

SELECTION INTO CREDIT MARKETS: EVIDENCE FROM AGRICULTURE IN MALI

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We use a two-stage experiment on agricultural lending in Mali to test whether selection into lending is predictive of heterogeneous returns to capital. Understanding this heterogeneity, and the selection process which reveals it, is critical for guiding modeling of credit markets in developing countries, as well as for policy. We find such heterogeneity: returns to capital are higher for farmers who borrow than for those who do not. In our first stage, we offer loans in 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 the farmers who do not borrow in villages where loans were offered. We estimate seasonal returns to the grant of 130% for would-be borrowers, whereas we find returns near zero for the sample representative of non-borrowers. We also provide evidence that there are some farmers—particularly those that are poor at baseline—that have high returns but do not receive a loan.

KEYWORDS: Credit markets, agriculture, returns to capital.

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, effort, and constraints of producers. Some of this variation may be apparent to outside observers; much may not. Some of this variation may be apparent to producers themselves; much may not. Financial markets ought to help capital flow to the highest return activities. But do they?

The efficiency of capital allocation matters for our understanding of both the macroeconomy and financial markets for low-income households. In macroeconomics, there is an extensive literature that incorporates financial frictions into models of growth with agents

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that have heterogeneous returns (Buera and Shin (2013), Itskhoki and Moll (2019)). This work shows that in economies with imperfect financial markets, heterogeneity exacerbates problems and capital does not necessarily get allocated to the highest return firms (Buera, Kaboski, and Shin (2021)). The microeconomic literature documents evidence of market failures for both credit and savings services for low-income households. These joint failures could result in an allocation of capital such that agents have differing returns to investment. With that in mind, we examine the extent to which a lending program for smallholder farmers in Mali successfully identifies and allocates credit to the farmers with higher returns to investment.

In a two-stage randomized controlled trial of loans and grants for low-income farmers in rural Mali, we demonstrate positive selection into borrowing with respect to returns to investments in cultivation. The sample consists of poor farmers in a capital-poor economy not well integrated into global financial markets. In stage one (the loan stage), a microcredit organization (Soro Yiriwaso, “Soro”) identified 198 villages which were within their expansion plans but which they had not previously entered. Soro then offered group-liability loans to all women farmers in 88 villages, randomly selected from the 198 villages. In these loan treatment villages, some farmers choose, or are chosen by their peers, to borrow via group-liability loans within a community association. In stage two (the cash grant stage), after first waiting for households and the associations to make their loan decisions from stage one, we announced and immediately gave cash grants (40,000 FCFA, about US\$140)¹ to a random subset of households that did not borrow in the loan villages and to a random subset of all households in the no-loan villages.

The first stage effectively creates two samples over which we compare the returns to the stage-two cash grants: 88 “loan villages” (where we measure returns to the cash grant for individuals who did not borrow) and 110 “no-loan” villages (where we measure returns to the cash grant for all individuals, i.e., those who would have borrowed had they been offered a loan as well as those who would not have borrowed). Comparing the average returns in these two samples allows us to test an important selection question: do those who do not borrow have lower average returns to a grant² than the implied returns to a grant among farmers who did borrow?

We find large average increases in investment and agricultural profits for the non-selected population (i.e., grant recipients vs. non-grant-recipients in no-loan villages). Specifically, the cash grants in no-loan villages led to a statistically significant increase in land being cultivated (12%, se = 3%), fertilizer use (19%, se = 5%), and overall input expenditures (18%, se = 5%). These households also experienced an increase in the value of their agricultural output and in gross profit³ by 14% (se = 4%) and 13% (se = 5%), respectively. Thus, we observe a statistically significant and economically meaningful increase in investments in cultivation and gross profit from cash grants. This impact on gross profit persists after an additional agricultural season. These results demonstrate clearly that capital constraints limit investments in cultivation,⁴ and that farmers do not

¹Throughout, we use the 2011 PPP exchange rate with the Malian FCFA (284 FCFA per USD).

²We are not estimating the marginal product of capital as in de Mel, McKenzie, and Woodruff (2008) but instead the “total return to capital”—that is, cash. Beaman, Karlan, Thuysbaert, and Udry (2013) showed that labor inputs adjust along with agricultural inputs, making it impossible to separate the returns to capital from the returns to labor without an instrument for labor inputs. We are therefore capturing the total change in gross profits and investment behavior when cash grants are received.

³We do not have a complete profit measure and use instead the term “gross profit” for agricultural revenue net of most, but not all, expenses. Importantly, the value of land, family and unpaid labor is not subtracted.

⁴In the absence of behavioral effects, 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.

accumulate savings over time to capture these high investment returns. Accumulation of savings may be hindered by transaction costs (e.g., transportation, theft, or devaluation), social pressure (within households or across social networks), or behavioral constraints (e.g., attention, over-optimism, or commitment).⁵

However, we find low, indeed zero, average returns to the cash grants for those who did *not* borrow (i.e., the difference between randomly receiving a grant and not among non-borrowers in loan villages). In loan villages, non-borrower households given grants did not earn any higher gross profit from the farm than non-borrowing, non-grant-receiving households. This contrasts sharply with households given grants in the no-loan villages: they had large increases in gross profit relative to those not provided grants. Therefore, we conclude that households that borrowed, and were thus selected out of the sample in loan villages eligible to receive grants, had higher returns to the grants than those that did not borrow. The differences in the impact of the grants between households that would borrow and those that do not are substantial. Among borrowing households, farm output would have increased by US\$198 (se = 92) and farm gross profit by US\$146 (se = 71) had those households received grants. In contrast, among the households that do not borrow, receipt of the grant generates only US\$21 of additional output and US\$0.28 less gross profit (neither being statistically significantly different from zero).

Thus, putting the findings from the two samples together, we infer that farmers with particularly high returns to investment in cultivation are much more likely to select—or be selected—into borrowing. This implies that some of the variation in returns is predictable *ex ante*, and that farmers are aware of and act on this heterogeneity in expected returns.

Although 93% of non-borrowing households report farming as their primary source of income, perhaps non-borrowers did not invest in farming because they had higher-return opportunities elsewhere. To examine this, we also look at other outcomes such as livestock ownership and small business operations. However, we do not find evidence that non-borrowers in loan villages invest the grant in alternative activities more than their counterparts in no-loan villages.

Thus, farmers with high returns to grants are differentially selected into borrowing from Soro. But how effective is this selection? Are there identifiable women with high-return investment opportunities who do not borrow—an outcome that we refer to as “excess selection”? Many frictions, which our design does not disentangle, may drive some of those with high expected returns to not borrow. Two examples: first, women’s groups screen out potential borrowers based on ability to repay (rather than return on capital); or, second, heterogeneity with respect to risk aversion leads some women to self-select out. Specifically, we find that in no-loan villages (thus a representative, non-selected sample of the village), returns to the grant are positively correlated with baseline levels of economic well-being: gross profits, food and non-food consumption, farm size, and livestock holdings. However, in loan villages (thus only those selected out from borrowing, either by themselves or their peers), returns to the grant are *negatively* correlated with these baseline

For example, in models akin to Banerjee and Duflo (2014) with an upward-sloping supply of credit for 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 strict sense. However, there is no evidence that these grants lowered total borrowing. Experiment demand effects might lead grant recipients to invest in agriculture in hope of attracting future grants or loans but would not generate increases in profits. We therefore refer to capital market imperfections that could cause investment responses to cash grants simply as capital constraints.

⁵See Karlan, Ratan, and Zinman (2014) for an overview of such savings markets failures, with a focus on the household perspective.

characteristics. Thus, the selection into borrowing by farmers with high-return projects is more complete among wealthier farmers. Because these characteristics are plausibly associated with both a borrower's ability to repay and risk aversion, we cannot disentangle the excess selection into borrower-driven versus lender-driven. In fact, the best policy response to this excess selection may be via interconnected markets such as insurance instead of credit directly.

The heterogeneity in returns to loans that we discover is consistent with Meager (2020), who used Bayesian hierarchical modeling of the quantiles of response to seven different microcredit interventions with RCTs to show evidence of strongly positive returns for a small set of borrowers, but near-zero returns to borrowing for the large majority. Crépon, El Komi, and Osman (2020) also found heterogeneity in the returns to loans (and grants) among microentrepreneurs in Egypt. Similarly, Bryan, Karlan, and Osman (2021) found important heterogeneity, but predicted via psychometric data only, not data typically available to lenders for underwriting decisions.

More recent work has focused on whether individuals and peers are able to predict returns to capital. Hussam, Rigol, and Roth (2020) found that businesses (in their case, non-farm enterprises in urban India) have widely varying marginal returns to grants, and that entrepreneurs themselves and community members are able to distinguish between those with relatively high and low returns. Similarly, Barboni and Agarwal (2021) found that financially sophisticated borrowers positively-select into more flexible lending contracts. In other settings, accurate predictions were more elusive: for enterprise business plan competitions in Nigeria and in Ghana, several studies provide evidence of the difficulty in predicting the most successful, although average estimated returns are high (Fafchamps and Woodruff (2017), McKenzie (2017, 2018), McKenzie and Sansone (2019)).

Our experiment also speaks to three additional questions important to academia and policy. First, do loans generate different investment behavior than grants? Second, what is the impact of a microlending program that targets farmers (as compared to the more standard microenterprise focus of microlenders)? Third, are the impacts of the cash grants persistent after seven years?

First, on comparing grants to loans, about 21% of households in our sample received loans (in loan villages), which is typical of other microcredit contexts. Grant take-up rate was 100%. Loans averaged US\$113 (versus grants of \$140). Both the grants and loans were timed around the crop growing season. Like the grants, 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 gross profit. Our treatment on the treated estimates on the cultivation activities and harvests are large and consistent in magnitude 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. This is consistent with Crépon, El Komi, and Osman (2020).

Second, underlying our experiment is an estimate of the impact of an agriculture microcredit program: we find high average returns, particularly when compared to experiments estimating the impact of microcredit designed for entrepreneurship.⁶ Such results could emerge when farmers lack capital and face credit and savings constraints. Microcredit

⁶The evidence from traditional microcredit, targeting micro enterprises, is more mixed; some randomized evaluations find an increase in investment in self-employment activity (Crépon, Devoto, Duflo, and Pariente (2015), Angelucci, Karlan, and Zinman (2015)) while others do not (Karlan and Zinman (2011), Attanasio, Augsburg, De Haas, Fitzsimons, and Harmgart (2015), Augsburg, De Haas, Harmgart, and Meghir (2015), Banerjee, Duflo, Glennerster, and Kinnan (2015), Tarozzi, Desai, and Johnson (2015)). See Banerjee, Karlan,

organizations have attempted to relieve credit constraints, but most microcredit lenders focus on small or micro business entrepreneurial financing. Furthermore, the typical microcredit loan requires frequent, small repayments and therefore does not facilitate investments in agriculture, where income comes as a lump sum once or twice a year (see [Karlan and Mullainathan \(2007\)](#) for a discussion of loans designed to match borrower cash flow needs; see [Fink, Jack, and Masiye \(2020\)](#) for an experiment demonstrating the importance of timing for farmers). 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 at harvest. [Maitra, Mitra, Mookherjee, and Visaria \(2020\)](#) also found positive impacts from an agricultural microcredit program on farm value-added in India for one version of the program, though not for a version which targeted the program differently. However, lending may not be sufficient to induce investments in the presence of other constraints. Farmers may be constrained by a lack of insurance ([Karlan, Osei, Osei-Akoto, and Udry \(2014\)](#)), have time-inconsistent preferences ([Duflo, Kremer, and Robinson \(2011\)](#)), or face high costs of acquiring inputs ([Suri \(2011\)](#)).

