Savings Defaults and Payment Delays for Cash Transfers: Field Experimental Evidence from Malawi*

Lasse Brune† Xavier Giné‡ Jessica Goldberg§ and Dean Yang¶

Preliminary and incomplete. Please do not cite.

December 6, 2015

Abstract

Financial products and transfer schemes are often designed to help individuals follow through intertemporal plans by not giving into "temptation". We implement an artefactual field experiment in Malawi to test the ability of households to manage a cash windfall and learn whether savings defaults and delayed payments change consumption patterns in ways that are likely to be welfare improving. We vary whether 600 households receive a windfall payment in cash or through direct deposit into pre-established accounts at a local bank, and whether payments are made immediately, with one day's delay, or with eight days' delay. Defaulting the payment into a savings account leads to higher bank savings, an effect that persists for a number of weeks afterwards. However, neither savings defaults nor payment delays affect the amount or composition of spending, suggesting that households manage cash effectively without the use of formal financial products.

*This project was funded by the World Bank Research Committee and GLM-LIC. We thank Stacy Pancratz for her excellent oversight of field work, and we appreciate helpful comments from Susan Godlonton and Margaret McConnell. All errors and omissions are our own.

†Yale University, lasse.brune@yale.edu

‡World Bank, xgine@worldbank.org

§University of Maryland, goldberg@econ.umd.edu

¶University of Michigan, deanyang@umich.edu

1 Introduction

According to the standard neoclassical model agents should be indifferent to the timing and savings default of an expected windfall. In contrast, recent behavioral theories suggest that both the timing and defaults have welfare implications. Quasi-hyperbolic preferences (Liaison 1997) and dual-self models (Thaler & Shefrin 1981, Fudenberg & Levine 2006) predict that choices made in one period will be regretted in the next. There is not much direct empirical evidence about regret in the economics literature, but the data indicate that even extremely poor households spend substantially on non-food goods such as alcohol and tobacco. Banerjee & Duflo (2007) report that across 13 countries, food accounts for only 56 to 78 percent of total consumption for those living on less than $1.08 USD per person per day (1993 PPP adjusted). Though these extremely poor households report poor health and nutrition, some devote considerable portions of their budget – five percent in India, six percent in Indonesia, and eight percent in Mexico – to tobacco and alcohol.

Empirical studies of access to finance often report spending on temptation goods as an outcome measure, with reduced spending considered a positive impact. For example, five of six randomized controlled trials evaluating microcredit products published in a recent special edition of the *American Economics Journal: Applied Economics* measure "discretionary spending," which is defined as spending on temptation goods, recreation, entertainment, and celebrations. A decline in this sort of discretionary spending is one of the few consistent findings across the microcredit studies (Banerjee, Karlan & Zinman 2015).

Additionally, there is empirical evidence that time inconsistent preferences can lead to suboptimal investments (Duflo, Kremer & Robinson 2011) or undermine the ability to follow through on planned use of future income (Gine et al. 2015). There is also a growing body of literature showing that financial products with features designed to address time inconsistency have been successful in increasing asset accumulation and improving welfare along a range of measures (Ashraf, Karlan & Yin 2006, Dupas & Robinson 2013*a*, Dupas & Robinson 2013*b*).

Thaler & Benartzi (2004) demonstrate the effect of default savings options. In developing countries, Aker et al. (2014) finds differences in the use of aid payments received via mobile money compared to those distributed in cash and Brune et al. (2016) shows that savings balances and subsequent investments increase sharply when agricultural proceeds are directly deposited into individual accounts.

In some contexts, then, individuals face important barriers to saving for investment or smoothing consumption, and appropriately designed financial products can help overcome these barriers. However, there is also reason to question whether these products are always necessary. For example, tobacco farmers in Malawi did not benefit from the opening of a

commitment savings account once a regular checking account was opened (Brune et al. 2016). Relatedly, entrepreneurs in Kenya failed to open savings accounts at an accessible local bank despite their apparently high returns (Dupas & Robinson 2013*a*). More in general and despite the oft-expressed concern by policy makers, there is little evidence that money from transfer schemes paid in cash is actually used for the purchase of temptation goods (Evans & Popova 2014).

In order to learn whether savings defaults and the timing of transfers can be used to improve welfare for households in Malawi, we implement an experiment varying the conditions under which 600 households receive a one-time, sixty dollar windfall.[1] Participants in our study either receive large cash transfers or equivalent-value deposits into their savings accounts. We vary whether the transfer is paid immediately, with one day's delay, or with eight days' delay.

Directly depositing income instead of paying in cash can affect savings and consumption through either transaction costs or psychological channels. Our study minimizes transaction costs by calling all respondents to the bank in order to receive the transfer. Thus, individuals whose money was directly deposited could withdraw it immediately, and conversely, those who received cash could make a deposit. By equating the transactions costs across experimental arms, differences in savings and consumption can be attributed to one of two psychological channels: the default effect, a pervasive phenomenon that for example explains differences in saving behavior (Thaler & Benartzi 2004) or organ donation (Johnson & Goldstein 2003), and mental accounting (Thaler 1999). Directly depositing money by default into the savings account may have signaled to respondents that the money was intended for saving rather than spending, causing them treat money in the account as though it was to be used differently than cash on hand. We cannot distinguish between default effects and mental accounting, but we can separate these two behavioral explanations from transaction costs. Throughout the paper, we emphasize the psychological mechanism for the effect of direct deposit by referring to that treatment as creating a "savings default."

Payment delays are used to test time inconsistent preferences. Respondents with exponential discount rates should spend and save similarly whether they receive money immediately or with a short delay. However, those with quasi-hyperbolic preferences experience a discontinuous decrease in utility for all future periods, and are therefore expected to consume more when they receive transfers immediately than when their transfers are delayed. Savings defaults may mediate differences in consumption for immediate versus delayed payments through competing psychological channels.

Our paper thus contributes to the literature on the effect of savings defaults albeit in a

[1]The transfer was MK 25,000, and was sufficient to purchase 50 kg of maize, 10 kg of beans, and two liters of cooking oil. The exchange rate at the time of the transfer was MK 420 to $US 1.

specific context. We study a one-time transfer, precluding the opportunity for habit formation or learning which may be present in other savings schemes like Save More Tomorrow (Thaler & Benartzi 2004). A key and novel feature is that the transaction costs of accessing the funds are equalized. Our experiment is thus a strict test of the direct psychological effects of defaults, abstracting from the effects that arise from the asymmetric transaction costs typically present in real-world settings.

In addition, the intervention takes place during the lean season when households may be most subject to temptation, and thus when nudges in the form of savings defaults or the ability to plan expenditures may be most relevant. Mani et al. (2013) show that economic scarcity by itself impedes cognitive performance in the lab and in the field, where farmers perform worse on cognitive tasks during the lean season (pre-harvest) compared to the post-harvest period. Banerjee & Mullainathan (2010) develop a framework in which the poor are more susceptible to temptation spending than the rich. In their model, savings defaults or payment delays have the potential to arrest the temptation spending that would result from windfall income received during the lean season.

We find that savings defaults do affect bank balances (and the converse, cash on hand). Those who initially receive cash deposit MK 1200 (five percent of the total transfer), while those whose transfer is directly deposited to a bank account – and thus are defaulted into saving – withdraw MK 18,000 (75% of the total). On average, net deposits are MK 2,500 higher in the savings default group (compared to the cash group) in the 30 days after the transfer. The impact of the savings default on savings is larger and more persistent for female respondents, compared to males.

Despite differences in cash-on-hand, transfer recipients with different savings defaults do not differ in their spending patterns in the weeks following the transfer. Both groups spend about 60% of the transfer in the first two weeks and only less than 15 percent is spent on food.

The overall spending patterns and the comparison between cash and savings default recipients refutes the notion that poor households cannot manage an unexpected cash windfall when income sources are limited. Unplanned expenditures account for only 5-10 percent of total spending, and are not different between the cash and direct deposit recipients. Recipients do not spend all cash on hand: after two weeks, 85% of the amount transferred is not in the bank, but only 60% has been spent. Similarly, and in contrast to the predictions of a model with strict quasi-hyperbolic preferences, delaying payment by either one or eight days does not meaningfully affect subsequent consumption patterns. Importantly and as we argue in the paper, these null results are not the result of limited statistical power.

Our finding that defaulting funds into savings account raises savings balances is consistent with existing behavioral economics research that savings defaults matter (Thaler & Benartzi

2004). At the same time, we find little indication that respondents' well-being is affected by savings defaults or payment delays. Savings defaults may be a valuable tool for smoothing consumption and alleviating barriers to saving in some settings, such as bulky income from agriculture or other seasonal enterprises, or when habit formation is possible with repeat transfers like wages. However, our results indicate that even poor households facing high marginal utility of consumption during the lean season have some capacity to manage cash on hand.

We describe our experiment and results in more detail, as follows. Section 2 offers a detailed description of the intervention and timeline. Section 3 describes the data. Section 4 explains our analytical framework. We discuss the effect of savings defaults in Section 5, and the impact of delayed payment in Section 6. Section 8 concludes.

2 Experimental design

The windfall income experiment described in this paper is one of a set of interventions designed to encourage savings and understand the mechanisms through which formal bank accounts affect consumption and spending. Other components of the research agenda on promoting savings studied using labeled accounts and showing respondents a video designed to raise their aspirations about future welfare. Random assignment in the windfall experiment was orthogonal to these other treatment conditions, and controlling for treatment group in other arms of the design does not affect the results of the windfall income study.

Crucially, the umbrella project offered subsidized bank accounts with the commercial bank NBS to households in villages whose centers fell within six kilometers of the bank's Mulanje branch location.[2] The branch is located in the local trading center, an approximately one-kilometer stretch along the main road with shops, government offices, and branches for other local banks. The field teams completed village listings in ten villages and randomly selected 872 households for surveys and account offers in July 2012. Of those households, approximately five percent already had accounts with NBS and another 15 percent had accounts with one or more other banks. Ultimately, 742 individuals opened new accounts. The final sample included 704 new and existing NBS account holders who participated in the savings promotion studies. From that sample of account holders, a random subset of 600 were selected for the windfall cash experiment.

The windfall experiment varied whether respondents received a payment (MK 25,000, equivalent at the time to $59.52 USD) in cash or directly deposited into their bank accounts. The savings default treatment was cross-randomized with the timing of payment: immedi-

[2]Individual household locations were measured via GPS, and could exceed 6 KM.

ately, after one day, or after eight days. In order to equalize the transactions costs of accessing the payment, all participants had to return to the bank in order to receive their payment whether it was made in cash or directly deposited into a bank account. Twenty percent of respondents (120 individuals) received a small, immediate cash payment of MK 1,000 instead of the large transfer of MK 25,000 and serve as the control group. Participants in the control group received a small payment to offset their travel and time costs and to preserve good will for participation in future survey waves.The final design thus includes six large transfer treatment arms that vary in savings default and timing of payment, and one control arm. These groups are summarized in Figure 1.

