Development Policy through the Lens of Social Structure*

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Abstract

This paper studies how the social structure of village economies affects policy implementation by local agents. We randomly select one of two viable candidates to deliver an agricultural extension program in rural Ugandan villages. We show that delivery agents favor their own social ties over ex-ante identical farmers connected to the other (non-selected) candidate and that this is inconsistent with output maximization or targeting the poorest. Favoritism disappears when the potential delivery agents belong to the same social group. Using the randomized allocation of the program across villages, we show how unobserved social structures explain the variation in delivery rates and program effectiveness that we often observe in the data.

Keywords: social structure, development policy, social ties, agriculture extension

JEL Classication: O10, O20, D80

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1 Introduction

Social structures, that is the organization of people in groups and the relationships between these groups, sustain cooperation and exchange in village economies (Bardhan 1984; Deaton 1997; Srinivas 1976; Udry 1990; Coate and Ravallion 1993; Stiglitz et al. 1993; Townsend 1994; Besley 1995; Udry 1994, 1995; Munshi 2014, 2019; Munshi and Rosenzweig 2017) and have been linked to development through their impact on gender norms, democratic institutions, values and conflict (Akyeampong et al. 2014; Naidu et al. 2015; Alesina and Giuliano 2015; Lowes et al. 2017; Moscona et al. 2020).

This paper studies how social structures affect the implementation of development policy. This is of interest because it is increasingly common for governments and NGOs to recruit delivery agents from their own communities,[1] thereby placing substantial resources under the control of an individual who is embedded in an existing social structure that determines how and where resources flow.[2]

The context we analyze is one of a common development policy – agricultural extension – implemented by the NGO BRAC in rural Uganda to promote the use of improved seeds and modern agricultural techniques among poor women farmers.

Our research design has two layers. The first layer is a standard clustered randomized controlled trial where we randomly select 60 out of 119 villages to receive the agriculture extension program. The second layer is a field experiment designed to investigate how social structure affects program implementation. We bring together two dimensions of social structure that have been central in the literature: the ties between the agent and individual beneficiaries (e.g., Basurto et al. 2017; Fisman et al. 2017; Alatas et al. 2019) and group divisions along ethnic, religious or political lines (e.g., Ferrara 2003; Alesina and Ferrara 2005; Naidu et al. 2015; Bauer et al. 2016).

In each of the 60 treatment villages, the second layer of our experiment randomizes the

[1]The standard model has been to recruit agents from *outside* the village as a means of ensuring impartiality (Northcote et al. 1854; Weber 1922; Xu 2018). Following a strong shift towards the localization of delivery (World Bank 2004; Mansuri and Rao 2012; Casey 2018) that started in the 1990s, agents are increasingly recruited *within* the village and thus have social ties with potential beneficiaries. This has been justified as a response to low state capacity and the need to capitalize on social incentives for local delivery agents to serve local citizens. The World Bank, for example, spent $85 billion on participatory development programs between 2003 and 2015, which was a radical departure from expenditures in the prior two decades (Mansuri and Rao 2012).

[2]The relevance of this channel is likely to grow as the share of external aid delivered by NGOs grows. In the past twenty years, the number of NGOs and overall aid from major donors such as USAID and the European bilateral agencies channeled through NGOs have more than quadrupled (Aldashev and Navarra 2018; Deserranno et al. 2019a). Our partner NGO, BRAC, makes extensive use of the local delivery model.

choice of the delivery agent out of two candidates selected by BRAC. The design, which is illustrated in Figure 1, creates two groups of potential beneficiaries who are identical *ex-ante* (in the sense of being connected to someone who could be a delivery agent) but differ in connections to the delivery agent *ex-post*. By doing so, the design creates random variation in the first dimension of social structure, that is the individual ties between the agent and potential beneficiaries. This is complementary to several recent papers that exploit *cross-village* random variation in the choice of the delivery agent to identify which type of agent is more effective at delivery (e.g., BenYishay and Mobarak (2019); Casey et al. (2018); Maitra et al. (2020); BenYishay et al. (2020)).[3] We create random variation *within the village* to study how agent-farmers connections shape the allocation of resources and program coverage.

The empirical design also gives us something else we do not normally see, namely the counterfactual agent to the chosen delivery agent. This is important because when choosing whether to favor her ties over the ties of the counterfactual agent, the relationships between the two agents might matter for a number of reasons including that favors can be exchanged through common links (Jackson et al. 2012), that cooperation is driven by group identity rather than individual ties, or that cooperation depends on the existence of a common "enemy" (Henrich 2004; Choi and Bowles 2007; Bauer et al. 2016). In our setting, political affiliation is the most salient dimension of group identity and the two agents belong to the same party in half the villages and to different parties in the other half.

We find that the two elements of social structure – individual ties and group divisions – interact to determine beneficiaries' selection. Indeed, the delivery agent is 6pp more likely to target her own social ties relative to observationally equivalent ties of the counterfactual agent. However, the preferential treatment of individual ties depends on group level ties as it occurs only in villages where the two agents support different parties. In other words, a common group identity between agents mutes the effect of individual ties. Taken together the findings suggest that the same intervention, delivered by the same organization in the same context, will take different shapes once it "hits the ground" as social structures direct the flow of resources towards different beneficiaries. This contributes to the debate on the external validity of experimental estimates, highlighting implementation as a key driver of heterogeneity (Basu 2005; Allcott and Mullainathan 2012; Davis et al. 2017).

[3] BenYishay and Mobarak (2019) compare an agriculture extension program delivered by more educated "lead farmers" vs. more representative "peer farmers." BenYishay et al. (2020) compare women vs. men agriculture extension workers. Maitra et al. (2020) compare local programs delivered by traders vs. politicians, Casey et al. (2018) compare local chiefs to technocrats.

The welfare implications of social structure fundamentally depend on the reasons underpinning preferential treatment by the delivery agent for her ties. The first set includes all match specific features that benefit both the agent and the organization due to improved information and enforcement (Griliches 1957; Foster and Rosenzweig 1995; Munshi 2004; Conley and Udry 2010; Bandiera and Rasul 2006; BenYishay and Mobarak 2019; Cai et al. 2015; Fisman et al. 2017; Maitra et al. 2020). For instance, the agent might know her connections better, which allows her to target the neediest or tailor the program to their type, thereby increasing its returns. The second set includes private benefits, for example, that it might be easier to extract rents from one's ties as part of a repeated game (Galasso and Ravallion 2005; Bardhan and Mookherjee 2006; Robinson and Verdier 2013; Deserranno et al. 2019b).

We find that the delivery agent prioritizes her rich ties over the poor ties of the counterfactual agent, which indicates that if the agent has better information she does not use it to target the poor. To test whether the delivery agent's allocation maximizes output we rely on the simple intuition that under the null the output produced by her ties and by the counterfactual farmers should be equal at the margin. We compare profits per acre of treated delivery agent ties and counterfactual agent ties and find that treated delivery agent ties have lower profits per acre. This indicates that output could be increased by swapping one treated delivery agent tie with a counterfactual agent tie keeping the number of treated farmers constant. In other words, the delivery agent could be getting private rents in exchange for lower output. We show this by charting the evolution of the agent's wealth over the course of the study. Consistent with the idea that favor exchange shapes service delivery, we show that (i) the delivery agents' actual wealth exceeds the predicted wealth, (ii) this difference is increasing in the number of own ties and that (iii) this is only the case when the delivery and counterfactual agents belong to different groups.

