Seasonal liquidity, rural labor markets and agricultural production: Evidence from Zambia*

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PRELIMINARY AND INCOMPLETE

Abstract

A majority of rural households in low and middle income countries continue to rely on small-scale agriculture as their primary source of income. In the absence of improved technologies, low harvest profits have to satisfy consumption and input needs until the subsequent harvest. We develop a model to show that, in this setting, frictions in capital market access not only undermine households' ability to smooth consumption over the cropping cycle, but also distort labor markets by encouraging liquidity-constrained farmers to sell family labor rather than using it on their own land. To identify the impact of intra-season credit availability on labor allocation and agricultural production, we conducted a two-year randomized controlled trial with small-scale farmers in rural Zambia. We show that lowering the cost of borrowing at the time of the year when farmers are most constrained (the lean season) results in increased wages and a reallocation of labor within communities from better-off to worse-off farms. This reallocation reduces differences in the marginal product of labor across farms, increasing agricultural output, on average, reducing consumption and income inequality.

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1 Introduction

A majority of rural households in developing countries continue to rely on small-scale farming as their primary source of income, and to rely on labor as the primary input to agricultural production process. In the absence of irrigation and advanced farming technologies, agricultural incomes tend to be low and have to cover both consumption and input needs until the subsequent harvest. With frictionless capital markets, the extent to which households smooth consumption over the cropping cycle depends primarily on subjective discount rates and any seasonal variation in the price of consumption.[1] The same is not true in the face of capital market frictions that drive a wedge between saving and borrowing rates. Low returns to saving and a high cost of borrowing exacerbates consumption seasonality, and distorts local labor markets by incentivizing farms facing high borrowing costs to sell family labor to finance consumption when liquid resources are most scarce (the "hungry season" in the months leading up to harvest). We formalize this intuition in a simple two-period agricultural household model. We show that, in a setting where rational farmers choose labor inputs during the hungry season, credit market frictions increase local labor supply by the poorest farmers, who are least able to consume out of savings. This, in turn, drives down local wages, and leads to higher labor inputs by farms facing lower borrowing costs. Reducing capital market frictions results in a more efficient allocation of labor across farms, higher wages, more agricultural output, on average, and a reduction in within-community inequality in consumption and income.

Our empirical setting is broadly representative of many parts of sub-Saharan Africa, and drives many of our modeling choices. Agriculture is rain-fed, resulting in a single harvest each year. Access to formal saving opportunities is limited, and alternatives such as grain storage or livestock holdings are risky. Access to formal credit is reported by less than 5 percent of our sample, while informal credit carries a reported average monthly interest rate of 40 percent. These frictions in the capital market are reflected in savings and consumption patterns in our sample: both cash and food reserves and consumption are highly seasonal, peaking after harvest, and reaching a minimum level the hungry season. When asked how they will cover short-term needs (in addition to restricting consumption as the name "hungry season" suggests), a majority of households say they will sell family labor in local labor markets. These labor sales – locally referred to as *ganyu* – typically occur within a given village, with better-off farmers hiring additional labor inputs from relatively poorer farmers in their communities at an individually negotiated rate. While these labor flows could be output-maximizing in principle, this will – as highlighted in our model – not be the case

[1] A substantial literature documents seasonality in grain prices and consumption (see, for example, Kaminski et al. (2014); Devereux et al. (2013)). Note that grain prices will exhibit seasonality even where capital markets function well, given storage costs and interest rates, though Gilbert et al. (2017) and Kaminski et al. (2014) argue that the seasonal fluctuations observed in Africa exceed the level explained just by interest rates. Whether predictable intra-annual fluctuations in grain prices translate into seasonal fluctuations in consumption will depend on the cost to households of accessing capital markets and other coping strategies.

if capital market frictions distort households' labor allocation and result in differential marginal product of labor across farms.[2] When we estimate the marginal product of hungry season labor across quartiles of grain and cash savings at baseline, we find the marginal product of labor is highest on the poorest and lowest on farms with relatively larger reserves.

To test the causal effect of credit market frictions during the hungry season on labor market outcomes and agricultural production, we conducted a 2-year randomized controlled trial with 3,139 small-scale farmers across 175 rural villages in Zambia's Eastern province. As part of the trial, villages were randomly selected for subsidized seasonal loans. The loans provided farmers with cash or food during the hungry season with repayment due after harvest. In the first year of the study, households in two-thirds of communities were eligible for loans; in the second year, 50 percent of communities received the program, with rotation of treatment status between years (i.e., some communities received two years of the program, some one year and some zero years). Despite an implicit interest rate of 30 percent over a six-month period, more than 98 percent of eligible farmers took up the offer. Close to 95 percent of loans were repaid in the first year, and 98 percent of farmers offered the same loan program in the second year signed up for the loan program again, highlighting both the high demand for hungry season credit and the high cost of alternative financing options.

Consistent with our theoretical framework, we find that loan access affects household labor allocation decisions. The likelihood that a family sold *ganyu* during the hungry season fell by 2.9 percentage points (10 percent) in response to treatment, and the likelihood of hiring increased by around 2 percentage points (18 percent). The pattern is similar for a continuous measure of hours hired or sold, with a decrease in ganyu hours of around 24 percent. When we stratify households by their baseline cash and grain reserves (what we refer to as their capital endowment), we find that the labor sales (ganyu) response to the loan program is concentrated among the least well-off farmers, while the increase in hiring is concentrated among the relatively best-off farmers. Total hours of labor input on-farm during the lean season increases at all levels of capital endowments.

As predicted by our model, these labor market adjustments drive up local wages. Average wages, which we measure as daily earnings, increase by about 1.2 Kwacha, or about 9 percent relative to average daily earnings in the control group. Since our intervention targeted half of farmers in each village for the loans, on average, these wage impacts have to be interpreted as partial adjustments, with larger effects likely if loan access was provided to all farmers in these communities. We also document increases in average agricultural output among treated farmers, with an estimated 8 percent increase in average harvest value; as expected, these gains are larger among farmers with

[2]Our model assumes land endowments and productivity are the same across farms. More generally, if all farmers optimally choose labor inputs such that the marginal product of labor is equal to the marginal cost of capital, this would only be the case if the marginal cost of capital of poorer households would be lower than the cost faced by better endowed farms. Given that that starting resources or capital endowments define both households' financing needs and their credit worthiness, this seems unlikely theoretically; empirically, the relationship between self-reported interest rates on borrowing and capital endowments is negative.

relatively small capital reserves. We also find substantial increases in hungry season consumption and reductions in consumption seasonality, in response both to the lower borrowing cost (substitution effect) and higher expected income. These effects are largest among the poorest households in our sample lowering overall inequality in consumption and output as predicted by the model.

We find little evidence that the impact of the loan is driven by increasing households' ability to smooth unanticipated shocks. Instead, the effects are concentrated among farmers who anticipate running short of food at the beginning of the farming seasonal To test whether the anticipation of cash shortages also affects decisions at planting (such as plot size and crop mix, which we abstract from in the model), we informed a subset of farmers about the loans at the beginning of the agricultural season in the second year of the program. While our power to detect differential impacts in this subset is limited, we find that farmers who were informed early about the loans show larger treatment effects on the value of agricultural output than do farmers who were informed at the start of the hungry season. This difference appears to be driven both by increased capital inputs (fertilizer, seeds, etc.) and the allocation of additional land to cash crops, as well as more pronounced shifts in labor allocation compared to farmers notified of the program at the start of the hungry season. These results suggest that larger and less restricted credit availability might generate larger increases in agricultural production than the intervention analyzed here.

Our paper is closely related to an extensive literature highlighting the links between credit market frictions, agricultural labor markets, and aggregate output.[3] Our theoretical model builds on Jayachandran (2006), who shows that lack of credit access leads to increased labor supply and lower wages among landless rural laborers when the economy is exposed to aggregate productivity shocks. On the extensive margin, Bandiera et al. (2017) show that high borrowing costs prevent poor women in Bangladesh from accessing the labor market opportunities of their richer neighbors, which effectively keeps them poor. More related to our study, Pitt and Khandker (2002) show a link between seasonal hunger, demand for microcredit and male labor supply in Bangladesh. Our model differs from this literature by explicitly focusing on households' trade-off between labor inputs on their own land and immediate revenue obtainable through labor sales on other farms. The critical role of family labor sales for smoothing consumption has been well documented (Kochar 1995, 1999; Rose 2001; Ito and Kurosaki 2009). We extend this literature in two ways: first, we show that family labor sales are not only important in the presence of unanticipated shocks, but also to cover anticipated liquidity shortages. [4] Second, we show that these labor sales result in sub-optimal labor inputs on farms with low capital endowments as well as an inefficient allocation of labor across farms. As we demonstrate in this paper, lowering frictions in the capital market results in a reallocation of

[3]More broadly, disruptions in financial markets have been a major threat for promoting economic growth historically (Reinhart and Rogoff 2014), and continue, as the recent U.S. history shows, to have severe repercussions on all sectors of the economy, including labor markets (Elsby et al. 2010).

[4]This consumption smoothing role of local labor markets is also tied to the substantial literature on informal smoothing strategies (see, for example, Morduch (1995) for a review), some of which – like labor sales – may carry long run costs (e.g., Rosenzweig and Wolpin (1993)).

labor within communities and increased output, and also reduces within community inequality in consumption and income.

Recent evidence on the impacts of capital access on agricultural productivity is mixed.[5] In Ghana, Karlan et al. (2014) find no evidence that liquidity constraints impede agricultural investments. On the other hand, Beaman et al. (2014) find that relaxing credit constraints through grants increases agricultural investment and yields among rice farmers in Mali, but that the same is not true for loans. These studies focus on capital inputs (seeds, fertilizer or pesticides) as the primary mechanism through which credit impacts yields; we show that labor inputs can be highly sensitive to capital access, with impacts on production. In addition to affecting the quantity of labor applied to the family farm, we provide suggestive evidence that credit access during the hungry season increases agricultural incomes via adjustments in ex ante production plans, and potentially also through higher quality of family labor inputs due to higher consumption (Pitt and Rosenzweig 1986; Strauss 1986; Behrman et al. 1997; Schofield 2013). This important role for capital markets in smoothing the highly seasonal income and capital needs of an agricultural economy was an important part of the history of economic development in the United States (e.g., Redenius and Weiman 2011), but has largely been overlooked in studies of credit access in developing countries.

Finally, we join a growing number of papers that revive research on seasonal agricultural incomes and income and consumption smoothing strategies.[6] Most related is a small literature investigating the impact of seasonal transfer or loan programs. Bergquist et al. (2014) offered farmers in Kenya a loan product that allowed them to exploit seasonal variation in maize prices and find significant effects on total maize revenues and household expenditures. Relatedly, Aggarwal et al. (2017) subsidize better storage technologies to provide similar price arbitrage opportunities. Bryan, Chowdhury and Mobarak (2014) show that providing credit and grants leads to large increases in seasonal labor migration in Bangladesh, arguing that credit market failures and highly uncertain returns likely keep long-distance labor supply below optimal levels. More similar to our study, Basu and Wong (2015) evaluate a seasonal food credit and improved storage program in Indonesia; similar to the results presented here, they find that food loans increase non-staple food consumption during the hungry season and income from crop sales at harvest, but do not analyze impacts on labor allocation or yields. Our findings contribute to that literature by providing the first direct evidence that capital market interventions timed to coincide with the hungry season not only affect consumption and output, but also affect agricultural labor market outcomes and the distribution of labor within

[5] An earlier literature uses observables to define whether households are credit constrained, and compares productivity and consumption across constrained and unconstrained households (e.g. Feder et al. 1990).

[6] The relationship between income seasonality and consumption smoothing is a subject of some debate. While some studies suggest that precautionary savings are sufficient to smooth consumption even if income is highly seasonal (Paxson 1993; Chaudhuri and Paxson 2002; Jacoby and Skoufias 1998), others have highlighted the pronounced consumption differences over the year as hard to reconcile with optimal smoothing (Dercon and Krishnan 2000; Khandker 2012). We take consumption seasonality as given and are agnostic as to its origins, though our findings show that high borrowing costs contribute to seasonal fluctuations in consumption.

communities.

From a policy perspective, seasonal loans are attractive due to the potentially large welfare gains generated from higher consumption at the time of year when consumption is at its lowest, by higher average agricultural output, and by within-community redistribution of resources from better-off to worse-off households. Despite this, seasonal lending may not be viable unless coupled with other interventions that lower the substantial transaction costs involved involved in operation, some of which may lie at the root of high informal interest rates in rural settings. Mobile-based savings and borrowing platforms, or interventions that piggyback on existing rural networks may offer scaling opportunities for seasonal credit access. Other strategies for decreasing the cost of consumption smoothing, such as more secure savings, may also decrease reliance on family labor for consumption smoothing, and improve agricultural production.

The paper proceeds as follows. In the next section, we present a simple model that highlights the linkages between capital markets, labor allocation and agricultural output, and generates testable predictions for our empirical analysis. Section 3 provides information on the local context as well as the experimental design. We present the data and descriptive statistics in section 4 and the experimental results in section 5. Section 6 shows robustness checks and further explores alternative mechanisms and explanations. We conclude with a short discussion and summary in section 7.

2 An agrarian economy with capital market frictions

We study a rural agricultural economy, in which farming households maximize utility over consumption and leisure, and have access to local labor and capital markets. The objectives of the model are to 1) derive the relationship between household resource endowments (cash and grain reserves), capital market frictions and local labor market outcomes, and 2) generate predictions regarding household and village-level responses to exogenous shifts in credit availability.

2.1 Setup

Our theoretical framework builds on the agrarian labor market model introduced in Jayachandran (2006).[7] Each village economy has a finite number N of households that maximize utility over two periods ($t = 1, 2$). Each household i has an initial capital endowment S_{i0}, distributed within the local economy according to some generic distribution $f(S_{i0})$.[8] All households have the same

[7] We modify Jayachandran's model in two important ways to more closely match the local setup: first, we assume that all farmers own land and can thus create income both from their own farms and from selling labor to others. Second, we assume that farming income is earned in the second period to highlight the trade-off between financing hungry season consumption and receiving greater output in the future.

[8] We assume that the initial distribution of endowments in exogenous. Empirically, this initial distribution could also be interpreted as the result of a stochastic process where all farms start with an initial endowment of zero, and accumulate a limited amount of resources over time. The main implication of this exogeneity assumption is that the distribution of income is independent of productivity and output. Allowing for heterogeneous productivity levels or

endowment of land, k, and time, $\bar{h}$, which they allocate between labor, h_i, and leisure, l_i, in period 1 (the farming period). In the second period (the harvest period), they consume their harvest production net of outstanding debt. Production y is Cobb-Douglas in labor d_i and land k, and proportional to aggregate productivity A:

$$y(d_i, k) = A d_i^{\beta} k^{1-\beta}. \tag{1}$$

Total on-farm labor inputs d_i include both own (family) labor on farm and labor hired. $\beta \epsilon (0,1)$ reflects the marginal product of on-farm labor.

Households have Stone-Geary preferences over consumption and leisure. Period-specific utility is given by

$$u(c_{it}, l_i) = log(c_{it} - \underline{c}) + \frac{1-\alpha}{\alpha} log(l_i), \tag{2}$$

where $\alpha \in (0,1)$, and $\underline{c} > 0$ is the minimum (subsistence) level all households must consume. Utility is additive and separable across the two periods; second period utility is discounted at a subjective discount factor $\rho < 1$.

Households can save at a rate r^s and borrow at a rate r^b; due to frictions in the local capital market we assume that $r^b > r^s$.[9] We assume that all farmers have access to the same saving technology, but that local borrowing rates increase with the amount of resources borrowed in a given period.[10] All borrowing needs to be repaid by the end of the second period. The labor market clears at the endogenous wage w such that average farm labor input equals average labor supply:

$$\sum_{i=1}^{N} d_i(w) = \sum_{i=1}^{N} (\bar{h} - l_i(w)). \tag{3}$$

2.2 Household utility maximization

Rational households maximize their utility over consumption and leisure over two periods:

$$\max_{c,l} \; log(c_{i1} - \underline{c}) + \frac{1-\alpha}{\alpha} log(l_{i1}) + \rho log(c_{i2} - \underline{c}) \tag{4}$$

land endowments would complicate the model without changing the main insights.

[9] We discuss these frictions in greater detail in Section 4. For example, transaction costs in the formal credit market include high transport costs, the absence of legal infrastructure, and a general lack of assets to be used as collateral. Informal saving options include the storage of grain and the purchase of livestock, both of which are subject to substantial risk, including fire, theft and and pests or disease. Alternative borrowing strategies such as the temporary sale of assets or livestock are also costly because of correlated rural shocks and large distances to urban markets.

[10] This assumption is consistent with any model where the probability of full repayment decreases with loan size. In practice, this requires that total loan amounts are known to lenders, which may not always be the case, but should generally be true given the small size of these local markets.

subject to

$$c_{i1} \leq S_{i0} + (\bar{h} - l_i - d_i)w + B_i$$

$$c_{i2} \leq y_i(d_i) - B_i[(1 + r^b)1(B_i > 0) - (1 + r^s)1(B_i < 0)],$$

where B_i is net resources borrowed ($B_i > 0$ implies borrowing and $B_i < 0$ implies saving) during the first period. In period 1, households optimize over three variables: leisure, consumption, and labor input on the farm. In period two, households only choose consumption. Period 2 income (and consumption) is given by harvest production, y_i, plus period 1 borrowing or savings times the respective interest rate. Period 1 consumption is given by initial savings, S_{i0}, net labor income, given by the labor endowment, $\bar{h}$, minus time allocated to leisure, l_i, and on-farm labor inputs, d_i, (some of which may be hired) times the wage rate, w, and net borrowing, B_i.

2.2.1 Optimal labor input

Since farmers can freely access labor and capital inputs labor inputs will always be chosen such that the marginal product of labor in period 2 equals the marginal cost:

$$\frac{\partial y}{\partial d_i} = \beta A(\frac{k}{d_i})^{1-\beta} = wr^e \tag{5}$$

where

$$r_i^e = \begin{cases} 1 + r^b(B_i) & \text{if } B_i > 0 \\ 1 + r^s & \text{if } B_i < 0 \end{cases}$$

is the effective interest rate. Re-arranging equation 5, optimal labor input can be expressed as function of productivity A, wage w, and the effective interest rate:

$$d_i^* = k\left(\frac{\beta A}{wr_i^e}\right)^{\frac{1}{1-\beta}}. \tag{6}$$

2.2.2 Optimal consumption, labor supply and leisure

To maximize utility, the usual inter-temporal optimality condition must hold:

$$\frac{c_2 - \underline{c}}{\rho(c_1 - \underline{c})} = r_i^e. \tag{7}$$

Combining optimality conditions (5) and (6), we can derive an interior solution for the optimal allocation of time toward labor, $h_i = \bar{h} - l_i$:

$$h_i = \frac{\frac{\alpha}{1-\alpha}(1+\rho)\bar{h} + \frac{1}{w}\left[\underline{c}(1+\frac{1}{r_e}) - S^*\right].}{(1+(1+\rho)\frac{\alpha}{1-\alpha})} \tag{8}$$

where $S^* = S_{i0} + \frac{1}{1+r_i^e} y(d_i^*(r_i^e, w)) - d_i^*(r_i^e, w)w$ are total household resources made up of the initial capital endowment and the discounted value of farm production net of labor costs. Optimal levels of consumption in period 1 and period 2 are given by

$$c_1^* = \frac{\underline{c}(\frac{1}{r_i^e} + \frac{\alpha}{1-\alpha} + \rho) + S^* + \overline{h}w}{\frac{1}{1-\alpha} + \rho} \tag{9}$$

and

$$c_2^* = \frac{\underline{c}(\frac{1}{1-\alpha} - r_i^e\rho) + \rho r_e S^* + \overline{h}\rho r_i^e w}{\frac{1}{1-\alpha} + \rho} \tag{10}$$

respectively.