The randomization into the different treatment (and control) arms took place at the bank itself to avoid differential take-up. First, each head of household was visited by a field team for a midline survey, after which they were told they were eligible for a cash prize of up to MK 25,000 if they visited the bank branch exactly two days after the survey (which becomes "day zero" in the intervention timeline). In advance of the midline visit, households were randomly assigned (by computer, and stratified by village) to either a morning or afternoon visit to the bank branch. The shift implicitly determined whether the household would receive the transfer in cash or directly deposited into the bank account. The correspondence between shift time and savings default alternated daily, so respondents who interacted with each other at the bank all received the transfer in the same way. The savings default determination was not known to respondents until they visited the bank.

Assignment to disbursement timing took place at the bank. Respondents drew (without replacement) a token from a bag assigned to their village and bank shift.[3] The tokens corresponded to one of four groups: a control condition that received a small, immediate cash transfer or one of three timing conditions for the large transfers. The three timing conditions for the large payments were immediate, in one day, or in eight days. The savings default was cross randomized and determined by pre-assignment to morning or afternoon shift as explained above, but was revealed to respondents following the token draw. From the respondents' perspective, the token draw determined whether the transfer was large or small; whether it was defaulted into savings; and when it was received.

While produce, meat, dry goods, farming equipment, clothing, and sundries are available in local shops throughout the week, better prices and more variety is available at a weekly market that takes place on Fridays. For this reason, initial household visits, and thus bank visits and transfers, were staggered to permit inclusion of day-of-week fixed effects. All analysis is conducted relative to the day a household was assigned to visit the bank. Follow up surveys were carefully timed to capture spending at key intervals. The recall period for

[3]Two villages were very small and thus morning and afternoon shifts drew from the same bag. One village was so small that it was combined with the immediately adjacent, larger village.

each survey was one week. For those who received transfers immediately or one day after the initial bank visit (groups T1, T2, T4, and T5), pre-transfer expenditures come from the survey conducted at the initial visit, on day $t = -2$ as indicated in Figure 2. Spending in the week after the transfer (including day of the transfer) is measured in Survey 1, conducted on day $t = 7$ for the immediate-transfer group and day $t = 8$ for the one day delay group. For the eight day delay group, Survey 1 measures spending in the week after the announcement, but before the transfer was made. As we will discuss, households may spend in anticipation of receiving a large transfer. Survey 2, conducted on day $t = 15$, measures post-transfer expenditures for this group. The only exception to a one-week recall period is this survey, which includes an eight-day recall period to capture spending on the day of the transfer.

Transfers were implemented in March and April 2014. They were timed to coincide with the end of the lean season, just before many households harvest and sell crops. The value of the transfer is equivalent to relief payments made by Oxfam to flood victims in the region in early 2015.

3 Data

The household surveys described above contained the detailed expenditure module taken from Malawi's Third Integrated Household Survey (IHS-3). In addition to asking about the quantity purchased and total paid for 218 consumption goods, durable goods and services, we also ask whether each purchase was planned before the respondent arrived at the store or market, or was made on the spot.

We also use survey data collected between June and Aug 2013, before bank accounts were opened. These data include information about household demographics, expenditures, asset ownership and time preferences.

Table 1, panels A and B provide summary statistics for variables collected during baseline in 2013 and during round 1 of the midline interviews just prior to the transfer experiment. The majority of respondents are male. Sixty-three percent are married. Households have 4.7 members on average. Households own 1.5 acres of land on average and non-fixed assets worth MK 177,697 (about $US 555). During the 7 days leading up the round 1 interview, households spent a total of MK 11,500 ($US 36) on average, with MK 4,593 ($US14) spent on food.[4] For all but the most expensive items in the expenditure module, respondents were asked whether they had planned to buy the item in advance or whether they had made the decision only at the place of purchase. Respondents reported spending an average of MK 422 on unplanned food purchases, and MK114 on unplanned non-food items.

[4]Using the March 2013 exchange rate of MK 320 to $US 1.

Our outcomes of interest are computed using administrative data from NBS and expenditures measured in the household surveys. We use NBS data to measure the saving balances at different points in time. Conceptually, savings defaults could change spending patterns through a mental accounting framework even if all money was immediately withdrawn, if direct deposit recipients still treated the transfer as funds to be saved rather than spent. More likely, though, savings defaults may influence use of the transfer through a flypaper effect, with some of the money remaining in the account even though there were very low transaction costs to withdrawing it immediately.[5] Therefore, we examine banking activity on the day of the transfer; within three days of the transfer; within seven days of the transfer; and within 14 days of the transfer. We consider three outcomes: withdrawals, deposits, and net deposits (deposits minus withdrawals).

In the period immediately preceding the intervention, respondents seldom use their accounts. Panel C of Table 1 shows that only 7% of respondents had any bank account transaction in the 7 days prior to the round 1 survey collected just prior to respondent's first bank visit for this experiment. Thirty-three percent of respondents had at least one transaction in the 90 days prior to survey round 1. Deposits over the same period averaged MK 13,045 (about $US 41) and net deposits were slightly negative on average. The standard deviation for deposit and withdrawals values reveals large sample variation, with a relatively small number of very large deposit and withdrawals.

To ease interpretation, we present two sets of balancing tests. In Table 2, we report the mean and standard deviation of each baseline variable in the control group, the mean of each baseline variable in each of the six separate treatment cells, and the p-value for the test that the means are jointly equal in the control group and all treatment groups. There are no statistically significant differences for any of the baseline characteristics. Inspection of the data shows that the economically meaningful differences in asset ownership and in net deposits in the 90 days before Survey round 1 are driven by a few outlier observations.

The second set of balancing tests reported in Table 3 foreshadows our analysis by pooling across treatment arms. The left hand side corresponds to our analysis of savings defaults. We report the mean and standard deviation of the baseline variables in the control group, and the means in the cash and direct deposit treatment groups. There are no statistically significant differences in baseline characteristics of groups assigned to different savings defaults. The right hand side of the table repeats the mean and standard deviation in the control group, and then reports the means for each of the three payment time frames: immediate, one day delay, and eight day delay. Only the indicator for any banking activity in the 90 days before the survey is significantly different across the four groups (p=0.098). Overall, we conclude

[5]Withdrawal fees are flat and do not depend on the amount of the transaction.

that the randomization process produced balanced control and treatment groups.

4 Analysis

The experiment is designed to address the following interrelated questions. Does defaulting payment of a cash transfer into a savings account affect savings balances? Is income used differently when it is directly deposited to a bank account compared to when received in cash? And does delaying receipt change consumption and savings decisions?

We first examine the impact of the savings default on bank transactions, using the administrative data described in the previous section. We compare outcomes for respondents who received large cash payments and large direct deposits to those who received small cash payments, and to each other, by estimating the following equation:

$$\mathrm{Y}_i = \alpha + \beta_1 \text{Large transfer}_i + \beta_2 \text{Large transfer}_i \times \mathrm{SD}_i + \Theta \text{Village}_v + \epsilon_i \tag{1}$$

The coefficient β_1 measures the effect of receiving MK 25,000 in cash compared to receiving MK 1,000 in cash. β_2 captures the marginal effect of receiving the large transfer directly into the bank account, and the sum $\beta_1 + \beta_2$ is the effect of an MK 25,000 transfer with a savings default relative to cash disbursement of MK 1,000. The specification includes village fixed effects. Standard errors are robust to heteroskedasticity, but not clustered because randomization is at the individual level, a decision that is conservative in light of our emphasis on null results. We report results for each of the three outcomes – deposits, withdrawals, and net deposits – in four time horizons to observe whether there is a persistent effect of the savings default. We present results for dependent variables in levels (Malawi kwacha) as well as in the quintic root, which helps reduce the influence of outliers.

Next, we look at spending in several categories, one and two weeks after the transfer. Recall that expenditure data were collected before prizes were announced (survey 0). We use pooled OLS specifications that incorporate these pre-treatment values of outcomes to measure the effect of savings defaults on spending one week post-transfer and, where available – that is, for the immediate payment and one day delay groups – two weeks post-transfer. We estimate

$$\mathrm{Y}_{it} = \alpha + \beta_1 \text{Post1}_{it} + \beta_2 \text{Post2}_{it} + \beta_3 \mathrm{SD}_i \times \text{Post1}_{it} + \beta_4 \mathrm{SD}_i \times \text{Post2}_{it} + \delta \mathrm{Y}_{i0} + \Gamma \text{Round}_t + \Theta \text{Village}_v + \epsilon_i \tag{2}$$

where, as in equation (1), all specifications include village fixed effects. We also include a survey round fixed effect. As before, the comparison group are those who received MK 1,000 in cash immediately. β_1 (β_2) is the increase in spending for those who received the

large transfer instead of the small transfer after one (two) survey rounds. β_3 and β_4 are the marginal effects of the savings default. The marginal propensity to consume from the windfall income in the first week after it is received is $\frac{\beta_1}{24{,}000}$ for the cash group and $\frac{\beta_1+\beta_3}{24{,}000}$ for the savings default group, since both receive MK 24,000 more than the control group. Since we only have one round of post-transfer data for those assigned to the eight day delay, β_2 and β_4 are identified by the immediate and one-day delay groups only.

Finally, we study the effect of payment delay on expenditures. The outcome measure is spending one week post-transfer since using the two-week follow up data would eliminate the comparison to the eight-day lag treatment group. We estimate

$$\mathrm{Y}_{it} = \alpha + \beta_1 \mathrm{Post1}_{it} + \beta_2 \mathrm{Post1}_{it} \times \mathrm{Delay1}_i + \beta_3 \mathrm{Post1}_{it} \times \mathrm{Delay8}_i + \delta \mathrm{Y}_{i0} + \Gamma \mathrm{Round}_t + \Theta \mathrm{Village}_v + \epsilon_i \tag{3}$$

with fixed effects and standard errors as in equation (2). Here, β_1 is the marginal effect of receiving MK 25,000 compared to the small transfer. β_2 is the additional effect of receiving that transfer with one day's delay instead of immediately, and β_3 is the effect of eight days' delay instead of immediate payment.

5 Savings default results

5.1 Administrative outcomes

Estimates of equation (1) test whether the savings default had any impact on immediate cash-on-hand. Since the experiment equated the transaction costs to accessing the transfer, the mode of payment should affect outcomes only through psychological channels. Differences in bank activity for cash compared to direct deposit recipients is sufficient (though not necessary) evidence that savings defaults matter.

Table 4, Panel A estimates the effect of windfall income and savings defaults on deposits. For recipients defaulted into saving, the total deposits of MK 24,683 (the sum $\alpha + \beta_1 + \beta_2$, from column 1) on the day of the transfer is mechanical, and confirms that direct deposits were made as intended. Cash recipients of large transfers deposited MK 1,219 more than those who received the small transfer. These immediate deposits account for five percent of the cash transfer, and represent a significant increase in deposits relative to the control group. Columns 2-4 indicate some additional deposits over time among the cash recipients of large transfers, but none by the direct deposit recipients.