Having identified the dimensions of social structure that affect program delivery, we then study the consequences of social structure for the success of the program. We start by assessing the effect of social structure on coverage – i.e., on the number of farmers treated in the village. We do so by exploiting the cross-village variation in the number of delivery agent ties, which is exogenous conditional on the total number of delivery agent and counterfactual agent ties in the village. We find that more-connected agents treat more farmers: one more farmer for every three ties they have. This positive effect of the size of agents' network on coverage has been stressed by the literature on social networks and is the rationale for choosing highly connected agents (Banerjee et al. 2013; Kim et al.

2015; Beaman et al. 2018). Here we show that this rationale only holds if the two agents belong to different groups. When they belong to the same group, the size of the delivery agent's network does not matter. This suggests that connections between agents, which we do not typically measure, can explain the variation in delivery rates that we often observe in the data.

We then use the program randomized control trial to illustrate how program effectiveness correlates with social structures. Relative to farmers in control villages, we find that farmers in treatment villages are significantly more likely to have received the training or improved seeds at endline, and have significantly higher profits per acre and consumption per adult equivalent. We document strong heterogeneity in treatment rates within the treatment villages, with training rates being four times as large in villages where the delivery agent belongs to a different group than her counterfactual *and* has many ties compared to the other villages (same group or different group with few ties). Despite these large differences in treatment rates, differences in average consumption and profits across treatment villages are an order of magnitudes smaller. This indicates that the delivery agents who treat more farmers (i.e., different groups with many ties) target farmers with lower returns. In line with this, both quantile treatment effects at the individual level and inequality at the village level reveal that the program increases inequality in profits and consumption, especially so in villages where the potential agents belong to different groups and the delivery agent has many ties.

Our overarching conclusion is that social structure has a fundamental impact on how program resources flow through the village. The same program delivered to villages containing individuals with similar characteristics leads to radically different program outcomes depending on village social structure. This helps us to understand why we see such heterogeneity in program outcomes across seemingly similar communities across the developing world (Buehren et al. 2017; Beaman et al. 2018).

The paper is structured as follows. Section 2 provides background information and describes the research design. Section 3 introduces a model of the delivery agent's decision to treat each farmer and makes clear how our research design allows us to identify the effect of social structure on this choice. Section 4 presents our results on the effect of social structure on the allocation of program resources. Section 5 illustrates the implications for program coverage and program effectiveness. Section 6 concludes.

2 Context and Research Design

2.1 BRAC's Agriculture Extension Program

In Uganda, as in most of sub-Saharan Africa, agriculture is the main source of employment and income for a large fraction of the population, and especially for the poor. BRAC's agricultural extension program aims to raise the productivity of the poorest women farmers and encourage a shift from subsistence to commercial agriculture. In a setting where non-agricultural employment opportunities are limited, this represents a potentially important stepping stone out of poverty. BRAC's program targets women farmers, who tend to be the poorest in the population and are often by-passed by government extension programs that typically serve men (Boserup et al. 2013, BenYishay et al. 2020).[4] The program was launched in August 2008 and currently operates in 41 districts in rural Uganda, engaging more than 800 delivery agents, and reaching over 40,000 women farmers per year (Barua 2011).

The program provides training in modern agricultural techniques as well as improved seeds, addressing two fundamental market failures: lack of information on modern techniques and adverse selection in the seeds market. In particular, the training covers a bundle of four techniques of which three – zero tillage, line sowing, and avoidance of mixed cropping – are rarely used by the sample farmers.[5] Improved seeds are well known – 93% of our sample farmers know what improved seeds are and 70% believe that the adoption of high-quality improved seeds has positive agriculture returns – but only 31% of the sample farmers had ever used them due to lack of reliable suppliers. Seeds sold in local shops are often of low quality: a recent study conducted in 120 local shops/markets in rural Uganda finds that the most popular high-yield variety of maize seeds contains less than 50% authentic seeds and documents that such low quality results in negative average returns (Bold et al. 2017).[6] BRAC's solution to this problem is to produce improved seeds in their own farms and to sell them with BRAC certification at below-market prices.[7] Techniques and seeds are complementary but either can increase productivity on

[4]BRAC differs from most agriculture program in that it employs only women agriculture workers and ask them to serve women farmers only.

[5]The techniques are intercropping (adopted by 62% of the farmers at baseline), zero tillage (11%), avoidance of mixed cropping (10%), and line sowing (44%).

[6]The presence of counterfeit agricultural inputs is not specific to Uganda. For example, Tjernström et al. (2018) document that nearly 25% of hybrid maize seeds in Kenya do not germinate.

[7]BRAC sells seeds for marketable crops, defined as high value crops that are primarily cultivated to sell on the market (potato, eggplant, cabbage) but also crops typically grown for own consumption (maize and beans).

its own.

In line with the global trend towards the localization of delivery (World Bank 2003; Mansuri and Rao 2012; Casey 2018), the program is provided by a local delivery agent who is hired by BRAC to offer training and improved seeds to farmers in her community. The recruitment process is divided into two steps. First, BRAC's program officers identify, in each village, individuals who satisfy the eligibility criteria for becoming a delivery agent, i.e., being a woman, aged between 24 and 45, engaged in commercial agriculture, owning at least one acre of land, and being literate and trusted in the community. BRAC then contacts the best-suited agent out of a handful of candidates and appoints her. Agents are approached by BRAC and farmers cannot apply for the post. In the villages we study, the median number of qualified agents to choose from is two. These are elite model farmers who have engaged in commercial agriculture and are therefore best positioned to impart these skills to poorer farmers engaged in subsistence agriculture.

Following their appointment, delivery agents receive six days of training in crop production techniques and adoption of improved seeds, in addition to monthly follow-up refresher courses. Their task is to train farmers on modern agriculture practices and to sell improved seeds at the beginning of each growing season.[8] The agents are offered an open-ended contract and are compensated in kind with free training, free seeds worth 2000 UGX (about $1), and with a commission on seeds sales. The commission ranges between 5% and 10% of the sale price depending on the season and the specific seed, and agents can purchase seeds wholesale from BRAC. Financial incentives are very weak, even if the agents were to sell the maximum quantity of seeds available to her (worth 40,000 UGX), she would earn at most 4000 UGX, which corresponds to 3% of yearly per capita consumption expenditures. Accordingly, the main reason delivery agents cite for doing the job is that they value the training provided by BRAC.[9] This then raises the question as to what self-interest is there in treating other farmers? To understand this we study how village social structure – connections between agents and farmers, the agents' social groups, and the interaction of the two – shapes the agents' choices of *which* and *how many*

[8] BRAC asks agents to train 15-20 farmers per cropping season but the lack of monitoring makes this hard to enforce.

[9] 64% of the delivery agents report doing the job to "gain agriculture knowledge and skills through the training", 7% report doing it to "earn money", 6% to "serve the community", 3% to "get free seeds."

farmers to treat and ultimately the effectiveness of the program.

2.2 Research Design

The research design has two stages. The first is a clustered RCT to evaluate BRAC's agriculture extension program. We randomly allocate 119 villages to two groups: 60 that received the agriculture program in 2012 (treatment group) and 59 that received it in 2015 (control group).[10] The second is a field experiment that creates random variation in whom is selected as the delivery agent, and through this, random variation in social connections between the delivery agent and the beneficiaries. In each of the 60 treatment villages, we randomized the choice of the delivery agent out of the two most suitable candidates. To do so, we followed BRAC's standard hiring protocol (described above), up to the final stage, at which point we randomized the choice of the agent. The whole process, from candidate selection to delivery agent appointment, lasted a couple of days in each village. The two candidates were informed that, out of the eligible candidates in the village, the delivery agent would be selected by lottery but would not be revealed the name of the other eligible candidate.

