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

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Abstract

Recent evaluations have found that traditional microloans have insignificant impacts on incomes and output. We examine if this is because microfinance institutions are unable to target productive borrowers. We conduct a randomized field experiment in West Bengal with a different method of borrower selection for individual liability loans: this was delegated to local trader-lender agents 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, we find that 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. Differences in selection across observable borrower characteristics correlated with productivity accounts for at least 14% of this difference. Repayment rates were equally high in both schemes, while TRAIL had higher take-up rates and lower administrative costs.

Key words: Agricultural Finance, Agent-based Lending, Group Lending, Selection, Repayment

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1 Introduction

Microcredit was famously heralded as a solution to global poverty; yet across a large number of experimental evaluations there has been no evidence of significant impacts on borrower incomes or production (see Kaboski and Townsend, 2011, Banerjee, Karlan, and Zinman, 2015). This is true not only for the traditional group-based lending schemes (where selection, monitoring and enforcement is devolved to borrower groups) that have been evaluated most commonly, but also for variants with individual liability loans (see Giné and Karlan, 2014, Attanasio, Augsburg, De Haas, Fitzsimons, and Harmgart, 2015). Other experiments have found that when the rigid repayment schedules that restrict borrowers' project choice are relaxed, microloans increase farm activity and business incomes, but at the cost of increased default rates(Field, Pande, Papp, and Rigol, 2013, Feigenberg, Field, and Pande, 2013, Beaman, Karlan, Thuysbaert, and Udry, 2015).[1] No microcredit variation thus far has achieved a significant average treatment effect on production or borrower incomes, while maintaining high repayment rates. The reasons for this are not well understood. It is also not known whether alternative variants would be more likely to succeed.

In this paper we explore the hypothesis that traditional group-based microfinance schemes fail to increase borrower incomes partly because they do not manage to screen out unproductive borrowers. Since unproductive borrowers are more likely to default, they pay high interest rates in the informal credit market. Therefore they have a strong incentive to apply for MFI loans. Unlike informal lenders, MFI loan officers lack fine-grained information about the risk and productivity characteristics of poor borrowers, and cannot screen them with sufficient precision. Moreover, group lending schemes are rarely as sophisticated in practice as typically modelled in theoretical formulations (Ghatak, 1999, 2000): they rarely offer a menu of credit contracts within the same village in order to induce different borrower groups to sort according to their type.

These considerations motivate experimentation with alternative methods of selecting borrowers: formal financial institutions or MFIs could collaborate with local informal lenders and tap into their knowledge of the characteristics of different borrowers within the community. This paper considers an alternative variant of microfinance called TRAIL (Trader Agent Intermediated Loans), in which the lender delegates borrower selection to an agent randomly chosen from among the informal trader/lenders in the community. The agent is incentivized by commissions that depend on interest paid by recommended clients. This could motivate the agent to select borrowers who are less likely to default. Under the plausible hypothesis that default risk and productivity are negatively correlated, this could result in a borrower pool that is more productive.

To test this idea, we devised and conducted a field experiment in two districts of West Bengal in India. The experiment implements a version of TRAIL and compares its effects with a version of group-based lending (GBL). Villages were randomly assigned to the TRAIL or the GBL scheme. In each TRAIL village an agent was randomly selected from a list of established trader-lenders within the village, and asked to recommend as potential borrowers 30 village residents who owned no more than 1.5 acres of cultivable land. A random subset of these recommended households were offered TRAIL loans. The agent was promised a commission equal to 75 percent of the interest

[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.

payments received at the end of each 4-month loan cycle. He incurred penalties for borrower defaults, via termination clauses and the forfeiture of modest deposits posted upfront.

Shree Sanchari, a Kolkata-based MFI, implemented the GBL scheme in which village residents from households owning less than 1.5 acres of cultivable land were invited to form 5-member groups. Consistent with standard MFI practices in rural West Bengal, groups were required to meet with loan officers each month and make regular savings deposits over a six-month period before the loan scheme began.[2] A random subset of the groups that completed the six-month initiation process were offered joint liability loans. The monthly group meetings and savings continued throughout the loan cycle. As described further below, GBL loans featured the same interest rate, loan duration, growth in credit access and covariate risk insurance as the TRAIL loans. Just as in the TRAIL intervention, the MFI received a commission equal to 75 percent of the interest payments received from GBL borrowers. Neither the TRAIL agent or the MFI were responsible for providing loan capital: we provided this from our research funds.

Unlike the GBL loans where groups were jointly liable for repayment, TRAIL borrowers were individually liable for their loans. We designed the TRAIL loan contracts this way because local lenders had no experience lending to groups. Traders and moneylenders who were offered the opportunity to become the TRAIL agent were only interested in participating if the borrowers were individually liable to repay their loans. Apart from the selection mechanism and 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 first 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 amount to be repaid 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 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. This was expected to incentivize borrowers to repay the loan.

The experiment ran for three years from October 2010 to September 2013, allowing us to examine both short- and intermediate-term impacts of the program. We conducted household surveys every four months and thus have high-quality high-frequency data about sample households' cultivation decisions, harvest and sales of all crops grown. There was no attrition in the sample over time. Further details about the context and experimental design are provided in Section 2.

The difference in liability rules between the TRAIL and GBL schemes undoubtedly complicates the task of making inferences about the role of borrower selection in the differential performance of the two schemes. The type of liability could affect borrower incentives and generate differences in outcomes even if the borrowers were identical in both schemes. By its very nature, joint liability

[2] Many group-lending schemes in different parts of the world require that members save regularly for a preassigned duration or meet a savings target before they can begin to borrow. Many MFI officials argue that this helps to improve borrower selection and builds the financial discipline required to repay regularly.

raises the effective interest rate that any given borrower faces because he or she may be called upon to pay up on behalf of a defaulting group member. This can limit borrower incentives to expand the scale of borrowing. Equally, to avoid incurring this "joint liability tax", group members might monitor each other and prevent risk-taking, which could reduce the adoption of high-value high-risk cash crops. These incentive effects could generate significant differences in the impacts of the two schemes on borrowers' crop output and value added. Other features of traditional group lending such as mandated attendance in frequent group meetings and savings targets could generate additional selection and incentive effects. Our version of GBL also includes these features, whereas TRAIL does not, making it even more difficult to identify underlying causal reasons for difference in the performance of the two schemes.

To address this, we develop a theoretical model of borrower heterogeneity and incentives. This allows us to test our hypotheses about underlying causal mechanisms and to disentangle the role of selection and incentive differences. We assume borrowers have heterogeneous ability, where ability is negatively correlated with default risk and positively with high productivity. Ability can be observed by members of the local community, not by MFIs (or researchers). TRAIL agents are motivated to select high ability borrowers. In contrast, the GBL scheme attracts both low and high ability borrowers. The effect of these selection differences are compounded by differential incentive effects: the GBL loans have a smaller impact on borrowing, cultivation and farm incomes for a borrower of any given ability because the joint liability tax raises the effective interest rate. Both differences work in the same direction: TRAIL loans have a larger average treatment effect on production and farm incomes than GBL loans do.

The model generates a number of testable predictions under the additional hypothesis that observable characteristics such as caste, religion, and gender of household head are significantly correlated with ability. Specifically, the model imposes restrictions on the pattern of correlations of such characteristics with informal interest rates, the scale of borrowing, cultivation and farm incomes either before or after the interventions. In addition, it predicts that TRAIL treatment effects will vary across households with different characteristics in particular ways. We test these predictions in our data, and then obtain lower bound estimates of the contribution of selection differences *per se* to differences in average treatment effects between the TRAIL and GBL schemes. This controls for incentive effects (or any other factor such as differences in trust or social capital) that might generate differential impacts for borrowers with a given set of observable characteristics.

Our main findings are that the TRAIL loans generated significant average treatment effects (ATE) on production and incomes, whereas GBL loans did not. The ATE differences between the two schemes are both large in magnitude (e.g., the farm income impact difference is about 25% of mean farm income) and statistically significant (with a p-value of 0.077). Consistent with the predictions of the model, TRAIL agents tended to recommend borrowers who borrowed, cultivated and earned more, and paid lower interest rates on informal loans taken before the schemes were introduced. The recommended households were less likely to be low-caste or non-Hindu, were less likely to belong to a female-headed household, and had higher landholding on average (while remaining below the mandated 1.5 acre threshold). Households with these characteristics experienced significantly larger treatment effects of TRAIL loans, while the treatment effects of GBL loans were not large enough to be precisely estimated. Our estimates imply gender, religion and caste selection differences account for at least 14% of the overall difference between TRAIL and GBL ATEs. Part of the success of the TRAIL scheme may have owed to superior incentives it provided borrowers to increase the cultivation of high-value high-risk crops (such as potatoes) (Fischer, 2013), or differences in trust or social capital. We are unable to quantify the contribution of such factors.

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 find no evidence that TRAIL agents siphoned off the benefits of treated borrowers by manipulating the terms of other trading relationships with them. Conversely, we also do not find evidence that the agent helped create these large effects by providing input subsidies to recommended borrowers, or marketing advice enabling them to realize higher prices for output sales.[4] Finally, the administrative costs of the TRAIL scheme were lower than those of the GBL scheme. The reason is that 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 were redundant in TRAIL. Hence with regard to financial performance, TRAIL performed at least as well on all dimensions as GBL.

Our paper contributes to the literature by providing a possible explanation for why traditional microcredit had disappointingly small impacts on borrower productivity and incomes: its failure to target productive borrowers.[5] 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 third-party community leaders in their setting and the TRAIL agents in our setting play a similar role by screening out less productive loan applicants. An additional contribution is our design and implementation of an alternative variant, that generated large and significant impacts on production and incomes, while maintaining the high repayment rates of traditional microcredit.

A few qualifications are in order. The scale of our intervention was smaller than most other microcredit experiments, since only ten loans were offered in each village. It is not possible to project these results to estimate the consequences of a larger scale intervention.[6] Also, our analysis is restricted to impacts on production and incomes; we do not examine impacts on consumption smoothing, liquidity management, investment or social empowerment. For these reasons we do not argue that the TRAIL scheme is welfare-superior to existing group-lending schemes. Instead, our objective is to understand better whether selection can explain (some of) the failure of group-based lending to generate high growth impacts, and to test a promising alternative.

[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 the GBL scheme the take-up rate was 88% to begin with, and fell to 49% by the end of the third year.

[4] Although TRAIL agents rented out power tillers at a lower price to TRAIL borrowers, this accounts for less than 5% of the TRAIL impact on farm incomes.

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

[6] The small scale of our interventions also imply that spill-overs on non-beneficiaries in the experimental villages were unlikely.

2 Experimental Design and Data

Our field experiment was conducted in Hugli and West Medinipur in the state of West Bengal, India. These two districts are among the largest producers of potatoes in West Bengal, which itself produces about a third of all the potato output in India. As shown below, potatoes are a particularly high-value crop. It was expected that microcredit would finance potato cultivation and thereby increase incomes and productivity. To this end, the loan product had several features to facilitate its use for potato cultivation. The project team provided the funds for all loans in the two schemes. The TRAIL scheme was implemented in 24 randomly selected villages. One agent was randomly chosen from the traders/moneylenders operating in each village and asked to recommended potential borrowers for the loans. In another 24 villages, we collaborated with a local MFI to implement the GBL scheme.[7] 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 each scheme, 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 entire amount that was due became eligible for a 33 percent larger loan, on the same terms as before. 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.[8] Both schemes had an in-built index insurance scheme, according to which the required repayment would be 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 the TRAIL 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 the method of borrower selection in the TRAIL scheme, 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.

The scale of our interventions was also different from many of the other experimental interventions in the literature. We rationed loan offers to 10 borrowers in each village. In contrast, the scale of most other interventions was determined by the demand for the loan product: any eligible individual in the treatment slum or village could participate in the loan scheme. The impacts estimated in those studies combine selection and loan treatment effects, and are interpreted as

[7]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. The treatment effects of the GRAIL program will be analysed in a separate paper.

[8]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.

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, estimates of loan treatment effects control for "selection" into the scheme (either through recommendation by a TRAIL agent or through participation in a GBL group). This is possible because only a subset of households who were recommended (in the TRAIL villages) or joined groups (in the GBL villages) were offered the program loans. In TRAIL villages, the agent recommended 30 individuals for loans, and 10 of these were randomly chosen through a public lottery to receive them. In GBL villages, two of the groups that had survived a 6-month initiation period were randomly chosen, and their members offered loans. The treatment effects are then estimated by differences in outcomes between those randomly chosen to receive a loan offer (we call these Treatment households in what follows), and those who were recommended or formed a group but unlucky in the lottery and did not receive the loan offer (we call these Control 1 households). This is similar to the analysis of loan treatment effects in Karlan and Zinman (2011), where loan assignment was randomized among borrowers deemed marginally creditworthy by a credit scoring algorithm.[9]

The villages where the experiment was conducted had an average of 350 households per village. Sixty percent of the households had access to electricity, 78% of villages had a primary school, 24% had a primary health centre, 15% had a bank branch and 35% of the villages had access to a metalled road. Households had 5 members on average. The majority of the households were Hindu, among whom the low and the high castes were equally distributed. The average landholding was 0.75 acres. 95% of households had male heads, 48% of the household heads had completed primary schooling and 47% of them reported their primary occupation as cultivation (see Table 2).

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.[10] The agent was asked to recommend as potential borrowers 30 village residents who owned no more than 1.5 acres of agricultural land. In October 2010, our project officer selected 10 out of these 30 individuals in a lottery conducted in the presence of village leaders. Loan officers visited the treatment households in their homes to explain the loan terms and later to disburse the loan if it was accepted.

