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DECENTRALIZED TARGETING OF AGRICULTURAL CREDIT PROGRAMS: PRIVATE VERSUS POLITICAL INTERMEDIARIES

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Dilip Mookherjee - dilipm@bu.edu
Boston University and CEPR

Pushkar Maitra - pushkar.maitra@monash.edu
Monash University

Sandip Mitra - sandipisi@gmail.com
 Indian Statistical Institute

Sujata Visaria - svisaria@ust.hk
Hong Kong University of Science and Technology

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

Pushkar Maitra[†], Sandip Mitra[‡], Dilip Mookherjee[§], Sujata Visaria[¶]

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Abstract

We compare two different methods of appointing a local commission agent as an intermediary for a credit program. In the Trader-Agent Intermediated Lending Scheme (TRAIL), the agent was a randomly selected established private trader, while in the Gram Panchayat-Agent Intermediated-Lending Scheme (GRAIL), he was randomly chosen from nominations by the elected village council. More TRAIL loans were taken up, but repayment rates were similar, and TRAIL loans had larger average impacts on borrowers' farm incomes. The majority of this difference in impacts is due to differences in treatment effects *conditional* on farmer productivity, rather than differences in borrower selection patterns. The findings can be explained by a model where TRAIL agents increased their middleman profits by helping more able treated borrowers reduce their unit costs and increase output. In contrast, for political reasons GRAIL agents monitored the less able treated borrowers and reduced their default risk.

Key Words: Targeting, Intermediation, Decentralization, Community Driven Development, Agricultural Credit, Networks.

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

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

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

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

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

1 Introduction

Public programs in developing countries are increasingly being implemented at the local level. Communities are monitoring service providers, and local governments are delivering development and welfare programs. Despite the promise that decentralization can improve targeting and implementation through better information and accountability, a growing literature argues that these programs have a mixed record.[1] Community-led programs are often captured by the local elite (see, for example World Development Report 2004; Mansuri and Rao 2013; Vera-Cossio 2018; Deserranno et al. 2018), and there is evidence that local governments target their vote bank, rather than those who stand to benefit the most (Stokes 2005; Robinson and Verdier 2013; Bardhan et al. 2015; Bardhan and Mookherjee 2016; Devarajan and Khemani 2016; Dey and Sen 2016).

In this paper we present evidence on a new alternative designed in the context of an agricultural credit program in West Bengal, India. The program offered subsidized individual liability loans to smallholder farmers. Our approach, called Agent Intermediated Lending (AIL), delegates the selection of beneficiaries to an intermediary chosen from within the community. It sought to leverage the intermediary's specialized information and connections with local residents, while avoiding the pitfalls associated with elite capture.

During 2010–2013 we collaborated with a microfinance institution that implemented the AIL scheme in 48 randomly chosen villages in two potato-growing districts of West Bengal, India. We experimented with two different versions of the AIL scheme.[2] In the 24 villages randomly assigned to the Trader-Agent Intermediated Lending (TRAIL) treatment, the agent was randomly chosen from a list of local private traders with a track record of lending to, and selling and buying from farmers in the village. In the remaining 24 villages assigned to the Gram Panchayat Agent Intermediated Lending (GRAIL) treatment, the agent was randomly selected from a list of local individuals provided by the elected village council in GRAIL. The agents then recommended village households for individual liability loans designed to finance the cultivation of the local

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[1]In her survey of community-driven development (CDD) programs around the world, Casey (2018) argues that although CDDs have successfully provided public goods in countries with weak states (for example, Sierra Leone, Indonesia, Afghanistan and Sudan), they have generally failed to transform local decision-making or increase empowerment.

[2]In a different set of 24 randomly chosen villages in the same districts, we implemented a group-based lending (GBL) approach, where borrowers self-selected into joint liability groups. Our previous research (Maitra et al. 2017) compares TRAIL with GBL. In the current paper we focus on a comparison of the TRAIL and GRAIL schemes.

cash crop, potatoes. They earned as commission 75 percent of the interest payments made by each borrower they recommended.[3]

The goal of this paper is to evaluate these alternative methods of devolution of beneficiary selection. Specifically, we ask if the programs had positive impacts on farmer cultivation decisions, output and incomes. We examine whether the impacts differ across the two approaches, and why. In particular, the agent's identity, networks and motivations could have affected the success of the intervention. Given their experience lending and trading farm inputs and output within the community, the TRAIL agents may be better informed about borrowers' farm productivity and reliability. They could also expect to earn middleman profits from trading the farmers' output, and so may have chosen to direct credit and business advice to their most productive farmer-clients.[4] GRAIL agents, on the other hand, would have likely acted in line with the priorities of their political party, for example, selecting poor beneficiaries to further their party's pro-poor agenda. They may also have been motivated differently vis-a-vis their beneficiaries' projects: they were unlikely to benefit directly if their borrowers had large harvests, and instead could have faced the blame if the borrowers' projects failed and they fell into economic distress. The agents would also likely impose informal sanctions on defaulting borrowers. A TRAIL agent might withhold future business transactions, and the GRAIL agent might apply social or political pressure.

Our experimental design allows us to separate the effect of receiving an AIL loan from the confound caused by endogenous borrower selection. In each village, the agent was asked to recommend 30 village residents as potential borrowers, but only 10 randomly chosen from this list were offered the AIL loan. Among the recommended, a comparison of those who were offered the loan with those who were not, provides an estimate of the treatment effect of the loan, conditional on selection. Among those who were not offered the loans, the underlying differences between those who were and were not recommended allow us to separately estimate the selection effect.

Our estimates show that TRAIL loans were more likely to be taken up than GRAIL loans. Among the loans taken up, repayment rates were a similarly high 93 percent in both schemes. In both schemes, beneficiaries borrowed more, cultivated more area and had larger potato harvests. However, potato profits and farm incomes only increased for

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[3]See Fuentes (1996), Varghese (2005) and Mansuri (2007) for theoretical arguments about credit delivery using local agents and bank-money-lender linkages in developing countries.

[4]The potato value chain in this region is characterized by large middlemen margins: Mitra et al. (2018) estimate that in 2008 farmgate prices were 45% of wholesale prices, with middlemen earning at least 50-70% of this gap.

beneficiaries in the TRAIL scheme (by 40 percent and 21 percent, respectively).[5] This is because TRAIL farmers' expansion of cultivation was accompanied by a reduction in unit production costs, whereas GRAIL farmers continued to produce at previous (high) costs per acre.

We develop a simple model with a decreasing returns Cobb-Douglas production function where farmers vary in a single productivity attribute, and at baseline, the allocation of informal credit is efficient. We use this model to estimate farmer productivity, and find that the TRAIL agent selected more productive farmers than the GRAIL agent did. And yet, this selection difference accounts for less than 10 percent of the TRAIL–GRAIL difference in the average treatment effects on farm income. We show that alternative explanations such as superior selection on other farmer attributes (such as wealth or unit costs), and credit rationing cannot explain why the TRAIL scheme outperforms the GRAIL. Instead, our analysis indicates that most of the difference can be attributed to the TRAIL scheme generating larger treatment effects on farm income, *conditional* on farmer productivity. Specifically, our findings suggest that a beneficiary in the TRAIL scheme produced more output at significantly lower per-unit cost and earned larger profit, than a GRAIL beneficiary of the same productivity. Since the loan product and hence borrower incentives were identical in the two schemes, this suggests that the key difference was the borrower's relationship with their agent.

Accordingly, we extend our simple model of selection on a single productivity dimension to incorporate interactions between agents and borrowers. Since TRAIL agents are also middlemen in the potato trade, they are motivated to increase their profits by helping their clients produce and sell more(Mitra et al. 2018). In our model, they advise farmers on ways to lower unit costs of production, inducing them to cultivate more potatoes, and earn larger profits per kilogram. This advice is most effective for the most able farmers. On the other hand, the GRAIL agent is motivated by the objectives of the political party controlling the village council. His primary goal is to minimize loan defaults, and accordingly he may intensively monitor poor, less able borrowers and induce them undertake actions and expenditures that ensure crop success, but also lower expected profits. As a result GRAIL borrowers would incur higher costs than TRAIL borrower. 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.

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

Our paper contributes to the recent literature that examines the role that community agents play in development interventions. Several recent papers find that central individuals in the network can help to effectively 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 2018; Beaman et al. 2018; Banerjee et al. 2013; Chandrasekhar et al. 2018). We bring to this literature the observation that villages tend to have a number of different networks: economic, political and social. The nodal agents of different networks may have different information sets and different incentives to deliver development programs. This raises the question of which network to tap into. Further, the intervention itself may affect network relationships, and thereby also affect borrowers' profits. Our results indicate the importance of looking beyond the architecture of local networks, and incorporating the effects of policy treatments on relationships within the network. Our work also sheds light on the specific role that intermediaries play.[6] We provide evidence on interactions between the beneficiary and program intermediary, and quantify the importance of such interactions vis-a-vis beneficiary selection.

Our experiment provides a rare instance of a microcredit experiment that successfully raised borrower production and incomes, while maintaining high repayment rates and take-up. In a previous paper (Maitra et al. 2017), we found that the TRAIL scheme also outperformed a traditional group-based micro-lending (GBL) scheme. We found that differences in borrower selection accounted for at least 30-40% of the difference in the average treatment effects across the two schemes. Instead, this paper finds that selection differences explain very little of the difference in the outcomes of the TRAIL and GRAIL schemes; accordingly we focus on the role of interactions between the agent and farmers.[7] Interestingly, the large profits that the traders earn as middlemen appear to be the precise reason why their incentives are aligned with increasing farmers' output.

The paper is organized as follows. In Section 2 we describe the two loan intervention schemes that we will analyse in this paper. Section 3 describes our data and Section 4

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[6]For example, Beaman and Magruder (2012) argue that members of social networks can be incentivized to help select and refer high ability workers to jobs. On the other hand, Heath (2018) finds evidence that firms mitigate shirking by workers referred by employees that interact with them socially, relying on these interactions to help weaken the effect of limited liability constraints.

