

Income Timing, Savings Constraints, and Temptation Spending: Evidence from a Randomized Field Experiment

Lasse Brune and Jason T. Kerwin*

March 1, 2017

[Click here for the latest version of this paper](#)

Abstract

We study a savings technology that is popular but underutilized in developing countries: short-term deferred compensation, which replaces regular wage payments with a single, later lump sum. Workers who are randomly assigned to lump-sum payments spend 25% less of their income immediately and are five percentage points more likely to purchase an artificial investment. These effects are likely due to savings constraints: 72% of workers prefer deferred payments, and rationalizing workers' choices without savings constraints requires implausibly low discount factors. Although workers report temptation spending as an important driver of savings constraints, we find little evidence for that mechanism.

JEL Codes: D14, J33, O12, O16

Keywords: Savings Constraints, Financial Inclusion, Time Preference, Discounting, Labor Economics, Development Economics

*Brune: Department of Economics, Yale University, 27 Hillhouse Avenue, New Haven, CT 06511 (lasse.brune@yale.edu); Kerwin (corresponding author): Department of Applied Economics, University of Minnesota, 1994 Buford Avenue, St. Paul, MN 55108 (jkerwin@umn.edu). A previous version of this paper circulated under the title "Income Timing, Temptation and Expenditures: A Field Experiment in Malawi." We thank Ndema Longwe for outstanding fieldwork management, and Moffat Kayembe and Carl Bruessow from Mulanje Mountain Conservation Trust for their cooperation and guidance. Esperanza Martinez Maldonado provided excellent research assistance. We are grateful to Dean Yang, Mel Stephens, Charlie Brown, Steve Leider, Rebecca Thornton, Jeff Smith, David Lam, John DiNardo, Aditya Aladangady, Eric Chyn, Jay Coggins, Johannes Haushofer, Ben Meiselman, Terry Roe, and seminar participants at Michigan, Minnesota, Yale, IPA, and the Society of Labor Economics for helpful comments. We are grateful for research support from the IPA/Yale Savings and Payments Research Fund (funded by the Bill and Melinda Gates Foundation), the University of Michigan Population Studies Center, and the Michigan Institute for Teaching and Research in Economics. Kerwin's work on this study was supported in part by an NIA training grant to the Population Studies Center at the University of Michigan (T32 AG000221), as well as by fellowship funding from the Rackham Graduate School. This study is registered with the AEA RCT Registry under registration number AEARCTR-0000437. All errors and omissions are our own. [Click here](#) to access the online appendices to the paper.

People in developing countries invest extensive time and effort to match their often irregular and unreliable income streams to their desired consumption and savings goals. For many poor people, a particularly important cash flow management goal is generating usefully-large lump sums. These lump sums are needed to make indivisible expenditures, ranging from purchasing durable consumer goods, to buying in bulk, to making business investments (Collins et al., 2009). The importance of usefully-large lump sums also undergirds the broad-based interest in microcredit in developing countries.

This paper studies a savings method that is commonly demanded by developing-country workers but potentially underappreciated by employers and policymakers: deferred wages. Workers in developing countries often ask for some or all of their income to be deferred — held back from their regular pay and paid out as a lump sum on a later date. We use a randomized controlled trial to measure the effects of deferred wage payments for participants in a rural livelihoods program in Malawi that provides supplemental cash income during the agricultural offseason. Workers in the experiment are randomly assigned to receive their wages in either four weekly installments or a single lump sum at the end of four weeks. Our experiment includes 365 workers over two rounds, with the random assignments to weekly installments or lump sum payments cross-randomized across rounds.

Existing evidence suggests that developing-country workers commonly desire deferred payment schemes. Among our sample of workers, 72% state a preference for receiving all their wages at the end of a four-week period instead of payments once a week. Casaburi and Macchiavello (2015) show that dairy farmers in Kenya forgo up to 30% of their earnings to receive payments at the end of the month instead of immediately. More broadly, the rotating savings groups common across the developing world also serve to defer part of one’s income into a later lump sum (Besley, Coate and Louny, 1993).

Despite the evident demand for deferred wage payments in the developing world, previous research on their effects has been fairly limited. The only direct research on the topic that we are aware of is Beegle, Galasso and Goldberg (2015), which randomizes workers into receiving wages every three days compared to a 12-day single lump sum, with spending and saving outcomes measured about two to four weeks after the lump-sum payment. A closely-related study is Haushofer and Shapiro (2016), which focuses on windfall income rather than wages. Examining the effects of lump-sum payments between one and fourteen months after they are paid out, they find impacts on food security and cortisol levels, each of which improve by about a quarter of a standard deviation. They also find some effects on asset purchases, particularly iron sheets for roofing.

We contribute to this body of evidence by analyzing how deferred wage payments affect savings and consumption behavior in the very short term, ranging from the day of the income

receipt to a week later. Our surveys were collected with a recall period of no more than a week, and just one day for some outcomes. In addition, we exploit administrative data on purchases of an artificial “bond” that pays a high return with zero risk. The bond can only be purchased in lumpy tranches that require workers to generate sufficient liquidity at the time of purchase. Thus, the bond serves as an objective measure of both changes in the ability of workers to save up for a lumpy investments as well as the forgone returns from saving. These features of the data allow us to examine how deferred income payments affect expenditure and savings behavior in immediately after they occur, shedding light on the mechanisms by which any longer-run impacts could occur.

Workers who receive their wages as a deferred lump sum reduce the share of their income that they spend immediately by 25 percentage points (relative to a weekly-payment mean of 63%) and increase their cash holdings 2-5 days after the final payday by a third. The higher expenditure and lower cash holdings of the frequent payment group have real consequences: deferred lump-sum payments cause purchases of the high-interest bond to rise by five percentage points relative to a control-group mean of 10%. Crucially, our experiment holds time and transportation costs equal across study arms: workers in the lump-sum group still show up at the pay point on each payday, even when they are not receiving their wages. Thus the differences we measure are due to the timing of income rather than differences in time or transportation costs across study arms.

We argue that these results are most likely due to liquidity constraints, specifically savings constraints: the workers are unable or unwilling to hold on to cash due to internal or external factors that effectively tax any money they save (Karlan, Ratan and Zinman, 2014). This is consistent with the workers’ stated preferences: nearly three quarters of our sample stated a preference for deferred lump-sum payments over weekly installments under the conditions of our experiment, in which time and transportation costs are held equal. Moreover, the main treatment effects that we detect remain significant if we restrict our analysis to those workers who said they wanted deferred payments — although the small number of workers who did not want lump sums means that we cannot reject equal effects across the two groups. In addition to the evidence from the workers’ *stated* preferences, savings constraints also better fit workers’ *revealed* preferences from the data on purchases of the bond. In the absence of such constraints, given the high rate of return on the bond (33% over two weeks), workers would have to have implausibly low discount factors to justify the treatment effect of deferred wages on bond purchases.

A prominent explanation for savings constraints in the developing world is that workers face temptations to overspend on impulse purchases, particularly for alcohol, tobacco, or sweets. This “temptation spending” acts as an implicit tax on savings, reducing people’s

ability to save (Gul and Pesendorfer, 2001; Fudenberg and Levine, 2006; Ozdenoren, Salant and Silverman, 2012; Banerjee and Mullainathan, 2010). In line with this reasoning, workers in our sample commonly identify temptation spending as an important barrier to saving. In response to an open-ended question on reasons why workers “waste” money, temptation spending was the most common response, and was mentioned by over 40% of our sample. This accords with the growing role of temptation spending in analyzing financial decision-making in the developing world in contexts ranging from cash transfers (Haushofer and Shapiro, 2016) to microfinance (Banerjee et al., 2015) to banking (Ashraf, Karlan and Yin, 2006).

Our results on temptation spending do not provide strong evidence in favor of the typical temptation spending narrative. To examine the importance of temptation spending in driving savings constraints in our sample, we collect rich data on temptation spending — defining it not only using the standard definitions from the literature, but also by allowing the workers to categorize their own expenditures as wasteful or impulsive. This goes beyond the standard paternalistic approach, which defines specific goods such as alcohol or tobacco to be wasteful (Evans and Popova, 2014). By the expanded definitions respondents report wasting non-trivial amounts and — crucially — respondents’ own designations of temptation goods can differ sharply from those that would be chosen by a researcher. We analyze these measures using a number of complementary approaches and find limited evidence of a role for temptation spending.

One novel approach we employ is to cross-randomize worker’s exposure to temptation at the time they receive wages, which exploits the idea that workers with more temptation exposure should benefit more from the deferred lump sum. We attempt to induce variation in temptation by requiring some workers to pick up their pay during the major local market, which takes place weekly on Saturdays and is commonly identified by members of our sample as a highly-tempting environment.

Despite the tempting nature of markets, this second experiment does not induce substantial changes in temptation spending. Exposure to the market at the time of pay receipt also does not lead to substantive changes in other spending behavior, and does not alter the effect of deferred wages on bond purchases. We discuss several potential explanations for the lack of detectable effects from the market exposure treatment.

Our results suggest that lump sum payments do not cause large increases in temptation spending, and suggest that temptation spending plays a limited role in driving savings constraints among our sample of workers. These findings align with the results of cash transfer studies, which consistently show that cash transfers either decrease temptation spending or leave it unchanged (Evans and Popova, 2014), and also with previous research in Malawi,

which has found that recipients of a large cash windfall spent little on temptation goods (Brune et al., 2016).¹ Our study shows that altering the timing of income receipt does not substantially change that finding: the minimum detectable effect sizes on temptation spending for both our lump sum treatment and our Saturday payday treatment are less than 0.25 SDs, letting us rule out larger effects with reasonable confidence.

1 Theoretical Framework

We outline a simple theory of consumption choice in the presence of savings constraints to characterize how our two cross-randomized experiments shed light on the potential mechanisms for our results. We use this model to derive predictions for the behavior of expenditure and savings choices under savings constraints. A conventional consumption-smoothing model specifies total utility as the discounted sum of period-specific utility $u_t(\cdot)$:

$$U(c_0, \dots, c_T) = \sum_{t=0}^T \delta^t u_t(c_t)$$

where the marginal utility of current-period consumption is positive and diminishing: $u'_t(\cdot) > 0$, $u''_t(\cdot) < 0$. Agents maximize utility by picking values of c_t for each t , subject to an intertemporal budget constraint that relates assets A_t , income y_t , and consumption c_t across periods:

$$A_{t+1} = R(A_t + y_t - c_t)$$

The first order condition yields the standard Euler equation that relates the marginal utility of current consumption to the marginal utility of future consumption:

$$u'_t(c_t) = \delta R u'_{t+1}(c_{t+1})$$

Here R , taken as fixed, is the gross interest rate, $1 + r$, faced by a specific individual.

¹ These results are notably different from the literature on lump-sum payments in developed countries, which typically finds that lump sum payments cause decreases in recipients' ability to save and increases in potentially-wasteful spending (Stephens Jr., 2003; Shapiro, 2005; Hastings and Washington, 2010; Michelmore and Jones, 2015). This could be due to people in developed countries facing a more-tempting environment, or because they face fewer savings constraints and thus have fewer beneficial investments available to spend the lump sum payments on.

Taking their lifetime pool of resources (from income and assets) as fixed, the agent equalizes the marginal benefit of current-period consumption with its cost in terms of foregone consumption in the future. Circumstances that raise the marginal utility of consumption in the future — such as an opportunity to purchase a valuable indivisible good — will increase $u'_{t+1}(c_{t+1})$ relative to $u'_t(c_t)$. This will tend to drive up savings and hence drive down current consumption.