Results in Panel B indicate that those defaulted into saving immediately withdrew most but not all of the transfer. On the day of the transfer, savings default recipients withdrew MK 17,957 more than cash recipients – in other words, they withdrew 75 percent of the

transfer (relative to the MK 1000 received by the control group). As expected, large cash transfer recipients' withdrawals were not statistically different from small transfer recipients.

Panel C reports net deposits, which are a proxy for bank balances. On the day of the transfer, cash recipients have MK 1,232 higher net deposits that the control group while recipients whose transfer was directly deposited into their account have net deposits that are MK 6,752 higher than the control group and MK 5,520 higher than the cash transfer group. Initially, then, the savings default induced recipients to keep 23 percent of their transfer in the bank. The total amount saved by the savings default group is nearly constant over the two weeks following the transfers. Because deposits increase for the cash recipients, the initial effect of the savings default is significant after seven days (column 3) but not after 14 days (column 4).

The initial differences in savings between participants who received transfers in cash compared to those defaulted into savings are striking because the experiment design ensured similar conditions for the two groups. Yet, despite minimal transaction costs, directly depositing the transfer induced 4.4 times higher savings on the day of the transaction (Panel C, column 1) and 2.8 times more savings a week later (Panel C, column 3).[6]

Thus, relative to the control group the cash transfer increased cash-on-hand by MK 22,768 and direct deposit increased cash-on-hand by MK 17,248 on the day of the transfer.

In Table 5, we examine the impact of savings defaults on net deposits over longer time periods. Impacts on net deposits in Malawi kwacha are in Panel A, and in Panel B we present impacts on the quartic root of net deposits (a specification that reduces the influence of outliers). The first four columns present impacts on average net deposits in week-long periods immediately following the transfer: 0-6, 7-13, 14-20, and 21-27 days later. A positive and statistically significant effect of the savings default persists through the third week post-transfer in the levels specification, and through the fourth week in the quartic root specification.

The remaining six columns of the table present impacts on average net deposits in month-long periods following the transfer: 0-29, 30-59, 60-89, 90-119, 120-149, and 150-179 days later. In the levels specification, the impact of the savings default is statistically significant in the first month (days 0-29), while in the quintic root regressions the savings default is statistically significantly different from zero in the first and second months (days 0-29 and 30-59) post-treatment.

This evidence of positive and persistent effect of the savings defaults on bank savings motivates the examination of the composition of expenditures in the next subsection.

[6]While the focus is on assessing the impact of default savings, we note that recipients of the large transfer compared to the control group saved a significant portion of the transfer in the bank for more than two weeks and that higher net deposits (relative to the control group) persist for 90 days after the initial transfer (result not shown).

5.2 Household survey outcomes

We are interested in the composition and timing of expenditures by cash and savings default recipients. Table 6 reports total spending and spending on food and non-food items. Recipients of large cash transfers spend MK 9,632 more than the control group in the one week following treatment. That accounts for 40 percent of their transfer, and more than doubles total weekly spending relative to the control group mean of MK 8,296. More importantly, there is virtually no difference in the total spending between the cash and savings default groups.

In the second week following the transfer, cash recipients spend an additional MK 3,389 relative to the control group. Those in the savings default group spend an additional MK 1,262, but the difference relative to the cash group is again not statistically significant.

Despite making the transfers during the lean season, the bulk of the transfer is *not* spent on food. Cash transfer recipients spent MK 1,567 of their surplus on food in the first week. This is 6.5 percent of the transfer, and an increase of 47 percent relative to the control group. Those whose transfer was directly deposited into their accounts spend an additional MK 780, which is significantly higher than the cash recipients at the 90 percent confidence level. In the second week after the transfer, large transfer recipients do not spend significantly more on food than the control group.

Instead, both cash and savings default recipients spent most of the transfer on non-food items. Cash transfer recipients spent MK 8,043 and savings default recipients MK 7,181 ($= 8042.463 - 861.486$) more than the control group on non-food items in the first week. These increases are about trice the control group's weekly spending of MK 4,744.

It is remarkable that though cash recipients had nearly MK 23,000 more cash on hand than the control group, they spent less than half of that within the first week and only 58 percent after two weeks. Savings default recipients had slightly less cash on hand initially and spent slightly more after two weeks, but both groups spent about half of a transfer scaled to cover one month's food costs in two weeks, and both groups had considerable amounts of unspent cash that was not in the bank. This is clear evidence of intertemporal smoothing, and evidence that households can overcome short-term constraints to saving without using bank accounts.

A second test of households' ability to manage cash is presented in Table 7, which considers planned and unplanned expenditures separately. If saving in the bank protects against temptation spending, one would expect to see more unplanned expenditures for the cash transfer group than the savings default group. Instead, we see extremely low levels of additional unplanned spending in either group, and no economically meaningful or statistically significant differences between the two.

Among the control group, 6.5 percent of all food expenditures – a total of MK 211 – were unplanned. The cash treatment group spent an additional MK 59 on unplanned food in the week after receiving the transfer, compared to an additional MK 1,507 on planned food expenditures. Not only is the extra unplanned food spending not statistically significant, but also, unplanned expenditures represent a smaller share of food purchases for the treatment groups than the control group. The savings default group spent an extra MK 52 on unplanned food, an amount not statistically different from the cash group.

Spending on non-food items exhibits similar patterns. The largest share of spending by the control group and both treatment groups is on planned purchases. Even after receiving a large windfall payment, households do not make spontaneous purchases, but rather seek out items they intended to buy all along. There are no significant differences in the spending of those who receive cash compared to those whose transfered was directly deposited into their bank account.

Together, Tables 6 and 7 present clear evidence of intertemporal smoothing and the ability to resist temptation or pressure to spend money immediately upon receiving a large sum of money. This findings are strengthened by the fact that the transfers are made during the lean season when the marginal utility of consumption is high.

6 Payment delay results

We cross-randomize the savings default treatments with zero, one, and eight day delays in transfers for an additional test of the effect of time-inconsistency and planning on savings and expenditures. Quasi-hyperbolic discounters would be more likely to spend, and to succumb to temptation spending, if they received money immediately. In a true test of quasi-hyperbolic discounting, a one-day delay would have a meaningful effect on expenditures. In contrast, those forced to wait eight days for their payments have greater opportunity to plan their purchases.

Table 8 reports estimates of equation (3) for total expenditures and food and non-food expenditures. Outcomes are spending in the week after receiving the transfer, counting from each individual's date of transfer. Those who received their transfers without delay spent MK 9,148 more than the control group in the first week. People who experienced a one-day delay spent MK 1,311 more, but the difference is not statistically significant. The eight-day delay group spent a statistically insignificant MK 281 less than those who received immediate payments. For all three groups, most of the spending is on non-food items, and there are no significant differences in the composition of spending by the duration of delay.

In Table 9, we break spending into planned and unplanned categories. As mentioned,

planned spending accounts for the majority of both food and non-food purchases. Recipients of immediate transfers had an extra MK 1,871 of planned food expenditures. Those whose payments were delayed by one day spent slightly less and those whose payments were delayed by eight days, slightly more. While unplanned food expenditures were small for all three treatment groups, the eight-delay group spent MK 120 less than the no-delay group (significant at the 10% level) and MK 167 less than the one-day delay group (significant at the 5% level). For non-food expenditures, the immediate payment group increases spending by MK 816, or 81 percent of planned non-food spending among the control group. Those whose payments were delayed by one day spent MK 609 more than the immediate payment group, a difference that is significant at the five percent level. The eight-day delay group spent MK 372 more than the immediate payment group, an increase that is not significant at conventional levels. Unplanned spending on non-food items is very low for all three treatment and the control groups.

Those who receive the transfer unexpectedly and without warning are no more likely to spend it, and do not spend it meaningfully differently, than people who receive advance notice of the transfer. As with the previous comparisons between cash transfers and direct deposit, this is remarkable for holding during the lean season, when marginal utility of consumption is likely highest and when individuals may be more subject to temptation (Banerjee & Mullainathan 2010, Mani et al. 2013).

In addition, the results of Table 9 indicate that the null results founds are not the result of limited statistical power in the analysis. Indeed, the differences in unplanned expenditures across treatment and control arms are statistically significant but not economically meaningful, and thus we have power to detect small differences in the outcomes of interest.

7 Heterogeneous effects

Respondents differ along characteristics that may mediate the effect of direct deposit on their savings or consumption. Heterogeneous responses to treatment could explain or refute our finding that direct deposit and payment delay do not affect spending, if average effects mask positive effects for some respondents and negative effects for others. Therefore, our motivation in testing for heterogeneous effects is both to understand whether the effect of direct deposit and payment delays depends on respondents' characteristics, and to examine the robustness of our null findings.

We test for heterogeneous effects along five dimensions: gender of the household head, use of the bank in the 90 days preceding the experiment, distance from the bank branch, asset ownership, and impatience. We split the sample by gender of household head, any bank use

in preceding 90 days, or, for the other three characteristics, an indicator for being above the sample median, and then separately estimate treatment effects within each subsample. For each dimension of heterogeneity, we report the p-values for the tests of equal coefficients in the two subsamples. The regression specifications correspond to those used in the previous sections: equation (1) for administrative outcomes and equations (2) and (3) for survey outcomes.

7.1 Savings default

7.1.1 Administrative outcomes

Of the three outcome variables from the administrative data, we focus on net deposits (Panel C of Table 4) because they represent changes in savings at the bank. Table 10 reports the effect of savings defaults on net deposits on the day of the transfer for subsamples along each of the five dimensions discussed above. For example, in Panel A column (1), we see that net deposits the day of the transfer for male-headed households who received the small amount (the control group) were MK 597. Male-headed households who received the large transfer in cash had MK 1,327 higher net deposits than the small-transfer group, and those who received the large transfer with a savings default had an additional MK 5,933 in net deposits. Panel B column (1) shows net deposits for female-headed households. The average net deposit in the small-transfer group was zero. Net deposits were MK 1,539 higher for female-headed households who received large transfers in cash, and increased by an additional MK 4,025 with a savings default. At the bottom of Panel B, we report two p-values. The first tests the null hypothesis that large transfers had equal effects on male- and female-headed households. We fail to reject this null with a p-value of 0.81. The second tests that the marginal effect of the savings default was equal for male- and female-headed households, and we fail to reject that null with a p-value of 0.17.[7]

The remaining four columns split the sample by use of the bank in the 90 days before the experiment, distance from the bank branch (those closer to the bank in the top panel), asset ownership, and patience (requiring a below-median amount of money in one month in

[7] In Appendix Table A1, we examine the effect of the savings default for male- and female-headed households separately, over longer time periods post-transfer, in levels as well as quintic root specifications of the dependent variable. There are considerable differences across gender of the household head in subsequent time periods. For male-headed households, the effect of the savings default is never statistically significant in the levels regressions, and only for the first month (days 0-29) in the quintic root specification. By contrast, for female-headed households, the savings default has a positive and statistically significant effect through the third month (days 60-89) in the levels regression and through the 4th month (days 90-119) in the quintic root regression. At the bottom of the table we report tests of the difference in coefficients across the male- and female-headed household subsamples. The coefficient on the savings default is different (and larger for females) across the gender subsamples in the first two months in the levels specification, and in the second through 4th months in the quintic specification.

exchange for money today), all as measured at baseline.[8] Comparing coefficients in Panel A and Panel B, we we see no significant differences in the impacts of either the windfall income or savings default. Direct deposit increases net deposits on the day of the transfer for all subgroups we examine.