[7]In another related paper (Maitra et al. 2019), we compare the distributive effects of TRAIL, GRAIL and GBL, by assessing their impacts on an Atkinson (1970) welfare function. We find that the TRAIL scheme had significantly positive average treatment effects on welfare that were consistently larger than the average treatment effects of the GRAIL and GBL schemes, irrespective of the degree of inequality aversion.

presents the estimates of the average treatment effects of the two schemes on borrower outcomes. In Section 5 we present statistics on the financial performance of the schemes. To explain our empirical findings, in Section 6 we examine how and why selection patterns varied across the two schemes. We then decompose the difference in average treatment effects into the part that can be explained by these selection differences, and the part that is due to differences in the treatment effects, conditional on borrower productivity. Since we find that this latter part is substantial, in Section 7 we develop a model of agent incentives and agent-farmer interactions and test its predictions. Section 8 concludes.

2 Empirical Context and Intervention Design

Our interventions were conducted across 48 randomly-chosen villages in the Hugli and West Medinipur districts of the Indian state of West Bengal.[8] The loans offered through the AIL schemes were designed to facilitate the cultivation of potatoes, a high return high-cost crop in this region (see Maitra et al. 2017, Table 2).

The 48 villages in our sample were randomly and evenly assigned to the two treatment arms. Our sample villages were at least 8 kilometres apart from one another; this helped to avoid information and other spillovers across the treatment arms. Each village belonged to the jurisdiction of a different village council or *gram panchayat* (GP). Panel A of Table 1 shows that across the villages in the two treatment arms there are no significant differences in village size, or number or landsize distribution of potato cultivators. Our partner MFI had not operated in any of these villages before. In general, when our intervention began in 2010, there was very little microfinance available in this area.

The institutional arrangements in this region are such that potato traders regularly visit farmers in their fields, and often engage in repeated sales and credit transactions with farmers. In each TRAIL village, our project team drew up a list of local traders who had at least 50 clients or had been operating for longer than 3 years. One trader was randomly drawn from this list and offered the position of agent.[9]

In India, gram panchayats are village councils of between 8 and 15 members, formed through direct elections once every five years. In the West Bengal village councils,

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[8]This is a subset of the random sample of villages where Mitra et al. (2018) conducted a potato price information intervention experiment in 2007–08.

[9]If this person had refused to participate, we would have offered the position to a second randomly chosen trader from the list. In practice the first trader approached always accepted the contract.

candidates tend to be affiliated with state-level political parties.[10] Many government schemes are implemented locally by village councils, and local party affiliates are often involved in both identifying beneficiaries and delivering benefits.[11]

In the villages assigned to the GRAIL treatment, we asked the village council to suggest as potential GRAIL agents individuals who had lived in the village for at least 3 years, were personally familiar with village farmers, and had a good local reputation. Given the background we described above, we expected the GP to recommend political affiliates experienced at delivering government benefits. One randomly drawn person from this list was offered the contract.[12]

In each intervention arm, the agent was asked to recommend 30 potential borrowers, from the land-poor households in the village.[13] Ten of these 30 recommended individuals were randomly chosen in a lottery that we conducted in the office of the local government.[14] The MFI then approached the selected individuals in their homes and offered them the loans.

Our partner MFI was limited to disbursing hte program loans and collecting repayment. It did not select borrowers or monitor them subsequently. The loans were funded through an external grant held by the principal investigators of this project.

The first loan cycle began with disbursals in October–November 2010, to coincide with the planting season for potatoes. Borrowers were individually liable for the repayment of their loans. The scheme featured "progressive lending" to generate dynamic repayment incentives. In cycle 1 the loan size was capped at Rs. 2000 (equivalent to US $30), repayable as a single lumpsum at the end of four months, at 6 percent interest. The loan size in each subsequent cycle was 133% of the principal amount repaid. Borrowers who repaid less than 50% of principal in any cycle, were terminated. We did not

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[10]West Bengal has a long history of cadre-based mass mobilization of voters through political rallies and campaigns. From 1977 to 2011, the Communist Party of India (Marxist) led a left-wing coalition government in West Bengal state, and held the majority of the village council seats. This long dominance ended when the Trinamool Congress captured the majority of state assembly seats in the 2011 state elections. After the 2013 village council elections most village councils also had a Trinamool Congress majority.

[11]There is considerable evidence that local governments in West Bengal direct benefits towards swing voters (Bardhan et al. 2015; Dey and Sen 2016).

[12]Only 1 of the 24 individuals selected in this way refused the position, citing a religious taboo on participating in a credit scheme. He was replaced by a second randomly drawn person from the list.

[13]Specifically, we required that recommended households owned no more than 1.5 acres of cultivable land.

[14]The list of recommended borrowers was kept confidential so as to avoid any spillovers on informal credit access or other relationships for households that had been recommended but were not eventually chosen to receive the loan.

want to create any pressure for borrowers to sell their harvest prematurely to meet their repayment obligation; therefore borrowers could also repay in potato "bonds"; we then calculated the rupee value of the repayment at the prevailing price of potato bonds.[15] Although the stated purpose of the loans was agricultural, we did not monitor or restrict how households spent the funds.[16]

Agents had both monetary and non-monetary incentives to participate in the scheme. At the end of each loan cycle, they received from the MFI a commission equal to 75% of the interest paid by all borrowers whom they had recommended. This high commission rate was meant to incentivize the agent to select productive borrowers who would repay the loan and benefit from it, and to discourage collusion between the agent and potential applicants.[17] In addition, the agent put down a deposit of Rs. 50 per borrower, which was refunded if his borrower survived in the program for two years. If more than one-half of the borrowers recommended by an agent defaulted on their loans, the agent was terminated and earned no further commissions. All agents who survived in the program for 2 years also received a paid holiday to a seaside resort. In conversations during our field visits, some TRAIL agents remarked that they expected the scheme to increase their prominence in the village, or to boost their business. GRAIL agents may also have viewed the scheme as an extension of government anti-poverty programs, or expected the public to view their political party more favorably because of association with the scheme.

3   Data and Selected Descriptive Statistics

We conducted surveys with a sample of 50 households per village. These households were selected as follows. In each village, all 10 households assigned to receive the loan (Treatment households) were included in the sample. Of the 20 households that the agent recommended but did not receive the loan, we included a random subset of 10 households; they are referred to as Control 1 households. We also included 30 households randomly chosen from the pool of the non-recommended, hereafter referred to as Control 2 households.

The first round of surveys was conducted in December 2010, two months after the

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

[16]Survey respondents reported to us their actual use of loan funds in our detailed four-monthly household surveys.

[17]Maitra et al. (2017) presents a model of borrower selection that illustrates this mechanism.

Cycle 1 loans were disbursed. Surveys were repeated every four months. In each sample household, we ensured that the same person responded to the survey in each round. There was no attrition in the sample over the eight survey rounds. In Panel B of Table 1, we present summary statistics of selected household-level characteristics for this sample. These household characteristics do not jointly explain assignment to treatment ($p = 0.996$).

Table 2 compares the characteristics of the TRAIL and GRAIL agents. Over 95% of the TRAIL agents were business persons and reported owning a shop or business. GRAIL agents were either farmers, or had a salaried government job. TRAIL agents were wealthier and had higher incomes, but were less likely to have studied beyond primary school. On the other hand GRAIL agents were 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.

Table 3 presents information about the relationships between agents and village residents. Both types of agents were well-known in the village. More than 90% of the non-recommended Control 2 households said they knew the agent, and 98% of these 90 percent said they saw him at least once a week. However Control 2 households were less likely to have close social ties with the agent, as depicted by belonging to the same caste or religious category, or visiting the agent's house on special occasions.

The main distinction between TRAIL and GRAIL agents is in their economic links with sample households. Nearly a fifth of Control 2 households and a quarter of Control 1 households in TRAIL villages reported that the agent was an important source of credit or inputs, or an important buyer of their crop output. The GRAIL agent was significantly less likely to fill these roles.[18] In survey round 1 in December 2010, we also asked households whether they had transacted with the agent in the product, credit or labour markets at any point in the previous three years. A third or more of TRAIL households had bought from the agent, and a tenth to a quarter had borrowed or worked for the agent. Households in GRAIL villages were less likely to have engaged with their agent in this way. This suggests that the TRAIL agents were better informed about the cultivation and borrowing activities of village residents.

Table A1 in the Appendix summarizes the characteristics of the loans that sample households held at the time that our intervention began. Two-thirds of the households

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[18]We note however that about 10% of sample households in both treatment arms reported that they had worked for the agent.

reported that they had an outstanding loan, the bulk of which were for production purposes. Of the agricultural borrowing, nearly two-thirds was from traders or moneylenders, for a mean duration of 4 months and an average annual interest rate of 25%. Credit cooperatives accounted for nearly one quarter of the loans, but loans from commercial banks and from microfinance institutions were rare. Cooperatives and commercial banks charged much lower interest rates, but generally required collateral, and so were inaccessible to poor households.

4   Average Treatment Effects of the Schemes

We start by examining the impacts of the TRAIL and GRAIL schemes on borrower outcomes. Since only a random subset of the recommended household were offered the loans, the average difference in the outcomes of the Treatment and Control 1 households is an estimate of the treatment effect of the loan, conditional on the household being selected into the scheme. The average difference in the outcomes of the Control 1 and Control 2 households is an estimate of the selection effect. Our regression specification takes the form:

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

Here y_{ivt} denotes the outcome variable of interest for household i in village v at time t. The omitted category is the Control 2 households in GRAIL villages. The average treatment effects of the TRAIL and GRAIL schemes are estimated by $\hat{\beta}_3 - \hat{\beta}_2$ and $\hat{\beta}_5 - \hat{\beta}_4$ respectively.[19] The coefficient $\hat{\beta}_2$ is the difference in outcomes of Control 1 and Control 2 borrowers in TRAIL villages, and so represents the TRAIL selection effect; analogously, $\hat{\beta}_5$ measures the GRAIL selection effect. The set $\mathbf{X}_{iv}$ contains measures of the household's landholding, religion and caste, and the age, education and occupation of the oldest male in the household.[20] $I(\text{Year}_t)$ denotes two year dummies to control for

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[19]All treatment effects are intent-to-treat estimates because they compare the outcomes of households *assigned* to the Treatment and Control 1 groups, regardless of whether the Treatment households actually took the loan.

