Credit and Welfare Across the Lean Season

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Abstract

Consumption in rural areas of low-income countries is often highly variable across seasons. What drives this seasonality, and can the welfare of households across the "lean season" be improved via the provision of credit? We measure prices and consumption for farm-households across seasons in Gombe, Nigeria, and at the same time elicit information about farmers' intertemporal marginal rates of substitution by offering them one-month bonds with different rates of return. Against this background, we also implement a randomized post-harvest loan (PHL) program, which provides credit—up to a generous ceiling—at a subsidized interest rate. Farmers randomly offered the loan almost universally borrow the maximum amount and, on average, store more grain. However, this is a risky investment, and in the year of our experiment it did not pay off, as maize prices did not increase following the harvest. Given this, it is unsurprising that we find no significant effects of the loan on consumption, investment or welfare—using the PHL to make a leveraged bet on maize prices going up was a bad investment *ex post*. Was it a bad investment *ex ante*? This depends on whether lean seasons are due to poorly functioning financial markets in Gombe, or because markets in Gombe are poorly integrated with the broader market. We adapt tools from the asset pricing literature to our data to test the null hypothesis of well-functioning local financial markets in Gombe. We fail to reject this null, suggesting that promoting spatial integration may improve lean-season welfare more than the local provision of credit would.

1 Introduction

Food consumption is often highly seasonal for poor farmers in developing countries. A root cause of this is the seasonality of the crop calendar—harvests happen at particular time of year, so farm income is seasonal. And since the seasonality of the crop calendar is more or less the same for other nearby farmers, there is seasonality in local supply. Then, *if* local markets are poorly integrated with the broader economy, then local prices will tend to fall at harvest, and rise later in the year, exacerbating the smoothing problem. In particular, farmers sell the bulk of their harvests during peak seasons and buy back those crops later during the subsequent hungry seasons after depleting their stocks—or in the words of Burke et al. (2019) farmers "sell low and buy high" in equilibrium.

There is ample evidence that in fact seasonal variation in food prices leads to variability in consumption that affects not only short-term welfare, but may also negatively affect long-run outcomes such as health and human capital formation (Christian & Dillon, 2018). However, we need to both quantify these welfare costs and identify their root causes in order to concretely consider the benefits and costs of programs to help households smooth their consumption. The former is challenging both because of the scarcity of high-frequency consumption data within the lean-season (Merfeld & Morduch, 2023) and because of the need to map disaggregated food expenditures into welfare. The latter raises the question: Do lean seasons exist because local markets fail to allocate scarce resources efficiently across households, or because local markets are poorly integrated with national and global markets?

The idea of offering post-harvest loans (PHLs) to farmers to smooth seasonal consumption has been widely studied (Burke et al., 2019; Channa et al., 2022). One may be surprised by the proposal to offer farmers credit after harvest. In an agricultural economy we would expect this to be the time when resources are relatively plentiful, and this plenitude reflected in low prices. It would seem to be a time to *save* for the hungry season, not to borrow. To this, advocates of PHLs would reply that if farmers could borrow at harvest to finance their households' consumption needs they could avoid selling their crop at the time when prices are lowest. Another way to think about this is that these programs are designed to encourage farmers to make leveraged bets on the price of the crop they grow, as any predictable seasonal variation in prices seems to suggest an arbitrage opportunity. Regardless of the economic logic, several of these studies have found generally positive effects of experiments involving PHLs and storage in sub-Saharan Africa on engagement in and profits from this sort of intertemporal arbitrage.

However, spelling out a simple model of intertemporal consumption smoothing reminds us that leveraged bets are inherently risky. In this case it turns out that local year-to-year variation in prices is large relative to any predictable seasonal component. Cardell and Michelson (2022) use price data from many sub-Saharan African grain markets and simulations to show that even a mild level of risk aversion can rationalize the apparent lack of arbitrage, despite its profitability in expectation.

The usefulness of PHLs also depends on the causes of lean seasons and consumption variability: A simple but general lesson from finance is that the efficient portfolio depends on how returns to different assets vary with farmers' marginal rates of substitution (IMRSs) in expectation. The IMRS can tell us directly us the rate at which farmers would like to transfer resources across periods, taking into account both intertemporal discounting and the effects of risk. For example, if IMRSs vary across time, that suggests credit market imperfections, while local variation across *farmers* suggests that idiosyncratic risk is not being efficiently shared. If IMRSs are highly correlated across farmers, but vary over time, this suggests that *local* asset markets function well, but are poorly integrated with the broader economy. In such cases, a local microcredit program is unlikely to address the root causes of seasonal hunger: It is simply injecting capital into local economies at below the market-clearing interest rate.[1] In other words, offering small amounts of subsidized credit is unlikely to overcome the scarcity and volatility of aggregate resources. The (expected) losses from such programs, including their adminstrative costs, would be better off spent on lowering *aggregate* risk, perhaps by improving market integration.[2] There is thus an apparent tension between the encouraging empirical results of PHLs in the literature and their more sobering theoretical justification. However, RCTs are only informative about treatment effects in realized states of the world (Rosenzweig & Udry, 2020), and are insufficient to determine whether the treatment is a good policy in expectation. To this end, we partnered with the Taimaka Project, an NGO in Gombe State in northeastern Nigeria, to implement a randomized evaluation of a postharvest loan program[3] modeled after the One Acre Fund scheme studied by Burke et al. (2019). To shed more light on the effects of PHLs throughout the lean season, we collect high-frequency survey data on stocks and expenditures supplemented with experimental elicitations of the IMRS. We first use these data to estimate the treatment effects of Taimaka's loan program, which contained both a cash loan and a similarly-valued in-kind loan of maize grain. However, we go beyond providing another data point on the effects of PHLs in a different region and state of nature at higher frequency; We test whether such programs are *in expectation* suited

[1]Essentially, such programs could be thought of as highly inefficient cash transfers, due to their administrative costs and risks of default.

[2]There are also supply-side issues facing lenders. Prices *within* a market are highly correlated, meaning that a lender needs to be sufficiently diversified to withstand correlated default when returns are low. However this is beyond the scope of this paper.

[3]See pre-specified descriptions of our tests at https://www.socialscienceregistry.org/trials/8022.

to address the relevant underlying market failures, using conditional moment restrictions that are familiar from the asset-pricing literature. Importantly, these tests are informative about the general state of credit markets during the lean season and are not specific to PHLs.

This all presupposes that we have measures of the relevant IMRSs at relatively high frequency throughout the season. This may seem obvious, but survey instruments to measure consumption and poverty are often poorly designed for capturing seasonal variation (Merfeld & Morduch, 2023). We therefore designed instruments and methods to measure IMRSs at 6–8 week intervals.

Our first method of measuring IMRSs uses variation in farmers' consumption portfolios to draw inferences regarding within-period marginal utilities of expenditure (MUEs). If farmers have time-separable preferences and exponential discounting (with a common discount factor) then their IMRSs will be proportional to ratios of these MUEs. Our second method was more direct (and didn't require any assumptions regarding intertemporal preferences): we used simple revealed preference methods to elicit farmers' intertemporal marginal rates of substitution, by offering to sell farmers a bond having a one-month maturity at different interest rates. This allowed us to directly measure farmers' demand for credit, while at the same time having only a modest effect on IMRSs, as the par value of the bond was rather small. Notably, the treatment of offering much larger post-harvest loans to randomly selected groups of farmers induced exogenous variation in farmers' IMRSs.

We first use these data to estimate the treatment effects of Taimaka's loan program, which contained both a cash loan and a similarly-valued in-kind loan of maize grain. In contrast to other papers, we find large effects on grain storage, but no significant effect on sales, consumption or welfare. In particular, households that received the cash treatment appear to have increased the value of their storage by up to 100,000 Naira, (twice the loan value). In contrast, we find a null effect of the in-kind loan on stocks, including for maize (despite the fact that the in-kind transfer is bags of maize). Further, we find no significant effects of either treatment on crop sales. This is consistent with the fact that grain prices did *not* increase as usual, instead staying relatively flat from the time when loans were disbursed after the 2021 harvest to the time when repayment was due in mid-2022. Most households which sold grain did so at prices that did not cover Taimaka's interest rate of 15%. It instead appears that these (cash) loans induced households to reschedule their consumption over the season as opposed to becoming arbitrageurs.[4]

We also find minimal effects of the program on various measures of welfare. In particular, we detect no significant changes in average consumption, estimated welfare (marginal

[4]As a postscript, many households defaulted on their loan and Taimaka decided not to continue the program the following year, though the direction of causality here is quite unclear.

utility of expenditure or MUE), self-reported hunger, or experimentally elicited intertemporal marginal rates of substitution (IMRS).[5] However, when breaking these results out by period, we find that estimated household MUE's are significantly higher for households in both treatments at endline. This is consistent with a story in which households used their loans to make investments in maize or other assets, but returns on those investments were disappointing. These households then had to repay their loans or forfeit assets after defaulting.

While the results from our RCT show that PHL programs do not always lead to major increases in profits and welfare, are they (and other types of credit programs) still a viable way to reduce poverty and seasonal hunger *in expectation*? This depends on whether farmers could improve their *ex ante* expected utility by changing the composition of their portfolio of stocks and financial assets. Borrowing tools from the finance literature, we test for systematically differential responsiveness of households' intertemporal marginal rates of substitution (IMRS) to changes in grain prices. If local financial markets are well-functioning and households optimize, then IMRSs may respond to price shocks, but would do so in the same way for all households. However, if there were idiosyncratic credit constraints, for example, these would cause households to hold different portfolios of grain stocks, causing their IMRSs to deviate from the economy average depending on returns.

We test for such predictable differences using lagged IMRS (both estimated from consumption data and experimentally elicited), lagged stocks, and random assignment to treatment in the RCT. In each case (and jointly) we fail to reject the hypothesis that local financial markets function efficiently. While perhaps surprising, this is consistent with evidence that village economies smooth out idiosyncratic risk fairly well but struggle to cope with aggregate risk (Samphantharak & Townsend, 2018).[6] This suggests that the main culprit for the low and highly variable levels of consumption comes from seasonal variability in *aggregate* resources. This is consistent with Gombe being poorly integrated with the rest of the Nigerian economy (which we illustrate using high-frequency price data). This implies that facilitating *spatial* rather than *intertemporal* arbitrage, to the extent possible, would more effectively address the underlying causes of seasonal poverty in Gombe, without the risks and logistical challenges of PHLs.

These results primarily contribute to a large literature on the seasonality of income and

[5] Note that full insurance would also generate null effects of the loan treatment on these welfare measures, even there were excess (realized) returns to arbitrage. We present tests of full insurance below, which help distinguish these mechanisms.

[6] One might be tempted to argue that the overwhelming demand for Taimaka's loan product is evidence of poorly functioning financial markets. However, positive demand for artificially cheap credit is not a sign of a market failure.

consumption in agricultural settings, including a number of papers on PHL and storage interventions. Basu and Wong (2015) find that providing households with storage drums and in-kind staple food loans leads to increases in expenditure and income, but that only credit led to smoother lean season consumption. Aggarwal et al. (2018) and Omotilewa et al. (2018) find that pure storage interventions using hermetic PICS bags increase maize storage volumes, duration and revenues in Kenya and Uganda, respectively. Channa et al. (2022) also include a treatment arm that only provided PICS bags and find slightly smaller and insignificant positive effects on maize storage than for households who received both credit and PICS bags (but cannot reject equality of the effects). Channa et al. (2022) and Burke et al. (2019) (who study the program on which Taimaka's PHLs were modeled) find that access to postharvest credit improves farmers' incomes, but do not detect any effects on consumption or investment.[7] We not only provide a cautionary tale about the effects of PHLs in years with minimal price increases, but show how providing credit in general may fail to address the root causes of seasonal hunger. Nevertheless, other schemes such as warrantage have shown promise at smoothing intertemporal consumption in similar environments (Delavallade & Godlonton, 2023).

We also contribute to a broader literature on consumption variability during the lean season. Barrett and Dorosh (1996) argue that poor households bear the brunt of price variability in Madagascar. Fink et al. (2020) find that offering credit during the lean season enabled a productive reallocation of household labor from off-farm to farm in Zambia. Also in Zambia, Augenblick et al. (2023) find that a simple budgeting exercise induces households to save maize significantly longer into the lean season. A series of papers (Bryan et al., 2014; Meghir et al., 2022) studies (subsidized) seasonal migration as a risky but profitable means of smoothing consumption. While these experimental studies point to a more positive role for credit, our results differ, likely due to the function of local financial markets in Gombe and/or the realization of state of the world with low returns to credit.

Finally, our paper relates to the trade and development literature on market integration and income volatility (Allen & Atkin, 2022; Burgess & Donaldson, 2010; Caselli et al., 2020; Fafchamps, 1992). While we don't provide direct evidence on the effects of market integration, we are among the first papers to consider the relative benefits of spatial and intertemporal arbitrage. The fact that study sites in Gombe appear relatively disconnected from the broader economy is consistent with the local general equilibrium effects on prices found by Burke et al. (2019) in Kenya. Bergquist et al. (2021) provide an example of a

[7] The idea that credit can alleviate the "sell low, buy high" phenomenon is also supported by Stephens and Barrett (2011) and Dillon (2021), while the idea that the price risk of arbitrage rationalize it is supported by Cardell and Michelson (2022).

low cost intervention that improved market integration in Uganda, which could be a way to facilitate spatial arbitrage in settings such as Nigeria as well.

The rest of the paper proceeds as follows. Section 2 lays out a simple model the household's decision to store or consume grain. Section 3 describes our experimental design while Section 4 describes the economic environment in Gombe, including price trends. Section 5 presents the RCT results, while Section 7 implements our test of local financial markets. Section 8 concludes.

2 Model

Consider a collection of farm-households in Gombe. Each household has per-period utility function $U(c)$ over a J-vector of goods c. U is assumed to be increasing, concave, and continuously differentiable. Goods can be bought and sold on local spot markets at prices p. Some of these goods can also be stored across periods, and some can be produced by the farm-household.

Our principal concern has to do with seasonality, so we focus on a single agricultural year, consisting of a sequence of season or periods within that year indexed by $s = 1, \ldots, S$. Agricultural output is affected by weather, and weather (while random) tends to vary across seasons, with season s having a weather realization w_s drawn from a cumulative distribution function $G_s(w)$. Over the course of the agricultural year the weather history $w^s = (w_1, w_2, \ldots, w_s)$ affects agricultural output, drawn from a cumulative conditional distribution function $F_s(y|w^s)$. Note that for seasons when there's no possible harvest this distribution may be degenerate (i.e., $F_s(0|w^s) = 1$). As with weather, from the point of view of the farm-households[8] prices may be random, but drawn from different distributions in different seasons, with a cumulative distribution function $H_s(p|w^s)$ which may also depend on the history of weather during the year.

In any season, the farm-household enters the period holding a vector of stocks k, to which any new production y is added. The new stocks $k + y$ can be adjusted (subject to possible bounds, *infra*) via purchases or sales at prices p, and from this the household consumes a vector c, then taking into the next period a vector of stocks k'. Writing this in terms of a flow budget constraint, we have

$$p(k' + c) = p(k + y). \tag{1}$$

[8] From the point of view of the economy more broadly, prices may *not* be random, but instead may adjust to clear markets. So we could close the model by describing distributions of prices H_s as equilibrium objects.

In addition, the quantity of certain stocks held may be bounded below (think, for example, of holding physical stocks of maize, which must be non-negative), though this need not be an issue for financial assets (think of bonds, which might be negative for a household which is a net debtor). In any event, we also have a set of J constraints of the form

$$k' \geq b, \tag{2}$$

where b is a vector of lower bounds on stocks of each good (some elements could be minus infinity), which we assume to be fixed.

So: in any season s, the household takes as given the stocks k brought into the period, a vector of prices p, and a history of weather to that period w^s. The problem facing the farm household can then written as a dynamic program, with the farm household solving

$$V_s(k, p, w^s) = \max_{k',c} U(c) + \beta \int V_t(k', p', w^t) dF(y|w^t) dH_t(p'|w^t) dG_t(w_t),$$

with $t = (s+1) \mod S$, subject to the budget constraint (1) and any bounds on stocks (2).

This yields the first order conditions

$$u_j(c)/p_j = \lambda \qquad j = 1, \ldots, J,$$

where λ is the Lagrange multiplier on the household's flow budget constraint in each period, and where $u_j(c)$ is short-hand for $\frac{\partial U}{\partial c_j}(c)$, and

$$p_j \lambda = \beta E \left[p'_j \frac{\partial V}{\partial k_j} \right] + \eta^j$$

where η_j is the Lagrange multiplier on the lower bound constraint for stocks of good j.

Modifying notation to allow for time subscripts and applying the envelope theorem delivers the standard asset pricing equation for each asset j, allowing for the possibility that the household is at a corner (i.e., (2) is binding for asset j), in which case the multiplier η_t^j will be positive:

$$\beta \, \mathrm{E}_t \frac{\lambda_{t+1}}{\lambda_t} \frac{p_{t+1}^j}{p_t^j} + \frac{\eta_t^j}{p_t^j \lambda_t} = 1 \qquad \text{for all } j = 1, 2 \ldots, J. \tag{3}$$

The intertemporal marginal rate of substitution (IMRS) between periods t and $t+1$ is given by $\beta \frac{\lambda_{t+1}}{\lambda_t}$. As λ_t can be interpreted as the marginal utility of expenditures at time t, and these expenditures in turn depend on income, prices, and consumption via the budget constraint (1) , Equation (3) implies that the farm-household's portfolio choices will be governed by the covariance of returns $\frac{p^j_{t+1}}{p^j_t}$ with potential realizations of the IMRS.

Now assume there are $i = 1, 2, \ldots, N$ households in a given location (e.g. Gombe). With data on (common) prices p^j_t and estimates of λ_{it}, the orthogonality conditions implied by 3 can be used as a tool to help understand the functioning of local financial markets.