Within each village, the empirical design divides farmers into four groups: those *exclusively* tied to the delivery agent, *exclusively* tied to the counterfactual agent, tied to both agents, or tied to none. The randomization ensures that the group of farmers tied *exclusively* to the delivery agent is *ex-ante* identical to the group of farmers tied *exclusively* to the counterfactual agent (see Figure 1). To estimate the causal effect of social ties, our empirical analysis will compare treatment rates between these two groups of farmers, both tied to one agent only.[11] Throughout the paper, we define farmers tied to the delivery agent ("delivery agent ties") as those who are *exclusively* tied to the delivery agent. Similarly, we define farmers tied to the counterfactual agent ("counterfactual agent ties" or "non-ties") as those who are *exclusively* tied to the counterfactual agent.

The empirical design has two key features. First, it eliminates endogenous tie formation as a common confounder, where farmers tied to a delivery agent and those who are not might otherwise differ in their unobservable characteristics in a manner that might

[10]The villages are located in the catchment area of four BRAC branch offices (Kabale, Rukungiri, Buyanja, Muhanga) that were opened by BRAC shortly before the experiment. The randomization was stratified by branch, size of the village (above vs. below median), percentage of farmers in the village (above vs. below median) and distance to the closest market (above vs. below median).

[11]Because the total number of ties with the agents is endogenous, we will not leverage variation from farmers tied to both agents or to none of the agents, but only from farmers tied to one agent only. In other words, we will not make comparisons between farmers tied to the delivery agent (regardless of whether they are tied to the other agent) vs. farmers not tied to the delivery agent.

affect the outcome of interest. Second, it gives us something else we do not normally see, namely a counterfactual agent to the chosen delivery agent. This is important because when choosing whether to favor her ties over the ties of the counterfactual agent, the relationships between the two agents might matter for several reasons including that favors can be exchanged through common links (Jackson et al. 2012), that cooperation is driven by group identity rather than individual ties, or that cooperation depends on the existence of a common "enemy" (Henrich 2004; Choi and Bowles 2007; Bauer et al. 2016). To study the effect of social structures on program delivery, we combine the experimental variation in ties between agents and farmers within village, the observational variation in whether the two agents belong to the same social group or different groups, and the interaction of the two.

In the first part of the paper, we zoom in on the sample of 60 treatment villages in which a delivery agent is selected and use this to study how social structure shapes the agents' choices. In the last section of the paper, we present results from the main program evaluation by comparing the 60 treatment villages to the 59 control villages. We show that differences in implementation driven by differences in social structure create substantial heterogeneity in how the program affects profits and consumption across villages.

2.3 Data

Figure A1 reports the project timeline. To define our sampling frame, we did a census of all households in the 119 sample villages and then randomly selected 20% to be surveyed in each village. Since the program targets women farmers, we always interviewed the female household head. The survey covered socio-economic background, agricultural practices, wealth and consumption at baseline (May-July 2012) and again two years later at endline (April-May 2014).[12] In the 60 treatment villages, we also asked each farmer about their social ties with the two agent candidates, and asked each candidate (two per village for a total of 120) about their social ties with the other agent. These data on social ties were collected in February 2013, after BRAC identified the two potential candidates for the agent position but before the agent randomization. This was timed to avoid strategic reporting of ties.

We measure ties between each farmer and each of the agents by asking each farmer whether she knows the two agents, whether she is friends with them or belongs to the same

[12] There is 7% attrition in farmers' response between baseline and endline. As we will show later, attrition is balanced between treatment and control and there is no evidence of differential attrition by household baseline characteristics.

family and whether they discuss agriculture.[13] Figure 2 shows that the agents are well known in their villages: 63% (68%) of the sample farmers know the delivery agent (the counterfactual agent), half of which are close friends or family, and 25% of the farmers know one agent but not the other. Moreover, the agents are a source of information about agriculture pre-program: 51% (47%) of the farmers report regularly discussing agriculture with the delivery agent (the counterfactual agent).

The random choice of the delivery agent creates a counterfactual group of farmers tied *exclusively* to the non-selected counterfactual agent who is similar on observables to the group of farmers tied *exclusively* to the delivery agent. These two groups are the focus of our analysis. Table 1 shows that the farmers who know only the delivery agent are observationally similar in terms of socio-economic background, usage of improved seeds, knowledge of agriculture techniques, profits per acre and consumption to farmers who know only the counterfactual agent (Columns 1-3).[14] Table 1 also shows that the delivery agent and her counterfactual are positively selected relative to other farmers in the village: they own more assets, are four times more likely to have used improved seeds, and their profits are between 6 and 8 times larger (Columns 4-6). Besides being wealthier and more profitable relative to the average farmer, the two agents are in the top quintile or decile relative to all the farmers in the village, confirming that they closely meet BRAC criteria for the job.

The experiment creates random variation in one dimension of social structure, that is the ties between the delivery agent and potential beneficiaries. When choosing which farmers to treat, the delivery agent will compare the benefits she draws from treating her ties to the benefits she draws from treating the ties of her counterfactual. Since, by definition, she is not connected to the ties of her counterfactual directly, the benefits she can draw depend on her relationship with the counterfactual agent. This is the second dimension of social structure that we are interested in measuring and that is normally not measured because the identity of the counterfactual agent is unknown. At one end of the spectrum, if the two agents are rivals, the delivery agent might put no weight or possibly a negative weight on the welfare of farmers tied to the counterfactual (Henrich 2004; Choi and Bowles 2007; Bauer et al. 2016). At the other end, if the two agents have the same group identity and can sustain cooperation, the weight is positive (Jackson et

[13] The wording of the questions is "Do you know [name]"?; "For how many years have you known [name]?"; "How would you best describe your relationship with [name]?" and "Do you normally discuss about agriculture with [name]?"

[14] We also obtain balance if we compare farmers who are friends/family of the delivery agent only vs. the counterfactual agent only, or if we compare farmers who regularly discuss agriculture with the delivery agent only vs. the counterfactual agent only. See Table A1.

al. 2012).

To identify the dimension that best defines social groups in this setting, we follow Berge et al. (2018) and ask village elders: "Besides being a citizen of Uganda, which specific group do you feel you belong to first and foremost?" 95% state that they identify with a political party, making political affiliation an obvious choice for measuring group identity and connections between the agents.[15] There are two parties in Uganda: the incumbent NRM and the runner-up FDC. Politics is a sensitive topic and people are often reluctant to state their party affiliation. To measure political connections between agents, we ask each of them if they support the same party as one another (without asking which party it is). Agents belong to the same political party in 50% of the villages.[16] For robustness, we also administer an implicit association test (IAT) to both agents to identify their political affiliation and use that to estimate whether this is shared between agents.[17] We will later show that the results are unchanged if we use this alternative measure of political connection, or if we proxy agents' relationship with whether they are friends or belong to the same (extended) family. Throughout the paper, we will use the self-reported political connection as our preferred measure of agents' ties because it has fewer missing values than the IAT one and exhibits more variation than the friend/family one. (The two agents are prominent figures, so know each other in 100% of the villages and are friends or part of the same extended family in 76% of the villages. That is there are only 13 villages where, by this measure, agents are not connected.)

We note that villages where the two agents belong to the same party will likely have different characteristics that determine political preferences in the village. For our purposes, it is important to note that the two sets of villages (those in which the two potential agents share the same political affiliation vs. those in which they do not) are equally polarized. Indeed, the incumbent share in the 2011 presidential elections is 62% in both cases (Columns 1-3 of Table A2), so we are not measuring the effect of political competition or lack thereof but we are potentially capturing elite cohesiveness, which is a broad dimension of social structure.[18] We come back to this issue in Section 4.2, where we show that our results survive the inclusion of political polarization and village infrastructure as controls.

[15]The remaining 5% answered either religion or their job. No-one answered ethnicity.

[16]We code the two agents as belonging to the same party if both agents report supporting the same party.