[20] In our study the administrative definition of a village corresponds to a collection of hamlets or *paras*. Households within the same hamlet tend to be more homogenous, are more likely to interact with each other, and arguably experience correlated shocks to cultivation and market prices. The results are

secular changes over time.[21] Standard errors are clustered at the hamlet level.

4.1 Treatment Effects on Agricultural Borrowing

Columns 1 and 2 of Table 4 present the average treatment effects on agricultural borrowing. The TRAIL scheme increased total agricultural borrowing for selected households by Rs. 2868 (or 137%) over the three-year study period, which is statistically significant at the 1% level. The GRAIL scheme caused a similar, significant, increase of Rs. 2754 (or 143%). The point estimates on non-program agricultural borrowing are small and statistically insignificant, suggesting that the TRAIL and GRAIL loans did not crowd out agricultural loans from other sources. Thus Treatment households took the additional subsidized credit, but did not substitute away from their more expensive pre-existing informal loans, possibly because they wanted to sustain their traditional informal credit relationships.

4.2 Treatment Effects on Farm Incomes

Columns 3, 4 and 5 of Table 4 present the estimates of average treatment effects on aggregate farm and non-farm income. In column 3, farm value-added is computed as the sum of the value-added for the four major crops grown in this region: potatoes, paddy, sesame and vegetables.[22] The TRAIL loans caused a statistically significant 21% increase in average farm value-added, whereas GRAIL loans had a non-significant impact, estimated at 1.3%. In column 4, non-agricultural income is calculated as the sum of rental, sales, labour and business income. The average treatment effect estimates for non-agricultural income are imprecise, possibly because these incomes are measured with greater error. Nevertheless, column 4 indicates that neither loan scheme increased non-agricultural incomes significantly. Column 5 shows that aggregate income increased by 11.3% in the TRAIL scheme, and decreased by 10% in the GRAIL scheme; this difference is statistically significant at the 10% level.

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robust to clustering at the village-level instead. See Panel B of Table A3.

[21]In Panel A of Table A3 we present the results from running equation (1) without the set of controls $\mathbf{X}_{iv}$. The key effects are qualitatively similar, although less precise.

[22]The separate results for paddy, sesame and vegetables are available on request.

4.3 Treatment Effects on Potato Cultivation

Recall that the AIL loans were designed specifically to enable the cultivation of potatoes, the highest-value cash crop in the region. Table 5 shows that the TRAIL loans led to large and statistically significant increases in potato cultivation.[23] The effect is concentrated on the intensive margin: although the TRAIL Treatment households were just as likely to cultivate potatoes as the Control 1 households (column 1), they planted an additional 0.09 acres (or 27%, column 2), harvested 950 kg (or 26%, column 3) more, and earned an additional Rs. 3900 (or 27%, column 5) in sales revenue.[24] Simultaneously they incurred an additional Rs. 1846 (or 22%, column 4) in production costs, so that on net, value-added increased by Rs. 2060 (or 36%, column 6). When we subtract the imputed cost of family labor, this works out to a statistically significant Rs. 1907 or 40% increase in profit (column 7).[25]

The households that the GRAIL agent recommended were less likely to cultivate potatoes in the absence of the loan: potatoes were planted in only 64% of Control 1 household-years in GRAIL villages, unlike the 72% in TRAIL villages. However, the loans increased the likelihood that a GRAIL borrower cultivated potatoes by 13% points (or 20%, Column 1, $p < 0.001$). Correspondingly, acreage (column 2) and output (column 3) increased significantly (23% and 24%, respectively). Revenue increased by 19% (column 5), but since the cost of production increased by a larger 28% (column 4), the GRAIL loans had a non-significant effect on value-added and imputed profit.[26] The difference in the average treatment effects on imputed profits (column 7) across the two schemes is statistically significant at 10%.

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[23]We aggregate the data from the four-monthly surveys into a household-year level dataset capturing the amount of land households planted with potatoes, harvested quantities, sales, revenues, production costs, value-added and imputed profits. This allows us to align the costs of cultivation with the revenues, since harvested potatoes can be stored and then sold at different points in the year.

[24]Households reported to us the quantity and price of each potato sale. When they held potatoes for self-consumption, we imputed the sales revenue by pricing the potatoes at the median sale price in the village.

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

[26]Since we analyse a large number of dependent variables, we correct for the increased chance of finding statistically significant results. Following Hochberg (1988), we report a conservative p-value for an index of variables in a family of outcomes taken together (see Kling et al. 2007). The variables are normalized by subtracting the mean in the control group and dividing by the standard deviation in the control group; the index is the simple average of the normalized variables. To adjust the p-value of the treatment effect for an index, the p-values for all indices are ranked in increasing order, and then each original p-value is multiplied by $m - k + 1$, where m is the number of indices and k is the rank of the original p-value. If the resulting value is greater than 1, we assign an adjusted p-value of > 0.999.

Thus, while both schemes led borrowers to plant more potatoes, only TRAIL borrowers earned larger profits as a result. This is mainly because they lowered their unit costs by 6%, whereas GRAIL borrowers' unit costs increased by 2%. The difference is statistically significant $(p < 0.05)$.[27]

5 Loan Performance

In Table 6 we examine the take-up and repayment rates for the program loans. In column 1, the dependent variable is the likelihood that a household eligible to take the loan in a given cycle actually took the loan.[28] The sample means in Panel A show that eligible TRAIL Treatment households accepted 94% of the program loans offered, whereas eligible GRAIL Treatment households took 87% of the loan offers. The difference is statistically significant at the 1% level. This result holds $(p < 0.01)$ even in Panel B when we control for household characteristics following the regression specification:

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

where X_{ivt} includes the landholding, religion, caste, and the age and educational attainment of the oldest male member of the household, and a vector of loan cycle dummies to control for seasonal or other time-varying changes in take-up and repayment rates.

We define a loan to be in default if the repayment amount due was not paid fully by the due date. The average default rate over the three-year intervention period is an identically low 7% in both schemes (Table 6, Panel A, column 2). The regression result in Panel B confirms that there is no difference in the loan default rates in the two schemes. Despite the fact that the scheme did not increase their incomes significantly, GRAIL Treatment households repaid their loans at the same high rates as TRAIL Treatment households.

6 Selection-based Explanation

Next we examine the mechanisms that underlie our empirical findings. We start by examining how much of the difference in the average treatment effects in the two schemes

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[27]TRAIL borrowers also faced a smaller decline in output price (0.6% instead of 3.6%), but this difference is not statistically significant $(p = 0.14)$.

[28]In loan cycles 2 and beyond, a Treatment household was only eligible to borrow if it had repaid at least 50% of the amount previously outstanding.

can be explained by selection differences. This requires us to estimate a measure of the underlying heterogeneity among farmers, based on the following simple model. The key assumptions are that (a) farmers are heterogenous in ability, (b) more able farmers have higher TFP and lower unit costs of production, (c) the production function follows the Cobb-Douglas form and exhibits decreasing returns to scale, and (d) the informal credit market is efficient because lenders have perfect information about borrower ability and compete in Bertrand fashion. In particular, borrowers are not credit-rationed. The program loans provide them an extra line of credit at an interest rate below the prevailing market rate, and induce them to expand borrowing and cultivation scale.

6.1   Estimation of Ability: Control Households

Consider farmer i in village v in year t who produces according to the function:

$$Y_{ivt} = P_{vt} A_i \frac{1}{1-\alpha} l_{ivt}^{1-\alpha} \tag{3}$$

P_{vt} denotes a village-level yield or price shock, l_{ivt} is the farmer's chosen scale of cultivation, and $\alpha \in (0,1)$. Farmer ability A_i follows a common, given, distribution both in GRAIL and in TRAIL villages. The cost of production per unit area c_i is constant for any given farmer, and decreases in farmer ability. Assume for now that P_{vt}, A_i and c_i are unaffected by the treatment.[29]

A farmer in village v, year t who does not receive a program loan (henceforth, a control farmer) borrows from informal lenders. All informal lenders face the cost of capital ρ_{vt}.[30] Since these lenders compete in the Bertrand fashion, the farmer pays expected interest cost ρ_{vt}.

To cultivate potatoes, the farmer must incur a fixed cost $F > 0$. Accordingly, the control farmer chooses $l = l_{ivt}^c$ to maximize

$$P_{vt} A_i \frac{l^{1-\alpha}}{1-\alpha} - \rho_{vt} c_i l - F$$

---

[29] The current model assumes that farmer productivity and costs are correlated with a measure of exogenous ability. This is a special case of the model in our previous paper (Maitra et al. 2017). In Section 7 we allow the trader and agent to endogenously determine the extent to which they help and monitor the farmers, and these in turn affect the values of A_i and c_i.

[30] In the presence of default risk, the informal interest rate also depends on, and is decreasing in, the farmer's ability, so that the expected interest cost of the borrower is ρ_{vt}.

For control farmers with a sufficiently high ability, this yields

$$\log l_{ivt}^c = \frac{1}{\alpha} \log \frac{A_i}{c_i} + \frac{1}{\alpha}[P_{vt} - \rho_{vt}] \tag{4}$$

Observe that $\log \frac{A_i}{c_i}$, or the ratio of TFP to per-unit cost, is increasing in farmer ability. This serves as an exogenous measure of relative productivity. In equation 4 this ratio is scaled relative to α; if α is close to 1 then it is also an absolute measure of the ratio of TFP to per-unit cost. Accordingly, among control farmers, productivity 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.

