Financing Smallholder Agriculture: An Experiment with Agent-Intermediated Microloans in India *

Pushkar Maitra,† Sandip Mitra,‡ Dilip Mookherjee,§
Alberto Motta¶ and Sujata Visaria‖

July 2015

Abstract

Recent evaluations have found that traditional microloans achieve high repayment rates but have insignificant impacts on incomes and output. We examine if this is because MFIs are unable to identify and exclude unproductive borrowers. We use a randomized field experiment in West Bengal to study the consequences of delegating selection of borrowers for individual liability loans to local trader-lender agents, who are incentivized by repayment-based commissions. We develop a theoretical model of differences in selection and production incentives between this variant (called TRAIL) and standard group loans (called GBL). Consistent with model predictions, TRAIL borrowers had lower average default risk and achieved higher rates of return on their loans. TRAIL loans increased production of the leading cash crop and farm incomes by 27–37%, but GBL loans had insignificant effects. Repayment rates were equally high in both schemes, but TRAIL had higher take-up rates and lower administrative costs.

Key words: Agricultural Finance, Agent Based Lending, Group Lending, Selection, Repayment

JEL Codes: D82, O16

---

*Funding was provided by the Australian Agency for International Development, the International Growth Centre, United States Agency for International Development and the Hong Kong Research Grants Council. We are grateful to Shree Sanchari for collaborating on the project. Jingyan Gao, Clarence Lee, Daijing Lv, Foez Mojumder, Moumita Poddar and Nina Yeung provided exceptional research assistance and Elizabeth Kwok provided excellent administrative support. Boston University Masters students Torry Ah-Tye, Ou Bai, Juan Blanco, Chantel Pfeiffer and Stefan Winata conducted useful analysis and provided insights from a field study of relations between agents and borrowers in the study. We thank Xavier Gine, Albert Park, Russell Toth, Bruce Wydick and a large number of seminar and conference participants for helpful comments on previous and related versions. Internal review board clearance was received from Monash University, Boston University and the Hong Kong University of Science and Technology. The authors are responsible for all errors.

†Pushkar Maitra, Department of Economics, Monash University, Clayton Campus, VIC 3800, Australia. Pushkar.Maitra@monash.edu.

‡Sandip Mitra, Sampling and Official Statistics Unit, Indian Statistical Institute, 203 B.T. Road, Kolkata 700108, India. Sandip@isical.ac.in.

§Dilip Mookherjee, Department of Economics, Boston University, 270 Bay State Road, Boston, MA 02215, USA. dilipm@bu.edu.

¶Alberto Motta, School of Economics, University of New South Wales, NSW 2052, Australia. motta@unsw.edu.au.

‖Sujata Visaria, Department of Economics, Lee Shau Kee Business Building, Hong Kong University of Science and Technology, Clear Water Bay, Hong Kong. svisaria@ust.hk.

1 Introduction

Microcredit was famously heralded as a solution to global poverty; yet experiments evaluating it in a range of developing and middle income countries have not found evidence of significant impacts on borrower incomes or production (see Kaboski and Townsend, 2011, Banerjee, Karlan, and Zinman, 2015). This is particularly true in the case of traditional group-based lending schemes, where financial institutions devolve selection, monitoring and enforcement to borrower groups. Variants involving individual liability loans also have not achieved significant production or income impacts. (Giné and Karlan, 2014, Attanasio, Augsburg, Haas, Fitzsimons, and Harmgart, 2015). Other experiments have relaxed the rigid repayment schedules that restrict borrowers' flexibility in project choice. Although more flexible microloans have been found to increase farm activity or business incomes, they also appear to increase default rates (Field, Pande, Papp, and Rigol, 2013, Feigenberg, Field, and Pande, 2013, Beaman, Karlan, Thuysbaert, and Udry, 2015).[1] Hence no interventions so far have achieved significant increases in borrower productivity and incomes, while maintaining repayment rates and administrative costs on par with traditional microcredit. The reasons for this are not well understood. Consequently, it is not known whether there might exist alternative variants which are more likely to be successful.

In this paper we explore the hypothesis that one weakness of traditional microfinance schemes may be their inability to identify and exclude unproductive borrowers. Specifically, peer monitoring and selection mechanisms can encourage high repayment rates, but do not ensure that high productivity borrowers will form groups and apply for loans. One reason for this is that there is no necessary connection between default risk and productivity: mutual insurance and peer pressure inherent in joint liability groups can allow groups of low productivity borrowers to achieve high repayment rates. As Ghatak (1999, 2000) argue, joint liability encourages assortative matching so that borrowers with similar risk profiles group together. However Ghatak (2000)'s arguments for the lack of adverse selection in joint liability schemes rely on the assumption that the lender offers a menu of contracts, which are designed to ensure that high-risk borrowers apply for individual liability loans while low-risk borrowers form groups and apply for joint liability loans. In reality it is rare for microfinance institutions (MFIs) to lend in such a sophisticated manner; instead they typically offer a single loan option at an interest rate below the informal market rate.[2] High-productivity as well as low-productivity borrowers are motivated to apply for such loans. In fact, low-productivity borrowers who pay high interest rates in informal credit markets especially stand to gain from the lower interest rates that MFIs charge. In the

[1] Field, Pande, Papp, and Rigol (2013) find that a longer grace period for repaying individual liability microloans increased weekly business profits by 41% and household incomes by almost 20%, but also tripled the default rate. Beaman, Karlan, Thuysbaert, and Udry (2015) find that when loans were repaid in a single lumpsum at harvest time, borrowers increased cultivation and value of agricultural output, although not farm profits.

[2] While in recent years microfinance institutions have started offering both joint liability and individual liability loans in the same village, new clients are not usually presented with a choice of contracts. Instead MFIs tend to offer the individual liability contracts to "graduated" clients, who have proven their creditworthiness by repaying their joint liability loans in the past.

absence of any additional screening mechanism, the MFI is unable to ensure that it lends only to groups of productive borrowers. In any case, as long as the loans are repaid, it has no particular incentive to do so.

Variants that remove group-based selection may continue to be similarly handicapped. In experiments with individual liability loans, MFIs have relied either on collateral requirements (which limit access of the poor), or on screening by loan officers who are not members of the local community. Most new applicants have traditionally relied entirely on informal loans from local lenders, and MFI loan officers do not have access to their credit history on those loans. These considerations suggest that an alternative approach where MFIs select borrowers on the advice of informal lenders experienced at lending within the local community, could be valuable. This paper experiments with such an alternative called TRAIL (Trader Agent Intermediated Loans), in which an MFI delegates borrower selection to an agent randomly chosen from informal trader/lenders in the community. The agent is incentivized by commissions that depend on interest payments by recommended clients. We use a theoretical model to argue that such commissions can motivate the agent to recommend low-risk, high-productivity borrowers. This obviates the need for other screening and monitoring devices such as joint liability, monitoring by peers or MFI officials.

We design and conduct an experiment with poor farmers in two districts of the Indian state of West Bengal. The experiment compares TRAIL with a traditional group-based approach that uses joint liability loans (Group-Based Liability, or GBL) and group-based selection. Since local informal lenders have experience only in extending individual liability loans within the community, it is natural to design TRAIL to offer individual liability loans. This implies that compared to traditional group-based joint liability loans, TRAIL differs along two important dimensions (selection and liability) simultaneously. We hypothesize that changes on both dimensions will encourage TRAIL borrowers to increase production and earn higher incomes, since individual liability loans avoid the "joint liability tax" (JLT). This tax arises in group liability loans because a successful borrower is liable to repay on behalf of other group members whose projects fail. This lowers each borrower's personal stake in ensuring the success of their projects. In turn, to minimize this tax, group members prevent each other from investing in high-value high-risk projects (Fischer, 2013). This is another possible cause for the lack of production impacts of traditional group loans. Therefore we expect that the TRAIL scheme would generate larger impacts on production than the GBL scheme would, because TRAIL induces the selection of more productive borrowers, and also provides them stronger incentives to adopt high-value high-risk projects and exert effort in their execution.

From an experimental point of view, it is typically preferable to compare variants which differ on a single dimension. In our context this would have involved using either a variant of TRAIL where the local agent would have selected or formed borrower groups who would receive joint liability loans, or a variant of GBL where groups would have played a role in selection but members would have received individual liability loans thereafter. Either variant would seriously compromise the selection attributes of the respective polar version. TRAIL involves agents have no experience in screening or lending to groups, even less so in forming borrower groups. Conversely, our partner MFI had no experience with offering individual liability loans. Moreover, the selection incentives and peer pressure inherent in joint liability would disappear if group members were no longer liable for each others' loans. Hence agent-based selection and group-based selection are naturally bundled with individual and joint liability loans respectively. Budgetary and logistical restrictions on the scale of the experiment limited our choice of the treatments to the polar variants of TRAIL and GBL, each administered in 24 randomly chosen villages in two districts of West Bengal. We believe this is a necessary first step in the research program: if the results of the TRAIL scheme are encouraging, this could motivate subsequent experimentation with hybrids of TRAIL and GBL in order to disentangle selection and incentive channels.

Apart from the selection mechanism and the loan liability, the two schemes were identical in all other loan features. They were designed to facilitate the cultivation of potatoes, the leading cash crop in the state. Loans were given out during the potato planting season. Repayments were due in a single lumpsum four months later, when potatoes were harvested. To provide insurance against local covariate risks, the repayment due was adjusted downward if revenues per acre (product of potato harvest price and yield per acre) in the village fell 25 percent below a 3-year moving average. In order to provide each borrower with strong repayment incentives, the size of the credit limit in the next four month cycle was set at 133% of the amount of the current loan repaid. Hence those who repaid satisfactorily were eligible for a larger loan that could be used to store potatoes or plant other crops. Those who repaid less than 50% of their current loan obligation were terminated. The interest rate was 18%, 8 percentage points below the prevailing market rate for non-collateralized informal loans of a similar duration. Thus the loss of future access to these loans would be costly, which was expected to incentivize borrowers to repay the loan.

In each TRAIL villages the lender appointed an agent as its intermediary. The agent was randomly selected from a list of established trader-lenders, and asked to recommend as potential borrowers 30 village residents owning no more than 1.5 acres of land. The experiment was designed to allow us to identify loan treatment effects while controlling for selection of borrowers into the schemes: hence, a random subset of these recommended households were offered the TRAIL loans. The agent was paid a commission equal to 75 percent of the interest payments received at the end of each four month cycle. The agent incurred limited penalties for borrower defaults, via termination clauses and modest deposits posted upfront which would be forfeited if repayment rates fell below a threshold. MFI officials then lent directly to recommended clients at pre-set terms, made no effort to monitor borrowers, and remained responsible for loan recovery. The TRAIL agent was not assigned responsibility for extending the loans or recovering them, thereby promoting their participation incentives. Nevertheless, the scheme could provide the agent with incentives "behind the scenes" to remind borrowers to repay, or help or exert pressure on them to ensure repayment.

For the GBL scheme, the lender appointed Shree Sanchari, a Kolkata-based MFI as its intermediary. In each GBL village, the MFI introduced the group-based lending scheme, featuring the same interest rate, loan duration, growth in credit access and covariate risk insurance as the TRAIL loans. Village residents were invited to form 5-member groups, attend monthly meetings with loan officers and make mandated savings deposits over a six-month period before the loan scheme began. A random subset of the groups that completed the six-month initiation process were offered GBL loans. The MFI organized monthly group meetings and collected savings deposits from members throughout the loan cycle. Analogous to TRAIL, the MFI also received a commission equal to 75 percent of the interest payments received from GBL borrowers.

The experiment ran for three years from 2010 to 2013, allowing us to examine both short and intermediate term impacts of the program. We conducted household surveys every four months, allowing us to collect high-quality high-frequency data about sample households' cultivation decisions, harvest and sales of all crops they grew, and ensuring complete absence of any attrition. Further details of the context and experimental design are provided in Section 2.

Section 3 presents a theoretical model of predicted differences between outcomes of TRAIL and GBL. The model assumes the informal credit market is segmented into a set of networks, each of which includes informal lenders and "connected" borrowers engaged in tight-knit economic and social relationships. The market also has a set of unconnected "floating" borrowers. Connected borrowers are more productive than floating borrowers because network members exchange information and help each other. Lenders are better able to enforce repayment by borrowers within their network, so connected borrowers are more likely to repay loans from own-network lenders. Floaters have a higher default risk, and subsequently pay higher interest rates in the informal credit market. In the TRAIL scheme, an informal lender from one of the networks is chosen as the agent. The repayment-based commissions create a strong incentive for the agent to recommend own-network borrowers instead of borrowers outside his network, since he can enforce their repayment better. In contrast, GBL borrowers are assortatively matched in groups, but both connected and unconnected borrowers are incentivized to form groups and there is no screening mechanism to exclude particular groups. As a result, the borrowers in the TRAIL scheme are more likely to be productive than in the GBL scheme. However, the joint liability in GBL means that group members cover each others' liabilities, which can compensate for the higher default risk of floating borrowers and thus generate high repayment rates in the GBL scheme as well.

The model predicts: (1) TRAIL agents will select borrowers of lower average default risk than those who participate in the GBL scheme, mainly because they are inclined to recommend borrowers from their own network. Hence TRAIL-selected borrowers will pay lower interest rates on average on the informal market; (2) Because the TRAIL agents selects more productive borrowers, who also have stronger incentives in the absence of the JLT, TRAIL loans will have larger impacts on output, value added and farmer incomes and a higher rate of return; (3) Either scheme may have higher repayment rates; (4) TRAIL loans will have greater take-up and lower administrative costs because no group meetings are required.

We use our survey and loan record data to test these predictions. In Section 4 we use data from sample households' prior credit and other transactions to test prediction 1. We find that the TRAIL agent was significantly more likely to recommend borrowers from his own clientele and social group. Within this category, he also recommended the safer borrowers: before the scheme began, households recommended from within the TRAIL agent's network had paid informal interest rates that were 5.2 percentage points lower than borrowers he did not recommend. He also recommended some out-of-network borrowers, so that the average informal interest rate among TRAIL recommended borrowers overall was close to the village average. However, GBL participant households paid 3.7 percentage points higher interest rates than non-participants, so that overall, selected households in the GBL scheme had paid a statistically significant 4.7 percentage points higher interest rates than households recommended by the TRAIL agents.

Section 5 discusses tests of the other predictions. Averaging over the three years, we find that the TRAIL loans caused borrowers to earn 37% higher value added in potato production and 26% higher value added across all major crops. These effects were statistically significant at the 1% level. In contrast, GBL loans had insignificant effects. We also find evidence that borrowers recommended by the TRAIL agent were more productive than those who participated in the GBL scheme. The point estimate of the implied annualized rates of return on the expansion in potato cultivation costs financed by the loans was 108% for TRAIL borrowers, but only 26% for GBL borrowers. The difference between the two was statistically significant at the 1% level.

Repayment rates were an equally high 95% over the 3 years in both the TRAIL and GBL schemes, but loan take-up rates were significantly higher for TRAIL loans.[3] We also find no evidence that TRAIL agents siphoned off the benefits of treated borrowers by manipulating the terms of other trading relationships with them. Finally, the costs of administering the TRAIL scheme were lower than those of the GBL scheme. Commission rates for both the TRAIL agents and the MFI that implemented the GBL scheme were the same, but the MFI's loan officers incurred substantial costs on high-frequency meetings with borrowers in the GBL scheme, which did not occur in the TRAIL scheme. Section 6 discusses robustness checks, sensitivity analyses and the financial sustainability of the schemes.

Our estimated loan impacts therefore contrast with the small and insignificant effects obtained in recent experimental evaluations of the more standard group liability loans, or individual liability loans where MFIs screen or require collateral from borrowers.[4] Our results on selection echo Beaman, Karlan, Thuysbaert, and Udry (2015)'s finding that community associations in Mali prevented low-productivity households from taking joint liability loans. The involvement of third-party community leaders in their setting is analo-

[3]Loan records show that 92% of households that were offered TRAIL loans took the loan in the first four-month cycle of the scheme. At the end of three years, the take-up rate was 62%. In GBL the take-up rate was 88% to begin with, and fell to 49% by the end of the third year.

[4]See Banerjee, Karlan, and Zinman (2015), Angelucci, Karlan, and Zinman (2015), Attanasio, Augsburg, Haas, Fitzsimons, and Harmgart (2015), Augsburg, Haas, Harmgart, and Meghir (2015), Banerjee, Duflo, Glennerster, and Kinnan (2015), Crépon, Devoto, Duflo, and Parienté (2015), Tarozzi, Desai, and Johnson (2015).

gous to the role of TRAIL agents in screening out less productive loan applicants.