We turn to net deposits 14 days after the transfer in Table 11. Some differences emerge, though the totality of the evidence again suggests that savings defaults affect groups with different characteristics in similar ways. Men in the control group who received the small transfer have negative net deposits. Net deposits increase by MK 4,326 for men who get large transfers in cash, and decrease by MK 439 less for men who get large transfers that are defaulted into savings (not significant). For women, net deposits are higher in the control group (MK 1,330), compared to men, and the effect of the large transfer is relatively smaller for cash (MK 467) and relatively larger for the marginal effect of savings defaults (MK 4,703). The differences between men and women for both the main effect of the large transfer and the marginal effect of savings defaults are statistically significant, though the total difference in savings between men and women who receive direct deposit (main effect of large transfer + marginal effect of the savings default) are smaller than the same differences in either main effect or marginal effect of savings defaults. The only additional evidence of heterogeneity in this table is for the effect of savings defaults on respondents who live closer to the bank compared to those who live further away (column 3). Direct deposit increases net deposits by MK 502 for households living further from the branch and by MK 2,994 for households living closer to the branch. The p-value for the test of no differences in the savings default coefficient between Panels A and B for all five columns jointly is 0.11, and there is no consistent pattern of direct deposit having larger effects on advantaged groups. While the effect of savings defaults dissipates over time, it increases net deposits two weeks after transfers for most subgroups.

7.1.2 Household survey outcomes

Panel A of Table 12 reports estimates of equation (2) for male-headed households, for households with any bank activity before the experiment, and for those above the median for the other three characteristics; Panel B reports the corresponding estimates for female-headed households, for those without bank activity before the experiment, and those below the median for the other characteristics. We estimate a total of 40 coefficients and report 20 p-values testing whether coefficients in Panel A are equal to corresponding coefficients in Panel B. We cannot reject equality in any of these tests. The p-value for the joint test that the effect of

[8] Gender of the household head, any previous transactions prior to the experiment, and MK required to prefer waiting one month are discrete variables, and splitting at the median does not result in equal sized samples above- and below-median.

savings defaults is equal after one week for the Panel A and Panel B subsamples in each of columns (1) to (5) is 0.49.

Tables 13 and 14 examine heterogeneous effects on food and non-food spending, respectively. The effect of the savings default for the subgroup in Panel A is never significantly different from the effect for the subgroup in Panel B. The p-value for the test that transfers and savings defaults had the same effect on all subgroups after one week is 0.82 for spending on food and 0.70 for spending on non-food items (p-values of joint tests are reported in the text but not included in the tables).

These estimates indicate that savings defaults did not have large differential effects on spending of respondents with different characteristics. Moreover, they confirm that the null effect of savings defaults reported in Table 8 does not mask heterogeneous effects on different subgroups. In each of ten subgroups examined in Table 12, direct deposit recipients do not differ from cash recipients in spending one week or two weeks after the transfer. These estimates of heterogeneous effects confirm that in this context, consumption is not significantly affected by mental accounting or defaults.

7.2 Payment timing

We conduct analogous tests for heterogeneous effects of delayed payments. Table 15 examines effects on total spending, using the subgroups defined in the previous section. For example, male-headed households who receive large transfers with no delay spend MK 9,274 more than male-headed households in the control group. A one-day delay increases spending by MK 435 and an eight-day delay decreases spending by MK 27 (neither statistically significantly so). Female-headed households start with lower household spending than those headed by men (MK 5,832 for female-headed households in the control group, compared to MK 9,658 for male-headed households), but the effect of the immediate transfer is very similar: MK 9,632. A one-day delay adds MK 3,161 and an eight-day delay adds MK 317. We cannot reject that the main effect of the large transfer is the same for male-headed households as for those headed by women (p=0.90), that the differential effect of the one-day delay is the same (p=0.39) or that the differential effect of the eight-day delay is the same (p=0.92).

Only two dimensions exhibit heterogeneity that is statistically significant, and only in the main effects of the large transfer but not in the effects of payment delay. The main effect of the transfer increases total spending by MK 5,657 for households that had at least one bank transaction in the 90 days before the transfers, compared to an increase of MK 10,707 for households that had *not* used the bank. The difference in point estimates of MK 5,050 is statistically significant with p-value 0.076. While payment delays increased spending more among bank-using households than other households, differences in the effect of the

payment delays are not significant at conventional levels. Similarly, the transfer increased spending by MK 5,115 ($= 11180.633 - 6066.088$) more for households that live closer to the bank than those living further away. The increase in spending in the large transfer group is partially offset by an eight-day delay for households that live near the bank, but exacerbated for households that live further away.

Tables 16 and 17 report results separately for spending on food and non-food items. There are no statistically significances in the effects of payment delays on food spending along any of the categories we examine. Any heterogeneity in total spending reported in Table 15 appears driven by spending on non-food items, and is mirrored by the results in Table 17, columns (2) and (3). The increase in spending on non-food items is higher for households that live close to the bank than for those that live further away and lower for those with prior bank use compared to no prior bank use; but the differences are not statistically significant.

As with savings defaults, estimates of heterogeneous effects of payment timing support the interpretation that payment timing is not a major determinant of spending. For total spending, delays of one or eight days significantly modify the impact of transfers for only one of the ten subgroups we study (households with previous bank use spend somewhat more following a one day delay than an immediate payment, and the interaction term is significant at the 10% level). We fail to reject that a one day delay has differential effects across all five characteristics with a p-value of 0.72, and that an eight day delay is jointly different with a p-value of 0.31 (not shown in tables). When we break spending out into food and non-food items, only one of twenty coefficients for payment delays is even marginally statistically significantly different from zero. This strengthens our conclusion that in this context, high discount rates or hyperbolic preferences are not important determinants of consumption patterns.

8 Conclusion

Defaulting a one-time cash transfer into a savings account (a savings default) leads to higher savings for transfer recipients in subsequent weeks. However, we find no evidence that either savings defaults or delayed payments affected respondents' consumption patterns. Households that received lump sum transfers during the lean season are able to smooth intertemporally without use of formal financial products, and report very low levels of unplanned spending. Formal financial products may be more important in other contexts: at different points in the year, for recurring payments, for earned income, or for different payment amounts.

The savings default we study differs in one crucial dimension from those in previous work:

it virtually eliminates the transaction cost to undo the default. All participants come to the bank in order to receive their transfers; those who receive direct deposit need only walk through the bank door to withdraw funds, and those who receive cash can do the same to make a deposit. In contrast, changing automatic contributions to a retirement plan requires requesting and completing new benefit deduction forms, and increasing savings by making deposits from wages requires either a trip to the bank or logging on to the bank's website to make a transfer (and forgoing the tax advantages of pre-tax contributions). If defaults affect behavior because psychological costs are amplified by small time or monetary costs of accessing directly deposited funds, then they will be ineffective in a setting when the transaction costs are equalized.

There are other potential explanations for our findings. Nutritional deprivation leading to very high marginal utility of consumption of food could explain the low levels of temptation spending observed, but in our study very little of the transfer is used for food. This is consistent with the behavior of very poor households studied by Banerjee & Duflo (2007), who have considerable non-food expenditures despite very low incomes, and with recent findings that income earned through Malawi's public works program (PWP) does not improve nutrition (Beegle, Galasso & Goldberg 2015).

We study a one-time transfer, which limits the opportunity for habit formation. Somville & Vandewalle (2015) study recurring transfers by paying Indian survey participants the equivalent of one day's wage for seven to 13 weeks. They vary whether payments are in cash or through individual accounts with local banks, and find lower consumption and nearly-equivalent higher savings for those paid through bank accounts. However, the effect dissipates as soon as payments are switched to cash. This works against the hypothesis that habit formation or learning is an important mechanism or a reason that our results would underestimate the impact of recurring direct deposits.

The transfers in our study are unearned and unanticipated. Previous work in Malawi shows that direct deposit of earned agricultural income does affect savings, investment, and consumption in the following year (Brune et al. 2016). In other studies of access to bank accounts, deposits come from the subjects' own assets or income (Dupas & Robinson 2013*a*). Mental accounting could lead to different use of earned and unearned income, and to different effects of payment structure on earned income than what we measure, for unearned income.

Similarly, payment delay does not affect expenditures in our study, but it may in other contexts. While there is evidence that direct deposit does affect spending and investment for earned income, the evidence on payment frequency or delay is less conclusive. In Malawi, paying public works beneficiaries every three days compared to every week does not affect consumption (Beegle, Galasso & Goldberg 2015), but paying participants in an NGO's livelihood program monthly instead of weekly reduces total short-run spending and increases take

up of a high yield short term investment opportunity (Brune & Kerwin 2014). In Indonesia, unanticipated delays to planned disbursements of a government-sponsored unconditional cash transfer reduced consumption growth of beneficiaries relative to non-beneficiaries as well as to those who received payments on schedule (Bazzi, Sumatro & Suryahadi Forthcoming).

Previous studies have established that financial access and savings defaults can change savings and investments, and our results do not contradict those findings. Rather, they suggest limits to the impact of or need for formal financial products to manage cash. Future research should identify situations in which savings defaults and other financial products are most likely to be effective in combating behavioral biases that lead to spending that is later regretted, and that an appreciation of the ability to manage cash will prevent unnecessary rigidities or complexities in the design of wage payments or cash transfers.

References

Aker, Jenny, Rachid Boumnijel, Amanda McClelland, and Niall Tierney. 2014. "Payment Mechanisms and Anti-Poverty Programs: Evidence from a Mobile Money Cash Transfer Experiment in Niger." Center for Global Development Working Paper 268.

Ashraf, Nava, Dean Karlan, and Wesley Yin. 2006. "Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines." *Quarterly Journal of Economics*, 121(2): 635–672.

Banerjee, Abhijit, and Esther Duflo. 2007. "The Economic Lives of the Poor." *Journal of Economic Perspectives*, 21(1): 141–168.

Banerjee, Abhijit, and Sendhil Mullainathan. 2010. "The Shape of Temptation: Implications for the Economic Lives of the Poor." NBER Working Paper No. 15973.

Banerjee, Abhijit, Dean Karlan, and Jonathan Zinman. 2015. "Six Randomized Evaluations of Microcredit: Introduction and Further Steps." *American Economic Journal: Applied Economics*, 7(1): 1–21.

Bazzi, Samuel, Sudarno Sumatro, and Asep Suryahadi. Forthcoming. "It's All in the Timing: Cash Transfers and Consumption Smoothing in a Developing Country." *Journal of Economic Behavior and Organization.*

Beegle, Kathleen, Emanuela Galasso, and Jessica Goldberg. 2015. "Direct and Indirect Effects of Malawi's Public Works Program on Food Security." Working paper.

Brune, Lasse, and Jason Kerwin. 2014. "Income Timing, Temptation and Expenditure: A Field Experiment in Malawi." Working Paper.

Brune, Lasse, Xavier Gine, Jessica Goldberg, and Dean Yang. 2016. "Facilitating Savings for Agriculture: Field Experimental Evidence from Malawi." *Economic Development and Cultural Change.*

Duflo, Esther, Michael Kremer, and Jonathan Robinson. 2011. "Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya." *American Economic Review*, 101(6): 2350–90.

Dupas, Pascaline, and Jonathan Robinson. 2013*a*. "Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya." *American Economic Journal: Applied Economics*, 99(2): 163–192.

Dupas, Pascaline, and Jonathan Robinson. 2013*b*. "Why Don't the Poor Save More? Evidence from Health Savings Experiments." *American Economic Review*, 103(4): 1138–1171.

Evans, David, and Anna Popova. 2014. "Cash Transfers and Temptation Goods: A Review of Global Evidence." World Bank Policy Research Working Paper WPS6886.

Fudenberg, Drew, and David Levine. 2006. "A Dual-Self Model of Impulse Control." *American Economic Review*, 96(5): 1449–1476.

Gine, Xavier, Jessica Goldberg, Dan Silverman, and Dean Yang. 2015. "Revising Commitments: Field Experimental Evidence on Adjustment of Prior Choices." Working Paper.

Johnson, E.J., and D.G. Goldstein. 2003. "Do Defaults Save Lives?" *Science*, 1338–1339.

Liaison, David. 1997. "Golden Eggs and Hyperbolic Discounting." *Quarterly Journal of Economics*, 112: 443–447.

Mani, Anand, Sendhil Mullainathan, Eldar Shafir, and Jiaying Zhao. 2013. "Poverty Impedes Cognitive Function." *Science*, 341(6149): 976–980.

Somville, Vincent, and Lore Vandewalle. 2015. "Savings by Default: Evidence from a Field Experiment in Rural India." Working Paper.

Thaler, Richard. 1999. "Mental Accounting Matters." *Journal of Behavioral Decision Making*, 12: 183–206.

Thaler, Richard, and H.M. Shefrin. 1981. "An Economic Theory of Self-Control." *Journal of Political Economy*, 89(2): 392–406.

Thaler, Richard, and Shlomo Benartzi. 2004. "Save More Tomorrow: Using Behavioral Economics to Increase Employee Saving." *Journal of Political Economy*, 112(2): S164–S187.

Figures

Figure 1: Experimental design

	Small Transfer (MK 1000)	Large Transfer (MK 25,000)	
	Cash	Cash	Direct Deposit
Immediate	Control group (120)	T1 (80)	T4 (80)
+1 day		T2 (80)	T5 (80)
+8 days		T3 (80)	T6 (80)

Savings default

Figure 2: Intervention and survey timing

Day	+0 days			+1 day		+8 days	
-2	HH Visit and Survey; Announcement of Money Prize						
-1							
0	MK 1,000	MK 25,000	MK 25,000				
1				MK 25,000	MK 25,000		
2							
3							
4							
5							
6							
7	Survey 1	Survey 1	Survey 1			Survey 1	Survey 1
8				Survey 1	Survey 1	MK 25,000	MK 25,000
9							
10							
11							
12							
13							
14	Survey 2	Survey 2	Survey 2				
15				Survey 2	Survey 2	Survey 2	Survey 2

Activity at home	Cash
Activity at bank	Direct deposit

Tables

Table 1: Summary statistics

	(1) Mean	(2) SD	(3) N	(4) 5th percentile	(5) 10th percentile	(6) Median	(7) 90th percentile	(8) 95th percentile
	Panel A: Baseline survey (June to August 2013)							
Male	0.66	0.48	593	0	0	1	1	1
Married	0.63	0.48	593	0	0	1	1	1
Number of hh members	4.7	2.1	593	2.0	2.0	4.0	7.0	8.0
Acres of land	1.5	1.2	593	0.3	0.5	1.2	2.8	3.5
Value of non-fixed assets [MK]	177,697	595,306	593	4,590	7,100	40,500	264,000	517,300
Asset index	-0.05	3.23	593	-3.10	-2.85	-1.00	3.74	6.28
Distance to branch [km]	3.70	1.71	593	1.27	1.63	3.60	6.34	7.10
Hyperbolic	0.23	0.42	593	0	0	0	1	1
Patient now, impatient later	0.27	0.45	593	0	0	0	1	1
Impatience (switching point, out of 6)	2.90	2.03	593	1	1	2	6	6
	Panel B: Total of itemized expenditures from round 1 survey							
Total	11,500	15,860	593	520	960	5,625	28,950	43,760
Food	4,593	5,377	593	220	480	2,780	10,710	16,386
Non-food	6,942	12,456	593	100	280	2,440	17,000	32,180
Unplanned food	422	815	593	0	0	60	1,300	2,130
Unplanned non-food	114	428	593	0	0	0	200	700
	Panel C: Bank transaction before round 1 survey							
Any activity 7 days prior	0.07	0.26	593	0	0	0	0	1
Any activity 90 days prior	0.33	0.47	593	0	0	0	1	1
Value of deposits 90 days prior	13,045	107,024	593	0	0	0	11,500	45,000
Value of withdrawals 90 days prior	15,230	171,546	593	40,500	11,440	0	0	0
Value of net deposits 90 days prior	-2,184	69,350	593	-2,000	-600	0	774	3,500

The impatience measure comes from a series of question asking whether the respondent would prefer MK 400 tomorrow or a different amount in one month. The choices increased as follows: MK 450, 500, 600, 800, 1000 or more. We report the ordinal number of the question for which the respondent preferred to wait; larger numbers indicate greater impatience.

Table 2: Balancing tests, full randomization

	Control		Cash no delay	Savings default no delay	Cash 1 day delay	Savings default 1 day delay	Cash 8 day delay	Savings default 8 day delay	P-value: equal mean in all groups
	Mean	SD	Mean	Mean	Mean	Mean	Mean	Mean	
	Panel A: Baseline survey (June to August 2013)								
Male	0.65	0.48	0.61	0.74	0.65	0.67	0.64	0.63	0.626
Married	0.65	0.48	0.61	0.67	0.61	0.61	0.65	0.61	0.952
Number of hh members	4.5	1.7	4.5	4.9	4.7	4.7	4.9	4.5	0.714
Acres of land	1.5	1.1	1.4	1.7	1.4	1.3	1.4	1.6	0.602
Value of non-fixed assets [MK]	235,940	775,245	171,487	147,338	285,700	115,339	126,699	133,939	0.558
Asset index	0.35	3.62	-0.31	-0.03	-0.07	-0.12	0.02	-0.44	0.696
Distance to branch [km]	3.69	1.66	3.75	3.66	3.55	3.69	3.90	3.64	0.925
Hyperbolic	0.22	0.41	0.28	0.18	0.27	0.28	0.22	0.18	0.515
Patient now, impatient later	0.30	0.46	0.19	0.23	0.28	0.28	0.33	0.27	0.442
Impatience (switching point, out of 6)	2.85	1.99	3.04	2.67	2.91	3.20	2.89	2.80	0.771
	Panel B: Total of itemized expenditures from round 1 survey								
Total	12,850	18,327	12,131	10,862	10,559	10,105	12,182	11,175	0.914
Food	4,812	5,268	5,361	3,768	4,174	4,185	5,121	4,689	0.388
Non-food	8,277	15,400	7,058	6,998	6,244	5,929	6,927	6,487	0.935
Unplanned food	491	843	397	386	441	320	415	469	0.873
Unplanned non-food	82	400	145	149	122	92	117	107	0.945
	Panel C: Bank transaction before round 1 survey								
Any activity 7 days prior	0.05	0.22	0.07	0.06	0.06	0.11	0.09	0.06	0.827
Any activity 90 days prior	0.34	0.47	0.31	0.32	0.39	0.39	0.28	0.24	0.340
Value of deposits 90 days prior	10,167	34,071	5,971	32,120	12,954	14,267	7,340	8,929	0.690
Value of withdrawals 90 days prior	-10,186	32,543	-5,486	-52,232	-12,940	-13,013	-5,531	-7,996	0.561
Value of net deposits 90 days prior	-19	7,279	484	-20,112	14	1,254	1,809	933	0.577
Number of observations	119		74	82	79	79	81	79	

The impatience measure comes from a series of question asking whether the respondent would prefer MK 400 tomorrow or a different amount in one month. The choices increased as follows: MK 450, 500, 600, 800, 1000 or more. We report the ordinal number of the question for which the respondent preferred to wait; larger numbers indicate greater impatience.