Equation (4) applies only to farmers with ability above the threshold $\underline{\theta}_{vt}$. These farmers eventually earn profits that cover the fixed cost F. Farmers with ability below this threshold 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. We call these households non-cultivators, and assign to them 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 7 we present the results from regressing each cultivator household's productivity estimate, obtained from the household fixed effects regression following equation (4), on household characteristics.[31] As the bottom panel of Table 7 shows, there is wide variation in the estimated productivity level across households: the 75th percentile is more than three times the size of the 25th percentile. Although the productivity estimate correlates positively with landholding and having a male household head, the R-squared indicates that variation in observable characteristics can only explain 16% of the variation in household productivity.[32] This justifies our working hypothesis that community-based agents have local information about farmers that an outsider would

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[31]Table A2 presents the differences in demographic characteristics between the cultivators and non-cultivators. Cultivators own more land, are more likely to be upper caste Hindus, have larger households, are more likely to be male-headed, and the oldest male in the household is likely to be older and is more likely to have completed primary school.

[32]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.

be unable to observe.

6.2 Estimation of Ability: Treatment Households

We cannot use the same procedure as in Section 6.1 to estimate the productivity of treated households, because the program loan would have a direct impact on treated households' scale of cultivation. Also, treated households could have engaged differently with their agent than control households. Instead, to recover their productivity estimate, we employ the *Order-Preserving Assumption (OPA)*, which states that the treatment did not change households' rank order in terms of productivity levels. In Section 7.1 we will provide theoretical justification for this assumption.[33] Under this assumption, in each treatment arm, we can rank Treatment farmers by cultivation scale, and assign to them the counterfactual productivity estimate A_i of the farmer in the Control 1 distribution at the same rank, as estimated in Section 6.1. This gives us the correct estimate for the latent productivity of this farmer.

6.3 Differences in 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; neither group received program loans.

In Panel A of Figure 1, we compare the distributions of the productivity estimates for Control 1 and Control 2 households.[34] 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 ($p = 0.005$).[35] 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 $p = 0.011$).[36]

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

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

[35]Since our productivity estimates are generated variables, we also simulate 2000 bootstrap samples and run the Kolmogorov-Smirnov test for each Control 1 vs Control 2 CDF comparison. In 87 percent of the simulations, we can reject the null hypothesis that the two distributions are identical.

[36]The Kolmogorov-Smirnov test rejects the null hypothesis that the two distributions are identical in 83 percent of our bootstrap simulations.

When in Panel B we again present the cumulative distribution functions for Control 1 households, we see that the graph for TRAIL households first-order stochastically dominates that for GRAIL households. A two-sample Kolmogorov-Smirnov test rejects the null hypothesis that the two distributions are identical ($p = 0.06$).[37] 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. Figure A2 shows that the GRAIL agent selected more Bin 1 borrowers (34.5% versus 27.3%), and fewer Bin 3 borrowers (32.3% versus 39.7%).[38]

6.4   Explaining Selection Differences

What explains this difference in selection patterns? The TRAIL agent's experience lending and trading with village residents could have given him more or better information about their productivity levels. Alternatively, he might have had different incentives than the GRAIL agent. For example, the prospect of increasing his middleman profits might incentivize him to help farmers increase their crop volumes. Conversely, the GRAIL agent would likely share the motives of the political party controlling the village council. If this party pursues a pro-poor welfarist ideology, this might motivate the GRAIL agent to select asset-poor (and possibly) low-ability farmers. Alternatively, if the party is opportunistic and uses welfare programs clientelistically to mobilize votes, then too the GRAIL agent would recommend poor households for the loan scheme.[39]

We examine whether GRAIL agents used the loan recommendations for political gains. In our final survey round, we asked survey respondents to participate in a straw

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[37]The Kolmogorov-Smirnov test rejects the null hypothesis that the two distributions are identical in 74 percent of our 2000 bootstrap simulations. Figure A1, in the Appendix, presents descriptive statistics on household productivity for TRAIL and GRAIL Control 1 households. TRAIL households had higher mean and maximum productivity estimates; the 25th, 50th and 75th percentiles or the distribution of productivity were also higher for TRAIL Control 1 households.

[38]The Bin 3 borrowers here could be thought of as equivalent to the *gung-ho* (GE) entrepreneurs in Banerjee et al. (2019). Indeed, the Bin 1 borrowers are akin to the non-GE entrepreneurs in that they do not cultivate potatoes absent the AIL loan. However, while Banerjee et al. (2019) argue that the overall differences in later outcomes between GE and non-GE entrepreneurs are driven by selection, we find in this paper that TRAIL and GRAIL borrowers have different outcomes *conditional* on selection, and that this difference is caused by differences in the agents' non-program incentives. See Section 6.5.

[39]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).

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.[40] If 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:

$$I_{iv} = \xi_0 + \xi_1 \text{Treatment}_{iv} + \xi_2 \text{Control } 1_{iv} + \gamma \mathbf{X}_{iv} + \varepsilon_{iv} \qquad (5)$$

where the dependent variable I_{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 8 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 selected to receive the program loan were 8% more likely to express support for the incumbent party than comparable households that were not offered the loan. Column 4 shows that the point estimate of the treatment effect is largest for the lowest-ability GRAIL households. 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 incumbent following a pro-poor welfarist policy and thereby inducing greater gratitude among poor beneficiaries, that they express by voting for the party.

6.5 The Role of Selection in Explaining ATE Differences

We have seen that the TRAIL agent selected more productive borrowers than the GRAIL agent did. However it does not follow that this superior selection alone caused the larger impacts on borrower outcomes. In what follows, 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 will decompose the difference in the ATEs of the TRAIL and GRAIL schemes into the component attributable to differences in productivity, and

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

the component due to differences in conditional treatment effects.

In Table 9 we estimate bin-specific heterogenous treatment effects (HTEs) using the following specification:

$$
y_{ivt} = \sum_{k=1}^{3} \xi_{1k} \, \widehat{\text{Bin}}_{ik} + \sum_{i=1}^{3} \xi_{2k} \left(\text{Control } 1_{iv} \times \widehat{\text{Bin}}_{ik} \right) + \sum_{k=1}^{3} \xi_{3k} \left(\text{Treatment}_{iv} \times \widehat{\text{Bin}}_{ik} \right)
$$

$$
+ \sum_{k=1}^{3} \xi_{4k} \, \widehat{\text{Bin}}_{ik} \times \text{GRAIL}_{v} + \sum_{k=1}^{3} \xi_{5k} \left(\text{Control } 1_{iv} \times \widehat{\text{Bin}}_{ik} \times \text{GRAIL}_{v} \right) \qquad (6)
$$

$$
+ \sum_{k=1}^{3} \xi_{6k} \left(\text{Treatment}_{iv} \times \widehat{\text{Bin}}_{ik} \times \text{GRAIL}_{v} \right) + \gamma \mathbf{X}'_{ivt} + \varepsilon_{ivt}
$$

Since household productivity is an estimated regressor, we bootstrap standard errors with 2000 iterations.[41] In both schemes, the HTEs on value-added (column 7) are larger for borrowers of higher productivity, which indicates that a scheme that selects more able borrowers will likely generate a larger average treatment effect. Comparing the TRAIL and GRAIL HTEs for any given productivity bin, we also see that the point estimates are larger for the TRAIL scheme, although Panel C indicates that the differences are not statistically significant.[42]

Table 10 shows the results of the decomposition. The penultimate row indicates that holding everything else constant, if the TRAIL agents had followed the GRAIL agents' selection patterns, then the average treatment effect of the TRAIL scheme on potato value-added would have been 8.38% smaller than our actual estimate. The last row shows that if the GRAIL agents had kept their own selection pattern but their selected borrowers had had the same HTEs as the TRAIL borrowers in that productivity bin, then the gap in the ATEs would have narrowed by 75%.

We infer that selection differences accounted for only a small fraction of the observed ATE difference. This conclusion is robust to using the continuous measure of productivity rather than the three discrete bins: with continuous productivity, selection

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[41] 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.

[42] 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 A3, the vertical difference in (smoothed) value-added between Treatment and Control 1 households gives us a visual estimate of the treatment effect at that productivity level. As the left panel shows, the vertical difference for TRAIL households is larger at larger productivity levels. The right panel shows that the GRAIL treatment effects are nearly always zero.

differences explain 17.5% of the difference in ATEs, whereas differences in HTEs explain 82.5%.

6.6 Alternative Selection-based Explanation: Multidimensional Screening with Credit Rationing

A potential concern with the selection model we described above is that in the presence of credit constraints, wealth levels affect credit access and thereby productivity (Galor and Zeira 1993; Banerjee and Newman 1993; Moll 2014; Kaboski and Townsend 2012; de Mel et al. 2008). If, in reality, borrowers differ both in ability and wealth, then even between two farmers of the same ability, the wealthier farmer would cultivate more. Further, if returns to scale are increasing, then the wealthier farmer would be more productive at the margin (Banerjee et al. 2019). Some part of the larger treatment effect of the TRAIL scheme could then be explained by the fact that the TRAIL agent selected wealthier farmers. If there are pecuniary scale economies, then these farmers' larger scale of cultivation could also explain their lower unit costs. In addition, farmers could differ in the extent to which this happens: lower-cost farmers could face larger reductions in unit cost as they expand their scale of cultivation. If cost and ability are not perfectly correlated, then our sole focus on ability differences might incorrectly attribute too little of the ATE of the TRAIL scheme to selection.

In Section A1 in the Appendix, we present a model where farmers differ on three dimensions: ability, wealth and cost. Assuming constant elasticity forms of the production function and the unit cost function, and assuming also that all farmers are credit rationed, we can use panel data on farmers' unit costs and revenue to back out these elasticities and the three dimensional "type" of each control farmer. Specifically, the production function is

$$\log y_{ivt} = \log a_i + \mu \log l_{ivt} + \delta_{2vt} \tag{7}$$

where a_i denotes ability, l_{ivt} denotes area cultivated, y_{ivt} denotes revenue, δ_{2vt} denotes a village-year productivity shock and μ represents returns to scale. The cost function is

$$\log u_{ivt} = \log c_i + \zeta \log l_{ivt} + \log q_{vt} \tag{8}$$

where u_{ivt} denotes cost per acre, c_i denotes the farmer's cost type, and q_{vt} denotes a village-year cost shock, and ζ represents elasticity of unit cost with respect to the scale of cultivation, representing pecuniary scale effects. The credit-rationed farmer's total

cultivation cost (C_{ivt}) is determined by his financial access according to the equation:

$$\log C_{ivt} = \log w_i + \log \gamma_{vt} \tag{9}$$

where w_i represents a measure of the farmer's wealth, and γ_{vt} is a village-year shock to the supply of credit. The three dimensions of type: ability, cost and wealth can be estimated as farmer fixed effects in the panel regressions corresponding to these three equations. As shown in the Appendix, the model predicts that the treatment effect of expanding credit access by one percent equals the "quasi-profit" $\pi_{ivt}^Q \equiv [\frac{\mu}{1+\nu}R_{ivt} - C_{ivt}]$ of Control 1 household i in village v in year t, where R_{ivt} denotes the revenue earned by farmer i in village v in year t, and C_{ivt} is the cultivation cost, as defined above. Taking expectations of this with respect to unit mean village-year shocks, the predicted treatment effect for a farmer is a function of the three dimensions of the farmer's type, according to the expression:

$$\pi_i^Q = a_i[\frac{w_i}{c_i}]^{\frac{\mu}{1+\zeta}} - \frac{w_i}{(c_i)^{\frac{1}{1+\zeta}}} \tag{10}$$

We refer to the quasi-profit as the "reduced form estimate" of the treatment effect, and to the expression in equation (10) above as the "structural estimate".

To examine if this model can explain our empirical results, we follow equations (7), (8) and (9) and estimate the three dimensions for each control household in the TRAIL and GRAIL villages. The cumulative distribution functions for each of the three types are presented in Figure A4. We see in Panel B that control farmers in TRAIL and GRAIL villages have almost identical ability distributions. In Panel A the cumulative distribution functions for cost type cross, although a larger fraction of TRAIL farmers have low unit costs over a wide range. Similarly, the CDFs for wealth cross, although a larger fraction of TRAIL farmers have high wealth over a wide range.

The corresponding estimates of ζ and μ are presented in Table 11. Row 4 displays "reduced form" estimates of average quasi-profit in the two schemes, using observed revenues and costs of Control 1 subjects, adjusted using the estimated elasticities. This is multiplied by the proportional change (Δ) in the scale of cultivation (Row 2) to generate a "reduced form" estimate of the ATEs predicted, in Row 5. The corresponding "structural estimates" of selected farmers' quasi-profit based on their estimated types is shown in Row 6, and the corresponding predicted percentage effect on potato value-added in Row 7.

We see that the model's reduced form estimate of the TRAIL ATE (Rs. 2732) is very similar to our actual empirical estimate (Rs. 2059, see Table 5 column 7). However for the GRAIL ATE the model produces a vast overestimate (Rs. 3481 v. Rs. 492). Similarly, the structural estimate of the TRAIL scheme's effect on value-added (36.48%) is similar to the 35.92% that we obtained in Table 5, but the estimate for the GRAIL scheme's effect (33.48%) is nearly four times as large as the estimated GRAIL ATE (8.45%).

Rows 8 through 12 show how the predicted ATEs are modified when we replace the estimated returns to scale elasticity (μ) with an IV estimate.[43] The predicted percentage effects on potato value-added are not significantly altered, so the predicted GRAIL ATE continues to be larger than the predicted TRAIL ATE. Hence allowing for credit rationing and extending the dimensions of heterogeneity to include wealth and unit cost does not help explain our empirical result that the TRAIL scheme generated a larger ATE than the GRAIL scheme.

Below we present an alternative model that instead emphasizes differences in the TRAIL and GRAIL outcomes conditional on borrower selection.

7 A Model Of Agent-Farmer Interactions

In this model, TRAIL and GRAIL agents interact with program borrowers so as to influence their cultivation, purchase and sales choices and crop outcomes. Interactions take the form of conversations about the weather, market prices, cultivation techniques and harvesting times. Through these conversations agents might monitor farmers' actions, and provide technical advice and marketing assistance.

Agents are motivated to engage with farmers because of their program as well as non-program incentives. Since the TRAIL agent is also a trader, he has a stake in both the upside and downside of the farmer's cultivation project. Not only does an abundant harvest ensure that he receives his TRAIL commission, it also earns him a larger middleman profit. On the downside, a failed crop wipes out both his commission and his profit. The GRAIL agent is a political appointee and so has incentives aligned with his village council. The elected village leaders may benefit if the scheme is perceived to be a success, and so the GRAIL agent may want to ensure that poor borrowers repay

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[43]This elasticity estimate is discussed in more detail in Section 7. It is obtained from a regression (equation (14) below) of log output per acre on log area cultivated and village year dummies using Treatment and Control households, with treatment dummies as an instrument for area cultivated.

their loans. This gives him a stake in the success of the crop, but conditional on crop success, he has no additional benefit from a large harvest.

In turn these different motivations will shape the specific manner in which the agents engage with the farmer: either helping to increase the harvest size, or monitoring to reduce default risk.

7.1 Assumptions and Predictions

Farmers vary in intrinsic ability, denoted by the single dimension θ. Their crop is successful 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. In the event of success, output is $a(\theta, m)f(l)$, 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.[44] Expected TFP $A(\theta, m) \equiv a(\theta, m)p(\theta, m)$ satisfies $A_m < 0$. The farmer's cultivation cost is $c(h, m)l$, where l is area cultivated, h is the level of help he receives, and $c_h < 0, c_{hh} > 0, c_m > 0$. We also assume $c_{hm} = 0$; this simplifies the analysis but is not critical. Whereas monitoring increases unit costs and lowers risk and expected productivity, help has no effect on risk and lowers unit cost. For instance, monitoring induces the farmer to act to lower the chance of crop failure, such as by spending more on pesticides. These activities are expensive and time consuming, and so the farmer's unit cost rises and productivity fall. On the other hand, the agent can help the farmer lower costs or raise quality of inputs, by providing valuable business advice about the brands to purchase and where to purchase them. We also assume that the relevant functions are smooth with well-behaved curvature, so that optimal allocations are interior, satisfying suitable first and second order conditions.

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 on an external market. The farmer cannot repay the loan if the crop fails, but otherwise repays at interest rate r, and receives a

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[44]This is in comparison to how much TFP a varies with θ. This assumption ensures that treatment effects of the TRAIL scheme increase in θ. See Maitra et al. (2017) for further details.

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:

$$(1 + \tau)A(\theta, m)f(l) - (1 + \rho)c(h, m)l - \gamma_T[m + h] \tag{11}$$

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 $A(\theta, m^c(\theta))f(l) - p(\theta, m^{c(\theta)}(1 + r^c(\theta))c^{c(\theta)}(h, m)l$.[45] The side-payment depends on the relative bargaining power of the trader and the farmer. The details of the model are presented in Section A2 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 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:

(i) *Higher ability farmers are less likely to default, and pay a lower interest rate;*

(ii) *Higher ability farmers produce more output, and incur lower unit costs.*

In TRAIL villages, a trader is selected as the agent. Suppose a farmer of type θ is offered the TRAIL loan. This allows him to supplement his informal loan from the trader at interest rate $r_T < \rho$, and thus expand acreage by l^t. We assume he cannot reduce the area that was already agreed upon before the scheme was introduced, while 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

$$(1+\tau)A(\theta, m)f(l^c(\theta)+l^t) - [(1+\rho)l^c(\theta)+\{1+r_T(1-\psi)\}p(\theta, m)l^t]c(h^t, m^t) - \gamma_T(h^t+m^t) \tag{12}$$

---

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

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:

(iii) *Among treated farmers in the TRAIL scheme, those with higher ability produce more and receive more help, and incur lower unit costs;*

(iv) *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, a third party who is not a trader is appointed as the agent. The GRAIL agent earns expected payoff $v(\theta)p(\theta, m) - \gamma_G m$ where $v(\theta)$ represents a welfare weight on the farmer, weighted by the default rate, and m is the extent to which the agent monitors the farmer. The welfare weight is decreasing in θ, reflecting the pro-poor ideological or opportunistic objectives of the political party the GRAIL agent represents. These objectives are achieved as long as the farmer does not default on the GRAIL loan.[46] The commission enters into the welfare weight. This may bias the GRAIL agent in favor of 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. Not being a trader, the GRAIL agent is not in a position to provide business advice and help to the farmer, but can monitor him. The cost of monitoring of the GRAIL agent is γ_G.

Given a farmer of type θ selected for a GRAIL loan, the GRAIL agent's monitoring level $m^G(\theta)$ will maximize $v(\theta)p(\theta, m) - \gamma_G m$. Given this monitoring level, 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

$$(1 + \tau)A(\theta, m^G(\theta))f(l^c(\theta) + l^g) - [(1 + \rho)l^c(\theta) + p(\theta, m)(1 + r_T)]c(h^g, m^g)l^g - \gamma_G h \quad (13)$$

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 As-

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[46]Here we simplify by assuming that the farmer's profits do not enter the GRAIL agent's objective. More substantively, the GRAIL agent worries about only the downside risk faced by the farmers they select, but does not obtain any benefit if the farmer avoids a default and earns a large profit.

sumption holds in the GRAIL scheme. We obtain the following predictions for GRAIL treated farmers:

(v) *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.*

(vi) *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, than a TRAIL treated farmer of the same ability.*

This model thus explains larger heterogenous treatment effects 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 more able farmers, while the GRAIL agent benefits more from less able farmers.

