatment arm. Ending balances are net of the upfront bonus. Fraction of the sample with 'Responded to phone survey' == 1: 0.844. All regressions include fixed effects for batch, meeting place, travel cost category, and pre-treatment mobile money usage category. p-value is for the test of equality of the interactions of interest.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

at all percentiles. The significant difference at the 70th percentile corresponds to the mass in the upfront distribution at the savings target.

Figure A.3: Quantile regression results, 10th-90th percentiles



A.1 Effects on mobile money usage

The pattern of participants withdrawing transfers immediately is also attested when we examine the effect of treatment on mobile money usage, as measured by the number of mobile wallet balance changes in the first month following the treatment. We find that participants in the upfront condition were much more likely to use their mobile money wallets following the treatment, as seen in Table A.3. This is consistent with these participants withdrawing the extra upfront bonus as well as the cash transfer itself.

B Mobile Wallet Balance Information

B.1 Missing mobile wallet balance dates

Dates for which no mobile wallet balance data could be provided, as provided by MNO partner:

Table A.3: Effect on mobile money usage: number of balance changes in the month post-treatment

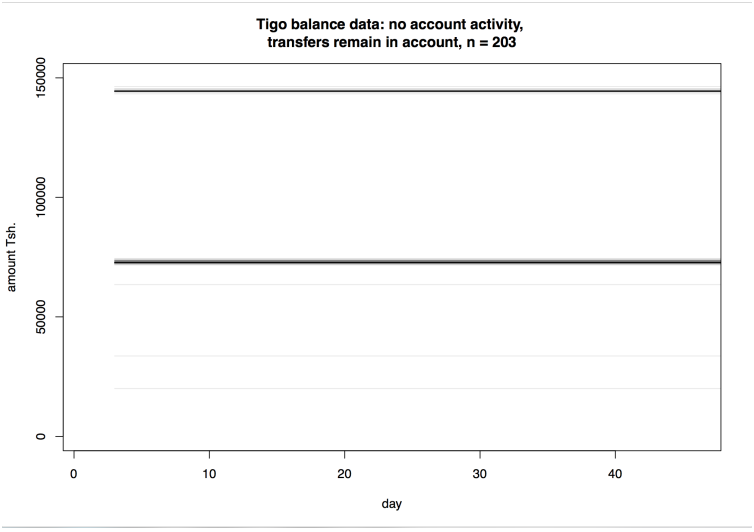
	Median		Mean	
Upfront	1.195***	0.961***	0.847**	0.957***
	(0.281)	(0.261)	(0.348)	(0.308)
Interest	-0.000	-0.060	0.270	0.114
	(0.223)	(0.220)	(0.321)	(0.286)
Pre-treat avg. daily bal. USD		-0.073**		-0.086*
		(0.034)		(0.045)
Num. bal. chgs per mo., pre		1.345***		1.185***
		(0.228)		(0.117)
p: interest $\neq$ upfront	0.000	0.000	0.092	0.006
Ctrls		✓		✓
N	371	371	371	371
Ctrl mean	4.419	4.419	4.419	4.419
Ctrl median	3.577	3.577	3.577	3.577

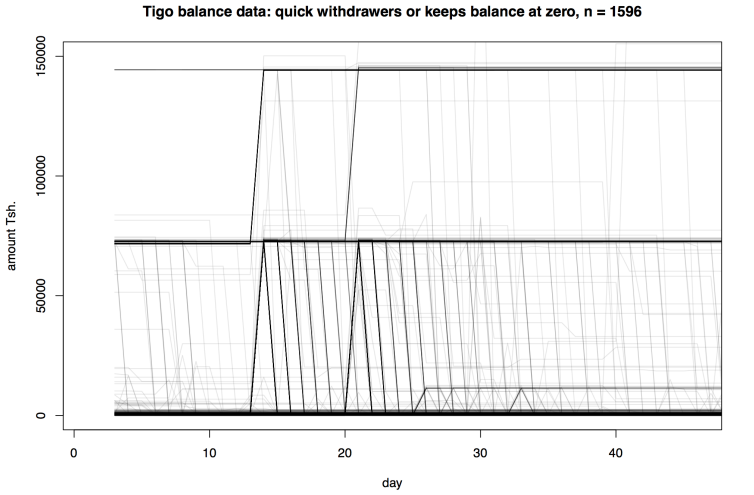
Observation is at the individual level. Omitted treatment category is the savings-info-only treatment arm. First column uses median regression with heteroskedasticity-robust standard errors from Stata package qreg2 by J.M.C. Santos Silva. All regressions include fixed effects for batch, meeting place, travel cost category, and pre-treatment mobile money usage category. 'Controls' are pre-treatment mobile money wallet balance, pre-treatment mobile wallet usage, and dummies for enrollment period and treatment delivery mixup. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

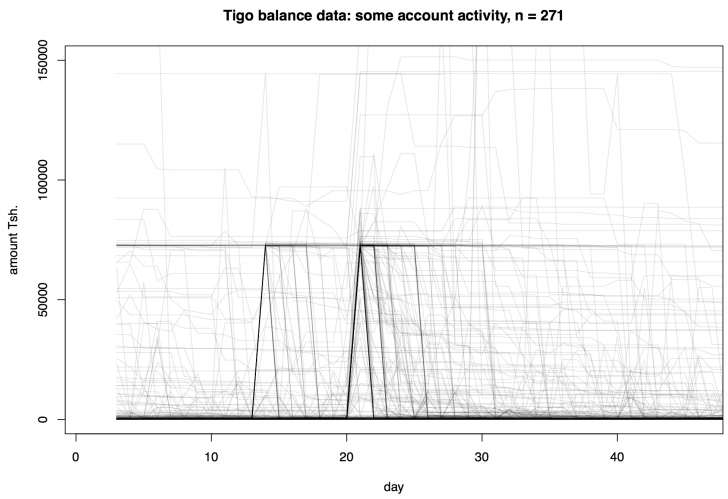
B.2 Baseline Mobile Wallet Use

The following figures show the mobile wallet balances for a 46-day period from January 1, 2018 through February 15, 2018, which data were used to code the mobile wallet use blocking variable. In the figures, each line represents the mobile wallet balances of a single participant during the period. The participants are split into three groups (and thus, three figures) based on the total number of unique balance amounts during the period. These figures include the analysis sample enrolled in April, the pilot sample from March, and the sample enrolled in July which ended up being dropped due to no cash transfer occurring after they were treated.

4 July - 30 Sept 2017 (89 days)
22 - 23 October 2017 (2 days)
1 - 22 November 2017 (22 days)
18 April 2018 (1 day)
18 May 2018 (1 day)
23 May - 31 Aug 2018 (101 days)
4 - 22 September 2018 (19 days)