[17]We code the agents as having the same political affiliation if their IAT scores have the same sign, indicating that they are biased towards the same party. This is the case in 49% of the villages.

[18]We use data from the Ugandan Electoral Commission which are available at the polling station level and aggregated at the village level.

3 Framework

We now model the delivery agent's decision to treat each farmer and make clear how our research design allows us to identify the effect of social structure on this choice. Our goal is to show how social structure affects the delivery agent's targeting choices and how this differs from the choice of a social planner who maximizes output.

3.1 Set Up

Let $i \in \{1, \ldots, N_v\}$ denote the potential beneficiaries of the program (farmers) in village v. The social structure of village v is defined by $(\mathbf{d}, h_v)$. Here, $\mathbf{d}$ is a vector of connections within village where the i^{th} element is $d_i = 1$ if farmer i is tied to the delivery agent and $d_i = 0$ if farmer i is tied to the counterfactual agent instead. This highlights a key feature of our design, namely, we take into account the fact that beneficiaries not connected to the delivery agent are likely to be connected to the other potential agent. The second dimension of social structure is h_v, where $h_v = 1$ if the two agents belong to the same group and $h_v = 0$ if they do not.

The delivery agent chooses a targeting profile $\boldsymbol{T_v} = (T_i)_{i=1}^{N_v}$, where $T_i = 1$ if the delivery agent treats farmer i and $T_i = 0$ if she does not, to maximize her (private) net benefit. We make three assumptions. First, we assume that the delivery agent bears a constant cost c for each farmer she treats.

Second, we assume that the agent draws benefits in proportion to the value-added of treatment. Treating farmer i generates a (total) value-added $Y_i = A(d_i)\theta_i$, where θ_i is i's latent potential benefit from the program, known by the agent but unobserved by the researcher. We assume that θ_i is iid and continuously distributed across farmers and villages, with CDF $F(\cdot)$. Social structure affects the surplus through the term $A(d_i)$, so that $A(1) \geq A(0)$ if the agent is more effective at treating farmers she is connected to, for instance, because she knows their needs better or because communication costs are lower.

Third, we assume that the agent can appropriate a share $s(d_i, h_v)$ of the value added of treatment. This could be actual resources that are transferred from farmers back to the agent, for instance via future favors, or it could capture social preferences in reduced form so that the agent puts more weight on the benefit accruing to her ties. In either case, $s(1, h_v) \geq s(0, h_v)$ for $h_v \in \{0, 1\}$. Note that the agent benefits more from her ties, and has an incentive to favor them, if $s(1, h_v) \geq s(0, h_v)$ or if $A(1) \geq A(0)$. Only the productivity boost $A(d_i)$ makes this favoritism desirable from the point of view of the organization. Rent sharing, on the other hand, only generates private benefits.

Importantly, we allow $s(d_i, h_v)$ to be a function of agents' group identity (h_v). For farmers with $d_i = 0$, a shared identity between the two agents ($h_v = 1$) can serve as a substitute for a direct tie between the delivery agent and the farmers, so $s(0,1) \geq s(0,0)$. This is in line with both the rent sharing and with the social preference interpretation of s because the delivery agent can delegate enforcement or care more about the ties of the other agent when they have a common identity. For farmers with $d_i = 1$, these mechanisms are not relevant because they have a direct connection with the delivery agent already. If agents' identity does not affect the value of the connection between the delivery agent and the farmers, then $s(1,0) = s(1,1)$. However, if the delivery agent cooperates more with her ties when the two agents are "rival" (belong to rival groups), then $s(1,0) \geq s(1,1)$.[19] Alternatively, if the delivery agent has more bargaining power when she is connected with another agent as that makes her more central, then $s(1,0) \leq s(1,1)$.

3.2 The Agent's Choice

Formally, the agent will choose $\boldsymbol{T_v^{DA*}} = (T_i^{DA*})_{i=1}^{N_v}$ to solve

$$\max_{(T_i)_{i=1}^{N_v} \in \{0,1\}^{N_v}} \sum_{i=1}^{N_v} T_i \big[s(d_i, h_v) A(d_i) \theta_i - c \big]. \tag{1}$$

With constant marginal costs, the solution to equation (1) entails treating all farmers for whom benefits exceed costs.[20] Since the objective function is monotonically increasing in each θ_i, the delivery agent will implement the following threshold policy:

$$T_i^{DA*} = T_i^{DA*}(d_i, h_v, \theta_i) = \mathbf{1}\big\{ \theta_i \geq \widehat{\theta}^{DA*}(d_i, h_v) \big\}$$

where the threshold

$$\hat{\theta}^{DA*}(d_i, h_v) = \frac{c}{s(d_i, h_v) A(t_i)}$$

is a function of social structure (d_i, h_v). In particular, the threshold is decreasing in both s and A, which implies that the delivery agent is more likely to treat a farmer when she can extract more surplus s and when the surplus is larger due to the match-specific

[19] A number of papers show that individuals have stronger social preferences towards their "in-group" in the presence of a rival "out-group" (see the literature on parochial altruistic, e.g., Henrich 2004; Choi and Bowles 2007; Bauer et al. 2016). In our context, this would translate in the delivery agent having stronger preferences towards their ties when the two agents belong to rival groups, i.e., $s(1,0) \geq s(1,1)$.

[20] The results below follow if we allow the agent's cost of treating a farmer to be lower for ties than non-ties ($c(1, h_v) \leq c(0, h_v)$). See Model Appendix B.1 for a more detailed discussion.

component A. Recognizing that the expression above must hold for all pairs (d_i, h_v), we can rearrange it to obtain:

$$s(1, h_v)A(1)\widehat{\theta}^{DA*}(1, h_v) = s(0, h_v)A(0)\widehat{\theta}^{DA*}(0, h_v)$$

That is, at the optimum, the agent must receive the same output from her marginal treated tie and her marginal treated non-tie. Below we show how we can back out the parameters from empirical differences in treatment probabilities.

The probability that farmer i is treated conditional on her tie status with the agent (d_i) and the agents' group identity (h_v) is

$$\mathbb{E}\big[T_i^{DA*} \,\big|\, d_i, h_v\big] = \Pr\big(\theta_i \geq \widehat{\theta}^{DA*}(d_i, h_v) \,\big|\, d_i, h_v\big) = \mathrm{G}\big(\widehat{\theta}^{\mathrm{DA}*}(\mathrm{d_i}, \mathrm{h_v})\big)$$

where $G(x) = 1 - F(x)$. The second equality holds because, by randomization, θ_i is assumed to be independent of d_i and h_v.[21] This implies that we can use the within-village comparison of treatment rates of ties of the delivery agent and her counterfactual together with the between-village variation in agents' identity to identify how social structure shapes the allocation of program resources.

The empirical difference in treatment probabilities between farmers tied to delivery agents and farmers tied to counterfactual agents is

$$G\big(\widehat{\theta}^{DA*}(1, h_v)\big) - G\big(\widehat{\theta}^{DA*}(0, h_v)\big).$$

Since $G(\cdot)$ is strictly decreasing in $\widehat{\theta}^{DA*}$, and $\widehat{\theta}^{DA*}$ is strictly decreasing in $s(d_i, h_v)A(d_i)$, the sign of $G\big(\widehat{\theta}^{DA*}(1, h_v)\big) - G\big(\widehat{\theta}^{DA*}(0, h_v)\big)$ is the same as the sign of $s(1, h_v)A(1) - s(0, h_v)A(0)$. Thus the experimental design eliminates differences in expected ability. The delivery agent treats more of her own ties if $s(1, h_v)A(1) > s(0, h_v)A(0)$, either because she cares more about or cooperates better with her ties, $s(1, h_v) > s(0, h_v)$, or because she is more productive with them, $A(1) > A(0)$.

The difference in the treatment probabilities between farmers tied to the delivery agent and farmers tied to the counterfactual agent is larger when the agents are not part of the same social group $(h_v = 0)$ than when they are $(h_v = 1)$ if $\frac{s(1,0)-s(1,1)}{s(0,0)-s(0,1)} > \frac{A(0)}{A(1)}$.

[21] Our within-village specification includes village fixed effects, and h_v is thus kept fixed. This implies $\Pr\big(\theta_i \geq \widehat{\theta}^{DA*}(d_i, h_v) \,\big|\, d_i, h_v\big) = Pr\big(\theta_i \geq \widehat{\theta}^{DA*}(d_i, h_v) \,\big|\, h_v\big) = Pr\big(\theta_i \geq \widehat{\theta}^{DA*}(d_i, h_v)\big)$.

3.3 The Social Planner's Choice

We now compare the choice of the agent to the choice of a social planner that maximizes total surplus. This will allow us to make precise the conditions under which social ties between the agent and the farmers distort the allocation away from the optimum and to design a test for the null hypothesis that they do not.

As our focus is on targeting, we keep expected coverage to be the same as that chosen by the agent, and we restrict the targeting strategy to the same class of policy that the agent follows, i.e., by choosing $\boldsymbol{T_v} = (T_i)_{i=1}^{N_v}$ for each possible tie status and given h_v. We choose the allocation that maximizes expected total surplus

$$\max_{(T_i)_{i=1}^{N_v} \in \{0,1\}^{N_v}} \sum_{i=1}^{N_v} T_i A(d_i)\theta_i$$

subject to

$$\sum_{i=1}^{N_v} T_i = N_v^T.$$

$N_v^T \equiv N_v^{DA} G(\widehat{\theta}^{DA*}(1, h_v)) + (N_v - N_v^{DA}) G(\widehat{\theta}^{DA*}(0, h_v))$ is the expected coverage as chosen by the agent, where N_v^{DA} is the number of ties of the delivery agent and N_v is the number of ties of the delivery agent plus those of the counterfactual agent.

It can be shown (see Model Appendix B.2) that the solution of this problem must satisfy

$$T_i^{SP*} = T_i^{SP*}(d_i, h_v, \theta_i) = \mathbf{1}\big\{\theta_i \geq \widehat{\theta}^{SP*}(d_i, h_v)\big\}, \tag{2}$$

with

$$\widehat{\theta}^{SP*}(d_i, h_v) \equiv \frac{\lambda}{A(d_i)}$$

where λ is the Lagrange multiplier for the constraint in the social planner's maximization problem. Since λ does not depend on the social structure of the village, we obtain

$$A(1)\widehat{\theta}^{SP*}(1, h_v) = A(0)\widehat{\theta}^{SP*}(0, h_v).$$

The condition requires that the output produced by the marginal connection of the delivery agent is equal to the output produced by the marginal connection of the counterfactual agent. The intuition is as follows: increasing the threshold for one group decreases the expected output of that group but increases coverage of the other group. In the optimum, the marginal decrease in expected output has to be the same as the marginal value of relaxing the constraint on coverage. Because this has to hold for both groups, the output is maximized when the outputs of the marginal connections are equal. If this were not the case, for instance, if the output produced by the marginal connection of the delivery agent – that is, the one with the lowest θ_i that is treated – were to be lower than the output produced by the marginal connection of the counterfactual agent, swapping the pair would increase total output without increasing the number of farmers treated.

We note that this does not necessarily imply that the threshold should be the same for the two groups of farmers even if their θ_i are the same in expectation. Indeed, if $A(1) \geq A(0)$, then the social planner solution would have a lower threshold and hence more ties of the delivery agent should be treated at the social optimum. This captures the fact that social connections can increase productivity, so that if the social planner could choose which farmers the agent should treat, she would internalize this benefit.

As we show in the previous subsection, the solution for the agent's problem will differ from the condition above because the agent chooses a targeting profile $\boldsymbol{T}^{DA*}$ that maximizes her share of total output, and treating her social ties might allow her to get a larger share of a smaller pie. The delivery agent's thresholds satisfy the following equality, which we reproduce for convenience:

$$s(1, h_v)A(1)\widehat{\theta}^{DA*}(1, h_v) = s(0, h_v)A(0)\widehat{\theta}^{DA*}(0, h_v).$$

The difference between these two optimality conditions implies that under the null hypothesis of no misallocation, the output of the marginal connection of the delivery agent equals the output of the marginal connection of the counterfactual agent. By contrast, if the output of the marginal connection of the delivery agent is smaller, it must be that social ties create a wedge between the social and the private optima.

4 Evidence

4.1 Ties Between Agents and Farmers

Our first test uses the within-village variation in ties between agents and farmers to uncover whether the delivery agent draws larger benefits from treating her own ties. We estimate:

$$T_{iv} = \alpha + \beta D_i + X_{iv}\delta + \rho_v + u_{iv} \tag{3}$$

where $T_{iv} = 1$ if farmer i in village v is treated (either trained in the use of new techniques or given seeds before endline). $D_i = 1$ if farmer i is connected only to the delivery agent and 0 otherwise. X_{iv} is a vector of controls that contains an indicator of whether the farmer is connected to both agents, an indicator of whether the farmer is connected to no agent, and the distance from the farmer's and the delivery agent's house.[22] ρ_v are village fixed effects. u_{iv} are errors clustered at the level of the randomization, i.e., by connection status and village.[23] We report p-values from randomization inference using 500 random permutations at the bottom of each result table.

In equation (3), the omitted group of farmers are those connected to the counterfactual agent only. Our coefficient of interest, β, thus captures the within-village difference in treatment probability between farmers connected only to the delivery agent relative to farmers connected only to the counterfactual agent. In our theoretical framework, this corresponds to:

$$G\big(\widehat{\theta}(1, h_v)\big) - G\big(\widehat{\theta}(0, h_v)\big)$$

hence $\beta > 0 \iff [s(1, h_v)A(1) - s(0, h_v)A(0)] > 0$, that is, the experimental design eliminates differences in expected ability but does not tell us whether the agent cares more or cooperates better with her ties, $s(1, h_v) > s(0, h_v)$, or whether she is more productive with them, $A(1) > A(0)$.

Table 2 estimates equation (3) using the broadest definition of agent-farmer social ties, which pools together close friends, family, and acquaintances (i.e., whether the farmer knows who the agent is).[24] In the appendix, we show that that the results are robust to using more narrow definitions such as friends and family alone or work links (farmers who regularly discuss agriculture with the agent); see Table A4.

Table 2 reveals that, relative to ties of the counterfactual agent, farmers connected to the delivery agent are 6.1pp (3.8 times) more likely to be trained and 5.1pp (6.3 times) more likely to have received seeds at endline (Columns 1-2). Delivery agents are thus more likely to target their ties. We also show that counterfactual ties do not compensate for the fact that they are less targeted by the delivery agent by walking to the central

[22]We control for distance because we do not want our effects to pick up differences in targeting due to geographical proximity. Note, though, that all the results in this paper are robust to not controlling for this variable.

[23]Connection status takes one of four values depending on whether the farmer is connected only to the delivery agent, only to the counterfactual agent, to both or to none. All results are robust to only clustering at the village level.

[24]The distribution of farmers is as follows: 25% of sample farmer know one of the two agents (either the delivery or the counterfactual agent), 53% know both and 22% know none. See Figure 2.

BRAC branch office (located in a more urban area) to get BRAC seeds, nor by buying more seeds from other non-BRAC sources (Columns 3-4). Finally, Table 2 shows that training is paired with technique adoption (Columns 5-6). The effects are similar using the self-reported number of techniques and that measured by the enumerators.[25]

Columns 1-2 of Table A3 present the results of equation (3) removing the village fixed effects (ρ_v) and including instead a dummy (H_v) which takes value 1 if the two potential agents belong to the same group (party). Conditional on the farmer-agent tie, whether or not the delivery agent and the counterfactual agent belong to the same party is found to *not* affect the probability that a given farmer is treated. This rules out a level effect, that agents being in the same social group does not seem to uniformly increase or decrease treatment probabilities. The next section tests whether this interacts with agent-farmers ties so that its effect depends on whether the agent is connected to the farmer.

4.2 Group identity

Table 3 estimates:

$$T_{iv} = \alpha + \gamma_1 D_i * H_v + \gamma_2 D_i * (1 - H_v) + X_{iv}\delta + \rho_v + u_{iv} \tag{4}$$

where $H_v = 1$ in villages where the agents belong to the same group (party), ρ_v are village fixed effects that absorb omitted factors correlated with agents' group affiliation, and X_{iv} are the same controls as in equation (3). The table shows that the estimated γ_1 are close to zero and precise, suggesting that in villages where the two agents belong to the same party, the delivery agent does *not* favor her ties. In contrast, the estimated γ_2 is positive: farmers connected to the delivery agent are 10pp (3 times) more likely to be trained and 9pp (5.5 times) more likely to have received seeds at endline than farmers connected to the counterfactual agent.

These results are unchanged if we use the implicit association test to measure political affiliation, or if we proxy for group membership with individual ties, that is whether the agents are friends or belong to the same family (see Table A5).

Given that the test exploits cross-village variation in agents' group identity, we investigate whether the variation of interest can be identified separately from variation in village-level variables. Table A6 adds a set of village-level controls interacted with ties:

[25] We asked enumerators to check the plot of land of a random 60% of the respondents. For sake of comparison, both variables (self-reported and measured) are restricted to that sub-sample. Results are robust if we estimate the effects on self-reported adoption of techniques in the full sample of farmers.

$D_i * Z_v$, where Z_v include village-level polarization (share of votes for the majority party in the 2011 presidential elections), village infrastructure (roads, electricity, newspaper access, distance to BRAC branch), population, and density of ties. The estimates are unchanged.[26]

Taken together, the findings highlight the importance of taking into account different dimensions of social structure. In fact, the effect of ties within a group, here the agent and her social ties, depends on the relationship between groups. The chosen delivery agent does not favor her ties if she and the counterfactual agent belong to the same party. Being able to identify this interaction effect represents a key advantage of our research design.

4.3 Social vs. Private Benefits

We have shown that delivery agents are more likely to target their ties. As described in our theoretical framework, this can be ascribed to the fact that the agent gains personal benefits from treating her ties (either because she cares about them or because they can enforce an informal contract through repeated interactions) or to the fact that she is more effective at treating them or better able to identify the neediest. We test these in turn below.

4.3.1 Social Ties and Pro-Poor Targeting

The stated objective of the program is to help the poorest farmers and the delivery agent might be better able to identify those among her ties.[27] In other words, the delivery agent might be sacrificing output to meet the poverty reduction objective of the organization. To establish whether this is the case, Table 4 tests whether, by treating more of her ties, the delivery agent reaches the poorest farmers. We extend equation (3) to incorporate heterogeneous effects by farmer's wealth:

$$T_{iv} = \alpha + \gamma^P D_i * P_i + \gamma^R D_i * (1 - P_i) + \beta P_i + X_{iv}\delta + \rho_v + u_{iv} \tag{5}$$

[26] In line with this, Table A2 (Columns 1-3, Panel A) shows that political polarization and village infrastructure are similar in villages where the agents belong to the same party or not. These two sets of villages are also similar in terms of the characteristics of the selected delivery agents (Panel B).

[27] The targeting of anti-poverty programs is often delegated to better informed – but potentially less accountable – local agents. Various papers have studied whether such decentralization improves pro-poor targeting and how: e.g., Galasso and Ravallion (2005); Alatas et al. (2012); Niehaus and Atanassova (2013); Basurto et al. (2017); Alatas et al. (2019).

where $P_i = 1$ if the farmer is in the bottom quartile of consumption of her own village.[28] γ^P (γ^R) is the difference in the likelihood of treatment for poor (rich) delivery agent ties vs. poor (rich) counterfactual agent ties. β is the difference in the likelihood of treatment for poor vs. rich ties of the counterfactual agent. We present the estimates in the subsample of villages in which the two agents belong to the same party (Columns 1-2) separately from the subsample in which they belong to different parties (Columns 3-4). Table A7 shows the results for alternative measures of farmers' wealth (value of assets owned and food security).

Regardless of the wealth measure, we find that $\hat{\gamma}^R > \hat{\gamma}^P \geq 0$ when agents are not connected, that is, rich delivery agent ties are treated more than rich counterfactual agent ties, and this difference is larger than the comparison between poor delivery agent ties and poor counterfactual agent ties. Moreover, rich delivery agent ties are significantly more likely to be treated than poor counterfactual agent ties ($\hat{\gamma}^R - \hat{\beta} > 0$).[29] That the delivery agent treats her rich ties over poor counterfactual agent ties rules out that, when agents are not connected, the delivery agent sacrifices output to target the neediest. When agents are connected, neither $\hat{\gamma}^R - \hat{\gamma}^P$ nor $\hat{\gamma}^R - \hat{\beta}$ are statistically significant.

4.3.2 Social Ties and Output

The theoretical framework suggests a test based on the intuition that, at the social optimum, the marginal gain from the program must be equal for all treated farmers. If so, treated delivery agent ties and treated counterfactual agent ties should have the same return at the margin. In contrast, if the threshold is lower for the ties of the delivery agent, there is a range $\widehat{\theta}_0 - \widehat{\theta}_1$ such that all ties of the delivery agent with θ in that range are treated while equivalent ties of the counterfactual agent are not. This implies that the counterfactual tie with the lowest return will have higher productivity than the delivery agent tie with the lowest return but, given that the distribution of θ is the same, returns at the top will be equal. Note that even if the two groups are balanced, it might still be optimal to treat more of one's own ties if there exists a match-specific productivity boost, A. However, in that case, while the output of a tie will always be higher than the output of a non-tie with the same underlying productivity, the output maximizing condition remains the same but will require the number of treated ties to be larger than the number of treated counterfactual ties. In contrast, if the underlying motive is personal gain, we

[28] X_{iv} includes the same controls as equation (3) and adds the interaction between the indicators for the farmer being connected to both agents and to no agent with P_i and $(1 - P_i)$.

[29] The p-values for $(\hat{\gamma}^R - \hat{\gamma}^P)$ and $(\hat{\gamma}^R - \hat{\rho})$ are statistically significant in both Columns 1 and 2 of Table 4 and reported at the bottom of the table.

will find that the marginal treated delivery agent tie is less productive than the marginal counterfactual agent tie.

To provide evidence on whether this holds, we run quantile regressions comparing treated delivery agent ties who received the training or the seeds to treated counterfactual agent ties. To measure returns, we use profits per acre cultivated.[30] If the allocation is indeed optimal, we will observe no difference between treated ties for a given θ. Figure 3 reports the coefficient on the difference between the treated delivery agent ties and the treated counterfactual agent ties at each decile between 10 and 90, controlling for the baseline value of the same variable (solid line). For comparison, the figure also reports the difference in profits per acre between the same farmers at baseline (dashed line).

Two points are of note. First, at baseline the distribution of profits per acre is the same for the two groups of farmers. Second, the difference in returns to treatment is negative up to the decile 70 and zero thereafter suggesting that treated ties of the delivery agent have lower returns than treated ties of the counterfactual agent, which implies the threshold is lower for them. Following from the theory, this implies that the delivery agent could increase output keeping the number of treated farmers constant by swapping one of her ties with a counterfactual tie. In terms of the model parameters, we reject the null of $s_1 = s_0$ in favor of $s_1 > s_0$, that is the possibility to extract favors from social ties creates a wedge between the objective of the delivery agent and that of a social planner who maximizes output.

4.4 Private Benefits

The delivery agent can benefit in two ways: she can draw utility from helping her ties or she can get material favors back. Beneficiaries can repay in a multitude of ways and it is unrealistic to measure these directly. An alternative way of detecting their effect involves comparing the actual wealth growth of delivery agents to their predicted wealth growth, employing methods from the tax evasion literature (Pissarides and Weber 1989). We predict wealth growth using the baseline wealth of hypothetical delivery agents in control villages, which we identify using BRAC criteria.[31] Figure 4 plots actual wealth at endline against predicted wealth and shows that all but two delivery agents are above

[30] Households reported to us the quantity and price of crops sold. When they hold crops for self-consumption, we impute the sales revenue by pricing the crop at the median sale price in the village.

[31] The criteria are: (1) being a woman, (2) aged between 24 and 45, (3) engaged in commercial agriculture, (4) owning at least one acre of land, (5) literate and (6) trusted in the community. We proxy criterion (6) with the fraction of households in the village who name the agent as a main source of agriculture advice at baseline.

the 45-degree line, meaning their wealth exceeds the prediction.

To assess whether this excess wealth is indeed due to favor exchange, we test whether it is increasing in the number of farmers tied to the delivery agent. The estimates reported in Table A8 show that this is indeed the case: one more delivery agent tie increases excess wealth by 11%. In line with the patterns we observe throughout the paper, the number of delivery agent ties only matters when the delivery agent and the counterfactual agent belong to different groups.

5 Social Structure and Treatment Effects

We have shown that social structure shapes the choices of the delivery agent. We now study the consequences of social structure for the evaluation of the program. In Section 5.1, we show that social structure affects program coverage – i.e., how many farmers are treated – in addition to which farmers are treated. In Section 5.2, we show that the variation in program coverage – generated by the underlying variation in the social structure – leads to wide heterogeneity in the ultimate impact of the program on farmer profits and consumption.

5.1 Coverage

Let $N_v^T = \sum_{i=1}^{N_v} T_i$ denote the total number of treated farmers in a given village. Expected coverage conditional on the profile of connections $\mathbf{d}$ and agents' group affiliation h_v is

$$\begin{aligned}\mathbb{E}\big[N_v^T \,\big|\, \mathbf{d}, h_v\big] &= \sum_{i=1}^{N_v} \mathbb{E}\big[T_i \,\big|\, d_i, h_v\big] \\ &= N_v^{DA}\, G\big(\widehat{\theta}(1, h_v)\big) + (N_v - N_v^{DA})\, G\big(\widehat{\theta}(0, h_v)\big) \\ &= a(h_v) + b(h_v) N_v^{DA},\end{aligned}$$

that is, an affine of the number of ties of the delivery agent, N_v^{DA} with slope $b(h_v) = G\big(\widehat{\theta}(1, h_v)\big) - G\big(\widehat{\theta}(0, h_v)\big)$ and intercept $a(h_v) = N_v\, G\big(\widehat{\theta}(0, h_v)\big)$. From the results on farmer-agent ties (Section 4.1), we have $b(1) = 0$ and $b(0) > 0$. The results on agents' group affiliation (Section 4.2) instead tell us that, in villages of the same size differing only in whether the two agents belong to the same group, $a(1) > a(0)$. This implies that the expected coverage as a function of the number of delivery agent ties should be flat in villages where the delivery agent and the counterfactual agent have the same group identity and increasing in villages where they have not.

Table 5 evaluates the effect of social structure on program coverage. To do so, we exploit the cross-village variation in the number of exclusive delivery agent ties, as illustrated in Figure A2. Given that the delivery agent is chosen randomly, this variation is exogenous conditional on the total number of exclusive ties in the village (exclusive ties of the delivery agent plus exclusive ties of the counterfactual agent).[32] We estimate:

$$N_v^T = \alpha + \beta N_v^{DA} + \gamma H_v + X_v \delta + u_v \tag{6}$$

where N_v^T is the outcome of interest (number of farmers trained and number of farmers who received seeds before endline) in village v. N_v^{DA} is the number of exclusive delivery agent ties. X_v is a vector of controls that contains: the total number of exclusive ties, the aggregate number of farmers, and area (branch) fixed effects. $H_v = 1$ if the two agents belong to the same party.[33]

Table 5 shows that delivery agents with more ties train and give seeds to more people ($\hat{\beta} > 0$): having one more tie increases the number of farmers trained by 0.26 and the number receiving seeds by 0.22 (Columns 1 and 2). This is the motivation effect stressed by the literature on social networks and the rationale for choosing highly connected agents (Banerjee et al. 2013; Kim et al. 2015; Beaman et al. 2018).

Next, we allow the effect of the number of delivery agent ties on coverage to depend on agents' group identity. We estimate

$$N_v^T = \alpha + \beta_1 N_v^{DA} * H_v + \beta_2 N_v^{DA} * (1 - H_v) + \gamma H_v + X_v \delta + u_v,$$

where X_v includes the same controls as in equation (6) and, in addition, the total number of exclusive ties interacted with H_v. Columns 3-4 of Table 5 show that, when the agents belong to different parties, an extra delivery agent tie increases the number of farmers trained and the number receiving seeds by 0.29 and 0.23 respectively ($\hat{\beta}_2$ is positive and statistically significant at least at the 5% level). In contrast, when the agents belong to the same party, an extra delivery agent tie does not significantly increase coverage ($\hat{\beta}_1$ is not statistically significant). All this is robust to using alternative measures of agents' group affiliation – i.e., the politics implicit association test, or friends and family members (see Table A9). Overall, these results support the earlier finding that $b(0) > b(1) = 0$.

[32] As expected, villages in which the ratio between the number of delivery agent ties and the total number of delivery agent and counterfactual agent ties is above vs. below median are comparable in terms of village infrastructure and delivery agents' characteristics (Table A2, Columns 4-6).

[33] Controlling for H_v reduces the sample size from 60 to 53 due to 7 missing variables. The results are similar if we do not control for H_v. See Table A3 Columns 3-4.

They also support the finding that $a(1) > a(0)$: when the delivery agent has no direct tie with sample farmers, coverage is higher in villages where agents belong to the same group than in those where they do not ($\hat{\alpha} > 0$ although not statistically significant).

Comparing expected coverage when agents share vs. do not share group identity allows us to back out whether $s(1,1)$ is larger or smaller than $s(1,0)$ in our context, i.e., whether the delivery agent cares more or less about her ties when she has the same group identity as the other agent ($h_v = 1$) or when she does not ($h_v = 0$). We can do so because, conditional on $a(1) > a(0)$ and $b(0) > b(1) = 0$, the theory has different predictions on expected coverage. If $s(1,1) > s(1,0)$, the theory predicts expected coverage to always be lower in villages where $h_v = 0$ than in same-sized villages where $h_v = 1$. If $s(1,1) < s(1,0)$, the theory instead predicts expected coverage to be higher in villages where $h_v = 0$ as long as the number of delivery agent ties is sufficiently large. The findings in Table 5 are consistent with the latter: when the number of delivery agent ties is sufficiently large, expected coverage is higher when $h_v = 0$ than when $h_v = 1$,[34] while the contrary is true if the number of delivery agent ties is low enough.

