# Capitalizing Village Economies*

Wyatt Brooks Danice Guzman Joseph Kaboski

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**Abstract**

We conduct a randomized controlled trial that adds exogenous funds to local village financial markets in rural Uganda. Existing savings groups allow members to save and borrow with interest from the group. We study 92 such groups, and in half we increase the supply of funds available for loans by an amount equal to one third of average annual group savings. Since groups differ in their size, adding a fixed level of funds generates variation in treatment intensity. We find that this exogenous increase in funds induces local general equilibrium effects. More intensively treated groups experience a significant decrease in interest rates and increase in loan volume. They experience fewer missed meals and more investment. In addition, we find that the intervention concentrates production: the log-variance of investment increases and local wages rise. We use these results to quantify a model of the village economy and show that increasing the supply of loanable funds reduces misallocation. All households, including those that are net savers at baseline, benefit due to improved risk sharing and higher wages.

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# 1 Introduction

The poorest populations in the world are concentrated in rural areas that are poorly integrated with outside markets, including limited access to outside financial markets.[1] Formal financial intermediaries are much less likely to operate in rural areas, and people in rural locations cannot easily borrow or save at the interest rates realized in urban areas. Approximately 1.4 billion people in the world do not have access to formal banking. Financial markets that do exist among these rural populations are local with all buyers and sellers located in the limited geographic area. In addition, the primary economic activity in these poor, rural areas is agriculture, which relies on upfront investments (in the form of seed, fertilizer, and other agricultural intermediates) at the beginning of the growing season well in advance of revenue realized at harvests. Hence, demand for credit in these markets is high, and, due to the poverty of the local population, the interest rates needed to clear local financial markets are often high.

We conduct a market-level experiment where we exogenously increase the supply of loanable funds in existing village credit markets. While this is not equivalent to financial market integration, this experiment does introduce outside capital into a previously closed credit market.[2] We then study how introducing outside capital into such a market affects interest rates and lending within the market and, correspondingly, how investment and consumption decisions of local households are affected. We conduct a randomized controlled trial in 92 savings groups in rural Uganda. We become silent members of treated groups and make a single large deposit into the group's pool of loanable funds. We are paid back one year later according to the same formula and on the same timeline as all other group members. We made no such deposit in the untreated savings groups. Because the deposit was the same amount in all savings groups we use the differential sizes of the groups at

[1] The effects of limited access to other types of markets have been studied in many contexts, such as product markets (Sotelo (2020), Bartkus et al. (2022)), labor markets (Brooks et al. (2021), Brooks and Donovan (2020)) and input markets (Asturias et al. (2019)).

[2] At baseline, the credit markets we study use the savings of local group members to make loans to other local group members. They are not capitalized by outside loans. All members at baseline are members of the same local community.

baseline to give us a measure of effective intensity of treatment (increased loan supply per person) to show that more intensively treated villages experience: 1) a reduction in interest rates, 2) an increase in farm investment, 3) an increase in loans made, 4) a reduction in missed meals, 5) an increase in harvests and 6) an increase in the variance of log-harvests.

We then use a general equilibrium model of the village economy to interpret these results. We use the effects from the experiment to parameterize the model by conducting the same RCT in the model as in the data.[3] We find that the intervention reduces misallocation (that is, reduces the variation in marginal product across farms) and reduces the volatility of consumption. This implies that even households that were initially savers benefit from the intervention since their loses from lower interest rates are more than offset by higher wages and improved consumption smoothing. We show that the social rate of return on the deposit made as part of the RCT was 19.9%, of which 13% was realized by the financier making the deposit, and the other 6.9% was realized by the group members gaining access to those funds through financial markets. The households benefiting the most are those with high farming productivity but low assets that are now operate at a large scale in farming due to lower financing costs. The households benefiting the least are those with low farm productivity and high assets who are net savers at baseline. This group gets no benefit from cheaper financing since they do no farming, but they benefit higher wages paid in local labor markets by productive farmers who now operate at a greater scale. The mean treatment we provided in our experiment was equal to 45,800 Ugandan shillings per group member (approximately 12 US dollars) of additional funds available for borrowing. The benefit in the model to the highest productivity and lowest asset household was 8,600 shillings (2.25 US dollars) and the benefit to the lowest productivity and highest asset household was (60 US cents). Hence, it is notable that every household benefits from the increase in outside funds in the local financial market, even those who save the most at baseline.

This paper builds on the large literature studying how access to finance and financial

[3] In the model, the intervention is an exogenous increase in loanable funds from an outside investor whose funds appear in the credit market clearing condition. Those funds receive market interest payments. Therefore, like in the data, the intervention is not a grant or zero interest loan.

markets affect development. A very large literature experimentally studies the introduction of microcredit to individuals or communities.[4] The two most-closely related papers to ours are Kaboski and Townsend (2011), which studies a public program establishing community lending programs throughout Thailand, and Banerjee et al. (2015b), which studies expansion of microcredit into urban neighborhoods in Hyderabad, India. Our project considers existing savings groups in a rural context that then have additional capital added to them. This context is very different than the case in Banerjee et al. (2015b), where participants live in an urban environment and invest in urban businesses (rather than farms). They are very likely to face difficulty in accessing financing due to limited collateral, but this is very different than the rural context where broader financial markets are much more difficult to reach at all. In the Kaboski and Townsend (2011) context, the program evaluated was intended to be a microcredit program that imposed loan size limits in order to make loans widely available. This is important given that our results show that our intervention caused a concentration of agricultural production, which means large loans are needed to finance those large levels of investment.

## 2 Setting, Data and Intervention

We study 92 pre-existing savings groups in Mpigi District in Uganda. We provide a thorough discussion of the context and operation of these groups in a companion paper, Guzman et al. (2024), that only studies group-level operations. These groups exist within rural farming villages and are composed of members of the local community. These groups conduct weekly meetings in which members make deposits into the funds of the group, and members have the right to make requests for loans to be paid out of the available funds. Borrowers are required to pay interest on these loans, and members are paid interest on their savings out of these loan interest payments in proportion to their funds saved. Hence, these groups closely approximate traditional banks: deposits from members serve as a pool of capital for

[4] See Banerjee et al. (2015a) for an example summarizing six studies.

loans to borrowers.

However, these groups differ from banks in two important ways. First, savings in the group are not demand deposits. The groups operate on an annual cycle. At the end of the year, all outstanding loans must be repaid, and all members are paid out from the pool of funds based on their savings. Hence, the only way to access funds from the group within the year is to take out a loan. In addition, the second difference from a traditional bank is that the interest rates paid on deposits is not fixed. Indeed, savings in the group are more like equity shares than traditional deposits in that the return on savings fluctuates with the performance of the loans made by the group. Any losses due to default by group members is borne by the group as a whole in that the pool of capital will be necessarily smaller at the end of the annual cycle, and returns will therefore be smaller for savers.[5]

The villages in which our intervention is conducted are rural and low income. The vast majority of the local labor force works in agriculture. The mean number of members in the groups in our study is 32.7 and the median is 32. Important to our analytical strategy, groups range in size from 15 to 66 with a standard deviation of 10.

Our intervention is timed to occur at the beginning of each group's annual cycle. For the treated groups, at the first meeting of the cycle the group was informed that we would act as a silent member of the group where our sole action was to make an initial deposit of 1.5 million Ugandan shillings (at the time valued at approximately 450 US dollars). This money would be part of the pool of group savings available for loans during the year. One year later at the end of the cycle, we would then be paid back according to the same formula as all other members. Hence, we would lose money if defaults were high or would earn returns from interest paid on loans at the same rate and time as all other group members.[6] Because we never requested loans from the group, this acts as an exogenous increase in loan supply on the market.

[5] In Guzman et al. (2024) we show that there is no evidence of increased default on loans due to the intervention discussed in this paper.

[6] It is important for interpretation of this intervention to emphasize that the group was expecting us to be paid interest at the same rate as the rest of the group, so this was not simply a grant or zero interest loan to the group. Our intervention is identical to the case where a single member makes a very large deposit in the group. In all cases, we were paid back at the end of the cycle.

It is important to note that this intervention differs from the case of joint liability loans take by the group from a traditional lender (such as an outside bank). Our intervention was designed to work within the structure of the savings groups. We did not require a schedule of interim payments from the group that may not have corresponded to the timing of their available liquidity. Instead, we were paid out at the end of the cycle when the group has liquidated. In addition, our intervention is equity financing rather than debt financing. If the group had lost money, our returns would have been negative (just as it would have been for the members). This is unlike a joint liability bank loan taken on by a group, which may worry about their ability to repay in all states of the world.

To measure the effects of this intervention, we conducted household surveys with group members before and after the intervention in both treatment and control groups. The intervention and data collection were timed to coincide with the beginning of each group's annual cycle. Treatment assignment was randomized within starting months to assure seasonal balance. Household surveys were conducted just in advance of the first group meeting, and treatment was announced and conducted at the first meeting. Endline data collection occurred one year later to coincide with the end of the annual cycle. The baseline occurred from November 2019 to October 2020, and endline was from November 2020 to October 2021.

This study period overlaps with the onset of the COVID-19 global pandemic. Uganda experienced relatively low mortality from the pandemic, but did go through a period of travel restrictions that affected our data collection. In April, May and June of 2020, we paused data collection, which was then resumed in the summer of 2020. This coincided with a pause in meetings by the savings groups themselves, who had delayed the start date of their cycles. Hence, we were still able to conduct our intervention at the start of the annual cycle.

While these rural areas were not much affected by the pandemic on most outcomes that we study in this paper, one outcome that was affected was school enrollment. All schools in Uganda were shut down for virtually all of the period that we studied. Therefore, we cannot look for evidence on how the increase in loan supply affected school enrollments and payment of school fees, since all schools were closed.

Our household surveys included a variety of questions on household demographics, consumption, farming, business activity, debt and interaction with the savings group.

Table 1: Balance across Treatment Arms

| Variable | (1) Control | (2) Treatment | (3) Difference |
|---|---|---|---|
| Married | 0.742 | 0.741 | -0.001 |
| | (0.438) | (0.438) | (0.975) |
| Household Size | 4.342 | 4.528 | 0.186 |
| | (2.351) | (2.552) | (0.285) |
| Age | 38.780 | 38.901 | 0.121 |
| | (14.706) | (13.854) | (0.912) |
| Female | 0.677 | 0.695 | 0.018 |
| | (0.468) | (0.461) | (0.578) |
| Secondary School | 0.132 | 0.147 | 0.015 |
| | (0.338) | (0.354) | (0.697) |
| Market Income | 41,023.688 | 44,544.535 | 3,520.846 |
| | (92,371.539) | (90,975.523) | (0.605) |
| Missed Meal | 0.056 | 0.086 | 0.030 |
| | (0.230) | (0.280) | (0.123) |
| Harvest Value | 1.422e+06 | 1.466e+06 | 43,816.539 |
| | (6.103e+06) | (6.148e+06) | (0.878) |
| Intermediate Exp. | 1.993e+06 | 2.003e+06 | 9,759.938 |
| | (988301.312) | (972401.375) | (0.884) |
| No Farming | 0.474 | 0.505 | 0.031 |
| | (0.500) | (0.500) | (0.430) |
| Took Loan | 0.298 | 0.360 | 0.062 |
| | (0.458) | (0.480) | (0.104) |
| Num. Group Members | 31.784 | 32.801 | 1.018 |
| | (10.945) | (9.812) | (0.675) |
| Log(Group Savings) | 15.763 | 15.713 | -0.050 |
| | (0.800) | (0.748) | (0.757) |
| Observations | 1,022 | 1,107 | 2,129 |

Before turning to the empirical analysis, we first check for balance across characteristics and outcomes of interest between members of treated and control groups. In Table 1 we show that demographic characteristics and outcomes of interest are balanced at baseline. Although no differences are significant, we do note that whether or not one took a loan has a p-value of 0.104 (with more members of the treated group have taken a loan at baseline) and whether or not one missed a meal has a p-value of 0.123 (with more the treated group more likely to have missed a meal). We would note that neither of these is a targeted moment in the quantitative analysis presented later on.

# 3 Empirical Results

We now use the surveys and treatment described to measure the effect of the intervention on members of savings groups, and the general equilibrium effects on their village economies. To do so, we measure group-level outcomes with the following regression specification:

$$y_{jkt} = \theta_{kt} + \beta_1 \text{Treated}_{jkt} + \beta_2 \text{Treated}_{jkt} \times \text{Intensity}_{jk} + \gamma \text{Intensity}_{jk} + \varepsilon_{jkt} \tag{3.1}$$

where group $j$ in sub-district $k$ in wave $t$ has outcome of interest $y_{jkt}$. Our measure of Intensity is the size of the transfer (1.5 million Ugandan shillings) divided by the number of members of group $j$ at baseline. Our Intensity measure is then normalized by its standard deviation so that its point estimates can be interpreted as the effect of a one standard deviation increase in intensity of treatment. The $\theta_{kt}$ term captures differential changes over time by sub-district (of which there are 8), which may be important for capturing differential time series shocks due to local agricultural shocks.

When considering household-level outcomes, we augment the regression with individual household fixed effects to capture all demographic differences and other characteristics invariant to time:

$$y_{ijkt} = \alpha_i + \theta_{kt} + \beta_1 \text{Treated}_{jkt} + \beta_2 \text{Treated}_{jkt} \times \text{Intensity}_{jk} + \gamma \text{Intensity}_{jk} + \varepsilon_{jkt} \tag{3.2}$$

In our regression results, we focus attention on the $\beta_1$ and $\beta_2$ terms. We would typically expect the $\beta_1$ term to be zero as this would capture the average effect of the treatment on a group with infinitely many members (so that transfer per group member is zero). The $\beta_2$ term is our main parameter of interest, which shows how a 1 standard deviation increase in the intensity of the treatment affects the outcomes.

The first set of outcomes that we focus on are the effects of treatment on interest rates within the group. The results are presented in Table 2. Here we express interest rates in

Table 2: Changes in Interest Rate

| | Log(R) | | Gross Interest Rate | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treated | -0.109* | 0.318 | -0.114 | 0.435 |
| | (0.060) | (0.211) | (0.072) | (0.267) |
| Treated × Intensity | | -0.119** | | -0.153** |
| | | (0.057) | | (0.073) |
| Observations | 92 | 92 | 92 | 92 |
| R-squared | 0.427 | 0.448 | 0.445 | 0.469 |

*Table notes:* Standard errors clustered at the savings group level are in parentheses. The "Intensity" measure is expressed in standard deviation units. All regressions include baseline month and sub-district fixed effects. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

both logs (columns 1 and 2) and levels (columns 3 and 4).[7] In levels, the unconditional mean interest rate in 22.5% in untreated groups and 12.6% in treated groups. This decrease in interest rates is what one should expect with an increase in the supply of loanable funds within the groups. When we take out the Intensity term in columns 1 and 3, we can see that the point estimate (taking into account the controls) is very similar to the difference in unconditional means, though the difference is imprecise. When we include the interaction of Intensity and Treated in columns 2 and 4 we can see that the negative effect on interest rates is concentrated in the more intensely treated groups. Moreover, the $\beta_2$ term is statistically significant at the 5% level in all specifications, whereas we fail to reject the null hypothesis of $\beta_1 = 0$ in all four specifications at the 5% level.

Lower interest rates encourage households to take on activities where financing costs are relevant. In the rural context of households in this study, that most obviously impacts their farming decisions. Farming requires upfront, sunk investments that have an uncertain payoff at a later date. Hence, households need to finance their investment during the time between planting and harvest. Sufficiently wealthy households can self-finance this investment. But if households have few assets accumulated, their investments can be financed with loans. Therefore, in theory financing costs should directly affect the agricultural choices of farmers.

To evaluate this empirically, we now check to see if the more intensively-treated house-

[7] If interest rates are close to zero, these should be nearly the same.

Table 3: Farming Outcomes

| | (1) Input Expenditure | (2) Harvest | (3) Not Farming | (4) Var(Log(Harvests)) |
|---|---|---|---|---|
| Treated × Intensity | 188,679.56*** | 700,752.56* | 0.10** | 6.98* |
| | (68,322.64) | (402,658.13) | (0.04) | (4.12) |
| Treated | -15,070.44 | -412,079.59 | -0.01 | -3.86 |
| | (80,736.93) | (489,240.45) | (0.04) | (3.98) |
| Observations | 1,523 | 1,523 | 1,523 | 92 |
| Baseline Mean | 1997860 | 1444641 | 0.489 | 22.6 |
| R-squared | 0.04 | 0.04 | 0.10 | 0.24 |

*Table notes:* Standard errors clustered at the savings group level are in parentheses. The "Intensity" measure is expressed in standard deviation units. All regressions include baseline month and sub-district fixed effects and controls for Intensity. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

holds, which are those households getting lower interest rates, have different agricultural choices and outcomes. These results are presented in Table 3. In column 1 we see that more intensively treated households spend more on agricultural inputs.[8] As predicted by theory, households that now face lower interest rates purchase more agricultural inputs as they can now finance those investments more cheaply. In column 2, we can see that this leads to greater harvests.[9] Interestingly, the positive result on harvests is true despite the result in column 3, which is that participation in farming is declining in treatment intensity. In partial equilibrium this is puzzling, because lower financing costs should make farming more profitable. However, in general equilibrium this could come about if local wages are rising in more intensively treated groups. Higher wages cause marginally productive farmers to stop farming and put all their effort into the labor market to get the now-higher wages. This explanation would predict that more productive farms should expand and small farms should shrink (or disappear). To check this, in column 4 we look at the variance of the log of harvests and find that the variance is increasing. This is consistent with increasing concentration of farming now that interest rates are lower and those large farms find it profitable to expand.

[8] This is the sum of expenditure fertilizer, seed, herbicide and pesticide, and includes zeros for households purchasing no intermediates (including those that do not farm). The sum is winsorized at the 99% level. Because there are many zeros in the data, including people who do not farm, we present this result in levels instead of logs.

[9] This is the sum of the market value of all harvested crops, winsorized at the 99% level. Like with intermediate expenditures, harvests have many zeros so we present this result in levels.

Table 4: Other Household Outcomes

| | (1) Log(Labor Income) | (2) Loan Taken | (3) Missed Meals |
|---|---|---|---|
| Treated × Intensity | 0.24* | 0.08** | 0.01 |
| | (0.14) | (0.04) | (0.02) |
| Treated | -0.12 | -0.02 | -0.06*** |
| | (0.15) | (0.04) | (0.02) |
| Observations | 303 | 1,523 | 1,494 |
| Baseline Mean | 4.39 | 0.33 | 0.07 |
| R-squared | 0.17 | 0.05 | 0.09 |

*Table notes:* Standard errors clustered at the savings group level are in parentheses. The "Intensity" measure is expressed in standard deviation units. All regressions include baseline month, household and sub-district-by-wave fixed effects, and control for Intensity. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

We can further look for evidence of this in labor income. In Table 4 we consider other household outcomes including labor income. There we do see that labor income is rising in treatment intensity. Again, this evidence supports the theory that lower interest rates promotes a reallocation of labor from small, marginal farms into market work. In addition, in column 2 we can see that more people take on loans in groups treated more intensively. This is not obvious given the results above. Fewer people are now engaged in farming, so if all loans were used to finance agricultural intermediates, we should expect to see fewer loans (of larger average size) taken. However, loans serve another purpose besides financing investment: they can be used for consumption smoothing. Consistent with that theoretical possibility, in column 3 we see that the treatment causes a decrease in whether or not the household has missed any meals in the prior week. However it is surprising to note that unlike the other results observed thus far, for this outcome we have that $\beta_1$ is significant and has the expected sign, which $\beta_2$ is not significant (no evidence of association between intensity and treatment effect).

# 4 Model

A village is composed of a mass of infinitely-lived households with measure $N$. Each household has an exogenously determined productivity $z$ and endogenously holds cash-on-hand

$a$ across periods. Each household is endowed with a unit of labor every period that may be split between being sold in the local labor market at wage $w$ or used in their own farm technology. The farm technology uses labor $l$ (either of the owner or purchased in the labor market) and agricultural inputs $x$ (such as seed, fertilizer, or pesticide) as inputs, and produces the final good as output. The final good can either be consumed or used as the agricultural input.

Households within the village interact in a village financial market trading non-contingent bond $b$. There is a market clear interest rate $R$ that clears the local bond market.

The problem of the household can be characterized by the following Bellman equation:

$$V(a, z) = \max_{c,x,l,b,\phi} u(c) + \beta \int V(\phi z' F(x, l) - Rb, z') dG(z'|z) \tag{4.1}$$

subject to:

$$c + qx \leq a + w(1 - l - f\phi) + b$$

$$b \leq \bar{b}$$

$$\phi \in \{0, 1\}$$

Here $F(x, l)$ is the concave agricultural production function, and $G(z'|z)$ is the exogenous productivity process.

The timing of the agricultural choice is such that input choices are made before uncertainty is realized. This follows the mechanism of Donovan (2021), where farm investment choices interact with household risk aversion.

The solution to the household's problem generates an endogenous probability distribution over states given by $M : \mathbb{R}^2 \to [0, 1]$. For any given $M$, the wage $w$ satisfies the local labor market clearing condition:

$$N = N \int [l(a, z) + f\phi(a, z)] dM(a, z). \tag{4.2}$$

Likewise, the financial market clears if bonds are in net supply equal to $T$.[10] The gross interest rate $R$ satisfies the bond market clearing condition:

$$T = N \int b(a,z) dM(a,z) \tag{4.3}$$

## 4.1 Characterization of Household's Problem

Taking first order conditions from the household's problem leads to a few illustrative results. First, we can derive the familiar Euler equation by combining the envelope condition and first order conditions. If $\lambda$ is the Lagrange multiplier on the budget constraint, and $\mu$ is the Lagrange multiplier on the borrowing limit, these can be written as:

$$V_1 = \lambda, \tag{4.4}$$

$$u'(c) = \lambda, \tag{4.5}$$

$$\lambda + \mu = R\beta \int V_1 dG(z'|z), \tag{4.6}$$

which together imply the Euler equation:

$$1 + \frac{\mu}{u'(c')} = R\beta \int \frac{u'(c(a',z'))}{u'(c)} dG(z'|z) \tag{4.7}$$

In addition, if we consider households that chose to pay the fixed cost to operate the farming technology, the input choices are determined by:

$$\lambda q = \beta F_1 \int z' V_1 dG(z'|z) \tag{4.8}$$

$$\lambda n = \beta F_2 \int z' V_1 dG(z'|z) \tag{4.9}$$

[10] In our baseline case, we assume that the financial market is closed so that $T = 0$. When we conduct the intervention in the model, we set $T > 0$.

Together these imply an undistorted input mix given by:

$$\frac{F_2}{F_1} = \frac{n}{q} \tag{4.10}$$

However, while the mix of inputs is undistorted, we can combine the Euler equation and first order condition on inputs to see that the level of input choice is affected by risk:

$$\frac{qu'(c')}{\mu + u'(c')} = F_1 \int z' \frac{u'(c(a', z'))}{\int u'(c(a', t'))dG(t'|z)} dG(z'|z) \tag{4.11}$$

This shows that household investment decisions are affected by the possibility of reaching their borrowing constraint ($\mu > 0$) and also by variation in marginal utility across states. When households face consumption requirements, poorer households (who are closer to their consumption requirements) are unwilling to take on risky investments even when they have high average returns.[11] Since this is true of all households in the village economy, low income levels lead to low investment in the aggregate causing lower wages and smaller levels of savings, as in Donovan (2021).

# 5 Quantitative Results

In order to parameterize the model, we first need to understand how to interpret the intervention in the context of the model. Using the credit market clearing condition, equation (4.3), we can simply divide both sides by population to get:

$$\frac{T}{N} = \text{Treatment Intensity} = \int b(a, z)dM(a, z). \tag{5.1}$$

Therefore the left-hand side of this equation is exactly the intensity measure used to derive our empirical results. To put this in units interpretable in the model, we divide by average harvest value. For the median group, the transfer per group member is 0.033 times the

[11] Besides Stone-Geary, this argument is true of any preferences that exhibit decreasing risk aversion.

average harvest. For a group one standard deviation more intensely treated, the transfer per member is 0.046 times the average harvest. Hence, we solve the model with $T/N = 0$ and compute mean harvest value, then set $T/N$ to 0.033 times that value and solve the model, as well as 0.046 times it. We use the difference in outcomes between those two models to match treatment effects. As we change $T/N$ we will get changes in $R$ and other endogenous outcomes. Our strategy is to use that elasticity from the data to identify model parameters.[12] We interpret the treatment to last one period in the model, and that our measured effects come in the period of treatment. Because the treatment induces changes that have dynamic effects, despite the fact that the treatment only lasts one period, we do have to compute the model's transition path back to its initial steady state.

We assume that productivity follows a two state process: $z \in \{z_L, z_H\}$. With probability $\rho$, $z' = z$, and with complementary probability it switches to its other value.

We also assume that the production function $F(x, l)$ is a CES aggregator:

$$F(x,l) = \left(\alpha^{1/\sigma} x^{1-1/\sigma} + (1-\alpha)^{1/\sigma} l^{1-1/\sigma}\right)^{\frac{\gamma\sigma}{\sigma-1}} \tag{5.2}$$

where $\alpha$ is a share parameter, $\sigma$ is the elasticity of substitution, and $\gamma$ controls the returns to scale in production.

Finally, we assume preferences are Stone-Geary, so that $u(c) = \log(c - \bar{c})$.

Hence our set of parameters is $\{\alpha, \beta, \bar{c}, \sigma, \gamma, \rho, q, f, \bar{b}, z_L, z_H\}$. We can normalize $q = z_L = 1$. We are left with 9 parameters, and we choose 9 moments to match them.

In particular, we want to match the treatment effects on interest rates, fertilizer, harvests and wages.

First, we can get $\sigma$ directly as follows:

$$\frac{x}{l} = \frac{\alpha}{1-\alpha}\left(\frac{w}{q}\right)^{\sigma} \implies \log(x'/x) - \log(l'/l) = \sigma \log(w'/w) \tag{5.3}$$

[12] As in Brooks et al. (2024), model parameters have a much more clear connection to *variation* in treatment effects than to average treatment effects.

$$\implies \sigma = \frac{\log(x'/x) - \log(l'/l)}{\log(w'/w)}$$

This means that conditional on matching two out of the three empirical changes in $x$, $l$ or $w$, then this choice of $\sigma$ guarantees that the third is matched.

The other 8 parameters are determined jointly, but it is useful to describe heuristically how they are identified. The returns to scale parameter $\gamma$ is determined by the change in $x$, and the fixed cost $f$ is determined by the change in labor. The baseline level of labor is matched with $\alpha$. We match the change and level of $R$ jointly using $\rho$ and $\bar{b}$. We set $z_H$ to match the variance of log-harvests at baseline, and we match its change with $\bar{c}$. We set $\beta$ exogenously to 0.96.

Table 5: Moments in Model and Data

| Moment | Model | Data | Parameter | Value |
|---|---|---|---|---|
| Baseline $l$ | 0.48 | 0.49 | $\alpha$ | 0.82 |
| Baseline $R$ | 0.22 | 0.23 | $\bar{b}$ | 0.77 |
| Baseline $Var(log(Harvests))$ | 19.5 | 22.6 | $z_H$ | 3.68 |
| Change in $Var(log(Harvests))$ | 7.12 | 6.98 | $\bar{c}$ | 0.08 |
| Change in $R$ | -0.09 | -0.12 | $\rho$ | 0.64 |
| Change in $x$ | 0.12 | 0.13 | $\gamma$ | 0.65 |
| Change in $l$ | 0.06 | 0.06 | $f$ | 2.25 |
| Change in $w$ | 0.21 | 0.22 | $\sigma$ | 0.29 |

Using these results we find that aggregate income increased by 5.6%.[13] We conduct the following decomposition: value added in farming in the aggregate is equal to:

$$Y_a = \int \int \phi(a,z) \left[ z' \left( \alpha^{1/\sigma} x(a,z)^{1-1/\sigma} + (1-\alpha)^{1/\sigma} n(a,z)^{1-1/\sigma} \right)^{\frac{\gamma\sigma}{\sigma-1}} - qx(a,z) \right] dG(z'|z) dM(a,z)$$

(5.4)

$$= \int \phi(a,z) x(a,z) \int \left[ z' \alpha^{\frac{\gamma}{\sigma-1}} \left( 1 + \frac{1-\alpha}{\alpha} \left( \frac{q}{w} \right)^{\sigma-1} \right)^{\frac{\gamma\sigma}{\sigma-1}} - q \right] dG(z'|z) dM(a,z)$$

[13] Here, aggregate income is the total value of all harvests less the cost of investment and the financing cost of the treatment loan: $(R-1)T/N$.

If we define:

$$\Gamma(z) = \int \left[ z' \alpha^{\frac{\gamma}{\sigma-1}} \left( 1 + \frac{1-\alpha}{\alpha} \left( \frac{q}{w} \right)^{\sigma-1} \right)^{\frac{\gamma\sigma}{\sigma-1}} - q \right] dG(z'|z) \tag{5.5}$$

then we can write $Y_a$ as:

$$Y_a = E(x)E(\Gamma) + SD(x)SD(\Gamma)Corr(x,\Gamma) = E(x)E(\Gamma) \left( 1 + \frac{SD(x)}{E(x)} \frac{SD(\Gamma)}{E(\Gamma)} Corr(x,\Gamma) \right) \tag{5.6}$$

Taking logs of both sides, and using the approximation $\log(1+a) \approx a$:

$$\log(Y_a) \approx \log(E(x)) + \log(E(\Gamma)) + \frac{SD(x)}{E(x)} \frac{SD(\Gamma)}{E(\Gamma)} Corr(x,\Gamma) \tag{5.7}$$

This decomposes output into three terms: investment, average productivity, and allocative efficiency. The treatment causes all three terms to improve. Investment improves due to cheaper financing. Average productivity increases because wages rise (filtering out lower productivity farmers), and because some high productivity farmers with low assets start farming. Allocative efficiency improves because more productive farmers expand by more than low productivity farmers. Difference this decomposition comparing the period after treatment with the initial stationary equilibrium, we find that the 5.6% increase in output comes 4.3% from the increase in investment, 0.9% from increased productivity and 0.4% from allocative efficiency.

We can measure the welfare effects of the intervention.[14] As our welfare metric, we report the increase in the state $a$ that would be needed to make the household indifferent (in partial equilibrium) between experiencing the treatment or not. That is, for each $(a,z)$, we compute the $\Delta(a,z)$ that solves: $V(a+\Delta(a,z),z) = V^T(a,z)$, where $V^T(a,z)$ is the value function evaluated in the period of the intervention. As above, we then convert $\Delta$ into Ugandan shillings by comparing the average value of harvests in the model to its value in shillings in the data. For the median group, the value of the treatment was approximately

[14] In this exercise, we measure the effect for the median community.

45,800 shillings per member and that transfer earned approximately 5,900 shillings per member in interest. We find that the average value per group member of the intervention, $\int \Delta(a,z)dM(a,z)$, was approximately 3,200 shillings. Hence the average social return on investment of the intervention was 19.9% (5,900 plus 3,200 divided by 45,800) of which 13% was realized by the external financier and 6.9% was realized by the community.

We find that the largest $\Delta(a,z)$ is realized by the lowest $a$, high $z$ household. At the 10th percentile of assets and $z = z_H$, $\Delta$ was equal to 8,600 shillings. Correspondingly the lowest $\Delta$ was for the high $a$, low $z$ household. For the 90th percentile of assets a $z = z_L$, $\Delta$ is equal to 2,300 shillings. Hence, even for the very high asset households that are savers at baseline, the intervention has positive value.

# 6 Conclusion

We have shown that external capital has positive welfare effects in village economies. In addition to improving risk smoothing, lower interest rates improves allocative efficiency, raises average income, and increases average farm size. This suggests that weak financial market integration may play a role in the observed ubiquity of very small farms.

In this paper we took the external capital as given and did not consider the welfare or decision-making of the outside financier. However, we think our results are directly informative about the effects of integrating the village financial market with an outside financial market that has lower interest rates. Indeed, measuring the welfare and efficiency consequences of fully integrating financial markets at the national level in Uganda (setting aside the question of how to do it) would rely on many of the elasticities that we measured in this paper using well-identified empirical moments.

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