Selection into Credit Markets: Evidence from Agriculture in Mali

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Abstract

We find that returns to capital are higher for farmers who borrow than for those who do not. We measure this using a two-stage loan and grant experiment. In our first stage, we offer loans to some villages and not others. In the second stage, we provide cash grants to a random subset of all farmers in villages where no loans were offered, and to a random subset of farmers who chose not to borrow in villages where loans were offered. We estimate [] returns to capital for the representative sample of all farmers, whereas we find [] return for those who had recently decided not to borrow. Critical for both theory and policy, this heterogeneity persists even after conditioning on a wide range of observed characteristics.

JEL: D21, D92, O12, O16, Q12, Q14

Keywords: credit markets; agriculture; returns to capital

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1 Introduction

The return to investment in productive activities depends on a myriad of influences, reflecting both the realization of risk and underlying heterogeneity in the characteristics of and opportunities available to producers. Some of this variation may be apparent to outside observers; much may not. A primary role of financial markets is to permit investment flows to respond to this variation. We study this process of allocation across farmers in poor villages in Mali, in the context of the randomized expansion of a microcredit program.

We show that agricultural investment is subject to liquidity constraints, and measure the return to agricultural investment in the general population of rural Mali. We show that returns are on average quite high, as can be expected in a capital-poor economy not well integrated into global financial markets. We also show that there is a great deal of variation in the return to agricultural investment across farmers, even across farmers who on many measurable dimensions appear quite similar. We do so by comparing the distribution of returns to investment among the endogenously selected sample of farmers who do *not* borrow in the expanded microcredit program, to the distribution of returns in the general rural population. For those who did *not* borrow, returns to investment are significantly lower, indeed, zero, on average. Thus farmers with particularly high returns to investment are much more likely to select – or be selected -- into borrowing. This implies that much of the variation in returns is *ex ante*, and that farmers are aware of the heterogeneity in expected returns.

High average returns to agricultural investment could emerge when farmers lack capital and face credit constraints. Microcredit organizations have attempted to relieve credit constraints, but most microcredit lenders focus on small business financing. The typical microcredit loan requires frequent, small repayments and therefore does not facilitate investments in agriculture, where income comes as lump sums once or twice a year. By contrast, the loan product studied here is designed for farmers by providing capital at the beginning of the planting season and requiring repayment as a lump sum after the harvest. However, lending may not be sufficient to induce investments in the presence of other constraints.[2] Farmers may

[2] The evidence from traditional microcredit, targeting micro enterprises, is mixed: some randomized studies find an increase in investment in self-employment activity (Crépon et al. 2015; Angelucci, Karlan, and Zinman 2015) while others do not (Attanasio et al. 2015; Augsburg et al. 2015; Banerjee, Duflo, et al. 2015; Tarozzi, Desai, and Johnson 2015). See Banerjee, Karlan and Zinman (2015) for an overview of the above six studies. Rarely have randomized evaluations of microcredit found an increase in the profitability of small businesses as a result of access to microcredit, at least at the mean or median (Banerjee, Duflo, et al. 2015; see Crépon et al. 2015 as the exception). These limited results from microcredit come in spite of evidence that the marginal returns to capital can be quite high in micro-enterprise (de Mel, McKenzie, and Woodruff 2008).

be constrained by a lack of insurance (Karlan et al. 2013), have time inconsistent preferences (Duflo, Kremer, and Robinson 2011), or face high costs of acquiring inputs (Suri 2011). We investigate whether capital constraints are binding among farmers in Mali, and then, critically, if farmers with higher marginal returns to investment are those most likely to borrow.

We use an experiment which offered some farmers access to loans and other farmers unrestricted cash grants. Out of 198 study villages, our partner microcredit organization, Soro Yiriwaso, offered loans in 88 randomly assigned villages. In those "loan" villages, women could get loans by joining a local community association. In the remaining "no-loan" villages, no loans were offered. In the no-loan villages, we randomly selected households to receive grants worth 40,000 FCFA (US$140). In loan villages, we waited until households (and the associations) had made their loan decisions and then we gave grants to a random subset of those households who did not borrow. We compare the average returns to the grant in the representative set of farmers in no-loan villages to the average returns to the grant in the self-selected sample of households who did not take out loans in loan villages. This allows us to test an important question on selection: do those who do not borrow have lower average returns than those who do borrow?

The cash grants in no-loan villages led to a significant increase in investments in cultivation. We observe more land being cultivated (8.4%, se=3.2%), more fertilizer use (16.2%, se=6.0%), and overall more input expenditures (15.0%, se=4.4%). These households also experienced an increase in the value of their agricultural output and in net revenue[3] by 13.4% (se=3.8%) and 12.7% (se=4.9%), respectively. Thus, we observe a statistically significant and economically meaningful increase in investments in cultivation and an increase in net revenue from relaxing capital constraints. This impact on net revenue even persists after an additional agricultural season. Thus in this environment, capital constraints are limiting investments in cultivation.[4]

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[3] We do not have a complete measure of profits, and thus are using the term "net revenue" as this is the value of agricultural output net of most, but not all, expenses. Net revenue is the value of harvest (whether sold, stored or consumed) minus the cost of fertilizer, manure, herbicide, insecticide, hired labor, cart and traction animal expenses (rental or maintenance), and seed expenses (although valuing last year's seeds at zero). We do not subtract the value of own, family or other unpaid labor or the implicit rental value of land used, because both the labor and land markets are too thin to provide reliable guidance on these values. Instead, we examine the use of these inputs directly.

[4] The increase in investment contingent upon receipt of the grant is sufficient to reject neoclassical separation, but not to demonstrate the existence of binding capital constraints. For example, in models akin to Banerjee and Duflo (2012) with an upward-sloping supply of credit each farmer, a capital grant could completely displace borrowing from high-cost lenders, lower the opportunity cost of capital to the farmer and induce greater investment even though the farmer could have borrowed more from the high cost lender and thus was not capital constrained in a

In loan villages, households given grants did not earn any higher net revenue from the farm than households not provided grants. This contrasts sharply with households given grants in the no-loan villages who had large increases in net revenue relative to those not provided grants. Therefore, we conclude that households which borrowed, and were thus selected out of the sample frame in loan villages, had higher marginal returns than those who did not borrow. The differences in the impact of the grants between households who borrow and those who do not are substantial. We estimate that among borrowing households, $110 of the $140 grant is accounted for by increases in cultivation expenses, while farm output increases by $240 (both impacts significantly different from zero at the 1% level). In contrast, we estimate that among households who do not borrow, receipt of the grant generates only $20 of additional expenditure on cultivation and output (neither being statistically significantly different from zero).

We also look at other outcomes such as livestock ownership and small business operations. There is no evidence that grant recipients in loan villages are investing the capital in alternative activities more than their counterparts in no-loan villages. We conclude that there are heterogeneous returns across farmers, and specifically that the lending process sorts farmers into higher and lower productivity farmers.

Thus the impacts of cash grants in the loan villages versus no-loan villages reveal important selection effects induced by the lending process. The experimental design allows us to show that farmers who use capital more productively are also more likely to take loans and to measure the magnitude of that difference. We can then ask whether this composition effect is predictable by observables. If the heterogeneity is predictable by information observable to the lender *ex-ante*, then the lender could use this information both for social purposes (to focus their efforts marketing to those who stand the most to gain, from a poverty alleviation perspective) as well as expand access to credit (i.e., risk-based pricing, to alleviate adverse selection problems). We find that even after conditioning on the rich set of characteristics in our data, the positive selection induced by the lending process remains strong.

But which aspects of the lending process create the positive selection? Is this driven by borrower self-selection, lender selection or both? The experimental design itself does not allow us to separate these mechanisms, nor does the institutional setting of this credit market provide benefit or cost shifters that would permit estimates of the selection process using local

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strict sense. However, there is no evidence that these grants lowered total borrowing. Therefore, we refer to the range of capital market imperfections that could cause investment responses to cash grants simply as credit constraints.

instrumental variables methods as in Heckman (2010) or Eisenhauer et al (2015). We instead provide a simple economic model of the selection process and combine this with information generated by the second stage randomization of grants in the random and selected samples to suggest that the positive effect operates through a combination of both self-selection and lender screening. By looking at the distribution of returns, we find that whereas in no-loan villages there is no correlation between baseline net revenue and marginal returns to the grant, in the loan villages, the marginal returns to the grant are close to zero for those with high baseline net revenue, but positive for those with low baseline net revenue. If the lender (either the outside organization or the community association) were selecting borrowers, they would select based on profit *level,* not *marginal* profits, since profit levels are more important in determining repayment. On the other hand, if the borrower is self-selecting, the borrower will do the reverse: select in based on *marginal* profits, not profit *level*. Using our best proxy of profits, net revenues, we find both that high marginal, low average profit farmers are under-represented among borrowers, suggesting that they are screened out of borrowing by the lender; and that low marginal, high average profit farmers are under-represented among borrowers, suggesting self-selection.

We also estimate the intent-to-treat impacts of offering loans on a range of agricultural decisions, in order to compare behavior changes induced by the loan and cash grants. About 21% of households in our sample received loans (in loan villages), which is a take-up rate far below that of the grants - all households accepted the grants - but similar to other microcredit contexts. The average loan size was 32,000 FCFA (US$113). Like the grants, we find that offering loans led to an increase in investments in cultivation, particularly fertilizer, insecticides and herbicides, and an increase in agricultural output. We do not detect, however, a statistically significant increase in net revenue. Therefore we observe farmers investing in cultivation when capital constraints are relaxed through credit. Our treatment on the treated (ToT) estimates of the impact of borrowing on the cultivation activities and harvests of those who borrowed are large and consistent with our entirely separate estimates of the impact of grants on borrowers. Therefore, it does not appear that the lending process leads to dramatically different behavior on the part of farmers than cash grants.

These loan impact results are in stark contrast to a long history of failed agricultural credit programs (Adams 1971), which often were implemented as government programs and thus plagued by politics (Adams, Graham, and Von Pischke 1984). In the expansion of microcredit in the 1980s and onward, we have seen several changes occur at once: a shift from individual to group lending processes (although now this trend is reversing (Giné and Karlan 2014; de Quidt, Fetzer, and Ghatak 2012)), a shift from balloon payments to high frequency repayment (Field et al. 2013 study a lending product that partially reverses this trend, with a delayed start to repayments), a shift from government to nongovernment (and now to for-profit) institutions, and a shift from agricultural focus to entrepreneurial focus (Karlan and Morduch 2009; Armendariz de Aghion and Morduch 2010). The loan impact component of this study effectively returns to this older question, but tests an agricultural lending model that is different than had been employed in the past, one with group liability, little to no subsidy, and no government involvement.

The random choice of communities into which to enter by the lender is sufficient for us to estimate ITT effects of the lending program, avoiding strong assumptions on the selection process. Our results provide evidence of quantitatively important selection on unobserved variables, which has methodological implications for impact evaluation. Had we matched borrowers to non-borrowers on observable characteristics to assess the impact of lending to farmers, we would have overestimated the impact of credit, since conditional on an unusually wide range of observed characteristics those who borrow have substantially higher returns to capital than those who do not borrow.

2 The Setting, experimental design and data

Agriculture in most of Mali, and in all of our study area, is exclusively rainfed. Evidence from nearby Burkina Faso suggests that income shocks translate into consumption volatility (Kazianga and Udry 2006), so improving agricultural output can have important welfare consequences not only on the level of consumption but also the household's ability to smooth consumption within a year. The main crops grown in the area include millet/sorghum, maize, cotton (mostly grown by men); and rice and groundnuts (mostly grown by women). At baseline, about 40% of households were using fertilizer[5], and 51% were using other chemical inputs (herbicides, insecticide).

The loans were marketed, implemented, serviced and financed by Soro Yiriwaso (SY), a Malian microcredit organization (and an affiliate of Save the Children, an international nongovernmental organization based in the United States). The cash grants were implemented by Innovations for Poverty Action. Figure 1 demonstrates the design, and Figure 2 presents the timeline.

[5] The government of Mali introduced heavy fertilizer subsidies in 2008. The price of fertilizer was fixed to 12,500 FCFA per 50kg of fertilizer. This constituted a 20% to 40% subsidy, depending on the type of fertilizer and year. Initial usage of the subsidy was low in rural areas initially but has grown over time, helping to explain the increase in input expenses we observe in our data from baseline to endline (Druilhe and Barreiro-Huré 2012).

2.1 Experimental design

The sample frame consisted of 198 villages, located in two *cercles* (an administrative unit larger than the village but smaller than a region) in the Sikasso region of Mali.[6] The randomization consisted of two steps: First, we assigned villages to either loan (88) or no-loan (110) treatment. In loan villages, anyone could receive a loan by joining a women's association created for the purpose of administering loans for SY. Second, after loan participation had been decided, those households who did not borrow were randomly assigned to either receive a grant or not. Below we describe each component in detail.

Loans

SY offered their standard agricultural loan product, called *Prêt de Campagne*, in 88 of the study villages (village-level randomization). This product is given exclusively to women, but money is fungible within the household. Unlike most microloan products, it is designed specifically for farmers: loans are dispersed at the beginning of the agricultural cycle in May-July and repayment occurs after harvest. Administratively the loan is given to groups of women organized into village associations, but each individual woman receives an informal contract with the association. Qualitative interviews with members outside the study villages, prior to the intervention, revealed that the application process is informal with few administrative records at the village level. For example, there are no records of loan applications or denial. Nor is a record kept of more subtle, informal processes of "application" or "denial", such as women who discuss the possibility of joining the group to get a loan but who are discouraged from joining (such data would have been helpful for ascertaining the extent of peer versus self-selection, for instance). The size of the group is not constrained by the lender: a group could add a member without decreasing the size of loan each woman received. The size of the loan to each woman is also determined though an informal, iterative process. Repayment is tracked only at the group level, and there is nominally joint liability. On average there are about 30 women per group and typically 1, though up to 3, associations per village. This is a limited liability environment since these households have few assets and the legal environment of Mali would make any formal recourse on the part of the bank nearly impossible. However, given that loans are administered through community associations, the social costs of default could

[6] Bougouni and Yanfolila are the two cercles. Both are in the northwest portion of the region and were chosen because they were in the expansion zone of the MFI, Soro Yiriwaso. The sample frame was determined by randomly selecting 198 villages from the 1998 Malian census that met three criteria: (1) were within the planned expansion zone of Soro Yiriwaso, (2) were not currently being serviced by Soro Yiriwaso, and (3) had at least 350 individuals (i.e., sufficient population to generate a lending group).

be quite high. In practice we observe no defaults over the two agricultural cycles where we were collaborating with SY.[7]

The annual interest rate is 25% plus 3% in fees and a mandatory savings of 10%. SY offered loans in the study villages for the 2010 and 2011 agricultural seasons. The average loan size in 2010 was 32,000 FCFA (US$113).[8]

Grants

Grants worth 40,000 FCFA (US$140) were distributed by Innovations for Poverty Action (with no stated relationship to the loans or SY) to about 1,600 female survey respondents in May and June of the agricultural season of 2010-2011. In the 110 no-loan villages, households were randomly selected to receive grants and a female household member – to parallel the loans – was always the direct recipient. US$140 is a large grant: average input expenses, in the absence of the grant, were US$196 and the value of agricultural output was US$522. The size of the grant was chosen to closely mimic the size of the average loan provided by SY, though *ex post* the grant ended up being slightly larger on average than loans. In no-loan villages, we also provided some grants to a randomly selected set of men, but we exclude those households from the analysis in this paper.[9]

In loan villages, grant recipients were randomly selected among survey respondents who did not take out a loan.[10] We attempted to deliver grants at the same time in all villages, but administrative delays on the loan side meant that most grants were delivered first in no-loan villages, and there is an average 20-day difference between when no-loan households received their grants from their counterparts in loan villages. We discuss the implications of this delay in section 3.2.1.

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[7] This is not atypical for Soro. In an assessment conducted by Save the Children in 2009, 0% of Soro's overall portfolio for this loan product was at risk (> 30 days overdue) in years 2004-2006, rising to only .7% in 2007.

[8] We use the 2011 PPP exchange rate with the Malian FCFA at 284 FCFA per USD throughout the paper.

[9] These data are intended for a separate paper analyzing household dynamics and bargaining, and we do not consider them useful for the analysis here since loans were only given to women.

[10] We determined who took out a loan by matching names and basic demographic characteristics from the loan contracts between the client and Soro Yiriwaso, which Soro Yiriwaso shared with us on an ongoing basis. There were a few cases (67) where Soro Yiriwaso allowed late applications for loans and households received both a grant and a loan. The majority (41 out of 67) of these cases occurred because there were multiple adult women in the household, and one took out a loan and another received a grant. We include controls for these households. The results are similar if the observations are excluded.

In order to minimize the possibility of dynamic incentives to not borrow, we informed recipients that the grants were a one-time grant, not an ongoing program, and also distributed some grants in loan villages to some borrowers who were not in the survey, so that it was not obvious that borrowing precluded someone from being a grant recipient.

2.2 Data

Figure 2 shows the timeline of the project. The baseline was conducted in January-May 2010. A first follow-up survey was conducted after the first year of treatment and the conclusion of the 2010 agricultural season[11] in January-May 2011, and a second follow-up survey was conducted after the second year of treatment and the conclusion of the 2011 agricultural season in January-May 2012. In the three rounds, similar survey instruments covered a large set of household characteristics and socioeconomic variables, with a strong focus on agricultural data including cultivated area, input use and production output at individual and household levels. We also collected data on food and non-food expenses of the household as well as on financial activities (formal and informal loans and savings) and livestock holdings.

2.3 Randomization, balance check and attrition

The randomization was done after the baseline using a re-randomization technique ensuring balance on key variables.[12] The randomization of the provision of grants was done at the household level, while the loan randomization was at the village level. Moreover, we did

[11] We also conducted an "input survey" on a subsample of the sample frame right after planting in the first year (September-October 2010), in order to collect more accurate data on inputs such as seeds, fertilizer and other chemicals, labor and equipment use. This input survey covered a randomly selected two thirds of our study villages (133 villages) and randomly selected half of the households (stratifying by treatment status) to obtain a subsample of 2,400 households. We use the input survey if conducted, and if not we use the end of season survey. We also control for timing of the collection of the data in all relevant specifications.

[12] First, a loop with a set of number of iterations randomly assigned villages to either loan or no-loan, and then we selected the random draw that minimized the t-values for all pairwise orthogonality tests. This is done because of the difficulties stratifying using a block randomization technique with this many baseline variables. The variables used for the loan randomization were: village size, an indicator for whether the village was all Bambara (the dominant ethnic group in the area), distance to a paved road, distance to the nearest market, the percent of households having a plough, the percentage of women having a plough, fertilizer use among women in the village, average literacy rate, and the distance to the nearest health center. For household-level randomization we used: whether the household was part of an extended family; was polygamous; the primary female respondent's: land size, fertilizer use, and whether she had access to a plough; an index of the household's agricultural assets and other assets, and per capita food consumption. See Bruhn and McKenzie (2009) for a more detailed description of the randomization procedure.

separate randomization routines for the grant recipients in the loan and no-loan villages. We control for all village and household-level variables used in the re-randomization routine and interactions of the household-level variables with village type (loan or no-loan) in all analyses.

We conduct different tests to verify that there are no important observable differences between the different groups in the sample, using variables not included in the randomization procedure. Appendix Table A1 looks at baseline characteristics across three comparisons: (i) loan to no-loan villages; (ii) grant to no-grant households in no-loan villages; and (iii) grant to no-grant households in loan villages. Few covariates are individually significantly different across the three comparisons, and an aggregate test in which we regress assignment to treatment on the set of 11 covariates fails to reject orthogonality for each of the 3 comparisons (p-value of 0.26, 0.91 and 0.67, respectively, reported at the bottom of the table).

Our attrition rate is low: approximately one percent each round. Regardless, Appendix Table A2 reports tests for differential attrition comparing the same groups as in Table A1, from baseline to the first follow-up and to the endline. For each of the three comparisons, we fail to reject that attrition rates are on average the same in the compared groups for both follow up years. In a regression of attrition on the nine covariates, treatment status, and the interaction of nine covariates and treatment status, a test that the coefficients on treatment status and the interaction terms are jointly zero fails to reject for all but one of the six regressions (results on bottom row of Appendix Table 2).

3 Selection into loans and the return to cash grants

We focus on agricultural outcomes, so consider agricultural output Q. $\{Q(0,0), Q(0,1), Q(1,0)\}$ represents the set of possible outputs in year 1 of households in our sample. $Q(0,0)$ is a random variable representing potential output if the household neither borrows nor receives a grant; $Q(1,0)$ and $Q(0,1)$ are similarly defined for households who receive a loan but not a grant, and for those who receive a grant but not a loan.[13] [14] The joint distribution of potential outcomes is $F\big(Q(0,0), Q(0,1), Q(1,0)\big)$, and the three marginal distributions are denoted $F_{NG}\big(Q(0,0)\big)$, $F_G\big(Q(0,1)\big)$ and $F_B\big(Q(1,0)\big)$.

[13] There is a fourth logically possible potential outcome, $Q(1,1)$ for households who both borrow and receive a grant, but this is irrelevant in our context because no one who receives a loan is ever assigned to the grant treatment.

[14] This is a minor adaptation of the standard potential outcomes notation building on Rubin (1974); Heckman (1992, 1997); Imbens and Angrist (1994); Angrist, Imbens and Rubin (1996); Heckman, Ichimura and Todd (1997).

Define $B = 1$ if the household would take up a loan if located in a loan village, and $B = 0$ if the household would not take up a loan if in a loan village. This is the selection process of interest; our first goal is to identify the expected value at the effect on output of receiving a grant for households for which $B = 1$ versus those for which $B = 0$. The two-stage randomization provides straightforward identification of the expected value of the return to an exogenous cash grant for these two groups.

Define $G \in \{0,1\}$ and $L \in \{0,1\}$ as random variables designating a household's status in the grant treatment arm and in a loan treatment village, respectively. The first stage randomization of villages ensures

$$\{Q(0,0), Q(0,1), Q(1,0), B\} \perp L. \tag{1}$$

The second stage randomization of grants across the random sample when $L = 0$ and across non-borrowers when $L = 1$ ensures

$$\{Q(0,0), Q(0,1), Q(1,0), B\} \perp G | L = 0 \tag{2}$$

$$\{Q(0,0), Q(0,1)\} \perp G | (B = 0, L = 1). \tag{3}$$

There is 100 percent takeup of the offer of a grant, so in our sample of non-borrowers in loan villages, we observe

$$Q|(L = 1) = L(1 - B)[Q(0,1)G + Q(0,0)(1 - G)]$$

and in our sample of the full population of no-loan villages we observe

$$Q|(L = 0) = (1 - L)[Q(0,1)G + Q(0,0)(1 - G)]$$

Therefore, (1) and (2) imply that data from the full population of no-loan villages can be used to identify the conditional marginal distributions

$$F_G(Q(0,1)|L = 0, G = 1) = F_G(Q(0,1)|L = 0, G = 0) = F_G\big(Q(0,1)\big) \tag{4}$$

and

$$F_{NG}(Q(0,0)|L = 0, G = 0) = F_{NG}(Q(0,0)|L = 0, G = 1) = F_{NG}\big(Q(0,0)\big). \tag{5}$$

(1) and (3) imply that data from the population of non-borrowers in loan villages can be used to identify

$$\begin{aligned} F_G(Q(0,1)|B = 0, L = 1, G = 1) &= F_G(Q(0,1)|B = 0, L = 1, G = 0) \\ &= F_G(Q(0,1)|B = 0) \end{aligned} \tag{6}$$

$$F_{NG}(Q(0,0)|B=0,L=1,G=0)=F_{NG}(Q(0,0)|B=0,L=1,G=1) \\ =F_{NG}(Q(0,0)|B=0). \tag{7}$$

The loan village population provides an estimate of $\mathbb{P}(B=1|L=1)=\mathbb{P}(B=1)$, which together with (4) and (6) provides (8a); and together with (5) and (7) provides (8b):

$$F_G(Q(0,1)|B=1)=\frac{F_G\big(Q(0,1)\big)-F_G(Q(0,1)|B=0)\big(1-\mathbb{P}(B=1)\big)}{\mathbb{P}(B=1)}$$
$$F_{NG}(Q(0,0)|B=1)=\frac{F_{NG}\big(Q(0,0)\big)-F_{NG}(Q(0,0)|B=0)\big(1-\mathbb{P}(B=1)\big)}{\mathbb{P}(B=1)} \tag{8}$$

If we define the effect of receiving a grant without a loan as $\Delta_G Q\equiv Q(0,1)-Q(0,0)$, then from (2), we have

$$\mathbb{E}(Q(0,1)|B=0,L=1,G=1)-\mathbb{E}(Q(0,0)|B=0,L=1,G=0) \\ =\mathbb{E}(Q(0,1)-Q(0,0)|B=0,L=1)\equiv\mathbb{E}(\Delta_\mathrm{G}Q|B=0,L=1) \tag{9}$$

and

$$\mathbb{E}(Q(0,1)|L=0,G=1)-\mathbb{E}(Q(0,0)|L=0,G=0)=\mathbb{E}(Q(0,1)-Q(0,0)|L=0) \\ \equiv\mathbb{E}(\Delta_\mathrm{G}Q|L=0). \tag{10}$$

(9), along with $\mathbb{P}(B=1|L=1)=\mathbb{P}(B=1)$, can be estimated from the loan villages and (4) can be estimated with data from the no-loan villages. Equation (1) then implies that we can identify three average treatment effects of immediate interest:

$$\mathbb{E}(\Delta_\mathrm{G}Q)=\mathbb{E}(\Delta_\mathrm{G}Q|L=0)$$
$$\mathbb{E}(\Delta_\mathrm{G}Q|B=0)=\mathbb{E}(\Delta_\mathrm{G}Q|B=0,L=1) \tag{11}$$
$$\mathbb{E}(\Delta_\mathrm{G}Q|B=1)=\frac{\mathbb{E}(\Delta_\mathrm{G}Q)-\mathbb{E}(\Delta_\mathrm{G}Q|B=0)}{\mathbb{P}(B=1)}+\mathbb{E}(\Delta_\mathrm{G}Q|B=0)$$

which are the average effects of receiving a grant (without a loan) amongst the general population, amongst those who would not borrow if they were in a loan village, and amongst those who would borrow if they were in a loan village.