The model offers several implications for our setting setting. First, as shown in equation (6), on farm labor inputs decrease with the effective interest rate faced by farmers, as farmers expect yield returns proportional to the cost of capital. Since farms with small endowments need to borrow more than farms with large endowments, effective rates for borrowing are higher for less well endowed farms. This means that labor inputs (per unit of land) will on average be higher on well-endowed farms than on farms with low capital endowments, and the opposite must be true for the marginal product of labor. Second, net labor supply ($h_i - d_i$) is a decreasing function of a household's capital endowment, S_{i0}. As shown in equation (8), farms' labor supply h_i decreases with their overall resources, S^*. Since on-farm inputs increase with overall resources, S^*, and because labor markets clear locally, household net labor supply is directly determined by the farm's relative capital endowment, S_{i0}: the lower a farm's capital endowment relative to other farms in the same community, the more it will sell labor in the local labor market; the higher a household's capital endowment relative to others in the same community, the more it will rely on external hiring for on-farm labor inputs.Third, consumption seasonality – the degree to which consumption in the first period is compressed relative to consumption in the second period, is a decreasing function of capital endowment, S_{i0}. As shown in the inter-temporal optimality condition in equation (7), relative consumption levels are driven by the effective interest rate faced by each farmer: the higher the effective interest rate, the more households will cut back on consumption in the hungry season relative to consumption in the harvest season.[11]

[11]Higher food prices in the hungry season may further increase seasonality. Note that this model normalizes the price of consumption to one in all periods, and so suppresses the effect of grain price fluctuations – which may arise due to storage costs, for example – on consumption seasonality. We test for treatment effects on grain prices in Section 6.1.5.

2.2.3 The effect of decreasing borrowing frictions

Our experimental intervention subsidized access to credit to small-scale farmers. This corresponds to lowering the effective interest rate r_i^e faced by farmers. Given the setup outlined above, we predict the following adjustments in labor, wages, output, and consumption:

1. Lowering effective borrowing rates increases labor demand (d_i) and decreases labor supply (h_i) of treated farmers. Lower r_i^e implies lower opportunity cost of capital and thus increased on-farm labor investment for borrowing farms, while the positive income effects created by lower interest rates will lower their own willingness to work. This results in a decrease in aggregate labor supply as well as an increase in aggregate labor demand, driving up market clearing wages.

2. Access to lower borrowing rates will reduce within-community differences in the marginal product of labor and will increase output overall. Lower borrowing rates will increase labor inputs for farmers with effective rates above the rate offered, but will not affect labor demand of farms facing lower effective rates including farmers with positive net savings ($B_i < 0$). These net savers will increase their net labor supply - work more and hire less - in response to higher equilibrium wages. Lower effective borrowing rates thus result in a reallocation of labor from better to less well-endowed farms. Since the marginal product of labor is directly proportional to the effective interest rates faced by farmers, agricultural output will increase overall as labor is transferred from low to high marginal product of labor farms.

3. Lowering effective borrowing rates increases hungry season consumption (c_1), reduces consumption seasonality (c_2/c_1) and reduces consumption inequality within communities. Given that both income and substitution effects are positive for first period consumption, lower effective interest rates will always increase first period consumption for low capital endowment farmers. The same is not necessarily true for the second period because positive income effects for second period consumption are partially offset by a temporal substitution effect towards the first period. Given this, lower effective interest rates will always decrease seasonality in consumption (c_2/c_1) among low endowment farmers. Since lower borrowing rates increase labor inputs and output among less well-endowed farms and lower output on better endowed farms, within community inequality in both income and consumption will decline.

3 Experimental design and implementation

We turn now to our experimental setting, design and implementation. We offer further descriptive detail on our study setting in Section 4.2, where we examine the descriptive implications of our model and how they match our data.

3.1 Study setting

The study was implemented between October 2013 and September 2015 (with survey data covering three agricultural cycles/years) in Chipata District, Zambia. Chipata District is located at the southeastern border of Zambia, with an estimated population of 456,000 in 2010 (CSO 2010). Approximately 100,000 people live in Chipata town, the district and provincial capital; the remaining population lives in rural areas, with small-scale farming as primary source of income. According to the 2010 Living Conditions Monitoring Survey (CSO 2010), 63 percent of households in rural Chipata were classified as very poor compared to 32 percent in Zambia overall. Average monthly expenditure of rural households was estimated at US$ 122 in 2010 (US$ 0.8 per person-day), corresponding to about one third of the national average (US$ 389). Access to electricity lines and piped water is virtually non-existent.

The study implementation targeted small-scale farmers, i.e. households growing crops on 5 hectares (12 acres) or less. The label "small-scale" is somewhat misleading since it suggests that these farmers are unusually small; in fact, small-scale farmers represent the overwhelming majority of households in rural villages in Zambia. In our study villages, we document that over 95 percent of households meet this definition.

Study sample

The study sample was drawn from the population of small-scale farmers living in rural areas of Chipata District. The district is divided into 8 administrative blocks, each of which contains a number of camps. We randomly sampled 5 villages from 50 of the 53 camps in the district, omitting the camps that contain Chipata town. The village list was assembled from the Ministry of Agriculture's farm registry, which included 99,000 registered farms in the district in 2013. To facilitate sampling, villages with less than 20 or more than 100 farms listed in the registry were excluded from the initial village selection.

Study enumerators visited sampled villages to record the number of households, and screen for eligibility.[12] Enumerator screening visits stopped once 201 villages met all eligibility criteria. During the baseline survey, 25 additional villages were eliminated for a failure to meet one or more of the eligibility criteria that had been overlooked during the screening process. In addition, one village refused to participate in the baseline survey. This left us with a sample of 175 villages for the study.

Within each eligible village, households were sampled from the village rosters collected during the initial screening visits. Only small farms – less than 5 hectares according to the Zambian Ministry of Agriculture – were eligible for the program.[13] Eligible households were randomly sorted

[12] Villages were ineligible if: (1) other projects had been conducted there in the recent past, (2) the village bordered a village that was in the study pilot, (3) the village bordered a village already listed, (4) the village had fewer than 17 households, or (5) it was impossible to get a 4x4 vehicle within a 5km radius of the village during rainy season.

[13] We restricted our sample to households with at least 2 acres of land to distinguish households with very small

and the first 22 selected for the baseline survey. This resulted in 53 percent of household on average being selected for the project; across all villages, the share of households enrolled in the study varied between 15 and 100 percent. A total of 3,701 households were sampled for the baseline and 3,139 were surveyed at baseline (85 percent). The majority of sampled households not interviewed either had moved away from the village (N=219) or turned out to be ineligible because their plots were too small or too large to be classified as small scale farmer(N=146).

3.2 Experimental design

The main objective of the project was to estimate the impact of short run loans offered during the hungry season on household-level outcomes. The study took place over two years and was designed to coincide with the agricultural calendar (see Appendix figure A.1), which starts with field preparation in September, followed by planting activities around the time of the first rains in November. Planting is followed by weeding between January and April, which is also the time referred to as the "hungry season" or "lean season". In April, early crops start to become available and harvest begins in earnest in May. Between August and October, few agricultural activities take place. We refer to study year 1 as covering the 2013-14 agricultural cycle (beginning in September) and study year 2 covering the 2014-15 agricultural cycle. The study design is summarized in Figure 1.

The study included two main loan treatment arms: a *cash loan treatment* and a *maize loan treatment*, both at the start of the hungry season (January). Repayment was due at harvest, and loans could be repaid in either cash or maize (or both).The choice to implement two different loan modalities was primarily driven by concerns related to utilization of resources as well as feasibility of repayment. Maize loans were chosen because maize is the primary crop grown by farmers and the primary source of food in the local context, with families typically eating nshima (maize porridge) three times per day. Cash was the obvious and more flexible alternative, but was considered to be potentially more prone to wasteful consumption than maize. Both treatment arms were rolled out in January 2014. Of the 175 study villages, 58 (1033 farms) were assigned to a control group, which received no intervention, 58 (1092 farms) were assigned to a cash loan program, and 59 (1095 farms) were assigned to a maize loan program in the first year of the program. In the second year of the program, the treatment groups were rotated: 20 villages that were in the control group in year 1 were rotated to either the maize loan or cash loan treatment arms (10 each), and 29 cash loan villages and 28 maize loan villages were rotated to the control group. Treatment rotation was designed to investigate the persistence of the results for villages phased out after one year, and to separate the impact of repeated treatment from first time treatment. In our main results, we focus on the treatment effects of being treated for the first time in either year, and analyze the effects of

scale home gardens from households engaged in crop production, and also to increase the likelihood of sufficient harvest to repay the loan.

repeated treatment and persistence in separate analyses.

To measure the extent to which farmers adjust their production plan with earlier knowledge of hungry season credit, we also varied the timing of the loan announcement in the second year of the program. Half (40) of the treated villages received notification before the start of the planting season, in September, while the other half of treated villages was only informed about the loan program in January. In addition, to identify the extent to which repayment modalities affected uptake and repayment, half (40) of the treated villages in Year 2 were required to repay the loan in cash only (i.e. no maize repayment allowed).

In addition to the main loan treatments, a small number of villages (6 villages, 91 farms in year 1 and 5 villages, 81 farms in year 2) were assigned to an *income effect control* group, which provided a cash grant of 60 Kwacha, which was the median value assigned to participation in the loan groups in our choice experiments.[14] Cash grant villages were randomly selected within geographic blocks from villages initially assigned to the control group.

Details of the cash and maize loans

In the *maize loan treatment* arm, households were offered three 50 kilogram bags of unpounded maize. Maize is the staple crop in Zambia and 150 kilograms was was chosen so that the typical family of five would be able to cover its basic maize consumption needs for at least two months during the peak hungry season. In the *cash loan treatment* arm, households were offered 200 Kwacha (~ USD 33), which corresponded approximately to the value of the three maize bags at official government prices (KG 65 per bag) at baseline. In both treatment arms, repayment was due in July when most harvest activities were completed. In the first year of the program, households could repay either 4 bags of maize or 260 Kwacha (or a mix at K65 per bag). Villages randomly selected for the "cash only" repayment program in the second year of the study had to repay 260 Kwacha. While both treatment arms were designed to reflect an interest rate of about 30 percent (over 5 months), actual interest rates are hard to compute in practice due to substantial regional and seasonal price fluctuations in grain prices, as well as limited information on transaction costs associated with buying and selling maize locally. As shown in Table 1, interest rates in the maize arm vary between -11 and 33 percent depending on which maize price is used in the calculation, and also depending on what repayment modality is chosen by farmers.

To make the two loan programs as comparable as possible, we conducted a series of hypothetical choice experiments in non-target villages within the study area in November 2013. In these choice experiments, respondents (N=72) were asked a series of dichotomous choice questions on whether they would prefer a loan for three bags of maize over a cash loan of x Kwacha, with x varied between 50 and 600 Kwacha.[15] 84.7 percent of respondents preferred a maize loan over a cash loan

[14] For further details on choice experiments, see Appendix C.1.

[15] Hypothetical loan dates were consistent with program offered (pay out in January and repayment in June), but

of 175 Kwacha, while only 36 percent preferred the maize loan over a cash loan of 250 Kwacha. As part of these choice experiments, we also asked about timing and acceptable interest rates. Specifically, respondents were asked if they would take up a maize (cash) loan that paid 3 bags (200 Kwacha) in January with a repayment of 4 bags (265 Kwacha) due in subsequent months. While only 27.8 (maize) and 20.8 (cash) respondents were interested in a loan with repayments in May, acceptance rate jumped to to 81.9 and 83.3 with repayment in June for maize and cash loans, respectively. Responses to these questions on value and timing determined final design decisions for the treatments. Further detail on the implementation of the choice experiments is provided in Appendix C.1.

3.3 Implementation

Both loan programs were administered under the "Chipata Loan Project (CLP)" to distinguish loan operations from the survey visits conducted by Innovations for Poverty Action (IPA). This distinction between the CLP and IPA brands and staffing was primarily made to assure participants that survey responses would not affect loan eligibility. We also ensured that staff members working on loan implementation would not do household surveys to minimize the risk of surveyor bias. All study households in villages randomly selected for treatment were eligible for loans in the first year. In year 2, the same rules applied. In villages treated in both years, i.e. in villages where loans were offered again in the second year of the study, eligibility was further restricted to households who fully repaid in year 1. Any household that did not fully repay in year 1 was not eligible for a loan in year 2. Loan program participation did not affect any of the survey activities. The loan intervention was announced to households during a village meeting to which eligible households were invited.[16] At the meeting, project staff began by describing eligibility for the program to clarify why only some households were invited to the meeting. The terms of the loan were then described, followed by details on how the loan distribution would be organized. Loan enrollment and consent forms were provided to eligible households. If a household wished to join the program, they were required to present both forms with a signature of the household head when picking up the loan.

Loans were distributed between 3 days and one week after the village meeting at a location convenient for transportation, which was selected in cooperation with the village headman. Project staff registered attendees, confirmed their identity using the national registration card,[17] and collected their signed enrollment and consent forms. Before finalizing the transaction, project staff confirmed that the participant understood the terms of the loan. The loans (cash or maize) were

the hypothetical loans involved no interest.

[16]Ineligible households were not barred from listening in. Eligible households could send an adult representative if the household head was not available to attend. All village headmen were eligible for the loan, even if they were not sampled for the baseline survey (and are therefore not in our study sample). In addition, the baseline data for 3 households who were surveyed was lost. They are dropped from the sample.

[17]In select cases, a household representative picked up the loan. In these cases, the representative needed to carry the loan-holder's NRC card with him or her.

handed over and a receipt was provided to the household and kept for project records.

Repayment was due in early July. Villages were notified in advance about the date of repayment as well as the central locations at which repayment would be collected. Two attempts at collecting repayment were made. Households were provided with a repayment receipt upon full repayment. Throughout the project, households were told that the program might or might not continue in future years, which accurately represented the study team's knowledge. Further summary statistics on repayment patterns are described below.

Randomization In year 1, treatments were assigned at the village level using min-max T randomization (Bruhn and McKenzie 2009), checking balance on both household and village characteristics. The approach relies on repeated village-level assignment to treatment and selects the draw that results in the smallest maximum t-statistic for any pairwise comparison across treatment arms. Balance was tested for 14 household level variables, village size and geographic block dummies, with results described in Section 5.1. The smallest p-value for the pairwise comparisons observed in the final draw was $p = 0.213$. In year 2, treatment assignment was balanced on the same variables plus harvest output from year 1, and stratified by year 1 treatment. In other words, year 2 treatment assignment was carried out within each year 1 treatment arm, with assignment to both the main treatment arms (control, cash loan and maize loan) and the sub-treatments (income effect control, early notification and cash repayment).

Attrition and selection

Appendix table A.2 reports the number of households sampled in each survey round, and the probability of being in the survey round as a function of treatment. Panel A shows year 1 treatments and panel B year 2 treatments. The coefficients and standard errors are from OLS regressions for each survey round, with errors clustered at the village level. Overall, attrition rates are low: 3,030 out of the 3,139 households (96.5 percent) enrolled at baseline completed the endline survey. We do not find any differences in attrition overall or the probability of participating in specific survey rounds across treatment arms.

We also examine whether household self-selection into the program varied by treatment. Appendix table A.3 shows the stages of program implementation. First, households were invited to participate in the village meeting based on random sampling (year 1). To be eligible for the loan programs, households had to both attend the meeting and hand in a consent form. The latter step was completed after learning treatment status and so is the most susceptible to non-random attrition (column 3). In year 1, there was no selection into meeting attendance or eligibility. In year 2, there was some modest selection into meeting attendance (over 90 percent attendance in all treatments and sub-treatments), and no further selection into eligibility. Column 4 of Appendix table A.3 also previews our take-up results, which we turn to next.

Take up and repayment

As described above, loan conditions were established through focus groups and choice experiments to be both feasible from an implementation perspective, and attractive for farmers from an interest rates perspective.

As shown in Table 2, take up was over 98 percent in both years, which suggests that the borrowing rates available through the intervention were on average well below those associated with comparable borrowing opportunities in local markets. High repayment rates (94 percent) in year 1, followed by high take up rates in villages treated in both years, indicate that high take up was not driven by expectations of default.

Repayment was substantially lower in year 2, with an average repayment rate of 80% in villages receiving the program for the first time. These general decline in repayment appears to be at least driven by worse rainfall patterns and resulting lower overall agricultural output in the 2015 harvest. Section 6 provides a more detailed discussion of the differences between years 1 and 2. In addition to these differences in harvest values, we also observed behavioral differences in villages treated for the second time in year 2, with a 6% point decline in repayment in villages where nobody had previously defaulted, and a 29% point decline in repayment in villages where at least one farmer had defaulted in year 1. These general declines in repayment could be interpreted as evidence for farmers generally making less of an effort in the second year of the program. The particularly large drop in repayment in villages with prior default definitely also suggests that loan enforcement was not perceived as particularly strict. In the absence of legal consequences - loan companies generally report to police which we did not as part of this study - defaulting on the loan may have been the rationale choice even if it could prevent farmers from participating in (highly uncertain) future programs.

4 Data and descriptive statistics

We start this section with further description of the data and our main outcome variables. Then we turn to a set of descriptive results that provide contextual information and compare our setting to the descriptive implications of the model.

4.1 Data

We rely on both household survey and administrative loan data in our analysis. Comprehensive surveys of all study households were conducted at baseline (November 2013), harvest of year 1 (August 2014) and harvest of year 2 (August 2015). We refer to these as long recall surveys since they ask questions about the preceding agricultural cycle. Additional sub-sample surveys on labor activities, consumption and farming practices were collected on an ongoing rolling sample between

comprehensive survey rounds. We refer to these as short recall surveys since they primarily ask about activities in the past two days to two weeks. A total of 15,044 observations from the sample of 3,139 households were collected over the course of the study. Appendix A.3 summarizes sample sizes and key content collected in each survey.