A simple modification of the model to admit savings constraints is to allow r to be negative and hence R to be less than one (Blattman et al., 2016). Pushing the gross interest rate R down will decrease the value of $u'_{t+1}(c_{t+1})$ at the optimum, which means that the choice of current-period consumption is revised upward. Thus all else equal, a savings constraint, which pushes R below one, will lead to more current consumption and less future consumption. In contexts like ours where people consume a large share of what they purchase immediately, the same conclusion will hold for expenditures as well.

Negative net interest rates are a very real possibility across much of Africa. In much of the developing world poor face steep fees to save, whether formally or informally (Collins et al., 2009). Dupas et al. (2016) found that among unbanked Malawians who took up fee-free bank accounts, the usual fees would have exceeded interest payments for 95% of the population. Even if people save informally, they may be subject to kin taxes or theft (Goldberg, 2011; Jakiela and Ozier, 2016), and may face transaction costs to put money into hiding places. These factors can also combine to generate negative interest rates.

Consider the case where agents would have positive demand for savings at a positive interest rate. If they are savings-constrained ($R < 1$), then a technology that allows them to move money into the future will effectively increase R . Such a technology will be desirable to the agent, and will lead to an increase of optimal current period savings. In our experiment, paying workers' wages in a single, deferred, lump-sum payment raises the nominal interest rate from a large negative number to zero, and raises the real interest rate from a large negative number to a small one.²

A model of savings constraints thus makes two testable predictions for the workers in our experiment:

1. Introducing deferred wages will tend to shift expenditure into the future.
2. Workers would opt in to a (costless) technology that allows them to shift money from the present into the future.
 - (a) Workers will prefer the lump-sum deferred payment of the supplemental income that the scheme provides over weekly installments.

²The real interest rate remains slightly negative due to inflation.

- (b) They will also choose other contracts or financial products that easily allow money to be shifted to a later date.

Prediction 2 does not hold for a model that has credit constraints but no savings constraints. Credit-constrained workers will still respond to the introduction of deferred wages by decreasing current expenditures (matching Prediction 1), because they are unable to borrow against their future wages. However, this is not a desirable outcome for them: they will not prefer deferred wages, and demand for other technologies that move their income into the future will be low. The desirability of deferred wages for savings-constrained workers (but not for credit-constrained ones) is a crucial point that we will return to in section 5.1 when we discuss the mechanisms behind our results.

Beyond examining the impact of deferred wages on savings and expenditure, and the role of savings constraints in driving those effects, another goal of this paper is to understand the extent to which those constraints arise due to temptation spending. An extensive theoretical literature has examined how self-control problems can impede saving (Thaler and Shefrin, 1981; Laibson, 1997; O’Donoghue and Rabin, 1999; Bernheim, Ray and Yeltekin, 2015). Several papers have shown that these self-control problems can arise as a result of temptation spending (Gul and Pesendorfer, 2001; Fudenberg and Levine, 2006; Ozdenoren, Salant and Silverman, 2012). Banerjee and Mullainathan (2010) formally model the effect of temptation on intertemporal choice as a tax on saving, leading to a negative interest rate in the Euler equation. Intuitively, if people save money now, some of that will be wasted on temptation goods by their future selves. Since temptation goods are valued only in the moment, and not ahead of time, that money is as good as wasted.

In both Fudenberg and Levine (2006) and Banerjee and Mullainathan (2010), self-control problems tax savings in two ways. First, self-control problems directly lead to wasteful spending now, reducing the pool of resources available for savings. Second, sophisticated individuals respond strategically to this waste by cutting back on savings, to reduce the amount of money available for their future self to waste.

Thus the interest rate can be expressed as a function of both current- and next-period temptation, $R = R(\tau_t, \tau_{t+1})$. R depends on temptation in the following way:

1. $\frac{\partial R}{\partial \tau_t} < 0$ if people face temptation-based savings constraints
2. $\frac{\partial R}{\partial \tau_{t+1}} < 0$ if people face temptation-based savings constraints and are sophisticated about them

Our second experimental manipulation focuses on the first of these effects, which does not require workers to be sophisticated about their temptation spending. We operationalize

changes in temptation by exposing people to a more- and less-tempting environment when they receive their pay. Thus we are directly varying τ_t , under the assumption that the temptations people face only matter if they have cash on hand. We will return to this assumption when we discuss our results; one possible reason for the limited variation in temptation induced by our experiment is that the amount of cash people have on hand either does not vary much or is not important for the degree of temptation they face.

Conditional on varying the exposure to temptation that people face, a model of temptation-based savings constraints predicts that increases in τ_t have two observable effects. First, higher levels of τ_t should lead to more total spending and less saving. Second, higher levels of τ_t may interact with savings devices that allow people to shift money into the future — like our lump-sum deferred-wage treatment. Since the deferred wages lump sum arrives during the same period as workers are exposed to temptation, the deferred wage treatment does not directly address the savings constraints imposed by temptation exposure by varying the effective interest rate. However, some models of temptation and savings constraints imply that a greater degree of cash on hand should enhance people’s ability to resist temptation (Banerjee and Mullainathan, 2010). This implies that the deferred wages lump-sum payment will mitigate the negative impact of exposure to temptation. An alternative prediction comes from models in which income can “burn a hole in your pocket” (Gul and Pesendorfer, 2001). In this case, a higher levels of cash on hand will lead to more waste, and so the deferred wages treatment will exacerbate the negative impact of temptation exposure on savings.

2 Study Design

To understand the impact of deferred wages on short-run savings and consumption, and test the implications of temptation-based savings constraints, we designed a field experiment that randomly varied the timing of wage payments for a set of workers in Malawi. The work was part of a cash income generation program organized by the Mulanje Mountain Conservation Trust (MMCT), a local NGO in the Mulanje District of Malawi’s Southern Region. The program offered temporary informal employment opportunities during the agricultural offseason when incomes are low.³

The experiment was organized into two rounds that occurred over a period of three months from November 2013 to January 2014. An initial sample of 350 workers were recruited into the study for round 1 and an additional 15 workers were added for round 2 to replace

³ Recruitment into the study followed the partner NGO’s standard recruitment procedures for this program, which focuses on poor households. For details on the choice of study location and worker recruitment, see Appendix A.1.

workers who dropped out after round 1. All workers were interviewed in a baseline survey.⁴

Each subject worked for two weeks during each round of the project, for about four days per week, at a daily wage rate of MK400 (USD \$2.50). The wage rate was set at the national minimum wage, and corresponds to about 160% of average daily spending for our workers. Workers were employed in conservation-oriented activities that promoted the sustainable use of natural resources.⁵

Workers received identical nominal wages for their work, but were randomly assigned, independently by round, to receive their pay with different timing.⁶ Workers received their pay either in weekly installments beginning at the end of the second week of work or in a single, deferred lump sum, about three weeks after the last day of work (i.e. about four weeks after the end of the first work week).

During the week after the last payday in each round, all workers were visited for a detailed survey about their expenditure and income. Figure 1 shows the timing of the different components of the experiment. It illustrates both when the two rounds of work and payments took place and when we conducted the different rounds of data collection.

In addition to variation in payment frequency, workers received their pay either on Fridays or on Saturdays. Since the payments were made at the site of a major local market that is open on Saturdays, this additional variation was intended to induce variation in how tempting workers' environments were *at the time of receipt of wages*. The two variations in the timing of pay — weekly vs. lump sum and Friday vs. Saturday — were cross-randomized, creating four study arms in each round. Hence each round of work was followed by eight paydays: two per week for four weeks, starting on the Friday and Saturday immediately following the end of the work period.

Subjects were informed about how they would be receiving their pay (weekly or in a lump sum, Fridays or Saturdays) at the beginning of each round of work; each worker had a fixed pay schedule for each round. The procedure was explained verbally and they were also given a simple handout explaining their group assignment.

To ensure that transit and time costs were held equal across the four study arms, all subjects were required to come to the payroll site on all eight paydays during each round — even when they were not being paid their wages. In order to encourage attendance and defray workers' time costs, all subjects received an MK100 show-up stipend for each day,

⁴ For a full description of worker selection, attrition, and replacement, see Appendix A.2.

⁵ See Appendix A.3 for descriptions of the work activities undertaken as part of the project.

⁶ The official inflation rate in Malawi was about 23% per annum during the study period (https://www.rbm.mw/inflation_rates_detailed.aspx). Each round of payments in the study took one month, so consumer price increases would have reduced the value of income by at most 1.7% per round. We therefore ignore the distinction between nominal and real wages in our analysis.

on top of any money they were slated to receive as part of their pay for the project. For example, a person who was paid in a lump sum on Fridays was required to come to the trading center on all the preceding Fridays and Saturdays, and received MK100 on each of those days; on the day she received her pay, she received MK100 plus her entire wages for the project.

Table 1 summarizes the payment schedule in each round across the four payday weekends resulting from the show-up stipends and wage disbursements according to study arm. The lump sum payment (excluding the MK100 show-up stipend) was MK2800 in round one and MK3200 in round two; the weekly payments were one quarter of that amount. Total wages were higher in round 2 since there were 7 work days during the first round and 8 days during the second.

We employed a within-person cross-randomized design in order to maximize statistical power. Individuals were randomly assigned to one study arm in the first round of the study and then to another study arm (potentially the same one) for the second round.⁷ The randomization for both rounds of the study was done prior to the baseline survey, but the group assignments were not revealed to the workers until the beginning of each round of work. For the first round, the randomized assignment was stratified by village and gender. The randomization for round 2 was then stratified on the round 1 assignment and village. The first column Table 1 shows the number of workers in each study arm for each round of the study. To improve statistical power, in our analysis we generally pool observations across rounds and cross-randomized treatments.

3 Data

Our data comes from three sources: a detailed survey, focused on expenditures in the past week; several single-item recall questions asked during the payroll; and, as an objective measure of savings behaviors, respondents' choices about purchasing an artificial, indivisible investment offered by the project at the end of each study round.

3.1 Survey Data

We surveyed workers three times: once at baseline and two midline surveys, one after each round of the study. The midline surveys began on the Monday immediately following the last payday of each round, and the order in which respondents were visited for the surveys

⁷ This within-worker repeat-randomization design has the potential for generating order effects, in which a worker's past treatment status changes their responses to the treatment. In Appendix D we examine the round two data for order effects based on the round one assignments, and find no evidence for this issue.

was randomized by village. Table 1 shows the days covered by the surveys for each round of the study.

Subjects were interviewed at their homes, and answered questions about income, physical assets, savings, and financial transfers, as well as a detailed module about their expenditures since the previous Friday, which was the first day of the final payday weekend.

The total amount of money spent on the itemized list of goods is a measure of spending since the final payday weekend, but is limited in an important way: this list of goods was not exhaustive, but instead focused on purchases that were likely to be common. The items that were excluded tended to be either rare consumption goods or high-value, infrequent purchases. At the end of each broad category of goods, there was an “other” option, which was intended to be used for these other items. However, we discovered after the data was collected that this field was almost never used by the enumerators. In particular, for the “other items” category, which covers durable goods, the “other” option was used on just 0.6% of the follow-up surveys. As a result this variable is likely to omit major asset purchases, a potentially-important component of expenditures. These omissions are likely to work against finding effects of the lump sum treatment, since they will tend to be large purchases that would be easier to afford if people have access to a lump sum. We therefore include this variable in our analyses, but do not compute expenditure shares by category of good, since the denominator is mismeasured.

