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JEL Classification: H42, I38, O13, O16, O17

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Decentralized Targeting of Agricultural Credit Programs: Private versus Political Intermediaries*

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1 Introduction

Increasingly, public programs in developing countries are being implemented at the local level. Communities are involved in monitoring service providers and selecting beneficiaries, and local governments are taking responsibility for delivering development and welfare programs. This move toward decentralized program delivery rests on the presumption that local providers correctly recognize beneficiary merit and need, and are more accountable to the beneficiary population than distant officials. Indeed, several recent papers find that community networks and connections accurately target beneficiaries, diffuse information and increase program take-up [Bandiera and Rasul, 2006, Alatas et al., 2012, 2016, Fisman et al., 2017, Hussam et al., 2018, Berg et al., 2018, Beaman and Magruder, 2012, Debnath and Jain, 2020, Beaman et al., 2021, Banerjee et al., 2013, 2019].

However some delegated programs have had an anti-poor bias and mis-allocated benefits, such as when local community agents target their personal contacts, or when local elites influence beneficiary selection [Deserranno et al., 2019, Bandiera et al., 2020, Vera-Cossio, 2021, Banerjee et al., 2019].[1] This raises several questions: how to choose local delivery agents, what responsibilities to delegate and how to incentivize them. For instance, one could limit diversion by restricting eligibility to individuals with low assets, income or consumption. Agent commissions could depend on specific performance indicators, so as to minimize collusion or mis-targeting. In this paper we argue that even with such safeguards in place, the decision of which kind of agent to appoint is an important element of program design. Village residents participate in different networks in different spheres of their lives: economic, social and political. The nodal agents of these networks are experts in different domains, and can have different motivations. Accordingly, the effectiveness of the intervention could depend on which nodal agent is appointed.

To examine this issue we use data from a field experiment we conducted during 2010–13 in 48 randomly selected villages in West Bengal, India. The experiment sought to evaluate the effectiveness of appointing nodal agents from within the community to act as intermediaries in a subsidized rural credit program for smallholder farmers. In the 24 randomly selected villages where we ran the Trader-Agent Intermediated Lending (or TRAIL) scheme, these were prominent traders or lenders in the village. In the remaining villages, where we ran the Gram Panchayat Agent Intermediated Lending (or GRAIL) scheme, agents were nominated by the local government council.[2] The two types of agents therefore occupied nodal positions in different village networks and accordingly they may have had different motivations and expertise, may have held different kinds of local information about and had different relationships with village residents.

[1]For broader overviews of the evidence on elite capture and clientelism in decentralized programs, see World Development Report [2004], Mansuri and Rao [2013], Mookherjee [2015], Bardhan and Mookherjee [2020].

[2]In 24 other randomly chosen villages we implemented a traditional group-based lending (GBL) scheme. In Maitra et al. [2017] we compared the TRAIL scheme with the GBL scheme and found that superior borrower selection explained about 40% of the larger increase in farm value-added in the TRAIL scheme.

The GRAIL scheme resembles political decentralization, where program implementation is delegated to the local government. Conceivably, it could be vulnerable to political distortions such as elite capture or clientelism. The TRAIL scheme represents a more novel form of decentralization, where delivery of a development program is delegated to a private agent. Compared to a political appointee, a private trader-lender with extensive business connections in the village may be better informed of farmers' productivity and reliability, and have weaker political motives. However, this scheme could be susceptible to diversion: the agents could selectively recommend their own clients, collude with them, or attempt to extract their benefits by strategically manipulating their own trades [Floro and Ray, 1997, Mitra et al., 2018].

In both treatment arms, the program had some common in-built safeguards against the kind of mis-targeting documented in recent experiments in Uganda and Thailand [Bandiera et al., 2020, Vera-Cossio, 2021]. To ensure that loans were targeted to smallholder farmers, agents could only recommend households that owned less than 1.5 acres of cultivable land. The agents had no control over the interest rate, duration or other terms of the loan. They were incentivised through commissions equal to 75% of the interest payments made by the borrowers they recommended.[3] Once the agent had recommended borrowers, they had no further formal role in the scheme. The microfinance institution that we collaborated with disbursed the loans to farmers and collected the repayments.

Interest rates on the program loans were set below the informal market rate. Borrowers were not required post any collateral. They were only liable for the repayment of their own loans. Loan cycles and durations were synchronized with crop seasons, to enable borrowers to finance working capital for the planting of potatoes, the major cash crop in the region, and easy repayment upon harvest. Repayment obligations were reduced if the village faced a yield or output price shock. The initial loan size of Rs. 2000 was equal to approximately 25% of the average amount that sample households borrowed from informal sources in the potato planting season. We conducted the experiment over eight successive four month loan cycles spanning October 2010—July 2013. Conditional on adequate repayment, credit limits increased by 33% in each successive loan cycle, thus providing dynamic incentives to repay.

The experiment was designed to separately identify borrower selection and treatment effects. Each agent had to recommend 30 potential borrowers. Of the 30 households that the agent recommended in each village, a randomly selected subset of 10 households were offered the loans. All other (untreated) households, therefore, belong to either of two groups, depending on whether they were recommended by the agent or not. Comparisons between these two groups reveal pure selection differences, while comparisons between treated and recommended but untreated groups reveal treatment effects.

Take-up rates were high in both schemes, but were significantly higher (94%) in the villages assigned to the TRAIL scheme than in those assigned to the GRAIL scheme (87%).

[3] In Maitra et al. [2017] we present a model that explains how this reduces the risk of collusion, and motivates the agent to recommend high productivity borrowers.

Repayment rates were equally high in both schemes (93%). In both schemes, farmers who were randomly selected to receive the loans borrowed more, planted more potatoes and had greater potato output than other borrowers whom the agents had also recommended, but did not receive the loan. However, profits from potatoes and aggregate farm profits only increased for the TRAIL beneficiaries (by 40 percent and 28 percent, respectively).[4] The corresponding point estimates in GRAIL are small and statistically indistinguishable from zero. The difference in the average treatment effects of the two schemes is both statistically and economically significant. The contrast between treatment impacts on production and profits is explained partly by the statistically significant 6 percent decline in the the unit cost of production for TRAIL beneficiaries. In the GRAIL scheme we do not detect such an effect; the difference in the TRAIL and GRAIL average treatment effects on input cost per acre is statistically significant ($p-value = 0.02$).

Since the agent's formal role was to recommend borrowers, one might expect that the different outcomes are explained by differences in borrower selection patterns. In both schemes, the agents were more likely to recommend poor borrowers, although the correlation with observable indicators of poverty varied: TRAIL agents selected households who owned less land, whereas GRAIL agents selected households with less-educated heads. However, TRAIL agents were more likely to select farmers that had borrowed from them in the past, while the GRAIL agent tended to recommend borrowers of the same religion as himself, and those who expressed support for his political party.

Despite being selected by entirely different methods, we find that selected but untreated farmers in the two treatment arms had very similar farm outcomes: borrowing, potato area planted, harvest quantity, revenues and unit costs did not differ significantly between TRAIL and GRAIL. We then examine whether there were any significant differences in underlying productivity among selected farmers. We estimate productivity using a standard semi-structural approach, based on a model with a decreasing returns Cobb-Douglas technology, and undistorted factor markets. Our estimates suggest that on average, TRAIL agents selected more productive farmers. However, when we decompose the difference in the average treatment effects of the two schemes on potato profits and farm profits, productivity differences account for at most 14% of the difference in average treatment effects.

We examine whether this finding is robust to alternative specifications of the selection model. We extend the model to allow for credit rationing, technical and pecuniary scale economies and farmer heterogeneity in productivity, unit costs and credit limits. We find that farmers selected by the TRAIL agents had greater pecuniary scale economies while those selected by the GRAIL agents had greater technical scale economies. This model predicts that the ATEs in the GRAIL scheme would be larger than that in the TRAIL scheme, a result that is, however, not supported by the observed patterns and estimated ATEs.

These results indicate the difficulty of explaining our finding solely on the basis of differences

[4]These percentage effects are mirrored in the absolute effects on borrower profits and aggregate farm profits, since control farmers in both schemes had similar profits and incomes.

in borrower selection patterns. In the remainder of the paper, we consider an alternative explanation where the TRAIL and GRAIL agents, in addition to selecting beneficiaries also monitor or help farmers in the village. In our model, the credit intervention schemes change the agents' incentives to provide these services.[5] TRAIL agents are also middlemen in the potato trade and so their profits depend on the volumes they buy from farmers. By advising farmers on ways to lower unit costs of production, they can help farmers produce more, resulting in larger trade volumes [Mitra et al., 2018]. GRAIL agents are possibly motivated to further the objectives of the political party in power in the local government. As long as the schemes' beneficiaries are solvent, GRAIL agents do not earn any additional benefit from their farm success. However, politicians suffer a loss of reputation if the farmers they recommended for the loans are forced to default. GRAIL agents, therefore, intensively monitor low-ability borrowers (who are at the highest risk of defaulting), inducing them to undertake costly actions and expenditures that lower crop risk, but do not raise productivity. Moreover, GRAIL agents, not being traders, lack the business expertise of TRAIL agents, so cannot help borrowers lower their costs. We show that our model's predictions match empirical patterns in default rates and conditional treatment effects on borrowers' farm output and incomes, as well as the frequency of their interactions with the agent and local traders.

These results suggest that even when the intermediaries' formal task is limited to selecting beneficiaries, their subsequent informal engagement with the beneficiaries can have important consequences for program outcomes. More broadly, this paper highlights the importance of considering the context in which delegated agents operate. Programs need to consider both their explicit incentives as well as their implicit personal and professional motivations. We show that where these align, private agents can be successfully engaged to deliver credit programs that increase farm incomes and also have high repayment rates.[6]

The paper is organized as follows. We describe the two loan intervention schemes in Section 2, and the data used in this paper in Section 3. Section 4 presents the estimates of the average treatment effects of the two schemes on borrower outcomes, while Section 5 provides evidence on their financial performance. Section 6 evaluates the extent to which these results can be explained by two alternative selection-based mechanisms. Section 7 provides our proposed explanation where the schemes changed the way that agents interacted with borrowers, and presents empirical evidence consistent with this mechanism. Section 8 concludes.

[5] The idea that agents' influence can extend beyond selection echoes Heath [2018]'s findings in a setting of worker referrals.

[6] Following recommendations by experts appointed by the Reserve Bank of India, there has been a move to engage private "business correspondents" to deliver banking services in rural areas [Kishore, 2012, RBI, 2011, 2013]. However the literature provides little guidance on how to select or incentivize these correspondents.

2 Context and Intervention Design

Potatoes generate the highest return of any agricultural crop in the districts of Hugli and West Medinipur in West Bengal [see Maitra et al., 2017, Table 2]. However, large cultivation costs constrain farmers' ability to plant potatoes. The agent-intermediated lending interventions were designed to provide farmers with the means to invest in the working capital needed to plant this crop.

The experiment was carried out in 48 randomly selected villages, each at least 8 kilometres apart from the others, and belonging to the jurisdiction of different local government councils (gram panchayats, or GPs).[7] Panel A of Table 1 presents descriptive statistics for the sample villages.[8] As column 3 shows, there are no significant differences in village size, number of potato cultivators in the village or the number of potato cultivators in the different landholding categories across the two treatment arms.

The credit scheme was implemented by Shree Sanchari, a microfinance institution headquartered in Kolkata. In each of the 24 villages in the TRAIL arm, our field team drew up a list of local traders who had at least 50 clients, or had been operating in the village for longer than 3 years. One name was randomly drawn from this list, and the individual was offered the contract to become the local agent for the scheme. In all cases, the first trader we approached accepted the contract. In the GRAIL arm, the field team requested the gram panchayat to recommend for the agent's position persons who had lived in the village for at least 3 years, were personally familiar with farmers in the village and had a good local reputation. One randomly drawn person from this list was offered the position of the GRAIL agent.[9]

In each village, the agent was asked to recommend as potential borrowers 30 residents who owned no more than 1.5 acres of land. The field team conducted a lottery in the office of the local government to draw the names of 10 individuals from this list.[10] Selected individuals were then offered the loans. In what follows we refer to these households as Treated households.

The first loans, of Rs. 2000, were disbursed during the potato planting season in October–

[7]Each Gram Panchayat has 8–15 representatives directly elected every five years from a group of villages. In West Bengal village council elections, candidates typically declare an affiliation with a state-level political party. West Bengal has a long history of cadre-based mobilization of voters through political rallies and campaigns. Local political party workers are often instrumental in identifying beneficiaries for government programs and delivering benefits.

[8]Panel A of Table 1 uses data from a 2007 house listing exercise that we carried out in 46 of these 48 villages for a different project [see Mitra et al., 2018], well before the credit interventions began. In 2010, political violence prevented us from working in 2 villages from the 2007 sample; we do not have house listing data for the 2 replacement villages.

[9]One individual refused to participate for religious reasons; he was replaced by a second randomly chosen individual from the list.

[10]Our field team kept the list of recommended individuals confidential, so as to avoid any spillover effects on informal credit access or other relationships for recommended households that were not randomly assigned to receive the loan.

November 2010. Borrowers were liable only for repaying their own loans, in a single lump-sum with 6 percent interest that was due four months after disbursal. Conditional on successful repayment, loans became progressively larger by 33% in each subsequent cycle, so that in cycle 8 the maximum loan size would have been Rs. 8300. Only borrowers who repaid at least 50% of principal due were allowed to borrow again in the next cycle. To avoid pressuring borrowers to sell their harvest prematurely to repay their loan, both schemes allowed farmers to repay in the form of potato "bonds". In this case the repaid amount was calculated at the prevailing price of the bonds.[11]

At the beginning of cycle 1, the agent put down a deposit of Rs. 50 per borrower, which was returned to him if his borrower survived in the program for two years. At the end of each loan cycle, the agent received a commission equal to 75% of the interest paid by all borrowers whom he had recommended and who had been randomly selected to receive the loan. This high commission rate was meant to incentivize the agent to select productive borrowers who would repay the loan, and to discourage collusion with potential borrowers. If more than one-half of the recommended borrowers defaulted on their loans, the agent would have been terminated and he would have not earned any further commissions. At the end of two years, all surviving agents received a refund of their deposit as well as a paid holiday to a seaside resort. These formal incentives were likely supplemented by informal motivations: in conversations during our field visits, some TRAIL agents remarked that they expected the scheme to increase their prominence in the village and boost their business. On the other hand, GRAIL agents may have viewed the scheme as an extension of government anti-poverty programs, or as a means to increase the popularity of the incumbent political party.

There was very little microfinance available in this area in 2010, and our MFI partner had not operated in any of these villages before.[12] In our interventions, they only conducted loan transactions with treated households; they did not screen borrowers in any way, check how they used the loans, or monitor them.[13] The loans were funded by an external grant held by the principal investigators of this project.

3 Data and Descriptive Statistics

Every four months during 2010–2103, we conducted detailed crop and credit surveys with 50 households in each of the 48 sample villages. In each village, all 10 Treated households were included in our sample. In addition, of the 20 households that the agent had recommended

[11]Farmers can store their harvested crop in cold storages for a maximum of 11 months. Potato "bonds" are receipts from the cold store facility that are traded between farmers and traders.

[12]Table A1 in the Appendix, where we present some descriptive statistics on the baseline (pre-intervention) credit market characteristics in these villages shows that only 3% of all loans and 2% of agricultural loans were from MFIs.

[13]A loan module in our household survey questionnaire allowed us to collect detailed information about each households' borrowing and loan utilization.

but did not receive the loan, we surveyed a random subset of 10 households. We refer to these as Control 1 households. We also included 30 additional households randomly chosen from those the agent did not recommended. We call these the Control 2 households. The same person in each household answered the survey in each round. There was no attrition in the sample over the eight survey cycles.

3.1 Pre-intervention Differences in Observable Characteristics

Panel B of Table 1 presents summary statistics for selected household-level characteristics for the complete sample. We see in column 1 that the average household in our sample owned 0.45 acres of land. Most households were occupied in agricultural cultivation or labour. In the average household, the oldest male member was about 48 years old and had not studied beyond primary school. Columns 3 and 4 show differences between Treated and Control 1 households within each treatment arm: these are never statistically significant. The F-statistics at the bottom of the table indicate that these household characteristics do not jointly explain whether a recommended household was assigned to receive the AIL loan in either TRAIL or GRAIL villages.[14]

3.2 Agent Characteristics in TRAIL and GRAIL

Table 2 presents data about the TRAIL and GRAIL agents from a short questionnaire we fielded with the agents at the time they were recruited. Nearly all agents were male. Predictably, the agents in the two scheme differ in their professions. Most TRAIL agents reported that they were business persons, but GRAIL agents' professions varied: 38% were farmers, 29% were business persons, 12% held government jobs and about 20% reported other occupations. Compared to the average sample household, agents in both schemes held more land and had greater education. TRAIL agents were wealthier and reported larger weekly incomes but had less formal education than GRAIL agents. GRAIL agents were significantly more involved in civil society and politics: 30% were members of a village organization, 17% were political party workers, and 13% had been members of the local government. None of the TRAIL agents were directly involved in politics in this way.

[14] Table A1 shows that two-thirds of the households had borrowed in the month preceeding the first survey and 59% of households had borrowed for agricultural purposes. The majority of borrowings were from traders and money lenders and there is very little MFI presence in these villages. There is a wide variation in interest rates: from 1% a month on bank loans to 2% a month on informal loans and 3% a month on microfinance loans. While loans from banks and cooperatives involve collateral, loans from the other sources rarely do. The average duration of loans from traders and money lenders is approximately 4 months, matching crop cycles.