To develop this tool, let $\delta^j_{it} \in \{0, 1\}$ be an indicator of whether household i holds positive stocks of asset j at time t. Let $R^j_t = p^j_t/p^j_{t-1}$ denote the one-period return to holding asset j, and let m_{it} denote i's intertemporal marginal rate of substitution at time t. By complementary slackness we then have

$$\delta^j_{it}\,\mathrm{E}_t\,\frac{\lambda_{t+1}}{\lambda_t}\frac{p^j_{t+1}}{p^j_t} = \delta^j_{it} \qquad \text{for all } j = 1, 2 \ldots, J. \tag{4}$$

Now let $\bar{m}^j_t = \sum_{i=1}^N m_{it}\delta^j_{it}/\sum_{i=1}^N \delta^j_{it}$ denote the average IMRS of those who hold positive stocks of the asset. Then in the aggregate we have

$$\mathrm{E}_{t-1}\,\bar{m}^j_t R^j_t = 1. \tag{5}$$

How is this consistent with seasonality? If seasonality affects local prices for asset j in a way which is at all predictable, then we can't have $\mathrm{E}_{t-1}\,R^j_t = 1$, so for then for (5) to hold $\bar{m}^j_t$ must covary negatively with R^j_t, or (alternatively) marginal utilities must decrease as prices of j increase. The logic of this is simple: if it is known that a storable asset will likely have higher prices next period, then money can be made (and consumption increased) by investing in more of that asset. On the other hand, if there's increased aggregate storage of asset j ("buy low sell high") then this may retard the rate of increase in local prices for good j, particularly if the local economy is poorly integrated with the broader economy.

So, we can look at evidence that local prices for storable goods (such as maize) change predictably over the season. If this change is only in *local* prices (e.g., prices don't covary with world prices, or with prices for the same goods elsewhere in Nigeria), then this is evidence not only that maize markets are poorly integrated, but that Gombe's *other* markets are also poorly integrated. Why? Suppose prices for good j are seasonal and can be predicted to increase and the market for good j poorly integrated. At the same time the market for some other good is *well-integrated.* Then Gombe farm-households could take a short position in the latter market—since they're small relative to the broader economy this only modestly affect prices—and use the funds from the short position to store Gombe maize. Increasing the size of this bet on Gombe maize prices will be profitable up to the point at which the aggregate supply response (less maize for consumption this period, more maize stored for next period) reduces returns to holding maize so that (5) is satisfied.

Let us return our attention to the choices of individual farm-households. Substituting (5) into (4), we obtain

$$\mathrm{E}_{t-1}(m_{it} - \bar{m}_t^j) R_t^j \delta_{it-1}^j = 0. \tag{6}$$

Interpreting this, focus first on the factor $(m_{it} - \bar{m}_t^j)$. This is the *deviation* of household i's IMRS from the average IMRS of other households which hold asset j. If all households were fully insured this term would always be equal to zero—though aggregate shocks to Gombe might change IMRSs, they would change in precisely the same way for every household. However, (6) may hold even in the absence of full insurance. If we think of $(m_{it} - \bar{m}_t^j)$ as a measure of idiosyncratic shocks, then (6) is telling us that these idiosyncratic shocks are orthogonal to returns on assets held by household i. Another way of putting this is that this idiosyncratic risk *uninsurable* using the assets held in positive quantity by the household.

Where i's IMRS differs from the aggregate we might think this was because he held a different portfolio of assets from other households. If, for example, a household held proportionally more maize in its portfolio than did others, then $(m_{it} - \bar{m}_t^j)$ would be negatively correlated with returns to maize. Thus, we're looking for evidence that some households have IMRSs which predictably respond differently to returns than does the average household.

The central prediction is that realizations of $y_{it}^j = (m_{it} - \bar{m}_t^j) R_t^j \delta_{it-1}^j$ should be uncorrelated with any variable z_{it-1}^j in the time $t-1$ information set. In particular, lags of IMRS and stocks, as well as treatment assignment would plausibly be correlated with y_{it}^j absent the null of well-functioning local financial markets.

3 Experimental Design and Data

Taimaka randomly assigned offers of either (i) a cash loan; (ii) a similarly-valued in-kind loan of maize; or (iii) no loan to groups of applicants. From each of these groups we collected eight rounds of high-frequency consumption, investment and storage data over a 12 month period (pre- and post-treatment) from a sample of 935 farmers. In addition, Together with data on the consumption expenditures we collected, this gives us two distinct ways to measure households' welfare and demand for credit at each point throughout the season. We also have highly localized price data for the full range of crops grown and stored by sample households at the local market level within Gombe and the time series of prices over a longer period across several markets in Nigeria from USAID's FEWS project (Famine Early Warning Systems Network, 2023).

3.1 Sample Frame and Household Selection

Communities were selected from the 10 percent poorest locations in Gombe state as predicted by satellite data following (Aiken et al., 2020) (about 50 sites). Of these, 20 were randomly selected for a rapid rural appraisal by Taimaka. Half (10) of these sites were chosen for household listing based on perceived need and accessibility. Then six of these sites were selected for program implementation based on the household listing.

In each of the six sample sites, households were selected as follows. Taimaka visited the sites to advertise the loan program and met with traditional leaders after obtaining their assent to move forward to the program. After two days, they returned to hold an informational session designed to emphasize the terms of the loan, the loan's theory of change, and group indemnity. They then advised interested farmers to start forming groups of 5 and that they would return in a few days to begin taking applications.

Each group completed an enrollment form, which included the 10 question from the Poverty Probability Index (PPI) for Nigeria, some brief questions about farming, and demographic information and included pictures of farmers. After receiving applications, Taimaka developed a ranked list of groups in order of desirability, based on ability to repay and need for the loan.

Taimaka then met with traditional leaders to verify that the members of groups were indeed residents of that location, were indeed farmers, and were known to be creditworthy. If a single member was deemed unqualified, groups were given an opportunity to choose a replacement individual. If more than two candidates were deemed unqualified, then the group was struck. The ranked list was then updated accordingly.

Loan officers were then given a target number of clients to enroll in the program in each site. They went down the ranked list, visiting three farmers' households in each group to verify the information given on their applications. If any farmer was found to have made material misrepresentations in their application, the group was dropped from the list. Otherwise, the group was enrolled, which made them eligible to be selected for the sample.

The ordered list was then partitioned into strata of 6 adjacently ranked groups. A randomly selected pair of groups in each stratum was assigned to receive the cash loan, another was assigned to receive the in-kind maize loan, while the remaining pair was assigned to a control group. This draws on the idea of a finely stratified assignment mechanism advocated by Athey and Imbens (2017). This led to a sample of 935 individuals from 187 groups.

3.2 Treatment

The treatments were an offer of a joint-liability loan of up to 50,000 Naira (approximately USD 100) in value to each of the 5 group members. The terms for the maize and cash loan were slightly different.

For the cash loan, each farmer was asked to commit up to 4 bags to store until July 15th 2022, the due date of the loan. Farmers received 11,900 Naira in cash plus a hermetic PICS bag priced at 600 Naira for each bag they committed. The vast majority of farmers in this arm chose to commit 4 bags, and received the maximum loan value of 50,000 Naira (approximately $100). The loans were to be repaid with a 15% user fee in July 2022. Delivery of the loans took place between September 16th and October 10th 2021. Farmers were also required to make monthly repayments of at least 3,000 Naira starting in December 2021. If one takes all these payments and their timing into account and discounts appropriately using a constant interest rate, one finds that the present value of the Taimaka "loan" is zero at an annualized interest rate of about 35%, or about 2.6% per month. The median (across lenders) "prime" rates for commercial projects in agriculture in Nigeria were around 25% at this time,[9] so the Taimaka PHL involved a considerable level of subsidy (about 20% of total capital outlay) to farmers even before considering administrative costs and the possibility of default.

For the in-kind maize loan, all farmers were offered a loan of three 100kg bags of maize. Farmers in this arm were also required to repay the loan in July 2022 with a 15% user fee. The amount required to pay was equal to the value of the price at which Taimaka purchased the maize, plus the cost of transporting to the household plus the cost of 3 PICS bags. In total, this ranged from 62,000-63,000 Naira, depending on the group's distance from the market. Due to logistical issues on Taimaka's end, these loans were disbursed in late November 2021 and repayment was required to begin in January 2022. Otherwise, the conditions of the loan were identical to those in the cash loan treatment arm.

Members of the control group were not offered any loan by Taimaka.

3.3 Surveys

We collected 8 rounds of household surveys at approximately two-month intervals between August 2021 and November 2022. Each survey included modules on household composition, grain stocks, food acquisitions, other expenditures, and measures of seasonal hunger. During baseline and endline surveys, we also asked questions about agricultural inputs and output from the previous season and assets, including livestock.

[9]See https://cbn.gov.ng/Documents/depositandlending.asp

3.3.1 Food Acquisition and Stocks

In each wave, we elicited information on recent acquisitions of a list of 22 different goods that households in this setting consume. However, while many of these acquisitions are fairly clearly for consumption (e.g., milk, sugar) in the fairly near future, others may be held for some combination of present consumption and investment. In particular, households often hold positive stocks of maize, millet, beans, guinea corn (sorghum), and less frequently hold stocks of rice, Bambara nut, and groundnut. A few households also reported holding stocks of cassava. During each wave, we asked households for information on these stocks. At baseline, we asked for the amount of each crop that the household had stored. In waves 1-3, we also asked how much of each crop they had harvested, purchased in bulk, and sold (or given away) since the previous visit, in addition to asking them about their stocks. After wave 3, enumerators informed us that households appeared to consider questions about their stocks sensitive. Therefore, for waves 4-6 we asked about how much of households' stocks they had consumed since the previous visit instead of asking about their current position. This gives us an account of households' grain flows at each period, allowing us to impute stocks. At endine, we added back in the question about stocks.

Unsurprisingly, households' reported stocks and flows over time do not always balance over time in an accounting sense. As a result, we had to make substantial imputations, which we describe in the Appendix.

3.3.2 IMRS Elicitation

We also attempted to measure individuals' intertemporal marginal rates of substitution by measuring willingness to pay for bonds at different interest rates. After each survey wave, enumerators asked individuals whether they would be willing to invest 500 Naira the following survey to receive $500(1+x)$ Naira for each x between -0.1 and 1 in increments of 0.1.[10] The enumerator then used an app to randomly select an interest rate. If the individual had agreed to the selected rate, then they were required to bring at least 500 but up to 2,500 Naira during the following survey wave. The money was then given to the enumerator and the money was repaid with interest the following survey wave.[11] Otherwise, no deal was

[10]Starting in Wave 3, we added an additional option of an interest rate of 2 to the choice set, with the logic that refusing this option would likely indicate misunderstanding of or refusal to participate in the exercise. A total of 1 participant was marginal to this option, suggesting that null responses are unlikely to contain meaningful information about the IMRS.

[11]Choices at endline were made with the knowledge that enumerators would return after one month to implement the choices as usual. However, households were (unexpectedly) told during the subsequent visit that payouts would be made instantaneously, rather than after a month, for operational reasons. Thus, the choices made at endline were still valid elicitations of IMRS, given that households expected that the procedure to be implemented as usual.

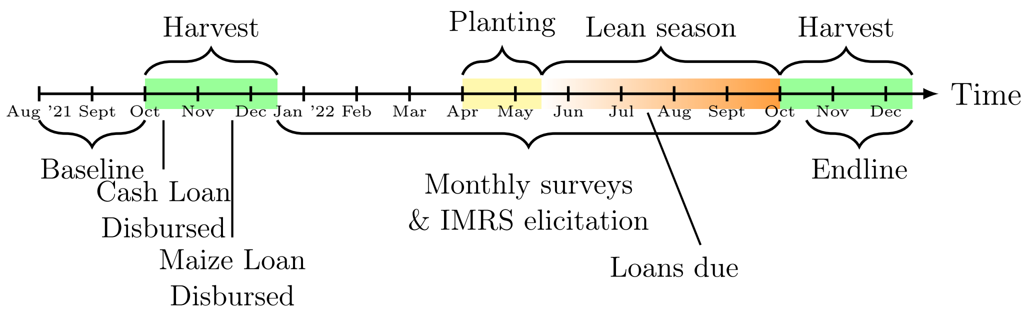


Figure 1: Intervention and Survey Timeline

implemented. Households who had agreed to the interest rate that was drawn were told that they would be barred from future payouts if they failed to honor the deal. These real-stakes choices identify the range of households' intertemporal marginal rate of substitution during each survey round.

3.3.3 Other variables

We also asked whether households had any large non-food purchases of at least 10,000 Naira ($20), whether they engaged in another business and, if so, what their expenses and revenues were, and whether and how much money they borrowed and repaid. We also pre-specified the Reduced Coping Strategies Index (RCSI) (Maxwell et al., 2014) which asks households questions about the number of days in a month they restricted food consumption because of lack of resources, as a measure of seasonal hunger.

3.4 Other Data sources

We use two sources of price data for our analysis. Taimaka collected prices of staple commodities from local markets in Gombe State on a (roughly) monthly basis. We also use weekly price data from the Famine Early Warning System Network (FEWS-Net) for Nigeria, which covers major regional markets, including Gombe Town. We also use Taimaka's administrative records on loan applications (including the scoring of groups), disbursement, and repayment.

3.5 Attrition

We dealt with attrition, both in terms of households not being surveyed in a given wave and entire groups permanently dropping out of the sample. In particular, 7 groups (35 individuals) assigned to the cash loan dropped out after treatment assignment after either being disqualified from the loan program for having lied on their applications or voluntarily withdrawing. However, 5 maize groups and 2 control groups withdrew their consent to be surveyed after Round 1. The stratified design helps us deal with the latter problem, as we are able to the entire stratum of any group that attritted (Bruhn & McKenzie, 2009). We present our main results, which do not change substantially, when dropping these clusters in Appendix B. However, conditional on the group remaining in the program we achieve a response rate over 90% for the following rounds, which is evenly balanced across the three arms (Table A9). Moreover, as shown in Table A10, baseline covariates are not predictive of subsequent individual attrition — in a regression of probability of being surveyed on 21 covariates, only one is statistically significant. However, due to time constraints on the rollout of the loan program, we were only able to survey slightly over 60% of the sample at baseline (Round 0), although this does not affect our main estimates.

4 Descriptive Statistics

4.1 Prices

In Figure 2, using over 10 years of monthly price data from FEWS Network, as well as data collected from markets in Gombe State during 2021–22, we show that the magnitude of seasonal price increases is highly variable. While on average prices increase by 61% from floor to peak, in the median year, the increase is 38%. This difference is driven by extreme price increases of 239% and 187% in 2016 and 2020, respectively. On the other hand, price increases were lower than Taimaka's interest rate of 15% in three of 11 years. Thus, intertemporal arbitrage typically yields moderate positive returns but is highly influenced by tail events at both extremes. As Cardell and Michelson (2022) argue, it appears that moderate degrees of risk aversion could rationalize the lack of intertemporal arbitrage.

Notably, maize prices in the 2021-2022 season increased by about 20% (from 16,500 to 22,000 Naira per 100kg bag) between November and January, then stayed between 18,000 and 20,000 Naira, where they stayed throughout the loan period. At least 2/3 of the maize in the sample was sold after the January peak, meaning that households would have obtained a return on harvested maize stored between 11 and 23%, depending on the week they sold, not factoring in depreciation. In practice, 33% of maize sold in the sample was sold below 18,000 Naira per bag and 83% was sold for below 20,000 Naira per bag. Overall, few people made significant profits from arbitrage and many made negative profits net of Taimaka's interest.

The price data also exhibit limited spatial correlation, both within Gombe, across markets

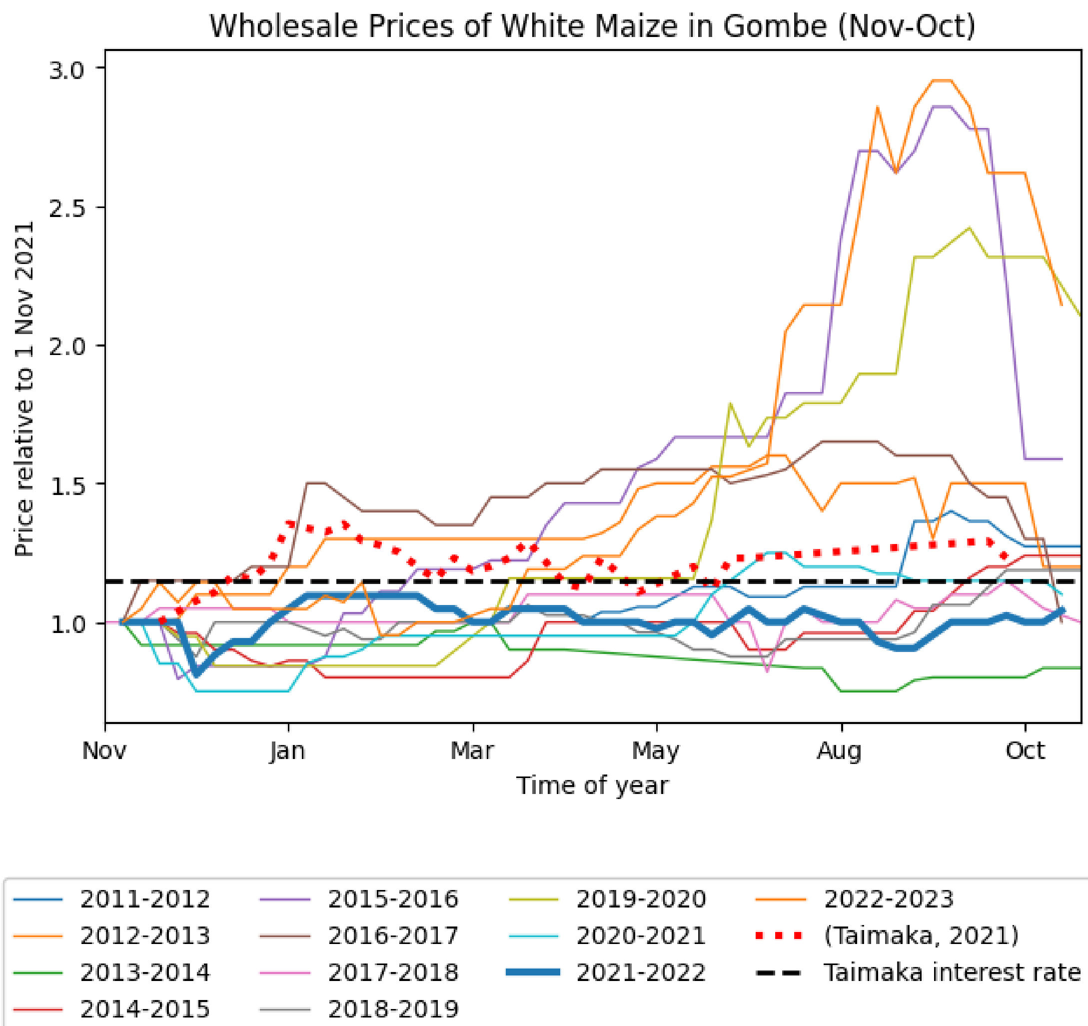


Figure 2: Yearly maize price increases in Gombe

in Nigeria, and between Nigeria and world markets, suggesting there may be scope for spatial arbitrage. Using FEWS data on prices at weekly frequencies we can reject the null hypothesis of no spatial correlation in maize prices (using a Skillings-Mack test; $p = 0.035$); when prices go up in one location they are very likely to go up in another (see Figure A1 for FEWS prices across Nigeria and Figure A2 for prices in local markets in Gombe State collected by Taimaka). However the magnitude of this correlation is not particularly large. The R^2 from regressing the times-sseries of local prices on that of international prices time-series is .26, although the bulk of the correlation is driven in recent years (see Figure A3).