3.2 High-Frequency Payday Survey Panel

Our second data source is a set of questions asked during the payroll process. On each of the eight paydays, all respondents were required to come to the payroll site as described above. Prior to receiving their wage payments or show-up stipends, they were asked simple aggregate questions about the money they had on them at the time (not including their pay, which they had yet to receive) and the amount of money they spent at the trading center on the previous payday. Hence on Fridays, people were asked about the money they spent on the Saturday of the previous week, and on Saturdays, they were asked about the money they spent yesterday. During the second round of the study, we also asked two additional questions as sensitivity checks: first, we asked people to recall their spending from the Friday of the previous week, to look at the influence of recall bias. Second, we asked people about money they spent outside of the trading center, in case there were differential patterns by the location of the spending. We find evidence of small amounts of differential recall bias, but it does not drive our main findings. There is no evidence that recording market spending, as opposed to total spending, is important. See Appendix C.1 for details of these checks.

3.3 Artificial “Bond” Sales

A third source of data comes from purchases of an artificial “bond” offered to respondents at the end of each round of the study. Respondents were offered the chance to buy the bond only once per round, immediately after we visited them for the midline survey for the round in question. The bond could only be purchased in indivisible shares that cost MK1,500 to purchase and that paid back the principal plus MK500 interest after exactly two weeks. Each respondent could buy a maximum of two shares, and no fractional shares were allowed. All respondents who purchased the bond were paid back on time according to the terms of the investment. The investment good was intentionally offered only once per round, in the week after the final payment was made. This allows us to use it to test for the existence of savings constraints, since members of the weekly group had to save their pay in order to use it for this high-return savings vehicle. An alternative design would have been to offer the investment opportunity each week. This would have lowered the amount of time that the weekly group needed to save in order to purchase it, thus relaxing the savings constraint somewhat. We chose the single-offer design in order to maximize our statistical power to detect differences across the two groups.

The timing of the bond sales was identical across rounds of the study, but the timing of the announcement differed by round. In round one it was announced after payments had begun, just one week before the bonds were made available for sale. In round two it was announced prior to the beginning of the payments, and before workers knew their treatment status for that round. Figure 1 shows the timing of the announcement of the bond opportunity in each round of the study.

3.4 Sample Balance and Summary Statistics

Table 2 shows summary statistics and balance tests for basic demographics and baseline values for our main outcome variables as well as an index of asset holdings (which is used as a control in the main results tables). About one third of the sample is male, the majority of respondents are married and the average age is just over 40 years. Respondents have about three and a half years of completed schooling. We conduct two types of standard balance tests. The p -values in column 7 are from tests of equal means in treatment and control. The p -value in the last row of the table is from a test that the twelve baseline covariates do not jointly predict treatment status. The two treatment groups are balanced: we cannot reject the equality of covariate means separately ($p > 0.20$) nor that baseline covariates do not

jointly predict treatment status ($p = 0.62$).⁸

4 Empirical Strategy

Our main analyses focus on examining the mean effects of the two cross-randomized experiments on savings and expenditures. To do this we estimate regressions of the following form:

$$Y_{ir} = \alpha + \beta T_{ir} + \boldsymbol{\gamma}' \mathbf{X}_{ir} + \varepsilon_{ir} \quad (1)$$

Y_{ir} is the outcome of interest for worker i in round r .⁹ T_{ir} is an indicator variable for individual-level assignment to the treatment group. We analyze two different treatments: receiving one's wages in a deferred lump-sum payment, and receiving one's wages on Saturday. The definition of the treatment indicator varies by table in our results, but the rest of the specification is unchanged. \mathbf{X}_{ir} is a vector of controls that includes stratification cell dummies, two household financial variables measured prior to the randomized assignment (an index of physical asset and livestock ownership using principal component analysis and total spending out of income received since the past Friday), indicators for the day-of-week of the exogenously-assigned (first attempted) interview date, and (if available) baseline values of the outcome variable.¹⁰ ε_{ir} is a mean-zero error term.

Whenever we use pooled data from both rounds, we cluster standard errors at the worker level to account for the statistical dependence of outcome measures for the same individual across the two rounds. The stratification cells are defined separately by round and thus control for round fixed effects whenever the analysis includes multiple rounds.

The workers in our sample do interact with each other, so we cannot rule out the possibility that workers assigned to one experimental group had an impact on workers in another; since only one person from each household was eligible to participate, we can rule out any within-household spillovers. Our design does not allow us to address potential spillovers of effects from one study arm to another. In the context of our design, spillovers will most likely

⁸ Two appendix tables repeat the same summary statistics and balance tests separately by round (Appendix Table B.1) and for the second cross-randomized experiment that assigned workers to either a Friday or Saturday payday (Appendix Table B.2). The tables show comparable means and show balance of baseline covariates across the respective groups.

⁹ We Winsorize all outcome variables at the 1st and 99th percentiles.

¹⁰ We dummy out any missing values of the controls. None of our results are sensitive to the specific choice of baseline financial controls or to the inclusion of the controls for stratification cells; see Appendix E for regression results without controls.

bias our estimated effects toward zero: for example, if monthly payment group members gave loans to weekly payment group members, this should reduce any differences in expenditures between the two groups. Additionally, we find no empirical evidence of increased cash or in-kind transfers for any of the experimental groups (Table 5, Columns 5 and 6).

We supplement our main regressions with two other analyses. First, we estimate the effects of the deferred lump-sum wage payments on various quantiles of the outcome distribution. We do this by separately estimating the conditional quantile equivalent of equation 1 for each ventile from 0.05 to 0.95, and then combining the results into a single graph. Second, we examine how the treatment effect varies by several important baseline characteristics. We do this by breaking each baseline characteristic W_i into J brackets with associated indicator variables W_{ij} , and then including those indicators and their interactions with the treatment indicator in the regression:

$$Y_{ir} = \alpha + \sum_{j=1}^J [\beta_j T_{ir} W_{ij} + \delta_j W_{ij}] + \gamma' \mathbf{X}_{ir} + \varepsilon_{ir} \quad (2)$$

Here β_j is the treatment effect experienced by workers whose baseline values of W fall into bracket j .

5 The Impact of Lump Sum Wage Payments on Expenditure and Saving

The deferred lump-sum wage payments induce large variations in the timing of workers' expenditures. Figure 2 shows the randomized variation in cash income from the experiment's work program (Panel A) and the resulting variation in workers' mean expenditures on the same day they received their pay (Panel B). The average worker in the deferred wage treatment spends nearly MK2,000 on the last payday weekend, whereas the average control-group worker spends just over MK1,000. While the pattern of expenditures resembles that of income, workers are not literally living hand-to-mouth on a daily basis. Panel C shows the spending on the payday weekends as a share of the income received from the work program. Treatment-group workers spend more than what they earn on the first three weekends (when they are only getting paid the MK100 show-up stipend) but spend barely 50% of their earnings on the last payday. In contrast, control-group workers spend nearly all their income as they receive it.

Table 3 presents regression estimates of equation 1, examining the effect of deferred wage

payments on expenditures using different data sources. Columns 1-5 focus on the payday survey panel for which workers were asked very short questions while receiving their pay. Workers who are paid in a lump sum spend MK1,297 less on the first three weekends (when they receive only the MK100 show-up stipend) and MK786 more on the fourth weekend (when they receive their lump sum wage payment). This means that total expenditures on eight Fridays and Saturdays observed in the payday panel fall by MK517.

Notably, the deferred wage treatment causes the share of income spent immediately — on the same day that the money is received — to fall by 24 percentage points, or nearly 40% of the control-group mean (column 5). This suggests that treatment-group workers should have higher short-run cash holdings. Quantile regression results reveal that the treatment effects are consistently negative across the distribution of the share of income spent immediately, and somewhat larger in magnitude for the middle quantiles (see Panel A of Appendix Figure F.1).

The changes in income and spending in the payday panel are also evident in the in-depth survey, which covers only the last weekend. Workers paid in a deferred lump sum report receiving an additional MK1,656 in income since the payday (Table 3 column 6), relative to the control group. This is somewhat less than the difference in what workers were paid, and could imply that control-group workers pursue other income-generation opportunities that treatment workers do not pursue in expectation of the lump-sum transfer. Consistent with that theory, control-group workers received MK2,309 in total cash income — more than double what they were paid in the final weekend of each round.

Expenditures also increase compared to the control group. The itemized expenditure data reveal an MK365 increase in spending (column 8); these data do not include some durables (see discussion in Section 3.1 for more details) and so this effect is likely to be a lower bound. The quantile regression results for expenditures largely mirror those for the share of income spent immediately: there is a fairly uniform increase in spending across the distribution that is somewhat higher for the middle quantiles (Appendix Figure F.1, Panel B).

The in-depth survey data also suggests an increase in cash holdings by the treatment group. Out of the income they had received since Friday, they report retaining MK145 more than the control group — a 30% increase relative to the control group (Table 3, column 7).¹¹ Appendix Figure F.1, Panel C shows how the change in remaining cash-on-hand varies across the quantiles of the outcome variable distribution. Since nearly half of treatment-

¹¹ Since remaining cash holdings is not necessarily equal to the change savings, and since the expenditure variable, as noted above, is missing certain large purchases, column 6 does not equal the sum of columns 7 and 8.

group workers (and more than half of the control group) report that they have none of the cash left, the changes are concentrated toward the top of the distribution.¹² These effects are consistent with the model in Section 1, where savings constraints imply negative interest rates. Workers who receive weekly payments would have to save their income for several weeks, compared to just a few days for the deferred lump-sum payment, paying a negative effective interest rate for a longer period. This reduces their cash holdings relative to the lump sum group in two ways. First, they are likely to choose to save less. Second, any amount they do save will be attenuated due to the negative interest rate they must pay. Practically, this could mean losses due to theft, transfers to relatives, and temptation spending.

In Table 4 we look for evidence of these changes in savings for other financial outcomes, but note that we are underpowered to detect anything but very large effects on most of these outcomes. Based on our standard error estimates, we would have 80% statistical power only to detect changes of 40 to 80% of the control-group means for all of the financial outcomes in the table. Such large effects seem implausibly high for outcomes like total asset purchases or total loans, and arguably for transfers as well.¹³

Because of our study’s lower power to detect effects on many financial outcomes (given the common problem of noisy survey measures for these types of outcomes) our experiment also embedded an additional administrative data source for changes in savings and investment. During the in-depth surveys, workers were offered the opportunity to buy zero-risk “bonds” in each round that paid a 33% return after two weeks. Bonds could be bought only in tranches of MK1,500, with a maximum of two tranches per worker. In Table 5 we present the impact of the lump sum wage payments on purchases of these bonds. Column 1 shows effects on an indicator for purchasing the bond, while Column 2 shows effects on the total amount spent on the bonds. Columns 3 and 4 repeat the remaining cash on hand and total income since Friday results from Table 3. The overall take-up of the bond is fairly low, at just 10% in the control group. This may seem puzzling considering the high interest rate, but it is consistent with previous research on the purchase of investments in rural Africa. Carter, Laajaj and Yang (2015), for example, offer farmers in Mozambique the opportunity to save money at a 50% match rate — an even higher return than our bond — but find very limited impacts of this offer on consumption or savings.