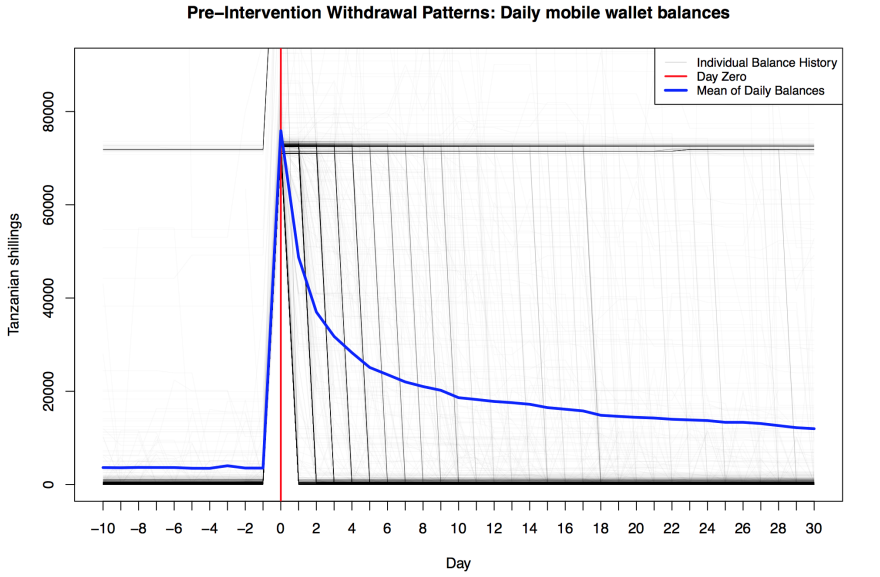


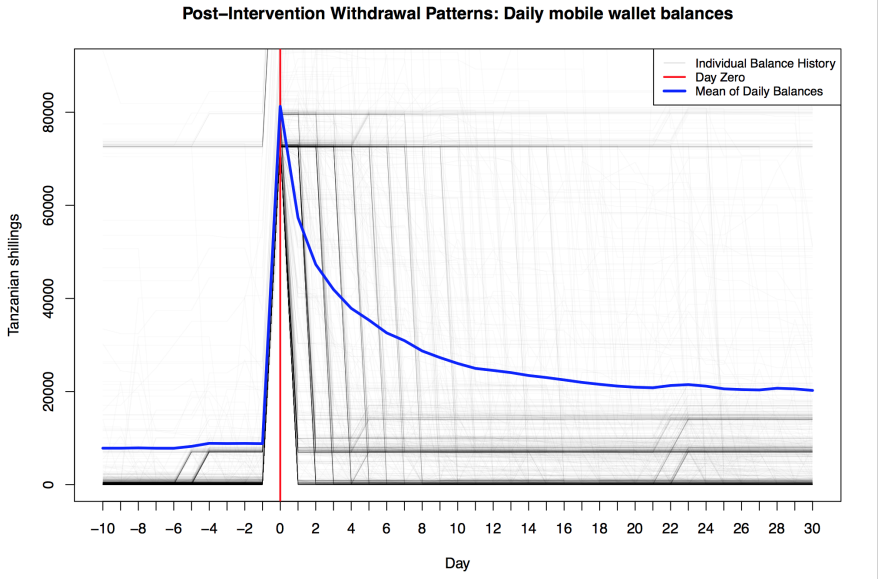
B.3 Patterns of mobile money withdrawal before and after treatment

Figure B.4 shows raw daily mobile wallet balance data around the date of a cash transfer, before treatment (top) and after (bottom). (This figure includes a larger set of mobile money users: the analysis sample enrolled in April, as well as the pilot sample from March and the dropped sample enrolled in July.) In both periods, immediate cash-out was common across the entire sample. In the top panel, we collapse all pre-intervention transfers, observing from ten days prior to the transfer through to 30 days after; the bottom panel shows withdrawal patterns for the transfer that occurred post-intervention. In both panels, we pool participants across all rounds and treatments. The day of a cash transfer from the NGO is indicated by the vertical red line. The blue line shows the mean of all users' balances, and each gray line represents the balance of a single user – the darker the line, the more users have that balance. The amount of each of the NGO's cash transfer was 70,000 TZS for each participant, and we can see that on the day of a transfer prior to the intervention, the average was 70,000. Some users left their 70,000 TZS in their account, but the average fell off sharply in the following days as more and more individual users drew their accounts down to close to zero.

The panel on the right shows that this pattern altered slightly in the period after the treatment. The horizontal gray lines at the 7000 TZS mark (and, to a lesser extent, at the 14,000 TZS mark) show visually that at least some participants understood the savings incentive messaging and chose to keep 10% of their transfer in their account.

Figure B.4: Cash Transfer Withdrawal Behavior Among Sample, before and after treatment





Each gray line represents one participant's mobile wallet balance; darker lines represent more individuals.

C Training materials

Figure C.5: Upfront Savings Poster for Round 1 Participants, in Kiswahili