Pay Me Later:
Savings Constraints and the Demand for Deferred Payments

Lasse Brune
Northwestern University

Eric Chyn
Dartmouth College and NBER

Jason T. Kerwin
University of Minnesota

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Abstract: We study a simple savings scheme that allows workers to defer receipt of part of their wages for three months at zero interest. The scheme significantly increases savings during the deferral period, leading to higher post-disbursement spending on lumpy expenditures. Two years later, after two additional rounds of the savings scheme, we find that treated workers have made permanent improvements to their homes. The popularity of the scheme suggests a lack of good alternative savings options, and analysis of a follow-up experiment shows that demand for the scheme is also due to the scheme's ability to address self-control issues.

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Acknowledgements: Brune: Global Poverty Research Lab, Northwestern University, Evanston, IL 60208, USA; lasse.brune@northwestern.edu; Chyn: Department of Economics, Dartmouth College, 6106 Rockefeller Center, Hanover, NH 03755, USA, and NBER; eric.t.chyn@dartmouth.edu; Kerwin: Department of Applied Economics, University of Minnesota, 1994 Buford Avenue, St. Paul, MN 55108, USA; jkerwin@umn.edu. We are grateful for insightful comments from Achyuta Adhvaryu, Emily Breza, Lorenzo Casaburi, Michael Callen, Reshma Hussam, Kelsey Jack, Namrata Kala, Dean Karlan, Chris Woodruff, Jonathan Zinman and from seminar participants at the IPA Researcher Gathering on Financial Inclusion, Yale University, the Consumer Financial Protection Bureau, the University of Hong Kong, HKUST, Peking University, China Agricultural University, the Federal Reserve Bank of Philadelphia, the University of Utah, the University of Washington, MIEDC, CSAE, NEUDC, and ASSA. We gratefully acknowledge support from the Financial Services for the Poor Research Fund at Innovations for Poverty Action, sponsored by a grant from the Bill & Melinda Gates Foundation. We thank Rachel Sander for providing excellent research assistance, and Ndema Longwe for his diligent work in managing data collection. This project would not have been possible without the help of the management at the Lujeri Tea Estate and the employees who participated in the study, who generously shared their time with us. This work was supported by the USDA National Institute of Food and Agriculture, Hatch project MIN14-164. This study was reviewed and approved by IRBs in Malawi (NCRSH, protocol # P.08/16/131) and at IPA (protocol # 13888). This study is registered with the AEA RCT Registry under registration number AEARCTR-0001554.

1. Introduction

Standard economic models predict that individuals should prefer to be paid early. However, an emerging literature in developing countries documents notable demand for deferred payments for goods and services (Casaburi and Macchiavello 2019, Brune and Kerwin 2019, Kramer and Kunst 2019). Furthermore, millions of workers in developed countries choose to defer income by opting to overwithhold tax payments that are later returned as refunds (Thaler 1994).

One potential benefit of deferred payment is that it naturally generates the lump sums needed for purchasing durable goods, making business investments or buying in bulk. This may be important in developing countries: qualitative evidence suggests poor households exert substantial effort to generate lump sums (Collins et al. 2009). People in developing countries may prefer saving through deferred payments because access to high-quality formal banking is limited and informal saving options are generally risky (Dupas et al. 2014; Karlan et al. 2014). In addition, deferred pay may help address behavioral constraints such as self-control problems (Laibson 1997; Ashraf et al. 2006; Bryan et al. 2010) or limited attention (Karlan et al 2016).

This paper provides new experimental evidence on the demand for deferred payments as a saving method and its effects on downstream outcomes. We study a sample of 870 full-time workers at a large agricultural employer in rural Malawi. We randomized access to a simple savings scheme, which allowed each worker to choose a fraction of their pay to be deferred at zero interest and paid out as a lump sum at the end of a three-month accumulation period. All payments were provided in cash through the firm's regular payroll infrastructure. This scheme created a simple, no-frills option for workers to save for lumpy purchases by piggybacking on existing firm payroll infrastructure to shift the timing of payments.

We find that this savings scheme is popular and changes worker behavior. Fifty-one

percent of workers sign up for the scheme, and participants opted to save 14 percent of regular wages on average.[1,2] We find that the scheme raises overall savings during the accumulation period, rather than just substituting for other forms of savings.[3] Much of the money saved was spent on lumpy purchases: more than half of treatment workers' additional spending in the two weeks after payout goes toward lumpy purchases, including for durable purchases related to housing investment.[4]

Participation in the deferred wages scheme has significant effects on downstream outcomes. Four months after the end of the scheme, a broad measure of the value of durable assets increases by 10 percent. This increase is concentrated in stored materials for house improvements such as sheets of metal roofing. The main threat to the interpretation of this result is that we do not find impacts on asset purchases, which was our pre-specified primary outcome. However, we find results that are aligned in long-run follow-up data, after treatment-group workers were offered the scheme two more times.[5] Specifically, two years after the initial round of the scheme (and ten months after the last round), treatment-group workers are 7.6 percentage points more likely to have metal roofs on their homes. This result is robust to adjustment for multiple hypothesis testing.

The popularity and effectiveness of the deferred wages savings scheme implies a lack of safe and convenient existing alternative savings options. Evidence from a follow-up experiment supports this view and shows that an additional factor for the scheme's success is its ability to help

[1] We offered the product by holding information sessions and enrollment meetings, but we provided no subsidies and conducted no other marketing.
[2] Take-up and use of the product do not appear to be due to mistakes. First, workers had the option to drop out of the scheme for self-declared "emergencies" at no monetary cost, and only 3.7 percent did so. Second, we allowed treated workers to re-enroll for two additional rounds of the product, finding a take-up rate of about 80 percent in each round.
[3] Treated workers substitute away from informal savings methods somewhat, but there is a 24 percent net increase in savings. A small part of this increase comes from higher earnings: worker productivity rises by 4.6 percent.
[4] Supplementary experiments show that lump-sum payments are an important feature of the scheme. Only 36 percent of workers enrolled in a version of the product with a smooth payout — significantly fewer than the original scheme.
[5] Treatment-group workers were signed up for these repeat schemes during a follow-up survey. Control-group workers were also told about the repeat schemes, but they could only enroll at the payroll office. Fewer than 20 percent did so.

workers with behavioral constraints. We randomly offered a new sample of workers either the original scheme or a modified version of the product that required *manual* deposits at an easy-to-reach workplace location. The manual deposits scheme resulted in substantially lower savings. While initial sign-up rates were similar for the two schemes, workers in the manual deposits scheme save 50 percent less. Crucially, this reduction is driven by workers who report having self-control problems. In contrast, workers with high self-control are unaffected.[6] At the same time, take-up of both versions of the scheme is substantial even among workers who do not report self-control problems, which suggests workers also lack safe places to store money.

This paper contributes to recent work on payment deferral in developing countries (Casaburi and Macchiavello 2019; Brune and Kerwin 2019; Kramer and Kunst 2019). We build on prior studies in four main ways. First, we show robust demand for deferred payments: workers sign-up for a real-world savings scheme that pays them later, deposits are sizeable, and the rate of repeat take-up is high. Second, we demonstrate sustained downstream impacts. This finding suggests deferral of payments can substantially relax existing constraints. In addition, the impact on downstream outcomes and the high rate of repeat take-up suggest deferred pay is welfare-enhancing in our setting (Chetty 2015). Third, we provide new evidence on mechanisms driving demand for deferred payments. Consistent with Casaburi and Macchivello (2019), we find that self-control problems are key. At the same time, our results show this is not the only explanation; our evidence suggests that workers also lack safe and convenient ways to store money. Fourth, we use the firm's administrative records to show that deferred wage payments increase worker productivity, in line with recent results on the relationship between savings and labor supply from Callen et al. (2019).

[6] Patterns of take-up for the original scheme also suggest that self-control issues are important: low self-control is positively correlated with deposits, even after adjusting for other potential drivers of demand.

This study also adds to our knowledge on the optimal design of savings products in developing countries. Prior research has demonstrated that simply reducing the financial cost of savings products such as bank accounts has little impact (Karlan et al. 2014; Dupas et al. 2018). In contrast, we find that reducing time and transit costs are helpful. When workers had to self-enroll at the payroll office, take-up of the scheme fell by 75 percent. Also, contributions fell by 50 percent when workers had to make deposits manually. These findings on the importance of time and transaction costs are broadly consistent with Dupas and Robinson (2013a) and Prina (2015). Our results also build on other research on automatic deposits (Breza et al. 2017; Somville and Vandewalle 2018), the role of saving defaults (Brune et al. 2017; Blumenstock et al. 2018), limited attention (Karlan et al. 2016) and commitment saving schemes (Ashraf et al. 2006; Karlan et al. 2012; Dupas and Robinson 2013a; Karlan and Linden 2014; Beshears et al. 2015; Brune et al. 2015). Relative to the commitment savings literature, we contribute by testing for repeat take-up, which is an important but understudied step to rule out the possibility that impacts of products with commitment features are the result of mistakes (Laibson 2015, John 2019).

Finally, this paper provides new evidence on the link between savings and downstream outcomes for poor households in developing countries. Most previously-studied savings interventions have low rates of product take-up or utilization (Karlan et al. 2014). Correspondingly, many studies find no evidence of detectable impacts on outcomes such as household income, expenditures or wealth (Dupas et al. 2018). Our study is one of few to show effects on asset accumulation and wealth. Schaner (2018) shows that providing short-term interest subsidies for bank accounts leads to a long-run increase in household assets, but due to habit formation and not through the use of the accounts. Dupas and Robinson (2013a) find that access to savings accounts has impacts on business investment and no impacts on household assets.

2. Background and Main Intervention

This study took place in partnership with Lujeri Tea Estates, a large agricultural firm in Malawi. The target population for this intervention is the employees of the estate. Two broad categories of workers constitute Lujeri's workforce. "Pluckers" pick tea for a piece rate per kilogram of tea they harvest. They earn approximately PPP USD $7 (MK 5,400) per day on average during the main season (December to April).[7] Pluckers can increase their earnings by working harder because they are paid a piece rates. Other workers do jobs like pruning, weeding, applying fertilizer, and monitoring and management tasks. We refer to these other employees as "non-pluckers." Non-pluckers receive fixed daily wages based on the task they are performing.[8] For all employees, Lujeri pays earnings every two weeks.

Over the course of a year, workers at Lujeri experience substantial variation in income. During the main season, income rises and falls due to variation in plant growth. Incomes in the offseason are lower because tea growth is limited.[9] Thus, the main season is when workers have a relatively high demand for savings, both to smooth consumption across seasons and to be able to make lumpy purchases of durable goods (such as iron roof sheets and other building materials) and other indivisible investments such as school fees.

In our main intervention, we provided randomly-selected workers the option of receiving a portion of their earnings as a deferred lump sum payment at the end of the main season in May

[7] The local currency is the Malawian Kwacha (MK). During the study period the exchange rate was approximately MK 750 per USD.

[8] Non-pluckers occasionally pick tea and pluckers occasionally do other tasks. A worker's pay is based on the task she does on a specific day: if a plucker spends a day doing pruning, she gets the fixed daily wage for pruning, and if a non-plucker spends a day plucking tea, she is paid based on the number of kilograms of tea she harvests.

[9] For the same reason, Lujeri lays off some workers, while the majority of workers from the main season remain working at a reduced schedule.