The deferred wages treatment leads to large increases in expenditures on the bonds, which

¹² This survey question covers only the cash they have received since the previous Friday, not total cash holdings.

¹³ “Transfers received” does show a decline of MK 172 ($p < 0.1$). Incoming transfers could well have decreased for the treatment group if they were in a position to ask for fewer or smaller transfers from their social network (or if their social network deemed their requests less worthy of consideration given the recent influx of cash).

are reflected in the cash-on-hand variable. This suggests that much of the remaining money was being held in order to purchase bonds. The estimated impacts are highly heterogeneous by round, however — they are statistically significant overall, and when we restrict the sample to round two, but not when we restrict the sample to round one.

These differences in effects potentially reflect differences in the setup of the bond product in each round. In round one the bond was only announced in the week preceding the final payday. This means that the workers did not know about the investment opportunity until a week before it was made available to them. This could make our estimated treatment effect either larger or smaller than it would have been with more advance notice. We may have seen a smaller treatment effect with more notice because the weekly payment group members did not know about this opportunity until they had received three-quarters of their total wages. The wage amount remaining to be paid in the last payday weekend was smaller than the minimum required amount for the investment opportunity; the remaining weekly payment was MK800, while one unit of the investment offer was priced at MK1,500. This would eliminate the subset of weekly workers who had less than MK700 available in savings or other income from being able to purchase the investment good. In contrast, we could have seen a larger effect with more notice because lump sum payment group members may have already committed their pay to other expenditures. This would limit their ability to purchase the investment good, thus understating any measured effects.

In round two the investment opportunity was announced before the work began for the round. All workers across both groups knew they would have the opportunity to purchase the bond, prior to learning which payment group they were in. Workers therefore had advance notice of the prospect of this opportunity before any wage payments began, and before they could potentially commit any of their wages to other expenditures in a way that depended on their study arm assignment.

Workers might also have had greater trust in the product in round two. The bond was offered as a zero-risk product, and in both rounds 100% of payments were made on time and as originally agreed, so in round two workers would know that the project had paid as agreed. To the extent that workers initially lacked trust in the bond, the second-round results could be a more realistic representation of the response to a risk-free investment.

Another reason for differences in results across rounds is that the underlying intervention was also different: in round one of the study, workers were employed for only seven days, as opposed to eight days in round two. This means that the weekly payments were MK700 in round one and MK800 in round two, and the monthly payments were MK2800 in round one and MK3200 in round two, so the size of the lump-sum treatment was MK300 larger in round two. The effect of the lump sum on income receipt since Friday could reflect these

differences: Column 4 of Table 5 shows that the treatment effect was MK746 larger in round two than in round one, a difference that is significant at the 0.05 level.

The gap is even larger than the expected MK300 difference, and could reflect an overall tightening in the availability of other cash income because the second round of the study occurred further into the annual lean season. Outside income for the control group falls by one third between rounds: in round one the control group had received MK2604 since the previous Friday, or MK1504 more than what they were paid as part of the study; this figure fell to MK2010 in round two, which is MK1010 more than the amount they received through the experiment.

The differences in the timing and experimental intervention mean that the lump-sum treatment was over 50% stronger in round two than in round one. Since the bond is indivisible by design, it is possible that the intervention in round one was not strong enough to induce any additional purchases — especially because the effect of the lump sum treatment on income receipt was only MK1301 in round one, which is less than the MK1500 minimum cost of purchasing a bond. In round two the lump sum treatment raises income received since Friday by MK2047, easily clearing the MK1500 threshold for purchasing the bond.

Because of these differences in the setup of the bond and the intensity of the treatment across rounds, our preferred estimates come from the pooled regression (Panel A), which shows that the treatment increases bond uptake by 5 percentage points over a control-group mean of 10% — a 49% increase. These results are driven by round two, where overall take-up is lower — which could suggest that experience with the bond causes people to dislike it, or that the bond is more popular overall when it is a surprise. However, we cannot draw any strong conclusions from this pattern because of general seasonal variations in behavior, most importantly the fact that round two was further into the lean season, reducing the availability of cash and leading to overall lower spending levels.

5.1 Evidence for Savings Constraints

We argue that the increase in investment caused by the deferred wages — as measured through the bond purchases — is the result of savings constraints rather than credit constraints. In principle, a credit-constrained worker might respond in the same way that we observe. Suppose that workers were unable to borrow against their future earnings and would prefer to consume all the income they earned smoothly over time. Workers in the deferred wages group would be forced into very low consumption during the first three weeks, and then have more income than they want to spend in week four. They would thus be left with more cash on hand in week four; not needing all of it, they might invest in the bond offered

by the project.

There are two separate pieces of evidence that line up against that proposition, and in favor of an explanation based on savings constraints. First, the majority of workers state that they prefer the deferred lump-sum wage payments over the weekly installment payments. At baseline, we asked an (unincentivized) question about which payment structure they would prefer. The question imposed the same rules as the actual study: everyone had to show up on all four paydays regardless of when they actually got the money.

Under these conditions, 74% of workers preferred the deferred, lump-sum payments. Moreover, our treatment effects on the bond purchases remain statistically significant when we restrict our sample to the 74% of workers who preferred the lump-sum payments (we cannot reject equal effects for the remaining 26%). These choices are inconsistent with binding credit constraints: workers who would prefer to smooth, and would only save as a result of the deferred wage payments because they cannot borrow against them, would never volunteer to defer their income. Doing so would only decrease their welfare by pushing them away from their optimal time path of consumption.

Our second piece of evidence in favor of savings constraints comes from workers' revealed preferences based on the bond purchases. To justify the low rates of takeup of the bond, workers would need to have implausibly low discount factors. In the absence of savings constraints, a worker faces the following stylized maximization problem:

$$\max_{c_1, c_2} u(c_1 + w) + \delta u(c_2 + w) \quad (3)$$

Here w is some underlying level of income that we assume to be constant irrespective of the worker's choice; for simplicity we assume this income level is the same in both periods. c_t is the transient income the worker receives as part of the study. The bond offer creates a discrete choice between consuming MK1,500 of transient income now ($c_1 = 1500, c_2 = 0$) or choosing MK2,000 of income two weeks in the future ($c_1 = 0, c_2 = 2000$). δ is the biweekly (14-day) discount factor. We ignore any other savings technology, which will have a lower return than the 33% available via the bond.

A worker who chooses to forgo purchasing the bond reveals that

$$u(1500 + w) + \delta u(w) > u(w) + \delta u(2000 + w) \quad (4)$$

To solve for the implied value of δ , we impose a CRRA functional form for the period-specific utility function: $u(c_t + w) = \frac{(c_t + w)^{1-\rho}}{1-\rho}$. Thus we can solve for an upper bound on

δ ,¹⁴

$$\delta < \frac{(1500 + w)^{1-\rho} - (w)^{1-\rho}}{(2000 + w)^{1-\rho} - (w)^{1-\rho}} \quad (5)$$

We can determine plausible values for δ by using known values for w and ρ . We draw on our own survey data for w . As estimates of the plausible range of values that our workers might have for their regular income streams (excluding the bond), we use the 10th, 50th, and 90th percentiles of the endline income distribution (MK900, MK3,000, and MK5,000 respectively). For ρ we draw on the estimates of [Balakrishnan, Haushofer and Jakiela \(2015\)](#), who compute utility function parameters using a lab experiment in Kenya; we test both the lowest and highest value of ρ measured in their study (0.578 and 1.42 respectively), as well as the median estimate (0.978).

Table 6 presents the results of this calibration exercise. The highest value of δ we can compute via this exercise is 0.87 for the two-week time horizon in question. This implies an annual discount factor of 0.026 — or nearly zero. Interpreting our results as arising from simple unconstrained utility maximization thus implies that workers place nearly zero value on events that happen more than a year in the future.

One way of explaining the low values we estimate is to assume that the workers' non-experiment income will rise substantially over the next two weeks. Large increases in income seem implausible, however. Our project took place during a season where incomes typically decline steadily until the harvest, which happens around May.

A useful point of comparison for these results comes from [Balakrishnan, Haushofer and Jakiela \(2015\)](#), who also compute daily discount rates. Their median daily discount rate estimate is 0.994, which implies a biweekly discount factor of 0.92 and an annual discount factor of 0.11. This is over forty times higher than our median estimate, and over four times higher than any of the annual discount factors we estimate. Indeed, their *lowest* annual discount factor is 0.036, which is 40% higher than our *highest* estimate. Even relative to the high levels of impatience observed elsewhere in Africa, these figures are implausibly small. The result are even more striking relative to the typical discount factors used in the US, which are around 95%, or a 5% discount rate.

Savings constraints provide an appealing alternative to these low discount rates: participants fail to purchase the bond at the moment that it is offered not because they would not want to, but because they cannot. They either lack the available liquidity to purchase it at all, or they are up against a minimum consumption constraint. Given the additional income

¹⁴ For details of the derivation, see Appendix G.

provided to all workers in our sample by the experiment, all workers should have the aggregate income needed to buy two shares of the investment. Thus the failure of most workers to buy the bond is apparently at odds with their stated preference for deferred lump-sum wages: why would workers prefer later lump-sum income payments at a zero interest rate but not a positive interest rate? This seeming contradiction can be explained if workers face a savings constraint. Workers who are paid weekly would have to save much of their income to buy the bond. If they are constrained in their ability to save, they will not be able to do that.¹⁵

Our survey data is also consistent with important savings constraints. Just 4% of our sample reports ever saving money at a bank at baseline. The predominant mode of savings for our sample of workers is to hide money at home, which is used by 80% of them. The second-most prevalent option, used by 13% of workers, is to use a rotating savings club. Both of these are quite risky: the former carries the chance of losing money to theft, fire, or family members, while the latter runs the risk of having another club member fail to contribute or disappear entirely.

The deferred lump-sum wage payments relax that constraint, making it substantially easier for workers to have the necessary liquidity on hand to buy the bond. Savings constraints can also explain the low levels of take-up among the treatment group: while the treatment reduces the period of time that workers need to save, it does not make it zero, because the bonds were not sold until the in-depth surveys. The typical worker in our treatment group had to hold onto his pay for 4.5 days to buy the bond. If the interest rate is sufficiently negative, that could be a long enough period to make purchasing the bond too costly to be attractive for a majority of treatment-group workers. Lending credibility to this explanation is that the (randomly-assigned) delay between the payday and the bond offer is a strong predictor of take-up even within the lump sum study arm. We thus take these discount rate calculations as additional evidence that our results are being driven by savings constraints.

6 The Role of Temptation Spending in Driving Savings Constraints

Where do the savings constraints that bind the workers in our study come from? Our experiment was designed to test one specific source of such potential constraints: temptation spending, which can act as a tax on saving and drive effective interest rates below zero.

¹⁵ Another explanation is that the workers in our sample are extremely present-biased. Present bias common in this context (Giné et al., 2016). However, most models of present-biased behavior imply that consumers will be savings-constrained (e.g. (Laibson, 1997; Banerjee and Mullainathan, 2010)).

6.1 Measures of Temptation Spending

A crucial question for our analysis is how to define temptation spending. [Evans and Popova \(2014\)](#) note that temptation goods are typically defined by researchers to include goods that are commonly perceived as harmful. The most common choices are alcohol and tobacco, but high-calorie savory and sweet foods are sometimes included as well. Temptation spending is money “wasted” by the poor on things that policymakers would prefer they not buy.