Together, this paints a picture of not only volatile maize prices, but of poorly integrated markets, suggesting the scope for spatial arbitrage in addition to (or perhaps instead of) intertemporal arbitrage.

We now document seasonality in consumption in our sample during the study period by looking at both our high-frequency measures of welfare: the directly elicited IMRS and the marginal utilities of expenditure estimated from survey data.

4.2 Elicited IMRS

We encountered some challenges implementing the IMRS elicitation exercise described in Subsection 3.3.2 in the initial survey rounds. At first, many individuals refused to participate in the exercise due to religious concerns that it was too similar to gambling or usury. We attempted to allay these concerns by explaining the exercise to local imams in each subsite, who relayed to participants that it was permissible. After refusal rates above 40% in the first two rounds, refusal rates lowered to 20-26% for the remaining rounds. We also made sure to distinguish between refusal to participate and choosing the empty set of interest rates. Other issues were comprehension and trust, although we believe that these also became less relevant over time. In Wave 1, 28% of responses exhibited violations of the Weak Axiom of Revealed Preference (WARP), while in subsequent rounds, only 2% of responses exhibited WARP violations.[12] Payouts from implemented deals in Wave 1 were also made in Wave 3, perhaps increasing our credibility in the eyes of participants.

It can also be challenging to interpret observations in which the individual did not agree to any interest rate, as well as those in which the individual refused to participate. In our preferred specifications, we include only observations in which an interest rate was selected. This is because we believe null choices are more likely to indicate reluctance to participate for religious regions, lack of trust, or misunderstanding of the exercise rather than true intertemporal preferences. However, we also show results coding null results, as well as

[12] In our preferred specifications, we drop observations violating WARP.

refusals, as the maximum interest rate available.

Conversely, we may not believe that individuals would choose a -10% or 0% interest rate, assuming that they have a discount factor less than 1.[13] We therefore also present results dropping observations that select a (weakly) negative interest rate.

The elicited measures of IMRS described in Subsection 3.3.2 exhibit an increasing pattern over the season, as shown in Panel A of Figure 3. During the lean season, which becomes increasingly acute during the March-September growing season, we see households become increasingly willing to forgo higher returns on bonds. In particular, the median household goes from requiring a 10% return in January to a 30% return from June onward. This is consistent with resources becoming increasingly scarce over the course of the season. However, results from the first few waves are somewhat sensitive to topcoding and dropping selections of the 0 rate.

4.3 Consumption-based λ

Our second measure of seasonal welfare is the estimate of marginal utility of expenditure using survey data. This is obtained by estimated a Constant Frisch Elasticity (CFE) demand system following (Ligon, 2020). CFE demand systems nest demand systems commonly used in the literature and flexibly accommodates non-homotheticity and a wide range of substitution elasticities. In particular, with panel data it permits the estimation of an Index Marginal Utility of Expenditure, which under certain cardinalizations of utility can be interpreted as the log of the marginal utility of expenditure, ($\lambda\backslash$). Since marginal utility is decreasing in wealth, we present $-\log\lambda$ as our preferred measure of welfare.

As shown in Panel b of Figure 3, our consumption-based welfare estimate also shows a slight decreasing trend over the course of the season. Values are particularly low during the lean season (March-August) before recovering slightly at endline, after harvests began to come in.

While the elicited IMRS and consumption-based $\log\lambda$ are not directly comparable,[14] one would still expect them to be systematically correlated with one another. Panel C of Figure 3 presents box plots of $-\log\lambda$ at each of the possible interest rates from the IMRS elicitation. While these are raw correlations pooled across all survey waves, households who are willing to forgo higher interest rates do tend to have lower welfare (higher estimated λ). Given their

[13] In theory, it is possible that individuals would accept negative interest rates due to intrahousehold taxation or present focus, e.g. (Dupas & Robinson, 2013), although misunderstanding the exercise appears more likely in our case.

[14] These measures are hard to quantitatively validate against each other, since the experimentally elicited IMRS involves expectations over future marginal utility, which precludes linear comparisons.

correlation and similar seasonal patterns, it's reasonable to believe that both the elicited IMRS and consumption-based λ are capturing something similar.

5 Experimental results

As described in Section 3, farmer groups in each of the six study sides were partitioned into strata of six groups ranked adjacently in terms of Taimaka's willingness to lend to them, with two groups offered the cash loan, two groups offered the maize loan and two groups offered no loan. Where the number of groups in a site was not divisible by 6, treatment was randomly assigned to the remaining groups; for the analysis, we merge these groups with the preceding stratum in their respective sites. In our main specifications, we use the Neyman estimator suggested by Athey and Imbens (2017). While simple, this estimator can offer precision gains over OLS with fixed effects and a small number of clusters. In Appendix B.2, we show results from the regression-based approach, which are qualitatively similar albeit noisier.

Denote the treatment of assignment of household i in stratum s as $D_{is} \in \{C, M, 0\}$, corresponding to Cash, Maize and Control, respectively. Within each stratum, we have a fully randomized experiment and obtain the estimate of the cash treatment on outcome Y_{ist} (which may be observed at different periods t) as $\hat{\tau}_s^C = \bar{Y}_s^C - \bar{Y}_s^0$ and of the maize treatment as $\hat{\tau}_s^M = \bar{Y}_s^M - \bar{Y}_s^0$. The variances of $\hat{\tau}_s^C$ and $\hat{\tau}_s^M$ can be written as

$$\hat{\mathbb{V}}(\hat{\tau}_s^C) = \frac{s^2_{C,s}}{N_{C,s}} + \frac{s^2_{0,s}}{N_{0,s}} \ , \hat{\mathbb{V}}(\hat{\tau}_s^M) = \frac{s^2_{M,s}}{N_{M,s}} + \frac{s^2_{0,s}}{N_{0,s}},$$

where $s^2_{D,s}$ is the sample variance of Y_{ist} for households in arm D. These stratum-level estimates are aggregated over strata as follows

$$\hat{\tau}^C = \sum_{s=1}^{S} \hat{\tau}_s^C \frac{N_s}{N} \ , \hat{\mathbb{V}}(\hat{\tau}^C) = \sum_{s=1}^{S} \hat{\mathbb{V}}(\hat{\tau}_s^C) \left(\frac{N_s}{N}\right)^2$$

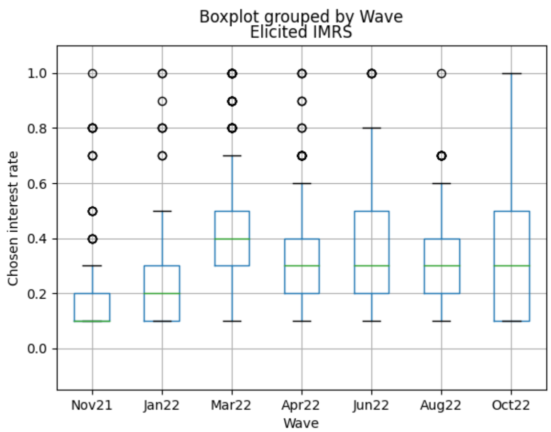
and

$$\hat{\tau}^M = \sum_{s=1}^{S} \hat{\tau}_s^M \frac{N_s}{N} \ , \hat{\mathbb{V}}(\hat{\tau}^M) = \sum_{s=1}^{S} \hat{\mathbb{V}}(\hat{\tau}_s^M) \left(\frac{N_s}{N}\right)^2$$

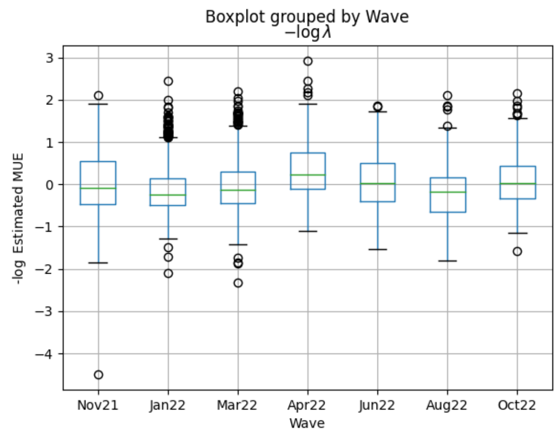
Note that $N_{D,s} = 2$ and $N_s = 6$ except in the last stratum of each subsite in cases where the number of groups is not divisible by 6. For outcome variables that are measured in each survey round, we estimate separate τs for each round as well as summing or averaging over the season, as pre-specified.

Figure 3: Distributions of IMRS and $-\log \lambda$

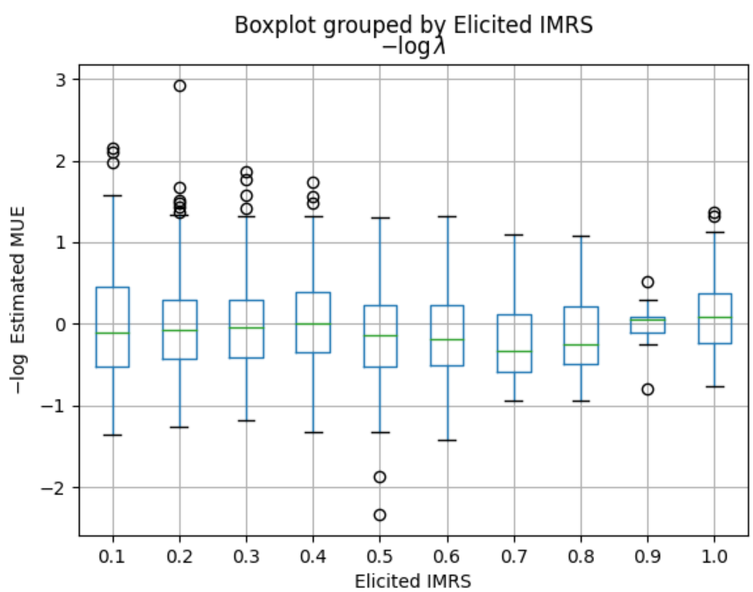
(a) Elicited IMRS over time



(b) Estimated $-\log \lambda$ over time



(c) Estimated $-\log \lambda$ by elicited IMRS



5.1 Effects on Stocks

We first present treatment effects on grain flows. Figure 4 show the effects of the cash and loan treatment, cumulatively and in each survey wave on the value of grain flows (sales, consumption from own stock, harvests and purchases) summed across crops. The effects on the sum of each flow variable across the season are shown to the left of the black bar in each figure, while the coefficients for flows during each survey wave are shown to the right.[15] The total effects over the season are disaggregated by crop in Appendix Tables A-A.

First, we find that total stocks increase by large amounts for the cash loan group relative to the control group throughout the season (Panel A). Our preferred measurement of stocks is simply the cumulative sum of flows, although we also compute results making additional imputations to ensure that stocks make accounting sense. The estimated treatment effect increases throughout the season, surpassing 100,000 Naira (twice the value of the loan) and becoming significant at the 10% level starting in June 2022, corresponding to the onset of the lean season. . On the other hand, the point estimates for the maize treatment are negative and insignificant in all rounds, although they are also quite large during the onset of the lean season. Despite Taimaka priming households to use the loans to store maize, we don't see in increases in maize storage for either arm, although we do see increases in maize sales for both arms early in the season. Instead, most of the results are driven by beans and millet. [17] Together, these results suggest that households preferred investing their loans in a fairly diversified portfolio of crops, and that offering in-kind loans of a single crop is less effective for encouraging arbitrage.

Panel B of Figure 4 shows no statistically significant effects of either loan on the total value of grain sold during the season. While the point estimate for the cash loan is about 20,000 Naira (about $40 or one bag of maize) and the coefficient for the maize loan is about -20,000 Naira, the confidence intervals are wide. Estimates during the course of the season are more precise and show a marginally significant increase in sales in January offset by

[15] As shown in Figure 3.4, the cash loans were disbursed in October 2021 and the maize loans were disbursed in November 2021, after most surveys for this wave were completed. In principle, loans were due by the end of July 2022, but in practice Taimaka allowed for some flexibility in repayment. The endline survey took place in November 2022, during the middle of the bean and millet harvests and towards the beginning of the maize harvest . Since flow variables were not elicited at baseline[16], coefficients for baseline are not shown in these figures.

[16] We only elicited households' initial stocks at baseline. Since this was before the harvest and few households had grain stocks at this point, there would have been little if any meaningful variation in flows at this point.

[17] It is surprising that we do not observe significant increases in the quantity of maize stored for the in-kind loan group, which suggests that these households either disposed of it immediately after receiving it or that cash and control households contemporaneously acquired similar amounts of maize. However, we do see an increase in sales immediately after treatment (Figure A6).

an equivalent decrease in March and another increase in sales in August for the cash loan. However at no point is there any discernable effect for the maize loan. Breaking the total effects out by crop in Table A, we don't see significant effects across the season for any crop.

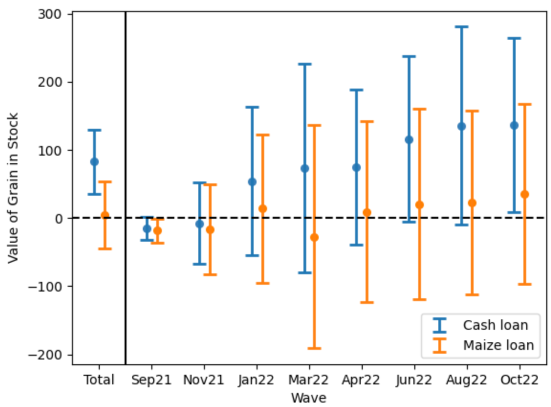
So if households in the cash treatment stored more grain and sold the same amount of grain as control households, where did this come from? We see no effects of either treatment on additional grain purchased for storage, the baseline level of which was already quite low (Panel E of Figure 4) Instead, Panel D shows that they reported "harvesting" more grain. This may seem confusing, given that treatment was assigned towards the end of the growing season and acres planted is balanced across treatment arms. Therefore it would be highly unlikely that households in the cash arm achieved significantly higher physical yields. We believe the explanation is instead related to the phrasing of survey questions about grain stocks and flows. While these questions explicitly referred to grain brought home to store, it is common in Northeastern Nigeria to leave crops in storage sheds in the field after harvesting. It is also common to use the crops to pay for harvest labor or sell them without bringing them back home. We therefore believe that the large effect on reported harvests for the cash loan comes from these households disposing of less maize directly from the field along with possibly leaving less maize unharvested due to lack of labor. At endline, which occurred during the bean and millet harvests but finished early in the maize harvest, we also find that the cash arm reported bringing signficantly (at the 10% level) more harvested grain into storage. This corresponds with our finding that these households had higher output during the 2022 season, discussed below.

The above results imply that households in the cash arm increased consumption of their own grain. The challenge is that up to Wave 3, the recall period from the general consumption module does not align with the stocks module, meaning that we need to infer it as a residual. From Wave 4 onward, consumption from own stocks was directly elicited in addition to the general consumption module. The results are shown in Panel C of 4. Households receiving the maize loan increase the number of own-produced crops consumed (and are more likely to consume maize) shortly after treatment, but this effect fades by the lean season. Meanwhile we see households in the cash arm consuming more of their own crops during the lean season (April-June), but no differences by endline (Figure A5).

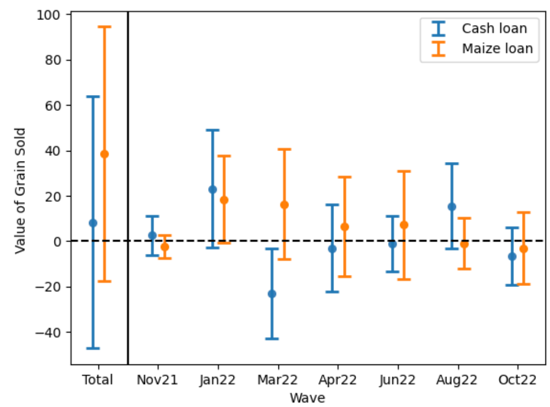
Despite the challenge of eliciting stocks that add up in an account sense, these results suggest that the loan helped farmers maintain positive stocks further into the lean season but did not lead them to become net arbitrageurs in a season in which prices did not appreciate.