7.2   Testing Predictions of the Model

Before testing the predictions, we test the underlying assumption of decreasing returns to scale. We regress per acre productivity as a double-log-linear function of log area cultivated, farmer fixed effects and village-year shocks

$$\log[\frac{R_{ivt}}{l_{ivt}}] = \log a_i + (\mu - 1)\log l_{ivt} + \log q_{vt} + e_{ivt} \tag{14}$$

The elasticity $(\mu - 1)$ with respect to area cultivated can be estimated by instrumental variables on the sample of Treatment and Control 1 farmers, with the treatment dummy as the instrument. We have already seen that treatment has a significant positive effect on area cultivated, while the randomization ensures that the exclusion restriction is satisfied. We obtain an IV estimate of $(\mu - 1) = -.094$, which is statistically insignificant

(p-value of the one-sided test of the null hypothesis $(\mu - 1) = 0$ is 0.81). Hence we have weak evidence for decreasing returns to scale.

To test prediction (i) about the variation of control farmers' interest rates with ability, Column 1 of Table 12 presents the OLS regression results of interest rates paid on informal loans by Control 1 households, using the pooled sample of TRAIL and GRAIL households. We restrict our sample to informal loans taken before our intervention began, to avoid potential contamination from the intervention on borrowers' interest rates.[47] The coefficient estimate of productivity is negative $(p = 0.11)$ indicating that for control households, there is a decline in interest rate as productivity increases. 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 $(p = 0.03)$ and the 16% that Bin 3 households reported $(p = 0.04)$.[48] Hence the evidence confirms prediction (i).

Next consider prediction (ii) about how output and unit cost vary with ability among control farmers. Columns 2 and 3 of Table 12 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 pertain to all Control 1 and Control 2 households. Consistent with prediction (ii), column 2 shows that output is increasing in productivity, while column 3 shows that unit costs are decreasing in productivity.

Now turn to prediction (iii) 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 (separately) about 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.[49] In column 4 of Table 12 we see that TRAIL Treatment households' interacted more with the agent if they had higher ability. Columns 5 and 6 of Table 12 present the OLS regressions of quantity of potato cultivated and input cost per acre in potato cultivation on productivity for TRAIL Treatment

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[47]Since only 10 households in each village received the program loans, we do not believe there were any spillover or general equilibrium effects.

[48]These averages are presented in the left panel of Figure A6. The right panel of this Figure uses continuous variation in productivity. Recall that we do not have a continuous measure of productivity for those who did not cultivate potatoes; for this group we compute the mean informal interest rate and plot it as a single point in the right panel of Figure A6. For Bins 2 and 3, we run a locally-weighted polynomial regression of the mean interest rate on farmer productivity.

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

households. Consistent with prediction (*iii*), more productive TRAIL Treatment farmers interacted more with the agent, and produced more potatoes, at a lower input cost per acre.

Table 9 provides evidence about prediction (*iv*). The TRAIL HTEs on potato acreage, potato output and input cost per acre in potato cultivation (see columns 2, 3 and 10 respectively) conform to prediction (*iv*), and most are statistically significant.

Finally, we test the predictions about the GRAIL scheme, and its comparison with the TRAIL scheme. Start with prediction (*v*). Column 11 of Table 9 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 indicating that the GRAIL agent interacts more with low ability farmers. On the other hand they are increasing in ability in the TRAIL scheme, indicating that the TRAIL agent increases his interaction with higher-ability farmers more.

Figure 2 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 ($p = 0.03$). 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 (*v*).

To check prediction (*vi*), consider the results presented in Panel C of Table 9. For every productivity bin, TRAIL treatment effects on acreage, output and value-added exceed the corresponding GRAIL treatment effects (although the differences are not statistically significant): see columns 2, 3 and 7. 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 Bin 3. See column 11. This is consistent with prediction (*vi*).

8 Concluding Comments

This paper finds evidence that appointing private traders as intermediaries to deliver credit, as in the TRAIL scheme, resulted in significant impacts on borrower production and farm incomes. When the local elected council appointed the intermediary, as in the GRAIL scheme, this did not have a significant impact on borrower incomes, though it did increase production. The difference was due to the different impacts on cultivation costs: TRAIL treated households reduced their unit costs of production while the

GRAIL treated households did not. Further, although the TRAIL agent selected more productive farmers to participate in the scheme than the GRAIL agent did, this could only explain very little of the difference in outcomes. Instead, the TRAIL agents' interaction with treated farmers were far more important: they increased their interactions with treated farmers, and to a greater extent with more productive farmers. GRAIL agents also interacted more with treated farmers, but to a greater extent with less productive farmers; this appears to have caused them to default less than their TRAIL counterparts. The results are consistent with a model of endogenous agent-farmer interactions where the TRAIL agent helped treated farmers reduce production costs by providing helpful advice, while the GRAIL agent monitored treated farmers more intensively, thus reducing their default rates and raising production costs. In this model, the GRAIL agent resembles a private holder of debt, motivated primarily to reduce borrower default risk, while the TRAIL agent is like an equity holder who shares risk evenly with the farmer and is motivated to provide useful information and advice. Although the TRAIL agent did not monitor the treated farmers, TRAIL and GRAIL loans still had similar repayment rates, because the TRAIL borrowers were more able farmers to start with, and so were less likely to default. The GRAIL agent selected a larger proportion of low ability farmers with higher ex ante risk, and then monitored them intensively to lower these risks.

The more general lesson is that the role of the intermediary was not confined to selecting beneficiaries and achieving better targeting. The differences in the nature of network connections with the intermediary mattered significantly for the eventual success of the program. In particular the endogenous change in this relationship resulting from the intervention accounts for most of the differences in impacts on beneficiary incomes. Policy interventions that employ local intermediaries could benefit from considering this mechanism at the time of intervention design.

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Figure 1: Selection on Productivity: TRAIL and GRAIL

Panel A: Productivity Distribution
Selected v. Non-selected households
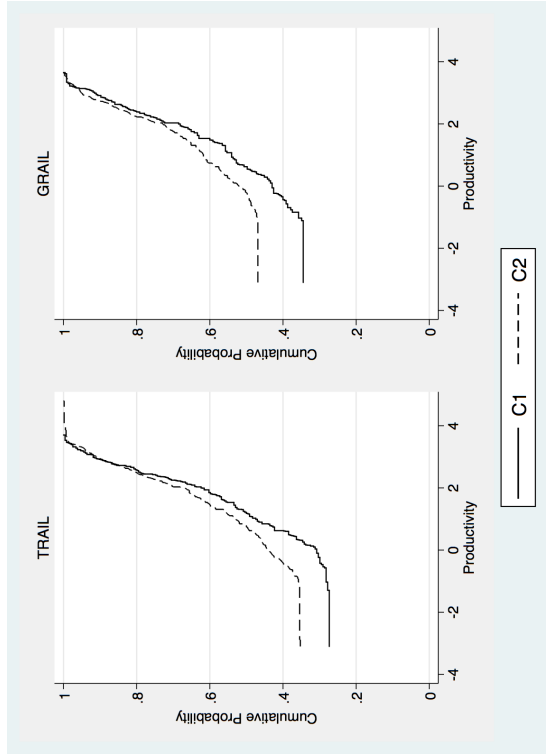
Panel B: Productivity estimates
Selected households
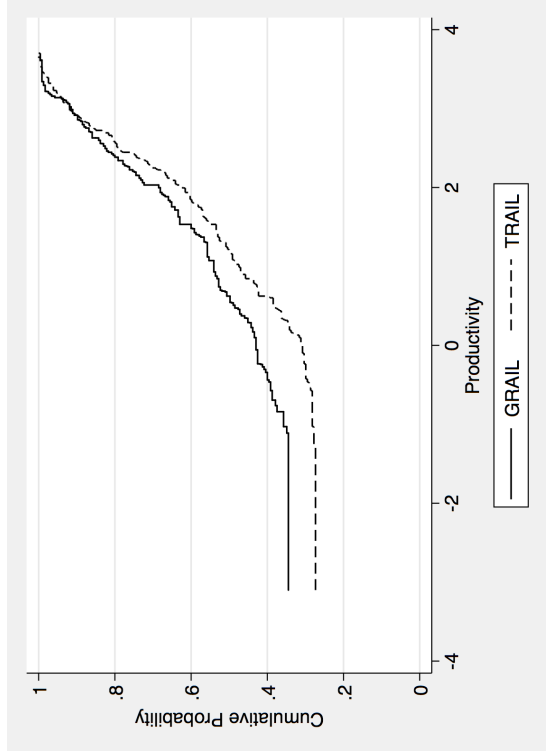
Notes: In Panel A, the sample is restricted to Control 1 and Control 2 households in TRAIL and GRAIL villages. Productivity is computed using the logarithm of acreage under potato cultivation. The null hypothesis that the distributions of estimated productivity for Control 1 and Control 2 households 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 2: Default Rates on TRAIL and GRAIL Loans, by Productivity Bin
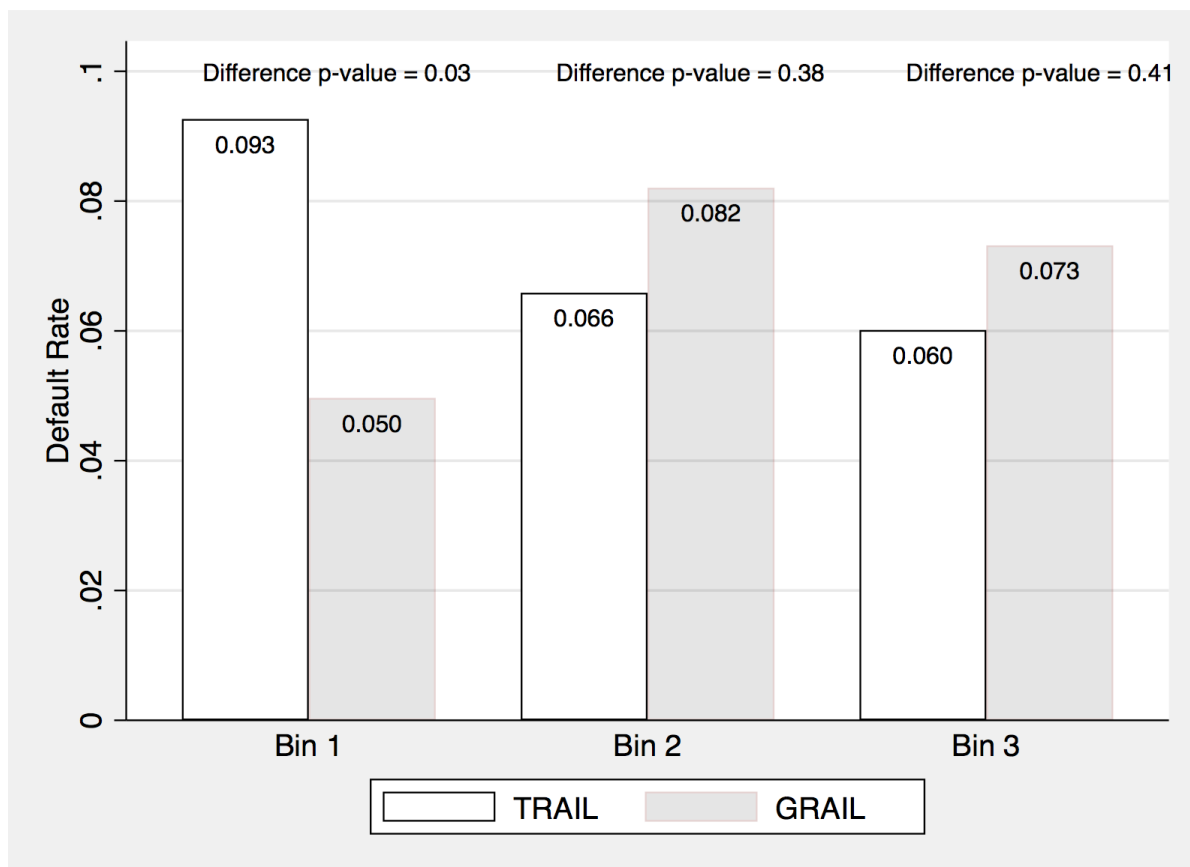
Notes: The height of each bar measures the fraction of program loans that were not repaid fully by the due date. The sample is restricted to Treatment households in TRAIL and GRAIL villages with at most 1.5 acres of land. p-value of difference between default rate between TRAIL and GRAIL for each Bin presented. Productivity is computed using the logarithm of the acreage under potato cultivation.