This approach presumes that that perfectly competent adults cannot be trusted to make their own decisions, and that policymakers or people in other countries could do better on their behalf. At the same time, the poor very commonly identify categories of spending that they wish to reduce, and the expenditures that they — like most consumers — most often identify as problematic are alcohol and tobacco ([Banerjee and Duflo, 2007](#)).

Motivated by [Banerjee and Duflo’s](#) findings, we take a loosely revealed-preference approach to categorizing temptation spending. We allow respondents to identify categories of expenditure that they themselves see as problematic, and compute the share of all expenditure that is deemed to be temptation spending. Our preferred approach is to ask people about goods that they are tempted into purchasing and match those categories to detailed survey data on actual expenditures. This allows us to classify purchases as temptation goods based on people’s own perceptions of goods that are problematic purchases. Our household surveys include three different definitions of temptation goods: 1) purchases that the respondent commonly regrets after the fact; 2) goods that are commonly unplanned purchases; and 3) goods that the respondent is tempted into purchasing that they should not buy or that are wastes of money. For each respondent, we match the goods that they personally deem to be problematic with itemized lists of purchases they have made since the previous Friday, also from the household survey. We do this separately for the three definitions above.

We also use two other self-reports of temptation spending. The first is simply the respondents’ own recall of the total amount of money they wasted. For the second, we ask, for every good in the itemized list, whether the purchase was planned beforehand, an approach first developed by [Brune et al. \(2016\)](#). Unplanned purchases are taken to be temptation spending in this case. The English translations of the exact survey questions we used for respondent self-reports of temptation spending are shown in Appendix Table C.2.

We supplement these subjective self-judgments of temptation goods with two objective measures drawn from the previous literature. First, following [Evans and Popova \(2014\)](#), we consider purchases of alcohol and tobacco to be temptation spending. Second, we use an expanded version of their definition, by including all goods that are mentioned as temptation good in the studies they summarize and that also appear in our surveys’ itemized lists of purchases. For each of these objective, paternalistic definitions, we follow the same procedure

described above – we match them to our itemized lists of purchases and compute total expenditures.

Table 7 presents the various definitions of temptation spending. The recorded level of temptation spending varies significantly based on the definition we employ. Moreover, the various measures are only weakly correlated with one another: the only correlation coefficient that exceeds 0.25 is between “Alcohol and Tobacco” (Row D) and “Alcohol, Tobacco, Doughnuts, and Soda” (Row E) — an artifact of the overlapping definitions.

Our preferred measures of temptation spending are purchases of goods the workers say they often waste money on (Row C, “Waste/Temptation”) and self-reported aggregate money wasted (Row F, “Money wasted”). That is because, first, regrets (Row A) and unplanned purchases (Row G) often capture other mistakes and deviations from plans that are not conceptually equivalent to being tempted into wasting money. For example, our workers often report regrets due to price fluctuations or quality — they recognize *ex post* that they overpaid for something. Unplanned purchases can result from a similar pattern: if something is available at a bargain price then people may deviate from plans and purchase it, but this is the result of re-optimization, not a mistake.

Second, the common researcher-imposed definitions of temptation spending (Rows D and E) miss important categories of goods that the workers in our sample report being tempted into purchasing. These include fried meat (which is often available from vendors during market days) and clothing (both for personal use or as gifts to family members).

The preferred measures show non-trivial average levels of temptation spending — 3% of average income for Row C and 10% for Row F — and also have higher variances than the other measures.¹⁶

Although we think the definitions on rows D and F are the best measures of temptation spending, we utilize all seven definitions to avoid any potential issues of cherry-picking. We report our main analyses for each definition. We also focus on a combined index of temptation spending. We do this by taking the first principal component of the seven individual temptation measures for the control (weekly payment) group, constructing predicted values for the entire sample, and normalizing to the control group. Since one of the seven measures (total money wasted) was collected only in round 2 of the study, we construct the index two ways: one that includes all seven outcomes but is only computed for round 2, and one that excludes the “total money wasted” variable and is computed for both rounds.

¹⁶ Average income in the time span covered by the survey was about MK3000 (see Appendix Table B.3, Panel C).

6.2 Deferred lump sum wage payments and temptation spending

If paying workers in deferred lump sums is helping relax a temptation-based savings constraint, this should appear in the data in the form of changes in expenditures on temptation goods. We examine this in Table 8. The data show no evidence of consistent effects of the lump sum treatment on temptation spending. While Columns 3 and 4 show statistically-significant increases, the combined indices show no change, suggesting that those effects are the result of multiple comparisons. Based on the estimated standard errors in column 1, the minimal detectable effect size on temptation spending (at 80% power) lets us rule out effects of larger than 0.2 SDs with reasonable confidence.

Overall expenditure does increase as a result of the deferred wage treatment (Table 3). The null effects in Table 8 thus suggest that the *share* of temptation spending may have gone down. We are unable to explore this, however, because the measures in Table 3 that cover the same time period as our temptation spending variables are missing certain key components of overall expenditure (in particular durables, as discussed in Section 3.1). These results are consistent with the theory that the lump sum payments increased savings by reducing temptation spending, but do not decisively point to this conclusion.

6.3 Heterogeneity in the impacts of deferred lump sum wage payments

Another implication of temptation-based savings constraints is that the impact of the deferred wage treatment should be larger for people who face stronger constraints. We look for this kind of heterogeneity in our results by estimating equation 2, using present bias and temptation spending as our baseline variables of interest.

We do not observe consistent patterns of heterogeneity by baseline present bias in the effect of the treatment on buying the bond (Figure 3, Panel A). Turning to heterogeneity by baseline temptation spending (Figure 3, Panels B and C), there is some evidence that workers with higher baseline temptation spending have larger treatment effects for bond purchases and larger treatment-induced decreases in endline temptation spending.

Overall, our analyses of treatment effect heterogeneity do not support a conventional model of behavioral savings constraints due to present bias as in Laibson (1997) or O'Donoghue and Rabin (1999), but do suggest that there may be a role for temptation spending. Our evidence about the former pattern is somewhat limited due to the fairly noisy point estimates for each subgroup, and so we cannot conclusively rule out a role for present bias either.

6.4 Experimental evidence on temptation spending and savings constraints

To study the causal effect of temptation spending on savings behavior, our study embedded a second experimental treatment that was cross-randomized against the deferred wage treatment. Workers were assigned to receive their wages either on Saturday, during the major local market day (treatment) or on Friday, the day before in the same location (control). We did this to induce variation in workers’ exposure to temptation goods while they had cash on hand. All workers had to show up at the payroll site on both days each week, even when they were not receiving their pay, and everyone received an MK100 show-up stipend on top of their wages as compensation.¹⁷ The goal of this second treatment was to induce random variation in the temptingness of the participants’ environment. If money is received in a tempting environment, like the local market day, this should increase the cost of resisting those temptations and increase temptation spending.

6.4.1 Market days as a source of temptation

We chose market days as the tempting environment for our study based on extensive qualitative and descriptive work with people in the local area. Weekly market days are common across rural Africa. Markets in Malawi are held at trading centers that contain a few fixed businesses and have a large number of spaces for other vendors to come in and sell additional goods on the market day. In the local area where we ran the experiment, there are seven of these trading centers, and typically each one holds two market days per week. Market days are often the only feasible option for people living in rural Malawi to buy common consumption goods. These days tend to offer a fairly stark contrast to ordinary days in rural Malawi. They are typically lively, noisy affairs with many goods on offer, presenting environments that try to tempt consumers into spending their money. Anecdotally, people in Mulanje District often describe market days as tempting situations, in which excitement can cause them to purchase things they would rather not.

Our survey data (Appendix Table H.1) confirms that people find markets tempting: for a free-response question about situations that are tempting or in which respondents may waste money, 37% of all respondents volunteered market days as a tempting situation, by far the most common response (Panel A).¹⁸ Multiple-choice questions (Panel B) show the same

¹⁷ Compliance with this requirement was very high. On the average payday, over 95% of workers were present, and 82% of workers did not miss a single payday.

¹⁸ Since 39% of respondents said they were never tempted, this constituted 58% of people who believe they ever waste money. The next-most frequent answer was “Going to the trading center in general (not just market days)” with 4% mentioning it. The exact phrasing of the question in English was “In general, what

pattern: 69% of people said that market days are more tempting than the day before market days, and 65% of people said having a lot of cash on hand at the trading center was more tempting than having it on hand elsewhere.

These answers suggest that payments during market days could exacerbate temptation-based psychological savings constraints, by inducing people to spend money on tempting goods that they would prefer to save. Panel D confirms that markets are an important part of life in the area, with the typical person reporting going to the market six times in the past month. Saturdays are the most common days that people visit the market (32% of all visits), although other trading centers do hold market days on Fridays and so 26% of visits happen on Fridays.¹⁹

We compare payments during the market day to payments at the same site the day before, when the market does not take place. We chose the day before — Friday — as the alternate day for several reasons. First, it was logistically simpler to manage payments on two consecutive days than on non-adjacent ones; Sunday was not an option because the vast majority of our sample goes to church on Sunday mornings. Second, using the day before the market ensured that all respondents had the liquid cash needed to make purchases at the market — if we had paid the control group on a later day, then for the first week they would not have had any money to spend at the market on Saturday. Third, and most important, if the control group was paid after the Saturday group, then any differences in savings could simply be a function of having to hang on to the money for a shorter period. By choosing Friday as the control group, we ensured that any such effects worked against the expected direction of the results.

The location and timing of the payroll was specifically chosen to maximize the likelihood that people would be exposed to temptation goods. The market at Mwanamulanje happens only on Wednesdays and Saturdays (with Saturdays having the larger market out of the two days), and principally in the morning, which is when people were paid. Shops are still open on Fridays, and there are some mobile vendors, but the majority of market activity happens on Saturdays.

are situations in which you waste money or are tempted to spend money that you would rather not spend?” The term used in the local language has a less judgmental connotation than “waste” does in American English.

¹⁹ Some of these Friday trips to the market could be non-market-day visits to the Mwanamulanje Trading Centre where we made our payments; the phrasing of the survey question does not allow us to distinguish between the two.

6.4.2 The impact of being paid during the market on temptation spending

Table 9 shows the effect of the Saturday payday treatment on our key outcomes. Panel A examines treatment effects just for workers in the lump-sum treatment group, since our main goal with this treatment is to examine how the exposure to temptation affects the savings constraints suggested by that intervention. Panel B shows pooled results across both workers paid weekly and workers paid in deferred lump sums. There are no substantive differences across the two approaches, so our discussion focuses on Panel A.²⁰

The Saturday payday treatment leads to large shifts in the exact timing of expenditure. Column 1 shows that total spending on Friday and Saturday drops by MK756 for the Saturday treatment group relative to the workers who are paid on Friday. In the presence of liquidity constraints, this is to be expected: workers paid on Friday have had an additional day to spend their income. Taking into account the difference in income timing, the Saturday treatment induces no meaningful changes in total expenditure: workers spend a similar amount immediately upon receiving their income (Columns 2 and 3) and have statistically indistinguishable total income, remaining cash holdings, and total spending between the previous Friday and the survey date.²¹

Crucially, the Saturday payments induce no appreciable changes in temptation spending. The estimates in Column 7 are statistically insignificant, but they do not allow us to rule out changes smaller than 0.32 SDs in magnitude. Unsurprisingly, given the limited effects on other outcomes, we also see no treatment effects on purchases of the bond.