2017.[10] Savings in the scheme earned zero interest.[11] Participants determine their contributions to the scheme by setting two parameters for their bi-weekly take-home payment: a minimum level of take-home pay and maximum deferral amount. For example, a worker might set a minimum take-home pay of MK 9,000 per payday and a maximum deduction of MK 3,000. If this worker earned MK 10,000 in a pay period, they would contribute MK 1,000 to the scheme and take home the remaining MK 9,000. If this worker instead earned MK 14,000, they would contribute MK 3,000 to the scheme and take home the remaining MK 11,000. Workers could only access the balance in the deferred wages scheme by exiting the program permanently. We explained that this process applied to cases of emergency and emphasized that no future deductions through the deferred wage scheme would take place after exit. However, there were no procedures in place for verifying that the reasons for exiting qualified as actual emergencies.[12]

Figure 1 provides an overview of the timing of the intervention, sample recruitment, and surveys for the initial implementation of the program. We identified workers as being eligible for treatment based on their responses to a product interest survey that we conducted from October 31 to December 29, 2016, with a sample of 1,897 individuals who currently worked full-time at the company and had also worked full-time during the previous main season. Out of those full-time workers, 65.4 percent (1,240 workers) indicated that they would be interested in participating in a deferred wages scheme. We targeted those who indicated interest in deferred wages to attempt a baseline survey and a subsequent social network survey. A total of 1,092 individuals were interviewed for both baseline and for social network surveys in January 2017. For the deferred

[10] The deferred wages program covered six two-week payment periods starting in February 2017 and covering the bulk of the main tea harvest season.
[11] Inflation was roughly 15 percent per year during the study period, or 3.75 percent over the course of the deduction period, so the real interest rate on savings was slightly negative (Reserve Bank of Malawi 2019).
[12] When filing the required exit paperwork at the firm's offices, workers often listed reasons that did not appear to be emergencies (e.g., wanting to finish construction of a house).

wages experiment, we found that 870, or 78 percent of the 1,092 individuals in the baseline survey remained interested in the deferred wages scheme.[13] Figure 2 presents a flow chart of the study sample recruitment process, illustrating how we went from the initial 1,897 workers in the information sessions to the final sample of 870 workers in the experiment. The 15 percentage-point decline from the share of initially interested workers to the share who are actually willing to enroll is attributable partially to the delay between the initial elicitation of demand and the actual sign-ups, which for many workers was as long as three months. Workers' survey responses suggest that 20 percent did not sign up because they had already joined a savings group with their co-workers at the estate (or in their villages) and did not want to break that commitment.

The set of individuals who were still interested is our main analysis sample for studying impacts of the scheme, and we randomly offered treatment to 50 percent of these workers. Randomization was conducted by using a pre-specified randomized list of treatment statuses for all workers. Using the baseline data, we stratified workers by the division of the estate and then randomly assigned them to either the treatment or the control group. We then checked for balance on a set of 18 variables from the baseline survey and pre-treatment administrative data, repeating this process 1,000 times. We selected the randomization with the lowest maximal *t*-statistic across the 18 balance variables.[14] Appendix Table A1 provides summary statistics and balance test results for the final experimental sample. As expected, there are no statistically-significant differences for

[13] We elicited this choice by telling workers that we would randomly select half of them for implementation and sign up would occur on the spot for those who expressed interest and were chosen. All workers who were chosen for implementation actually enrolled in the product.

[14] The variables we used included the following administrative variables measured from October 3rd 2016 up to the baseline survey: attendance rate, average number of KGs of tea harvested, the share of days on which they plucked tea, and total net pay. They also included the following variables captured on the baseline survey: total expenditures in the past 14 days, total value of stored food, total income in the past 14 days, PCA index of asset values, PCA index of work motivation questions, savings motivation scale, participation in a ROSCA in the previous season, average daily number of meals eaten in past week, age, years of education, and indicators for being married and female. Finally, we include two savings scheme preference variables captured for all workers in our experimental sample, since everyone expressed initial interest in the product: desired minimum take-home pay and maximum deduction.

any characteristic measured at baseline, and we fail to reject the null hypothesis in a test of joint significance (p-value = 0.384).

To study features that drive demand for deferred wage savings, we also conducted a set of supplementary experiments after the initial experiment in 2017. Figure 3 provides a timeline for the additional experiments. As shown in Panel A, the first follow-up experiments offered the treatment-group workers the option to re-enroll in the savings scheme for the subsequent offseason and next main season.[15] For the experiments listed in Panel B, we used new samples of workers who were not involved in the initial experiment and had never been exposed to the savings scheme. In one of these experiments, we randomized offers to enroll either in (a) the original scheme or (b) a version in which workers had to make deposits manually by handing cash to a project employee stationed next to the payroll site. The sample of 186 workers for this experiment was recruited in a different organizational division from the main experiment. Workers' choices were implemented with certainty. Finally, in another experiment, we elicited preferences in a different sample of workers (but in the same divisions as the original experiment) over enrolling in (a) the original scheme; (b) a version of the scheme where the savings were paid out smoothly over a period of several weeks instead of in a lump sum; or (c) a version of the scheme where we relaxed the restrictions on accessing savings during the scheme. We elicited incentivized preferences for all three versions of the savings scheme from a sample of 542 workers: workers were asked about whether they preferred each option to no savings scheme at all, and were told that one randomly-selected worker in their division would have one of their choices would be implemented.

3. Data

To study the effects of deferred wage savings accounts in our main experiment, we use two

[15] Treatment-group workers were enrolled on the spot during in-person interviews four months after the end of the initial savings scheme. Control-group workers were also allowed to enroll by going to the payroll office.

main sources of data for the set of workers who were included in our experiment (*N*=870). First, we rely on individual-level administrative data from Lujeri, which is available from January 2017 to May 2018 (i.e., the beginning of the main tea harvest season for the first iteration of the savings scheme to the end of the main season for the repeat offer of the scheme). This administrative data has two components. It includes daily attendance and activity records for all workers at the firm, including how much tea a worker harvested (if applicable). The dataset also contains bi-weekly payroll data that shows how much workers earned, taxes paid, deductions and take-home pay. In addition, this payroll data also records the balances the workers held in the deferred wages scheme.

Second, we conducted a baseline and five follow-up surveys (FS1 — FS5) that began after randomization of the deferred wages savings accounts. Figure 1 includes an overview of the timing of data collection. The first two follow-up surveys occurred during the main season in February and April 2017. This data allows us to measure impacts during the deduction period of the intervention: changes in expenditure and savings stocks and flows. A third follow-up survey took place in May 2017 after the lump sum payment of the deferred wages scheme, to measure how the money from the scheme was spent. In order to study the effects of the lump sum payout over time, we partially randomized the order in which workers for the third follow-up survey. We randomized the order in which we visited workers at the 11 divisions of the estate in order to vary the timing when individual surveys took place. Within each division, we randomly assigned workers to a first wave or a second wave. A fourth follow-up survey took place in August and September of 2017 to measure impacts four months after the payout, when the scheme had completely ended. This lets us test for downstream effects beyond mere shifts in the timing of expenditures caused by the specific timing of the scheme. We also conducted a fifth follow-up survey in February through April of 2019, between nine and eleven months after the second main-season treatment ended and

over two years after the start of the first round of the scheme. This survey was intended to capture longer-run impacts on asset accumulation and home improvements. We located 83 and 76 percent of the original sample for the fourth and fifth follow-up surveys. For these surveys, Appendix Table A2 shows that attrition is not systematically correlated with treatment status, nor are there differential attrition patterns by the interaction of treatment and baseline covariates.

A key objective of our experiment is to evaluate the impact of the savings scheme on financial behaviors and outcomes, particularly asset accumulation. With this in mind, we designed our surveys to measure asset ownership and purchases, total expenditures, food consumption, income, transfers (i.e., loans received and credits made) and savings balances and flows. While our main analysis focuses on these totals, we first asked individuals to report spending and financial transactions for specific items within each category. We did this to reduce measurement error in the overall totals and to provide details about changes within the broad categories. For example, we asked about detailed expenditures within the last two weeks on specific items such as maize, house improvements, and household purchases.

4. Empirical Strategy

Our main specification to capture the impact of deferred wages is:

$$y_{ist} = \alpha + \beta Treat_i + \delta_s + \gamma Z_i + y_{isB} + \epsilon_{ist} \quad (1)$$

where y_{ist} is the outcome of interest for individual *i* measured at time *t* in strata *s*. The variable $Treat_i$ is an indicator that takes the value 1 if the individual was treated by offering them the deferred wages intervention. Fixed effects for randomization strata (divisions of the estate) are included as δ_s. We control for all the individual covariates Z_i used in the re-randomization exercise, following the recommendation of Bruhn and McKenzie (2009). In Equation 1, our main interest is in the estimate of the parameter β, which is the intention-to-treat (ITT) effect of

providing access to deferred wages. Our random experiment ensures that the treatment is uncorrelated with the error term in expectation, so Equation 1 yields unbiased estimates of the ITT effect of deferred wages.

We also conduct analyses of the effects of the treatment on measures of work effort y_{ist} (e.g., attendance or daily output) for the time period t that begins after the first contribution to deferred wages and continues until the last week of deductions during the main season. These analyses include additional controls for workers' pre-experiment performance, to provide increased precision for these analyses. Specifically, we control for the following variables for the period from October 3 to January 13: the mean, standard deviation, 25th, 50th, and 75th percentiles of their daily KGs of tea plucked (including days with no tea plucking as zero KGs), as well the share of work days they attended work and the share of work days they plucked tea.[16]

4.1 Multiple Hypothesis Testing

We focus our adjustments for multiple hypothesis testing on the main downstream outcome of interest, which is asset ownership and home improvements. Our analysis plan documents specified multiple testing adjustments for each group of intermediate outcomes, but we view those results as primarily shedding light on how the savings scheme works rather than evaluating whether it has an impact on, for example, short-term spending just after the disbursement of funds. Consistent with our analysis plan, we adjust for multiple testing only within domains of outcomes. Because we are interested in the effects of the savings scheme on all asset ownership outcomes across the last two waves of the survey, as a conservative choice we take the union of the primary and secondary outcomes from both waves and conduct multiple-testing adjustments across *all* of the outcomes, separately by wave. We implement multiple-testing adjustment using the Haushofer

[16] All our choices of control variables are laid out in our pre-specified analysis plans, which are available here: https://www.socialscienceregistry.org/trials/1554

and Shapiro (2016) implementation of the Anderson (2008) method of controlling the family-wise error rate (FWER). We pre-specified that these adjustments would be done on PCA indices of the outcomes to improve statistical power, but the main tables show the asset values for ease of interpretation (and to focus on a subset of the outcomes). We present results with the PCA index in Appendix Table A6. (In the appendix tables, we also provide assets results that include all the pre-specified outcomes and show FWER-corrected *p*-values).

Note that our supplementary experiments designed to understand mechanisms driving demand for, and utilization of, the savings scheme were conducted on separate samples and have very limited survey data and outcome variables. We therefore did not file pre-specified analysis plans for these additional experiments, and our analyses are straightforward. For the first supplementary experiment, we simply report mean take-up across the three variants of the scheme, and the *p*-values for differences between the two variant schemes and the original scheme. In the second supplementary experiment, we collected a brief survey about potential moderators of take-up and the effect of manual deposits on the utilization of the savings scheme. We present simple regressions of making a deposit, the number of deposits, and the final balance on the treatment indicator. We also show how the treatment effect varies by the moderators we collected, by interacting the treatment indicator with each moderator.

5. Main Results

5.1 Take-up, Account Use, and Drop-out

Panel A of Table 1 reports statistics on stated interest and enrollment in the deferred wages scheme. As described in Section 2, 1,897 workers attended product information sessions during Fall 2016. At this stage, the first row shows that 65.4 percent of workers were interested in the scheme. We later completed baseline and social network surveys in January 2017 with 1,092

workers who could be found and had expressed initial interest in the deferred wages scheme during the information session. Panel A shows that 77.9 percent workers remained interested in the program at the time of surveying. We made offers for sign-up to a pre-randomized list of these workers.[17]

Overall, the program take-up rate was 50.9 percent (65.4 percent of those stating interest during the information session multiplied by the 77.9 percent who actually enrolled during the surveys). Our final experimental sample is composed of the 870 workers who indicated they were still interested in the savings scheme during the offer visit in January 2017. The treatment and control groups have 438 and 432 workers, respectively.

Panel B of Table 1 reports summary statistics for individual choices in the deferred wage program. As noted in Section 2, treated workers choose two thresholds. First, the minimum take-home pay indicates how much cash they want to take home on given payday before there are any deductions for the deferred wages scheme. Second, the maximum deduction sets an upper limit on the amount of money deducted from a given paycheck.

The first two rows of Panel B show that the average threshold for the minimum take-home pay and maximum deduction are MK 8,239 and MK 2,832, respectively. Note that the actual amount deducted depends on the workers' level of earnings from the firm in given pay period. During the deduction period, workers earn MK 14,555 on average in income. Given the respondent scheme choices, the resulting savings contributions through the scheme are MK 2,056 per payday. This amount is large, representing 14 percent of average earned income for the sample.[18] Overall,

[17] We randomly assigned workers to treatment and control groups using the initial sample of 1,182 workers who had expressed interested in the deferred wages scheme and participated in the baseline survey. In this sample, all of the workers who had expressed interest and were assigned to the treatment group subsequently enrolled in the deferred wages scheme during the actual season.

[18] The vast majority of treated workers hold a positive balance during the wage deferral period. Only five percent of the treatment group holds a zero balance for the entire season.

this pattern of usage differs notably from several prior studies that find low take-up and use of savings products in other contexts (Dupas and Robinson 2013b; Dupas et al. 2014; Dupas et al. 2018).

Figure 4 illustrates how account balances in the deferred wage account change during the season. Workers steadily accumulate savings in the scheme over six pay periods. By the sixth pay period — the final pay period before the savings are disbursed — median savings reached about MK 12,000. Balances drop to zero for the following pay periods as funds are disbursed.

Panel B shows there was minimal attrition from the treatment group. During the deduction period, workers could not access their savings except in the case of an emergency (which they had report in person at the division office). Anyone who pursued the emergency option was required to exit the program, and their balance would pay out at the next payday (typically between 1 and 3 weeks). In the sample, only 16 workers out of 438 in the treatment group (3.7 percent of treated workers) exited.

Consistent with these low exit rates, very few workers express a desire to leave the scheme or reduce their savings contributions. Panel C of Table 1 presents incentivized choices for a 50 percent random sample of treated workers after the scheme had been running for two paydays. During the FS1 survey, we asked these workers whether they wanted to exit the scheme or change their contributions, and implemented the choices for 5 percent of the sampled workers. Just 4 percent of this sample wanted to exit the scheme immediately. Including those 4 percent, a total of 10 percent of workers wanted to reduce their contributions to the scheme, while 15 percent wanted to increase their contributions.

5.2 Impacts of Participation in the Deferred Wages Scheme

5.2.1 Savings Behavior

Table 2 shows estimated effects on savings behavior during the deduction period for the main experiment. For this analysis, we study measures of financial behavior from two rounds of surveys (FS1 and FS2) collected during the deductions period (i.e., February to April 2017). For the analysis, we pool observations across the two rounds for flow variables (e.g. expenditures) to improve precision. Note that stock variables, (e.g., savings balances), were only collected at the second follow-up, which occurred the end of the deferred wages deduction period.

In the 14 days prior to the interview date, Columns 2-3 show that the scheme has some negative impacts on the use of other savings options. For the treatment group, the scheme has a marginally significant 1.7 percentage point negative impact on the likelihood of making any type of deposit in another type of formal savings. For informal financial savings, we see a much larger drop of 7.0 percentage points. This is driven by a 6.2 percentage point reduction in the probability of contributing to an informal savings group.

Columns 4-9 in Table 2 present impacts on savings balances as measured at the second follow-up survey (FS2) , which occurred shortly *before* the end of the deferred wages deduction period. For total net savings, Column 4 shows that we find an increase of MK 7,113, which is 24 percent of the control-group mean. At the same time, we find a decline of MK 3,595 in informal savings (Column 7). This negative effect is driven by effects on the value of stored food (a common form of informal savings in this context).

5.2.2 Labor Market Outcomes

Table 3 presents effects of the deferred wages scheme on work outcomes based on the firm's administrative data covering the deductions period of the main season (February to April

2017). We focus on daily productivity and attendance, as well as effects on income and savings (with the firm) from bi-weekly payroll data. We use two specifications for this analysis. First, we estimate average treatment effects based on Equation 1. Second, we augment our base specification by interacting the treatment indicator with two separate indicators for whether a worker was classified as a plucker or non-plucker during our sampling procedure (based on data available before treatment was assigned).[19] The motivation for this analysis is that pluckers receive piece-rate earnings, whereas non-pluckers work fixed hours for a fixed wage. This implies that pluckers are the only group of workers who can adjust their productivity in meaningful and measurable ways. Approximately 77 percent of workers in the sample are pluckers.[20]

Column 1 of Table 3 shows that the treatment increased average productivity in the pooled sample by 1.6 kg (4.2 percent of the control-group mean). As expected, this effect is concentrated among pluckers (Column 2). In line with this result, Column 4 shows the increase in productivity is associated with an MK 341 (2.4 percent) impact on income earned.[21] These results for worker effort are consistent with prior research on the impact of savings on labor supply. Callen et al. (2019) find that an intervention which increased formal savings also had large positive impacts on labor market activity. They explain this relationship between financial products and labor supply in terms of changes in the effective interest rate on savings. This is also a potential explanation for our results. That is, the costs associated with formal savings mean that our sample of workers may face a negative effective interest rate on their savings, which we raise to zero through our intervention. Lastly, Columns 5 and 6 show that the treatment group saved about MK 2,040 in

[19] This specification includes main effects for whether a worker is a plucker or non-plucker, although we do not report these estimates.
[20] Our pre-treatment plucker classification is highly predictive of working as a plucker during the experiment. Pluckers spend 77 percent of all working days during the experiment plucking tea and harvest an average of 43.6 kg of tea per day, whereas non-pluckers pluck tea just 5 percent of the time and harvest an average of 2.6 kg of tea per day.
[21] There are no treatment effects on attendance or on the share of days spent plucking tea (results available upon request).

deductions per two-week pay period, which amounts to about 14 percent of total income earned from Lujeri. As expected, these savings results are consistent with the choices reported in Table 1.[22]

5.2.3 Expenditures During the Deduction Period

Appendix Table A3 presents treatment effects on expenditures during the deduction period. Despite pooling observations from the first two follow-ups (FS1 and FS2) to increase precision, our power to detect effects on expenditures is limited. Across major categories, the coefficients on the treatment indicator are relatively small and effects are not statistically significant. In Column 1, the 95 percent confidence interval for the impact on total expenditures ranges from MK -1,366 to MK +1,951. Relative to the control group mean, this implies treatment effects of -7.2 and +10.3 percent, respectively. As such, we can only confidently rule relatively large negative effects on expenditures. Similarly, we find no detectable impacts on detailed expenditure categories (Column 2-8) and no strong evidence of effects on large purchases during the past 30 days (Columns 9-11).

Our point estimates present a puzzle, which is that there is no detectable decrease in expenditures despite an increase in savings. A partial explanation is that some of the increase in savings is driven by pluckers (who work for piece rates) and have significantly more earned income. The aggregate change in earnings across our whole sample is MK 1,332, which is 19 percent of the net change in savings from Table 2 (MK 7,113). The remainder of the increase in savings, MK 5,781 implies a decline in expenditure of MK 963.5 per week.[23]

This change is well within the confidence interval for our effects on expenditures, which implies that we are underpowered to detect the changes that took place. Consistent with that story,

[22] The treatment effect estimated in Column 5 of Table 3 does not exactly match the two-weekly deduction in Table 1 because of the control variables included in our regression analyses.

[23] This is true as long as other sources of income do not change. In supplementary analyses, we also find that loans and transfers were unaffected by the treatment (results available on request).

our estimated standard errors imply that we have 80 percent power to detect effects on expenditures of MK 2,400, which is over two times as large as the actual change implied by the earnings and savings data.[24]

5.2.3 Expenditures After the Deferred Wages Payout

Table 4 reports effects on expenditures shortly after the lump-sum disbursement of the deferred wages, which occurred on May 6. Measures for this analysis were collected from a third survey round (FS3). We anticipated that most spending would occur immediately after the payout, and we designed the survey accordingly. The recall period for the surveys conducted in the first 14 days after the deferred wages payout covers the day of disbursement and the following days until the day of the survey date. This was done both to improve the precision of flow measures (like expenditures and changes in savings and transfers), as well as to focus measurement on the post-disbursement period.[25] For surveys conducted more than 14 days after the disbursement we revert to the 14-day recall window used in the preceding survey rounds. Given the design of the third follow-up survey, we conduct separate analyses of the outcomes for those interviewed within 14 days of the scheme payout (May 6) and for those interviewed afterward. This facilitates interpretation of the result given the differences in recall period. Note that the timing of the surveys is plausibly exogenous because we explicitly randomized the timing of interviews (see Section 3 for details).

Panel A of Table 4 shows that total expenditures increased sharply: they rose by MK 5,728 in the 14-day period immediately following the scheme payout.[26] Panel B shows there is no

[24] The MDE at 80 percent power is 2.8 times the standard error (Ioannidis et al. 2017).
[25] Recall periods for this half of the sample ranged from 4 to 14 days. The median recall period was 7 days and the mean was 8.4 days.
[26] Appendix Table A4 shows that the treatment group also had higher savings, net loans given and net transfers, but only the effect on net loans made is statistically significant (p-value<0.10).

detectable treatment effect on spending for the portion of the sample that we interviewed at least 15 days after the lump sum payout. Overall, the pattern of results suggests that workers use funds shortly after disbursement of the lump sum and revert to financial behavior that matches the control group quickly.

Columns 2-6 report results for detailed measures of expenditures. Panel A shows that expenditures in major spending categories (i.e., food and durables) increased in the period immediately following the payout. Notably, spending significantly increased in the sub-categories that respondents had self-reported as savings goals at baseline: food for storage (in particular maize grain) and house improvements.[27] Columns 7 and 10 show that the incidence of large single purchases over MK 5,000 and over MK 10,000 increased by 13 and 13.6 percentage points, respectively. The total amount spent on single purchases greater than MK 5,000 also increased, indicating that treatment caused respondents to use the lump sum disbursement of savings to buy in bulk or buy durables. The effect on the sum of purchases greater than MK 5,000 accounts for two thirds of the increase in total short-run spending. The effects of the lump sum payout on expenditures fade quickly: Panel B shows that there are no detectable impacts on major categories of expenditures for workers interviewed at least 15 days after the deferred wages payout.

5.2.4 Longer-run Outcomes

Panel A of Table 5 reports results for treatment effects on measures from a fourth survey round (FS4) fielded four months after the deferred wage payout. This analysis tests for downstream impacts with a focus on testing whether treated workers had more assets. During the baseline survey, substantial fractions of the sample indicated that their savings goals included building a house (24 percent), purchasing a household asset (15 percent) and making other house-

[27] The most common main savings goals at baseline were building a house (23.9 percent), food (21.3 percent), purchasing household assets (14.7 percent), and working on one's house (13.5 percent).

related investments (14 percent).

Column 1 shows that treated workers have MK 11,326 and MK 7,430 more in durable assets (including livestock) and house materials, respectively. These effects are large: relative to the control group mean, the point estimates represent increases of 10 and 42 percent for all durables and house materials, respectively. Columns 5 demonstrates that much of the impact on total assets is due to an impact on stored building materials. The impact on building materials is largely driven by an increase in purchases of iron sheets for roofing (Column 6).

During the FS4 survey, treatment-group workers were offered the chance to re-enroll in the savings scheme two additional times: once during the off-season and once during the next main season. Interested workers were re-enrolled on the spot. Appendix Table A5 shows the take-up of these additional savings schemes: 81 percent for the off-season and 78 percent for the main season.[28] If the timing of the program were important, we would expect to see different take-up rates; we fail to reject the hypothesis that the take-up rates are equal (p-value = 0.18). This suggests that seasonal timing is not an important feature driving take-up and use of deferred wages in our context and makes it unlikely that participants of the initial experiment during the 2017 main season signed up in order to smooth consumption between the main season and the off-season. The fact that workers re-enroll in the product for a second (and a third) time helps rule out the possibility that they chose it by mistake (Laibson 2015; Bai et al. 2017; John 2019). The high levels of repeat take-up also suggest that the scheme is likely to be welfare-enhancing, since they imply that there is not an important wedge between workers decision utility and experienced utility for the product (Chetty 2015).

Panel B of Table 5 reports impacts on the same asset measures collected on our fifth follow-

[28] Control-group workers were also allowed to enroll, but had to do so at the payroll office. Just 10 percent enrolled for the off-season and 19 percent for the next main season.

up survey (FS5). This survey was two years after the payout for the initial deferred wages scheme, and about ten months after the end of the two additional schemes. We find effects on household improvements that are consistent with the four-month asset ownership results. Treated workers are 7.6 percentage points more likely to report that they have improved their roof through the installation of iron sheets. This effect is a 9 percent increase relative to the control-group mean. We do not find evidence of sustained increases in other assets. Overall, we conclude that the program appeared to have strong impacts on assets. Downstream effects on assets are fairly rare in the literature. Many interventions to increase savings have no effects on any other financial outcome (Dupas et al. 2018). We are aware of only two previous studies that show an effect on assets. Schaner (2018) finds that short-term interest rate subsidies for bank accounts lead to long run increases in assets, but shows that her result is due to habit formation rather than the direct use of the subsidized accounts. Dupas and Robinson (2013a) find that access to savings accounts has impacts on business investments, but do not find effects on household asset ownership.

5.2.5 Robustness Checks

We conduct several robustness checks for our analysis of the impacts of the deferred wages scheme on asset ownership. First, we correct the estimated *p*-values for multiple hypothesis testing. As described in Section 4.1, we adjust for multiple testing across all asset outcomes pre-specified across the two analysis plans for FS4 and FS5, using the Haushofer and Shapiro (2016) implementation of the Anderson (2008) FWER correction. This is a slightly different list of assets from the ones reported in Table 5, but it covers three of the outcomes for which we see treatment effects (i.e., total assets, stored building materials, and whether the worker's house has an iron sheet roof). We also pre-specified the use of PCA indices for the outcomes, to reduce measurement

error, whereas Table 5 presents the values of assets for ease of interpretation.[29] The results are shown in Appendix Table A6. In Panel A, the treatment effects on overall asset ownership (adjusted *p*-value = 0.065) and stored building materials (adjusted *p*-value = 0.001) remain significant after the FWER correction, suggesting we are measuring real effects. The long-run effect on improved roofing is also robust to this correction. In Panel B, which shows the long-run effects of repeated exposure to the savings scheme, the treatment effect on having an metal roof has a FWER-corrected *p*-value of 0.007.

Second, we address potential spillovers between treatment and control workers. Since all workers are employees of the same company, they interact socially and financially.[30] We collected social network data prior to assigning workers to treatment, and we use this data to address potential spillovers, following the approach from Blumenstock et al. (2018). Specifically, we augment Equation 1 by including terms for the workers' total number of peers, and number of treated peers. The results are reported in Appendix Table A7. We see little evidence of peer effects for these financial outcomes, except for negative spillovers on the ownership of stored agricultural inputs. Critically, our main point estimates are nearly unchanged, indicating that our estimates of the effect of the savings scheme on asset ownership are not biased by spillovers.

Third, we repeat the multiple hypothesis testing adjustments in a specification that controls for potential spillovers. Appendix Table A8 shows the results, which are qualitatively similar to the earlier multiple testing-corrected results. The FWER-adjusted *p*-values are nearly unchanged for the effects on total assets and stored building materials in Panel A. For the effect on metal roofing in Panel B, the FWER-adjusted *p*-value is somewhat higher, at 0.037, but remains

[29] Each asset index is a weighted average of the numbers of assets owned, where the weights are the first principal component of the number of items owned in the control group.
[30] Brune et al. (2019) conduct an evaluation of peer effects on worker productivity at Lujeri using data from an experiment conducted in 2015.

statistically significant at conventional levels.

6. Explaining Take-up and Utilization of the Deferred Wages Scheme

In contrast to most prior studies of savings products in developing countries, our intervention has high take-up and usage within the treatment group. For example, Dupas et al. (2018) study basic bank accounts in three countries and find that less than 20 percent of treated households in each country make five or more deposits. Karlan et al. (2014) note similarly low usage rates in evaluations of other types of savings products.[31]

6.1 Manual vs. Automatic Deposits

To shed light on the mechanisms behind the high uptake and utilization of this savings scheme, we conducted a supplementary experiment that varied whether the deposits were made automatically or manually. Workers might value the automatic deposits in the deferred wages scheme — money is deducted automatically from their paychecks and accumulated into the lump sum — because this feature lowers transaction costs and mitigates self-control issues. In the original scheme, workers make an allocation once, and funds are automatically set aside each payday. This feature might be important for present-biased workers who are aware that they will fail to set aside funds even if safe storage options with low transaction costs are available.

As noted in Section 2, the supplementary experiment used a sample of 186 workers, excluding workers who were part of the earlier study. We randomly assigned these workers into two groups. One group of workers received an offer to participate in the original version of the deferred wages scheme. The other group of workers were offered a version of the deferred wage

[31] Two notable exceptions are work by Dupas and Robinson (2013a) and Prina (2015). Dupas and Robinson covered bank account fees and helped people open accounts, finding a take-up rate of 87 percent with 37 percent of people making more than one deposit; average weekly deposits averaged 12 percent of weekly income. Studying a no-fee savings account, Prina found that 84 percent of those offered took up the account, and 80 percent made more than one deposit. The average weekly amount deposited was about 8 percent of average weekly income among those who were offered the account. Both studies find usage numbers comparable to our setting (an average 14 percent of income deposited among the about 50 percent who signed up for the scheme).

scheme that required them to make deposits manually with a research staff member. The location for these deposits was easy-to-reach: workers made deposits at a station next to the division office where the firm pays out the workers' wages. For workers who received the standard deferred wages offer, the take-up rate was 51 percent. Among workers who received the variation with manual deposits, 48 percent signed up for the program. These rates are similar to the take-up for the initial deferred wage scheme offered in 2017. Prior to telling workers their treatment status, we also conducted a brief survey to measure the determinants of take-up for each version of the scheme.

Table 6 reports differences in savings behavior between workers offered the original (automatic) and manual deposits versions of deferred wages scheme. Panel A shows that requiring manual deposits did not make workers any less likely to express interest (Column 1), but substantially reduced follow-through (Column 2). The manual deposits treatment reduced the likelihood of making at least one deposit (out of six possible deposits) by 30.3 percentage points, or about 60 percent of the control-group mean. This difference is strongly statistically significant (p-value < 0.001). It is also worth noting that the rate of initial interest matches the rate of making any deposit in the original scheme: everyone who signed up made at least one deposit. In addition to the reductions in overall participation, Columns 3 and 4 show that manual deposits significantly reduce the number of deposits by 1.85 and the account balance by MK 3,516. These effects are all large relative to the mean outcomes in the automatic deduction group: contributions fall by over 50 percent as a result of the manual deposits requirement.

These results are consistent with the idea that present bias and self-control issues hamper savings behavior for workers in our sample. Panel B of Table 6 provides further support for this hypothesis by examining heterogeneity in the effects of the manual deposit feature on deferred wages balances. Specifically, this model adds interactions between the indicator for whether a

worker had manual deposit offer with indicators categories of several other pre-treatment variables. The set of indicators is for self-reported feelings of regret in consumption choices, which we interpret as a measure of self-control problems.[32] This could also indicate workers have problems with limited attention (Karlan et al. 2016). We also include interactions between indicators for manual deposit and a measure of whether the worker faced kin taxes, as well as whether the worker had a formal savings account.[33] The omitted group in the specification is the set of workers who have the lowest self-reported self-control, face high kin taxes, and do not have formal savings accounts. The results in Columns 1 and 2 show that self-control does not moderate the effect of the manual deposits treatment on take-up. However, while low-self-control workers do sign-up for both schemes, they save significantly *less* when they are required to make deposits manually. Moreover, the estimates for the interaction terms show that manual deposits do not reduce deposits for high-self-control workers.

These results imply that an important driver of the take-up and utilization of the original savings scheme is that it helps mitigate workers' self-control problems. Workers would like to save money in these savings schemes, but they are unable to follow through — particularly if they report having issues with self-control. This finding parallels Casaburi and Macchiavello (2019), who show that people who have issues with self-control may demand deferred payments. Additional evidence on this front comes from an analysis of the determinants of the take-up of the

[32] During the survey for this supplementary experiment, we asked respondents the following question: "Which of the following statements would best describe your situation. When you buy things: a) you usually regret buying them afterwards because you did not think enough about the purchase beforehand and you bought the item on impulse; b) you sometimes regret buying them; c) you rarely regret buying them."

[33] We measured kin taxes by asking the following question: "If you had the choice between receiving an unexpected gift of MK 5000 privately without anyone knowing that you received any money *or* receiving MK 8000 in front of everybody at the office during payday, which one would you prefer? a) 5,000 privately; b) 8,000 publicly." This question was motivated by evidence that publicly-received money is more likely to be taxed by kin (Goldberg 2017) and that people are willing to forgo part of their earnings in order to hide money from kin (Jakiela and Ozier 2016).

original scheme from the same experiment (Appendix Table A9).[34] Low self-control is not correlated with extensive-margin take-up of the original savings scheme, but it is correlated with deposits. This suggests that, at least to some extent, enrollees may be aware of their own self-control problems (Laibson 2015).

At the same time, there is also evidence of that the scheme is beneficial for workers with no self-control problems at all. Nearly 40 percent of the workers who sign up for the manual deposits scheme do make deposits into it, and workers who do not report self-control issues have deposits rates that are comparable in both versions of the scheme. Why, in that case, would they want to put their money away at zero nominal interest? The most-plausible explanation is a lack of alternative safe places to store their money. Consistent with this explanation, take-up of the scheme is positively correlated with past participation in rotating savings groups, which can serve as another safe way to move money into the future (Beaman et al. 2014; Karlan et al. 2017). This indicates that the demand for safe storage is one of the drivers of the high take-up of this product. Also consistent with that view is the fact that current participation in savings group is negatively correlated with take-up. If savings groups are substitutes for our savings scheme, this is what we would expect; also, there are strong sanctions against leaving savings groups once you have joined, and so membership in one will tend to prevent switching to alternative savings options (Besley et al. 1993).

6.2 Choice Experiment on Other Features

We also conducted an additional choice experiment to examine the importance of (a) disbursing funds in a lump sum and (b) restricting access to the funds during the season. Workers

[34] Our research design for the main experiment focused on collecting data only on workers who expressed interest in the saving scheme; we do not have baseline survey data from that experiment for workers who were not interested in the scheme, and thus cannot replicate this analysis with that larger sample.

were asked whether they wanted to enroll in one of three savings schemes: the original scheme or two modified schemes.[35] Under a "smooth payout" scheme, workers would receive six payments in two-week intervals after the end of the deductions period. Under a "more access" scheme, workers could withdraw accumulated funds at any point during the deduction period.[36]

Appendix Table A10 reports the results from this additional experiment. For the original version of the scheme, the take-up rate was 55.9 percent. This is significantly higher than the take-up rate of 36.2 percent for the smooth payout version of the deferred wages scheme (p-value < 0.001). This pattern suggests that workers value the lump sum payout in the original deferred wage scheme, but with the caveat that this is a very specific alternative payout. We did not test other alternatives, such as providing the ability to drawn down the funds at any time. We also find that 51.8 percent of workers choose the version of the program with more access. This rate is statistically indistinguishable from the take-up for the original version of the program (p-value = 0.430). This suggests that the value of the additional commitment being offered was limited, but the "more access" scheme still has a meaningful commitment feature since funds could not be accessed until at least the next payday.

7. Conclusion

Many people in developing countries prefer deferred income receipt (Casaburi and Macchiavello 2019; Brune and Kerwin 2019; Kramer and Kunst 2019). We experimentally study demand for payment deferral and its effects on outcomes by offering a no-frills employer-based savings scheme that allowed workers to receive a fraction of pay as a lump sum at the end of three

[35] We elicited preferences about all three schemes from each worker, but we randomized the order presented. We also incentivized choices about the first scheme that we asked about by informing workers that one randomly selected choice would be implemented. For this reason, we use only the first question that was asked in our analysis.

[36] Note that withdrawal was not immediate nor completely frictionless in this option: participants would have to give at least notice one week in advance by signing a request form at the division office and funds would be disbursed through the regular, bi-weekly payroll.

months at zero interest. By changing the timing of pay, workers could transfer money across time safely and conveniently, allowing them to better match income streams to liquidity needs.

We find that this savings scheme is popular, with high take-up and usage. Net savings increased sharply, and participants spent their savings on lumpy purchases. Consistent with Callen et al. (2019), we find that the scheme increased labor supply. We also find large and significant downstream impacts. Four months after the savings scheme ended, we find positive impacts on household assets. Follow-up data collected two years later shows that treatment group workers were 7.6 percentage points more likely to have metal roofs on their homes.

Rationalizing the popularity of this product requires modifying the standard model of intertemporal choice and introducing either storage costs or behavioral constraints. We show evidence for both factors. Requiring manual (rather than automatic) deposits does not affect take-up of the scheme but drastically reduces follow-through, particularly among workers with self-control problems. At the same time, workers who do not report self-control problems contribute just as much with manual deposits (relative to having automatic deposits). This suggests that these workers must value this scheme for reasons other than self-control. A plausible reason is that the deferred wages scheme provides a safe place to store money, and take-up regressions are consistent with this explanation.

We conclude by noting that the savings scheme that we study has potential applications in a wide range of settings across the developing world, not just for employers but also in cash transfer schemes and workfare programs. Deferred wages sidestep the frictions in underdeveloped financial markets by eliminating the need for local financial intermediaries. This type of savings scheme is a potentially cheap alternative to formal savings accounts that could help anyone who receives regular payments, whether from an employer or a government.

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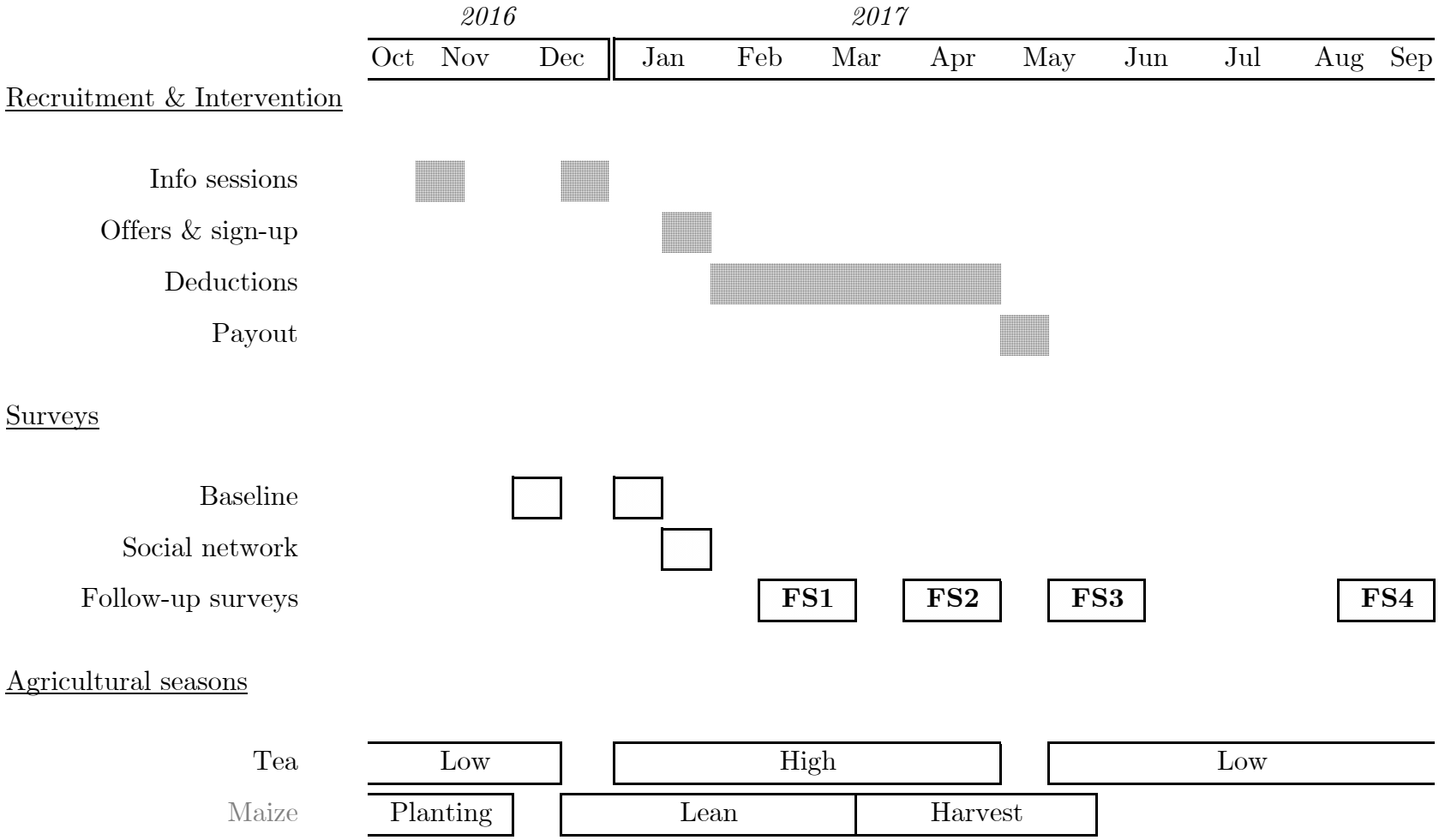
Figure 1

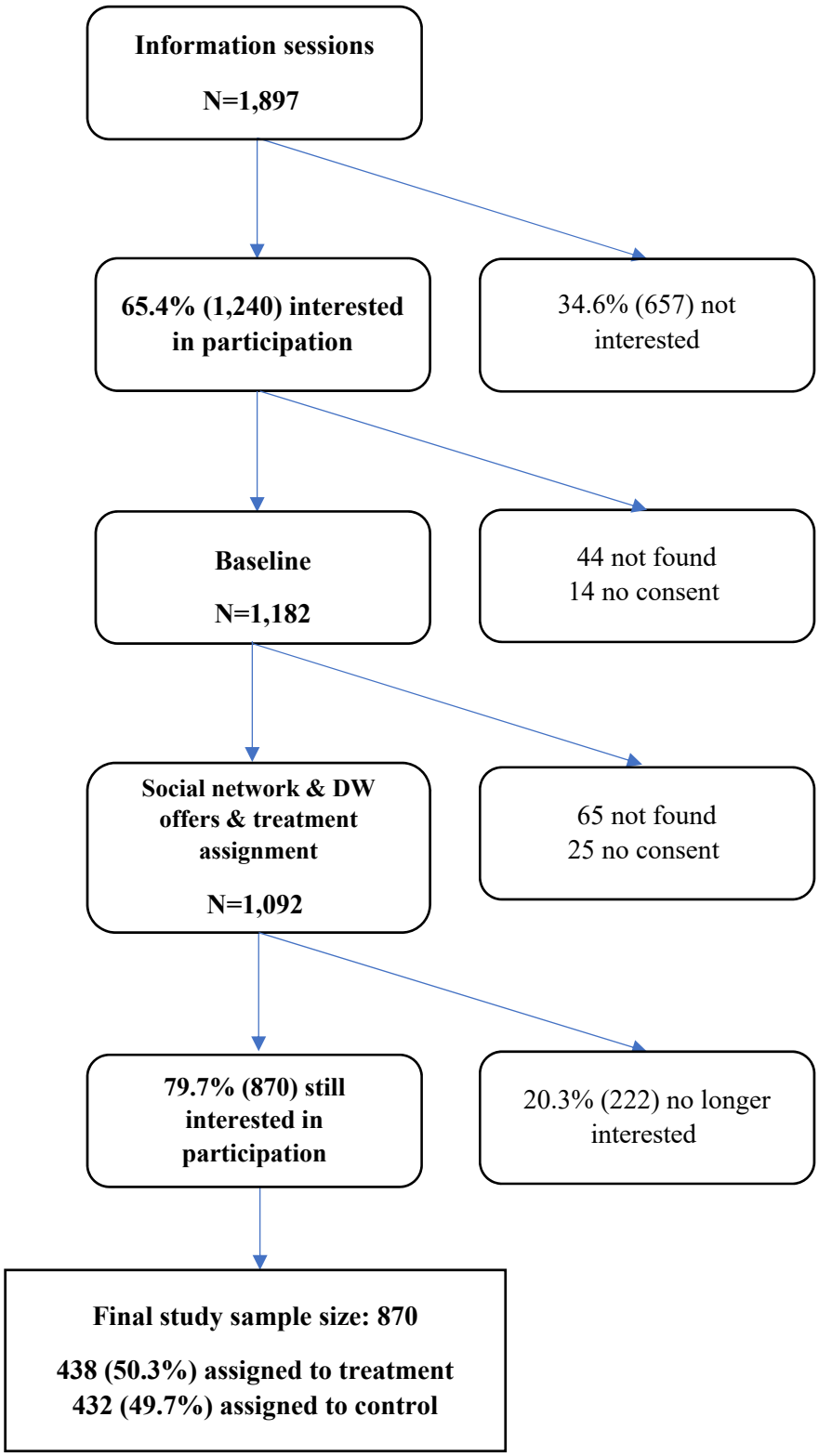
Timeline for Initial Intervention and Data Collection



Notes: Timing of the intervention, data collection, and agricultural seasons for the main experiment.

Figure 2

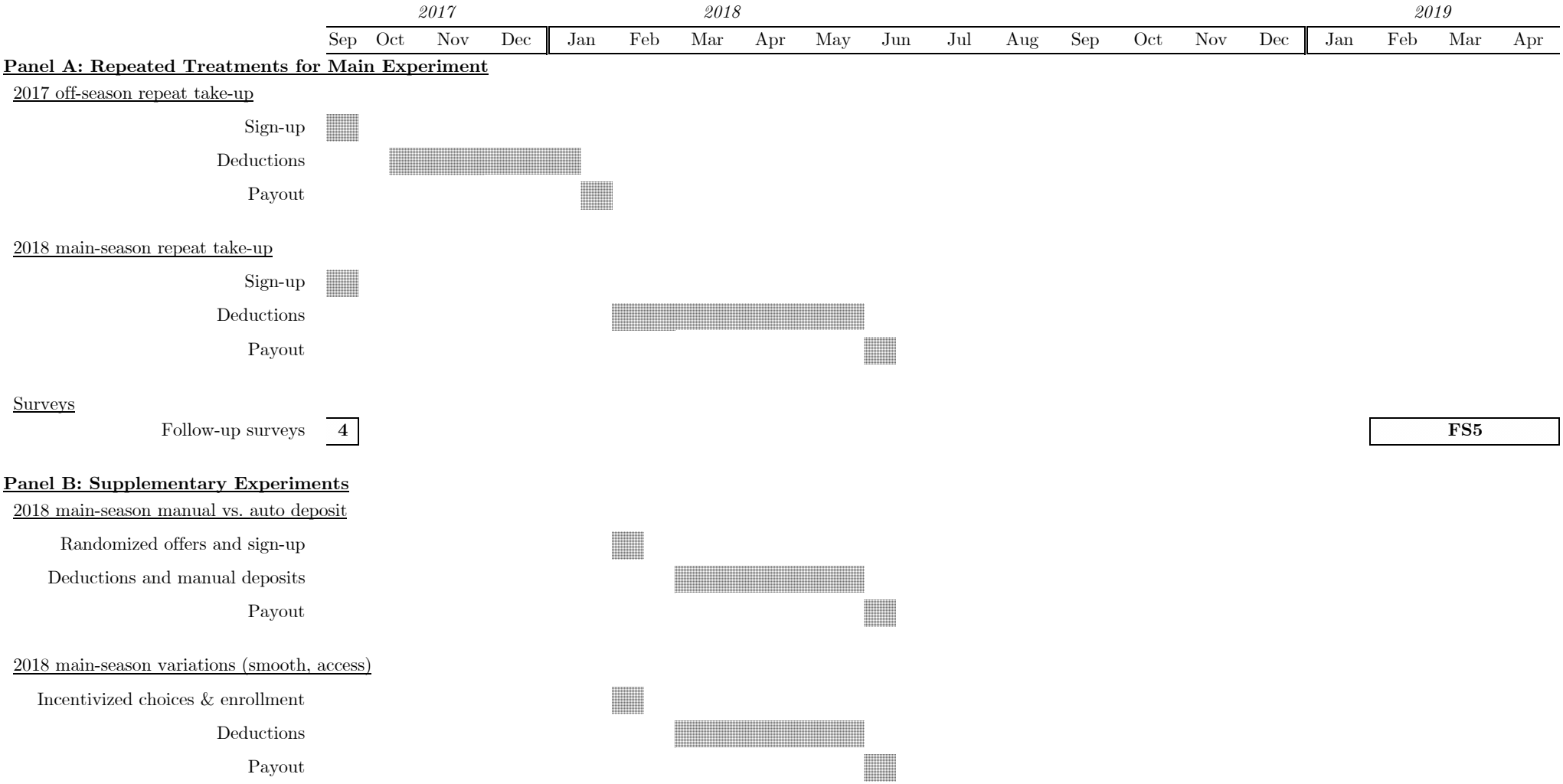
Study Sample Recruitment



Notes: Flow chart of sample selection showing the number of workers screened out of the sample at each step of the process.

Figure 3

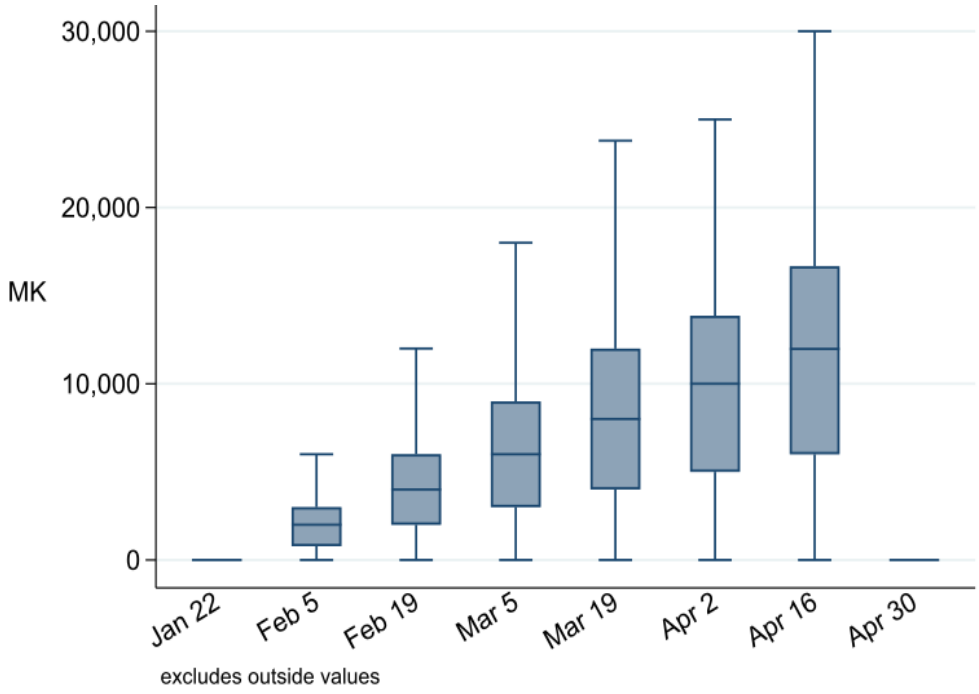
Timeline of Repeated Treatments and Supplementary Experiments



Notes: Timing of the intervention and data collection for the repeated treatments of our initial experimental sample (Panel A), and timing of the interventions for our supplemental experiments (Panel B).

Figure 4

Deferred Wages Balances Over Time



Notes: Figure shows a box plot of accumulated balances in deferred wages scheme at two-weekly paydays during the 2018 main season. Balances are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Deductions from incomes were made over the course of six paydays (Feb 5 through Apr 16) with payout on the seventh (Apr 30). The boxes show the locations and distance between first and third quartile. The whiskers show the data's closest values inside Tukey's fences.

Table 1

Savings Scheme Take-up and Utilization

	(1) Obs.	(2) Mean	(3) SD
Panel A: Take-up and Final Sample Selection			
Interested in scheme at information session (1/0)	1,897	0.654	0.476
Would enroll in product if offered (1/0)	1,092	0.779	0.415
Panel B: Treatment Group Utilization			
Minimum take-home pay [MK]	438	8,239	4,971
Maximum deduction [MK]	438	2,832	1,395
Average two-weekly income from firm [MK]	437	14,555	2,916
Average two-weekly deduction [MK]	437	2,056	1,387
Average share deductions/income	436	0.140	0.095
Savings balance before disbursement	437	12,079	8,517
Early exit from savings scheme	438	0.037	0.188
Panel C: Desired participation changes after two paydays			
Wants to drop out early right away	207	0.039	0.193
Wants to reduce contributions (incl. drop out)	207	0.097	0.296
Wants to increase contributions	207	0.145	0.353

Notes: Panel A provides statistics on expressed interest in participating in the savings scheme at the Lujeri Tea Estates. Panel B presents statistics on product choices for the individuals that we randomly assigned to the Treat group, which had access to the deferred wages savings accounts in 2017. Panel C covers a randomly-selected subset of 50% treatment workers interviewed who follow-up survey round 2 who were asked if they wanted to drop out early or change their thresholds and who were informed that their choice would be implemented with a 5% chance.

Table 2

Impacts on Savings Outcomes (February-April 2017)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Any deposits in past 14 days to:			Savings balances at end of deduction period [MK]					
	Savings scheme	Other Formal	Informal Financial	Total	Formal	Savings Scheme	Informal	Food storage	Informal financial
Source:	Admin	Surveys	Surveys	Mixed	Mixed	Admin	Surveys	Surveys	Surveys
Treatment	0.820***	-0.0156*	-0.0698***	7,113***	10,915***	11,272***	-3,595*	-2,247**	-1,292
S.E.	(0.0155)	(0.00854)	(0.0252)	(2,376)	(586.5)	(389.4)	(2,124)	(941.7)	(1,340)
Sample									
Pooled Follow-up 1 & 2	X	X	X						
Follow-up 2 only				X	X	X	X	X	X
Observations	1,651	1,651	1,651	810	810	810	810	810	810
Adjusted R-squared	0.699	0.0377	0.119	0.159	0.340	0.514	0.134	0.241	0.0598
Control-group Mean	0	0.0370	0.695	29,730	1,598	0	27,430	14,123	12,002

Notes: All measures of savings outcomes are recorded during the deductions period of the savings scheme. This period covered February to April 2017. Each measure analyzed is an aggregate or detailed measure of savings. FS1 and FS2 refer to follow-up surveys 1 and 2, respectively. Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Heteroskedatisticity-robust standard errors, clustered by worker, in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Table 3

Impacts on Work Outcomes

	(1) Daily output [kg]	(2) Daily output [kg]	(3) Income earned [MK]	(4) Income earned [MK]	(5) Savings deduction [MK]	(6) Savings deduction [MK]
Treatment	1.618**		222.0		2,001***	
S.E.	(0.752)		(155.8)		(65.98)	
Treatment x Plucker		1.791**		341.6**		2,002***
S.E.		(0.881)		(172.6)		(75.58)
Treatment x Non-Plucker		0.644		-156.8		2,003***
S.E.		(1.115)		(351.9)		(135.5)
Worker-day level	X	X				
Worker-pay-period level			X	X	X	X
Observations	64,908	64,908	5,214	5,214	5,214	5,214
Adjusted R-squared	0.428	0.437	0.218	0.218	0.425	0.430
Control-group Mean						
All	37.59		14,091		0	
Pluckers		48.86		14,079		0
Non-pluckers		2.796		14,131		0

Notes: All results are based on administrative data for workers at the Lujeri Tea Estates covering the months of the deductions period for the savings scheme (February to April 2017). Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Results in Column 5 are not identical to the average deductions in Table 1 because of the control variables in the regression. Heteroskedatisticity-robust standard errors, clustered by worker, in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Table 4

Impacts on Short-term Expenditures Following Payout

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Detailed expenditures [MK]						Bulk purchases		
	Total	Food	Storable	Maize grain	Durables	House improve-ments	Any purchase $>$ 5k [0/1]	Sum of purchases $>$ 5k [MK]	Any purchase $>$ 10k [0/1]
Panel A: Interviewed within 14 days of payout (recall period = number of days since payout)									
Treatment	5,728***	3,028***	2,670***	2,386***	2,233**	1,541***	0.130***	3,797***	0.136***
S.E.	(1,255)	(529.1)	(483.0)	(456.5)	(990.1)	(558.9)	(0.0502)	(1,118)	(0.0394)
Observations	342	342	342	342	342	342	342	342	342
Adjusted R-squared	0.107	0.171	0.131	0.0883	0.0283	0.0107	0.0271	0.0621	0.0320
Control-group Mean	16060	8437	6122	4041	5007	1483	0.310	4933	0.167
Panel B: Interviewed more than 14 days after payout (recall period fixed at 14 days)									
Treatment	-766.5	-32.46	391.0	368.6	-455.3	173.6	0.0227	-283.5	0.000
S.E.	(1,242)	(602.5)	(490.5)	(436.2)	(853.6)	(428.6)	(0.0447)	(998.9)	(0.0364)
Observations	446	446	446	446	446	446	446	446	446
Adjusted R-squared	0.0352	0.0290	0.0225	0.0161	0.0238	0.0384	-0.0204	0.0182	0.00735
Control-group Mean	17,598	9,408	6,596	4,257	5,592	1,183	0.309	5,168	0.175

Notes: All measures of expenditure outcomes are recorded in the period after the payout of the savings scheme (May 6). This data comes from the third follow-up survey (FS3). Each measure analyzed is an aggregate or detailed measure of a type of expenditure. Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Heteroskedatisticity-robust standard errors, clustered by worker, in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Table 5

Downstream Effects on Asset Ownership

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			Asset values [MK]					Has improved type of	
	Total durable assets and livestock (col's 2, 3, 4, 5, 7)	Household durables	Productive assets	Stored agricultural inputs	Stored building materials	Iron sheets (component of col 5)	Livestock	Floor (cement)	Roof (Iron sheets)
Panel A: Four Months After Initial Scheme Ended									
Treatment	11,326**	3,464	856.8***	1,102**	7,430***	4,559**	-1,758	0.0277	0.00917
	(5,703)	(3,279)	(284.3)	(546.1)	(2,397)	(1,866)	(1,756)	(0.0299)	(0.0320)
Observations	723	723	723	723	723	723	723	723	723
Adjusted R-squared	0.300	0.355	0.288	0.072	0.037	0.020	0.129	0.106	0.024
Control-group Mean	112,239	67,899	7,258	1,624	17,682	13,426	15,409	0.217	0.752
Panel B: Two Years After Initial Scheme (Ten Months After Repeated Schemes)									
Treatment	-2,779	-4,889	940.3	707.0	755.2	-250.0	683.6	-0.0125	0.0766***
	(7,548)	(5,591)	(684.7)	(587.4)	(2,539)	(1,947)	(2,244)	(0.0316)	(0.0284)
Observations	662	662	662	662	662	662	662	662	662
Adjusted R-squared	0.308	0.281	0.194	0.087	0.030	0.008	0.123	0.080	0.052
Control-group Mean	145,169	94,503	11,134	2,229	19,087	13,402	15,785	0.243	0.788

Notes: Assets measured four months (Panel A) or two years (Panel B) after the payout of the initial savings scheme, using survey data from FS4 and FS5 respectively. The treatment group was re-treated twice between the four-month and the two-year followup, and there was then a nine-month delay before data collection. Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Heteroskedatisticity-robust standard errors in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Table 6

Effect of Manual Deposit Requirement on Take-up and Contribution Rates

	(1)	(2)	(3)	(4)
	Interested at initial meeting [0/1]	Any deposit [0/1]	Number of deposits (out of 6)	Final Balance after 6 paydays [MK]
Panel A - Average Effects				
Manual Deposits	0.0904	-0.303***	-1.854***	-3,516**
	(0.0731)	(0.0666)	(0.353)	(1,477)
Observations	186	186	186	186
Adjusted R-squared	0.00284	0.0945	0.122	0.025
Control-Group Mean	0.505	0.505	2.753	6,930
Panel B - Heterogeneous Effects				
Manual Deposits (Low Self Control, High Kin Tax, No Account)	0.0203	-0.400**	-2.596***	-8,215**
	(0.183)	(0.171)	(0.924)	(3,768)
Manual Deposits ×				
Medium Self Control	-0.00265	-0.0800	0.0928	3,600
	(0.202)	(0.186)	(1.016)	(3,076)
High Self Control	0.160	0.191	1.588*	7,792***
	(0.175)	(0.161)	(0.813)	(2,927)
Medium Kin Tax	-0.245	-0.207	-0.764	-1,463
	(0.243)	(0.212)	(1.147)	(5,842)
Low Kin Tax	-0.00342	0.0998	0.254	-1,314
	(0.178)	(0.153)	(0.849)	(4,772)
Any Formal Savings Account	0.143	0.0541	0.271	5,337
	(0.152)	(0.135)	(0.701)	(3,255)
P-values of joint test of interactions with				
Self-control	0.581	0.239	0.096	0.031
Kin Tax	0.504	0.288	0.597	0.960
Observations	186	186	186	186
Adjusted R-squared	-0.0157	0.0842	0.113	0.0397
Control-Group Mean	0.505	0.505	2.753	6,930

Notes: This table reports the results of a supplementary experiment on a sample of 186 workers that were not part of the original savings scheme or involved in the main experiment, in which the treatment was having to make deposits manually instead of having them automatically deducted. Panel A presents the main effects of the treatment; Panel B presents treatment effect heterogeneity by baseline characteristics. Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Heteroskedatisticity-robust standard errors, clustered by worker, in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Appendix Table A1

Baseline Descriptive Statistics and Balance

	(1)	(2)	(3)	(4)	(5)
	Control		Treatment		p-value
	Mean	(SD)	Mean	(SD)	(1)=(3)
Panel A: Demographics					
Male	0.345	(0.476)	0.347	(0.477)	0.952
Married	1.706	(1.100)	1.658	(1.064)	0.524
Age	39.530	(11.203)	39.502	(10.328)	0.986
Years of schooling	4.845	(3.601)	4.735	(3.435)	0.599
Number of children in household	2.498	(1.514)	2.692	(1.461)	0.0553
Panel B: Work and Income					
Plucker	0.759	(0.428)	0.773	(0.419)	0.636
Share of Days Plucked Tea, admin.	0.415	(0.276)	0.430	(0.279)	0.448
Average daily Output [kg], admin.	10.532	(10.026)	11.111	(10.220)	0.338
Total housheold income past 14 days [MK], survey	18,668.109	(21,052.973)	19,424.674	(23,492.234)	0.651
Panel C: Financial Behaviors					
Total Savings [MK]	33,918.574	(54,154.285)	30,235.703	(47,801.289)	0.246
Formal Savings [MK]	1,552.593	(7,561.865)	1,070.591	(5,944.160)	0.270
Informal Savings [MK]	32,054.408	(50,509.195)	29,101.186	(45,615.660)	0.319
Asset Index (PCA)	0.000	(1.000)	-0.094	(1.018)	0.151
Total 14-day expenditures [MK]	16,737.141	(11,508.438)	16,162.648	(12,040.615)	0.415
Any Purchase $>$ 5k, past 30 days [0/1]	0.014	(0.117)	0.025	(0.157)	0.241
Observations	432		438		
P-value of joint test					0.384

Notes: Sample includes 870 permanent full-time employees who wanted to enroll in Pay Me Later at the social network survey. Treatment-control differences and p-values estimated by running equation 1 with the balance variable on the left-hand-side. The joint balance test is conducted by putting the treatment indicator on the left-hand side of equation 1 and adding all the balance variables to the right-hand side. Monetary values are in Malawi Kwacha; \$1 USD equalled approximately MK 750 at the time of the experiment. Heteroskedatisticity-robust standard errors, clustered by worker, in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Appendix Table A2

Predictors of Attrition by Treatment Status

	(1)	(2)	(3)	(4)
	Outcome: Attrited (=1)			
	Survey Round 4		Survey Round 5	
Treatment	-0.0327	-0.0324	-0.00655	-0.00568
	(0.0257)	(0.0256)	(0.0287)	(0.0287)
Male	0.0134	-0.0123	-0.00643	0.0433
	(0.0392)	(0.0660)	(0.0434)	(0.0694)
Married	0.0174	0.0286	0.0187	-0.0279
	(0.0173)	(0.0285)	(0.0189)	(0.0296)
Age	0.00184	0.000790	0.00114	0.00294
	(0.00138)	(0.00195)	(0.00152)	(0.00212)
Years of schooling	0.00578	0.00534	0.00232	0.00124
	(0.00446)	(0.00629)	(0.00506)	(0.00706)
Number of children in household	-0.00107	0.00615	-0.0161	-0.0189
	(0.00922)	(0.0141)	(0.0106)	(0.0149)
Plucker	-0.0212	-0.0827	0.0110	0.0948
	(0.0520)	(0.0748)	(0.0582)	(0.0785)
Share of Days Plucked Tea, admin.	0.00166	-0.0335	-0.225	-0.267
	(0.112)	(0.156)	(0.138)	(0.186)
Average daily Output [10 kg], admin.	-0.000528	0.00293	0.00504*	0.00516
	(0.00238)	(0.00327)	(0.00287)	(0.00386)
Total housheold income past 14 days [1000 MK], survey	0.00148	0.00269	-0.00629	-0.00157
	(0.00710)	(0.0107)	(0.00692)	(0.0101)
Formal Savings [1000 MK]	-0.0335**	-0.0243	0.00519	-0.0215
	(0.0140)	(0.0213)	(0.0250)	(0.0271)
Informal Savings [1000 MK]	0.00297	-0.00380	-0.00140	-0.000608
	(0.00361)	(0.00449)	(0.00359)	(0.00483)
Asset Index (PCA)	0.0171	0.00979	0.0387**	0.0572**
	(0.0150)	(0.0229)	(0.0179)	(0.0264)
Total 14-day expenditures [1000 MK]	0.00180	0.0115	0.0113	0.00555
	(0.0126)	(0.0199)	(0.0134)	(0.0195)
Any Purchase > 5k, past 30 days [0/1]	-0.154***	-0.147***	-0.111	-0.218***
	(0.0239)	(0.0505)	(0.0703)	(0.0492)
Treatment interacted with:				
Male		0.0335		-0.0679
		(0.0827)		(0.0879)
Married		-0.0160		0.0814**
		(0.0356)		(0.0381)
Age		0.00177		-0.00361
		(0.00275)		(0.00305)
Years of schooling		0.000409		0.00244
		(0.00889)		(0.00994)
Number of children in household		-0.0153		0.00210
		(0.0185)		(0.0214)
Plucker		0.102		-0.158
		(0.0998)		(0.115)
Share of Days Plucked Tea, admin.		0.100		0.113
		(0.216)		(0.253)
Average daily Output [10 kg], admin.		-0.00673		-0.00106
		(0.00437)		(0.00495)
Total housheold income past 14 days [1000 MK], survey		-0.00419		-0.00876
		(0.0145)		(0.0137)
Formal Savings [1000 MK]		-0.0237		0.0731
		(0.0282)		(0.0505)
Informal Savings [1000 MK]		0.0158**		-0.00134
		(0.00722)		(0.00726)
Asset Index (PCA)		0.0142		-0.0401
		(0.0305)		(0.0353)
Total 14-day expenditures [1000 MK]		-0.0207		0.0147
		(0.0243)		(0.0262)
Any Purchase > 5k, past 30 days [0/1]		0.0199		0.116
		(0.0625)		(0.111)
p-values for				
Treatment indicator	0.203	0.206	0.820	0.843
Interactions		0.612		0.219
Treatment and interactions		0.493		0.274
Observations	869	869	869	869
Adjusted R-squared	0.0202	0.0196	0.0407	0.0438
Control-group Mean	0.188	0.188	0.248	0.248

Notes: Sample includes 870 permanent full-time employees who wanted to enroll in Pay Me Later at the social network survey. Heteroskedatisticity-robust standard errors, clustered by worker, in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Appendix Table A3

Effects on Expenditures During Deduction Period — Pooled Across Follow-ups 1 and 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	14-day detailed expenditures [MK]								Bulk purchases in past 30 days		
	Total	Food	Perishable	Storable	Maize grain	Durables	House improve-ments	Non-food consum-ables	Any purchase $>$ 5k [0/1]	Sum of purchases $>$ 5k [MK]	Any purchase $>$ 10k [0/1]
Treatment	292.7	40.36	5.307	88.53	36.45	53.29	26.31	179.3	-0.0237	751.1	-0.00551
S.E.	(846.3)	(279.1)	(106.9)	(221.7)	(186.4)	(625.5)	(270.5)	(177.2)	(0.0255)	(787.4)	(0.0218)
Observations:											
from Follow-up 1	841	841	841	841	841	841	841	841	841	841	841
from Follow-up 2	810	810	810	810	810	810	810	810	810	810	810
total	1,651	1,651	1,651	1,651	1,651	1,651	1,651	1,651	1,651	1,651	1,651
Adjusted R-squared	0.0659	0.0583	0.0709	0.0371	0.0214	0.0441	0.0179	0.0309	0.0643	0.0537	0.0440
Control-Group Mean	18,938	9,157	2,745	6,347	3,930	7,286	1,662	2,317	0.504	7671	0.236

Notes: All measures of expenditure outcomes are recorded during the deductions period of the savings scheme. This period covered February to April 2017. Each measure analyzed is an aggregate or detailed measure of a type of expenditure. FS1 and FS2 refer to follow-up surveys 1 and 2, respectively. Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Heteroskedatisticity-robust standard errors, clustered by worker, in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Appendix Table A4

Flow of Funds After Lump Sum Disbursement

	(1)	(2)	(3)	(4)
	Total expenditures [MK]	Net savings deposits (excluding savings scheme) [MK]	Net money loaned [MK]	Net transfers made [MK]
Panel A: Interviewed within 14 days of payout (recall period = days since payout)				
Treatment	5,728***	255.9	711.1*	147.4
S.E.	(1,255)	(706.3)	(363.4)	(175.8)
Observations	342	342	342	342
Adjusted R-squared	0.107	0.0173	-0.0280	-0.00924
Control-group Mean	16060	1783	-304.5	9.474
Panel B: Interviewed more than 14 days after payout (recall period = 14 days, fixed)				
Treatment	-766.5	-144.5	628.9**	-103.3
S.E.	(1,242)	(695.6)	(303.6)	(163.2)
Observations	446	446	446	446
Adjusted R-squared	0.0352	0.00889	0.00464	-0.00976
Control-group Mean	17,598	737.6	-123.6	26.6

Notes: Lump sum payout of deferred wages occurred on May 6, 2017. The data for post-payout outcomes comes from the third follow-up survey (FS3). Panel A provides results for the sample of workers who we interviewed within the first 14 days of payout. Panel B. provides results for the sample of workers who we interviewed after 14 days. We randomized the interview date for all workers in the sample. Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Heteroskedatisticity-robust standard errors in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Appendix Table A5

Repeat Take-up in Treatment Group

	(1) Obs.	(2) Mean	(3) SD
Repeat take-up of Savings Scheme			
Off-season 2017	372	0.812	0.153
Main season 2018	372	0.780	0.172
Threshold choices among off-season 2017 repeat takers			
Minimum take-home pay [MK]	302	7,559	4,855
Maximum deduction [MK]	302	3,476	1,796
Threshold choices among main-season 2018 repeat takers			
Minimum take-home pay [MK]	290	8,453	5,565
Maximum deduction [MK]	290	4,195	2,156

Notes: This table reports repeat take-up and savings choice statistics for the original treatment group of workers in the 2017 off-season and 2018 main agricultural season. Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Sample is 372 treatment-group workers who were found for the fourth followup survey.