Finally, we are also interested in the expected treatment effect of borrowing on those who would borrow if loans were available: $E(\Delta_B Q|B=1)\equiv E(Q(1,0)-Q(0,0)|B=1)$. We have already noted that (1) implies that $E(Q(0,0)|B=0)$ is identified from data on the output of nonborrowers in loan villages, and that $E\big(Q(0,0)\big)$ is identified from average output in nonloan villages. From (1), data from the loan villages identifies $\mathbb{P}(B=1|L=1)=\mathbb{P}(B=1)$. Then, in parallel with (11)

$$\mathbb{E}(Q(0,0)|B=1)=\frac{\mathbb{E}\big(Q(0,0)\big)-\mathbb{E}(Q(0,0)|B=0)}{\mathbb{P}(B=1)}+\mathbb{E}(Q(0,0)|B=0) \tag{12}$$

(12) and (1) imply that we can identify the average treatment effect on the treated of borrowing:

$$\begin{aligned}\mathbb{E}(Q(1,0)|L=1,B=1,G=0)-\mathbb{E}(Q(0,0)|B=1)\\ =\mathbb{E}(Q(1,0)|B=1)-\mathbb{E}(Q(0,0)|B=1)=\mathbb{E}(\Delta_B Q|B=1).\end{aligned} \tag{13}$$

3.1 Observable characteristics of borrowers versus non-borrowers

Take-up of the loans, determined by matching names from administrative records of SY with our sample, was 21% in the first agricultural season (2010-11) and 22% in the second (2011-2012). Despite the similarity in overall take-up numbers, there is a lot of turnover in clients. Only about 65% of clients who borrowed in year 1 took out another loan in year 2. This overall take-up figure is similar to other evaluations of group-=based microcredit focusing on small enterprise (Angelucci, Karlan, and Zinman 2015; Attanasio et al. 2015; Banerjee, Duflo, et al. 2015; Banerjee, Karlan, and Zinman 2015; Crépon et al. 2015; Tarozzi, Desai, and Johnson 2015). Table 1 provides descriptive statistics from the baseline on households who choose to take out loans in loan villages, compared to non-clients in those villages. Information on the household as a whole as well as the primary female respondent and primary male respondent is reported. There is a striking pattern of selection into loan take-up: households that invest more in agriculture, have higher agricultural output and net revenue. Net revenue is our best proxy for profits: it is net of most, but not all, expenses. It is the value of harvest (whether sold, stored or consumed) minus the cost of fertilizer, manure, herbicide, insecticide, hired labor, cart and traction animal expenses (rental or maintenance), and seed expenses (although valuing last year's seeds at zero). We do not subtract the value of own, family or other unpaid labor or the implicit rental value of land used, because both the land and labor markets are too thin to have relevant market prices to use in a calculation of profits. Borrowers also have more agricultural assets and livestock. Figure 3 demonstrates that this holds across the whole distribution. Women in households who borrow are also more likely to own a business and are more "empowered" by three metrics: they have higher intra-household decision-making power, are more socially integrated, and are more engaged in community decisions.[15] Households that borrow also have higher consumption at baseline than non-clients.

3.2 Returns to the grant in loan and no-loan villages

Panel A of Table 2 shows the estimates from the following regression using the two years of follow up data we have on farm investments and output.

$$
\begin{aligned}
(14) \qquad & Y_{ijt} = \alpha + \beta_1 grant_i \cdot I\{t = 2011\} + \beta_2 grant_i \cdot I\{t = 2011\} \cdot loan_j \\
& + \beta_3 grant_i \cdot I\{t = 2012\} + \beta_4 grant_i \cdot I\{t = 2012\} \cdot loan_j \\
& + \beta_5 I\{t = 2012\} + \beta_6 I\{t = 2012\} \cdot loan_j + X_{ijt}\pi + \lambda_j + \epsilon_{ijt}
\end{aligned}
$$

where $grant_i$ indicates individual i received a grant in May-June 2010, and $loan_j$ indicates that the MFI offered loans in village j. $I\{t = 2011\}$ is an indicator of the data round. We also include year by village type (loan vs no-loan) controls, and additional baseline controls (X) which include the baseline value of the dependent variable y_0[16] plus its interaction with year by village type, village fixed effects, and stratification controls described in section 2.3 and listed in the notes of the table. β_1 and β_2 are the primary coefficients of interest. β_1 is the effect of the cash grant on the outcome Y_{ijt} in the no-loan villages, i.e., the average effect of the cash grant among all potential borrowers. β_2 shows the differential impact of receiving a grant on the outcome Y_{ijt} for the households that did not borrow (in loan villages) compared to the random, representative sample in no-loan villages.

Panel A of Table 2 shows the estimates from this regression for a variety of cultivation outcomes (inputs along with harvest output and net revenue) and Panel A of Table 3 shows the analogous estimates for other, non-cultivation outcomes such as livestock, small business ownership, consumption, and female empowerment.

---

[15] All three of these variables are indices, normalized by the no-grant households in no-loan villages. The household decision-making index includes questions on how much influence she has on decisions in the following domains: food for the household, children's schooling expenses, their own health, her own travel within the village, and economic activities such as fertilizer purchases and raw materials for small business activities. The community action index includes questions on: how frequently she speaks with different village leaders, and different types of participation in village meetings and activities. The social capital index includes questions about 7 other randomly selected community members from our sample and whether the respondent knows the person, are in the same organization, would engage in informal risk sharing and transfers with the person, and topics of their discussions (if any).

[16] In cases where the observation is missing a baseline value, we instead give the lagged variable a value of -9 and also include an indicator for a missing value.

3.2.1 Agriculture

Columns (1)-(6) look at agricultural inputs. We see in the first row that in households who did receive a grant in no-loan villages, compared to those who did not, the amount of land cultivated increased (0.17 ha, se=0.065) a small but significant amount. The grant also induced an increased in hired labor days (2.7 days, se=0.80). 2.7 days over the entire agricultural season is a small number, but these households use very little hired labor: the mean in the control in 2011 is only 17 days. Fertilizer ($12, se=4.3) and other chemical inputs ($9, se=2.2) also increased by 14 and 19 percent respectively. Total input expenses (excluding family labor and the value of land) increased by US$28 (se=8.2), a 14 percent increase. The grants therefore led to an increase in agricultural investment. Columns (7)-(8) show that output and farm net revenue also went up significantly. Output went up by 13 percent ($67, se=19) and net revenue by 13 percent ($40, se=15). Overall, we see significant increases in investments and ultimately net revenue from relaxing capital constraints. [17]

Table 2 shows that the selected sample of households who did not take out a loan do not experience such positive returns when capital constraints are relaxed. Across the board, the estimates of the impact of the grant in loan villages in 2011 (year 1) are near zero. Column (1) shows that while households in no-loan villages increased the amount of land cultivated as a result of the grant, households in loan villages (who did not take out a loan) by contrast did not (β_2 is -0.15 ha, se=0.09 and the p value of the test that the sum of β_2 and β_1 is zero is 0.69). The interaction term for family labor days (-8, se 6.5), fertilizer expenses (-$9, se=6.5) and other chemical expenses (-$6, se=3) are all negative, though only the latter is statistically significant. Total input expenses in loan villages do increase in response to the grant by $20 (*p* value is 0.03), which is not statistically different from the estimate in no-loan villages of $28. However, we see no corresponding increase in output nor in net revenue. The β_2 interaction coefficient for output is similar in magnitude and negative (-$47, se=28), offsetting the increase in output in no-loan villages ($67, se=19). The test that the sum of the two coefficients is different from zero is not rejected (p=0.33). Similarly for net revenue, the total effect in loan villages is actually negative (-$3.30) and not significantly different from zero (p=.84). Thus while there is some evidence that among households who did not take out loans, the grant induced some increase

[17] We are not estimating the marginal product of capital as in de Mel, McKenzie, and Woodruff (2008) but instead the "total return to capital"– i.e., cash. Beaman et al. (2013) showed in this same area that labor inputs also adjust along with agricultural inputs, making it impossible to separate the returns to capital from the returns to labor without an additional instrument for labor inputs. We are therefore capturing the total change in profits and investment behavior when capital constraints are relaxed.

in inputs, there is no evidence of increases in agricultural output nor net revenue – in stark contrast to the random sample of households in no-loan villages.

These estimates imply that there is a great deal of heterogeneity in marginal returns to relaxing capital constraints across farmers, and that those who borrow are disproportionately those with high returns. The return in year 1 to the grant implied for would-be borrowers in no-loan villages is $145.96 (se=67.75) in additional net revenue per $100 of grant.[18] In contrast, the return for non-borrowers is negative, although not statistically significantly different from zero.

The analysis indicates that households who do not borrow are those without high returns in agriculture to cash transfers. In contrast to the literature on health products, where much of the evidence points towards limited screening benefits from cost sharing (Cohen and Dupas 2010; Tarozzi et al. 2013), we find that the repayment liability does lead lower return households to be screened out. The design does not allow us to experimentally determine whether households are self-selecting (demand side) or being screened by the lender (supply side). We return to this question in section 4.

Year 2

We observe a persistent increase in output and net revenue in the 2011-2012 agricultural season (year 2) from the grant given in 2010, as shown by the β_3 coefficients in Panel A of Table 2: output is higher in grant recipient households by $50 (se=22) in Column (7) of Table 2 and net revenue by $46 (se=17). This is striking since we do not observe grant-recipient households spending more on inputs in Column 6 ($2, se=10). One thing to note, however, is that some of the investments in year 1 may benefit year 2 output. There are also changes in agricultural practices which we may not capture with our measure of input expenses. For example, in 2011 grant-recipient households spend more on purchasing seeds. In 2012 these households spend no more on seeds than control households but they do use a larger quantity of seeds. This could reflect learning but also could reflect the use of hybrid seeds in year 2011 which provide some yield benefits the following year, even without re-purchasing seeds. This highlights that our simple accounting of 2011 net revenue as 2011 output minus 2011 inputs is imperfect as a measure of profits, but we have no way of constructing a depreciation rate for the various inputs. We also see a continued increase in the extensive margin of fertilizer use but not in (average) expenses.

[18] Calculated as $(\beta_1 + 0.79\beta_2)/(.21 * 1.4)$ where 0.21 is the loan takeup rate in loan villages, and the grant size is $140.

In year 2, we see a similar negative interaction term, β_4, on net revenue in Column (8) as in year 1, though not significant at the 10% level (-$33, se=23). The lower net revenue may be a result of higher input use: Column (6) shows that, in loan villages, grant-recipient households spent more on input expenses ($30, se=17.1) than control households in 2012.

Timing

One concern about our interpretation of the results is that on average, households received grants in loan villages 20 days later than in no-loan villages because of delays in the administration of the loans. If farmers in no-loan villages received grants too late in the agricultural cycle to make productive investments, we would erroneously conclude that there is positive selection into agricultural loans when in reality the result is attributable to our experimental implementation. This is particularly a concern since we observe farmers increase the amount of land they farm, which is a decision which occurs very early in the agricultural cycle. In Appendix Table A3, we look at land cultivated (i.e., an investment decision made early in the process) and an index of all the agricultural outcomes and find no relationship with the timing of the grant, among the grant-recipient households in no-loan villages.[19]

Spillovers

It is possible that households received neither grants nor loans were indirectly affected by the study interventions, either positively (if grants or loans were shared) or negatively (through general equilibrium effects on locally determined prices). We do not have a perfect way to address such spillovers. We do, however, have data from an additional 69 villages in the same administrative units (cercles) as our study villages.[20] Appendix Table 4 shows that no-grant households in no-loan villages had similar agricultural practices to households in villages where we did no intervention. There are no significant differences in land cultivated, suggesting that the increase in land cultivated among grant recipients was not zero-sum with households who did not get a grant. There are also no significant differences in total input expenses, value of the

[19] We look at two main specifications: one in which we include date the grant was received linearly and with its square, and a second which splits the sample into the first half of the grant period and the second half (since most of the grants in the loan-available villages were distributed in the second half). In both cases we control for whether this was the team’s first visit to the village (revisit to village).

[20] Our partner organization would only commit to not enter 110 villages, which serve as our no-loan villages. The villages we use as no-intervention villages were leftover replacement villages and not entirely randomly selected. For example, the no-intervention villages have larger average population size but fewer children per household than study villages. SY may have offered loans in up to 15 of the 69 villages in year 1. Removing those 15 villages leaves Appendix Table 4 qualitatively unchanged.

harvest, and net revenue. The one significant difference is the number of hired labor days (column 3). Non-grant recipients in no-loan villages hired more labor by four labor days. While this is precisely estimated and a point estimate comparable to main treatment effect in Panel A of Table 2, recall that this is four man-days over the entire course of the agricultural season and therefore unlikely to have affected total output and net revenue.

3.2.2 Other outcomes

Table 3 shows the estimates of equation (1) looking at outcomes other than agriculture. The most striking result is in Columns (1) and (2): grant-recipients households in no-loan villages are more likely to own livestock (11 percentage points, se=0.014), and there is a large ($163, se=70) increase in the value of total livestock compared to no-grant households. This represents a 13% increase in the value of household livestock, and is slightly larger than the value of the grant itself. Recall we saw in Table 3 that households also spent an extra $28 on cultivation investments. The livestock value is measured several months after harvest; these results may indicate that post-harvest, households moved some of their additional farming profits into livestock.[21] We also find evidence that the grant increased the likelihood in no-loan villages that a recipient household had a small enterprise (3.8 percentage points higher, se=0.015), as shown in Column (3).[22] Grant recipient households also consumed more, including 12% more food (Column 4, $0.38 per day in adult equivalency, se=0.11) and 6% in non-food expenditures (Column 5, $2.69 per month, se=1.4). We find the latter persistent in year 2 but food consumption not. Columns (6)-(9) show no main effect of the grant on whether the household has any financial savings, membership in rotating, savings and loans associations (ROSCAs), education expenses or medical expenses.[23]

The investment and spending patterns among grant recipient households in loan villages for the most part echo those described above in no-loan villages. Column (1) shows that while grant recipients in loan villages were overall more likely to own livestock than their control counterparts, the magnitude of the effect is about half as large as in the no-loan villages

[21] We may also over-value recently-purchased livestock which may be younger or smaller in treatment households since we use village-level reports of livestock prices to value livestock quantities for all households.

[22] Appendix Table 5 shows in Column (1) that despite increasing the extensive margin of small business, we do not measure an increase in business profits after year 1.

[23] Columns (2) through (4) of Appendix Table 5 also show no impact in year 1 on women's empowerment, involvement in community decisions nor social capital, respectively.

(interaction term is -3.9 percentage points, se=0.022). The remainder of the outcomes however show few differences.[24]

Taken together, Panel A of Table 3 shows that the grants benefited households in a variety of ways. However, we have no strong evidence that households in loan villages, who did not experience higher agricultural output and net revenue as in no-loan villages, used their grants to invest in alternative higher-return activities other than cultivation.

Year 2

In year 2, we see persistent impacts for some key outcomes in no-loan villages (β_3). Columns (1) and (2) demonstrate that grant-recipient households are more likely to own livestock (0.09, se=0.015) and continue to hold more livestock assets ($180, se=101) than control households in no-loan villages. They are also more likely to own a business (3 percentage points, se=0.013).[25] There is no increase in food consumption in year 2 ($0.05, se=0.17) but an increase in monthly non-food expenditure ($3.72, se=2.1). Households are also more likely to have financial savings (3.5 percentage points, se=0.019) and be members of rotating savings and loans associations (ROSCAs) (3.9 percentage points, se=0.019). Columns (9)-(10) show that there continues to be no measurable impact on educational expenses ($0.42, se=3.64), or medical expenses (-$0.76, se=1.80).[26]

Table 3 shows that, similar to year 1, there is little evidence of households in no-loan villages using grants differently than those in loan villages across this set of non-agricultural outcomes

[24] The only outcome which suggests potential heterogeneity in behavior upon receiving a grant between our random, representative households in no-loan villages and our selected sample in loan villages is medical expenses, in Column (9). Medical expenses (in the last 30 days) are marginally-significantly higher in loan grant households ($4.90, se=2.51), since medical expenses may have declined (-$2.53, se=1.85) among grant recipients in no-loan villages. The total effect in loan villages is not statistically different from zero (p=0.16). This is a difficult outcome to interpret because having more resources could mean a household is more likely to treat illnesses they experience but are also more able to invest in preventative care, making the prediction of the treatment effect ambiguous.

[25] Appendix Table A5 shows in Column (1) that business profits increase by 18% ($41, se=18.5) in year 2.

[26] Appendix Table A5 also suggests no change in intra-household bargaining (0.059 of a standard deviation, se=0.039) or community action (0.021, se=0.045). The social capital index in column (4) shows a significant rise of 0.09 of a standard deviation (se=0.034) in year 2.

(livestock ownership, owning a small business, and consumption) in year 2. There is an alternative hypothesis that the loan selected in people with short-run investments (i.e., those with payoffs within one year), and non-borrowers invested their grants in longer-term investments. However, even by the end of the second year, we do not see profit increases (for non-borrowers in loan villages who receive grants) from enterprise investment, longer-term farm investments, or other long-term investments such as education, to support this hypothesis; nor does the qualitative information from the field support this alternative hypothesis.

3.3 Unobservable versus observable predictors of marginal returns

Table 1 demonstrated that loan-takers are systematically different at baseline than those who do not take out loans on a number of characteristics, including those which are surely important in cultivation: they have more land, spend more in inputs, and enjoy higher output and net revenue. These baseline characteristics may be enough to predict who could most productively use capital on their farm. Theoretically the prediction is ambiguous: many models would predict that those who have the highest returns are households who are the most credit constrained. We observe individuals who take out loans have on average *more* wealth in the form of livestock. This could mean they have lower returns to investments in cultivation. However, they may also have access to better technologies, like a plough, which could increase their returns to capital.

Here we examine whether the marginal returns from grants and the selection effect discussed above are predicted fully by characteristics observed in the baseline, or if there is additional selection that occurs based on unobservables. We use the same specification as earlier but also include baseline characteristics (Z) interacted with an indicator for receiving a grant, for year 1 and year 2.

$$\begin{aligned}(15)\qquad Y_{ijt} &= \alpha + \beta_1 grant_i \cdot I\{t=2011\} + \beta_2 grant_i \cdot I\{t=2011\} \cdot loan_j \\ &+ \beta_3 grant_i \cdot I\{t=2012\} + \beta_4 grant_i \cdot I\{t=2012\} \cdot loan_j \\ &+ \gamma_1 grant_i \cdot Z_{ijt} \cdot I\{t=2011\} + \gamma_2 grant_i \cdot Z_{ijt} \cdot I\{t=2012\} \\ &+ \gamma_3 Z_{ijt} \cdot I\{t=2011\} + \gamma_4 Z_{ijt} \cdot I\{t=2012\} \\ &+ \beta_5 I\{t=2012\} + \beta_6 I\{t=2012\} \cdot loan_j + X_{ijt}\pi + \lambda_j + \epsilon_{ijt}\end{aligned}$$

We structure our analysis by sequentially increasing the controls we include in the regression, by first focusing on Z variables which would be fairly observable to microcredit institutions (MFIs), then including variables which would be fairly observable to the community and therefore may be included in peer screening mechanisms (as in group-lending), and finally adding in our measure of risk aversion.

Table 4 shows our main empirical specification with net revenue as the outcome, with different baseline household-level controls. Column (1) is identical to Column (8) in Table 2 and is included for ease of comparison. Column (2) includes Z variables measured at baseline, and their interactions with grant receipt, that an MFI may be able to easily observe: the household's landholdings (in hectares), the value of their own livestock, agricultural net revenue, an indicator for whether the household has six or more adults (the 90^{th} percentile), an indicator for the presence of an extended family, and the number of children in the household. Column (2) shows that the estimates of the differential effect of the grant in loan versus no-loan villages is reduced in magnitude slightly (-$38.75, se=21.97 compared to -$44 without controls) but continues to be significant at the 10% level. We show the coefficients from the interactions between some of these Z variables and grant receipt. Strikingly, higher baseline net revenues do not predict higher returns to the grant, on average. We also do not observe a statistically significant relationship between baseline livestock value or land size and returns to the grant. However, larger households do benefit more from the grants in years 1 and 2 than smaller households.

Column (3) adds in additional information which would likely be known within the community and thus usable in a peer lending screening process: the primary female respondent's intra-household decision-making power, her engagement in community decision-making and her social capital. Finally, Column (4) also adds in a measure of risk aversion. Respondents were asked to choose between a series of lotteries, which vary in terms of their expected value vs risk. We include an indicator for choosing the perfectly safe lottery, which about half the sample chooses. In all specifications, the estimates on the differential impacts of the grants in loan versus no-loan villages are slightly smaller in magnitude but still negative and statistically significant at the 10% level. We therefore conclude that our estimates of selection effects are not driven by the rich set of observables we measure at the baseline, but by characteristics more difficult for outsiders to observe, such as land productivity, access to complementary inputs, or farmer skill. In the next section we examine whether the selection is a demand-side effect (people choosing whether to borrow or not) or a supply-side effect (lenders or peers choosing whether to let a farmer into their lending circle).

4 Is screening driven by supply-side or demand-side forces?

In section 3.2 we showed that providing cash grants to households who did not take out loans led to lower agricultural returns – and in fact zero returns – compared to households who were randomly selected in no-loan villages. The experimental design provided us with a transparent method for showing that the impact of the grants on agricultural output in the random sample of households is greater than their impact in the selected sample of non-borrowers. In contrast, the experimental design itself does not allow us to differentiate how the screening itself occurs:

it may be the result of self-selection on the part of farmers (demand-side) or due to lender screening on the part of the MFI or community associations (supply-side).[27]

We begin with a simple model to illustrate what we mean by self-selection and by lender screening. In order to distinguish these concepts, the model requires three elements. First, there is liquidity constraint that generates a potential demand for credit. Second, some potential conflict of interest between the borrower and the lender is required if self-selection and lender screening are to be distinguished. In this model, the conflict emerges from limited liability. Third, multiple dimensions of heterogeneity across borrowers will generate patterns of self-selection that differ from those caused by lender screening. Self-selection will be largely driven by heterogeneity in marginal productivity; screening of borrowers by lenders will depend more on heterogeneity that affects the total value of output.

The heterogeneity was introduced in section 3 as a vector of characteristics of the household *Z*. We focus on two dimensions of this heterogeneity of endowments: $z_i = (\eta_i, \theta_i)$. θ_i is an average productivity shifter that affects output but not the marginal product of the input, and η_i affects the marginal product. We consider a situation of symmetric information – both the farmer and the lender know the farmer's endowment before the loan is transacted, but neither knows the realization of ϵ_i, which we think of as a random shock to output realized after borrowing is completed. The lender provides loans normalized to size *1*, at an interest rate of *r* (these parameters are set exogenously at a national level by SY). At the start of the farming season, *i* chooses whether to borrow $B_i \in \{0,1\}$, and the lender chooses whether to lend $L_i \in \{0,1\}$. The loan is made if and only if $B_i L_i = 1$. We are assuming that the farmer has no alternative use for capital outside of agriculture.[28]

Farm net revenue for borrowers (non-borrowers) is $q_i = Q_B(\theta_i, \eta_i, \varepsilon_i)$ ($q_i = Q_N(\theta_i, \eta_i, \varepsilon_i)$). A convenient specification for net revenue that satisfies the assumptions on η and θ is

$$q_i = \eta_i(f_N + (f_B - f_N)B_i L_i) + \theta_i + \epsilon_i \tag{16}$$

[27] The MFI itself has little to no information about individual loan applicants. However, women must go through a community association – which in principle has joint liability for the loan – in order to get a contract with the MFI. It is therefore possible that the associations are screening out some farmers who want to borrow.

[28] This assumption implies that $Q_G \equiv Q_B$, that is, that the farmer uses the grant in the same way he/she would use the loan. This possibly unrealistic assumption can be generalized, at the cost of additional notation, while preserving the lessons we draw for patterns of selection in the following paragraphs. In section 5, we provide evidence that the uses of and returns to the grants are similar to those of the loans.

where $f_B \geq f_N$ reflects the liquidity constraint that generates the demand for credit. For shorthand, we refer to differences across farmers in θ as differences in average productivity and to differences in η as differences in marginal productivity.

Output is produced, and because of limited liability, the loan is repaid in full if and only if net revenue is sufficiently high.[29] The lender receives $\min\{(1+r)B_iL_i, q_i\} - (1+\rho)B_iL_i$ and the farmer keeps $\max\{q_i - (1+r)B_iL_i, 0\}$, where $\rho(< r)$ is the cost of funds to the lender. We assume that both farmer and the lender maximize expected profits.

As a consequence of limited liability, a borrowing farmer earns zero if $\epsilon \leq \overline{\epsilon}$, where

(17) $$\overline{\epsilon} \equiv (1+r) - \eta_i f_B - \theta_i$$

The farmer will want to take a loan if and only if

(18) $$\left(\eta_i f_B + \theta_i - (1+r)\right)\left(1 - G(\overline{\epsilon})\right) + \int_{\overline{\epsilon}}^{\epsilon^H} \epsilon g(\epsilon) d\epsilon \geq \eta_i f_N + \theta_i$$

The set

$$B^{rej} = \left\{(\eta, \theta) | \left(\eta_i f_B + \theta_i - (1+r)\right)\left(1 - G(\overline{\epsilon})\right) + \int_{\overline{\epsilon}}^{\epsilon^H} \epsilon g(\epsilon) d\epsilon < \eta_i f_N + \theta_i\right\}$$

defines the characteristics of farmers who would choose not to borrow. Define η_i^* as the level of marginal productivity such that a loan that will not be defaulted on has an expected return just equal to the interest cost:

(19) $$\eta_i^*(f_B - f_N) = 1 + r$$

η_i^* is independent of θ_i. We can say something about the magnitude of η^*, because the marginal cost of borrowing is on the order of 30%. No household with expected returns under this magnitude will borrow to invest in agriculture unless the probability of default is positive.

Second, define $\theta_i^* \equiv (1+r) - \eta_i^* f_B - \epsilon^L$. A borrowing farmer with an endowment $\{\theta_i^*, \eta_i^*\}$ never defaults, and (18) is satisfied with equality. For all $\eta_i \geq \eta_i^*$, a farmer with endowment $\{\theta_i^*, \eta_i\}$ chooses to borrow. Similarly, for all $\theta_i < \theta_i^*$, a farmer with endowment $\{\theta_i, \eta_i^*\}$ defaults

[29] We make assumptions to ensure that output is nonnegative. So $(\eta_i, \theta_i) \in P = [\eta^L, \eta^H] \times [\theta^L, \theta^H]$ with $\eta^L f_N + \theta^L \equiv q^L > 0$, and ϵ_i is drawn from a continuous density $g(\epsilon)$ with positive support on $[\epsilon^L, \epsilon^H]$ with $\epsilon^L \geq -q^L$. The expected value of ϵ is *0.*

with positive probability, and chooses to borrow because of the limited liability constraint.[30] In Figure 4, we show the set of (η, θ) such that a farmer would choose $B_i = 1$. The solid curve labeled *B* partitions the space such that farmers with endowments to the southeast of *B* choose to borrow.[31]

The lender will choose to make the loan if expected profits are positive. So the lender is willing to lend to *i* if and only if

(20) $$(1+r)\big(1-G(\overline{\epsilon})\big) + (\eta_i f_B + \theta_i)G(\overline{\epsilon}) + \int_{\epsilon}^{\overline{\epsilon}} \epsilon g(\epsilon) d\epsilon \geq (1+\rho).$$

$L^{rej} = \left\{(\eta, \theta) | (1+r)\big(1-G(\overline{\epsilon})\big) + (\eta_i f_B + \theta_i)G(\overline{\epsilon}) + \int_{\epsilon}^{\overline{\epsilon}} \epsilon g(\epsilon) d\epsilon < (1+\rho)\right\}$ defines the set of borrower characteristics such that the lender would not be willing to lend to the borrower with those characteristics. Equation (20) is satisfied for farmer *i* with endowment $\{\theta_i^*, \eta_i^*\}$, because $r > \rho$. In Figure 4, the dashed curve labeled L partitions the space such that the lender is willing to make a loan to farmers with endowments to the northeast of *L*; the set L^{rej} is the area to the southwest of *L*.

We can now consider the consequences of self-selection versus lender screening for the observed distribution of net revenue. In the no-loan villages, where grants were given to a random sample of the population, we have

(21) $$\begin{gathered} q_i^{NG} = \eta_i f_N + \theta_i + \epsilon_i \\ q_j^{Grant} = \eta_j f_B + \theta_j + \epsilon_j \end{gathered}$$

for farmers *i* and *j* randomly selected into the no grant and grant treatment groups, respectively. Let $h(\eta, \theta)$ denote the joint density of η and θ in the rural population of our study area, then given our randomization, the distributions of q_j^{Grant} and q_i^{NG} simply reflect draws from the full density *h().* The left panel of Figure 5 depicts these distributions; as can be anticipated from our preceding results, the distribution of q_j^{Grant} lies to the right of that of q_i^{NG} over virtually the whole range.

---

[30] From (16) $\big(\eta_i^* f_B - (1+r)\big) = \eta_i^* f_N$ and $\epsilon^L = (1+r) - \eta_i^* f_B - \theta_i^*$. For $\theta < \theta_i^*$, $G\big((1+r) - \eta_i^* f_B - \theta\big) > 0$. Therefore, $Emax(\eta_i^* f_B + \theta + \epsilon_i - (1+r), 0) > \eta_i^* f_N + \theta$.

[31] B is upward-sloping below $\{\theta_i^*, \eta_i^*\}$ by reasoning analogous to that in the preceding note. If $\{\tilde{\theta}, \tilde{\eta}\}$ is a point on B with a positive probability of default: then for all $\theta < \tilde{\theta}$, $G\left((1+r) - \tilde{\eta} f_B - \tilde{\theta}\right) < G\big((1+r) - \tilde{\eta} f\_B - \theta\big)$ and the farmer endowed with $\{\theta, \tilde{\eta}\}$ strictly prefers to borrow.

In the loan villages, grants were given to a random sample of non-borrowers. Suppose that selection into borrowing is being driven by the simultaneous operation of both borrower side self-selection and by lender-side screening; that is, that the selection is driven jointly by equations (18) and (20). In this case, the joint density of η and θ in the population of non-borrowers is the truncated probability distribution

$$h^{rej}(\eta,\theta)=\frac{h(\eta,\theta)}{prob\big((\eta,\theta)\in\{L^{rej}\cup B^{rej}\}\big)} \tag{22}$$

with support $(\eta,\theta)\in\{L^{rej}\cup B^{rej}\}$. As can be seen in Figure 4, the endowments of the approximately 80 percent of the population who do not borrow differ from the overall population in two ways. First, because of lender screening, the distribution of endowments in the selected population of non-borrowers has greater weight on low values of average productivity θ. Second, because of borrower self-selection, the selected population contains a higher proportion of farmers with low marginal productivity. Put differently, self-selection implies that among non-borrowers with high average productivity, a disproportionately large share will have low marginal productivity. And lender screening implies that among non-borrowers with high marginal productivity, a disproportionate share will have low average productivity.

The right panel of Figure 5 depicts the distributions of $q_j^{Grant}$and q_i^{NG} for the randomly chosen grant recipients and non-grant recipients among the population of non-borrowers in the loan villages. There are two distinctive feature of this graph. First is the presence of a significant fraction of non-borrowers with relatively high net revenue (> $500), but approximately zero marginal return from the grant. This feature corresponds to the mechanism of self-selection. Second is the presence of a significant fraction of non-borrowers with high marginal productivity but low average productivity (measured by net revenue). This feature corresponds to the mechanism of lender screening. We infer that the realizations of q_j^{Grant} and $q_i^{NG}$are determined jointly by equations (18) and (7), so that non-borrower endowments are drawn from $h^{rej}(\eta,\theta)$. Both self-selection and lender screening are occurring in this credit market.

Correlations between observable characteristics of borrowers and non-borrowers and the return to grants are also informative of the nature of the selection process. We saw in Table 1 that there are a number of observable characteristics that are strongly (positively) correlated with loan take-up. Consider any such attribute, Z, that we *a priori* expect to be correlated with average productivity, θ. For example, baseline net revenue would be one such attribute. In Table 5, we report the results of estimating

$$
\begin{aligned}
(23) \qquad Y_{ijt} = {} & \alpha + \beta_1 grant_i \cdot I\{t = 2011\} + \beta_2 grant_i \cdot I\{t = 2011\} \cdot loan_j \\
& + \beta_3 grant_i \cdot I\{t = 2012\} + \beta_4 grant_i \cdot I\{t = 2012\} \cdot loan_j \\
& + \gamma_1 grant_i \cdot Z_{ijt} \cdot I\{t = 2011\} + \gamma_2 grant_i \cdot Z_{ijt} \cdot I\{t = 2012\} \\
& + \gamma_3 Z_{ijt} \cdot I\{t = 2011\} + \gamma_4 Z_{ijt} \cdot I\{t = 2012\} \\
& + \delta_1 grant_i \cdot Z_{ijt} \cdot I\{t = 2011\} \cdot loan_j + \beta_5 I\{t = 2012\} \\
& + \beta_6 I\{t = 2012\} \cdot loan_j + X_{ijt}\pi + \lambda_j + \epsilon_{ijt}
\end{aligned}
$$

where we have augmented specification (2) with an additional interaction of Grant * Z * Loan village * Year 1. This additional interaction permits us to examine whether the correlation between Z and the marginal return to the grant is different for the general population (γ_1) than for a selected population of non-borrowers ($\gamma_1 + \delta_1$). This helps illuminate whether the underlying mechanism is self-selection driven, lender driven, or both. The higher average productivity, θ, associated with the higher value of Z reduces the likelihood that the farmer has been screened out of borrowing by the lender, so non-borrowers with higher values of Z are more likely to have self-selected out of borrowing because they have low marginal productivity. Hence, among the population of non-borrowers in loan villages, higher values of Z are associated with lower values of η, relative to the association in the population in general.

Column (1) of Table 5 examines the association between baseline net revenue and the marginal return to the grant in the overall population and in the selected sample of non-borrowers. In accord with our model, households in loan villages have a significantly more negative correlation between baseline net revenue and the return to a grant than households in the overall population (-\$0.17, se=0.07). The inclusion of the additional interaction terms erodes the primary selection effect on Grant * Loan village * year 1. In the context of our model, both lender screening and self-selection are required to generate this pattern. Screening by the lender generates the positive correlation between baseline net revenue and loan take-up, and households with low returns to additional liquidity self-select out of borrowing.

In columns (2)-(4), we report the estimates of (10) for three additional characteristics of households that are positively associated with loan take-up and plausibly with average productivity θ. In column (2), we find no significant difference in the correlation between baseline livestock and the return to the grant in the overall population and in the loan villages (-\$0.017, se=0.013), so this measure provides no evidence in support of the hypothesis that both dimensions of selection are operating. In column (3), we examine baseline harvest period expenditure on food (choosing the harvest period to minimize the likelihood of strong nutrition-productivity effects). The association between baseline food expenditure and the return to the grant in loan villages is much lower than the same correlation in the overall population villages (-\$14.53, se=6.15). Similarly, in column (4), we use baseline non-food consumption per capita as

Z, hypothesizing that this quantity may be strongly positively correlated with a household's permanent income (and hence with θ) and less strongly correlated with the marginal product of additional agricultural investment. Again, we find a much lower association between non-food consumption and the effect of the grant on net revenue in loan villages than in the overall population (-\$1.53, se=0.60).

5 Impact of the loans

We also show our estimates of the intent-to-treat (ITT) effects of being offered an agricultural loan on the same set of outcomes already discussed in section 3. In this analysis, we exclude all grant recipients, from both loan and ineligible villages. Panel B of Tables 2 and 3 show the results of the loan intent-to-treat analysis. We use the following specification:

$$Y_{ijt} = \alpha + \beta_1 loan_j \cdot I\{t = 2011\} + \beta_2 loan_j \cdot I\{t = 2012\} + X_{ijt}\pi + \epsilon_{ijt} \tag{24}$$

where (X) includes the baseline value of the dependent variable y_0, *cercle* fixed effects, and the village stratification controls described in section 2.3 and listed in the notes of the Table 2. The specification uses probability weights to account for the sampling strategy, which depends on take-up in the loan villages.

Panel B of Tables 2 and 3 show the ITT estimates. In Table 2, we observe an increase in input expenditures on family labor days (8.6, se=4.8) and in fertilizer expenses (\$9.23, se=4.79); total input expenses rose by \$19.87 (se=8.87) in villages offered loans. Land cultivated also increases but is not statistical significant at conventional levels (0.082 ha, se=0.057). The value of the harvest also increases by \$34 (se=19), but we do not measure a statistically significant increase in net revenues (\$19, se=16). Year 1 Treatment on the Treated (ToT) estimates for the 18% of the population who take up loans in the treatment population (divided by average loan size/100) are reported in row 3 of Panel B. The per \$100 dollar estimated effects of grants for would-be borrowers in the control villages are reported in row 7 of Panel A. Rows 4 and 5 of Panel B show that it is not possible to reject the hypothesis that the per \$100 dollar effects of grants and loans are the same for any of the agricultural outcomes in Table 2, including net revenue. The standard errors are calculated using a bootstrap routine: the difference in the impact of the grant and loan is estimated for 1,000 draws of households (with replacement), with probability weights for households calculated in each bootstrap sample for the loan impact estimation. Taken as a whole, the grants and loans are having similar effects on agricultural outcomes.

Panel B of Table 3 shows a reduction in medical expenses (-\$5.03, se=1.64) in Column (9). We do not detect an impact on the other outcomes, including food and non-food consumption, whether the household has a small business, nor educational expenses.[32] The comparison of ToT for loans and the per $100 dollar impact of the grants is provided in rows 4 and 5 of Panel B. The only outcome which differs significantly is whether the household owns any livestock: the grant has a larger impact on livestock ownership than the loan. This is intuitive since the loan has to be repaid and households would be less likely to use a loan to acquire buffer stock savings.

These results on impact of loans stand in stark contrast both to the recent literature on the impact of entrepreneurially-focused credit (see Angelucci, Karlan, and Zinman 2015; Attanasio et al. 2015; Augsburg et al. 2015; Banerjee, Duflo, et al. 2015; Crépon et al. 2015; Karlan and Zinman 2011; Tarozzi, Desai, and Johnson 2015, and an overview in Banerjee, Karlan, and Zinman 2015), and an earlier agricultural lending literature that documented consistent institutional failures, typically with high default rates (Adams, Graham, and Von Pischke 1984; Adams 1971). The institutional results are also promising: the perfect repayment, and the retention to the following year (65%) is on par with typical client retention rates for sustainable, entrepreneurially-focused microcredit operations.

6 Conclusion

Capital constraints are binding for at least some farmers in Southern Mali, and we find that agricultural lending with balloon payments (i.e., with cash flows matched to those of the intended productive activity) can increase investments in agriculture. This is an important policy lesson since the majority of microcredit has focused on small enterprise lending, and the typical microcredit loan contract – where clients must start repayment after a few weeks – is simply ill-suited for agriculture. Field et. al. (2013) find similar results merely from delaying the onset of high frequency repayment, within the context of microenterprise. In Mali, for example, Soro Yiriwaso is among very few microcredit organizations with a product specially designed for agriculture, despite the fact that the vast majority of households in rural Mali depend on agriculture for a sizeable part of their livelihood.

These results are also important for policy, for example the targeting of social programs. Cash transfer programs are often means-tested and recent work suggests that both community targeting, where community members rank-order households to identify the poor, and ordeal

[32] Appendix Table 4 further shows no detectable effect on business profits, women's decision-making power within the household, women's involvement in community decisions, nor on women's social capital. This is similar to the existing evaluations of microcredit (Attanasio et al. 2015; Augsburg et al. 2015; Banerjee, Duflo, et al. 2015; Crépon et al. 2015; except Angelucci, Karlan, and Zinman 2015). SY did not have any explicit component of the program emphasizing women's empowerment.

mechanisms can be an effective way of generating screening on wealth/income in developing countries (Alatas et al. 2012; Alatas et al. 2013). Price is the screening mechanism we look at here with agricultural loans. In a different agricultural setting, Jack (2013) finds that a willingness to accept mechanism can induce self-selection among landholders in Malawi, leading to improved project success for tree planting. We find that the lending process is a mechanism that generates positive selection so farmers who benefit the most from relaxing capital constraints are more likely to choose to borrow.

We find that the returns to capital in cultivation are heterogeneous and that higher marginal-return farmers self-select into borrowing more so than low marginal-return farmers. This has important implications for models of credit markets. In particular, our results provide rigorous empirical evidence for optimal selection into contracts, which is embedded in models like Evans and Jovanovic (1989), Buera (2009) and Moll (2013) but which has lacked clear empirical evidence. Our results also highlight the need to incorporate heterogeneity of returns in such models, as recognized by Banerjee et al (2015) and Kaboski and Townsend (2011).

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Table 1: Comparison of baseline characteristics of clients vs. non-clients in loan treatment villages

	Tookup	Did Not Takeup	Difference (from regression with village fixed effects)	
	(1)	(2)	(3)	
A. Agriculture, Livestock & Business				
Household				
Land size (ha)	2.64	2.21	0.59	***
	(2.71)	(2.64)	(0.13)	
Total input expenses	205.82	151.87	46.37	***
	(300.42)	(285.75)	(14.22)	
Value of output	709.04	596.10	132.60	***
	(752.17)	(827.66)	(39.79)	
Net revenue	503.22	444.23	86.23	***
	(555.12)	(642.11)	(30.84)	
Total value of livestock	1871.22	1294.65	504.65	***
	(3037.90)	(2549.92)	(135.22)	
B. Household Demographics				
Nb of people in small HH	8.66	7.29	1.63	***
	(3.67)	(3.51)	(0.18)	
C. Primary Female Respondent				
Age	36.58	34.92	2.46	***
	(10.29)	(11.68)	(0.58)	
Married (0/1)	0.98	0.92	0.07	***
	(0.13)	(0.27)	(0.01)	
Not first wife (0/1)	0.33	0.19	0.13	***
	(0.47)	(0.39)	(0.02)	
Number of children	4.86	4.34	0.70	***
	(2.34)	(2.40)	(0.12)	
Risk aversion: safe lottery	0.46	0.50	-0.03	
	(0.50)	(0.50)	(0.02)	
Index of intra-household decision making power	0.08	-0.03	0.14	***
	(0.97)	(1.05)	(0.05)	
Index of community action	0.28	-0.03	0.26	***
	(1.03)	(0.99)	(0.05)	
Social integration index	0.23	-0.09	0.18	***
	(1.04)	(0.98)	(0.05)	
D. Consumption				
Value of food consumed per adult equiv (past 7 days)	3.93	3.83	0.40	*
	(4.69)	(4.82)	(0.24)	
Non-food expenses by HH (past 30 days)	48.09	39.77	10.04	***
	(45.38)	(38.44)	(2.03)	

Notes

1 The household decision-making index includes questions on how much influence she has on decisions in the following domains: food for the household, children's schooling expenses, their own health, her own travel within the village, and economic activities such as fertilizer purchases and raw materials for small business activities. The community action index includes questions on: how frequently she speaks with different village leaders, and different types of participation in village meetings and activities. The social capital index includes questions about 7 other randomly selected community members from our sample and whether the respondent knows the person, are in the same organization, would engage in informal risk sharing and transfers with the person, and topics of their discussions (if any). All three of these variables are indices, normalized by the no-grant households in loan-unavailable villages.

2 Clients are defined by households who took out a loan in the 2010 agricultural season.

Table 2: Agriculture

	Land cultivated (ha)	Family labor (days)	Hired labor (days)	Fertilizer expenses	Other chemicals expenses	Total input expenses	Value output	Net Revenue
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Grant recipients, Size of grant: $140								
Grant * year 1	0.174***	6.2	2.7***	11.61***	9.06***	28.02***	67.03***	40.33***
	(0.065)	(4.3)	(0.8)	(4.32)	(2.19)	(8.23)	(19.08)	(15.36)
Grant * loan village * year 1	-0.147	-8.0	1.4	-9.19	-5.89*	-8.30	-46.78*	-43.60*
	(0.094)	(6.5)	(1.5)	(6.45)	(3.01)	(12.14)	(28.17)	(22.22)
Grant * year 2	0.071	-5.3	1.1	-3.16	0.85	1.94	49.73**	46.22***
	(0.077)	(4.0)	(0.8)	(6.03)	(2.77)	(10.32)	(22.34)	(16.90)
Grant * loan village * year 2	0.089	11.0*	1.6	12.42	8.49**	30.19*	-6.60	-33.27
	(0.111)	(6.1)	(1.2)	(9.98)	(4.26)	(17.21)	(32.17)	(23.16)
Grant + Grant * loan village = 0 (year 1)	0.688	0.707	0.001	0.615	0.125	0.029	0.330	0.839
Grant + Grant * loan village = 0 (year 2)	0.047	0.218	0.003	0.245	0.004	0.021	0.064	0.414
Per $100 impact for loan takers, year 1	0.520*	25.94	-1.778	33.00*	22.29***	42.33	173.57**	145.96**
	(0.286)	(19.60)	(4.257)	(19.53)	(9.30)	(36.86)	(85.50)	(67.75)
N	10643	10642	10640	10639	10640	10641	10639	10533
Mean of control (year 1)	2.066	134.16	17.03	71.52	46.41	186.83	501.91	316.46
SD (year 1)	2.221	128.02	23.24	144.78	65.09	251.75	595.30	428.12
Panel B. Loan ITT, Average loan size: $113								
Loan village - year 1	0.082	8.61*	-0.88	9.23*	4.14	19.87**	34.49*	18.97
	(0.057)	(4.82)	(1.01)	(4.79)	(2.63)	(8.67)	(19.52)	(16.08)
Loan Village - year 2	-0.002	-1.16	-1.08	1.40	-0.54	6.48	17.18	14.53
	(0.070)	(4.72)	(1.06)	(6.03)	(3.08)	(11.40)	(23.51)	(16.04)
Per $100 impact, TOT, year 1	0.347	36.29*	-3.702	38.90*	17.45	83.73**	145.36*	79.94
	(0.238)	(20.32)	(4.244)	(20.19)	(11.08)	(36.53)	(82.27)	(67.74)
Diff in per $100 impact: Grants - Loans	0.174	-10.35	1.92	-5.91	4.84	-41.40	28.21	66.02
SE from Bootstrap on Difference	(0.315)	(18.86)	(4.15)	(21.76)	(10.20)	(36.71)	(85.81)	(66.65)
N	8768	8770	8769	8766	8766	8768	8767	8687
Mean of control (year 1)	2.066	134.16	17.07	71.52	46.57	186.24	500.49	315.44
SD (year 1)	2.221	128.02	23.35	144.78	65.50	250.17	591.41	425.38

Notes

1 Rows showing Grant + Grant * loan village = 0 (year 1) shows the p value of the test of whether the total effect of grants in loan villages is statistically different from zero.

2 Total input expenses includes fertilizer, manure, herbicide, insecticide, farming equipment and hired labor but excludes the value of family labor. Net revenue is revenue net of most, but not all, expenses. Specifically, the formula includes value of harvest (whether sold, stored or consumed) minus fertilizer, manure, herbicide, insecticide, hired labor, cart and traction animal expenses (rental or maintenance), and seed expenses (although valuing last year's seeds at zero). Thus this does not substract value of own labor, value of family (i.e., any unpaid) labor, and the implicit rental value of land used.

3 Additional controls include in Panel A include: the baseline value of the dependent variable, village fixed effects, round x village type (loan-village vs no-loan-village) fixed effects, the baseline value of the dependent variable interacted with round x village type effects, an indicator for whether the baseline value is missing, an indicator for the HH being administered the input survey in 2011, and stratification controls (whether the household was part of an extended family; was polygamous; an index of the household's agricultural assets and other assets; per capita food consumption; and for the primary female respondent her baseline: land size, fertilizer use, and whether she had access to a plough). Village-level stratification controls are not included since there are village fixed effects.

4 Additional controls in Panel B include: cercle fixed effects; the baseline value of the dependent variable, along with a dummy when missing, interacted with year of survey indicators; and village-level stratification controls: population size, distance to nearest road, distance to nearest paved road, whether the community is all bambara (dominant ethnic group) distance to the nearest market, percentage of households with a plough, percentage of women with access to plough in village, percentage of women in village using fertilizer and the fraction of children enrolled in school. The specification uses probability weights to reflect sampling design. All grant-recipients households are removed from the analysis in both loan and no-loan villages.

5 Standard errors are in paranetheses and clustered at the village level in all specifications.

6 Mean of control in Panel A is the mean of the dependent variable in the column heading among households that received no grants in no-loan villages. In panel B, it is households in no-loan villages.

7 In Panel A, the per dollar return for loan takers is calculated as: (Grant * Yr1-.79*(Grant * Yr1+Grant * loan village * Yr1))/(.21*140) where .21 is the loan take up rate and 140 is the value of the grant. In Panel B, the per dollar return, TOT, year 1 is analogously: Loan village - year 1 / (.21*113) since the average value of the loan was $113. The standard error on the difference in per dollar impact is the result of a bootstrap of 1000 draws comparing the per dollar impact of the grant vs the loan using re-sampling of households. Probably weights were calculated in each bootstrap sample and used in the estimate of the loan impact.

Table 3: Other Outcomes

	Own any livestock	Total value of livestock	HH has a business	Food consumption EQ (past 7 days)	Monthly non-food exp	HH has any financial savings	Primary is member of ROSCA	Educ expenses	Medical expenses
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Grant recipients vs control									
Grant * year 1	0.114***	163.03**	0.038**	0.38***	2.69**	0.024	0.017	2.22	-2.53
	(0.014)	(70.32)	(0.015)	(0.11)	(1.37)	(0.016)	(0.015)	(3.04)	(1.85)
Grant * loan village * year 1	-0.039*	-27.64	-0.003	-0.12	2.19	0.033	-0.004	-0.17	4.90*
	(0.022)	(101.72)	(0.023)	(0.17)	(2.01)	(0.028)	(0.023)	(5.36)	(2.51)
Grant * year 2	0.092***	180.44*	0.030**	0.05	3.72*	0.035*	0.039**	0.42	-0.76
	(0.015)	(101.17)	(0.013)	(0.17)	(2.09)	(0.019)	(0.019)	(3.64)	(1.80)
Grant * loan village * year 2	0.006	-159.14	-0.023	0.31	-1.09	0.039	-0.011	1.89	1.78
	(0.023)	(136.89)	(0.020)	(0.24)	(2.76)	(0.026)	(0.025)	(5.14)	(2.75)
Grant + Grant * loan village = 0 (year 1)	0.000	0.067	0.034	0.043	0.001	0.014	0.451	0.643	0.163
Grant + Grant * loan village = 0 (year 2)	0.000	0.818	0.667	0.036	0.144	0.000	0.105	0.525	0.625
Per $100 impact for loan takers, year 1	0.186***	190.7	0.035	0.596	-3.96	-0.073	0.023	2.04	-14.97*
	(0.066)	(310.2)	(0.068)	(0.515)	(6.10)	(0.083)	(0.069)	(15.75)	(7.76)
N	10462	10358	10464	10367	10063	10347	10347	7194	10370
Mean of control (year 1)	0.777	1213	0.833	3.17	43.83	0.635	0.263	69.87	33.66
SD (year 1)	0.417	2049	0.373	3.17	37.31	0.482	0.440	81.20	45.92
Panel B. Loan villages vs control									
Loan village - year 1	0.009	112.9	-0.008	0.10	-0.19	0.016	-0.012	2.70	-5.03***
	(0.014)	(74.9)	(0.023)	(0.13)	(2.10)	(0.024)	(0.024)	(4.01)	(1.64)
Loan Village - year 2	-0.011	68.93	0.002	0.07	-0.60	0.003	-0.019	1.86	-1.36
	(0.017)	(97.64)	(0.015)	(0.17)	(2.50)	(0.027)	(0.026)	(3.44)	(1.81)
Per $100 impact, TOT, year 1	0.036	475.9	-0.033	0.433	-0.8	0.065	-0.052	11.37	-21.18***
	(0.061)	(315.7)	(0.099)	(0.535)	(8.8)	(0.101)	(0.100)	(16.88)	(6.899)
Diff in per dollar impact	0.150***	-285.1	0.068	0.163	-3.2	-0.138	0.075	-9.33	6.22
SE from Bootstrap on difference	(0.059)	(377.7)	(0.048)	(0.528)	(6.4)	(0.079)	(0.692)	(15.56)	(7.18)
N	8634	8558	8634	8564	8294	8533	8533	6021	8550
Mean of control (year 1)	0.777	1219	0.833	3.17	43.99	0.635	0.263	69.87	33.46
SD (year 1)	0.417	2071	0.373	3.17	37.67	0.482	0.440	81.20	45.44

Notes

1 Rows showing Grant + Grant * loan village = 0 (year 1) shows the p value of the test of whether the total effect of grants in loan villages is statistically different from zero.

2 See the notes of Table 2 for details on specifications.

Table 4: Are Returns Predicted by Baseline Characteristics?

	Net Revenue			
	(1)	(2)	(3)	(4)
Grant * Year 1	40.33 ***	66.89 *	70.31 *	76.55 *
	(15.36)	(39.80)	(40.94)	(42.01)
Grant * Loan village * Year 1	-43.60 *	-38.75 *	-38.66 *	-40.77 *
	(22.22)	(21.97)	(21.89)	(21.98)
Grant * Year 2	46.22 ***	42.88	45.37	28.58
	(16.90)	(41.58)	(41.76)	(45.69)
Grant * Loan village * Year 2	-33.27	-31.97	-32.75	-33.29
	(23.16)	(23.31)	(23.39)	(23.92)
Grant * Baseline social index * Year 1			-9.06	-8.68
			(12.15)	(12.11)
Grant * Baseline social index * Year 2			5.50	6.27
			(13.30)	(13.36)
Grant * Baseline net revenue * Year 1		0.03	0.03	0.03
		(0.05)	(0.05)	(0.05)
Grant * Baseline net revenue * Year 2		-0.04	-0.04	-0.04
		(0.03)	(0.03)	(0.03)
Grant * Baseline land * Year 1		-16.76 *	-16.66 *	-16.74 *
		(9.89)	(9.86)	(9.87)
Grant * Baseline land * Year 2		3.16	3.52	3.53
		(9.81)	(9.81)	(9.76)
Grant * Large HH at baseline* Year 1		78.51 *	77.41 *	76.58 *
		(43.65)	(43.41)	(43.47)
Grant * Large HH at baseline * Year 2		47.02	45.94	43.59
		(43.54)	(43.20)	(43.10)
Grant * Risk averse at baseline* Year 1				-9.59
				(19.89)
Grant * Risk averse at baseline * Year 2				31.80
				(26.76)
Grant + Grant * loan village = 0 (Year 1)	0.839	0.471	0.439	0.397
Grant + Grant * loan village = 0 (Year 2)	0.414	0.796	0.765	0.914
N	10533	10531	10528	10506
Additional HH structure controls interacted with grant & year	No	Yes	Yes	Yes
HH decision-making/community action interacted with grant & year	No	No	Yes	Yes
Mean of Baseline profits		396.14		
SD of Baseline profits		481.35		
Mean of Baseline land		2.11		
SD of Baseline land		2.53		

Notes
1 Rows showing Grant + Grant * loan village = 0 (Year 1) shows the p value of the test of whether the total effect of grants in loan villages is statistically different from zero.
2 See the notes 3 and 5 of Table 2 for details on specification.
3 Risk averse is an indicator for the household choosing the safe lottery, which about half the sample seleted. Large household is 6 or more adults in the household.
4 Other household structure controls include: an indicator for the presence of an extended family and the number of children in the household.
5 The value of livestock interacted with grant receipt in year 1 and 2 is also included in columns 2-4.

Table 5: Peer and Lender Selection

	Net Revenue			
	(1)	(2)	(3)	(4)
Grant * Year 1	19.88	32.04 *	28.54 *	16.33
	(22.78)	(17.09)	(17.09)	(19.61)
Grant * Loan village * Year 1	25.16	-20.34	9.28	18.58
	(28.91)	(24.37)	(26.21)	(30.27)
Grant * Year 2	48.72 ***	44.61 ***	33.33 *	22.26
	(17.09)	(17.08)	(18.54)	(20.43)
Grant * Loan village * Year 2	-14.17	-31.05	-0.20	5.79
	(24.40)	(24.71)	(25.48)	(30.08)
Grant * Baseline net revenue * Year 1	0.05			
	(0.06)			
Grant * Baseline net revenue * Loan village * Year 1	-0.17 **			
	(0.07)			
Grant * Baseline livestock * Year 1		0.006		
		(0.008)		
Grant * Baseline livestock * Loan village * Year 1		-0.017		
		(0.013)		
Grant * Baseline food consumption * Year 1			3.41	
			(4.49)	
Grant * Baseline food consumption * Loan village * Year 1			-14.53 **	
			(6.15)	
Grant * Baseline non-food expenditure * Year 1				0.58
				(0.39)
Grant * Baseline non-food exp * Loan village * Year 1				-1.53 **
				(0.60)
N	10533	10531	10497	10207
Mean of Baseline Variable	396.14	1424.46	3.54	41.10
SD of Baseline Variable	481.35	2795.29	4.59	41.29

Notes

1 Rows showing Grant + Grant * loan village = 0 (Year 1) shows the p value of the test of whether the total effect of grants in loan villages is statistically different from zero.

2 See the notes of Table 2 for details on specification.

Figure 1: Experimental Design

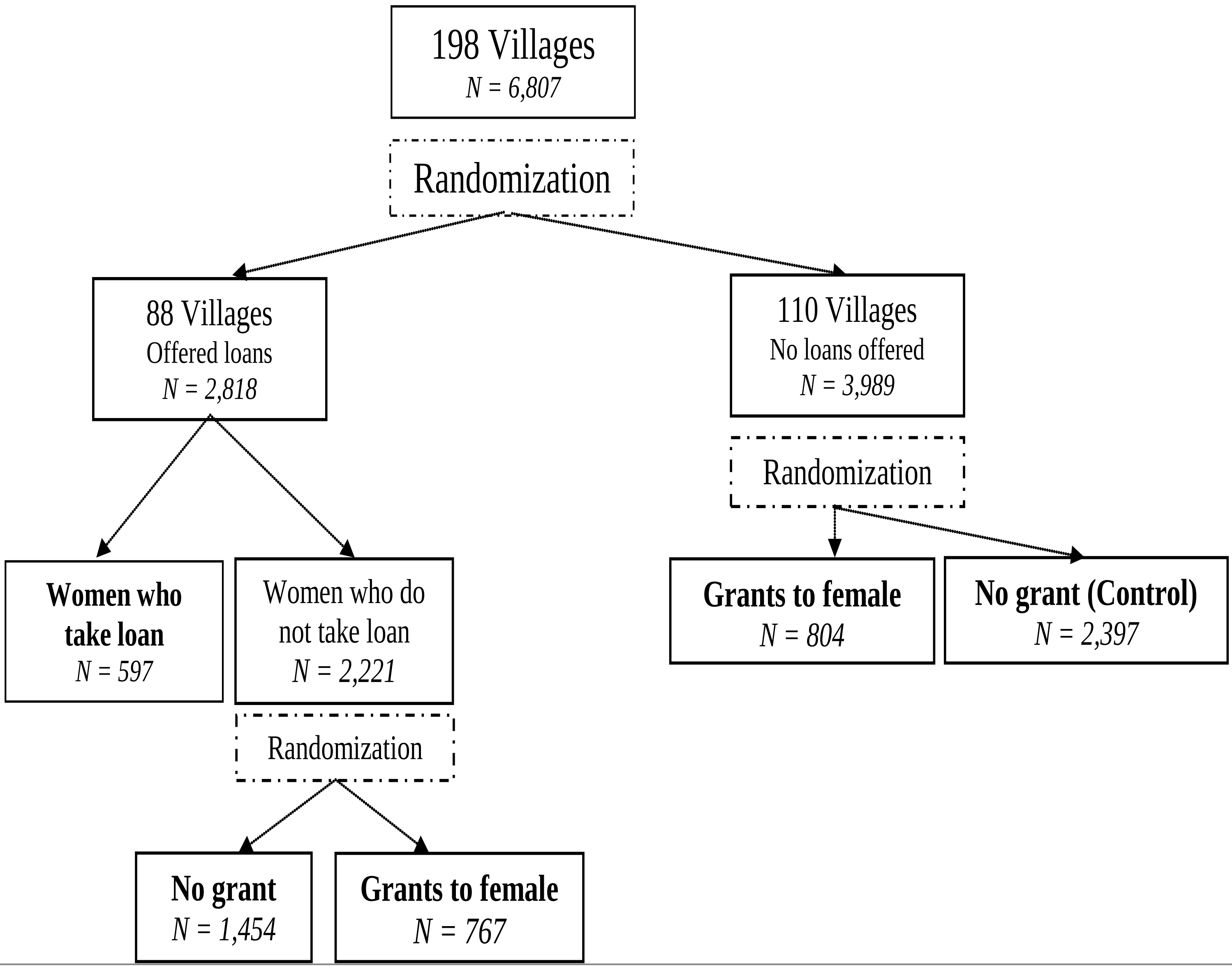
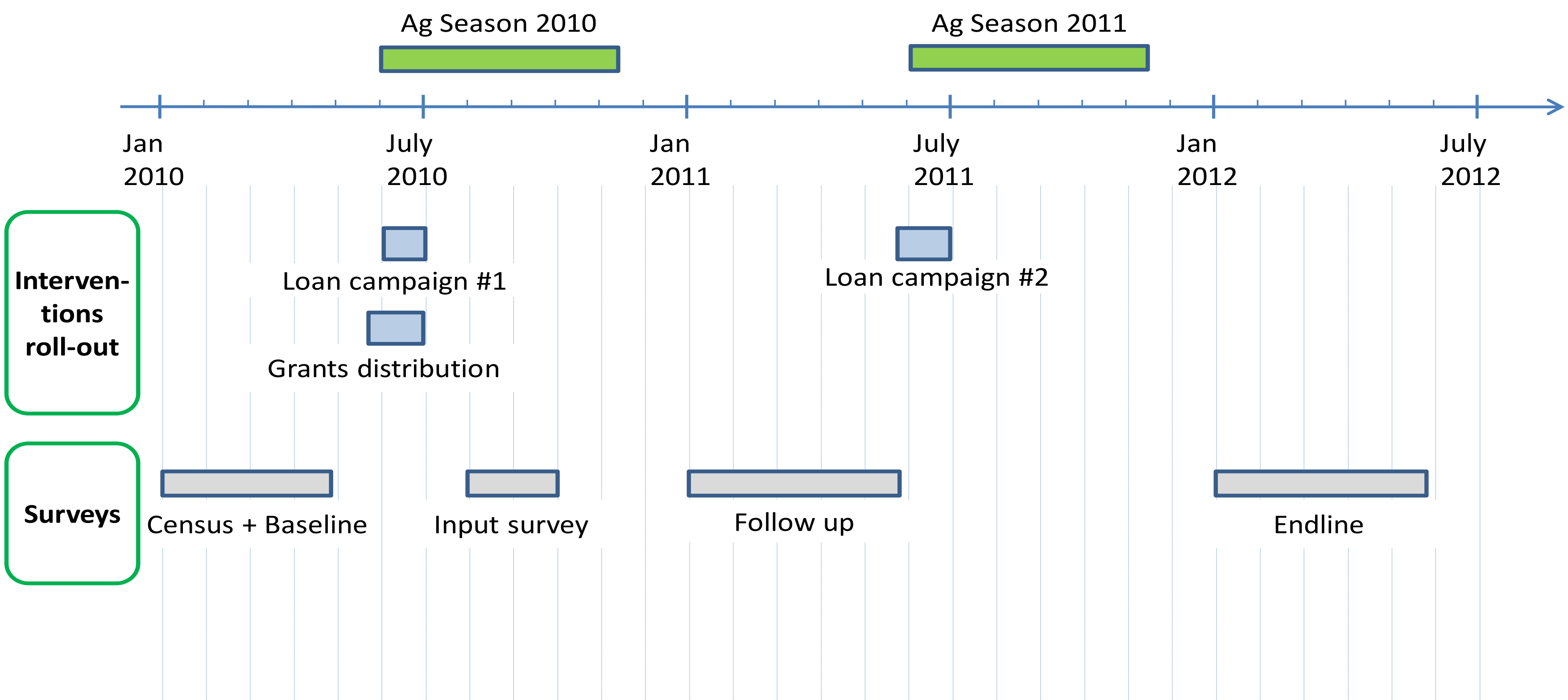


Figure 2: Timeline of the study



Notes

1 Grant distribution, across all villages, spans a longer time than loan distribution since grants distribution started in no-loan villages, followed by loan disbursement in loan villages, then grants in loan and some no-loan villages.

Figure 3: Baseline characteristics of borrwers vs. non borrowers in loan treatment villages

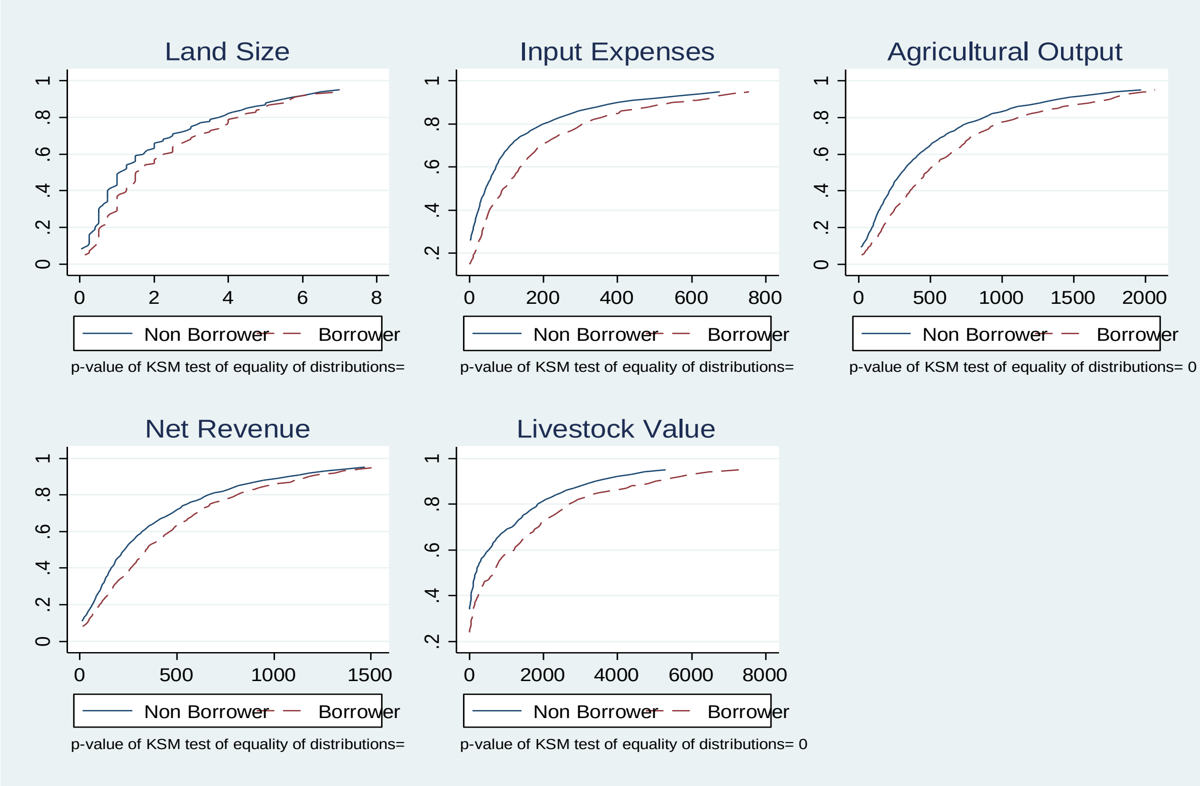


Figure 4: Selection into borrowing

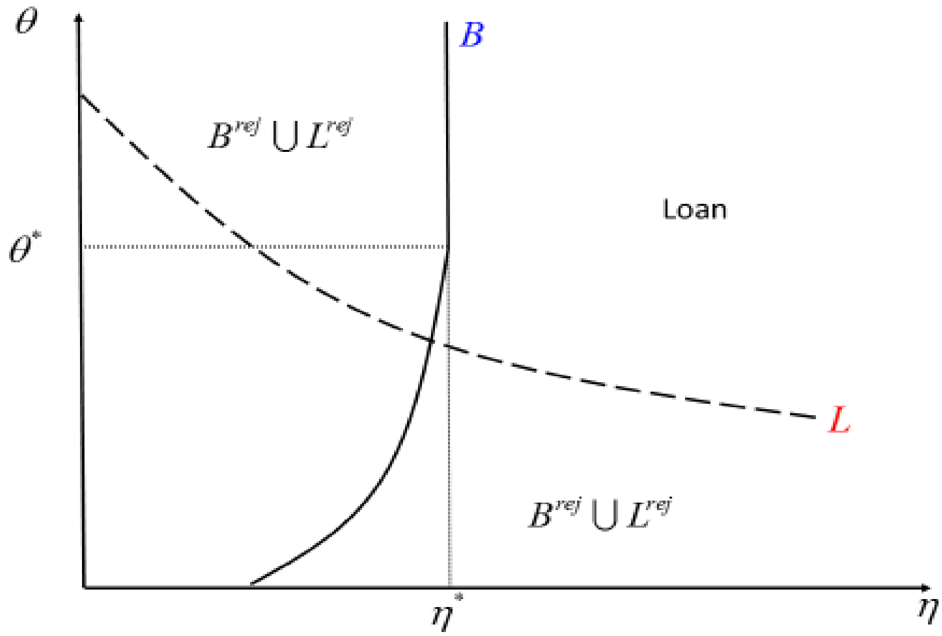
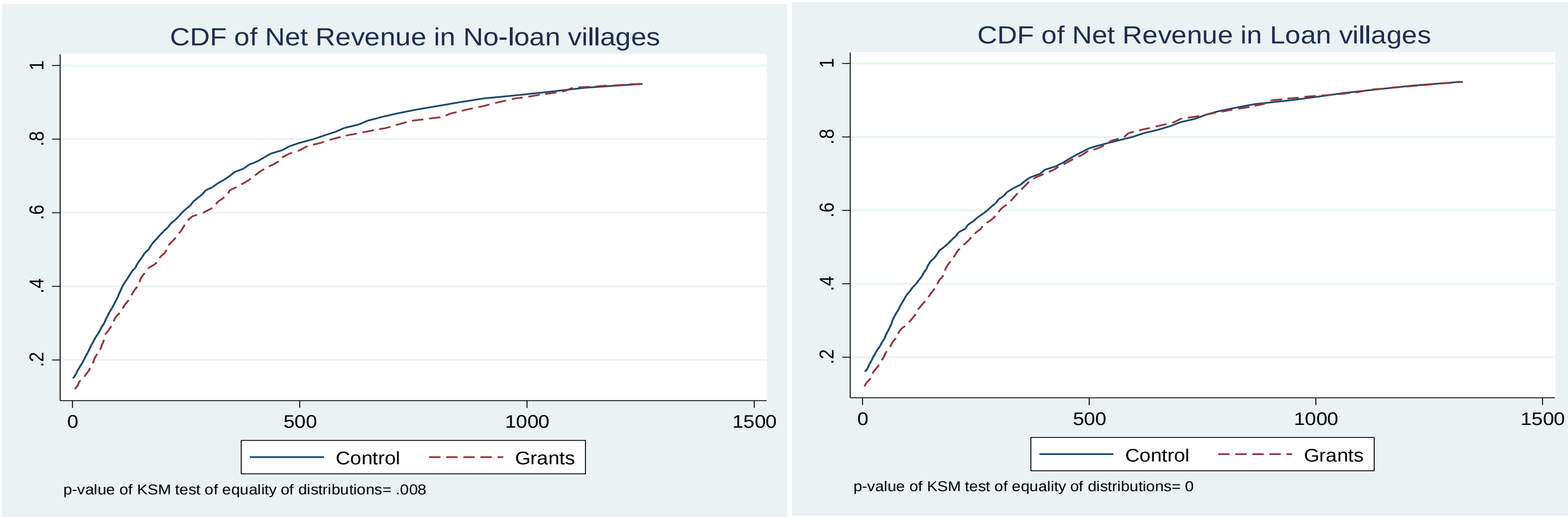


Figure 5: CDF of Net Revenue



Appendix Table 1: Balance check

	Loan vs no-loan villages				Grants vs no-grants in no-loan villages				Grants vs no-grants in loan villages			
	Mean of control group	Difference between T and C	p-value	N	Mean of control group	Difference between T and C	p-value	N	Mean of control group	Difference between T and C	p-value	N
Household size	7.41	0.03	0.76	6,828	7.43	-0.06	0.62	3,151	7.37	-0.05	0.75	2,415
Land	1.92	0.22	0.03	6,856	1.92	0.04	0.68	3,174	2.09	-0.00	0.96	2,422
Days of family labor	139.41	-0.13	0.98	6,858	139.61	2.91	0.60	3,165	133.69	4.94	0.29	2,426
Days of hired labor	11	1.02	0.32	6,856	10	0.08	0.91	3,170	11	-0.56	0.45	2,419
Input expenses	126.95	17.68	0.13	6,856	127.49	9.80	0.25	3,172	138.55	0.55	0.95	2,422
Agricultural output	523.02	36.67	0.24	6,856	523.74	5.07	0.84	3,176	537.61	11.06	0.66	2,415
Livestock value	1,520.29	-120.52	0.28	6,924	1,515.83	2.63	0.98	3,199	1,389.71	-36.17	0.79	2,448
Has a Business	0.54	0.01	0.67	6,924	0.53	0.02	0.35	3,200	0.54	0.01	0.61	2,447
Monthly non-food expenses	39.48	0.18	0.92	6,568	39.75	-0.83	0.52	3,041	38.82	0.58	0.68	2,322
Male age	46.57	0.19	0.66	6,427	46.67	-0.35	0.50	2,947	45.93	0.53	0.31	2,272
Male is illiterate	0.77	-0.01	0.45	6,562	0.78	-0.00	0.82	3,015	0.77	0.01	0.58	2,321
F- test for joint significance				0.26				0.91				0.67

Appendix Table 2: Attrition

	Loan vs no-loan villages				Grants vs no-grants in no-loan villages				Grants vs no-grants in loan villages			
	Year 1		Year 2		Year 1		Year 2		Year 1		Year 2	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	0.0002	-0.0073	0.0075	0.0058	0.0062	0.0166	-0.0004	0.0123	-0.0001	0.0056	-0.0036	-0.0020
	(0.0032)	(0.0117)	(0.0056)	(0.0168)	(0.0051)	(0.0198)	(0.0046)	(0.0220)	(0.0043)	(0.0213)	(0.0059)	(0.0234)
Interaction of treatment and:				-0.0010				-0.0001				0.0022
Household size		-0.0002		(0.0010)		-0.0002		(0.0013)		-0.0014		(0.0022)
		(0.0009)		-0.0021		(0.0019)		0.0056		(0.0016)		0.0015
Land		0.0003		(0.0035)		0.0009		(0.0057)		0.0015		(0.0045)
		(0.0026)		-0.0005		(0.0046)		-0.0008		(0.0043)		-0.0016 *
Days of family labor†		0.0003		(0.0005)		-0.0008		(0.0005)		-0.0012		(0.0008)
		(0.0004)		-0.0024		(0.0006)		0.0050		(0.0009)		-0.0018
Input expenses*		0.0007		(0.0033)		0.0029		(0.0042)		0.0027		(0.0064)
		(0.0027)		0.0041 *		(0.0041)		-0.0017		(0.0065)		-0.0018
Ag Output*		0.0003		(0.0022)		-0.0007		(0.0016)		-0.0008		(0.0028)
		(0.0010)		-0.0002		(0.0016)		-0.0001		(0.0021)		-0.0001
Livestock value*		-0.0001		(0.0002)		0.0001		(0.0002)		-0.0001		(0.0003)
		(0.0001)		0.0227 ***		(0.0002)		-0.0009		(0.0002)		0.0238 *
Has a small business		0.0133 ***		(0.0066)		0.0080		(0.0099)		0.0129		(0.0125)
		(0.0050)		-0.0001		(0.0119)		0.0000		(0.0106)		0.0001
				(0.0001)				(0.0001)				(0.0002)
Monthly non-food exp		-0.0002 *				-0.0001				0.0003		
		(0.0001)		-0.0068		(0.0001)		-0.0058		(0.0002)		-0.0054
Household head is illiterate		0.0014		(0.0109)		0.0021		(0.0165)		-0.0031 *		(0.0205)
		(0.0095)				(0.0151)				(0.0195)		
Number of observations	6926	6022	6926	6022	3201	2779	3201	2779	2448	2118	2448	2118
Mean attrition control	0.013		0.013		0.012		0.012		0.015		0.015	
F- test for joint significance of coefficients of treatment and interaction terms		0.08		0.16		0.60		0.62		0.20		0.17

Notes
* Variables divided by 100 for ease of exposition.
† Variable divided by 10 for ease of exposition.

Appendix Table 3: Timing robustness (No-loan villages)

	Index			Land Size		
	(1)	(2)	(3)	(4)	(5)	(6)
Date (linear)	0.00094	0.00290		0.002	0.005	
	(0.004)	(0.008)		(0.011)	(0.023)	
Date squared		-0.00007			-0.00011	
		(0.000)			(0.001)	
1 if before June 1st			-0.045			-0.176
			(0.140)			(0.407)
Revisit to Village	-0.022	-0.007	-0.034	0.124	0.147	0.051
	(0.106)	(0.119)	(0.121)	(0.307)	(0.344)	(0.351)
Observations	787	787	787	774	774	774
Fixed effects	None	None	None	None	None	None

Notes

1 Index includes: land area, number of family labor days, number of hired labor days, an indicator for whether fertilizer was used, value of fertilizer expenses, value of other chemical expenses, value of al input expenses, value of harvest, and profits.

2 Sample includes only grant recipients in no-loan villages.

Appendix Table 4: Spillovers in No-loan Villages

	Land cultivated (ha)	Family labor (days)	Hired labor (days)	Fertilizer expenses	Other chemicals expenses	Total input expenses	Value output	Net Revenue
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intervention (No-loan) village	-0.171	10.6	4.0 ***	-0.19	-5.82	-9.61	-18.79	-15.61
	(0.148)	(7.9)	(1.4)	(6.88)	(4.48)	(16.04)	(45.05)	(29.65)
N	3654	3650	3652	3653	3648	3654	3652	3619
Mean of excluded group	2.1	135.4	16.9	71.5	46.4	186.8	504.9	325.4
SD of excluded group	2.3	130.8	23.0	144.8	65.1	251.7	603.5	447.1

Notes

1 The sample includes households in (i) no-intervention villages and (ii) households in no-loan villages who did not receive a grant (Intervention villages). The analysis uses only data from follow up year 1.

2 The excluded group are households in no-intervention villages.

3 Additional controls include: cercle fixed effects; the baseline value of the dependent variable, along with a dummy when missing, interacted with whether the No-intervention village dummy; and village-level stratification controls: population size, distance to nearest road, distance to nearest paved road, whether the community is all bambara (dominant ethnic group) distance to the nearest market, percentage of households with a plough, percentage of women with access to plough in village, percentage of women in village using fertilizer and the fraction of children enrolled in school. Standard errors are clustered at the village level.

4 Also included are the following individual controls: the number of adult household members, the number of children in the household, the average age of adults in the household and the share of adults with primary school education level.

Appendix Table 5: Additional Outcomes

	Business Profits: 12 months		Intra HH Decision-making Index	Community Action Index	Social Capital Index	
	(1)		(2)	(3)	(4)	
Panel A. Grant recipients vs control						
Grant - year 1	20.54		-0.0003	0.068	0.031	
	(13.74)		(0.042)	(0.043)	(0.039)	
Grant * loan village - year 1	-25.54		0.076	0.018	0.076	
	(17.41)		(0.058)	(0.061)	(0.051)	
Grant - year 2	41.49	**	0.059	0.021	0.090	***
	(18.50)		(0.039)	(0.045)	(0.034)	
Grant * loan village - year 2	-11.14		0.007	0.106	0.019	
	(27.11)		(0.058)	(0.064)	(0.050)	
Grant + Grant * loan village = 0 (year 1)	0.640		0.056	0.045	0.001	
Grant + Grant * loan village = 0 (year 2)	0.127		0.122	0.006	0.004	
N	10359		9599	9639	9476	
Mean of control (year 1)	228		0.035	-0.024	-0.065	
SD (year 1)	362		0.958	0.983	0.931	
Panel B. Loan villages vs control						
Loan village - year 1	2.06		0.000	0.052	-0.001	
	(19.41)		(0.043)	(0.052)	(0.048)	
Loan Village - year 2	9.75		0.038	0.065	0.043	
	(26.71)		(0.054)	(0.048)	(0.043)	
N	8552		7900	7934	7808	
Mean of control (year 1)	228		0.035	-0.024	-0.063	
SD (year 1)	362		0.958	0.983	0.933	

Notes

1 Rows showing Grant + Grant * loan village = 0 (year 1) shows the p value of the test of whether the total effect of grants in loan villages is statistically different from zero.

2 See the notes of Table 2 for details on specification.