There are three potential reasons why the Saturday payday treatment may not have produced detectable effects on temptation spending and savings behavior. One is that the effects could be real and simply be smaller than what we have statistical power to detect. Based on our estimated standard errors, we have 80% power to detect 7 percentage point changes in bond purchases and 0.35 SD changes in temptation spending. Another related reason is that the effects may be very small — or even zero.

The third potential reason is that people might substitute toward other temptation spending opportunities. Our treatment was designed around the market at Mwanamulanje Trading Centre, which operates on Saturdays (with a smaller market day on Wednesdays). However, there are a number of other nearby trading centers that do have market days on Fridays. It is possible that the workers who are assigned to a low-temptation environment on payday (Friday at Mwanamulanje) simply substitute toward other sources of temptation, such as the

²⁰ Appendix Table I.1 shows the fully-interacted specification, confirming that there are no differential effects of Saturday payments by lump-sum treatment status.

²¹ We also examined treatment effects on all the other outcomes analyzed in our main regression analyses of the lump sum treatment (Tables 3, 4, 5, and 8) and found no evidence of effects on any of them.

market days happening elsewhere. Another form of substitution may be over time instead of across space: if workers primarily save by holding cash on their persons, then workers who are paid on Fridays may simply hold onto the cash and face the same temptations as those paid on Saturdays.

This explanation helps reconcile our results with the workers' own evaluations of markets as being extremely tempting, and with the fairly high levels of temptation spending we observe (1-10% of total expenditure, depending on the definition we use).²² At the same time, if people substitute toward other temptation spending opportunities, then temptation spending is conceptually quite different from how it is typically conceived in economic models. It is hard to reconcile the active seeking of temptations with dual-self style theoretical frameworks in which temptations are valued only by the instantaneous, current self.

Overall these results imply a fairly limited role for temptation spending in driving the savings constraints we observe among our sample of workers. Lump sum wage payments relax the relevant savings constraint but do not have appreciable impacts on temptation spending — and direct exposure to a tempting environment affects neither temptation nor expenditure.

7 Conclusion

Financial markets in developing countries are imperfect. People adapt to these imperfections in numerous ways, developing informal arrangements and institutions to replace formal structures that are either flawed or missing entirely. This paper analyzes one such adaptation — structuring wage payments so that they are received in later, concentrated lump sums instead of smooth streams.

We show that deferred wage payments lead to meaningful increases in short-term savings, and reduce the share of income spent immediately. These results, which are based on survey data, have several important limitations. Our itemized purchase data systematically undermeasures expenditures because it omits large asset purchases, and our key cash on hand variable does not necessarily capture a change in savings. We also cannot rule out the possibility that the lump sum payments had direct effects on how workers reported or recalled expenditures or savings, by focusing their attention differently. We are able to mitigate these limitations using the data on purchases of an artificial “bond,” which do not have suffer from mismeasurement or recall bias.

The bond sales data confirms that workers save significantly more as a result of being

²² These expenditure shares are estimates at best, and probably lower bounds, because our measures of total expenditure are incomplete.

paid in a lump sum. Equivalently, workers who were paid frequently — the control group — are more likely forgo the high return bond purchase than workers who are paid all at once at a later date. This is important, because it suggests that consumers in this context face costly savings constraints.

Where do these savings constraints come from? We explore a prominent theory, which states that they arise from temptation — which can tax savings and causes self-control problems. We find only limited evidence to support this theory. Our treatment effects are somewhat larger for workers who spend more (at baseline) on temptation goods. But an experimental intervention that attempted to test this theory by exposing some workers to a more-tempting environment on payday did not affect temptation spending nor any of our major outcomes. Because it effectively lacked a first stage, this second randomized experiment cannot shed additional light on the role of temptation spending in driving savings constraints in this context.

The second experiment suggests that the exact context in which workers are paid may not be an important consideration for designing payment systems. However, we cannot reject that the environment in which people are paid *ever* matters. Our experiment took place in seven villages around one particular trading center in Malawi. In this setting, other trading centers with complementary market days — e.g. ones that take place on Fridays, when the payday trading center’s market was not occurring — are within 30 minutes’ travel. In other settings in which there are no complementary nearby market days, the day and location of payment may matter more. Nevertheless, the setting of our study is fairly typical for many rural areas in Malawi and other countries sub-Saharan Africa, where there are very often trading centers whose market days cover most days of the week, located within distances that can be traveled in reasonable times. Thus, the findings of our study suggest that the specific day of income receipt is not a major driver of spending decisions in a broad range of settings in rural Africa.

Our core findings center on workers’ purchases of the bond that was offered as part of the project. While this investment was artificial, it was designed to match key features of other common investments and major purchases in developing Africa. It is indivisible, much like school fees, government-subsidized bags of fertilizer, and common home improvements like corrugated iron roofing sheets. Its cost was also calibrated to be similar to that of school fees. Thus the large effects we see on bond purchases suggest that there could be significant payoffs to restructuring the timing of wage payments and cash transfers across similar contexts elsewhere in Africa.

The effects we see on bond purchases, and the fact that nearly three-quarters of our respondents prefer deferred lump sums prior to the study, suggest that organizations should

pay at least part of wages or cash transfers in deferred lump sums as a form of commitment savings — or at least offer people that payment structure as an option. A closely-related alternative would be to offer workers the ability to choose the arrival date of lump sums to coincide with indivisible expenditures like school fees and inputs for planting season. This change in timing would potentially bring considerable benefits at relatively little cost. Indeed, organizing payouts just once a month could even be cheaper for the paying organization than paying weekly. Our study finds no significant downsides to lump sum payments even when they are received during one of the most tempting environments that people typically experience in rural Africa.

Our results are specific to the type of income stream faced by the workers in our sample, who are earning irregular, one-off income. Other types of workers will not necessarily respond in the same way. For example, factory workers, who receive regular income over the entire year are less likely to want to shift money into the future, since income arrives for them on a predictable biweekly or monthly basis. The value of deferred wages for such workers is beyond the scope of this paper. However, the income stream faced by our sample of workers is extremely common across the developing world, where most people are farmers and seek additional cash income during the agricultural low season. As a result, our findings are likely to generalize to a wide variety of important settings across the world.

The results in this paper also provide several lessons for future research on savings, as well as on the role of self-control problems in driving savings constraints. First, people are aware of the self-control problems they face, and thus survey questions that directly ask people about temptation and wasteful spending are a useful way to measure people’s self-control issues. Second, offering study participants a meaningful investment opportunity that bears actual interest can be a helpful way to isolate an intervention’s effects on savings constraints. Other outcomes have two important limitations: non-financial investments such as health and education may not be perceived as investments by respondents, and heterogeneity in returns may generate misleading inferences about the extent of savings constraints. Third, to the extent that self-control problems are generating internal savings constraints in rural Africa, they may not be particularly amenable to policy interventions. Receiving one’s pay during the market — a context commonly listed as being tempting by the respondents in our study — generated only small variations in their level of self-reported wasteful spending, possibly because people continue to select into other tempting situations. This suggests that other causes of savings constraints may merit further research.

References

- Ashraf, N., D. Karlan, and W. Yin. 2006. “Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines.” *Quarterly Journal of Economics*, 121(2): 635–672.
- Balakrishnan, Uttara, Johannes Haushofer, and Pamela Jakiela. 2015. “How Soon Is Now? Evidence of Present Bias from Convex Time Budget Experiments.” Working Paper.
- Banerjee, Abhijit, Esther Duflo, Rachel Glennerster, and Cynthia Kinnan. 2015. “The Miracle of Microfinance? Evidence from a Randomized Evaluation.” *American Economic Journal: Applied Economics*, 7(1): 22–53.
- Banerjee, Abhijit V., and Sendhil Mullainathan. 2010. “The Shape of Temptation: Implications for the Economic Lives of the Poor.” NBER Working Paper No. 15973, Cambridge, MA.
- Banerjee, A. V, and E. Duflo. 2007. “The economic lives of the poor.” *Journal of Economic Perspectives*, 21(1): 141.
- Beegle, Kathleen, Emanuela Galasso, and Jessica Goldberg. 2015. “Direct and Indirect Effects of Malawi’s Public Works Program on Food Security.” Working Paper.
- Bernheim, B. Douglas, Debraj Ray, and Şevin Yeltekin. 2015. “Poverty and Self-Control.” *Econometrica*, 83(5): 1877–1911.
- Besley, Timothy, Stephen Coate, and Glenn Loury. 1993. “The Economics of Rotating Savings and Credit Associations.” *American Economic Review*, 83(4): 792–810.
- Blattman, Christopher, Eric P. Green, Julian Jamison, M. Christian Lehmann, and Jeannie Annan. 2016. “The Returns to Microenterprise Support among the Ultra-poor: A Field Experiment in Postwar Uganda.” *American Economic Journal: Applied Economics*, 8(2): 35–64.
- Brune, Lasse, Xavier Giné, Jessica Goldberg, and Dean Yang. 2016. “Savings Defaults and Payment Delays for Cash Transfers: Field Experimental Evidence from Malawi.” World Bank Policy Research Working Paper WPS7807.
- Carter, Michael R., Rachid Laajaj, and Dean Yang. 2015. “Raising Returns, Managing Risk: A Randomized Experiment on Combining Input Subsidies with Financial Services Interventions.” Working Paper.

- Casaburi, Lorenzo, and Rocco Macchiavello.** 2015. “Market Anomalies Under Saving Constraints: Evidence from the Kenya Dairy Industry.” Stanford University Working Paper.
- Collins, Daryl, Jonathan Morduch, Stuart Rutherford, and Orlanda Ruthven.** 2009. *Portfolios of the Poor, How the World’s Poor Live on \$2 a Day*. New Jersey: Princeton University Press.
- Dupas, Pascaline, Dean Karlan, Jonathan Robinson, and Diego Ubfal.** 2016. “Banking the Unbanked? Evidence from three countries.” National Bureau of Economic Research Working Paper 22463.
- Evans, David, and Anna Popova.** 2014. “Cash transfers and temptation goods: a review of global evidence.” World Bank Policy Research Working Paper 6886.
- Fudenberg, Drew, and David K. Levine.** 2006. “A Dual-Self Model of Impulse Control.” *American Economic Review*, 96(5): 1449–1476.
- Giné, Xavier, Jessica Goldberg, Dan Silverman, and Dean Yang.** 2016. “Revising commitments: Field evidence on the adjustment of prior choices.” *The Economic Journal*, in press.
- Goldberg, Jessica.** 2011. “The lesser of two evils: The roles of social pressure and impatience in consumption decisions.” University of Michigan Working Paper.
- Gul, Faruk, and Wolfgang Pesendorfer.** 2001. “Temptation and self-control.” *Econometrica*, 69(6): 1403–1435.
- Hastings, Justine, and Ebonya Washington.** 2010. “The First of the Month Effect: Consumer Behavior and Store Responses.” *American Economic Journal: Economic Policy*, 2(2): 142–62.
- Haushofer, Johannes, and Jeremy Shapiro.** 2016. “The Short-term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya.” *Quarterly Journal of Economics*, 131(4): 1973–2042.
- Jakiela, Pamela, and Owen Ozier.** 2016. “Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies.” *Review of Economic Studies*, 83(1): 231–268.
- Karlan, Dean, Aishwarya Ratan, and Jonathan Zinman.** 2014. “Savings by and for the Poor: A Research Review and Agenda.” *Review of Income and Wealth*, 60(1): 36–78.

