

# Income Timing and Liquidity Constraints: Evidence from a Randomized Field Experiment

Lasse Brune and Jason T. Kerwin\*

January 23, 2019

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

## Abstract

People in developing countries sometimes desire deferred income streams, which replace more-frequent income flows with a single, later lump sum. We study the effects of short-term wage deferral using a randomized experiment with participants in a temporary cash-for-work program. Workers who are assigned to lump-sum payments are five percentage points more likely to purchase a high-return investment. We discuss the role of both barriers to saving and credit constraints in explaining our results. While stated preferences for deferred payments suggest a role for savings constraints, the evidence is also consistent with a simpler model of credit constraints alone.

JEL Codes: D14, J33, O12, O16

Keywords: Savings Constraints, Credit Constraints, Financial Inclusion, Income Timing

---

\*Brune: Global Poverty Research Lab, Buffett Institute for Global Studies, Northwestern University, Evanston, IL 60208 (lasse.brune@northwestern.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, Savings Constraints, and Temptation Spending: Evidence from a Randomized Field Experiment.” 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.

Can delaying wage payments make people better off? In a world with free storage, and in the absence of behavioral biases, the answer is clearly no: if people prefer higher consumption later they could simply save their income. But in the context of developing countries, anecdotal evidence suggests that the assumption of free storage does not hold. Consistent with that narrative, there is evidence suggesting that people sometimes prefer to receive income in a later lump sum as a way to save (Casaburi and Macchiavello, 2018; Kramer and Kunst, 2018). These lump sums are needed to make indivisible expenditures, ranging from purchasing durable consumer goods, to buying in bulk, to making investments (Collins et al., 2009).<sup>1</sup> One reason that people might prefer delayed lump-sum wage payments is that they are constrained in their ability to save. Saving could be costly due to internal or external factors that effectively tax any money they save (Karlan, Ratan and Zinman, 2014) and a deferred, lumpy payment could alleviate those constraints.

Motivated by qualitative evidence on barriers to saving and an apparent preference for delayed lump-sum payments in some contexts, we ran a randomized experiment to shed light on whether deferred lump-sum income receipt has real consequences.<sup>2</sup> We paid a group of workers in Malawi in one of two ways: either in four weekly installments or in a single deferred lump sum. The workers were participants in a rural livelihoods program that provides supplemental cash income during the agricultural off-season. Our experiment included 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. 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 any differences we measure are due to the timing of income rather than differences in time or transportation costs across study arms.

Our main result is that deferred lump-sum wages affect the take-up of a short-term, high-interest bond that we designed and offered to respondents as part of the project. Delayed lump-sum payments increase spending on the bond by five percentage points — a 50% increase over the mean for people who received weekly payments. Consistent with the notion that savings constraints are a driver of this result, prior to the experiment 74% of our sample stated a preference for the lump sum wage payments, holding transaction costs equal.

A competing explanation for our main result is based on credit constraints alone, with

---

<sup>1</sup> One function of the rotating savings and credit associations (ROSCAs) that are common across the developing world is to defer part of a person’s income into a later lump sum (Besley, Coate and Loury, 1993).

<sup>2</sup> Our study focuses on deferred lump-sum income payments, in which income is moved from smaller, earlier installments into a deferred lump sum. We use the terms “deferred lump-sum wages” and “lump sum wages” interchangeably in the text.

no role for savings constraints, and indeed the bulk of our empirical evidence supports this explanation. In this interpretation, workers who received the lump sum wage payments were forced to “oversave” instead of consuming their income earlier. This caused them to end up with unwanted excess liquidity during the final week of payments which they used to invest in the short-term bond.

We develop a simple theoretical model to highlight the role of credit constraints in explaining our results. The model examines how liquidity-constrained workers will react to a shift from weekly to lump-sum wage payments, and lets us compare the predictions of savings and credit constraints. The model has three periods, each representing two weeks. Project payments take place in periods 1 and 2. In period 2 the bond sales occur. Period 3 is when the bond pays back and its returns are available for spending. The experiment’s randomized change in income timing shifts money between period 1 (the first two weeks) and period 2 (the second two weeks). In the model, workers are credit-constrained in period 2, and are assumed to be either credit-constrained or savings-constrained in period 1.

Based on the theoretical framework, there are two points that favor an explanation of our empirical results based on credit constraints alone. First, in order for binding savings constraints to explain the effects on bond purchases, workers must also face some form of credit constraint in period 2 at the time of the bond sales; otherwise, they could simply borrow the capital needed to purchase the bond and take-up would be universal. Thus, in terms of the number of different kinds of constraint needed to drive our results, an explanation based on credit constraints alone is simpler.

Second, if our treatment effects are only driven by credit constraints then the lump sum treatment should not affect purchases of durable goods, but should plausibly increase the use of other highly-liquid savings options. In line with this reasoning, we see no effect of the lump sum treatment on asset accumulation. However, we do observe an effect on bulk purchases of unprocessed staple foods, a high-return method to smooth consumption in our context.

Additional support for the credit constraints model comes from patterns that we do not observe, but would have expected to see if savings constraints were driving our results. First, we find that the treatment effects on bond purchases do not differ by workers’ baseline preferences for lump sum versus weekly wage payments. Second, we conducted an additional experiment that varied workers’ exposure to a tempting environment when they received their wages. A number of prominent models of intertemporal choice predict that exposure to temptation should reduce people’s ability to save ([Gul and Pesendorfer, 2001](#); [Fudenberg and Levine, 2006](#); [Ozdenoren, Salant and Silverman, 2012](#); [Banerjee and Mullainathan, 2010](#)). However, this exposure did not meaningfully affect any of our main outcomes, despite strong

descriptive evidence that suggests that the experimental manipulation was reasonable. This result is in line with a model based on credit constraints alone, but not consistent with the particular model of “internal” savings constraints driven by behavioral biases.

The credit constraints explanation poses an intriguing puzzle, because high demand for a delayed lump-sum payment is not consistent with a model of credit constraints alone. This raises the possibility that workers’ apparent preferences for deferred lump-sum wage payments do not translate into downstream financial consequences after all. We consider a number of potential explanations for the puzzle, including respondent confusion, demand effects, and substitution effects. Workers may also be heterogeneous in terms of the liquidity constraints they face. In this scenario, some workers are made worse off and buy the bond due to excess liquidity after facing a binding credit constraint, while others are made better off by having their savings constraints alleviated. One advantage of a model of heterogeneity in liquidity constraints is that it can readily be harmonized with the [Casaburi and Macchiavello \(2018\)](#) finding that some people demand deferred income under real-world, incentivized conditions due to savings constraints. Although heterogeneous liquidity constraints are a relatively compelling explanation, we ultimately cannot provide strong evidence in this paper for any single resolution to this puzzle.

Notwithstanding the relative importance of either mechanism, our results show that liquidity constraints matter even when liquidity needs are relatively modest, time horizons are fairly short, and income streams are known in advance. A deterministic variation in payment schedules over the course of four weeks matters for how much workers invested in a safe, high-return, short-term bond at the end of each project round. Our study provides the first evidence that deferred lump-sum wage payments can affect savings and investment decisions in developing countries. Previous research on the effects of deferred lump-sum wage payments has been fairly limited. The [Casaburi and Macchiavello \(2018\)](#) 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).<sup>3,4</sup> Our results are consistent with

---

<sup>3</sup> 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.