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

[9] We also use the data on Control 2 households in our analysis; the difference between Control 1 and Control 2 households tells us how those who participated in the scheme differed from those who did not.

[10] 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.

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.[11]

A potential concern with the TRAIL intervention is that agents could have extracted 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 (suggested that they default, and divided up the loan funds instead) or coerced borrowers to repay. The scheme contained several features to help guard against these possibilities. All loan transactions took place directly between project loan officers and the borrower. The agent could recommend only landless and marginal landowners (households owning $\leq$ 1.5 acres), and the interest rate was fixed and communicated clearly to all borrowers. As we discuss in Section 3, the agent's borrower selection pattern would have been qualitatively similar even if collusion were possible. Also, as we shall see in Section 4.3.3, our data do not support the hypothesis that the agent extracted a substantial proportion of borrower benefits by manipulating transactions in credit, input or output markets.

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% of the interest received on these loans.[12]

2.3 Data and Descriptive Statistics

From December 2010 to 2013, we collected household survey data from 50 households in each village. This included information about household demographics, assets, landholding, cultivation,

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

[12] Thus the incentives provided to TRAIL agents and to the MFI were roughly equivalent. Both faced the same formula for commissions. The paid holiday for surviving in the scheme offered to TRAIL agents was akin to (although not identical to) the internal bonuses and enhanced promotion prospects within Shree Sanchari that loan officers could expect if their job performance was considered satisfactory.

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. 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 loans were also randomly chosen into 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 collection 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) led to a balance of characteristics across the Control 1 and Treatment groups. 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 shows how observable household characteristics are correlated in our sample. As we see in column 1, households headed by women owned on average 0.29 acres of land. Only 20% of these female heads had completed primary schooling. Compare this to male-headed high caste Hindu households, where 63% of household heads had completed primary schooling and the average landholding was 0.59 acres.[13]

Table 4 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 planting season for potatoes, the crop with the highest working capital requirements in this region (as shown below in Table 5), 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 both total borrowing and borrowing for agricultural purposes.[14] Note first that nearly 70 percent of sample households borrowed in this 4-month period. 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

[13] To compute sample means for the representative household in the village, we re-weight the households in each of these three categories. Each recommended household (Treatment or Control 1) household is weighted by $\frac{30}{N}$ and each Control 2 household is weighted by $\frac{N-30}{N}$, where N is the village size.

[14] 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.

the agricultural credit, but they lent mainly to households with relatively larger landholdings.[15]

The average interest rate on loans from traders and moneylenders was 26.6%, substantially above the 18% interest rate charged on the TRAIL and GBL program loans. Loans from family and friends were also more expensive than the program loans.[16] The average duration of loans from traders and moneylenders was 4 months, reflecting the 4-month agricultural cycles in this area. Loans from family and friends were given for closer to 6 months. It was extremely rare for any of the informal loans to be secured by collateral. Cooperatives and government banks charged substantially lower interest rates and had longer average durations. However the share of informal lenders in agricultural credit became progressively larger as household landholding decreased from 1.5 acres to zero. Landless households received 87% of their agricultural credit from them, 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 5 describes the mean characteristics of the major categories of crops grown by sample farmers during the three years of our study. 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

Our model is based on two key features: borrower heterogeneity, and a segmented informal credit market. Borrowers vary in (exogenously-determined) ability; more able borrowers have lower default risk and higher productivity. The model abstracts from moral hazard, although similar results can be obtained in extensions that incorporate moral hazard (which were presented in previous versions of this paper). Defaults arise from incidents of crop failure (such as a pest attack) combined with limited liability: when their crop fails, farmers do not have the means to repay their loans. More able farmers are less likely to experience crop failure because they are better at preventing the pest attack. The risk of crop failure is not correlated across farmers.

Conditional on avoiding crop failure, more able farmers are more productive insofar as they produce a higher output from a given amount of expenditure on purchased inputs. Specifically, a farmer of ability i experiences crop failure with probability $(1-p_i) \in (0,1)$ and produces nothing; otherwise he produces $\theta_i f(l)$ where l denotes the level of input ($\equiv$ loan size) chosen by the farmer, and f is a smooth, strictly increasing, strictly concave production function wth $f'(0)$ large enough to ensure interior production for all parameter values and ability levels. Both p_i and θ_i are non-decreasing in i, while their product (or expected productivity) $\bar{\theta}_i \equiv p_i\theta_i$ is strictly increasing. It

[15]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.

[16]Note, we do not consider loans where the repayment amount due was reported to be equal to the principal.

will turn out that the limited liability constraint will never bind in the absence of a crop failure: farmers will always cultivate on a scale such that the output generated is sufficient to repay their loans. Informal lenders are able to monitor whether their borrowers experienced a crop failure, and can impose sufficient penalties to deter voluntary default. Hence $1 - p_i$ is the default risk of a farmer of ability i.

Note that productivity variations could reflect either differences in total factor productivity (e.g., based on differences in experience or farming skill) or ownership of complementary fixed factors, such as land or household labor stock. In the simplest version of the theory, there are just two possible ability levels: high ($i = H$) and low ($i = L$), with $H > L$ and a given proportion μ_H of borrowers are highly able. Extension to the case of more types is straightforward. To keep the exposition simple we restrict attention to the two-type case from this point onwards.

3.1 Pre-Intervention Informal Credit Market

Each village is partitioned into different segments (hamlets, neighborhoods or networks) on the basis of physical or social proximity. The location of each agent in the village is given exogenously. There are N borrowers in the village divided equally into k segments, each of which has the same proportion of H type borrowers. Each segment has at least two informal lenders who can distinguish borrower types in their own segment, but not in any other segment. All borrowers have the same cost of capital ρ per unit loaned, and are not capacity constrained. They compete with one another in Bertrand fashion, making credit offers to borrowers. A credit offer consists either of an interest rate (with the borrower deciding how much to borrow), or of a loan size and interest rate pair.

Standard arguments imply that the (unique) outcome of Bertrand competition will be the following. Lenders in any given segment will specialize in lending to highly able borrowers in their own segment, and will compete with each other so that the equilibrium offer will be to lend them any amount at interest rate $\frac{\rho}{p_H}$. Low ability borrowers will be able to borrow from any lender in the village at the interest rate $\frac{\rho}{p_L}$, because each lender will be prepared to lend to any borrower in the village at this rate.[17]

Thus, before the MFI intervention, borrower of type i will take an informal loan of size $\bar{l}_i$ where

$$\bar{\theta}_i f'(\bar{l}_i) = \rho \tag{1}$$

which is a Walrasian allocation. The segmentation of the market has no consequence for the allocation: it would remain the same even if it were not segmented. However, segmentation affects the outcomes of the TRAIL intervention, to which we now turn.

[17] An informal lender will not be willing to lower the interest rate below $\frac{\rho}{p_L}$ for any low ability borrower in her own segment. They will not offer borrowers from other segments an interest rate below $\frac{\rho}{p_L}$ because the only borrowers who would accept that offer would be the low ability ones, resulting in losses.

3.2 TRAIL Intervention

Suppose now that the MFI enters and wishes to offer loans at interest rate r_T which is below ρ, the cost of capital for informal lenders. Its comparative advantage over the informal lenders is its lower capital cost. However, it suffers from an informational disadvantage: it is unable to identify the ability of any given borrower. To overcome this, it randomly selects an informal lender, and appoints him as its agent. The agent recommends to the MFI n borrowers from the village as potential borrowers for TRAIL individual liability loans at interest rate r_T. The MFI then selects a fraction of these recommended borrowers randomly and offers them these loans. The agent stands to receive a fixed commission at the rate of $m \in (0, 1)$ per unit of interest repaid by the borrowers he recommended. This incentivizes him to recommend borrowers with lower default risk (i.e., lower risk of crop failure).

As with informal loans, we assume that the borrower is always incentivized to repay the loan and so there is no voluntary default.[18] We assume that the TRAIL loans supplement the informal loans that the borrowers have already contracted with informal lenders.[19] We also simplify by assuming that the TRAIL credit limit is not binding. In other words, each farmer's desired TRAIL loan size is smaller than the ceiling. The main conclusions continue to apply when the ceiling is binding for some borrowers.[20]

We can now predict the impact of the TRAIL intervention. A selected farmer of ability i will select a TRAIL loan l_i^T satisfying

$$\bar{\theta}_i f'(\bar{l}_i + l_i^T) = p_i r_T \tag{2}$$

The TRAIL agent's selection incentives are as follows. Assuming that the TRAIL agent and lenders do not collude, the agent's objective is to maximize the likelihood that the TRAIL loans are repaid.[21] To achieve this he will most prefer to recommend H-type borrowers in his own segment, followed by randomly chosen borrowers from other segments, followed finally by L-type borrowers in his own segment. If $n \leq \frac{N}{k}\mu_H$, all the borrowers recommended will be H-type from his own segment. Otherwise, he will recommend all the H-type borrowers in his own segment and then fill the remaining slots by randomly choosing borrowers from other segments.[22]

Notice first that conditions (1) and (2) can easily be used to compare *levels* of borrowing, output and farmer income across types, both before and after the intervention, as stated in the proposition below.

[18] This can be due to the threat that future access to TRAIL loans will be cut off, or due to pressure from the informal lender.

[19] This can be the result of borrower reluctance to disrupt their pre-existing credit channels, given their uncertainty about how long the TRAIL intervention will last. Alternatively, it is possible that informal loans have features (that we do not formally model) that make them more attractive, such as greater flexibility in duration or repayment terms, so that MFI loans are not close substitutes for them. If we did not assume this, we would expect that once TRAIL loans become available borrowers cut back on their informal loans. We do not observe this in the data.

[20] It is evident that a binding credit ceiling will not affect the selection incentives of the TRAIL agent. However the loan treatment impact will be decreasing in ability if the ceiling is binding for both high and low ability borrowers (since the more able borrow more on the informal market to start with). We shall see in the data that instead, the TRAIL scheme caused borrowing to increase by more for the high ability borrowers. Given this, if there was a credit ceiling it must have been binding for high ability borrowers but not for low ability borrowers. In this case, the same reasoning will continue to apply as when the limits are non-binding for both types of borrowers.

[21] We shall discuss below how the analysis changes if the agent colludes with borrowers.

[22] This is under the reasonable assumption that other segments have a total population that exceeds n.

Proposition 1 Selection (Comparing Levels): *Higher ability types borrow, produce and earn more than lower ability types, both before and after being offered the TRAIL loan.*

The less trivial question is how treatment effects on borrowing, output or income vary by borrower type. This is ambiguous in general. Starting with the loan treatment effect, the question is will more able farmers take larger TRAIL loans? There are three relevant forces here:

(a) TFP Difference: More able farmers have larger TFP, so they derive larger benefits from expanding the scale of cultivation;

(b) Diminishing Returns: More able farmers produced more before the intervention, and so after controlling for productivity differences, they face a lower marginal rate of return from expanding cultivation;

(c) Subsidy Difference: More able farmers paid a lower interest rate to start with and so the intervention lowers their interest rate by less.

The TFP difference induces more able farmers to take larger TRAIL loans, but the diminishing returns and smaller interest rate subsidy works in the opposite direction. As a result it is unclear whether the overall treatment effect would be larger for more able types.

In the absence of any productivity differences across high and low ability farmers, it is evident that the loan treatment effect will be decreasing in ability. To see this, note that any given borrower of type i selects the TRAIL loan size $l = l_i^T$ to maximize net income conditional on crop success $\theta_i f(\bar{l}_i + l) - r_T l$. If there are no productivity differences, θ_i does not vary with i: then all ability types would have the same aggregate borrowing, cultivation, output and income (conditional on crop success). Since higher ability types borrow more before the credit intervention, the loan treatment effect would decrease in i.

Now consider productivity differences, wherein θ_i increases in i. This induces higher ability borrowers offered TRAIL loans to borrow a larger total volume $(\bar{l}_i + l_i^T)$. The pre-intervention scale of borrowing depends entirely on expected productivity $\bar{\theta}_i$. Hence if we keep the expected productivity schedule unaffected, and productivity differences account for more of this variation (and correspondingly the risk of crop failure accounts for less of this variation), then the pattern of initial loans is given, while the pattern of total borrowing post-intervention increases more steeply in i. This will raise the slope of the loan treatment effect with respect to ability. In the limiting case where crop risk does not vary at all with ability, we show below that the loan treatment effect must increase in i. Hence the relative importance of productivity variations relative to crop risk variations in ability determines how loan treatment effects vary with ability.

In the following result, we restrict attention to production functions satisfying a *Regularity Condition* (RC): $\frac{-f''}{f'}$ is decreasing. This condition is satisfied by a constant elasticity function $[f(l) = \frac{1}{\alpha} l^{\alpha}$ with $\alpha < 1$, different from 0 (which corresponds to a log function)], as well as exponential function $[f(l) = K[1 - \exp(-al)]$ with $a > 0]$.

Proposition 2 Selection Effects (Comparing TRAIL Treatment Effects Across Types): *Suppose that the production function satisfies RC, and that expected productivity $\bar{\theta}_i$ is strictly increasing in ability i.*

(a) If the loan treatment effect is rising in ability, the output treatment effect must be rising in ability.

(b) If all (or most) of the variation in expected productivity is accounted for by variation in productivity, i.e. the crop success probability p_i is entirely (or nearly) independent of ability, then loan, output and farm income treatment effects will be rising in ability,

(c) If all (or most) of the variation in expected productivity is accounted for by variation in the probability of crop success, i.e. productivity is entirely (or nearly) independent of ability, then loan and output treatment effects will be falling in ability.