A few qualifications are in order. The scale of our intervention was small in comparison with most other microcredit experiments, since only ten loans were offered in each village. It is not possible to project these results to estimate likely consequences of a larger scale intervention. The small scale of our interventions motivate our assumption of absence of spillover effects on non-beneficiaries in the experimental villages. Also, our analysis is restricted to impacts on production and incomes; we do not examine impacts on consumption, investment or empowerment. We also do not examine distributive impacts across land ownership, caste or gender lines. Finally, as stated earlier, we cannot disentangle the role of selection from other possible channels that may have accounted for the differential performance of the two schemes, such as borrower incentives (individual liability in TRAIL versus the joint liability tax in GBL), control or social capital (vertical links in TRAIL versus horizontal links in GBL). These issues remain to be investigated in future research.

2 Experimental Design and Data

Our field experiment was conducted in the districts of Hugli and West Medinipur in the state of West Bengal, India. These districts are among the largest producers of potatoes in West Bengal, which itself produces about a third of all the potato output in India. The TRAIL scheme was implemented in 24 randomly selected villages, by appointing one agent per village who then recommended potential borrowers for the loans. In another 24 villages, an MFI implemented the GBL scheme.[5] Each sample village was at least 10 kilometers away from all other sample villages, to help minimize contamination of the experimental interventions through the spread of information. The MFI had not operated in any of the sample villages before our project started, and in general MFI penetration was low.

In both schemes, borrowers were offered multiple cycles of loans of 4-month durations at an annual interest rate of 18%. The first loans were capped at ₹2000 (equivalent to approximately $US40 at the prevailing exchange rate), and were disbursed in October-November 2010, to coincide with the potato-planting season. Repayment was due in a single lumpsum after 4 months. In each subsequent cycle borrowers who repaid the full amount became eligible for a 33 percent larger loan, with all other loan terms remaining unchanged. Those who repaid less than 50 percent of the repayment due were not allowed to borrow again. Others were eligible to borrow 133 percent of the principal repaid.[6] Both schemes had an in-built index insurance scheme, so that the required repayment would be

[5] In yet another 24 villages, an alternative version of the agent intermediated lending scheme (called GRAIL) was implemented, where a member of the village council (*Gram Panchayat*) was appointed as the agent. The GRAIL agent is likely to have been motivated by the political benefits of participating in the scheme, and so that treatment will be analysed in a separate paper.

[6] To facilitate credit access for post-harvest storage, borrowers were allowed to repay the loan in the form of cold storage receipts (or "bonds") instead of cash. In that case the repayment was calculated at the prevailing price of potato bonds.

revised downwards if the revenue per acre for potatoes fell 25 percent below a three year average in the village, as assessed through a separate village survey.

Table 1 summarizes the differences between our intervention and other related microcredit interventions recently studied in the literature (see the summary presented in Banerjee, Karlan, and Zinman, 2015, Table 1). Apart from TRAIL borrower selection procedures, an important difference is in repayment frequency: loans were due in a single lumpsum at the end of 4 months in both TRAIL and GBL schemes, whereas repayment was due on weekly, bi-monthly or monthly schedules in the other studies. Many of the other loan features are comparable between TRAIL and GBL, and other microcredit products.

However, one important difference is in the scale of the program. We rationed loan offers to 10 eligible borrowers in each village. In contrast, in most evaluations in the literature, the scale of the program was determined by the demand for the loan product: sampling units (slums or villages) were randomly allocated to either the treatment or control groups and the MFI only operated in the treatment clusters. The impacts estimated in those studies combine selection and loan treatment effects, and are interpreted as the effects of MFI entry on a representative member of the "eligible" sub-population within that sampling unit, where loan take-up within the sub-population is entirely demand-determined. In our study, loan treatment effects are estimated controlling for selection (or "eligibility") into the scheme (either by recommendation by a TRAIL agent or by participation in a GBL group). This is done by measuring differences in outcomes between those randomly chosen to receive a loan offer, and those who were "eligible" but unlucky in the lottery and did not receive the loan offer. This is similar to the analysis of loan treatment effects in Karlan and Zinman (2011) which also controlled for selection, with loan assignment randomized among borrowers deemed marginally creditworthy by a credit scoring algorithm.

2.1 The Trader-Agent-Intermediated Lending (TRAIL) Scheme

In the TRAIL villages, the lender consulted with prominent persons in the village to draw up a list of traders and business people who had at least 50 clients in the village, and had been in business in the village for at least three years. One person from the list was randomly chosen and invited to become an agent.[7] The agent was asked to recommend as potential borrowers 30 village residents who owned no more than 1.5 acres of agricultural land. Our project officer conducted a lottery in the presence of village leaders to select 10 out of these 30 individuals, who were then offered the loan. Loan officers visited these randomly chosen individuals in their homes to explain the loan terms and disburse the loan if they accepted the offer.

At the beginning of Cycle 1, the agent was required to put down a deposit of ₹50 per

[7]The experimental protocol stated that if the person approached rejected the offer, the position would be offered to another randomly chosen person from the list. In practice, the first person offered the position accepted it in every village.

borrower. The deposit was refunded to the agent at the end of two years, in proportion to the loan repayment rates of his recommended borrowers. At the end of each loan cycle he received as commission 75% of the interest received on these loans. The agent's contract was terminated at the end of any cycle in which 50% of borrowers he recommended failed to repay. Agents were also promised an expenses-paid holiday at a local sea-side resort if they survived in the program for two years.

Interactions between loan officers and borrowers were limited to loan disbursement and collections at the beginning and end of each cycle, which occurred at the borrowers' residences. Loan officers were not required to engage in any monitoring or collection effort, and borrowers were not required to report to the loan officers their intended or actual use of the loan.[8]

A potential concern with TRAIL is whether agents could abuse their power by extracting rents from borrowers. For instance, they might have tried to charge high interest rates (through kickbacks), selected unsuitable borrowers (high default risks, less productive individuals, wealthy individuals, cronies or persons willing to pay bribes), extracted borrower benefits by manipulating other relationships, colluded with borrowers (recommended non-repayment and divided up loan funds) or coerced borrowers to repay. The scheme contained several features to help guard against these possibilities. All loan transactions took place directly between loan officers and the borrower. The agent could recommend only landless and marginal landowners (households owning ≤ 1.5 acres), and the interest rate was fixed by the lender and communicated clearly to all borrowers. We argue that theoretically TRAIL selection patterns would not differ qualitatively even if collusion were possible. Moreover we use data about the borrowers' credit, input and output transactions to show that there is no evidence the agent extracted borrower benefits by manipulating these transaction on the side.

2.2 The Group-based Lending (GBL) Scheme

In the GBL villages, the MFI began operations in February-March 2010 by inviting residents to form 5-member groups, and then organized bi-monthly meetings for each group, where each member was expected to deposit ₹50 per month into the group account. Of the groups that survived until October 15, 2010, two were randomly selected into the scheme through a public lottery. Each group member received a loan of ₹2,000 in Cycle 1, for a total of ₹10,000 for the entire group, with a four-month duration, payable in a single lump sum. All group members shared liability for the entire ₹10,000: if less than 50% of the due amount was repaid in any cycle, all members were disqualified from future loans; otherwise the group was eligible for a new loan, which was 33% larger than the previous loan. Bi-monthly group meetings continued throughout, in keeping with the MFI's standard protocol for joint liability lending. At the end of each loan cycle the MFI received as commission 75%

[8]However in our household surveys we did ask respondents to tell us how they used each loan.

of the interest received on these loans.

2.3 Data and Descriptive Statistics

From December 2010 to 2013, we collected household survey data from 50 households in each village at four-month intervals. This included information about household demographics, assets, landholding, cultivation, land use, agricultural input use, sale and storage of agricultural output, credit received and given, incomes, and economic relationships within the village. In each village, the household sample was composed of three sub-groups of villagers. In TRAIL villages, the agent recommended 30 borrowers for loans, 10 of whom were randomly chosen to receive the loan offer. All 10 Treatment borrowers were included in the sample. Of the remaining 20 recommended individuals, a random subset of 10 were also included in the sample; we call these the Control 1 group. Finally, we included 30 households that were not recommended (Control 2). In the GBL villages, of all the groups that formed, 2 were randomly selected to receive the loan offer, and all 10 households from these two groups (Treatment households) were included in the sample. Two groups that were not offered treatment were also randomly included in the sample (Control 1). Finally, 30 households that did not form groups were included (Control 2).

Our analysis is restricted to the 2042 sample households who owned less than or equal to 1.5 acres of land. We conducted surveys every four months over a three year period. This high-frequency data helped to minimize measurement error. There was no attrition in the sample over the three years. In each sample household the same respondent answered survey questions in each round. Panel A in Table 2 provides checks of balance for the randomization of villages into the TRAIL versus GBL treatment categories. As can be seen, there were no significant differences in village-level characteristics across the two groups.

Within each treatment category, Panel B checks whether the randomization of selected households (recommended households in TRAIL villages/participating households in GBL villages) achieved a balance of characteristics across the Control 1 and Treatment groups.[9] For most characteristics, there are only minor differences across households assigned to the Treatment or Control 1 arms. The F-statistic shows that we cannot reject the joint hypothesis of no differences across the two categories in either the TRAIL or GBL villages.

Table 3 describes credit market transactions that took place during September–December 2010 in all sample households that owned less than 1.5 acres of land. Since this was the

[9] It is unlikely that our full sample of households would be balanced across treatment groups, since both Treatment and Control 1 households were systematically selected into the sample by virtue of being recommended by the agent (TRAIL villages)/joining a group (GBL villages). In contrast, Control 2 households were selected by virtue of not being recommended, and form an unknown proportion of the population of households that the agent would not have wanted to recommend. Thus it is unclear how to re-weight these two groups to arrive at a representative sample of village households.

planting season for potatoes, the crop with the highest working capital requirements in this region (as shown below in Table 4), these data provide a picture of the main sources of agricultural credit, and characteristics of the loans. The sample households self-reported all borrowing, regardless of source or loan purpose. We present here data on total borrowing and also borrowing for agricultural purposes.[10] Note first that nearly 70 percent of sample households borrowed in this 4-month period. Informal lenders (traders and moneylenders) provided two-third of all agricultural credit and thus were the single most important lender category. Credit cooperatives provided about a quarter of the agricultural credit, but they lent mainly to households with relatively larger landholdings.[11]

The average interest rate on loans from informal lenders was 26%, substantially above the 18% interest rate charged on the program loans. The average duration of informal loans was 4 months, reflecting the 4-month agricultural cycles in this area. Only 1% of informal loans were secured by collateral. Cooperatives and government banks charged substantially lower interest rates and had longer average durations. However informal lending became a progressively more important source of agricultural credit as household landholding decreased from 1.5 acres to zero. Landless households received 87% of their agricultural credit from informal lenders, and only 6% from cooperatives (statistics available upon request). Presumably this is because cooperatives lend against collateral: nearly three quarters of cooperative loans were collateralized.

Table 4 describes the mean characteristics of the major categories of crops grown by sample farmers in the three years of data used in our analysis. Paddy was grown two or three times a year, on an average of 0.47 acres of land. Potatoes and sesame are both winter crops planted only once a year, and the average farmer planted each on similar quantities of land: potatoes on 0.31 acres and sesame on 0.22 acres. A small subset of sample farmers grew a range of vegetables such as cauliflower, cabbage, gourd, chillies and lentils year-round at high profits, but on average this accounts for only 0.02 acres per year. As the table makes clear, potatoes were the major cash crop for the farmers in our sample: they accounted for a significant proportion of acreage, had the highest working capital needs, and generated the highest value added per acre.

3 Theoretical Model of Selection

In this section we develop a theoretical model of the informal credit market and the effects of the two different interventions, focusing in particular on how borrowers are selected into the two schemes, and their subsequent production incentives. The model is based

[10]Importantly, the data also include information on trade credit from input suppliers. Since we collected detailed data on input purchases, we are able to cross-check that all inputs purchased on credit are counted as loans.

[11]MFIs other than Shree Sanchari had a very small share of the overall credit provided in these villages, which is consistent with low MFI penetration in this region at the start of the intervention.

on heterogeneity across borrowers and moral hazard in lending whereby borrowers can strategically default on their loans. Qualitatively similar results obtain in a pure adverse selection setting, as shown in previous versions of this paper (see Maitra, Mitra, Mookherjee, Motta, and Visaria, 2014).

We assume the village population is segmented into a collection of identical networks, each containing n *connected* (c) borrowers and one or more lenders, and a group of *floating* borrowers (f) who belong to no network. Network lenders have more frequent and intensive social and economic relationships with borrowers in their own network. So they can impose stronger sanctions on them in the event of default. This results in lower defaults in within-network loans. Lenders engage in Bertrand price competition with one another, both within and across networks. All lenders face a common cost of capital ρ.

We also make the following simplifying assumptions: (i) Production is not subject to risk, but borrowers are subject to random consumption need shocks that may cause them to default in some states of the world; (ii) borrowers have no collateral; (iii) limited liability constraints do not bind; (iv) all parties are risk neutral; (v) borrowers within any given network are identical, and all floating borrowers are identical; and (vi) loan contracts are stationary and exhibit no "memory", with lenders imposing sanctions immediately following default and continuing to lend the same amount in the next period. This last assumption ensures we can analyse credit relationships as a static problem and ignore dynamic complications.

Borrowers' projects are always successful but borrowers can default on their loans intentionally. We simplify the analysis by assuming that loan default rates depend only on whether and which network the borrower is connected to, but not on the loan size or interest rate. This can be justified by an underlying model of default penalties and borrower shock distributions with suitable non-overlapping supports.[12] The default risk is lowest $(1 - p_c)$ for within-network or connected relationships, followed by loans given to floaters (who default with probability $1 - p_f$). The highest default rates $1 - p_o$ arise in cross-network loans.[13] Here $p_c > p_f > p_o$.

A borrower with a given productivity parameter g produces agricultural output according to the function $gf(l)$, where l denotes the scale of cultivation (which is assumed equal to

[12] Penalties P_c, P_f, P_o are imposed by lenders on connected, floater and other-network borrowers respectively in any period when they default. In addition, every borrower incurs an additional reputational cost or personal disutility θ of default, with an identical and independent distribution which takes possible values $\theta_j, j = 1, \ldots, 4$, where $\theta_j < \theta_{j+1}$. A borrower of type $i = c, f, o$ with default disutility θ defaults on a loan of size L and interest rate r if the repayment obligation $(1 + r)L$ exceeds $P_i + \theta$. Possible realizations θ_i are spread out as follows given a relevant range of loan repayment values $R = (1 + r)l$: $\theta_1 < R - P_c < \theta_2 < R - P_f < \theta_3 < R - P_o < \theta_4$ for all R in this range. Then connected borrowers default only in state θ_1, floaters in states θ_1, θ_2 and other-network borrowers in states $\theta_1, \theta_2, \theta_3$. These events have probabilities $1 - p_c, 1 - p_f, 1 - p_o$ respectively.

[13] This can be justified by lower vulnerability of other-network borrowers to sanctions by a lender, compared to a floating borrower. The ordering of default risk between floaters and other-network borrowers is relatively inessential to our main results.

loan size). Connected borrowers are more productive than floaters (i.e. $g_c > g_f$), because help and information are exchanged within networks, or because connected borrowers own more assets than floating borrowers. It is assumed that the function f is strictly increasing, strictly concave and smooth, and satisfies Inada conditions.

Use $l_i(r)$ to denote the Walrasian loan demand of borrower of type $i = c, f$ at expected credit cost (ECC) r, i.e., the loan size that maximizes $g_i f(l) - rl$. Let the maximized payoff be denoted by $\Pi_i(r)$. For reasons explained below, we assume these loan demands are interest inelastic, i.e., loan repayment $rl_i(r)$ is non-decreasing in r.

3.1 The Informal Credit Market

We first describe how the informal market functions before the loan intervention is introduced. We abstract from any frictions in the informal market, so the outcome ends up being Walrasian: no borrower is credit-rationed. Lenders compete with one another in the credit market, and can make different contract offers to different borrowers. Besides their advantage with respect to enforcement of repayments *ex post*, a lender has a slight locational advantage over other lenders in transactions with own-network borrowers: whenever these borrowers are indifferent between borrowing from different lenders they choose to borrow from their own-network lender.

At stage 1 of the game in the informal credit market, lenders announce contract offers to every borrower in the village. At stage 2, each borrower accepts at most one offer, takes a loan of size l at interest rate r and produces $g_i f(l)$. At stage 3, the borrowers' default disutility θ is realized and they decide whether to repay or not.

Proposition 1 *There is a unique equilibrium outcome in the informal market, in which connected types take loans of size $l_c(\rho)$ from their own-network lender at interest rate ρ/p_c, and floaters borrow (from some lender) loan size $l_f(\rho)$ at interest rate ρ/p_f. All lenders earn zero profit.*

Proof: Bertrand competition implies all lenders will offer to lend to floaters at a common interest rate $\frac{\rho}{p_f}$. Since the loan will be repaid with probability p_f, the expected cost of credit for the borrower will be ρ, and each floater will choose loan size $l_f(\rho)$.

Other-network lenders will offer to lend at interest rate $\frac{\rho}{p_o}$ to any given connected borrower, which will result in an expected cost of credit of ρ for the borrower, and an expected payoff of $\Pi_c(\rho)$. Own-network lenders will have to offer the connected borrower at least this payoff, i.e., select r, l to maximize $p_c rl - \rho l$ subject to $g_c f(l) - rl \geq \Pi_c(\rho)$. It is easy to verify that the best response of any own-network lender is to offer $l_c(\rho)$ at interest rate $\frac{\rho}{p_c}$. ■

Interest rates adjust perfectly for differences in default rates, which ensures that the expected cost of lending for any given lender does not vary with borrower type. Hence all lenders compete on an equal footing for each type of borrower. Loan sizes however vary across borrower types, with floaters demanding less credit because they are less productive.

3.2 The Loan Interventions

We now consider what happens when the loan intervention is introduced. Both TRAIL and GBL loans are given at interest rate $r_T < \rho$. We first consider the case that credit limits do not bind: The optimal loan size for all borrowers at interest rate r_T is smaller than the size of the program loan. Later we shall argue that similar results obtain when credit limits are binding. As it turns out, the results obtain more simply in that case. In our experiment the majority of borrowers in both schemes borrowed the entire amount they were eligible for, indicating that the limits were binding for most.

As we shall see below, another feature of the data is that the interventions did not cause the volume of informal borrowing to change significantly. To accommodate this, we assume that borrowers had pre-committed to their informal loans before the program loans were introduced.[14] This feature makes no difference to the qualitative results. One simply has to reinterpret the production function for any given type of borrower as the production made possible by supplementary loans from the scheme, taking as given the predetermined informal loans.[15]

3.2.1 GBL outcomes

Suppose the lender appoints an MFI to implement the GBL scheme. We simplify as in Besley and Coate (1995) and Ghatak (2000) and assume that group size is two. We also assume that utility is transferable within the group, so groups will maximize the sum of *ex ante* expected utilities of members.

The costs of attending group meetings and meeting savings requirements vary idiosyncratically across GBL participants and follow the distribution function $G(.)$ with support $[0, \infty)$.[16]

[14] Alternatively, borrowers did not want to disrupt their traditional channels of credit from informal sources, in the face of cheaper credit offers from a previously unknown entity from outside the village.

[15] Specifically, if $\bar{l}$ is the predetermined informal loan size, and l is the additional size of the program loan, then the production function (net of the TFP g_i for type i) can be defined by $\tilde{f}(l) = f(\bar{l} + l)$. We can replace the (net-of-TFP) production function f by $\tilde{f}$ in calculating borrower profit functions, in what follows below. The same results obtain when the borrowers are not precommitted, the (net-of-TFP) production function is f and the borrowers are not credit-rationed.

[16] It is possible that these costs differ between connected borrowers and floaters, but the difference is ambiguous. Floaters may be less wealthy than network members, so that savings requirements impose a higher cost. Alternatively floaters may have lower opportunity costs of the time spent at group meetings. Since there is no compelling argument in either direction, we abstract from any difference.

Three different types of groups are possible: (C, C) with both members from the same network, (F, F) with two floating borrowers and (C, F) with one connected and one floating borrower. Cross-network groups are also possible, but as we shall see below, these are similar to (C, F). A member who fails to repay her loan makes the other member liable for it, and invites sanctions. We assume these sanctions operate in the same way as the sanctions imposed by lenders, and generate the same kind of repayment behavior. Hence each member of a (C, C) group repays with probability p_c.[17] In the (F, F) and the (C, F) groups, members cannot leverage the same social capital, so each member defaults with probability p_f.

A loan given to a member of a (C, C) group is repaid with probability $1-(1-p_c)^2 = p_c(2-p_c)$. Therefore, the ECC for such a group member is $p_c(2-p_c)r_T$, which is lower than their credit cost in the informal market and so provides an incentive to join a group. However this has to be weighed against the cost c of group meetings and savings. A member selects loan size $l_c(p_c(2-p_c)r_T)$. She receives expected utility $\Pi_c(p_c(2-p_c)r_T)$ net of meeting cost c, as compared with the benefit $\Pi_c(\rho)$ on the informal market. The group survives from one period to the next with probability $p_c(2-p_c)$, and so each (C, C) group member's present value of utility is $W_{CC} \equiv \frac{\Pi_c(p_c(2-p_c)r_T)-c}{1-\delta p_c(2-p_c)}$.

As Besley and Coate (1995) point out, a (C, C) group could default more often than we described above. For instance the members could agree to repay their loans with probability p_f rather than p_c, and not punish each other for default following a θ_2 shock to either member. This would lower their ECC to $p_f(2-p_f)r_T$. However it would also lower the probability that the group would survive to a subsequent cycle. In what follows we assume that δ is large enough that a (C, C) group prefers to lower its default rate to the minimum feasible.[18] Then the proportion of (C, C) groups that could potentially form is $G(\Pi_c(p_c(2-p_c)r_T))$.[19]

Analogously, an (F, F) group repays with probability $p_f(2-p_f)$, and faces an ECC of $p_f(2-p_f)r_T$, which is lower than the ECC of a (C, C) group. Group members select loans of size $l_f(p_f(2-p_f)r_T)$ and earn a present value utility of $W_{FF} \equiv \frac{\Pi_f(p_f(2-p_f)r_T)-c}{1-\delta p_f(2-p_f)}$. The proportion of (F, F) groups that could potentially form is $G(\Pi_f(p_f(2-p_f)r_T))$.[20]

A mixed group (C, F) has weak sanctions within the group, and therefore repays with probability $p_f(2-p_f)$. Both members face an ECC of $p_f(2-p_f)r_T$. The C member achieves a present value utility of $\frac{\Pi_c(p_f(2-p_f)r_T)-c}{1-\delta p_f(2-p_f)}$, which is what a (C, C) group could achieve by each repaying with probability p_f rather than p_c. By virtue of the assumption on δ above,

[17] This is unless it is in the members' mutual interest to default more frequently, a matter discussed below.

[18] If this assumption is violated, it further causes repayment rates for TRAIL loans to be higher than for GBL loans.

[19] In case borrowers were not pre-committed to their pre-program loans, this expression would be modified to $G(\Pi_c(p_c(2-p_c)r_T) - \Pi_c(\rho))$.

[20] Analogous to the (C, C) groups, this expression would be modified to $G(\Pi_f(p_f(2-p_f)r_T) - \Pi_f(\rho))$ if the borrowers were not pre-committed to their informal loans.

this would be smaller than V_{CC}. On the other hand, the F member would receive the same present value utility as in a (F, F) group, so that there would be no incentive for mixed groups (or cross-network groups) to form. As in Ghatak (2000), assortative matching obtains: mixed groups do not form.

The key point to note is there is an incentive for (F, F) groups to form, and there is no screening mechanism to exclude them. Before the GBL scheme was introduced, all borrowers were borrowing optimally relative to an ECC of ρ. The scheme provides an opportunity to borrow a larger amount at the ECC of $p_i(2-p_i)r_T$, which is lower than the informal market ECC of ρ; hence $\Pi_i(p_i(2-p_i)r_T) > 0$. Borrowers of either type that have sufficiently low costs of attending meetings, will want to take a group loan. In Ghatak (2000), the MFI is assumed to offer a menu of contracts that induces high-risk groups to self-select into individual liability loans, and low risk groups into joint liability loans. In practice MFIs rarely offer such an array of options to clients within any given village. With a single option involving a joint liability loan at interest rate r_T, as in our experiment, (C, C) groups would form with probability $\pi_c \equiv G(\Pi_c(p_c(2-p_c)r_T))$ and (F, F) groups would form with probability $\pi_f \equiv G(\Pi_f(p_f(2-p_f)r_T))$.[21]

3.2.2 TRAIL

Now the TRAIL scheme is introduced and one of the informal network lenders is selected to be the agent. The agent receives a commission $K < 1$ per rupee interest paid by the client. If the borrower defaults, the agent imposes the same punishments as in the informal credit market, thereby generating the same default rates.

To start with, we assume (i) borrowers cannot bribe the agent in return for a recommendation, and (ii) the agent is asked to recommend L borrowers, where L is smaller than his network size n. Later we discuss the consequences of relaxing these assumptions.

We derive the agent's preference between recommending an own-network borrower, a floater and a cross-network borrower. Recommending type $i = c, f$ generates the following present value utility to the agent:

$$V_i \equiv \frac{Kp_i r_T l_i(p_i r_T)}{1-\delta p_i}$$

while recommending type o generates:

$$V_o \equiv \frac{Kp_o r_T l_c(p_o r_T)}{1-\delta p_0}$$

Since loan demands are interest inelastic, the agent prefers to recommend own-network borrowers as they generate the highest expected commissions: $p_c r_T l_c(p_c r_T)$ exceeds both

[21]Without pre-commitment to informal loans, these would be modified to $G(\Pi_c(p_c(2-p_c)r_T) - \Pi_c(\rho))$ and $G(\Pi_f(p_f(2-p_f)r_T) - \Pi_f(\rho))$. Since $\Pi_c(\rho) > \Pi_f(\rho)$, the relative likelihood that (F, F) groups rather than (C, C) groups form, becomes even larger.

$p_f r_T l_f(p_f r_T)$ and $p_o r_T l_c(p_o r_T)$. Moreover, own network borrowers are less likely to default, generating a higher probability of continuation into the future.

Since $L \leq n$, it follows the TRAIL agent will recommend only own-network borrowers, who will borrow $l_c(p_c r_T)$ and repay with probability p_c. In contrast, GBL will involve both kinds of groups: (C, C) and (F, F). *It follows that the average* TRAIL *borrower has lower default risk and higher productivity than the average* GBL *borrower.*

For any given type of borrower, TRAIL loans also generate greater production and income impacts because they avoid the joint liability tax (JLT) inherent in GBL. The latter causes the ECC to be higher in GBL ($p_i(2-p_i)r_T$, compared with $p_i r_T$ in TRAIL). The lower effective interest cost of loans in TRAIL therefore generates a larger impact on production and income, for a given borrower type. These effects therefore both supplement and complement the selection advantages of TRAIL.

The model leads to the following predictions, which are tested empirically.

Prediction 1 *TRAIL agents will select borrowers of lower average default risk compared with those selected in GBL, mainly because they are inclined to recommend borrowers from their own network. Hence TRAIL-selected borrowers will pay lower interest rates on average on the informal market;*

Prediction 2 *Because of the selection of more productive borrowers, and providing them with stronger incentives through the elimination of the JLT, TRAIL loans will lead to higher impacts on output, value added and farmer incomes, and will be associated with a higher rate-of-return;*

Prediction 3 *Comparison of repayment rates between the two schemes is ambiguous;*[22]

Prediction 4 *TRAIL loans will experience greater take-up and lower administrative costs due to the elimination of group meetings.*

These predictions are summarized in Table 5.

3.2.3 Extensions

We now discuss the implications of modifying some of the assumptions made above.

[22]Despite the higher average default risk of GBL borrowers, the joint liability feature of GBL causes higher repayment rates than TRAIL for any given borrower type (i.e., for type i, repayment rate in GBL is $p_i(2-p_i)$ instead of p_i in TRAIL).

Consider first the case where program loan size limits are binding for all borrowers. Let the fixed loan size be $\bar{l}$. Then the expression for per-period net benefit of a group loan to a borrower of type i with meeting cost c is $g_i f(\bar{l}) - p_i(2-p_i)r_T\bar{l} - c$, obtained by replacing the profit function $\Pi_i(p_i(2-p_i)r_T)$ with $g_i f(\bar{l}) - p_i(2-p_i)r_T\bar{l}$. This benefit will continue to be strictly positive for both types of borrower groups, although it will now be quantitatively smaller. Hence both types of groups will form. In TRAIL however, the loan sizes are fixed and hence perfectly inelastic, so the TRAIL agent will choose according to default risks alone. As a result, he will continue to prefer to recommend own-network borrowers, followed by floaters and then other-network borrowers. Hence selection patterns are unaffected.

Second, suppose that the agent must recommend more borrowers than his network size n. Then the agent will recommend all own-network borrowers, and fill the remaining slots with either floaters or other-network borrowers. Either way, the TRAIL borrower pool will be diluted.[23] The comparison between average default risk between TRAIL and GBL selection then becomes theoretically ambiguous. This issue is discussed in more detail in Section 5 since it is empirically relevant in our case.

Third, suppose the agent can collude with borrowers. Then the TRAIL agent will continue to prefer to recommend own-network borrowers, under the reasonable assumption that the agent's bargaining power with respect to own-network borrowers is at least as large as with out-of-network borrowers. As connected borrowers gain more from the program loan than floaters, this allows a larger joint surplus to be shared between the agent and the recommended borrower. Between own-network and other-network borrowers, the agent would prefer own-network borrowers since they have lower default risk. Hence the agent's preference for own-network borrowers will be reinforced, and selection patterns will be unaffected. Collusion will of course have implications for distribution of gains between the agent and selected borrowers: the former will gain at the expense of the latter. In our empirical work we shall analyze data on various economic transactions between borrowers and agents to examine whether there is any evidence of such behavior.

[23] In the agent's choice between floaters and other-network borrowers there is a trade-off between productivity and risk. Ceteris paribus, other network borrowers are more productive and so select larger loans and repayment obligations, thereby generating larger commissions. However, they are less likely to repay. The overall comparison can go either way. The agent prefers floaters if other-network borrowers are sufficiently riskier, or productivity differences are not too large. If loan demands exhibit constant elasticity: $l_i(r) = [\frac{g_i}{r}]^\epsilon$, then floaters are preferred if and only if:

$$g_c^\epsilon \frac{p_o^{1-\epsilon}}{1-\delta p_o} < g_f^\epsilon \frac{p_f^{1-\epsilon}}{1-\delta p_f}.$$

4 Empirical Results About Selection On Default Risk

The first set of predictions (Prediction 1) of the model concern selection patterns: (i) the TRAIL agent will prefer to recommend households that belong to his network; and (ii) borrowers recommended by the TRAIl agent have lower default risk on average than borrowers who formed groups in order to apply for GBL loans. In this section we use data from credit and other trading relationships from the household surveys to test these predictions.

First, we test if TRAIL agents preferred to recommend households that belonged to their network. Note however, that it is difficult to directly identify households that belong to the agent's own network. In our surveys households reported their caste and/or tribal identity, and whom they had conducted economic transactions with in the three years before the interventions began. We expect from the theory that the likelihood of recommendation is positively correlated with the existence of caste affiliation and/or prior economic relationships with the agent.[24] We run the linear probability regression

$$\text{Recommended}_{iv} = \alpha_0 + \sum_{k=1}^{3} \beta_k (\text{Interacted with agent in market } k)_{iv} + \gamma_1 \mathbf{X}_{1iv} + \varepsilon_{iv} \quad (1)$$

on the sample of households who owned at most 1.5 acres of cultivable land in TRAIL villages. The dependent variable takes the value 1 if the TRAIL agent recommended household i in village v for a loan, and 0 otherwise.[25] On the right hand side we include three indicator variables for whether the household had interacted with the agent in the three years prior to the study – by buying inputs from, borrowing from or working for the agent. We also examine whether the agent was more likely to recommend borrowers belonging to the same caste group as himself. We control for the household head's age, gender, educational status and primary occupation, household size, landholding, and receipt of government benefits.

In line with our prediction, we see in Table 6 that within our sample, households that had borrowed from the agent in the past were 15 percentage points (or 44 percent) more likely to be recommended than households that had not interacted with the agent. Households that had bought from or worked for the agent were no more likely to be recommended than those who had not. There is also evidence that caste networks were important: scheduled caste agents were significantly more likely to recommend scheduled caste households; and high caste agents were significantly less likely to recommend scheduled caste households.

Consider now the interest rate that the households in TRAIL villages paid on informal loans they took before the intervention, and how these were related to whether they were

[24]The match is not exact, since the theory predicts that the agent will conduct some economic transactions outside his own network.

[25]Thus both Treatment and Control 1 households receive a value of 1, and Control 2 households have a value of zero.

recommended and whether they belonged to the agent's clientele. We do this by running the following regression using data from Cycle 1, on the sample of households in TRAIL villages that owned at most 1.5 acres of land:

$$r_{iv} = \beta_0 + \beta_1 \text{Selected}_{iv} + \beta_2 \text{Own Clientele}_{iv} + \beta_3 (\text{Selected}_{iv} \times \text{Own Clientele}_{iv}) + \gamma_2 \mathbf{X}_{2iv} + u_{iv} \tag{2}$$

Here r_{iv} denotes the average interest rate the household paid on informal loans they had taken in Cycle 1. The variable Selected$_{iv}$ indicates whether the household was recommended by the TRAIL agent. Given the evidence in Table 6 that a previous credit relationship predicted whether the TRAIL agent recommended a household, we define the variable Own Clientele$_{iv}$ to indicate whether the household had borrowed from the agent in the three years prior to the study. Also, we cluster standard errors at the network level within each village, where within-village networks are defined as follows: those belonging to the agent's own clientele, and/or those belonging to the same caste/tribe category. As column 1 of Table 7 shows, from among households within his clientele, the TRAIL agent recommended households with a significantly lower default risk: households recommended by the TRAIL agents from their own clientele paid 5.2 percentage points lower interest rates in the informal market prior to the intervention, compared with households not recommended from the agent's clientele. However, the agent also recommended some households from outside his clientele: the latter did not differ significantly in default risk from the non-recommended. Averaging across these two categories, the default risk of the recommended household also did not significantly differ from the non-recommended (Column 2).

We now turn to selection patterns in the GBL scheme. We use data from Cycle 1 to run the following regression on the sample of households that owned at most 1.5 acres of land in GBL villages:

$$r_{iv} = \alpha_0 + \alpha_1 \text{Selected}_{iv} + \gamma_3 \mathbf{X}_{2iv} + \epsilon_{iv} \tag{3}$$

where Selected$_{iv}$ takes the value 1 if household i in GBL village v joined a GBL group. Standard errors are clustered at the network level, where, within each village a network is defined as those belonging to the same GBL group, or those belonging to the same caste/tribe category. Recall from Section 3 that both (C, C) and (F, F) groups form in the GBL villages. As a result it is theoretically ambiguous whether GBL participants have different default risk from non-participants. In line with this, the point estimate of α in column 3 of Table 7 is positive, but not statistically significant. Therefore we are unable to reject the hypothesis that the GBL scheme attracted borrowers of average default risk.

In column 4 we compare the default risk of selected borrowers in the two schemes by running the regression on the data pooled over the TRAIL and GBL villages:

$$r_{iv} = \theta_0 + \theta_1 \text{Selected}_{iv} + \theta_2 \text{TRAIL}_v + \theta_3 (\text{TRAIL}_v \times \text{Selected}_{iv}) + \gamma_4 \mathbf{X}_{2iv} + \varepsilon_{iv}. \tag{4}$$

Standard errors are clustered at the network level, where the networks are defined in TRAIL villages as in Columns 1 and 2, and in GBL villages as in Column 3. Column 4 shows that households selected to participate in the TRAIL scheme paid on average a statistically significant 4.7 percentage points lower interest rate than those that participated in the GBL scheme.

Finally column 5 uses the specification

$$
\begin{aligned}
r_{iv} &= \kappa_0 + \kappa_1 \text{Selected}_{iv} + \kappa_2 \text{TRAIL}_v + \kappa_3 (\text{TRAIL}_v \times \text{Selected}_{iv}) \\
&+ \kappa_4 (\text{TRAIL}_v \times \text{Own Clientele}_{iv}) + \kappa_5 (\text{TRAIL}_v \times \text{Selected}_{iv} \times \text{Own Clientele}_{iv}) \\
&+ \gamma_5 \mathbf{X}_{2iv} + \zeta_{iv} \qquad (5)
\end{aligned}
$$

to summarize the results of the first three columns. The triple interaction between TRAIL, Selected and Own Clientele is -6.8 percentage points (significant at the 10% level). The total effects shown in the Table (below the regression coefficients) indicate that borrowers that the TRAIL agent recommended from within his own clientele had paid a statistically significant 6.2 percentage points lower interest rate on informal loans taken before the schemes were introduced, compared to borrowers who participated in the GBL scheme.

The results presented in Table 7 thus support our theoretical prediction that the average TRAIL-selected borrower had lower default risk than the average GBL-self-selected borrower. A comparison of the productivity of selected borrowers is postponed to the next section, after we present estimates of the impact of the two loan treatments on farm production and income.

5 Empirical Results About Loan Treatment Impacts on Borrower Production and Income

To estimate the *treatment effect* of the two lending mechanisms, we rely on the fact that only a randomly chosen subset of the selected borrowers (recommended by the agent in the TRAIL villages or members of self-formed groups in the GBL villages) were offered the loans. Any difference between households that were recommended but were not offered the loan (Control 1 households) and those that were both recommended and offered the loans (Treatment households) must be caused by the loans. The corresponding *selection effect* is estimated by the difference between the Control 1 (those selected but did not receive loans) and Control 2 households (those not selected).

Our regression specification takes the following form:

$$
\begin{aligned}
y_{ivt} &= \beta_0 + \beta_1 \text{TRAIL}_v + \beta_2 (\text{TRAIL}_v \times \text{Control } 1_{iv}) + \beta_3 (\text{TRAIL}_v \times \text{Treatment}_{iv}) \\
&+ \beta_4 (\text{GBL}_v \times \text{Control } 1_{iv}) + \beta_5 (\text{GBL}_v \times \text{Treatment}_{iv}) + \gamma \mathbf{X}_{ivt} + \varepsilon_{ivt} \qquad (6)
\end{aligned}
$$

Here y_{ivt} denotes the outcome variable of interest for household i in village v in year t. The omitted category is the Control 2 group in GBL villages. As discussed above, $\hat{\beta}_3 - \hat{\beta}_2$ estimates the treatment effect in the TRAIL scheme and $\hat{\beta}_5 - \hat{\beta}_4$ estimates the treatment effect in the GBL scheme.[26] $\mathbf{X}_{iv}$ is a set of additional controls, including land owned by the households, two year dummies to control for secular changes over time and a dummy variable indicating whether the village received a separate intervention informing residents of the prevailing market price for potatoes.[27] Since agricultural activity involves a long delay from planting to harvest, and the harvest can be sold over a period of several months, we need to aggregate our data to the annual level in order to correctly compute the costs and revenues of each crop. Once again, standard errors are clustered at the level of the network the household belongs to.[28]

5.1 Treatment effects on Borrowing, Cultivation and Farm Incomes

Tables 8–10 present estimates of the treatment effects of the main outcomes of interest: borrowing (Table 8), cultivation of and incomes from potatoes (Panel A, Table 9), cultivation of and incomes from other crops (Panel B, Table 9) and total farm income (Table 10). These treatment effects are computed from the regressions specified in equation (6).

Since we analyze a large number of outcome variables, the null of no treatment effect could be rejected by mere chance, even if the null were actually true. To correct for this, we follow Hochberg (1988) and compute a conservative p-value for an index of variables in a family of outcomes taken together (see Kling, Liebman, and Katz, 2007).[29]

5.1.1 Effects on Borrowing

We see in Table 8 that the TRAIL treatment caused borrowers' overall borrowing to increase by ₹5088, which is a 91% increase over the ₹5590 mean borrowing by TRAIL Control 1 households. The GBL treatment caused overall borrowing to increase significantly by ₹3660,

[26] All treatment effects presented in the tables below are intent-to-treat estimates because they compare the outcomes of households *assigned* to Treatment and Control 1 groups, regardless of actual take-up.

[27] This information intervention was undertaken for a separate project aimed at examining the effect of providing information about potato prices to farmers and is similar to the public information treatment described in Mitra, Mookherjee, Torero, and Visaria (2015). Villages were assigned to the information treatment randomly and orthogonally to the credit intervention that is the focus of this paper.

[28] Recall, the network is defined as follows: in each TRAIL village, households that borrowed from the agent in the previous 3 years are assumed to belong to that agent's network. In each GBL village, households that belonged to the same GBL group are assumed to belong to that group network. All other households are classified into village-specific caste/tribe networks on the basis of their caste or tribe category. The results are robust to clustering at the village level instead.

[29] The variables are normalized by subtracting the mean in the control group and dividing by the standard deviation in the control group; the index is the simple average of the normalized variables. To adjust the p-value of the treatment effect for an index, the p-values for all indices are ranked in increasing order, and then each original p-value is multiplied by $(m-1+k)$, where m is the number of indices and k is the rank of the original p-value. If the resulting value is greater than 1, an adjusted p-value of > 0.999 is assigned.

which is also a 90% increase over the mean for the GBL Control 1 households.[30]

To check if the program loans crowded out loans from other sources, column 2 in Table 8 examines if the treatment caused a decrease in total borrowing for agricultural purposes through non-program loans. The treatment effects are small in magnitude and are not statistically significant for either TRAIL or GBL borrowers. Thus the program loans represented a net addition to treated households' agricultural borrowing, either because of the reduction in cost of borrowing, or credit rationing in the informal market.

When both borrowing outcomes are considered together, we find that TRAIL loans caused a 0.24 standard deviation increase in household borrowing, which is significant even according to the more conservative Hochberg test that corrects for multiple hypothesis testing (p-value = 0.03). However, the effect of the GBL treatment is not statistically significant (effect = 0.18 sd, Hochberg p-value = 0.37).

5.1.2 Effects on Cultivation and Farm Incomes

We now check if the increased agricultural borrowing translated into increased agricultural activity, output and incomes. Since the loan durations matched the potato planting cycle, we first present the estimated effects on potato cultivation. Panel A of Table 9 shows that the TRAIL treatment caused a statistically significant 29 percent increase in acreage devoted to potato cultivation (column 2). TRAIL treatment households also spent more on inputs (column 4) and produced higher output (column 3: treatment effect is 27% of Control 1 mean). As a result they earned 29% higher revenue and 37% higher value-added (column 6) than households who were recommended by the agent but randomly excluded from receiving program loans. Value-added is computed by subtracting from revenues only the costs of purchased or rented inputs, but in column 7 we impute the cost of family labor using the average market wage rate for hired labor in the village (an upper bound for the shadow cost of family labor), and find that the imputed net profit from potato cultivation increased by ₹1963, or 41.4% of the mean for Control 1 households.

The treatment effects of the GBL scheme are not statistically significant for any variable. The point estimates suggest that the GBL treatment caused households to place greater area under potato cultivation, and also increased their expenditure, output, revenue and value-added. But the average treatment effects are small and estimated imprecisely.[31] Average treatment impacts in GBL on value added and imputed profits are quantitatively small in comparison with the magnitude of the TRAIL treatment effects.

[30]Note in this connection that TRAIL Control 1 households had much higher borrowing than GBL Control 1 households, consistent with the prediction of our model that the former were more productive farmers.

[31]The higher standard errors for the GBL treatment effects are consistent with the theoretical prediction from Section 3 that both low- and high-productivity borrowers participate in the GBL scheme, whereas the TRAIL agent mainly recommends high-productivity borrowers.

On the index of outcomes related to potato production, the TRAIL treatment caused a 0.20 standard deviation increase (Hochberg p-value = 0.00), while the GBL treatment had a statistically insignificant effect (Hochberg p-value > 0.999).

Panel B of Table 9 presents the treatment effects on acreage and value-added of the other main crops: sesame, paddy and vegetables. TRAIL loans significantly increased the acreage that borrowers devoted to all of these crops. We also find a positive treatment effect on value added for all three crops, though only the treatment effect on value added in sesame production is statistically significant. In contrast, GBL loans had no effect on either acreage or value-added for any of the crops.

Finally, column 1 of Table 10 presents the treatment effects on total farm value-added aggregating across all four crop categories. The TRAIL treatment led to a 25.5 percent increase in overall farm value-added over the Control 1 mean. The GBL treatment effect was negligible and statistically insignificant.

5.2 Comparing Productivity of Selected TRAIL and GBL borrowers

Next we compute the rate of return on program loans as the ratio of the treatment effect on value-added to the treatment effect on cultivation cost. In columns 2 and 3 of Table 10, we report the rate of return on the costs of cultivation of potatoes and all major crops considered together. Since the rate of return is the ratio of two treatment effect estimates, the standard errors are cluster-bootstrapped, with 600 replications. The rate of return on potato cultivation expenses was a statistically significant 108% for TRAIL borrowers. GBL borrowers earned a substantially lower but still statistically significant 26% rate of return on potato cultivation. We reject the null hypothesis that these rates of return were the same. Thus TRAIL borrowers earned a statistically significantly higher rate of return on potato cultivation than GBL borrowers did. Across all major crops, TRAIL borrowers earned a statistically significant rate of return of 120%, whereas the rate of return earned by GBL borrowers was non-significant. These are consistent with our model predictions based on either or both of the different selection and production incentive effects of TRAIL.

5.3 Loan Performance

To examine loan performance, we use administrative data on take-up of loans, continual participation in the lending scheme, and repayment. Over the three years of the intervention, both TRAIL and GBL borrowers paid up their loans on time in more than 95% of instances, well within the range of the industry standard.

In our model, it is ambiguous how the repayment rate on TRAIL loans would compare to the rate on GBL loans: although TRAIL loans had higher rates of return, GBL borrowers had the benefit of joint liability, so that even if their own projects failed, their group members might have repaid on their behalf. Column 1 in Table 11 shows that the difference is not statistically significant.

The take-up of loans in the two schemes is also a useful metric of the *ex ante* impact of these loans on borrower welfare. Columns 2 and 3 show that households that were offered the loan in the TRAIL scheme were more likely to accept it than those offered the loan in the GBL scheme. This result holds whether we use as the denominator the households households that were offered the loan in that cycle (take up, Column 2), or instead all households that were offered the loan at the beginning of the intervention (continuation, Column 3).[32]

6 Ancillary Issues

Next we examine (*i*) possible offsetting effects on non-farm incomes; (*ii*) year-specific effects; (*iii*) heterogeneity of treatment effects on value-added; (*iv*) the possibility that borrower benefits might have been siphoned off by the TRAIL agent through higher input prices, lower output prices or higher interest rates on loans; and (*v*) administrative costs.

6.1 Effect on Non-Farm Incomes

We address the concern that the positive TRAIL impacts on farm incomes might have occurred at the expense of non-farm incomes. Alternatively it is possible that group liability prevented GBL borrowers from investing the loan capital in high-risk agriculture but they used it to increase non-farm incomes instead. In Table 12, we see positive but imprecisely estimated effects of the TRAIL treatment on rental income, income from sales of animal products, labor income, reported business profits, current value of business and total household income from non-agricultural sources. The treatment effects of GBL loans are smaller and also estimated imprecisely. The point estimate of the GBL treatment effect on aggregate non-farm income is actually negative, while that for TRAIL is positive, while both are statistically indistinguishable from zero. Thus there is no evidence that either scheme had a non-zero effect on non-farm incomes.

[32]The denominator for take up differs from the denominator for the measure of continuation because past default would disqualify a treatment household from borrowing subsequently.

6.2 Year-Specific Effects

The TRAIL and GBL treatment effects on agricultural output in Table 9 averaged across three years of the intervention. It is informative to consider how these effects varied across years. As Figure 1 shows, the point estimates of the TRAIL treatment effects on potato acreage and the rate of return from potato cultivation were positive and higher than the corresponding GBL effects in each year of the intervention. There is no clear evidence of learning effects in TRAIL: while acreage and rate of return increased somewhat from the first year to the next, both declined from the second year to the third. As a result, the confidence intervals for the first and third years overlap. While GBL treatment effects are too imprecise to permit any clear inference, the point estimates show a tendency for acreage and rate of return to increase across years.

6.3 Heterogeneity in Effects

The average treatment effects presented in Table 9 could mask significant heterogeneity of the treatment effect. For example, Banerjee, Duflo, Glennerster, and Kinnan (2015) find that the positive effects of group loans were concentrated at the very top of the distribution of entrepreneurs. Figure 2 presents the TRAIL and GBL treatment effects for value-added in potato cultivation and the associated 90% confidence interval at different quantiles of the distribution of value-added. The TRAIL treatment effect increases over the distribution. The positive average TRAIL treatment effect is driven by the positive and generally statistically significant TRAIL treatment effects for most of the top half of the value added distribution. Therefore the large average treatment effects of TRAIL are not driven by a few outliers. In contrast, the GBL treatment effect is relatively stable and small across the different quantiles.

6.4 Extraction by Agent in Other Spheres of Interaction

One may wonder whether the TRAIL agent extracted benefits from the borrowers, either by requiring bribes before he recommended them, demanding side-payments, or by manipulating other transactions with them. Although it is understandably difficult to collect data on bribes or side-payments, we do have detailed data about input purchases from and output sale to the TRAIL agent, collected every four months. These can be used to test if the agent extracted rents from TRAIL borrowers through manipulation of these transactions. Alternatively, we can examine if the agent tried to help the TRAIL borrowers by lowering prices of inputs they purchased from him, or raising the price at which they bought their outputs.[33]

[33] A group of students in Boston University's Masters of Global Development Studies program did field-work and very useful analysis addressing this question (see Ah-Tye, Bai, Blanco, Pheiffer, and Winata,

In Table 13 we analyse input, output and credit transactions reported by sample households in TRAIL villages. Column 3 shows the mean incidence of such transactions for the Control 1 households. Note first that there is no evidence that recommended households interacted exclusively with the TRAIL agent in these markets. The first two rows of Panel A show that over the 3 years, Control 1 households conducted only about 8% of input transactions by Control 1 households with the agent, accounting for less than 6% of the value of inputs purchased. The top rows of Panel B show that they conducted 19% of output transactions with the agent, representing less than 16% of the value of transactions, and the top two rows of Panel C show that 16% of Control 1 households borrowed from the agent, accounting for 5% of their total borrowing.