- Laibson, David.** 1997. “Golden Eggs and Hyperbolic Discounting.” *Quarterly Journal of Economics*, 112(2): 443–478.
- Micheltmore, Katherine, and Lauren Jones.** 2015. “Timing is Money: Does Lump-Sum Payment of Tax Credits Induce High-Cost Borrowing?” Working Paper, University of Michigan.
- O’Donoghue, Ted, and Matthew Rabin.** 1999. “Doing It Now or Later.” *American Economic Review*, 89(1): 103–124.
- Ozdenoren, Emre, Stephen W. Salant, and Dan Silverman.** 2012. “Willpower and the Optimal Control of Visceral Urges.” *Journal of the European Economic Association*, 10(2): 342–368.
- Shapiro, Jesse M.** 2005. “Is there a daily discount rate? Evidence from the food stamp nutrition cycle.” *Journal of Public Economics*, 89(2–3): 303–325.
- Stephens Jr., Melvin.** 2003. “‘3rd of the Month’: Do Social Security Recipients Smooth Consumption Between Checks?” *American Economic Review*, 93(1): 406–422.
- Thaler, Richard H., and Hersh M. Shefrin.** 1981. “An Economic Theory of Self-Control.” *Journal of Political Economy*, 89(2): 392–406.

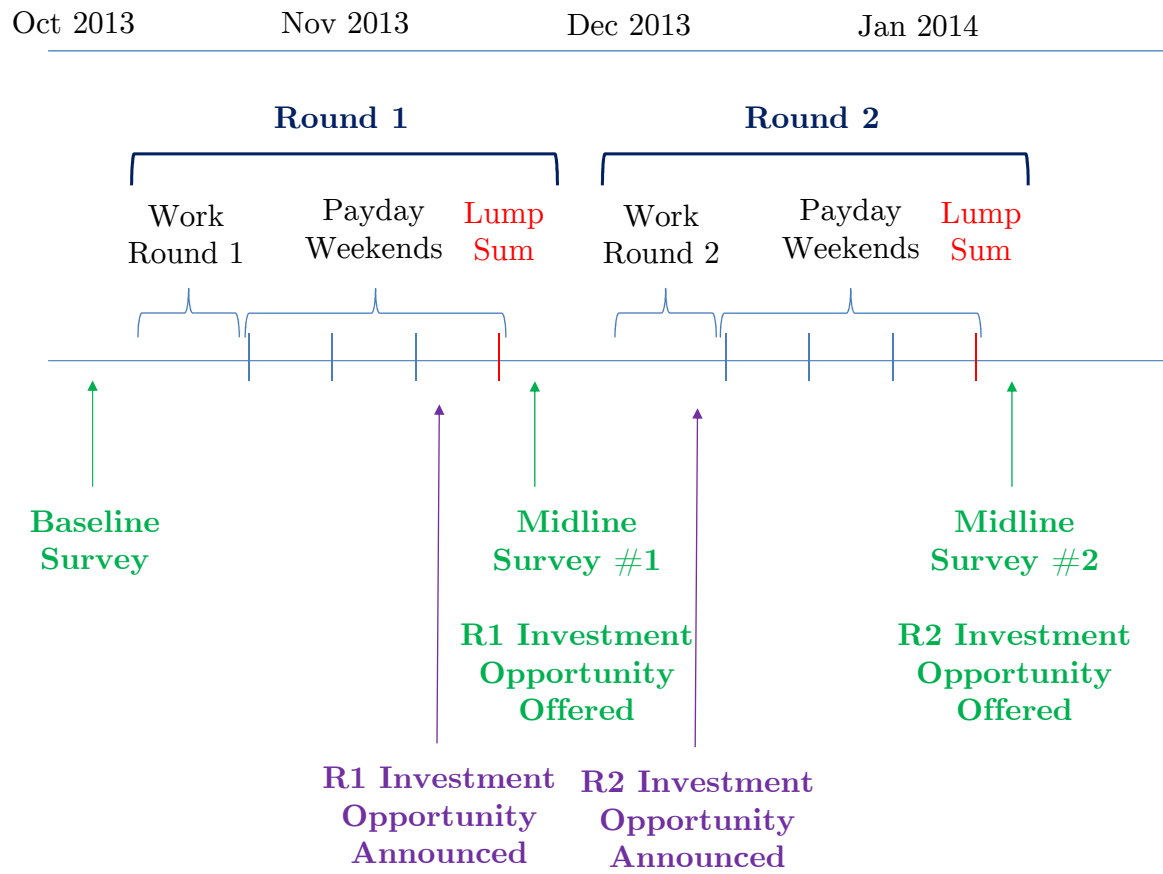
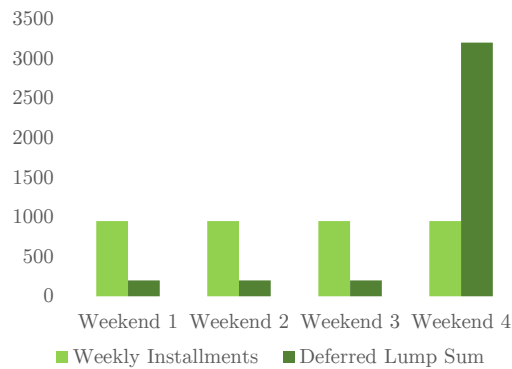
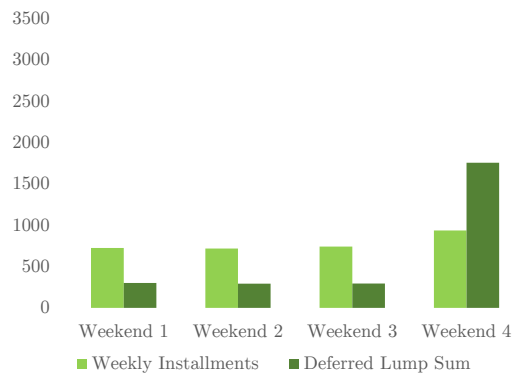


Figure 1
Timing of work, payments and data collection

Panel A: Wage payments on payday
(MK)



Panel B: Expenditures on payday
(MK)



Panel C: Expenditures on payday as share of wage payments

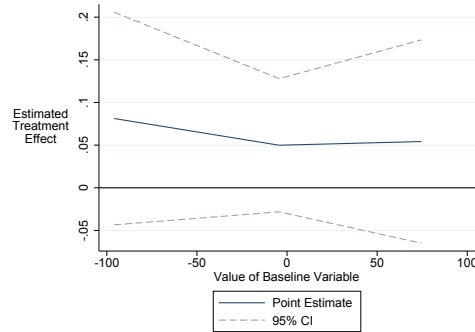


Figure 2

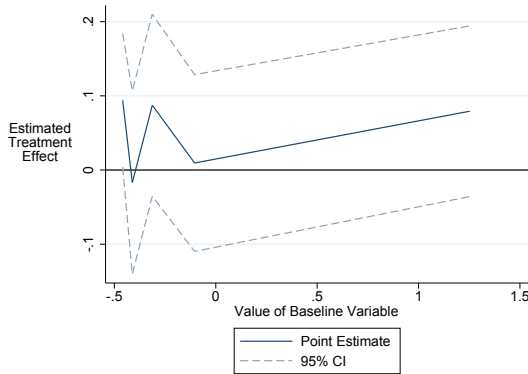
Wage payments and expenditures by payday weekend

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have payday data for that round. Income data comes from project records, based on workers' assigned treatment arms. Expenditure data comes from questions administered during payroll that asked workers about their expenditure on the previous payday (the day before for Saturday payments, the previous Saturday for Friday payments). During the study period the market exchange rate was approximately MK400 to the US dollar, and the PPP exchange rate was approximately MK160 to the US dollar.

Panel A: Effect of treatment on
1(Bought any shares)
by baseline present bias



Panel B: Effect of treatment on
1(Bought any shares)
by baseline PCA temptation index



Panel C: Effect of treatment on
endline PCA temptation index
by baseline PCA temptation index

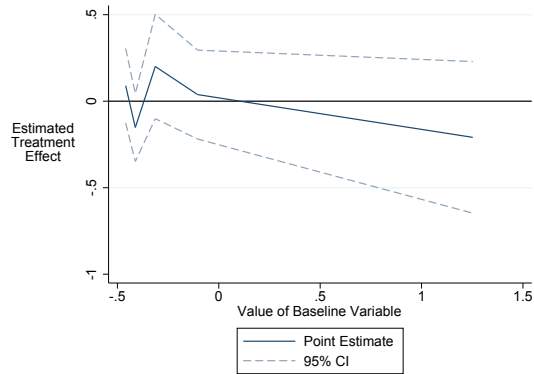


Figure 3

Heterogeneity in lump sum treatment effect
by baseline present bias and temptation spending
(both rounds pooled)

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. All regressions control for stratification cell fixed effects, an index of baseline asset ownership based on first principal components, indicators for the number of days after the weekend the interview occurred, baseline total spending and, if available, the baseline value of the outcome variable. For details of the empirical strategy see section 4, and for complete variable definitions see Appendix C. Heteroskedasticity-robust 95% confidence intervals, clustered by worker, indicated using dashed lines.

Table 1
Timing of wage payments and follow-up surveys

	N	Payment amounts (MK)								Sun	Mon	Tue	Wed	Thu	Fri
		Weekend #1		Weekend #2		Weekend #3		Weekend #4							
		Fri	Sat	Fri	Sat	Fri	Sat	Fri	Sat						
Panel A: Round 1															
Weekly Payments, Friday	84	800	100	800	100	800	100	800	100						
Weekly Payments, Saturday	89	100	800	100	800	100	800	100	800						
Lump Sum Payment, Friday	87	100	100	100	100	100	100	2,900	100						
Lump Sum Payment, Saturday	83	100	100	100	100	100	100	100	2,900						
	$\Sigma=343$														
Panel B: Round 2															
Weekly Payments, Friday	85	900	100	900	100	900	100	900	100						
Weekly Payments, Saturday	86	100	900	100	900	100	900	100	900						
Lump Sum Payment, Friday	90	100	100	100	100	100	100	3,300	100						
Lump Sum Payment, Saturday	85	100	100	100	100	100	100	100	3,300						
	$\Sigma=346$														

Notes: Sample includes 359 workers who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). All money amounts are in Malawian Kwacha (MK); during the study period the market exchange rate was approximately MK400 to the US dollar, and the PPP exchange rate was approximately MK160 to the US dollar.

Table 2
Balance of baseline variables

	Control group:			Treatment group:			Balance test <i>p</i> -value
	Weekly payments			Lump sum payments			
	Mean	SD	N	Mean	SD	N	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Demographics</i>							
Male	0.30	0.46	347	0.34	0.48	349	0.279
Married	0.72	0.45	347	0.69	0.46	349	0.296
Age (Years)	40.6	16.0	347	41.3	16.4	349	0.741
Years of Education Completed	3.7	3.4	347	3.8	3.5	349	0.757
<i>Financial outcomes (in units of MK unless noted)</i>							
Income received since past Friday	3,400	6,336	347	3,202	5,900	349	0.206
Remaining cash holdings out of income received	835	2,438	347	816	2,517	349	0.209
Total spending since Friday	4,118	5,473	347	4,254	5,539	349	0.768
Asset Ownership (PCA)	-0.07	2.67	341	0.08	2.72	340	0.613
Loans received in past month	3,192	9,005	347	4,125	11,092	349	0.367
Loans made in past month	729	1,944	347	768	2,047	349	0.777
Transfers received in past month	1,063	2,544	347	1,057	2,471	349	0.435
Transfers made in past month	654	2,015	347	888	2,439	349	0.374
<i>p</i> -value from joint significance of 12 covariates:			0.62				

Notes: Sample includes 359 workers who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). All money amounts are in Malawian Kwacha (MK); during the study period the market exchange rate was approximately MK400 to the US dollar, and the PPP exchange rate was approximately MK160 to the US dollar. Asset index is constructed by taking the first principal component of all asset variables and is normalized to have a mean of zero. For complete variable definitions see Appendix C.