Table 3: Balancing tests, savings default and payment delay comparisons

	Savings default comparisons				Payment delay comparisons				
	Control group	Cash	Savings default	P-value: equal means	Control group	No delay	1 day delay	8 day delay	P-value: equal means
	Panel A: Baseline survey (June to August 2013)								
Male	0.65	0.63	0.68	0.492	0.65	0.68	0.66	0.64	0.880
Married	0.65	0.62	0.63	0.911	0.65	0.64	0.61	0.63	0.905
Number of hh members	4.5	4.7	4.7	0.471	4.5	4.7	4.7	4.7	0.675
Acres of land	1.5	1.4	1.5	0.437	1.5	1.5	1.4	1.5	0.538
Value of non-fixed assets [MK]	235,940	194,543	132,395	0.229	235,940	158,794	200,520	130,273	0.446
Asset index	0.35	-0.12	-0.19	0.354	0.35	-0.16	-0.10	-0.21	0.542
Distance to branch [km]	3.69	3.73	3.66	0.900	3.69	3.70	3.62	3.77	0.885
Hyperbolic	0.22	0.26	0.21	0.502	0.22	0.23	0.27	0.20	0.496
Patient now, impatient later	0.30	0.27	0.26	0.684	0.30	0.21	0.28	0.30	0.214
Impatience (switching point, out of 6)	2.85	2.94	2.89	0.906	2.85	2.85	3.06	2.84	0.742
	Panel B: Total of itemized expenditures from round 1 survey								
Total	12850.34	11617.70	10715.78	0.513	12850.34	11463.73	10331.93	11684.64	0.675
Food	4812.40	4877.12	4208.28	0.321	4812.40	4523.54	4179.59	4907.43	0.639
Non-food	8,277	6,738	6,478	0.524	8,277	7,026	6,087	6,710	0.636
Unplanned food	491	418	392	0.574	491	391	381	441	0.675
Unplanned non-food	82	128	116	0.602	82	147	107	112	0.681
	Panel C: Bank transaction before round 1 survey								
Any activity 7 days prior	0.05	0.07	0.08	0.539	0.05	0.06	0.09	0.08	0.629
Any activity 90 days prior	0.34	0.33	0.32	0.923	0.34	0.31	0.39	0.26	0.098
Value of deposits 90 days prior	10166.54	8802.45	18609.84	0.650	10166.54	19716.02	13610.77	8124.57	0.707
Value of withdrawals 90 days prior	-10185.76	-8018.19	-24761.42	0.548	-10185.76	-30057.94	-12976.72	-6747.74	0.455
Value of net deposits 90 days prior	-19.22	784.25	-6151.59	0.511	-19.22	-10341.92	634.05	1376.83	0.543
Number of observations	119	234	240		119	156	158	160	

The impatience measure comes from a series of question asking whether the respondent would prefer MK 400 tomorrow or a different amount in one month. The choices increased as follows: MK 450, 500, 600, 800, 1000 or more. We report the ordinal number of the question for which the respondent preferred to wait; larger numbers indicate greater impatience.

Table 4: Effect of savings default on bank transactions

	(1) Day of transfer	(2) +3 days	(3) +7 days	(4) +14 days
	Panel A: Deposits			
Large transfer	1218.689**	1815.862***	2241.135***	3774.977**
	(465.743)	(512.870)	(555.502)	(1316.478)
Large transfer * savings default	23464.068***	23680.970***	23925.490***	22589.247***
	(379.432)	(625.373)	(707.302)	(1298.605)
Observations	593	593	593	593
Mean of dependent variable in control group	429.20	455.34	599.11	1691.55
R-squared	0.90	0.80	0.82	0.80
	Panel B: Withdrawals			
Large transfer	9.195	215.854	-248.259	-960.366
	(179.995)	(334.472)	(427.075)	(1142.183)
Large transfer * savings default	-17956.980***	-19977.028***	-20465.209***	-21119.597***
	(646.896)	(558.653)	(705.043)	(1299.871)
Observations	593	593	593	593
Mean of dependent variable in control group	-43.24	-573.91	-726.72	-1510.42
R-squared	0.71	0.81	0.76	0.52
	Panel C: Net Deposits			
Large transfer	1232.484**	2033.602***	1984.204***	2768.588**
	(488.661)	(430.551)	(361.523)	(1094.064)
Large transfer * savings default	5519.736***	3727.008***	3420.995***	1466.356
	(721.770)	(833.221)	(708.321)	(957.258)
Observations	593	593	593	593
Mean of dependent variable in control group	385.97	-118.57	-127.61	181.13
R-squared	0.29	0.25	0.16	0.62

OLS regressions. All specifications include village and week-of survey fixed effects, and the baseline value of the outcome. * p<0.10, ** p<0.05, *** p<0.001

Table 5: Effect of savings default on long-term net deposits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Days post transfer:	0-6	7-13	14-20	21-27	0-29	30-59	60-89	90-119	120-149	150-179
	Panel A: Level									
Large transfer	1,852.634***	2,134.818***	2,388.110**	2,657.179**	2,290.917***	2,254.365**	1,799.658	1,714.211	2,341.955*	3,353.782**
	(391.027)	(585.941)	(1,088.618)	(1,041.785)	(670.696)	(908.867)	(1,171.696)	(1,258.418)	(1,312.227)	(1,566.541)
Large transfer * savings default	3,641.717***	3,148.964***	2,514.690*	1,405.062	2,526.719***	560.610	1,474.429	224.417	216.353	-1,069.567
	(678.174)	(946.301)	(1,476.275)	(930.315)	(763.874)	(937.426)	(1,158.043)	(1,110.963)	(1,199.825)	(1,593.932)
Observations	593	593	593	593	593	593	593	593	593	593
Mean of dependent variable in control group	86.692	238.198	470.839	-63.279	143.504	-895.688	-1086.360	-1086.160	-1681.678	-1918.221
R-squared	0.12	0.07	0.04	0.05	0.08	0.04	0.06	0.05	0.04	0.04
P-value: Total effect of large transfer + savings default = 0	0.000	0.000	0.002	0.000	0.000	0.014	0.012	0.136	0.067	0.211
	Panel B: Quintic root									
Large transfer	1.238***	1.448***	1.467***	1.242***	1.497***	1.317***	1.232***	1.383***	1.567***	1.560***
	(0.214)	(0.245)	(0.263)	(0.267)	(0.257)	(0.270)	(0.274)	(0.297)	(0.304)	(0.321)
Large transfer * savings default	2.020***	1.480***	0.934***	0.962***	1.501***	0.712**	0.448	0.296	0.070	0.203
	(0.270)	(0.276)	(0.284)	(0.282)	(0.270)	(0.293)	(0.297)	(0.306)	(0.314)	(0.316)
Observations	593	593	593	593	593	593	593	593	593	593
Mean of dependent variable in control group	-0.027	0.004	0.188	0.293	0.285	0.053	0.088	-0.240	-0.578	-0.566
R-squared	0.20	0.16	0.12	0.11	0.17	0.10	0.07	0.07	0.07	0.08
P-value: total effect of large transfer + savings default = 0	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

OLS regressions. All specifications include village and survey round fixed effects.
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Effect of windfall cash and savings default on expenditures

	(1) Total	(2) Food	(3) Non-food
One week post treatment	9631.848***	1567.184***	8042.463***
	(944.282)	(327.853)	(785.150)
Savings default * one week post	-23.295	779.821*	-861.486
	(1201.000)	(437.534)	(1000.502)
Two weeks post treatment	3389.269**	623.178	2804.772**
	(1089.284)	(417.542)	(908.599)
Savings default * two weeks post	1262.014	311.460	821.487
	(1204.966)	(429.257)	(1012.672)
Observations	1180	1180	1180
Mean of dependent variable in control group	8295.83	3328.18	4743.60
R-squared	0.36	0.34	0.27

OLS regressions. All specifications include village and survey round fixed effects.
* p<0.10, ** p<0.05, *** p<0.001

Table 7: Effect of windfall cash and savings default on planned vs. unplanned expenditures

	(1)	(2)	(3)	(4)
	Food		Non-food	
	Planned	Unplanned	Planned	Unplanned
One week post treatment	1507.537***	59.428	1392.693***	1.706
	(324.044)	(41.766)	(173.835)	(23.496)
Savings default * one week post	692.694	51.755	-273.787	30.833
	(436.227)	(50.513)	(212.316)	(30.192)
Two weeks post treatment	563.296	72.307	601.031**	-6.359
	(409.181)	(49.682)	(191.444)	(29.093)
Savings default * two weeks post	297.841	-5.437	359.294	19.117
	(425.821)	(46.664)	(230.781)	(30.744)
Observations	1180	1180	1180	1180
Mean of dependent variable in control group	3103.77	210.83	1003.47	69.70
R-squared	0.30	0.28	0.26	0.03

OLS regressions. All specifications include village and survey round fixed effects.
* p<0.10, ** p<0.05, *** p<0.001

Table 8: Effect of delayed transfer on expenditures

	(1) Total	(2) Food	(3) Non-food
One week post treatment	9147.888***	2035.452***	7239.783***
	(1383.601)	(482.408)	(1071.954)
One week post treatment * 1 day delay	1311.418	-194.288	1388.529
	(1504.699)	(516.616)	(1226.962)
One week post treatment * 8 day delay	-280.692	-59.399	-416.182
	(1794.111)	(652.535)	(1411.631)
Observations	708	708	708
Mean of dependent variable in control group	8295.83	3328.18	4743.60
R-squared	0.39	0.31	0.30
P-value: 1 day delay = 8 day delay	0.394	0.831	0.206

OLS regressions. All specifications include village and survey round fixed effects.
Sample limited to one post-treatment observation per respondent.
* p<0.10, ** p<0.05, *** p<0.001

Table 9: Effect of delayed transfer on planned vs. unplanned expenditures

	(1)	(2)	(3)	(4)
	Food		Non-food	
	Planned	Unplanned	Planned	Unplanned
One week post treatment	1871.088***	132.218**	816.206***	-7.403
	(483.011)	(61.473)	(209.794)	(37.318)
One week post treatment * 1 day delay	-279.388	47.497	608.656**	63.081
	(514.911)	(69.506)	(262.252)	(41.970)
One week post treatment * 8 day delay	144.970	-119.660*	371.874	-25.421
	(648.850)	(68.355)	(272.820)	(48.739)
Observations	708	708	708	708
Mean of dependent variable in control group	3103.77	210.83	1003.47	69.70
R-squared	0.27	0.29	0.28	0.06
P-value: one day delay = 8 day delay	0.495	0.022	0.426	0.120

OLS regressions. All specifications include village and survey round fixed effects.
Sample limited to one post-treatment observation per respondent.
* p<0.10, ** p<0.05, *** p<0.001

Table 10: Heterogeneous effects of savings default on net deposits, day of transfer (+0 days)

	(1)	(2)	(3)	(4)	(5)
Panel A:	Male HH head	Any previous bank use	Close to branch	Above median: Assets	Above median: Patience
Large transfer	1326.683*	1920.191	1339.261	1261.765	1998.270**
	(710.207)	(1391.476)	(822.190)	(824.512)	(744.135)
Large transfer * savings default	5932.769***	5231.954***	4820.041***	6227.426***	5502.546***
	(943.061)	(1513.385)	(985.028)	(1124.876)	(1146.381)
Observations	389	193	296	296	239
Mean of dependent variable in control group	596.50	1148.26	661.94	665.66	-85.63
R-squared	0.34	0.42	0.40	0.40	0.23
Panel B:	Female HH head	No previous bank use	Far from branch	Below median: Assets	Below median: Patience
Large transfer	1539.022**	1030.729**	1194.202**	1559.998**	996.498
	(533.267)	(343.548)	(523.750)	(548.453)	(723.475)
Large transfer * savings default	4024.845***	5489.953***	6128.125***	4568.709***	5567.544***
	(1113.224)	(810.204)	(1077.023)	(936.056)	(941.237)
Observations	204	400	297	297	354
Mean of dependent variable in control group	0.00	0.00	85.79	0.00	704.79
R-squared	0.20	0.21	0.18	0.20	0.36
P-value: equal effect of large transfer	0.805	0.514	0.878	0.756	0.318
P-value: equal effect of savings default	0.174	0.875	0.356	0.242	0.964