We focus on three main outcome types, based on the predictions in our conceptual framework: (1) measures of labor allocation and daily earnings, (2) consumption indicators, and (3) measures of agricultural output. In many cases, we focus on data collected during the hungry season (January - March) of each year, since this is the period of interest in our conceptual model and the time most likely to be directly affected by the loan intervention.

We rely on the short recall survey rounds to construct labor allocation measures over the week prior to the survey during the hungry season. Labor allocation outcomes include (a) family labor sold to other farms (*ganyu* sold), (b) labor labor purchased (*ganyu* hired) and (c) family labor invested on-farm. We construct measures on both the extensive margin and as a continuous measure, at the household level. Our continuous measure is in hours, summed across all individuals in the household (i.e. a total hours measure), to account for the fact that ganyu does not always last the full day and a partial day of ganyu sold might still allow for some time invested on-farm.

Our main consumption measure is the number of meals consumed in a day, by adult members of the household, measured during our short recall (midline and labor surveys) data collection rounds. While this is a coarse measure of consumption, reductions in the number of meals per day points to severe food shortages, and has the advantage of being relatively easy to measure. We collected this outcome consistently across survey rounds, and rely, in our main analyses, on the short-recall measures from the labor surveys, with a recall period of two days. We supplement the measure of meals consumed with a measure of grain available for consumption, which we code as a binary indicator that equals one if the household had fully depleted their grain reserves at the time of the survey. In addition, we collect data on households' perceived food security and construct an index of z-scores based on responses in the control group.

We also construct a measure of daily earnings during the hungry season (when most ganyu is reported). We again use the short recall surveys, which ask respondents about earnings from ganyu sold by each household member over the past week, and calculate daily earnings based on days worked as well as total earnings. We winsorize the top 1 percent of household-level responses to address outlier observations and analyze daily ganyu earnings - as our proxy for local wage levels - first at the household and then at the village level. where we can further reduce noisy by focusing on median earning outcomes. Villages with no ganyu activities reported in a month receive a missing value, and are excluded from the village level analysis.

To measure agricultural output, farmers were asked to report, by crop, output in kilograms as well as the the total value of the harvest, including early consumption and crops still on the field at the time of the interview. We aggregate the total value across all crops, and calculate a constant

price series to remove fluctuations in crop value across survey rounds in our main specification. We also construct an alternative value measure based on own reported prices.

We categorize households by their endowments using baseline savings in grain (valued in Kwacha) and cash. Conceptually, this is intended to represent the starting endowment at the beginning of period 1 in the model, S_{i0}. In addition to these main variables, we analyze several additional outcomes in Section 6, which we describe as they arise.

4.2 Descriptive results

Seasonality in resources and consumption As illustrated in Figure 2, households accumulate grain and cash reserves after harvest, which are mostly depleted by early January. The period between January and March is referred to as the "hungry season" throughout rural Zambia. It also coincides with the time when farmers have all crops on their fields and on-field activities (particularly weeding) peak, as illustrated in the agricultural calendar shown in Appendix figure A.1. This shortage of reserves during the hungry season appears to be mostly anticipated by farmers: at baseline, 76 percent of households stated that they did not think their maize reserves would last to harvest, and most expected to run out of maize in January or February. This implies that general reserve holdings are low (as one would expect from local poverty rates), but that there is nevertheless quite a bit of within-community variation in households' reserves and therefore also households' ability to smooth consumption across periods. Figure 3 shows seasonal patterns in food availability and consumption for the top and bottom quartile of our sample in terms of baseline food and cash reserves for the first year of the study (2014). The left panel shows the average number of meals consumed per day by adult members of the household in the control group. The right panel shows the share of households in that quartile with any grain stock at home. Both measures are somewhat coarse measures of consumption, but still show considerable seasonality in consumption, which – consistent with the model described in Section 2 – is more severe for low endowment than for high endowment households.

Savings, credit and effective interest rates As in many rural developing country settings, access to formal savings and formal credit markets is limited in rural Zambia. At baseline, only 5.6 percent of households report saving in a bank; slightly more (9.1 percent) report saving with friends, family or employers. By far the most common savings strategy, reported by 76.7 percent of households, is saving money at home. The median self reported cash savings (a measure likely to be reported with substantial error) at the start of the planting season of the first year was 80 Kwacha or around 14 USD. Non-cash savings also occurs through grain storage, which typically occurs in a bamboo (62 percent of respondents) or thatch (28 percent of respondents) granary. The median grain storage amount at baseline was four bags, which would the maize needs of a typical family of five about 2-3 months. Sixty percent of households report storage losses in the past season.

Credit markets also appear highly imperfect: only 5 percent of household respondents report accessing formal cash loans from banks, credit unions, NGOs or government sources. Informal borrowing channels are slightly more common: around 7 percent of baseline respondents report taking high interest loans, locally referred to as *kaloba*, with monthly interest rates of 50 percent on average reported in our survey data. Informal loans from friends and family are reported by around 8.5 percent of baseline respondents. Participation in microcredit institutions, rotating savings and credit associations (ROSCAs) and village savings and loan associations (VSLAs) are each reported by around 1 percent of baseline respondents. In-kind input loans are more common than opportunities for borrowing in cash or food: 39.7 percent of baseline respondents accessed an in-kind input loan, typically seeds and chemicals provided by outgrower companies or agro-dealers. During year 2 data collection, we ask households how much they would have to repay in one month if they borrowed 50 Kwacha today from a source other than a friend or family member. The implied monthly interest rate is around 39 percent for households with below median baseline grain and cash reserves; implied interest rates decline slightly with baseline reserves for households with above median reserves, though rates are still high (around 34 percent) for even the "richest" decile of our study sample (see Appendix figure A.2). In the hungry season, sixty percent of households report that they would be unable to borrow 50 Kwacha in cash that day. These descriptive patterns justify the modeling choice that effective borrowing rates (r_i^e) are a function of capital endowments (S_{i0}).

Local labor markets Local wage earning opportunities for study households are defined largely by piece-wise labor contracts locally referred to as *ganyu*. In focus groups, a majority of small-scale farmers in Chipata described ganyu both as the most common strategy to cope with temporary cash needs and as an activity most farmers would rather avoid if possible.[18] In the baseline survey, the most common response to why an individual in the household worked ganyu during the previous agricultural season was to obtain food. The second most common reason was to access cash for a personal purchase, and the third was to deal with an emergency. When asked what the household would do in the coming year if they ran out of food, 56 percent reported that they would do ganyu. The next most common answers include borrow from friends or family (28 percent), using savings (22 percent) and sell assets or livestock (17 percent), all of which may be difficult to rely on during the hungry season. On average, ganyu contracts specify relatively small tasks (such as weeding an acre of land), which can be completed by an individual within a working day; larger contracts do however also occur, and it is quite common for families or groups of individuals to complete specific tasks together. Further detail on how ganyu participation varies with age and gender of household members is discussed in Fink et al. (2014), which describes a pilot for the current study.

[18] In our baseline survey, around 90 percent of households disagreed with the statements "Doing ganyu increases people's respect for you in the community" and "Successful farmers do lots of ganyu". Conversely, around 60 percent of households agree with the statements "Lazy people do lots of ganyu" and "People who can't budget do lots of ganyu".

In terms of household exposure to ganyu, almost two thirds of farmers in our sample reported to have engaged in ganyu activities in the previous season at baseline, and about the same fraction of farmers reported that they would engage in ganyu labor in the subsequent season. Households appear reasonably accurate in their forecast of whether they will have to engage in ganyu in a given year. Among control group households that predicted at baseline that they would have to do ganyu in the coming year, around 76 percent did; among those that predicted not doing ganyu, around 41 percent ended up working off-farm. At the same time, the likelihood that a household sells ganyu is not constant across years. Among control group households that did not engage in ganyu the year before the study, 40 percent sold ganyu the following year.

Ganyu wage rates are typically negotiated on a case-by-case basis, and anecdotally are highly responsive both to demand and supply shocks. The majority of casual labor transactions take place in or near the worker's own village, which may be explained by the relatively large distances and general absence of motorized transport. The majority ($> 80\%$) of farms hiring ganyu are small (i.e. fewer than 5 hectares of land), with some farms acting as both buyers and sellers during a single season (thought typically at different points in the season). Consequently, as described in the model, wages are determined by local market clearing, which either means within a village or within a small group of villages.

Seasonal migration is uncommon in rural Zambia. In our sample, in any given month, only around 3 percent of households report that someone who is typically a member of the household moved away temporarily. This number peaks around and immediately after harvest, and is lowest during the lean season when around 2 percent of households report temporary migration. Permanent migration is more common, as suggested by data on remittances: around 20 percent of households report that someone who does not live in the village contributes regularly to household income.

Household resources and labor market outcomes One of the central implications of our theoretical model is that households' relative resource endowment critically shape households net labor supply in local markets. To show this holds empirically, we rank households with respect to their baseline cash and food reserves, and then plot several measures of household labor activities and transactions. The left bar of Figure 4 show the number of hours household members worked for other farms; the middle bar shows labor effort on their own farm land; the right bar shows hiring of external labor.[19] Hours sold is substantially higher in the bottom quartile than in the top quartile, while households in the top quartile supply slightly more labor to their own farms overall, but less per acre of land. Hiring is most common among households with top-quartile endowments. In terms of the general labor flows, low endowment households thus sell their labor to households

[19]These numbers ignore the substantial additional time burden associated with searching for ganyu. Surveys during the hungry season of year 1 of the project indicate that control group households spend around 3 hours searching for ganyu each week, conditional on searching, while they work around 6 hours per week, conditional on working. The average hungry season ganyu work day in the control group lasts for just over four hours, not including search time.

with relative large capital endowments.

Our model also implies that the marginal product of labor will vary with baseline endowments. To identify average marginal products of labor, we use a production function approach and regress the log of agricultural output value on a continuous measure of the number of days of labor input supplied over the previous season by households in the control group in year one. The estimates are plotted for the bottom and top endowment quartiles in Figure 5. While the estimates are noisy – they come from a sample of 223 bottom quartile and 258 top quartile households in the control group in year 1, and include baseline and geographic controls but no other contemporaneous inputs – they indicate a significantly higher marginal product of labor in the bottom quartile households than in the top quartile households.

We turn next to daily earnings. Given that virtually all labor transactions appear to happen within a given village, we consider each village as separate market, and construct village-month level average daily earnings as a proxy for equilibrium wages. The two years of the study differed in weather realizations, with considerably worse rainfall in year 2 resulting in agricultural output that was around 12 percent lower, on average. For now, we make the simplifying assumption that comparing daily earnings across years in the control group is informative of the relationship between TFP and wages. The monthly median (mean) daily earnings in control group villages was 16.2 (17.5) Kwacha in year 1 and 13.9 (15.3) in year 2 (excluding villages treated in year 1). Earnings are unadjusted for inflation, which was considerable between the two years, indicating even more dramatic declines with poor weather (and, presumably, TFP) realizations in year 2.

Next, we consider the relationship between capital endowments and equilibrium earnings. We calculate the average capital endowment at the village level, using the same baseline measure of household cash and grain reserves. Regressing the median daily earnings on the median endowment (in 100 Kwacha) in the 69 control village-months in year 1 that have non-missing earnings data, controlling for month, survey round, and geographic controls indicates that a 100 Kwacha increase in the average endowment in the village is associated with a 1.2 (s.e. 0.62) Kwacha increase in equilibrium earnings. Figure 6 shows both the raw data and best fit line (excluding the geographic controls included in the regression).[20] Of course, this relationship may reflect omitted village-level characteristics associated both with equilibrium wages and average capital endowments. We control for two of these – distance to a paved road and village size – and the coefficient remains largely unchanged and marginally significant (1.1, s.e. 0.62).

[20]If the outlier observations shown in the figure (median daily earnings above 40 Kwacha and median reserves above 1500 Kwacha) are dropped from the regression, the coefficient falls to 0.67 and remains marginally significant.

5 Experimental results

In this section, we present our main experimental results. We start by outlining our empirical strategy, and then present key results for consumption, labor allocation and overall distribution of output reviewing and testing the main empirical predictions of our model.

5.1 Empirical strategy and identification

We estimate intention-to-treat regressions, including all households regardless of whether they selected into the loan. High take up in both years means that these estimates are very close to the treatment on the treated effect. We order our analysis as follows. For our main outcomes, we begin with a pooled specification that averages across years and loan treatment arms:

$$y_{ivt} = \alpha + \theta loan_{vt} + \tau_t + X_{i0}\beta + u_{ivt} \tag{11}$$

where y_{ivt} is an outcome of interest for household or individual i located in village v at time t, where t is a year for our long recall surveys and a month-year for our short recall surveys. $loan_{vt}$ indicates that the village was assigned to one of the two loan treatments, τ_t are time dummies and X_{i0} are household-level controls, measured at baseline. Treatment assignment varies over time according to the treatment rotation between years, as described in Section3.2. In our main results, we focus on villages assigned to receive treatment for the first time and exclude year 2 data from villages that were treated in year 1. We analyze villages with repeated or discontinued programs separately to investigate persistence of the treatment effects. We define each year's treated period as lasting from loan distribution (January) until harvest in June. We focus both data collection and analysis on the hungry season and harvest period, with an emphasis on the former. Errors are clustered at the level of the randomization unit, the village v, which addresses both unobserved village level shocks and serial correlation across study years. Next, we separate the treatment effect by treatment arm, using estimating equation (11) and allow θ to differ for the cash and maize loans.

We also estimate treatment effects by quartile of the distribution of baseline grain and cash endowments, following the descriptive results presented above.

$$y_{ivt} = \alpha + \theta loan_{vt} + \sum_{q=2}^{4} \delta_q Q_q + \sum_{q=2}^{4} \gamma_q Q_q \times loan_{vt} + X_{i0}\beta + \tau_t + u_{ivt} \tag{12}$$

delivers coefficients for each quartile. To illustrate these subgroups effects, we plot the quartile-specific coefficients δ_q for the control group and $\theta + \delta_q + \gamma_q$ for the treatment group.

We provide a number of supplementary analyses to further investigate potential mechanisms and also to address alternative interpretations of our main effect. In a first step, we examine the degree to which hungry season shortages and treatment effects are anticipated. We first examine

heterogeneous treatment effects based on anticipation of resource shortages at baseline, restricting attention to year 1, for which baseline measures are most relevant. Second, we exploit the variation in the timing of loan announcement (holding fixed the timing of loan delivery) in the second treatment year. We also investigate persistence by estimating cross-sectional OLS models for each year of the project. For year 2, we interact an indicator for a loan treatment in year 2 with treatment status in year 1. Finally, we show treatment effects for two additional design features: the impacts of a small cash grant to rule out income effects and the impacts of requiring repayment in cash (year 2 only).

Our regression estimates identify the causal effect of the loan under the identifying assumption that treatment assignment is orthogonal to u_{iv}. Appendix table A.1 presents the means and standard deviations of baseline survey characteristics among study households, by main treatment arm for years 1 and 2 (columns 1-3 and 4-6, respectively). Column 7 shows the largest pairwise t-statistic and column 8 the largest pairwise normalized difference. Normalized differences are small and all fall below the rule of thumb cutoff of 0.25 (Imbens and Wooldridge 2009), despite some marginal differences across treatments. Overall, the randomization successfully balanced households across treatment arms. The variables shown in Appendix table A.1 are our household-level controls, used throughout the analysis.

5.2 Impact of lowering borrowing rates through seasonal loans

The model presented in Section 2 predicts labor, consumption and production impacts from changing borrowing rates, in a general equilibrium framework where all farmers benefit from lower interest rates and labor markets clear. In this setting, any decrease in net labor supply must be directly compensated by an increase in net labor supply from other households in the community. The primary result of lowering borrowing interest rates is thus a reallocation of labor from households with larger capital endowments toward households with lower capital endowments, equalizing labor inputs in a setting with no other sources of heterogeneity. In our study setting, only some (on average about 50% of) households in a village are eligible for the loan. With partial treatment, the intervention will not only result in a re-allocation of labor from farms with high to farms with low endowments, but also to a reallocation of labor from untreated to treated farmers. Since the demand and supply functions of untreated farmers does not change, wage adjustments will be lower than with full treatment saturation, while labor adjustments among treated farmers will more positive (larger increase in net hiring) in partially treated communities than with full treatment saturation.

We turn now to the three main predictions presented in Section 2.

Prediction 1: Lowering effective borrowing rates decreases net labor supply among low-endowment farmers We examine impacts both on the extensive and intensive margins of labor in Table 3. Panel A shows the average treatment effect for both loan arms, and Panel B estimates separate effects by treatment arm. We present and discuss all of our average treatment

effect tables with a similar layout.

Our main prediction is that loans will reduce net labor supply, which implies a decrease in family labor sales (ganyu sold) among households with less endowment and r an increase in hiring (ganyu hired) among relatively better-off farms. We test for these responses using data from the short-recall surveys during the hungry season, where households report on labor activities in the previous week. Labor outcomes are for the entire household. Column 1 shows that the likelihood of selling ganyu falls by around 3 percentage points, on average, off of a mean of 30 percent among sampled farmers. On average, this extensive margin effect is not significantly different from zero, though the 4.2 percentage point decrease in the maize loan treatment arm (Panel B) is significant at $p < 0.10$. Turning to the continuous measure of total family hours sold, treated households sell an average of 0.9 fewer hours or 24 percent less ganyu per week (column 2), on average. The effect on hours sold is also (insignificantly) larger in the maize loan treatment arm.

Treatment also has positive, but statistically insignificant, effects on hiring, on both the extensive margin (column 3) and continuous measures (column 4). The likelihood of any hiring (column 3) increased by 2 percentage points, off of a mean of 11 percent, and the number of hours hired increases by around 1 hour or 27 percent (column 4). The impact is insignificantly larger in the cash loan arm (Panel B) for both outcomes.

Hours of family labor invested on the farm over the past week increase by around 4.5 hours or around 8.5 percent (column 5), with similar impacts across treatment arms. This results in a similar measure of total hours invested on the family farm (column 6). This increase in labor hours on farm is consistent with the idea of households increasing on-farm labor inputs in response to lower borrowing rates. The estimated magnitudes – a 4.5 hour increase on-farm relative to a 0.9 hour decline in labor sales – suggest that households actually work more on average (increase labor supply), in contrast with the model's predicted decrease in labor supply due to positive income effects. Given that ganyu work is strenuous and accompanied by substantial search costs (and social stigma), it may be that households no longer obliged to do ganyu are both better rested and more motivated for field work; the consumption impacts we present below highlight the further possibility that higher caloric intake led to more energy for on-farm work. At the same time, the loan program itself may motivate farmers to work harder, or the (insignificant) increase in hiring results in additional supervision needs on the farm.