Calculations based on pooled data set with observations at the worker-round level, from workers who have any follow-up data. All variables denominated in MK are Winsorized at the ninety-ninth and first percentiles to control outliers. The *p*-values in column 7 are from a test that the treatment indicator is zero in a OLS regressions of baseline covariates on an indicator for treatment plus stratification cell fixed effects and using heteroskedasticity-robust standard errors, clustered at the worker level.

Table 3

Effects of lump sum payments on expenditure levels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Payday survey panel - Spending at market on the four payday weekends					Household survey data		
<u>Dependent variable:</u>	Amount spent on Friday and Saturday (MK)		Amount spent on payday (MK)	(Spending on payday)/ (Income received)	Income received since last Friday (MK)	Remaining cash out of income received since last Friday (MK)	Total spending [†] since Friday from itemized expenditure data (MK)	
	Weekends 1-3	Weekend 4	All Weekends					
Lump sum treatment	-1,297*** (73.09)	786.3*** (90.51)	-517.4*** (127.8)	-940.8*** (82.41)	-0.243*** (0.0254)	1,656*** (172.7)	145.3** (71.57)	365.3** (153.3)
Dependent variable mean, control group (weekly payments)	2,180	936.4	3,126	2,138	0.630	2,309	468.5	2,962
Number of observations	689	689	689	689	689	689	689	689

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). Regressions are run on pooled data from round 1 and round 2. 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. All regressions control for stratification cell fixed effects, an index of baseline asset ownership based on first principal components, indicators for the number of days after the weekend the interview occurred, baseline total spending and (if available) the baseline value of the outcome variable. For details of the empirical strategy see section 4, and for complete variable definitions see Appendix C. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses.

† Itemized expenditure data does not include all purchases, and so these estimates are likely to be a lower bound; see Section 3.1 for details.

Table 4

Effects of lump sum payments on asset accumulation, loans, and transfers

	(1)	(2)	(3)	(4)	(5)
<u>Dependent variable:</u>	Value of net asset purchases in past two months (MK)	Loans received in past month (MK)	Loans made in past month (MK)	Transfers received in past month (MK)	Transfers made in past month (MK)
Lump sum treatment	-176.1 (527.6)	-119.3 (365.8)	-144.3 (118.0)	-171.6* (101.8)	-30.15 (44.48)
Dependent variable mean, control group (weekly payments)	2,271	2,008	596.2	688.9	249.7
Number of observations	689	689	689	689	689

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. Regressions are run on pooled data from round 1 and round 2. All regressions control for stratification cell fixed effects, an index of baseline asset ownership based on first principal components, indicators for the number of days after the weekend the interview occurred, baseline total spending and (if available) the baseline value of the outcome variable. Asset purchases are measured since the previous survey, a period of approximately two months. Loans are measured since November 1st in round 1 and since January 1st in round 2, a period of approximately one month. Transfers are measured over the month leading up to the survey interview. For details of the empirical strategy see section 4, and for complete variable definitions see Appendix C. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses.

Table 5
Effects of lump sum payments on purchases of risk-free, high-return “bond”

	(1)	(2)	(3)	(4)
	Administrative data on bond sales		Survey data	
<u>Dependent variable:</u>	1(Bought any shares)	Total spent on shares (MK)	Remaining cash out of income received since last Friday (MK)	Income received since last Friday (MK)
<hr/>				
<u>Panel A - Round 1 and 2 pooled</u>				
Lump sum treatment	0.0508** (0.0250)	124.7** (59.88)	145.3** (71.57)	1,656*** (172.7)
Dependent variable mean, control group (weekly payments)	0.108	226.7	468.5	2,309
Number of observations	689	689	689	689
 <u>Panel B - Round 1 only</u>				
Lump sum treatment	0.0101 (0.0385)	62.15 (81.63)	39.37 (108.3)	1,301*** (281.5)
Dependent variable mean, control group (weekly payments)	0.150	277.5	543.0	2,604
Number of observations	343	343	343	343
 <u>Panel C - Round 2 only</u>				
Lump sum treatment	0.0918*** (0.0330)	188.9** (85.86)	258.7** (101.0)	2,047*** (207.4)
Dependent variable mean, control group (weekly payments)	0.0643	175.4	393.1	2,010
Number of observations	346	346	346	346

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. All regressions control for stratification cell fixed effects, an index of baseline asset ownership based on first principal components, indicators for the number of days after the weekend the interview occurred, baseline total spending and (if available) the baseline value of the outcome variable. For details of the empirical strategy see section 4, and for complete variable definitions see Appendix C. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses.

Table 6

Upper-bound discount factors for workers who do not purchase the “bond”

Coefficient of Relative Risk Aversion (ρ)	Underlying Income Level (w)	Upper-Bound Discount Factor (δ)	
		Biweekly	Annual
0.578	900	0.80	0.003
0.578	3000	0.78	0.001
0.578	5000	0.77	0.001
0.978	900	0.84	0.010
0.978	3000	0.79	0.002
0.978	5000	0.78	0.002
1.417	900	0.87	0.026
1.417	3000	0.81	0.004
1.417	5000	0.79	0.002

Notes: Upper-bound values of the 14-day discount factor are calculated as $\delta = \frac{(1500+w)^{1-\rho} - (w)^{1-\rho}}{(2000+w)^{1-\rho} - (w)^{1-\rho}}$; see section 5.1 for a derivation. The annual discount factor is δ^{26} . Values for ρ are the highest, median, and lowest values reported in Balakrishnan, Haushofer and Jakiela (2015). Values for w come from our sample of 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). We use the 10th, 50th, and 90th percentiles of the endline income distribution respectively. 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period.

Table 7
Comparison of available definitions of temptation spending

	<u>Panel A -</u> <u>Summary Statistics for</u> <u>Spending at Follow-up</u>			<u>Panel B -</u> <u>Pairwise Correlations</u>					
	Mean	SD	N						
A. Regrets Goods respondent often regrets purchasing	44.59	181.55	689	A. Regrets					
B. Against Plans Goods respondent often buys in violation of prior plans	62.21	287.56	689	0.05	B. Against Plans				
C. Waste/Temptation Goods respondent says they waste money on or are tempted to buy	116.55	343.91	689	0.16	0.16	C. Waste/Temptation			
D. Alcohol and Tobacco	14.33	46.79	689	-0.01	-0.02	0.07	D. Alcohol and Tobacco		
E. Alcohol, Tobacco, Doughnuts, and Soda	65.28	89.35	689	0.03	0.06	0.13	0.61	E. Alcohol, Tobacco, Doughnuts, and Soda	
F. Money Wasted Self-reported total of money "wasted" (Round 2 Only)	305.85	685.04	346	0.13	0.02	0.25	0.01	0.23	F. Money Wasted
G. Unplanned Purchases	49.57	121.38	689	0.10	-0.03	-0.01	0.09	0.19	0.00

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). Correlations are estimated using pooled data for rounds 1 and 2 except for the "Money Wasted" variable (Row F) which exists only in Round 2. All money amounts are in Malawian Kwacha (MK); during the study period the market exchange rate was approximately MK400 to the US dollar, and the PPP exchange rate was approximately MK160 to the US dollar.

Table 8

Effects of lump sum payments on temptation spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	PCA indices of temptation spending		Measures of temptation spending (MK)						
<u>Dependent variable:</u>	Omitting Col. 9 (Both Rounds)	Including Col. 9 (Round 2 Only)	Goods respondent often regrets purchasing	Goods respondent often buys in violation of prior plans	Goods respondent says they waste money on or are tempted to buy	Alcohol and Tobacco	Alcohol, Tobacco, Doughnuts, and Soda	Unplanned Purchases	Self-reported total of money "wasted"
Rounds Available	1 & 2	2 only	1 & 2	1 & 2	1 & 2	1 & 2	1 & 2	1 & 2	2 only
Lump sum treatment	0.0136 (0.0658)	-0.0871 (0.125)	21.87* (12.93)	45.98** (21.20)	-2.540 (21.28)	2.400 (3.322)	1.315 (6.594)	-2.128 (2.380)	89.12 (69.93)
Dependent variable mean, control group (weekly payments)	-0.0423	-0.0106	32.94	36.54	128.7	12.92	64.03	48.73	261.8
Number of observations	689	346	689	689	689	689	689	689	346

Notes: PCA index computed by taking the first principal component of all the temptation variables for the control group (weekly payments), and normalizing the predicted values to the control group.

Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. All regressions control for stratification cell fixed effects, an index of baseline asset ownership based on first principal components, indicators for the number of days after the weekend the interview occurred, baseline total spending and (if available) the baseline value of the outcome variable. For details of the empirical strategy see section 4, and for complete variable definitions see Appendix C. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses.

Table 9
Effects of receiving pay during major weekly market on main outcome variables

	(1)	(2)	(2)	(4)	(5)	(6)	(7)	(8)
	Payday survey panel - Spending at market on the four payday weekends			Household survey data			Bond sales	
<u>Dependent variable:</u>	Amount spent on Friday and Saturday, all Weekends (MK)	Amount spent on payday (MK)	(Spending on payday)/(Income received)	Income received since last Friday (MK)	Remaining cash out of income received since last Friday (MK)	Total spending [†] since Friday from itemized expenditure data (MK)	PCA Index of temptation spending (MK)	1(Bought any shares)
<u>Panel A - Lump sum payment group only</u>								
Saturday payday treatment	-756.4*** (171.9)	-25.77 (119.8)	-0.00809 (0.0386)	161.6 (230.4)	-162.5 (107.2)	193.3 (237.8)	0.0722 (0.126)	-0.0260 (0.0402)
Dependent variable mean, control group (Friday paydays)	3,068	1,247	0.402	3,753	670.6	3,341	-0.00820	0.175
Number of observations	345	345	345	345	345	345	345	345
<u>Panel B - Lump sum and weekly payment group pooled</u>								
Saturday payday treatment	-812.5*** (113.5)	-26.91 (89.43)	-0.00731 (0.0269)	17.88 (194.9)	-92.03 (76.05)	128.0 (161.3)	-0.00991 (0.0769)	-0.0265 (0.0260)
Dependent variable mean, control group (Friday paydays)	3,293	1,688	0.514	3,081	579.2	3,147	-0.0124	0.145
Number of observations	689	689	689	689	689	689	689	689

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). Regressions are run on pooled data from round 1 and round 2. 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. All regressions control for stratification cell fixed effects, an index of baseline asset ownership based on first principal components, indicators for the number of days after the weekend the interview occurred, baseline total spending and (if available) the baseline value of the outcome variable. PCA index computed by taking the first principal component of all the temptation variables for the control group (Friday payments), and normalizing the predicted values to the control group. It mirrors column 1 of Table 8 in omitting the variable observed only in round 2 and computes the index for both rounds. For details of the empirical strategy see section 4, and for complete variable definitions see Appendix C. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses.