

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

Lasse Brune and Jason T. Kerwin*

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Abstract

We study a savings technology that is popular but potentially underutilized in developing countries: short-term deferral of wages, which replaces parts of regular wage payments with a single, later lump sum. Participants in a temporary cash-for-work program who are randomly assigned to lump-sum payments spend 25% less of their income immediately and are five percentage points more likely to purchase a high-return artificial investment. Ancillary evidence suggests that these effects are likely due to savings constraints: the majority of participants state they prefer the lump sum payment, and survey evidence suggests that saving is very difficult in this context. Workers report temptation spending as an important driver of savings constraints. However, we find little evidence for that particular mechanism based on heterogeneous treatment effects and additional experiment designed to vary the temptation participants face.

JEL Codes: D14, J33, O12, O16

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*Brune: Center for the Study of Development Economics, Northwestern University, Evanston, IL 60208 (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 off-season. 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 participants who work for a total of 15 days across two experimental rounds, with the assignments to weekly installments or lump sum payments cross-randomized across rounds.

The canonical model of intertemporal choice predicts that deferred wage payments should be very unpopular, because more money earlier is always better. When choosing between two potential income streams of equal nominal value, an unconstrained agent prefers smaller payments that arrive sooner over a single larger payment that arrives later; the sooner smaller amounts could always be kept for later. However, if keeping money for later is costly, due to internal or external savings constraints (Karlan, Ratan and Zinman, 2014), people may be better off with deferred income payments. Deferred wage payments can therefore function as a simple form of commitment savings (Ashraf, Karlan and Yin 2006, Dupas and Robinson 2013).

In contrast with the prediction of the canonical model, the 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 (2016) 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 and credit associations (ROSCAs) that are common across the developing world also serve to defer part of one's income into a later lump sum (Besley, Coate and Loury, 1993). Lump-sum payments are particularly attractive if large, indivisible purchases are an important portion of desired consumption. This is true

of many developing-country contexts: people need lump sums to pay school fees, to buy agricultural capital, and to make home improvements like new roofs. The stated preferences of the workers in our study are consistent with this pattern. At baseline, 45% say they plan to spend their project earnings on an indivisible investment, most commonly subsidized fertilizer (which is only sold by the bag). Over 50% say their main planned purchase will cost at least MK2800 — the entirety of their earnings from a single round of work.

Despite the evident demand for deferred wage payments in the developing world, previous research on their effects has been fairly limited. The [Casaburi and Macchiavello \(2016\)](#) study, conducted at the same time as our own experiment, shows that people prefer deferred lump sum payments but does not examine the effect of lump sum payments on financial behaviors. [Haushofer and Shapiro \(2016\)](#) examine the impacts of lump sum payments of a windfall cash transfer compared to nine monthly installments, but their lump sum is not delayed (the month of the delivery was randomly selected for each person).^{1,2}

We contribute to the literature by analyzing how deferred payments affect savings and consumption behavior. We focus on the short term; our follow-up 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 lumpy investments as well as the forgone returns from saving. These features of the data allow us to examine how deferred income payments affect expenditures and savings behavior immediately after they occur, shedding light on the mechanisms by which any longer-run impacts could occur.

Our study is related to the literature on commitment savings products, which have been shown to significantly increase savings in developing countries ([Ashraf, Karlan and Yin 2006](#), [Dupas and Robinson 2013](#)). The deferred wage payments we study differ from typical commitment savings products in that they are much cheaper and simpler: they do not involve bank accounts or other devices (such as lockboxes), but simply a change in the timing of when people are paid. The deferred wage payments that we study also differ from the typical

¹ 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.

² [Beegle, Galasso and Goldberg \(2015\)](#) report results from an experimental evaluation of the Malawi government’s public works program that includes a randomization of workers to either receive their wages every three days or in a lump sum after twelve days. They measure spending and saving outcomes about two to four weeks after the lump-sum payment. The results of this manipulation are not reported in the referenced paper; however, based on communications with the authors, the difference in income timing led to no discernible differences in the outcomes they measured.

commitment savings productions in that they are not optional; workers are assigned to be paid either smoothly or in a lump sum. However, they are very popular: nearly three-quarters of the workers in our sample say they would prefer the delayed lump sum over smooth payments, even at equal transaction costs.

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 at least partly due to savings constraints: people 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). First, 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, which is not consistent with a model featuring only borrowing constraints but no savings constraints. Finally, survey evidence suggests that in our context existing savings options are indeed unattractive due to risk and high explicit or implicit costs.

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).³ Our study shows that altering the timing of income receipt does not substantially change that finding: the minimum detectable effect sizes on temptation spend-

³ 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.

ing 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. 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 if the worker wants to shift consumption from period t to period $t + 1$ (Blattman et al., 2016).⁴ 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 the 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.⁵

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 to receive the supplemental income that the scheme provides in a lump-sum deferred payment instead of weekly installments.
 - (b) They will also choose other contracts or financial products that easily allow money to be shifted to a later date.

⁴ To allow for credit constraints, we could instead allow r to be positive for workers who want to shift consumption from the future into the present (from period $t + 1$ to period t). We focus here on savings constraints alone to simplify the exposition of the model.

⁵ The real interest rate remains slightly negative due to inflation.

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.⁶ The workers in our sample do have other sources of income, but the jobs provided as part of this study are an important supplement to that income.

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 one and an additional 15 workers were added for round two to replace

⁶ 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 one. All workers were interviewed in a baseline survey.⁷

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.⁸

The focus of this project was on exogenous differences in income timing, as opposed to effects that might originate from differential labor supply under the program. Therefore, by design, payments started after the work is completed. While workers knew their treatment status during the initial work period, take-up and labor supply was virtually universal and so we can interpret treatment effects as due to the direct effects of income timing.⁹

Workers received identical nominal wages for their work, but were randomly assigned, independently by round, to receive their pay with different timing.¹⁰ 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

⁷ For a full description of worker selection, attrition, and replacement, see Appendix A.2.

⁸ See Appendix A.3 for descriptions of the work activities undertaken as part of the project.

⁹ See Appendix A.3 for details on participation.

¹⁰ 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.

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, 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 two since there were seven work days during the first round and eight 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.¹¹ 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 two was then stratified on the round one 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 behavior, respondents’ choices about purchasing an artificial, indivisible investment offered by the project at the end of each study round.

¹¹ 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 of this issue.

3.1 In-Depth Survey Data

We conducted in-depth surveys of 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 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 the hypothesis that baseline covariates do not jointly predict treatment status ($p = 0.62$).¹²

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:

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

Y_{ir} is the outcome of interest for worker i in round r .¹³ 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.¹⁴ 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;

¹² 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.

¹³ We Winsorize all outcome variables at the 1st and 99th percentiles.

¹⁴ 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.

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 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 heterogeneous treatment effect analyses. We do this by examining 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 changes in the timing of workers' expenditures. Panel A of Figure 2 shows the randomized variation in cash income from the experiment's work program. Panel B shows the resulting variation in workers' mean expenditures on the same day they received their pay. The average worker in the deferred wage treatment spends nearly MK2,000 during the last payday weekend, whereas the average control-group worker spends just over MK1,000. 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' spending on paydays is close to their program income (Panel C).

Table 3 presents regression estimates of equation 1, examining the effect of deferred wage payments on expenditures using different data sources. Columns 1-4 focus on expenditures on paydays (Fridays for workers who were randomized into receiving their pay on Friday, Saturdays for those paid on Saturday), using data from our high-frequency payday survey

panel.

Workers who are paid in a lump sum spend MK1,095 less on the first three paydays (when they receive only the MK100 show-up stipend; column 1) and MK578 more on the fourth payday (when they receive their lump sum wage payment; column 2). The net effect is that total same-day expenditure on paydays falls by MK518 (column 3). The share of project income spent on paydays falls by 17 percentage points (column 4). Together with the timing of income receipt, these results suggest that the treatment group may have more cash on hand immediately following the fourth payday.

The in-depth survey data on expenditures also suggests an increase in cash holdings by the treatment group. Columns 5, 6, and 7 of Table 3 report results for income, remaining cash, and spending, for a period running from the last payday up through the day of the interview (on average 5 days after the last payday). Workers paid in a deferred lump sum report receiving an additional MK1,656 in income (column 5) relative to the control group.¹⁵ Lump sum recipients report retaining an additional MK145 out of the income they had received since Friday — a 30% increase relative to the control group mean (column 6).

The treatment increases expenditures immediately after the lump sum payment. The itemized expenditure data show a MK365 increase in spending (Table 3, column 7). As discussed in Section 3.1, this measure of expenditures does not include some durables and so this effect is likely to be a lower bound of the additional spending in the treatment group. For this reason, and since remaining cash holdings are not necessarily equal to the change savings, column 7 does not equal the sum of columns 5 and 6.

The effects on expenditures are consistent with the idea that workers are savings constrained, facing large negative effective interest rates on liquid savings. If control-group workers wanted to match the surplus cash holdings of the treatment group following the fourth payday, they would have to save some of their income for several weeks – compared to just a few days for the deferred lump-sum payment. A negative effective interest rate would reduce their cash holdings in two ways. First, they would likely choose to save less. Second, any amount they did save would be attenuated due to the negative interest rate they must pay. Practically, this could mean actual fees paid to formal or informal savings institutions, losses due to theft, transfers to relatives, and temptation spending.

In Table 4 we look for changes in other financial outcomes. 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%

¹⁵ 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.

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.¹⁶

In order to address the common problem of noisy survey measures for these types of outcomes, our experiment included the offer of an artificial investment good. The idea of selling this “bond” was to capture potential effects on 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. For reference, columns 3 and 4 repeat the remaining cash on hand and total income since Friday results from Table 3. Overall, lump sum recipients had about a 5 percentage points higher take-up of the bonds compared to a control group mean of 10% (Panel A, column 1), spending an additional MK 125 (column 2). The increases in spending are similar in magnitude to the effects on cash-on-hand (column 3), suggesting that the remaining money from the project was used to purchase bonds.

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 uncertainty associated with this new product may have led to lower demand compared to a hypothetical state of world in which respondents are familiar with the specific offer.¹⁷ In addition, the low take-up 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 estimated impacts on bond take-up and spending are highly heterogeneous by round. They are statistically significant overall (Table 5, Panel A), and when we restrict the sample

¹⁶ “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).

¹⁷ Alternatively, the fact that the maximum scale of the investment was two shares of the bond could have limited takeup. [Banerjee and Mullainathan \(2010\)](#) show that in a model with temptation-driven savings constraints, agents will not want to make profitable investments that are below a certain size. Intuitively, they believe that their future self will waste the income from the investment on temptation goods, and this will only change if the scale of the investment is large enough that future temptation spending will be easily satiated. This may not be an important factor in driving the low take-up of the bond in our study, as we find limited evidence of temptation-based savings constraints. However, our analyses of temptation spending have important limitations (see Section 6 for details).

to round two (Panel C), but not when we restrict the sample to round one (Panel B). The varying treatment effects potentially reflect differences in the economic environment, the intervention, and the setup of the bond product across rounds.¹⁸

First, the two rounds took place at two different points in time, leading to differences in the economic environment that might have interacted with the intervention. In particular, the second round occurred further into the annual lean season than the first round. As a result, the availability of paid work outside of our project was more limited. 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; this figure fell to MK2010 in round two.

Second, the work scheme differed across the rounds: in round one of the study, workers were employed for only seven days, as opposed to eight days in round two. This means that 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. As a result, the size of the lump-sum treatment was MK300 larger in round two. Due to this difference, and the lower availability of income from other sources mentioned above, the treatment effect of the lump sum payment income receipt since Friday (column 4) was MK746 larger in round two than in round one, a difference that is significant at the 0.05 level (not shown).

This means 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, on average, which is less than the MK1500 minimum cost of purchasing a bond. In contrast, in round two the lump sum treatment raises average income received since Friday by MK2047, easily clearing the MK1500 threshold for purchasing at least one tranche of the bond.

Third, the bond product differed slightly across rounds. One potential difference across rounds is trust in the product, since the people who bought the bond in round one had all received their payout. However, higher trust in round two cannot fully explain the larger treatment effects, since the control-group take-up is lower in round two than in round one.

Another difference is that the bond was announced differently across the two rounds. In round one the bond was only announced in the week preceding the final payday. This means that in round one the workers did not know about the investment opportunity until a week before it was made available to them. In round two the investment opportunity was announced before the work began for the round. All workers across both groups knew they

¹⁸ We can rule out any role for dynamic effects across rounds, such as the round one treatment moderating the round two treatment effect. There are no order effects (Appendix Table D.1) and controlling for round one treatment status does not substantively affect the estimated effect of the treatment for round two (Appendix Table D.2).

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.

The effect of this difference is theoretically ambiguous. On the one hand, one might expect shorter notice to lead to a larger treatment effect because the weekly payment group members did not know about this opportunity until they had received three-quarters of their total wages. Their remaining unpaid wages for the last payday weekend were less 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. On the other hand, shorter notice could lead to a smaller treatment effect because the members of the lump sum payment group might have committed their lump sum to other larger expenses (e.g., durable goods or buying food in bulk), leaving no extra funds for the (surprise) investment opportunity.

In summary, while the overall bond results are driven by round two we cannot draw strong conclusions from this pattern because several features of the intervention, offer and environment changed – preventing us from making *ceteris paribus* comparisons of individual features across rounds.

5.1 The Role of Savings Constraints

We argue that the increase in investment caused by the deferred wages — as measured through the bond purchases — is the result of binding savings constraints. That is, workers are unable or unwilling to hold on to cash due to internal or external factors that effectively tax savings (Karlan, Ratan and Zinman, 2014). In this scenario participants fail to purchase the bond at the moment that it is offered not because they do 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 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. Workers who are paid weekly would have to save for a significantly longer period to buy the bond. If they are constrained in their ability to save, they will not be able to do that. In contrast, we argue that credit constraints alone cannot explain our results.

Two pieces of evidence support the argument that our results are due to savings constraints rather than credit constraints. First, workers' stated preferences are consistent with savings constraints, and inconsistent with credit constraints. Second, survey data on savings in our sample supports the notion that saving money is very difficult in this context. We

discuss each piece of evidence in turn.

5.1.1 Stated Preferences for Lump-Sum Wage Payments

The idea that savings constraints are binding for a large part of the sample is consistent with workers' stated preferences at baseline. 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, 72% 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 72% of workers who preferred the lump-sum payments (we cannot reject equal effects for the remaining 28%). Thus the majority of workers' stated preferences are inconsistent with binding credit constraints as the driver of our results: workers who would prefer to smooth consumption should not volunteer to defer their income.

One limitation of the evidence from the workers' stated preference is that their choices are unincentivized, and strictly hypothetical. However, it is not clear that incentivized choices would better capture true preferences given the low levels of education and financial sophistication in our context. [Chuang and Schechter \(2015\)](#) review papers that measure time, risk, and social preferences, mostly from developed countries, and find that incentivized and unincentivized choices are comparably stable. In their own experiment, in rural Paraguay, unincentivized survey measures of social preferences were more stable than incentivized experimental measures from survey data. This may be due to difficulty understanding the incentivized games in their low-education sample.

Another limitation is that there is some evidence that people may overestimate the value of commitment and end up in harmful commitment contracts (e.g. [John 2017](#)). As a result, the workers in our sample could mistakenly think a lump sum payment would be good and then end up being worse off than if they had received their income smoothly.

5.1.2 Survey Evidence on Barriers to Saving

To help address the limitations of the workers unincentivized stated preferences, we turn to evidence about savings constraints from our survey data, which provides contextual information about the plausibility of substantial barriers to savings. 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. A recent survey of a sample of permanent wage workers in the same district found that 19% reported having lost any savings in the three months prior to the interview. Workers who did lose money lost the equivalent of 15% of monthly earnings. The majority of that sample reports losing cash savings kept on hand while traveling; the second most important reported reason is theft. Compared to the available risky alternatives in this context, deferring wage income from a trusted employer is an attractive alternative.

This lack of affordable, trustworthy savings options can help explain why the takeup of the bond is so low in the treatment group. Treatment-group workers are not offered the bond immediately after they receive their lump-sum wage payment. Instead, they are sold the bond during the household surveys, which occur, on average, 5 days after their received their lump sums. If saving is costly, these costs could easily push the excess savings of treatment-group workers below the MK1,500 threshold to buy a share of the bond. Consider a worker who has exactly MK1,656 in disposable income due to the treatment (which is the estimated effect of lump-sum wages on income in the days immediately prior to the follow-up survey). A total savings cost of MK157 would make the bond unaffordable. In percentage terms, this is an extremely high cost — a negative interest rate of 2% per day — but savings costs of this magnitude are within the norm for developing countries. For example, [Dupas et al. \(2016\)](#) studies the effects of basic savings accounts in Malawi that normally carry monthly maintenance fees of \$0.65, or MK230.

A worker faced with such high savings costs would be unlikely to attempt to save for the bond, and would instead make other purchases. Consistent with this story, we not only see low take-up of the bond among the treatment group, but also that take-up rates are significantly negatively impacted by the amount of time between the payday and the exogenously-assigned date of the survey visit. Each additional day of delay causes a 3.9 percentage-point decline in bond purchases for the treatment group, and a slightly larger decline for the control group.

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.

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 spending 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 goods in the studies they summarize and that also appear in our surveys' itemized lists of purchases. This adds doughnuts and soda to their list. 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 6 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.¹⁹

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

¹⁹ Average income in the time span covered by the survey was about MK3000 (see Appendix Table B.3, Panel C).

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.

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. Our data on temptation spending covers only the period during and following the lump-sum payout, so we cannot examine changes in temptation spending in the pre-payout period. This means that we may miss reductions in temptation spending before the treatment group receives their income. Our estimated treatment effects are therefore biased away from finding reductions in temptation spending for the treatment group, and can be thought of as lower bounds on the true potential reduction in temptation spending.

The data show no evidence of consistent effects of the lump sum treatment on temptation spending in the period following the lump sum payout (Table 7). 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 7 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 (as measured by bond purchases) 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.²⁰

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. This experiment tests only the effect of current-period temptation on savings constraints, that is, whether $\frac{\partial R}{\partial \tau_t} < 0$. As discussed in Section 1, we would expect this inequality to hold whenever people face temptation-based savings constraints, even if they are unsophisticated about the temptations they face. Our experiment also assumes that temptations are only salient when people have cash to spend on them (i.e. during the payday), which may not be true.

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

²⁰ 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.

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 F.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).²¹ Multiple-choice questions (Panel B) show the same 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.²²

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

²¹ 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 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.

²² 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.

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.

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

Table 8 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.²³

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.²⁴

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.

²³ Appendix Table G.1 shows the fully-interacted specification, confirming that there are no differential effects of Saturday payments by lump-sum treatment status.

²⁴ 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 7) and found no evidence of effects on any of them.

There are a 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 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).²⁵ 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

²⁵ These expenditure shares are estimates at best, and probably upper bounds, because our measures of total expenditure are incomplete.

— 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 cash holdings, 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 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 by using data on purchases of an artificial “bond,” which does not suffer from mismeasurement or recall bias.

This data confirms that workers save significantly more (as measured by bond purchases) as a result of being 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 in 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 consider offering people the option of receiving a portion of their wages (or cash transfers) in deferred lump sums as a form of commitment savings. 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: first, non-financial investments such as health and education may not be perceived as investments by respondents; second, even when an investment earns direct financial returns, heterogeneity in those 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.

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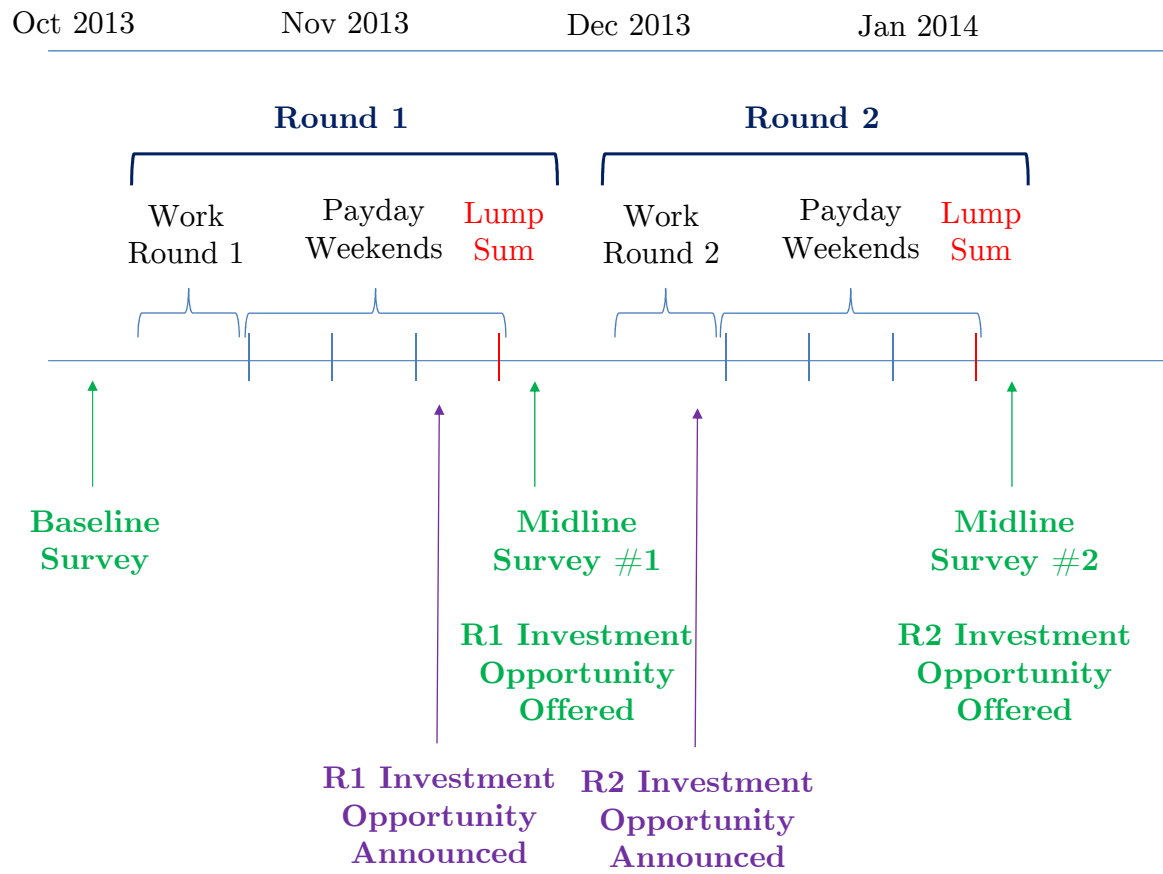
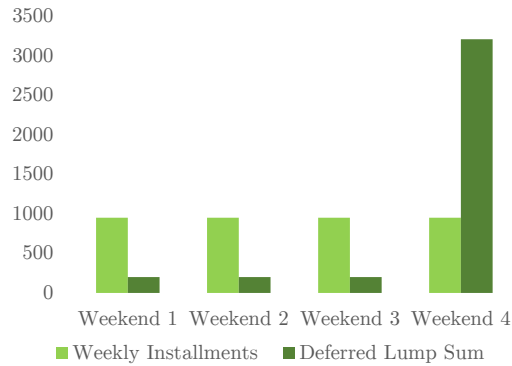
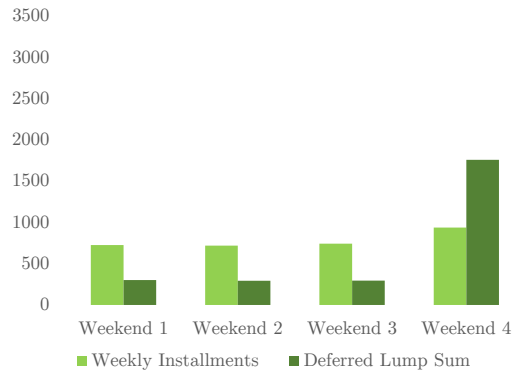


Figure 1
Timing of work, payments and data collection

Panel A: Wage payments on payday (MK)



Panel B: Expenditures on payday (MK)



Panel C: Expenditures on payday as share of wage payments

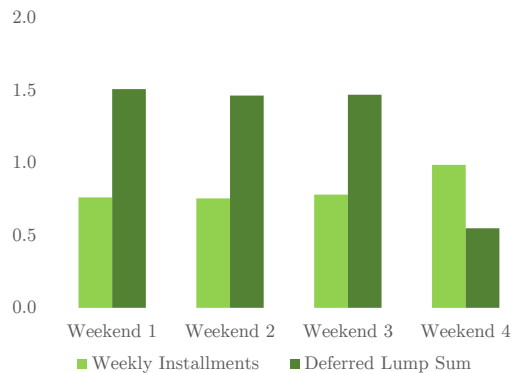
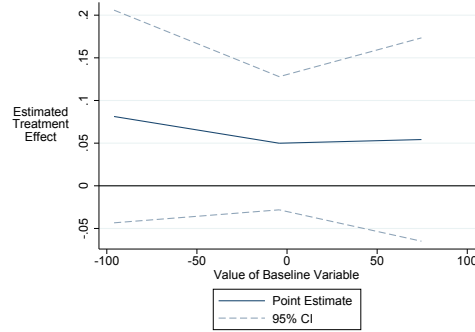


Figure 2

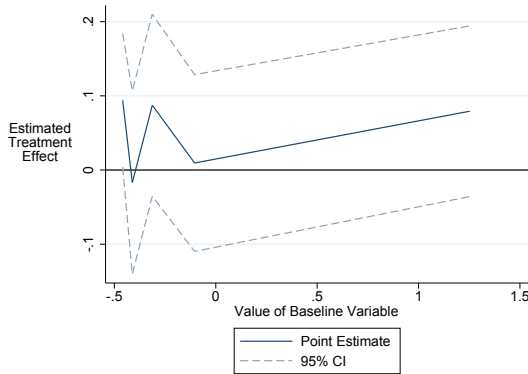
Wage payments and expenditures by payday weekend

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

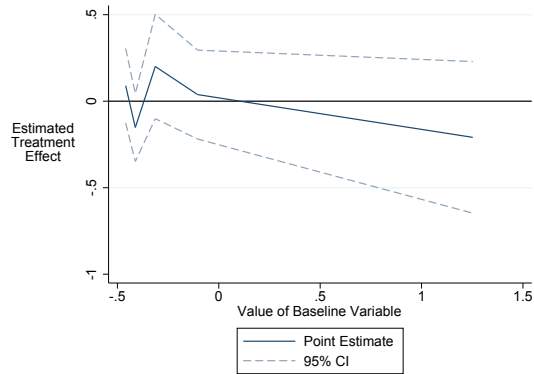


Figure 3

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 (7)
	Weekly payments			Lump sum payments			
	Mean (1)	SD (2)	N (3)	Mean (4)	SD (5)	N (6)	
<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)
	Payday survey panel - Spending at market on the four paydays				Household survey data		
<u>Dependent variable:</u>	Amount spent on paydays (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)
	Paydays 1-3	Payday 4	Paydays 1-4				
Lump sum treatment	-1,095*** (50.96)	577.5*** (64.68)	-518.0*** (84.53)	-0.173*** (0.0283)	1,656*** (172.7)	145.3** (71.57)	365.3** (153.3)
Dependent variable mean, control group (weekly payments)	1,528	606.8	2,131	0.713	2,309	468.5	2,962
Number of observations	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 one and round two. 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 one and round two. 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 one and since January 1st in round two, 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)
<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
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 7

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"
	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 8
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)	on (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 one and round two. 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 7 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.