Figure 4: Treatment effects on value of grain stocks and flows by wave

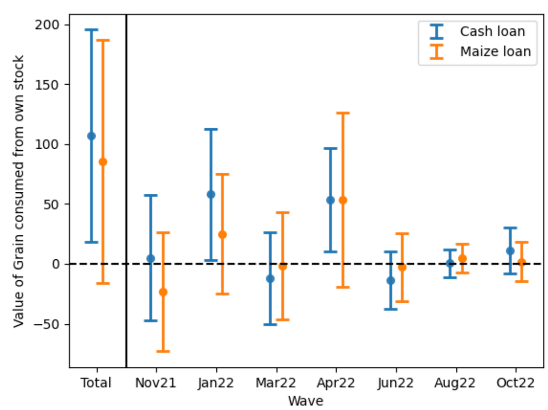
(a) Stocks



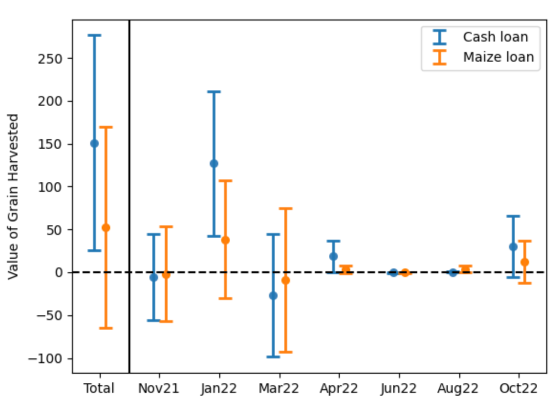
(b) Sales



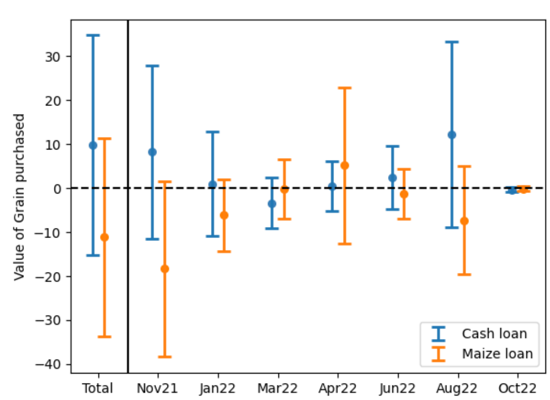
(c) Consumption



(d) Harvested grain brought home



(e) Purchases intended for storage



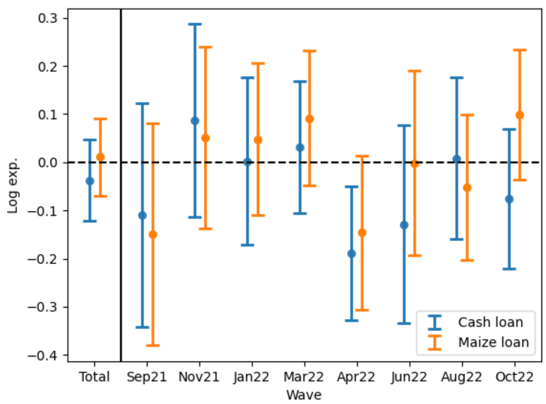
5.2 Effects on Consumption

We now evaluate the program's effect on consumption and welfare. We use four main (pre-specified) measures: the log of monthly expenditure; the MUE, as estimated from (Ligon, 2020); our experimentally elicited IMRS and the RCSI measure of seasonal hunger (Maxwell et al., 2014). However, there did not turn out to be any meaningful variation in the RCSI in most survey rounds between the baseline and endline, as most households simply reported 0s We additionally use the log of non-storable expenditures as an outcome, given that expenditure may be ambiguously correlated with welfare for crops that households can store.

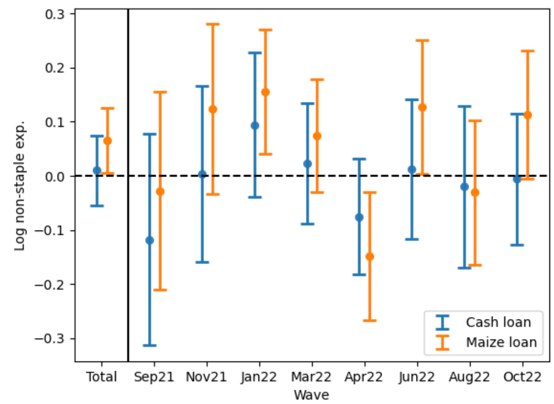
We find few overall effects on direct measures of consumption albeit with some notable seasonal effects. Panel A of Figure B.2 shows a null effect on total expenditure but we see a significant negative effect of nearly 20 log points for both treatment arms in April 2022. However, Panel B of B.2 shows significant increases in non-storable food expenditure throughout the season for households that received the maize loan. In Panel C: we se no discernable effects on other large consumption expenditures, which mostly include clothing, festivals, medical care and travel, despite some imbalances at baseline. Mapping consumption expenditure into welfare, however, we see null effects on the average estimated IMUE (a higher value corresponding to lower welfare) over the season, but we see two spikes for both treatment arms in April and at endline (Figure 6). The endline results are consistent with treated households reducing their consumption when repaying loans or forfeiting assets after defaulting.

However, in Figure 7, we do see significant decreases of about 3% in the elicited IMRS (also corresponding to higher welfare) for both arms, although the timing of these effects depends on how one deals with null responses. If one drops all observations where the household accepts none of the interest rates on the menu (Panel A), there are actually small increases in the IMRS for the cash arm early in the season, but larger decreases for the IMRS later on that dominate. However, if one interprets empty choices as the household being at a corner (i.e. unwilling to purchase a bond with 100% interest) and top-codes them (Panel B), then we see negative effects early in the season and zeros later in the season, as more treated households made non-empty choices. Taken together, these results show little evidence of higher or smoother consumption throughout the season, with the exception of non-storable expenditures in the maize loan arm. We can rule out increases of 3.9% (6.6%) in consumption expenditure and decreases of 2.6% (3.0%) in the IMUE from the cash (maize) treatment. However, treated households do appear more willing to invest slightly more in bonds in the IMRS elicitation exercise, which would suggest small welfare improvements. Nevertheless, the significantly lower estimated welfare at endline suggests that default for households that had taken the loan and not realized high returns may have been costly.

(a) Log food expenditure



(b) Log non-storable expenditure



(c) Other large expenditure

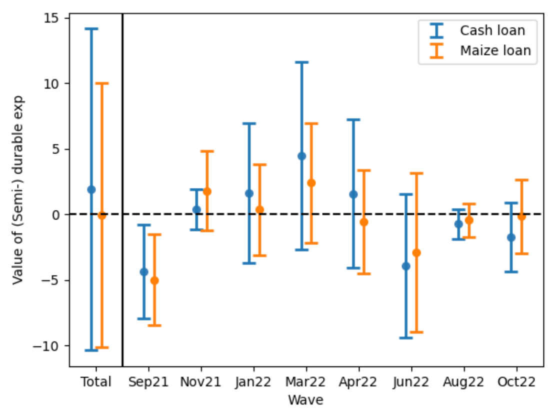


Figure 5: Treatment Effects on Expenditures, by Wave

5.3 Effects on Investment

Other pre-specified outcomes of interest include farm investment and profits from the 2022 planting season, non-agricultural business expenditures, borrowing and lending, and semi-durable/durable purchases. We find that households in the cash arm signficantly increased their investment in non-farm businesses (Figure 8) and livestock (Table 5.3), much like they did with their stocks. We see positive but noisy effects on acreage planted and agricultural expenditure for the 2022 season (Table 5.3). Given that endline occurred early in the 2022 harvest season, when mostly millet and beans were being harvested, we find noisy positive effects on harvest values but large and significant effects on expected total harvest. House-

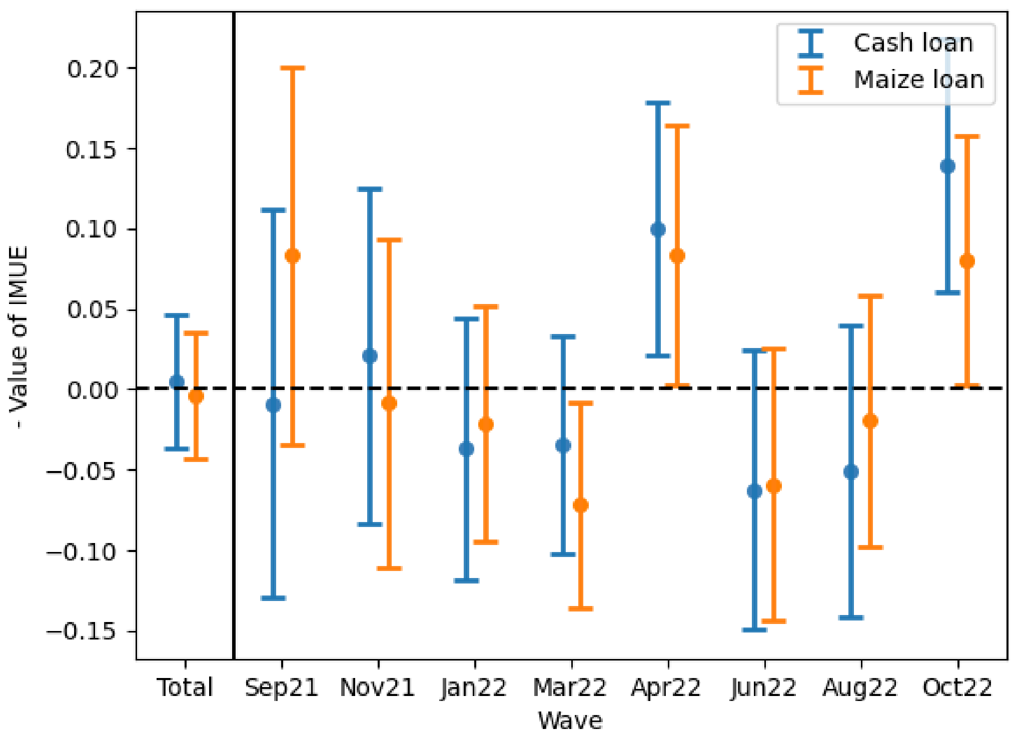
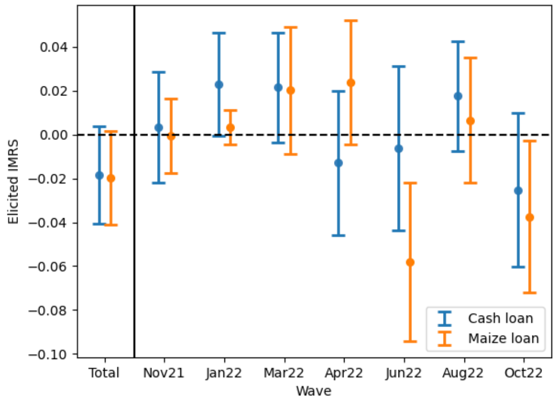


Figure 6: Treatment effects on the MUE, by wave

(a) Dropping null choices



(b) Top-coding null choices

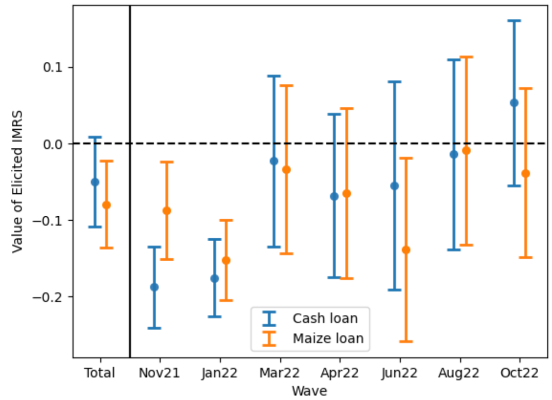
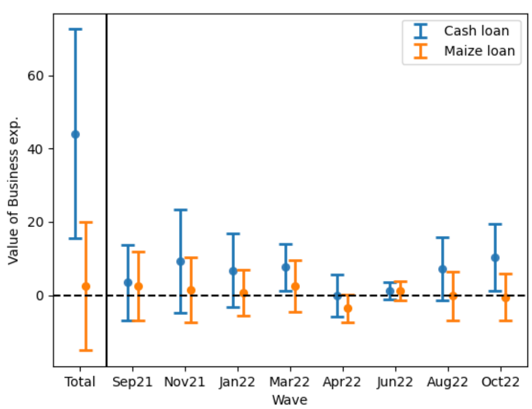


Figure 7: Treatment Effects on Elicited IMRS, by wave

holds in the cash arm also have signficantly more chickens, goats and cows than control households at endline; they own similar levels of livestock as at baseline, while other households disinvested. However, we find a null effect of the maize loan on each of the investment outcomes.

(a) Business expenditure



(b) Business profits

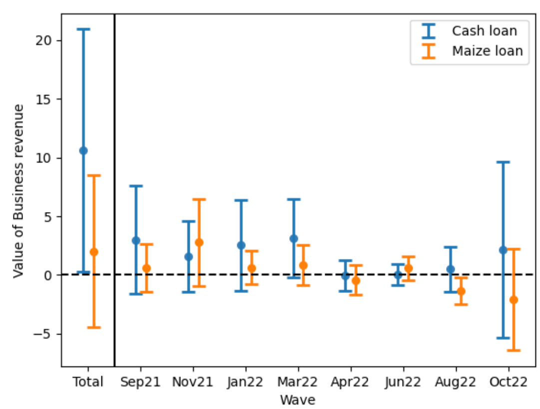


Figure 8: Treatment effects on non-farm business, by wave

5.4 Robustness

We also pre-specified robustness checks using the Double Post LASSO method of Belloni et al. (2012) to select controls, as well as controlling directly for the score Taimaka assigned to groups rather than using the strata fixed effects. We also show robustness to controlling

Table 1: Effects of treatment on agricultural investment and output

Coefficient	Planted area (ha)	Ag. exp. ('000 Naira)	Dry season area (ha)	Dry season ag. exp.	Harvest value	Exp. harv value
Cash Loan	0.082	15.665	0.000	0.108	37.621*	184.961**
	(0.466)	(16.031)	(0.022)	(0.840)	(22.604)	(89.188)
Maize Loan	−0.334	4.677	−0.011	−0.101	23.778	130.060
	(0.394)	(14.005)	(0.019)	(0.884)	(17.361)	(110.670)
Any Loan	−0.137	8.882	−0.006	−0.021	27.543*	145.054**
	(0.387)	(12.792)	(0.017)	(0.721)	(15.956)	(73.524)
Control mean	5.482	144.122	0.024	0.894	92.447	784.949
N	829	829	829	829	829	829

This table contains estimates of the treatment effects of cash and maize loans on measures of agricultural investment in the 2022 planting season. The outcome in the first column is total area planted in hectares during the main (rainy) season of 2022, in hectares. The second column is total farm expenditure, including land, labor, fertilizer, seed, and equipment during the 2022 rainy season in 000's of Naira. The next two columns repeat the same outcomes for the 2022 dry season. The fifth column reports the value of crops harvested from the 2022 season by endline in 000's of Naira (at current market prices), while the final column combines actual and anticipated harvests as not all households had completed harvesting at endline. Results obtained using the estimator proposed by Athey and Imbens (2017) for tuple-wise randomized experiments.

Table 2: Effects of treatment on endline livestock holdings

	Chickens	Goats	Cows	Sheep	Donkeys
Cash Loan	1.747**	0.923**	0.519	0.448	−0.004
	(0.827)	(0.400)	(0.325)	(0.434)	(0.012)
Maize Loan	0.144	−0.354	−0.234	−0.494	−0.007
	(0.538)	(0.353)	(0.251)	(0.362)	(0.011)
Any Loan	0.807	0.260	0.133	−0.033	−0.005
	(0.548)	(0.315)	(0.249)	(0.364)	(0.010)
Control mean	2.262	3.843	1.528	2.472	0.010
N	829	829	829	829	829

This table contains estimates of the treatment effects of cash and maize loans on the number of different animals held at endline Results obtained using the estimator proposed by Athey and Imbens (2017) for tuple-wise randomized experiments.

Table 3: Estimated elasticities of w with respect to different shocks.

Shocks	$\hat{\delta}$
Business Expenses	0.085^{***}
$N = 749$	(0.026)
Business Revenue	0.144^{***}
$N = 2189$	(0.038)

Regressions of $w \equiv -\log \lambda$ logs of business expenses and revenues, estimated separately. Zeros are dropped. Robust standard errors are used.

for baseline outcomes in an ANCOVA specification following McKenzie (2012). None of the results alter the main conclusions.

6 Test of Full Insurance

Elsewhere we consider idiosyncratic variation in intertemporal marginal rates of substitution, and test whether this is orthogonal to returns on different assets. That can be thought of as a test of whether portfolios are efficiently constructed, given some set of available assets.

A stronger test supposes that asset markets are complete, so that (within Gombe) households can perfectly smooth out idiosyncratic varation in $w = -\log \lambda$. This is essentially the well-known two-way fixed effects test of full insurance (Deaton, 1992; Townsend, 1994), implemented using our constructed MUEs, as theory suggests, rather than aggregate consumption expenditures. The estimating equation in this case is explored by Ligon (2023), and takes the form

$$-\log \lambda = \alpha_i + \eta_t + \delta \text{Shock}_{it} + \epsilon_{it},$$

where α_i is a household fixed effect, η_t is a time effect, and the disturbance ϵ_{it} is then idiosyncratic variation in households' MUEs. The question is whether this idiosyncratic variation is related to "shocks", and is answered by testing whether the estimated value of δ is significantly different from zero or not.

We consider two (related) sources of idiosyncratic shocks: "business" revenues and expenses (which explicitly excludes revenue and expenses from agricultural operations). Not all farmers report any revenue or expenses, and few report these in every period of data collection. As one would expect from agricultural operations, there's considerable seasonality in both expenses (usually early in the season) and the receipt of revenues (usually after harvest). The two-way fixed effects regression eliminates any seasonality in both w and in the log of revenues and expenses, and eliminates also any cross-sectional correlation between

w and these flows. Table 3 reports the estimated elasticities of w with respect to expenses and revenues. Both are positive and significant. In particular, an idiosyncratic one percent increase in revenue is associated with roughly a 0.14% increase in w, while the corresponding expense elasticity is about 0.09%. The sign of the latter is positive, which may seem a puzzle. But of course expenses are endogenous, and farmers who decide to spend more on their operations presumably expect a positive return to these expenditures. The news here is not that we are claiming a positive causal impact of expenses (or of revenues, for that matter) on w, but that idiosyncratic variation in these flows appears to be systematically predictive of idiosyncratic variation in w, allowing us to reject the (strong!) null hypothesis of full insurance in this environment.

7 Test of Euler Equation

In Section 5, we established that while the cash treatment may have helped households smooth consumption, neither treatment led to an actual significant increase in average arbitrage profits. But *could* households have made themselves better off *ex ante* by holding more maize? Though price changes in maize during 2021–22 were not large by historical standards, a purchase of maize in April 2022 followed by a sale in August would have given a return of about 10% over four months, or an annualized yield of 33%. It would have been profitable to finance this using the loans offered by Taimaka (15% interest) or commercial banks. But of course these are *ex post* returns. We would like to say something about the *expected* welfare effects of investing in stocks of grain given the uncertainty over seasonal price increases. We now turn to testing the predictions of a simple model of intertemporal arbitrage with our high-frequency consumption and storage data.

Recall from Section 2 that the central prediction of the model with complete financial local markets is that realizations of $y^j_{it} = (m_{it} - \bar{m}^j_t)R^j_t\delta^j_{it-1}$ should be uncorrelated with any variable z^j_{it-1} in the time $t-1$ information set. One particular variable which should be uncorrelated under the null, but not under some important alternative hypothesis is lagged IMRS. If, for example, credit constraints were important for some households then the IMRSs would be lower than for other households, and the credit constraint would alter the portfolio of investments. Another is the assignment to treatment: does giving a bag of maize to a farmer make that farmer better off when prices rise? And a third is lagged stocks—it seems natural to suppose that there's considerable persistence in stocks held, so if a farmer held more maize last period then we might we expect his IMRS to respond differently to changes in maize prices than for the average farmer in Gombe.

We can construct estimated IMRSs m_{it} by using the ratios of time t to time $t-1$ MUEs for

Table 4: Tests of the Euler asset pricing equations

	Treatment	L^2Stocks	$L^2 m$ x loan	IMRS	All
χ^2	2.19	6.97	9.83	8.06	30.59
df	11	17	29	5	35
p-value	1.00	0.98	0.91	0.15	0.68

Different columns involve tests of the orthogonality of errors to different sets of variables. Column 1 uses a dummy for assignment to either the cash or maize treatment arms. L^2 denotes the second lag of the IMRS, m_{it-2}, which is interacted with the value of stocks and the treatment dummy. Column 4 uses the elicited IMRS and Column 5 tests these restrictions jointly.

each household i, and multiplying this by some β. But our use of data from contemporaneous expenditures to estimate $\log \lambda_{it}$ only identifies MUE up to an unknown scalar σ. Thus,

$$m_{it} = \beta \left(\frac{\lambda_{it}}{\lambda_{it-1}} \right)^{\sigma}.$$

Using these, along with data on stock-holdings, allows us to construct $\bar{m}^h_t$, and so

$$\mathrm{E}_{t-1} \left(\left(\frac{\lambda_{it}}{\lambda_{it-1}} \right)^{\sigma} - \sum_{k=1}^{N} \left(\frac{\lambda_{kt}}{\lambda_{kt-1}} \right)^{\sigma} \frac{\delta^j_{kt-1}}{\sum_{k=1}^{N} \delta^j_{kt-1}} \right) R^j_t \delta^j_{it-1} = 0. \tag{7}$$

Note that the unknown discount factor β cancels out of this condition, leaving only the unknown scale factor σ to estimate.

We estimate σ and test (7) using the continuously-updated GMM estimator of Hansen et al. (1996). Our preferred estimate of σ is 0.84, and Table 7 reports results of this test. We are interested in testing the null hypothesis that forecast errors y_{it} are orthogonal to a variety of different variables that are in the time $t-1$ information set. The first column asks whether or not treatment status (receiving a loan of cash or kind) is correlated with these errors; there is no evidence at all of such a correlation. The second asks whether lagged stocks have predictive power; whatever power they have is very weak. The third asks whether or not twice-lagged m_{it-2}, interacted with receiving a loan, is orthogonal—one might think that having more or fewer resources in the past might help to predict the forecast error, but again, it does not. We next consider the experimental measures of IMRS we elicited, lagged one period. There is some suggestive evidence of such a correlation, but not enough for us to reject the null hypothesis ($p = 0.15$). Then finally we put all of these variables together; unsurprisingly, we again fail to reject the null. This leads us to conclude that the low and seasonally variable levels of consumption in Gombe are an aggregate phenomenon, which offering PHLs to a subset of poor households is unlikely to address.

8 Conclusion

PHLs have gained attention as a potential way to help farm households earn additional income and smooth consumption across the season. However, this theory of change rests on grain prices rising enough to cover the loans' interest rate, which is a highly risky proposition in sub-Saharn Africa. This has ambiguous implications for the expected returns to PHL programs. In addition to the possibility of low returns and high default when prices fail to rise *ex post*, loans also shift *ex ante* risk from states of the world with extremely high prices to those with low prices. As such, demand for the loans can be influenced by how the household expects its consumption to covary with grain prices.

Our results from an RCT of a PHL in Gombe State Nigeria, in a year in which prices did not rise, show that the loan induced households offered a cash loan to store more grain for longer, but those offered an in kind loan. However, neither treatment led to large increases in profits or welfare according to most measures – apart from a marginally significant reduction in the experimentally elicited IMRS. Over the course of the season, we also see small increases in business investment and livestock holdings in the cash loan group. This is consistent with relaxing credit constraints allowing households to store more, but these investments not paying off

We attempt to use our model of intertemporal arbitrage to say something more broadly about the *expected* returns to storing grain, particularly whether households could be better off *ex ante* by holding different portfolios. We do so by testing whether variation in households' realized IMRS is correlated with variables in their prior period's information set. We fail to reject the null that treatment assignment, lagged (estimated) IMRS, stocks, and experimentally elicited IMRS are uncorrelated with this variation, although we are nearly able to for the latter case. This suggests that while the integration of Gombe with the broader economy is poor, we find little evidence that farmers are failing to respond optimally to local prices and returns. Nevertheless, this by no means rules out that significant market failures are at play.

Taken together, the unfavorable (for arbitrageurs) realizations of prices, the lack of evidence against the *ex ante* optimality of portfolios, and the potential adverse selection channels suggest that PHLs may not be the optimal policy to improve seasonal welfare. As a practical matter, further research could consider alternatives such as forward contracts, which would provide farmers liquidity without exposing them to price risk later in the season. More broadly, There is poor evidence against the efficiency of allocation and production within Gombe *given local prices*. But there is solid evidence that the integration of Gombe with the broader economy seems to be poor. On the consumption side this is supported by the evidence of a "lean season" and seasonal variation in average IMRSs, indicating that Gombe is poorly integrated with broader credit markets. But on the production side there's considerable uncertainty regarding local grain prices. This would be fine if variation in these prices was mirrored by world prices in these commodities, but the correlation here, while significant, is quite weak. The consequence is that local supply shocks affect prices more than they would were the economy better integrated; these highly variable prices lead to high variation in IMRSs and limit incentives for investment.

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A Additional Tables and Figures

Table A1: Covariate balance between treatment arms at baseline.

	Control	Cash	Maize	p-val Cash vs. Control	p-val Maize vs. Control
Female HH head	0.076	0.056	0.059	0.320	0.413
HH Size	6.471	5.996	5.782	0.182	0.039
Age HH Head	39.165	38.641	37.047	0.621	0.028
No educ.	0.685	0.672	0.677	0.724	0.815
Some pri. school	0.083	0.096	0.076	0.583	0.755
Finished pri. school	0.116	0.103	0.092	0.631	0.345
Finished sec. school	0.109	0.070	0.086	0.100	0.332
Tertiary educ.	0.007	0.059	0.069	0.001	0.000
Cows	4.305	4.732	4.742	0.559	0.565
Goats	6.077	6.522	6.354	0.342	0.540
Sheep	5.419	6.068	6.124	0.260	0.182
Chickens	11.957	12.219	11.046	0.857	0.509
Has Business	0.169	0.186	0.194	0.598	0.416
Has loan	0.043	0.057	0.068	0.432	0.171
Cultivated area (ha)	5.867	5.698	6.621	0.663	0.367
Ag. exp.	81.532	76.019	87,146	0.462	0.498
Ag. revenue	106.790	92.667	84.520	0.392	0.175

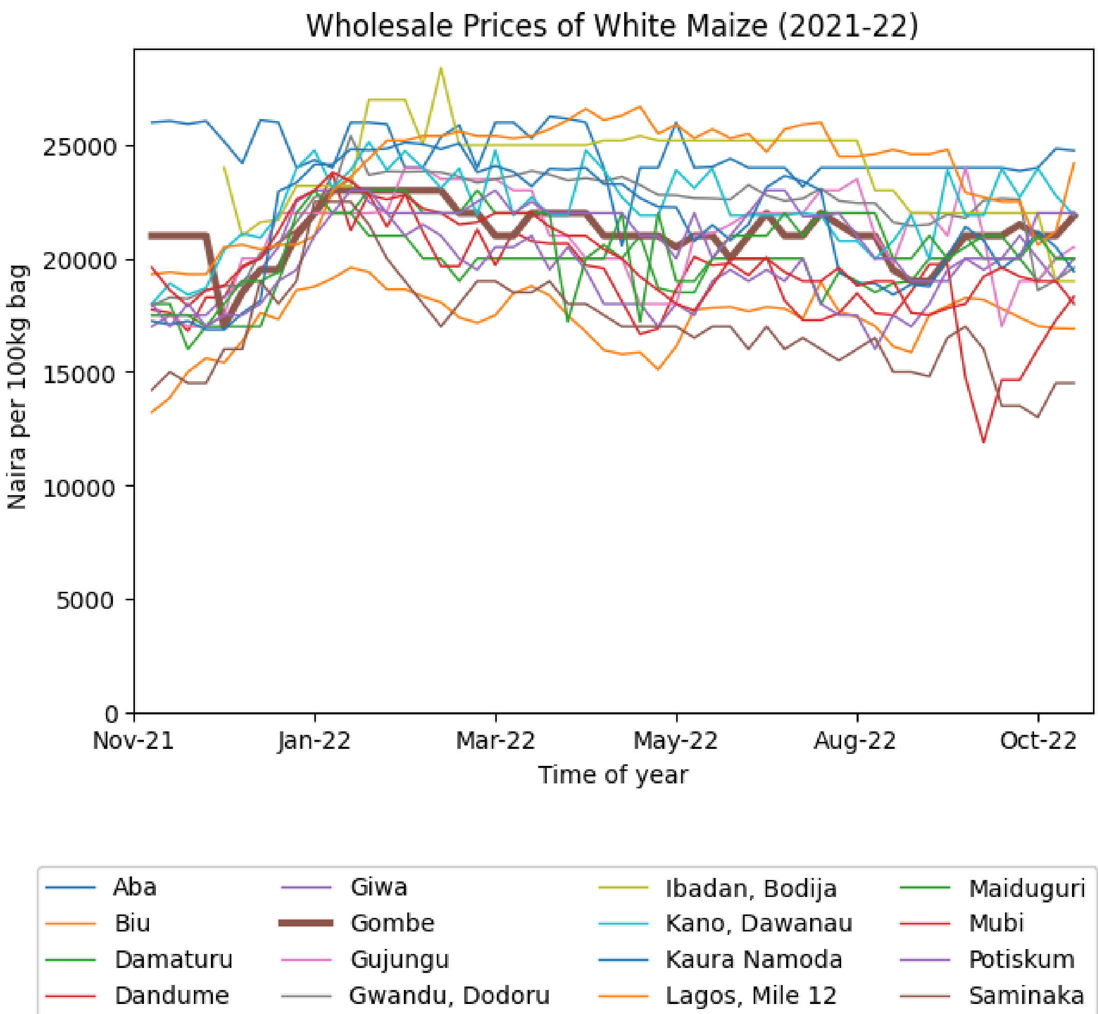


Figure A1: Maize prices across Nigeria during study period

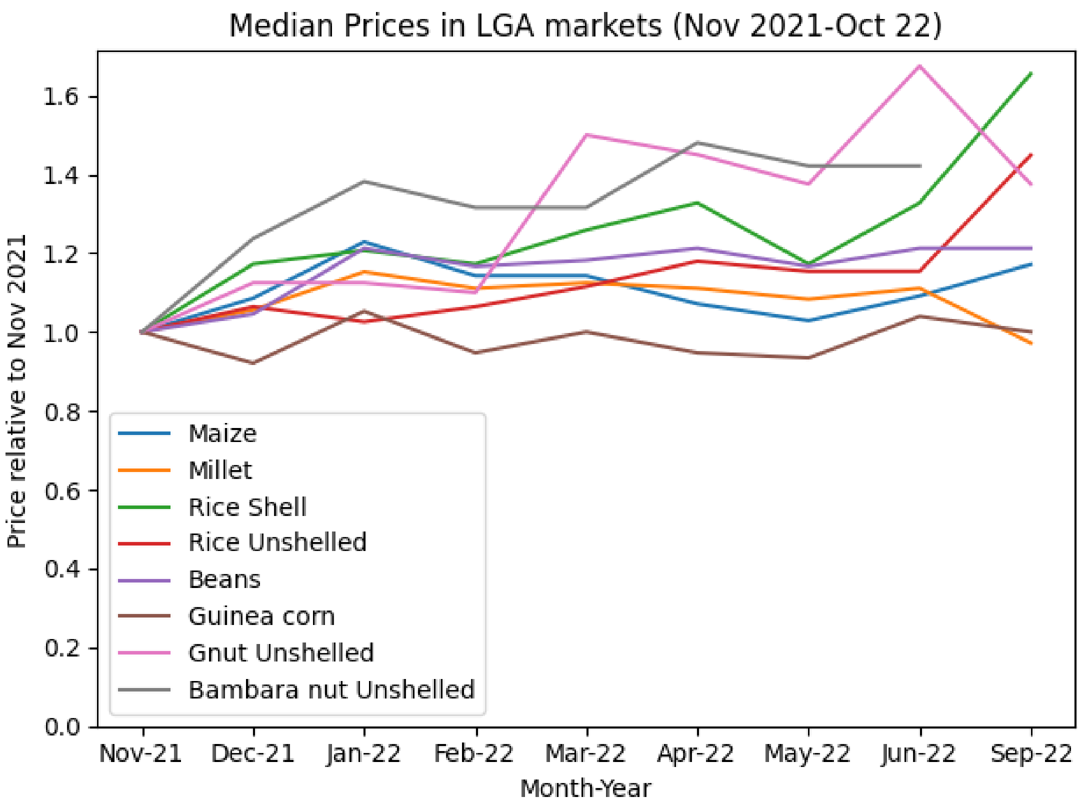


Figure A2: Prices at local markets within Gombe collected during study period

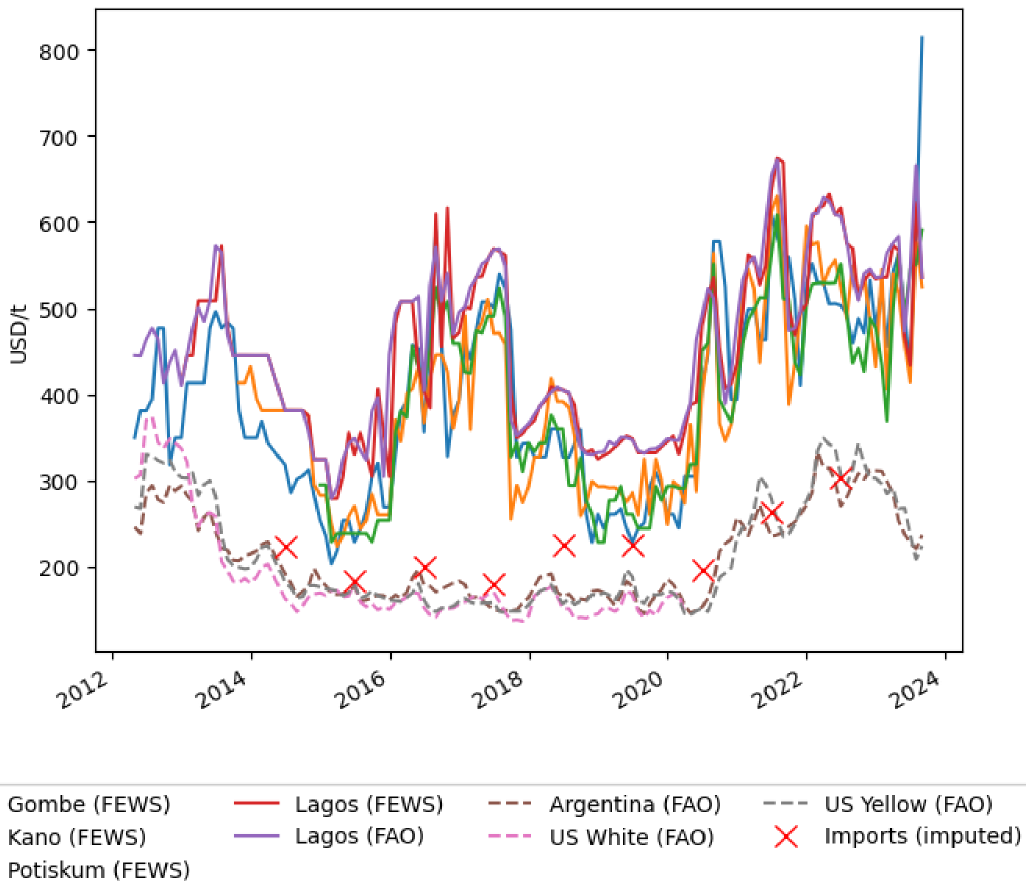


Figure A3: Series of local Nigerian (solid) and international (dashed) maize prices in USD

Table A2: Effects of treatment on total value of crops sold (000's Naira).