OLS regressions. All specifications include village fixed effects. The top panel includes respondents from female-headed households and those with above-median values for number of transactions at NBS in the 90 days preceding the transfer, those who live above the median distance from the bank branch, those with above-median values of a principle-index of asset ownership, and those with above-median patience, as measured at baseline by amount required to prefer a transfer in one month instead of immediately. * p<0.10, ** p<0.05, *** p<0.001

Table 11: Heterogeneous effects of savings default on net deposits, within two weeks of transfer (+14 days)

	(1)	(2)	(3)	(4)	(5)
Panel A:	Male HH head	Any previous bank use	Close to branch	Above median: Assets	Above median: Patience
Large transfer	4326.022**	6134.381*	1961.323*	3608.466*	2801.390***
	(1668.076)	(3374.801)	(1085.997)	(1884.236)	(716.551)
Large transfer * savings default	-439.144	-756.302	2994.381**	1263.403	1779.887**
	(1500.647)	(2528.925)	(1034.960)	(1683.655)	(903.384)
Observations	389	193	296	296	239
Mean of dependent variable in control group	-445.58	476.35	827.00	344.55	-763.54
R-squared	0.68	0.69	0.87	0.66	0.16
Panel B:	Female HH head	No previous bank use	Far from branch	Below median: Assets	Below median: Patience
Large transfer	466.684	1287.403***	3240.157**	1642.952***	2825.416*
	(940.291)	(334.119)	(1282.109)	(492.352)	(1623.429)
Large transfer * savings default	4702.765***	2155.773**	502.212	1478.194**	1522.179
	(1396.094)	(717.112)	(909.095)	(700.754)	(1349.040)
Observations	204	400	297	297	354
Mean of dependent variable in control group	1330.09	31.65	-521.40	-44.40	819.78
R-squared	0.21	0.08	0.58	0.13	0.65
P-value: equal effect of large transfer	0.038	0.133	0.433	0.297	0.989
P-value: equal effect of savings default	0.009	0.245	0.063	0.903	0.870

OLS regressions. All specifications include village fixed effects. The top panel includes respondents from female-headed households and those with above-median values for number of transactions at NBS in the 90 days preceding the transfer, those who live above the median distance from the bank branch, those with above-median values of a principle-index of asset ownership, and those with above-median patience, as measured at baseline by amount required to prefer a transfer in one month instead of immediately. * p<0.10, ** p<0.05, *** p<0.001

Table 12: Heterogeneous effects of savings default on total spending

	(1)	(2)	(3)	(4)	(5)
Panel A:	Male HH head	Any previous bank use	Close to branch	Above median: Assets	Above median: Patience
One week post treatment	8911.797***	10211.466***	10944.589***	9435.812***	9440.455***
	(1208.252)	(1904.808)	(1274.082)	(1408.185)	(1447.866)
Savings default * one week post	710.688	-2706.606	-603.418	-766.451	2208.070
	(1503.338)	(2054.788)	(1640.704)	(1852.419)	(2070.354)
Two weeks post treatment	3466.551**	3543.095*	4375.764**	4357.755**	4339.137**
	(1548.323)	(1977.554)	(1461.368)	(1659.994)	(1900.053)
Savings default * two weeks post	808.519	1887.379	-321.776	-655.123	361.638
	(1581.683)	(2217.013)	(1623.178)	(1842.504)	(2205.988)
Observations	772	384	589	590	475
Mean of dependent variable in control group	9657.47	10633.13	7176.42	10619.20	6516.74
R-squared	0.36	0.45	0.43	0.42	0.35
Panel B:	Female HH head	No previous bank use	Far from branch	Below median: Assets	Below median: Patience
One week post treatment	11518.149***	9613.928***	8137.578***	10120.968***	10056.740***
	(1644.899)	(1067.659)	(1396.638)	(1269.622)	(1238.094)
Savings default * one week post	-1678.153	1402.991	250.688	566.365	-1561.826
	(1844.039)	(1445.001)	(1731.179)	(1573.777)	(1384.685)
Two weeks post treatment	3752.064**	3620.134**	2069.710	2695.547**	3154.629**
	(1437.737)	(1305.490)	(1619.967)	(1326.482)	(1294.625)
Savings default * two weeks post	1691.563	1201.648	2348.302	3238.438**	1873.655
	(1738.664)	(1425.485)	(1771.693)	(1585.937)	(1357.804)
Observations	408	796	591	590	705
Mean of dependent variable in control group	5831.90	7097.21	9514.29	5024.13	9494.50
R-squared	0.32	0.32	0.30	0.26	0.40
P-value: equal effect of large transfer one week post	0.193	0.780	0.132	0.713	0.742
P-value: equal effect of savings default one week post	0.306	0.095	0.716	0.577	0.123
P-value: equal effect of large transfer two weeks post	0.891	0.973	0.283	0.427	0.600
P-value: equal effect of savings default two weeks post	0.702	0.790	0.259	0.104	0.552

OLS regressions. All specifications include village and survey round fixed effects. The top panel includes respondents from female-headed households and those with above-median values for number of transactions at NBS in the 90 days preceding the transfer, those who live above the median distance from the bank branch, those with above-median values of a principle-index of asset ownership, and those with above-median patience, as measured at baseline by amount required to prefer a transfer in one month instead of immediately. * p<0.10, ** p<0.05, *** p<0.001

Table 13: Heterogeneous effects of savings default on spending, food

	(1)	(2)	(3)	(4)	(5)
Panel A:	Male HH head	Any previous bank use	Close to branch	Above median: Assets	Above median: Patience
One week post treatment	1139.299**	1165.785*	1835.298***	1365.689**	1767.511**
	(414.254)	(697.498)	(418.835)	(491.576)	(577.099)
Savings default * one week post	760.608	-48.353	788.544	829.264	1012.107
	(513.187)	(675.825)	(571.993)	(664.460)	(810.625)
Two weeks post treatment	842.485	854.027	950.734	661.269	1790.450**
	(574.471)	(950.606)	(588.751)	(701.581)	(667.691)
Savings default * two weeks post	56.674	-429.552	209.626	71.547	-81.816
	(596.295)	(863.361)	(597.590)	(771.969)	(704.391)
Observations	772	384	589	590	475
Mean of dependent variable in control group	3593.95	4164.94	3040.16	3902.86	2615.37
R-squared	0.37	0.41	0.42	0.36	0.35
Panel B:	Female HH head	No previous bank use	Far from branch	Below median: Assets	Below median: Patience
One week post treatment	2300.708***	1887.936***	1208.317**	1800.949***	1446.407***
	(545.878)	(362.403)	(508.747)	(428.535)	(400.946)
Savings default * one week post	764.113	1105.921**	792.792	713.188	531.392
	(785.739)	(551.969)	(677.672)	(585.523)	(484.922)
Two weeks post treatment	-40.731	635.127	162.636	620.299	-123.972
	(558.707)	(417.722)	(590.345)	(469.654)	(549.475)
Savings default * two weeks post	753.218	630.579	457.746	569.782	611.597
	(516.300)	(436.741)	(611.327)	(464.346)	(551.568)
Observations	408	796	591	590	705
Mean of dependent variable in control group	2847.26	2899.07	3641.68	2518.93	3808.44
R-squared	0.27	0.31	0.27	0.25	0.35
P-value: equal effect of large transfer one week post	0.084	0.347	0.334	0.497	0.642
P-value: equal effect of savings default one week post	0.997	0.177	0.996	0.894	0.604
P-value: equal effect of large transfer two weeks post	0.262	0.829	0.337	0.961	0.024
P-value: equal effect of savings default two weeks post	0.369	0.262	0.768	0.574	0.430

OLS regressions. All specifications include village and survey round fixed effects. The top panel includes respondents from female-headed households and those with above-median values for number of transactions at NBS in the 90 days preceding the transfer, those who live above the median distance from the bank branch, those with above-median values of a principle-index of asset ownership, and those with above-median patience, as measured at baseline by amount required to prefer a transfer in one month instead of immediately. * p<0.10, ** p<0.05, *** p<0.001

Table 14: Heterogeneous effects of savings default on spending, non-food

	(1)	(2)	(3)	(4)	(5)
Panel A:	Male HH head	Any previous bank use	Close to branch	Above median: Assets	Above median: Patience
One week post treatment	7945.550***	8523.011***	8737.693***	7833.567***	7694.921***
	(1020.335)	(1500.540)	(1052.010)	(1138.718)	(1206.495)
Savings default * one week post	-331.598	-2050.890	-1108.162	-1532.816	821.374
	(1267.322)	(1774.516)	(1310.400)	(1494.660)	(1632.209)
Two weeks post treatment	2792.739**	2560.476	3435.465**	3631.821**	2832.476*
	(1295.781)	(1704.337)	(1266.054)	(1413.861)	(1633.240)
Savings default * two weeks post	466.365	2104.233	-541.398	-950.656	44.264
	(1348.709)	(1914.383)	(1417.257)	(1592.011)	(1860.423)
Observations	772	384	589	590	475
Mean of dependent variable in control group	5716.51	6143.50	4101.87	6334.71	3670.26
R-squared	0.28	0.35	0.34	0.33	0.24
Panel B:	Female HH head	No previous bank use	Far from branch	Below median: Assets	Below median: Patience
One week post treatment	8552.439***	7943.269***	7237.488***	8711.066***	8455.138***
	(1326.544)	(913.122)	(1177.105)	(1075.796)	(1011.543)
Savings default * one week post	-2022.315	-83.510	-906.476	-453.551	-1918.262
	(1547.397)	(1209.508)	(1469.441)	(1355.223)	(1226.274)
Two weeks post treatment	3238.224**	3064.046**	2000.481	2470.909**	3030.977**
	(1200.788)	(1065.411)	(1323.362)	(1043.932)	(1064.981)
Savings default * two weeks post	1114.962	497.278	1638.155	2506.427**	1309.833
	(1387.501)	(1173.623)	(1439.868)	(1262.840)	(1130.967)
Observations	408	796	591	590	705
Mean of dependent variable in control group	2983.10	4025.71	5442.12	2503.06	5466.77
R-squared	0.21	0.24	0.23	0.21	0.32
P-value: equal effect of large transfer one week post	0.711	0.736	0.335	0.569	0.623
P-value: equal effect of savings default one week post	0.389	0.349	0.917	0.587	0.172
P-value: equal effect of large transfer two weeks post	0.797	0.798	0.427	0.502	0.917
P-value: equal effect of savings default two weeks post	0.733	0.464	0.274	0.084	0.554

OLS regressions. All specifications include village and survey round fixed effects. The top panel includes respondents from female-headed households and those with above-median values for number of transactions at NBS in the 90 days preceding the transfer, those who live above the median distance from the bank branch, those with above-median values of a principle-index of asset ownership, and those with above-median patience, as measured at baseline by amount required to prefer a transfer in one month instead of immediately. * p<0.10, ** p<0.05, *** p<0.001

Table 15: Heterogeneous effects of payment delay on total spending

	(1)	(2)	(3)	(4)	(5)
Panel A:	Male HH head	Any previous bank use	Close to branch	Above median: Assets	Above median: Patience
One week post treatment	9274.426***	5657.416**	11180.633***	8402.374***	9530.272***
	(1878.617)	(2432.525)	(1789.749)	(2101.925)	(2460.565)
One week post treatment * 1 day delay	434.615	4378.432*	898.554	1515.564	-391.162
	(1837.061)	(2631.840)	(2277.756)	(2344.273)	(2616.278)
One week post treatment * 8 day delay	-26.909	3997.998	-2799.330	605.336	1611.956
	(2511.656)	(3512.039)	(2292.077)	(2813.602)	(3272.051)
Observations	462	232	356	364	285
Mean of dependent variable in control group	9657.47	10633.13	7176.42	10619.20	6516.74
R-squared	0.42	0.48	0.43	0.43	0.38
Panel B:	Female HH head	No previous bank use	Far from branch	Below median: Assets	Below median: Patience
One week post treatment	9631.650***	10707.039***	6066.088**	10111.896***	9533.815***
	(2106.681)	(1660.094)	(2017.675)	(1697.146)	(1718.943)
One week post treatment * 1 day delay	3160.583	612.187	2277.166	1061.232	2198.111
	(2664.076)	(1930.545)	(2071.823)	(2067.854)	(1826.500)
One week post treatment * 8 day delay	316.867	-2046.125	2891.827	-796.661	-1446.329
	(2250.866)	(2094.958)	(2668.644)	(2055.901)	(2158.713)
Observations	246	476	352	344	423
Mean of dependent variable in control group	5831.90	7097.21	9514.29	5024.13	9494.50
R-squared	0.31	0.37	0.38	0.33	0.44
P-value: equal effects of transfer	0.896	0.076	0.052	0.516	0.999
P-value: equal effects of 1 day delay	0.385	0.233	0.647	0.881	0.404
P-value: equal effects of 8 day delay	0.917	0.126	0.098	0.680	0.422

OLS regressions. All specifications include village and survey round fixed effects. The top panel includes respondents from female-headed households and those with above-median values for number of transactions at NBS in the 90 days preceding the transfer, those who live above the median distance from the bank branch, those with above-median values of a principle-index of asset ownership, and those with above-median patience, as measured at baseline by amount required to prefer a transfer in one month instead of immediately. * p<0.10, ** p<0.05, *** p<0.001

Table 16: Heterogeneous effects of payment delay on spending, food

	(1)	(2)	(3)	(4)	(5)
Panel A:	Male HH head	Any previous bank use	Close to branch	Above median: Assets	Above median: Patience
One week post treatment	2008.921***	863.174	2541.905***	1716.921**	2482.330**
	(591.443)	(975.215)	(594.003)	(686.660)	(874.114)
One week post treatment * 1 day delay	-331.109	-34.703	-403.369	249.000	-36.031
	(593.776)	(795.631)	(665.745)	(757.338)	(1016.962)
One week post treatment * 8 day delay	-598.463	836.622	-310.804	184.602	-716.169
	(818.131)	(1303.844)	(877.512)	(977.673)	(1133.314)
Observations	462	232	356	364	285
Mean of dependent variable in control group	3593.95	4164.94	3040.16	3902.86	2615.37
R-squared	0.38	0.40	0.36	0.34	0.31
Panel B:	Female HH head	No previous bank use	Far from branch	Below median: Assets	Below median: Patience
One week post treatment	2108.673**	2560.311***	1441.766*	2271.104**	1714.464**
	(883.649)	(574.057)	(776.431)	(686.851)	(571.054)
One week post treatment * 1 day delay	-198.050	38.164	-77.945	-621.452	-207.028
	(943.724)	(699.211)	(828.013)	(706.778)	(545.396)
One week post treatment * 8 day delay	1229.329	-403.782	173.840	-54.918	344.583
	(1035.369)	(752.594)	(984.552)	(843.834)	(783.155)
Observations	246	476	352	344	423
Mean of dependent variable in control group	2847.26	2899.07	3641.68	2518.93	3808.44
R-squared	0.25	0.29	0.29	0.26	0.35
P-value: equal effects of transfer	0.923	0.121	0.250	0.558	0.449
P-value: equal effects of 1 day delay	0.902	0.943	0.754	0.389	0.879
P-value: equal effects of 8 day delay	0.153	0.394	0.707	0.849	0.428

OLS regressions. All specifications include village and survey round fixed effects. The top panel includes respondents from female-headed households and those with above-median values for number of transactions at NBS in the 90 days preceding the transfer, those who live above the median distance from the bank branch, those with above-median values of a principle-index of asset ownership, and those with above-median patience, as measured at baseline by amount required to prefer a transfer in one month instead of immediately. * p<0.10, ** p<0.05, *** p<0.001

Table 17: Heterogeneous effects of payment delay on spending, non-food

	(1)	(2)	(3)	(4)	(5)
Panel A:	Male HH head	Any previous bank use	Close to branch	Above median: Assets	Above median: Patience
One week post treatment	7489.338***	4871.278**	8258.375***	6747.035***	7016.485***
	(1397.313)	(1886.442)	(1476.306)	(1559.637)	(1675.007)
One week post treatment * 1 day delay	928.806	4076.055*	1310.141	1299.176	-14.014
	(1481.659)	(2161.293)	(1846.393)	(1829.128)	(1991.452)
One week post treatment * 8 day delay	288.543	3068.722	-1991.901	297.610	2441.345
	(1908.186)	(2513.919)	(1865.871)	(2083.602)	(2474.249)
Observations	462	232	356	364	285
Mean of dependent variable in control group	5716.51	6143.50	4101.87	6334.71	3670.26
R-squared	0.33	0.41	0.37	0.38	0.30
Panel B:	Female HH head	No previous bank use	Far from branch	Below median: Assets	Below median: Patience
One week post treatment	7017.913***	8390.520***	5173.976***	8220.825***	7945.065***
	(1827.040)	(1295.802)	(1489.656)	(1428.850)	(1428.021)
One week post treatment * 1 day delay	2785.166	381.140	2208.785	1434.106	1961.507
	(2214.920)	(1534.848)	(1699.194)	(1719.552)	(1562.976)
One week post treatment * 8 day delay	-1002.946	-1892.844	1895.260	-820.990	-2194.485
	(1979.548)	(1740.067)	(2043.320)	(1824.021)	(1758.939)
Observations	246	476	352	344	423
Mean of dependent variable in control group	2983.10	4025.71	5442.12	2503.06	5466.77
R-squared	0.21	0.28	0.28	0.25	0.35
P-value: equal effects of transfer	0.833	0.112	0.133	0.475	0.665
P-value: equal effects of 1 day delay	0.472	0.150	0.714	0.956	0.422
P-value: equal effects of 8 day delay	0.629	0.094	0.151	0.679	0.116

OLS regressions. All specifications include village and survey round fixed effects. The top panel includes respondents from female-headed households and those with above-median values for number of transactions at NBS in the 90 days preceding the transfer, those who live above the median distance from the bank branch, those with above-median values of a principle-index of asset ownership, and those with above-median patience, as measured at baseline by amount required to prefer a transfer in one month instead of immediately. * p<0.10, ** p<0.05, *** p<0.001

Online Appendix (not for publication)

Table A1: Effect of savings default on long-term net deposits, by gender of household head

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
				Level						Quintic root		
Days post transfer:	0-29	30-59	60-89	90-119	120-149	150-179	0-29	30-59	60-89	90-119	120-149	150-179
	Panel A: Male-headed households											
Large transfer	3,312.784***	2,627.825*	2,131.553	1,154.824	2,086.741	3,132.181	1.625***	1.418***	1.315***	1.358***	1.632***	1.677***
	(1,024.033)	(1,376.009)	(1,836.643)	(1,864.751)	(1,895.714)	(2,291.658)	(0.329)	(0.356)	(0.362)	(0.389)	(0.397)	(0.426)
Large transfer * savings default	1,417.701	-461.790	1,168.705	231.238	438.637	-1,478.246	1.284***	0.307	0.012	-0.127	-0.164	-0.189
	(1,032.921)	(1,191.759)	(1,648.372)	(1,592.462)	(1,651.803)	(2,006.139)	(0.330)	(0.371)	(0.375)	(0.380)	(0.387)	(0.395)
Observations	389	389	389	389	389	389	389	389	389	389	389	389
Mean of dependent variable in control group	-102.460	-1482.234	-1689.388	-1585.207	-2403.404	-2771.304	0.303	-0.049	0.011	-0.163	-0.497	-0.485
R-squared	0.10	0.05	0.09	0.07	0.07	0.06	0.18	0.10	0.07	0.08	0.08	0.08
P-value: Total effect of large transfer + savings default = 0	0.000	0.183	0.086	0.454	0.198	0.534	0.000	0.000	0.000	0.002	0.000	0.000
	Panel B: Female-headed householdst											
Large transfer	843.334	975.516**	1,364.345***	2,026.529**	2,525.157**	3,031.745**	1.373***	1.110***	1.102***	1.499***	1.717***	1.649***
	(802.617)	(449.804)	(450.253)	(788.154)	(1,060.199)	(1,496.828)	(0.415)	(0.404)	(0.416)	(0.472)	(0.471)	(0.475)
Large transfer * savings default	3,870.978***	2,904.802***	1,590.604**	459.946	-180.311	170.663	1.713***	1.595***	1.284**	1.047*	0.359	0.826
	(1,100.934)	(914.711)	(768.413)	(1,095.161)	(1,503.770)	(2,072.821)	(0.475)	(0.488)	(0.502)	(0.536)	(0.555)	(0.543)
Observations	204	204	204	204	204	204	204	204	204	204	204	204
Mean of dependent variable in control group	594.438	179.646	19.190	-171.239	-358.512	-354.237	0.252	0.242	0.227	-0.380	-0.728	-0.714
R-squared	0.17	0.16	0.10	0.14	0.14	0.15	0.25	0.22	0.16	0.18	0.18	0.23
P-value: total effect of large transfer + savings default = 0	0.000	0.000	0.000	0.007	0.081	0.054	0.000	0.000	0.000	0.000	0.000	0.000
Transfer top vs bottom	0.050	0.242	0.678	0.658	0.836	0.970	0.622	0.553	0.690	0.812	0.886	0.964
Transfer_SD top vs bottom	0.092	0.021	0.812	0.903	0.775	0.554	0.441	0.029	0.035	0.064	0.422	0.117
T+T_SD top vs bottom	0.991	0.331	0.862	0.583	0.938	0.610	0.750	0.070	0.056	0.035	0.349	0.133

OLS regressions. All specifications include village and survey round fixed effects.
*** p<0.01, ** p<0.05, * p<0.1