3.3 Pre-Intervention Engagement with the Agent

Table 3 provides additional evidence about the connections between the agents and the residents in their villages. In the first round of household surveys conducted in December 2010, we asked if the household respondent personally knew the agent, how often they met them, and if they had interacted in land, labour, credit or farm output markets, or on social occasions. We also use data from the household and agent surveys to identify whether the agent and sample households belonged to the same occupation or social group.[15]

The agents were well-known in their villages: both in TRAIL and GRAIL villages, more than 90 percent of sample households reported knowing the agent, and nearly all of these farmers said they met the agent at least once a week.[16] As we saw before, the TRAIL agent was almost always a trader, and so his occupation did not overlap with sample households who were usually cultivators or labourers. However GRAIL agents were more likely to be cultivators themselves. Nearly all sample households belonged to the same religion as the GRAIL agent, and nearly two-thirds reported the same caste category.

However TRAIL agents had stronger economic links: in the TRAIL villages 11–20% of households reported that the agent was an important source of credit, inputs or employment, or an important trader of their produce. In line with this, one-third of the sample households had purchased inputs from the agent, and 15% had borrowed from him in the three years prior to the start of our study. GRAIL agents were significantly less likely to have engaged with sample households in this way.

We infer that although both agent types knew village residents well, the nature of their interactions differed: GRAIL agents interacted more in the social and political spheres, whereas TRAIL agents predominantly had business relationships with residents.

4 Estimating Treatment and Selection Effects

We aggregate the data from multiple survey rounds into a household-year level dataset spanning three years from 2010–11 to 2012–2013. This contains information about our sample farmers' annual borrowing for agricultural and non-agricultural purposes, acreage planted with potatoes, potato output, sales, revenues, production costs, value-added and imputed profits.[17] We also have information on non-farm incomes coming from wage em-

[15]Political connections between the agent and sample households can only be inferred in GRAIL villages. In 2013, households participated in a straw poll and indicated their preferred political party. If they supported the same party that was in majority control of the gram panchayat in 2010, then we infer that they support the same party that the GRAIL agent belonged to.

[16]The estimation sample consists of all sample households that owned at most 1.5 acres of land. We report weighted means, where each Treatment and Control 1 household is assigned a weight of $\frac{30}{N}$ and each Control 2 household is assigned a weight of $\frac{N-30}{N}$. N is the total number of households in the village.

[17]Farmers often store some of their potato harvest and sell it at different points in the year. We track the harvested potatoes over multiple survey rounds in order to calculate the sales revenues and align them

ployment and non-farm businesses.

To examine the impact of the schemes on household outcomes, we run OLS regressions according to the following specification:

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

Here y_{ivt} denotes the outcome variable of interest for household i in village v at time t. The dummy variables TRAIL_v and GRAIL_v take value 1 if the household belongs to a TRAIL or GRAIL village respectively, and similarly the variables Treatment_{iv} and $\text{Control } 1_{iv}$ are self-explanatory. The omitted category is the Control 2 households in GRAIL villages.

Since only a random subset of the recommended household were offered the loans, the difference in the outcomes of the Treated and Control 1 households is an estimate of the average treatment effect of the loan, conditional on being recommended to participate in the scheme. Accordingly, the conditional average treatment effect of the TRAIL scheme is estimated as $\hat{\beta}_3 - \hat{\beta}_2$ and of the GRAIL scheme is estimated as $\hat{\beta}_5 - \hat{\beta}_4$. All households that were randomly drawn to receive the loan are assigned a value of 1 for the Treatment dummy variable, therefore these are intent-to-treat estimates.

Neither Control 1 nor Control 2 households received the AIL loans, although Control 1 households were recommended by the agents. Since only 10 households in each village received the loans, it is unlikely that the schemes generated general equilibrium effects, therefore the difference in the outcomes of the Control 1 and Control 2 households tells us how the recommended households differ from the non-recommended. Specifically, $\hat{\beta}_2$ estimates the TRAIL selection effect and $\hat{\beta}_4$ estimates the GRAIL selection effect. $\mathbf{X}_{ivt}$ contains measures of the household's landholding, religion and caste, and the age, education and occupation of the oldest male in the household. $I(\text{Year}_t)$ denotes two year dummies. Standard errors are clustered at the village level.

4.1 Treatment Effects on Agricultural Borrowing and Informal Interest Rates

As we see in column 1 of Table 4, Treatment households in both schemes significantly increased their borrowing: TRAIL Treatment households borrowed Rs. 2873 (or 55%) more, and GRAIL Treatment households borrowed a very similar Rs. 2754 (or 64%) more than Control 1 households. The point estimates on non-program agricultural borrowing are small and statistically insignificant (column 2), indicating that program loans did not crowd out agricultural loans from other sources. We conjecture this was because farmers

with the costs of production, transport and sales.

were reluctant to disrupt their traditional informal credit relationships in response to a new intervention.

4.2 Treatment Effects on Potato Cultivation

Table 5 shows that in both schemes, the increased borrowing translated into large and statistically significant increases in the cultivation of potatoes. In both TRAIL and GRAIL villages, the effect is concentrated on the intensive margin. TRAIL treatment households planted 0.09 additional acres with potatoes (27%, column 1) and harvested 946 kilograms more (26%, column 2). GRAIL treatment households,planted an additional 0.07 acres (23%, column 1) and their potato output increased by 772 kg (24%, column 2).

In column 5 we see that the average TRAIL Treatment household earned an additional Rs. 3900 (27%) in potato sales revenue. Their total cost of production increased by Rs. 1845 (or 22%, column 4), so that on net, value-added increased by Rs. 2060 (or 36%, column 6). When we subtract the imputed cost of family labor employed in potato farming, this works out to a statistically significant Rs. 1907 or 40% increase in profit (column 7).[18]

Sales revenues also increased for the average GRAIL Treatment household, although the point estimate is smaller at Rs. 2504 (19%).[19] Their cost of production increased by 28 percent, so that there was no change in their value-added or imputed profits (Rs. 494 and Rs. 91, respectively).[20]

Thus, although both schemes helped beneficiaries to increase potato acreage and output, profits only increased in the TRAIL scheme. We can reject the null hypothesis that the average treatment effects on imputed profits were identical in the two schemes ($p-value =$ 0.052, column 7). This difference in outcomes is driven by different effects on unit costs of production: the input cost per acre of cultivation decreased by 6% for TRAIL Treatment households, whereas the point estimate for GRAIL Treatment households was an increase of 1.2%. The difference is statistically significant ($p - value = 0.0186$).

In Figure 1 we examine whether the average treatment effects varied over the three years of the program. We find that in both schemes, in all three years, the average treatment effects on potato acreage and output were positive and statistically significant. However, profits increased in the TRAIL scheme each year, while the effect was never statistically significantly different from zero in the GRAIL scheme. Similarly, the treatment effects on input costs per acre were negative and statistically significant in the TRAIL scheme in all

[18] To calculate the shadow cost of family labour, we price the family labor time for male, female and child labor spent on the crop at the median wage for hired labor of that type in that year, crop and village.

[19] TRAIL borrowers also experienced a smaller decline in output price than GRAIL borrowers did (0.6% instead of 3.6%), although this difference is not statistically significant ($p - value = 0.37$). We collected quantity and price data for each potato sale by sample households. If farmers held potatoes for self-consumption, we impute the sales revenue by pricing that quantity at the median sale price in the village.

[20] We find qualitatively similar results when we run equation (1) without controlling for $\mathbf{X_{ivt}}$. These results are available on request.

three years, but never in the GRAIL scheme.

4.3 Treatment Effects on Farm Incomes

In Table 6 column 1, we estimate average treatment effects on farm income, aggregating the profits from the four major crops grown in this area (potato, sesame, paddy and vegetables).[21] The results indicate that farm profits of TRAIL treatment households increased by a statistically significant 28%, whereas the point estimate for the GRAIL scheme is a non-significant 3.8%. Note also that in each scheme, the treatment effects on potato profits account for a large fraction of the impact on farm income (79% for the TRAIL scheme and 65% for the GRAIL).

Column 2 presents effects on non-agricultural income, calculated as the sum of rental, sales, labour and business income. Possibly due to large measurement error, the point estimates are very imprecise and so we have no evidence that non-agricultural incomes increased as a result of either scheme. Finally, column 3 shows that TRAIL households' aggregate income increased by 9.1%, whereas it decreased by 9% for GRAIL treatment households; this difference between TRAIL and GRAIL households is statistically significant at the 10% level.[22]

5 Loan Performance

In Table 7 we examine loan take-up and repayment rates. In column 1, the dependent variable is the likelihood that a household eligible to borrow in any given cycle actually took the loan. As discussed, initial eligibility to borrow was determined through a random draw from the pool of recommended borrowers; in subsequent cycles the household remained eligible to borrow if it had repaid at least 50% of its previous loan. The sample means in Panel A show that TRAIL Treated households borrowed in 94% of the instances when they were eligible. GRAIL Treated households borrowed in a significantly lower 87% of instances (column 1). This difference remains statistically significant even in Panel B when we control for cycle fixed effects and observable borrower characteristics using the regression:

$$y_{hvt} = \alpha_0 + \alpha_1 \text{GRAIL}_v + \gamma \mathbf{X}_t + \varepsilon_{hvt} \tag{2}$$

We define a loan to be in default if any part of the repayment amount remained unpaid on the due date. In both schemes, on average 7% of loans were in default per cycle (Panel A,

[21] The average treatment effects for sesame, paddy and vegetables are available upon request.

[22] Figure 1 also presents the year-specific average treatment effects on aggregate farm profits. The treatment effects are positive and statistically significant in the TRAIL scheme in all three years. The effects in the GRAIL scheme are never statistically significant.

column 2). The regression result in Panel B confirms that there is no difference in default rates in the two schemes.

6 Selection-based Explanations

The empirical results above make it clear that the TRAIL scheme was significantly more successful than the GRAIL scheme at raising farm incomes for its borrowers. In what follows, we examine the reasons for this difference in impacts. Recall that the borrowers in each scheme were recommended by the agents, who themselves belonged to different professions, and interacted with village residents in different spheres. It is plausible that in line with this they chose different borrower types. We now explore whether the preceding results can be explained by different patterns of borrower selection in the two schemes. We start by examining whether network linkages with farmers help predict recommendation patterns by TRAIL and GRAIL agents. Specifically, we run two separate regressions for the TRAIL and GRAIL schemes, with the following specification:

$$R_{iv} = \xi_0 + \xi_1 L_{iv} + \xi_2 Z_{iv} + \epsilon_{iv} \tag{3}$$

The estimation sample consists of Control 1 and Control 2 households. The dependent variable R_{iv} takes the value 1 if household i in village v is recommended by the agent but not selected to receive a loan, and 0 if it is not recommended. The explanatory variables include economic, social and political links between household i in village v and the AIL agent in that village (L_{iv}) and a set of household characteristics (Z_{iv}).

As Table 8 shows, the determinants of selection likelihood vary with network connections between the households and the agent in their village. TRAIL agents were significantly more likely to recommend households who had borrowed from them in the past. In contrast, GRAIL agents were significantly more likely to recommend households that belonged to their religion or caste, or supported their political party.[23]

However, it is not clear how the different *processes* of selection followed by the two agents translated into differences in farm *outcomes* or underlying relevant characteristics such as productivity. We start by comparing farm outcomes for Control 1 households in Table 9. Recall that Control 1 households were recommended by the agents but not selected to participate in the intervention. Since their outcomes are unaffected by the treatment, any differences will indicate the effect of differential selection. We run the following regression on Control 1 households:

$$y_{ivt} = \eta_0 + \eta_1 \text{TRAIL}_v + \lambda \mathbf{X}_{ivt} + I(\text{Year}_t) + \varepsilon_{ivt} \tag{4}$$

where y_{ivt} denotes the outcome variable of interest for household i in village v at time t.

[23] As we see in columns (1) and (3) in Table 8, these results hold irrespective of whether or not we control for household characteristics and wealth.

The dummy variable TRAIL_v takes value 1 if the household resides in a TRAIL village, and 0 if it is from a GRAIL village.

Column 1 Table 9 presents the mean value of the variable of interest for Control 1 households in GRAIL villages, and column 2 presents the coefficient on the TRAIL dummy from equation (4). As the percentage effects in column 3 imply, Control 1 households in TRAIL villages devoted 13% greater area to potato cultivation and produced nearly 12% more potatoes. On the other hand, their unit costs per acre were slightly higher (by 2.5%), while the price at which they sold was slightly lower (by 3.1%), so that they earned a slightly lower value added (1.5%) and profit (4.3%). However their aggregate farm profit was larger by 14%.

Overall, there is no difference in farm outcomes of the households selected by the two types of agents in the absence of the intervention.

6.1 Selection on Productivity Alone

However, simple comparisons of farm outcomes may not be sufficiently precise in what they reveal about the underlying productivity of the chosen farmers. In this section we therefore turn to estimating their productivity using a semi-structural approach which is a simplified version of standard methods of productivity estimation in the IO literature [Olley and Pakes, 1996, Levinsohn and Petrin, 2003, Ackerberg et al., 2015] using the three year panel data we have for farmers in our sample.[24]

We make the following simplifying assumptions, which we will relax in the later sections. Farmers are heterogenous in the single dimension of ability or total factor productivity (TFP). There is a single crop (potato) produced by a single variable scale input (area cultivated); implicitly this is tantamount to assuming different inputs are required in fixed proportions to area cultivated. Output is described by a Cobb-Douglas function with decreasing returns to scale. This preliminary model abstracts from any price or production risk; the extended version will incorporate these. Factor markets are perfect: all farmers face the same production cost per unit area cultivated. Hence the model abstracts from frictions in the informal credit market, or farm input markets. In particular, this implies the absence of any credit rationing in the informal market.

A farmer that is treated will gain access to a supplementary line of credit at an interest rate below the prevailing rate in the informal market, which will therefore induce an expansion in area cultivated, output and profits. By construction, this simple version will be unable to explain any differential treatment effects on unit production costs, since these will fall by the same amount for both TRAIL and GRAIL treated farmers. Nevertheless, as a first cut, this model is still useful to estimate farmer productivity, the extent to which it differed between farmers recommended by the respective agents, and whether this can account for

[24] We simplify their approach by assuming farmer productivity is fixed over time, rather than following a Markov process.

a significant portion of the observed differences in treatment effects on potato profits.

6.2 Estimation of Farm Productivity: Control Households

We start with farmers in the control group. Farmer i in village v in year t earns revenues given by the production function:

$$R_{ivt} = p_{vt} a_i [\frac{1}{1-\alpha} l_{ivt}^{1-\alpha}] \tag{5}$$

where p_{vt} denotes yield or price varying at the village-year level known to (or expected by) the farmer at the time of planting, l_{ivt} is the farmer's chosen scale of cultivation, and $\alpha \in (0,1)$. Farmer ability or TFP A_i is exogenous and follows a common distribution in GRAIL and TRAIL villages. The cost of production per unit area c is constant and identical across farmers.

A control group farmer in village v, year t borrows from informal lenders who all have a common cost of capital ρ_{vt}. The lenders compete in Bertrand fashion, so each farmer pays interest cost ρ_{vt}, thus ending up with an (interest-inclusive) unit cultivation cost of $c\rho_{vt}$. To cultivate potatoes, the farmer must incur a fixed cost $F > 0$. Accordingly, he chooses $l = l_{ivt}^c$ to maximize

$$p_{vt} a_i \frac{l^{1-\alpha}}{1-\alpha} - \rho_{vt} c l - F \mathcal{I}_{l>0}$$

where $\mathcal{I}_{l>0}$ denotes an indicator function taking the value 1 if $l > 0$ and 0 if $l = 0$.

For control farmers with ability high enough, selecting a positive scale is optimal, and they choose area:

$$\log l_{ivt}^c = \frac{1}{\alpha} \log \frac{a_i}{c} + \frac{1}{\alpha} [p_{vt} - \rho_{vt}] \tag{6}$$

Observe that $\frac{1}{\alpha} \log \frac{a_i}{c}$ is increasing in farmer ability. This serves as an alternative measure of productivity. Accordingly, among control farmers, this productivity measure can be estimated as the household fixed effect in a household-year level panel regression, where the (log) scale of potato cultivation (acreage or output) is regressed on farmer, village and year dummies.

Farmers with ability below some threshold $\underline{\theta}_{vt}$ would choose not to cultivate potatoes. Our data show that roughly 30 percent of Control 1 and Control 2 group farmers planted potatoes in at most one of the three years in our study period; we cannot estimate household fixed effects for these households. To these "non-cultivator" households, we assign the lower endpoint of the estimated productivity distribution among the cultivators. This is an upper bound to their true latent productivity. None of the comparisons below are affected if we replace this upper bound with any lower estimate. Throughout we will use our estimate of productivity as a proxy for underlying unobserved ability.

In Table 10 we present the results from regressing each cultivator household's productivity estimate, on observable household characteristics. As the bottom panel of Table 10 shows, there is wide variation in the estimated productivity level across households: the 75^{th} percentile is more than three times the size of the 25^{th} percentile. We see that the productivity estimate correlates positively with landholding and having a male household head. However, the R-squared indicates that variation in observable characteristics can only explain 15% of the variation in household productivity.[25] This indicates that observable characteristics are only incomplete predictors of farmer productivity. This underlines one of the principal rationales for hiring community-level agents who may have additional information about farmer productivity that is not easily observable to MFIs.

6.3 Differences in Farming Ability Selection

We can now examine whether the TRAIL and GRAIL agents systematically recommended households of different productivity levels. We focus on Control 1 and Control 2 households; since neither group received program loans.

In Panel A of Figure 2, we compare the distributions of the productivity estimates for Control 1 and Control 2 households.[26] The figure on the left shows that in TRAIL villages, the cumulative distribution function for Control 1 households first-order stochastically dominates that for the Control 2 households. A two-sample Kolmogorov-Smirnov test rejects the null hypothesis that the two distributions are identical ($p-value = 0.005$). Thus, the TRAIL agent positively selected borrowers on productivity. The figure on the right shows that the GRAIL agent also selected borrowers positively (K-S test $p-value = 0.011$).[27]

In Panel B, we compare the selection patterns in the TRAIL and GRAIL schemes by plotting the cumulative distribution functions for Control 1 households. The distribution for TRAIL households first-order stochastically dominates that for GRAIL households (K-S test $p-value = 0.06$).[28] Thus, the TRAIL agent selected more productive borrowers than the GRAIL agent.

For what follows, it is convenient to group sample households into productivity classes, or bins. Accordingly, we place all non-cultivator households in Bin 1. Among the rest, we use a median split to create Bins 2 and 3. The GRAIL agent selected more Bin 1 borrowers (34.5% versus 27.3%), and fewer Bin 3 borrowers (32.3% versus 39.7%). See columns 1 and

[25] A LASSO estimator performs only slightly better than the ordinary least squares estimator. Under the Extended Bayesian Information Criterion the selected LASSO model has an R-squared of 0.23.

[26] The flat segment in the bottom end of the plotted CDFs depicts the upper bound of the estimates for non-cultivators.

[27] Since our productivity estimates are generated variables, we also simulate 2000 bootstrap samples and run the K-S test for each Control 1 vs Control 2 CDF comparison. In the case of TRAIL, In 87 percent of the simulations, we can reject the null hypothesis that the two distributions are identical. In the case of GRAIL, the K-S test rejects the null hypothesis that the two distributions are identical in 83 percent of our bootstrap simulations.

[28] The Kolmogorov-Smirnov test rejects the null hypothesis that the two distributions are identical in 74 percent of our 2000 bootstrap simulations.

2 of Table 13.

6.4 Explaining Farmer Ability Selection Differences

What explains this difference in selection patterns? Through his past experience lending to and trading with village residents, the TRAIL agent might have acquired better information about their productivity levels. Alternatively, he might have been more strongly motivated to recommend higher ability farmers, as they may be expected to expand cultivation and output by more, thereby generating higher crop sales and middleman profits. In contrast, the GRAIL agent would likely share the motives of the political party controlling the village council. This might lead him to select asset-poor (and possibly) low-ability farmers, either for ideological reasons, in line with the political party's pro-poor welfarist platform, or for opportunistic reasons to mobilize their votes.[29] These arguments are elaborated further and incorporated in the extended model in Section 7.1 below.

Evidence frm our survey is consistent with GRAIL agents being guided by political motivations. In the final survey round conducted in 2013, we asked respondents to participate in a straw poll. Survey investigators gave them a sheet of paper resembling a ballot, and asked them to mark the symbol of their preferred political party, and then fold and place the paper in a box.[30] If we believe that the straw poll vote indicates the political party that the respondent supports, then we can interpret a straw vote for the political party that was incumbent in 2010 as support for the political party of the GRAIL agent. We use data from the West Bengal State Election Commission to identify the incumbent political party in each local village council in 2010. We run the regression:

$$\text{Vote Incumbent}_{iv} = \xi_0 + \xi_1 \text{Treatment }_{iv} + \xi_2 \text{Control 1}_{iv} + \gamma \mathbf{X}_{iv} + \varepsilon_{iv} \tag{7}$$

where the dependent variable Vote Incumbent_{iv} takes the value 1 if the respondent i in village v voted for the incumbent party in the straw poll. The set of controls $\mathbf{X}_{iv}$ includes the same household characteristics included in equation (1). We run the regression separately for TRAIL and GRAIL villages. In Table 11 we see that the estimated treatment effect $\hat{\xi}_1 - \hat{\xi}_2$ is positive and statistically significant in GRAIL villages (column 2), but insignificant in TRAIL villages (column 1).

Hence in GRAIL villages, those who were randomly assigned to receive the program loan were 8% more likely to express support for the incumbent party than households that were also recommended but randomly not selected to receive the loan, suggesting that the loans may have generated support for the incumbent party. Column 4 shows that the point estimate of the treatment effect is largest for the GRAIL households in the lowest

[29] It is a standard feature of the literature on political clientelism that political parties target swing voters among the poor. This is because poor voters sell their vote for a lower price [see, for example, Stokes, 2005, Kitschelt and Wilkinson, 2007].

[30] Respondents were assured the response would be kept confidential and used only for research purposes. Less than 1% of households refused to participate.

productivity bin. This is consistent with opportunistic selection by the GRAIL agent, since clientelistic transfers are most likely to mobilize votes among the poor. It is also consistent with the idea that poor beneficiaries might reciprocate the most for the receipt of benefits.

6.5 The Role of Farmer Ability Selection in Explaining ATE Differences

We have seen that the TRAIL agent selected borrowers who were more productive than the GRAIL agent did. Next, we ask whether this can explain the larger impacts on borrower outcomes. Suppose that farmers of a given productivity level convert the AIL loan into the same increase in profit regardless of which scheme they are in. Since more able farmers would be expected to increase their profits by more, a scheme that had more productive borrowers should generate larger increases in farm profits. However, it is also possible that the scheme itself affects a given borrower's productivity. To separate out these effects, we examine how much of the differential impact of the TRAIL scheme on profits can be explained by differences in borrower productivity levels, and how much by differential effects *conditional* on productivity. Specifically, we decompose the difference in the average treatment effects of the TRAIL and GRAIL schemes into the components that can be attributed to these different factors.

In order to do this we need to obtain estimates of ability of treated households. The same method for estimating ability for control households cannot be applied to treated households, since access to the program loans would likely cause farmers to increase their potato acreage. Hence acreage planted by treated farmers would be increasing in treatment status as well as their underlying productivity, rather than productivity alone. We can recover their productivity estimate under an *Order-Preserving Assumption* (OPA), which assumes that the treatment impact on area cultivated is monotonic in farmer productivity, so the treatment would not change the relative productivity ranking of treated households.[31] A theoretical justification for this assumption is provided in Maitra et al. [2017], as well as in Section 7.1. Combining this assumption with the assumption that the productivity follows the same distribution for the treated and Control 1 farmers within any treatment arm (because recommended farmers were randomly assigned to treatment), we can rank treated farmers within any treatment arm by cultivation scale, and assign to them the counterfactual productivity estimate of the farmer at the same rank within the Control 1 distribution.

Next, we can estimate treatment effects conditional on each ability level: the difference in farm outcomes between a treated farmer and corresponding Control 1 farmer of the same ability. While these can be estimated for each ability level, for simplicity we form three ability groups or bins and estimate ability-bin-specific heterogenous treatment effects

[31] Athey and Imbens [2006] use a similar assumption in order to identify treatment effects in non-linear difference-of-difference settings.

(HTEs). We use the following regression specification:

$$
\begin{aligned}
y_{ivt} = & \sum_{k=1}^{3} \xi_{1k} \, \widehat{\text{Bin}}_{ik} + \sum_{i=1}^{3} \xi_{2k} \, (\text{Control 1}_{iv} \times \widehat{\text{Bin}}_{ik}) + \sum_{k=1}^{3} \xi_{3k} \, (\text{Treatment}_{iv} \times \widehat{\text{Bin}}_{ik}) \\
& + \sum_{k=1}^{3} \xi_{4k} \, \widehat{\text{Bin}}_{ik} \times \text{GRAIL}_v + \sum_{k=1}^{3} \xi_{5k} \, (\text{Control 1}_{iv} \times \widehat{\text{Bin}}_{ik} \times \text{GRAIL}_v) \qquad (8) \\
& + \sum_{k=1}^{3} \xi_{6k} \, (\text{Treatment}_{iv} \times \widehat{\text{Bin}}_{ik} \times \text{GRAIL}_v) + \gamma \mathbf{X'}_{ivt} + \varepsilon_{ivt}
\end{aligned}
$$

The estimated HTEs are presented in Table 12. Since household productivity is an estimated regressor, we present standard errors bootstrapped with 2000 iterations.[32] As might be expected, households of greater productivity do indeed earn more profit. Moreover, in both schemes the HTEs on profit from profit cultivation and aggregate farm profit (columns 7 and 9 respectively) are larger for borrowers of higher productivity. Hence a scheme that selects more able borrowers on average will tend to have a larger average treatment effect, even if they have the same HTEs.

Within any given productivity bin, the treatment effects are larger in the TRAIL scheme. However, the estimated differences, presented in Panel C, are not statistically significant.[33] Hence the larger ATEs in TRAIL can only partly be explained by the greater ability of the TRAIL borrowers (the **Selection effect**); the rest is due to larger HTEs in TRAIL (the **Conditional Treatment effect**, or CTE).

To estimate the magnitude of the Selection effect we calculate how much lower the TRAIL ATE would have been if the selected farmers in the TRAIL scheme had the same ability composition as in the GRAIL scheme, but the HTEs for each ability bin were as estimated for TRAIL. The CTE is estimated by measuring how much lower the average treatment effect of the TRAIL scheme would have been if the GRAIL agents had kept their own selection patterns but their selected borrowers experienced the same HTEs as their TRAIL counterparts.

Panel A of Table 13 shows this decomposition for the treatment effects on profit from potato cultivation. In Panel B, we decompose the difference in average treatment effects on aggregate farm profit. Columns 2 and 3 show the proportions selected in the three

[32] For households that did not cultivate potatoes in any study year, we replace the value of potato area cultivated, output produced or profits earned with zero, thus we continue to include these households in the estimating sample. However the treatment effects on unit costs are only estimated in the subset of observations where potatoes were cultivated.

[33] Here we are abstracting from any heterogeneity of treatment effects within productivity bins. Our findings are similar when we consider continuous productivity levels instead. In Figure A1, the vertical difference in (smoothed) potato profit (Panel A) and aggregate farm profit (Panel B) between Treatment and Control 1 households gives us a visual estimate of the treatment effect at that productivity level. As the figures in both Panels A and B show, the vertical difference for TRAIL households is larger at larger productivity levels. The GRAIL treatment effects are nearly always zero.

ability bins in TRAIL and GRAIL respectively, and column 3 the difference. HTEs are shown in columns 4 and 5, and contributions of each bin to the Selection effect and CTE in columns 7 and 8. As we see in the penultimate row, the Selection effect explains 13.7% of the difference in ATEs on potato profits (Panel A), and 14.1% of the difference in ATEs on farm profits (Panel B). By contrast, the CTE accounts for 47% of the difference in potato profit ATEs (Panel A) and 32.4% for aggregate farm profits (Panel B).[34]

6.6 Extended Selection Model with Multiple Attributes and Credit Rationing

The model studied so far could potentially be subject to a number of possible criticisms. First, it was based on the assumption that farmers vary only in a single (productivity) attribute. In reality, however, farmers could also vary in other attributes such as business skill, which affects unit costs. By ignoring other dimensions of farmer heterogeneity, out analysis could have under estimated the role of selection. In particular, the model is incapable of explaining the negative treatment effect observed for unit costs in TRAIL. Second, the model assumed no frictions in input markets, thereby ruling out features such as credit rationing and scale economies, which many scholars have highlighted as important explanations for poverty traps in developing countries [see, for example, Galor and Zeira, 1993, Banerjee and Newman, 1993, Banerjee et al., 2019, Balboni et al., 2021]. It is possible that these traps were more acute for selected TRAIL farmers. For instance, perhaps selected TRAIL farmers were more subject to binding credit constraints, or were capable of realizing greater scale economies than selected GRAIL farmers, and for these reasons experienced larger treatment effects on unit cost reductions and profits.

In what follows, we consider an alternative model which simultaneously addresses these concerns. It incorporates three different dimensions of farmer i's type: productivity a_i, business skill represented by unit cost parameter c_i, and wealth w_i a proxy for a farmer-level attribute determining a binding credit limit. Moreover, we allow for technological and pecuniary returns to scale, represented respectively by elasticities μ, ζ of potato revenues and unit costs respectively with respect to scale of cultivation, where the magnitude of μ is unrestricted, and ζ is allowed to be negative.

Specifically, the production function determining potato revenues R_{ivt} of farmer i in village v in year t is

$$\log R_{ivt} = \log a_i + \mu \log l_{ivt} + \delta_{vt} \tag{9}$$

where l_{ivt} denotes area cultivated and δ_{vt} denotes a village-year yield-cum-price shock. The

[34] As borrowers are partitioned into three productivity bins, this procedure ignores variations within each bin. To examine the robustness of these results we conduct the decomposition exercise using a continuous measure of productivity instead. We find that selection explains 9.7% of difference in average treatments on potato profit and 12.5% of the difference in ATEs on aggregate farm profit. The corresponding CTEs account for 41.7% and 33.9% of the ATE difference in potato profit and aggregate farm profit respectively. Details of this robustness exercise are not included in the paper, and are available on request.

unit cost function is

$$\log u_{ivt} = \log c_i + \zeta \log l_{ivt} + \log q_{vt} \tag{10}$$

where u_{ivt} denotes cost per acre and q_{vt} denotes a village-year cost shock. Hence an expansion in area cultivated will allow unit costs to fall in this model if ζ is negative. Moreover, the extent to which unit costs fall depends on the cost type c_i of the farmer. If farmers selected in TRAIL had a higher cost type c_i on average, a given rate of expansion in area will cause a larger absolute drop in unit costs for treated farmers in TRAIL. So this model could potentially explain a larger TRAIL treatment effect on unit cost reduction.

The farmer's total cultivation cost (C_{ivt}) is determined by his credit access according to the equation:

$$\log C_{ivt} = \log w_i + \log \gamma_{vt} \tag{11}$$

where w_i depends on the farmer's wealth, and γ_{vt} is a village-year shock to the supply of credit. As credit constraints are binding, total cultivation costs equal the credit limit:

$$\log C_{ivt} = u_{ivt} l_{ivt}. \tag{12}$$

Combining equations (11) and (12) we obtain

$$\log l_{ivt} = \log w_i - \log u_{ivt} + \log \gamma_{vt} \tag{13}$$

Equations (10) and (13) jointly determine area cultivated and unit costs (where we restrict $\zeta > -1$ in order to ensure the existence of a unique, stable solution):

$$\log l_{ivt} = \frac{1}{1+\zeta}[\log w_i - \log c_i - \log q_{vt} + \log \gamma_{vt}] \tag{14}$$

$$\log u_{ivt} = \frac{\zeta}{1+\zeta}[\log w_i + \log c_i + \log q_{vt}] + \frac{1}{1+\zeta} \log \gamma_{vt} \tag{15}$$

i.e., by the wealth and cost types of the farmer, in conjunction with village and year shocks in the supply of credit and input prices. Finally, given l_{ivt}, revenues are determined by the production function given by equation (9).

Let the proportional change of credit access $d \log C_{ivt}$ resulting from the treatment be denoted Δ. Then the proportional increase in area cultivated equals $d \log l_{ivt} = \frac{1}{1+\zeta}\Delta$, leading to a proportional increase in revenues $d \log R_{ivt} = \mu d \log l_{ivt} = \frac{\mu}{1+\zeta}\Delta$. Hence the increase in potato profit equals

$$d\Pi_{ivt} \equiv dR_{ivt} - dC_{ivt} = [\frac{\mu}{1+\zeta} R_{ivt} - C_{ivt}]\Delta \tag{16}$$

Relative to the growth in borrowing, revenues grow at a rate equal to $\frac{\mu}{1+\zeta}$, i.e, on technological and pecuniary scale economy elasticities. If scale economies exist, revenues (and hence profits) expand at a rate faster than costs.

Note that the rate of growth of revenues does not depend farmer type. The *absolute* change in revenues and costs of course depends on type, since type affects baseline revenues and costs. However, it is apparent that this version of the model will also be unable to explain the larger profit treatment effects in TRAIL, since the baseline (i.e., Control 1 farmers') revenues and costs do not differ significantly between TRAIL and GRAIL (as seen in Table 9). Given common scale economy elasticities μ, ζ applicable to both sets of selected farmers, as well as similar baseline revenues and costs, equation (16) shows that the predicted ATE on potato profits must also be similar.

Instead, it could be argued that farmers differ also in the extent to which they are capable of realizing technological and pecuniary scale economies. Then μ and ζ could also be farmer-specific. Perhaps, farmers selected in TRAIL had a higher average $\frac{\mu}{1+\zeta}$ resulting in a higher profit ATE despite a similar baseline revenue and cost. To allow for this possibility, we estimate the scale economy elasticities μ and ζ separately for selected TRAIL and GRAIL farmers. For either treatment arm, we restrict the sample to Treated and Control 1 farmers, and use a treatment dummy as an instrument for the area cultivated in regressions corresponding to equations (9) and (15) respectively to obtain an IV estimate of μ and ζ for selected TRAIL and GRAIL subjects.

These estimates of ζ and μ are presented in the first two rows of Table 14. We do see a significant pecuniary scale economy elasticity for TRAIL selected farmers, but not for GRAIL farmers. This reflects the significant difference in ATEs on unit cost we have already seen. On the other hand, we find a larger technical scale economy elasticity μ for GRAIL selected farmers (1.69 rather than 1.30), and a greater rate of expansion in credit access Δ among GRAIL treated farmers (28% rather than 22%). Hence the predicted ATE on potato profit turns out to be larger in GRAIL both in terms of absolute and percent changes. Moreover, the predicted ATEs for both treatment arms are substantially larger (nearly 70% increase for both) than the actual ATEs (40% and 4%) respectively. We conclude therefore that this extended model (also) cannot satisfactorily account for the observed ATE patterns in the data.

7 Explaining Differences in Conditional Treatment Effects

Therefore, in this section we develop an alternative model which explains conditional treatment effect differences aside from selection differences. In particular, we need to explain how farmers of the same type, and offered exactly the same loan program, could end up with markedly different outcomes for unit cost and profits in potato cultivation across the two treatment arms. The only differentiating feature between the two arms is in the nature of the respective agents and the informal role they must play when interacting with treated farmers. So differences in conditional treatment effects must depend on the distinct network relationships between treated subjects and the appointed agent, and possible impacts on these relationships. The model we develop below focuses on the nature of informal contracts for credit and output sales between farmers and traders, and the role that agents play in monitoring farmers' actions, providing them with advice and marketing assistance.

TRAIL and GRAIL agents vary with respect to their expertise and motivations. The TRAIL agent may have greater business expertise and ability to provide advice to farmers on how to procure inputs of better quality or at lower prices. He also expects that a larger potato harvest realized by the farmer is likely to result in greater sales placed through the trader which would boost the latter's middleman profits. On the downside, if the crop failed, he would both lose his agent commission, and earn a reduced middleman profit. As higher ability farmers are likely to grow and sell more potatoes, the TRAIL agent would be motivated to recommend high ability farmers. The expansion in production and sales would be larger, the lower the unit costs of the farmers, which would motivate the TRAIL agent to spend additional time providing them with business advice enabling them to lower unit costs.

The GRAIL agent by contrast was generally not a trader and so would be less likely to have the same business expertise or profit motive as a TRAIL agent. Instead, he was more likely to have an ideological or political motive, which may be more sharply focused on poorer, low ability farmers unlike the traders. Moreover the GRAIL agent would not benefit from upside gains of farmers. He may therefore be more focused on ensuring that selected farmers not default on their loans, which (besides the loss of the agent commission) would be associated with farmer distress, reflecting unfavorably on the competence and performance of the agent and the political party that appointed him. Hence he may be particularly motivated to monitor treated farmers and ensure that they undertake actions and expenditures that reduce the risk of a crop failure, which raise unit costs of cultivation.

Below, we show that under suitable parametric assumptions on the technology and the nature of production risk, a model that incorporates these features can explain superior selection as well as larger conditional treatment effects in the TRAIL scheme. It also generates a set of additional testable predictions regarding HTEs on time spent by the farmers interacting with their respective agents, on loan default rates, potato production, profits and unit costs. In particular, the TRAIL agent tends to devote more time to interacting with higher ability farmers whose unit costs then fall by more and potato output and profit increase by more. The GRAIL agent, besides selecting more farmers in the lowest ability bin (least productive farmer), spends most of his time interacting with these low ability farmers, who end up with lower default rates than TRAIL treated farmers in the lowest ability bin, and also realize the largest increases in unit costs.

7.1 Assumptions and Predictions

As in the first model that we discussed in Section 6.2, we assume that farmers vary only in farm ability, denoted by θ. The farmer's crop succeeds with probability $p(\theta, m)$ where $p_\theta > 0, p_m > 0, p_{mm} < 0, p_{\theta m} < 0$: lower ability farmers are riskier, and monitoring (m) reduces their risk by more. If the crop succeeds, the farmer produces $a(\theta, m)f(l)$, where

$a_\theta > 0, a_m < 0,\ f_l > 0, f_{ll} < 0,\ -\frac{f"}{f'}$ is non-increasing, and p varies 'relatively little' with θ.[35] Expected TFP $A(\theta, m) \equiv a(\theta, m)p(\theta, m)$ is then increasing in ability and decreasing in m. We denote the farmer's per-unit cultivation cost by $c(h, m)$, where h is the level of help he receives, and $c_h < 0, c_{hh} > 0, c_m > 0$, so that the trader's help reduces, but monitoring increases, the farmer's per-unit cultivation costs. Thus, whereas monitoring increases unit costs and lowers risk and expected productivity, help has no effect on risk and lowers unit cost. For instance, monitoring can cause the farmer to increase his use of pesticides, which increases his chance of crop success. But this also raises his unit costs, and lowers his productivity because he spends time procuring and spraying more pesticides, which diverts time away from other cultivation tasks. On the other hand, the agent can give the farmer valuable business advice about the brands to purchase and where to purchase them, which can raise input quality, or lower their cost.

We assume that the relevant functions are smooth with well-behaved curvature, so optimal allocations are interior. We also assume $c_{hm} = 0$; this simplifies the analysis but is not critical.

Traders and farmers enter into bilateral interlinked credit-cum-output contracts, where the trader provides the farmer credit for working capital, help and monitoring, and the farmer cultivates the specified area, and then sells his harvest to the trader. Both parties are risk-neutral. Farmers have zero wealth, and traders have unlimited access to credit at a constant cost ρ. The contracts are the outcome of an efficient equilibrium in a frictionless contract market where traders know farmers' ability. Traders incur interaction costs of $\gamma_T(h + m)$ and credit costs of ρ per rupee loaned, and earn per unit return of τ when they sell the farmer's crop in an external market. The farmer cannot repay the loan if the crop fails, but otherwise repays at interest rate r, and receives a lumpsum side payment s from the trader when the contract is signed. Hence a control farmer of type θ enters into a contract specifying $(l^c(\theta), m^c(\theta), h^c(\theta), r^c(\theta), s^c(\theta))$, where $(l, m, h) = (l^c(\theta), m^c(\theta), h^c(\theta))$ maximizes joint expected payoffs of the farmer-trader pair:

$$(1 + \tau)A(\theta, m)f(l) - (1 + \rho)c(h, m)l - \gamma_T[m + h] \tag{17}$$

The interest rate $r^c(\theta)$ is then set to "decentralize" the efficient scale decision $l^c(\theta)$ to the farmer, so that the farmer selects the efficient area $l^c(\theta)$ in his own self-interest: $l = l^c(\theta)$ maximizes $A(\theta, m^c(\theta))f(l) - p(\theta, m^{c(\theta)}(1 + r^c(\theta))c^{c(\theta)}(h, m)l$.[36] The side-payment depends on the relative bargaining power of the trader and the farmer. Additional details of the model are presented in Section A1 in the Appendix.

In an optimal contract, traders do not monitor control farmers ($m^c(\theta) \equiv 0$), but they do help them ($h^c(\theta) > 0$). Monitoring is inefficient because it lowers expected productivity and is costly for both farmer and trader, and produces no benefits, since neither party is

[35]By "relatively little" we mean in comparison to how much TFP a varies with θ. See Maitra et al. [2017] for further details, where it is shown that these assumptions ensure that treatment effects of the TRAIL scheme are increasing in θ.

[36]Note that $1 + r^c(\theta) = \frac{1+\rho}{(1+\tau)p(\theta,0)}$, so $r^c(\theta)$ decreases in θ.

risk-averse. Help is efficient because it raises expected productivity, and so it is provided as long as γ_T is not too large. The help induces the farmer to plant more area (l), and earn greater profit per acre. More able farmers receive more help, because per unit of trader's time, help generates a greater expected return when the farmer has higher ability. Hence we obtain the following testable predictions for control farmers:

Prediction 1 *Higher ability farmers are less likely to default, and pay a lower (informal) interest rate.*

Prediction 2 *Higher ability farmers produce more output, and incur lower unit costs.*

Turning next to the interventions, in TRAIL villages, a trader is selected as the agent. Suppose in turn, he recommends a farmer of type θ to receive a TRAIL loan at interest rate $r_T < \rho$. The TRAIL loan is a supplement to the farmer's informal loan from the trader, and allows him to expand acreage by l^t. We assume he cannot reduce the area that was already agreed upon before the scheme was introduced, but that trader help and monitoring decisions can be freely modified. The farmer repays the TRAIL loan only if his crop succeeds. The TRAIL agent receives a commission $\psi < 1$ per rupee interest repaid. The trader-farmer pair then modify their contract decisions by choosing $(l^t, m^t, h^t) = (l^t(\theta), m^t(\theta), h^t(\theta))$ to maximize

$$(1+\tau)A(\theta,m)f(l^c(\theta)+l^t)-[(1+\rho)l^c(\theta)+\{1+r_T(1-\psi)\}p(\theta,m)l^t]c(h^t,m^t)-\gamma_T(h^t+m^t) \quad (18)$$

Let the resulting outcomes for TRAIL treated farmer of type θ be denoted $l^T(\theta) \equiv l^c(\theta) + l^t(\theta), m^T(\theta) = m^t(\theta), h^T(\theta) = h^t(\theta)$. The variables $l^T(\theta)$ and $h^T(\theta)$ are increasing in θ, while $m^T(\theta) \equiv 0$. Hence the Order-Preserving Assumption (OPA) holds. The model then predicts:

Prediction 3 *Among treated farmers in the TRAIL scheme, those with higher ability produce more and receive more help, and incur lower unit costs.*

Prediction 4 *In the TRAIL scheme, a treated farmer produces more, earns more profit and incurs lower unit costs than a control farmer of the same ability level.*

In the GRAIL scheme, the agent is not a trader but a political appointee. This agent earns expected payoff $v(\theta)p(\theta,m) - \gamma_G m$ where $v(\theta)$ represents the weight that the GRAIL agent places on the farmer's success, defined as lack of loan default, and m is the extent to which he monitors the farmer. The welfare weight is decreasing in θ. The welfare weight is also a function of the expected commission that the agent will earn from the farmer. This might lead us to think that the agent earns a higher payoff from high ability farmers, because they borrow more, cultivate larger areas and produce more. We assume that this consideration for personal financial gain is outweighed by the political considerations which create a bias in the opposite direction (since richer farmers receive a lower welfare weight). Since he is not a trader, the GRAIL agent is not in a position to provide business advice and help to the farmer, but he can monitor the farmer at the unit monitoring cost of γ_G.

If a farmer of type θ is selected to receive a GRAIL loan, the GRAIL agent will choose the monitoring level $m^G(\theta)$ that maximizes $v(\theta)p(\theta, m) - \gamma_G m$. Given $m^G(\theta)$, the farmer renegotiates his contract with his trade partner. It is easy to check the trader will continue to have no incentive to monitor the farmer. Hence, given $m^G(\theta)$, the revised contract will select supplementary area cultivated $l^g = l^g(\theta)$ and revised help $h^g = h^g(\theta)$ to maximize their joint payoff

$$(1+\tau)A(\theta, m^G(\theta))f(l^c(\theta) + l^g) - [(1+\rho)l^c(\theta) + p(\theta, m)(1+r_T)]c(h^g, m^g)l^g - \gamma_G h \quad (19)$$

Let the resulting GRAIL treated outcomes be denoted $(l^G(\theta) \equiv l^c(\theta) + l^g(\theta), m^G(\theta), h^G(\theta))$. $m^G(\theta) \geq 0 = m^T(\theta)$ and $m^G(\theta)$ is decreasing. We find that the Order Preserving Assumption holds in the GRAIL scheme. We obtain the following predictions for GRAIL treated farmers:

Prediction 5 *The GRAIL agent interacts more with low ability agents. GRAIL borrowers are less likely to default on their loans than TRAIL borrowers of the same ability level.*

Prediction 6 *If the production function has constant elasticity, a GRAIL treated farmer cultivates a smaller area, receives less help, achieves a smaller reduction in unit costs, and a smaller increase in expected profits, compared to a TRAIL treated farmer of the same ability.*

This model thus explains larger treatment effects (conditional on ability) for the TRAIL scheme, which can account for a larger TRAIL ATE even in the absence of any selection differences. This is mainly because TRAIL treated farmers receive more help. In turn, this is due to the different non-program objectives of the TRAIL and GRAIL agents. TRAIL agents farmers want the borrowers to produce more, so that they can earn larger middleman profits. The help is more effective at raising crop output if the farmer is more able. On the other hand, the GRAIL agent monitors treated farmers so as to reduce default risk. This raises their unit cost and lowers their productivity, and so GRAIL treated farmers produce less and earn smaller profits than TRAIL treated farmers. These effects are larger, the less able the farmer. Observe finally that the model also explains the different selection incentives of the two agent types: the TRAIL agent benefits more from recommending more able farmers, compared to the GRAIL agent.

7.2 Testing Predictions of the Model

Prediction 1: To test Prediction 1, which relates to the variation of control farmers' interest rates with ability, we first consider the average interest rate paid by Control 1 households in TRAIL and GRAIL in each of the three bins. We restrict our sample to informal loans taken before our intervention began, to avoid potential contamination from the intervention on borrowers' interest rates. The average control household in Bin 1 reported taking loans at 21% interest per annum. This is significantly higher than the 15% that Bin 2 ($p-value = 0.03$) and the 16% that Bin 3 households reported ($p-value = 0.04$). These averages are presented in Figure A2.

Column 1 of Table 15 presents the OLS regression results of interest rates paid on informal loans by Control 1 and Control 2 households (using the pooled sample of TRAIL and GRAIL households). The coefficient estimate of productivity is negative, indicating that for control households, there is a decline in interest rate as productivity increases. The results presented in Figure A2 and the regression results presented in column 1 of Table 15 therefore both confirm Prediction 1.

Prediction 2: Next consider Prediction 2 about how output and unit cost vary with ability among control farmers. Columns 2 and 3 of Table 15 present OLS regression results of potato ouput (in kg) and input cost per acre in potato cultivation (in Rs.) on the productivity estimate. The regressions control for year dummies, and the estimation sample includes Control 1 and Control 2 households with at most 1.5 acres of landholding in TRAIL and GRAIL villages. Consistent with Prediction 2, column 2 shows that output is increasing in productivity (both productivity and productivity squared are positive), while column 3 shows that unit costs are decreasing in productivity (both productivity and productivity squared are negative).

Prediction 3: Now turn to Prediction 3 for TRAIL treated farmers. At each four-monthly survey interview, we asked sample households whether in the previous three days they had spoken with the local trader or the agent about either cultivation, the harvest, or output sales. Since in the TRAIL scheme the agent is also a trader, we include interaction with the trader as well as the agent (in case the agent is a different trader) to measure number of interactions with traders.[37] In column 4 of Table 15 we see that TRAIL Treatment households' interacted more with the agent if they had higher ability. Columns 5 and 6 of Table 15 present the OLS regressions of quantity of potato cultivated and input cost per acre in potato cultivation on productivity for TRAIL Treatment households. Consistent with Prediction 3, more productive TRAIL Treatment farmers interacted more with the agent, and produced more potatoes, at a lower input cost per acre.

[37]Since the GRAIL agent is not a trader, when we measure the number of interactions with the GRAIL agent we do not include interactions with the trader.

Prediction 4: Table 12 provides evidence about Prediction 4. The TRAIL HTEs on potato acreage, potato output and input cost per acre in potato cultivation (see columns 2, 3 and 9 respectively) are larger than the conditional treatment effects on GRAIL borrowers in the same productivity bin. In most cases the differences between the TRAIL and GRAIL treatment effects are statistically significant.

Prediction 5: Finally, we test the predictions concerning farmer-agent interactions. Column 14 of Table 12 presents the HTEs on agent interactions in TRAIL and GRAIL. As the results in Panel B show, they are decreasing in ability in the GRAIL scheme. On the other hand they are increasing in ability in the TRAIL scheme, indicating that the TRAIL agent increased his interaction with higher-ability farmers more.

Figure 3 presents the default rates on program loans by TRAIL and GRAIL treated households by productivity bin. TRAIL Treatment households in productivity Bin 1 defaulted on 9.3 percent of their loans, whereas GRAIL Bin 1 Treatment households defaulted at a significantly lower 5 percent ($p-value = 0.03$). As we just saw when discussing Prediction 5, the GRAIL agent spent the most time interacting with Bin 1 farmers, and so the lower default rate among low-productivity GRAIL Treatment borrowers is consistent with the prediction that the GRAIL agent may have increased their crop success rate. In Bins 2 and 3 the differences in default rates across TRAIL and GRAIL go the other way, although they are not statistically significant. This is consistent with Prediction 5.

Prediction 6: To check Prediction 6, consider the results presented in Panel C of Table 12. The results presented in column 8 show that for every productivity bin, TRAIL treatment effects on potato profit exceeds the corresponding GRAIL treatment effects, although the differences are not statistically significant. The results presented in column 9 show that for every productivity bin, the TRAIL treatment effects on unit costs are lower than the corresponding GRAIL treatment effects and the difference is statistically significant for the most productive borrowers. This is consistent with Prediction 6.

8 Concluding Comments

This paper finds evidence that a rural credit program where borrower selection was delegated to private traders significantly increases borrower production and farm incomes. When instead the local village council appointed the agent, production increased to a similar extent, but farm incomes were unchanged. The discrepancy between the treatment effects on farmer profits was driven partly by different impacts on unit costs of cultivation.[38]

[38]This paper does not discuss the impacts of the two schemes on the distribution of farm incomes. In a parallel paper [Maitra et al., 2021] we find that the TRAIL scheme increased Atkinson measures of household welfare by significantly more than the GRAIL scheme, over a wide range of parameters of inequality aversion. Hence TRAIL appears to have generated bettter outcomes even accounting for

In terms of underlying mechanisms, we find some evidence that TRAIL agents selected more productive farmers. However this seems to explain only a small fraction of the observed ATE differences in farm profits. A model of selection based on additional attributes of farmers: credit constraints (reflecting wealth) and unit costs (reflecting business skills) apart from productivity (reflecting farming skills and ownership of complementary assets) also failed to account for the observed ATE differences. Therefore, we offered an alternative explanation based on interactions between agents and farmers reflecting the role of agents as monitors and advisors, which were endogenously impacted by the programs, and provided evidence consistent with this. However, we cannot definitively rule out other competing explanations for the empirical findings. We also do not seek to argue that the selection of beneficiaries is an unimportant task. Instead, the TRAIL and GRAIL agents both seemed to be equally effective in selecting productive farmers, and the different outcomes in the two schemes is driven by differences in their informal supervision of farmers

Overall, the paper demonstrates the scope for appointing private agents as intermediaries in the delivery of agricultural development programs, provided these agency contracts are designed suitably to align incentives. This alignment may depend on the nature of the specific crops involved, and the role played by these private agents in lending or trading with farmers. Designing suitable agency contracts requires a good understanding of these specificities, which may vary across contexts. We hope our paper will inspire other attempts to experiment with similar mechanisms in other contexts.

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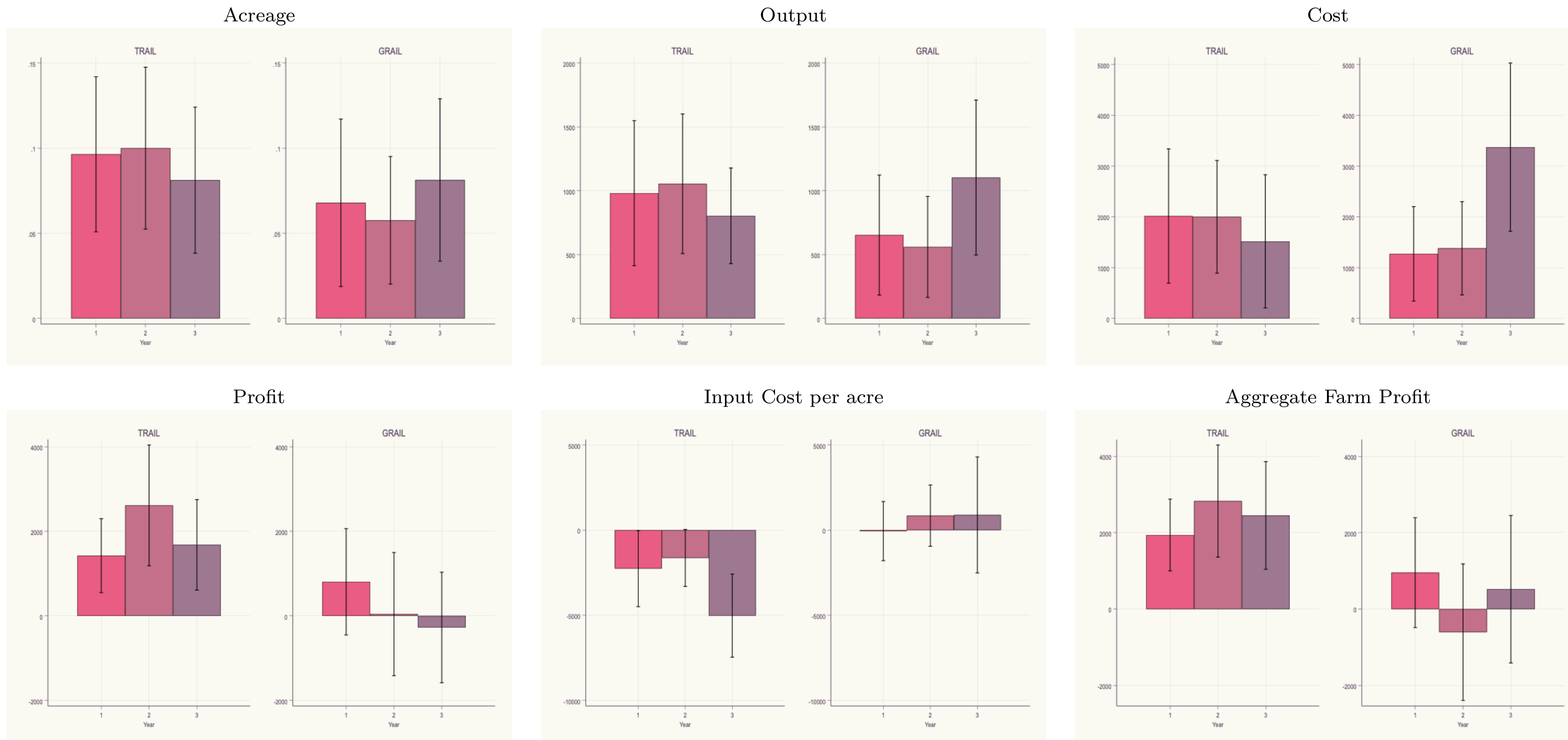
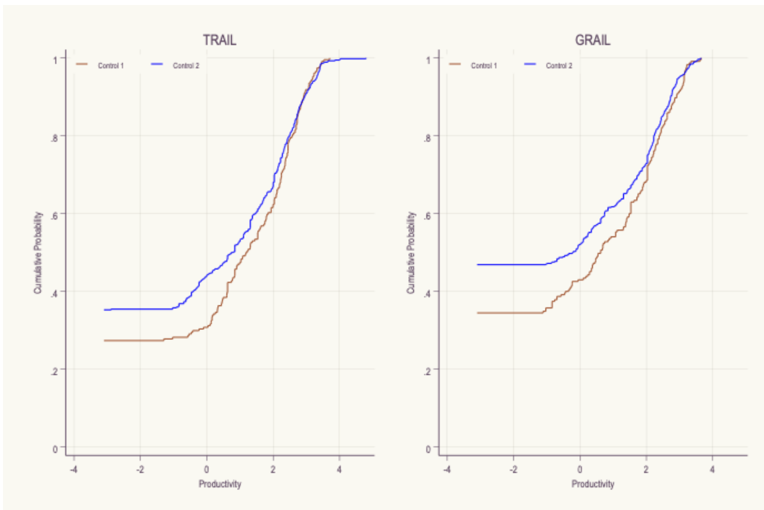
Figure 1: Year Specific Average Treatment Effects.



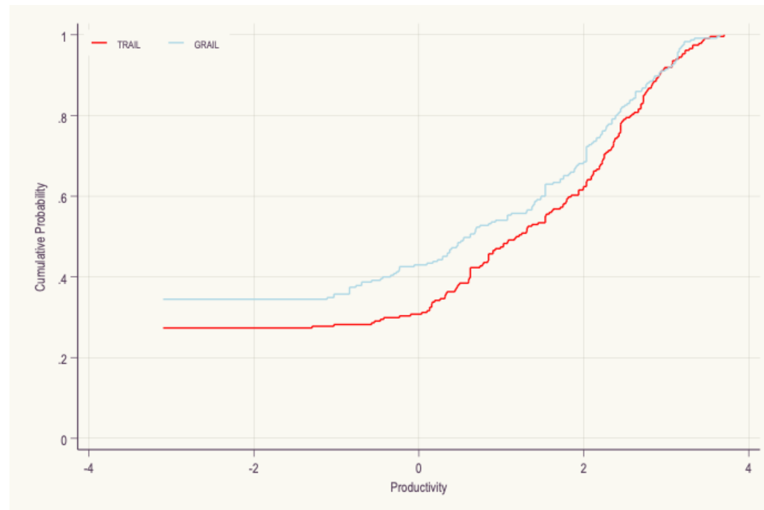
Notes: Estimates Average Treatment effects by year presented (regression specification given by equation (1)) and 90% confidence interval presented. Acreage, output, cost, profit and input cost per acre refer to acreage in potato cultivation, potato output, imputed profit and input cost per acre in potato cultivation respectively. Aggregate Profit refers to aggregate farm profit, computed as the sum of imputed profit for the four major crops grown in this region: potatoes, paddy, sesame and vegetables.

Figure 2: Selection on Productivity: TRAIL and GRAIL

Panel A: Productivity Distribution
Selected v. Non-selected households

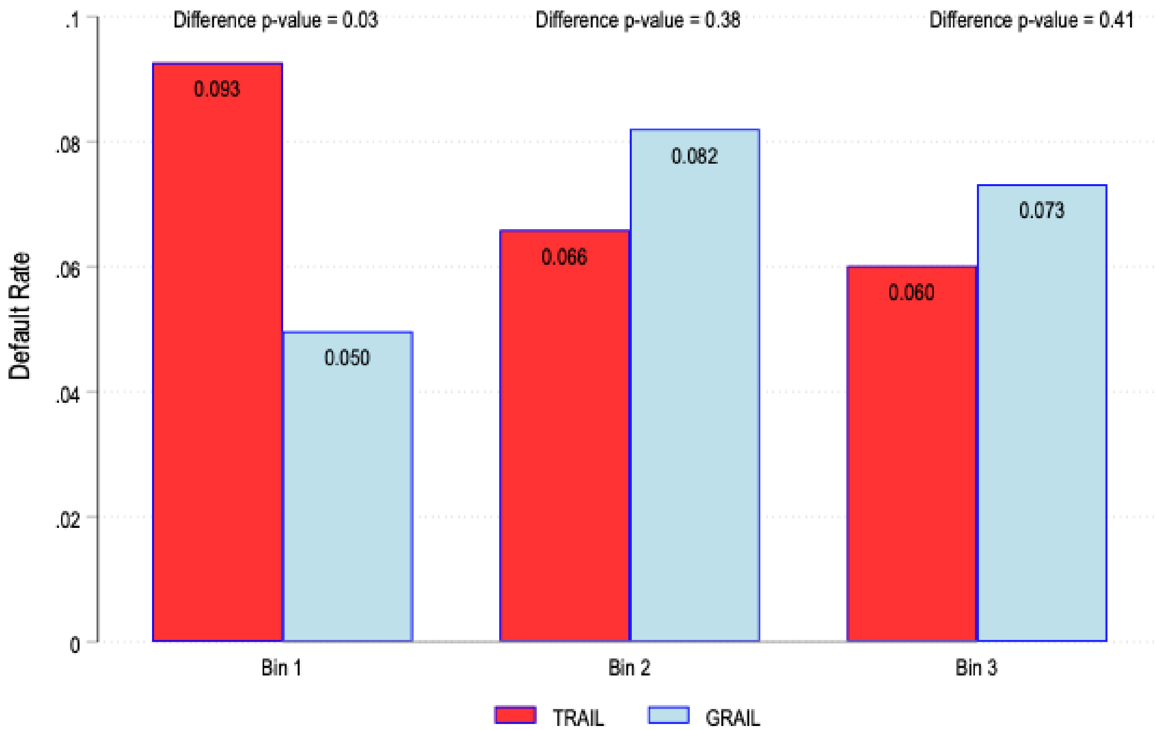


Panel B: Productivity Distribution
Selected households: TRAIL vs GRAIL



Notes: In Panel A, the sample is restricted to Control 1 and Control 2 households in TRAIL and GRAIL villages. Productivity is computed using the logarithm of acreage under potato cultivation. The null hypothesis that the distributions of estimated productivity for Control 1 and Control 2 households are equal is rejected by a two sample Kolmogorov-Smirnov (KS) test with $p-value = 0.005$ for TRAIL and 0.011 for GRAIL. In Panel B, the sample is restricted to Control 1 households in TRAIL and GRAIL villages. Productivity is computed using the logarithm of acreage under potato cultivation. The null hypothesis that the distributions of estimated productivity for TRAIL Control 1 and GRAIL Control 1 households are equal is rejected by a two sample Kolmogorov-Smirnov (KS) test with $p-value = 0.06$.

Figure 3: Default Rates on TRAIL and GRAIL Loans, by Productivity Bin



Notes: The height of each bar measures the fraction of program loans that were not repaid fully by the due date. The sample is restricted to Treatment households in TRAIL and GRAIL villages with at most 1.5 acres of land. p-value of difference between default rate between TRAIL and GRAIL for each Bin presented. Productivity is computed using the logarithm of the acreage under potato cultivation.

Table 1: Descriptive Statistics

Panel A: Village Characteristics

	TRAIL (1)	GRAIL (2)	TRAIL — GRAIL (3)
Number of Households	276.04	252.21	23.83
	(201.59)	(238.36)	
Number of Potato Cultivators	164.63	160.75	3.88
	(130.30)	(168.39)	
Of which:			
Landless	15.96	27.96	-12.00
	(18.98)	(75.63)	
Own 0 – 1.25 acres	113.88	99.67	14.21
	(103.22)	(78.00)	
Own 1.25 – 2.50 acres	25.58	24.63	0.96
	(16.27)	(25.20)	
Own 2.50 – 5.00 acres	10.88	12.83	-1.96
	(7.39)	(17.11)	
Own 5.00 – 12.50 acres	1.38	1.17	0.21
	(1.79)	(1.95)	
Own more than 12.50 acres	0.00	0.04	-0.04
	(0.00)	(0.20)	

Panel B: Household Characteristics

	All	TRAIL		GRAIL	
		Control 1	Treatment–Control 1	Control 1	Treatment–Control 1
	(1)	(2)	(3)	(4)	(5)
Landholding	0.457	0.454	-0.017	0.451	0.063
	(0.009)	(0.025)	(0.078)	(0.026)	(0.068)
Non Hindu	0.173	0.171	-0.027	0.140	0.060
	(0.008)	(0.025)	(0.060)	(0.023)	(0.039)
Low Caste	0.376	0.389	-0.020	0.353	0.001
	(0.011)	(0.032)	(0.068)	(0.031)	(0.046)
Age of Oldest Male	47.498	47.145	-0.002	47.970	0.000
	(0.283)	(0.773)	(0.002)	(0.757)	(0.003)
Schooling of Oldest Male:	0.430	0.470	0.006	0.366	0.098
More than Primary School	(0.011)	(0.033)	(0.060)	(0.031)	(0.053)
Occupation of Oldest Male: Cultivation	0.756	0.825	0.022	0.813	-0.017
	(0.009)	(0.025)	(0.053)	(0.026)	(0.078)
Occupation of Oldest Male: Labour	0.519	0.551	-0.006	0.570	-0.033
	(0.011)	(0.033)	(0.056)	(0.032)	(0.049)
Joint Test F-statistic			0.49		1.51
p-value			*0.83*		*0.21*
Sample Size	2081		460		454

Notes: Panel A uses data from the house listing exercise we carried out in 2007. Since 2 of the villages from the 2007 sample had to be replaced due to political violence, Panel A uses a sample of 46 villages. In Panel B, Column 1 presents means for all sample households. This includes Treatment, Control 1 and Control 2 households across all 48 villages. Note that only Control 2 households that owned no more than 1.5 acres of land are included in the estimation sample. By construction, all Treatment and Control 1 households own less than or equal to 1.5 acres. Occupation includes main or secondary occupation. Standard errors in parentheses.

Table 2: Agent Characteristics

	GRAIL (1)	TRAIL (2)	Difference (3)
Male	1.00	0.958	0.042
	(0.00)	(0.042)	(0.042)
SC/ST	0.208	0.083	0.125
	(0.085)	(0.058)	(0.102)
Non-Hindu	0.125	0.083	0.042
	(0.069)	(0.058)	(0.090)
General caste	0.667	0.833	-0.167
	(0.098)	(0.078)	(0.125)
Primary Occupation: Cultivator	0.375	0.042	0.33
	(0.101)	(0.042)	(0.109)
Primary Occupation: Shop/business	0.292	0.958	-0.667
	(0.095)	(0.042)	(0.104)
Primary Occupation: Government Job	0.125	0.000	0.125
	(0.069)	(0.000)	(0.069)
Primary Occupation: Other	0.208	0.000	0.208
	(0.085)	(0.000)	(0.085)
Owns agricultural land	2.63	3.29	-0.667
	(0.198)	(0.244)	(0.314)
Total owned land	4.08	5.04	-0.958
	(0.248)	(0.292)	(0.383)
Has *pucca* house	0.375	0.458	-0.083
	(0.101)	(0.104)	(0.145)
Educated above primary school	0.958	0.792	0.167
	(0.042)	(0.085)	(0.094)
Weekly income (Rs.)	1102.895	1668.75	-565.855
	(138.99)	(278.16)	(336.78)
Village society member	0.292	0.083	0.208
	(0.095)	(0.058)	(0.111)
Party hierarchy member	0.167	0.000	0.167
	(0.078)	(0.00)	(0.079)
Panchayat member	0.125	0.000	0.125
	(0.069)	(0.00)	(0.069)
Self/family ran for village head	0.083	0.000	0.083
	(0.058)	(0.00)	(0.058)

Notes: Sample consists of 24 agents in TRAIL villages and 24 agents in GRAIL villages. Standard errors are in parentheses.

Table 3: Pre-Intervention Social and Economic Engagement with the Agent

	TRAIL (1)	GRAIL (2)	Difference (3)	p-value (4)
Networks: Agent and Household belong to				
Same Occupation	0.014	0.287	-0.272	*0.000*
	(0.004)	(0.014)		
Same Caste Category	0.575	0.654	-0.079	*0.002*
	(0.015)	(0.015)		
Same Religion	0.796	0.950	-0.154	*0.000*
	(0.013)	(0.007)		
Same Political Party		0.313		
		(0.014)		
Relationship with Agent				
Household knows Agent	0.905	0.911	-0.006	*0.692*
	(0.009)	(0.009)		
Household meets Agent at least once a week†	0.978	0.984	-0.007	*0.415*
	(0.005)	(0.004)		
Household member invited by Agent on special occasions†	0.325	0.295	0.030	*0.224*
	(0.015)	(0.015)		
Agent is one of the two most important				
Money Lenders	0.169	0.087	0.082	*0.000*
	(0.012)	(0.009)		
Input suppliers	0.184	0.077	0.107	*0.000*
	(0.012)	(0.008)		
Output buyers	0.185	0.024	0.162	*0.000*
	(0.012)	(0.005)		
Employers	0.114	0.077	0.037	*0.016*
	(0.010)	(0.008)		
In the past 3 years household has				
Bought from Agent	0.331	0.047	0.283	*0.000*
	(0.015)	(0.007)		
Borrowed from Agent	0.153	0.052	0.101	*0.000*
	(0.011)	(0.007)		
Worked for Agent	0.102	0.093	0.009	*0.548*
	(0.009)	(0.009)		

Notes: The TRAIL agent was a randomly selected trader in the village. The GRAIL agent was selected by the local government. Recommended households include Treatment and Control 1 households. Non-recommended households include Control 2 households. Sample restricted to all households with 1.5 acres of land in TRAIL and GRAIL villages.†: Conditional on knowing the agent. Weighted averages over Treatment, Control 1 and Control 2 households are presented. Treatment and Control 1 households are assigned a weight of $\frac{30}{N}$, where as Control 2 households are assigned a weight of $\frac{N-30}{N}$, where N is the total number of households in the village. p-value of the TRAIL v GRAIL difference presented in itatics.

Table 4: Average Treatment Effects on Agricultural Borrowing

	All Loans (Rs.) (1)	Non-Program Loans (Rs.) (2)
TRAIL		
Treatment Effect	2873	-448.5
	(727.2)	(634.8)
	0.000	*0.480*
FDR Sharpened q	[0.001]	[0.471]
Mean Control 1	5226	5226
% Effect	54.98	-8.58
GRAIL		
Treatment Effect	2754	-104.9
	(526.2)	(551.3)
	0.000	*0.849*
FDR Sharpened q	[0.001]	[0.849]
Mean Control 1	4330	4330
% Effect	63.60	-2.42
Difference TRAIL v GRAIL		
$p-value$	*0.894*	*0.684*
Sample Size	6,159	6,156

Notes: Treatment effects are computed from regressions that follow equation (1) in the text and are run on household-year level data for all sample households in TRAIL and GRAIL villages with at most 1.5 acres of land. Sample restricted to cycles 1, 4 and 7 (the potato planting cycles). Regressions also control for the religion and caste of the household, age, educational attainment and occupation of the eldest male member of the household, household's 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. †: Non-Program loans refer to loans from sources other than the TRAIL or GRAIL schemes. The FDR sharpened q values computed using the procedure in Anderson [2008] are in square brackets.

Table 5: Average Treatment Effects on Potato Cultivation

	Acreage (Acres) (1)	Production (Kg) (2)	Cost of Production (Rs.) (3)	Price (Rs.) (4)	Revenue (Rs.) (5)	Value Added (Rs.) (6)	Imputed profit (Rs.) (7)	Input Cost per Acre (Rs.) (8)
TRAIL								
Treatment	0.0925	946	1845	-0.0301	3897	2059	1906	-2911
	(0.0247)	(256.3)	(648.5)	(0.0913)	(1099)	(559.9)	(544.4)	(900.7)
p-value TRAIL	*0.001*	*0.001*	*0.007*	*0.743*	*0.001*	*0.001*	*0.001*	*0.002*
FDR Sharpened q-value	[0.003]	[0.003]	[0.004]	[0.262]	[0.003]	[0.003]	[0.003]	[0.003]
Mean Control 1	0.336	3646	8482	4.627	14285	5732	4734	49077
% Effect	27.530	25.946	21.752	-0.651	27.280	35.921	40.262	-5.931
GRAIL								
Treatment	0.0689	771.7	2009	-0.176	2504	493.7	191.4	551.2
	(0.0243)	(273.5)	(624.7)	(0.136)	(1060)	(676.8)	(652.8)	(1092)
	0.007	*0.007*	*0.002*	*0.203*	*0.022*	*0.469*	*0.771*	*0.616*
FDR Sharpened q-value	[0.015]	[0.015]	[0.011]	[0.17]	[0.028]	[0.307]	[0.409]	[0.377]
Mean Control 1	0.296	3237	7071	4.800	12965	5828	4942	47511
% Effect	23.277	23.840	28.412	-3.667	19.314	8.471	3.873	1.160
Difference TRAIL v GRAIL								
$p-value$	*0.508*	*0.652*	*0.859*	*0.370*	*0.377*	*0.0854*	*0.0523*	*0.0186*
Sample Size	6,150	6,150	6,150	3,818	6,150	6,150	6,150	4,038

Notes: Treatment effects are computed from regressions that follow equation (1) in the text and are run on household-year level data for all sample households in TRAIL and GRAIL villages with at most 1.5 acres of land. The dependent variable in column 1 takes the value of 1 if the household cultivated potato in that year, 0 otherwise. The dependent variables in columns 2—8 take the actual value reported by the household if it did, or take the value zero if it did not cultivate potatoes in that year. In column 9, households that did not cultivate potatoes in a year are dropped from the estimating sample. Regressions also control for the religion and caste of the household, age, educational attainment and occupation of the eldest male member of the household, household's 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. Imputed profit = Value Added – shadow cost of labour. Standard errors in parentheses are clustered at the village level. p-values are in italics. The FDR sharpened q values computed using the procedure in Anderson [2008] are in square brackets.

Table 6: Average Treatment Effects on Farm Profit, Non Agricultural Income and Total Household Income

	Aggregate Farm Profit (1)	Non Agricultural Income (2)	Total Household Income (3)
TRAIL	2406	1436	3843
	(597.2)	(3077)	(2872)
	0.000	*0.643*	*0.187*
FDR Sharpened q	[0.001]	[0.390]	[0.231]
Mean Control 1	8564	33618	42182
% Effect TRAIL	28.09	4.27	9.11
GRAIL	290.3	-4313	-4023
	(768)	(2950)	(3254)
	0.707	*0.150*	*0.222*
FDR Sharpened q	[0.5]	[0.5]	[0.5]
Mean Control 1	7580	37171	44751
% Effect GRAIL	3.83	-11.60	-8.99
Difference TRAIL v GRAIL			
$p-value$	*0.0380*	*0.183*	*0.0735*
Sample Size	6,150	6,150	6,150

Notes: Treatment effects are computed from regressions that follow equation (1) in the text and are run on household-year level data for all sample households in TRAIL and GRAIL villages with at most 1.5 acres of land. Regressions also control for the religion and caste of the household, age, educational attainment and occupation of the eldest male member of the household, household's 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. Standard errors in parentheses are clustered at the village level. p-values are in italics. The FDR sharpened q values computed using the procedure in Anderson [2008] are in square brackets.

Table 7: Loan Performance

	Take-up (1)	**Default** (2)
Panel A: Sample Means		
TRAIL	0.937	0.070
	(0.006)	(0.007)
GRAIL	0.872	0.070
	(0.009)	(0.008)
Difference p-value	*0.000*	*0.987*
Panel B: Regression Results		
GRAIL	-0.066	0.005
	(0.011)	(0.010)
	0.000	*0.619*
R^2	0.08	0.05
Sample Size	2667	2422

Notes: The sample consists of household-cycle level observations of Treatment households in TRAIL and GRAIL villages with at most 1.5 acres of landholding. The dependent variable in column 1 takes value 1 if the household took the program loan in the particular cycle, provided the household was eligible for the loan in that cycle. The dependent variable in column 2 takes value 1 if a borrowing household fails to fully repay the amount due on the loan taken in that cycle on the due date. The regression specification In Panel B is given by equation (2). Regressions also control for landholding, religion and caste of the household and age and educational attainment of the oldest male in the household. Robust standard errors presented in parenthesis. p-values are presented in italics.

Table 8: Likelihood of Recommendation: Observable Characteristics

	Specification 1		Specification 2		Specification 3	
	TRAIL	GRAIL	TRAIL	GRAIL	TRAIL	GRAIL
	(1)	(2)	(3)	(4)	(5)	(6)
Economic Links:						
Bought from agent	-0.031	0.107			-0.025	0.121
	(0.042)	(0.076)			(0.044)	(0.079)
	0.469	*0.170*			*0.578*	*0.140*
Borrowed from agent	0.170	0.008			0.149	0.005
	(0.032)	(0.109)			(0.033)	(0.107)
	0.000	*0.943*			*0.000*	*0.964*
Worked for agent	0.064	0.003			0.030	-0.017
	(0.070)	(0.042)			(0.073)	(0.042)
	0.370	*0.941*			*0.685*	*0.695*
Social and Political Links:						
Same Occupation	-0.003	0.057			-0.007	0.042
	(0.032)	(0.043)			(0.041)	(0.044)
	0.919	*0.201*			*0.864*	*0.350*
Same Caste	0.009	0.033			-0.030	0.040
	(0.038)	(0.029)			(0.057)	(0.040)
	0.815	*0.268*			*0.600*	*0.322*
Same Religion	0.063	0.151			0.063	0.187
	(0.042)	(0.045)			(0.091)	(0.048)
	0.149	*0.003*			*0.499*	*0.001*
Same Political Party		0.070				0.074
		(0.035)				(0.033)
		0.055				*0.032*
Household Characteristics:						
Non Hindu			-0.059	-0.047	-0.004	0.008
			(0.045)	(0.034)	(0.099)	(0.028)
			0.205	*0.176*	*0.971*	*0.778*
Male headed household			0.251	0.081	0.222	0.067
			(0.056)	(0.101)	(0.055)	(0.097)
			0.000	*0.432*	*0.001*	*0.495*
High Caste			0.034	0.039	0.056	0.007
			(0.042)	(0.031)	(0.068)	(0.035)
			0.433	*0.222*	*0.419*	*0.841*
Household Size			0.005	0.006	0.005	0.005
			(0.010)	(0.007)	(0.010)	(0.007)
			0.589	*0.364*	*0.640*	*0.446*
Age of Oldest Male			-0.003	-0.000	-0.002	-0.000
			(0.002)	(0.001)	(0.002)	(0.001)
			0.124	*0.903*	*0.186*	*0.830*
Education of Oldest Male: Primary			0.051	-0.062	0.053	-0.060
			(0.045)	(0.027)	(0.043)	(0.028)
			0.267	*0.028*	*0.233*	*0.039*
Household Wealth:						
Spline Landholding $\leq$ Median			0.326	0.199	0.300	0.147
			(0.181)	(0.162)	(0.180)	(0.165)
			0.085	*0.232*	*0.108*	*0.380*
Spline Landholding $>$ Median			-0.493	-0.217	-0.478	-0.150
			(0.225)	(0.203)	(0.228)	(0.214)
			0.039	*0.297*	*0.047*	*0.490*

Continued . . .

Likelihood of Recommendation: Observable Characteristics (Continued)

	Specification 1		Specification 2		Specification 3	
	TRAIL	GRAIL	TRAIL	GRAIL	TRAIL	GRAIL
	(1)	(2)	(3)	(4)	(5)	(6)
Pucca (brick) house			-0.081	-0.064	-0.072	-0.074
			(0.041)	(0.034)	(0.045)	(0.035)
			0.059	*0.073*	*0.122*	*0.048*
Constant	0.209	0.075	0.097	0.172	0.031	-0.027
	(0.043)	(0.042)	(0.110)	(0.066)	(0.134)	(0.084)
	0.000	*0.090*	*0.390*	*0.015*	*0.816*	*0.756*
Sample Size	797	822	787	804	787	804

Notes: Dependent variable is recommended by the agent. Sample restricted to Control 1 and Control 2 households with at most 1.5 acres of land in the TRAIL and GRAIL villages. OLS regression results presented. Specification 1 includes only Economics and Political Links; Specification 2 includes only Household Characteristics and Household Wealth and finally Specification 3 is the complete specification. Standard errors (clustered at the village level) are presented in parenthesis. p-values are presented in italics.

Table 9: Selection Effects

	Mean GRAIL C1 (1)	Additional Effect TRAIL (2)	% Effect (3)	p-value (4)	Sample Size (5)
Agricultural Borrowing					
All Borrowing	4330	915.30 (979.93)	21.14	*0.355*	1,392
Potato Cultivation					
Acreage	0.296	0.038 (0.049)	12.84	*0.442*	1,392
Production	3237	402.25 (559.23)	12.43	*0.476*	1,392
Cost of Production	7071	1,384.18 (1,265.31)	19.58	*0.280*	1,392
Price	4.80	-0.15 (0.13)	-3.13	*0.254*	904
Revenue	12965	1,298.61 (2,240.85)	10.02	*0.565*	1,392
Value Added	5828	-84.87 (1,098.97)	-1.46	*0.939*	1,392
Imputed Profit	4942	-214.02 (996.38)	-4.33	*0.831*	1,392
Input cost per acre	47511	1,195.98 (1,708.00)	2.52	*0.487*	959
Farm Profit, Non-Agricultural Income and Household Income					
Aggregate Farm Profit	7580	1,073.87 (1,181.68)	14.17	*0.368*	1,392
Non-Agricultural Income	37171	-4,246.43 (3,695.77)	-11.42	*0.256*	1,392
Household Income	44751	-3,172.56 (3,623.67)	7.09	*0.386*	1,392

Notes: Estimating equation given by equation (4). Regressions run on household-year level data for all Control 1 households in TRAIL and GRAIL villages. % Effect is given by the TRAIL coefficient in column 2 as a percentage of the Mean for GRAIL Control 1 households in column 1. Regressions also control for the religion and caste of the household, age, educational attainment and occupation of the eldest male member of the household, household's landholding, a set of year dummies and an information village dummy.

Table 10: Variation of Productivity with Observable Household Characteristics

Regression Results	
Landholding	1.061
	(0.168)
	0.000
Non Hindu	-0.144
	(0.156)
	0.362
Low caste	-0.139
	(0.173)
	0.427
Household Size	0.013
	(0.020)
	0.517
Female Headed Household	-0.436
	(0.202)
	0.036
Age of Oldest Male	-0.004
	(0.003)
	0.285
Oldest Male: Completed Primary School	0.094
	(0.088)
	0.292
Constant	1.237
	(0.198)
	0.000
Sample Size	1,001
R-squared	0.152
Descriptives of Productivity	
Mean of Productivity	1.707
SD of Productivity	1.148
Minimum Productivity	-1.294
Productivity Quartile: 25%	0.811
Productivity Quartile: 50%	2.015
Productivity Quartile: 75%	2.628
Maximum Productivity	3.702

Notes: The dependent variable is the household productivity estimate (computed using log(acreage). Standard errors in parentheses are clustered at the village level. p-values are presented in italics. The etimating sample includes cultivator Control 1 and Control 2 households in TRAIL and GRAIL villages with at most 1.5 acres of land.

Table 11: Effect of Treatment on Voting Patterns in Straw Poll

	Average Treatment Effect		**Heterogeneous Treatment Effect**	
	TRAIL	GRAIL	TRAIL	GRAIL
	(1)	(2)	(3)	(4)
Treatment Effect	0.0241	0.0782		
	(0.0496)	(0.0340)		
	0.631	*0.0308*		
Treatment Effect: Bin 1			0.108	0.171
			(0.0750)	(0.0598)
			[-0.01, 0.23]	[0.08, 0.27]
Treatment Effect: Bin 2			-0.0545	0.0326
			(0.0770)	(0.0780)
			[-0.18, 0.06]	[-0.09, 0.16]
Treatment Effect: Bin 3			0.0594	0.0315
			(0.0777)	(0.0552)
			[-0.07, 0.18]	[-0.06, 0.13]
Selection Effect	-0.0649	0.0825		
	(0.0447)	(0.0369)		
	0.160	*0.0352*		
Selection Effect: Bin 1			-0.143	0.0126
			(0.0847)	(0.0514)
			[-0.28, -0.01]	[-0.07, 0.10]
Selection Effect Bin 2			-0.0191	0.151
			(0.0721)	(0.0555)
			[-0.13, 0.10]	[0.07, 0.25]
Selection Effect: Bin 3			-0.0395	0.0868
			(0.0439)	(0.0581)
			[-0.11, 0.03]	[-0.01, 0.18]
Sample Size	1,011	1,026	1,011	1,026

Notes: Estimating sample includes all sample households in TRAIL and GRAIL villages with atmost 1.5 acres of land. OLS regression results presented. Estimating sample includes all sample households in TRAIL and GRAIL villages with at most 1.5 acres of land. Productivity computed using logarithm of acreage under potato cultivation. In columns 1 and 2, p-values (using clustered standard errors) presented in italics. In columns 3 and 4, bootstrapped 90% confidence intervals (with 2000 iterations) presented in square brackets.