Proof of Proposition 2:

(a) Suppose l_i^T is nondecreasing in i. Take any pair of types satisfying $i > j$. Then $l_i^T \geq l_j^T$. Applying condition RC we obtain $\frac{f'(\bar{l}_i+l)}{f'(\bar{l}_j+l)}$ is increasing in l. Combining this with (1) which implies $\frac{f'(\bar{l}_i)}{f'(\bar{l}_j)} = \frac{\bar{\theta}_j}{\bar{\theta}_i}$, we therefore obtain $\bar{\theta}_i f'(\bar{l}_i + l) > \bar{\theta}_j f'(\bar{l}_j + l)$ for any $l > 0$. Hence the output treatment effect for type i is

$$\begin{aligned}
\bar{\theta}_i[f(\bar{l}_i + l_i^T) - f(\bar{l}_i)] &\geq \bar{\theta}_i[f(\bar{l}_i + l_j^T) - f(\bar{l}_i)] \\
&= \bar{\theta}_i \int_0^{l_j^T} f'(\bar{l}_i + l)dl \\
&> \bar{\theta}_j \int_0^{l_j^T} f'(\bar{l}_j + l)dl \\
&= \bar{\theta}_i[f(\bar{l}_j + l_j^T) - f(\bar{l}_j)]
\end{aligned}$$

the output treatment effect for j.

(b) If the probability of crop success p_i is independent of i, equal to p, equations (1, 2) imply $\frac{f'(\bar{l}_i+l_i^T)}{f'(\bar{l}_i)}$ is independent of i. This implies $\log f'(\bar{l}_i + l_i^T) - \log f'(\bar{l}_i)$ is a constant. Differentiating this expression and setting equal to zero, we obtain

$$1 + \frac{l_i^{T'}}{\bar{l}_i'} = [-\frac{f''(\bar{l}_i)}{f'(\bar{l}_i)}][-\frac{f''(\bar{l}_i + l_i^T)}{f'(\bar{l}_i + l_i^T)}]^{-1} > 1 \quad (3)$$

owing to RC. Hence the loan treatment effect is rising in i. Applying part (a) we infer the output treatment effect is rising in i.

Finally consider the income treatment effect for i:

$$\begin{aligned}
\bar{\theta}_i[f(\bar{l}_i + l_i^T) - f(\bar{l}_i)] - p_i r_T l_i^T &= [\bar{\theta}_i f(\bar{l}_i + l_i^T) - p_i r_T l_i^T] - \bar{\theta}_i f(\bar{l}_i) \\
&\geq [\bar{\theta}_i f(\bar{l}_i + l_j^T) - p_i r_T l_j^T] - \bar{\theta}_i f(\bar{l}_i) \\
&= \bar{\theta}_i[f(\bar{l}_i + l_j^T) - f(\bar{l}_i)] - p_i r_T l_j^T \\
&= \bar{\theta}_i \int_0^{l_j^T} f'(\bar{l}_i + l)dl - p_i r_T l_j^T \\
&> \bar{\theta}_j \int_0^{l_j^T} f'(\bar{l}_j + l)dl - p_i r_T l_j^T
\end{aligned}$$

where the first inequality uses the property that type i chooses TRAIL loan size to maximize his post-intervention income, and has the option of choosing the loan size selected by type j. The last inequality again uses RC in the way described in the proof of (a) above. The expression in the last line above equals the income treatment effect for type j, less $(p_i - p_j)r^T l_j^T$. This last 'correction' term equals zero (approximately) when p_i does not vary (varies very little) with i.

(c) If productivity does not vary with i, the pre-intervention loan size and output are rising in i, but after the intervention do not vary with i. Hence the loan and output treatment effects are falling in i. QED

Parts (b) and (c) show that how the treatment effects vary with ability depends on whether productivity or crop risk is more sensitive to variations in ability.[23] These results help to see why the model must incorporate variations in both default risk and productivity. Variations in default risk alone would impose very strong restrictions on variations in treatment impacts that might be (and indeed turn out to be) empirically invalid. A model based on productivity variations alone would also not work, since variations in default risk that co-move with productivity are required to generate the predictions about the TRAIL agent's selection incentives. Nevertheless, the model enables us to empirically disentangle the two sources of variations to some extent: differences in informal interest rates will reflect variations in default risk, and the pattern of variation of TRAIL treatment effects will (given Proposition 2 above) then tell us something about the importance of productivity differences. For instance, if treatment effects are rising in proxies for ability, while interest rates are falling, higher ability farmers have lower default risk and must also be significantly more productive.

3.2.1 Collusion between the TRAIL agent and borrowers

In this section we consider the case where the TRAIL agent can collude with borrowers when deciding whom to recommend. He would then charge bribes in return for recommendations. Loan sizes could also be collusively chosen, so that recommended TRAIL borrowers would internalize the larger commissions that the agent would earn if the loan were to become larger. If so, the

[23] In case (c) we are not able to provide a definite result about how treatment effects on farm income vary across types. They can be shown to be decreasing in ability when the scale of the TRAIL loans is small enough, i.e, when $[\frac{\rho}{p_L} - r_T]$ is not too large.

effective interest rate on the loan for the coalition would be $(1-m)r_T$ (where m is the agent's commission rate) instead of the r_T from the non-collusive equilibrium.

Proposition 2 would continue to hold (with the effective TRAIL interest rate adjusted from r_T to $(1-m)r_T$, as above). If TFP variations are larger than default risk variations, case (b) applies and the borrower income treatment effects would increase in ability. Then high ability borrowers would benefit more from the loan than low ability borrowers, and would be willing to pay larger bribes. Thus collusion would reinforce the agent's incentive to recommend high ability borrowers.[24]

3.3 GBL Intervention

As is standard in the literature (e.g., Besley and Coate (1995), Ghatak (1999, 2000)), we assume that each GBL group consists of two members. The MFI requires individuals to self-select into groups. Group members then apply for a joint liability loan, which is offered at the same interest rate r_T as the TRAIL loan. Each member is potentially liable for the loans of both members. In addition, the GBL program requires members to periodically attend group meetings and meet savings targets. The cost of meeting these requirements varies idiosyncratically in the population and is uncorrelated with their type: we assume the cost for any borrower c is drawn from a distribution with positive density g over the nonnegative reals. As in the analysis of TRAIL we abstract from repayment incentives, and assume that borrowers honor their obligations whenever their own project does not fail. We also assume that it is feasible for members of a group to enter into side payments without any friction, so the loan size choices for any group will maximize the sum of their respective *ex ante* payoffs. Hence a group consisting of members of types i, j will select loan sizes l^G_{ij}, l^G_{ji} to maximize the sum of expected payoffs of the two members $\bar{\theta}_i f(\bar{l}_i + l^G_i) + \bar{\theta}_j f(\bar{l}_j + l^G_j) - r_T[p_i\{l^G_i + (1-p_j)l^G_j\} + p_j\{l^G_j + (1-p_i)l^G_i\}]$, implying

$$\bar{\theta}_i f'(\bar{l}_i + l^G_{ij}) = p_i[1 + p_j(1-p_i)]r_T \tag{4}$$

In this context it turns out to be very difficult to find conditions under which groups will be formed through assortative matching of types.[25] Hence we must consider the possibility that groups could be form as a result of any of three kinds of matching: (H,H), (L,L) or (H,L).

When comparing GBL outcomes with TRAIL outcomes, the first point to note is that for any given type i of borrower, a GBL loan will have a smaller impact on borrowing, output and income. The reason is joint liability: conditional on his own crop succeeding, borrower i pays an effective interest rate of $[1 + p_j(1-p_i)]r_T$, which is higher than the interest rate r_T he would pay for a TRAIL loan. This obtains irrespective of the type of group member that i is matched with. Hence we obtain

Proposition 3 Incentive Effect (Comparing TRAIL and GBL Impacts for a Given

[24] This would obtain regardless of whether the collusion game were modeled cooperatively with stable matching (followed by Nash Bargaining), or non-cooperatively (where either side makes take-it-or-leave-it offers to the other). We omit the details here.

[25] This is because the usual arguments provided by Ghatak (1999) for the case of loans of fixed size do not extend in a straightforward manner when loan sizes are endogenously chosen.

Borrower Type): *For any given ability type, the TRAIL treatment impact on loans, output and income is larger than the GBL treatment impact.*

The "joint liability tax" or the incentive effect provides one explanation for a smaller average treatment effect of GBL loans. Another explanation might be a difference in selection of types. Note that *both* high and low ability borrowers have an incentive to participate in the GBL scheme (ignoring the costs of attending group meetings and savings requirements). To see this, consider first a homogenous group, both of whose members are of type i. Each member of such a group faces an expected interest rate of $(2 - p_i)r_T$, which is smaller than what she pays in the informal market, since $r_T < \frac{\rho}{p_i(2-p_i)}$.[26] Hence, homogenous groups of either type would prefer a GBL loan to the status quo. If conditions for assortative matching do not obtain, heterogenous (H, L) groups would earn a larger surplus than the average surplus of homogenous $(H, H), (L, L)$ groups, and would have an even higher incentive to apply for GBL loans. Either way, both low and high risk types would form and apply for GBL loans. The composition of the GBL applicant pool would depend on the relative treatment impacts for different types of groups: e.g., with assortative matching, if the treatment effects are larger for higher ability groups the composition would be biased in favor of high ability groups.

However, the key point is that the proportion of low ability GBL applicants will be bounded away from zero.[27] Unlike the TRAIL agent who acts as a gatekeeper, there is no mechanism in the GBL scheme to keep out low ability types. We therefore expect the TRAIL agent to recommend a larger proportion of high ability agents than self-select into the GBL scheme, provided the number of recommendations required does not greatly exceed the number of high ability borrowers in the agent's own segment of the informal credit market.

Proposition 4 Differences in Selection Patterns between TRAIL and GBL:

(a) *If $n \leq \frac{N}{k}\mu_H$, all TRAIL borrowers are H-type, while only a fraction of GBL groups are of H-type.*

(b) *If $n > \frac{N}{k}\mu_H$, the proportion of borrowers who are H-type in the TRAIL scheme is weakly larger than μ_H. It has relatively more H-type borrowers, unless GBL treatment effects are rising in ability and n is sufficiently large relative to $\frac{N}{k}\mu_H$.*

3.4 Predicting and Decomposing TRAIL-GBL Differences in Average Treatment Effects

We can express the average treatment effect on any given outcome of intervention v (where $v =$ T if the scheme is TRAIL, and $v =$ G if the scheme is GBL) as an average of the treatment effects for different types, using as weights the composition of selected borrowers between types:

$$T^v \equiv \omega^v T^v_H + (1 - \omega^v)T^v_L \tag{5}$$

[26] This follows from the fact that $p_i(2 - p_i) < 1$, for any i.

[27] If matching were assortative, treatment effects for both high and low ability homogenous groups would be bounded away from zero. If instead matching were not assortative, mixed type groups containing low ability types would form.

where for intervention v, T_i^v denotes the treatment effect on a type i borrower, T^v the average treatment effect and ω^v the fraction of H types selected. Then the difference between TRAIL and GBL average treatment effects can be expressed as

$$T^T - T^G \equiv \quad \{\omega^G[T_H^T - T_H^G] + (1-\omega^G)[T_L^T - T_L^G]\} + (\omega^T - \omega^G)[T_H^T - T_L^T] \tag{6}$$

The difference in average treatment effects is the sum of two expressions: the first is a weighted average of the Incentive Effect for various types, using as weights the selection likelihoods in the GBL scheme. The second, the Selection Effect, is the product of two terms: the difference in TRAIL treatment effects between the two types, and the difference in the proportion of H-types between the two interventions. From this and the preceding propositions it follows that

Proposition 5 Sufficient Condition for Comparing Average Treatment Effects*: The average treatment effect of TRAIL loans is larger than the average treatment effect of GBL loans if the TRAIL agent recommends a larger proportion of H types (e.g. if n is smaller than $\frac{N}{k}\mu_H$, or is not much larger), and if TRAIL treatment effects increase in ability.*

3.5 Summary of Predictions and Strategy for Empirical Analysis

At this stage it is helpful to summarize the theoretical predictions that we can test in the data. The key problem is that farmer ability is unobservable. The TRAIL agent has access to information about a range of borrower characteristics that help him to predict the farmer's ability. To test whether selected borrowers' default risk patterns match theoretical predictions, we examine the interest rate that borrowers paid on informal loans taken before the intervention. This is a valid proxy, given the plausible assumption that informal lenders can evaluate borrowers' default risk and vary the interest rate accordingly. Specifically, we can test the following prediction.

(a) In the TRAIL intervention, the borrowers that the agent recommended from within his own clientele paid a lower informal interest rate than own-clientele borrowers whom he did not recommend. Among households outside his clientele, those he recommended paid an interest rate that was no higher than what the non-recommended paid.[28]

We also observe a subset of the borrower characteristics that are potentially correlated with ability, such as caste, landownership, education or gender of the household head. However, the strength of the correlation needs to be assessed empirically. We can test the hypothesis that a particular observable characteristic denoted by x is positively correlated with ability by testing the following implications of this hypothesis:

[28] This follows from the discussion following equation (2). Note that from outside his own clientele the model predicts that the agent recommends two kinds of borrowers: highly able borrowers in his own segment that are served by other lenders in the segment, and randomly chosen borrowers from other segments. Hence outside the agent's own clientele, the average informal rate paid by the recommended will be equal to or lower than that paid by the non-recommended (i.e., the low ability types).