Table 1: Randomization

	TRAIL (1)	GRAIL (2)	TRAIL — GRAIL (3)
Panel A: Village level characteristics			
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 level characteristics			
Head: More than Primary School	0.407	0.420	-0.013
	(0.015)	(0.015)	
Head: Cultivator	0.441	0.415	0.026
	(0.015)	(0.015)	
Head: Labourer	0.340	0.343	-0.003
	(0.015)	(0.015)	
Landholding (Acres)	0.456	0.443	0.013
	(0.013)	(0.013)	
Area of house and homestead (Acres)	0.052	0.052	0.000
	(0.001)	(0.002)	
Separate toilet in house	0.564	0.608	-0.044
	(0.015)	(0.015)	
Own a motorized vehicle	0.124	0.126	-0.002
	(0.010)	(0.010)	
Own a Savings Bank Account	0.447	0.475	-0.028
	(0.015)	(0.015)	

Notes: Panel A uses data from the houselisting exercise we carried out in 2007. Since 2 of the villages from the 2007 sample had to be replaced due to Maoist violence, Panel A uses a sample of only 46 villages. Panel B uses household survey data from 2010 for all 48 villages for sample households that owned no more than 1.5 acres of land. 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	0.208	0.958	-0.667
	(0.095)	(0.042)	(0.104)
Primary Occupation: Other	0.417	0.000	0.125
	(0.690)	(0.000)	(0.690)
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 (Rupees)	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 in parenthesis.

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

| | TRAIL | | GRAIL | | Difference (TRAIL−GRAIL)† | |
	Non-recommended	Recommended	Non-recommended	Recommended	Non-recommended	Recommended
	(1)	(2)	(3)	(4)	(5)	(6)
Agent and Household belong to same caste/religion category‡	0.45 (0.021)	0.53 (0.023)	0.61 (0.020)	0.68 (0.022)	-0.16 (0.03)	-0.15 (0.03)
Respondent knows Agent	0.90 (0.013)	0.96 (0.009)	0.91 (0.012)	0.97 (0.008)	-0.01 (0.02)	-0.01 (0.02)
Respondent meets Agent at least once a week*	0.98 (0.007)	1.00 (0.002)	0.98 (0.006)	1.00 (0.003)	-0.01 (0.01)	0.00 (0.00)
Household member invited by Agent on special occasions	0.31 (0.020)	0.45 (0.024)	0.27 (0.019)	0.39 (0.023)	0.04 (0.03)	0.06 (0.03)
Agent is one of the two most important						
Moneylenders	0.17 (0.016)	0.21 (0.019)	0.08 (0.012)	0.10 (0.014)	0.08 (0.12)	0.11 (0.02)
Input suppliers	0.18 (0.016)	0.24 (0.020)	0.08 (0.011)	0.09 (0.014)	0.10 (0.02)	0.14 (0.02)
Output buyers	0.18 (0.016)	0.25 (0.020)	0.02 (0.006)	0.04 (0.009)	0.16 (0.02)	0.21 (0.02)
Employers	0.12 (0.013)	0.10 (0.014)	0.08 (0.011)	0.07 (0.012)	0.04 (0.02)	0.03 (0.02)
In the last 3 years household has						
Bought from Agent	0.33 (0.020)	0.41 (0.023)	0.05 (0.009)	0.07 (0.012)	0.28 (0.02)	0.34 (0.03)
Borrowed from Agent	0.14 (0.015)	0.25 (0.020)	0.04 (0.008)	0.09 (0.013)	0.10 (0.01)	0.16 (0.02)
Worked for Agent	0.10 (0.012)	0.13 (0.016)	0.09 (0.012)	0.11 (0.014)	0.01 (0.02)	0.02 (0.02)
Household Sold Potatoes to Agent (% of Transactions)	0.109 (0.312)	0.171 (0.377)	0.020 (0.142)	0.027 (0.163)	0.089 (0.02)	0.143 (0.02)
Household has been selling Potatoes to Agent for longer than one year♣	0.870 (0.339)	0.877 (0.331)	0.556 (0.527)	0.615 (0.506)	0.315 (0.133)	0.261 (0.107)

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.‡: The farmer belongs to same the religion category (Hindu/Non-Hindu) or caste category (Scheduled caste/Scheduled tribe, or General) as the agent. ♣: Conditional on selling to the agent. *: Conditional on knowing the agent. †: The differences in columns 5 and 6 denote TRAIL—GRAIL for the specific sub-group (Non-recommended or Recommended) households.

Table 4: Average Treatment Effects on Agricultural Borrowing, Farm Value Added, Non Agricultural Income and Total Household Income

	Agricultural Loans		Farm	Income	
	All	Non-Program[†]	Value Added	Non Agricultural	Household
	(Rs.)	(Rs.)	(Rs.)	(Rs.)	(Rs.)
	(1)	(2)	(3)	(4)	(5)
TRAIL Treatment	2868	-448.50	2147.874	1506.785	5612.479
	(658.90)	(634.80)	(723.935)	(3655.543)	(4125.591)
Mean TRAIL C1	2096	2096	10336.511	33617.764	49765.238
% Effect TRAIL	136.83	-21.39	20.779	4.482	11.278
GRAIL Treatment	2754	-104.90	138.905	-4326.522	-5434.789
	(593.40)	(551.30)	(929.675)	(3225.654)	(4496.421)
Mean GRAIL C1	1931	1931	10501.064	37170.751	56082.489
% Effect GRAIL	142.62	-5.43	1.323	-11.640	-9.691
TRAIL v. GRAIL	114.50	-343.6	2008.969	5833.307	11047.268
Treatment	(890.60)	(842.6)	(1183.349)	(4879.593)	(6110.838)
Sample Size	6,156	6,156	6,156	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. 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. Standard errors in parentheses are clustered at the hamlet level. .

Table 5: Average Treatment Effects in Potato Cultivation

	Cultivate	Acreage (Acres)	Production (Kgs)	Cost of Production (Rs.)	Price (Rs./Kgs)	Revenue (Rs.)	Value Added (Rs.)	Imputed Profit (Rs.)	Input Cost (Rs.)	Input Cost Per acre (Rs./Acres)	Index Variable
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
TRAIL Treatment	0.040	0.093	946.368	1846.025	-0.030	3898.401	2059.235	1906.696	3049.126	-2908.838	0.180
	(0.028)	(0.028)	(302.239)	(722.561)	(0.099)	(1204.271)	(631.241)	(600.595)	(1316.601)	(1015.032)	(0.054)
Mean TRAIL C1	0.715	0.336	3646.124	8481.751	4.627	14285.467	5732.357	4733.770	16288.501	49076.876	
% Effect TRAIL	5.63	27.58	25.96	21.76	-0.650	27.29	35.92	40.28	18.719	-5.927	
GRAIL Treatment	0.130	0.069	770.916	2007.117	-0.176	2500.996	492.395	190.292	3886.833	554.147	0.142
	(0.029)	(0.029)	(340.084)	(779.852)	(0.110)	(1377.891)	(744.349)	(720.068)	(1389.776)	(1098.176)	(0.062)
Mean GRAIL C1	0.641	0.296	3236.730	7070.653	4.800	12964.776	5827.663	4941.511	13454.993	47510.733	
% Effect GRAIL	20.33	23.25	23.82	28.39	-3.665	19.29	8.45	3.85	28.888	1.166	
TRAIL v. GRAIL Treatment	-0.090	0.024	175.452	-161.092	0.146	1397.405	1566.839	1716.404	-837.707	-3462.985	0.038
	(0.040)	(0.041)	(457.836)	(1067.944)	(0.145)	(1838.410)	(979.876)	(941.279)	(1923.972)	(1499.369)	(0.083)
Sample Size	6,150	6,150	6,150	6,150	3,820	6,150	6,150	6,150	6150	4038	6150

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—9 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 10, 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. In column 11, the dependent variable is an index of z-scores of the outcome variables in the panel; the p-values for treatment effects in this column are computed according to Hochberg (1988)'s step-up method to control for the family-weighted error rate across all index outcomes. Standard errors in parentheses are clustered at the hamlet level. .

