

Income Timing, Temptation and Expenditures: A Field Experiment in Malawi

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Abstract

The canonical model of expenditure choices assumes that people are able to smooth their consumption. However, extensive empirical and theoretical work suggests that consumption smoothing is imperfect, so the precise timing of individuals' income may affect their behavior. We report results from a randomized field experiment in Malawi that varied the timing of workers' income receipt in two ways. First, payments were made either in weekly installments or as a monthly lump sum, in order to vary the extent to which workers had to save up to make profitable investments. Second, payments at a local market were made either on the weekend market day (Saturday) or the day before the market day (Friday), in order to vary the degree of temptation workers faced when receiving payments. We provide novel evidence that the frequency of payments matters for workers' ability to benefit from high-return investment opportunities. Workers in the monthly group have more cash left in the week after the last payday when the lump sum payment was made. Moreover, they are 9.5 percentage points more likely than the weekly payment group (mean of 6.3%) to invest in a risk-free short-term "bond" that required a large payment and that was offered by the project in the week after the last payday. We argue that this result is driven by the lump sum group's decreased savings constraints relative to the weekly payment group. In contrast, despite anecdotal evidence and suggestive survey data that, in this study's context, market days increase the temptation to overspend, being paid at the site of the local market on Saturday compared to Friday did not strongly matter for expenditure levels or temptation spending.

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1 Introduction

Savings rates in developing countries appear to be very low. People save very little, whether in cash or other liquid assets. Moreover, despite evidently high returns to investment in domains ranging from health (Jones et al., 2003) to agriculture and small business (de Mel, McKenzie and Woodruff, 2008, 2012), people do not seem to be making those investments. In theory, even in the face of borrowing constraints, if returns are high enough households should be able to save up and invest. However, households appear to have trouble saving: households in developing countries act as if they are “savings constrained”, meaning that shifting liquid wealth across time periods is costly.

Households in developing countries face a range of explicit and implicit “external” costs to savings, e.g. risk of theft, high transaction costs, lack of access to formal savings, or social pressure to share earned income or wealth.¹ In addition, savings constraints can be “internal” – people might be present-biased, causing them to save less than they would like. Present-biased preferences have been documented extensively in laboratory studies, and recent field research has confirmed that some people do exhibit present-biased preferences in the context of real-life choices (Giné et al., 2012). A number of papers have studied the potential of commitment savings accounts to manage this kind of internal savings constraint (Dupas and Robinson, 2013; Ashraf, Karlan and Yin, 2006; Bune et al., 2014).² However, the cause of present-biased preferences, and the best way to mitigate their impact on the poor’s ability to save, remains unclear: in their review of the constraints that hinder savings among the poor, Karlan, Ratan and Zinman (2014) conclude that “remarkably little is known about which behavioral biases actually drive savings behavior.” The canonical model of present bias is the Laibson (1997) model of quasi-hyperbolic discounting, but this sheds little light on why some people are present-biased and others are not. A possible explanation for variations in present bias comes from Banerjee and Mullainathan (2013, henceforth BM), who point out that one potential cause of variation in present bias is temptation: people may be biased toward present consumption because they are tempted to spend on goods and services that they later regret spending on, such as alcohol, tobacco, or fatty foods. Savings constraints could prevent people from saving up for large, discrete purchases (such as certain investments or durable goods), and could prevent people from having access to savings in the case of emergencies.

In light of this documented inability or unwillingness to save, the time structure of income streams is likely to be important. People in developing countries invest considerable effort and expenditure into aggregating streams of small installments of income into lump sums, in order to make purchases that cannot be broken up into small pieces (Collins et al., 2009). As a result, larger income installments may lead to more saving by easing this process. Lump-sum payments could

¹e.g. Jakiela and Ozier (2012) and Goldberg (2011)

²In the developed world, research on self-control (e.g. Thaler and Shefrin (1981)) has identified Christmas clubs – savings accounts that pay no interest and lock up one’s money until December 1st – as a form of commitment savings used to overcome internal savings constraints

also help savings under a BM-style temptation-based model of time inconsistency: BM show that having a larger sum of money on hand can help people overcome the fear that, if they do save, their future self will simply “waste” all of the money on temptation goods. This line of reasoning is consistent with previous research on self-control problems and lump-sum payments: [Thaler and Shefrin \(1981\)](#) argue that a worker who receives part of his salary in a lump-sum bonus (rather than always in equal monthly installments) would be able to save more, since typical rules-of-thumb used to constrain consumption would lead people to spend roughly the same amount as they make in each month.

However, an alternative possibility is that converting smooth income streams into larger, deferred sums will instead lead to increased temptation and potentially poor choices. [Fudenberg and Levine \(2006\)](#) note that ATMs are frequently placed in locations where lottery tickets are sold, or in nightclubs, in order to induce impulse purchases by myopic consumers. This proverbial effect of “money burning a hole in your pocket” is a potential concern in the microfinance industry, where recent research has studied whether access to microcredit can induce temptation spending due to the generation of large lump sums ([Angelucci, Karlan and Zinman, 2013](#)). In addition, this phenomenon is consistent with both theoretical and empirical work in developed countries ([Ozdenoren, Salant and Silverman, 2012](#); [Stephens Jr., 2003](#); [Shapiro, 2005](#)) as well as with anecdotal reports of behavior around payday in developing countries.

In this paper we report results from a field experiment in Malawi designed to examine the role of timing of income for spending and savings decisions and its interaction with issues of self-control. We vary the time structure of wage payments for 363 casual laborers, with workers paid either in four weekly installments or a single lump sum at the end of the month. Our survey data demonstrates that this is a salient and potentially-important variation in how income is received. Before the start of the experiment we described to workers a non-incentivized, purely hypothetical situation in which they have two choices of wage payments: weekly payments or a lump sum payment in the end. Workers were informed that they would be required to come to the same location the same number of times (just as in the experiment we conducted; the hypothetical wage amounts were also nearly identical to the actual ones). 72% of workers said they preferred a lump sum payment. This preference appears to be related to savings constraints: of those 72%, a great majority (83%) stated, in an open ended question with at most one answer, that the reason for this preference is that enables people to “make a better plan” for the money, and an additional 13% openly stated that their reason was to avoid wasteful spending. These answers imply either a commitment problem as the reason for the lump sum preference, or at the least an expected inability to save – either due to internal constraints such as self-control problems or external constraints such as fear of theft.

This first treatment is cross-randomized against a second intervention in which we vary the day of the week on which workers are paid, with half of the sample being paid on Fridays, and half on Saturdays. All payments take place at the same location: the site of the local weekend market, which takes place on Saturdays and is reported to be an extremely tempting environment.

Qualitative evidence from the study area found that people reported market days as tempting environments. This was confirmed by survey responses from our experimental sample. We also use respondents' own perceptions of regretted or mistaken expenditure, as reported on the surveys, as one of our measures of spending on temptation goods. While goods the respondents self-reported as regretted purchases included alcohol, tobacco, and sweets, the most common category was clothing. This is consistent with anecdotal reports from the local area: clothing is a major expenditure at the markets, with people making expensive purchases and then later regretting them. Workers who are paid on Saturdays are therefore exposed to a much more tempting environment at the time when they receive their pay (relative to members of the Friday group), with all other factors being held constant.³

Workers in all study arms receive the same total amount of money: about MK3000, or around 30% of their total cash income over the work period; they are employed in collaboration with a local NGO in two separate rounds of work that are followed by payments with re-randomization of experimental conditions after round 1. The travel and time costs of purchasing goods at the market are held constant across study arms by requiring attendance at the payday site by all participants on all potential paydays, even when they do not receive money.

The experiment has both a practical and a conceptual dimension: it was designed to evaluate the role of internal savings constraints in a practically relevant context – temptations to overspend on paydays and at weekend markets and local trading centers in particular – and to test conceptually the role of temptation in mediating the differential effects on spending of income stream frequency.

Research using randomized variation in the frequency of income streams is rare. To the best of our knowledge, the first experimental studies of the effect of lump sum wage payments relative to smoother streams of labor income are [Beegle, Galasso and Goldberg \(2014\)](#), studying the Malawi Social Action Fund's Public Works Project. They compare outcomes for workers who receive their wages in a single lump sum against those of workers who are paid in 5 installments over the course of 15 days. The variation in the frequency of payments is cross-randomized with the season of employment. One important difference between the two seasons they study is that marginal utility of immediate consumption is generally considered to be low in one season (agricultural investments are more important in the planting season) and high in the other (basic food consumption is more important in the lean season). Our study cross-randomizes the frequency of payments with whether payday is a market day, which anecdotally is considered to induce temptation for immediate spending, or whether payday is a non-market day. Hence we vary the immediate, short-run context in which pay is received rather than the larger seasonal context. In line with the differences in the exact type of variation in payment timing between the two studies the focus of data collection in our study is more short-term. While [Beegle, Galasso and Goldberg \(2014\)](#) collects consumption and expenditure data with a recall period of one week, within a period of up to a month after respondents

³Friday was chosen as the control group, rather than Sunday or Monday, in order to eliminate the possibility that people in the Saturday group save less of their income simply because the time frame is longer.

receive their payments, our study documents short-run differences in spending and saving on the day of receipt of pay and immediately after. Along this dimension, therefore, our study can be viewed as a short-run complement to [Beegle, Galasso and Goldberg \(2014\)](#). Another paper that randomizes whether income is received in lump sums is the [Haushofer and Shapiro \(2013\)](#) evaluation of the GiveDirectly program. The study randomizes the time structure of windfall income, rather than labor earnings, and looks at much longer-run changes in behavior, on the order of one year. They find a decrease in measured cortisol levels among people who receive annual lump-sum, as opposed to monthly installment, transfers, suggesting lower levels of stress.

This paper provides novel empirical evidence in three ways. First, we provide evidence that lump sum payments have an effect on purchases of an actual investment: a high-return, short-term “bond” offered by the project to all respondents. Second, we study the effect of the timing of payments within a week, which has not been examined in the previous literature. Third, we exploit the effect of the timing of payments within a week to explore the role of temptation in driving internal savings constraints.

The potential of temptation-driven waste due to market days, the frequency of payments and their interaction are not merely theoretical concerns. Many organizations in Malawi are presently moving to direct-deposit based payment schemes on an infrequent schedule that bring their employees to major cities on focal dates, potentially triggering the sorts of temptation issues discussed above. One example is Malawi’s Ministry of Education; teachers now receive their pay via direct deposits into their bank accounts, as opposed to cash payments. This in turn induces a large fraction to travel to urban areas once a month to withdraw all their pay in a lump sum. A similar pattern holds for unconditional cash transfers like GiveDirectly: what makes that program logistically feasible is that the payments are sent through the M-Pesa mobile payments service. [Haushofer and Shapiro \(2013\)](#) state that GiveDirectly recipients “typically withdraw the entire balance of the transfer upon receipt.” Since withdrawals must be done at a participating M-Pesa agent, this will tend to draw recipients to potentially-tempting trading centers at the same time as they receive their pay. This study evaluates how infrequent payments and payments on market days in particular influence spending decisions, for a highly-relevant category of income for people in rural Africa. Prior to the beginning of our study, 77% of our sample reported having done informal agricultural work; it is a more common source of cash income than any other activity except for selling one’s own crops for cash. Our intervention also involves a smaller proportion of income these other contexts: GiveDirectly provided income worth more than two months of expenditures, and the Malawi Ministry of Education’s direct deposit program covers all of a teacher’s income. Our respondents received additional income worth approximately 50% of their existing cash income. This limits our ability to draw conclusions about the effect of changing the timing of larger proportions of income, but also means that our study more closely resembles realistic cash transfer programs for people in rural Africa, who are likely to have existing sources of cash income as well.

We present two sets of findings from the experiment: the effect of being paid during the major

local market, and the effect of being paid monthly rather than weekly. In our experimental context, being paid at the site of the local market during the market day, Saturday, does not strongly matter for expenditure decisions relative to being paid at the same location on a Friday – despite strong motivation from anecdotes and suggestive survey data. Drawing on a range of outcomes we document that neither the level nor composition of expenditures exhibits statistically-significant variation by the day of the week that people were paid, and that the frequency of payments does not affect this result. We focus on a set of outcomes related to spending at the market on each Friday and Saturday of the study, for which we can reject even moderate-sized effects of being paid on Saturdays relative to Fridays. However, some of our alternate outcome measures are noisy enough that we cannot conclude that the day of week of income receipt has moderate-sized effects. This result does not conclusively rule out important payday effects in settings other than that of our specific experiment – we discuss external validity in the conclusion – and it does not necessarily imply that self-control more broadly is not a binding constraint for savings. The result should, however, lower our priors about the empirical relevance of the market payday effect, certainly in contexts that are similar to the ones of this study.

In contrast, we find strong effects on spending and savings patterns by payment frequency. While there is no evidence that the composition of expenditures (including in particular self-reported wasteful consumption) varies with payment frequency,⁴ we do find strong evidence that the mode of payment frequency matters for workers’ ability to benefit from high-return investment opportunities with a large minimum investment size. Workers in the monthly group have more cash left in the week after the last payday when the lump sum payment was made. Moreover, they are 9.5 percentage points more likely than the weekly payment group (a relative increase of 151% over the weekly mean of 6.3%) to invest in a risk-free short-term “bond” that required a large minimum installment size payment and that was offered by the project in the week after the last payday. The investment was returned to the respondent together with 33% interest after exactly two weeks. Workers knew about this opportunity before the beginning of round two of the experiment and had gained experience with the product in a pilot offer at the end of the first round.⁵ In total, lump sum group workers spent about twice as much as weekly payment group workers on the investment opportunity. We cannot entirely rule out borrowing constraints as an explanation for this result. However, based on other data, we argue that the result is driven by savings constraints.

These results, using a novel outcome measure for investments with a large minimum installment size, also make an important contribution to existing research on the relationship between

⁴We elaborate on the specific features of this experiment that maybe have mitigated potential effects in the discussion of the empirical results.

⁵We focus here on the effects for round 2 of the study, when all respondents knew about the possibility of purchasing the bond prior to receiving any payments or learning which study arm they were in. The results from round 1, in which the investment opportunity was announced after three of the four weekly payments had been disbursed, are smaller and statistically insignificant. We discuss possible reasons for this difference in Section 5; the most likely explanation is that members of the monthly group had already committed their income to other purposes.

savings constraints and high returns to investment. Previous research has found that the return to investment is high, but that people do not appear to make those investments – implying that people are constrained in their ability to save up for these investments. However, prior studies either have not measured objective returns (relying on e.g. purchases of health products), or have observed high average returns in a cross-section (e.g. cash drop experiments). Research that uses investments in health products as an outcome relies on the assumption that the return to health investment is actually high, and also that respondents understand these high returns. Cash drop experiments also do not necessarily show that people are failing to pursue high-return investments. Under heterogeneous returns and borrowing constraints it is possible to observe high average returns without a binding savings constraint. Those with access to high-return investments might be limited in how much they invest at any given time because they face either a) borrowing constraints or b) they prefer to not decrease present consumption too much. As a result, people do not take advantage of all their high-return investment opportunities, allowing high returns to persist over time. Our experiment resolves both of these concerns. First, we use an actual investment with high returns and zero risk as an outcome. Second, we ensure that returns are homogeneous. In our experiment everyone has access to the same high-return investment offer, but, compared to the lump sum group, the weekly group – who are otherwise identical due to randomization – need to save to be able to invest. We observe that they do invest, but to a much lesser extent. Thus this paper provides novel evidence for savings constraints being a relevant driver of the persistence of the observed high returns to capital in developing countries.

2 Study Design and Data

We designed a randomized experiment with informal agricultural workers from the Mulanje District of Southern Malawi. These workers took part in an expansion of an existing income-generation program that operates in Mulanje District. The subjects in the study received identical nominal⁶ wages for their work, but were randomly assigned to receive the pay with different timing.

We worked with the Mulanje District Executive Council to expand a previously-existing income-generation program to an additional 365 workers⁷, who worked for a total of up to 15 days in two separate rounds of work and payments. This program was part of the Sustainable Livelihoods program run by Mulanje Mountain Conservation Trust (MMCT), an NGO based in Mulanje District that is focused on environmental protection and promoting sustainability in the Mulanje Mountain Forest Reserve and adjoining areas. MMCT provided detailed guidance on how to mirror their

⁶The official inflation rate in Malawi was about 23% per annum during the study period (https://www.rbm.mw/inflation_rates_detailed.aspx), so prices would have risen just 1.7% per month. We therefore ignore the distinction between nominal and real wages for the purposes of our analysis.

⁷The original recruitment included 350 workers two of which dropped early (one never showed up for work; one never showed up to receive his wage); 15 workers were added for round 2 to replace workers who dropped out after the round 1.

existing practices; as with the majority of MMCT’s other projects, work oversight was conducted by officials from partnering government departments of Mulanje District.

The experiment was organized into two rounds that occurred over a period of three months from November 2013 to January 2014, with subjects randomized into treatment conditions separately by round. During each round, subjects worked for two weeks and then received their pay either a) in weekly installments beginning at the end of the second week of work; or b) in a single lump sum, about three weeks after the last day of work. Figure 1 shows the timing of the different components of the experiment: the two rounds of work and payments and the different rounds of data collection. In addition to variation in payment frequency, workers received their pay either c) on Fridays or d) on Saturdays. The two variations on the timing of pay – weekly vs. monthly and Friday vs. Saturday – were cross-randomized, creating four study arms in each round. The distribution of workers into experimental groups is shown in Table 1a (pooled) and Table 1b (separate by round); details of the randomization follow further below. The payments were made at the site of a major local market that occurs on Saturdays, with the intention of inducing variation in people’s temptation to overspend. During the week after the last payday in each round, all workers were visited for a detailed survey about their expenditure and income.

2.1 Recruitment of Workers

We worked with MMCT to locate a set of villages that were potential targets for expanding their Sustainable Livelihoods program. The key criteria for a village to be eligible were:

1. *Location.* Villages had to lie within walking distance of the Forest Reserve, because the work activities supported by the program are centered around natural resource management and conservation.
2. *No previous Sustainable Livelihoods program participation.* Because this was an expansion of the program, we excluded areas that were already actively participating in the program, or which had been included in the past.
3. *Not included in any other recent income-generation programs.* The expansion was targeted toward underserved communities to maximize the benefits brought to the neediest people.
4. *Limited geographic range.* The villages for the study had to be physically close enough to each other to allow work and payroll to be organized across all of them together.

Given the criteria above, we settled on a region of Traditional Authority (TA) Nkanda near the Forest Reserve as the target location for the project; this area had not previously been included in the Sustainable Livelihoods program, nor recently participated in other major income-generating programs such as the Malawi government’s Public Works Programme (PWP). Within that region,

Figure 1: Timing of work, payments and data collection

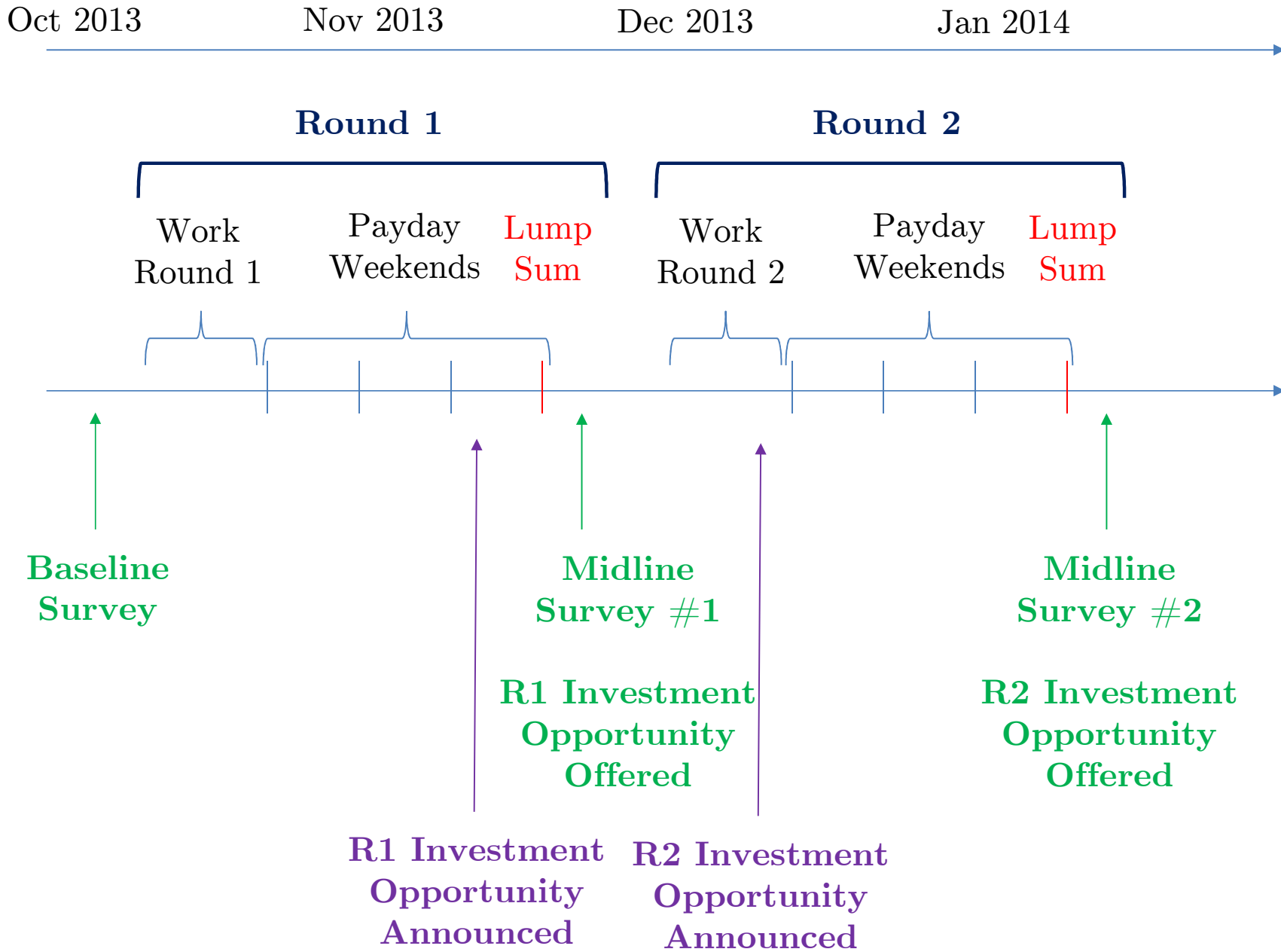


Table 1: Distribution of worker-round observations into experimental groups, (a) pooled across round 1 and 2 and (b) separately for round 1 and round 2

a)

Frequency \ Payday	Payday		
	<u>Friday</u>	<u>Saturday</u>	
<u>Weekly Installment Payments</u>	172	177	349
<u>Single Monthly Payment</u>	178	172	350
	350	349	699

b)

Experimental group	Round 1	Round 2	Total
Weekly Installment Payments, Friday	86	86	172
Weekly Installment Payments, Saturday	89	88	177
Single Monthly Payment, Friday	87	91	178
Single Monthly Payment, Saturday	86	86	172
Total	348	351	699

we picked seven villages that all lie within the catchment area of Mwanamulanje trading centre, one of the largest markets in TA Nkanda.

The selection of workers was handled by the standard operating procedure employed by the Sustainable Livelihoods program. The nature of the program, including the kind of work, the pay rate, and the expected length of employment, was explained at a meeting with the village head and the village development committee (VDC). Each VDC was then tasked with selecting a set of 50 participants and 15 substitutes. They were told to use the same criteria they generally use for deciding who should benefit from social programs. Discussions with MMCT and the VDCs revealed that the main criterion used was generally poverty, with some tendency to favor women as being more likely to be disadvantaged. The VDCs were asked to list the workers in order of preference from 1 to 65, and told we would replace workers who dropped out of the program by moving in order from position 51 to position 65 on the list of workers from their own village. This was done for a total of 15 workers at the end of the first round of the study.

This process generated an initial sample of 350 workers, all of whom were interviewed in a baseline survey. One person dropped out before the work started and one person never showed up at payday (only an additional nine people missed any day of work). After all payments of round 1 were done, 343 workers were successfully interviewed in the Midline 1 survey. Before the start of round 2 of the program, 13 workers left the study, and a total of 15 replacement workers were added.⁸ A total of 352 workers participated in round 2 of the study, of which all but 3 workers had full attendance and 346 were surveyed at Midline 2. The sample is similar to the broader population of the local region in most respects, differing chiefly in ways that are consistent with the selection criteria; for example, we recruited more women (69% compared to 55% in the district) and our sample is slightly worse off socio-economically than the rest of Mulanje District.⁹ We consider the sample to be representative of the type of person likely to be involved in government- or non-government-provided income-generation programs in Mulanje district.

2.2 Random Variation in Income Timing

Our study exploits exogenous variation in the timing of individuals' pay. We designed this to vary in two ways. First, the payments are either in weekly installments for four weeks, or in a single lump sum at the end of the month. Second, the payments are made either on Fridays or Saturdays.

The effect of monthly lump sum payments, as opposed to weekly installments, is theoretically ambiguous. In a context where people have problems aggregating streams of income, receiving one's pay in a lump sum at the end of the payment period would increase take-up of profitable

⁸The study protocol specified that only 13 new workers should have been added (to replace the drop-outs); too many were mistakenly added, and the extra 2 workers were allowed to stay in the study in order to avoid disappointing them after they had already begun working.

⁹See Appendix A for detailed summary statistics on demographic characteristics.

investments that are available after the end of the fourth week. However, if people’s temptation to overspend is an increasing function of their potential immediate consumption, lump-sum payments could reduce savings instead. This would be the case if the lump sum were received concurrently with opportunities to purchase temptation goods, in which case the money could “burn a hole in people’s pockets”, causing them to spend money on things that *ex ante* they would prefer not to purchase. If these were the only two potential mechanisms, the variation in the frequency of pay would allow us to see which one dominates in our sample. However, the lump-sum payment could also increase savings through borrowing constraints, if people would prefer a smoother stream of income and would ideally prefer to borrow against the future lump sum payment.

The variation in the day of the week of the payment is designed to shed light on the mechanisms behind the savings constraints people face. If money is received in a tempting environment, like the local market day, then arguably costs to resisting that temptation increase and workers would decide to spend and consume more right at the market when receiving their pay.

We picked Saturdays at the local trading center – so that payroll for this group happened during the major market in the local area – as a tempting context for the receipt of income. This choice was based on extensive qualitative and descriptive work with people in the local area. Anecdotally, people in Mulanje District often describe market days as tempting situations, in which excitement can cause them to purchase things they would rather not. Our survey data confirms this: for a free-response question about situations that are tempting or in which respondents may “waste” money, 37% of all respondents volunteered Market Days as a tempting situation, by far the most common among those being ever tempted.¹⁰¹¹ Multiple-choice questions confirmed this pattern: 69% of people said that market days are more tempting than the day before market days, and 65% of people said having a lot of cash on hand at the trading center was more tempting than having it on hand elsewhere. Based on these answers, payments during market days could exacerbate temptation-based psychological savings constraints, by inducing people to spend money on tempting goods that they would prefer to save. The alternate day – Friday – should not have the same effect on temptation spending, because the market does not take place on that day.

We chose Friday as the alternate day for several reasons. First, it was logistically simpler to manage payments on two consecutive days than on non-adjacent ones; Sunday was not an option because the vast majority of our sample goes to church on Sunday mornings. Second, using the day before the market ensured that all respondents had the liquid cash needed to make purchases at the market – if we had paid the control group on a later day, then for the first week they would not have had any money to spend at the market on Saturday. Third, and most important, if the control group was paid after the Saturday group, then any differences in savings could simply be a

¹⁰Since 39% of respondents said they were never tempted, this constituted 58% of people who believe they ever waste money. The next-most frequent answer was “Going to the Trading Centre in general (not just market days)” with 4% mentioning it.

¹¹The exact phrasing of the question in English was “In general, what are situations in which you waste money or are tempted to spend money that you would rather not spend?” The term used in the local language has a less-judgmental sense than “waste” does in American English, and this corresponds.

function of having to hang on to the money for a shorter period. By choosing Friday as the control group, we ensured that any such effects worked against the expected direction of the results.

There are also a number of reasons why the Saturday payday might not increase temptation, as well as mechanisms that might mute the effects. First, as noted above, many respondents report that having cash at the trading center is more tempting than having it elsewhere. While this is likely due to the market day itself, part of it could be independent of market days: people might just be more tempted to spend at the trading center even if the weekend market is not currently active; the selection of goods is always greater than at the village. Second, while Saturday is the major market day for the local region, there are other markets nearby that operate on Friday. Third, on an open-ended question about reasons they waste money (where the options were not read aloud), only 42% of people report being spending in response to temptation as one of the reasons they spend money they later regret spending. This is an appreciable fraction, but if it represents all the people who could possibly be affected by the Saturday treatment, any measured effects will tend to be muted.

We employed a within-person cross-randomized design in order to maximize statistical power. Individuals were randomly assigned to one study arm in the first round of the study and then to another study arm (potentially the same one) for the second round. 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 each round of the study, all workers were randomly assigned to one of four study arms: Weekly Installment payments on Fridays, Weekly Installment Payments on Saturdays, Single Monthly Payments on Fridays, or Single Monthly Payments on Saturdays. For the first round, the randomization assignment was stratified by village and gender. The randomization for round 2 was then stratified on the round 1 assignment and village.

2.3 Work Activities

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. There were 7 work days during the first round of the project and 8 days during round two. Workers were employed in conservation-oriented activities that promoted the sustainable use of natural resources. At the beginning of each round of work, representatives from the project met with the workers from each village to help them decide on the specific activities to pursue for that round, based on guidance from MMCT's Sustainable Livelihoods program. The two kinds of work done by the subjects during the study fell under the categories of *Tree Planting* and *Milambala*.

Tree Planting had two separate aspects. During the first round of the project, workers prepared pits for trees to be planted in, and nurseries to house the seedlings for later planting; the seedlings were provided by the Department of Forestry as part of a reforestation program in the area. During round two, which happened once the rainy season had begun, workers did the actual planting of

trees. Milambala is a land conservation activity that focuses on building small bund walls to prevent the inundation of fields and limit environmentally harmful erosion of the topsoil. The principal tools needed for the work were hoes, which all the workers already owned. Milambala also required line levels and ropes, which were provided by the project.

Workers were trained in the tasks for each work activity by officials from Mulanje’s District Forestry and District Agricultural Offices for Tree Planting and Milambala respectively. Progress on the work was also overseen by officials from the two departments, who set targets for the work to be done on each day and checked in to make sure it was accomplished.

2.4 Payroll

Payroll for the project was organized at Mwanamulanje Trading Centre, a major local market in TA Nkanda that was within 4 kilometers of all the villages included in the study. Subjects were informed about how they would be receiving their pay (weekly or monthly, Fridays or Saturdays) at the beginning of each round of work; the procedure was explained verbally, and they were also given a simple handout explaining their group assignment. Each round of work was followed by eight paydaydays: two per week for four weeks, starting on the Friday and Saturday immediately following the end of the work period.

To ensure that transit and time costs were held equal across the four study arms, all subjects were required to come to the payroll site on all eight paydaydays during each round – even when they were not being paid their wages. This also allowed us to collect high-frequency data on people’s cash holdings and spending behavior, via questions that we asked during the payroll administration. In order to encourage attendance and defray some of people’s time costs, all subjects received an MK100 show-up fee 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 monthly on Fridays was required to come to the market on all the preceding Fridays and Saturdays, and received MK100; on the day she received her pay, she received MK100 plus her entire wages for the project. The payment schedule in each round across the four payday weekends resulting from the show-up fees and payment of wages according to treatment group and number of work days are overviewed in Table 2.

MMCT ordinarily manages payroll for its activities using experienced cashiers who work for the organization. For this project, the cashiers were instead employees from the Mulanje District council.

The location and timing of the payroll was specifically chosen to maximize the likelihood that people would be exposed to temptation goods. In pilot testing and qualitative work, people commonly reported market days as periods when they were tempted to spend against their ex ante plans, or tended to waste money. The market at Mwanamulanje happens only on Wednesdays and Saturdays (with Saturdays having the larger market out of the two days), and principally in the morning, which is when people were paid. Shops are still open on Fridays, and there are some mobile vendors, but the majority of market activity happens on Saturdays.

Table 2: Payment schedules by payday group and round (all values in MK)

	Payday weekends							
	#1		#2		#3		#4	
	Fri	Sat	Fri	Sat	Fri	Sat	Fri	Sat
Round 1								
<u>Payment group</u>								
Weekly Installment Payments, Friday	800	100	800	100	800	100	800	100
Weekly Installment Payments, Saturday	100	800	100	800	100	800	100	800
Single Monthly Payment, Friday	100	100	100	100	100	100	2,900	100
Single Monthly Payment, Saturday	100	100	100	100	100	100	100	2,900
Round 2								
<u>Payment group</u>								
Weekly Installment Payments, Friday	900	100	900	100	900	100	900	100
Weekly Installment Payments, Saturday	100	900	100	900	100	900	100	900
Single Monthly Payment, Friday	100	100	100	100	100	100	3,300	100
Single Monthly Payment, Saturday	100	100	100	100	100	100	100	3,300

While the purpose of the show-up fee on non-payday days was to equalize transaction costs across treatment groups and make spending patterns comparable, the fact that some amount of money was paid each time may have reduced the potential to observe differences across groups: it is possible that workers satisfied most of their temptation consumption needs with the MK 100 they received each time they showed up at the market.

2.5 Data

Our data comes from three distinct sources. A detailed survey, focused on expenditures in the past week; several single-item recall questions administered during the payroll; and, as an objective measure of savings behaviors, respondents' choices about purchasing a short-term, high-return, zero-risk investment offered by the project at the end of the second round of the study.

The survey data was collected three times: once at baseline, and once after each round of the study. Subjects were interviewed at their homes, and answered questions about income, assets, savings, and financial transfers, as well as a detailed module about their expenditures since the previous Friday. This module went through a list of goods and asked respondents if they had bought the good since the previous Friday. If they said "yes" to a good, they were asked about how much they bought on each of Friday, Saturday, and Sunday up to now.

Also part of the survey data were a set of questions on wasting money and being tempted to buy things one should not. Respondents were asked about goods that they found particularly tempting, or that they thought they wasted money on, as well as situations in which they felt they wasted money. They were also asked for *ex post* judgments about whether they felt they had wasted money in the period since they received their pay; this question was only included on the survey after the second round.

Our second data source is a set of questions asked during the payroll process. On each of the eight paydays, all respondents were required to come to the payroll site as described above. Prior to receiving their pay or show-up fee, 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 market on the previous payday. Hence on Fridays, people were asked about the money they spent on the Friday of the previous week, and on Saturdays, they were asked about the money they spent yesterday. During the second round of the study, we also asked two additional questions as sensitivity checks: first, we asked people to recall their spending from the Friday of the previous week, to look at the influence of recall bias. Second, we asked people about money they spent outside of the market, in case there were differential patterns in non-market spending.

A third source of data comes from an investment opportunity offered to respondents at the end of each round of the study. Respondents were offered the chance to buy the investment good only once per round, immediately after we visited them for the midline survey for the round in question. The investment took the form of a "bond", with shares that cost MK1500 to purchase and that

paid back the principal plus MK500 interest after exactly two weeks. Each respondent could buy a maximum of two shares, and no fractional shares were allowed. All respondents who purchased the bond were paid back on time according to the terms of the investment.

The investment good was intentionally offered only once per round, in the week after the final payment was made. This allows us to use it to test for the existence of savings constraints, since members of the weekly group had to save their pay in order to use it for the investment good. An alternative design would have offered the investment opportunity each week. This would have lowered the amount of time that the weekly group needed to save in order to purchase it, thus relaxing the savings constraint somewhat. We chose this design in order to maximize our statistical power to detect differences across the two groups.

Summary statistics from these data sources for all variables used in the regression analysis are presented in Table 3, separately for pre-experiment baseline and for outcome variables. At baseline, the households' total spending considering all expenditures from the last Friday prior to being interviewed up to the day of the survey averages MK2,257 (about US\$5.6 or PPP\$14). Respondents report having an average of MK670 (about US\$1.7 or PPP\$4.2) left out of the money they had received since the Friday prior to interviewing. Households spend about 69% of their total expenditures on food for preparation at home, another about 6% on immediate consumption away from home and about 28% on non-food items.¹² About a third of food expenditure was on maize, which is the principal staple crop in the region. Randomization led to a sample with no notable differences in pre-program characteristics across study arms. See discussion in Appendix A.

3 Empirical Specification

We study the effects of the experimentally-induced variation in payment timing on several sets of outcomes: expenditure at the market when payment was received; total expenditure levels and composition over the last weekend of each round, including self-reported wasteful expenditures; asset accumulation; and take-up of the large installment-size, risk-free, high-return investment opportunity.

We present two regression specifications reported as separate panels in the main results tables. The first tests the effect of being randomly assigned to be paid in a single monthly lump sum as relative to four weekly installments. In Panel A of the subsequent tables (and in the only specification shown in Table 7), we run regressions of the form

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

Y_{ir} is the dependent variable of interest for worker i in round r . $\text{SingleMonthlyPayment}_{ir}$

¹²The shares do not add to 1 exactly due to Winsorizing.

Table 3: Summary statistics

	<u>Mean</u>	<u>Std. dev.</u>	<u>10th</u> <u>percentile</u>	<u>Median</u>	<u>90th</u> <u>percentile</u>	<u>Obs.</u>
<u>Baseline variables</u>						
Index of asset ownership	-0.02	2.695	-2.489	-0.713	3.061	342
Total spending since last Friday, inclusive [MK]	2257	3763	200	1000	4600	321
Remaining cash out of received since last Friday, inclusive [MK]	670	2623	0	20	1400	321
Expenditure shares based on itemized elicitation						
Food for consumption at home	0.690	0.214	0.361	0.742	0.937	341
Maize only	0.234	0.260	0.000	0.170	0.605	341
Food for consumption out of home	0.061	0.069	0.000	0.038	0.144	341
Non-Food	0.279	0.235	0.040	0.189	0.655	341
<u>Outcome variables</u>						
<i>Market spending on paydays</i>						
Amount spent on day of wage receipt	1645	1151	200	1500	3200	683
Amount spent at market on Fridays 1, 2, & 3	651	685	200	300	1895	690
Amount spent at market on Saturdays 1, 2, & 3	829	759	200	480	2300	691
Amount spent at market on Friday 4	524	761	50	120	1500	675
Amount spent at market on Saturday 4	823	939	60	500	2300	689
<i>Follow-up survey measures</i>						
Total spending since last Friday, inclusive [MK]	2509	2395	800	2300	4000	689
Remaining cash out of received since last Friday, inclusive [MK]	529	996	0	0	2000	689
Expenditure shares based on itemized elicitation						
Food for consumption at home	0.698	0.212	0.371	0.751	0.930	689
Maize only	0.359	0.266	0.000	0.371	0.709	689
Food for consumption out of home	0.051	0.056	0.000	0.034	0.125	689
Non-Food	0.251	0.206	0.043	0.188	0.572	689
Value of net asset purchases since last interview	2154	7486	0	0	5300	689
<i>Self-reported wasteful spending on weekend 4 of round 2</i>						
Total since last Friday, inclusive [MK]	306	685	0	25	800	346
Friday [MK]	164	462	0	0	400	346
Saturday [MK]	73	256	0	0	150	346
Sunday and after [MK]	66	281	0	0	90	346
<i>Round 2 investment opportunity take-up</i>						
Bought any shares [0/1]	0.108	0.311				351
Total spent on shares [MK]	265	798	0	0	1500	351

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). 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. See Appendix B for variable definitions.

is an indicator variable for individual-level assignment to receive one’s wages in a single payment at the end of the month instead of in four weekly installments, during round r . The coefficient α measures the effect of receiving wages on in a single monthly lump-sum (on either Friday or Saturday). \mathbf{X}_{ir} is a vector that includes stratification cell dummies; two household financial variables measured at baseline prior to the randomized assignment;¹³ and a linear function of the weekday of the exogenously-assigned (first attempted) interview date. The available baseline controls are summarized in Table 3. ε_{ir} is a mean-zero error term.

Whenever data from both rounds are used (so $r=1, 2$ in the equation above) standard errors are clustered at the worker level to account for statistical dependence of outcome measures for the same individual across the two rounds. The stratification cell dummies are separate by round, so these implicitly control for round fixed effects when multiple rounds are used.

Panel B is analogous to Panel A, except the included experimental group indicator compares the impact of being assigned to receive one’s pay on Saturday as opposed to Friday. Regressions are of the form

$$Y_{ir} = \gamma Saturday_{ir} + \beta' \mathbf{X}_{ir} + \varepsilon_{ir} \tag{2}$$

where Y_{ir} and \mathbf{X}_{ir} are defined as above, and $Saturday_{ir}$ is an indicator for assignment to the Saturday payday group. The coefficient γ represents the effect of assignment to the Saturday payday group relative to the Friday payday group. Because the effect of being paid on during the tempting Saturday market may differ by the amount of pay received, these regressions are estimated separately for the workers in the monthly and the weekly study arms.

In general, workers in this project interact with each other and so in theory we cannot exclude that workers assigned to one experimental group had an impact on workers in another. Our design does not allow us to address potential spillovers of effects from one study arm to another. In the context of our design, any spillovers should bias our estimated effects toward zero: for example, if monthly payment group members gave loans to weekly payment group members, this should reduce any differences in expenditures between the two groups. Additionally, we find no empirical evidence of increased cash or in-kind transfers for any of the experimental groups (results not shown).

¹³Our baseline financial controls are an index of asset and livestock ownership (using principal component analysis) and the total amount of money the respondent spent out of their income received since the Friday prior to the baseline survey. Results are not sensitive to the specific choice of baseline financial controls.

4 Empirical Results

4.1 Lump Sum Payment vs. Weekly Payments

We begin by focusing on the effect of receiving a lump-sum payment relative to receiving weekly installments. Workers were randomized into one of the two payment frequency conditions; the lump sum group received wage payments on the last of four weekends at which the weekly payment condition received their wages. However, all workers were required to come to the site where payroll was administered every Friday and Saturday on all four payday weekends, even if no wages were received. Workers received a small “show-up fee” of MK 100 and were also asked the payday questions described in Section 2 above.

Table 4 columns 1 to 5 show the effect of the treatment on the amount people spent on specific days that they came to the market. Because people received income on different days, however, a better comparison is given by column 6, which presents the effect of the treatment on the amount of money people spend at the market on the day that they receive their wages. This variable measures expenditure on Fridays for the Friday condition and on Saturdays for the Saturday condition; it includes spending on all four paydays for the weekly condition, but only on the fourth week of paydays for the lump sum condition. Column 7 presents the same figure, but as a share of income received. Panel A of Table 4 shows the effect of lump sum payments vis-à-vis the weekly payment condition, all of which are strongly statistically significant. Focusing on column 6, we see that respondents in the monthly group spent MK 940 less of their total pay at the market on the same day that they received it. Column 7 shows that they reduced the share of their pay they spent on the day of receipt by 24 percentage points. In payday weekends 1 through 3, when the lump sum condition was not receiving any wages, market expenditure on paydays was lower in the lump sum condition: on Fridays 1, 2 and 3 in total workers only spend about 38% of the average in the weekly payment condition (column 1) and the same rate is about 42% for Saturdays (column 2). On the last payday weekend, when those in the lump sum group receive their wages, expenditures are higher by MK 318 and MK 495, respectively. The increase in the monthly group’s expenditures during the fourth weekend is smaller than their decline in expenditures in weekends 1, 2 and 3.

Table 4 concerned expenditures at the market; Table 5 (Panel A) looks at survey measures of total expenditures during the fourth payday weekend. Table 5 columns 3 through 6 show effects on self-reported wasteful spending (“How much did you spend on items that you later thought you should not have spent money on?”), both in total for the last payday weekend as well as separately for Friday, Saturday and after. Consistent with the payday data about market expenditure, total expenditures over the weekend and into the following week are higher for the lump sum group (by MK 1,451, column 1). Despite the higher spending, cash remaining on hand out of the money received since the Friday prior to the follow-up interview is marginally statistically significantly higher, with a point estimate of ca. MK 139. Wasteful spending, however, was not significantly different for the lump sum group (columns 3 through 6), suggesting that the higher receipt of

Table 4: Effects of treatment assignment on market spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<u>Dependent variable:</u>	Total spent at market on Fridays 1, 2, 3	Total spent at market on Saturdays 1, 2, 3	Amount spent at market on Friday 4	Amount spent at market on Saturday 4	Total spent at market on Fri and Sat 1 - 4	Amount spent on day of income receipt	Ratio amount spent over received on day of income receipt
<u>Panel A - Lump sum vs. weekly</u>							
Lump sum payment	-604.6*** (49.03)	-697.9*** (53.25)	318.2*** (55.24)	495.0*** (72.61)	-488.5*** (126.6)	-938.8*** (81.73)	-0.242*** (0.0252)
Mean dep. var., weekly payment group	980.7	1201	365.6	576.3	3129	2142	0.631
<u>Number of observations</u>	696	696	696	696	696	696	696
<u>Panel B - Saturday vs. Friday</u>							
<i>i) Weekly study arm only</i>							
Saturday payday	-1,203*** (64.08)	588.0*** (88.79)	-372.4*** (35.99)	192.6** (75.80)	-795.6*** (151.9)	-3.884 (113.7)	0.00214 (0.0336)
Mean dep. var., Fri payment group	1595	899.8	555.9	474.5	3532	2151	0.631
<u>Number of observations</u>	347	347	347	347	347	347	347
<i>ii) Monthly study arm only</i>							
Saturday payday	16.26 (29.10)	28.08 (49.93)	-1,105*** (82.62)	228.0** (113.8)	-843.0*** (175.3)	-53.70 (118.0)	-0.0177 (0.0381)
Mean dep. var., Fri payment group	365.9	503.4	1244	959.1	3082	1244	0.401
<u>Number of observations</u>	349	349	349	349	349	349	349

Notes: Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels. Regressions are run on pooled data from round 1 and round 2 (see Empirical Strategy for details). Standard errors are clustered at the individual level in parentheses. USD 1 is ca. MK 400 for study period. All regressions include stratification cell fixed effects and an index of baseline asset ownership based on first principal components, difference in days between date of interview and the preceding weekend, baseline total spending. For complete variable definitions, see Appendix B, and Table 3 for summary statistics.

cash in one chunk does not lead recipients to overspend on goods they later regret – at least in this context. While the standard errors are large enough that we cannot reject a doubling of wasteful expenditure, the results from Panel A of Table 6 are consistent with the idea that the composition of expenditure did not change in the monthly group. Table 6 columns 1 through 4 show expenditure shares in broad categories. These data are constructed from detailed, itemized listings. The shares of expenditure in different broad item categories were not significantly different between the monthly and weekly payment groups.

The wasteful spending variables in Table 5 are only available for round 2; we choose to show this set of outcomes as it most unambiguously reflects temptation spending and avoids constructing outcomes with researcher-imposed ideas of which expenditures are temptation purchases. There are multiple ways of constructing outcomes with the same intention. One variation that we have explored is based on reports of unplanned purchases of items: we have considered both items that are commonly unplanned purchases across the whole sample, as well as individual self-reports that a specific purchase was not planned. Neither of these variations affects the pattern of no significant treatment effects, and so we omit these alternative specifications for brevity.

Column 5 of the same table examines whether higher expenditures lead to differential asset purchases. The estimates show that net asset accumulation over the course of the all payday weekends does not appear to be different between lump sum and weekly payment conditions. However, the standard errors are large and so economically-significant effects cannot be ruled out by these estimates.

Lastly, in Table 7 we examine the effect of lump sum payments on take up of a large minimum-installment, high-return, risk-free “investment opportunity” that was offered to respondents right after the follow-up interview.¹⁴ Workers were able to buy either 1 or 2 “shares” from the project that had a risk-free return of 33% and were repaid after exactly two weeks. This investment opportunity was offered to test whether the timing of payments affects respondents’ ability to take up profitable investment opportunities that cannot be purchased in small parts. The main advantages of this novel outcome variable are that it provides a controlled investment instrument with known features, and, moreover, that it makes a high-return investment opportunity, that requires a large minimum investment, homogeneously available to every respondent at the time of surveying. In real life respondents’ opportunities vary widely cross-sectionally and, importantly, over time – e.g. farming investments are largely only available during a limited period of the year.

In round 1 the opportunity to invest was only announced in the week preceding the final payday. This limits the usefulness of the round 1 results, because workers already knew their treatment status but did not know about the investment opportunity until a week before it was made available to them. This could bias any estimated effects either upwards or downwards. An upward bias could occur because the weekly payment group members did not know about this

¹⁴There is no effect of Saturday vs. Friday payments on these outcomes, consistent with the lack of difference in remaining cash after weekend 4. For clarity of presentation we omit the specifications of Panel B and focus only on the regressions analogous to Panel A in the preceding results tables.

Table 5: Effects of treatment assignment on total spending and cash saving and wasteful spending

	(1)	(2)	(3)	(4)	(5)	(6)
			Self-reported wasteful spending, <i>round 2 only</i>			
<u>Dependent variable:</u>	Total spending since last Fri, inclusive [MK]	Remaining cash out of received since last Fri, inclusive [MK]	Friday [MK]	Saturday [MK]	Sunday and after [MK]	Total since last Fri (cols 3+4+5) [MK]
Panel A - Lump sum vs. weekly						
Lump sum payment	1,451*** (159.1)	139.2* (71.40)	66.54 (47.43)	32.28 (27.63)	-7.165 (31.06)	92.77 (70.53)
Mean dep. var., weekly payment group	1836	468.5	132.3	58.36	67.60	261.8
<u>Number of observations</u>	689	689	346	346	346	346
Panel B - Saturday vs. Friday						
<i>i) Weekly study arm only</i>						
Saturday payday	-80.14 (221.3)	-50.32 (114.8)	-113.2** (56.42)	14.74 (28.55)	-28.25 (51.81)	-131.0 (83.46)
Mean dep. var., Fri payment group	1881	483.5	189.1	52.59	73.53	322.3
<u>Number of observations</u>	344	344	171	171	171	171
<i>ii) Monthly study arm only</i>						
Saturday payday	256.4 (222.3)	-165.2 (110.9)	-126.4 (79.62)	79.94* (45.47)	14.49 (41.94)	-0.125 (117.5)
Mean dep. var., Fri payment group	3091	670.6	250.4	34.89	55.22	326.1
<u>Number of observations</u>	345	345	175	175	175	175

Notes: Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels. Regressions of columns 1 and 2 are run on pooled data from round 1 and round 2 for which standard errors are clustered at the individual level; remaining columns use only round 2 data since outcomes are not available in round 1. All regressions include stratification cell fixed effects and an index of baseline asset ownership based on first principal components, difference in days between date of interview and the preceding weekend, baseline total spending and -if available- the baseline value of the outcome variable. For complete variable definitions, see Appendix B, and Table 2 for summary statistics.

Table 6: Effects of treatment assignment on expenditure composition and asset accumulation

<u>Dependent variable:</u>	(1)	(2)	(3)	(4)	(5)
	Expenditure shares based on itemized elicitation				Value of net asset purchases since last interview
	Food for consumption at home	Maize only	Food for consumption out of home	Non-Food	
<u>Panel A - Lump sum vs. weekly</u>					
Lump sum payment	-0.0153 (0.0162)	0.0182 (0.0192)	-0.00416 (0.00449)	0.0190 (0.0161)	19.61 (525.7)
Mean dep. var., weekly payment group	0.707	0.352	0.0523	0.240	2271
<u>Number of observations</u>	689	689	689	689	689
<u>Panel B - Saturday vs. Friday</u>					
<i>i) Weekly study arm only</i>					
Saturday payday	0.0124 (0.0224)	0.0124 (0.0278)	0.0100* (0.00577)	-0.0234 (0.0219)	-395.5 (848.2)
Mean dep. var., Fri payment group	0.702	0.348	0.0473	0.250	2604
<u>Number of observations</u>	344	344	344	344	344
<i>ii) Monthly study arm only</i>					
Saturday payday	-0.0222 (0.0224)	-0.00801 (0.0246)	-0.000380 (0.00614)	0.0224 (0.0224)	-1,230* (718.8)
Mean dep. var., Fri payment group	0.698	0.371	0.0508	0.251	2558
<u>Number of observations</u>	345	345	345	345	345

Notes: Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels. Regressions are run on pooled data from round 1 and round 2 (see Empirical Strategy for details). Standard errors clustered at the individual level in parentheses. USD 1 is ca. MK 400 for study period. All regressions include stratification cell fixed effects and an index of baseline asset ownership based on first principal components, difference in days between date of interview and the preceding weekend, baseline total spending and -if available- the baseline value of the outcome variable. For complete variable definitions, see Appendix B, and Table 2 for summary statistics.

Table 7: Effects of treatment assignment on post-interview risk-free, high-return investment offer

	(1)	(2)	(3)	(4)
<u>Dependent variable:</u>	Total spending since last Fri, inclusive [MK]	Remaining cash out of received since last Fri, inclusive [MK]	Bought any shares [0/1]	Total spent on shares [MK]
<u>Round 1 and 2 pooled</u>				
Lump sum payment	1,451*** (159.1)	139.2* (71.40)	0.0484* (0.0247)	121.7** (58.81)
Mean dep. var., weekly payment group	1836	468.5	0.106	223.5
<u>Number of observations</u>	689	689	699	699
<u>Round 1 only</u>				
Lump sum payment	1,252*** (245.2)	-4.320 (109.6)	0.00396 (0.0381)	52.51 (79.20)
Mean dep. var., weekly payment group	2036	543.0	0.149	274.3
<u>Number of observations</u>	343	343	348	348
<u>Round 2 only</u>				
Lump sum payment	1,658*** (190.6)	274.0*** (96.82)	0.0949*** (0.0327)	196.2** (84.80)
Mean dep. var., weekly payment group	1634	393.1	0.0632	172.4
<u>Number of observations</u>	346	346	351	351

Notes: Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels. Regressions in Panel A are run on pooled data from round 1 and round 2 (standard errors clustered at the individual level in parentheses); Panels B & C are run separately on round 1 and round 2, respectively (robust standard errors in parentheses). USD 1 is ca. MK 400 for study period. All regressions include stratification cell fixed effects and an index of baseline asset ownership based on first principal components, difference in days between date of interview and the preceding weekend, baseline total spending and -if available- the baseline value of the outcome variable. For complete variable definitions, see Appendix B, and Table 2 for summary statistics.

opportunity until they had received three quarters of their wage. The wage amount remaining to be paid in the last payday weekend was smaller than the minimum required amount for the investment opportunity (the remaining payment was MK 800 but one unit of the investment offer was priced at MK 1500); this would eliminate the subset of weekly workers who had less than MK700 in weekly income from being able to purchase the investment good. A downward bias could occur because lump sum payment group members may have already committed their pay to other expenditures. This would limit their ability to purchase the investment good, thus understating any measured effects.

In contrast, in round 2 the investment opportunity was announced before the start of the round, so all respondents across both groups knew they would have the opportunity prior to learning which payment group they were in. Workers therefore had advance notice of the prospect of this opportunity before any wage payments began, and before they could potentially commit any of their wages to other expenditures in a way that depended on their study arm assignment. Because of these differences in setup across rounds, we show results both from regressions on pooled data from both rounds and then specifically for round 2.

Table 7 columns 1 and 2 repeat outcome variables from Table 5 columns 1 and 2 (cf. Panel A) to be able to track differences due to changing sets of observations across the Round 2, Round 1, and Pooled specifications respectively. Columns 3 and 4 of Table 7 show effects on take-up of the investment opportunity. When we pool observations across the two rounds, lump sum payment group members had a 4.8 percentage-point higher probability of buying any share (significant at the 10% level) and the total amount spent on the investment opportunity was about MK 122 higher (significant at the 5% level). The comparison to the separate specifications for Round 1 and Round 2 show that this effect is concentrated in round 2 where the effect of lump sum payments on probability of taking up 9.5 percentage points, relative to a base of only 6.3% among the weekly payment group.¹⁵ Total spending – number of shares times the price per share – was about MK 196 higher in the lump sum group, relative to a base of MK 172 in the weekly payment group. Both differences are statistically significant, at the 1 percent and 5 percent levels, respectively.

The results from Table 7 suggests that paying workers in a lump sum enabled them to hold enough cash to make use of a high-return large minimum installment size investment opportunity, while the weekly group did not have sufficient extra cash holdings at the time the opportunity was offered – despite experience with the product (from round 1) and sufficient advance notice.

In theory, the higher investment by the lump sum payment group could be driven by credit constraints alone, as opposed to savings constraints. Consider the case in which workers assigned to the lump sum payment group really wanted to smooth their consumption in the way the weekly payment group was able to, but could not due to a borrowing constraint. In that case, lump

¹⁵Takeup actually remains the same across rounds for the monthly group and declines from round 1 to round 2 for the weekly group. However, we cannot draw any strong conclusions from this pattern because of general seasonal variations in behavior - for example, spending levels are generally higher in round 1 before the start of the lean season in round 2.

sum workers would “involuntarily” end up with more cash at the time the investment opportunity was offered and so they make use of it. While borrowing constraints are likely binding for many in the economic environment of this study, several arguments make this model an unlikely driver of our result: 72% of workers at baseline report preferring to be paid in a lump sum after four weeks as opposed to receiving four weekly installments (with the same twice-weekly attendance requirements in the hypothetical scenario that respondents were asked about as were imposed in this experiment). Of those 72%, a great majority (83%) state, in an open ended question with at most one answer, that the reason for this preference is that it enables them to “make a better plan” for the money. 13% outright list avoiding wasteful spending as the reason. These answers imply either a commitment problem as the reason for the lump sum preference, or at the least an expected inability to save – either due to internal constraints, such as self-control problems, or external constraints, such as fear of theft. Lastly, if lump sum payment group members truly preferred to smooth consumption in the way the weekly group was able to, then they should not prefer to invest in the shares offered in this project as it locks up half (if they bought one share) or all (if they bought two shares) of total received wage payments for two weeks without any opportunity to access it. While in theory workers could have potentially borrowed against the future income receipt to access the money in the investment, this would also have held for the receipt of their wages, implying borrowing constraints could not be driving the results.

If lump sum condition households were limited in their ability to smooth consumption in the face of shocks then we would also expect that lump sum condition households would – relative to the weekly payment condition – receive more transfers from their social network over the course of the four payday weekends or request more loans – two of the most common risk coping mechanisms for workers of this study. However, we do not find statistically-significant effects on either of these outcomes; the point estimates are small, but the standard errors are large and so even sizeable effects along these dimensions cannot be ruled out (results not shown).

4.2 Saturday vs. Friday Payday

Having demonstrated that receiving pay in a lump sum increases uptake of the investment good, and that this appears to be the result of savings constraints, we now look to whether receiving pay in a tempting environment alters this effect. To do this, we examine the effects of the experimentally-induced variation in whether workers are paid on Saturday compared to Friday on expenditures and saving. We consider estimates of equation (2) in Panel B, respectively, of Tables 4 through 6, with results shown separately for respondents in the monthly lump sum and weekly installment payment conditions.

We first examine how the specific day on which people were paid affected their spending at the market over the course of the eight paydays during each round. Table 4 presents estimates for outcomes from the panel of data collected during paydays.

Columns 6 and 7 of Table 4, Panel B indicates that the day of receipt did not matter for same-

day market expenditures. If receiving pay in the environment of Saturday’s weekend market was tempting for workers then we should expect to see workers in the Saturday group spending more at the market on the day they were paid. The point estimate is close to zero and relatively tightly bounded: the mean of the dependent variable in the Friday group is MK 1244 for the monthly group, the point estimate for the Saturday effect is MK -53.70 with a standard error of MK 118. The estimated null effect is even tighter for the weekly group; there is no evidence of a differential effect by payment frequency.

Table 4 columns 1 through 5 reveal that those workers with payments on Friday spend more money at the market on Fridays – the estimate of the Saturday coefficient is negative for Friday expenditures – and those with payments on Saturday spend more on Saturdays. The negative coefficient on the Saturday dummy is larger in absolute value for Friday outcomes than for Saturday outcomes, suggesting that Friday wage receivers spend some of their money on Saturday, while Saturday wage receivers do not have extra funds to spend on Friday – the day before their pay receipt. There is no meaningful evidence of a differential effect by payment frequency: columns 1 and 2 cover a period when the monthly group was not receiving any pay, so no effects are to be expected.

The natural follow-up question is to ask whether total expenditures over the whole weekend and in the days following the payday weekend were different by Saturday vs. Friday payment. Thus, we turn to Table 5 column 1 which presents the effects on spending during and after the fourth payday weekend for each of the two rounds, including also non-market expenditures. In Panel A, the point estimate for the Saturday effect is negative for the weekly group and positive for the monthly group, but far from statistically significant in either case. Taken at face value, the point estimate of MK 256.4 for the monthly group would imply a relative effect of ca. 12.3% of the Saturday assignment on total expenditures compared to the Friday assignment (mean of MK 3091). The effect would be about -4.3% for the weekly group. Compared to the market data of Table 4 that was available for all payday weekends, the standard errors are higher, and so moderate Saturday effects cannot be rejected with high confidence for this outcome variable.

Column 2 shows a statistically-insignificant but negative estimated effect of the Saturday condition on the amount of cash respondents had received since the Friday before the interview but had not yet spent. The differences of about 50 for the weekly group and MK 165 for the monthly group are large relative to the respective Friday payday condition means of MK 483 and MK 670, and so we cannot reject moderate-sized effects on this outcome.

We have established that there is no detectable Saturday effect on the level of expenditures on market day and beyond. However, if Saturdays are tempting, being paid on Saturdays could also affect the composition of expenditures. To explore this we look at the two sets of outcome variables: self-reported wasteful expenditures, in Table 5 columns 3 through 6, and the composition of spending in broad expenditure categories, in Table 6 columns 1 to 4. Again, we find no robustly-significant Saturday effects on average or in interactions with payday frequency. Lastly, column 5

of Table 6 shows that over the course of the entire payment period, Saturday payments did not differentially affect asset accumulation compared to Friday payments. While some of the coefficients in columns 3 to 5 of Table 5 are statistically-significant, the overall effect on wasteful spending in column 6 is not. Two of the coefficients in Table 6 are statistically-significant, but only at the 10% level, and each for only one of the two variations in payment frequency. For all the outcomes in Tables 5 and 6, however, the standard errors are relatively large and so we cannot reject the hypothesis that effects are in fact economically significant.

Overall, we find no strong evidence that receiving one’s pay during the Saturday market affected expenditure or savings behavior. The implied confidence bands around many of our sets of point estimates given the standard errors are, however, not very narrow. We therefore cannot reject relatively large differences – compared to the mean in the control group – with confidence for any of the outcomes except for expenditure levels.

5 Discussion and Conclusion

Markets for financial intermediations in developing countries are imperfect. Besides the “external” constraints this creates for households, these market imperfections may exacerbate “internal” constraints such as time-inconsistent preferences and limited attention. In such a setting the exact timing of income streams can matter for spending and savings decisions. Spending may be higher, or skewed towards unplanned or wasteful expenditures in environments that are tempting, and spending may be different depending on the frequency of payments. If the timing of income receipt matters, this may have implications for the payment policies of employers and cash transfer programs, who may be interested in structuring payments to maximize benefits to income recipients.

In the specific context of this study, and in developing countries in general, there are two concerns about how wage payments are structured across time. First, when income is received in tempting environments, recipients may end up spending more, or may spend more on different items than they had planned *ex ante*, or than they deem prudent *ex post*. Second, when income is received in small installments, people may find it harder to generate meaningful sums that can be used for large-installment expenditures such as durable goods purchases, buying in bulk to receive quantity discounts, or high-return investments. In order to determine if these concerns are empirically relevant we designed a field experiment that varied the degree of temptation people faced when receiving payments, as well as whether payments were received in small installments or as a lump sum.

Based on ample qualitative evidence suggesting that spending – in particular frivolous spending – might be higher if income is received on market days, our experiment used the day of the week that workers were paid to vary the level of temptation workers faced when receiving income. Half of our sample received their income during the major local market day, which happened on Saturdays; the other half received their income at the same site on Fridays. However, we do not find evidence,

for the sample of casual workers in Malawi that were part of our study, that the specific day of the receipt of income is an important driver of expenditures. Observed spending and savings behavior had no statistically-significant differences between those paid on Fridays and those paid on Saturdays, and we can rule out moderate-sized effects. This pattern does not depend on whether people are paid in a single lump sum or in small installments.

These findings do not reject the general idea that the environment in which people are paid matters. We worked in seven villages around one particular trading center in Malawi. In this setting, other trading centers with complementary market days – e.g. ones that take place on Fridays, when the payday trading center’s market was not occurring – are within 30 minutes’ travel. In other settings in which there are no complementary nearby market days, the day of payment may matter more. However, the setting of our study is fairly typical for many rural areas in Malawi and other countries in the region, where there are very often trading centers with a market day covering most days of the week, located within distances that can be traveled in reasonable times. Thus, the findings of our study should imply that the specific day of income receipt is not a major driver of spending decisions in a broad range of settings in rural Africa.

We also investigate the impact of paying workers in one lump sum compared to weekly payments. Our findings suggest that organizations can help income recipients overcome savings constraints by providing income in larger installments rather than smaller ones. Workers in the lump sum payment group spend relatively less of it immediately on receipt. Since they also receive more money on the last payday weekend – the full amount of wages compared to the weekly group that is receiving only the fourth of four equal installments – lump sum payment group members remain with more cash in the week after the last payday. In general, receiving income in a lump sum does not appear to affect the composition of expenditure, only the level. This mitigates concerns that lump sums “burn a hole in workers’ pockets”. Moreover, we find evidence that lump-sum income receipt promotes saving: people in the lump sum payment group show a higher propensity to save in a high interest, relatively short-term asset that was offered to all respondents and required a large minimum investment. We argue that the differential investment is largely a function of the weekly payment group workers’ inability to have cash available at the time of the investment offer (the timing of which was known to all workers before any payments were made).

The findings suggest that it is preferable for recipients that organizations pay at least part of wages or cash transfers in lump sums as a form of pre-committed savings. There is a trade-off between the desire to smooth consumption and the ability to generate lump sums; and so in an environment with borrowing constraints and generally high costs of risk coping, receiving all household income infrequently is unlikely to be desirable for households. In the context of this study, however, almost all households had some other means – besides the income from this project – of securing basic levels of consumption. Furthermore, a majority of households reports that they prefer to receive this additional income as a lump sum. This supports the idea that projects designed to generate income for people in developing countries, such as GiveDirectly, should provide

income in strategically-timed lump sums (or at least offer this option) in order to maximize benefits to recipients.

The investment opportunity was artificially provided to study participants as part of this project in order to improve measurement of investment behavior in a small sample observed over a short time horizon and in a context where absolute income differentials across treatment groups were small. In addition, overall take-up of the investment opportunity was low. As such, the observed effects mainly support the overall conceptual point. However, the implied magnitudes are also interesting: we provided both the weekly and the lump group households with identical total additional income of MK 4000 (MK 3200 wages + 8 x MK 100 show up fees) over the course of the second round of this project. The point estimates imply that on average each member of the lump sum group was able to increase household income by an additional MK 65¹⁶ – about 1.6% of income from the project’s employment – within two weeks of the last payday via the investment opportunity, solely because of the changed timing of payments.

Practically speaking, the effect of changing the payments from small installments to lump sums will depend on the return to the relevant investment. We can get a sense of this by considering an example of an investment that is conceptually similar to the one we offered: secondary school fees, which are approximately MK3000 (\$7.50) per year in Malawi, and which generally must be paid in total at the beginning of the school year, rather than in installments. If people do think about education as an investment, we would expect that a project that pays respondents’ total wages of MK3000 in a single lump sum timed for the beginning of the school year, rather than in small installments, to increase school fees payments by as much as 9 percentage points. This could have significant social benefits: if school fees are the only barrier to attending secondary school (and they are commonly cited as a reason teenagers do not go to school in Malawi) then that shift would have similar effects on the rate of school attendance. To get a sense of the total social benefit of this change in timing, note that Malawi has a GNI per capita of \$320, and that research on the returns to education generally estimates figures of at least 10% per year in developing countries. Thus the additional 9% of children who are able to attend school would earn an additional \$32 per year. Over a 40-year working life, starting 4 years after the investment, and at a social discount rate of 10%, this would raise a child’s income by \$213, for a net benefit of \$206 per person. This is a substantial payoff for a relatively minor change.

School fees also highlight the external validity of our results for the investment good: they are time-sensitive, as are many other investment opportunities in the developing world, such as farm input purchases, which must be timed for the planting season.¹⁷ This exacerbates the savings constraints that people face: it is easier to save up for an investment if you can make the purchase whenever you have the money, as opposed to needing to bring the money on a specific day. There

¹⁶33% of 196.2, from Table 7, column 4.

¹⁷While some farm inputs can be bought and stored, others cannot for various reasons. For example, Malawi’s government subsidizes fertilizer purchases immediately before the planting season, so farmers must have the cash to purchase the subsidized fertilizer within a fairly tight window.

are other important investments that do not have this same time-sensitive feature: for example, metal roofing has a large minimum installment size, but can be purchased whenever people have the money for it. Due to the design of the investment option used in this study, we cannot be sure that our results hold for alternative, less time-sensitive goods.

These benefits would come at relatively little cost, and organizing payroll just once a month could even be cheaper for the paying organization. We also see no significant downsides to partial lump sum payments, even when they are received during one of the most tempting environments that people typically experience in rural Africa. However, further research is needed in order to better-establish whether lump-sum payments can potentially backfire in developing countries.

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

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A Balance and comparison demographic characteristics of sample to census data

Appendix Table A1 shows summary statistics for important baseline characteristics: all available baseline measures corresponding to outcome variables used in the results tables as well as an index of asset holdings (used as a control in the main results tables). Columns 4 and 5 present formal statistical tests of the null hypothesis that pre-program characteristics have equal means across all four study arms. For each covariate, the test is conducted by running two linear regressions as seemingly-unrelated regressions (SURs) of the variable on a saturated set of categorical indicator variables for study arm, one regression for each round. We then run a joint test of the null hypothesis that all the coefficients are zero. Column 5 shows the p-values, which are uniformly above 0.3. The last row shows the test statistic and p-value for a joint test of the hypothesis that all the coefficients equal zero across all 26 regressions. We fail to reject the null of no differences (p-value of 0.81). The sample is similarly balanced on demographic covariates; see the analogous test statistics in Appendix Table A2.

Table A2, columns 1 to 3, presents summary statistics of demographic characteristics for the 350 workers from our sample for whom baseline data is available. As a basis for comparison, we also present statistics for Mulanje District as a whole, taken from the IPUMS-International 10% sample of the 2008 Malawi Population and Housing Census. A comparison of our sample with the rest of the district suggests that it is generally representative of the local area, with differences that are likely due to the criteria used by the Village Development Committee (VDCs) to select workers for the program. Our sample is 69% female, which is substantially higher than the district average of 55%. It also has a larger share of people from the Lomwe ethnic group, at 90% compared with 75%. It is otherwise quite similar to the district as a whole, with similar rates of marriage (70%) and Christian religion (90%). The differences in the other variables are fairly small, and consistent with the VDCs selecting people of lower socioeconomic status for the program. For example, our sample averages 3.5 years of completed schooling, compared with 4.4 years for the district as a whole, and has a mean age of 40 compared with 37 for Mulanje District. Our workers are also more likely to be divorced and less likely to be single.

Table A1: Balance of baseline variables

Variable	Worker Sample Summary Statistics			Test for Difference Across Study Arms	
	(1) Mean	(2) SD	(3) N	(4) Chi ²	(5) p-value
Income and Spending					
Total spending since last Friday, inclusive [MK]	2271.04	3728.39	329	3.84	0.70
Cash remaining out of total received since last Friday, inclusive [MK]	683.81	2618.13	329	7.00	0.32
Expenditure Composition					
Food for consumption at home	0.66	0.23	349	3.05	0.80
Maize only	0.23	0.26	349	2.24	0.90
Food for consumption out of home	0.06	0.07	349	1.77	0.94
Non-Food	0.28	0.23	349	3.83	0.70
Assets					
Baseline Asset Ownership Index (First Principal Component)	0.00	2.68	350	5.53	0.48
Combined Test Across All Variables				28.50	0.81

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). 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. Tests for any difference in means across study arms use seemingly-unrelated regressions of a variable on a full set of categorical indicator variables for study arm, clustered by respondent, to do pooled tests of the null hypothesis that all study arms have equal means in both rounds; the test statistics are chi-square distributed with 6 degrees of freedom. The Combined Test Across All Variables is a combined SUR of all 8 covariates in both rounds; its chi-square test statistic has 36 degrees of freedom due to collinearity between some of the equations estimated.

Table A2: Demographic characteristics of sample - balance and comparison to census

Variable	Worker Sample Summary Statistics			Test for Difference Across Study Arms		Mulanje District 2008 Census Summary Statistics	
	(1) Mean	(2) SD	(3) N	(4) Chi-square	(5) p-value	(6) Mean	(7) SD
Male	0.31	0.46	344	3.79	0.70	0.45	0.50
Religion							
Christian	0.90	0.30	341	5.93	0.43	0.91	0.28
Muslim	0.10	0.30	341	5.93	0.43	0.05	0.22
Marital Status							
Married	0.69	0.46	338	5.46	0.49	0.71	0.45
Divorced/Widowed	0.25	0.44	338	5.44	0.49	0.17	0.37
Single	0.05	0.21	338	8.74	0.19	0.12	0.33
Ethnic Group							
Lomwe	0.89	0.31	344	1.66	0.95	0.75	0.43
Yao	0.07	0.26	344	2.29	0.89	0.05	0.22
Mang'anja	0.02	0.13	344	6.15	0.41	†	
Other	0.02	0.15	344	8.27	0.22	0.20	0.40
Years of Education Completed	3.54	3.15	341	3.47	0.75	4.45	3.91
Age (Years)	40.03	15.40	344	4.24	0.64	37.35	17.27
<u>Combined Test Across All Variables</u>				<u>61.42</u>	<u>0.73</u>		

Notes: Pooled Sample includes 359 respondents who participated in at least one round of the work program. Tests for any difference in means across study arms use seemingly-unrelated regressions of a variable on a full set of categorical indicator variables for study arm, clustered by respondent, to do pooled tests of the null hypothesis that all study arms have equal means in both rounds; the test statistics are chi-square distributed with 6 degrees of freedom. The Combined Test Across All Variables is a combined SUR of all 13 covariates in both rounds; its chi-square test statistic has 69 degrees of freedom due to collinearity between some of the equations estimated. † The 2008 Malawi Census does not report Mang'anja ethnicity as a separate category, so it is included in "other".

B Variable definitions

Data used in this paper come from three rounds of “full length” surveys (a baseline and two follow-up interviews), from two- to four-question surveys during paydays as well as from administrative records of the project. We conducted a baseline survey from 4 Oct 2013 to 19 Oct 2013 and two follow-up surveys after the last payday weekend of each round, once from 2 Dec 2013 to 7 Dec 2013 and once from 27 Jan 2014 to 31 Jan 2014. All variables that are created from survey data are Winsorized at the 1st and 99th percentile. All figures in money terms are in local currency units, Malawi Kwacha (MK).

B.1 Variables from payday surveys

Amount spent on same day as income receipt is total market spending on all days that workers received their wages (sum of all four payday Fridays or Saturdays for the weekly payment group; the fourth payday Friday or Saturday for the lump sum payment group).

Money spent at market on Fridays 1, 2, 3 is the sum of total market spending on the first three payday Fridays.

Money spent at market on Saturdays 1, 2, 3 is the sum of total market spending on the first three payday Saturdays.

Money spent at market on Friday 4 is the total market spending on the fourth payday Friday.

Money spent at market on Saturday 4 is the total market spending on the fourth payday Saturday.

B.2 Variables from follow-up surveys

Total spending since last Friday, inclusive [MK] is the total household spending starting from the fourth payday Friday until the day of the survey interview in the week after the fourth payday. The variable is derived from the difference of the answers to the questions “Since last Friday, how much cash have you received?” and “How much of that cash do you have left?”, respectively.

Remaining cash out of received since last Friday, inclusive [MK] is the household’s remaining cash holdings out of money received starting from the fourth payday Friday until the day of the survey interview.

Self-reported wasteful spending on weekend 4 of round 2 variables ask for money that respondents report as “wasted” or spending which the respondent was tempted into spending that he/she should not have spent:

- *Total since last Friday, inclusive [MK]* is the sum of total wasteful spending starting from the fourth payday Friday until the day of the survey interview in the week after the fourth payday.
- *Friday [MK]* is total wasteful spending on the fourth payday Friday.

- *Saturday [MK]* is total wasteful spending on the fourth payday Saturday.
- *Sunday and after [MK]* is the sum of total wasteful spending starting from the fourth payday Sunday until the day of the survey interview in the week after the fourth payday.

Expenditure shares based on itemized elicitation is the sum of itemized expenditures, grouped into different categories as a share of total expenditures across all items based on an large listing of possible items (with items derived from Malawi’s Integrated Household Survey; a select number of items was consolidated or omitted but each category had an “other” option to capture items that were left out; total number of 105 items in 12 categories).

- *Food for consumption at home* includes eight categories of food items typically used for home consumption.
- *Maize only* includes only maize flour and maize grain.
- *Food for consumption out of home* includes all items from the categories “cooked foods from vendor” and “Beverages” which are typically consumed away from home.
- *Non-Food* includes all non-food items.

Value of net asset purchases since last interview is the sum of the difference between the value of assets bought and assets sold from an itemized list of common assets (as well as an “other” category) considering purchases and sales since the last interview, i.e. since baseline interview for follow-up 1 and since follow-up 1 for follow-up 2.

B.3 Variables from baseline surveys

Assets index is an index based on the first principal component of the number of items owned out of 64 common non-financial, non-livestock assets and the number of animals owned out of 9 common types of livestock.

Total spending is defined similarly to “Total spending since last Friday, inclusive” described under follow-up variables above, covering the last Friday prior to the interview until the day of the survey interview.

B.4 Variables from project records

Bought any shares is an indicator for whether the respondent bought at least one “share” of the investment opportunity offered after the follow-up interviews (see details in main text in Data Collection section).

Total spent on shares is the total amount spent on the investment opportunity offered and equals the number shares bought times the price of one share (MK 1,500).