Table 12: HTEs by Productivity Bin. Potato Cultivation, Aggregate Incomes and Interaction with Agent

	Acreage	Output	Cost (Total)	Price	Revenue	Value Added	Imputed Profit	Input Cost per acre
	(Acres) (1)	(Kgs) (2)	(Rs.) (3)	(Rs./Kgs) (4)	(Rs.) (5)	(Rs.) (6)	(Rs.) (7)	(Rs./Acres) (8)
TRAIL Treatment Effects								
Bin 1	0.0195	189.3	414	0.198	834	419.8	350.3	-1582
	(0.0132)	(141.5)	(432.8)	(0.162)	(610.7)	(255.6)	(234.6)	(4780)
	[-0.00, 0.04]	[-81.80, 405.99]	[-346.55, 1114.14]	[-0.05, 0.49]	[-391.98, 1684.79]	[-114.36, 771.07]	[-138.02, 676.30]	[-7989.51, 8322.39]
Bin 2	0.0749	772.9	1886	-0.185	2901	1015	910.6	-3054
	(0.0250)	(231.4)	(522.8)	(0.149)	(920.2)	(533.9)	(537.6)	(1623)
	[0.03, 0.12]	[391.60, 1171.02]	[1006.36, 2699.27]	[-0.41, 0.08]	[1320.22, 4391.13]	[149.09, 1880.50]	[48.71, 1801.30]	[-5823.75, -366.08]
Bin 3	0.128	1297	2158	0.0668	5559	3425	3244	-3088
	(0.0480)	(498)	(1262)	(0.135)	(2368)	(1328)	(1314)	(1373)
	[0.05, 0.21]	[495.17, 2110.65]	[128.06, 4255.35]	[-0.18, 0.26]	[1769.72, 9383.66]	[1179.99, 5487.38]	[1014.13, 5299.60]	[-5557.67, -996.52]
GRAIL Treatment Effects								
Bin 1	0.0503	506.5	1327	-0.132	1927	594.9	261.6	7553
	(0.0181)	(189.8)	(355.9)	(0.198)	(723.4)	(417.5)	(401)	(3095)
	[0.02, 0.08]	[184.58, 819.69]	[746.04, 1917.34]	[-0.45, 0.23]	[689.34, 3099.20]	[-130.67, 1260.77]	[-465.21, 899.15]	[2686.22, 12861.40]
Bin 2	0.0839	816.1	2485	-0.274	2924	420.4	166.8	-1443
	(0.0270)	(224.1)	(436.7)	(0.284)	(1087)	(767.1)	(735.6)	(1277)
	[0.04, 0.13]	[443.64, 1190.95]	[1765.59, 3206.73]	[-0.72, 0.18]	[1157.65, 4667.26]	[-825.16, 1619.35]	[-1022.17, 1316.92]	[-3607.14, 641.58]
Bin 3	0.128	1584	3400	-0.139	5351	1950	1592	1419
	(0.0537)	(566.7)	(1243)	(0.117)	(2328)	(1782)	(1810)	(1555)
	[0.04, 0.22]	[640.98, 2540.38]	[1294.48, 5458.99]	[-0.34, 0.04]	[1238.86, 9020.16]	[-1345.31, 4636.73]	[-1728.13, 4325.62]	[-1363.50, 3808.97]
Difference: TRAIL vs GRAIL								
Bin 1	[-0.07, 0.01]	[-724.82, 59.62]	[-1894.27, -5.64]	[-0.12, 0.77]	[-2761.32, 409.73]	[-1029.34, 657.35]	[-696.45, 911.98]	[-17839.34, 2010.97]
Bin 2	[-0.07, 0.05]	[-558.03, 513.17]	[-1785.12, 518.52]	[-0.43, 0.62]	[-2359.84, 2360.66]	[-912.02, 2117.90]	[-721.36, 2213.27]	[-5150.62, 1700.01]
Bin 3	[-0.12, 0.12]	[-1607.69, 940.72]	[-4194.23, 1691.30]	[-0.09, 0.49]	[-5388.48, 5713.05]	[-2095.22, 5241.67]	[-1986.35, 5324.85]	[-8072.78, -1085.17]
Sample Size	6,237	6,237	6,237	3,841	6,237	6,237	6,237	4,061

Continued . . .

Table 12 (Continued): HTEs by Productivity Bin. Potato Cultivation, Aggregate Incomes and Interaction with Agent

	Farm Profit (Rs.) (9)	Non-agricultural Income (Rs.) (10)	Household Income (Rs.) (11)	Interaction with Agent (12)
TRAIL Treatment Effects				
Bin 1	217.8	-4657	-4439	0.044
	(654.8)	(4658)	(4415)	(0.038)
	[-850.29, 1314.04]	[-12648.62, 3412.55]	[-11851.10, 3342.11]	[-0.012, 0.126]
Bin 2	888	10863	11751	0.057
	(1089)	(5744)	(5836)	(0.032)
	[-776.83, 2811.50]	[1302.93, 20465.48]	[1804.70, 21128.38]	[-0.001, 0.109]
Bin 3	4275	-3281	993.9	0.322
	(1569)	(6326)	(5853)	(0.137)
	[1578.81, 6728.17]	[-13191.19, 7373.11]	[-8209.63, 10638.43]	[0.125, 0.579]
GRAIL Treatment Effects				
Bin 1	1094	-3236	-2142	1.466
	(778.6)	(5654)	(5358)	(0.618)
	[-280.22, 2319.92]	[-13465.25, 5320.29]	[-12160.59, 5907.39]	[0.520, 2.524]
Bin 2	762.2	699.3	1462	0.364
	(1016)	(6872)	(6893)	(0.294)
	[-752.99, 2446.27]	[-10699.57, 11123.61]	[-10021.37, 12004.64]	[0.046, 0.981]
Bin 3	1368	-8662	-7294	0.122
	(1958)	(5251)	(5819)	(0.049)
	[-2144.99, 4399.77]	[-17809.78, -152.75]	[-17791.70, 1812.85]	[0.054, 0.234]
Difference: TRAIL vs GRAIL				
Bin 1	[-2476.27, 843.93]	[-13091.15, 11252.10]	[-13304.59, 10071.59]	[-2.464, -0.479]
Bin 2	[-2172.79, 2590.94]	[-3493.86, 24703.14]	[-3752.99, 25118.22]	[-0.915, 0.016]
Bin 3	[-1190.50, 7081.99]	[-7476.58, 19351.74]	[-4585.66, 22392.23]	[-0.014, 0.449]
Sample Size	6,237	6,237	6,237	2760

Notes: The estimating equation follows equation (8) in the text. The estimating sample includes all sample households in TRAIL and GRAIL villages with at most 1.5 acres of land. The dependent variable in column 1 takes the value of 1 if the household cultivated potato in that year, 0 otherwise. The dependent variables in columns 1—11 take the actual value reported by the household if it did, or take the value zero if it did not cultivate potatoes in that year. In column 8, households that did not cultivate potatoes in a year are dropped from the estimating sample. In column 12, agent-farmer interaction is measured by the number of times in the past year the responder talked to the agent about agricultural cultivation related matters. In this column the sample includes Treatment and Control 1 households in TRAIL and GRAIL villages with at most 1.5 acres of land. The regressions also control for a set of year dummies and an information village dummy. Productivity is computed using the logarithm of the acreage under potato cultivation. Standard errors in parentheses are clustered at the village level. Bootstrapped 90% confidence interval (with 2000 iterations) is presented in square brackets.

Table 13: Decomposition of ATE Differences in Imputed Profit from Potato Cultivation and Aggregate Farm Profit. TRAIL v. GRAIL

Bin (k)	σ_k^T	σ_k^G	$\sigma_k^T - \sigma_k^G$	TRAIL HTE's (T_k^T)	GRAIL HTE's (T_k^G)	TRAIL – GRAIL HTE's ($T_k^T - T_k^G$)	$(\sigma_k^T - \sigma_k^G) \times T_k^T$	$\sigma_k^G \times (T_k^T - T_k^G)$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Potato Profit								
1	0.27	0.34	-0.07	350.3	261.6	88.70	-24.52	30.16
2	0.33	0.33	0.00	910.6	166.8	743.80	0.00	245.45
3	0.40	0.32	0.08	3244	1592	1652.00	259.52	528.64
ATE				1906	191.4	1715		
% of ATE Due to Selection								13.70
% of ATE Due to CTE								46.90
Panel B: Aggregate Farm Profit								
1	0.27	0.34	-0.07	217.8	1094	-876.20	-15.51	-302.03
2	0.33	0.33	0.00	888	762.2	125.80	-2.49	41.75
3	0.40	0.32	0.07	4275	1368	2907.00	316.35	940.12
ATE				2406	290.4	2116		
% of ATE Due to Selection								14.10
% of ATE Due to CTE								32.14

Notes: σ_k^T denotes the proportion of TRAIL households in Bin k; σ_k^G denotes the proportion of GRAIL households in Bin k. % of ATE due to selection computed as $\sum_{k=1}^{3} \frac{((\sigma_k^T - \sigma_k^G) \times T_k^T}{\text{ATE}}$, while % of ATE Due to CTE computed as $\sum_{k=1}^{3} \frac{\sigma_k^G \times (T_k^T - T_k^G)}{\text{ATE}}$.

Table 14: Estimates from the Multidimensional Screening Model

	Variable	TRAIL (1)	GRAIL (2)
(1)	Pecuniary Scale Economy Elasticity ζ	-0.202	0.135
		(0.099)	(0.319)
		0.041	*0.673*
(2)	Technical Scale Economy Elasticity μ	1.304	1.694
		(0.168)	(0.085)
		0.000	*0.000*
(3)	Percent Expansion in Borrowing Δ	0.218	0.284
(4)	Predicted ATE: $[\frac{\mu}{1+\zeta}R - C] \times \Delta$	3239.68	3487.35
(5)	Predicted ATE (%)	68.43	70.57

Notes: Estimate of μ, ζ obtained from IV regression of log potato revenue and log unit cost respectively on log area cultivated for Treated and Control 1 subjects for each treatment arm, with Treatment dummy as instrument for log area cultivated. Δ defined as the proportional increase in the cost of cultivation owing to the treatment defined by the treatment effect as a proportion of the Mean for Control 1 households in column 4 of Table 5. R and C defined by the mean Revenue and Cost of cultivation for Control 1 households in columns 6 and 4 of Table 5. In Row 5, predicted ATE is expressed as a percentage of the corresponding Control 1 households (see Table 5). Regressions also control for the religion and caste of the household, age, educational attainment and occupation of the eldest male member of the household, household's landholding, a set of year dummies and an information village dummy. Standard errors in parentheses are clustered at the village level. p-values are in italics.

Table 15: Variation of Informal Interest Rate, Trader Interactions, Output and Input Cost per acre by Productivity

	TRAIL and GRAIL Control Households			TRAIL Treatment Households		
	Interest Rate	Output	Input Cost (per acre)	Trader Interactions	Output	Input Cost (per acre)
	(1)	(2)	(3)	(4)	(5)	(6)
Productivity	-0.007	1,564.128	-294.871	0.367	1,743.912	-960.240
	(0.004)	(54.594)	(280.972)	(0.089)	(73.686)	(475.765)
	[-0.016, 0.001]	[1468.691, 1682.361]	[-856.779, 267.871]	[0.197, 0 .542]	[1617.678, 1910.246]	[-1836.34, -52.101]
Productivity Squared	0.003	579.615	-346.739	-0.034	596.827	24.094
	(0.002)	(44.149)	(160.825)	(0.060)	(40.854)	(207.543)
	[-0.001, 0.007]	[508.70, 684.688]	[-630.933, -27.350]	[-0.152, 0.072]	[518.381, 678.518]	[-376.257, 427.693]
Constant	0.157	-291.544	43,318.507	3.087	319.804	41,199.639
	(0.026)	(283.495)	(1,356.550)	(0.509)	(175.959)	(1,518.124)
Sample Size	761	4,890	3,011	681	678	517

Notes: OLS regression results presented. In column 1, the dependent variable is average interest rate paid on informal loans by households prior to the intervention. In columns 2 and 5, the dependent variable is the quantity of potato produced (in Kgs). In columns 3 and 6, the dependent variable is input cost per acre in potato cultivation (in Rs.). In column 4, the dependent variable is interactions with traders, measured as the number of times in the year the household reported engaging with local traders on agricultural matters. Sample in columns 1–3 restricted to Control 1 and Control 2 households in TRAIL and GRAIL villages with at most 1.5 acres of land. Sample in columns 4–6 restricted to Treatment households in TRAIL villages with at most 1.5 acres of land. In columns 2–6, the regressions also control for year dummies. Standard errors in parentheses are cluster-bootstrapped at the hamlet-level with 2000 iterations. Productivity is computed using the logarithm of the acreage under potato cultivation.

Online Appendix

A1 Model Of Agent-Farmer Interactions: Details

Control Farmers

A contract between farmer F of ability θ and trader T is represented by a scale of cultivation l, help h, monitoring m, an interest rate r and a side-payment s. The first three determine the size of the loan $c(h,m)l$. The farmer repays the loan if his crop succeeds. Hence the farmer's expected payoff (excluding fixed cost F) is

$$p(\theta,m)[a(\theta,m)f(l)-(1+r)c(h,m)l]+s \tag{A1}$$

while the trader's payoff is

$$\tau p(\theta,m)a(\theta,m)f(l)+[(1+r)p(\theta,m)-(1+\rho)]c(h,m)l-\gamma_T(m+h)-s \tag{A2}$$

where τ represents an exogenous middleman margin earned by the trader per unit output. An efficient contract maximizes the joint payoff given by

$$(1+\tau)A(\theta,m)f(l)-(1+\rho)c(h,m)l-\gamma_T[m+h] \tag{A3}$$

It is optimal for the trader to not monitor the farmer at all ($m^c(\theta)=0$), since monitoring is costly, lowers expected productivity A and increases the production cost. Next, observe that given a certain level of help h, the optimal scale of cultivation $l^c(\theta,h)$ which maximizes

$$(1+\tau)A(\theta,0)f(l)-(1+\rho)c(h,0)l \tag{A4}$$

is increasing in θ and h. Let the maximized value of the expression in equation (A4) be denoted by $\Pi(h,\theta)$. Then help $h^c(\theta)$ is chosen to maximize

$$\Pi(h,\theta)-\gamma_T h \tag{A5}$$

By the Envelope Theorem, Π is a supermodular function: the marginal return to help increases with the farmer's ability.[39] Hence $h^c(\theta)$ is increasing: higher ability farmers receive more help, and end up with higher scale of cultivation, productivity, and lower unit cost. This rationalizes our use of scale of cultivation as a proxy for ability and for productivity among control farmers.

Observe also that the choice of scale of cultivation can be delegated to the farmer, if the interest rate is set at

$$1+r^c(\theta)=\frac{1+\rho}{(1+\tau)p(\theta,0)} \tag{A6}$$

This interest rate adjusts the cost of capital up for default risk, and then subsidized by the trader in order to induce the farmer to internalize the effect of cultivation scale on T's profits. Hence we obtain predictions (i) and (ii).

[39] This is because Π_h equals $-\rho c_h(h,0)l^c(\theta,h)$ which is rising in θ.

TRAIL Treatment Effects

In TRAIL, a trader is appointed the agent, and recommends borrowers for TRAIL loans. These loans are offered at interest rate r_T, which is lower than the informal cost of capital for traders ρ. Agents earn a commission of $\psi \in (0,1)$ per rupee interest paid by the borrowers they recommended. We assume that any farmer whom the agent selects is already committed to cultivating l^c, financed by informal loans taken before the TRAIL loan was offered to him/her.[40] As a result the TRAIL loan finances an increase in the cultivation scale.[41] This applies to farmers in productivity Bins 2 and 3; for those in Bin 1 there are no pre-existing plans for cultivating potatoes. In what follows, we present calculations for farmers in Bins 2 and 3; for those in Bin 1 we set the pre-existing cultivation scale $L^c(\theta)$ to zero.

The efficient contract between T and F will now involve a supplementary cultivation scale of l^t, resulting in total scale of $l^T \equiv l^c + l^t$. The levels of monitoring and help will be adjusted to m^T, h^T. Then the joint payoff of T and F is

$$(1+\tau)A(\theta,m)f(L^c(\theta)+l^t)-[(1+\rho)L^c(\theta)+\{1+r_T(1-\psi)\}p(\theta,m)l^t]c(h,m)-\gamma_T[h+m] \quad \text{(A7)}$$

where $L^c(\theta) \equiv l^c(\theta, h^c(\theta))$.