(b) Before or after either intervention, x is positively correlated with the scale of production loans, cultivation and farm income, and negatively correlated with the informal interest rate the farmer pays (Proposition 1).

(c) In either credit intervention, if loan treatment effects increase in x, then the treatment effects on farm revenues also increase in x (part (a) of Proposition 2).

If these predictions are verified in the data, we can use equation (6) to obtain a lower bound estimate of the role of the selection effect in accounting for average treatment effect differences between TRAIL and GBL, by using characteristic x as a proxy for ability. Specifically, we can estimate how TRAIL treatment effects differed across borrowers varying in x, and multiply this with corresponding differences in proportions of borrowers selected as classified by x. This provides an estimate of the role of selection differences using only characteristic x to classify borrowers. It is an underestimate of the true selection effect, since that would include other observable characteristics as well as characteristics observed by the TRAIL agent that we are unable to capture in our data.

4 Empirical Results

We start by presenting the average treatment effects in the two loan schemes on borrowers' cultivation, output and farm value added. Next we test the model's predictions, and whether (and how much) the difference in selection patterns can explain the difference in their average treatment effects. Later, we present results concerning take-up and repayment behavior of borrowers in the two schemes. This is followed by a discussion of a number of ancillary issues: loan repayment and take-up rates, time variation of impacts, possible side-transactions between borrowers and TRAIL agents, and comparisons of administrative costs in the two schemes.

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

To examine the average *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. We can also investigate how outcomes differed between the recommended households and the non-recommended. This is estimated by the difference in outcomes between the Control 1 households (who were selected but did not receive program loans) and Control 2 households (who were not selected and did not receive program loans).

Our regression specification takes the following form:

$$\begin{aligned} y_{ivt} &= \beta_0 + \beta_1 \text{TRAIL}_v + \beta_2(\text{TRAIL}_v \times \text{Control } 1_{iv}) + \beta_3(\text{TRAIL}_v \times \text{Treatment}_{iv}) \\ &+ \beta_4(\text{GBL}_v \times \text{Control } 1_{iv}) + \beta_5(\text{GBL}_v \times \text{Treatment}_{iv}) + \gamma \mathbf{X}_{ivt} + \varepsilon_{ivt} \end{aligned} \tag{7}$$

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, so that $\hat{\beta}_0$ estimates the mean y_{ivt} for Control 2 households in GBL villages. All other coefficients then estimate the level of y_{ivt} for a different group, relative to these GBL Control 2 households. Here $\hat{\beta}_3 - \hat{\beta}_2$ estimates the treatment effect in the TRAIL scheme and $\hat{\beta}_5 - \hat{\beta}_4$ estimates the treatment effect in the GBL scheme.[29] $\hat{\beta}_2$ and $\hat{\beta}_4$ measure differences between Control 1 and 2 households within TRAIL and GBL villages, respectively. $\mathbf{X}_{iv}$ is a set of additional controls, including land owned by the households, caste, gender and educational attainment of the household head, 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.[30]

To reiterate, our sample consists of 2042 households across 24 TRAIL and 24 GBL villages. In each village our sample consists of 10 Treatment, 10 Control 1 and 30 Control 2 households. We exclude households with more than 1.5 acres of land from the estimating sample. Since agricultural activity involves a long delay from planting to harvest, and the harvest can be sold over several months, we aggregate our data to the annual level in order to correctly compute the costs and revenues of each crop. Our unit of observation is then household-year. Standard errors are clustered at the hamlet level.[31]

4.1.1 Treatment effects on Borrowing, Cultivation and Farm Incomes

The regression results corresponding to equation (7) are presented in Tables 6–8: treatment effects on borrowing are in Table 6, treatment effects on cultivation of and incomes from potatoes are in Panel A of Table 7, effects on cultivation of and incomes from other crops are in Panel B of Table 7, and effects on total farm income are in Table 8.

Since we analyze a large number of outcome variables, the null of no treatment effect could be rejected by mere chance for some of them, even if the null were actually true. To correct for

[29] 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.

[30] 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. The results are unchanged if we drop this information village dummy.

[31] In Indian villages households are clustered by *paras* (or hamlets/neighbourhoods). Administrative definitions of a village correspond to a collection of 10-15 *paras* geographically isolated from each other. Households within the same *para* tend to be more homogenous, are more likely to interact with each other, and experience geographic shocks to cultivation and market prices that are highly correlated. The results are robust to clustering at the village-level instead (see columns 1 and 2 of Table R-4 and Table R-5). The treatment effects are also unchanged qualitatively if we restrict the sample to the Treatment and Control 1 households only (see columns 3 and 4 of Table R-4 and Table R-6).

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).[32]

Effects on Borrowing

We see in Table 6 that as a result of the TRAIL scheme, treatment households' overall borrowing increased by ₹5050, which is a 90% increase over the ₹5590 mean borrowing by TRAIL Control 1 households. In the GBL scheme treatment households increased overall borrowing by a statistically significant ₹3732, which is a 92% increase over the mean for the GBL Control 1 households. Note in this connection that the fact that TRAIL Control 1 households borrowed significantly more (p-value 0.000) than the GBL Control 1 households is consistent with the model prediction that TRAIL borrowers were more productive farmers.

In column 2 of Table 6 we examine if program loans crowded out agricultural loans from other sources. There is no evidence that this happened in either scheme: the treatment effects on non-program loans are small and statistically insignificant.

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 0.025). However, the effect of the GBL treatment is not statistically significant using the conservative test (effect 0.18 sd, Hochberg p-value 0.368).

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 cycles matched the potato production cycle, we first present the estimated effects on potato cultivation. Panel A of Table 7 shows that TRAIL Treatment households committed a statistically significant 28 percent more in acreage to potato cultivation than Control 1 households did (column 2). They 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 28% higher revenue and 37% higher value-added (column 6) and imputed net profit from potato cultivation increased by ₹1939, or 41% of the mean for Control 1 households (column 7).[33]

The GBL scheme did not significantly change the probability that Treatment households cultivated potatoes or the acreage they devoted to potatoes. It did cause them to spend 27% more on purchased inputs, but although the point estimate is similar to that for TRAIL treatment

[32] 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.

[33] Value-added in column 6 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).

households, the effect on revenue is statistically insignificant. The higher standard errors for the GBL treatment effects are consistent with the theoretical prediction from Section 3 that both low- and high-ability borrowers participated in the GBL scheme.

On the index of outcomes related to potato production, the TRAIL treatment caused a 0.20 standard deviation increase (Hochberg p-value 0.003), while the GBL treatment had a statistically insignificant effect (Hochberg p-value 0.861).

Panel B of Table 7 presents the treatment effects on acreage and value-added of the other main crops: sesame, paddy and vegetables. The TRAIL scheme significantly increased the acreage that treatment households put under paddy and sesame. The treatment effect on value added is also positive for all three crops, but it is only statistically significantly different from zero for sesame. The GBL scheme did not change the acreage or value-added for any of the crops.[34]

Finally, column 1 of Table 8 presents the treatment effects on total farm value-added, computed by aggregating across the four crop categories. The TRAIL treatment led to a 25% increase in overall farm value-added over the Control 1 mean. The GBL treatment effect was negligible and statistically insignificant. As the last row in column 1 shows, the TRAIL treatment effect on farm value-added was statistically significantly larger than the GBL treatment effect (p-value 0.077).

In column 2 we see that neither the TRAIL nor the GBL loans significantly affected borrowers' non-agricultural incomes. However, when we take both farm and non-farm income into account, column 3 shows that TRAIL loans increased borrower incomes by 11.6 standard deviations (Hochberg p-value 0.034). GBL loans had no effect (Hochberg p-value > 0.999).

4.1.2 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 4 and 5 of Table 8, we report the rate of return on the costs of cultivation of potatoes and all major crops considered together, respectively. Since the rate of return is the ratio of two treatment effect estimates, the standard errors are cluster-bootstrapped, with 2000 replications. The rate of return on potato cultivation expenses was a statistically significant 123% for TRAIL borrowers. GBL borrowers earned a substantially lower but still statistically significant 37% 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 132%, whereas the rate of return earned by GBL borrowers was non-significant. These results are consistent with our model predictions based on the different selection and/or production incentive effects of TRAIL.

[34] Although the magnitude of the effect on paddy is similar to that for TRAIL treatment households, it is imprecisely estimated.

4.2 Testing Theoretical Predictions

4.2.1 Empirical Results About Selection On Default Risk

We begin by examining prediction (a) in Section 3.5. Consider the interest rate that the households paid on informal loans they took before the intervention (as reported in the survey cycle 1), as a proxy for the loan default probability, which is negatively correlated with ability. The model predicts that among households within his clientele, the TRAIL agent recommended borrowers with higher ability, and from outside their clientele he selected randomly. It follows that within the agent's own clientele, the informal interest rate that recommended TRAIL borrowers paid before the intervention was below the interest rate paid by non-recommended borrowers. The corresponding difference would be smaller for borrowers outside the agent's own clientele.

In column 1 of Table 9 we present the results of the following regression on the sample of households in TRAIL villages that owned at most 1.5 acres of land:

$$r_{iv} = \delta_0 + \delta_1 \text{Selected}_{iv} + \delta_2 \text{Own Clientele}_{iv} + \delta_3(\text{Selected}_{iv} \times \text{Own Clientele}_{iv}) + \gamma_1 \mathbf{X}_{2iv} + u_{iv} \quad (8)$$

Here r_{iv} denotes the average interest rate that household i paid on informal loans it had taken in Cycle 1. The variable Selected_{iv} indicates whether the household was recommended by the TRAIL agent. We define the variable $\text{Own Clientele}_{iv}$ to indicate whether the household had borrowed from the agent in the three years prior to the study. We include household landholding as a control. The total effects in the second panel confirm that the prediction holds in the data: from within his own clientele, the TRAIL agent recommended households who paid significantly lower interest rates. The point estimate of this difference is almost 5 percentage points (almost one-fifth the average interest rate in the village), and it is statistically significant at 10%. Outside the agent's own clientele, the corresponding difference (estimated by $\hat{\delta_1}$) is quantitatively smaller and not statistically significantly different from zero.

Proposition 4 provides sufficient conditions under which TRAIL will select borrowers that pay lower interest rates on average, which generate a presumption that this is likely to happen, though it is not inevitable. To examine this empirically we run the following regression on pooled data from the TRAIL and GBL villages:

$$r_{iv} = \theta_0 + \theta_1 \text{Selected}_{iv} + \theta_2 \text{TRAIL}_v + \theta_3(\text{TRAIL}_v \times \text{Selected}_{iv}) + \gamma_2 \mathbf{X}_{2iv} + \varepsilon_{iv}. \quad (9)$$

The results presented in column 2 (second panel) show that the interest rate that households recommended in the TRAIL scheme paid was a statistically significant 4 percentage points than what GBL group members paid.

Finally column 3 uses the specification

$$\begin{aligned} r_{iv} &= \kappa_0 + \kappa_1 \text{Selected}_{iv} + \kappa_2 \text{TRAIL}_v + \kappa_3(\text{TRAIL}_v \times \text{Selected}_{iv}) \\ &+ \kappa_4(\text{TRAIL}_v \times \text{Own Clientele}_{iv}) + \kappa_5(\text{TRAIL}_v \times \text{Selected}_{iv} \times \text{Own Clientele}_{iv}) \\ &+ \gamma_3 \mathbf{X}_{2iv} + \zeta_{iv} \end{aligned} \quad (10)$$

The difference effects shown in the second panel of Table 9 indicate that borrowers whom the TRAIL agent recommended from within his own clientele had paid lower interest rates on average than GBL group members did. The difference is a statistically significant 5 percentage points. It follows that the TRAIL scheme outperformed the GBL scheme at selecting able borrower.

4.2.2 Relationship between Household Characteristics and Scale of Productive Activity

Prediction (*b*) of the model is that if a particular characteristic x is positively correlated with ability, then households with a larger value of x produce on a larger scale and earn higher farm income than other households. To finance this, they borrow more. Since informal lenders know that this characteristic is correlated with ability, they pay lower interest rates. In Table 10 we examine if the data support these predictions.

We split our sample households (with at most 1.5 acres of land) into four categories defined by the gender and religion/caste affiliation of the household head. We then examine the mean potato output, farm revenue (as a measure of the value of total agricultural output) and farm value-added, as well as total borrowing for agricultural purposes and the interest rate paid by households in each category. Table 10 indicates that on average, female-headed households borrowed the least, produced the smallest quantities of potatoes and aggregate farm output, and earned the lowest farm value-added. Male-headed high caste Hindu households comprised the other extreme. Male-headed non-Hindu households produced and borrowed less than high caste Hindu households, but more than low-caste Hindu households. The tests of difference presented in the lower panel of the table show that differences across the gender and caste/religion groups are systematically significant. This suggests the following ordering by ability (from lowest to highest): female-headed households, male-headed Hindu low caste, non-Hindu and finally Hindu high-caste households.

The ordering by mean informal interest rates conforms to this among male-headed households. High caste Hindus paid the lowest rates of interest, followed by non-Hindus, with Hindu low-caste households paying the highest rates. However, one exception is that female-headed households paid the lowest interest rates on agricultural loans.