Appendix Table A6

Downstream Effects on Asset Ownership — Full List of Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	PCA Indices								
	Assets and livestock								
	All, number owned	All, number purchased since baseline	Stored building materials, number owned	Improvements to house, any	Bought any asset	Made any improvements to house	Wall material improved since baseline	Started new house	Iron sheet roof
Panel A: Four Months After Initial Scheme Ended									
Treatment	0.164***	-0.0110	0.384***	0.0414	0.0492	0.0332	0.008	-0.001	0.009
S.E.	(0.0621)	(0.0734)	(0.100)	(0.0771)	(0.0363)	(0.0319)	(0.0171)	(0.0136)	(0.0320)
p-value (naïve)	0.009	0.881	0.000	0.592	0.175	0.299	0.631	0.970	0.775
FWER-corrected p-value	0.065	0.990	0.001	0.988	0.699	0.853	0.988	0.990	0.990
Observations	723	723	723	723	723	723	723	723	723
Adjusted R-squared	0.386	0.0605	0.0414	0.0279	0.0428	0.00992	0.0114	-0.0157	0.0235
Control-Group Mean	0.00	0.00	0.00	0.00	0.550	0.219	0.0513	0.0313	0.752
Control-Group SD	1.00	1.00	1.00	1.00	0.498	0.414	0.221	0.174	0.432
Panel B: Two Years After Initial Scheme, Nine Months After Repeated Schemes – ITT Estimates									
Treatment	0.0637	0.0662	0.0943	0.132	0.0225	0.0381	0.00983	0.0319	0.0766***
S.E.	(0.0610)	(0.0784)	(0.0759)	(0.0836)	(0.0274)	(0.0382)	(0.0218)	(0.0230)	(0.0284)
p-value (naïve)	0.297	0.398	0.215	0.115	0.412	0.319	0.652	0.166	0.007
FWER-corrected p-value	0.761	0.761	0.758	0.515	0.761	0.761	0.761	0.645	0.032
Observations	659	661	661	662	662	662	662	662	662
Adjusted R-squared	0.404	0.109	0.0811	0.000929	0.0301	0.00869	0.00816	0.000726	0.0518
Control-group Mean	0.00	0.00	0.00	0.00	0.843	0.369	0.0831	0.0800	0.788
Control-group SD	1.00	1.00	1.00	1.00	0.364	0.483	0.276	0.272	0.410

Notes: Assets measured four months (Panel A) or two years (Panel B) after the payout of the initial savings scheme, using survey data from FS-4 and FS-5 respectively. The treatment group was re-treated twice between the four-month and the two-year followup, and there was then a nine-month delay before data collection. Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Heteroskedatisticity-robust standard errors, clustered by worker, in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Appendix Table A7

Downstream Effects on Asset Ownership — Adjusting for Potential Spillovers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			Asset values [MK]					Has improved type of	
	Total durable assets and livestock (col's 2, 3, 4, 5, 7)	Household durables	Productive assets	Stored agricultural inputs	Stored building materials	Iron sheets (component of col 5)	Livestock	Floor (cement)	Roof (Iron sheets)
Panel A: Four Months After Initial Scheme Ended									
Treatment	11,308**	3,509	857.4***	1,096**	7,382***	4,520**	-1,760	0.0276	0.00926
	(5,700)	(3,282)	(284.8)	(540.7)	(2,383)	(1,857)	(1,758)	(0.0300)	(0.0320)
# peers	1,870	-171.2	-33.39	364.9**	642.0	492.8	-199.1	-0.00410	-0.00677
	(2,102)	(920.1)	(81.64)	(184.2)	(738.8)	(564.1)	(538.0)	(0.00858)	(0.00843)
# peers in treatment	-1,497	-872.5	19.17	-238.1	463.6	406.0	238.1	0.00756	0.00495
	(2,572)	(1,343)	(119.6)	(221.9)	(988.8)	(794.4)	(810.2)	(0.0128)	(0.0126)
Observations	723	723	723	723	723	723	723	723	723
Adjusted R-squared	0.300	0.356	0.286	0.0831	0.0433	0.0278	0.127	0.104	0.0218
Control-Group Mean	112,239	67,899	7,258	1,624	17,682	13,426	15,409	0.217	0.752
Panel B: Two Years After Initial Scheme, Nine Months After Repeated Schemes									
Treatment	-2,789	-4,779	918.9	708.3	691.1	-291.3	655.7	-0.0131	0.0757***
	(7,573)	(5,619)	(685.6)	(580.8)	(2,540)	(1,950)	(2,258)	(0.0317)	(0.0284)
# peers	3,291	1,485	221.4	656.5***	522.3	224.5	195.6	-0.00437	-0.00315
	(2,281)	(1,423)	(221.1)	(158.9)	(773.3)	(583.7)	(755.5)	(0.00947)	(0.00818)
# peers in treatment	-3,713	-2,891	-28.75	-775.4***	77.96	179.5	70.56	0.0119	0.0131
	(3,189)	(2,204)	(286.3)	(234.2)	(1,040)	(827.5)	(1,129)	(0.0141)	(0.0115)
Observations	662	662	662	662	662	662	662	662	662
Adjusted R-squared	0.309	0.280	0.196	0.108	0.0301	0.00671	0.121	0.0783	0.0519
Control-Group Mean	145,169	94,503	11,134	2,229	19,087	13,402	15,785	0.243	0.788

Notes: Assets measured four months (Panel A) or two years (Panel B) after the payout of the initial savings scheme, using survey data from FS-4 and FS-5 respectively. The treatment group was re-treated twice between the four-month and the two-year followup, and there was then a nine-month delay before data collection. Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Heteroskedatisticity-robust standard errors, clustered by worker, in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Appendix Table A8

Downstream Effects on Asset Ownership — Full List of Outcomes, Adjusting for Potential Spillovers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	PCA Indices								
	Assets and livestock								
	All, number owned	All, number purchased since baseline	Stored building materials, number owned	Improvements to house, any	Bought any asset	Made any improvements to house	Wall material improved since baseline	Started new house	Iron sheet roof
Panel A: Four Months After Initial Scheme Ended									
Treatment	0.164***	-0.0120	0.383***	0.0411	0.0487	0.0334	0.00832	-0.000419	0.00926
S.E.	(0.0622)	(0.0732)	(0.100)	(0.0770)	(0.0362)	(0.0319)	(0.0171)	(0.0136)	(0.0320)
p-value (naïve)	0.00855	0.870	0.000139	0.594	0.179	0.295	0.627	0.975	0.773
FWER-corrected p-valu	0.068	0.989	0.001	0.981	0.712	0.848	0.981	0.989	0.989
# peers	0.000351	0.0152	0.0250	0.0349	-0.00404	0.0180*	-0.00220	2.89e-05	-0.00677
	(0.0189)	(0.0195)	(0.0342)	(0.0216)	(0.0106)	(0.00924)	(0.00469)	(0.00409)	(0.00843)
# peers in treatment	-0.00463	0.00811	0.00167	-0.0293	0.0168	-0.0242*	-7.22e-05	-0.00227	0.00495
	(0.0268)	(0.0299)	(0.0429)	(0.0302)	(0.0160)	(0.0133)	(0.00726)	(0.00617)	(0.0126)
Observations	723	723	723	723	723	723	723	723	723
Adjusted R-squared	0.385	0.0627	0.0432	0.0301	0.0432	0.0127	0.00966	-0.0178	0.0218
Control-Group Mean	0.00	0.00	0.00	0.00	0.550	0.219	0.0513	0.0313	0.752
Control-Group SD	1.00	1.00	1.00	1.00	0.498	0.414	0.221	0.174	0.432
Panel B: Two Years After Initial Scheme, Nine Months After Repeated Schemes – ITT Estimates									
Treatment	0.0630	0.0643	0.0923	0.131	0.0211	0.0376	0.00981	0.0317	0.0757***
S.E.	(0.0613)	(0.0784)	(0.0757)	(0.0836)	(0.0272)	(0.0383)	(0.0218)	(0.0231)	(0.0284)
p-value (naïve)	0.304	0.413	0.223	0.118	0.438	0.326	0.653	0.170	0.00786
FWER-corrected p-valu	0.750	0.750	0.750	0.514	0.750	0.750	0.750	0.664	0.037
# peers	0.0197	0.00840	0.0286	0.00338	-0.0144**	0.00201	-0.00425	0.00642	-0.00315
	(0.0187)	(0.0261)	(0.0226)	(0.0243)	(0.00706)	(0.0108)	(0.00601)	(0.00729)	(0.00818)
# peers in treatment	-0.0125	0.0111	-0.0137	0.00809	0.0313***	0.00239	0.00518	-0.00504	0.0131
	(0.0257)	(0.0326)	(0.0291)	(0.0392)	(0.0104)	(0.0162)	(0.00985)	(0.0102)	(0.0115)
Observations	659	661	661	662	662	662	662	662	662
Adjusted R-squared	0.404	0.109	0.0833	-0.00158	0.0393	0.00610	0.00573	-0.000418	0.0519
Control-group Mean	0.00	0.00	0.00	0.00	0.843	0.369	0.0831	0.0800	0.788
Control-group SD	1.00	1.00	1.00	1.00	0.364	0.483	0.276	0.272	0.410

Notes: Assets measured four months (Panel A) or two years (Panel B) after the payout of the initial savings scheme, using survey data from FS-4 and FS-5 respectively. The treatment group was re-treated twice between the four-month and the two-year followup, and there was then a nine-month delay before data collection. Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Heteroskedatisticity-robust standard errors, clustered by worker, in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Appendix Table A9

Determinants of Take-up and Utilization of the Savings Scheme

	(1)	(2)	(3)
	Enrolled [0/1]	Number of deposits (out of 6)	Final Balance after 6 paydays [MK]
Demographics			
Female	-0.0666	-0.490	359.7
	(0.164)	(0.919)	(2,981)
Age [years]	0.00402	0.0181	67.58
	(0.00701)	(0.0394)	(115.0)
Marital status			
Single	-0.0320	-0.423	-356.8
	(0.171)	(0.906)	(3,007)
Divorced	-0.0373	-0.359	-2,281
	(0.167)	(0.920)	(2,843)
Widowed	0.314	2.531*	1,627
	(0.260)	(1.469)	(4,102)
Education [years]	0.0155	0.0517	334.4
	(0.0245)	(0.142)	(361.5)
Can read letter in local lang [=1	-0.0735	-0.0985	582.9
	(0.189)	(1.075)	(3,272)
Tenture at tea firm [years]	0.0107	0.0680	131.5
	(0.0106)	(0.0606)	(177.7)
Self control			
Medium	0.0954	0.861	1,520
	(0.161)	(0.909)	(2,390)
Low	0.170	1.340**	4,890**
	(0.116)	(0.642)	(2,015)
Kin ship tax			
Low	0.0907	0.158	-358.9
	(0.153)	(0.885)	(2,722)
Medium	0.0123	0.0293	-1,107
	(0.116)	(0.659)	(1,997)
Any formal account	0.0926	0.257	-1,322
	(0.117)	(0.653)	(1,808)
Participation in savings groups			
This year at work	-0.171	-0.841	-2,403
	(0.118)	(0.675)	(1,981)
This year in village	-0.430***	-2.063**	-5,995**
	(0.156)	(0.875)	(2,310)
Last year at work	0.118	0.504	793.2
	(0.134)	(0.760)	(2,290)
Last year in village	0.282**	1.306*	1,959
	(0.131)	(0.765)	(2,146)
P-values of joint tests of variable groups			
Demographics	0.615	0.278	0.726
Self-control	0.345	0.119	0.056
Kinship Tax	0.829	0.984	0.844
Savings groups	0.035	0.111	0.091
Observations	97	97	97
Adjusted R-squared	0.005	0.005	0.016
Mean of outcome variable	0.505	2.753	6,930

Notes: This table reports take-up regressions for a sample of workers that were not part of the original savings scheme or involved in the main experiment, but were offered the original version of the product during the manual deposits experiment. These workers made choices to participate in a version of the savings scheme offered during the 2018 main agricultural season. Monetary values are in Malawi Kwacha; $1 USD equalled approximately MK 750 at the time of the experiment. Heteroskedatisticity-robust standard errors, clustered by worker, in parentheses: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Appendix Table A10

Take-up of Scheme Variations

Take-up of scheme variations for 2018 main season (stocastically incentivized choice)	(1) Mean	(2) Obs.	(3) Test of difference from Original (p-values)
(1) Original: lump sum payout, no regular access to funds	0.559	177	
(2) Smooth payout	0.362	174	0.000
(3) More access	0.518	191	0.432

Notes: This table reports take-up statistics for a sample of workers that were not part of the original savings scheme. These workers made choices to participate in a version of the savings scheme offered during the 2018 main agricultural season. Modification 1 refers to an offer to participate in a version of the savings scheme where payout would occur as six separate payouts in two-week intervals at the end of the deductions period. Modification 2 refers to an offer to participate in a version of the savings scheme where workers could withdraw accumulated funds at any point during the deductions period.