Even though our program reached on average only 50 percent of farmers in each village, we expect the observed changes in net labor supply to alter the local labor market equilibrium. Table 4 shows our estimated wage impacts; since wages are not defined in the largely piece rate work arrangements that we observe, we use daily earnings as our proxy for local wage rates. As expected, we find modest increases in daily earnings, which are sensitive to the right hand tail of the distribution. Columns 1 and 2 show regression results using household level data, winsorizing the top 1 and 5 percent of observations within treatment and year, respectively. Earnings increase by 2.4 or 1.2

Kwacha per day on average, corresponding to an 9 to 16 percent increase over the control group mean, on average (Panel A). Increases are larger in the maize arm for both measures (Panel B). Column 3 shows village-level regressions using median reported daily earnings at the village-month level. The magnitudes line up reasonably well with the household level data reported in column 2. On average, the increase in daily earnings is expected given our theoretical model and the labor adjustments observed among treated households. Given that equilibrium effects should be proportional to treatment intensity,we show estimated earnings impacts by the share of the village eligible for treatment in Appendix Figure A.3. The estimates are noisy, but show the expected positive gradient.

Prediction 2: Lowering effective borrowing rates reallocates labor from better to worse endowed farms and increases output on average Figure 7 plots quartile specific treatment effects on our continuous measures (total hours over the past week) of labor market adjustments for both the control and treatment groups in year 1, by quartile of the baseline distribution of grain and cash reserves.[21] The top panel shows effects on hours of ganyu sold, the middle panel shows the effect on hiring, and the bottom panel shows total family hours spent on-farm. As expected, labor inputs increase for all treated farms. In the bottom two quartiles, the primary adjustments are a shift from selling ganyu to working on-farm; in the top two quartiles, increases in labor inputs come both from more ganyu hiring and more family labor invested on-farm. Overall, all households in the sample appear to increase on-farm labor inputs, which suggests that non-treated farms absorb most of the additional labor demand created.

The average increases in hiring and family labor investments on farm may increase agricultural output, on average, and particularly among low baseline endowment households if the marginal product of labor is, indeed, larger for low-endowment farms.We show average effects on agricultural output in Table 5. Panel A shows that agricultural production (as measured by total harvest value using nominal (column 1) and constant (column 2) prices increased by around 8 percent on average, with slightly larger point estimates in the cash loan treatment arm. Figure 8 shows that the average treatment effect on agricultural output in year 1 is largest in the bottom quartile, and declines with capital endowments.

Prediction 3: Lowering effective borrowing rates increases consumption during the hungry season, reduces consumption seasonality, and reduces consumption inequality Table 7 shows the treatment effect on consumption and food security measures, with a primary

[21]The underlying regression omits baseline controls to allow for other household characteristics correlated with baseline grain and cash reserves to vary by quartile. We focus on year 1 in these figures because of the overall less precisely estimated impacts in year 2 and because baseline measures of resource endowments continue to predict year 2 outcomes in many cases, but less strongly than for year 1. The set of figures presented in this section for year 2 (excluding villages treated in year 1) is shown in Appendix tables A.4, A.5 and A.6.

focus on the hungry season, when constrained households have to compress their consumption most as shown in Figure 3. To assess the impact of lower effective borrowing rates, we rely on the consumption outcomes used in Figure 3: the number of adult meals consumed per day in the past week and food availability in the home, along with an index of food security.

We find substantial average improvements in consumption and food security outcomes in response to loan access. As shown in column 1, the likelihood of having any grain in the home increased by 11 percentage points an effect that is almost entirely driven by maize loan arm. The food security index improved by around one-third of a standard deviation, on average, with larger effects in the maize loan arm (column 2). Adults eat more meals of the staple food during the hungry season as a result of treatment, as shown in column 3. Daily meals increase by around 0.08 meals or 0.19 standard deviations, on average, which is comparable to the difference between average meals consumed in the control group between the hungry season (column 3) and the harvest season (column 4). In contrast, at harvest, we see no effect of loan access on consumption outcomes (column 4). This means that the treatment did not just increase consumption in general, but rather resulted in a reallocation of consumption towards the hungry season, and thus yielded the reduction in consumption seasonality predicted by our theoretical model. Though it is outside of the scope of the model's predictions, Appendix table A.5 shows modest reductions in illness within the home and in mental health, with no changes in treatment seeking probabilities. Thus, the changes in family labor inputs may be driven in part by fewer days lost to illness, and the quality of labor may also have improved.

A final implication of the model is a reduction in consumption inequality between low and high endowment households. We compare our main measures of consumption (food availability and adult meals per day) across baseline endowment quartiles for treatment and control villages during year 1 in Figure 9. The top panel of the figure shows that the likelihood that a household had any food reserves during the hungry season is correlated with baseline endowments, and that the treatment effect is largest at the bottom end of the endowment distribution. In the top quartile, the treatment effect on food availability is statistically insignificant. The bottom panel of the figure shows similar effects on meals consumed during the hungry season in all quartiles. Given that base consumption is substantially lower for the bottom quartile, this indicates a larger proportional improvement in consumption for low endowment households, and a reduction in consumption inequality as illustrated by the substantially flatter endowment-consumption relationship in Figure 9.

6 Interpretation and robustness checks

While our model and main results highlight the impact of lowering effective borrowing rates on households' consumption and labor decisions, the provision of loans may have also have affected other household decisions, which may in turn have influenced both labor allocation and production.

We discuss other potential interpretations of our findings in this section, along with results that fall outside of our model, including some robustness checks.

6.1 Interpretation

6.1.1 Anticipated versus unanticipated shortages

Much of the existing literature linking labor markets to capital availability in rural agricultural settings has focused on smoothing unanticipated shortages arising from aggregate or idiosyncratic shocks (e.g., Jayachandran (2006); Kochar (1999)). Our model and results suggest that capital market frictions shape labor market outcomes even in the absence of unanticipated shocks; farms with low reserves sell labor locally and reduce own farm labor inputs in response to high borrowing costs. Households may, of course, also use loans to smooth unanticipated shortages. To further investigate whether labor adjustments are "structural" and anticipated, we use baseline information on households' anticipation of food shortages in the coming season, and estimate treatment effects by subgroup. If loan availability primarily modified household responses to unanticipated shocks, we should see that treatment impact is independent of households' anticipated shortages. If, on the other hand, loan availability primarily matters for those who anticipate running short of resources, we should see largest treatment impacts among those farms most dependent on labor sales to finance consumption.

We show results in Table 6, pooling across years and treatment arms, with villages treated in year 1 omitted from the year 2 results (i.e., the same sample as we use in the main results, above). The correlations between the anticipation measure and the outcome suggest that many outcomes are predictable: households that anticipate food shortages at baseline consume fewer meals (column 1), sell more ganyu (column 2), and have lower agricultural output (column 5). Treatment effects also appear to depend on anticipated shortages for these outcomes: the effect on consumption is driven entirely by households who anticipated food shortages at baseline. Effects on ganyu sales are also driven by this sub-group, though the interaction term is imprecisely estimated. Finally, treatment effects on agricultural output are driven entirely by households that anticipated running out of food. Interestingly, the effect on family labor inputs or hiring does not appear to be concentrated in the sub-group that anticipated shortages, though the inclusion of household controls imposes a strong *ceteris paribus* assumption that may impact the results. Overall, it appears that anticipated shortages play an important role in shaping treatment effects, making it less likely that our main results are driven by unanticipated shocks. We turn next to timing of when farmers learned about the program in year 2 as a further check on whether adjustments are anticipated.

6.1.2 Anticipated versus unanticipated loans

Table 9 reports the effects of the year 2 sub-treatment that varied whether eligible households learned about the loan program at the start of the hungry season (the same timing that was used in year 1) or at planting time (early notification). If hungry season cash constraints (and interest rates) are anticipated, farmers are likely to adjust production plans to accommodate outside labor activities. Notifying farmers at the time they are making their production plans could increase the impact of lower effective borrowing rates, since farms can adjust crop mix and crop timing in anticipation of loan availability. We interact treatment status with an indicator for early notification (the reference timing is notification at the start of the hungry season) for our main outcomes (columns 1-5) and for two supplementary outcomes relevant to planting season adjustments (columns 6-7): the total value of capital inputs applied to the household's fields and the acres devoted to cash crops. The top set of results pools the cash and maize loan treatment arms; the bottom results shows effects for each treatment arm interacted with notification timing. To facilitate comparisons with the main results, villages treated in year 1 are excluded from the analysis.

While most estimates are imprecise due to the small sample size, we observe qualitatively larger impacts on labor allocation in villages notified at planting about the loan program, and larger effects on agricultural output value (Panel A). Columns 6 and 7 suggest that some of this may be due to planting stage adjustments: households apply more capital inputs if they know the loan is coming and devote more acres to cash crops. Both the value of inputs used and the acres under cash crops in the early notification treatment (i.e. the sum of the main effect of the loan and the differential effect of early notification) is about 30 percent higher than in the control group. To put these numbers into perspective, the 250 Kwacha of additional inputs is about the value of one 50 kilogram bag of fertilizer (roughly half of the recommended fertilizer for one acre), and the impact on total acres under production is around 5 percent of the average land area under production. Breaking the results out by treatment arm (Panel B) offers suggestive evidence that planting season notification results in higher output value in the cash loan arm because of additional adjustment in inputs and cropping decisions for this subgroup. This may suggest that the cash loan notification at planting enabled some short run borrowing for planting stage investments, though we lack additional data to back up that interpretation.

6.1.3 Persistence of treatment effects

Table 9 shows the persistence of effects from year 1 into year 2. Columns 1-5 show the effects on our main outcomes by year, while columns 6 and 7 show planting season outcomes that may have been affected if year 1 treatment effects led to adjustments in production in the following year. Panel A shows the effects in year 1; Panel B breaks year 2 out by year 1 treatment status. Recall that the main results described above pool year 1 treated villages with those treated for the first time in

year 2, i.e. they estimate the effect of receiving the program for the first time.

Panel A of Table 9 shows that many of our main results are driven by outcomes in Year 1. Specifically, we see statistically significant increases in meals consumed (column 1), decreases in hours sold (column 2), increases in hours hired (column 3), increases in family hours spent on-farm (column 4) and increases in output (column 5). Changes associated with planting season decisions show insignificant effects on agricultural inputs and acres devoted to cash crops in year 1, consistent with the timing of loan notification that year. Panel B shows similar patterns and magnitudes for villages treated for the first time for consumption, ganyu sales, family labor investments and output. Unlike in year 1, hours hired (column 3) is statistically insignificant with a negative coefficient. Inputs and cash crops increase more in year 2 than in year 1 for villages treated for the first time, consistent with the notification timing results discussed above.

Panel B also shows results for villages treated in year 1, which are excluded from most of the main analyses. While the estimates are noisy, we relatively little evidence of persistence. On average, villages treated in year 1 look similar to the year 2 control group, aside from large but imprecise increases in hours of ganyu sold and in output value. More surprisingly, we find no effect for villages treated twice, where farmers seem to have lower family labor on-farm and other inputs, and lower output than farmers in in villages treated for the first time in year 2. In general the total effect of repeated treatment (sum of all three coefficients in Panel B) is agricultural outputs that are very similar to the pure control, i.e. villages that were never part of the loan program. It is possible that the lack of repeated impact is simply a result of the small sample across year 2 sub-treatments; it is however also possible that repeated loan availability led to behavioral adjustments that were overlooked by our data collection. Further research is needed to better understand these dynamic effects of lowering borrowing rates.

6.1.4 Other margins of adjustment

In addition to increasing hungry season consumption and on-farm labor, it also seemed likely that at least some farmers would use the loans as substitutes for other more expensive financing strategies. We collect data on several of these and show treatment effects in Appendix table A.6. We begin by assessing the degree to which the program substitutes for other types of borrowing. We analyze program impact on three outcomes: formal loans (column 1), low interest informal loans from friends or family (column 2) or informal high interest loans, typically from moneylenders (column 3). All types of borrowing were reported retrospectively at the end of each season as part of the long recall surveys, and coded into basic binary extensive margin variables, with villages treated in year 1 omitted from the year 2 data (i.e., we replicate the main specifications for these outcomes).

We find no effect on input loans (column 1). The baseline mean rate of input loans is close to 40 percent, while all other forms of borrowing are relatively rare. Column 2 shows that informal low interest borrowing declined insignificantly by about 1 percentage points, on average, relative

to a mean of 7 percent of control group households. Column 3 shows that loan access had the largest (relative) impact on high interest borrowing, which declined by around 2.6 percentage points (40 percent) relative to a baseline mean of 6.5 percent. While we observe little detail about the conditions around these informal loans, these results suggest that at least some households reduced their exposure to other forms of borrowing, and likely had direct financial benefits through lower interest payments as a result of the treatment. We also conduct additional analysis for livestock or asset sales as alternative smoothing strategies (columns 4 and 5), and observe small increases in livestock sales of around 3.5 percentage points or around 10 percent of the control group mean. Both the effects on high interest borrowing (column 3) and livestock sales (column 5) are largest among households in the maize loan treatment arm.

6.1.5 Grain prices and transactions

Households were told that they could do what they liked with the cash and maize provided to them through the loans. Therefore, an increase in maize sales in maize loan villages and/or an increase in maize purchases in cash loan villages may have changed the price of the staple crop, affecting the value of consumption or of output. Appendix table A.7 shows the treatment effect, in year 2 only, for maize prices and transactions. Overall, we observe no effect on either prices or transaction probabilities, with point estimates very close to zero and standard errors that allow for adjustments of up to around 25 percent of the control group mean price of 1 Kwacha/kg. These results focus on year 2 since we did not start recording transactions carefully until after the year 1 harvest survey. They average across all months to improve power, but if we restrict the regressions to the hungry season months shortly after the loan was delivered, results are similar. A decline in prices does not appear to drive the main consumption findings.

6.1.6 Maize versus cash loans

In our main results, we show effects for both the maize and cash treatment arms, and observe generally statistically indistinguishable responses in both arms. In terms of the estimated magnitudes, effects on consumption and some labor outcomes appear to be somewhat larger in the maize treatment arm, perhaps reflecting the higher value of the resources provided to eligible households. On the other hand, the estimated increase in agricultural outputs was slightly higher in the cash treatment arm. In terms of the main rationale for maize loans - the lower anticipated likelihood of loan utilization for wasteful consumption, we see very little difference in reported and observed behaviors. In Appendix table A.8 we show estimates on treatment effects with respect to a range of different (potentially wasteful) expenditure categories. We see little evidence of major expenditure adjustments in response to treatment overall, and find no evidence of increased conspicuous consumption.

6.2 Robustness checks

Income effects To distinguish temporal reallocation effects from positive income effects created by the loan programs (due to below-market interest rates), we implemented a small cash transfer sub-treatment, as described in Section 3.2. In Table A.9, we test whether a transfer of 60 Kwacha at the start of the hungry season led to a measurable effect on our main outcomes. While our power is somewhat limited due to the small sample size, we do not find any evidence for labor adjustments in response to the cash gift, which suggests that the treatment effects we measure are not driven by the relatively small net transfers (income effects) embedded in the loan program.

Reporting bias

Given that we mostly rely on self-reported outcomes in our analysis, one obvious concern is that household responses may have been affected by reporting bias and thus threaten the validity of our analysis. Even though we made an effort to separate data collection from the loan program as described above, it seems likely that at least some participating households associated the survey with the loan program. Given that participants were generally happy with the loan programs (and wanted more of them), it was possible that our treatment effects could have been biased by treated households responding more positively or by making more effort to provide the socially desired answer. We test for bias in our self reported survey measures in two ways. First, we included a social psychology scale, adapted from Marlow and Crowne (1961), designed to directly measure social desirability bias in both a survey conducted during the hungry season and our endline survey. The module includes a series of questions designed to prime social desirability bias, such as "Are you always courteous, even to people who are disagreeable?" We construct a social desirability index based on responses to the eight questions and test whether it is affected by treatment, and then test whether there are any differences in the response patterns across treatment arms. As a second robustness check, we also collected objective measures of maize output in years 1 and 2 of the study. In year 1, enumerators visited the fields of a sub-sample of respondents and measured the height of a typical maize stalk in the field. In year 2, the approach was more systematic and involved sampling maize cobs and counting the corresponding number of kernels. In both cases, we can directly compare objective measures of output with reported measures, and test whether there are any systematic differences between the two variables. It should also be noted that we considered direct observation measures for all harvest outcomes (as alternative dependent variable). Such measurements were however not feasible in practice due to the large number of different crops grown by farmers in our sample as well as the often very large distances between various plots used by the household.

Results are shown in Table 10. Panel A shows that the social desirability index is unaffected by treatment in either survey round. The estimated treatment effects are statistically insignificant and

very close to zero. We include no baseline controls in these regressions so that we can interpret the r-squared associated with the regression. Treatment explains almost none of the variation in the social desirability index measure.

In order to test whether treatment affects yield reporting, we regress self reported maize yields on our objective measure of productivity, the loan treatment and the interaction of the two, along with a measure of the share of the maize yield that comes from hybrid maize in Panel B. The interaction term is the regressor of interest, since it shows whether the objective measure is differentially predictive of self reported outcomes among treated farmers. If treatment leads to biased self reporting, we expect this term to be negative. In year 1, the sign is positive. In year 2, it is negative, but small relative to the raw correlation between the objective measure and the self report. Furthermore, the overall variation in the outcome explained by the regressors is unchanged when the treatment variable is added (move from column 1 to 2 and 3 to 4). Both of these tests suggest that treatment status did not alter the reporting behavior of study participant

7 Discussion and conclusions

Our main results show that the loans increased the value of agricultural output of by about 8.1 percent, which corresponds to approximately 260 Kwacha on average over the study period (Column 1, Table 5).[22] We also estimate that the loan program resulted in approximately 5 additional hours of labor inputs per week on the family farm (Column 6, Table 3). Taking the thirteen weeks of the hungry season from January to March as our time reference, and assuming an average five hour work day on the field (as reported in our time use questions), 5 hours per week means approximately 13 additional days of labor on the farm during the hungry season. This implies an average marginal product of labor of around 20 Kwacha, which is of similar magnitude but about 30 percent higher than the treatment group average daily earnings reported in Table 4. Even at a slightly lower marginal productivity measure (in light with the wage rate, for example), this suggests that a relatively large part of the observed harvest value impact likely occurred through the labor channel. However, other adjustments including changes in the crop mix in year 2, improvements in nutrition, and the addition of some other types of inputs may have further enhanced agricultural production in response to the loans.