<sup>4</sup> [Beegle, Galasso and Goldberg \(2017\)](#) 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

previous work from Kenya that has found that the seasonal timing of income is important for agricultural investments (Duflo, Kremer and Robinson, 2011) and that credit constraints prevent farmers from making investments to arbitrage seasonal fluctuations in grain prices (Burke, Bergquist and Miguel, 2018).

An important contribution of our study is to utilize administrative data on purchases of an investment that we designed as part of the experiment. The bond could only be purchased in lumpy tranches that required workers to generate sufficient liquidity at the time of purchase and paid a fixed return. Thus, it serves as a quantifiable measure of savings and investment decisions. Jakiela and Ozier (2016) use similar experimenter-designed investments in their lab-in-the-field study of kin taxes in Kenya. The bond used in our study is different in that it had to be purchased via door-to-door sales rather than as part of a lab experiment. The investments in their study also feature a risk-reward trade-off, whereas our bond paid a high return with certainty.

The remainder of this paper proceeds as follows. In Section 1, we develop a theoretical framework that describes the role of either credit or savings constraints in explaining the effects of our experiment’s variation in income timing. Section 2 lays out the design of the study, Section 3 details the data sources we use, and Section 4 describes our empirical strategy. In Section 5 we present the effect of the lump sum regime on bond purchases and other outcomes. Section 6 discusses the arguments in favor of credit and savings constraints. Section 7 concludes.

## 1 Theoretical Framework

To illustrate how liquidity constraints could drive the responses to the variations in income induced by our experiment, we develop a stylized model based on the framework developed in Besley (1995) and Jappelli and Pistaferri (2017). The agent decides how much to consume or save in each of three two-week periods. She also decides whether to buy an indivisible, short-term investment in period 2 (the bond from our study). This is a discrete choice, so  $b \in \{0, B\}$ . The bond pays a certain return,  $R > 1$ , that is available for consumption in period 3. The three periods in the model cover the four weeks of wage payments in the experiment and then the two weeks between the bond purchases and the bond payouts. The third period is a catch-all for future, post-payment periods.

Income is denoted  $y_t$ . The two study arms differ only with respect to income timing in periods 1 and 2. The two groups have the same total income, but their income timing differs because some money is shifted from period 1 to period 2. For example, a switch from weekly

---

to no discernible differences in the outcomes they measured.

to monthly payments moves income from the first period in the model to the second one, keeping total income constant.

The agent chooses consumption levels  $c_t$  and decides whether to purchase the bond  $b$  in period 2 to maximize the undiscounted sum of period utilities:

$$\begin{aligned} \max_{c_1, c_2, c_3, b} U(c_1, c_2, c_3) &= u(c_1) + u(c_2) + u(c_3) \\ \text{s.t. } c_1 + s_1 &= y_1 \\ c_2 + s_2 + b &= y_2 + s_1 \\ c_3 + s_3 &= y_3 + s_2 + bR \end{aligned}$$

where  $u(\cdot)$  is an increasing and concave, continuously differentiable function and  $s_t$  is the agent's cash savings at the end of period  $t$ . Given the short time spans involved, we assume no interest on cash savings and no discounting (i.e.  $r = \delta = 0$ ).<sup>5</sup>

We solve the model by working backward from period 3, and show that it reduces to the following first-period optimization problem where the agent chooses only  $s_1$ :

$$\max_{s_1} u(y_1 - s_1) + u(y_2 + s_1 - s_2 - b^*(s_1)) + u(y_3 + s_2^*(s_1) + b^*R) \quad (1)$$

For derivations of our theoretical results, see Appendix A. Our core theoretical results can be summarized as follows:

1. **Period 2 credit constraints are necessary for the treatment to affect bond purchases.** In the absence of a second-period credit constraint (or a sufficiently-high interest rate), the agent will always buy the bond, since doing so raises her overall income. Hence the treatment cannot affect bond purchases.
2. **Period 1 liquidity constraints are necessary for the treatment to affect bond purchases.** Intuitively, if resources can be costlessly moved across the two periods (i.e. no liquidity constraints or interest) then the timing of income is irrelevant. The liquidity constraint could be either a credit constraint, a savings constraint, or both.
3. **Period 1 credit constraints will cause the treatment to increase bond purchases among agents who would prefer smoother income streams.** For the period 1 credit constraint to bind, the agent must want to move income into earlier

---

<sup>5</sup> With these assumptions we also follow [Jappelli and Pistaferri \(2017\)](#). Like them, we also impose the standard Inada condition that  $\lim_{c \rightarrow 0} u(c) = -\infty$ ; this guarantees positive consumption in all periods.

periods — i.e. the deferred lump sum wage payments move income further into the future than she would desire. Because there is a binding credit constraint, the deferred wages treatment causes the agent end up with excess liquidity in period 2, which then makes purchasing the bond her constrained optimal choice.

- 4. Period 1 savings constraints will cause the treatment to increase bond purchases among agents who would prefer lumpier income streams.** For the period 1 savings constraint to bind, the agent must want to move income into later periods — i.e. the weekly wage payments move income further into the present than she would desire. In particular, such agents may want to save up to purchase the bond. The deferred wages treatment relaxes this binding savings constraint, allowing agents to purchase the bond.

The group of workers that is induced to purchase the bond by the treatment is qualitatively different under period 1 credit constraints and period 1 savings constraints. Under credit constraints, it is workers who are liquidity-constrained under deferred wage payments who purchase the bond; under savings constraints, it is workers who are *not* liquidity-constrained under the deferred wage payments who purchase the bond.

## 2 Study Design

To understand the impact of deferred lump-sum wage payments on short-run investment decisions 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.<sup>6</sup> 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 workers who dropped out after round one. All workers were interviewed in a baseline survey.<sup>7</sup>

---

<sup>6</sup> 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 B.1.

<sup>7</sup> For a full description of worker selection, attrition, and replacement, see Appendix B.2.

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.<sup>8</sup>

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 was completed. While workers knew which study arm they were assigned to during the initial work period, take-up and labor supply was virtually universal and so we can interpret the estimated effects as being the result of changes in income timing rather than any shifts in labor supply.<sup>9</sup>

Workers received identical nominal wages for their work, but were randomly assigned, independently by round, to receive their pay with different timing.<sup>10</sup> 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. 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 workers were also given a simple handout explaining their group assignment.

All transaction costs, such as transit and time costs, were held constant across payment modes. Payments were all provided at the same location irrespective of the study arm a worker was assigned to. To ensure that transit and time costs were the same 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

---

<sup>8</sup> See Appendix B.3 for descriptions of the work activities undertaken as part of the project.

<sup>9</sup> See Appendix B.3 for details on participation.

<sup>10</sup> The official inflation rate in Malawi was about 23% per annum during the study period ([https://www.rbm.mw/inflation\\_rates\\_detailed.aspx](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.

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.

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.<sup>11</sup> 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 of the study.

## 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 indivisible, high-return bond offered by the project at the end of each study round.

### 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

---

<sup>11</sup> 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 E we examine the round two data for order effects based on the round one assignments, and find no evidence of this issue.

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.

Our baseline survey also collected workers’ stated preferences about whether they wanted to be paid weekly or in a deferred lump sum at the end of the month, holding transaction costs constant. Our survey collected simple binary measures of stated preferences, rather than incentivized choices or willingness-to-pay.<sup>12</sup>

## 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 (which was at a trading center that is the site of the local major market) 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

---

<sup>12</sup>This decision was driven by the limited education levels in our sample (just 12% had finished primary school) and the fact that the underlying question was already complex, requiring an extensive explanation of the scenario.

spent yesterday.<sup>13</sup>

### 3.3 Bond Sales

A third source of data comes from purchases of a bond that we designed and offered to respondents as part 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 liquidity constraints, since members of the weekly group had to save their pay in order to use it for this high-return savings vehicle.