These loan impacts show that well-timed credit can make a strong difference in agricultural impacts for households that have difficulty (due to credit and savings markets failures) accumulating enough cash at the moments needed. Such logic was the premise behind 20th century *crédit de campagne* (seasonal policy-led agricultural lending, timed to coincide with when cash is needed for investments). But these programs suffered from politics ([Adams, Graham, and Von Pischke \(2022\)](#)) as well as from multilateral and bilateral donors discouraging subsidized lending so as to promote for-profit lenders ([Morduch \(2000\)](#)). In the expansion of microcredit in the 1980s and onward, several shifts occurred: group instead of individual lending; high-frequency repayment instead of one-time balloon payments (see [Field, Pande, Papp, and Rigol \(2013\)](#) for an important demonstration of potential benefits to delayed-start repayment); enterprise targeted loans instead of agricultural ([Karlan and Morduch \(2009\)](#)); and non-government lending instead of government. These changes were typically simultaneous and rarely experimental, thus often left unstudied. The lending analyzed here represents a return to the lending philosophy of “matching cash flows,” that is, timing both issuance of loans to when capital is most needed and repayment to when revenue will be generated (but within a group structure, and without subsidy).

Third, we conducted a long-term follow-up survey in 2017, about seven years after the grants. We find no evidence that the grants had a persistent effect over this extended period, which was marked by political upheaval, systematic changes in cropping patterns, and highly variable seasonal rainfall typical of the West African semiarid tropics. While it is difficult to interpret the lack of long-run effects given the large number of shocks in this context, farmers could need sustained access to financial services that are tailored to their needs, specifically facilitating access to lump sums when needed.

2. THE EXPERIMENTAL DESIGN AND DATA

2.1. *The Experimental Design*

Agriculture in most of Mali, and in all of our study area, is exclusively rain fed. Evidence from nearby Burkina Faso suggests that income shocks translate into consumption volatil-

and [Zinman \(2015\)](#) and [Meager \(2019\)](#) for an overview of the above seven studies. Most randomized evaluations of microcredit find little or no increase at the mean on profitability of small businesses. These modest results come despite evidence of fairly high marginal returns to capital for micro enterprises ([de Mel, McKenzie, and Woodruff \(2008\)](#)).

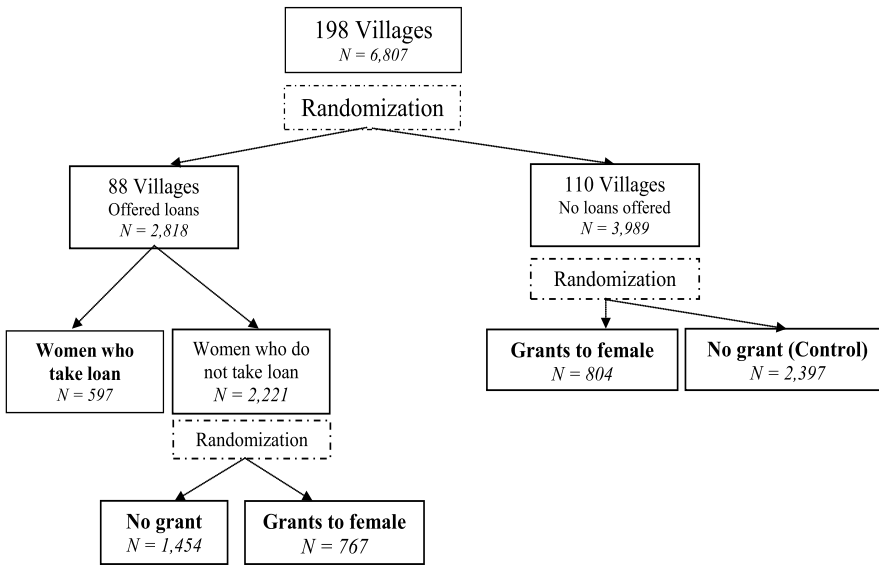


FIGURE 1.—Experimental design: allocation of households to treatments.

ity (Kazianga and Udry (2006)), so improved credit markets can have important welfare consequences from both increasing average production and insulating consumption from output volatility. 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,⁷ and 51% were using other chemical inputs (herbicides, insecticide).

The sample consists of 198 villages identified by Soro as villages not previously entered but within their expansion plans. Households in these villages have limited access to formal financial institutions: only 5% of households report receiving a formal loan at baseline.⁸ Figure 1 presents the design, and Appendix A1 of the Supplemental Material (Beaman, Karlan, Thuysbaert, and Udry (2023)) provides more detail on the sample and randomization procedures.

Stage One: Loans

Soro, a Malian microcredit organization and affiliate of the international organization Save the Children, marketed, financed, implemented, and serviced the loans. After a baseline survey, we randomly assigned the 198 villages to either loan (88 villages) or no-loan (110 villages) status using a re-randomization technique ensuring balance on key variables (Appendix A1).

Soro offered their standard agricultural loan product, called *Prêt de Campagne*, in the 88 loan villages. There was no screening of the villages by Soro: loans were offered to

⁷The government of Mali introduced heavy fertilizer subsidies in 2008 and fixed the price of fertilizer at 12,500 FCFA (US\$44) per 50 kg of fertilizer. This constituted a 20% to 40% subsidy, depending on the type of fertilizer and year. 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)).

⁸Informal financing is present via savings groups and loans from family or friends (50% report such loans at baseline).

women's associations formed for the purpose of borrowing. This product is given exclusively to women, but naturally money may be fungible within the household. Unlike most microloan products, the loan is designed specifically for farmers: loans are dispersed at the beginning of the agricultural cycle in May–July and repayment is required after harvest. The loan is administered to groups of women organized into village associations, and each individual woman then receives an informal contract with their village 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 records of neither loan applications nor denials. 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 the loan each woman received. Soro itself was not directly involved in selecting who would receive a loan. The size of the loan to each woman is also determined through an informal, iterative process. Repayment is tracked only at the group level, and nominally there is joint liability. On average, there are about 30 women per group and typically one, though up to three, associations per village. This is a limited liability environment since these households have few assets and the legal environment of Mali makes formal recourse for the bank nearly impossible. However, given that loans are administered through community associations, the social costs of default could be quite high. We observe no defaults over the two agricultural cycles of our study.⁹

Soro offered loans in the loan villages for two years, the 2010 and 2011 agricultural seasons. Loans averaged 32,000 FCFA (US\$113) and charged 25% per annum interest plus 3% fees.¹⁰

Stage Two: Grants

Grants worth 40,000 FCFA (US\$140) were distributed by Innovations for Poverty Action (IPA), and with no stated relationship to the loans or to Soro, to about 1600 female survey respondents in May and June of 2010 (i.e., planting time).

In the 110 no-loan villages, households were randomly selected to receive grants and—to parallel the loans—a female household member was always the direct recipient. This corresponds to the boxes on the right side of Figure 1. US\$140 is a large grant; average input expenses, in the absence of the grant, were US\$130 and the value of agricultural output was US\$530. The size of the grant was chosen to approximate the average loan size provided by Soro, though *ex post* the grant is slightly larger on average than the 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.¹¹

In the 88 loan villages, grant recipients were randomly selected among survey respondents who did not take out a loan (see Figure 1).¹² We attempted to deliver grants at

⁹This 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 (more than 30 days overdue) in years 2004–2006, rising to only 0.7% in 2007.

¹⁰Ten percent of loan proceeds was deducted from loan proceeds and held at Soro as savings. This deposit requirement may serve as a screening mechanism based on wealth or liquidity, as discussed in Section 3.3.

¹¹The grants to men are intended for a separate paper analyzing household dynamics and bargaining, and we do not consider them useful for the analysis here since the loans were only given to women.

¹²We determined who took out a loan by matching names and basic demographic characteristics from the loan contracts between the client and Soro, which Soro shared with us on an ongoing basis. There were a few

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 difference of 20 days between when grants were received by households in no-loan villages and their counterparts in loan villages. We discuss the implications of this delay in Appendix A6.1 of the Supplemental Material.

To minimize the risk that our experimental design led individuals in the future to not borrow in order to be eligible for a grant, we informed recipients that the grants were a one-time grant not an ongoing program, and we distributed an additional 104 grants (one or two per village) to loan village women not in our sample (aiming to minimize the risk that individuals inferred the link between borrowing and grant eligibility).

2.2. Data

A baseline survey 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 season¹³ in January–May 2011; 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; and a third follow-up survey was conducted seven years after the initial grant distribution in January–May 2017. The four rounds used similar survey instruments, which 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 both the individual and the household level.

Throughout, we refer to “gross profit” as a key outcome variable. We do not have a complete measure of profits. Gross profit is the value of agricultural output net of most, but not all, expenses. Specifically, gross profit 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 either the value of unpaid labor (own, family, or other) 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. We will, however, examine the use of these inputs directly.

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. The food expenditure module asked about consumption of over 50 food items over the previous seven days. We calculate prices using village-level reports in all sample villages. We use these sample-wide prices to convert consumption of all items into expenditure. It is important to note that there is a lot of consumption seasonality in Mali (Beaman, Karlan, and Thuysbaert (2014)). Our measure of food expenditure reflects consumption in the post-harvest season only.

cases (67) where Soro allowed late applications for loans and households received both a grant and a loan. The majority (41 of 67) 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 these observations are excluded.

¹³Immediately after the first year planting agricultural phase and on a subsample of 2400 households, we conducted an “input survey” with extensive questions on inputs such as seeds, fertilizer and other chemicals, labor, and equipment use. The sample was generated by randomly selecting half of the households from a randomly chosen subset of 133 villages (and stratifying by treatment status). For each household, we use the input survey if conducted and otherwise the end of season survey. We also control for survey timing in all relevant specifications.

2.3. Randomization Balance Check and Attrition

Several tests verify that we cannot reject the orthogonality of treatment assignment to baseline characteristics and attrition. Appendix Table I examines 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 statistically significantly different across the three comparisons, and an aggregate test on all 11 covariates fails to reject orthogonality for each of the three comparisons (p -value of 0.13, 0.41, and 0.89, respectively). Our attrition rate is approximately one percent each round.¹⁴

3. IDENTIFICATION

We focus on agricultural outcomes. Let $\{Q^{NG}, Q^G, Q^B\}$ represent the set of potential gross profits in year 1 of households in our sample, where Q^{NG} is a random variable representing potential profit if the household neither borrows nor receives a grant, and Q^G and Q^B are similarly defined for households that receive a grant but do not borrow, and for those that borrow but do not get a grant, respectively. The joint distribution of potential outcomes is $F(Q^{NG}, Q^G, Q^B)$, and the three marginal distributions are denoted $F_{NG}(Q^{NG})$, $F_G(Q^G)$, and $F_B(Q^B)$.

Define $G \in \{0, 1\}$ and $L \in \{0, 1\}$ as random variables that designate a household's status in the grant treatment arm and in a loan treatment village, respectively. Not all women in loan communities borrow. Define $C = 1$ (for *complier*) if the household would borrow if its village is a loan village, and $C = 0$ if it would not borrow if located in a loan village.

Therefore, we can write a binary indicator of borrowing as

$$B = CL.$$

Furthermore, define the effect on profit of receiving a grant as $\Delta_G Q \equiv Q^G - Q^{NG}$. We seek to identify the expected value of the effect on profit of receiving a grant for households for which $C = 1$ versus those for which $C = 0$, that is, $\mathbb{E}(\Delta_G Q | C = 1)$, and $\mathbb{E}(\Delta_G Q | C = 0)$. The two-stage randomization provides identification of these expected treatment effects.

3.1. Returns to Grants for Borrowers and Non-Borrowers

The first-stage randomization of villages ensures

$$\{Q^{NG}, Q^G, Q^B, C\} \perp L. \quad (1)$$

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

$$\{Q^{NG}, Q^G, Q^B, C\} \perp G | L = 0, \quad (2)$$

$$\{Q^{NG}, Q^G\} \perp G | (C = 0, L = 1). \quad (3)$$

There is 100% take-up of the offer of a grant, so in our sample of the full population of no-loan villages and in our sample of non-borrowers in loan villages, we observe

$$Q = Q^G G + Q^{NG} (1 - G).$$

¹⁴Despite the low attrition rate, we report differential attrition tests in Appendix Table II.

Data from grant recipients in no-loan villages identify $F_G(Q^G|L = 0, G = 1)$. Equation (3) implies that non-grant recipients in the no-loan villages have the same distribution of (for them, counterfactual) Q^G , providing the first equality of (5):

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

The fact that the population of any loan village is partitioned into grant recipients and non-recipients provides the second equality of (4). Equation (1) implies the third equality for the distribution of Q^G in the overall population. Similarly, (1) and (2) imply that data from non-grant recipients in no-loan villages identify $F_{NG}(Q^{NG}|L = 0, G = 0)$, which equals the marginal distribution of Q^{NG} in the general population (dropping the intermediate equalities for brevity in (6) and (8)):

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

In parallel, data from non-borrowers in loan villages identify the conditional (on $C = 0$) marginal distributions of the profits of those who receive and do not receive a grant, respectively:

$$\begin{aligned} F_G(Q^G|C = 0, L = 1, G = 1) &= F_G(Q^G|C = 0, L = 1, G = 0) = F_G(Q^G|C = 0), \\ F_{NG}(Q^{NG}|C = 0, L = 1, G = 0) &= F_{NG}(Q^{NG}|C = 0). \end{aligned} \tag{6}$$

The loan village sample provides an estimate of $\mathbb{P}(C = 1|L = 1)$, which with (1) identifies the share of compliers in the population $\mathbb{P}(C = 1|L = 1) = \mathbb{P}(C = 1|L = 0) = \mathbb{P}(C = 1)$. Therefore, we have identified the marginal distributions of profits for grant recipients and non-recipients among the selected population of those who would borrow:

$$\begin{aligned} F_G(Q^G|C = 1) &= \frac{F_G(Q^G) - F_G(Q^G|C = 0)(1 - \mathbb{P}(C = 1))}{\mathbb{P}(C = 1)}, \\ F_{NG}(Q^{NG}|C = 1) &= \frac{F_{NG}(Q^{NG}) - F_{NG}(Q^{NG}|C = 0)(1 - \mathbb{P}(C = 1))}{\mathbb{P}(C = 1)}. \end{aligned} \tag{7}$$

With these marginal distributions identified, we can calculate the average effects of receiving a *grant* 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:

$$\begin{aligned} \mathbb{E}(Q^G) - \mathbb{E}(Q^{NG}) &\equiv \mathbb{E}(\Delta_G Q), \\ \mathbb{E}(Q^G|C = 0) - \mathbb{E}(Q^{NG}|C = 0) &\equiv \mathbb{E}(\Delta_G Q|C = 0), \\ \mathbb{E}(Q^G|C = 1) - \mathbb{E}(Q^{NG}|C = 1) &\equiv \mathbb{E}(\Delta_G Q|C = 1). \end{aligned} \tag{8}$$

We provide estimates of these three expectations in Section 4.2 by estimating

$$Y_i = \alpha + \beta_1 \text{grant}_i + \beta_2 \text{grant}_i \cdot \text{loan}_{v(i)} + X_i \pi + \lambda_{v(i)} + \epsilon_i, \tag{9}$$

where $\hat{\beta}_1$ is our estimate of $\mathbb{E}(\Delta_G Q)$ and $\hat{\beta}_1 + \hat{\beta}_2$ is our estimate of $\mathbb{E}(\Delta_G Q|C = 0)$ when the outcome Y_i is gross profit of farmer i in village $v(i)$, $\lambda_{v(i)}$ is a village fixed effect, and X_i is a vector of baseline controls to be discussed below.

3.2. Average Return to Borrowing

Similarly, define the effect on profit of borrowing as $\Delta_B Q \equiv Q^B - Q^{NG}$. We also identify the expected treatment effect of borrowing on those who would borrow if loans were available: $E(\Delta_B Q | C = 1)$. Equation (1) implies that data from the population of borrowers in loan villages can be used to identify the conditional marginal distribution:

$$F_B(Q^B | C = 1, L = 1) = F_B(Q^B | C = 1, L = 0) = F_B(Q^B | C = 1).$$

We have already noted that (1), (2), and (3) imply that $F_{NG}(Q^{NG} | C = 0)$ is identified from data on the profits of non-borrowers who do not receive a grant in loan villages, and shown in (7) that combining this with estimates of $\mathbb{P}(C = 1)$ and $F_{NG}(Q^{NG})$ identifies $F_{NG}(Q^{NG} | C = 1)$. Thus, we can identify the average treatment effect on the treated (TOT) of borrowing:

$$\mathbb{E}(Q^B | C = 1) - \mathbb{E}(Q^{NG} | C = 1) \equiv \mathbb{E}(\Delta_B Q | C = 1). \quad (10)$$

Note that we needed no assumption about whether farmers make the same investment decisions with a loan as they would with a grant in order to identify either (8) or (10).

3.3. Selection and Frictions in the Allocation of Capital

Our experimental design does not allow us to directly compare the identified selection into borrowing with what would be optimal. Here, we provide a theoretical framework that will guide our examination of frictions in the allocation of loans to potential borrowers.

Our benchmark is a frictionless allocation of loans of fixed size from a lender: any household with a project with expected return greater than the opportunity cost of the loan to the lender borrows, and other households do not. Suppose that the common opportunity cost of funds to the lender is ρ . Aggregate expected gross profits are maximized if all households with $\Delta_G Q \geq \rho$ borrow, while other households do not. However, in an environment of transaction costs, imperfect enforcement, incomplete information, and uninsured risk, there may be potential borrowers that do not receive loans but have projects that could generate high returns. Screening by the lender, self-selection, or both could drive this “excess selection.”

Among these frictions, the two most salient in our setting are (i) lender (more specifically, women’s group) screening on ability to repay in a context with limited liability, and (ii) borrower risk aversion. In Appendix A2 of the Supplemental Material, we present two simple canonical models to provide guidance as to why certain high-expected-return borrowers do not take loans. In the first, poorer or less collateralized potential borrowers with high marginal returns may be unable to make a credible repayment commitment. In the second, risk aversion may deter poorer or more risk-averse farmers with high-expected-return projects from borrowing for those projects. In both cases, the frictions imply that there will be non-borrowers for whom their marginal return exceeds the opportunity cost of funds, and that the extent of this wedge decreases as a farmer’s baseline gross profits, collateral, or wealth increase. We base our empirical tests that the allocation of loans maximizes profit on these common implications of the two models; we do not, therefore, distinguish between self-selection (based on risk aversion) and lender-selection (based on limited liability) as the source of the frictions that result in excess selection. We provide these two models as examples to demonstrate the possibility that poorer farmers with high returns to investment might be excluded from borrowing; other frictions that we do not

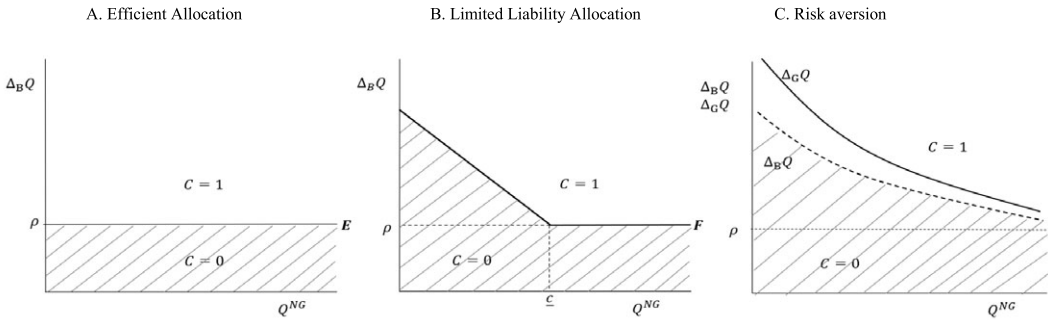


FIGURE 2.—Selection into borrowing. *Notes.* The y axis is the change in gross profit in response to receiving a loan (or a grant in panel C). ρ is the lender’s gross cost of funds. The x axis represents gross profit in the absence of a grant or loan. c is the minimum consumption required below which the limited liability constraint binds. In Panel C, borrowers have DARA preferences; as Q^{NG} increases, a risk-averse farmer requires a smaller wedge between her expected returns and r to borrow. The project chosen by a DARA borrower given a grant will have expected return weakly larger than the project she chooses with a loan, with that gap declining with Q^{NG} .

model could do the same. For example, moral hazard in labor markets, transaction costs in output markets, incomplete knowledge of technologies, and heterogeneity in access to complementary inputs could all generate what we denote excess selection; furthermore, these other frictions might be more salient in other environments.

We illustrate the basic predictions of the models in Figure 2. The frictionless allocation is depicted in the left panel of Figure 2: the horizontal curve E defines the boundary in $(Q^{NG}, \Delta_B Q)$ between those who borrow and those who do not in a profit-maximizing allocation assigning credit exclusively to all farmers with a sufficiently profitable investment opportunity. A farmer i with values of $(Q_i^{NG}, \Delta_B Q_i)$ in the region $C = 0$ does not borrow because her returns are too low; her no-grant level of profits is irrelevant to the allocation. In Panel B, the curve F defines the boundary in an allocation constrained by limited liability. The set of values of $(Q_i^{NG}, \Delta_B Q_i)$ such that a farmer does not borrow expands due to the friction. The dashed curve in Panel C depicts the boundary in the allocation in the presence of farmers’ decreasing absolute risk aversion (DARA). With either friction, the wedge between the lender’s cost of funds and the farmer’s required expected return from the loan (weakly) decreases with the no-grant gross profit of the farmer. The wedge exists when a limited liability constraint binds, but this constraint is relaxed by increases in no-grant gross profits. Similarly, if a farmer has decreasing absolute risk aversion, then the expected return from borrowing she requires to accept the additional risk associated with borrowing declines with her no-grant gross profit.

Sections 3.1 and 3.2 demonstrated that our experimental design gives us clean identification of the returns to grants and the returns to loans. To evaluate the extent of frictions in the allocation of credit, however, we must consider the relationship between the two. In a frictionless allocation, $\Delta_G Q = \Delta_B Q$, because both maximize profits. However, risk aversion generates selection across projects of a farmer as well as across farmers. The project chosen by a risk-averse borrower who is given a grant will have an expected return (weakly) greater than the project that that farmer would have chosen to implement with a loan. Figure 2c also illustrates this: the (solid line) boundary in $(E(Q^{NG}), E(\Delta_G Q))$ between borrowers and non-borrowers does not lie above that (dashed line) boundary in $(E(Q^{NG}), E(\Delta_B Q))$, and with DARA preferences, the difference between the boundaries declines as $E(Q^{NG})$ rises. The key takeaway is that if farmers in the non-borrower sam-

ple demonstrate that they have high-return projects (from their returns to the grants), we have evidence of excess selection. In Section 6, we will test empirically the hypothesis that the expected agricultural returns to grants for those who would borrow are equal to the expected agricultural returns to a loan for those who do borrow, but our interpretation of the evidence does not rely on farmers choosing the same projects with loans versus grants.

We take two complementary approaches to investigate empirically the extent of excess selection out of borrowing by poor households with high-return projects. First, assuming rank invariance (the ordering of farmers’ potential gross profits is the same across treatments), the comparison of the gap between the distributions of profits of grant recipients and non-grant recipients in the no-loan villages with the analogous gap in the selected sample of non-borrowers in loan villages is informative that borrowing frictions exist (Abbring and Heckman (2007)). At high enough levels of non-grant gross profits in the loan villages, the only non-borrowers eligible to receive grants would be those farmers without high-return projects. Thus, there will be small differences between gross profits of grant recipients and non-recipients for sufficiently high Q^{NG} . For low non-grant profits Q^{NG} , even farmers with high-return projects are unable to borrow; then the gross profit distribution for grant recipients will be shifted rightward compared to non-recipients in loan villages (similar to no-loan villages). Thus, we expect $F_{NG}(Q|C = 0) - F_G(Q|C = 0) - (F_{NG}(Q) - F_G(Q))$ to decline in Q with excess selection. Section 5 examines this empirically.

Second, we relax the rank invariance assumption. Excess selection can be distinguished from frictionless selection via differing implications for heterogeneous treatment effects. We start with a simple extension of (8) that implies we can estimate the conditional (on any observable characteristic X^k) average treatment effects $\mathbb{E}(\Delta_G Q|X^k = x^k)$ and $\mathbb{E}(\Delta_G Q|X^k = x_i^k, C = 0)$. We estimate linear approximations to these conditional expectations via the regression

$$Y_i = \alpha + \beta_1 \text{grant}_i + \beta_2 \text{grant}_i \cdot \text{loan}_{v(i)} + \gamma_1 \text{grant}_i \cdot X_i^k + \gamma_2 \text{grant}_i \cdot X_i^k \cdot \text{loan}_{v(i)} + X_i \pi + \lambda_{v(i)} + \epsilon_i. \tag{11}$$

From this regression we construct $\hat{\beta}_1 + \hat{\gamma}_1 \cdot X_i^k$ as our estimate of $\mathbb{E}(\Delta_G Q|X^k = x_i^k)$, and $\hat{\beta}_1 + \hat{\beta}_2 + (\hat{\gamma}_1 + \hat{\gamma}_2) \cdot x_i^k$ as our estimate of $\mathbb{E}(\Delta_G Q|X^k = x_i^k, C = 0)$ when the dependent variable Y is gross profits Q .

The actual return to the grant for farmer i (which is unobserved to us, but perhaps is known to the farmer and/or the lender) is

$$\Delta_G Q_i = \mathbb{E}(\Delta_G Q|X = \mathbf{x}_i) + \mu_i, \tag{12}$$

with $\mathbb{E}(\mu_i|X = \mathbf{x}_i) = 0$ in the general population. Let $h(X_i)$ represent the wedge between the lender’s cost of funds and the farmer’s required expected return from the loan shown in Figure 2, so farmer i in a loan village borrows if and only if $\Delta_G Q_i > \rho + h(X_i)$.¹⁵ If lending is frictionless, $h(X_i) = 0$. With frictions, $h(X_i) \geq 0$ and we consider heterogeneity along dimensions k such that these frictions decline with X_i^k . This implies that non-borrowers have realizations of μ_i less than a threshold $\bar{\mu}_i \equiv \rho + h(X_i) - \mathbb{E}(\Delta_G Q|X = \mathbf{x}_i)$; therefore, $\mathbb{E}(\mu|X = \mathbf{x}_i, \mu < \bar{\mu}_i) \leq 0$. Taking expectations of (12) over the non-borrowers

¹⁵This abstracts from risk and thus implies that the farmer knows μ_i . To permit risk, we let $\mu_i = v_i + \xi_i$ with v_i known to the farmer and $E(\xi|x_i, v_i) = 0$. Now frictionless borrowing is determined by $E(\Delta_G Q|v_i) > \rho$ and the following argument proceeds as stated, with the added notation.

in loan villages, we have $\mathbb{E}(\Delta_G Q|X = x_i, C = 0) \leq \mathbb{E}(\Delta_G Q|X = x_i)$. Conditional on any observed characteristic, the average return to grants should be higher in the no-loan villages.

Selection also has implications for patterns of heterogeneity in returns to grants. The treatment effect heterogeneity along dimension k in loan villages depends on (a) how that variable (say, baseline gross profit) is correlated with expected returns to the grant in the full population; (b) how changes in those expected returns affect selection ($\mu < \bar{\mu}_i$); and (c) how X^k is correlated with frictions in borrowing.

Assume that μ is independent of X^k , and has a normal, power, double exponential, or Pareto distribution. Heterogeneity along dimension k among non-borrowers is related to that in the random sample by

$$\begin{aligned} & \frac{d\mathbb{E}(\Delta_G Q|X = x_i, C = 0)}{dX^k} \\ &= \frac{d\mathbb{E}(\mathbb{E}(\Delta_G Q|X = x_i) + \mu_i|X = x_i, \mu < \bar{\mu}_i)}{dX^k} \\ &= \left(1 - \frac{\partial \mathbb{E}(\mu|\mu < \bar{\mu}_i)}{\partial \bar{\mu}_i}\right) \frac{d\mathbb{E}(\Delta_G Q|X = x_i)}{dX^k} + \frac{\partial \mathbb{E}(\mu|\mu < \bar{\mu}_i)}{\partial \bar{\mu}_i} \frac{dh(x_i)}{dX^k}. \end{aligned} \tag{13}$$

Suppose that the grant treatment effect in no-loan villages is increasing in X^k (the argument is symmetric around zero). Then, farmers in the no-loan villages with higher values of X^k have higher expected returns ($\frac{d\mathbb{E}(\Delta_G Q|X=x_i)}{dX^k} = \gamma_1$). If there are no frictions, then the third term above is zero, and in loan villages, the increase in expected returns reduces the critical value $\bar{\mu}_i^k$, partially (but only partially) offsetting the increase in expected returns to the grant among non-borrowers in loan villages ($0 \leq \frac{\partial \mathbb{E}(\mu|\mu < \bar{\mu}_i)}{\partial \bar{\mu}_i} \leq 1$). So, $\gamma_1 > \gamma_1 + \gamma_2 > 0$. With frictionless selection, if expected returns to grants are increasing in X^k in the no-loan villages, then expected returns to grants in loan villages must also be increasing in X^k , but the slope is attenuated towards zero.

If there is excess selection, $\frac{dh(X_i)}{dX_i^k} < 0$ and the additional third term is negative. Average returns to grants are larger in the general population than among the non-borrower subpopulation. But this gap is attenuated at sufficiently low levels of X^k , because even farmers with high-return projects are not borrowing due to the high wedge generated by the friction. Excess selection always reduces the slope of the relationship between average returns to grants and any X^k that is negatively correlated with borrowing frictions. If $\gamma_1 > 0$ (expected returns to the grant are increasing in X^k in the random sample), then $\gamma_1 > \gamma_1 + \gamma_2$. Recall that in the case of frictionless allocation of loans, this effect could only attenuate the heterogeneity. By contrast, if excess selection is sufficiently strong, the sign can change: $\gamma_1 > 0 > \gamma_1 + \gamma_2$.¹⁶

We also examine the joint and potentially non-linear effects of a vector of baseline observables X that might be associated with excess selection. We implement a causal forest algorithm to estimate conditional average treatment effects (CATEs) flexibly (see Appendix A3 for methodological details). We use the algorithm trained on no-loan villages to predict the CATE of a grant for farmer j , $E(\Delta_G Q|X = x_j)$. We use the algorithm trained on non-borrowers in loan villages to estimate the CATE of a grant for non-borrowing farmer i , $E(\Delta_G Q|X = x_i, c_i = 0)$. Excess selection into borrowing has the same observable implications for the relative slopes of the CATEs estimated using causal forests as they do in the linear regression (11).

¹⁶Similarly, if $\gamma_1 < 0$, $\gamma_1 + \gamma_2 < \gamma_1 < 0$ with sufficiently strong excess selection.

Section 5 examines the hypothesis that the observed selection is frictionless by focusing on a series of observable characteristics plausibly correlated with the salient borrowing frictions of ability to repay and borrower risk aversion (baseline gross profits, livestock ownership, food consumption or non-food expenditure at baseline¹⁷) and by using the causal forest algorithms.

4. SELECTION INTO LOANS AND THE RETURN TO CASH GRANTS

4.1. *Observable Characteristics of Borrowers Versus Non-Borrowers in Loan Villages*

Take-up of the loans, determined by matching names from administrative records of Soro with our sample, was 21% in the first agricultural season (2010–2011) and 22% in the second (2011–2012). Despite the similarity in overall take-up numbers, there is turnover in clients. 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 (for analysis of randomized evaluations of group-based microcredit, see Angelucci, Karlan, and Zinman (2015), Attanasio et al. (2015), Banerjee et al. (2015), Crépon et al. (2015), Tarozzi, Desai, and Johnson (2015); and for a summary discussion of these studies, see Banerjee, Karlan, and Zinman (2015)).

Table I provides descriptive statistics from the baseline on households who choose to take out loans in loan villages, compared to non-clients in those villages. There is a striking pattern of selection into loan take-up: households that invest more in agriculture, have higher agricultural output, or earn higher gross profits are more likely to take out a loan. Borrowers also have more agricultural assets and livestock. The causal forest algorithm trained on data from no-loan villages provides estimates of the CATEs of the grant treatment given baseline characteristics of a household. We apply that model to all households in the loan villages to obtain predicted treatment effects for borrowers and non-borrowers. The final row of Panel A of Table I shows that households that borrow have higher predicted CATEs from the grant treatment than do non-borrowing households. 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.¹⁸ Households that borrow also have higher consumption at baseline than non-clients.

4.2. *Experimental Results on Returns to Grants in Loan and No-Loan Villages*

Next, we present the estimated returns to receiving a grant amongst the general population, amongst those who would not borrow if they were in a loan village, and amongst

¹⁷We attempted to measure risk aversion in the baseline survey. However, the data are very noisy and feedback from the field suggests the survey respondents did not understand well the questions. As seen in Table I, loan take-up is not correlated with our measure of risk aversion, and poor-quality data are one possible reason. We therefore focus on proxies of risk aversion: variables which would suggest households are close to subsistence.

¹⁸All 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 the woman has on decisions regarding: food for the household, children’s schooling expenses, their health, their travel, and their economic activities. The community action index includes questions on the frequency she speaks with different village leaders and participates in village meetings and activities. The social capital index includes questions about seven other randomly selected community members and whether the respondent knows the person, is in the same organization, would engage in informal risk-sharing and transfers with the person, and topics of their discussions (if any).

TABLE I

COMPARISON OF BASELINE CHARACTERISTICS OF CLIENTS VERSUS NON-CLIENTS IN LOAN TREATMENT VILLAGES.

	Took up (1)	Did not take up (2)	Difference ³ (3)
A. Agriculture, Livestock & Business			
Land size (ha)	2.64 (2.71)	2.21 (2.64)	0.59 (0.13)
Total input expenses	205.82 (300.42)	151.87 (285.75)	46.37 (14.22)
Value of agricultural output	709.04 (752.17)	596.10 (827.66)	132.60 (39.79)
Gross profit	503.22 (555.12)	444.23 (642.11)	86.23 (30.84)
Total value of livestock	1871.22 (3037.9)	1294.65 (2549.9)	504.65 (135.2)
Predicted grant treatment effects (CATEs)	53.53 (23.04)	52.44 (23.03)	3.31 (1.15)
B. Household Demographics			
Age of female respondent	36.58 (10.29)	34.92 (11.68)	2.46 (0.58)
Married (0/1)	0.98 (0.13)	0.92 (0.27)	0.07 (0.01)
Not first wife (0/1)	0.33 (0.47)	0.19 (0.39)	0.13 (0.02)
Number of children	4.86 (2.34)	4.34 (2.40)	0.70 (0.12)
Risk aversion: safe lottery	0.46 (0.50)	0.50 (0.50)	-0.03 (0.02)
Index of intra-household decision making power	0.08 (0.97)	-0.03 (1.05)	0.14 (0.05)
Index of community action	0.28 (1.03)	-0.03 (0.99)	0.26 (0.05)
Social integration index	0.23 (1.04)	-0.09 (0.98)	0.18 (0.05)
D. Consumption			
Food consumption EQ (past 7 days, USD)	6.89 (4.17)	6.70 (4.22)	0.40 (0.21)
Monthly non-food exp (USD)	48.09 (45.38)	39.77 (38.44)	10.04 (2.03)

¹The 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 seven 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 no-loan villages.

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

³Column (3) shows the difference using a regression specification which also includes village fixed effects.

⁴The *Predicted grant treatment effects (CATEs)* in Panel A are the predicted CATEs for non-borrowers and borrowers in loan villages using the model estimated by the causal forest algorithm trained on no-loan villages ($E(\Delta_G Q|X = x_j)$).

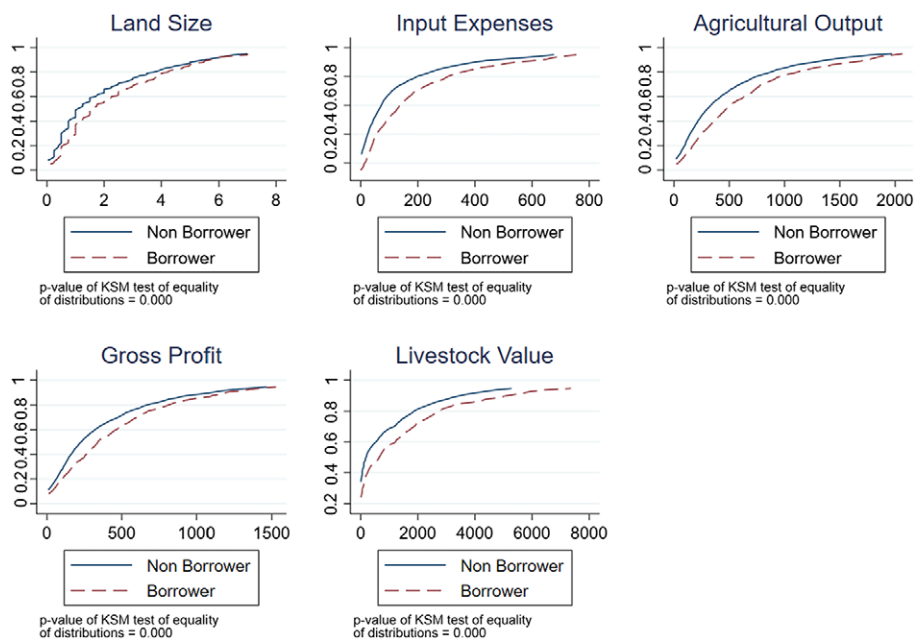


FIGURE 3.—Baseline characteristics of borrowers versus non-borrowers in loan villages.

those who would borrow if they were in a loan village (equation (10)). To isolate the role of selection into loans, we focus on the first year of the experiment.¹⁹ Table II shows the estimates from regression (9) using the first follow-up data on farm investments and output. Loan recipients are removed from the analysis sample. The baseline controls (X) include the baseline value of the dependent variable y_0 ²⁰ and the baseline variables used in the re-randomization routine (listed in the notes of Table II). Standard errors are clustered at the village level. Randomization inference p -values (Young (2019)) account for both the re-randomization routine used to assign treatment status and multiple comparisons within families of outcomes (details discussed in table notes).

Table II shows the estimates from this regression for a variety of cultivation outcomes (inputs along with harvest output and gross profits), and Table III shows the analogous estimates for non-cultivation outcomes such as livestock, enterprise, consumption, and female empowerment.

4.2.1. Agriculture

Table II, columns (1)–(8) examine agricultural inputs and crop choice. We first focus on the first row, β_1 , which captures the impact of the grant in no-loan villages. Households that received a grant in no-loan villages cultivated more land than those that did not (0.26 ha, se = 0.07). This indicates an 11.9% increase (control mean = 2.15) compared to non-grant recipient households in no-loan villages. Households also allocate their land to a

¹⁹The year-2 results (Appendix A7) are more difficult to interpret. In loan villages, a different set of households borrowed in year 2. Receiving a grant in year 1 leads to a modest but positive treatment effect on taking out a year-2 loan. Thus, the grant impact in year 2 in loan villages combines mechanisms and does not isolate selection.

²⁰When baseline value is missing, we instead code the lagged value as -9 and include an indicator for missing.

TABLE II
AGRICULTURE—YEAR 1.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Land cultivated (ha)	Land planted with rice and groundnut (ha)	Used plough (0/1)	Quantity seeds (Kg)	Family labor (days)	Hired labor (days)	Fertilizer and chemical expenses (USD)	Total input expenses (USD)	Value agricultural output (USD)	Gross Profit (USD)
Grant β_1	0.26 (0.07) [0.000]	0.09 (0.02) [0.000]	0.06 (0.01) [0.000]	7.32 (2.49) [0.004]	6.49 (4.55) [0.111]	3.22 (0.99) [0.000]	24.06 (6.85) [0.000]	34.39 (8.84) [0.000]	74.73 (21.46) [0.000]	42.77 (16.84) [0.002]
Grant * loan village β_2	-0.22 (0.11) [0.023]	0.01 (0.04) [0.875]	0.00 (0.02) [0.936]	0.85 (4.02) [0.820]	-5.85 (7.00) [0.348]	2.03 (1.60) [0.126]	-19.74 (9.47) [0.039]	-16.49 (12.81) [0.184]	-53.95 (30.11) [0.203]	-43.05 (23.18) [0.239]
p -value for $\beta_1 + \beta_2 = 0$	0.637	0.001	0.001	0.010	0.905	0.000	0.507	0.054	0.327	0.986
N	5393	5440	5393	5392	5393	5393	5440	5393	5392	5392
Mean of control (year 1)	2.15	0.90	0.80	91.16	140.54	18.02	125.64	196.24	526.74	330.51
SD of control (year 1)	2.38	0.78	0.40	83.51	140.99	25.39	221.74	275.56	660.14	475.35
Per \$100 impact for loan takers	0.77 (0.32)	0.05 (0.11)	0.05 (0.07)	2.95 (11.97)	20.35 (21.06)	-3.14 (4.77)	70.23 (29.19)	68.88 (39.05)	198.35 (92.45)	146.24 (71.48)

¹ Size of grant was \$140. Loan recipients are excluded from the analysis sample.

² Rows showing Grant + Grant * loan village = 0 show the p -value of the test of whether the total effect of grants in loan villages is statistically different from zero.

³ Standard errors are in parentheses and clustered at the village level in all specifications.

⁴ In brackets are randomization inference p -values following Young (2019). They are the randomization- c p -values from a two-tailed test of significance for each treatment effect. There are three independent families of outcomes: (i) agricultural inputs and crop choice in columns (1)–(7), (ii) total input expenses and value of output in columns (8)–(9), and (iii) gross profit in column (10). The RI p -values for joint Wald tests of significance of the treatment effects of the grant and its interaction with village type on each outcome individually are in brackets. The p -values for the omnibus test of the overall experimental significance for each family is as follows: $p < 0.001$; and $p = 0.029$. Appendix A5 discusses implementation details.

⁵ Total input expenses include fertilizer, manure, herbicide, insecticide, rental and maintenance costs of farming equipment, purchased seeds, and hired labor, but excludes the value of family labor. Gross profit is revenue minus 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 (last year's seeds valued at zero). Thus, this does not subtract value of own labor, value of family (i.e., any unpaid) labor, and the implicit rental value of land used.

⁶ Additional controls include: village fixed effects; the baseline value of the dependent variable; an indicator for whether the baseline value is missing; an indicator for the HH being administered the input survey in 2011, and household stratification controls (from baseline: whether the household was part of an extended family; was polygamous; an index of the household's agricultural assets; an index of household's other assets; per capita food consumption); and for the primary female respondent her baseline: land size, 0/1 on whether she used fertilizer in the previous agricultural season, and whether she had access to a plough).

⁷ Mean of control is the mean of the dependent variable in the column heading among households that received no grants in no-loan villages in year 1.

⁸ The per dollar return for loan takers is calculated as: $(\beta_1 - 0.79 * (\beta_1 + \beta_2)) / (0.21 * 140)$ where 0.21 is the loan take up rate and 140 is the value of the grant.

TABLE III
ADDITIONAL OUTCOMES OF GRANTS IN YEAR 1.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Own any livestock (0/1)	Total value of livestock (USD)	HH has a business (0/1)	Food consumption EQ (past 7 days, USD)	Monthly non-food exp (USD)	HH has any financial savings (0/1)	Educ expenses (USD)	Medical expenses (USD)	HH member migrated in past 12 mo (0/1)
Grant β_1	0.12 (0.01)	254.94 (93.67)	0.04 (0.02)	0.37 (0.15)	3.32 (1.54)	0.03 (0.02)	4.74 (3.49)	-3.93 (1.89)	-0.01 (0.02)
Grant * loan village β_2	-0.04 (0.02)	-143.17 (134.30)	0.00 (0.02)	0.12 (0.22)	0.87 (2.20)	0.03 (0.03)	-4.12 (5.61)	5.34 (2.79)	-0.04 (0.03)
p -value for $\beta_1 + \beta_2 = 0$	0.000	0.249	0.032	0.003	0.008	0.013	0.888	0.491	0.046
N	5264	5264	5263	5193	5157	5204	3638	5268	5280
Mean of control (year 1)	0.78	1341.12	0.83	6.01	44.94	0.63	72.34	36.64	0.59
SD (year 1)	(0.41)	(2413.69)	(0.37)	(3.41)	(40.86)	(0.48)	(88.43)	(52.53)	(0.49)
Per \$100 impact for loan takers	0.20 (0.07)	566.82 (410.14)	0.04 (0.07)	-0.07 (0.67)	0.04 (6.73)	-0.07 (0.08)	14.46 (16.73)	-17.17 (8.47)	0.09 (0.09)

¹ See the notes of Table II for details on specification and additional controls.

² The dependent variable in column (4) is weekly food consumption, per capita using adult equivalency scales. In column (5), the dependent variable is household monthly non-food expenditure. Education expenses and medical expenses are household annual expenses.

different crop mix: column (2) shows that 0.09 more hectares ($se = 0.02$) are dedicated to growing rice and groundnuts, local cash crops. The grant also induced increased plough use (6pp, $se = 1$), the quantity of seeds used (7.3 kg, $se = 2.5$), and hired labor days (3.22 days, $se = 0.99$). While 3.22 days over the entire agricultural season is small, these households use little hired labor: the mean in the control in 2011 was only 18 days. We observe no change in family labor. Fertilizer and other chemical inputs increased by 19% (US\$24, $se = 7$). The agricultural inputs and crop choice variables in columns (1)–(7) are grouped together as a family of outcomes for the randomization- c p -values (Young (2019)). The adjusted p -values yield qualitatively similar inference. Moreover, the omnibus test indicates a statistically significant ($p < 0.001$) experimental effect.

The grants led to an overall increase in agricultural investment: column (8) shows that measured input expenses increased by US\$34 ($se = 9$). Columns (9)–(10) report statistically significant and economically meaningful increases in output and gross profits: output increased by US\$75 ($se = 21$) and gross profits increased by US\$43 ($se = 17$), equivalent to 14% and 13% increases, respectively. Overall, we see statistically significant increases in investments and ultimately gross profits from relaxing capital constraints.

Critically, the coefficient on Grant * Loan village (β_2) demonstrates heterogeneity in the returns to the cash grant between households in no-loan villages and non-borrower households in loan villages. The β_2 coefficient shows that the selected sample of households who did not take out a loan do not experience the same positive returns when capital constraints are relaxed.

Column (1) shows that non-borrower households in loan villages did not increase the amount of land they cultivated when randomly selected to receive a grant ($\beta_2 = -0.22$ ha, $se = 0.11$ and the p -value of the test that the sum of β_1 and β_2 is zero is 0.64). The interaction terms for family labor and fertilizer/other chemical expenses are also negative (-5.9 days, $se = 7.0$ and $-US\$20$, $se = 9.5$, respectively). Non-borrower households who received grants in loan villages did seem to increase some inputs, such as quantity of seeds and hired labor, although neither is statistically significant as shown in columns (2)–(6). Column (8) shows that total input expenses among non-borrowers in loan villages increase in response to the grant by US\$18 (p -value = 0.05), which is not statistically different from the estimate in no-loan villages of US\$34. Note, however, that the inputs that are measured with the most precision—fertilizer and chemical expenses in column (7)—demonstrate a statistically significant difference in the impact of the grant on investment choices between loan and no-loan villages.

However, even though we observe increased inputs for the (non-borrower) grant recipients in loan villages, we see no corresponding increase in either agricultural output or in gross profits. The β_2 interaction coefficient for output is similar in magnitude to β_1 but *negative* ($-US\$54$, $se = 30$), offsetting the increase in output in no-loan villages (US\$75, $se = 21$). The test that the sum of the two coefficients is different from zero is not rejected ($p = 0.33$), indicating that the (intentionally) selected sample did not experience a statistically significant increase in output when given a grant. Similarly, the total effect on gross profits among non-borrowers in loan villages is essentially zero ($-US\$0.28$), which is not significantly different from zero ($p = 0.99$) and fairly precisely measured. Thus, households that did not take out loans used some of the grant to increase agricultural inputs; there is—in stark contrast to the random sample of households in no-loan villages—no evidence of average increases in either agricultural output or gross profits.

These point estimates imply important heterogeneity in marginal returns to relaxing capital constraints across farmers, and that those who borrow are disproportionately those

with high marginal returns. The return to the grant implied for would-be borrowers in no-loan villages is US\$146 (se = 71) in additional gross profits per US\$100 of grant.²¹ In contrast, the return for non-borrowers is close to zero.

The analysis indicates that non-borrowing households do not have high agricultural returns from 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), Ashraf, Berry, and Shapiro (2010), Tarozzi, Mahajan, Blackburn, Kopf, Krishnan, and Yoong (2014)), we find that the repayment liability leads to lower-return households being screened out. Appendix A4 (and Appendix Table VI) explores this in depth and demonstrates that we can predict neither the returns to the grants nor the heterogeneity in returns using baseline characteristics in no-loan villages. In the non-selected sample, high-return and low-return borrowers are nearly indistinguishable on observables. In Appendix A7, we examine the persistence of the effect of the grants in year 2 as well as at year 7.

4.2.2. *Other Outcomes*

Table III shows the estimates of equation (11) on non-agricultural outcomes. The most striking results are in columns (1) and (2): grant-recipient households in no-loan villages are more likely to own livestock (12 percentage points, se = 1), and there is a large (US\$255, se = 94), statistically significant increase in the value of total livestock compared to no-grant households. This represents a 19% increase in the value of household livestock and is slightly larger than the value of the grant itself. Recall we saw in Table II that households only spent part of the grant on input expenses. The livestock value is measured several months after harvest; these results could indicate that households moved some of their additional farming profits into livestock post-harvest, or they may reflect measurement challenges.²² We also see that the grant increased the likelihood in no-loan villages that a recipient household had a small enterprise (column (3); +4 percentage points, se = 2, control group mean = 0.83). Grant-recipient households also consumed more, including 6.2% more food (column (4); US\$0.37 per day per adult equivalent, se = 0.15, control group mean = 6.01) and 7.4% in non-food expenditures (column (5); US\$3.32 per month, se = 1.54, control group mean = 44.94). Columns (6)–(9) show no statistically significant main effect of the grant on whether the household has any financial savings, education expenses, medical expenses, or whether a household member has migrated.

The investment and spending patterns among grant-recipient (non-borrower) households in loan villages for the most part echo those described above in no-loan villages. Column (1) shows that while non-borrower grant recipients in loan villages were overall more likely to own livestock than their non-borrower, no-grant counterparts, the magnitude of the effect is smaller than in the no-loan villages (interaction term is -4 percentage points, se = 2). The remainder of the outcomes, however, show few differences.²³

²¹ Calculated as $(\beta_1 - 0.79(\beta_1 + \beta_2))/(0.21) * (100/140)$. The average return in the entire village is β_1 . The take-up rate of loans is 21%, so 79% of households in the village would be non-borrowers and would have earned a return of $(\beta_1 + \beta_2)$. The return is then scaled to be per US\$100, so we divide by the grant size of US\$140/100.

²² We may over-value recently purchased livestock. At the household level, we collected data on the quantity of animals, whereas we gather prices from village-level reports. Therefore, if recently purchased livestock are younger or smaller in treatment households, we would over-estimate the treatment effect.

²³ Medical expenses (column (8)) is the only outcome which suggests potential heterogeneity in behavior between loan and no-loan villages. Medical expenses (in the last 30 days) are marginally statistically signifi-

Taken together, Table III shows that the grants benefited households in a variety of ways. However, we have no strong evidence that non-borrower households in loan villages, who did not experience higher agricultural output and gross profits than in no-loan villages, used their grants to invest in alternative activities that offered higher returns than cultivation.

4.2.3. *Spillovers*

Households that received neither grants nor loans could have been *indirectly* affected. Such spillovers could be either positive (if grants or loans were shared; through positive general equilibrium effects via increased local economic activity; through positive psychological effects from positive social contagion effects on aspirations and investment decisions) or negative (through general equilibrium effects on locally determined prices or competition over land; through negative psychological effects of disappointment or social comparison). The key concern, however, is whether the patterns of spillovers are different in loan versus no-loan villages. Larger (if positive) spillovers in loan villages than in no-loan villages could generate a similar pattern to what we see in Table II. While we do not have experimental variation to estimate these spillovers, we provide evidence that strongly suggests that differential spillovers are not driving our main results. Appendix A6.1 provides more detail. Using data from an additional 69 villages in the same administrative units (*cercles*) as our study villages, we focus on two sets of outcomes: first, using the same outcomes as in Table II, plus prices, we compare households that did not receive a grant in no-loan villages to households in no-intervention villages. We see few differences, including no differences in prices. Second, we examine informal transfers and loans between peers. Comparing households in no-intervention villages with non-borrowers in the loan villages and households in no-loan villages, we find evidence of positive spillovers from grant recipients in no-loan villages to no-grant households, suggesting our estimates of the returns to the grant could be downwardly biased. However, non-borrowers in loan villages are not benefiting even more from such spillovers; if anything, spillovers may be weaker in loan villages.

5. EVIDENCE OF FRICTIONS IN THE ALLOCATION OF LOANS

If there is excess selection of poorer farmers out of borrowing, we expect to observe two empirical patterns. First, the gap between the distributions of observed gross profits of grant recipients and non-recipients will differ in no-loan villages from that among non-borrowers in loan villages. Second, the gap between the average returns to grants in no-loan and among non-borrowers in loan villages is positive, but this gap is attenuated at sufficiently low levels of observed baseline gross profits (or any other observable correlated with the friction generating the excess selection). If the excess selection is sufficiently strong, an observable characteristic that is positively correlated with average grant returns in the full population can be negatively correlated with average grant returns in the selected sample of non-borrowers in loan villages, a sign change that does not occur with frictionless selection.

cantly higher in loan-village grant households (US\$5.34, $se = 2.78$), since medical expenses may have declined (–US\$3.93, $se = 1.89$) among grant recipients in no-loan villages. The total effect in loan villages is not distinguishable from zero ($p = 0.49$). This is difficult to interpret because (i) having more resources could mean a household is more able to treat illnesses, but (ii) having more resources could lead to higher preventative care, which should lower total medical expenses.

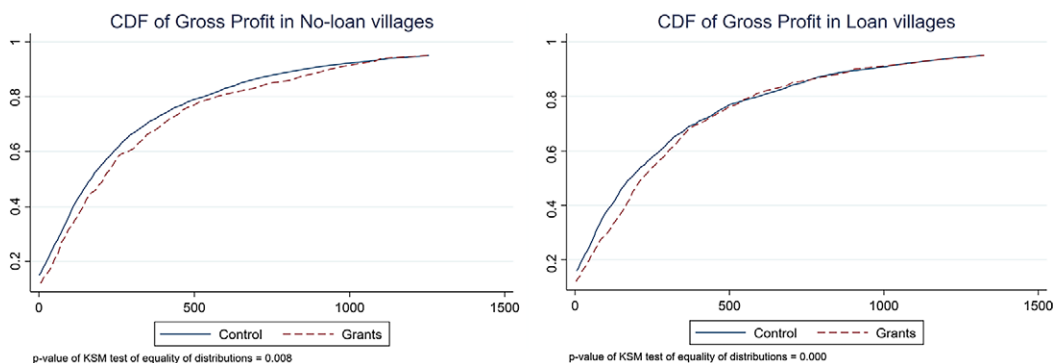


FIGURE 4.—CDF of gross profit.

First, the left panel of Figure 4 depicts the distributions of gross profits of grant recipients and non-recipients in no-loan villages. As anticipated from our preceding results, $F_G(Q^G)$ lies to the right of $F_{NG}(Q^{NG})$ over virtually the whole range. However, in the loan villages, grants were randomly allocated only within the selected sample of non-borrowers. In the right panel of Figure 4, above a certain relatively high level of gross profits ($> \$500$), grant recipients and non-recipients have identical profits.²⁴ Under the rank invariance assumption, these farmers have approximately zero marginal return from the grant. This pattern is broadly consistent with a frictionless allocation: farmers who have low returns to capital do not borrow and therefore show up in this sample. However, at lower levels of gross profits, $F_G(Q^G|C=0)$ lies to the right of $F_{NG}(Q^{NG}|C=0)$. These are non-borrowers with high returns to the grant but low gross profits. This feature corresponds to the exclusion of poor farmers who experience borrowing frictions. This suggests there are some potential borrowers with high-return projects who do not receive capital, highlighting excess selection.

Second, we analyze how observable characteristics of borrowers and non-borrowers are correlated with the return to grants. We saw in Table I that there are observable characteristics that are strongly correlated with loan take-up. Consider any such attribute, X^k , that we a priori expect might be negatively correlated with farmer-specific borrowing frictions. For example, baseline gross profits would be one such attribute. In Table IV, we report the results of estimating (11), which includes the interaction term $\text{Grant} * X^k * \text{Loan village}$. This additional interaction permits us to examine whether the correlation between X^k and the marginal return to the grant is different for the general population (γ_1) than for the selected population of non-borrowers ($\gamma_1 + \gamma_2$). The lower frictions associated with the higher value of X^k reduces the likelihood that the farmer has been screened out of borrowing by concerns of default or risk aversion. Non-borrowers with higher values of X^k are therefore more likely to have selected out of borrowing because they have low marginal productivity. Hence, among the population of non-borrowers in loan villages, higher values of X^k are associated with lower values of $\Delta_C Q$, relative to the association in the population in general.

Column (1) of Table IV examines the association between baseline gross profits and the marginal return to the grant in the overall population and in the selected sample of non-borrowers. In the overall population, there is no significant correlation between baseline

²⁴Note that this is the same sample as we use in Table II, and therefore continues to exclude households who borrowed in loan villages.

TABLE IV
HETEROGENEITY IN BORROWING FRICTION.

	Gross Profit				
	(1)	(2)	(3)	(4)	(5)
Grant	17.95 (23.58)	26.96 (19.44)	-18.71 (32.33)	14.31 (24.32)	41.95 (16.82)
Grant * Loan village	34.66 (30.39)	1.92 (27.35)	100.90 (46.57)	40.18 (32.84)	-40.02 (23.96)
Grant * Baseline gross profit (γ_1)	0.06 (0.05)				
Grant * Baseline gross profit * Loan village (γ_2)	-0.18 (0.07)				
Grant * Baseline livestock (γ_1)		0.010 (0.009)			
Grant * Baseline livestock * Loan village (γ_2)		-0.033 (0.014)			
Grant * Baseline food consumption (γ_1)			9.45 (5.42)		
Grant * Baseline food cons * Loan village (γ_2)			-21.94 (7.52)		
Grant * Baseline non-food expenditure (γ_1)				0.65 (0.42)	
Grant * Baseline non-food exp * Loan village (γ_2)				-2.00 (0.60)	
Grant * Baseline social integration index (γ_1)					-23.99 (14.63)
Grant * Baseline social index * Loan village (γ_2)					32.92 (24.33)
($\gamma_1 + \gamma_2$) coefficient	-0.12 (0.04)	-0.022 (0.011)	-12.49 (5.04)	-1.36 (0.43)	8.92 (19.43)
<i>N</i>	5392	5391	5294	5225	5391

¹See the notes of Table II for details on specification and additional controls.

gross profits and the return to grants. However, in accord with borrowing frictions that decline with baseline gross profits, households in loan villages have a statistically significantly negative correlation between baseline gross profits and the return to a grant (= -US\$0.12, se = 0.04). The negative correlation is evidence of excess selection.

In columns (2)–(4), we report the estimates of equation (11) for three additional characteristics of households that are positively correlated with a household's permanent income (and hence negatively with borrowing frictions): baseline value of livestock holdings, baseline food consumption per capita (in USD), and baseline non-food expenditure per capital (in USD). The point estimates for each show a positive correlation with returns to the grant in the overall population, although they are not statistically significant. And for each, the correlation is reversed for non-borrowers. For the non-borrowers, the returns to the grant are lower for those with more livestock (-US\$0.022, se = 0.011), higher food consumption (-US\$12.49, se = 5.04), and non-food consumption (-US\$1.36 se = 0.43). This sign change distinguishes excess selection from frictionless selection. In contrast, column (5) reveals that the index of social integration is not statistically significantly

correlated with returns to the grant. Nor is there a statistically significant difference in this correlation when we compare farmers in the no-loan villages to non-borrowing loan-village farmers. Thus, we do not find that our measure of social integration is correlated with borrowing frictions that generate excess selection.

We next estimate $E(\Delta_G Q | X = x_j)$, the predicted treatment effect (also known as the conditional average treatment effect or CATE) of a grant to a farmer with characteristics x_j using a causal forest trained on data from the no-loan villages. We also estimate $E(\Delta_G Q | X = x_i, c_i = 0)$, the predicted treatment effect of a grant for non-borrowing farmer i using the algorithm trained on non-borrowers in loan villages. In order to perform inference with these estimates, we follow the method by Chernozhukov, Demirer, Duflo, and Fernández-Val (2020) which is compatible with the causal forest algorithm we use to estimate heterogeneous treatment effects. In Appendix A6, we show that there is robust evidence of heterogeneity in grant treatment effects among the selected sample in loan villages, and little evidence of observable heterogeneity in no-loan villages. However, our empirical setting provides a second way to see if the model detects meaningful heterogeneity: as discussed in Section 4.1, we use the model estimated from the no-loan villages ($E(\Delta_G Q | X = x_j)$) to predict the CATES for borrowers and non-borrowers in loan villages. Table I shows that the predicted CATES are positively correlated with loan take-up.

Finally, we compare the CATES estimated in the no-loan villages to those estimated among non-borrowers in loan villages. Table V, column (1) shows that at baseline in the general population of no-loan villages, households with high CATES have *higher* baseline

TABLE V

CORRELATION OF CAUSAL FOREST PREDICTED TREATMENT EFFECTS WITH BASELINE CHARACTERISTICS.

	(1)	(2)
	No-loan villages model CATE	Non-borrowers in loan villages model CATE
Gross profit	0.0174 (0.0012)	-0.0609 (0.0020)
Food consumption EQ (past 7 days, USD)	3.18 (0.13)	-3.25 (0.25)
Monthly non-food exp (USD)	0.1629 (0.0120)	-0.3062 (0.0317)
Total value of livestock (USD)	0.0011 (0.0002)	-0.0065 (0.0005)
Social capital index	-4.48 (0.66)	-3.41 (0.97)
Land cultivated (ha)	3.60 (0.25)	-12.74 (0.65)
Value of agricultural assets owned	-0.0049 (0.0023)	-0.0181 (0.0098)
Total labor (days)	0.0505 (0.0035)	-0.1894 (0.0072)

¹ Each row reports the coefficients from two separate regressions of the predicted treatment effect generated by a causal forest algorithm on the subsample indicated in the column heading (and predicted only for the households in that subsample), on the baseline value of the covariate indicated in the row heading.

² Standard errors are in parentheses and clustered at the village level in all specifications.

gross profits, consumption, livestock and land holdings, and quantity of labor supplied. The pattern we see is that less poor households have higher treatment effects from grants.

As in equation (13), frictionless selection into borrowing implies that the response of the CATE to a change in any of the eight dimensions of X will be attenuated in the selected sample of non-borrowers relative to borrowers. However, only if there is excess selection, with the poorer households subject to higher borrowing frictions, can the correlation between these observables and the treatment effects of the grants turn negative in the selected sample in loan villages. Column (2) shows that in the causal forest, all of these correlations are statistically significantly negative in the selected sample. Among the selected (non-borrowers) sample in the loan villages, the less poor have lower returns. These are households that would be less likely to default, or to be less risk averse. This is consistent with Table I, where borrowers tended to be less poor than non-borrowers. The less poor households with expected high returns borrow, and left the sample that we used to train the model in the loan villages. Those that remain are the less poor households with low anticipated returns, and poorer households (many with high returns who do not borrow due to borrowing frictions), generating the negative correlations in column (2).

The exception to this pattern is the social integration index. There is no statistically significant difference in this correlation between farmers in the no-loan villages and non-borrowing farmers in the loan villages, and the point estimate of the correlation among the selected sample of non-borrowers is between zero and the estimate in the no-loan villages, so this provides no evidence that borrowing frictions are associated with our measure of social integration.

Figure 5 demonstrates visually that the sign changes reported in Table V are not artifacts of linearity. The vertical axis of each figure is the local linear regression estimate of the CATE of a grant; the horizontal axis is the (5th through 95th percentiles) of each of the eight baseline characteristics of households. As expected, CATES are lower among the non-borrowers in loan villages. In each case, excepting the measure of baseline social integration, we see a positive (or near-zero) relationship between the baseline measure of wealth and the estimated treatment effect of a grant in the no-loan villages. And in each case, we find a negative relationship between baseline wealth and the estimated treatment effect of a grant in the selected sample of non-borrowers in loan villages.

We observed in Table II that average agricultural returns to the grants for non-borrowers in loan villages are zero, while they are on average high for the random sample in no-loan villages. However, Figure 5 demonstrates that while agricultural returns to grants are uniformly higher in no-loan villages than for non-borrowers in loan villages, this gap is smaller for those with low baseline values of profits, food consumption, non-food consumption, livestock, farm size, or total labor. Indeed, Appendix Table VII shows that among non-borrowers in the first (i.e., poorest) tercile of the distribution of baseline food and non-food consumption, average returns to the grant are as high as the average returns in no-loan villages. Thus, among the poorest third, there are non-borrowing households with high returns to grants, implying frictions in the allocation of loans.

6. IMPACT OF THE LOANS

To validate the preceding analysis, we examine the average treatment effect of loans. If individuals with high returns to capital sort into borrowing, as we find implicitly from the grant experiment analyzed above, then we ought to also find a positive average treatment effect of the credit itself. Naturally, this estimate also has value for its own sake, as an impact evaluation of microcredit with cash flows designed for agriculture.

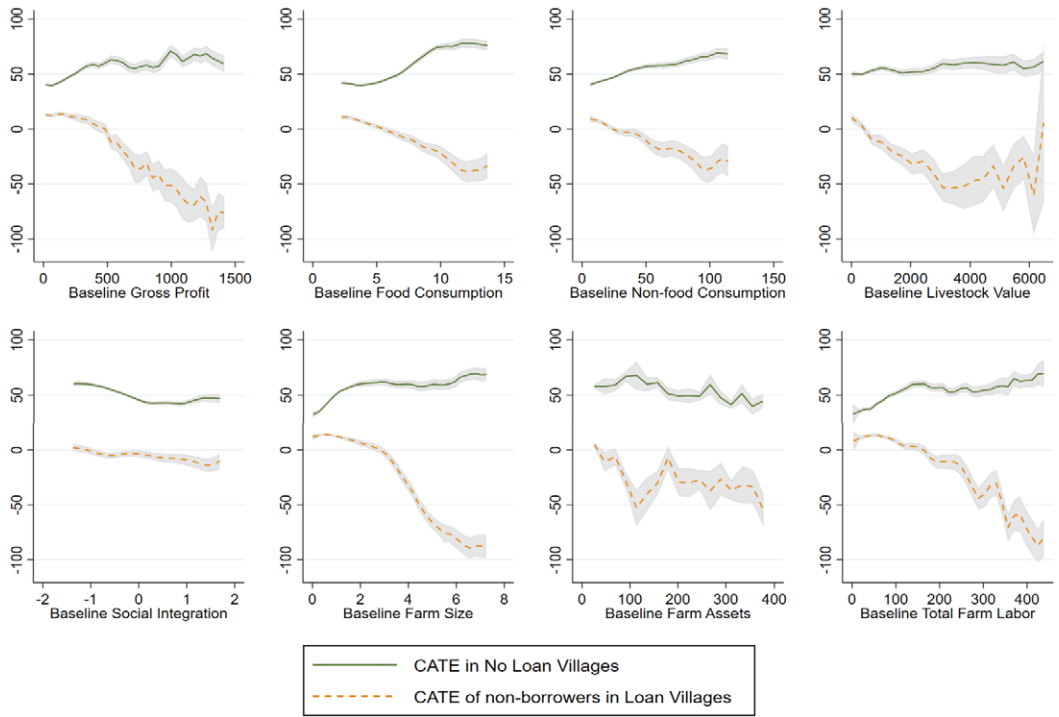


FIGURE 5.—Predicted treatment effects by baseline characteristics. *Notes.* Local linear reresion estimates using Epanechnikov kernel. Shaded region is pointwise 95 percent confidence interval.

Table VI (and Appendix Table VIII for secondary outcomes) presents the intent-to-treat (ITT) treatment effects of being offered an agricultural loan on the same set of outcomes already discussed in Section 4. Excluding all grant recipients from both loan and ineligible villages, we use the following specification:

$$Y_{it} = \alpha + \beta_1 \text{loan}_{v(i)} \cdot I\{t = 2011\} + \beta_2 \text{loan}_{v(i)} \cdot I\{t = 2012\} + X_{it} \pi + \epsilon_{it}, \quad (14)$$

where (X) includes the baseline value of the dependent variable y_0 , circle fixed effects, and the village stratification controls listed in the notes of Table II. The specification uses probability weights to account for the sampling strategy, which depends on take-up in the loan villages. See notes in Table VI for details.

For primary outcomes, we observe an increase in input expenditures on family labor days (7.3, $se = 5.1$) and in fertilizer and other chemicals expenses (US\$17, $se = 8$); total input expenses rose by US\$23 ($se = 10$) in villages offered loans. Land cultivated also increases but is not statistically significant (0.09 ha, $se = 0.06$). The value of the harvest rose by US\$36 ($se = 22$), but we do not find a statistically significant increase in gross profits (US\$17, $se = 18$).

Loans have to be repaid, while grants do not. Concerns about the costs of default or risk could deter borrowers from investing in the highest-return activities and lead loan recipients to use loans differently from the way in which they use grants and to realize different returns for loans than grants. The selection effect we have identified, in which women with high agricultural returns to grants are strongly selected into borrowing, may not imply that these same women have high agricultural returns to loans. We calculate

TABLE VI
AGRICULTURE ITT ESTIMATES FROM LOANS.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Land cultivated (ha)	Land planted with rice and groundnut (ha)	Used plough (0/1)	Quantity seeds (Kg)	Family labor (days)	Hired labor (days)	Fertilizer and chemical expenses (USD)	Total input expenses (USD)	Value agricultural output (USD)	Gross profit (USD)
Loan village—year 1	0.09 (0.06)	0.03 (0.03)	0.03 (0.02)	-0.66 (3.15)	7.32 (5.10)	-1.04 (1.06)	17.28 (7.92)	22.72 (9.82)	35.96 (21.96)	17.44 (18.11)
Loan village—year 2	0.07 (0.08)	0.01 (0.03)	0.02 (0.02)	-2.03 (3.45)	-0.05 (5.30)	-1.09 (1.16)	3.73 (10.31)	8.96 (13.33)	46.02 (28.67)	32.19 (19.72)
N	8805	8961	8848	8848	8848	8848	8961	8848	8804	8804
Mean of control (year 1)	2.15	0.90	0.80	91.16	140.54	18.02	125.55	196.10	526.17	330.16
SD (year 1)	(2.39)	(0.78)	(0.40)	(83.51)	(140.99)	(25.41)	(221.28)	(274.84)	(657.32)	(473.62)
Per \$100 impact, TOT, year 1	0.36 (0.26)	0.14 (0.13)	0.12 (0.07)	-2.80 (13.26)	30.85 (21.48)	-4.38 (4.48)	72.80 (33.39)	95.75 (41.37)	151.54 (92.54)	73.50 (76.30)
Diff in per \$100 impact: Grants—Loans	0.41	-0.10	-0.08	5.75	-10.50	1.24	-2.57	-26.87	46.81	72.74
SE from bootstrap on difference	(0.31)	(0.11)	(0.06)	(11.50)	(19.82)	(4.29)	(31.07)	(37.74)	(109.55)	(88.60)

¹ Grant recipients in both loan and no-loan villages are removed from the analysis sample. Probability weights are applied to account for the differences in the sampling probabilities in loan villages, which are a function of loan take-up. The probability weights of non-borrowers in loan villages are calculated as [(# of non-borrowers in sample in a loan village)/(# of these households who did not receive grant)], and are 1 for all other households in the sample.

² Total input expenses is the same variable as defined in Table II.

³ Additional controls include: circle 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.

⁴ Standard errors are in parentheses and clustered at the village level in all specifications.

⁵ Mean of control is the mean of the dependent variable in the column heading among households in no-loan villages in year 1.

⁶ The per-dollar return, TOT, year 1 is: the coefficient on Loan village—year 1/(0.21 * 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. Probability weights were calculated in each bootstrap sample and used in the estimate of the loan impact.

the Treatment on the Treated estimates for year 1 for the subpopulation who take up loans.²⁵ We compare this estimate to our estimate of the impact of the grant in no-loan villages on those who would borrow from the final row of Table II and we cannot reject the hypothesis that grants and loans treatment effects are the same (proportionally to dollar amount) for any of the agricultural outcomes.²⁶ Taken as a whole, the grants and loans are having similar effects on agricultural inputs and outcomes for those who would borrow.²⁷

Appendix Table VIII demonstrates that overall, the microcredit agricultural loans did not have broad or consistent impacts beyond agriculture. We do not detect an impact on outcomes such as non-food consumption, whether the household has a small business, savings, or educational expenses.²⁸ We observe a large but imprecisely estimated impact on livestock in year 2 but not year 1 (column 2). We do find a statistically significant *reduction* in food consumption and medical expenses in year 1 but not year 2 (columns (4) and (8)).

These results on the impact of loans stand in stark contrast to the recent experimental literature on the impact of entrepreneurially focused credit (see Angelucci, Karlan, and Zinman (2015), Attanasio et al. (2015), Augsburg et al. (2015), Banerjee 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); in contrast, Breza and Kinnan (2021) found noticeable general equilibrium effects as a consequence of a state-wide shut-down of the microcredit market). Analysis pooling these studies using a Bayesian hierarchical model, however, uncovers evidence of positive treatment effect at higher quantiles, even though the average treatment effect is a fairly precise null (Meager (2019, 2020)). An earlier agricultural lending literature also documented institutional failures, typically with high default rates (Adams, Graham, and Von Pischke (2022), Adams (1971)).

The impact estimates are also promising from the perspective of the microcredit institution: repayment was 100%, and the retention to the following year (65%) is on par with typical client retention rates for sustainable, entrepreneurially focused microcredit operations.

²⁵See Table VI notes. Interest charges and fees, plus the cost of the 10 percent deposit requirement, imply that a \$100 loan must generate \$131 in additional revenue to be profitable. We find that \$92 (se = 37) of each \$100 loaned is used for farm expenses, generating additional farm output valued at \$157 (se = 80). The remaining \$11 of the loan proceeds are likely invested in livestock (see Appendix Table VIII), which appears to generate an even higher return. These ToT estimates are noisy, but consistent with the high estimated returns to grants estimated for borrowers.

²⁶The standard errors are calculated using a bootstrap routine: the difference in the impact of the grant and loan is estimated for 1000 draws of households (with replacement), with probability weights for households calculated in each bootstrap sample for the loan impact estimation.

²⁷We do not remove the cost of the loan, that is, interest payments, from gross profits. The true difference in take-home profits between the grant and loan would be larger. We do this because the goal is to see if the behavior of farmers, in terms of investments and the associated agricultural output, differs between the grants and the loans. We see that there is no evidence that the fact that they must pay interest leads to different investment choices.

²⁸Columns (9)–(11) of Appendix Table VIII further show no detectable effect on women's decision-making power within the household, women's involvement in community decisions, or women's social capital. This is similar to the existing evaluations of microcredit (finding no impact on these measures: Attanasio et al. (2015), Augsburg et al. (2015), Banerjee et al. (2015), Crépon et al. (2015); one exception is Angelucci, Karlan, and Zinman (2015)). Soro Yiriwaso did not have any explicit component of the program emphasizing women's empowerment.

7. CONCLUSION

We find that the returns to investment in cultivation are heterogeneous and that higher-marginal-return farmers take up agricultural microfinance loans more than low-marginal-return farmers. But there is also a set of high-marginal-return, extremely poor households that are unable to borrow. This has important implications for models of rural markets, as well as social policy that aims to relax liquidity constraints for the most vulnerable. In particular, our results provide rigorous empirical evidence for systematic selection into contracts, which is embedded in several models (e.g., Evans and Jovanovic (1989), Moll (2014)) but which has lacked clear empirical evidence. As recognized by Banerjee, Breza, Duflo, and Kinnan (2021) and Kaboski and Townsend (2011), our results highlight the need to incorporate heterogeneity of returns in credit market models.

In Southern Mali, agricultural lending with balloon payments (i.e., with cash flows matched to those of the intended productive activity) can increase investments in agriculture and generate high returns. This is broadly consistent with other work showing that there is high demand for financial products, either credit or savings, which provide lump sums that are otherwise difficult to accumulate (Afzal, d'Adda, Fafchamps, Quinn, and Said (2018)). 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 ill-suited to agriculture. Given the lack-luster average estimated impact of entrepreneurial microcredit from marginal increases in access (Banerjee, Karlan, and Zinman (2015), Meager (2019)), our results suggest a path for microcredit lenders looking to shift their model towards a product that generates higher average returns for borrowers without increasing default. Further experimentation would be fruitful to demarcate the external validity of our results: to test, for example, whether each of the three changes from the more “normal” microcredit model (group liability, agricultural focus, balloon repayment) was necessary.

These results are also important for policy analysis and program evaluation. The random choice of communities into which to enter by the lender enables 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, for example, a quasi-experimental approach, to assess the impact of lending to farmers, we would have over-estimated 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.

Specifically, the results have important implications for expansion policies for lenders in low-income countries. Efforts to expand access to credit by pushing out loans to more borrowers in a given community, holding all else constant (e.g., training, terms of credit, etc.), may not only fail to generate higher income for marginal borrowers, but also be unprofitable. Thus, for example, incentives to credit officers to lend to more people within a fixed set of communities may not be good for business or policy.

We also believe this two-stage experimental design has promise for similar inquiries in other markets. Two-stage designs similar to ours have examined treatment effects conditional on willingness-to-pay (e.g., Berry, Fischer, and Guiteras (2020) for clean water; Cohen and Dupas (2010) for insecticide-treated bednets; and Karlan and Zinman (2009) for consumer credit), but this line of inquiry is still uncommon, and particularly uncommon for large programs and services. For example, many multi-faceted social protection programs transfer productive assets to low-income households with the aim of helping

households start income-generating livelihoods. Often such programs provide a set of choices for the household. Correlational analysis that notes one livelihood being more profitable than another could lead implementers to reduce the choice set, whereas the right answer was that the optimal matching was not uniform across households.

More broadly, the design and results also speak to some of the challenges in the evidence-to-policy nexus. If an evaluation yields promising estimates for the treatment-on-the-treated effect of a product or service (such as a loan, in this case), the implementing entity and funders may naturally want to then scale that product or service. While some scaling is horizontal, that is, by going to new geographies, often scaling implies deepening outreach within existing coverage areas. If the treatment effects for those who initially take-up are substantially different than the treatment effects for others, expansion via deepening outreach may be misguided. Thus, learning more from evaluations about treatment effects conditional on various methods of selection could provide critical information for forming optimal policy.

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