The changes in labor market activities that we document also had direct financial implications for farms. On average, households in the control group sold 3.7 hours of ganyu per week (Column 2, Table 3) and received daily earnings of 13.2 Kwacha, which implies total hungry season earnings of around 127 Kwacha (assuming 5 hour work days and 13 weeks during the hungry season). Households in treated villages sold an average of 2.8 hours of ganyu for daily earnings of 15.4 Kwacha,

[22]We use nominal prices for this calculation, to allow for comparisons with the nominal daily earnings, and to reflect actual income for farmers, which may have included adjustments in cropping in response to price variation between years.

resulting in 112 Kwacha of extra labor market income over the course of the hungry season. Thus, while treated households earned less on the labor market during the hungry season, some of their reduced hours were offset by higher daily earnings, and the estimated loss of labor market earnings (˜15 Kwacha) is relatively small. Treated households also spent around 61 extra Kwacha on labor, on average (based on the same calculations on the hiring side).

Quantifying the overall financial impact on farms is challenging. Even though the average estimated impact on agricultural output happens to correspond exactly to the repayment amount (KR 260), the net impact on farms' financial position is unclear. The magnitudes presented above suggest that a substantial proportion of the original 200 Kwacha loan was used for financing consumption otherwise covered by ganyu labor, to hiring additional labor. On top of these changes, we find some evidence that households borrowed less from informal channels, which should improve their overall fianncial position. We do, of course, also find evidence of increased consumption during the hungry season, which likely offsets some of these benefits, so that the net financial impact at the end of the agricultural season is not obvious; the limited data we collected on assets, livestock and net savings suggest that the overall impact on measures of wealth is likely small. The same is likely not true for farm welfare: as evidenced by a repeated take up rate of over 95 percent in the second year, farmers clearly did perceive the loans as welfare-improving overall. e.

The results presented in this paper highlight the importance of credit markets for agricultural labor markets in general, and for the within-community allocation of labor, income and consumption in particular. With limited initial capital endowments, capital market frictions exacerbate income and consumption inequality within communities, and lower agricultural output overall. From a policy perspective, providing rural farmers with subsidized access to credit has thus several attractive features: in addition to increasing financial inclusion at a societal level, the results presented in this paper suggest that loans targeted to poor farmers during the hungry season can improve food security and increase average agricultural output, while reducing farmers' reliance on stigmatized piece work labor. These improvements in welfare resulting from lower borrowing costs during the lean season are particularly large among farmers with fewer resources going in to the lean season. As we show both in our theoretical framework and our empirical results, local labor market equilibria respond strongly to decreasing labor market participation by poorer farmers, resulting in higher wages and reduced farming profits for farmers with relatively large capital endowments. Since increased access in capital reduces differences in the marginal product of labor across farms, the net effect of increased capital access on output should always be positive.

In the specific study area analyzed in this paper– and presumably in many others – the room for improvement in local credit markets is without any doubt large. Most farmers in our study report local interest rates well above 100 percent. These self-reports are backed up by the 98 percent take-up of the (high interest) loans we offer, even in the second year after farmers had repaid their year one loans. High interest rates should however not be equated with inefficient or failing credit

markets. As our own project experience shows, transaction costs involved in running rural lending schemes in settings with limited road infrastructure are large, and enforcing loan repayment is difficult in the absence of collateral.

The loan intervention we study was not designed to maximize impacts on agricultural production, but rather to relax borrowing costs during the hungry season and allow us to test specific predictions around seasonal incomes and credit market frictions. Further work is needed investigate the returns to capital at different points during the agricultural calendar, and to test more cost effective ways of extending credit in rural settings with poor transportation infrastructure.

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Figures and Tables

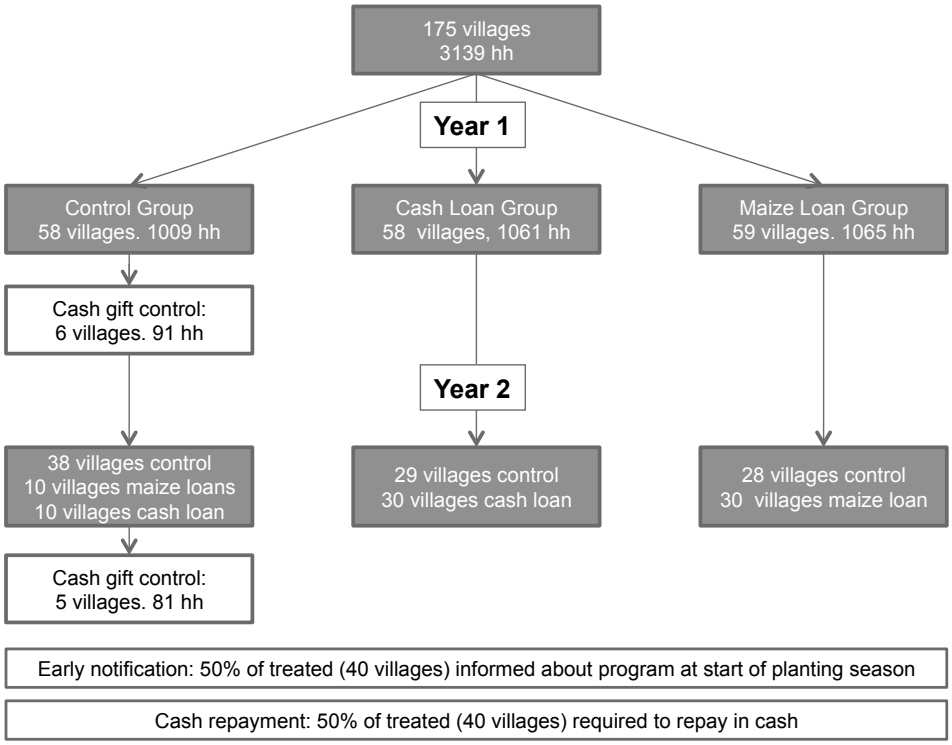


Figure 1: Study design

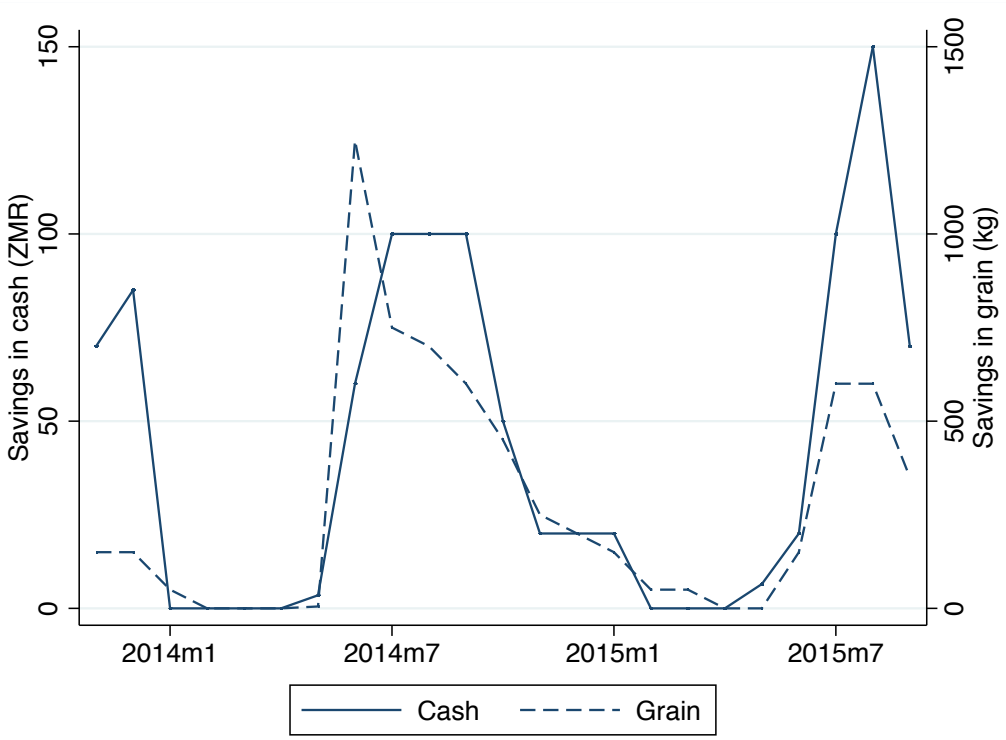


Figure 2: Cash and grain savings (median) in the control group

Notes: Cash savings and the value of stored maize (in Kwacha) held by the household at the time of survey, by month of the year in the control group. The months shown on the x-axis cover our full study, beginning with baseline data collection in November 2013 and ending with endline data collection in September 2015. For additional details on the survey rounds and data collection, see Appendix A.3.

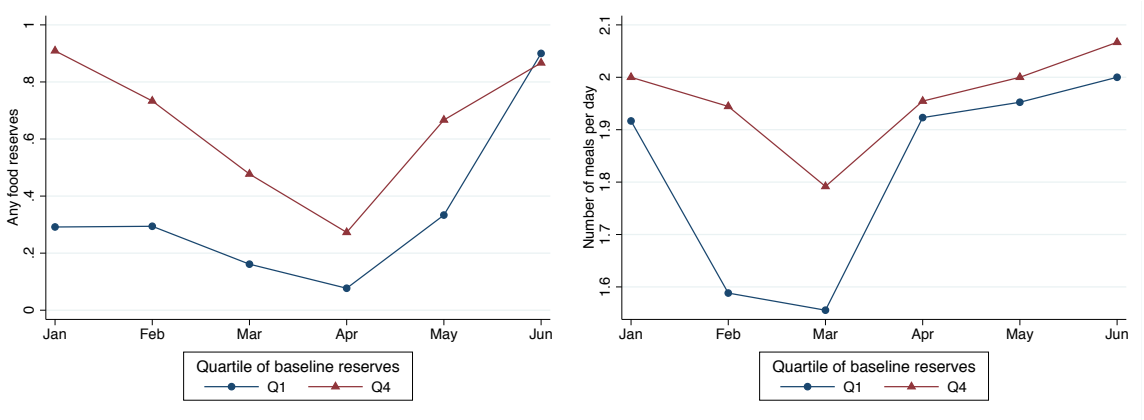


Figure 3: Seasonality in consumption, by baseline cash and grain endowment

Notes: Monthly consumption outcomes, for the top and bottom quartile of the distribution of baseline cash and grain endowments in the control group in year 1. The left graph shows an indicator for any food reserves available in the home and the right graph shows a measure of meals per day consumed by adults within the household.

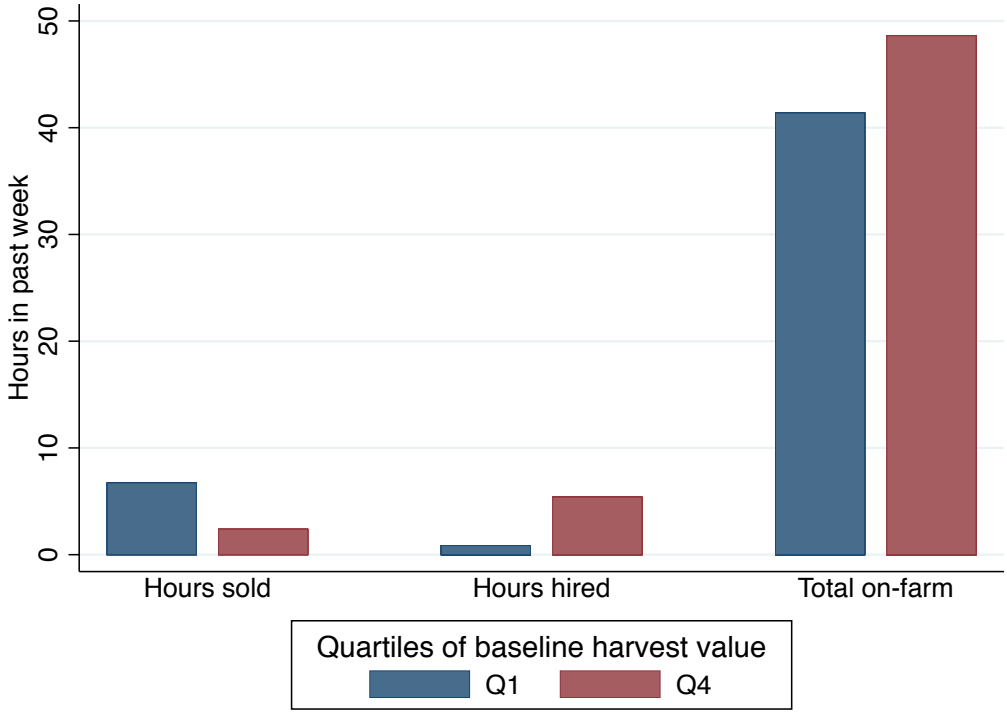


Figure 4: Labor market participation, by baseline cash and grain endowment

Notes: Hours of labor market activity over the past week, during the hungry season, for the control group in year 1. The bars represent mean winsorized (top 1 percent) hours in the top and bottom quartile of the distribution of baseline cash and grain endowments.

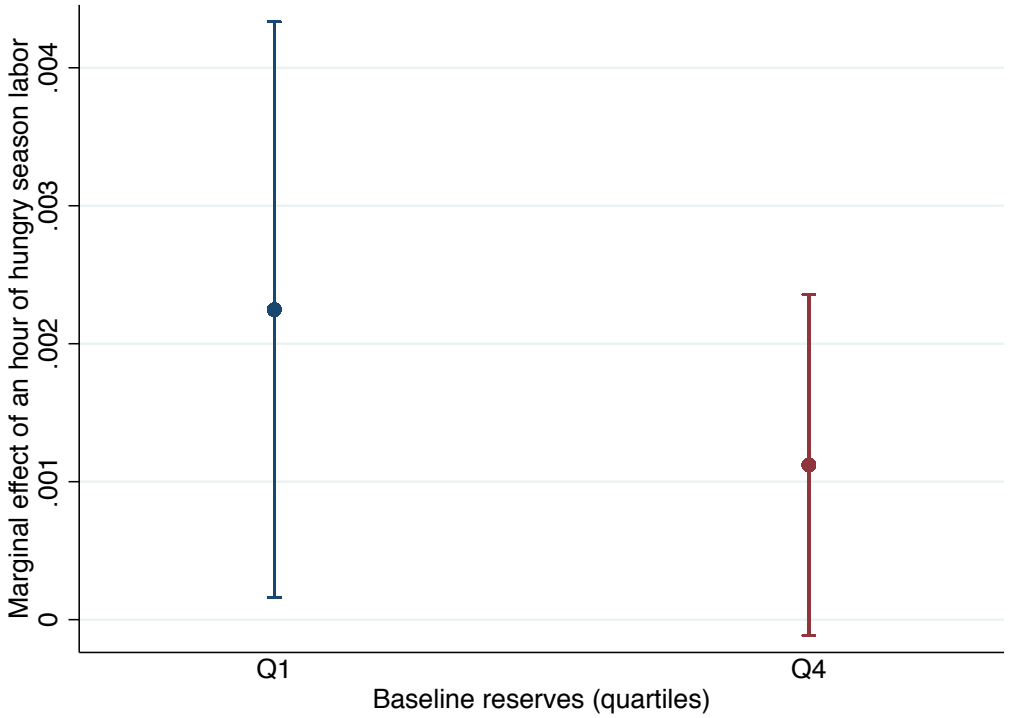


Figure 5: Marginal product of labor, by baseline cash and grain endowment

Notes: Point estimates and 90 percent confidence intervals are from a regression of log agricultural output on days of labor inputs on-farm during the hungry season, for the control group in year 1. Results are the marginal effects of an additional day of hungry season labor input for the top and bottom quartile of the distribution of baseline cash and grain endowments.

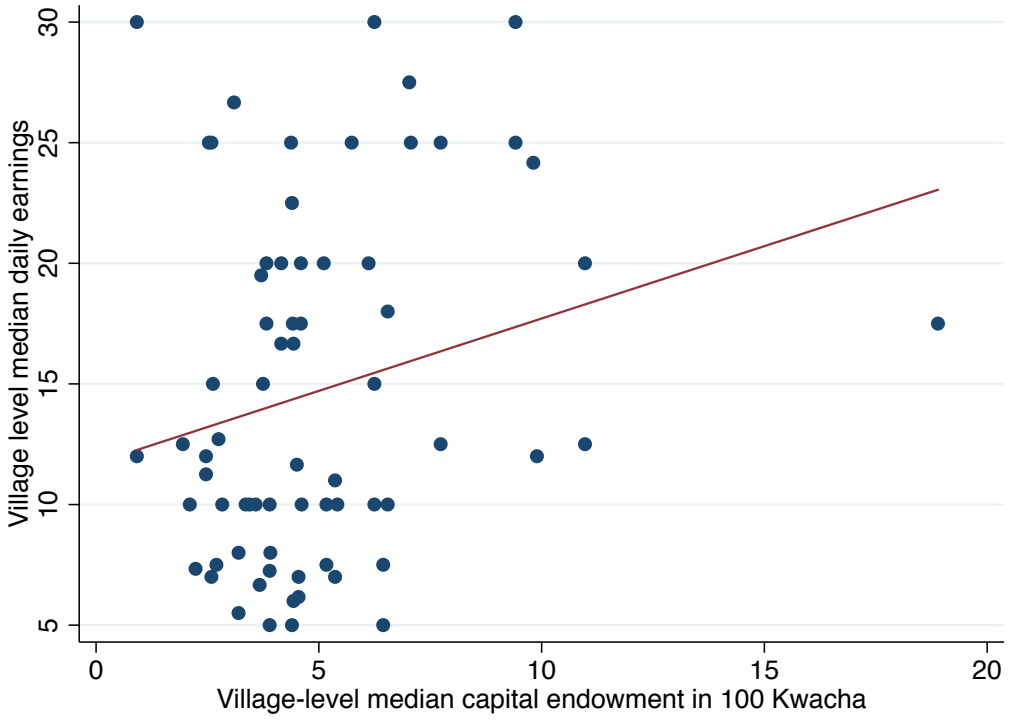


Figure 6: Village average daily earnings, by baseline cash and grain endowment

Notes: Village level median daily earnings in the hungry season, for the control group in year 1, versus the village median baseline value of grain and cash reserves. The slope on the line is 0.60 (s.e. 0.31). If the high outlier in baseline reserves is omitted, the slope is 0.83 (s.e. 0.39).

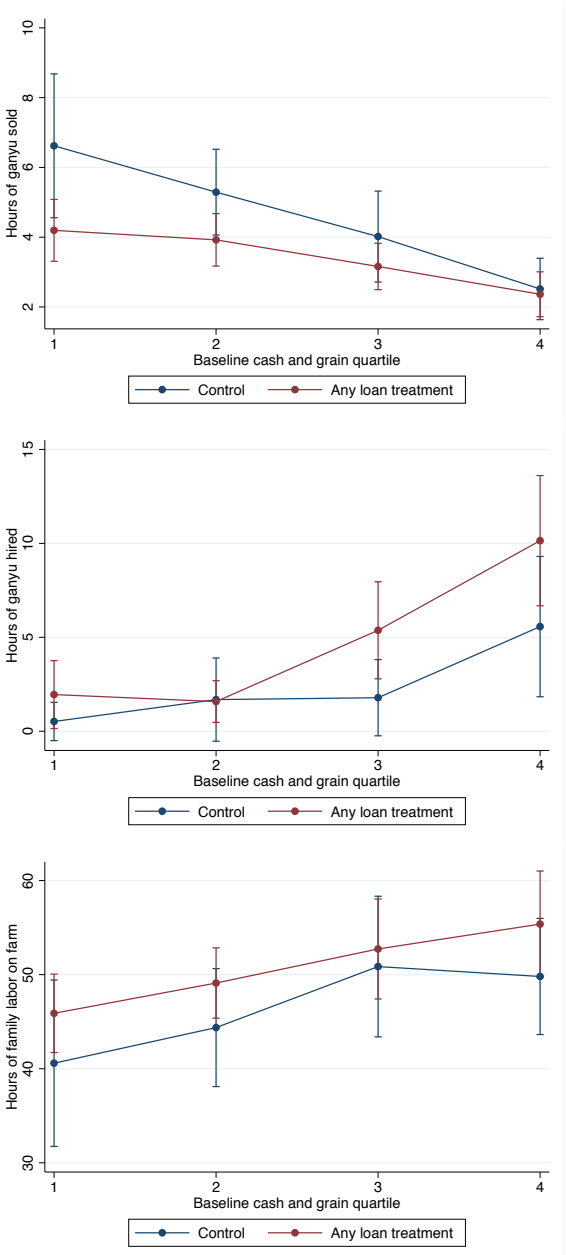


Figure 7: Effect on labor market participation, by baseline endowment

Notes: Heterogeneous impacts on labor market outcomes in the hungry season of year 1, by quartile of baseline reserve. Plots show estimated means, based on regressions that control for geographic variables only. 90 percent confidence intervals are plotted based on standard errors clustered at the village level. The same figures for year 2 of the project are shown in the appendix.

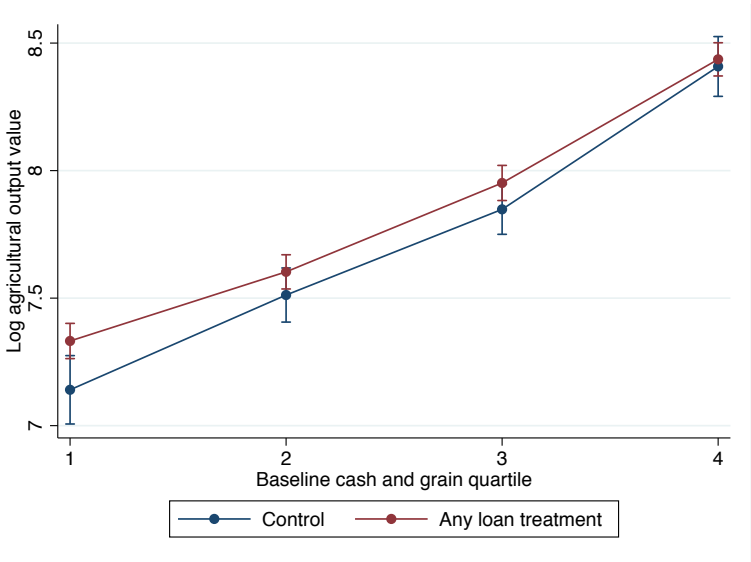


Figure 8: Effect on log agricultural output, by baseline endowment, year 1

Notes: Heterogeneous impacts on agricultural output value in year 1, by quartile of baseline reserve. Plots show estimated means, based on regressions that control for geographic variables only. 90 percent confidence intervals are plotted based on standard errors clustered at the village level. The same figure for year 2 of the project is shown in the appendix.

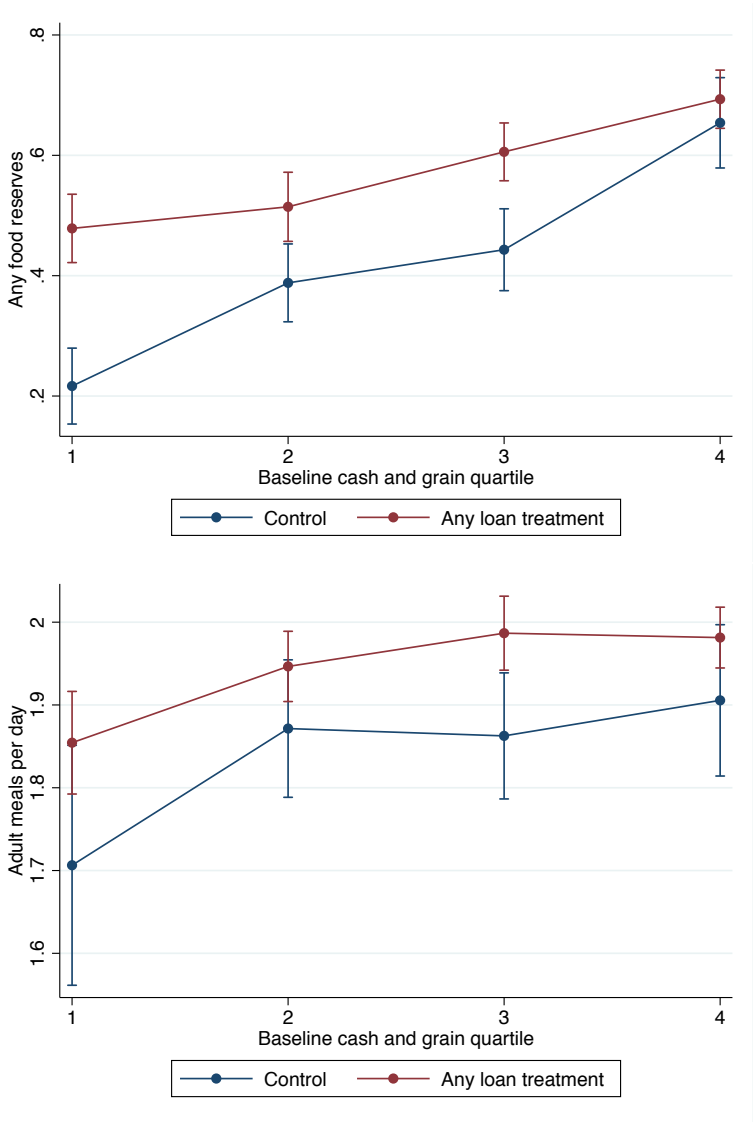


Figure 9: Effect on consumption variables, by baseline endowment, year 1

Notes: Heterogeneous impacts on consumption outcomes in the hungry season of year 1, by quartile of baseline reserve. Plots show estimated means, based on regressions that control for geographic variables only. 90 percent confidence intervals are plotted based on standard errors clustered at the village level. The same figures for year 2 of the project are shown in the appendix.

Table 1: Loan treatments

	Loan (January)	Repayment (July)	Implied interest
		A. Maize Loan	
Offer	3 bags (50 kg ea)	4 bags (50 kg ea)	30%
Value (official)	K 195	K 260	33%
Value (reported)	K 261	K 234	-10%
		B. Cash Loan	
Offer	K 200	K 260	30%

Notes: Columns describe the loan and repayment terms, and the implied interest rate for the maize and cash loan treatment arms. The official value is the government-set maize price. The reported value is the average reported in the harvest survey for buying and selling maize.

Table 2: Take up and repayment

	Take up	Full repayment	% repayment	Repaid any cash
		A. Year 1		
Cash loan mean	0.99	0.93	0.94	0.35
Maize loan	-0.003	0.013	0.015	-0.225***
	(0.007)	(0.020)	(0.018)	(0.035)
		B. Year 2		
Cash loan mean	0.98	0.76	0.78	0.55
Maize loan	-0.012	0.008	0.026	-0.084
	(0.015)	(0.058)	(0.052)	(0.089)
Treated in year 1	0.018	-0.046	-0.062	-0.142
	(0.016)	(0.069)	(0.055)	(0.098)
Any default in village in year 1	-0.025	-0.220***	-0.225***	-0.221**
	(0.026)	(0.068)	(0.066)	(0.099)
Early notification sub-treatment	0.010	-0.019	-0.007	0.012
	(0.016)	(0.058)	(0.052)	(0.090)
Cash repayment sub-treatment	-0.008	-0.049	-0.044	0.624***
	(0.015)	(0.058)	(0.052)	(0.049)

Notes: Table shows take up and repayment statistics in the cash loan treatment in year 1 (panel A) and year 2 (Panel B). Full repayment is a dummary variable that equals one if the loan was fully repaid. The means for the cash loan treatment are shown in the first row of each panel and coefficients on a dummy for the maize loan treatment are in each subsequent row. In year 2, each row corresponds to a separate regression. Standard errors are clustered at the village level.

Table 3: Average treatment effects: Labor

	Any ganyu sold	Hours sold	Any ganyu hired	Hours hired	Family hours on-farm	Total hours on-farm
	(1)	(2)	(3)	(4)	(5)	(6)
A. Pooled treatments						
Any loan treatment	-0.029	-0.898**	0.020	0.996	4.484*	4.407**
	(0.021)	(0.408)	(0.013)	(0.894)	(2.285)	(1.939)
B. By treatment						
Cash	-0.016	-0.736	0.022	1.525	4.514	4.574*
	(0.025)	(0.458)	(0.016)	(1.113)	(2.806)	(2.401)
Maize	-0.042*	-1.066**	0.018	0.429	4.452*	4.234*
	(0.023)	(0.470)	(0.015)	(1.108)	(2.552)	(2.202)
Control group mean	0.303	3.731	0.114	3.714	53.258	51.463
Observations	2824	2823	2827	1637	2826	2827

Notes: Treatment effects on labor outcomes, measured during the short recall surveys during the hungry season. Extensive margin outcomes (columns 1 and 3) indicate whether the household engaged in any of the labor activity over the past week. Other columns show the number of hours allocated to each activity, include zeros and are winsorized at the 99th percentile. All specifications are conditional on month fixed effects and include baseline controls, and cluster standard errors at the village level.

Table 4: Average treatment effects: Daily earnings

	Individual-level daily earnings (top 1% winsorized)	Individual-level daily earnings (top 5% winsorized)	Village median daily earnings
	(1)	(2)	(3)
A. Pooled treatments			
Any loan treatment	2.437*	1.197	1.277
	(1.249)	(0.740)	(0.969)
B. By treatment			
Cash	1.455	0.491	0.948
	(1.546)	(0.896)	(1.216)
Maize	3.399**	1.888**	1.617
	(1.551)	(0.913)	(1.127)
Control group mean	14.772	13.224	13.855
Observations	1079	1079	296

Notes: Treatment effects on reported daily earnings in Kwacha, restricted to the hungry season (Jan-Mar). Column 1 shows the impact on reported earnings using household level data. Column 2 shows the village level mean daily wage. Column 1 winsorizes the top 1 percent, Column 2 winsorizes the top 5 percent. Column 3 uses the individual level winsorized measures in column 2 before averaging at the village-month level. All columns include geographic controls, month-year fixed effects, and cluster standard errors at the village level. Columns 1 and 2 also include household controls and a control for the hours worked per day.

Table 5: Average treatment effects: Agricultural production

	Log harvest value	Log value - constant prices
	(1)	(2)
A. Pooled treatments		
Any loan treatment	0.081**	0.086**
	(0.037)	(0.034)
B. By treatment		
Cash	0.103**	0.114***
	(0.041)	(0.040)
Maize	0.060	0.058
	(0.043)	(0.039)
Control group mean	3187.586	3492.094
Observations	4015	4015

Notes: Treatment effects on agricultural output, measured in log ZMK. All specifications include baseline controls, year effects (panel A), and cluster standard errors at the village level.

Table 6: Heterogeneity by anticipated shortage

	Adult meals	Hours sold	Hours hired	Family hours on-farm	Log ag output
	(1)	(2)	(3)	(4)	(5)
Any loan treatment	-0.010	0.084	2.933	5.380	-0.007
	(0.031)	(0.491)	(2.088)	(3.865)	(0.037)
Anticipated food shortage	-0.110***	0.966**	2.138	-0.604	-0.191***
	(0.031)	(0.467)	(1.450)	(2.957)	(0.023)
Loan x Shortage	0.110***	-0.773	-3.136	-3.495	0.124***
	(0.040)	(0.632)	(2.200)	(4.176)	(0.037)
Control group mean	1.840	4.549	2.428	46.595	3448.630

Notes: Heterogeneous treatment effects on main outcomes, based on whether the household reported at baseline that it would run out of food before the coming harvest. Years 1 and 2 are pooled (restricted to villages newly treated in year 2). All specifications are conditional on month or year fixed effects and include baseline controls, and cluster standard errors at the village level.

Table 7: Average treatment effects: Consumption and food security

	Any grain stocks	Food security (z-score)	Adult meals - Hungry season	Adult meals - Harvest
	(1)	(2)	(3)	(4)
A. Pooled treatments				
Any loan treatment	0.114***	0.241***	0.086***	0.004
	(0.028)	(0.073)	(0.029)	(0.013)
B. By treatment				
Cash	0.005	0.084	0.072**	0.017
	(0.030)	(0.078)	(0.030)	(0.014)
Maize	0.232***	0.397***	0.103***	0.015
	(0.034)	(0.082)	(0.035)	(0.013)
Control group mean	0.530	0.000	1.874	1.993
Observations	2578	1651	1369	603

Notes: Treatment effects on consumption outcomes, measured during the short recall surveys during the hungry season (except column 4, which is measured at harvest). Outcome variables are: an indicator for whether the household had any remaining grain reserves (col 1), an index of food security (col 2), and the number of adult meals per day, where a meal is defined by consumption of the staple food, nshima, in the hungry season (col 3) and at harvest (col 4). All specifications are conditional on month fixed effects and include baseline controls, and cluster standard errors at the village level.

Table 8: Notification timing

	Adult meals	Hours sold	Hours hired	Hours on farm (family)	Log output	Input value	Acres cash crops
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Pooled treatments							
Any loan treatment	0.061*	-0.143	-1.696	3.121	0.054	114.584	0.060
	(0.034)	(0.571)	(1.490)	(5.663)	(0.111)	(187.540)	(0.150)
Loan x Early	0.006	-0.863	2.428	1.602	0.062	137.155	0.219
	(0.037)	(0.597)	(1.944)	(7.451)	(0.124)	(198.867)	(0.160)
B. By treatment							
Cash	0.004	0.559	-1.682	0.906	0.012	72.313	-0.074
	(0.034)	(0.700)	(2.118)	(7.633)	(0.129)	(313.500)	(0.186)
Maize	0.115***	-0.733	-1.618	5.330	0.101	147.407	0.204
	(0.035)	(0.712)	(1.822)	(7.125)	(0.167)	(187.781)	(0.218)
Cash x Early	0.029	-1.467*	1.095	-1.925	0.119	332.362	0.450*
	(0.046)	(0.838)	(2.545)	(12.087)	(0.198)	(359.561)	(0.247)
Maize x Early	-0.009	-0.410	3.674	5.283	0.001	-26.472	-0.008
	(0.050)	(0.778)	(3.093)	(8.525)	(0.181)	(203.939)	(0.221)
Control group mean	1.874	3.731	3.714	53.258	7.707	883.541	0.947
Observations	592	857	859	859	1004	1008	1018

Notes: Impacts of loan notification timing on main outcomes (columns 1-5) and planting season production decisions (columns 6 and 7), in year 2 for newly treated villages only. The reference group (i.e. not-early loan) is the timing used to announce the loans in year 1. Early notification involved informing farmers of the loan program at planting time in September. The timing of the actual loan delivery was the same across the timing sub-treatments (January). The first set of results pool the loan treatment arms; the second set show results by treatment arm. Note that all coefficients represent treatment effects for the relevant subgroup. All specifications include baseline controls, month or year effects, and cluster standard errors at the village level.

Table 9: Persistence and dynamics

	Adult meals	Hours sold	Hours hired	Hours on farm (family)	Log output	Input value	Acres cash crops
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Year 1							
Any loan treatment	0.072**	-1.200**	2.742**	4.876*	0.088**	72.297	0.043
	(0.031)	(0.546)	(1.136)	(2.619)	(0.040)	(53.128)	(0.054)
Control group mean	1.957	4.549	2.428	46.595	3640.440	842.568	0.918
Observations	2171	1966	778	1967	3011	3028	3028
B. Year 2							
Any loan treatment	0.056**	-0.702*	-0.329	4.662	0.088	177.529	0.183
	(0.025)	(0.406)	(1.233)	(4.835)	(0.087)	(141.648)	(0.121)
Treated in year 1	0.036	0.885	-0.322	0.790	0.081	-31.121	0.104
	(0.027)	(0.548)	(1.132)	(3.973)	(0.064)	(92.038)	(0.088)
Loan x Treated in year 1	-0.029	-0.953	-0.421	-6.393	-0.144	-179.041	-0.108
	(0.034)	(0.608)	(1.584)	(5.810)	(0.100)	(154.930)	(0.142)
Control group mean	1.905	2.733	4.319	61.358	3202.031	1370.579	0.963
Observations	2405	1762	1764	1764	2976	2987	3005

Notes: Impacts of loan notification timing on main outcomes (columns 1-5) and planting season production decisions (columns 6 and 7), in year 2. Hungry season notification is the timing used in year 1. Planting season notification involved informing farmers of the loan program at planting time in September. The timing of the actual loan delivery was the same across the timing sub-treatments (January). The first set of results pool the loan treatment arms; the second set show results by treatment arm and notification timing. Note that all coefficients represent treatment effects for the relevant subgroup. All specifications include baseline controls, month or year effects, and cluster standard errors at the village level.

Table 10: Reporting bias

	A. Social desirability bias			
	Labor survey		Endline	
Pooled treatments				
Any loan treatment	-0.041		0.041	
	(0.143)		(0.099)	
By treatment				
Cash		-0.124		-0.024
		(0.156)		(0.123)
Maize		0.043		0.104
		(0.195)		(0.118)
Observations	1387	1387	2992	2992
Control group mean		21.639		20.578

	B. Self-reported maize yields			
	Year 1		Year 2	
Objective measure	0.870***	0.775**	0.051***	0.053***
	(0.316)	(0.384)	(0.011)	(0.009)
Any loan treatment		-31.009		19.513
		(123.080)		(60.638)
Loan treatment x Objective measure		0.150		-0.002
		(0.623)		(0.019)
Share of maize yield from hybrid	292.001***	293.166***	171.017***	171.573***
	(55.952)	(57.899)	(47.567)	(47.920)
Observations	362	362	438	438
Control group mean		563.367		600.645

Notes: Tests for self-reporting bias by treatment. Panel A regresses an index of social desirability bias on treatment, with cross sectional data from two survey rounds: labor survey round 3 (hungry season, immediately after receiving year 2 loans) and endline survey (harvest survey, immediately after repaying year 2 loans). Panel B regresses self reported maize yields on an objective measure of maize productivity, the loan treatment and an interaction of the two, along with a control for the share of the self reported yield that comes from hybrid maize. In year 1 (columns 1 and 2), the objective measure is a measure of maize height during the hungry season. In year 2 (columns 3 and 4), the objective measure is based on the number of maize kernels counted during a systemic on-field sampling. See text for detail. No baseline controls are included in these regressions. Standard errors are clustered at the village level.