4.2.3 Heterogeneity in Treatment Effects by Household Characteristics

Next, consider model prediction (c): If treatment effects on borrowing increase in characteristic x, then treatment effects on farm revenue also increase in x. To test this prediction, we start in column 1 of Table 11 by comparing the treatment effects on borrowing for the same four borrower categories as above. The TRAIL treatment effect was a non-significant ₹459 for female-headed households, and was negative and non-significant for male-headed non-Hindu households. It was positive and statistically significant for both male-headed low caste and male-headed high caste households.

Given this, we check if the TRAIL treatment effects on farm revenue can be similarly ordered. Column 2 shows that high-caste Hindu male-headed households experienced the highest farm revenue treatment effects. They also experienced the highest treatment effects on farm value added. The difference with respect to low-caste Hindu male-headed households is statistically significant. Thus high caste households paid lower interest rates, cultivated and produced more, and experienced larger TRAIL treatment effects on borrowing, revenues and farm income than low caste households. Many of these comparisons are statistically significant.

Our model does not predict how treatment effects of the GBL loans should be ordered, but for the sake of completeness we present the estimates in the lower panel of Table 11. Column 1 shows that treatment effects are non-significant for both female-headed and male-headed non-Hindu households. They are positive and significant for both male-headed low caste and male-headed high-caste households, but the difference between the two is not significant. It is similarly difficult to order the treatment effects on farm revenues in column 2.

4.2.4 The Contribution of Borrower Selection to the Difference in Average Treatment Effects Across the Two Schemes

Finally, we follow equation (6) (extended to incorporate four rather than two groups) to calculate how much of the difference in average treatment effects on farm value added can be explained by differences in selection on observable characteristics. Table 12 presents these results. We find for example that compared to GBL group members, households recommended by TRAIL agents were more likely to be headed by high-caste Hindu males. The difference in the proportion of selected households who were male-headed high-caste Hindu was a statistically significant 8.8 percent. In column 5, this difference is multiplied with the TRAIL treatment effect for households in this category. This is the contribution of selection on this characteristic to the difference in average treatment effects between the two schemes. A similar calculation is done for the other categories. When aggregated, these individual contributions sum up to 14.3 percent of the difference between the average treatment effect of the TRAIL scheme and the GBL scheme on farm value-added.

Thus we conclude that at least 14 percent of the difference between the TRAIL and GBL average treatment effects can be attributed to differences in patterns of borrower selection.[35] Clearly this also means that 86 percent of the difference remains unexplained. This could be due to differences in selection along other dimensions that we do not observe, or could be due to other differences between the two schemes: incentives for borrowers to take on risky projects, monitoring, help provided by the agent to the borrower, or trust.

4.3 Ancillary Issues

Next we examine (*i*) loan performance; (*ii*) year-specific effects; (*iii*) 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, alternately that subsidies provided by the agent to TRAIL clients may have accounted for most of the observed treatment effects; and (*iv*) administrative costs.

[35]This is a lower bound on the overall contribution of selection differences, since it pertains to differences along a single dimension x, and therefore does not account for the contribution of other characteristics that the TRAIL agent may also have used to select borrowers. We could create a composite of multiple characteristics (for which predictions (a) and (b) are verified). But this would lower the effective sample size within any given observable category, thereby reducing the precision of the estimates of treatment effect heterogeneity and differences in selection.

4.3.1 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 (see Panel A of Table 13). The take-up of loans in the two schemes is a useful metric of the *ex ante* impact of these loans on borrower welfare. Take-up rates, measured as the proportion of households that were offered a program loan in a given cycle who accepted it, were 75% in the GBL scheme, and 86% in the TRAIL scheme. Continuation rates measure the proportion of households who were eligible in cycle 1 who took a program loan in a given cycle. This dependent variable depends both on repayment behavior in previous cycles and take-up in this cycle, and therefore the means are naturally lower.[36] Sixtynine percent of GBL borrowers continued in the scheme, compared to 81% of TRAIL borrowers.

To examine rigorously how their behavior differed in the two schemes, we estimate the equation

$$y_{ivt} = \alpha_0 + \alpha_1 TRAIL_v + \gamma \mathbf{X}_{ivt} + \varepsilon_{ivt} \quad (11)$$

where $y_{ivt} = 1$ if household i assigned to treatment in village v accepted the program loan in cycle t (continuation), $y_{ivt} = 1$ if treatment household i in village v who was eligible to receive a loan in cycle t accepted it (take-up), or $y_{ivt} = 1$ if treatment household i in village v repaid a loan taken in cycle t within 30 days of the due date (repayment). We include cycle dummies as additional controls.

In our model it is ambiguous how the repayment rate on TRAIL loans would compare with the rate on GBL loans. Although TRAIL borrowers are more able, GBL borrowers have the benefit of joint liability so that even if their own projects failed, their group members might repay on their behalf. Column 1 in Table 13 shows that the difference is not statistically significant.

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).[37]

4.3.2 Year-Specific Effects

The TRAIL and GBL treatment effects on agricultural output estimated in Table 7 were averages across the three years of the intervention. It is informative to examine 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

[36] Borrowers who repaid less than 50% of the amount due in a previous cycle would not have been offered a loan in this cycle and therefore would automatically be coded as zero.

[37] 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.

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.

4.3.3 Impact of TRAIL on Transactions with Agents

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 naturally difficult to collect data on bribes or side-payments, we do have detailed data about sample households' input purchases from, output sales to, and borrowing from the TRAIL agent. We use these to test if the agent extracted rents from TRAIL borrowers, or helped the TRAIL borrowers by manipulating transactions.[38]

In Table 14 we analyse input, output and credit transactions between sample households in TRAIL villages and the TRAIL agent. In each panel, the third row 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. As can be seen in Panel A, over the 3 years, Control 1 households conducted only about 8% of input transactions with the agent, accounting for less than 6% of the value of inputs purchased. Panel B shows that they conducted 19% of output transactions with the agent, representing less than 16% of the value of transactions, and Panel C shows 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 sold larger quantities of inputs at higher prices to the borrowers, bought larger quantities from them at discounted prices or adjusted downward the price he paid for their output. Alternatively he could have charged them higher interest rates on loans. To examine if this occurred, we run a regression of the form:

$$\begin{aligned} y_{ivt} &= \lambda_0 + \lambda_1 \text{Buy from agent}_{iv} + \lambda_2(\text{Buy from agent}_{iv} \times \text{Treatment}_{iv}) \\ &+ \lambda_3(\text{Buy from agent} \times \text{Control1}) + \gamma X_{ivt} + \epsilon_{ivt} \qquad (12) \end{aligned}$$

The difference $\lambda_2 - \lambda_3$ captures whether Treatment households interacted with the TRAIL agent on different terms than Control 1 households. In Panel B, the two significant treatment effects are a slight increase in the price at which farmers purchased seeds, and a reduction in the rate at which they rented power tillers from TRAIL agents. Thus the two effects go in opposite directions. Given the large number of input prices we examine, we worry that the null hypothesis of no treatment effect could be rejected by chance even if it were actually true. To correct for this, we follow Kling, Liebman, and Katz (2007) and regress an index variable of all input prices in column 3. The Hochberg (1988) p-value of 0.773 indicates that overall, borrowers assigned to

[38]A group of students in Boston University's Masters of Global Development Studies program did fieldwork and very useful analysis to help address this question (see Ah-Tye, Bai, Blanco, Pheiffer, and Winata, 2013).

a TRAIL loan continued to pay the same input prices to TRAIL agents, as households who were recommended but not offered a loan.[39]

In Panel B we find no significant effects on the quantities of output that borrowers sold to TRAIL agents, or the prices at which they sold them. 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.

4.3.4 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 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 MFI in GBL villages) received 75 percent of interest payments as commission, and the repayment rates were similar. Hence TRAIL loans had a higher financial return, as well as higher take-up.

5 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 part of this could be because it is difficult to select productive borrowers.

To overcome this, the trader-agent intermediated lending (TRAIL) scheme delegated borrower selection for individual liability loans to local lenders or traders who had experience in doing business with farmers in the local community. We compared the outcomes of this scheme with

[39]The Hochberg (1988) p-value corrects for the family-weighted error rate across the two index variables in Panels A and B.

the outcomes of a traditional group loan treatment. Both treatments provided loans at below-market-average interest rates, repayment durations that matched crop cycles of the predominant cash crop, and insurance against local yield and price shocks. The scheme was particularly successful at inducing selected beneficiaries to increase potato cultivation and output. TRAIL borrowers achieved significant increases in farm incomes and production of potatoes, without any offsetting decline in income from any other source. Consistent with other recent microcredit experiments, GBL loans failed to generate comparable outcomes.

Some features of the TRAIL loans were non-standard. These include below-market interest rates, longer loan durations and insurance against covariate risks. One may wonder how much these features contributed to its success at raising borrower incomes. Note that in our experiment the GBL loans had non-significant effects on borrower output and incomes even though they shared all of these features, making it unlikely that these variations in the TRAIL scheme were primarily responsible for its success. Some recent experiments have found that simply switching loan liability from groups to individuals also has no effect on production and incomes. For these reasons we are inclined to infer that the unique method of borrower selection in the TRAIL scheme was primarily responsible for its relative success.

Consistent with this idea, we found evidence that TRAIL agents recommended households with low default risk and high productivity. We explained these findings with the help of a model in which borrowers have heterogeneous ability, where ability is correlated with low default risk and high farm productivity. A well-informed TRAIL agent was incentivized to act as a gatekeeper and select high ability borrowers. In contrast, the GBL scheme had no comparable gatekeeper, and attracted both low and high ability borrowers.

The effects of these selection differences could have been compounded by differential incentive effects: theoretically GBL loans could have had a smaller impact on borrowing, cultivation and farm incomes because the joint liability tax raised the effective interest rate. Other factors such as differential scope for learning or social capital may also have played a role. To evaluate the contribution of selection differences *per se*, we used the model to generate a number of testable predictions under the additional hypothesis that some observable characteristics such as caste, religion, and gender of household head are significantly correlated with ability. We then utilized the observed selection differences and estimated heterogeneity of treatment effects across these characteristics to obtain lower bound estimates of the contribution of selection differences to differences in average treatment effects. TRAIL agents selected nearly 9% more borrowers from male-headed non-low-caste Hindu households. These households were more productive to start with, and had larger treatment effects. This difference in the composition of borrowers accounted for 14% of the difference between the average treatment effects of the two treatments. Thus it appears that selection differences accounted for at least part of the differential success of the TRAIL scheme. Once again, this leaves open the possibility that differences in incentives, monitoring or social capital may also have played a role.

Loan take-up rates were higher in the TRAIL scheme, suggesting higher *ex ante* effects on borrower welfare. TRAIL loans were repaid at the same high rate as GBL loans. At the same time, the costs of administering the TRAIL scheme were lower. We found no evidence that TRAIL agents siphoned off the benefits of recommended or treated borrowers by manipulating their other economic transactions with them. These results are encouraging and point to the value of further experimentation with alternative versions of TRAIL. In addition, future research needs to examine a number of issues neglected in this paper. These include the external validity of these results in other regions and contexts, effects of scaling the scheme up to more borrowers per village, financial sustainability, distributive impacts on farm incomes, impacts on empowerment of women or other disadvantaged social groups, household consumption and liquidity management.

Our paper contributes to the policy debate on ways to promote financial inclusion of the rural poor in the developing world. Several countries have attempted to expand financial services in rural areas by employing local agents, but with limited success.[40] 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 policy makers and central bank officials involved in the design of such schemes.

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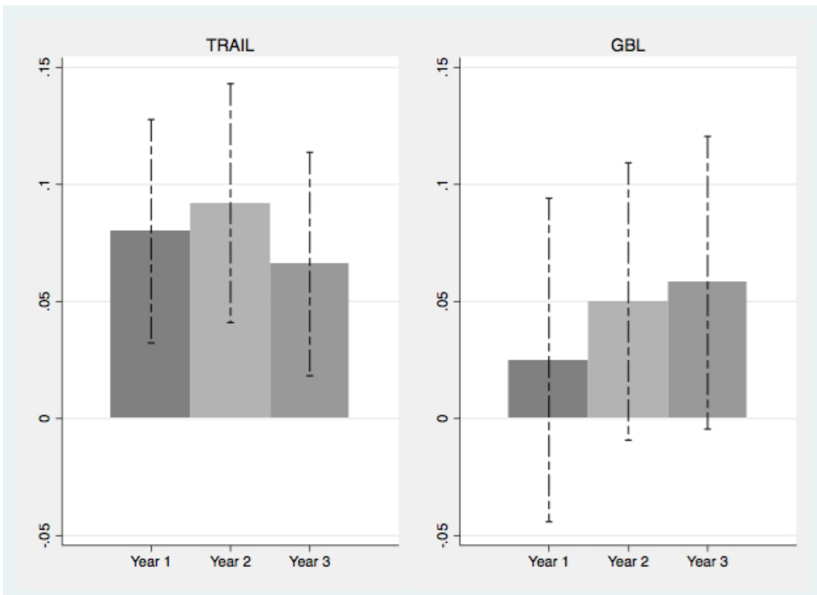
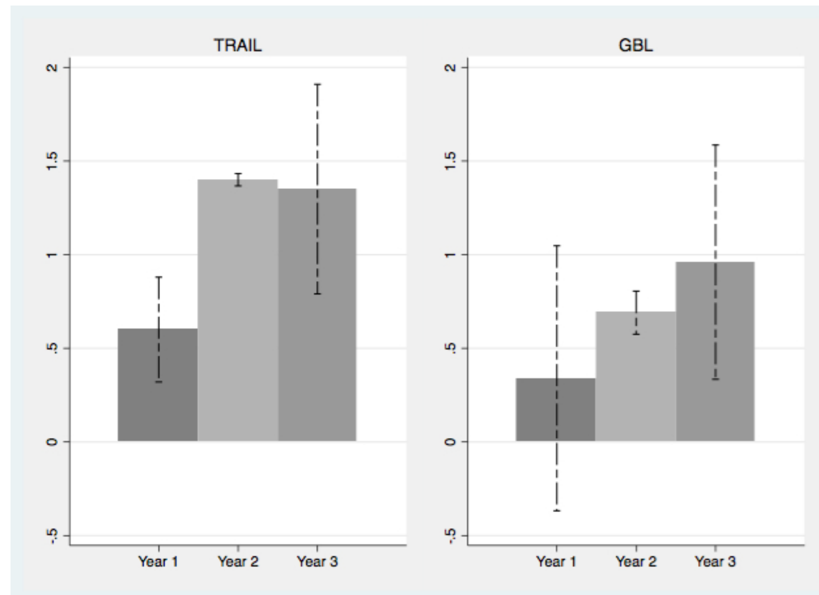
Figure 1: Year-Specific Effects on Potato Acreage and Rate of Return

Potato Acreage



Rate of Return



Notes:
The values represent the estimated treatment effects from regressions following equation (7) in the text, with each sample restricted to a single year of data. In the first panel, the vertical axis measures the treatment effect on acres devoted to potato cultivation. In the second panel the vertical axis measures the rate of return on value added across all four crop categories, 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.