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.<sup>14</sup>

### 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

---

<sup>13</sup> 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 D.1 for details of these checks.

<sup>14</sup> While it would have been ideal to randomize the timing of the announcement within rounds, this was infeasible given the design of the broader study. Workers were drawn from the same villages within a small geographic area, and interacted with one another at the work sites, so any announcement to some workers would have spread to the others. Also, the same workers participated in both rounds of the study. Therefore, once we announced the investment opportunity in round one, people were aware that it might be a possibility in the second round as well.

three and a half years of completed schooling on average. We conduct two types of standard balance tests. The  $p$ -values in column 7 are from tests of equal means in the treatment and control groups. 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 ( $p = 0.62$ ).<sup>15</sup>

## 4 Empirical Strategy

Our analysis focuses on examining the mean effect of lump sum wage payments, relative to weekly wage payments, on savings and expenditures. To do this we estimate regressions of the following form:

$$Y_{ir} = \alpha + \beta L_{ir} + \gamma' \mathbf{X}_{ir} + \varepsilon_{ir} \quad (2)$$

$Y_{ir}$  is the outcome of interest for worker  $i$  in round  $r$ .<sup>16</sup>  $L_{ir}$  is an indicator variable for individual-level assignment to lump-sum wage payments in round  $r$ .  $\mathbf{X}_{ir}$  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.<sup>17</sup>  $\varepsilon_{ir}$  is a mean-zero error term.<sup>18</sup>

---

<sup>15</sup> Appendix Table C.1 repeats the same summary statistics and balance tests separately by round. It shows comparable means across study arms within each round, and also overall balance of baseline covariates within each round.

<sup>16</sup> We Winsorize all outcome variables at the 1<sup>st</sup> and 99<sup>th</sup> percentiles.

<sup>17</sup> 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 H for regression results without controls. Our results are also robust to controlling for the cross-randomized assignment of workers to receive their pay on either the market day or a non-market day; see Appendix I.

<sup>18</sup> The workers in our sample do interact with each other, so we cannot rule out the possibility that workers assigned to one experimental group had an impact on workers in another; since only one person from each household was eligible to participate, we can rule out any within-household spillovers. Our design does not allow us to address potential spillovers of effects from one study arm to another. In the context of our design, spillovers will most likely bias our estimated effects toward zero: for example, if monthly payment group members gave loans to weekly payment group members in the period after the lump sum was paid out, 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 4, Columns 7 through 10).

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.

## 5 Results

In a world with no liquidity constraints, our experiment’s randomized variation in income timing would have no effects. As discussed in Section 1, liquidity-unconstrained workers could simply choose to consume their income in whichever period they want; given the short time horizons involved, interest and discounting should barely affect this result. Therefore we begin by showing that the experiment did in fact shift spending across periods. 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 lump-sum wage treatment spends nearly MK2,000 during the last payday weekend, whereas the average control-group worker spends just over MK1,000.

Table 3 presents regression estimates of equation 2, examining the effect of deferred lump-sum 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 lump sum group in the period immediately following the last payday. 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).<sup>19</sup> Workers paid in a deferred lump sum report receiving an additional MK1,656 in

---

<sup>19</sup> This time window differs in length across workers depending on the date of the survey. Our two study arms are balanced in terms of interview date, so the varying window should not create systematic differences in spending figures across treatment and control workers (see Table 2).

income (column 5) relative to the control group. This is somewhat less than the difference in what workers were paid, and could imply that control-group workers pursued other income-generation opportunities that treatment workers did 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. Lump sum recipients report retaining an additional MK145 out of the income they had received since Friday (column 6). The treatment increases expenditures immediately after the lump sum payment by MK365 (column 7).<sup>20</sup>

## 5.1 Effects on investment

Given the evidence that the experiment did induce changes in liquidity, the next question is whether these changes affected downstream financial outcomes. Table 4 examines changes in asset purchases, borrowing and lending, and transfers. The first column for each outcome presents effects on the levels of each outcome. The results are consistent with at most fairly modest effects: although the confidence intervals include large fractions of the control-group means, they are small enough relative to the variation in liquidity induced by the experiment to allow us to draw meaningful conclusions. For example, we can confidently rule out that treatment-group workers used more than about 40% of their additional income on the last weekend to purchase durables. The upper bound of the 95% confidence interval of column 1 is MK858 ( $-176.1 + 1.96 \times 527.6$ ) while the difference in income across the weekly and lump sum arms is MK2,250 (averaging over the two rounds of the study). In other words, our results suggest that workers did not leverage the additional liquidity at the last payday to increase durable asset holdings. The second column for each outcome presents effects on discretized versions of the variables. In these columns the outcome is equal to one if the variable is positive and zero otherwise. We see no evidence of changes in any of these outcomes.

### Effects on purchases of the bond

To create an outcome that is an objective measure of changes in investment, our study included the offer of an investment good that we designed as part of the experiment. 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

---

<sup>20</sup> As discussed in Section 3.1, this measure of expenditures does not include some durables and so this effect could 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 in savings, column 7 does not equal the sum of columns 5 and 6.

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, the lump sum treatment increased take-up of the bond by about 5 percentage points, and raised spending on the bond by MK125 (column 2). This is a 50% increase over the weekly group mean of 10% (Panel A, column 1). 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.<sup>21</sup>

### Effects on bulk purchases of maize

An alternative high-return, liquid investment in our context is bulk purchases of maize, the primary staple crop in the region. This investment can quickly be converted into consumption, since maize is part of nearly every meal in Malawi. It also carries a high effective return for two reasons. First, workers who buy a large quantity of maize at once can take advantage of bulk discounts. Second, our experiment took place during the pre-harvest season, during which maize prices steadily rise each year. For the same path of actual consumption, it was therefore cheaper to buy maize earlier and store it.

Table 6 shows the effect of lump sum wage payments on bulk purchases of maize. Column 1 defines a bulk purchase as a single transaction of MK2,000 or more, or roughly the difference in project income on the last payday between the weekly and lump sum study arms. These bulk purchases rise by 10 percentage points over the weekly payments-group mean of 5.8%, and the effect is statistically significant at the 1% level. Columns 2 and 3 show that this result is not an artifact of the exact cutoff we chose: the effect is 5.2 percentage points if we look at purchases that are strictly larger than MK2000, and 13.9 percentage points if we set the cutoff at MK1000 or more. Total spending on unprocessed maize rises by MK259.

Columns 5 to 7 present two different falsification tests for the maize results. The first is to see whether smaller purchases of maize also increase, which would indicate maize purchases increased in general, not just bulk purchases. We see no statistically-significant effect on

---

<sup>21</sup> One issue with our results is that 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. 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.

maize transactions that are smaller than MK2,000. The second is to look at purchases of processed maize flour. Maize can be stored for a fairly long time, but must be turned into flour in order to make the maize porridge (*nsima*) that is eaten as part of almost all meals. Purchasing maize flour is more expensive than buying unprocessed maize in bulk and grinding it into flour oneself, and maize flour does not store as well as unprocessed maize, so we would not expect any effects on flour purchases. Consistent with that expectation, we see no statistically-significant change in bulk purchases of maize flour nor in total spending on maize flour.