A.1 Model Appendix

As outlined in the model section, rational households maximize their utility over consumption and leisure over two periods, i.e.

$$\max_{c,l} \; log(c_{i1} - \underline{c}) + \frac{1-\alpha}{\alpha} log(l_{i1}) + \rho log(c_{i2} - \underline{c}) \tag{A.1}$$

subject to

$$c_{i1} < S_{i0} + (\bar{h} - l_i - d_i)w + B_i$$

$$c_{2i} < f_i + (1 + r + \phi_b)1(B_i > 0) + (1 + r - \phi_s)1(B_i < 0).$$

where B_i is net resources borrowed ($B > 0$ implies borrowing and $B < 0$ implies saving) during the first period. Defining the effective interest rate faced by each farms as $r_e = 1 + r \pm \phi$ we can express the overall l budget constraint faced by households as

$$S_0 + h_i w + \frac{1}{r_e} f - dw - c_1 - \frac{1}{r_e} c_2 \geq 0. \tag{A.2}$$

For any given land distribution, optimal labor input d^* is a direct function of productivity, wages and interest rates given by

$$d_i^* = k \left(\frac{\beta A}{w r_e} \right)^{\frac{1}{1-\beta}}. \tag{A.3}$$

This directly defines optimal output $y(d^*(A, w, r_e))$, so that we can define net wealth $S* = S_0 + \frac{1}{r} y(d^*) - d^* w$ as the sum of initial endowment and expected profit. The overall budget constraint can then be simplified to

$$S^* + h_i w - c_1 - \frac{1}{r_e} c_2 \geq 0. \tag{A.4}$$

The three first order conditions with respect to first and second period consumption and first period labor are given by

$$\frac{1}{c_1 - \underline{c}} - \lambda = 0 \tag{A.5}$$

$$\rho \frac{1}{c_2 - \underline{c}} - \lambda \frac{1}{r_e} = 0 \tag{A.6}$$

$$-1 \frac{1-\alpha}{\alpha} \frac{1}{\bar{h} - h_i} + \lambda w = 0 \tag{A.7}$$

Re-arranging this last equation to express λas a function of h, we get

$$\lambda = \frac{1-\alpha}{\alpha w}\frac{1}{\bar{h}-h_i} \tag{A.8}$$

which allows us to express the first two optimality conditions as

$$\frac{1}{c_1-\underline{c}} = \lambda = \frac{1-\alpha}{\alpha w}\frac{1}{\bar{h}-h_i} \tag{A.9}$$

which yields

$$c_1 = \frac{\alpha w}{1-\alpha}(\bar{h}-h_i)+\underline{c} \tag{A.10}$$

and similarly

$$c_2 = \rho r_e \frac{\alpha w}{1-\alpha}(\bar{h}-h_i)+\underline{c} \tag{A.11}$$

Plugging this into the simplified budget constraint yields

$$S* + h_i w - [\frac{\alpha w}{1-\alpha}(\bar{h}-h_i)+\underline{c}] - \frac{1}{r_e}[\rho r_e \frac{\alpha w}{1-\alpha}(\bar{h}-h_i)+\underline{c}] = 0 \tag{A.12}$$

Which can be rearranged to yield

$$h_i = \frac{\frac{\alpha}{1-\alpha}(1+\rho)\bar{h} + \frac{1}{w}\left[\underline{c}(1+\frac{1}{r_e}) - S*\right]}{(1+\frac{\alpha}{1-\alpha}+\rho\frac{\alpha}{1-\alpha})} \tag{A.13}$$

Optimal first period consumption is given by

$$c_1 = \frac{\underline{c}(1-\alpha)+\underline{c}(-\alpha r_e + r_e\rho(\alpha-1)) + r_e S^*(\alpha-1) + \overline{h} r_e w(\alpha-1)}{-r_e + r_e\rho(\alpha-1)} \tag{A.14}$$

which can be simplified to

$$c_1 = \frac{\underline{c}(\frac{1}{r_e}+\frac{\alpha}{1-\alpha}+\rho) + S^* + \overline{h}w}{\frac{1}{1-\alpha}+\rho} \tag{A.15}$$

Similarly, second period consumption is given by

$$c_2 = \frac{\underline{c}(-1+r_e\rho(1-\alpha)) + \rho r_e S^*(\alpha-1) + \overline{h}\rho r_e w(\alpha-1)}{-1+\rho(\alpha-1)} \tag{A.16}$$

which can be simplified to

$$c_2 = \frac{\underline{c}(\frac{1}{1-\alpha}-r_e\rho) + \rho r_e S^* + \overline{h}\rho r_e w}{\frac{1}{1-\alpha}+\rho} \tag{A.17}$$

assuming $\alpha = 0.5$, and normalizing household labor to 1, this expression simplifies to

$$h_i = \frac{1 + \rho + \frac{1}{w}\left[\bar{c}(1 + \frac{1}{r_e}) - S*\right]}{(2 + \rho)} \tag{A.18}$$

A.2 Appendix tables and figures

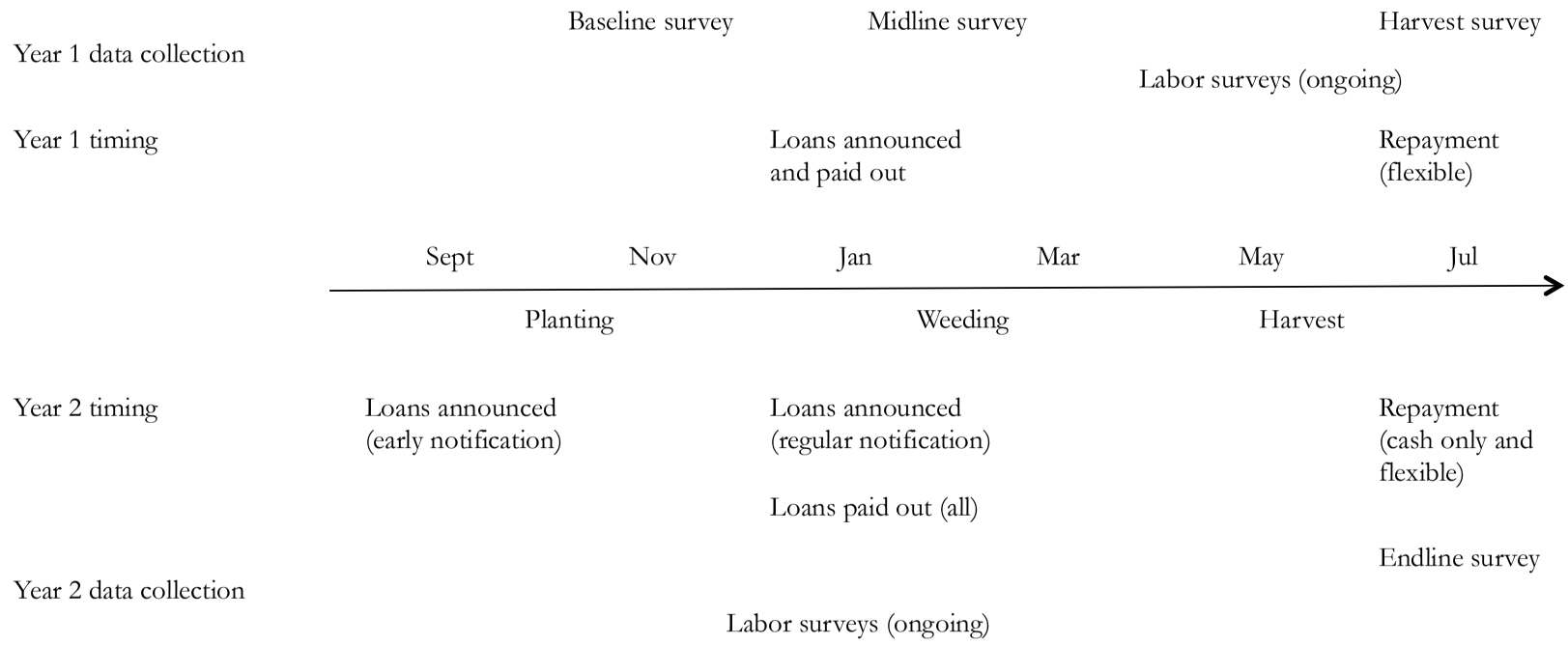


Figure A.1: Study implementation

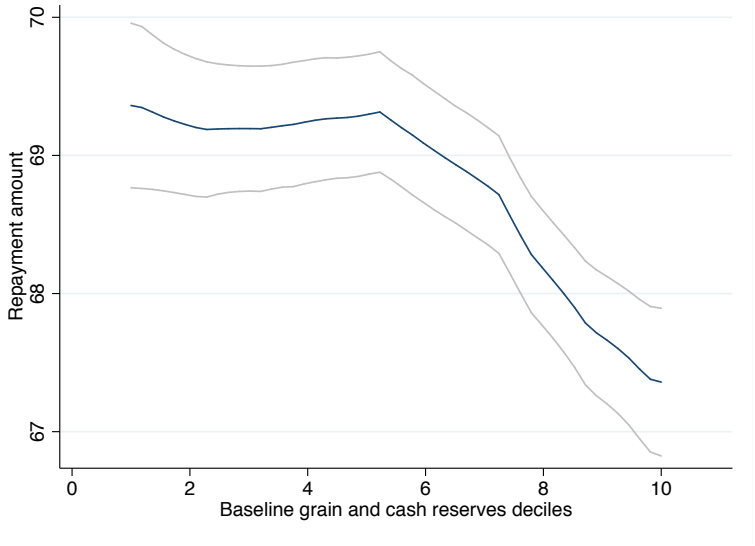


Figure A.2: Interest rates by baseline grain and cash reserve deciles

Notes: Responses to survey question asking how much respondent would have to repay in a month for 50 Kwacha borrowed today from a source other than friends or family. The sample is restricted to the control group and the confidence intervals are from a local polynomial smoothing.

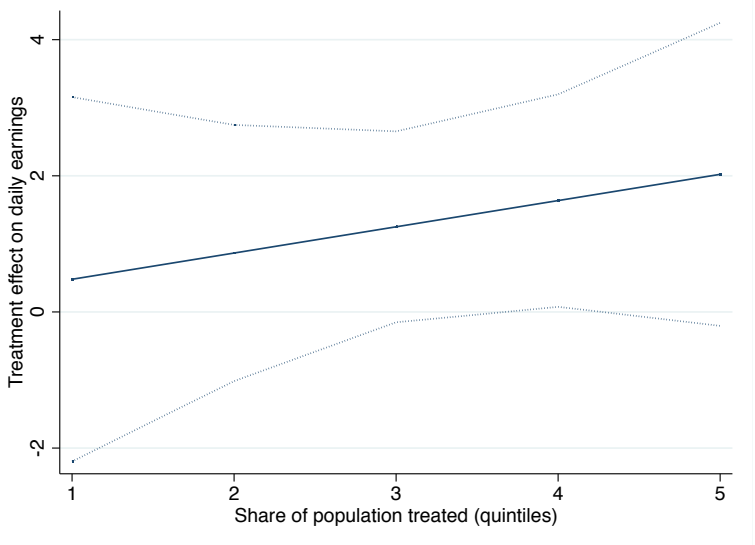


Figure A.3: Treatment effect on daily earnings, by share of village treated

Notes: Village level median daily reported earnings during the hungry season, in villages treated for the first time (pooled across years) relative to the control group. Regressions control for geographic variables, including distance to the nearest paved road. Figure shows 90 percent confidence intervals based on standard errors clustered at the village level.

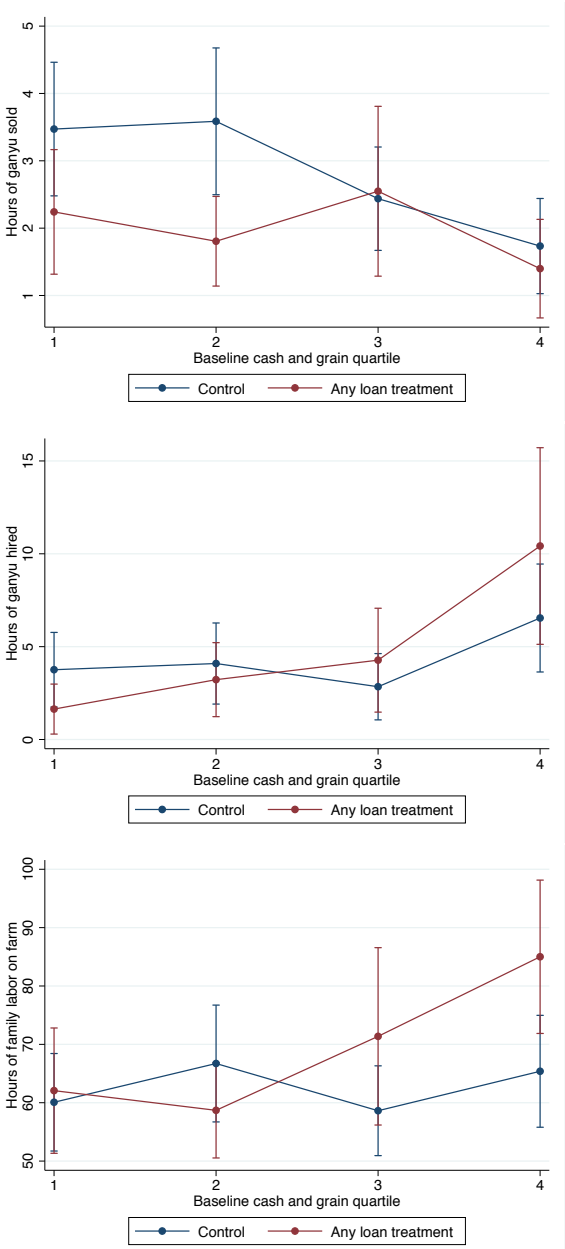


Figure A.4: Effect on labor market participation, by baseline endowment, year 2

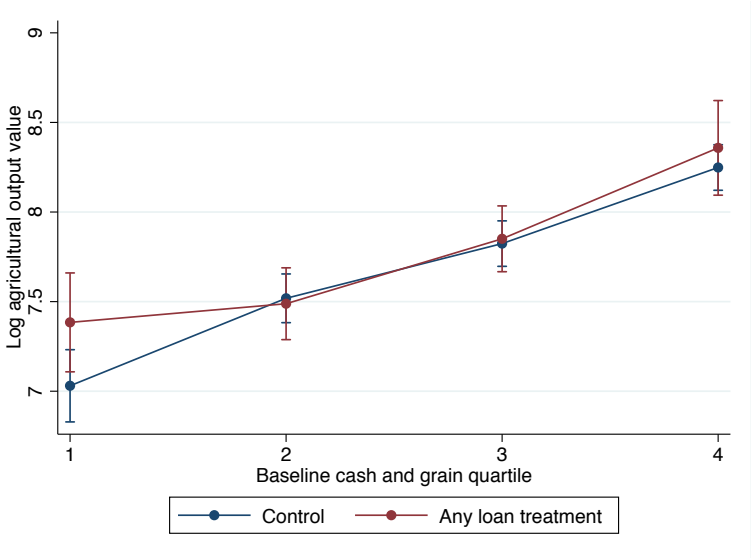


Figure A.5: Effect on log agricultural output, by baseline endowment, year 2

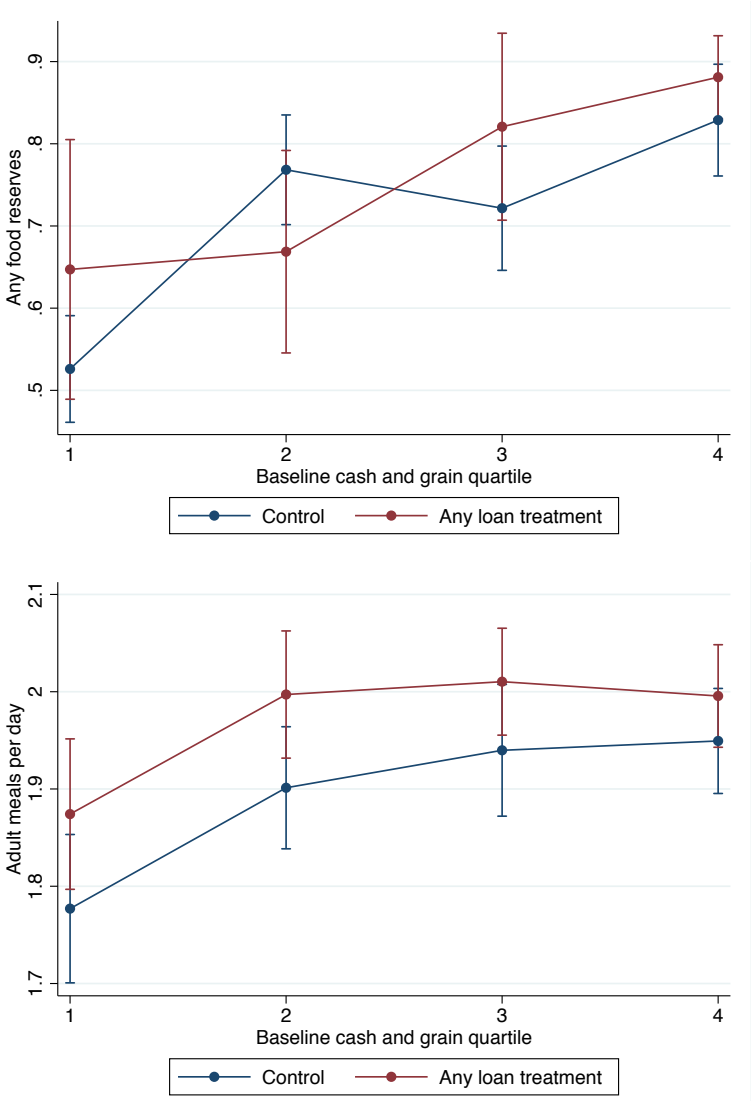


Figure A.6: Treatment effect on consumption variables, by baseline endowment, year 2