The TRAIL agent continues to find it optimal not to monitor the farmer: $m^T(\theta) = 0$. Given help h, the treatment effect on cultivation scale $l^t(\theta,h)$ maximizes

$$(1+\tau)A(\theta,0)f(L^c(\theta)+l^t) - [(1+\rho)L^c(\theta) + p(\theta,0)\{1+r_T(1-\psi)\}l^t]c(h,m) \quad \text{(A8)}$$

and therefore it also maximizes

$$(1+\tau)a(\theta,0)f(L^c(\theta)+l^t) - [\{1+r_T(1-\psi)\}l^t]c(h,m) \quad \text{(A9)}$$

Using the same argument as used in Lemma 2 in Maitra et al. [2017], the cultivation treatment effect $l^t(.,h)$ is increasing in θ. The Envelope Theorem implies that the help provided by the agent to the treated farmer $h^T(\theta)$ must satisfy the first order condition

$$[(1+\rho)L^c(\theta) + \{1+r_T(1-\psi)\}p(\theta,0)l^t(\theta,h^T(\theta))]c_h(h^T(\theta),0) + \gamma_T = 0. \quad \text{(A10)}$$

The corresponding second order condition implies that $h^T(\theta)$ is increasing. Among treated farmers the more able will receive more help, and thereby attain lower unit costs, cultivate a larger scale, and produce higher output: hence the Order Preserving Assumption holds in TRAIL.

We can also compare agent interactions between treated and control farmers with the same ability θ. Help $h^c(\theta)$ provided to a control farmer with the same ability satisfies the first order condition

$$[(1+\rho)L^c(\theta)]c_h(h^c(\theta),0) + \gamma_T = 0. \quad \text{(A11)}$$

Comparing equations (A10) and (A11), it is evident that $h^T(\theta) \geq h^c(\theta)$, so treated farmers obtain more help. The reason is that they cultivate a larger area compared to control farmers with the same ability, so the gains from unit cost reductions generate a larger reduction in total cost, which motivates the agent to provide more help. In turn this

[40] This is in order to explain the lack of treatment effects on informal borrowing.

[41] Recall that in Table 4 we did not see any evidence that the TRAIL loans crowded out informal loans.

implies treated farmers cultivate a larger area, produce more output and earn more profits compared with control farmers of the same ability. This is Prediction 4.

GRAIL Treatment Effects

In the GRAIL scheme, the political incumbent appoints an agent who is not a trader. This agent does not lend, or trade in inputs or crop output, and so does not have the same business-related incentives as a TRAIL agent. Instead, his objectives are political or ideological, represented by welfare weight $v(\theta)$, and seeks to maximize $v(\theta)p(\theta, m) - \gamma_G m$. The welfare weight also includes the commission earned by the agent. While this may bias the agent in favor of selecting more able borrowers because they select larger loans and are less likely to default, we assume this is outweighed by political considerations which bias them in favor of less able farmers, so v is a decreasing function. The optimal level of monitoring (positive if γ_G is small enough) satisfies

$$v(\theta)p_m(\theta, m^G(\theta)) = \gamma_G \tag{A12}$$

Since monitoring is more effective when farmers are less able, and the welfare weights are decreasing in ability, $m^G(\theta)$ is decreasing in ability, and is greater that $m^T(\theta) = 0$. This implies Prediction 6: the GRAIL agent interacts less with high ability farmers. And default rates on GRAIL loans are lower than on TRAIL loans: $p(\theta, m^G(\theta)) \geq p(\theta, 0)$.

Monitoring by the GRAIL agent affects the payoffs of treated farmers and the trader they contract with. Their joint payoff is given by

$$\begin{aligned}(1+\tau)A(\theta, m^G(\theta)+m))f(L^c(\theta)+l^g) - [(1+\rho)L^c(\theta) \\ +\{1+r_T\}p(\theta, m^G(\theta)+m)l^g]c(h, m^G(\theta)+m) - \gamma_T[h+m]\end{aligned} \tag{A13}$$

where l^g denotes the additional area that the GRAIL treated farmer cultivates, and (h, m) continues to denote help and monitoring activities of the trader. The commission does not enter this expression since it accrues to the GRAIL agent rather than the trader. The trader has no incentive to monitor. Hence the contract involves a treatment effect l^g on area cultivated and help h which maximize

$$(1+\tau)A(\theta, m^G(\theta)))f(L^c(\theta)+l^g) - [(1+\rho)L^c(\theta) + \{1+r_T\}p(\theta, m^G(\theta))l^g]c(h, m^G(\theta)) - \gamma_T h \tag{A14}$$

$l^g(\theta, h)$ must then maximize

$$(1+\tau)a(\theta, m^G(\theta)))f(L^c(\theta)+l^g) - [\{1+r_T\}l^g]c(h, m^G(\theta)) \tag{A15}$$

while help $h^G(\theta)$ minimizes

$$[(1+\rho)L^c(\theta) + \{1+r_T\}p(\theta, m^G(\theta))l^g(\theta, h^G(\theta))]c(h, m^G(\theta)) + \gamma_T h \tag{A16}$$

Arguments similar to those used for TRAIL treated subjects imply that higher ability farmers receive more help. To see this, note that if $l^g(\theta; h)$ denotes the area treatment effect in GRAIL for any given help h, the same argument (combined with $m^G(.)$ decreasing) implies $l^g(,,h)$ is increasing in θ. Hence $h^G(\theta)$ satisfies the first order condition

$$[(1+\rho)L^c(\theta) + \{1+r_T\}p(\theta, m^G(\theta))l^g(\theta, h^G(\theta))]c_h(h^G(\theta), m^G(\theta)) + \gamma_T = 0 \tag{A17}$$

$c_{hm} = 0$ then implies that $c_h(h^G(\theta), m^G(\theta)) = c_h(h^G(\theta), 0)$ and the second order condition for minimization of equation (A17) implies $h^G(.)$ is increasing. Hence the Order Preserving Assumption is also satisfied in GRAIL: treated farmers of higher ability have lower unit cost, cultivate larger area and produce more output. This is the second part of Prediction 5. The first part follows from the greater monitoring in the GRAIL scheme.

Observe next that the HTE on area cultivated is higher in TRAIL, for any θ. This follows from comparing maximization problems equations (A9) and (A15), and using $a(\theta, m^G(\theta)) \leq a(\theta, 0)$, $\{1 + r_T\} > \{1 + r_T(1-\psi)\}$ and $c(h, m^G(\theta)) \geq c(h, 0)$.

To obtain Prediction 6, compare the first order conditions in equation (A10) and (A17) for help provided by the trader to treated farmers in TRAIL and GRAIL. If

$$p(\theta, 0)\{1 + r_T(1-\psi)\}l^t(\theta, h^T(\theta)) > p(\theta, m^G(\theta))\{1+r_T\}l^g(\theta, h^G(\theta)) \tag{A18}$$

more help will be provided to TRAIL treated farmers, who will then end up with lower unit costs, higher output and profits than GRAIL treated farmers of the same ability (because the latter are less productive and incur higher unit costs).

Finally we show that equation (A18) holds if the production function has constant elasticity $f(l) = l^\alpha$ where $\alpha \in (0, 1)$. Since $A(\theta, m)$ is falling in m and $c(\theta, m)$ is rising in m, it follows that

$$\frac{A(\theta, m^G(\theta))}{c(\theta, m^G(\theta))} \leq \frac{A(\theta, 0)}{c(\theta, 0)} \tag{A19}$$

This implies

$$\frac{p(\theta, m^G(\theta))}{p(\theta, 0)} \leq [\frac{a(\theta, 0)c(h, m^G(\theta))}{a(\theta, m^G(\theta))c(h, 0)}] \tag{A20}$$

Since the right-hand-side of equation (A20) is larger than one:

$$\frac{p(\theta, m^G(\theta))}{p(\theta, 0)} \leq [\frac{a(\theta, 0)c(h, m^G(\theta))}{a(\theta, m^G(\theta))c(h, 0)}]^{\frac{1}{1-\alpha}} \tag{A21}$$

From the respective first-order conditions for maximization of equations (A9) and (A15), and using $f(l) = l^\alpha$, we have

$$\frac{a(\theta, 0)c(h, m^G(\theta))}{a(\theta, m^G(\theta))c(h, 0)} = [\frac{L^c(\theta) + l^t(\theta, 0)}{L^c(\theta) + l^g(\theta, m^G(\theta))}]^{1-\alpha} \frac{1 + r_T(1-\psi)}{1 + r_T} \tag{A22}$$

The right-hand-side of this is smaller than

$$[\frac{L^c(\theta) + l^t(\theta, 0)}{L^c(\theta) + l^g(\theta, m^G(\theta))} \frac{1 + r_T(1-\psi)}{1 + r_T}]^{1-\alpha} \tag{A23}$$

Therefore

$$[\frac{a(\theta,0)c(h,m^G(\theta))}{a(\theta,m^G(\theta))c(h,0)}]^{\frac{1}{1-\alpha}} < \frac{L^c(\theta)+l^t(\theta,0)}{L^c(\theta)+l^g(\theta,m^G(\theta))}\frac{1+r_T(1-\psi)}{1+r_T} \tag{A24}$$

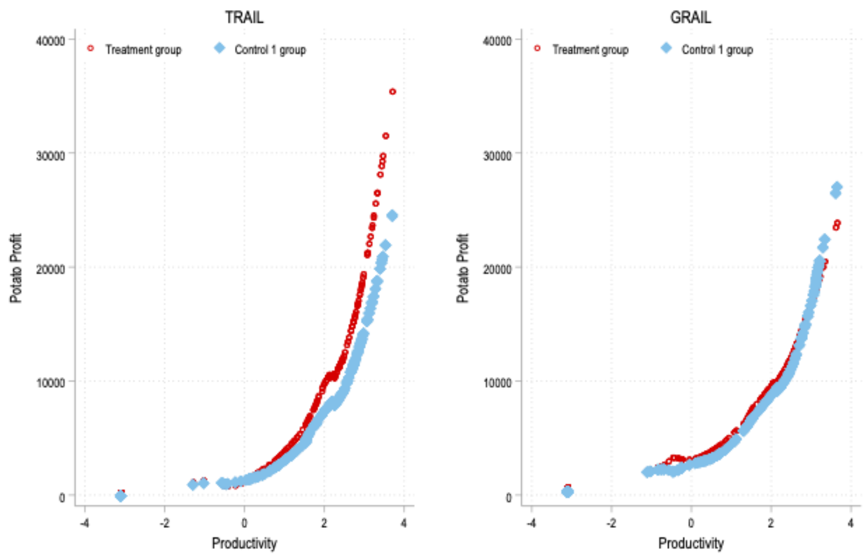
Combining this with equation (A21) we obtain

$$1 < \frac{p(\theta,0)\{1+r_T(1-\psi)\}(L^C(\theta)+l^t(\theta,0))}{p(\theta,m^G(\theta))\{1+r_T\}(L^C(\theta)+l^g(\theta,m^G(\theta)))} \tag{A25}$$

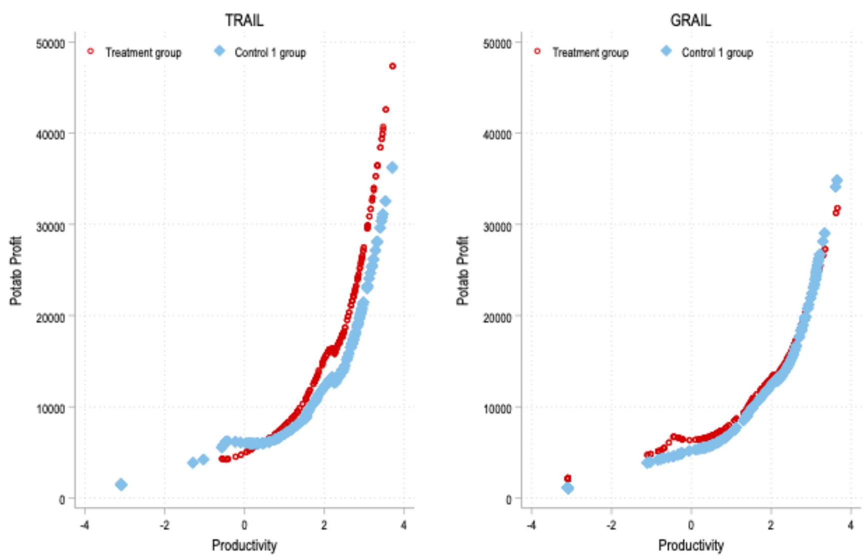
Since $l^g(\theta,m^G(\theta)) \leq l^t(\theta,0)$ we have $\frac{L^c(\theta)+l^t(\theta,0)}{L^C(\theta)+l^g(\theta,m^G(\theta)))} \leq \frac{l^t(\theta,0)}{l^g(\theta,m^G(\theta)))}$. So equation (A18) holds.

Figure A1: Variation in Potato Profit and Aggregate Farm Profit for Treatment and Control 1 groups by Productivity

Panel A: Potato Profit

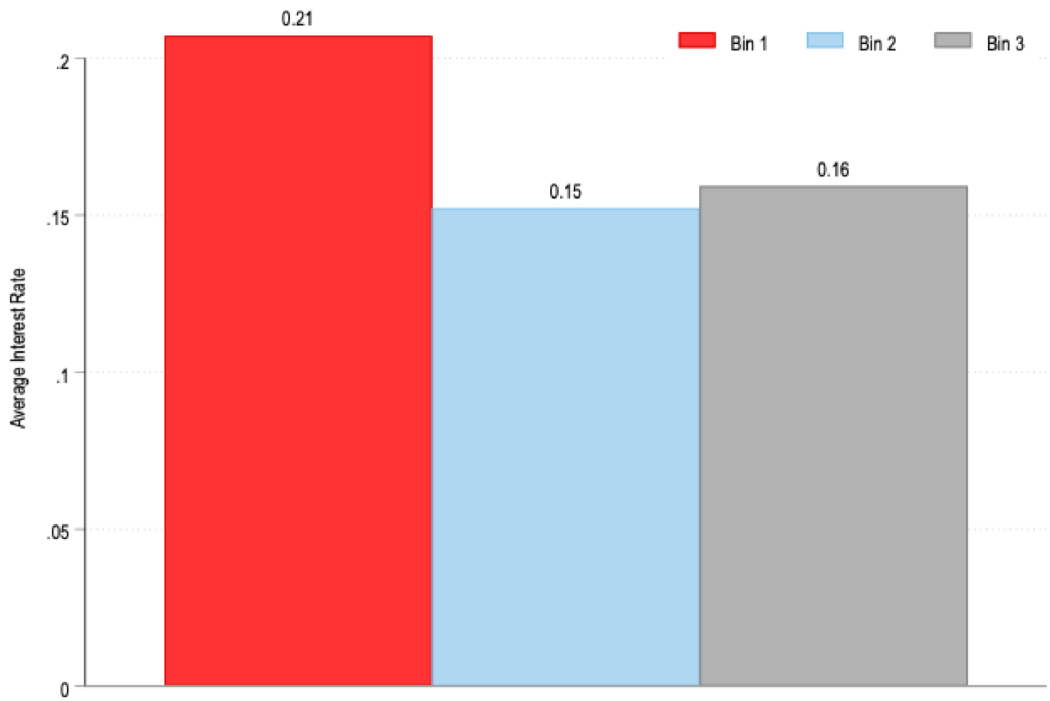


Panel B: Aggregate Farm Profit



Notes: Lowess plot of potato profit and aggregate farm profit from potato cultivation on productivity presented. Separate lowess plots presented for Treatment and Control1 households in TRAIL and GRAIL villages.

Figure A2: Interest Rate on Informal Loans and Productivity. Control Households Only



Notes: The vertical axis measures the average interest rate paid on informal loans by households. The horizontal axis shows the productivity estimate. In the left panel, we compute the average interest rate for households in each productivity bin. The average interest rate paid on informal loans by households in productivity Bin 1 is significantly higher than that paid by households in productivity Bin 2 ($p-value = 0.03$) and productivity Bin 3 ($p-value = 0.04$). The sample is restricted to Control 1 and Control 2 households in TRAIL and GRAIL villages with at most 1.5 acres of land. Productivity is computed using the logarithm of the acreage under potato cultivation.

Table A1: Baseline Credit Market characteristics

	All Loans (1)		Agricultural Loans (2)	
Household had borrowed	0.67		0.59	
Total Borrowing†	6352	(10421)	5054	(8776)
Proportion of Loans by Source‡				
Traders/Money Lenders	0.63		0.66	
Family and Friends	0.05		0.02	
Cooperatives	0.24		0.25	
Government Banks	0.05		0.05	
MFI and Other Sources	0.03		0.02	
Annualized Interest Rate by Source (percent)				
Traders/Money Lenders	24.93	(20.36)	25.19	(21.47)
Family and Friends	21.28	(14.12)	22.66	(16.50)
Cooperatives	15.51	(3.83)	15.70	(2.97)
Government Banks	11.33	(4.63)	11.87	(4.57)
MFI and Other Sources	37.26	(21.64)	34.38	(25.79)
Duration by Source (days)				
Traders/Money Lenders	125.08	(34.05)	122.80	(22.43)
Family and Friends	164.08	(97.40)	183.70	(104.25)
Cooperatives	323.34	(90.97)	327.25	(87.74)
Government Banks	271.86	(121.04)	324.67	(91.49)
MFI and Other Sources	238.03	(144.12)	272.80	(128.48)
Proportion of Loans Collateralized by Source				
Traders/Money Lenders	0.02		0.01	
Family and Friends	0.04		0.07	
Cooperatives	0.79		0.78	
Government Banks	0.81		0.83	
MFI and Other Sources	0.01		0.01	

Notes: Statistics are reported for all sample households in TRAIL and GRAIL villages with at most 1.5 acres of land. All characteristics are 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. To arrive at representative estimates for the study area, Treatment and Control 1 households are assigned a weight of $\frac{30}{N}$ and Control 2 households are assigned a weight of $\frac{N-30}{N}$, were N is the total number of households in their village. †: 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 borrowed. Standard deviations are in parentheses.