	Maize	Beans	Guinea Corn	Rice	Millet	All
Cash Loan	12.632	6.991	-2.577^{*}	-1.553	4.454	22.976
	(9.144)	(13.369)	(1.499)	(4.119)	(18.255)	(30.029)
Maize Loan	3.401	-23.186^{**}	-0.124	2.476	-12.002^{*}	-20.953
	(7.728)	(11.549)	(1.679)	(3.995)	(6.610)	(21.301)
Any Loan	8.779	-6.983	-1.141	1.053	-3.493	4.264
	(7.209)	(10.814)	(1.457)	(3.644)	(10.115)	(21.543)
Control Mean	51.799	7.148	13.389	6.799	11.267	90.601
N	930	930	930	930	930	930

$^{***}p < .01, **p < .05, *p < 0.10$

This table contains estimates of the treatment effects of cash and maize loans on the total reported sales of each crop, using the estimator proposed by Athey and Imbens (2017) for tuple-wise randomized experiments.

Table A3: Effects of treatment on total value of consumption of different crops.

	Maize	Beans	Guinea Corn	Rice	Millet	All
Cash Loan	25.946	48.289^{***}	5.053	-16.967	47.927^{***}	115.503^{**}
	(30.087)	(12.979)	(9.663)	(10.661)	(15.218)	(54.056)
Maize Loan	-7.334	30.206^{**}	-4.589	-15.577	0.697	0.661
	(26.327)	(13.163)	(8.398)	(11.158)	(12.951)	(49.465)
Any Loan	7.264	40.648^{***}	0.824	-15.154	25.713^{**}	60.792
	(22.833)	(10.443)	(8.239)	(9.767)	(12.032)	(42.418)
Control Mean	305.729	94.621	46.285	86.109	133.176	673.146
N	930	930	930	930	930	930

$^{***}p < .01, **p < .05, *p < 0.10$

This table contains estimates of the treatment effects of cash and maize loans on the consumption of own grain stocks by crop. Consumption values are imputed following the procedure in Appendix B.1. Results using the estimator proposed by Athey and Imbens (2017) for tuple-wise randomized experiments.

Table A4: Treatment effects on household consumption and welfare

	log exp.	log perishable exp.	$-$RCSI	$-\log\lambda$	IMRS
Cash Loan	-0.045	-0.008	-0.012	-0.015	-0.035
	(0.067)	(0.037)	(0.024)	(0.024)	(0.022)
Maize Loan	-0.000	0.063	-0.025	-0.002	-0.041^{**}
	(0.065)	(0.046)	(0.041)	(0.024)	(0.020)
Any Loan	-0.022	0.029	-0.019	-0.008	-0.038^{**}
	(0.059)	(0.035)	(0.025)	(0.020)	(0.018)
Fixed Effects	Strata	Strata	Strata	Strata	Strata
Control mean	7.954	6.841	0.024	1.691	0.013
N	6235	6174	5688	6404	4955

Different outcomes related to consumption using the estimator proposed by Athey and Imbens (2017) for tuple-wise randomized experiments. Log perishable expenditures sums all non-durable consumption expenditures. Log expenditures adds in expenditures on storable grains. $-$RCSI is the (negative) of the "reduced coping strategies index" which attempts to measure hunger. Minus the log of the marginal utility of expenditures is $-\log\lambda$, while IMRS gives an indication of how much the household is willing to save. The "cash loan" and "maize loan" treatments are as indicated in the text; the "any loan" treatment is a loan received in either cash or kind.

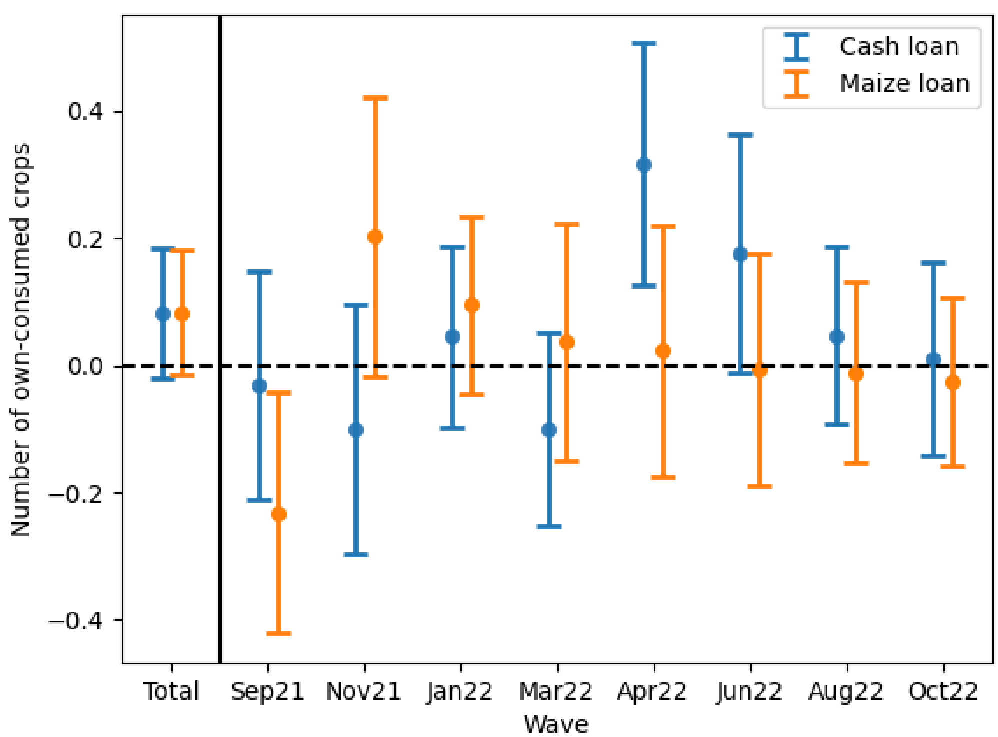


Figure A4: Treatment effects on number of crops consumed from own stock by wave

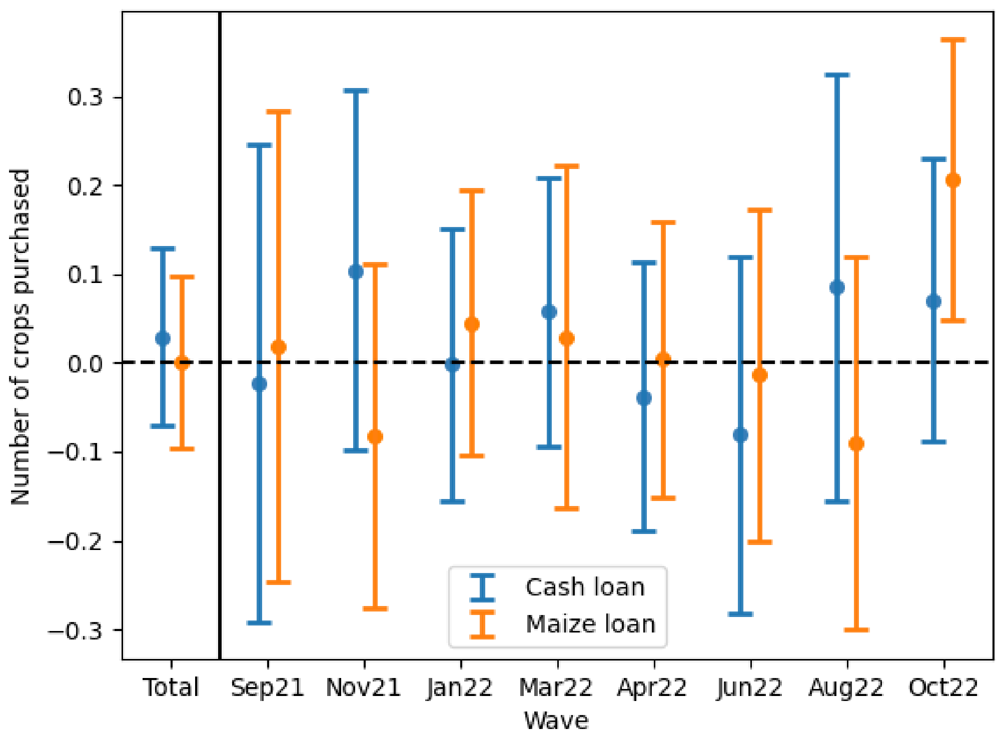


Figure A5: Treatment effects on number of crops purchased by wave

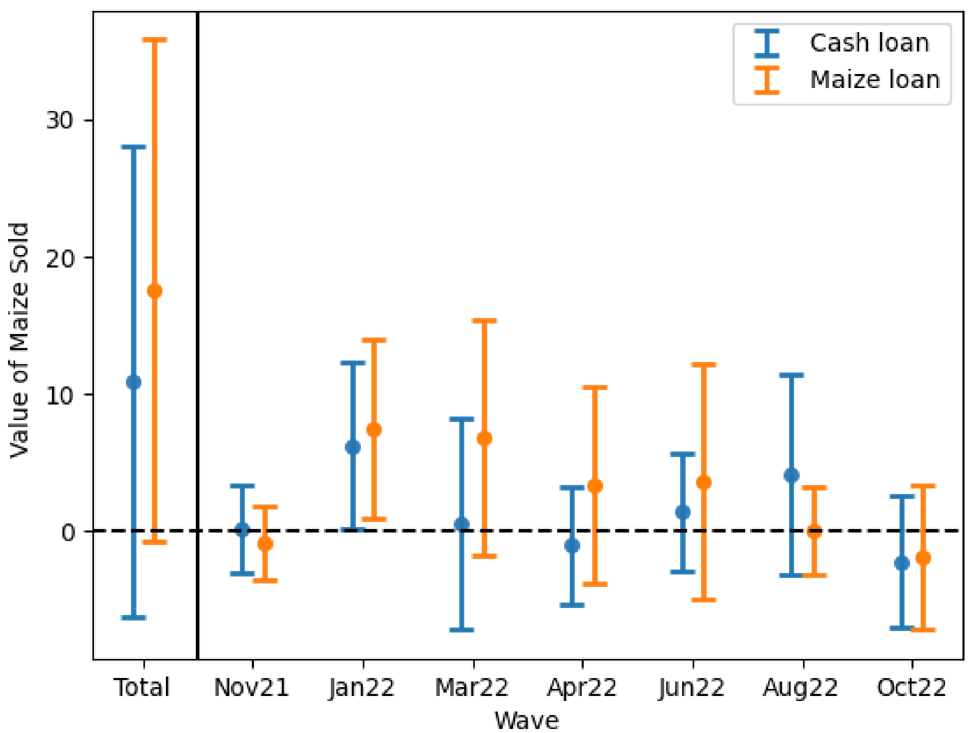


Figure A6: Treatment effects on maize sales

Table A5: Heterogeneous effects on consumption outcomes, by gender

	log exp.	log perishable exp.	−RCSI	$-\log\lambda$	IMRS
Cash Loan	−0.052	−0.012	−0.014	−0.013	−0.032
	(0.069)	(0.039)	(0.026)	(0.026)	(0.020)
Cash Loan $×$Female Head	−0.008	0.054	−0.024	−0.002	−0.033
	(0.065)	(0.043)	(0.043)	(0.022)	(0.020)
Maize Loan	−0.289**	−0.317***	0.025	−0.092***	0.102**
	(0.115)	(0.067)	(0.039)	(0.035)	(0.047)
Maize Loan $×$Female Head	−0.018	−0.084	0.035	−0.050	−0.008
	(0.126)	(0.200)	(0.037)	(0.076)	(0.057)
Female Head	−0.035	−0.018	−0.007	−0.065	−0.087*
	(0.144)	(0.136)	(0.053)	(0.061)	(0.051)
Any Loan	−0.029	0.023	−0.019	−0.007	−0.032*
	(0.061)	(0.037)	(0.027)	(0.021)	(0.017)
Loan $×$Female Head	−0.288**	−0.313***	0.023	−0.092***	0.099**
	(0.114)	(0.062)	(0.037)	(0.035)	(0.047)
Female Head	−0.030	−0.060	0.019	−0.058	−0.043
	(0.115)	(0.124)	(0.033)	(0.050)	(0.053)
Fixed Effects	Strata	Strata	Strata	Strata	Strata
Control mean	7.954	6.841	0.013	0.024	1.691
N	6235	6174	4955	5688	6404

Table A6: Heterogeneous effects on grain sales, by gender

	Maize	Beans	Guinea Corn	Rice	Millet	All
Cash Loan	9.18	−6.54	−10.10	4.86	14.07	13.33
	(14.36)	(27.27)	(7.48)	(5.39)	(37.90)	(53.36)
Cash Loan × Female Head	7.18	−10.38	−4.84	2.27	1.56	3.57
	(10.65)	(20.21)	(7.39)	(7.03)	(11.71)	(38.57)
Maize Loan	3.41	−33.42	−5.91	−13.60	0.47	−56.86
	(12.24)	(21.09)	(7.06)	(13.47)	(15.50)	(41.65)
Maize Loan × Female Head	−22.49	88.33*	4.97	−12.35	3.13	59.41
	(29.07)	(46.31)	(9.02)	(12.91)	(44.31)	(83.70)
Female Head	−21.46	−11.02	1.15	6.50	−25.51	−55.73
	(13.40)	(33.69)	(9.24)	(11.41)	(19.29)	(62.32)
Any Loan	8.10	−8.79	−7.26	3.49	7.24	7.85
	(9.93)	(17.60)	(7.30)	(5.24)	(18.99)	(30.19)
Loan × Female Head	3.44	−35.24*	−6.00	−13.23	0.00	−58.95
	(12.16)	(20.77)	(7.04)	(12.77)	(16.13)	(42.55)
Female Head	−21.92	37.40	2.92	−2.63	−11.35	0.48
	(15.02)	(35.12)	(8.85)	(7.08)	(23.17)	(52.61)
Fixed Effects	Strata	Strata	Strata	Strata	Strata	Strata
Control Mean	54.630	147.830	14.440	15.470	71.140	323.700
N	930	930	930	930	930	930

Table A7: Heterogeneous effects on consumption outcomes, by baseline wealth

	log exp.	log perishable exp.	−RCSI	$-\log\lambda$	IMRS
Cash Loan	−0.053	−0.008	−0.020	−0.020	−0.034
	(0.071)	(0.041)	(0.026)	(0.024)	(0.022)
Cash Loan × Bl Asset Index	−0.001	0.064	−0.028	−0.006	−0.040**
	(0.063)	(0.046)	(0.042)	(0.023)	(0.020)
Maize Loan	0.008	0.063**	−0.012	0.043***	−0.019
	(0.036)	(0.032)	(0.016)	(0.014)	(0.017)
Maize Loan × Bl Asset Index	0.029	0.008	0.014	−0.040***	−0.005
	(0.046)	(0.033)	(0.016)	(0.014)	(0.017)
Bl Asset Index	0.030	−0.037	0.027	−0.023	0.007
	(0.033)	(0.034)	(0.019)	(0.014)	(0.016)
Any Loan	−0.025	0.029	−0.024	−0.013	−0.037**
	(0.060)	(0.037)	(0.028)	(0.020)	(0.018)
Loan × Bl Asset Index	0.007	0.061*	−0.012	0.043***	−0.019
	(0.037)	(0.032)	(0.016)	(0.014)	(0.017)
Bl Asset Index	0.031	−0.015	0.021	−0.030**	0.002
	(0.037)	(0.032)	(0.016)	(0.014)	(0.016)
Fixed Effects	Strata	Strata	Strata	Strata	Strata
Control mean	7.954	6.841	0.013	0.024	1.691
N	6008	5947	4726	5499	6175

Table A8: Heterogeneous effects on grain sales, by baseline wealth

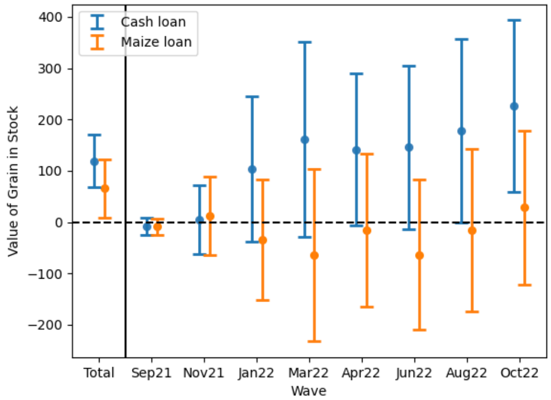
	Maize	Beans	Guinea Corn	Rice	Millet	All
Cash Loan	6.58	−0.87	−9.05	3.93	12.40	14.17
	(14.91)	(27.13)	(6.64)	(5.10)	(36.23)	(52.44)
Cash Loan × Bl Asset Index	9.30	−5.92	−4.63	2.90	2.44	11.74
	(10.02)	(19.22)	(7.34)	(7.10)	(11.11)	(37.14)
Maize Loan	2.62	25.52	−2.46	2.43	20.55***	52.46*
	(9.39)	(18.28)	(5.15)	(9.49)	(7.40)	(27.08)
Maize Loan × Bl Asset Index	−3.56	−23.56	1.45	−5.28	−4.32	−38.46
	(11.14)	(17.43)	(4.49)	(9.39)	(13.51)	(32.26)
Bl Asset Index	−1.88	−16.24	1.95	−2.45	−11.69	−33.89
	(9.71)	(20.18)	(4.80)	(9.87)	(7.73)	(30.74)
Any Loan	8.09	−3.52	−6.63	3.40	6.85	12.91
	(9.83)	(16.95)	(6.89)	(5.03)	(17.94)	(29.47)
Loan × Bl Asset Index	2.57	25.70	−2.58	2.48	20.75**	52.56*
	(9.30)	(18.38)	(5.11)	(9.51)	(8.11)	(27.42)
Bl Asset Index	−2.59	−19.71	1.81	−3.77	−8.52	−36.04
	(9.24)	(17.74)	(4.60)	(9.44)	(7.50)	(25.01)
Fixed Effects	Strata	Strata	Strata	Strata	Strata	Strata
Control Mean	54.630	147.830	14.440	15.470	71.140	323.700
N	887	887	887	887	887	887