Table A.1: Randomization balance

	Year 1			Year 2			Largest pairwise:	
	Control	Cash loan	Maize loan	Control	Cash loan	Maize loan	t-statistic	normalized difference
Age of HH head	42.785	42.875	42.439	42.623	42.994	42.574	0.783	0.026
	[14.990]	[15.280]	[14.960]	[15.335]	[14.669]	[14.901]		
Imputed the age of the household he	0.004	0.002	0.004	0.003	0.001	0.005	1.331	0.049
	[0.063]	[0.043]	[0.061]	[0.055]	[0.037]	[0.073]		
Female HH head	0.243	0.271	0.254	0.255	0.261	0.255	1.459	0.045
	[0.429]	[0.445]	[0.436]	[0.436]	[0.440]	[0.436]		
HH members < 5	0.925	0.975	0.928	0.922	0.941	0.989	1.609	0.051
	[0.907]	[0.940]	[0.940]	[0.910]	[0.941]	[0.959]		
HH member 5-14	1.775	1.72	1.758	1.721	1.747	1.82	1.474	0.046
	[1.530]	[1.545]	[1.492]	[1.506]	[1.549]	[1.531]		
HH members 15-64	2.473	2.424	2.41	2.429	2.478	2.408	1.137	0.038
	[1.259]	[1.325]	[1.264]	[1.279]	[1.311]	[1.267]		
HH members 65+	0.172	0.187	0.173	0.183	0.167	0.174	0.897	0.03
	[0.441]	[0.473]	[0.457]	[0.457]	[0.459]	[0.457]		
Household asset quintile	3.057	3.013	2.934	2.984	3.038	3.001	1.991	0.062
	[1.392]	[1.426]	[1.422]	[1.395]	[1.443]	[1.428]		
Baseline livestock value	3586.024	3304.445	3423.568	3250.009	3479.293	3805.57	1.921	0.06
	[6220.081]	[6023.122]	[6590.769]	[6326.978]	[5759.958]	[6657.904]		
HH did ganyu last year	0.614	0.636	0.611	0.617	0.634	0.616	1.19	0.036
	[0.487]	[0.481]	[0.488]	[0.486]	[0.482]	[0.487]		
Hired ganyu last season	0.315	0.337	0.313	0.312	0.333	0.33	1.356	0.038
	[0.465]	[0.473]	[0.464]	[0.464]	[0.472]	[0.471]		
HH plans to do ganyu this year	0.622	0.64	0.634	0.631	0.642	0.624	0.856	0.029
	[0.485]	[0.480]	[0.482]	[0.483]	[0.480]	[0.485]		
# of adults working on farm	2.687	2.688	2.646	2.671	2.717	2.635	1.187	0.044
	[1.356]	[1.361]	[1.347]	[1.377]	[1.343]	[1.315]		
# of adults working in other IGA	1.126	1.132	1.181	1.137	1.148	1.165	1.264	0.039
	[1.000]	[0.965]	[0.982]	[0.983]	[0.981]	[0.983]		
Acres of maize	2.368	2.3	2.299	2.342	2.246	2.351	1.816	0.062
	[1.411]	[1.294]	[1.403]	[1.332]	[1.368]	[1.449]		
Acres of cash crops	0.967	1.04	1.099	1.06	0.949	1.069	2.809	0.094
	[1.080]	[1.168]	[1.181]	[1.162]	[1.064]	[1.185]		
Baseline harvest value	3096.843	3037.518	3032.011	3092.466	3031.347	2993.88	0.849	0.027
	[2811.876]	[2607.473]	[2765.994]	[2712.900]	[2888.188]	[2597.123]		
Crop diversity index	2.978	3.042	3.011	3.05	2.955	2.98	2.071	0.064
	[1.120]	[1.018]	[1.084]	[1.109]	[1.001]	[1.063]		
Input value	533.167	494.109	472.324	482.011	556.276	481.579	1.603	0.05
	[915.210]	[1365.201]	[807.085]	[808.145]	[1652.910]	[759.373]		

Notes: Baseline means for each major treatment group by year with standard deviations in brackets. The final two columns show the largest t-stat for a pairwise test of equal means and the largest pairwise normalized difference.

Table A.2: Attrition, by survey round

Year 1 treatments								
	Baseline	Y1 Harvest	Y2 Endline	Midline	Labor 1	Labor 2	Labor 3	Labor 4
Sampled	3139	3139	3139	1223	1531	1679	1673	827
Cash loan treatment	0.34	0.009	-0.009	-0.008	-0.014	-0.003	0.029	0.010
		(0.007)	(0.008)	(0.010)	(0.021)	(0.025)	(0.022)	(0.021)
Maize loan treatment	0.34	-0.005	0.008	0.000	-0.008	-0.022	-0.011	-0.055***
		(0.008)	(0.008)	(0.010)	(0.024)	(0.028)	(0.028)	(0.020)
Year 2 treatments								
	Baseline	Y1 Harvest	Y2 Endline	Midline	Labor 1	Labor 2	Labor 3	Labor 4
Cash loan treatment	0.23	-0.000	-0.012	-0.010	-0.003	0.049*	0.026	0.003
		(0.008)	(0.009)	(0.013)	(0.025)	(0.026)	(0.024)	(0.021)
Maize loan treatment	0.24	-0.008	0.010	-0.001	0.015	-0.059	-0.013	-0.015
		(0.008)	(0.008)	(0.010)	(0.029)	(0.036)	(0.036)	(0.023)
Conditional on treatment								
Early notification sub-treatment	0.50	0.012	-0.025**	0.007	0.003	-0.048	0.008	-0.013
		(0.010)	(0.010)	(0.015)	(0.035)	(0.043)	(0.040)	(0.028)
Cash repayment sub-treatment	0.51	-0.003	0.002	-0.000	-0.017	-0.091**	-0.037	-0.033
		(0.010)	(0.011)	(0.015)	(0.035)	(0.041)	(0.040)	(0.027)

Notes: Column 1 shows the share of sampled baseline households by treatment arm in Year 1 (Panel A) and Year 2 (Panel B). Each subsequent column reports the number of sampled households and the regression of whether the household was in the survey round on treatment indicators, conditional on being sampled for the survey. Standard errors are clustered at the village level.

Table A.3: Attrition, by participation stage

Year 1		Invited	At meeting	Eligible	Take up
Cash loan treatment	N	1023	1023	1023	1009
	Share		1	1	0.99
Maize loan treatment	N	1019	1019	1016	999
	Share		1	1	0.98
Year 2		**Invited**	**At meeting**	**Eligible**	**Take up**
Cash loan treatment					
Pooled	N	701	660	658	643
	Share		0.94	1	0.98
Notification timing sub-treatment					
Standard notification	N	356	328	328	319
	Share		0.92	1	0.97
Early notification	N	345	332	330	324
	Share		0.96	0.99	0.98
Cash repayment sub-treatment					
Standard repayment	N	336	320	319	311
	Share		0.95	1	0.97
Cash only repayment	N	365	340	339	332
	Share		0.93	1	0.98
Maize loan treatment					
Pooled	N	718	663	662	639
	Share		0.92	1	0.97
Notification timing sub-treatment					
Standard notification	N	351	327	327	314
	Share		0.93	1	0.96
Early notification	N	367	336	335	325
	Share		0.92	1	0.97
Cash repayment sub-treatment					
Standard repayment	N	365	333	332	324
	Share		0.91	1	0.98
Cash only repayment	N	353	330	330	315
	Share		0.93	1	0.95

Notes: Table reports stages of household self-selection into eligibility. To be eligible, households had to attend the meeting (before learning treatment status) and hand in a consent form (after learning treatment status).

Table A.4: Capital inputs and cash crops

	Seed	Fertilizer	Chemicals	Total input value	Acres cash crops
	(1)	(2)	(3)	(4)	(5)
			A. Both years		
Pooled treatments					
Any loan treatment	-39.443***	-10.754	22.251	-33.617	0.052
	(13.929)	(45.412)	(25.428)	(64.577)	(0.049)
By treatment					
Cash	-41.197**	4.171	-1.083	-41.366	0.065
	(15.835)	(54.740)	(10.056)	(72.975)	(0.056)
Maize	-37.695**	-25.587	45.519	-25.914	0.038
	(15.180)	(50.598)	(49.763)	(82.513)	(0.059)
Control group mean	137.976	812.084	58.479	1051.607	1.005
Observations	4030	4034	4026	4036	4046

Notes: Treatment effects on input value (col 1-4) and on acres devoted to cash crops (col 5). All regressions cluster standard errors at the village level. Outcomes in columns 1-4 are self-reported expenditures (or value when received on credit) in ZMK over the past year. Column 5 includes acres under cotton, tobacco and soya.

Table A.5: Average treatment effects: Health outcomes (year 2)

	Any illness	Sought treatment	Mental health problem
	(1)	(2)	(3)
A. Pooled treatments			
Any loan treatment	-0.071	-0.002	-0.027*
	(0.045)	(0.040)	(0.016)
B. By treatment			
Cash	-0.102	-0.065	-0.030*
	(0.061)	(0.049)	(0.017)
Maize	-0.037	0.063	-0.024
	(0.054)	(0.049)	(0.025)
Control group mean	0.782	0.916	0.040
Observations	462	348	462

Notes: Treatment effects on self-reported health outcomes in year 2, measured during the short recall surveys during the hungry season, with a 2-week recall window in columns 1 and 2. Outcome variables are: an indicator for whether the household had any illnesses in the past two weeks (col 1), whether the household sought treatment conditional on an illness (col 2), and in indicator for whether the respondent scored above a threshold for mental health problems on a 19 point scale (col 3). All specifications are conditional on month fixed effects and include baseline controls, and cluster standard errors at the village level.

Table A.6: Other consumption smoothing

	Input loan	Low interest informal loan	High interest informal loan	Sold asset	Sold livestock
	(1)	(2)	(3)	(4)	(5)
A. Pooled treatments					
Any loan treatment	-0.011	-0.009	-0.026***	0.000	0.035*
	(0.021)	(0.007)	(0.007)	(0.011)	(0.018)
B. By treatment					
Cash	-0.004	-0.004	-0.017**	-0.005	0.028
	(0.024)	(0.008)	(0.008)	(0.011)	(0.021)
Maize	-0.018	-0.013	-0.034***	0.005	0.042*
	(0.025)	(0.008)	(0.007)	(0.013)	(0.022)
Control group mean	0.438	0.072	0.065	0.072	0.316
Observations	4045	4046	4046	4045	4046

Notes: Treatment effects on other consumption smoothing strategies, measured during the long recall surveys at year 1 and 2 harvest (omitting villages treated in year 1 in the year 2 data). Outcome variables are: an indicator for whether the household accessed a formal loan including in-kind inputs (col 1), or either low interest or high interest informal loan (col 2 and 3), or sold any assets or livestock. All specifications include baseline controls, geographic controls and year fixed effects, and cluster standard errors at the village level.

Table A.7: Grain prices

	Purchase price	Sales price	Any purchase	Any sale
	(1)	(2)	(3)	(4)
Pooled treatments				
Any loan treatment	0.011	-0.017	0.003	-0.002
	(0.064)	(0.032)	(0.011)	(0.011)
By treatment				
Cash	0.078	-0.028	0.008	-0.006
	(0.080)	(0.051)	(0.014)	(0.012)
Maize	-0.081	-0.007	-0.002	0.002
	(0.078)	(0.028)	(0.014)	(0.015)
Control group mean	1.035	0.977	0.048	0.061
Observations	131	110	2444	2444
By year 1 treatment status				
Any loan treatment	0.033	-0.012	-0.003	0.003
	(0.076)	(0.034)	(0.015)	(0.015)
Treated in year 1	-0.062	-0.025	0.019	0.011
	(0.078)	(0.040)	(0.013)	(0.013)
Loan x Treated in year 1	-0.045	-0.024	0.008	-0.012
	(0.111)	(0.096)	(0.020)	(0.016)
Control group mean	1.175	0.926	0.068	0.054
Observations	131	110	2444	2444

Notes: Treatment effects on maize market outcomes, measured during the short recall surveys in year 2 only (January to June). Panel A shows effects pooled across years, first for the pooled treatment effect, then by treatment arm, and Panels B and C show results by year. Outcome variables are: the price paid per kilogram for maize purchased (col 1) and sold (col 2), and indicators for whether the household made any purchase (col 3) or sale (col 4). All specifications are conditional on month fixed effects and include baseline controls, and cluster standard errors at the village level.

Table A.8: Spending on temptation and luxury goods

	Clothing	Beer	Tobacco	Sweets	Tea	Meat	Airtime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pooled treatments							
Any loan treatment	0.038*	0.017	-0.012	0.040**	0.013	0.022	0.002
	(0.022)	(0.018)	(0.018)	(0.020)	(0.022)	(0.019)	(0.023)
By treatment							
Cash	0.040	0.030	-0.010	0.039*	0.014	0.022	0.004
	(0.026)	(0.021)	(0.020)	(0.023)	(0.025)	(0.022)	(0.025)
Maize	0.037	0.003	-0.013	0.041*	0.012	0.021	0.000
	(0.024)	(0.021)	(0.020)	(0.023)	(0.026)	(0.022)	(0.027)
Control group mean	0.509	0.196	0.190	0.493	0.432	0.740	0.510
Observations	3937	3937	3937	3937	3937	3937	3937

Notes: Treatment effects on indicators of temptation spending over the past two weeks, measured during during the hungry season and at harvest in year 1 only. All columns are conditional on month fixed effects and include baseline controls, and cluster standard errors at the village level.

Table A.9: Income effects and repayment flexibility

	Adult meals	Hours sold	Hours hired	Family hours on-farm	Log ag output
	(1)	(2)	(3)	(4)	(5)
Cash grant	0.029	-0.035	-1.035	-3.726	-0.039
	(0.117)	(1.731)	(1.573)	(6.529)	(0.085)
Observations	250	648	250	648	1597

Notes: Impact of a small (60 Kwacha) cash grant given to 5-6 villages, relative to the pure control group, pooling across both years of the program. Year 2 results exclude villages treated in year 1. All specifications are conditional on month or year fixed effects and include baseline controls, and cluster standard errors at the village level.

A.3 Survey descriptions

1. Baseline survey (November-December 2013, N=3139): Survey of up to 22 households per village, conducted with household heads. The baseline survey includes sections on household demographics (including individual roster, employment roster of working household members, general household information about assets owned and food insecurity faced, farming information for 2012-2013 season, expected farming activity for 2013-2014 season, risk and time preferences).

2. Labor surveys (January 2014-August 2015, N=4679): Rolling survey of ˜70 households per week (7 of the baseline households in 2 villages per day). The list of baseline households for each village were randomized and the first ˜7 households interviewed, in cases where a household can't be interviewed (temporarily busy, moved, etc.), the household is skipped and the next household on list visited. Survey asks one week and one to two day recall questions on household labor allocation, ganyu earnings, and consumption. Four rounds of labor surveys were conducted (a new round started once all villages were visited). The third round coincided with the hungry season in year 2 and serves as a midline survey.

3. Employer survey (January 2014-August 2015): Rolling survey of ˜10 ganyu employers per week. Sampling is based on Labor survey records of where households in a village report doing ganyu. Additional sampling is done in a snowball method where employers interviewed then provide names of other employers of ganyu that they know. The employer survey tracks the labor survey by geographic block and rotates through villages rather than targeting an explicit sample.

4. Midline maize assessment (February-March 2014, N=380): On-field assessments of maize height (measurement) and visual records (photographs) for a sample of 380 households in 64 villages. Only households with their nearest field within a 30 minute walk were eligible.

5. Midline survey (February-March 2014, N=1193): Hungry season survey of 1200 randomly selected households, stratified on treatment. One week and one month recall questions on labor supply, ganyu earnings, consumption, basic strength and anthropometric measurement.

6. Harvest survey, year 1 (July-September 2014, N=3028): Survey of all baseline households. Includes sections on changes to household composition, shocks experienced by the household, agricultural productivity. Includes anthropometric measures for adults and children.

7. Endline survey, year 2 (July-September 2015, N=3005): Survey of all baseline households. Similar structure to harvest survey.

C.1 Choice experiments

Hypothetical choice experiments were conducted on a convenience sample of participants in November and December 2013. In the initial wave of questions, 72 respondents were interviewed, one-third of which were female. The surveys took place in villages in and around the study area, but not eligible for the study either because they were too large (>100 households) or they had participated in the pilot program. Respondents were approached by an enumerator who explained the exercise, emphasizing that the offers were hypothetical and that responses would not affect any future programs they might be offered. In spite of these disclaimers, which were intended to minimize strategic responses and avoid building expectations, respondents took the decision tasks seriously.

Six scenarios were presented to respondents, involving different dichotomous choices that varied a relevant parameter of the loan offer. The ordering of the parameter set were varied across respondents.

Scenario 1: Maize loan versus cash loan

Script: *Suppose that we had two loans available that would start in January. The first would offer three (3) bags of [50 kg maize] in January that you have to repay in June. The second would offer cash that you would have to repay in June. Please take your time to make your choice, as I will be going through different categories. Would you prefer a cash loan that paid _ _ _ _ Kwacha that you would pay back in June or would you prefer the [maize] loan that you would pay back in June?*

Parameters: 50, 110, 150, 175, 250, 275, 350, 375, 425, 450, 600 Kwacha

Scenario 2: Cash repayment

Script: *Now, supposed the loan changed so that you could still receive three (3) bags of [mealie meal / maize] in January. But instead of repaying in maize in June, you had to repay in cash. I'm going to go through some different repayment amounts. You should tell me whether you would choose to take up a loan that gave you [maize] in January and had to repay that amount of cash in June. Would you be willing to take up a loan that gave you 3 bags of [maize] in January and required that you repay _ _ _ Kwacha in June?*

Parameters: 600, 450, 400, 325, 275, 250, 200, 175, 125, 100, 75, 50 Kwacha

Scenario 3: Cash gift vs. maize loan

Script: *Again, suppose, we were to offer a loan that offered three (3) bags of [maize] in January that you had to repay in June. Would you prefer to take that loan or would you prefer to receive _ _ _ _ Kwacha in January, which you would not require to pay back?*

Parameters: 10, 30, 60, 80, 100, 110, 130, 150, 175, 200, 250 Kwacha

Scenario 4: Cash gift vs. cash loan

Script: *Suppose now that the loan was cash instead and we were to offer a loan that provided 200 Kwacha in January that you had to repay in June without any interest (repay 200 Kwacha in June). Would you prefer to take that loan or would you prefer to receive _____ Kwacha in January which you would not require to pay back.*

Parameters: 10, 30, 60, 80, 100, 110, 130, 150, 175, 200, 250 Kwacha

Scenario 5: Maize loan repayment month

Script: *Suppose, we were to offer a loan that offered three (3) bags of [maize] in January that required you to repay four (4) bags. I'd like you to think about whether you would choose to take that loan. I will list different months when the repayment would be due. Would you be willing to take a loan of three bags of [mealie meal / maize] in June that required you repay 4 bags if the repayment were due in _____?*

Parameters: February, March, April, May, June, July, August, September, October, November, December

Scenario 6: Cash loan repayment month

Script: *Again, let's look at this activity but considering a loan in cash instead of maize: Suppose, we were to offer a loan that offered 200 Kwacha in cash in January that required you to repay 330 Kwacha in cash. Would you be willing to take that loan for 200 Kwacha in cash that repaid 265 Kwacha if the repayment were due in _____?*

Parameters: February, March, April, May, June, July, August, September, October, November, December