### Differences in effects across rounds

The estimated impacts on bond take-up and spending are highly heterogeneous by round. We present separate regressions by round for transparency. The estimated treatment effect is statistically significant overall (Table 5, Panel A), and when we restrict the sample to round two (Panel C), but not when we restrict the sample to round one (Panel B).<sup>22</sup> The varying treatment effects potentially reflect differences in the economic environment, the intervention, and the setup of the bond product across rounds.

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 MK2,604 since the previous Friday; this figure fell to MK2,010 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 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 on 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

---

<sup>22</sup> Our design also allows us to study the effect of round one treatment status on round two outcomes. The results (see Appendix Table E.2) suggest that receiving a lump sum wage payment in round one may increase bond purchases in round two. The point estimate is positive and about half of the contemporaneous treatment effect; however, the coefficient is not precisely estimated ( $p = 0.22$ ).

the effect of the lump sum treatment on average income receipt was only MK1,301 in round one, which is less than the MK1,500 minimum cost of purchasing a bond. In contrast, in round two the lump sum treatment raises average income received since Friday by MK2,047, easily clearing the MK1,500 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 control-group take-up is lower in round two than in round one.<sup>23</sup>

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 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 in the timing of the announcement 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.

Therefore, while the overall bond results are driven by round two, we cannot draw strong conclusions from this pattern because several features of the intervention, bond offer, and environment changed — preventing us from making *ceteris paribus* comparisons of individual features across rounds. An argument can be made that the second-round effects are more

---

<sup>23</sup> A particular concern for our results is that exposure to the lump-sum payment scheme could have increased trust in the bond sales. We do not think that trust effects drive the higher take-up of the bond in the lump sum payment group, for two reasons. First, the fact that the project staff actually followed through on the lump sum payments was visible to all workers, since everyone received their pay at the same time. Therefore any effects on trust should have applied to both weekly and lump sum wage recipients. Second, we would expect trust in the bond to spill over across rounds, making workers more responsive to the lump sum wage payments in round two if they also received lump sum wages in round one. We see no evidence of this kind of order effect (Appendix Table E.1).

informative, because both groups had advance notice about the possibility of purchasing the bond. However, because many features of the experiment varied across rounds, and to minimize researcher degrees of freedom, we choose to focus on the pooled results as our best estimate of the effect of lump-sum wage payments on bond purchases.

We do have evidence that the timing of the bond offer was not the *only* reason for the changes in our results across rounds. Panels B and C of Table 6 show that the treatment effects on bulk purchases of maize are substantially larger in round two.<sup>24</sup> As this investment is unrelated to the timing of the bond announcement, these results suggest that the larger treatment effects in round two are driven at least in part by the larger wage payments or seasonal changes in income.

A difference in results across rounds driven by the intensity of the lump-sum wage treatment is exactly in line with the predictions of our theoretical framework from Section 1. We showed that a shift in income timing toward lump-sum wage payments raise the attractiveness of a liquid, indivisible investment like the bond or bulk purchases of maize. If such a shift is large enough, we show that workers will switch into purchasing the indivisible investment. Conversely, however, a change in income timing that is too small will not induce any additional purchases of the investment. This is consistent with the pattern we see across the two rounds of our experiment: the change in income timing is smaller in round one, and so are the effects on bond and bulk maize purchases.

## 6 Credit vs. Savings Constraints

We now interpret the empirical evidence through the lens of the model developed in section 1. Both the version of the model with credit constraints alone and the version that includes a savings constraint predict higher bond purchases under the lump sum payment scheme. But they do so for very different reasons, which allows us to differentiate between the two versions of the model.

---

<sup>24</sup> Maize prices are strongly seasonal in Malawi, and increase rapidly during the lean-season period covered by our study. According to the FAO, prices near our study site rose by 23% during round one and 13% during round two of our study. Differences in effects on bulk maize purchases across rounds could therefore be driven by maize becoming more expensive rather than real effects on maize quantities. To address this potential issue, Appendix J replicates our results but adjusts all maize prices to their round two values. The results are substantively the same, but the round one effects are slightly larger than with the unadjusted variables. The round two effects are still sharply higher than the round one effects with the adjustment (14.5 vs. 5.8 percentage points,  $p = 0.087$ ).

## 6.1 Demand for liquid savings and durable asset purchases

In the credit constraints-only case, the workers induced to buy the bond in the lump sum group are forced to oversave in period 1: they would have liked to spend more, but faced a binding credit constraint. In period 2, they buy the short-term bond since the bond was the best short-term use of the resulting excess liquidity. These workers, thus, had an *ex ante* preference for a smoother expenditure profile. We should therefore expect them to generally have relatively high demand for highly liquid savings options in period 2, as they seek ways to smooth out the spike in income they receive on the last payday. This would include the bond but also bulk purchases of maize.

Bulk purchases of maize play the same role as the bond,  $b$ , in our model. The investment is indivisible, since you need to purchase a large quantity at once to exploit bulk discounts. It also pays a higher return than holding cash: while the bond pays an explicit monetary return in period 3, bulk maize purchases raise consumption over several future periods and are a high-return investment (see Section 5.1 for details). We can represent that increase in consumption as a return paid in the third period. Importantly, both the bond and bulk maize purchases are effectively very liquid investments. The bond becomes cash again in the third period, and maize stockpiles substitute for regular cash expenditures on food. By a similar token, we should expect workers to have relatively low demand for durable good purchases. The latter lock up liquidity in the short-run and provide utility only over a much longer time horizon.

In contrast, in the savings constraints case, the workers induced to purchase the bond by the switch from weekly to lump-sum wage payments are those who would have preferred to buy the bond under weekly payments but faced a binding period 1 savings constraint. This savings constraint was alleviated by the shift in income from period 1 to period 2 induced by the lump sum wage payments. This type of worker would have liked to save up their extra income from the project, but could not do so under weekly wage payments because they were savings constrained.

To relate this to the model, consider the group of workers who want to buy a durable asset. That group is more likely to face a binding savings constraint under weekly wage payments. Formally, take  $b$  from the model to stand for a durable asset whose return is not money paid out in period 3 but is instead the discounted future stream of utility from the durable asset.<sup>25</sup> Those workers may lack access to credit to buy the durable asset, but

---

<sup>25</sup> This analogy is not exact since without a second good the marginal rate of substitution between the return from the asset and regular consumption is forced to be unity, and demand for the asset is driven by fixed return of  $R > 1$ . In contrast, in a model with two separate goods the demand for a durable good is driven precisely by imperfect substitutability between durables and other consumption.

they could have saved up for it over four weeks in the absence of savings constraints. Hence, if binding savings constraints in period 1 are indeed important in our experiment, then we should plausibly expect large positive effects of the lump sum payment regime on durable asset purchases in period 2.<sup>26</sup>

Our results on bulk maize purchases and durable assets are more in line with the predictions of the version of the model with credit constraints alone. As shown in Section 5 above, we find that the lump sum group buys more maize in bulk and does not have higher durable asset purchases. These effects hold for the pooled data and when separated by rounds.

## 6.2 Baseline preferences

The theory outlined in Section 1 tells us how the two model variations (credit constraints alone vs. savings constraints) map into baseline preferences. In the version with credit constraints alone, workers induced to buy the bond by the lump sum payments scheme will prefer weekly payments when asked at baseline. In the version with savings constraints, workers induced to buy the bond by the lump sum payment scheme should prefer to be paid in a lump sum when asked at baseline. More generally, discounting should drive demand for weekly payments. Our model ignores discounting for the sake of simplicity. However, if workers discount the future, even in the absence of liquidity constraints they should initially prefer the weekly payments since income arrives earlier.

At baseline, we asked an unincentivized question about which payment structure workers would prefer. Note that at this point workers were not yet allocated to experimental arms. The question imposed the same rules as the actual intervention: everyone had to show up on all four paydays irrespective of when they actually got the money. Under these conditions, 74% of workers preferred the deferred, lump-sum payments.<sup>27</sup>

If our results are mainly driven by savings constraints, all else equal we should find that effects of the lump sum payment regime are concentrated among those who preferred the lump sum before the start of the project. However, we find that effects do not vary significantly by baseline preference and are far from being concentrated among those who stated a preference for lump sums. To the extent that the stated preferences capture true preferences, this militates against savings constraint as an explanation.

---

<sup>26</sup> A clear caveat to this interpretation is that technically those workers could have first held off on buying the durable asset in order to buy the short-term bond, and then bought the durable asset two weeks later. In that case, we would not observe an effect on asset purchases even among the savings-constrained workers. However, this strategy requires additional planning; the fact that the overall level of bond purchases was low suggests that bond buying was not the obvious optimal choice for many workers.

<sup>27</sup> Two workers stated that they were indifferent between weekly and lump-sum payments; they are included in the denominator for this percentage

To test if treatment effects vary, we interact the treatment indicator with baseline preferences. The results are shown in Appendix Table F.2 for key outcomes (bond purchases, assets, and bulk maize purchases). For our main regressions using the pooled data across both rounds, the coefficient estimates are never larger, and never statistically significantly different, for those who preferred the lump sum at baseline. While we lack the precision needed to make definite statements, the pattern of results does not suggest that results were mainly concentrated among those who initially preferred the lump sum.

### 6.3 A temptation experiment without effects

One 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 (Gul and Pesendorfer, 2001; Fudenberg and Levine, 2006; Ozdenoren, Salant and Silverman, 2012; Banerjee and Mullainathan, 2010). Analyses of financial decision-making in the developing world increasingly examine temptation spending in contexts ranging from cash transfers (Haushofer and Shapiro, 2016) to microfinance (Banerjee et al., 2015) to banking (Ashraf, Karlan and Yin, 2006).

Motivated by this body of research, we ran a cross-randomized experiment that manipulated how tempting the spending environment was when workers received their income. For the temptation experiment, workers were randomized into receiving their wages either on Friday or Saturday. Both groups picked up their money at the site of major local market day, but the market day only takes place on Saturdays. Market days are the main situation described by respondents in this study when asked about the most tempting situations faced. Thus workers in the Saturday wage payment arm were exposed to a more-tempting environment while they had liquid cash on hand.

However, the cross-randomized market-day payment intervention does not appreciably change the effects of the deferred wages treatment (Appendix G). To the extent that temptations to overspend are a driver of savings constraints, not finding an effect of the cross-randomized experiment is evidence against the importance of savings constraints for driving actual behavior — but it is consistent with the credit constraints-only version of our model. An alternative explanation for this lack of effects, however, is that the cross-randomized intervention was unsuccessful in altering temptation. In line with this explanation, there are also no main effects of the market-day wage payment treatment.

## 6.4 Worker preferences at baseline: a puzzle

The evidence from our study can be readily explained by a model with credit constraints only, without the need to also add a binding savings constraint. The credit constraints model, however, implies that the workers’ stated *ex ante* preferences for lump-sum payments present a puzzle. In a model with credit constraints only, workers who are induced to buy the bond under the lump sum regime are made worse off by the lump sum payment scheme; they would have preferred weekly payments. Thus an *ex ante* preference for lump sum wage payments is not consistent with a model of credit constraints alone. Nevertheless, the majority of workers said they prefer the lump sum regime when asked at baseline. There are several potential explanations for this puzzle. We outline each in turn, but note that we have limited ability to provide convincing evidence for or against any particular one.

**Inattention or misunderstanding.** We obtained preferences based on an unincentivized, hypothetical question. Respondents may not have thought carefully given the absence of consequences. Furthermore, they may not have fully processed the hypothetical situation, resulting in misunderstandings and careless responses that could have led to biased or noisy answers. The answers to additional survey questions indicate, however, that respondents gave at least some thought to their answers. We asked about the reason for their choice of either the weekly or the lump sum payment scheme. The questions were asked open-ended, with pre-coded answer options for enumerators based on common responses we received in pilot-testing, including an “Other” option. The stated reasons for each chosen income stream are reported in Appendix Table F.1 and are very much consistent with the respective choice. Among workers who preferred the lump sum regime ( $N = 256$ ), 83% ( $N = 212$ ) say they did so because they can “make a better plan for the money” and 14% ( $N = 35$ ) say it will help them “not waste their money,” in line with the idea that the deferred lump sum helps workers overcome internal savings constraints. Among workers who preferred the weekly regime ( $N = 89$ ), 91% ( $N = 81$ ) state they would rather “have some money to buy necessary things”, in line with a preference for a smooth spending pattern over the course of the payment period.

**Experimenter demand effects.** Experimenter demand effects could have led workers to misstate their preference, given the hypothetical nature of the question (Orne, 1962; de Quidt, Haushofer and Roth, 2018). If respondents thought that indicating a lump sum preference is what enumerators wanted to hear they may have been drawn to that response. We cannot provide direct evidence that bears on this question but we believe this is a somewhat unlikely explanation for several reasons. First, all workers had already been enrolled in the study — and thus were guaranteed to receive any explicit benefit that was promised. Second, respondents were told their survey answers would not affect any of their

wage payments. Third, at no point in any of the interactions between the enumerators and the respondents was the lump sum described as a beneficial or more “prudent” option. The lump sum payments are not described anywhere else on the baseline survey. During subject recruitment, no special emphasis was put on the lump sum study arms. The four potential payment schemes<sup>28</sup> were labeled from A to D; the lump sum arms were B and D. The study description was kept general both in respondent consent forms and in training materials for enumerators.

**Substitution effects.** It is possible that many workers, when asked at baseline, truly do prefer the delayed lump sum, at least slightly. However, the consequences of receiving one’s preferred income stream may be small due to substitution effects. If workers can find alternate ways to generate liquidity that is approximately in line with their preferences, the types of income timing variations we are testing might simply not have meaningful impacts. This could be accomplished using informal savings options or by adjusting casual labor supply. In this scenario preferences for the lump sum stream could not have been too strong; the stronger the preference, the more likely it would manifest itself in the form of treatment effects. However, since we do not have a measure of preference intensity, it is possible that the skew of baseline preferences towards the lump sum regime is not particularly strong for any of the individuals in our sample.

**Heterogeneity of preferences.** Another possible way to reconcile the baseline preferences with our results is that the treatment effects may arise from a combination of some workers who face a binding savings constraint and some who face only credit constraints. Both subgroups would experience treatment effects, albeit for very different reasons. This would imply that savings constraints are at least part of the reason for our results.

The possibility that our treatment effects are driven by savings-constrained workers as well as credit-constrained workers is supported by survey evidence about the context, which strongly suggests that saving in this environment is challenging. 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 the sample. 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 (Brune, Chyn and Kerwin, 2018). Workers who did lose money lost the equivalent of 15% of monthly earnings. The majority of that sample reports losing cash

---

<sup>28</sup> Weekly vs. lump sum payments crossed with the market day vs. non-market day payment timing.

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.

## 7 Conclusion

Financial markets in developing countries are imperfect. People adapt to these imperfections by developing informal arrangements and institutions to replace formal structures that are either flawed or missing entirely. This paper analyzes one such adaptation — structuring wage payments so that they are received in later, concentrated lump sums instead of smooth streams. Such deferred lump-sum wage payments appear to be commonly desired in developing countries — 74% of our sample stated that they preferred such an arrangement — but no previous research tests if they are actually beneficial.

Using a randomized experiment with a sample of 359 workers in rural Malawi, we show that deferred lump-sum wage payments lead to substantial increases in investment. Our measure of investment is purchases of a bond that we designed and offered for sale as part of the study, which paid a 33% return over two weeks with zero risk. One interpretation of our results is that the workers in our sample were savings-constrained, and the deferred lump-sum wage payments relieved that constraint and allowed them to save up for a desirable asset.

An alternative explanation for our results is that the workers were credit-constrained, and that the deferred lump-sum wage payments forced them to “oversave” their earnings. This left them with excess liquidity after the lump-sum payout; the bond served as an attractive way to smooth that liquidity into later periods.

We develop a model to show that our main results can be rationalized by both credit constraints and savings constraints during the wage payment period. However, other evidence from our study suggests that credit constraints alone could explain our findings. In particular, we see no effects on purchases of durable assets, and workers who stated a preference for deferred lump-sum wage payments do not experience larger treatment effects from the shift to their preferred payment structure. Moreover, in order to explain our findings via savings constraints, we must also assume a binding credit constraint at the time of the bond sales; otherwise, workers could simply borrow to purchase the bond and then repay the loan with the proceeds.

Although either credit constraints or barriers to saving are theoretically consistent with our main results, the two models have very different welfare implications. Under the credit constraints model, the workers induced to buy the bond by the lump sum wage payments

would have been better off receiving weekly wage payments and not purchasing the bond. Those workers are credit-constrained under the lump sum payment and are only induced to buy the bond because they are prevented from smoothing their incomes and end up with extra money in period 2. In contrast, in the savings constraint model, some of the workers are prevented from buying the bond under weekly wage payments because they face a binding period 1 savings constraint. Those workers are made better off by the lump sum wage payments, which allow them to overcome the otherwise-binding savings constraint and purchase the bond. These divergent welfare implications mean that understanding the mechanism behind our results is crucial for evaluating whether deferred lump-sum wage payments would be a useful policy in developing countries. They also suggest that policymakers should be cautious in using people's stated, hypothetical preferences about income timing to design payment systems; in our context, it appears that adhering to workers' stated preferences would have made them worse off.

Finding that deferred lump-sum wage payments made workers worse off would be in line with 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). Recent research by Aguila, Kapteyn and Perez-Arce (2017) reaches similar conclusions using data from Mexico: people who were paid less frequently have more-volatile consumption, but purchase more durable assets.

Three features of our study may limit the generalizability of our results. First, the bond we study in this paper was created as part of the study, and we do not find impacts on actual assets. However, the bond was designed to match key features of other common investments and major purchases in developing Africa. It is indivisible, much like school fees or common home improvements such as corrugated iron roofing sheets, and its cost is comparable to the minimum scale of those expenditures. Moreover, we do see impacts on bulk purchases of maize, which has the properties of an indivisible investment due to its convex returns. Second, our results are specific to the type of income stream faced by the workers in our sample, who are earning irregular, one-off income that supplements their main livelihood. Other types of workers will not necessarily respond in the same way. That said, 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. Thus we believe our results are likely to be informative about the effects of deferred lump-sum wage payments on investment decisions in a wide variety of developing-country contexts. Third, the lump-sum wage payments we study are not voluntary: workers were randomly opted in to them irrespective of their preferred payment scheme. Thus our

findings are relevant for understanding the impacts of an employer or another payer switching everyone from weekly to monthly payments. They are less informative for understanding the effects of optional lump sum payment schemes.

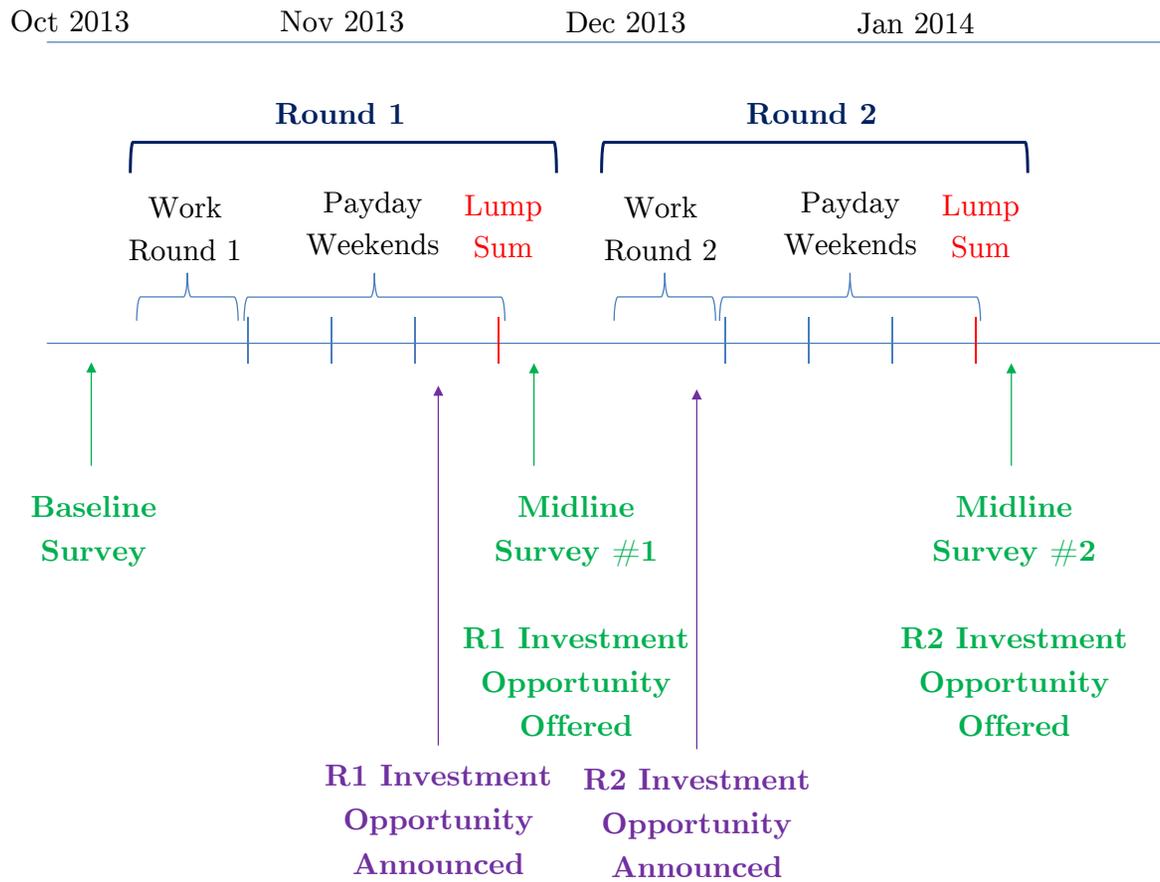
The results in this paper also provide a useful lesson for future research on financial inclusion in developing countries. Offering study participants a meaningful investment opportunity that bears actual interest can be a helpful way to isolate an intervention's effects on savings constraints. Other outcomes have two important limitations: non-financial investments such as health and education may not be perceived as investments by respondents, and even when an investment earns direct financial returns, heterogeneity in those returns may generate misleading inferences about the benefits of an intervention.

## References

- Aguila, Emma, Arie Kapteyn, and Francisco Perez-Arce.** 2017. "Consumption Smoothing and Frequency of Benefit Payments of Cash Transfer Programs." *American Economic Review*, 107(5): 430–35.
- Ashraf, Nava, Dean Karlan, and Wesley Yin.** 2006. "Tying Odysseus to the mast: Evidence from a commitment savings product in the Philippines." *Quarterly Journal of Economics*, 121(2): 635–672.
- Banerjee, Abhijit, Esther Duflo, Rachel Glennerster, and Cynthia Kinnan.** 2015. "The Miracle of Microfinance? Evidence from a Randomized Evaluation." *American Economic Journal: Applied Economics*, 7(1): 22–53.
- Banerjee, Abhijit V., and Sendhil Mullainathan.** 2010. "The Shape of Temptation: Implications for the Economic Lives of the Poor." NBER Working Paper No. 15973, Cambridge, MA.
- Beegle, Kathleen, Emanuela Galasso, and Jessica Goldberg.** 2017. "Direct and indirect effects of Malawi's public works program on food security." *Journal of Development Economics*, 128: 1–23.
- Besley, Timothy.** 1995. "Savings, credit and insurance." In *Handbook of Development Economics*. Vol. 3. 1 ed., , ed. Jere R. Behrman and T. N Srinivasan, 2123–2207. Elsevier.
- Besley, Timothy, Stephen Coate, and Glenn Loury.** 1993. "The Economics of Rotating Savings and Credit Associations." *American Economic Review*, 83(4): 792–810.

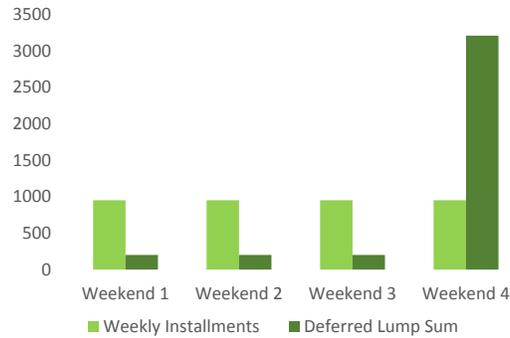
- Brune, Lasse, Eric Chyn, and Jason T. Kerwin.** 2018. “Pay Me Later: A Simple Employer-Based Savings Scheme.” Working Paper.
- Burke, Marshall, Lauren Falcao Bergquist, and Edward Miguel.** 2018. “Sell Low and Buy High: Arbitrage and Local Price Effects in Kenyan Markets.” *The Quarterly Journal of Economics*, forthcoming.
- Carter, Michael R., Rachid Laajaj, and Dean Yang.** 2015. “Directed vs. Enabling Interventions: A Study of Fertilizer Subsidies and Savings in Rural Mozambique.” Working Paper.
- Casaburi, Lorenzo, and Rocco Macchiavello.** 2018. “Demand and Supply of Infrequent Payments as a Commitment Device: Evidence from Kenya.” *American Economic Review*, forthcoming.
- Collins, Daryl, Jonathan Morduch, Stuart Rutherford, and Orlanda Ruthven.** 2009. *Portfolios of the Poor, How the World’s Poor Live on \$2 a Day*. New Jersey: Princeton University Press.
- de Quidt, Jonathan, Johannes Haushofer, and Christopher Roth.** 2018. “Measuring and Bounding Experimenter Demand.” *American Economic Review*, 108(11): 3266–3302.
- Duflo, Esther, Michael Kremer, and Jonathan Robinson.** 2011. “Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya.” *American Economic Review*, 101(6): 2350–2390.
- Fudenberg, Drew, and David K. Levine.** 2006. “A Dual-Self Model of Impulse Control.” *American Economic Review*, 96(5): 1449–1476.
- Gul, Faruk, and Wolfgang Pesendorfer.** 2001. “Temptation and self-control.” *Econometrica*, 69(6): 1403–1435.
- Hastings, Justine, and Ebonya Washington.** 2010. “The First of the Month Effect: Consumer Behavior and Store Responses.” *American Economic Journal: Economic Policy*, 2(2): 142–62.
- Haushofer, Johannes, and Jeremy Shapiro.** 2016. “The Short-term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya.” *Quarterly Journal of Economics*, 131(4): 1973–2042.

- Jakiela, Pamela, and Owen Ozier.** 2016. “Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies.” *Review of Economic Studies*, 83(1): 231–268.
- Jappelli, Tullio, and Luigi Pistaferri.** 2017. “Chapter 7: The Buffer Stock Model.” In *The Economics of Consumption: Theory and Evidence*. 115–130. Oxford University Press.
- Karlan, Dean, Aishwarya Ratan, and Jonathan Zinman.** 2014. “Savings by and for the Poor: A Research Review and Agenda.” *Review of Income and Wealth*, 60(1): 36–78.
- Kramer, Berber, and David Kunst.** 2018. “Intertemporal Choice and Income Regularity: Non-Fungibility in a Lab-in-the-Field Experiment.” Tinbergen University Discussion Paper 2018-012/V.
- Micheltore, Katherine, and Lauren Jones.** 2015. “Timing is Money: Does Lump-Sum Payment of Tax Credits Induce High-Cost Borrowing?” Working Paper, University of Michigan.
- Orne, Martin T.** 1962. “On the social psychology of the psychological experiment: With particular reference to demand characteristics and their implications.” *American Psychologist*, 17(11): 776–783.
- Ozdenoren, Emre, Stephen W. Salant, and Dan Silverman.** 2012. “Willpower and the Optimal Control of Visceral Urges.” *Journal of the European Economic Association*, 10(2): 342–368.
- Shapiro, Jesse M.** 2005. “Is there a daily discount rate? Evidence from the food stamp nutrition cycle.” *Journal of Public Economics*, 89(2–3): 303–325.
- Stephens Jr., Melvin.** 2003. “‘3rd of the Month’: Do Social Security Recipients Smooth Consumption Between Checks?” *American Economic Review*, 93(1): 406–422.

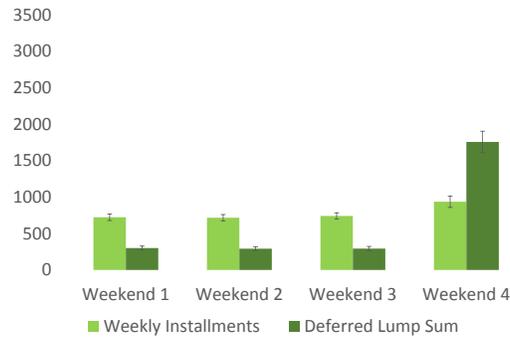


**Figure 1**  
Timing of work, payments and data collection

**Panel A:** Wage payments on payday (MK)



**Panel B:** Expenditures on payday (MK)



**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.

Whiskers in Panel B indicate 95% confidence intervals. No confidence intervals are shown in Panel A because there is no variation in wages within a given bar.

**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						
<b>Panel A: Round 1</b>															
Weekly Payments, Friday	84	<b>800</b>	100	<b>800</b>	100	<b>800</b>	100	<b>800</b>	100						
Weekly Payments, Saturday	89	100	<b>800</b>	100	<b>800</b>	100	<b>800</b>	100	<b>800</b>						
Lump Sum Payment, Friday	87	100	100	100	100	100	100	<b>2,900</b>	100						
Lump Sum Payment, Saturday	83	100	100	100	100	100	100	100	<b>2,900</b>						
	$\Sigma=343$														
<b>Panel B: Round 2</b>															
Weekly Payments, Friday	85	<b>900</b>	100	<b>900</b>	100	<b>900</b>	100	<b>900</b>	100						
Weekly Payments, Saturday	86	100	<b>900</b>	100	<b>900</b>	100	<b>900</b>	100	<b>900</b>						
Lump Sum Payment, Friday	90	100	100	100	100	100	100	<b>3,300</b>	100						
Lump Sum Payment, Saturday	85	100	100	100	100	100	100	100	<b>3,300</b>						
	$\Sigma=346$														

*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

	Weekly wage payments			Lump sum wage payments			Balance test
	Mean	SD	N	Mean	SD	N	<i>p</i> -value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Background characteristics</i>							
Male	0.31	0.46	350	0.33	0.47	350	0.495
Married	0.71	0.45	343	0.68	0.47	345	0.332
Age (Years)	39.67	15.21	350	40.09	15.46	350	0.717
Years of Education Completed	3.45	3.08	346	3.59	3.23	348	0.556
Midline survey date (days after Sunday)	2.59	1.15	344	2.49	1.14	345	0.237
Prefers lump sum wage payments	0.75	0.43	349	0.72	0.45	349	0.498
<i>Financial outcomes (in units of MK unless noted)</i>							
Income received since past Friday	3,482	9,488	350	2,522	4,003	350	0.174
Remaining cash holdings out of income received	717	2,763	350	568	2,300	350	0.380
Total spending since Friday	3,789	4,657	350	3,626	4,019	350	0.627
Asset Ownership (PCA)	-0.09	2.65	350	0.09	2.71	350	0.357
Loans received in past month	2,962	13,085	350	2,931	8,620	350	0.977
Loans made in past month	779	3,203	350	687	2,705	350	0.612
Transfers received in past month	922	2,462	350	785	1,848	350	0.381
Transfers made in past month	572	2,525	350	623	1,912	350	0.807
<i>p</i> -value from joint significance of 12 covariates:			0.64				

*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 D.

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 <sup>†</sup> since Friday from itemized expenditure data (MK)
	Paydays 1-3	Payday 4	Paydays 1-4				
<b>Lump sum wage payments</b>	<b>-1,095***</b> (50.96)	<b>577.5***</b> (64.68)	<b>-518.0***</b> (84.53)	<b>-0.173***</b> (0.0283)	<b>1,656***</b> (172.7)	<b>145.3**</b> (71.57)	<b>365.3**</b> (153.3)
Dependent variable mean, weekly wage payments group	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. Boldface type indicates the treatment variable of interest. 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 D. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

† 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)	(6)	(7)	(8)	(9)	(10)
<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)	
	Level	1(Level>0)	Level	1(Level>0)	Level	1(Level>0)	Level	1(Level>0)	Level	1(Level>0)
	<b>Lump sum wage payments</b>	<b>-176.1</b> (527.6)	<b>0.00984</b> (0.0383)	<b>-119.3</b> (365.8)	<b>0.0382</b> (0.0373)	<b>-144.3</b> (118.0)	<b>-0.0441</b> (0.0310)	<b>-171.6*</b> (101.8)	<b>-0.0466</b> (0.0376)	<b>-30.15</b> (44.48)
Dependent variable mean, weekly wage payments group	2,271	0.468	2,008	0.422	596.2	0.276	688.9	0.494	249.7	0.439
Number of observations	689	689	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). Boldface type indicates the treatment variable of interest. 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 1<sup>st</sup> in round one and since January 1<sup>st</sup> 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 D. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

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

	(1)	(2)	(3)	(4)
	<b>Administrative data on bond sales</b>		<b>Survey data</b>	
Dependent variable:	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>				
<b>Lump sum wage payments</b>	<b>0.0508**</b> (0.0250)	<b>124.7**</b> (59.88)	<b>145.3**</b> (71.57)	<b>1,656***</b> (172.7)
Dependent variable mean, weekly wage payments group	0.108	226.7	468.5	2,309
Number of observations	689	689	689	689
<u>Panel B - Round 1 only</u>				
<b>Lump sum wage payments</b>	<b>0.0101</b> (0.0385)	<b>62.15</b> (81.63)	<b>39.37</b> (108.3)	<b>1,301***</b> (281.5)
Dependent variable mean, weekly wage payments group	0.150	277.5	543.0	2,604
Number of observations	343	343	343	343
<u>Panel C - Round 2 only</u>				
<b>Lump sum wage payments</b>	<b>0.0918***</b> (0.0330)	<b>188.9**</b> (85.86)	<b>258.7**</b> (101.0)	<b>2,047***</b> (207.4)
Dependent variable mean, weekly wage payments group	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). Boldface type indicates the treatment variable of interest. 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. Columns 3 and 4 replicate columns 6 and 5 respectively from Table 3, for ease of reference. 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 D. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

**Table 6**  
Effects of lump sum payments on bulk purchases of unprocessed maize

<u>Dependent variable:</u>	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	<b>Bulk purchases of unprocessed maize</b>				<b>Falsification test: Other maize purchases</b>		
	1(Any unprocessed maize purchase $\geq$ MK 2000)	1(Any unprocessed maize purchase $>$ MK 2000)	1(Any unprocessed maize purchase $\geq$ MK 1000)	Total spending on unprocessed maize (MK)	1(Any unprocessed maize purchase $<$ MK2000)	1(Any maize flour purchase $\geq$ MK 2000)	Total spending on maize flour (MK)
<u>Panel A - Round 1 and 2 pooled</u>							
<b>Lump sum wage payments</b>	<b>0.102***</b> (0.0221)	<b>0.0522***</b> (0.0192)	<b>0.139***</b> (0.0343)	<b>259.2***</b> (78.08)	<b>-0.0330</b> (0.0360)	<b>0.0170</b> (0.0164)	<b>-7.720</b> (66.31)
Dependent variable mean, weekly wage payments group	0.0581	0.0494	0.203	583.1	0.390	0.0378	437.9
Number of observations	689	689	689	689	689	689	689
<u>Panel B - Round 1 only</u>							
<b>Lump sum wage payments</b>	<b>0.0504</b> (0.0323)	<b>0.0202</b> (0.0289)	<b>0.0662</b> (0.0458)	<b>89.84</b> (123.1)	<b>-0.0112</b> (0.0503)	<b>0.0327</b> (0.0236)	<b>31.31</b> (99.22)
Dependent variable mean, weekly wage payments group	0.0751	0.0636	0.208	630.8	0.358	0.0289	448.8
Number of observations	343	343	343	343	343	343	343
<u>Panel C - Round 2 only</u>							
<b>Lump sum wage payments</b>	<b>0.150***</b> (0.0337)	<b>0.0790***</b> (0.0281)	<b>0.196***</b> (0.0502)	<b>402.6***</b> (111.8)	<b>-0.0634</b> (0.0523)	<b>0.0118</b> (0.0242)	<b>4.451</b> (87.75)
Dependent variable mean, weekly wage payments group	0.0409	0.0351	0.199	534.9	0.421	0.0468	427.0
Number of observations	346	346	346	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). Boldface type indicates the treatment variable of interest. 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 D. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .