

Online Appendix to Income Timing and Liquidity Constraints: Evidence from a Randomized Field Experiment

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A Proofs of theoretical results

To solve the model, we start from the last period. It is never optimal to save in period 3 and we assume the agent cannot borrow either (since creditors would never be repaid). Thus she simply consumes the available resources, i.e. $c_3 = y_3 + s_2 + bR$. Using this result and the budget constraints to substitute for c_t , we can write the maximization problem from the perspective of period 2 as follows:

$$\max_{s_2, b} u(y_2 - s_2 - b + s_1) + u(y_3 + s_2 + bR)$$

We additionally assume agents are credit-constrained in period 2 (so $s_2 \geq 0$), a key assumption that we discuss in detail in section A.1 below. To solve for the optimal choice of s_2 , conditional on b , we can use the usual the Kuhn-Tucker first-order conditions for constrained maximization. If the borrowing constraint binds then $s_2^* = 0$. If the borrowing constraint does not bind, marginal utilities are equated between periods 2 and 3 by choosing s_2 to equalize consumption, $s_2 = \frac{1}{2}(y_2 + s_1 - y_3 - b(1 + R))$. This is positive as long as $y_2 + s_1 > y_3 + b(1 + R)$.

In summary,

$$s_2^*(b) = \begin{cases} \frac{1}{2}(y_2 + s_1 - y_3 - b(1 + R)) & \text{if } y_2 + s_1 \geq y_3 + b(1 + R) \\ 0 & \text{otherwise} \end{cases} \quad (1)$$

The agent will save only if the sum of resources available in period 2 (i.e. period 2 income plus net savings from period 1) is sufficiently high. Note that, conditional on b , s_2^* is

a continuous function of y_2 and s_1 with a kink at 0.

For the optimal choice of b , the agent maximizes the sum of utilities in periods 2 and 3 by choosing between $b = 0$ and $b = B$:

$$b^* = \operatorname{argmax}_{b \in \{0, B\}} u(y_2 - s_2^*(b) - b) + u(y_3 + s_2^*(b) + bR)$$

Finally, having characterized the behavior of the agent in periods 2 and 3, we examine her behavior in period 1. Plugging in the optimal choices of s_2^* and b^* , the maximization problem in period 1 is

$$\max_{s_1} u(y_1 - s_1) + u(y_2 + s_1 - s_2 - b^*(s_1)) + u(y_3 + s_2^*(s_1) + b^*R) \quad (2)$$

where the agent chooses only s_1 .

The main goal of the model is to examine how the income timing change between periods 1 and 2 induced by the experiment affects bond purchases. We begin by showing that, to predict any response to the income timing change from the experiment, our model must include both a) a credit constraint in period 2 and b) a liquidity constraint (either in terms of credit or savings) in period 1. We then show that credit-constrained agents who are switched from weekly to lump-sum payments will tend to increase their bond purchases, and that a similar logic will also apply to savings-constrained agents.

A.1 Period 2 credit constraints are necessary for effects on bond purchases

We assume that the agent faces a credit constraint in period 2. In the absence of such a constraint, she will always buy the bond irrespective of levels or timing of income — and thus the timing of income across periods 1 and 2 will not matter for bond purchases. If credit is freely available in period 2, the agent can always increase total consumption by borrowing to purchase the bond and paying back the loan in period 3.¹

¹ Note that this credit constraint does not have to be a total lack of credit. It could take the form of a limit on the size of the additional loans available for purchasing the bond. Thus, for example, $s_2 > -B$ would be sufficient to ensure that not all agents purchase the bond. It could also be a sufficiently-high interest rate. We assume a sharp credit constraint at zero to simplify the exposition of the model.

A.2 Period 1 liquidity constraints are necessary for effects on bond purchases

In addition, income timing changes of the sort we are studying will never affect the optimal bond purchase decision unless agents are liquidity-constrained in the first period. This is intuitive: in the absence of a constraint (and without discounting or interest) resources can be costlessly moved between the first two periods via saving or borrowing in period 1 (i.e. s_1 positive or negative) and thus the timing of income between the two periods is irrelevant. Formally, we show this in Lemma A.1:

Lemma A.1 *At the utility-maximizing value of s_1 , s_1^* , the optimal choice of $\{s_2, b\}$ is invariant to marginal changes in income timing that shift income between periods 1 and 2 and leave total income unchanged, so long as s_1 is locally unconstrained at the optimum.*

Proof:

Let $U^*(y_1, y_2)$ be the maximized utility from the optimal choice of $\{s_1, s_2, b\} = \{s_1^*, s_2^*, b^*\}$. Consider marginal changes in period 1 and 2 incomes that are equal in magnitude but opposite in sign, i.e. $dy_1 = -dy_2$.

The optimal choices of s_2 and b depend on y_2 directly but only depend on y_1 through s_1 . Note also that y_2 and s_1 are perfect substitutes for the choice of s_2^* in the sense that a change of y_2 has the same effect on s_2^* as an equal change in s_1 , and vice versa². This is true both in terms of the unconstrained level of s_2^* and also for the threshold value that governs whether the constraint binds. In addition, y_2 and s_1 are also perfect substitutes in the sum of period 2 and 3 utilities conditional on s_2^* , and (therefore) for the choice of b^* .

With the above observations in mind, define $a := y_2 + s_1$ as the total resources in period 2. Let ds_1 be the change in s_1 induced by the income timing change, and let ϵ be the total change in a , $da = dy_2 + ds_1 = \epsilon$. Suppose, first, this change increases utility to $U^{**}(y_1 - \epsilon, y_2 + \epsilon) > U^*(y_1, y_2)$. Then for any marginal change ϵ , in the absence of the income timing shift³ the agent could have picked $s_1^{alt} = s_1^* + \epsilon$, inducing the same change in both b^* and s_2^* as the income timing shift, and the same change in the sum of period 2 and period 3 utilities.

Next, note that for period 1 utility, a *decline* in period 1 income y_1 is a perfect substitute for an increase in s_1 of the same amount. Thus, for the income timing change (where $dy_1 = -dy_2 = \epsilon$), the period 1 consumption change under the income timing change is $dc_1 = dy_1 - ds_1 = -dy_2 - (-\epsilon - dy_2) = \epsilon$. But this is the same as the period 1 consumption

²This can be seen in equation (1) of the main text.

³Throughout this section, we use the term “income timing shift” to mean a pure change in the timing of income receipt, with no effect on the overall amounts of money received.

change induced by $ds_1^{alt} = -\epsilon$, holding y_1 fixed ($dy_1 = 0$; i.e. in the absence of the income change): $dc_1 = dy_1 - ds_1 = 0 - (-\epsilon) = \epsilon$. Thus the agent could have achieved the same utility that results from the income timing change, U^{**} , by moving s_1^* by ϵ in the same direction as the marginal change in y_2 . Therefore, the initial choice $\{s_1^*, s_2^*, b^*\}$ could not have been the utility maximizing choice, contrary to assumption.

Conversely, suppose now that after the income timing change the agent's utility level is lower: $U^{**}(y_1 - dy_1, y_2 + dy_2) < U^*(y_1, y_2)$. By a parallel argument to the one above the agent could freely reallocate resources to achieve the old, higher utility level; thus, the new utility level U^{**} cannot be lower either.

In conclusion, $U^{**}(y_1 - dy_1, y_2 + dy_2) = U^*(y_1, y_2)$. Therefore the optimal choices of b and s_2 prior to an income change must also be optimal choices after the income change. QED.

A.3 Liquidity constraints and bond purchase decisions

Agents must therefore face liquidity constraints in period 1 in order for the deferred wages treatment to affect bond purchase decisions. These liquidity constraints could be either credit constraints or savings constraints. We consider each in turn.

Period 1 credit constraints

This section considers the case in which the first-period liquidity constraint is a credit constraint. We show that a shift from weekly to lump-sum payments always weakly increases bond purchases, and that for sufficiently large such shifts bond buying strictly increases. The basic intuition is as follows. Under the lump sum payment relatively more income arrives in period 2, which tends to make the period 1 credit constraint bind (more tightly). This results in "excess" liquidity in period 2, which is then put towards the bond instead of being kept in cash.

Any income timing change from weekly to lump sum wage payments will only matter to the extent it takes place under a binding constraint. To see this, first note that any marginal income change from weekly toward lump sum wage payments will move an unconstrained agent closer to a binding credit constraint. We show that the unconstrained value of s_1^* moves in lockstep with y_1 (and $-y_2$) for the income timing changes we are considering, when s_1^* is locally unconstrained, in Lemma A.2:

Lemma A.2 *If s_1^* is locally unconstrained for a change in income timing in periods 1 and 2 such that the sum of income is unchanged ($dy_1 = -dy_2$), then $ds_1^*/dy_1 - ds_1^*/dy_2 = 1$ (and $-ds_1^*/dy_1 + ds_1^*/dy_2 = 1/2 + 1/2 = -1$).*

Proof:

From equation 2 in the main text, if s_1 is unconstrained in general or if the constraint is locally non-binding, the following first-order condition must hold:

$$\begin{aligned} -u'(y_1 - s_1) + (1 - ds_2^*/ds_1 + db^*/ds_1)(u'(y_2 + s_1 - s_2^*(s_1) - b^*(s_1))) \\ + (s_2^*/ds_1 - b^*/ds_1 R)u(y_3 + s_2^*(s_1) + b^*R) = 0 \end{aligned}$$

But by Lemma A.1, given the sum-neutral income shift, $db^*/ds_1 = ds_2^*/ds_1 = 0$ and the FOC reduces to:

$$\begin{aligned} u'(y_1 - s_1) = u'(y_2 + s_1 - s_2^* - b^*) \iff \\ y_1 - s_1 = y_2 + s_1 - s_2^* - b^*, \end{aligned}$$

by the continuity of u' , which implies $s_1^* = \frac{1}{2}(y_1 - y_2 + s_2^* + b^*)$. Thus, $ds_1^*/dy_1 - ds_1^*/dy_2 = 1/2 + 1/2 = 1$, using again that $db^*/ds_1 = ds_2^*/ds_1 = 0$. QED.

However, a marginal income timing change between periods 1 and 2 will have no effect on bond purchases as long as the credit constraint is not locally binding. This follows from the above proof that the timing of income across periods 1 and 2 is immaterial for bond purchase decisions unless there is a period-1 credit constraint.

As a result, we can focus on the case of an agent who is facing a *binding* credit constraint. In this case, a shift in income timing that decreases y_1 and increases y_2 can never lead to a decrease in bond buying. Formally,

Proposition 1 *If $s_1^*(y_1, y_2) = 0$, $U(y_1 - \epsilon, y_2 + \epsilon, b = B) - U(y_1, y_2, b = B) > U(y_1 - \epsilon, y_2 + \epsilon, b = 0) - U(y_1, y_2, b = 0)$ for all $\epsilon > 0$.*

Proof:

First, note we can ignore period 1 utilities and consider the effect of the decrease in y_2 only. Decisions about b^* and s_2^* are made based on period 2 and period 3 utilities taking s_1 as given, and we are considering the case of s_1^* fixed at zero.

Next, note that $V(y_2, b) := u(y_2 - s_2^* - b) + u(y_3 + s_2^* + bR)$ is continuous in y_2 since s_2^* is continuous in y_2 conditional on b . In addition, $V(y_2, b)$ is continuously differentiable with respect to y_2 everywhere except at $s_2^*(b) = 0$, conditional on b , since in particular $s_2^*(b)$ is

continuously differentiable everywhere with respect to y_2 everywhere except at the kink of $s_2^*(b)$, i.e. when $y_2 = y_3 + b(1 + R)$.

Except when the derivative does not exist,

$$ds_2^*/dy_2 = \begin{cases} \frac{1}{2} & \text{if } y_2 \geq y_3 + b(1 + R) \\ 0 & \text{otherwise.} \end{cases}$$

Thus, except when the derivative does not exist,

$$dV(y_2, b)/dy_2 = \begin{cases} \frac{1}{2}u'(y_2 - s_2^* - b) + \frac{1}{2}u'(y_3 + s_2^* + bR) & \text{if } y_2 \geq y_3 + b(1 + R) \\ u'(y_2 - b) & \text{otherwise.} \end{cases}$$

To compare the effect of a marginal increase of y_2 on the relative attractiveness of $b = B$ and $b = 0$, we first compare $dV(y_2, b = B)/dy_2$ with $dV(y_2, b = 0)/dy_2$ wherever the derivative exists. There are three relevant ranges.

1. $y_2 \leq y_3$, which implies $y_2 \leq y_3 + B(1 + R)$ and thus $s_2^*(b) = 0$ irrespective of b . In this case, $dV(y_2, b = B)/dy_2 - dV(y_2, b = 0)/dy_2 = u'(y_2 - B) - u'(y_2)$ which is positive since $B > 0$ and $u'' < 0$ using u concave.
2. $y_3 < y_2 < y_3 + B(1 + R)$. In this case,

$$dV(y_2, b = B)/dy_2 - dV(y_2, b = 0)/dy_2 = u'(y_2 - B) - \left(\frac{1}{2}u'(y_2 - s_2^*) + \frac{1}{2}u'(y_3 + s_2^*)\right),$$

From equation 1, here $s_2^*(b = 0) = \frac{1}{2}(y_2 - y_3)$, and so we have $y_2 - s_2^*(b = 0) = y_3 + s_2^*(b = 0)$. Thus:

$$\begin{aligned} dV(y_2, b = B)/dy_2 - dV(y_2, b = 0)/dy_2 &= u'(y_2 - B) - u'(y_2 - s_2^*(b = 0)) \\ &= u'(y_2 - B) - u'(y_2 - \frac{1}{2}(y_2 - y_3)) \\ &= u'(y_2 - B) - u'(\frac{1}{2}(y_2 + y_3)) \end{aligned}$$

This is positive since

$$\begin{aligned}
u'(y_2 - B) - u'\left(\frac{1}{2}(y_2 + y_3)\right) &> 0 \iff \\
u'(y_2 - B) &> u'\left(\frac{1}{2}(y_2 + y_3)\right) \iff \\
y_2 - B &< \frac{1}{2}(y_2 + y_3) \iff \\
0 &< \frac{1}{2}(y_3 - y_2) + B \iff \\
y_3 - y_2 + 2B &> 0,
\end{aligned}$$

which is implied by $y_3 - y_2 > 0$ from the range condition for this case ($y_2 > y_3$).

3. $y_3 + B(1 + R) \leq y_2$. In this case,

$$\begin{aligned}
&dV(y_2, b = B)/dy_2 - dV(y_2, b = 0)/dy_2 \\
&= \frac{1}{2} (u'(y_2 - s_2^*(B) - B) + u'(y_3 + s_2^*(B) + BR) - u'(y_2 - s_2^*(b = 0)) - u'(y_3 + s_2^*(b = 0))) \\
&= \frac{1}{2} (u'(y_2 - s_2^*(B) - B) + u'(y_3 + s_2^*(B) + BR) - 2u'(y_2 - s_2^*(b = 0))),
\end{aligned}$$

since, as in case 2, $y_2 - s_2^*(b = 0) = y_3 + s_2^*(b = 0)$.

The expression is positive iff

$$u'\left(c + \frac{1}{2}B(1 + R) - B\right) + u'\left(c - \frac{1}{2}B(1 + R) + BR\right) > 2u'(c)$$

where we define $c := \frac{1}{2}(y_2 + y_3)$.

We can show this inequality holds, given $R > 1$ and u concave, via Jensen's inequality:

$$\begin{aligned}
& u'(c + \frac{1}{2}B(1 + R) - B) + u'(c - \frac{1}{2}B(1 + R) + BR) \\
& > u'(c + \frac{1}{2}B(1 + R) - B) + u'(c - \frac{1}{2}B(1 + R) + B) \\
& > u'(c + \frac{1}{2}B(1 + R)) + u'(c - \frac{1}{2}B(1 + R)) \\
& > 2u'(c).
\end{aligned}$$

The above derivatives taken within the three ranges of y_2 show that within each of the ranges above, for a marginal *increase* in y_2 , $b = B$ becomes *more* favorable relative to $b = 0$. But the same conclusion holds for changes in y_2 that cross the boundaries of the above ranges since $dV(y_2, b = B)/dy_2 - dV(y_2, b = 0)/dy_2$ is continuous (which holds since $V(y_2, b)$ is continuously differentiable as shown above). QED.

The intuition behind Proposition 1 is that under a binding period 1 credit constraint, the income timing shift increases available resources in period 2. All else equal, this will always make purchasing the bond more favorable: buying the bond becomes less costly in terms of foregone period 2 utility, and more feasible because of the additional liquidity (in the case where the period 2 credit constraint was binding).

The above shows that a change in income timing from weekly to lump sum payments raises the desirability of $b = B$ relative to $b = 0$ and, thus, weakly increases bond purchases. For the income timing change to strictly increase bond purchases, it must further hold that $b^* = 0$ before the shift and $b^* = B$ after the shift. We show this always holds for a sufficiently large income shift from period 1 to period 2, starting from a sufficiently low initial income in period 1. If y_2 is sufficiently low, then *ceteris paribus* $b = 0$ is strictly preferred; and if y_2 is sufficiently high then $b = B$ is strictly preferred. We establish these two points formally in Propositions 2 and 3:

Proposition 2 *There exists y_2^{high} such that for any $y_2 \geq y_2^{high}$, $V(y_2, b = B) > V(y_2, b = 0)$.*

Proof: We have already established that $V(y_2, b = B) - V(y_2, b = 0)$ is continuous and strictly increasing in y_2 . Consider the case $y_2^{high} = y_3 + B(1 + R)$. For $y_2 = y_3 + B(1 + R)$ (so, the lowest y_2 of case 3 above), $s_2^*(B) = 0$, $s_2^*(b = 0) = \frac{1}{2}B(1 + R)$, and $c := y_2 - s_2^*(b = 0) = y_3 + s_2^*(b = 0) = y_2 - \frac{1}{2}B(1 + R) = y_3 + \frac{1}{2}B(1 + R)$. Then $V(y_2, b = B) > V(y_2, b = 0)$ iff

$$\begin{aligned}
& u(y_2 - B) + u(y_3 + BR) > u(c) + u(c) \iff \\
& u(y_2 - B) + u(y_3 + BR) > 2u(c) \iff \\
& u(y_2 - c + c - B) + u(y_3 - c + c + BR) > 2u(c) \iff \\
& u\left(\frac{1}{2}B(1 + R) + c - B\right) + u\left(\frac{1}{2}B(1 + R) + c + BR\right) > 2u(c) \iff \\
& u\left(\frac{1}{2}B(1 + R) + c - B\right) + u\left(\frac{1}{2}B(1 + R) + c + BR\right) > 2u(c), \iff \\
& u\left(c + \frac{1}{2}B(R - 1)\right) - u(c) + u\left(c + \frac{1}{2}B(1 + R) + BR\right) - u(c) > 0
\end{aligned}$$

which holds because $R > 1$ by assumption. QED.

Next, we show that there exists a sufficiently low y_2 that agent never purchases the bond. This result ensures that there is some level and timing of income in periods 1 and 2 for which no bond buying occurs. In particular, for agents in the weekly group, income in period 2 from the project is substantially less than what is required to buy the bond. Thus, for members of the weekly group with low income from other sources, $y_2 < B$, and not buying the bond is preferred.

Proposition 3 *There exists $y_2^{low} \geq 0$ such that for any $y_2 \leq y_2^{low}$, $V(y_2, b = 0) > V(y_2, b = B)$.*

Proof: First note that if $y_2 = B$, $u(B) + u(y_3) > u(0) + u(y_3 + B(1 + R))$ (whenever $s_{1*} = 0$). In words, if period 2 resources are just enough to only buy the bond and consume nothing else in period 2, then the agent prefers not to buy the bond. This holds because we have assumed that $u(c)$ approaches $-\infty$ as c approaches zero. Moreover, if $s_1^* = 0$ and $y_2 < B$, then the agent cannot afford the bond and hence $b = 0$ is always optimal. Thus for any $y_2 \leq B$, not buying the bond is optimal. QED.

In summary, under a period 1 credit constraint, bond purchases should be more prevalent among agents in the lump sum wage payments study arm. Some of the agents who would have preferred *not* to buy the bond under weekly wage payments face a binding credit constraint when they are in the lump sum payment group. They are prevented from moving the relatively high income they receive in period 2 into period 1. Given the excess period 2 resources those agents receive under the lump sum wage payments scheme, they choose to buy the bond. Crucially, the agents who are generally more likely to buy the bond are those who face a binding period 1 constraint.

Period 1 savings constraints

We now show that the same model can generate our main result with a savings constraint rather than a credit constraint in period 1: bond purchases should be higher under lump sum wage payments. The general intuition is very similar to the case above. Since the same is true for the formal proofs, which mirror the logic from the ones for credit constraints, we only outline the main claims here.

Consider a sharp savings constraint in period 1, $s_1 \leq 0$. Suppose the constraint is strictly nonbinding, i.e. the agent does not want to save in period 1 but instead is borrowing. Then a marginal income timing shift that increases period 1 income and decreases period 2 income (i.e. the direction of change caused by a shift from lump sum to weekly wage payments) will have no effect on bond purchases. At the same time, it will increase optimal period 1 net savings (i.e. borrowing decreases). Therefore, for a sufficiently large shift toward weekly payments, the savings constraint will become binding. Once the constraint binds, further income timing shifts of this kind will only make the constraint more binding; optimal period 1 savings will remain at 0 and there is no savings channel for an effect on bond purchases. The drop in period 2 income makes buying the bond less attractive. For sufficiently low values of period 2 income, *not* buying the bond is strictly preferred and for sufficiently high values of period 2 income, the opposite is true.

Hence under a period 1 savings constraint, bond purchases should be more prevalent among workers who receive lump sum wage payments. Some agents who would prefer to purchase the bond under lump sum wage payments will no longer choose to do so under weekly wage payments. This is because they face a binding savings constraint: without period 1 savings, the limited period 2 resources mean that buying the bond is too costly in terms of forgone period 2 consumption. It may even be unaffordable, given low income and a lack of credit in period 2. For those agents, shifting from weekly to lump-sum wage payments will tend to alleviate their liquidity constraint. Notably, the role of the binding liquidity constraint is reversed compared to the credit constraint scenario from above. Here, the agents who are liquidity-constrained are *less* likely to buy the bond, *ceteris paribus*.

B Details of worker recruitment, attrition, and work activities

B.1 Worker recruitment

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. Within that region, we picked seven villages that all lie within the catchment area of Mwanamulanje trading centre, the site of one of the largest weekly 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, with a maximum of one person per household. 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.

B.2 Participation, attrition, and replacement of workers

The original recruitment for round one included 350 workers but 2 people were removed from the sample: one person dropped out before the work started and one person never showed up at payday (only an additional 9 subjects missed any days of work, and all of them are included in the analysis). Before the start of round two of the program, 13 workers left the study, and a total of 15 replacement workers were added. The study protocol specified that only 13 new workers should have been added (to replace the dropouts); 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.

B.3 Specifics of work activities

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. Subjects did two kinds of work during the study: *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 retaining 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.

C Additional balance tests and summary statistics

C.1 Balance by round

Appendix Table C.1 presents the balance tests from Table 2 separately by round. The results are broadly the same as for the pooled balance tests. We have balance both on each individual covariate and for the overall test of any difference across covariates.

C.2 Outcome measure summary statistics

We utilize three data sources in the analysis in this paper. We present summary statistics from all three data sources in Appendix Table C.2. Respondents spent MK1,538 total during the first three weekends and MK1,347 on the last weekend. 13% of workers purchased the artificial “bond.” At midline, the households’ total spending considering all expenditures from the last Friday prior to being interviewed up to the day of the survey averages MK3,042 (about US\$7.60 or PPP\$19). Respondents report having an average of MK529 (about US\$1.30 or PPP\$3.30) left out of the money they had received since the Friday prior to interviewing.

Table C.1
Balance of baseline variables by round

	Weekly wage payments			Lump sum wage payments			Balance test
	Mean	SD	N	Mean	SD	N	<i>p</i> -value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<u>Panel A - Round 1</u>							
<i>Background characteristics</i>							
Male	0.32	0.47	177	0.32	0.47	173	0.884
Married	0.73	0.45	173	0.67	0.47	171	0.214
Age (Years)	40.31	15.75	177	39.45	14.94	173	0.600
Years of Education Completed	3.41	3.09	175	3.64	3.24	172	0.492
Survey date (days after Sunday)	2.71	1.24	173	2.54	1.20	170	0.215
Prefers lump sum wage payments	0.74	0.44	176	0.73	0.45	173	0.735
<i>Financial outcomes (in units of MK unless noted)</i>							
Income received since past Friday	3,579	9,420	177	2,412	4,071	173	0.132
Remaining cash holdings out of income received	686	2,111	177	599	2,926	173	0.752
Total spending since Friday	3,849	4,353	177	3,563	4,355	173	0.539
Asset Ownership (PCA)	-0.12	2.38	177	0.12	2.97	173	0.409
Loans received in past month	2,931	12,916	177	2,963	8,854	173	0.978
Loans made in past month	705	2,507	177	762	3,378	173	0.859
Transfers received in past month	834	2,159	177	873	2,203	173	0.866
Transfers made in past month	547	2,430	177	648	2,033	173	0.672
<i>p</i> -value from joint significance of 12 covariates:		0.78					
<u>Panel 2 - Round 2</u>							
<i>Background characteristics</i>							
Male	0.30	0.46	173	0.34	0.47	177	0.443
Married	0.70	0.46	170	0.70	0.46	174	0.926
Age (Years)	39.02	14.66	173	40.72	15.97	177	0.302
Years of Education Completed	3.50	3.08	171	3.55	3.24	176	0.887
Midline survey date (days after Sunday)	2.47	1.04	171	2.43	1.07	175	0.729
Prefers lump sum wage payments	0.75	0.43	173	0.72	0.45	176	0.528
<i>Financial outcomes (in units of MK unless noted)</i>							
Income received since past Friday	3,384	9,584	173	2,628	3,945	177	0.337
Remaining cash holdings out of income received	750	3,306	173	538	1,456	177	0.440
Total spending since Friday	3,727	4,960	173	3,688	3,672	177	0.934
Asset Ownership (PCA)	-0.07	2.91	173	0.06	2.45	177	0.652
Loans received in past month	2,994	13,293	173	2,900	8,410	177	0.937
Loans made in past month	854	3,792	173	615	1,831	177	0.454
Transfers received in past month	1,012	2,742	173	699	1,418	177	0.183
Transfers made in past month	597	2,625	173	598	1,792	177	0.996
<i>p</i> -value from joint significance of 12 covariates:		0.93					

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

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

^a Treatment assignment was stratified on gender in round one and so gender is perfectly predicted by stratification cell in this sample.

Table C.2
Summary statistics for outcomes

	Mean	Std. dev.	10th percentile	Median	90th percentile	Obs.
<u>Panel A - Payday data on spending at market on the four payday weekends</u>						
Amount spent on Friday and Saturday						
Weekends 1-3	1,538	1,110	535	1,150	3,000	696
Weekend 4	1,347	1,198	200	1,000	3,100	696
All Weekends	2,891	1,574	1,000	2,700	4,800	696
Amount spent on payday	1,677	1,146	200	1,537	3,200	696
Ratio: Spending on payday / Income received	0.511	0.345	0.061	0.483	1	696
<u>Panel B - Project administrative data - Investment opportunity take-up</u>						
Bought any shares	0.133	0.340	0.000	0.000	1.000	699
Total spent on shares	290	789	0	0	1,500	699
<u>Panel C: Follow-up survey measures</u>						
Income received since past Friday	3,043	2,676	900	3,000	5,000	689
Remaining cash holdings out of income received	529	996	0	0	2,000	689
Itemized Expenditures: Total spending [†]	3,147	2,353	1,010	2,550	5,880	689
Value of net asset purchases in past two months	2,154	7,486	0	0	5,300	689
Loans received in past month	1,957	5,810	0	0	4,400	689
Loans made in past month	524	1,833	0	0	1,000	689
Transfers received in past month	600	1,522	0	0	1,500	689
Transfers made in past month	238	624	0	0	600	689

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). All money amounts are in Malawian Kwacha (MK); during the study period the market exchange rate was approximately MK400 to the US dollar, and the PPP exchange rate was approximately MK160 to the US dollar. Asset purchases are measured since the previous survey, a period of approximately two months. Loans are measured since November 1st in round 1 and since January 1st in round 2, a period of approximately one month. Transfers are measured over the month leading up to the survey interview. For complete variable definitions see Appendix D.

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

D 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).

D.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.

Sensitivity of payday survey results to variable definitions

Our payday survey variable was recorded only for spending at the market. It also had a different recall period for Fridays and Saturdays: Friday spending was recorded on the following day (Saturday) while Saturday spending was recorded 6 days later (on the next Friday). To assess the importance of these decisions for our results, in round two of the study we also collected two alternate versions of the variable for some of the paydays. One variant had people recall their spending from one week prior. The second had them report spending *outside* the market.

Table D.1 presents estimates of the potential importance of our measurement procedure for our main results. Column 1 shows that there is some evidence of recall bias, and of differences across study arms: shorter recall periods have somewhat lower spending for the treatment group. However, these differences are very small relative to our effects on these

Table D.1

Treatment-control differences in the effect of alternative market spending measures

<u>Dependent variable:</u>	(1)	(2)
	Short-long recall difference (MK)	Nonmarket spending (MK)
Lump sum wage payments	-95.17** (42.54)	-34.32 (57.81)
Dependent variable mean, weekly wage payments group	152.1	317.3
Number of observations	346	346

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). 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.

outcomes. If the estimated treatment-control difference existed for all three Fridays (the short-recall days) before the final payday weekend, this would explain just 20% of our overall treatment effect.

Column 2 shows that the choice to focus on spending at the market makes a trivial difference for the total amounts recorded. The treatment-control difference in reported nonmarket spending is small and statistically insignificant.

D.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 a 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.

D.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.

D.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).

E Order effects and medium-term effects

Our experiment re-randomized workers into treatment and control status across the two rounds of the study. This raises the potential concern of order effects: perhaps responses in round two will differ for workers who were treated in round one and those who were untreated. Appendix Table E.1 examines whether order effects could affect our results. Specifically, it shows augmented versions of the analyses in Panel C of Table 5 in the main paper, which use the data from all workers for round two of the study. Appendix Table E.1 adds an interaction term between the round one treatment assignment and the round two treatment assignment, so our regression equation becomes

$$Y_{i2} = \alpha + \beta T_{i2} + \delta T_{i2} * T_{i1} + \gamma' \mathbf{X}_{i2} + \varepsilon_{i2} \quad (3)$$

where the subscript 2 indicates variables for round two and the subscript 1 indicates variables for round one. Because the round two treatment was stratified on round one treatment status, the main effect of the round one treatment is implicitly controlled for via the stratification cell dummies in \mathbf{X}_{i2} and so we do not control for it separately. The results show that there is no evidence that order effects drive our results: our main result, on bond purchases, is essentially identical for workers who were treated in round 1 and those who were not. However, this analysis has low power: we have 80% power to detect a 19 percentage-point difference between the two groups, which is substantially larger than our main effect estimates.

The fact that our experiment included the same workers over two rounds allows us to study the effect of round 1 treatment status on round 2 outcomes. In Appendix Table E.2 we examine whether there are direct effects of the round-one treatment on bond and bulk maize purchases in round two. Specifically, we estimate:

$$Y_{i2} = \alpha + \beta T_{i1} + \gamma' \mathbf{X}_{i1} + \varepsilon_{i2} \quad (4)$$

We also estimate a version of the model that controls for round two treatment status:

$$Y_{i2} = \alpha + \beta T_{i1} + \delta T_{i2} + \gamma' \mathbf{X}_{i1} + \varepsilon_{i2} \quad (5)$$

The controls in these regressions (\mathbf{X}_{i1}) include the round-one stratification cell indicators

(instead of round two) because otherwise the main effect of the round one treatment status would be absorbed by the stratification cell dummies. The evidence suggests that, if anything, receiving a lump sum wage payment in round one leads to more bond purchases in round two rather than fewer. The point estimate is positive and about half of the contemporaneous treatment effect; however, the coefficient is not precisely estimated ($p = 0.22$). We see a similar pattern for bulk maize purchases: the long-term treatment effect is about a four percentage-point increase, and is statistically insignificant, while the short-term effect is large and strongly statistically significant.

Table E.1
Analysis of order effects in bond purchase impacts for round two

	(1)	(2)	(3)	(4)
	Administrative data on bond sales		Survey data	
<u>Dependent variable:</u>	1(Bought any shares)	Total spent on shares (MK)	Remaining cash out of income received since last Friday (MK)	Income received since last Friday (MK)
<u>Round 2 only</u>				
Lump sum wage payments	0.0892** (0.0425)	161.3 (104.2)	197.3 (145.3)	2,190*** (330.1)
(Lump sum wage payments)X (Lump sum in round 1)	0.00559 (0.0690)	59.05 (181.7)	132.4 (208.7)	-303.2 (409.6)
Dependent variable mean, weekly wage payments group	0.0643	175.4	393.1	2,010
Number of observations	346	346	346	346

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

Table E.2
Effect of round one treatment on round two bond and bulk maize purchases

	(1)	(2)	(3)	(4)
	Administrative data on bond sales		Survey data	
<u>Dependent variable:</u>	1(Bought any shares)	1(Bought any shares)	1(Any unprocessed maize purchase ≥ MK 2000)	1(Any unprocessed maize purchase ≥ MK 2000)
<hr/>				
<u>Round 2 outcomes only</u>				
Lump sum wage payments in round 1	0.0414 (0.0338)	0.0421 (0.0335)	0.0430 (0.0342)	0.0443 (0.0333)
Lump sum wage payments in round 2		0.0846** (0.0340)		0.137*** (0.0342)
Dependent variable mean, weekly wage payments group	0.0643	0.0643	0.0409	0.0409
Number of observations	346	346	346	346

Notes: Sample includes 346 respondents who participated in both rounds of the work program and have survey data from the second round. All regressions control for stratification cell fixed effects, an index of baseline asset ownership based on first principal components, indicators for the number of days after the weekend the interview occurred, baseline total spending and (if available) the baseline value of the outcome variable. For details of the empirical strategy see Section 4, and for complete variable definitions see Appendix D. Heteroskedasticity-robust standard errors in parentheses.

F Preferences for lump sum wage payments

This section presents additional results about workers' preferences for lump sum wage payments. In table F.1 we present the reasons given by workers for their stated preference (lump sum payments or weekly payments). In table F.2 we show our main outcomes (bond purchases, asset purchases, and bulk maize purchases) in a specification that interacts the lump sum wage payment indicator with an indicator for whether the worker prefers lump sum payments.⁴ We find that there is no statistically significant difference in treatment effects between people who indicate a preference for lump sum payments at baseline and people who do not, in any of our specifications. For the pooled sample (Panel A), neither coefficient is statistically distinguishable from zero because these groups are small, but both coefficients are similar in magnitude to the pooled estimate. Our effects on bond purchases are concentrated in round two. If we restrict our analysis to that round (Panel C), the treatment effects are statistically significant and similar to the pooled estimates for both subgroups of workers. The difference in the coefficient estimates across subgroups is still not statistically significant.

⁴Two workers say they are indifferent between weekly and lump sum payments. In these regressions, we code these workers as zeros for the lump-sum preference indicator, i.e. we treat them as not preferring lump sum wage payments.

Table F.1
Reasons for preferring lump-sum or weekly wage payments

	Share	N	Total
<u>Panel A - Reasons workers prefer lump sum payments</u>			
Can make a better plan for the money	0.828	212	256
Will not waste the money	0.137	35	256
Better to have some money to buy necessary things	0.004	1	256
Other	0.031	8	256
<u>Panel B - Reasons workers prefer weekly payments</u>			
Can make a better plan for the money	0.022	2	89
Will not waste the money	0.022	2	89
Better to have some money to buy necessary things	0.910	81	89
Other	0.045	4	89

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).

Table F.2
Treatment effect heterogeneity
by preference for lump sum vs. weekly wage payments

	(1)	(2)	(3)	(4)
	Administrative data on bond sales		Survey data	
<u>Dependent variable:</u>	1(Bought any shares)	Total spent on shares (MK)	Value of net asset purchases in past two months (MK)	1(Any unprocessed maize purchase ≥ MK 2000)
<u>Panel A - Round 1 and 2 pooled</u>				
<u>Effect of lump sum wage payments for workers who:</u>				
I. Prefer lump sum payments	0.0377 (0.0287)	75.24 (67.94)	-350.9 (626.5)	0.0926*** (0.0248)
II. Do not prefer lump sum payments	0.0791 (0.0519)	251.9** (124.9)	222.0 (675.3)	0.130*** (0.0376)
Number of observations	674	674	674	674
p-value: I=II	0.485	0.214	0.467	0.376
<u>Panel B - Round 1 only</u>				
<u>Effect of lump sum wage payments for workers who:</u>				
I. Prefer lump sum payments	0.000122 (0.0464)	4.847 (96.19)	203.8 (1,086)	0.0474 (0.0370)
II. Do not prefer lump sum payments	0.0397 (0.0704)	219.5 (146.2)	1,798* (1,058)	0.0581 (0.0529)
Number of observations	343	343	343	343
p-value: I=II	0.641	0.217	0.221	0.858
<u>Panel C - Round 2 only</u>				
<u>Effect of lump sum payments for workers who:</u>				
I. Prefer lump sum payments	0.0796* (0.0407)	153.4 (107.0)	-728.1 (660.4)	0.135*** (0.0359)
II. Do not prefer lump sum payments	0.112* (0.0631)	274.9* (165.4)	-1,262* (709.1)	0.204*** (0.0617)
Number of observations	331	331	331	331
p-value: I=II	0.676	0.548	0.411	0.297

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

G Market day wage payment treatment

This section summarizes the results of the second cross-randomized experiment that was conducted as part of our study. Workers were randomized into receiving their wages either on Friday or Saturday. Both groups picked up their money at the site of major local market day, but the market day only takes place on Saturdays. Market days are commonly listed as one of the most tempting situations faced by people in our study. Thus workers in the Saturday wage payment arm were exposed to a more-tempting environment while they had liquid cash on hand. To ensure transaction costs were equalized across study arms,

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

Panel A of Table G.1 presents the effects of the second randomized experiment pooling together the main experiment study arms. Panel B presents a fully-interacted specification that allows the effect of receiving wages on the market day to vary based on whether the worker was paid in a lump sum. We find no effects of market day wage payments on any major outcome in either specification.

Table G.1
Effects of receiving wages during market day on main outcomes

	(1)	(2)	(3)	(4)
	Administrative data		Survey data	
<u>Dependent variable:</u>	1(Bought any shares)	Total spent on shares (MK)	Value of net asset purchases in past two months (MK)	1(Any unprocessed maize purchase ≥ MK 2000)
<hr/> <u>Panel A - Pooling main experiment study arms</u>				
Received wages during market day	-0.0265	-41.58	-467.0	0.0212
	(0.0260)	(63.41)	(535.4)	(0.0229)
Dependent variable mean, non-market day payments group	0.145	307.8	2,581	0.101
Number of observations	689	689	689	689
<hr/> <u>Panel B - Fully interacted specification</u>				
Received wages during market day	-0.0224	-28.87	-374.3	0.0383
	(0.0339)	(77.23)	(852.7)	(0.0256)
Lump sum wage payments	0.0532	133.7	-89.63	0.117***
	(0.0366)	(85.13)	(876.9)	(0.0299)
(Received wages during market day)X (Lump sum wage payments)	-0.00582	-19.56	-193.7	-0.0285
	(0.0515)	(123.4)	(1,130)	(0.0461)
Dependent variable mean, non-market day payments group (weekly only)	0.112	230.8	2,604	0.0414
Number of observations	689	689	689	689

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

H Robustness of results to omitting controls

The tables in this section repeat our main regression analyses, but omit all control variables (including stratification cell indicators). The regression specification for this section is hence:

$$Y_{ir} = \alpha + \beta L_{ir} + \varepsilon_{ir} \tag{6}$$

None of our results are substantively affected by the omission of the controls. Our core result, on bond purchases, remains statistically significant and has an almost-identical point estimate using the no-controls specification.

Table H.1Effects of lump sum payments on expenditure levels
(without controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Payday survey panel - Spending at market on the four payday weekends				Household survey data		
<u>Dependent variable:</u>	Amount spent on paydays (MK)			(Spending on payday)/ (Income received)	Income received since last Friday (MK)	Remaining cash out of income received since last Friday (MK)	Total spending [†] since Friday from itemized expenditure data (MK)
	Paydays 1-3	Payday 4	Paydays 1-4				
Lump sum wage payments	-1,097*** (51.12)	601.1*** (64.63)	-496.1*** (84.60)	-0.167*** (0.0283)	1,466*** (213.9)	121.0 (74.26)	370.6** (169.0)
Dependent variable mean, weekly wage payments group	1,530	606.7	2,133	0.713	2,309	468.5	2,962
Number of observations	696	696	696	696	689	689	689

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). Regressions are run on pooled data from round 1 and round 2. 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses.

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

Table H.2

Effects of lump sum payments on asset accumulation, loans, and transfers
(without controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<u>Dependent variable:</u>	Value of net asset purchases in past two months (MK)		Loans received in past month (MK)		Loans made in past month (MK)		Transfers received in past month (MK)		Transfers made in past month (MK)	
	Level	1(Level>0)	Level	1(Level>0)	Level	1(Level>0)	Level	1(Level>0)	Level	1(Level>0)
Lump sum wage payments	-233.8	0.00734	-101.2	0.0220	-143.3	-0.0385	-178.2*	-0.0304	-23.30	-0.0187
	(575.5)	(0.0387)	(428.7)	(0.0380)	(136.0)	(0.0310)	(102.7)	(0.0371)	(46.64)	(0.0374)
Dependent variable mean, weekly wage payments group	2,271	0.468	2,008	0.422	596.2	0.276	688.9	0.494	249.7	0.439
Number of observations	689	689	689	689	689	689	689	689	689	689

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. Regressions are run on pooled data from round 1 and round 2. Asset purchases are measured since the previous survey, a period of approximately two months. Loans are measured since November 1st in round 1 and since January 1st in round 2, a period of approximately one month. Transfers are measured over the month leading up to the survey interview. For details of the empirical strategy see section 4, and for complete variable definitions see Appendix D. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses.

Table H.3
Effects of lump sum payments on purchases of risk-free, high-return “bond”
(without controls)

	(1)	(2)	(3)	(4)
	Administrative data			
	on bond sales		Survey data	
<u>Dependent variable:</u>	1(Bought any shares)	Total spent on shares (MK)	Remaining cash out of income received since last Friday (MK)	Income received since last Friday (MK)
<u>Panel A - Round 1 and 2 pooled</u>				
Lump sum wage payments	0.0540** (0.0256)	132.2** (60.74)	121.0 (74.26)	1,466*** (213.9)
Dependent variable mean, weekly wage payments group	0.106	223.5	468.5	2,309
Number of observations	699	699	689	689
<u>Panel B - Round 1 only</u>				
Lump sum wage payments	0.0191 (0.0392)	81.21 (83.66)	-4.888 (111.1)	1,047*** (311.4)
Dependent variable mean, weekly wage payments group	0.149	274.3	543.0	2,604
Number of observations	348	348	343	343
<u>Panel C - Round 2 only</u>				
Lump sum wage payments	0.0893*** (0.0328)	183.5** (84.55)	246.2** (103.0)	1,885*** (237.1)
Dependent variable mean, weekly wage payments group	0.0632	172.4	393.1	2,010
Number of observations	351	351	346	346

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. For details of the empirical strategy see section 4, and for complete variable definitions see Appendix D. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses.

Table H.4
Effects of lump sum payments on bulk purchases of unprocessed maize
(without controls)

<u>Dependent variable:</u>	Bulk purchases of unprocessed maize				Falsification test: Other maize purchases		
	(1) 1(Any unprocessed maize purchase ≥ MK 2000)	(2) 1(Any unprocessed maize purchase > MK 2000)	(3) 1(Any unprocessed maize purchase ≥ MK 1000)	(4) Total spending on unprocessed maize (MK)	(5) 1(Any unprocessed maize purchase < MK2000)	(6) 1(Any maize flour purchase ≥ MK 2000)	(7) Total spending on maize flour (MK)
<u>Panel A - Round 1 and 2 pooled</u>							
Lump sum wage payments	0.0970*** (0.0214)	0.0525*** (0.0184)	0.135*** (0.0331)	245.5*** (77.03)	-0.0310 (0.0350)	0.0136 (0.0151)	-18.02 (63.43)
Dependent variable mean, weekly wage payments group	0.0562	0.0478	0.197	563.4	0.376	0.0365	423.2
Number of observations	715	715	715	715	715	715	715
<u>Panel B - Round 1 only</u>							
Lump sum wage payments	0.0537* (0.0321)	0.0246 (0.0281)	0.0683 (0.0455)	92.63 (124.2)	-0.00924 (0.0510)	0.0238 (0.0210)	-19.08 (99.83)
Dependent variable mean, weekly wage payments group	0.0734	0.0621	0.203	616.5	0.350	0.0282	438.7
Number of observations	350	350	350	350	350	350	350
<u>Panel C - Round 2 only</u>							
Lump sum wage payments	0.138*** (0.0316)	0.0794*** (0.0269)	0.197*** (0.0463)	390.9*** (106.9)	-0.0528 (0.0508)	0.00369 (0.0221)	-16.14 (81.95)
Dependent variable mean, weekly wage payments group	0.0391	0.0335	0.190	511.0	0.402	0.0447	407.9
Number of observations	365	365	365	365	365	365	365

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. Asset purchases are measured since the previous survey, a period of approximately two months. Loans are measured since November 1st in round 1 and since January 1st in round 2, a period of approximately one month. Transfers are measured over the month leading up to the survey interview. For details of the empirical strategy see section 4, and for complete variable definitions see Appendix D. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses.

I Robustness of results to controlling for cross-randomized market day treatment

The tables in this section repeat our main regression analyses, but add controls for the cross-randomized market day treatment and the interaction between the two treatments. We de-mean the market day treatment indicator M_{ir} prior to constructing the interaction term. This is the specification suggested by [Imbens and Rubin \(2015, p. 247\)](#), because it means the main effect of the monthly wage payments can be interpreted as the average treatment effect across the market day and non-market day subgroups. The regression specification for this section is hence:

$$Y_{ir} = \alpha + \beta L_{ir} + \delta M_{ir} + \rho L_{ir} * M_{ir} + \gamma' \mathbf{X}_{ir} + \varepsilon_{ir} \quad (7)$$

None of our results are substantively affected by the additional controls. Our core result, on bond purchases, remains statistically significant and has an almost-identical point estimate to the results in the paper.

Table I.1

Effects of lump sum payments on expenditure levels
(controlling for interaction with market-day treatment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Payday survey panel - Spending at market on the four paydays				Household survey data		
<u>Dependent variable:</u>	Amount spent on paydays (MK)			(Spending on payday)/ (Income received)	Income received since last Friday (MK)	Remaining cash out of income received since last Friday (MK)	Total spending [†] since Friday from itemized expenditure data (MK)
	Paydays 1-3	Payday 4	Paydays 1-4				
Lump sum wage payments	-1,096*** (50.84)	578.3*** (64.36)	-517.9*** (84.43)	-0.173*** (0.0283)	1,657*** (172.9)	143.9** (71.87)	368.2** (153.3)
Dependent variable mean, weekly wage payments group	1,528	606.8	2,131	0.713	2,309	468.5	2,962
Number of observations	689	689	689	689	689	689	689

Notes: Sample includes 359 respondents who participated in at least one round of the work program and have data from at least one data source for that round (either the payday data, the survey, or both). Regressions are run on pooled data from round 1 and round 2. 1 USD was worth approximately MK400 at market exchange rates and MK160 at PPP exchange rates during the study period. All regressions control for the market day treatment, the interaction of the market day treatment with the lump sum treatment, stratification cell fixed effects, an index of baseline asset ownership based on first principal components, indicators for the number of days after the weekend the interview occurred, baseline total spending and (if available) the baseline value of the outcome variable. Heteroskedasticity-robust standard errors, clustered by worker, in parentheses. Itemized expenditure data does not include all purchases, and so these estimates are likely to be a lower bound; see Section 3.1 for details.

Table I.2

Effects of lump sum payments on asset accumulation, loans, and transfers
(controlling for interaction with market-day treatment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<u>Dependent variable:</u>	Value of net asset purchases in past two months (MK)		Loans received in past month (MK)		Loans made in past month (MK)		Transfers received in past month (MK)		Transfers made in past month (MK)	
	Level	1(Level>0)	Level	1(Level>0)	Level	1(Level>0)	Level	1(Level>0)	Level	1(Level>0)
Lump sum wage payments	-186.6	0.00913	-130.1	0.0386	-145.8	-0.0446	-169.0*	-0.0471	-31.90	-0.0208
	(529.6)	(0.0383)	(366.7)	(0.0373)	(118.3)	(0.0311)	(101.5)	(0.0376)	(44.40)	(0.0375)
Dependent variable mean, weekly wage payments group	2,271	0.468	2,008	0.422	596.2	0.276	688.9	0.494	249.7	0.439
Number of observations	689	689	689	689	689	689	689	689	689	689

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

Table I.3
Effects of lump sum payments on purchases of risk-free, high-return “bond”
(controlling for interaction with market-day treatment)

	(1)	(2)	(3)	(4)
	Administrative data on bond sales		Survey data	
<u>Dependent variable:</u>	1(Bought any shares)	Total spent on shares (MK)	Remaining cash out of income received since last Friday (MK)	Income received since last Friday (MK)
<u>Panel A - Round 1 and 2 pooled</u>				
Lump sum wage payments	0.0502** (0.0251)	123.9** (60.19)	143.9** (71.87)	1,657*** (172.9)
Dependent variable mean, weekly wage payments group	0.108	226.7	468.5	2,309
Number of observations	689	689	689	689
<u>Panel B - Round 1 only</u>				
Lump sum wage payments	0.00844 (0.0387)	59.93 (82.17)	34.77 (109.5)	1,302*** (283.3)
Dependent variable mean, weekly wage payments group	0.150	277.5	543.0	2,604
Number of observations	343	343	343	343
<u>Panel C - Round 2 only</u>				
Lump sum wage payments	0.0915*** (0.0331)	188.0** (85.92)	256.2** (100.5)	2,050*** (208.5)
Dependent variable mean, weekly wage payments group	0.0643	175.4	393.1	2,010
Number of observations	346	346	346	346

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

Table I.4
Effects of lump sum payments on bulk purchases of unprocessed maize
(controlling for interaction with market-day treatment)

Dependent variable:	Bulk purchases of unprocessed maize				Falsification test: Other maize purchases		
	(1) 1(Any unprocessed maize purchase \geq MK 2000)	(2) 1(Any unprocessed maize purchase $>$ MK 2000)	(3) 1(Any unprocessed maize purchase \geq MK 1000)	(4) Total spending on unprocessed maize (MK)	(5) 1(Any unprocessed maize purchase $<$ MK2000)	(6) 1(Any maize flour purchase \geq MK 2000)	(7) Total spending on maize flour (MK)
<u>Panel A - Round 1 and 2 pooled</u>							
Lump sum wage payments	0.102*** (0.0221)	0.0528*** (0.0192)	0.139*** (0.0342)	260.9*** (78.41)	-0.0330 (0.0360)	0.0169 (0.0164)	-7.484 (66.40)
Dependent variable mean, weekly wage payments group	0.0581	0.0494	0.203	583.1	0.390	0.0378	437.9
Number of observations	689	689	689	689	689	689	689
<u>Panel B - Round 1 only</u>							
Lump sum wage payments	0.0517 (0.0322)	0.0212 (0.0288)	0.0646 (0.0459)	87.55 (124.0)	-0.0134 (0.0504)	0.0324 (0.0232)	33.35 (99.80)
Dependent variable mean, weekly wage payments group	0.0751	0.0636	0.208	630.8	0.358	0.0289	448.8
Number of observations	343	343	343	343	343	343	343
<u>Panel C - Round 2 only</u>							
Lump sum wage payments	0.150*** (0.0337)	0.0794*** (0.0282)	0.196*** (0.0503)	404.6*** (112.4)	-0.0633 (0.0523)	0.0125 (0.0242)	5.632 (87.81)
Dependent variable mean, weekly wage payments group	0.0409	0.0351	0.199	534.9	0.421	0.0468	427.0
Number of observations	346	346	346	346	346	346	346

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

J Bulk maize purchase results adjusting for price changes

Because maize prices are strongly seasonal in Malawi, the price of maize rose sharply over the course of our study. The FAO provides food price data through its Global Information and Early Warning System Food Price Monitoring and Analysis Tool (GIEWS FPMA). FPMA has monthly data on retail maize prices for four markets in Malawi; the closest one to our study site is Nsanje. We extracted the monthly price data for this market for the period covered by our study: round one ran from November to December 2013 and round two ran from December 2013 to January 2014. The monthly price data for Nsanje can be accessed via [this permalink](#).

The FPMA maize prices for Nsanje rose by 23% during round one and 13% during round two, for an overall increase of 39%. Since our measure of maize purchases is in nominal Kwacha, differences in treatment effects across rounds potentially reflect price changes as well as changes in real quantities. To address this issue, Appendix Table J.1 replicates Table 6, adjusting all maize prices to be in round two values. Specifically, we increase all baseline maize prices by 39% and all round one prices by 13%. The results are substantively unchanged by this adjustment.

Table J.1
Effects of lump sum payments on bulk purchases of unprocessed maize
(adjusting maize prices to round two dollars)

Dependent variable:	Bulk purchases of unprocessed maize				Falsification test: Other maize purchases		
	(1) 1(Any unprocessed maize purchase \geq MK 2000)	(2) 1(Any unprocessed maize purchase $>$ MK 2000)	(3) 1(Any unprocessed maize purchase \geq MK 1000)	(4) Total spending on unprocessed maize (MK)	(5) 1(Any unprocessed maize purchase $<$ MK2000)	(6) 1(Any maize flour purchase \geq MK 2000)	(7) Total spending on maize flour (MK)
<u>Panel A - Round 1 and 2 pooled</u>							
Lump sum wage payments	0.105*** (0.0225)	0.0710*** (0.0215)	0.151*** (0.0352)	265.4*** (84.24)	-0.0355 (0.0361)	0.0221 (0.0178)	-7.936 (71.54)
Dependent variable mean, weekly wage payments group	0.0610	0.0581	0.209	624.3	0.387	0.0465	467.3
Number of observations	689	689	689	689	689	689	689
<u>Panel B - Round 1 only</u>							
Lump sum wage payments	0.0583* (0.0337)	0.0583* (0.0337)	0.0902* (0.0471)	101.5 (139.1)	-0.0225 (0.0502)	0.0493* (0.0273)	35.38 (112.1)
Dependent variable mean, weekly wage payments group	0.0809	0.0809	0.220	712.7	0.353	0.0462	507.1
Number of observations	343	343	343	343	343	343	343
<u>Panel C - Round 2 only</u>							
Lump sum wage payments	0.145*** (0.0331)	0.0794*** (0.0281)	0.201*** (0.0501)	402.6*** (111.8)	-0.0601 (0.0524)	0.00953 (0.0243)	4.451 (87.75)
Dependent variable mean, weekly wage payments group	0.0409	0.0351	0.199	534.9	0.421	0.0468	427.0
Number of observations	346	346	346	346	346	346	346

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

References

Imbens, Guido W., and Donald B. Rubin. 2015. *Causal Inference for Statistics, Social, and Biomedical Sciences*. Cambridge University Press.