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 Characteristics

	TRAIL (1)	GBL (2)	Difference† (3)
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 Characteristics

	All		TRAIL			GBL	
		Treatment	Control 1	Difference†	Treatment	Control 1	Difference†
	(1)	(2)	(3)	(4)	(5)	(6)	
Non Hindu	0.182	0.163	0.171	-0.008	0.131	0.118	0.013
	(0.008)	(0.025)	(0.025)		(0.022)	(0.022)	
Low caste	0.404	0.374	0.385	-0.010	0.520	0.459	0.061
	(0.011)	(0.032)	(0.032)		(0.033)	(0.034)	
High caste	0.414	0.463	0.444	0.018	0.349	0.423	-0.073
	(0.011)	(0.033)	(0.033)		(0.032)	(0.033)	
Landholding	0.464	0.448	0.454	-0.006	0.354	0.395	-0.040
	(0.009)	(0.027)	(0.025)		(0.025)	(0.026)	
Male headed household	0.941	0.987	0.991	-0.005	0.930	0.895	0.035
	(0.005)	(0.008)	(0.006)		(0.017)	(0.021)	
Head Education: Primary Schooling	0.420	0.520	0.487	0.033	0.432	0.427	0.005
	(0.011)	(0.033)	(0.033)		(0.033)	(0.033)	
Joint Significance ($\chi^2(5)$)			1.04			5.41	

Notes:
Panel A uses village census data collected in 2007 (see Mitra, Mookherjee, Torero, and Visaria, 2015). Panel B restricts the sample to households with atmost 1.5 acres of land. Column 1 includes all households. Columns 2 and 5 include only Treatment households. Columns 3 and 6 include only Control 1 households. Standard errors are in parentheses. ‡: $\chi^2(5)$. †: Difference = Treatment – Control 1. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 3: Correlations across Observable Characteristics

	Gender and Caste/Religion of Household head			
	Female headed	Male headed, Non Hindu household	Male headed, Low caste household	Male headed, High caste household
	(1)	(2)	(3)	(4)
Landholding	0.285	0.458	0.318	0.587
	(0.034)	(0.022)	(0.013)	(0.014)
Education of Household head: Primary school	0.198	0.337	0.315	0.629
	(0.037)	(0.026)	(0.016)	(0.017)
Sample Size	116	341	796	816

Notes:
Means are computed for all sample households (with at most 1.5 acres of land) that fall into the given gender and religion/caste category. Sample weights are assigned as per the description in footnote 13. Standard errors are in parentheses.

Table 4: 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‡				
Traders/Money 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)				
Traders/Money 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)				
Traders/Money 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				
Traders/Money 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 households 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 5: 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 6: Program Impacts: Treatment Effects on Total Borrowing

	All Agricultural Loans (₹) (1)	Non Program Agricultural Loans† (₹) (2)	Index of dependent variables^II (3)
TRAIL	5050***	-364.60	0.242***
	(767.50)	(630.90)	(0.082)
Hochberg p-value			0.025
Mean TRAIL Control 1	5590	5590	
% Effect TRAIL	90.34	-6.52	
GBL	3732***	-157.8	0.176*
	(845.10)	(600.2)	(0.092)
Hochberg p-value			0.368
Mean GBL Control 1	4077	4077	
% Effect GBL	91.54	-3.87	
Sample size	6,204	6,204	

Notes:
Treatment effects are computed from regressions that follow equation (7) in the text and are run on household-year level data for all sample households with at most 1.5 acres of land. Regressions also control for gender and educational attainment of household head, household caste and religion and landholding, a set of year dummies and an information village dummy. % Effect: Treatment effect as a percentage of the mean of the relevant 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 schemes. The complete regression results are in Table A-1. Standard errors in parentheses are clustered at the hamlet level. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 7: Program Impacts: Treatment Effects in Agriculture

Panel A: Potatoes

	Cultivate (%) (1)	Land planted (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	0.047	0.095***	975.371***	1909.738***	4011.624***	2109.242***	1939.494***	0.198***
	(0.032)	(0.028)	(301.124)	(718.799)	(1186.538)	(621.037)	(591.339)	(0.057)
Hochberg p-value								0.003
Mean TRAIL Control 1	0.715	0.333	3646.124	8474.628	14285.467	5739.479	4740.893	
% Effect TRAIL	6.56	28.46	26.75	22.53	28.08	36.75	40.91	
GBL Treatment	0.053	0.052	514.435	1601.298*	2343.964	714.137	553.708	0.111
	(0.044)	(0.035)	(395.082)	(877.219)	(1729.723)	(918.671)	(866.430)	(0.081)
Hochberg p-value								0.861
Mean GBL Control 1	0.620	0.251	2761.127	5992.080	11014.286	4997.446	4018.796	
% Effect GBL	8.59	20.79	18.63	26.72	21.28	14.29	13.78	
Sample Size	6210	6210	6210	6210	6210	6210	6210	

Continued ...

Notes:
Treatment effects are computed from regressions that follow equation (7) 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. Regressions also control for gender and educational attainment of household head, household caste and religion and landholding, a set of year dummies and an information village dummy. 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 hamlet level. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 7 (*Continued*): Program Impacts: Treatment Effects in Agriculture

Panel B: Other Major Crops

	Sesame			Paddy			Vegetables		
	Land planted (Acres) (1)	Value Added (₹) (2)	Index of dependent variables^{II} (3)	Land planted (Acres) (4)	Value Added (₹) (5)	Index of dependent variables^{II} (6)	Land planted (Acres) (7)	Value Added (₹) (8)	Index of dependent variables^{II} (9)
TRAIL Treatment	0.044*	278.223*	0.096	0.036*	267.790	0.045	0.011	51.952	0.044
	(0.023)	(142.192)	(0.058)	(0.020)	(241.457)	(0.030)	(0.007)	(321.736)	(0.080)
Hochberg p-value			0.302			0.269			0.580
Mean TRAIL Control 1	0.266	1519.558		0.470	2556.755		0.015	889.229	
% Effect TRAIL	16.39	18.31		7.66	10.47		72.13	5.84	
GBL Treatment	0.003	-204.084	-0.041	0.011	213.527	-0.004	0.000	-323.404	-0.031
	(0.031)	(229.475)	(0.084)	(0.029)	(271.907)	(0.053)	(0.009)	(676.455)	(0.150)
Hochberg p-value			>0.999			0.943			>0.999
Mean GBL Control 1	0.193	1252.850		0.456	2336.837		0.022	1142.350	
% Effect GBL	1.46	-16.29		2.39	9.14		0.80	-28.31	
Sample Size	6210	6210		6210	6210		6210	6210	

Notes:
Treatment effects are computed from regressions that follow equation (7) 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. Regressions also control for gender and educational attainment of household head, household caste and religion and landholding, a set of year dummies and an information village dummy. 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 1–2 are in Table A-3, to columns 4–5 are in Table A-4, and to columns 7–8 are in Table A-5. Standard errors in parentheses are clustered at the hamlet level. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 8: Program Impacts: Effects on Farm Value Added and Rates of Return

	Farm Value Added (₹)	Non-Agricultural Income (₹)	Index of dependent variables[II]	Rate of Return	
				Potato Cultivation	Farm Value Added
	(1)	(2)	(3)	(4)	(5)
TRAIL Treatment	2707.21***	-608.000	0.116***	1.23***	1.32***
	(739.46)	(4153.557)	(0.044)	(0.03)	(0.02)
Hochberg p-value			0.034		
Mean TRAIL Control 1	10705.02	40115.81			
% Effect TRAIL	25.29	-1.52			
GBL Treatment	400.176	-6092.631	-0.017	0.37***	0.28
	(1069.61)	(4959.88)	(0.047)	(0.08)	(0.21)
Hochberg p-value			>0.999		
Mean GBL Control 1	9729.48	45645.10			
% Effect GBL	4.11	-13.35			
TRAIL vs GBL p-value	0.077			0.000	0.000
Sample Size	6,210	6,210			

Notes:
The analysis is run on household-year level data for all sample households with at most 1.5 acres of land. Regressions also control for gender and educational attainment of household head, household caste and religion and landholding, a set of year dummies and an information village dummy. 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 (7) in the text. The full set of results corresponding to the treatment effects in columns 1 and 2 are in Table A-6. [II]: In column 3 the dependent variable is 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 (1988)'s step-up method to control the FWER across all index outcomes. In columns 1 and 2, standard errors in parentheses are clustered at the hamlet level. In columns 3 and 4, standard errors are cluster-bootstrapped with 2000 replications. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 9: Interest Rate Comparisons Across the TRAIL and GBL Schemes

	TRAIL (1)	Pooled (2)	Pooled (3)
TRAIL × Selected†	0.016	-0.026	-0.011
	(0.016)	(0.049)	(0.032)
TRAIL × Own Clientele†	0.045		0.044*
	(0.033)		(0.026)
TRAIL × Selected × Own Clientele†	-0.065**		-0.064*
	(0.031)		(0.034)
TRAIL		-0.014	-0.023
		(0.043)	(0.025)
Selected		0.027	0.027
		(0.047)	(0.027)
Constant	0.209***	0.227***	0.228***
	(0.014)	(0.047)	(0.026)
Total Effects			
TRAIL Selected from Own Clientele	0.20***		
	(0.02)		
TRAIL Not Selected from Own Clientele	0.25***		
	(0.03)		
TRAIL Selected		0.21***	
		(0.02)	
GBL Selected		0.25***	0.25***
		(0.02)	(0.03)
TRAIL Selected Own Clientele			0.20***
			(0.02)
TRAIL Selected Other Clientele			0.22***
			(0.01)
Difference Estimates			
TRAIL: Selected Own Clientele - Not Selected Own Clientele	-0.05*		
	(0.03)		
TRAIL Selected - GBL Selected		-0.04*	
		(0.02)	
TRAIL Selected Own Clientele - GBL Selected			-0.05*
			(0.03)
TRAIL Selected Other Clientele - GBL Selected			-0.03
			(0.03)
Sample Size	448	872	872
Average Interest paid on Informal loans			
TRAIL		0.22	
GBL		0.23	
Sample Size	448	872	872

Notes:
The dependent variable is the average annualized interest rate paid on informal production loans from traders, moneylenders and family and friends, as reported in Cycle 1. In column 1 the estimating sample includes all sample households in TRAIL villages with at most 1.5 acres who had borrowed from traders, moneylenders and family and friends in Cycle 1. In columns 2 and 3 the estimating sample includes all sample households in TRAIL and GBL villages with at most 1.5 acres of land and had borrowed from traders, moneylenders and family and friends in Cycle 1. Regressions control for landholding. Loans where the principal amount is reported equal to the repayment amount are not included. Standard errors in parentheses are clustered at the hamlet level. †: By definition, TRAIL = 1 for all observations in column 1. $^{***}: p < 0.01$, $^{**}: p < 0.05$, $^{*}: p < 0.1$.

Table 10: Variation of Observable Characteristics with Interest rate, Scale of Borrowing, Output, Revenue and Value Added

	Interest Rate (1)	Agricultural Borrowing (₹) (2)	Potato output (kg) (3)	Farm revenue (₹) (4)	Farm value-added (₹) (5)
Female-headed	0.152	3287.77	3328.34	111419.3	3858.6
Male-headed low caste Hindu	0.222	5020.33	4428.07	117292.9	6876.8
Male-headed non-Hindu	0.209	6963.81	5507.65	125497.1	10912
Male-headed high caste Hindu	0.195	8929.90	6736.49	131758.5	13473.1
Tests of difference					
Male-headed low caste v. Female-headed household	0.070**	1732.6***	1099.7***	5873.6***	3018.2***
Male-headed non-Hindu v. Female-headed household	0.057*	3676.0***	2179.3***	14077.8***	7053.4***
Male-headed high caste v. Female-headed household	0.043	5642.1***	3408.2***	20339.2***	9614.5***
Male-headed non-Hindu v. Male-headed low caste household	-0.013	1943.5**	1079.6***	8204.2***	4035.2***
Male-headed high caste v. Male-headed low caste household	-0.027**	3909.6***	2308.4***	14465.6***	6596.3***
Male-headed high caste v. Male-headed non-Hindu household	-0.014	1966.1**	1228.8***	6261.4**	2561.1*
Sample Size	870	4830	4833	4833	4833

Notes:
Only Control 1 and Control 2 households are included in the estimating sample, and they are re-weighted using the weights described in footnote 13. Standard errors in parentheses are clustered at the hamlet level. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 11: Heterogeneity of Effects

	Total Borrowing (1)	Farm Revenue (2)	Farm Value Added (3)
Treatment Effects TRAIL			
Mean TRAIL Control 1	5590	22849	10705
Female headed household	459.4	5111	2404
	(1882)	(5977)	(3450)
% Effect	8.22	22.37	22.45
Male headed Non Hindu household	-3069	3686	1637
	(2766)	(4046)	(2072)
% Effect	-54.90	16.13	15.29
Male headed low caste household	4071***	1041	-174
	(1309)	(2413)	(1264)
% Effect	72.83	4.56	-1.63
Male headed high caste household	6527***	8089**	5190***
	(1600)	(3336)	(1846)
% Effect	116.76	35.40	48.49
Comparison of Treatment Effects across Groups (TRAIL)			
Male headed Non Hindu vs. Female	-3528*	-1426	-766
Male headed low caste vs. Female	3612	-4070	-2578
Male headed high caste vs. Female	6068**	2978	2787
Male headed low caste vs. Male headed Non Hindu	7140**	-2645	-1812
Male headed Non Hindu vs. Male headed high caste	-9596**	-4403	-3553
Male headed low caste vs. Male headed high caste	-2456	-7048	-5364**
Treatment Effects GBL			
Mean GBL Control 1	4077	19605	9729
Female headed household	-2789	-8094	-5057*
	(1892)	(5136)	(2645)
% Effect	-68.41	-41.28	-51.97
Male headed Non Hindu household	-3714	2101	2225
	(3258)	(7052)	(3007)
% Effect	-91.10	10.72	22.87
Male headed low caste household	3962***	3924	1551
	(1186)	(3313)	(1651)
% Effect	97.18	20.02	15.94
Male headed high caste household	4301**	-2085	-2421
	(2206)	(5604)	(3554)
% Effect	105.49	-10.64	-24.88
Comparison of Treatment Effects across Groups (GBL)			
Male headed Non Hindu vs. Female	-925	10195	7281*
Male headed low caste vs. Female	6751***	12018*	6608**
Male headed high caste vs. Female	7090**	6009	2636
Male headed low caste vs. Male headed Non Hindu	7676**	1823	-674
Male headed Non Hindu vs. Male headed high caste	-8015	4186	4646
Male headed low caste vs. Male headed high caste	-339	6009	3972
Sample Size	6,204	6,216	6216

Notes:
Treatment effects and differences in treatment effects are presented. The sample includes all households in TRAIL and GBL villages with at most 1.5 acres of land. Regressions control for year dummies and information village dummy. % Effect is computed as a percentage of the relevant mean for Control 1 households. Standard errors in parentheses are clustered at the hamlet level. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 12: Decomposition of Average Effect

	Fraction of recommended in		Difference	TRAIL	Difference ×	% of Difference
	TRAIL	GBL	(TRAIL–GBL)	treatment effect	TRAIL treatment effect	in average effect
	(1)	(2)	(3)	(4)	(5)	(6)
Female headed household	0.011	0.087	-0.076***	2406.952	-182.928	
Male headed Non Hindu household	0.163	0.122	0.041*	1639.060	67.201	
Male headed low caste household	0.375	0.428	-0.053*	-175.233	9.287	
Male headed high caste household	0.451	0.363	0.088**	5191.126	456.819	
% of Average effect due to Gender and Religion/caste						0.143

Notes:
The sample includes all households in TRAIL and GBL villages with at most 1.5 acres of land. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

Table 13: Loan Performance

	Repayment (1)	Take up (2)	Continuation (3)
Panel A: Sample Means			
TRAIL	0.958	0.856	0.805
	0.005	(0.008)	(0.009)
GBL	0.954	0.746	0.691
	(0.006)	(0.011)	(0.011)
Difference†	0.003	0.110***	0.114***
	(0.008)	(0.014)	(0.014)
Panel B: Regression Results			
TRAIL	0.006	0.117*	0.116*
	(0.020)	(0.067)	(0.067)
Constant	0.989***	0.838***	0.827***
	(0.015)	(0.053)	(0.053)
Mean GBL	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. In Panel B, regressions control for cycle dummies. Standard errors in parentheses are clustered at the hamlet level. †: Difference between mean in TRAIL and mean in GBL. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 14: Treatment Effects for Transactions with TRAIL agent

Panel A: Input Purchase†

	Purchased from agent	Agent's share in purchases	Input Price (Rs/unit) Fertilizer	Outside Seed	Pesticide	Power tiller	Water	Index of input prices[II]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TRAIL Treatment	0.002	0.000	0.136	2.099*	-32.41	-29.11***	109.80	0.023
	(0.014)	(0.011)	(0.929)	(1.131)	(48.30)	(4.854)	(109.80)	(0.026)
Hochberg p-value								0.773
Mean Control 1	0.0813	0.0620	15.77	24.82	536.8	211.2	72.30	
Sample Size	17,928	17,784	2,908	2,394	3,830	1,983	1,822	

Panel B: Output Sales†

	Sold to agent	Agent's share in sales	Output Price (Rs/kg) Potato	Paddy	Sesame	Index of output prices[II]
	(1)	(2)	(3)	(4)	(5)	(6)
TRAIL Treatment	0.020	0.027	-0.024	0.401	0.010	0.001
	(0.029)	(0.028)	(0.141)	(0.285)	(0.516)	(0.005)
Hochberg p-value						0.846
Mean Control 1	0.192	0.152	4.566	10.13	30.59	
Sample Size	4,303	4,098	2,026	791	1,280	

Panel C: Borrowing†

	Borrowed from Agent	Agent's share in borrowing	APR
	(1)	(2)	(3)
TRAIL Treatment	-0.076**	-0.036***	0.011
	(0.038)	(0.012)	0.043
Mean Control 1	0.161	0.0489	0.139
Sample Size	1,960	1,960	5,468

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. II: In column 8 in Panel A and Column 6 in Panel B the dependent variables are indices of z-scores of input prices and output price respectively, following Kling, Liebman, and Katz (2007); p-values for this regression are reported using Hochberg (1988)'s step-up method to control the FWER across the two indices. Standard errors in parentheses are clustered at the hamlet level. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

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

	All Loans (₹) (1)	Non Program Loans† (₹) (2)
TRAIL	1,314.487**	1,258.715**
	(597.577)	(597.740)
TRAIL × Control 1	-216.505	-162.695
	(623.478)	(616.692)
TRAIL × Treatment	4,833.918***	-527.303
	(631.596)	(491.950)
GBL × Control 1	324.865	254.498
	(674.202)	(674.256)
GBL × Treatment	4,056.411***	96.717
	(802.940)	(556.996)
Landholding	9,078.801***	8,275.706***
	(702.236)	(696.243)
Non Hindu household	-3,287.325***	-2,772.558***
	(643.155)	(585.505)
Low caste household	-1,692.419***	-1,658.744***
	(536.831)	(462.049)
Male headed household	1,746.050***	1,349.010***
	(502.763)	(498.760)
Household head: Completed Primary Schooling	-4.810	66.508
	(425.426)	(392.054)
Constant	-84.527	795.939
	(702.412)	(644.860)
Treatment Effect		
TRAIL	5050***	-364.60
	(767.50)	(630.90)
Mean TRAIL Control 1	5590	5590
% Effect TRAIL	90.34	-6.52
GBL	3732***	-157.8
	(845.10)	(600.2)
Mean GBL Control 1	4077	4077
% Effect GBL	91.54	-3.87
Recommendation/Group Formation Effect		
TRAIL Recommendation	-216.50	-162.7
	(623.50)	(616.7)
GBL Group Formation	324.90	254.50
	(674.20)	(674.30)
Sample size	6,204	6,204

Notes:
Treatment effects are computed from regressions that follow equation (7) in the text and are run on household-year level data for all sample households with at most 1.5 acres of land. Regressions also control for a set of year dummies and an information village dummy. % Effect: Treatment effect as a percentage of the mean of the relevant Control 1 group. †: Non-Program loans refer to loans from sources other than the TRAIL/GBL schemes. Standard errors in parentheses are clustered at the hamlet level. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

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

	Cultivate (%) (1)	Land planted (Acres) (2)	Harvested quantity (Kg) (3)	Cost of production (₹) (4)	Revenue (₹) (5)	Value added (₹) (6)	Imputed profit‡ (₹) (7)
TRAIL	0.057*	0.032	292.763	781.354	526.844	-263.985	-290.129
	(0.030)	(0.027)	(304.252)	(684.127)	(1,222.338)	(617.008)	(580.964)
TRAIL × Control 1	0.069*	0.01	137.647	446.827	433.107	-19.9	-99.093
	(0.035)	(0.026)	(293.854)	(705.030)	(1,186.249)	(600.392)	(565.336)
TRAIL × Treatment	0.116***	0.104***	1,113.018***	2,356.565***	4,444.731***	2,089.342***	1,840.401***
	(0.031)	(0.024)	(264.634)	(615.135)	(1,037.906)	(524.430)	(491.336)
GBL × Control 1	0.06	-0.002	-39.003	-360.801	-641.264	-260.993	-406.701
	(0.037)	(0.033)	(381.224)	(804.040)	(1,685.321)	(945.242)	(897.473)
GBL × Treatment	0.114***	0.050*	475.432	1,240.496*	1,702.70	453.143	147.007
	(0.035)	(0.027)	(299.069)	(703.861)	(1,249.871)	(616.922)	(569.248)
Landholding	0.364***	0.471***	5,172.909***	10,555.661***	21,918.658***	11,262.024***	10,370.559***
	(0.028)	(0.031)	(351.564)	(795.920)	(1,424.911)	(718.648)	(682.919)
Non Hindu household	-0.246***	-0.126***	-1,456.438***	-3,666.870***	-6,134.404***	-2,471.695***	-1,937.662***
	(0.041)	(0.032)	(353.577)	(791.591)	(1,424.167)	(705.968)	(649.551)
Low caste household	-0.091***	-0.067***	-785.399***	-2,350.417***	-3,662.629***	-1,299.159**	-1,201.942**
	(0.027)	(0.024)	(275.179)	(619.846)	(1,080.364)	(516.179)	(480.608)
Male headed household	0.173***	0.089***	994.458***	2,193.331***	3,533.360***	1,319.783**	925.504*
	(0.039)	(0.025)	(280.924)	(604.000)	(1,176.181)	(594.393)	(545.037)
Household head: Completed Primary Schooling	-0.032*	0.001	26.736	318.901	134.955	-198.726	-5.649
	(0.018)	(0.017)	(190.199)	(433.805)	(751.643)	(371.166)	(346.968)
Constant	0.372***	0.018	327.659	332.504	-421.257	-811.724	-926.514
	(0.048)	(0.033)	(377.208)	(816.221)	(1,567.057)	(826.290)	(781.899)
Treatment Effects							
TRAIL Treatment	0.047	0.095***	975.371***	1909.738***	4011.624***	2109.242***	1939.494***
	(0.032)	(0.028)	(301.124)	(718.799)	(1186.538)	(621.037)	(591.339)
Mean TRAIL Control 1	0.715	0.333	3646.124	8474.628	14285.467	5739.479	4740.893
% Effect TRAIL	6.56	28.46	26.75	22.53	28.08	36.75	40.91
GBL Treatment	0.053	0.052	514.435	1601.298*	2343.964	714.137	553.708
	(0.044)	(0.035)	(395.082)	(877.219)	(1729.723)	(918.671)	(866.430)
Mean GBL Control 1	0.620	0.251	2761.127	5992.080	11014.286	4997.446	4018.796
% Effect GBL	8.59	20.79	18.63	26.72	21.28	14.29	13.78
Recommendation/Group Formation Effects							
TRAIL Recommendation	0.069	0.010	137.647	446.827	433.107	-19.900	-99.093
	(0.035)	(0.026)	(293.854)	(705.030)	(1186.249)	(600.392)	(565.336)
GBL Group Formation	0.060	-0.002	-39.003	-360.801	-641.264	-260.993	-406.701
	(0.037)	(0.033)	(381.224)	(804.040)	(1685.321)	(945.242)	(897.473)
Sample Size	6210	6210	6210	6210	6210	6210	6210

Notes: Treatment effects are computed from regressions that follow equation (7) 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. Regressions also control for a set of year dummies and an information village dummy. Standard errors in parentheses are clustered at the hamlet level. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