The TRAIL agent could have manipulated transactions to extract borrowers' benefits in various ways. He could have bought larger quantities from the borrowers at discounted prices or adjusted downward the price he paid for the output. He could have sold larger quantities of inputs at higher prices to the borrowers, or he could have charged higher interest rates on loans he gave to the borrowers. As column 1 of Panel B shows, the only significant effect of TRAIL treatment was a reduction in the price at which borrowers rented power tillers. If anything, borrowers paid lower input prices to the agent, the opposite of a siphoning off of benefits. Column 1 in Panel C shows that instead of borrowing more at higher interest rates, treatment borrowers were less likely to borrow from the agent during the three years of the experiment. The average interest rate charged by the agent also did not change.

Thus, we do not find evidence that the agent extracted side-payments from the borrowers by engaging in a larger volume of transactions, charging higher prices for inputs sold or paying lower prices for outputs purchased from the borrowers. It appears likely that the TRAIL treatment households retained control over the program benefits that accrued to them. These results also cast doubt on the hypothesis that the agent gave extra concessions on output sales or input purchases to TRAIL borrowers, compared to others whom he recommended but who did not receive TRAIL loans.

6.5 Financial Performance

Lending institutions usually evaluate loan programs in terms of their repayment rates, take-up and administrative costs. We have shown that loan take-up rates were higher for the TRAIL than the GBL scheme and repayment rates were similar. Administrative costs also turned out to be lower in TRAIL. The per-month cost to the MFI of operating the GBL scheme in a village was ₹1463. The lender's cost of running the TRAIL scheme was nearly ₹1400 lower, at ₹68 per village. This difference is largely explained by the fact that the TRAIL scheme did not require group meetings, which involve high personnel and transport costs. Recall that in both schemes the intermediary (the agent in TRAIL villages and the

2013).

MFI in GBL villages) received 75 percent of interest payments as commission, and the repayment rates were similar. Hence TRAIL generated a higher financial return to the lender, while generating higher loan take-up.

7 Conclusion

The problem of identifying creditworthy borrowers and ensuring repayment in the absence of collateral has made agricultural finance in developing countries notoriously cost-ineffective for formal financial institutions. While microcredit has famously solved these problems by leveraging local information and enforcement, recent interventions have shown that it generally does not increase borrowers' incomes or production. In this paper we have examined the hypothesis that this could partly be due to problems associated with selecting productive borrowers.

The trader-agent intermediated lending (TRAIL) scheme involved individual liability loans at below-market-average interest rates, durations that matched crop cycles of potatoes, the most important cash crop in the region, and insurance against local yield and price shocks. The scheme was particularly successful at inducing selected beneficiaries to increase cultivation and output of potatoes. At the same time TRAIL borrowers repaid loans at the same high rate that group loans did, and the scheme had higher loan take-up rates and lower administrative costs. TRAIL borrowers achieved significant increases in farm incomes, without any offsetting decline in income from any other source.

Our explanation for this result focused primarily on the underlying selection patterns: TRAIL agents recommended households from among their own networks, who had low default risk and high productivity. The GBL scheme, which used the traditional group-based micro-finance approach to borrower selection, did not generate comparable effects on farm outcomes. We argued this was because there is no screening mechanism available in the GBL scheme (and in most group-based schemes) to exclude high-risk, low-productivity borrowers. While it is conceivable that these differences could also be the result of individual liability and the sharper incentives it provides for borrowers to take risk and expand production, we think this is an unlikely explanation. Other microcredit experiments using individual liability loans have not succeeded in generating significant production and income impacts. Training or help provided by TRAIL agents is another candidate explanation, but we saw no evidence of learning effects in TRAIL, nor any impacts on trades or credit with the agent. Moreover, there is no evidence that TRAIL borrowers pay lower prices for inputs or receive higher prices for output than non-borrowers[34], making it improbable that the TRAIL agent provided exclusive marketing or procurement advice to TRAIL borrowers.

Loan take-up rates were higher in the TRAIL scheme, suggesting higher *ex ante* effects on

[34] These results are available on request.

borrower welfare. We found no evidence that TRAIL agents siphoned off the benefits of recommended or treated borrowers by manipulating their other economic transactions with them. The TRAIL scheme was more cost-effective to administer, because loan officers were not responsible for monitoring or screening borrowers. It is also likely that the absence of mandatory group meetings, savings requirements and joint liability lowered borrowers' costs of participating in the scheme.

Our paper contributes to the policy debate on ways to promote financial inclusion of the rural poor in the developing world. Various countries have attempted to expand financial services in rural areas by employing local agents, but with limited success.[35] The TRAIL scheme is also related to a lending approach that India's central bank has been promoting recently, where "banking facilitators" are recruited from within the local communities to select and monitor borrowers on behalf of formal banks (Srinivasan, 2008). To our knowledge no rigorous evaluation of that approach has been carried out so far. The findings from our study could inform policymakers and central bank officials involved in the design of such schemes.

References

Ah-Tye, T., O. Bai, J. Blanco, C. Pheiffer, and S. Winata (2013): "The Effect of Social and Economic Ties on Loan Repayment in Agent-Intermediated Lending in West Bengal, India," Master's thesis, Boston University.

Angelucci, M., D. Karlan, and J. Zinman (2015): "Microcredit Impact: Evidence from a Randomized Microcredot Program Placement Experiment by Compartamos Banco," *American Economic Journal: Applied Economics*, 7(1), 151 – 182.

Attanasio, O., B. Augsburg, R. D. Haas, E. Fitzsimons, and H. Harmgart (2015): "The Impacts of Microfinance: Evidence from Joint-Liability Lending in Mongolia," *American Economic Journal: Applied Economics*, 7(1), 90 – 122.

Augsburg, B., R. D. Haas, H. Harmgart, and C. Meghir (2015): "The Impacts of Microcredit: Evidence from Bosnia and Herzegovina," *American Economic Journal: Applied Economics*, 7(1), 183 – 203.

Banerjee, A., E. Duflo, R. Glennerster, and C. Kinnan (2015): "The miracle of microfinance? Evidence from a randomized evaluation," *American Economic Journal: Applied Economics*, 7(1), 22 – 53.

Banerjee, A. V., D. Karlan, and J. Zinman (2015): "Six Randomized Evaluations of Microcredit: Introduction and Further Steps," *American Economic Journal: Applied Economics*, 7(1), 1 – 21.

Beaman, L., D. Karlan, B. Thuysbaert, and C. Udry (2015): "Selection into Credit Markets: Evidence from Agriculture in Mali," Discussion paper, Yale University.

[35]Agents have been employed to intermediate financial services in Thailand (Onchan, 1992), Philippines (Floro and Ray, 1997), Bangladesh (Maloney and Ahmad, 1988), Malaysia (Wells, 1978), Indonesia (Fuentes, 1996) and Senegal (Warning and Sadoulet, 1998).

Besley, T., and S. Coate (1995): "Group lending, repayment incentives and social collateral," *Journal of Development Economics*, 46(1), 1 – 18.

Crépon, B., F. Devoto, E. Duflo, and W. Parienté (2015): "Estimating the Impact of Microcredit on Those Who Take it Up: Evidence from a Randomized Experiment in Morocco," *American Economic Journal: Applied Economics*, 7(1), 123 – 150.

Feigenberg, B., E. Field, and R. Pande (2013): "The Economic Returns to Social Interaction: Experimental Evidence from Microfinance," *Review of Economic Studies*, 80(4), 1459 – 1483.

Field, E., R. Pande, J. Papp, and N. Rigol (2013): "Does the Classic Microfinance Model Discourage Entrepreneurship Among the Poor? Experimental Evidence from India," *American Economic Review*, 103(6), 2196 – 2226.

Fischer, G. (2013): "Contract Structure, Risk Sharing and Investment Choice," *Econometrica*, 81(3), 883 – 939.

Floro, M. S., and D. Ray (1997): "Vertical Links Between Formal and Informal Financial Institutions," *Review of Development Economics*, 1(1), 34 – 56.

Fuentes, G. (1996): "The Use of Village Agents in Rural Credit Delivery," *Journal of Development Studies*, 33(2), 188 – 209.

Ghatak, M. (1999): "Group lending, local information and peer selection," *Journal of Development Economics*, 60, 27 – 50.

——— (2000): "Screening by the company you keep: joint liability lending and the peer selection effect," *Economic Journal*, 110(465), 601 – 631.

Giné, X., and D. Karlan (2014): "Group versus individual liability: Long-term evidence from Philippine microcredit lending groups," *Journal of Development Economics*, 107, 65 – 83.

Hochberg, Y. (1988): "A sharper Bonferroni procedure for multiple tests of significance," *Biometrika*, 75(4), 800 – 802.

Kaboski, J. P., and R. M. Townsend (2011): "A structural evaluation of a large-scale quasi-experimental microfinance initiative," *Econometrica*, 79(5), 1357–1401.

Karlan, D., and J. Zinman (2011): "Microcredit in Theory and Practice: Using Randomized Credit Scoring for Impact Evaluation," *Science*, 332(6035), 1278 – 1284.

Kling, J. R., J. B. Liebman, and L. F. Katz (2007): "Experimental Analysis of Neighborhood Effects," *Econometrica*, 75, 83 – 119.

Maitra, P., S. Mitra, D. Mookherjee, A. Motta, and S. Visaria (2014): "Agent Intermediated Lending: A New Approach to Microfinance," Discussion paper, Mimeo, Boston University.

Maloney, C., and A. B. Ahmad (1988): *Rural Savings and Credit in Bangladesh*. University Press Ltd., Dhaka Bangladesh.

Mitra, S., D. Mookherjee, M. Torero, and S. Visaria (2015): "Asymmetric Information and Middleman Margins: An Experiment with West Bengal Potato Farmers," Discussion paper, Mimeo, Hong Kong University of Science and Technology.

Onchan, T. (1992): "Informal Rural Finance in Thailand," in *Informal Finance in Low-Income Countries*, ed. by D. W. Adams, and D. Fitchett. Westview Press, Boulder, CO.

Srinivasan, N. (2008): *Microfinance India. State of the Sector Report.* SAGE.

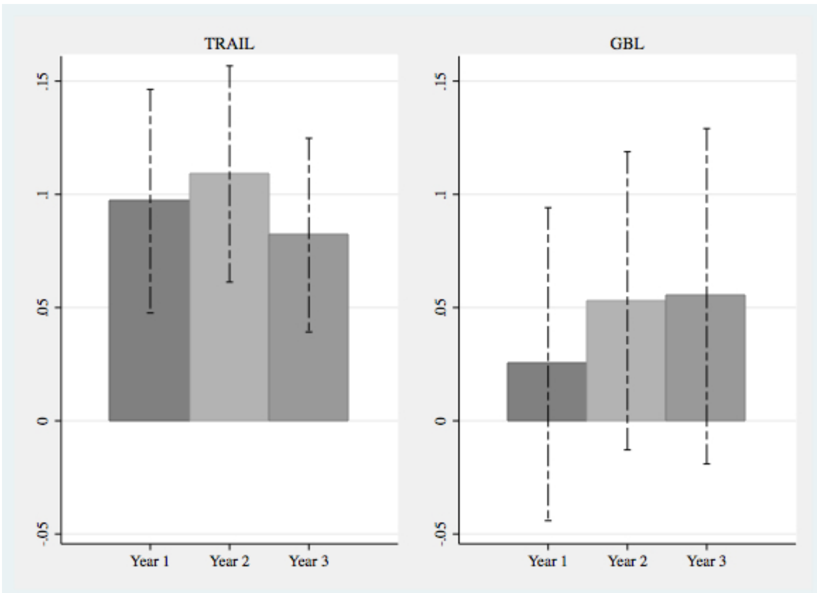
Tarozzi, A., J. Desai, and K. Johnson (2015): "The Impacts of Microcredit: Evidence from Ethiopia," *American Economic Journal: Applied Economics*, 7(1), 54 – 89.

Warning, M., and E. Sadoulet (1998): "The Performance of Village Intermediaries in Rural Credit Delivery under Changing Penalty Regimes: Evidence from Senegal," *Journal of Development Studies*, 35(1), 115 – 138.

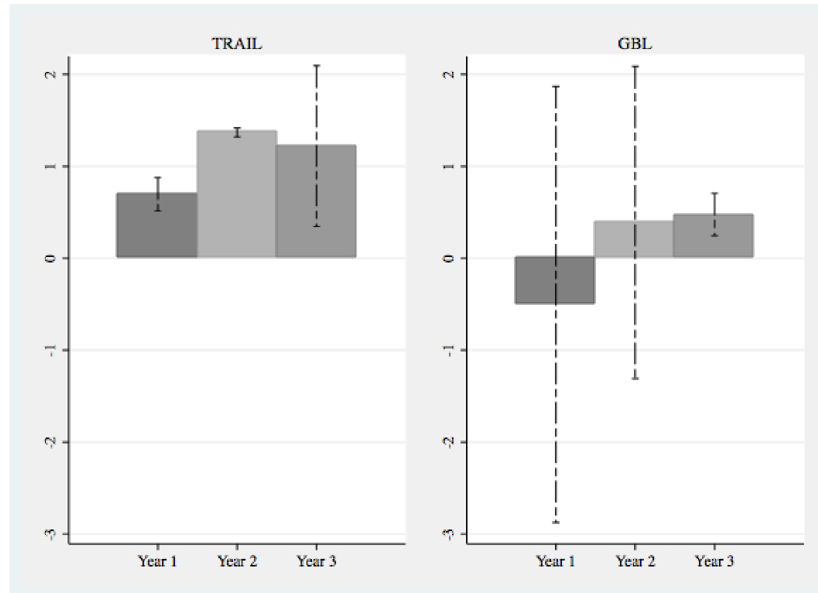
Wells, R. J. G. (1978): "An Input Credit Programme for Small Farmers in West Malaysia," *Journal of Administration Overseas*, 17, 4 – 16.

Figure 1: Year-Specific Effects on Potato Acreage and Rate of Return

Potato Acreage

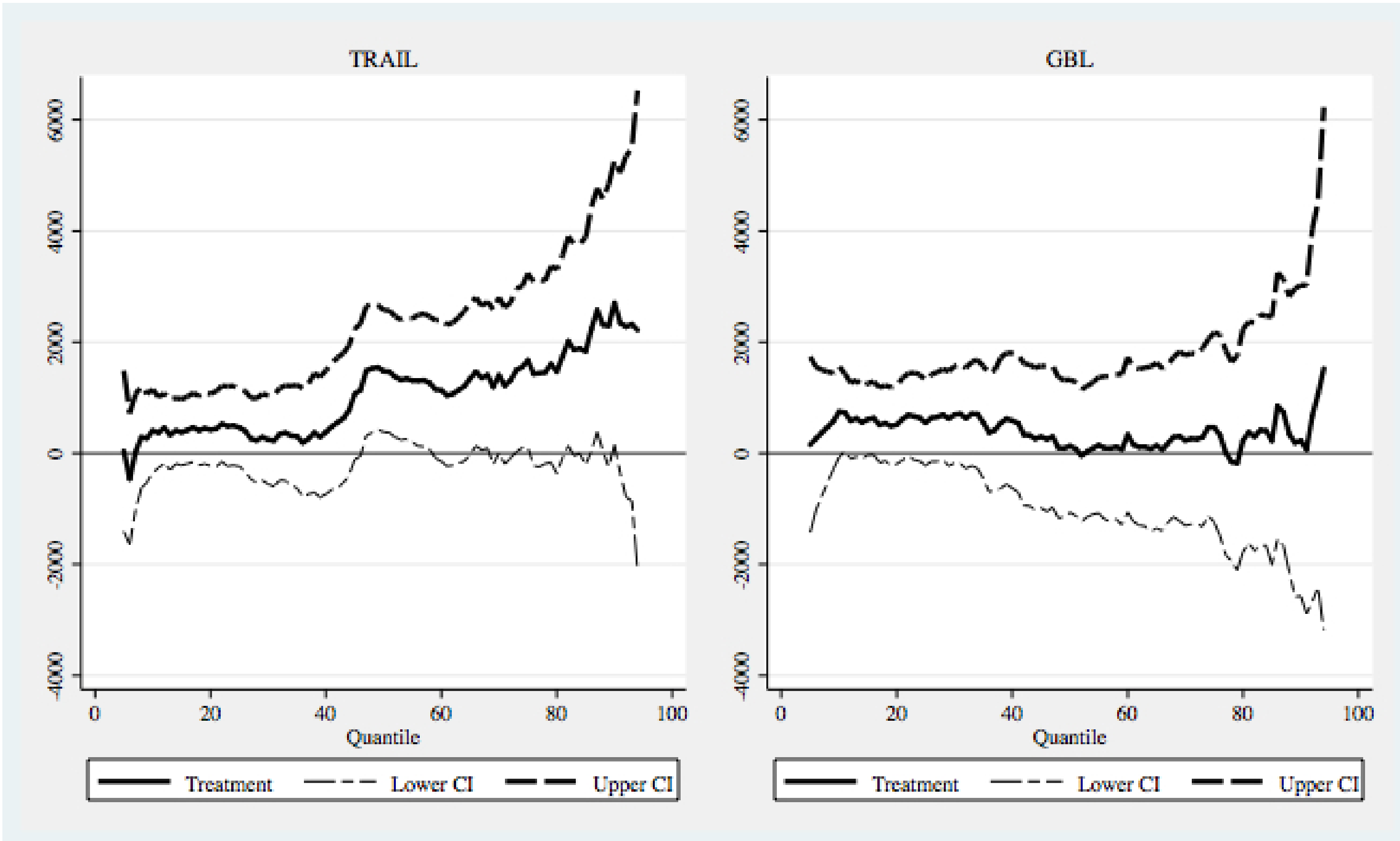


Rate of Return



Notes:
The values represent the estimated treatment effects from regressions following equation (6) in the text, with each sample restricted to a single year of data. The rate of return on value added is computed as the ratio of the treatment effect on value added to the treatment effect on the cost of cultivation. The dashed lines show the 90% confidence intervals.

Figure 2: Heterogeneity in Treatment effects



Notes:
The solid lines represent the estimated treatment effects at different quantiles of value-added, based on quantile regressions following the specification in equation (6) in the text. The dashed lines show the corresponding 90% confidence intervals.

Table 1: Terms of TRAIL and GBL loans relative to other recent interventions

	TRAIL (1)	GBL (2)	Summary: Six evaluations (Banerjee, Karlan, and Zinman, 2015, Table 1) (3)
Liability	Individual	Group (Joint)	Group (4), Individual (1), Both (1)
Interest Rate	18% APR	18% APR	12–27% APR (Mexico = 110% APR)
Market Interest Rate	24% APR	24% APR	16–47% APR (Mexico = 145% APR)
Loan Length	4 months	4 months	3–18 months
Repayment Frequency	4 months	4 months	Weekly/Bi-monthly/Monthly
Group Size	–	5	3–50
Collateralized	No	No	Yes (3), No (3)
Dynamic Incentives	Yes	Yes	Yes

Notes:
Columns 1 and 2 summarize the terms of the TRAIL and GBL loans; Column 3 summarizes the results presented in Table 1 of Banerjee, Karlan, and Zinman (2015).

Table 2: Balance of Characteristics across Treatment Categories

Panel A: Village Level Differences

	TRAIL	GBL	Difference TRAIL - GBL
Number of households	297.59	388.50	-90.91
	(48.06)	(80.36)	
Percent households electrified	0.60	0.59	0.01
	(0.06)	(0.05)	
Has primary school	0.77	0.79	-0.02
	(0.09)	(0.08)	
Has primary health centre	0.27	0.21	0.06
	(0.10)	(0.08)	
Has bank branch	0.14	0.17	-0.03
	(0.07)	(0.08)	
Has pucca road	0.27	0.42	-0.14
	(0.10)	(0.10)	

Panel B: Household Level Differences

	TRAIL			GBL		
	Treatment	Control 1	Difference†	Treatment	Control 1	Difference†
Male Headed Household	0.988	0.988	0.000	0.933	0.900	0.033
	(0.007)	(0.007)		(0.016)	(0.020)	
Non-Hindu	0.154	0.167	-0.013	0.126	0.114	0.013
	(0.023)	(0.024)		(0.022)	(0.021)	
Low Caste	0.367	0.392	-0.025	0.521	0.450	0.071
	(0.031)	(0.032)		(0.032)	(0.033)	
Household Size	4.792	4.663	0.129	4.836	4.782	0.054
	(0.120)	(0.121)		(0.121)	(0.125)	
Age of Household Head	44.204	46.358	-2.154**	42.122	44.493	-2.372**
	(0.692)	(0.719)		(0.761)	(0.760)	
Household Head: Married	0.942	0.946	-0.004	0.941	0.939	0.002
	(0.015)	(0.015)		(0.015)	(0.016)	
Household Head: Completed Primary School	0.529	0.496	0.033	0.424	0.428	-0.004
	(0.032)	(0.032)		(0.032)	(0.033)	
Household Head: Occupation Cultivator	0.533	0.446	0.088*	0.408	0.380	0.028
	(0.032)	(0.032)		(0.032)	(0.032)	
Household Head: Occupation Labor	0.346	0.400	-0.054	0.412	0.424	-0.012
	(0.031)	(0.032)		(0.032)	(0.033)	
Landholding (acres)	0.545	0.497	0.048	0.412	0.455	-0.042
	(0.038)	(0.030)		(0.031)	(0.033)	
Joint Significance‡		12.240			12.410	
		[0.269]			[0.259]	

Notes:
Panel A uses village census data collected in 2007–2008 (see Mitra, Mookherjee, Torero, and Visaria, 2015). In Panel B the sample is restricted to recommended/group-forming (Treatment and Control 1) households in TRAIL/GBL villages. Standard errors are in parentheses. The p-value for the test of joint significance of all variables in explaining assignment to treatment is in square brackets. ‡: $\chi^2(12)$. †: Difference = Treatment – Control 1. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 3: Credit Market Characteristics (before experiment)

	All Loans (1)		Agricultural Loans (2)	
Household had borrowed	0.69		0.59	
Total Borrowing†	6222	(10140)	4953	(8608)
Proportion of Loans by Source‡				
Informal Lenders	0.65		0.66	
Family and Friends	0.05		0.03	
Cooperatives	0.23		0.24	
Government Banks	0.05		0.05	
Annualized Interest Rate by Source (percent)				
Informal Lenders	26.57	(24.14)	26.36	(24.51)
Family and Friends	20.53	(15.09)	19.84	(16.32)
Cooperatives	15.41	(3.07)	15.62	(3.15)
Government Banks	11.91	(4.30)	11.83	(4.65)
Duration by Source (days)				
Informal Lenders	123.63	(27.54)	122.52	(20.29)
Family and Friends	168.92	(103.61)	174.13	(101.31)
Cooperatives	323.53	(91.19)	320.19	(93.97)
Government Banks	299.67	(108.95)	300.35	(108.74)
Proportion of Loans Collateralized by Source				
Informal Lenders	0.01		0.01	
Family and Friends	0.02		0.07	
Cooperatives	0.73		0.77	
Government Banks	0.77		0.83	

Notes:
Statistics are reported for all sample households in TRAIL and GBL villages with at most 1.5 acres of land. All loan characteristics are summarized for loans taken by the household in Cycle 1. Program loans are not included. For the interest rate summary statistics loans where the principal amount is reported equal to the repayment amount are not included. †: Total borrowing = 0 for households that do not borrow. ‡: Proportion of loans in terms of value of loans at the household level. All proportions are computed only over households that borrow. Standard deviations are in parentheses.

Table 4: Selected Crop Characteristics

	Sesame (1)	Paddy (2)	Potatoes (3)
Cultivate the crop (%)	0.49	0.69	0.64
	(0.006)	(0.006)	(0.006)
Acreage (acres)	0.22	0.47	0.31
	(0.004)	(0.006)	(0.005)
Harvested quantity (kg)	141	1175	5302
	(2.53)	(16.12)	(75.90)
Cost of production (Rs)	341	3012	7731
	(8.08)	(51.95)	(138.57)
Price (Rs/kg)	31	10	5
	(0.19)	(0.09)	(0.03)
Revenue (Rs)	1667	5554	13726
	(37.45)	(97.69)	(248.6)
Value added (Rs)	1325	2598	5938
	(32.85)	(67.12)	(145.82)
Value added per acre (Rs/acre)	6349	6568	17777
	(84.23)	(113.42)	(282.92)

Notes:
Statistics are annual averages over the 3-year study period, reported for all sample households in TRAIL and GBL villages with at most 1.5 acres of land. Standard errors are in parentheses.

Table 5: Predictions of TRAIL, GBL Effects, assuming no collusion and $L \leq n$

Treatment		*Composition* C=connected F=floaters	*Observed Interest rate*	*Repayment Rate*	*Loan/Cultivation Scale*
TRAIL	Treatment	C	r_T	p_c	$l_c(p_c r_T)$
	Control 1	C	$\frac{\rho}{p_c}$	p_c	$l_c(\rho)$
	Control 2	C, F	$\frac{\rho}{p_c}, \frac{\rho}{p_f}$	p_c, p_f	$l_c(\rho)$,$l_f(\rho)$
GBL	Treatment	CC, FF	r_T	$p_c(2-p_c), p_f(2-p_f)$	$l_c(p_c(2-p_c)r_T), l_f(p_f(2-p_f)r_T)$
	Control 1	CC, FF	$\frac{\rho}{p_c}, \frac{\rho}{p_f}$	p_c, p_f	$l_c(\rho)$,$l_f(\rho)$
	Control 2	C, F	$\frac{\rho}{p_c}, \frac{\rho}{p_f}$	p_c, p_f	$l_c(\rho)$,$l_f(\rho)$

Table 6: Selection Pattern in TRAIL Villages

	Recommended
Buy from Agent	0.014
	(0.065)
Borrow from Agent	0.148*
	(0.076)
Work for Agent	0.006
	(0.084)
Non Hindu Household	0.026
	(0.240)
Non Hindu Household × Agent Hindu	-0.099
	(0.249)
Scheduled Caste Household	0.522***
	(0.065)
Scheduled Caste Household × Agent High Caste	-0.588***
	(0.071)
Scheduled Tribe Household	-0.190
	(0.167)
Scheduled Tribe Household × Agent High Caste	0.202
	(0.218)
Constant	0.074
	(0.095)
Sample Size	1,031

Notes:
The dependent variable takes value 1 if the household was recommended by a TRAIL agent or joined a GBL group. Estimates are based on a linear probability regression. The regression is run on sample households with at most 1.5 acres of land, following the specification in equation (1) in the text. Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 7: Interest Rate Comparisons Across the TRAIL and GBL Schemes

	TRAIL (1)	TRAIL (2)	GBL (3)	Pooled (4)	Pooled (5)
Selected	0.017	0.001	0.037	0.031	0.031
	(0.017)	(0.014)	(0.043)	(0.045)	(0.046)
Own Clientele	0.046				
	(0.036)				
Selected × Own Clientele	-0.070**				
	(0.033)				
TRAIL				-0.012	-0.020
				(0.041)	(0.041)
TRAIL × Selected				-0.035	-0.019
				(0.047)	(0.048)
TRAIL × Own Clientele					0.045
					(0.037)
TRAIL × Selected × Own Clientele					-0.068**
					(0.033)
Constant	0.204***	0.210***	0.292***	0.264***	0.263***
	(0.021)	(0.019)	(0.051)	(0.046)	(0.046)
Difference					
TRAIL Selected from Own Clientele –Not Selected from Own Clientele	-0.052*				
	(0.027)				
TRAIL Selected–GBL Selected				-0.047*	
				(0.027)	
TRAIL Selected Own Clientele–GBL Selected					-0.062**
					(0.031)
TRAIL Selected Not Own Clientele–GBL Selected					-0.039
					(0.029)
Average interest rate paid on informal loans: Non-selected borrowers					
TRAIL			0.239		
GBL			0.253		
Sample Size	448	424	872	872	872

Notes:
The dependent variable is the average interest rate paid on informal production loans from traders or moneylenders, as reported in Cycle 1. Column 1 presents results from regression equation (2) and column 2 from regression equation (3). Regressions in columns 1 and 2 are run on all sample households with at most 1.5 acres in TRAIL villages and the regression in column 3 is run on all sample households with at most 1.5 acres in GBL villages. Columns 4 and 5 use all sample households in both treatment categories with at most 1.5 acres of land. Additional controls include landholding, and caste and religion dummies. Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 8: Program Impacts: Treatment Effects on Total Borrowing

	All Loans (₹) (1)	Non Program Loans† (₹) (2)	Index of dependent variables^{II} (3)
TRAIL Treatment	5088.00***	-327.70	0.24
	(845.30)	(629.30)	(0.07)
Hochberg p-value			0.03
Mean Control 1	5590.00	5590.00	
% Effect (TRAIL)	91.02	-5.86	
GBL Treatment	3660.00***	-233.00	0.21
	(879.70)	(625.60)	0.09
Hochberg p-value			0.37
Mean Control 1	4077.00	4077.00	
% Effect (GBL)	89.77	-5.72	
Sample Size	6,210	6,210	6,210

Notes:
Treatment effects are computed from regressions that follow equation (6) in the text and are run on household-year level data for all sample households with at most 1.5 acres of land. % Effect: Treatment effect as a percentage of the Mean of Control 1 group. ^{II}: In column 3 the dependent variable is an index of z-scores of the outcome variables in the panel; the p-values for treatment effects in this column are computed according to Hochberg (1988)'s step-up method to control for the family-weighted error rate across all index outcomes. †: Non-Program loans refer to loans from sources other than the TRAIL/GBL loan schemes. The complete regression results are in Table A-1. Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 9: Program Impacts: Treatment Effects in Agriculture

Panel A: Potatoes

	Cultivate (%) (1)	Acreage (Acres) (2)	Harvested quantity (Kg) (3)	Cost of Production (₹) (4)	Revenue (₹) (5)	Value Added (₹) (6)	Imputed Profit‡ (₹) (7)	Index of dependent variables II (8)
TRAIL Treatment	0.05	0.10***	991.82***	1965.49***	4088.12***	2129.44***	1962.86***	0.20
	(0.03)	(0.03)	(290.95)	(714.45)	(1234.13)	(644.51)	(630.71)	(0.06)
Hochberg p-value								0.00
Mean TRAIL Control 1	0.72	0.33	3646.12	8474.63	14285.47	5739.48	4740.89	
% Effect (TRAIL)	6.73	28.87	27.20	23.19	28.62	37.10	41.40	
GBL Treatment	0.08	0.04	405.85	1348.64	1732.50	356.25	187.28	0.09
	(0.06)	(0.04)	(430.02)	(990.31)	(1891.33)	(988.61)	(915.09)	(0.09)
Hochberg p-value								>0.999
Mean GBL Control 1	0.620	0.251	2761.127	5992.080	11014.286	4997.446	4018.796	
% Effect (GBL)	12.53	17.57	14.70	22.51	15.73	7.13	4.66	
Sample Size	6,126	6,126	6,126	6,126	6,126	6,126	6,126	6,126

Continued ...

Notes:
Treatment effects are computed from regressions that follow equation (6) in the text and are run on household-year level data for all sample households with at most 1.5 acres of land. ‡: Imputed profit = Value Added – shadow cost of labour. % Effect: Treatment effect as a percentage of the Mean of Control 1 group. II: In column 8 the dependent variable is an index of z-scores of the outcome variables in the panel; the p-values for treatment effects in this column are computed according to Hochberg (1988)'s step-up method to control for the family-weighted error rate across all index outcomes. The complete regression results are in Table A-2. Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 9 (*Continued*): Program Impacts: Treatment Effects in Agriculture

Panel B: Other Major Crops

	Sesame			Paddy			Vegetables		
	Acreage (Acres) (1)	Value Added (₹) (2)	Index of dependent variables^{II} (3)	Acreage (Acres) (4)	Value Added (₹) (5)	Index of dependent variables^{II} (6)	Acreage (Acres) (7)	Value Added (₹) (8)	Index of dependent variables^{II} (9)
TRAIL Treatment	0.04**	283.87**	0.10	0.03*	248.75	0.04	0.01*	61.92	0.05
	(0.02)	(139.33)	(0.06)	(0.02)	(191.84)	(0.03)	(0.01)	(218.27)	(0.05)
Hochberg p-value			0.39			0.39			0.38
Mean TRAIL Control 1	0.27	1519.56		0.47	2556.76		0.01	889.23	
% Effect (TRAIL)	16.69	18.68		7.21	9.73		72.69	6.96	
GBL Treatment	0.00	-265.05	-0.06	0.01	183.87	0.01	0.00	-477.38	-0.07
	(0.03)	(239.13)	(0.09)	(0.04)	(312.77)	(0.07)	(0.01)	(666.63)	(0.15)
Hochberg p-value			>0.999			0.91			>0.999
Mean GBL Control 1	0.19	1252.85		0.46	2336.84		0.02	1142.35	
% Effect (GBL)	-2.59	-21.16		2.93	7.87		-8.89	-41.79	
Sample Size	6,126	6,126	6,126	6,126	6,126	6,126	6,126	6,126	6,126

Notes:
Treatment effects are computed from regressions that follow equation (6) in the text and are run on household-year level data for all sample households with at most 1.5 acres of land. ‡: Imputed profit = Value Added – shadow cost of labour. % Effect: Treatment effect as a percentage of the Mean of Control 1 group. ^{II}: In columns 3, 6 & 9, the dependent variables are indices of z-scores of the outcome variables related to that crop; the p-values for treatment effects in these columns are computed according to Hochberg (1988)'s step-up method to control for the family-weighted error rate across all index outcomes. The complete regression results corresponding to columns 1–3 are in Table A-3, to columns 4–6 are in Table A-4, and to columns 7–9 are in Table A-5. Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 10: Program Impacts: Effects on Farm Value Added and Rates of Return

	Farm Value Added (₹)	Rate of Return	
		Potato Cultivation	Farm Value Added
	(1)	(2)	(3)
TRAIL Treatment	2730.73***	1.08***	1.20***
	(659.09)	(0.02)	(0.07)
Mean TRAIL Control 1	10705.02		
% Effect	25.51		
GBL Treatment	280.66	0.26***	-0.15
	(1141.11)	(0.03)	(2.46)
Mean GBL Control 1	9729.48		
% Effect	2.88		
TRAIL vs GBL p-value		0.00	0.63
Sample Size	6,216		

Notes:
The analysis is run on household-year level data for all sample households with at most 1.5 acres of land. The rate of return is defined as the ratio of the treatment effect on value added to the treatment effect on cost, estimated using the regression specification in equation (6) in the text. The full set of results corresponding to the treatment effects in column 1 are in Table A-6. In column (1), standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. In columns 2 and 3, standard errors are cluster-bootstrapped with 600 replications. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 11: Loan Performance

	Repayment (1)	Take up (2)	Continuation (3)
TRAIL	0.008	0.112***	0.112***
	(0.008)	(0.014)	(0.014)
Constant	1.006***	0.823***	0.820***
	(0.005)	(0.018)	(0.018)
Mean GBL Control 1	0.956	0.747	0.694
Sample Size	2,406	3,226	3,512

Notes:
The sample consists of household-cycle level observations of treatment households in TRAIL and GBL villages. The dependent variable in column (1) takes value 1 if a borrowing household repaid a loan taken in the cycle within 30 days of the due date, and that in columns (2) and (3) takes value 1 if the household took the program loan. In column (1) the sample consists of households that had taken a program loan in that cycle, in column (2) it consists of households that were eligible to take the program loan in that cycle, and in column (3) it consists of all households that were eligible to receive program loans in Cycle 1. Explanatory variables include cycle dummies and landholding. Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 12: Treatment Effects on Non-Farm Income

	Rental income (₹) (1)	Sale income (₹) (2)	Labour income (₹) (3)	Wage employment† (Hours) (4)	Self employment† (Hours) (5)	Reported profits (₹) (6)	Current value of business (₹) (7)	Total non-farm non-farm income (₹) (8)	Index of dependent variables^II (9)
TRAIL Treatment	5.62	159.65	334.55	-0.06	7.76	2223.94	4051.75	2723.75	0.05
	(641.33)	(166.21)	(3574.43)	(2.95)	(5.47)	(1712.27)	(3997.19)	(3767.84)	(0.04)
Hochberg p-value									0.34
Mean TRAIL Control 1	1928.28	953.51	36266.34	36.79	122.43	5810.96	11317.95	44959.08	
% Effect (TRAIL)	0.29	16.74	0.92	-0.17	6.34	38.27	35.80	6.06	
GBL Treatment	580.13	-4.33	-2037.68	-1.99	5.08	2234.12	4881.96	772.23	0.04
	(590.28)	(251.51)	(4856.63)	(3.38)	(5.86)	(2635.39)	(7115.96)	(5235.39)	(0.05)
Hochberg p-value									> 0.999
Mean GBL Control 1	1291.95	689.44	43545.21	44.24	125.84	6347.45	11157.32	51874.05	
% Effect (GBL)	44.90	-0.63	-4.68	-4.50	4.04	35.20	43.76	1.49	
Sample Size	6,123	6,123	6,123	6,123	6,123	6,123	6,123	6,123	6,123

Notes:
The regressions are run on household-year level data for all sample households with at most 1.5 acres of land. The regression specification follows equation (6) in the text. †: Wage and Self employment hours in the last 2 weeks. II: In column 9 the dependent variable is an index of z-scores of the outcome variables in the panel; the p-values for treatment effects in this column are computed according to Hochberg (1988)'s step-up method to control for the family-weighted error rate across all index outcomes. The complete regression results are in Table A-7. Standard errors in parenthesis are clustered at the network level, as described in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 13: Treatment Effects for Transactions with TRAIL agent

Panel A: Input Purchase

	Buy from agent	Share buy from agent	Input Price (Rs/unit)				
			Fertilizer	Outside Seed	Pesticide	Power tiller	Water/irrigation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TRAIL Treatment	0.00	0.00	0.09	1.71	-29.35	-29.98***	108.86
	(0.01)	(0.01)	(0.79)	(1.13)	(48.47)	(3.41)	(111.70)
Mean Control 1	0.08	0.06	15.77	24.82	536.80	211.18	72.30
Sample Size	17,974	17,830	2,915	2,400	3,840	1,989	1,828

Panel B: Output Sales

	Sell to agent	Share sell to agent	Output Price (Rs/kg)		
			Potato	Paddy	Sesame
	(1)	(2)	(3)	(4)	(5)
TRAIL Treatment	0.02	0.03	-0.00	0.40	-0.06
	(0.04)	(0.04)	(0.15)	(0.22)	(0.48)
Mean Control 1	0.19	0.15	4.57	10.13	30.59
Sample Size	4,310	4,105	2,030	793	1,281

Panel C: Borrowing

	Borrowed from Agent	Share loan from Agent	APR
	(1)	(2)	(3)
TRAIL Treatment	-0.08**	-0.04**	0.01
	(0.04)	(0.01)	(0.02)
Mean Control 1	0.16	0.05	0.14
Sample Size	1,966	1,966	5,491

Notes:
The regressions are run on household-year level data for sample households with at most 1.5 acres of land in TRAIL villages. In Panel C only borrowing for agricultural purposes is considered. †: Purchased inputs from, sold output to or borrowed from agent during the survey period. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$. Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text.

Table A-1: Program Impacts: Treatment Effects on Total Borrowing

	All Loans (₹) (1)	Non Program Loans† (₹) (2)	Index of dependent variables[II] (3)
TRAIL	1,164.548	1,133.889	0.122
	(787.037)	(787.366)	(0.084)
TRAIL × Control 1	66.597	80.521	0.004
	(535.236)	(530.416)	(0.057)
TRAIL × Treatment	5,154.400***	-247.189	0.246***
	(702.388)	(441.819)	(0.055)
GBL × Control 1	377.297	299.923	0.008
	(609.686)	(607.220)	(0.079)
GBL × Treatment	4,037.589***	66.655	0.221**
	(976.029)	(751.630)	(0.089)
Landholding	9,707.932***	8,900.170***	0.981***
	(702.183)	(677.597)	(0.075)
Year 2	-643.800***	-1,210.517***	-0.100***
	(192.401)	(167.645)	(0.018)
Year 3	-940.637***	-953.871***	-0.101***
	(298.528)	(263.868)	(0.030)
Information Village	884.432	749.175	0.085
	(685.156)	(628.099)	(0.071)
Constant	36.753	681.439	-0.525***
	(662.523)	(627.138)	(0.070)
Treatment Effects			
TRAIL Treatment	5088.00***	-327.70	0.24
	(845.30)	(629.30)	(0.07)
Hochberg p-value			0.03
Mean Control 1	5590.00	5590.00	
% Effect (TRAIL)	91.02	-5.86	
GBL Treatment	3660.00***	-233.00	0.21
	(879.70)	(625.60)	0.09
Hochberg p-value			0.37
Mean Control 1	4077.00	4077.00	
% Effect (GBL)	89.77	-5.72	

Continued ...

Table A-1 (*continued*):
Program Impacts: Treatment Effects on Total Borrowing

	All Loans (₹) (1)	Non Program Loans† (₹) (2)	Index of dependent variables[II] (3)
Selection Effects			
TRAIL Selection	66.60	80.52	
	(535.20)	(530.40)	
GBL Selection	377.30	299.90	
	(609.70)	(607.20)	
Sample Size	6,210	6,210	6,210

Notes:
The regressions are run on household-year level data for sample households with at most 1.5 acres of land in TRAIL villages.†: Non-Program loans are loans from sources other than the TRAIL or GBL schemes. [II]: Column 3 presents the TRAIL and GBL treatment effects in a regression on treatment of an index of z-scores of the outcome variables in the panel following Kling, Liebman, and Katz (2007); p-values for this regression are reported using Hochberg's step-up method to control the FWER across all index outcomes. Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table A-2: Program Impacts: Treatment Effects in Potato Cultivation

	Cultivate (1)	Land planted (2)	Harvested quantity (3)	Cost of production (4)	Revenue (5)	Value added (6)	Imputed profit‡ (7)	Index^II (8)
TRAIL	0.048	0.027	233.98	639.53	286.954	-362.339	-369.526	0.033
	(0.046)	(0.042)	(475.374)	(1,082.408)	(1,932.801)	(903.717)	(821.155)	(0.094)
TRAIL × Control 1	0.088***	0.022	281.835	817.873	1,013.53	186.837	78.263	0.066
	(0.032)	(0.024)	(269.972)	(648.271)	(1,045.196)	(489.944)	(474.895)	(0.051)
TRAIL × Treatment	0.136***	0.118***	1,273.651***	2,783.365***	5,101.647***	2,316.282***	2,041.124***	0.268***
	(0.032)	(0.025)	(268.835)	(649.939)	(1,069.931)	(526.479)	(499.567)	(0.051)
GBL × Control 1	0.041	0.012	136.212	45.91	228.484	199.438	52.757	0.027
	(0.055)	(0.042)	(481.461)	(1,068.123)	(2,018.578)	(1,029.497)	(953.097)	(0.099)
GBL × Treatment	0.118**	0.056	542.058	1,394.55	1,960.99	555.693	240.042	0.12
	(0.055)	(0.041)	(460.306)	(1,062.122)	(1,911.304)	(905.218)	(802.121)	(0.094)
Landholding	0.394***	0.503***	5,539.544***	11,676.533***	23,609.744***	11,819.640***	10,946.481***	1.164***
	(0.030)	(0.035)	(405.045)	(903.158)	(1,704.415)	(871.927)	(831.216)	(0.077)
Year 2	-0.054***	-0.023***	-468.281***	-356.495**	3,657.021***	4,133.377***	3,981.161***	0.100***
	(0.009)	(0.006)	(74.077)	(166.681)	(472.792)	(422.135)	(420.576)	(0.019)
Year 3	-0.053***	-0.011	-174.812*	2,535.319***	1,390.669***	-1,017.936***	-1,357.850***	-0.012
	(0.009)	(0.008)	(90.809)	(332.263)	(395.600)	(273.196)	(288.992)	(0.018)
Information Village	0.016	0.049	543.134	1,025.54	1,701.64	656.64	583.714	0.087
	(0.037)	(0.034)	(382.360)	(880.350)	(1,547.000)	(718.304)	(653.564)	(0.076)
Constant	0.428***	0.041	557.868	476.943	-265.817	-816.399	-1,102.31	-0.458***
	(0.046)	(0.034)	(396.221)	(860.205)	(1,541.297)	(741.773)	(684.114)	(0.078)
Treatment Effects								
TRAIL Treatment	0.05	0.10***	991.82***	1965.49***	4088.12***	2129.44***	1962.86***	0.20
	0.03	(0.03)	(290.95)	(714.45)	(1234.13)	(644.51)	(630.71)	(0.06)
Hochberg p-value								0.00
Mean TRAIL Control 1	0.72	0.33	3646.12	8474.63	14285.47	5739.48	4740.89	
% Effect (TRAIL)	6.73	28.87	27.20	23.19	28.62	37.10	41.40	
GBL Treatment	0.08	0.04	405.85	1348.64	1732.50	356.25	187.28	0.09
	(0.06)	(0.04)	(430.02)	(990.31)	(1891.33)	(988.61)	(915.09)	(0.09)
Hochberg p-value								> 0.999
Mean GBL Control 1	0.62	0.25	2761.13	5992.08	11014.29	4997.45	4018.80	
% Effect (GBL)	12.53	17.57	14.70	22.51	15.73	7.13	4.66	

Continued . . .

Table A-2 (*Continued*): Program Impacts: Treatment Effects in Potato Cultivation

	Cultivate (1)	Land planted (2)	Harvested quantity (3)	Cost of production (4)	Revenue (5)	Value added (6)	Imputed profit‡ (7)	Index$^{\text{II}}$ (8)
				Selection Effects				
TRAIL Selection	0.09**	0.02	281.83	817.87	1013.53	186.84	78.26	
	(0.03)	(0.02)	(269.97)	(648.27)	(1045.20)	(489.94)	(474.90)	
GBL Selection	0.04	0.01	136.21	45.91	228.48	199.44	52.76	
	(0.05)	(0.04)	(481.46)	(1068.12)	(2018.58)	(1029.50)	(953.10)	
Sample Size	6,126	6,126	6,126	6,126	6,126	6,126	6,126	6,126

Notes:
Sample restricted to households with at most 1.5 acres. ‡: Imputed profit = Value Added – shadow cost of labour.
$^{\text{II}}$: Column 8 presents the TRAIL and GBL treatment effects in a regression on treatment of an index of z-scores of the outcome variables in the panel following Kling, Liebman, and Katz (2007);
p-values for this regression are reported using Hochberg's step-up method to control the FWER across all index outcomes.
Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table A-3: Program Impacts: Treatment Effects in Sesame Cultivation

	Cultivate (1)	Land planted (2)	Harvested quantity (3)	Cost of production (4)	Revenue (5)	Value added (6)	Imputed profit‡ (7)	Index[II] (8)
TRAIL	0.068	0.037	12.069	25.796	183.2	156.735	86.951	0.085
	(0.053)	(0.033)	(12.106)	(56.535)	(277.702)	(229.331)	(195.978)	(0.098)
TRAIL × Control 1	0.103***	0.044**	10.392*	101.438***	296.428**	195.085*	129.749	0.128***
	(0.031)	(0.017)	(5.496)	(31.203)	(128.019)	(115.422)	(105.195)	(0.042)
TRAIL × Treatment	0.139***	0.089***	20.521***	131.400***	612.354***	480.128***	317.177**	0.228***
	(0.035)	(0.020)	(7.738)	(40.512)	(178.858)	(155.473)	(148.140)	(0.060)
GBL × Control 1	0.09	0.043	10.618	-19.571	252.7	271.366	185.808	0.099
	(0.057)	(0.036)	(12.710)	(56.616)	(313.800)	(267.436)	(231.216)	(0.106)
GBL × Treatment	0.071	0.038	2.448	-4.078	2.097	6.313	-18.824	0.04
	(0.058)	(0.034)	(11.188)	(55.341)	(266.832)	(219.831)	(185.844)	(0.095)
Landholding	0.388***	0.376***	110.717***	510.179***	2,836.677***	2,323.887***	1,995.774***	1.001***
	(0.028)	(0.028)	(9.401)	(38.636)	(226.429)	(201.363)	(190.094)	(0.072)
Year 2	-0.048***	-0.007	-4.592	-19.655	427.609***	447.233***	333.659***	0.045**
	(0.012)	(0.007)	(2.863)	(20.400)	(76.891)	(67.649)	(62.539)	(0.022)
Year 3	-0.028**	0.015**	-3.16	-45.526**	837.329***	883.035***	675.719***	0.130***
	(0.012)	(0.008)	(3.692)	(19.317)	(114.933)	(109.960)	(95.331)	(0.029)
Information Village	-0.035	0.032	4.17	5.933	114.804	107.835	144.169	0.035
	(0.043)	(0.027)	(9.186)	(43.385)	(215.182)	(179.237)	(152.136)	(0.076)
Constant	0.278***	-0.005	9.419	96.977*	-288.477	-384.253**	-450.407***	-0.451***
	(0.048)	(0.028)	(9.166)	(49.999)	(217.776)	(180.556)	(151.162)	(0.077)
Treatment Effects								
TRAIL Treatment	0.04	0.04**	10.13	29.96	314.76*	283.87**	186.26	0.10
	(0.03)	(0.02)	(6.92)	(50.52)	(173.53)	(139.33)	(126.29)	(0.06)
Hochberg p-value								0.39
Mean TRAIL Control 1	0.58	0.27	81.62	436.91	1957.50	1519.56	1080.80	
% Effect (TRAIL)	6.13	16.69	12.41	6.86	16.08	18.68	17.23	
GBL Treatment	-0.02	0.00	-8.17	15.49	-250.60	-265.05	-204.63	-0.06
	(0.06)	(0.03)	(10.78)	(47.67)	(274.54)	(239.13)	(207.93)	(0.09)
Hochberg p-value								> 0.999
Mean GBL Control 1	0.48	0.19	60.85	258.88	1513.14	1252.85	866.29	
% Effect (GBL)	-3.94	-2.59	-13.43	5.98	-16.56	-21.16	-23.62	

Continued ...