Table A9: Percent of target sample surveyed

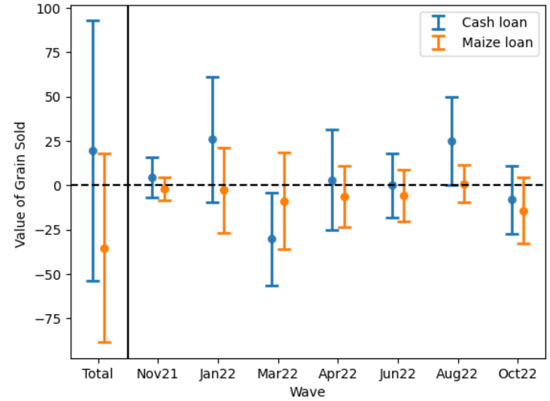
Wave	Cash Loan	Maize Loan	Control
0	0.644	0.600	0.612
1	0.803	0.921	0.868
2	0.961	0.897	0.911
3	0.950	0.932	0.940
4	0.889	0.900	0.924
5	0.886	0.806	0.879
6	0.921	0.906	0.940
7	0.921	0.919	0.908
All (post-treatment)	0.903	0.898	.910

Probability of being surveyed across survey waves by treatment arm. Note that baseline was cut short due to time constraints

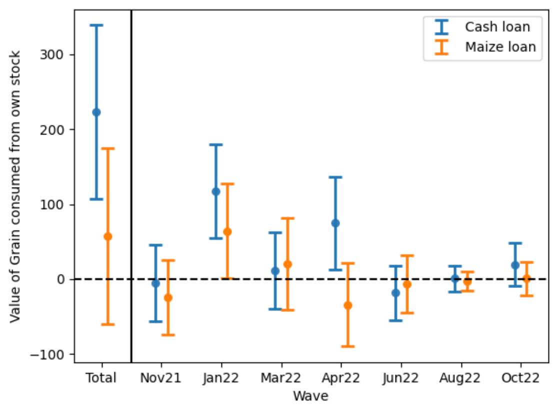
(a) Stocks



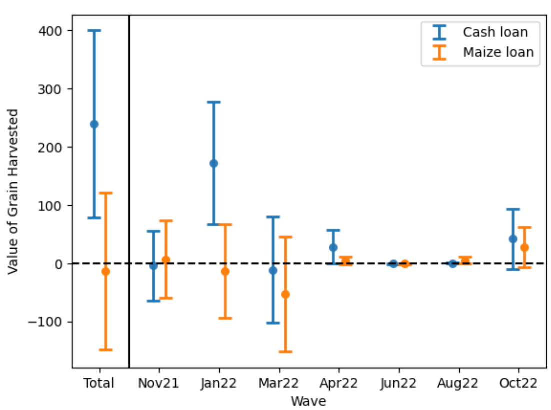
(b) Sales



(c) Consumption



(d) Harvested grain brought home



(e) Purchases intended for storage

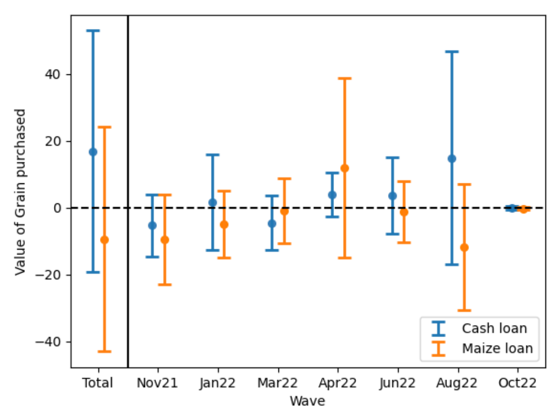
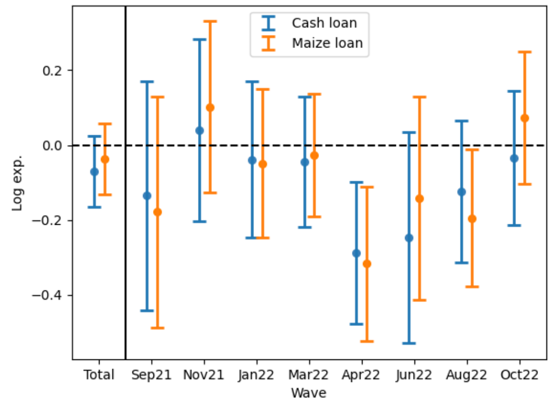
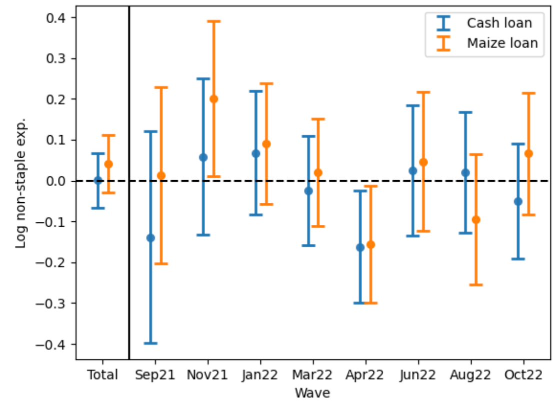


Figure A7: Treatment Effects on Stocks, by Wave, dropping unbalanced clusters due to attrition

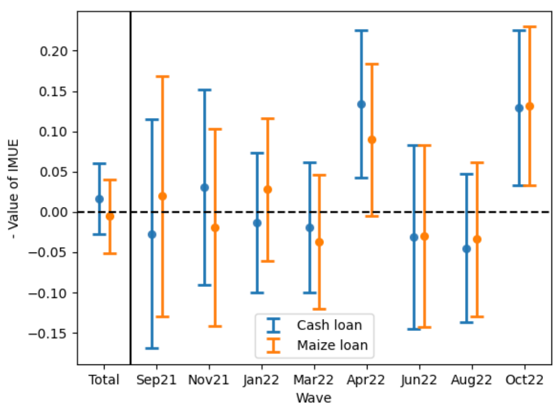
(a) Log food expenditure



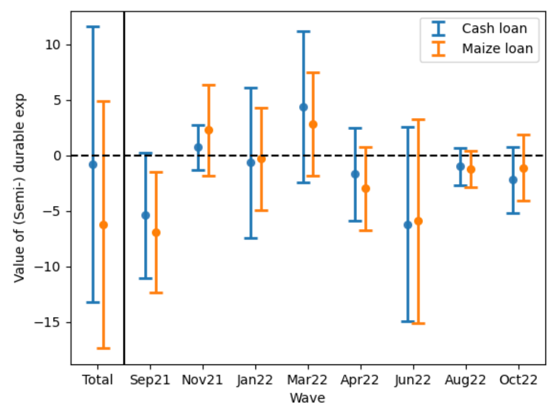
(b) Log non-storable expenditure



(c) $-\log\lambda$



(d) (Semi-)durable expenditure



(e) Elicited IMRS – topcoded

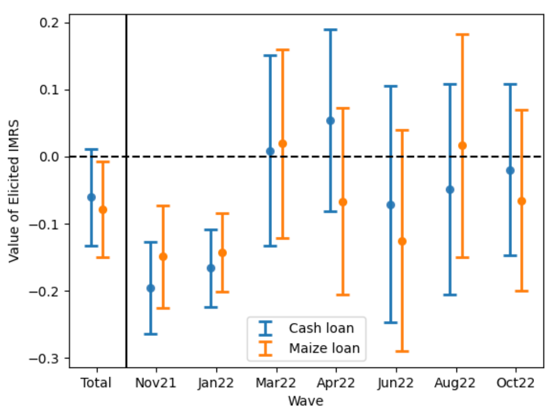


Figure A8: Treatment Effects on Expenditures, by wave, dropping unbalanced clusters due to attrition

Table A10: Predictors of attrition

$\mathbf{1}(\text{Surveyed}_{it})$			
hhsize	0.003 (0.002)	rainy rev	0.000 (0.000)
age head	0.000 (0.001)	lexp	0.021*** (0.004)
cows	-0.002 (0.002)	loglambda	-0.001 (0.011)
goats	0.003 (0.001)	biz exp	0.000 (0.000)
sheep	0.001 (0.002)	biz rev	0.000 (0.000)
chickens	0.000 (0.001)	large item exp	-0.000 (0.000)
any biz	-0.004 (0.022)	comppri	-0.018 (0.038)
any borrow	-0.017 (0.019)	compsec	-0.042 (0.046)
area ha	-0.003 (0.002)	none	-0.047 (0.039)
ag exp	-0.000 (0.000)	somepri	-0.009 (0.038)
female	0.019 (0.067)	N=	5368

Coefficients from a egression of a dummy for appearing in the sample on baseline covariates with wave fixed effects. Standard errors clustered at the farmer group level.

B Online Appendix

B.1 Imputation of Stocks

It is very clear that the raw data is not always an accurate accounting of farmers' inventory. Common inconsistencies include:

- Households will report selling or consuming goods that we don't see in previous stocks, purchases, or harvests
- Households will report purchasing or harvesting goods that we never see them dispose of, but at endline they report having less than the acquired amounts in stock
- In later rounds, households' answers to whether they have consumed a good from their own stock in the inventory module do not much their answers to whether they have acquired the good from their own production or purchased it.

To resolve these issues, we apply the following algorithm, which allows us to compute a number of different indicators of stocks, with higher indices corresponding to more imputation. We proceed until baseline stocks plus the cumulative sum of flows matches endline stocks, with the restriction that stocks never fall below 0 at any point in the period. We use responses from the consumption module as an additional source of information about whether households had positive stocks at a given period. The goal is to take flows as seriously as possible rather than reported stocks.

1. Save reported stocks (which are only available in waves 1-3 and at baseline). Assign dummy value of 1 if positive
2. Then compute stocks as cumulative sum of report flows (purchases + harvest - sales - consumption). Replace 0 dummy with 2 if this is positive. Add reported initial stocks (positive for only a few HH) and replace 0 dummy with 3 if this is positive
3. Replace 0 dummy with 4 if the household reports consuming crop from own stock in consecutive waves
4. For waves 1-3, impute consumption as the difference between the change in stocks between the previous and current period net of flows (harvest + purchases for storage - sales). Update flows accordingly (updating stocks is trivial)
5. If the household has harvested a positive amount between waves 1-3, add the residual between reported stocks and harvests to the harvest. Assign dummy value of 6 when

this turns stocks positive. Update flows and stocks accordingly. Note that we need to do this iteratively (because if a HH meets this condition for multiple waves but has activity in the interim, one iteration won't account for this.)

6. Still have some people who sell/consume crops they've harvested after period 3 that are unaccounted for. For example, bg-11d claims to harvest and sell 400kg of beans in period 2 but then sells another 200 in period 4 and 100 in period 5. It seems more likely that these came from the harvest rather than purchases. Likewise for consumption that's reported as being own-produced. We are going to want to attribute these to the latest feasible harvest (between waves 1-3). Assign dummy value of 7 when this turns stocks positive. Update stocks and flows again

7. Where there are unaccounted for sales/consumption in later waves for crops that a household never harvests, attribute these to purchases (`arb`). Assign dummy value of 8

8. When households have excess positive stocks in excess of the endline stocks they report, attribute the maximum amount that will not cause subsequent stocks to go negative at any point to current period consumption. This again needs to be done iteratively. Assign dummy value of 9 and update accordingly.

9. When households don't report consuming these crops from own production, attribute to sales and assign dummy value of 10.

10. Assume any remaining discrepancy is stock carried over to endline.

We end up with 5 stock variables:

- Stock 1 is raw sum of flows plus baseline stock
- Stock 2 Tries to ensure that stock1 never goes negative by attributing differences to previous harvests, if feasible based on timing and what the household grew
- Stock 3 Ensures that stocks don't go negative by offsetting excess outflows with previous purchases if they can't be attributed to harvests
- Stock 4 corrects for any excess stocks at endline (relative to reported) by attributing it to consumption such that stocks don't go negative
- Stock 5 Attributes remaining differences to unreported sales

B.2 Regression Estimates

In our pre-analysis plan, we pre-specified the tuple-wise randomization procedure described in Athey and Imbens (2017). The authors prescribe a simple procedure for estimation and inference in such settings — essentially a weighted average of within-cluster differences-in-means and variances. However, we also pre-specified a regression-based approach, controlling for strata fixed effects and clustering standard errors at the stratum level, following De Chaisemartin and Ramirez-Cuellar (2024). However, we prefer the former approach, given its direct link to the experimental design and the potential restrictiveness of the regression-based approach with only about 30 clusters. In this appendix, we reproduce our results our results using the regression-based approach. For the most part, the point estimates are qualitatively similar to those obtained using the preferred Athey and Imbens (2017) approach, but with wider confidence intervals.

The regression analysis is based on the following intent to treat specification

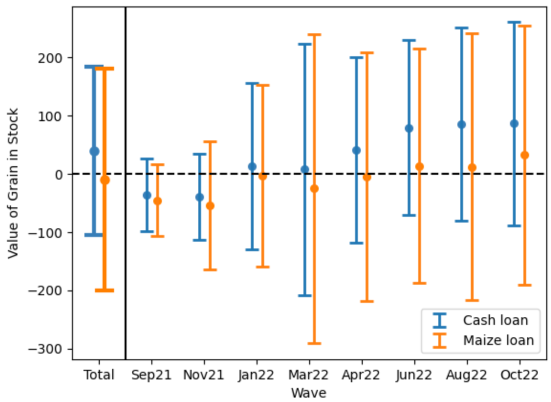
$$Y_{ist} = \beta_1 \text{Cash}_i + \beta_2 \text{Kind}_i + X_i \gamma + \delta_t + \delta_s + \varepsilon_{ist}$$

where Y_{ist} is outcome Y for household i in stratum s at time t, δ_t and δ_s are survey wave and stratum fixed effects, respectively, and $T = 7$.

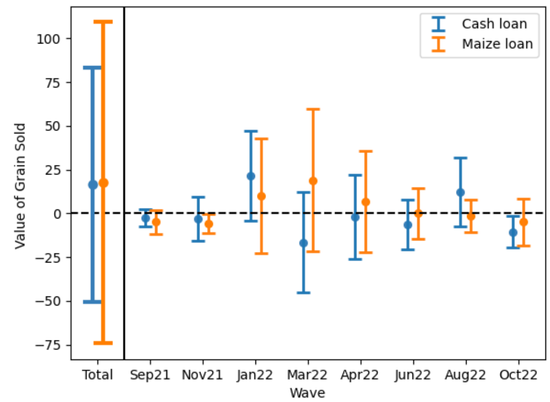
To examine outcomes at higher frequency, we also run the following dynamic specification to estimate separate treatment effects for each period

$$Y_{ist} = \sum_{\tau=0}^{7} \beta_1^{\tau} \text{Cash}_i + \sum_{\tau=0}^{7} \beta_2^{\tau} \text{Kind}_i + X_i \gamma + \delta_t + \delta_s + \varepsilon_{ist}.$$

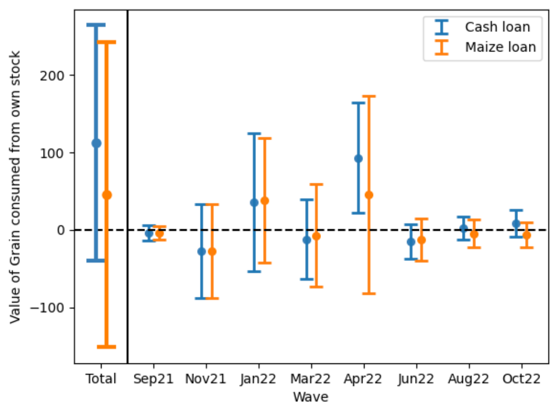
(a) Stocks



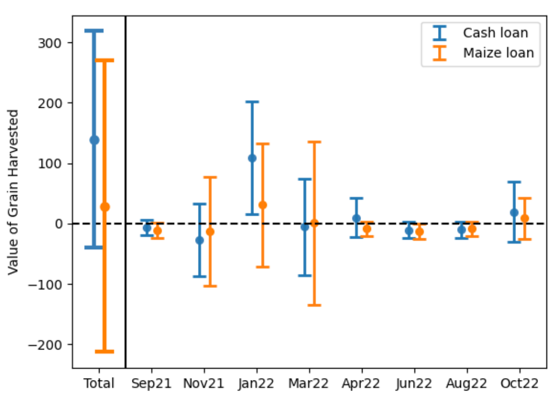
(b) Sales



(c) Consumption



(d) Harvested grain brought home



(e) Purchases intended for storage

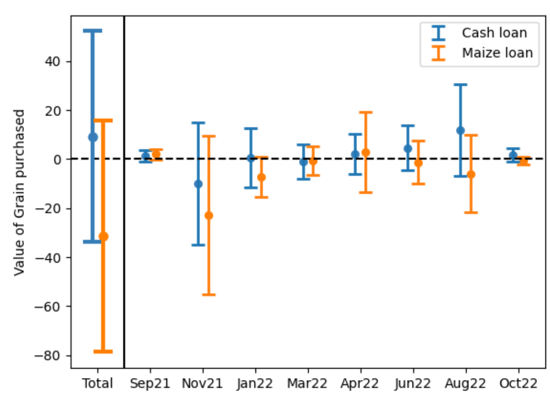


Figure B1: Treatment Effects on Stocks, by Wave, regressions with strata FEs

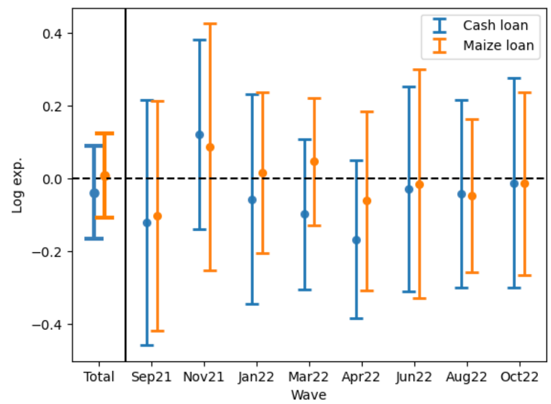
Table B1: Effects of treatment on total value of crops sold (000's Naira).

	Maize	Beans	Guinea Corn	Rice	Millet	All
Cash Loan	7.65	−0.23	−9.65	4.39	14.22	18.25
	(13.34)	(25.28)	(6.86)	(4.94)	(35.02)	(49.52)
Maize Loan	5.79	−10.15	−4.61	3.02	0.00	1.72
	(9.95)	(18.88)	(6.76)	(7.08)	(11.02)	(36.60)
Any Loan	6.65	−5.59	−6.93	3.65	6.54	9.32
	(9.27)	(15.89)	(6.70)	(4.96)	(17.38)	(27.84)
Fixed Effects	Strata	Strata	Strata	Strata	Strata	Strata
Control Mean	54.630	147.830	14.440	15.470	71.140	323.700
N	930	930	930	930	930	930

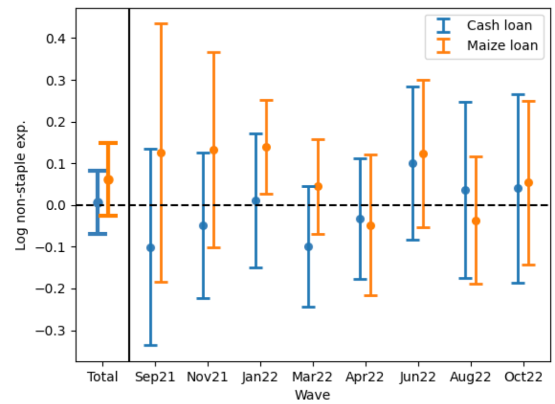
$^{*}\, p < 0.10$

This table contains estimates of the treatment effects of cash and maize loans on the total sales of each crop. Sales values are imputed following the procedure in Appendix B.1. Standard errors are clustered at the stratum level.

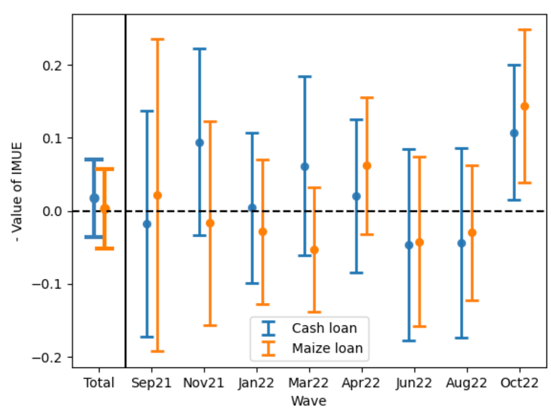
(a) Log food expenditure



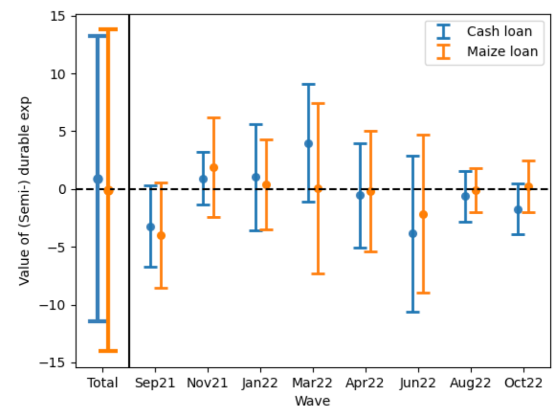
(b) Log non-storable expenditure



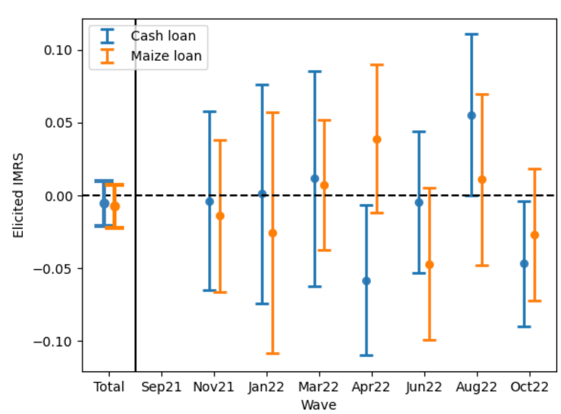
(c) $-\log \lambda$



(d) (Semi-)durable expenditure



(e) Elicited IMRS



(f) Elicited IMRS – topcoded

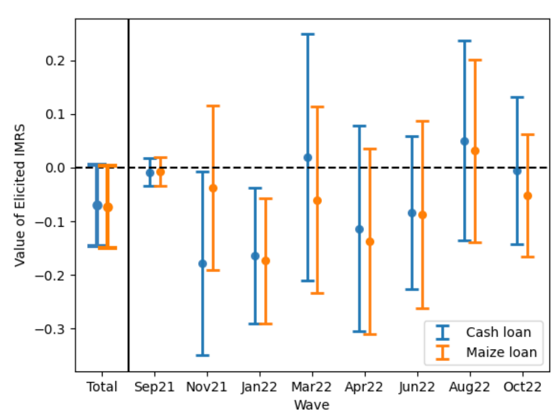
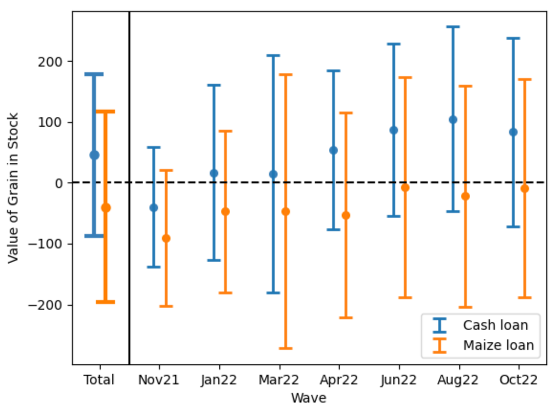


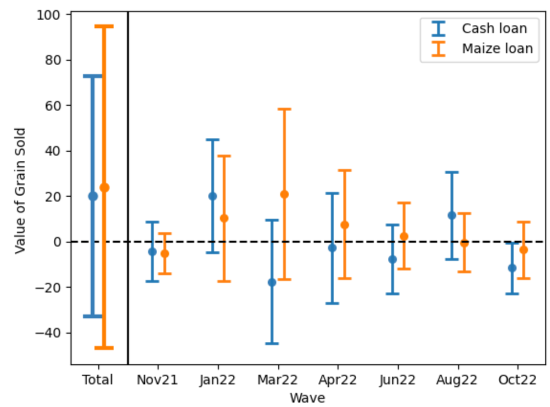
Figure B2: Treatment Effects on Expenditures, by Wave, regressions with strata FEs

Figure B3: Treatment effects on value of grain stocks and flows by wave, Double Post LASSO

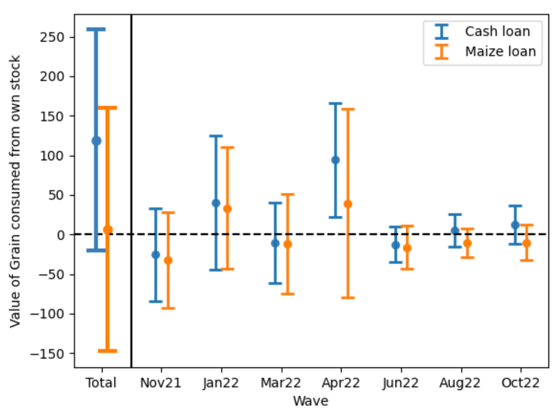
(a) Stocks



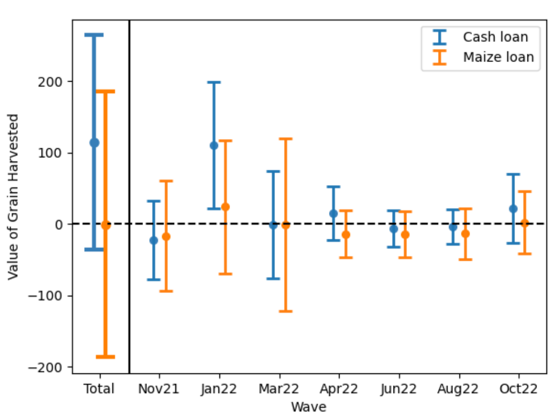
(b) Sales



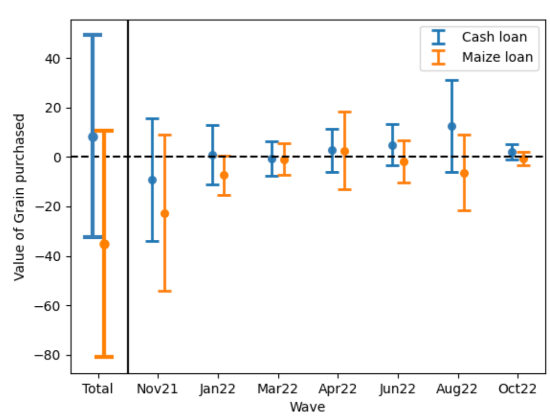
(c) Consumption



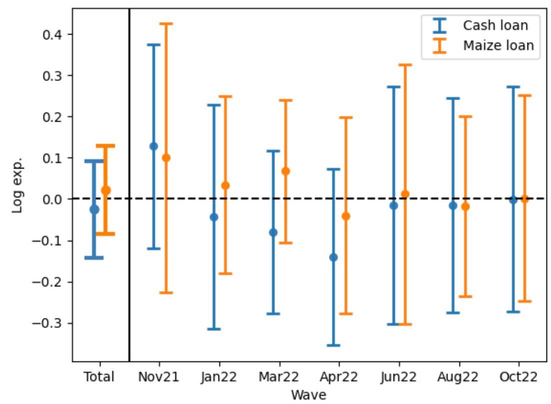
(d) Harvested grain brought home



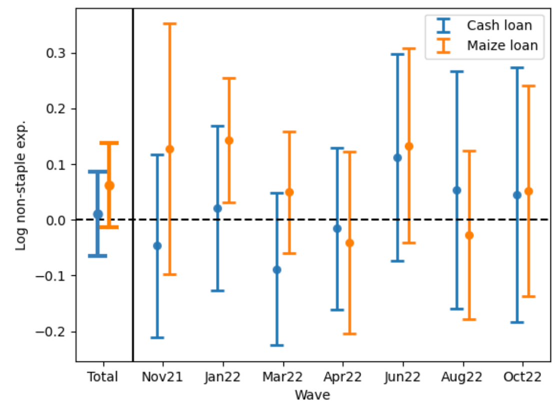
(e) Purchases intended for storage



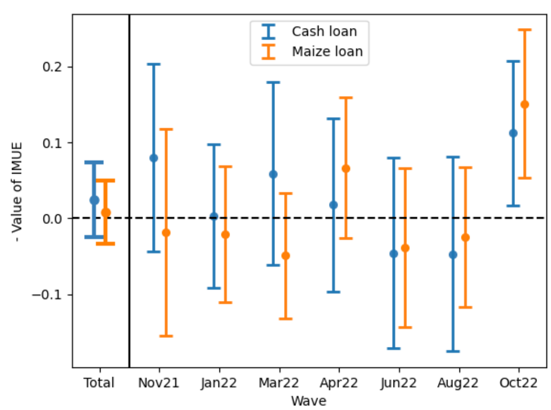
(a) Log food expenditure



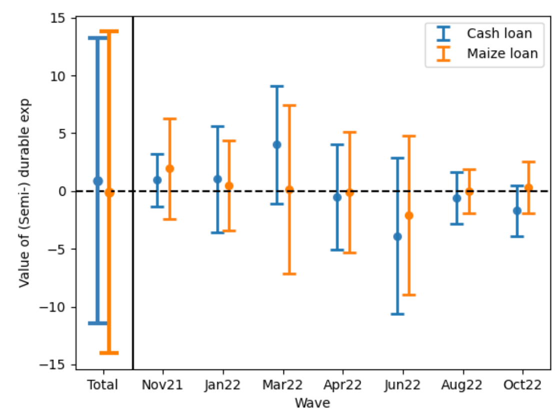
(b) Log non-storable expenditure



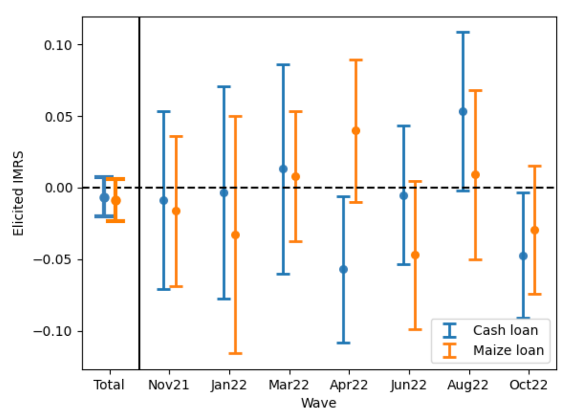
(c) $-\log\lambda$



(d) (Semi-)durable expenditure



(e) Elicited IMRS



(f) Elicited IMRS – topcoded

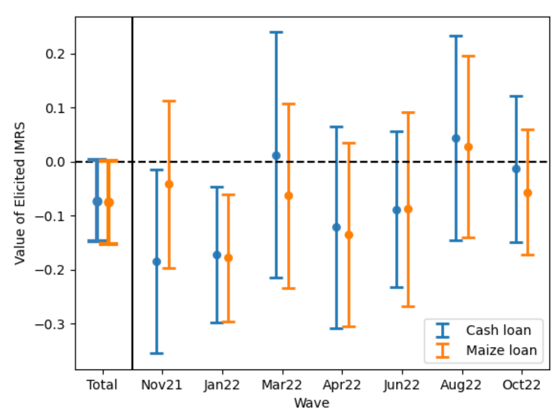
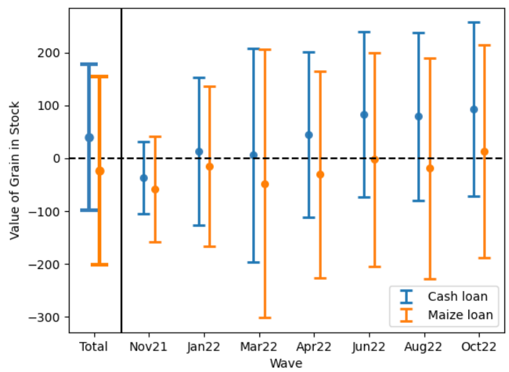
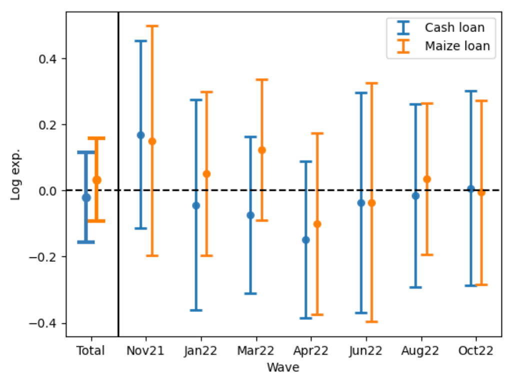


Figure B4: Treatment Effects on Expenditures, by Wave

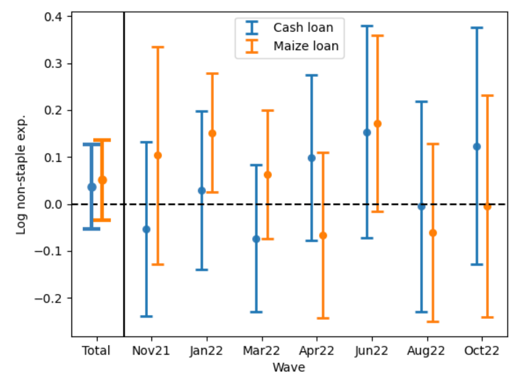
(a) Stock values



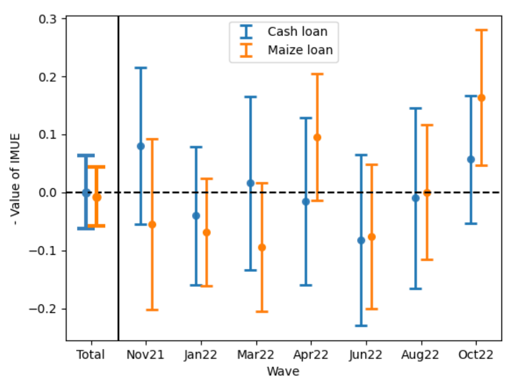
(b) Log food expenditure



(c) Log non-storable expenditure



(d) $-\log \lambda$



(e) (Semi-)durable expenditure

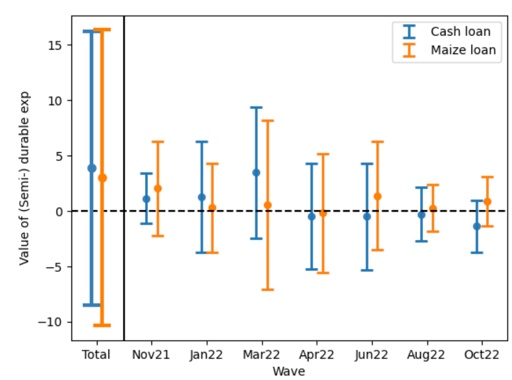


Figure B5: Treatment Effects on Stocks Expenditures, controlling for baseline outcome