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

	Cultivate (%) (1)	Land planted (Acres) (2)	Harvested quantity (Kg) (3)	Cost of production (₹) (4)	Revenue (₹) (5)	Value added (₹) (6)	Imputed profit‡ (₹) (7)
TRAIL	0.076**	0.042*	13.575*	34.763	216.493	180.985	103.803
	(0.035)	(0.021)	(7.615)	(38.950)	(175.758)	(148.649)	(127.428)
TRAIL × Control 1	0.087**	0.036*	6.924	82.209**	220.684	138.585	86.299
	(0.035)	(0.022)	(7.105)	(36.323)	(171.621)	(152.051)	(135.914)
TRAIL × Treatment	0.122***	0.079***	16.567**	108.064**	525.663***	416.808**	265.513*
	(0.035)	(0.020)	(7.703)	(42.835)	(199.094)	(175.922)	(160.609)
GBL × Control 1	0.087**	0.03	7.1	-29.721	177.119	205.714	109.877
	(0.038)	(0.029)	(9.517)	(35.876)	(249.834)	(226.649)	(202.945)
GBL × Treatment	0.063*	0.033	1.652	-12.931	-11.49	1.631	-19.497
	(0.034)	(0.023)	(7.225)	(37.340)	(169.964)	(144.475)	(124.771)
Landholding	0.369***	0.361***	101.112***	456.443***	2,619.484***	2,160.211***	1,824.131***
	(0.025)	(0.024)	(8.423)	(39.195)	(202.611)	(179.670)	(166.549)
Non Hindu household	-0.219***	-0.110***	-43.415***	-190.726***	-981.758***	-790.540***	-586.309***
	(0.040)	(0.024)	(8.564)	(37.854)	(225.407)	(197.268)	(174.952)
Low caste household	-0.046	-0.035*	-24.264***	-87.270**	-570.446***	-483.747***	-471.244***
	(0.032)	(0.019)	(6.397)	(34.847)	(148.469)	(123.300)	(104.570)
Male headed household	0.159***	0.058***	20.140***	60.999**	417.114***	357.395***	160.861
	(0.030)	(0.017)	(5.775)	(29.267)	(146.048)	(124.006)	(104.290)
Household head: Completed Primary Schooling	-0.028	-0.006	-0.845	55.517**	-31.173	-87.309	-17.108
	(0.019)	(0.012)	(4.247)	(23.008)	(102.636)	(89.738)	(79.863)
Constant	0.206***	-0.019	11.853	105.689**	-182.827	-287.901	-235.148
	(0.042)	(0.026)	(8.489)	(46.796)	(214.331)	(181.757)	(157.018)
Treatment Effects							
TRAIL Treatment	0.035	0.044**	9.643	25.855	304.979*	278.223**	179.214
	(0.033)	(0.023)	(6.738)	(44.185)	(170.527)	(142.192)	(124.683)
Mean TRAIL Control 1	0.581	0.266	81.624	436.910	1957.498	1519.558	1080.800
% Effect TRAIL	6.02	16.39	11.81	5.92	15.58	18.31	16.58
GBL Treatment	-0.024	0.003	-5.449	16.790	-188.605	-204.084	-129.374
	(0.044)	(0.031)	(9.768)	(39.016)	(256.021)	(229.475)	(203.585)
Mean GBL Control 1	0.484	0.193	60.848	258.878	1513.138	1252.850	866.288
% Effect GBL	-4.97	1.46	-8.95	6.49	-12.46	-16.29	-14.93
Recommendation/Group Formation Effects							
TRAIL Recommendation	0.087**	0.036*	6.924	82.209**	220.684	138.585	86.299
	(0.035)	(0.022)	(7.105)	(36.323)	(171.621)	(152.051)	(135.914)
GBL Group Formation	0.087**	0.030	7.100	-29.721	177.119	205.714	109.877
	(0.038)	(0.029)	(9.517)	(35.876)	(249.834)	(226.649)	(202.945)
Sample Size	6210	6210	6210	6210	6210	6210	6210

Notes: Treatment effects are computed from regressions that follow equation (7) 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. Regressions also control for a set of year dummies and an information village dummy. Standard errors in parentheses are clustered at the hamlet level. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

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

	Cultivate (%) (1)	Land planted (Acres) (2)	Harvested quantity (Kg) (3)	Cost of production (₹) (4)	Revenue (₹) (5)	Value added (₹) (6)	Imputed profit‡ (₹) (7)
TRAIL	-0.034	-0.016	-47.293	-275.251	-390.783	-82.75	-27.327
	(0.025)	(0.026)	(47.688)	(233.848)	(408.812)	(268.409)	(151.034)
TRAIL × Control 1	0.091***	0.015	26.824	87.853	112.528	5.425	-54.62
	(0.029)	(0.022)	(35.814)	(183.485)	(316.830)	(223.642)	(118.265)
TRAIL × Treatment	0.086***	0.051**	49.034	300.108	583.611*	273.215	80.825
	(0.030)	(0.021)	(31.896)	(184.719)	(303.178)	(234.779)	(117.951)
GBL × Control 1	0.04	0.04	153.415**	504.43	485.573	3.818	76.057
	(0.029)	(0.030)	(75.249)	(378.657)	(512.717)	(267.850)	(219.305)
GBL × Treatment	0.039	0.051*	116.894**	429.441	600.05	217.345	-44.305
	(0.031)	(0.029)	(57.721)	(307.952)	(432.811)	(231.699)	(166.892)
Landholding	0.461***	0.894***	1,095.056***	4,936.404***	9,176.493***	4,355.661***	965.438***
	(0.025)	(0.035)	(58.297)	(306.641)	(502.942)	(310.850)	(162.100)
Non Hindu household	-0.093***	0.034	74.091	529.416	653.812	246.985	287.07
	(0.030)	(0.036)	(78.212)	(416.421)	(578.789)	(311.277)	(193.235)
Low caste household	-0.068***	0.013	2.892	-233.103	-29.732	244.56	102.271
	(0.023)	(0.021)	(39.289)	(199.890)	(294.968)	(188.775)	(114.158)
Male headed household	0.203***	0.126***	217.535***	1,007.446***	1,883.556***	857.514***	-65.749
	(0.037)	(0.026)	(39.833)	(186.046)	(335.587)	(197.871)	(77.607)
Household head: Completed Primary Schooling	-0.042**	-0.033*	-78.276**	-59.974	-389.578	-317.842*	166.684**
	(0.017)	(0.019)	(31.112)	(161.223)	(288.158)	(175.847)	(80.034)
Constant	0.420***	0.009	-462.507***	104.135	-666.993	-710.122**	-545.365***
	(0.046)	(0.033)	(62.141)	(278.700)	(499.964)	(319.610)	(175.018)
Treatment Effects							
TRAIL Treatment	-0.005	0.036*	22.210	212.254	471.083*	267.790	135.445
	(0.032)	(0.020)	(30.817)	(178.716)	(280.807)	(241.457)	(131.079)
Mean TRAIL Control 1	0.744	0.470	569.726	2889.838	5398.490	2556.755	93.133
% Effect TRAIL	-0.71	7.66	3.90	7.35	8.73	10.47	145.43
GBL Treatment	-0.001	0.011	-36.521	-74.989	114.480	213.527	-120.362
	(0.039)	(0.029)	(68.446)	(354.916)	(447.467)	(271.907)	(227.270)
Mean GBL Control 1	0.689	0.456	672.894	3225.745	5513.227	2336.837	183.163
% Effect GBL	-0.07	2.39	-5.43	-2.33	2.08	9.14	-65.71
Recommendation/Group Formation Effects							
TRAIL Recommendation	0.091***	0.015	26.824	87.853	112.528	5.425	-54.620
	(0.029)	(0.022)	(35.814)	(183.485)	(316.830)	(223.642)	(118.265)
GBL Group Formation	0.040	0.040	153.415**	504.430	485.573	3.818	76.057
	(0.029)	(0.030)	(75.249)	(378.657)	(512.717)	(267.850)	(219.305)
Sample Size	6210	6210	6210	6210	6210	6210	6210

Notes: Treatment effects are computed from regressions that follow equation (7) 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. Regressions also control for a set of year dummies and an information village dummy. Standard errors in parentheses are clustered at the hamlet level. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.

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

	Cultivate (%) (1)	Land planted (Acres) (2)	Harvested quantity (Kg) (3)	Cost of production (₹) (4)	Revenue (₹) (5)	Value added (₹) (6)	Imputed profit‡ (₹) (7)
TRAIL	0.013	0.003	92.447	87.622	257.174	155.847	174.656
	(0.023)	(0.005)	(58.992)	(103.217)	(432.459)	(332.506)	(268.585)
TRAIL × Control 1	-0.014	-0.004	-38.028	-71.467	-26.997	52.419	35.741
	(0.016)	(0.003)	(34.800)	(71.290)	(236.179)	(174.366)	(144.961)
TRAIL × Treatment	-0.013	0.007	-10.403	9.881	110.163	104.37	25.055
	(0.018)	(0.007)	(54.886)	(108.466)	(407.183)	(310.625)	(243.261)
GBL × Control 1	0.043	0.009	75.01	173.209	819.137	631.206	537.885
	(0.032)	(0.008)	(56.237)	(167.108)	(819.597)	(642.212)	(562.809)
GBL × Treatment	0.054**	0.009**	76.132**	195.138**	511.006*	307.802	141.274
	(0.021)	(0.004)	(31.890)	(87.212)	(278.071)	(210.586)	(164.347)
Landholding	0.046***	0.015***	160.812**	194.991**	1,220.125***	1,007.720***	834.731***
	(0.016)	(0.005)	(62.363)	(80.159)	(381.881)	(305.442)	(267.465)
Non Hindu household	-0.088***	-0.019***	-212.183***	-378.125***	-1,870.716***	-1,468.966***	-1,169.962***
	(0.025)	(0.006)	(61.072)	(115.947)	(480.626)	(371.842)	(303.036)
Low caste household	-0.067***	-0.014***	-171.913***	-354.617***	-1,648.337***	-1,273.133***	-1,059.710***
	(0.024)	(0.005)	(51.936)	(102.421)	(453.177)	(352.217)	(287.474)
Male headed household	0.065***	0.012***	75.320***	265.743***	641.904***	367.626**	207.3
	(0.013)	(0.003)	(26.894)	(52.538)	(217.433)	(169.123)	(133.128)
Household head: Completed Primary Schooling	-0.021**	-0.005	-60.686**	-101.688**	-511.050***	-402.102***	-280.270**
	(0.010)	(0.003)	(28.471)	(47.914)	(189.477)	(149.930)	(134.469)
Constant	0.036*	0.003	-15.724	55.99	139.705	90.951	43.941
	(0.020)	(0.005)	(46.817)	(83.638)	(324.532)	(251.147)	(206.444)
Treatment Effects							
TRAIL Treatment	0.000	0.011	27.625	81.348	137.159	51.952	-10.686
	(0.021)	(0.007)	(45.780)	(106.058)	(418.599)	(321.736)	(236.536)
Mean TRAIL Control 1	0.080	0.015	142.823	307.071	1207.642	889.229	664.507
% Effect TRAIL	0.33	72.13	19.34	26.49	11.36	5.84	-1.61
GBL Treatment	0.010	0.000	1.122	21.929	-308.131	-323.404	-396.611
	(0.036)	(0.009)	(61.427)	(180.672)	(862.988)	(676.455)	(582.580)
Mean GBL Control 1	0.112	0.022	135.893	404.919	1564.029	1142.350	853.062
% Effect GBL	9.08	0.80	0.83	5.42	-19.70	-28.31	-46.49
Recommendation/Group Formation Effects							
TRAIL Recommendation	-0.014	-0.004	-38.028	-71.467	-26.997	52.419	35.741
	(0.016)	(0.003)	(34.800)	(71.290)	(236.179)	(174.366)	(144.961)
GBL Group Formation	0.043	0.009	75.010	173.209	819.137	631.206	537.885
	(0.032)	(0.008)	(56.237)	(167.108)	(819.597)	(642.212)	(562.809)
Sample Size	6210	6210	6210	6210	6210	6210	6210

Notes: Treatment effects are computed from regressions that follow equation (7) 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. Regressions also control for a set of year dummies and an information village dummy. Standard errors in parentheses are clustered at the hamlet level. $^{***}: p < 0.01,^{**}: p < 0.05,^{*}: p < 0.1$.

Table A-6: Program Impacts: Treatment Effects in Aggregate Farm Value-Added and Non Agricultural Incomes

	Farm Value-Added (₹) (1)	Non-Agricultural Income (₹) (2)
TRAIL	-9.903	1,167.37
	(751.40)	(4,619.44)
TRAIL × Control 1	176.529	-11,159.343***
	(777.903)	(3,686.649)
TRAIL × Treatment	2,883.735***	-11,767.343***
	(703.472)	(4,211.561)
GBL × Control 1	579.745	-4,744.83
	(1,106.787)	(5,088.544)
GBL × Treatment	979.921	-10,837.461**
	(747.682)	(4,600.405)
Landholding	18,785.617***	3,415.37
	(932.284)	(5,374.352)
Non Hindu household	-4,484.216***	5,675.42
	(904.270)	(4,559.521)
Low caste household	-2,811.479***	1,324.12
	(584.828)	(3,610.341)
Male headed household	2,902.318***	-4,912.29
	(857.462)	(10,315.913)
Household head: Completed Primary Schooling	-1,005.980**	-260.836
	(507.548)	(3,031.750)
Constant	-1,718.80	45,738.771***
	(1,065.959)	(8,109.731)
Treatment Effects		
TRAIL Treatment	2707.206***	-608.000
	(739.438)	(4153.557)
Mean TRAIL Control 1	10705.022	40115.808
% Effect TRAIL	25.29	-1.52
GBL Treatment	400.176	-6092.631
	(1069.611)	(4959.881)
Mean GBL	9729.483	45645.104
% Effect GBL	4.11	-13.35
TRAIL vs GBL p-value	0.077	0.393
Recommendation/Group Formation Effects		
TRAIL Recommendation	176.529	-11159.343***
	(777.903)	(3686.649)
GBL Group Formation	579.745	-4744.830
	(1106.787)	(5088.544)
Sample Size	6,210	6,210

Notes:
Treatment effects are computed from regressions that follow equation (7) 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. Regressions also control for a set of year dummies and an information village dummy. Standard errors in parentheses are clustered at the hamlet level. $^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$.