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Table 6: 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 (TRAIL–GRAIL)	0.065	0.000
	(0.011)	(0.010)
Panel B: Regression Results		
GRAIL	-0.066	0.005
	(0.011)	(0.010)
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.

Table 7: Variation of Productivity with Observable Household Characteristics

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Regression Results

Landholding	1.103
	(0.115)
Non Hindu	-0.157
	(0.147)
Low Caste	-0.110
	(0.120)
Household Size	0.019
	(0.021)
Female-headed Household	-0.457
	(0.217)
Age of Oldest Male	-0.004
	(0.003)
Oldest Male: Completed Primary School	0.080
	(0.084)
Constant	1.212
	(0.188)

Sample Size	1,000
R-squared	0.163

Descriptives on farm productivity

Mean of Productivity	1.707
SD of Productivity	1.148
Minimum Productivity	-1.294
Productivity Quartile 25%	0.811
Productivity Quartile 50%	2.014
Productivity Quartile 75%	2.638
Maximum Productivity	3.702

Notes: The dependent variable is the household productivity estimate. Standard errors in parentheses are clustered at the hamlet level. 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 8: 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)		
Treatment Effect: Bin 1			0.0915	0.130
			(0.0868)	(0.0697)
Treatment Effect: Bin 2			-0.0741	0.0309
			(0.0805)	(0.0702)
Treatment Effect: Bin 3			0.0568	0.0135
			(0.0564)	(0.0743)
Selection Effect	-0.0649	0.0825		
	(0.0447)	(0.0369)		
Selection Effect: Bin 1			-0.133	0.0217
			(0.0610)	(0.0580)
Selection Effect: Bin 2			-0.0291	0.117
			(0.0738)	(0.0664)
Selection Effect: Bin 3			-0.0343	0.105
			(0.0594)	(0.0718)
Sample Size	1,011	1,026	1,021	1,044

Notes: Estimating sample includes all sample households in TRAIL and GRAIL villages with atmost 1.5 acres of land. OLS regression results presented. Standard errors in parenthesis clustered at the hamlet level. 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.

Table 9: HTEs by Productivity Bin. Potato Cultivation and Interaction with Agent

	Cultivate (0/1) (1)	Acreage (Acres) (2)	Output (Kgs) (3)	Cost (Total) (Rs.) (4)	Price (Rs./Kgs) (5)	Revenue (Rs.) (6)	Value Added (Rs.) (7)	Imputed Profit (Rs.) (8)	Input Cost (Rs.) (9)	Input Cost (Per acre) (Rs./Acres) (10)	Agent Interaction (11)
Panel A: TRAIL											
Bin 1	0.08	0.04	383.82	696.46	0.27	1740.24	1040.43	930.02	1404.31	-1701.34	0.04
	(0.03)	(0.02)	(179.66)	(444.71)	(0.19)	(799.82)	(461.01)	(439.59)	(867.58)	(5217.85)	(0.03)
Bin 2	0.01	0.09	909.28	2132.54	-0.13	3701.22	1561.19	1458.58	3629.66	-2320.53	0.06
	(0.02)	(0.03)	(274.25)	(638.20)	(0.11)	(1116.50)	(616.88)	(590.20)	(1380.78)	(1624.29)	(0.08)
Bin 3	0.00	0.11	1126.38	1814.68	0.03	4617.76	2834.05	2668.16	2614.40	-3737.34	0.32
	(0.00)	(0.04)	(488.62)	(1252.25)	(0.16)	(2127.12)	(1296.70)	(1268.49)	(2162.73)	(1334.26)	(0.13)
Panel B: GRAIL											
Bin 1	0.26	0.03	287.04	892.40	-0.21	923.13	30.37	-265.08	1882.20	6768.55	1.47
	(0.04)	(0.02)	(191.71)	(392.02)	(0.16)	(754.42)	(436.82)	(419.17)	(782.34)	(2949.59)	(0.54)
Bin 2	0.12	0.08	844.40	2461.04	-0.27	3022.03	551.22	301.55	4002.05	-1881.26	0.36
	(0.02)	(0.03)	(277.39)	(622.18)	(0.22)	(1214.53)	(776.04)	(752.09)	(1350.70)	(1708.23)	(0.30)
Bin 3	0.00	0.11	1418.74	3210.20	-0.12	4499.38	1291.41	900.73	6777.32	1552.87	0.12
	(0.00)	(0.05)	(570.73)	(1254.91)	(0.10)	(2438.55)	(1705.78)	(1713.97)	(2271.26)	(1561.18)	(0.03)
Panel C: TRAIL − GRAIL Difference											
Bin 1	-0.18	0.01	96.79	-195.94	0.47	817.11	1010.06	1195.10	-477.89	-8469.90	-1.42
	(0.05)	(0.02)	(269.90)	(608.30)	(0.25)	(1135.25)	(652.15)	(622.94)	(1197.94)	(5981.13)	(0.54)
Bin 2	-0.11	0.00	64.88	-328.49	0.14	679.19	1009.97	1157.03	-372.38	-439.27	-0.31
	(0.03)	(0.04)	(388.58)	(887.63)	(0.24)	(1650.43)	(997.51)	(962.31)	(1921.03)	(2374.94)	(0.31)
Bin 3	0.00	0.00	-292.36	-1395.52	0.16	118.38	1542.63	1767.44	-4162.92	-5290.21	0.20
	(0.00)	(0.06)	(751.62)	(1773.03)	(0.19)	(3235.01)	(2147.61)	(2137.38)	(3134.61)	(2061.19)	(0.12)
Sample Size	6,150	6,150	6,150	6,150	3,820	6,150	6,150	6,150	6,150	4,038	2760

Notes: The estimating equation follows equation (6) 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 2—9 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 10, households that did not cultivate potatoes in a year are dropped from the estimating sample. In column 11, 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 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. Productivity is computed using the logarithm of the acreage under potato cultivation. Standard errors in parentheses are clustered at the hamlet level.

Table 10: Decomposition of ATE Differences in Value-added from Potato Cultivation; TRAIL v. GRAIL

	σ_k^T	σ_k^G	$\sigma_k^T - \sigma_k^G$	TRAIL HTEs (T_k^T)	GRAIL HTEs (T_k^G)	TRAIL–GRAIL HTEs $(T_k^T - T_k^G)$	$(\sigma_k^T - \sigma_k^G) \times T_k^T$	$\sigma_k^G \times (T_k^T - T_k^G)$
	(1)	(2)	(3)	(4)	(5)	(6)	(7 = (3) × (4))	(8 = (2) × (6))
Bin 1	0.27	0.34	-0.07	1040.4	30.4	1010.1	-74.1	348.2
Bin 2	0.33	0.33	0.00	1561.2	551.2	1010.0	-4.5	335.2
Bin 3	0.40	0.32	0.07	2834.1	1291.4	1542.6	209.8	498.9
ATE				2059.2	492.4	1566.8		
% of ATE due to Selection								8.38
% of ATE due to CTE								75.46

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 $\sum_{k=1}^{3} \frac{((\sigma_k^T - \sigma_k^G) \times T_k^T)}{\text{ATE}}$.

Table 11: Estimates from the Multidimensional Screening Model

	Variable	TRAIL	GRAIL
		(1)	(2)
(1)	ζ	-0.076	
(2)	Δ	0.21	0.28
(3)	μ (Fixed Effect Estimate†)	1.39	
	Reduced Form Estimates:		
(4)	Quasi Profit: $\frac{\mu}{1+\zeta}R_{ivt} - C_{ivt}$	13008.29	12432.64
(5)	Predicted ATE: $[\frac{\mu}{1+\zeta}R_{ivt} - C_{ivt}] \times \Delta$	2731.74	3481.14
	Structural Estimates:		
(6)	$[a_i[\frac{w_i}{c_i}]^{\frac{\mu}{1+\zeta}} - \frac{w_i}{(c_i)^{\frac{1}{1+\zeta}}}] \times \Delta$	1.35	1.46
(7)	Percentage Effect on Value Added	36.48	33.48
(8)	μ (IV Estimate‡)	0.90	
	Reduced Form Estimates:		
(9)	Quasi Profit: $\frac{\mu}{1+\zeta}R_{ivt} - C_{ivt}$	5432.67	5557.38
(10)	Predicted ATE: $[\frac{\mu}{1+\zeta}R_{ivt} - C_{ivt}] \times \Delta$	1140.86	1556.07
	Structural Estimates:		
(11)	$[a_i[\frac{w_i}{c_i}]^{\frac{\mu}{1+\zeta}} - \frac{w_i}{(c_i)^{\frac{1}{1+\zeta}}}] \times \Delta$	1.31	1.48
(12)	Percentage Effect on Value Added	35.27	33.95

Notes: †: Estimate of μ obtained from a household fixed effects regression of log quantity of potato cultivates on log acreage and village–year dummies. Sample includes Control 1 and Control 2 households in TRAIL and GRAIL villages with at most 1.5 acres of land. ‡: Estimate of μ obtained from a regression of average productivity on log acreage, household fixed effects and village-year dummies, with the treatment dummy as the instrument. 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. Sample includes Treatment and Control 1 households in TRAIL and GRAIL villages with at most 1.5 acres of land.

Table 12: 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	907.774	-182.203	0.368	1,219.652	-1,042.832
	(0.004)	(40.407)	(194.776)	(0.068)	(133.88)	(375.664)
Productivity squared	0.001	679.859	-352.578	-0.033	685.131	-101.632
	(0.002)	(23.831)	(89.164)	(0.044)	(48.367)	(168.138)
Constant	0.196	486.300	42,641.482	3.100	697.368	41,291.774
	(0.012)	(139.244)	(705.384)	(0.430)	(255.379)	(1090.398)
Sample Size	524	2,876	2,876	681	500	500

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.