Table A-3 (*Continued*): Program Impacts: Treatment Effects in Sesame Cultivation

	Cultivate (1)	Land planted (2)	Harvested quantity (3)	Cost of production (4)	Revenue (5)	Value added (6)	Imputed profit‡ (7)	Index$^{\text{II}}$ (8)
Selection Effects								
TRAIL Selection	0.10***	0.04**	10.39*	101.44***	296.43**	195.09*	129.75	
	(0.03)	(0.02)	(5.50)	(31.20)	(128.02)	(115.42)	(105.19)	
GBL Selection	0.09	0.04	10.62	-19.57	252.74	271.41	185.85	
	(0.06)	(0.04)	(12.71)	(56.62)	(313.80)	(267.43)	(231.21)	
Sample Size	6,126	6,126	6,126	6,126	6,126	6,126	6,126	6,126

Notes:
Sample restricted to households with at most 1.5 acres. ‡: Imputed profit = Value Added – shadow cost of labour.
$^{\text{II}}$: Column 8 presents the TRAIL and GBL treatment effects in a regression on treatment of an index of z-scores of the outcome variables in the panel following Kling, Liebman, and Katz (2007);
p-values for this regression are reported using Hochberg's step-up method to control the FWER across all index outcomes.
Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table A-4: Program Impacts: Treatment Effects in Paddy Cultivation

	Cultivate (1)	Land planted (2)	Harvested quantity (3)	Cost of production (4)	Revenue (5)	Value added (6)	Imputed profit‡ (7)	Index^{II} (8)
TRAIL	-0.034	-0.012	-39.852	-226.78	-324.513	-60.24	-22.91	-0.038
	(0.035)	(0.033)	(66.529)	(331.330)	(459.469)	(303.012)	(175.389)	(0.050)
TRAIL × Control 1	0.103***	0.016	27.066	109.599	144.146	9.512	-48.205	0.043
	(0.029)	(0.018)	(25.878)	(149.994)	(262.120)	(187.852)	(91.763)	(0.027)
TRAIL × Treatment	0.097***	0.050**	44.996	313.575*	588.870**	258.271	89.846	0.085***
	(0.027)	(0.021)	(29.005)	(184.781)	(271.730)	(216.399)	(127.779)	(0.028)
GBL × Control 1	0.034	0.044	132.602	424.29	481.612	88.125	114.112	0.079
	(0.041)	(0.041)	(96.651)	(508.574)	(637.948)	(310.133)	(204.966)	(0.072)
GBL × Treatment	0.048	0.058	118.448	438.343	661.346	271.939	-26.078	0.086
	(0.042)	(0.037)	(76.927)	(402.206)	(543.017)	(309.932)	(187.024)	(0.059)
Landholding	0.480***	0.888***	1,071.761***	5,027.445***	9,162.369***	4,243.487***	988.405***	1.119***
	(0.026)	(0.030)	(51.637)	(267.828)	(433.153)	(290.695)	(185.378)	(0.043)
Year 2	-0.021**	-0.014	669.726***	546.806***	843.830***	146.404	-347.247***	0.126***
	(0.008)	(0.009)	(41.707)	(100.974)	(226.868)	(192.187)	(75.508)	(0.017)
Year 3	-0.154***	-0.113***	559.714***	-527.604***	961.129***	1,339.555***	824.932***	0.103***
	(0.016)	(0.016)	(41.089)	(131.255)	(294.551)	(209.331)	(126.744)	(0.030)
Information Village	-0.049*	-0.047*	-82.583*	-518.931**	-410.495	99.968	40.88	-0.063*
	(0.029)	(0.024)	(47.795)	(241.223)	(335.587)	(225.053)	(137.210)	(0.037)
Constant	0.540***	0.128***	-271.841***	981.688***	1,027.858**	136.769	-463.969**	-0.373***
	(0.030)	(0.033)	(75.022)	(344.480)	(516.445)	(301.709)	(187.984)	(0.055)
Treatment Effects								
TRAIL Treatment	-0.01	0.03*	17.93	203.98	444.71*	248.75	138.04	0.04
	(0.02)	(0.02)	(33.26)	(186.07)	(241.71)	(191.84)	(145.25)	(0.03)
Hochberg p-value								0.39
Mean TRAIL Control 1	0.74	0.47	569.73	2889.84	5398.49	2556.76	93.13	
% Effect (TRAIL)	-0.82	7.21	3.15	7.06	8.24	9.73	148.22	
GBL Treatment	0.01	0.01	-14.15	14.05	179.79	183.87	-140.14	0.01
	(0.05)	(0.04)	(89.14)	(480.09)	(610.18)	(312.77)	(206.23)	(0.07)
Hochberg p-value								0.91
Mean GBL Control 1	0.69	0.46	672.89	3225.74	5513.23	2336.84	183.16	
% Effect (GBL)	2.07	2.93	-2.10	0.44	3.26	7.87	-76.51	

Continued . . .

Table A-4 (*Continued*): Program Impacts: Treatment Effects in Paddy Cultivation

	Cultivate (1)	Land planted (2)	Harvested quantity (3)	Cost of production (4)	Revenue (5)	Value added (6)	Imputed profit‡ (7)	Index^{II} (8)
Selection Effects								
TRAIL Selection	0.10***	0.02	27.07	109.60	141.93	7.30	-50.42	
	(0.03)	(0.02)	(25.88)	(149.99)	(262.27)	(187.83)	(91.80)	
GBL Selection	0.03	0.04	132.60	424.29	481.78	88.29	114.28	
	(0.04)	(0.04)	(96.65)	(508.57)	(637.91)	(310.09)	(204.96)	
Sample Size	6,126	6,126	6,126	6,126	6,126	6,126	6,126	6,126

Notes:
Sample restricted to households with at most 1.5 acres. ‡: Imputed profit = Value Added – shadow cost of labour.
^{II}: Column 8 presents the TRAIL and GBL treatment effects in a regression on treatment of an index of z-scores of the outcome variables in the panel following Kling, Liebman, and Katz (2007);
p-values for this regression are reported using Hochberg's step-up method to control the FWER across all index outcomes.
Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table A-5: Program Impacts: Treatment Effects in Vegetable Cultivation

	Cultivate (1)	Land planted (2)	Harvested quantity (3)	Cost of production (4)	Revenue (5)	Value added (6)	Imputed profit‡ (7)	Index^II (8)
TRAIL	0.012	0.003	88.486	83.127	228.098	131.641	155.548	0.062
	(0.039)	(0.009)	(92.584)	(177.654)	(757.370)	(583.733)	(480.445)	(0.147)
TRAIL × Control 1	-0.007	-0.003	-27.563	-44.968	67.035	118.814	88.354	-0.011
	(0.016)	(0.004)	(58.587)	(76.738)	(335.851)	(268.113)	(227.155)	(0.067)
TRAIL × Treatment	-0.007	0.008	1.188	38.57	216.467	180.724	87.685	0.035
	(0.015)	(0.007)	(75.123)	(96.033)	(429.927)	(351.963)	(290.452)	(0.083)
GBL × Control 1	0.041	0.01	75.409	171.098	851.27	664.995	568.878	0.159
	(0.042)	(0.011)	(70.617)	(209.109)	(968.815)	(754.565)	(643.117)	(0.172)
GBL × Treatment	0.041	0.008	59.904	145.235	340.068	187.608	85	0.092
	(0.036)	(0.008)	(52.039)	(151.764)	(638.112)	(497.756)	(409.327)	(0.119)
Landholding	0.063***	0.019***	198.739**	287.077***	1,613.235***	1,303.966***	1,088.037***	0.305***
	(0.019)	(0.007)	(82.926)	(98.480)	(528.598)	(430.623)	(383.032)	(0.102)
Year 2	-0.01	0	42.614*	48.539*	426.420**	369.477**	243.564	0.049*
	(0.008)	(0.002)	(25.574)	(28.418)	(173.982)	(161.563)	(150.399)	(0.028)
Year 3	-0.031***	-0.008***	-1.305	-34.528	273.099**	311.520**	262.120**	-0.006
	(0.008)	(0.002)	(11.983)	(26.259)	(131.588)	(132.847)	(127.142)	(0.016)
Information Village	0.041	0.013	137.428	228.98	1,114.741*	866.172*	725.251*	0.21
	(0.034)	(0.008)	(84.032)	(157.753)	(672.148)	(513.097)	(412.355)	(0.130)
Constant	0.04	0.002	-91.326	18.546	-614.614	-616.533	-607.250*	-0.250**
	(0.025)	(0.007)	(84.171)	(118.304)	(550.761)	(435.574)	(351.914)	(0.106)
Treatment Effects								
TRAIL Treatment	0.00	0.01*	28.75	83.54	149.44	61.92	-0.66	0.05
	(0.02)	(0.01)	(32.17)	(75.56)	(277.29)	(218.27)	(172.29)	(0.05)
Hochberg p-value								0.38
Mean TRAIL Control 1	0.08	0.01	142.82	307.07	1207.64	889.23	664.51	
% Effect (TRAIL)	0.87	72.69	20.13	27.20	12.37	6.96	-0.10	
GBL Treatment	0.00	0.00	-15.50	-25.86	-511.20	-477.38	-483.87	-0.07
	(0.04)	(0.01)	(62.83)	(193.58)	(857.61)	(666.63)	(570.56)	(0.15)
Hochberg p-value								>0.999
Mean GBL Control 1	0.11	0.02	135.89	404.92	1564.03	1142.35	853.06	
% Effect (GBL)	0.55	-8.89	-11.41	-6.39	-32.68	-41.79	-56.72	

Continued . . .

Table A-5 (*Continued*): Program Impacts: Treatment Effects in Vegetable Cultivation

	Cultivate (1)	Land planted (2)	Harvested quantity (3)	Cost of production (4)	Revenue (5)	Value added (6)	Imputed profit‡ (7)	Index^{II} (8)
Selection Effects								
TRAIL Selection	-0.01	0.00	-27.56	-44.97	67.04	118.82	88.36	
	(0.02)	(0.00)	(58.59)	(76.74)	(335.86)	(268.12)	(227.16)	
GBL Selection	0.04	0.01	75.41	171.10	850.79	664.51	568.40	
	(0.04)	(0.01)	(70.62)	(209.11)	(969.05)	(754.81)	(643.35)	
Sample Size	6,126	6,126	6,126	6,126	6,126	6,126	6,126	6,126

Notes:
Sample restricted to households with at most 1.5 acres. ‡: Imputed profit = Value Added – shadow cost of labour.
^{II}: Column 8 presents the TRAIL and GBL treatment effects in a regression on treatment of an index of z-scores of the outcome variables in the panel following Kling, Liebman, and Katz (2007);
p-values for this regression are reported using Hochberg's step-up method to control the FWER across all index outcomes.
Standard errors in parentheses are clustered at the network level, as described in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table A-6: Program Impacts: Treatment Effects in Aggregate Farm Value-Added

	Farm Value-Added (₹) (1)
TRAIL	-132.463
	(1,079.541)
TRAIL × Control 1	508.94
	(663.375)
TRAIL × Treatment	3,234.655***
	(715.724)
GBL × Control 1	1,224.21
	(1,234.263)
GBL × Treatment	1,021.84
	(1,098.227)
Landholding	19,690.624***
	(1,025.750)
Year 2	5,096.800***
	(518.429)
Year 3	1,517.870***
	(396.024)
Information Village	1,729.490**
	(843.608)
Constant	-1,680.730*
	(894.581)
Treatment Effects	
TRAIL Treatment	2725.72***
	(779.99)
Mean TRAIL Control 1	10705.08
% Effect (TRAIL)	57.23
GBL Treatment	-202.37
	(1205.06)
Mean GBL Control 1	9729.48
% Effect (GBL)	-2.08
Selection Effects	
TRAIL Selection	508.94
	(663.37)
GBL Selection	1224.21
	(1234.26)
Sample Size	6,126

Notes:
Sample restricted to households with at most 1.5 acres. Standard errors in parenthesis are clustered at the network level, as discussed in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table A-7: Program Impacts: Treatment Effects on Non-Agricultural Incomes

	Rental income (₹) (1)	Sale income (₹) (2)	Labour income (₹) (3)	Wage employment† (Hours) (4)	Self employment† (Hours) (5)	Reported profits (₹) (6)	Current value of business (₹) (7)	Total non-farm non-farm income (₹) (8)	Index of dependent variables[II] (9)
TRAIL	61.09	28.917	1,343.07	2.148	-0.308	-2,360.59	-1,138.29	-927.519	-0.006
	(422.720)	(0.11)	(0.24)	(0.63)	(0.08)	(1.42)	(0.45)	(0.14)	(0.043)
TRAIL × Control 1	-19.719	123.403	-12,034.712***	-6.487**	-0.768	-63.503	1,154.81	-11,994.530**	-0.065**
	(482.570)	(0.82)	(2.61)	(2.08)	(0.22)	(0.06)	(0.34)	(2.59)	(0.032)
TRAIL × Treatment	-14.097	283.049*	-11,700.165**	-6.548**	6.994	2,160.44	5,206.56	-9,270.78	-0.015
	(581.730)	(1.70)	(2.06)	(1.98)	(1.57)	(1.25)	(1.65)	(1.63)	(0.037)
GBL × Control 1	-276.913	-142.497	-7,673.93	-0.716	4.432	-1,425.54	1,440.69	-9,518.882*	-0.041
	(424.960)	(0.50)	(1.60)	(0.21)	(0.93)	(0.61)	(0.27)	(1.73)	(0.043)
GBL × Treatment	303.22	-146.831	-9,711.614**	-2.708	9.511*	808.576	6,322.65	-8,746.649*	-0.006
	(595.074)	(0.69)	(2.25)	(0.87)	(1.73)	(0.35)	(1.20)	(1.73)	(0.041)
Landholding	2,800.590***	359.961**	-2,150.01	-22.616***	38.625***	4,686.237***	11,234.866***	5,696.78	0.126***
	(400.181)	(2.36)	(0.50)	(11.47)	(10.37)	(3.36)	(4.02)	(1.29)	(0.031)
Year 2	641.826***	-47.352	9,075.755***	2.643***	10.193***	1,564.220***	-2.358	11,234.450***	0.090***
	(112.015)	(1.14)	(15.44)	(5.80)	(7.51)	(4.79)	(0.00)	(13.88)	(0.006)
Year 3	418.625***	-118.628	10,260.954***	1.362**	14.133***	1,143.929***	1,661.89	11,704.880***	0.092***
	(157.517)	(1.59)	(11.16)	(2.26)	(8.04)	(2.64)	(1.65)	(9.94)	(0.009)
Information Village	-133.051	-65.766	-691.716	-2.062	-1.899	680.897	-66.972	-209.635	-0.014
	(308.371)	(0.32)	(0.18)	(0.84)	(0.62)	(0.52)	(0.03)	(0.05)	(0.033)
Constant	330.077	726.387***	41,836.541***	51.095***	98.836***	4,863.154***	5,685.526**	47,756.158***	-0.094***
	(383.120)	(3.44)	(10.52)	(20.10)	(26.26)	(3.12)	(2.49)	(9.89)	(0.033)
Treatment Effects									
TRAIL Treatment	5.62	159.65	334.55	-0.06	7.76	2223.94	4051.75	2723.75	0.05
	(641.33)	(166.21)	(3574.43)	(2.95)	(5.47)	(1712.27)	(3997.19)	(3767.84)	(0.04)
Hochberg p-value									0.34
Mean TRAIL Control 1	1928.28	953.51	36266.34	36.79	122.43	5810.96	11317.95	44959.08	
% Effect (TRAIL)	0.29	16.74	0.92	-0.17	6.34	38.27	35.80	6.06	
GBL Treatment	580.13	-4.33	-2037.68	-1.99	5.08	2234.12	4881.96	772.23	0.04
	(590.28)	(251.51)	(4856.63)	(3.38)	(5.86)	(2635.39)	(7115.96)	(5235.39)	(0.05)
Hochberg p-value									> 0.999

Continued . . .

Table A-7 (*Continued*): Program Impacts: Treatment Effects on Non-Agricultural Incomes

	Rental income (₹) (1)	Sale income (₹) (2)	Labour income (₹) (3)	Wage employment† (Hours) (4)	Self employment† (Hours) (5)	Reported profits (₹) (6)	Current value of business (₹) (7)	Total non-farm non-farm income (₹) (8)	Index of dependent variables^{II} (9)
Mean GBL Control 1	1291.95	689.44	43545.21	44.24	125.84	6347.45	11157.32	51874.05	
% Effect (GBL)	44.90	-0.63	-4.68	-4.50	4.04	35.20	43.76	1.49	
Selection Effects									
TRAIL Selection	-19.72	123.40	-12034.71**	-6.49**	-0.77	-63.50	1154.81	-11994.53**	
	(482.57)	(149.80)	(4616.72)	(3.11)	(3.46)	(1088.40)	(3445.49)	(4627.36)	
GBL Selection	-276.91	-142.50	-7673.93	-0.72	4.43	-1425.54	1440.69	-9518.88*	
	(424.96)	(285.69)	(4809.56)	(3.33)	(4.76)	(2343.83)	(5316.93)	(5492.16)	
Sample Size	6,123	6,123	6,123	6,123	6,123	6,123	6,123	6,123	6,123

Notes:
Sample restricted to households with at most 1.5 acres. ‡: Imputed profit = Value Added – shadow cost of labour.
^{II}: Column 8 presents the TRAIL and GBL treatment effects in a regression on treatment of an index of z-scores of the outcome variables in the panel following Kling, Liebman, and Katz (2007);
p-values for this regression are reported using Hochberg's step-up method to control the FWER across all index outcomes.
Standard errors in parentheses are clustered at the network level, as discussed in footnote 28 in the text. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.