Overall, the empirical findings indicate that $s(1,1) < s(1,0)$, i.e., that favoritism towards own ties is stronger when the two potential agents belong to different groups than when they share a group identity. One potential explanation for this result, which has been demonstrated in various other contexts, is that agents are "parochial altruistic," i.e., that belonging to a rival group from the counterfactual agent makes them more altruistic or cooperative towards their own ties (which they perceive as an "in-group") while less altruistic towards the "out-group" (Henrich 2004; Choi and Bowles 2007; Bauer et al. 2016). More generally, our results suggest that social structure might be a factor in explaining the wide variation in program coverage that is often documented in development programs (e.g., Buehren et al. 2017; Beaman et al. 2018).

5.2 Treatment Effects

Having identified the dimensions of social structure that affect program delivery and coverage, we use the program RCT to illustrate how the effects of the program vary with the underlying social structure of the village. Recall from Section 2.2 that the agriculture extension program was randomly assigned to 60 treatment villages (which are the focus of the earlier analysis) out of 119 villages.[35]

[34] This is the case as long as the delivery agent has more than 1.6 ties with the sample farmers, i.e., if the delivery agent ties account for more than 15% of the population.

[35] Tables A10 and A11 show that farmers in treatment and control villages are similar at baseline, that attrition between baseline (2012) and endline (2015) is balanced and that there is no differential attrition

Table 6 Panel A estimates the overall intention-to-treat effect of the program on take-up rates, profits per acre, and consumption with the following ANCOVA model:

$$y_{iv} = \alpha + \beta Program_v + \gamma y_{io} + X_v \delta + u_{iv},$$

where y_{iv} is the outcome of interest for farmer i in village v at endline (take-up, profits per acre, consumption), $Program_v$ is a dummy that takes value 1 in treatment villages and 0 in control villages, y_{io} is the baseline value of the outcome variable, X_v are area (branch) fixed effects, and u_{iv} are errors clustered at the village level.

We find that farmers in treatment villages are 8.3pp more likely to have adopted BRAC improved seeds at endline than farmers in control villages. They are also 3.7pp more likely to have received the training from the delivery agent. This increase in take-up is not trivial – especially considering that the take-up rate is zero in control villages – and leads to a more than proportional increase in both profits and consumption, suggesting some degree of information diffusion.[36] While the program is largely beneficial for the average farmer, we find that it exacerbates village-level inequality, as measured with the standard deviation of profits and consumption in the village. This is consistent with the earlier finding that the program is unevenly distributed within the village, and is biased towards less productive or less deserving farmers.

Table 6 Panel B allows the effect of the program in treatment villages to differ depending on the village's underlying social structure. We document strong heterogeneity in take-up rates within the treatment villages: farmers who reside in villages where the agents belong to different groups and where the delivery agent has many ties are 11pp more likely to adopt improved seeds and 8.4pp more likely to be trained than farmers in control villages. These effects are roughly four times as large as the effects in villages where: (a) the two agents belong to the same group or (b) where they belong to different groups and the delivery agent has few ties.[37]

Differences in profits and consumption across the treatment villages go in the same direction as the take-up differences but are smaller in magnitude (especially for consump-

by baseline household characteristics.

[36] Relative to control villages, treatment village experience a 25% increase in profits per acre – as measured by revenues (or imputed revenues if self-consumption) minus expenses by acre cultivated. While this effect is large it is not precisely estimated. Unlike other agriculture extension programs, our program targets women farmers which have been shown to benefit more from extension services than men, and especially so when the extension workers are women – as in our case (e.g., O'Sullivan et al. (2014); Kondylis et al. (2015); Buehren et al. (2017)). This might explain why the returns of the program we analyze are so large.

[37] We define an agent to have "many" ("few") ties if the share of farmers in the village who know the delivery agent only is above the mean (below the mean).

tion).[38] This is consistent with our earlier finding that social structure affects both the number and the type of farmers who receive the program. More specifically, it is consistent with the fact that those villages with the highest coverage – those in which the agents belong to different groups and the delivery agent has many ties – are also those with the highest amount of mistargeting and misallocation, and thus with lower program returns *per farmer* targeted. Table 6 corroborates this by showing that the program increases village-level inequality everywhere, but that the effect is much stronger where the agents belong to different parties and the delivery agent has many ties (see Panel B, Columns 5-6). This is also consistent with the quantile treatment effects estimates in Figure 5, which show small effects on profits and consumption throughout the distribution and very large effects in the top decile, and especially so in villages where the agents belong to different parties and the delivery agent has many ties. This pattern is more pronounced for profits than for consumption, which is consistent with the finding that these are the villages where the delivery agent extracts most rents.

6 Conclusion

There has been enormous and long-standing interest in how social structure shapes economic development through its effects on norms, culture, conflict, and cooperation (Easterly and Levine 1997; Burgess et al. 2015; Alesina and Giuliano 2015; Lowes et al. 2017; Moscona et al. 2020). Through these mechanisms, social structure is seen to influence the policies that ultimately affect development outcomes. Development policy here is viewed as being mediated by social structure. Another large literature suggests that social structure fundamentally affects how resources are allocated within village economies (Bardhan 1984; Deaton 1997; Srinivas 1976; Udry 1990; Coate and Ravallion 1993; Stiglitz et al. 1993; Townsend 1994; Besley 1995; Udry 1994, 1995; Munshi 2014, 2019; Munshi and Rosenzweig 2017). We bring these two literatures together by examining whether social structure influences how development policies unfold within village economies. That is, we view development policy through the lens of social structure. When external resources are poured into villages with the objective of improving development outcomes, we want to know whether social structure affects to whom these resources flow and how this affects development outcomes.

The problem is that social structure is largely unobserved by external organizations

[38] Our goal here is to shed light on the heterogeneity of the impact of the program across villages. We do not claim causality: the social structure of a village may correlate with other variables that affect profits/consumption at endline.

trying to deliver resources into village economies. The contribution of this paper, therefore, is to open up the black box of whether and how social structure affects the implementation of development policies. We do this by exploiting the growing reliance on local agents to deliver external programs. By randomly selecting one of two potential local agents for an agricultural extension program in Uganda we are able to look at how social structure – connections between agents and farmers, agents' group affiliation and the interaction of the two – affect the delivery agent's targeting choices and how this differs from the choice of a social planner who maximizes output.

Our overarching conclusion is that social structure has a fundamental impact on the manner in which program resources flow through the village. The same program delivered to villages containing individuals with similar characteristics leads to radically different program outcomes depending on village social structure. This helps us to understand why we see such heterogeneity in program outcomes across seemingly similar communities across the developing world (Buehren et al. 2017; Beaman et al. 2018).

Our analysis also helps us to understand why this is the case. What we find is consistent with agents piling benefits on farmers who are better able to reciprocate favors. Though this same expectation of favor exchange also leads better-connected agents to extend coverage, this coverage is biased towards tied farmers who are less productive or deserving than farmers who are untied but more able to reciprocate favors. This tension disappears when the two potential agents belong to the same party. This indicates that the same intervention, delivered by the same organization in the same context, will take different shapes once it "hits the ground" as social structures direct the flow of resources towards different beneficiaries.

This means that if we cannot observe social structure (as is the case for most development organizations) then the external validity of many policy evaluations may be compromised. What may have worked in one place may not work in another even if the two places are in the same region or country. Similarly, pilot studies of a particular intervention done in a particular locality may not scale to a geography encompassing a range of social structures. This points to the importance of obtaining a better understanding of social structure when considering the implementation of development policies – something that sociologists and anthropologists have been arguing for some time.

Another clear implication from our paper is that there may be a dissonance between the objective of the implementing organization and that of the local agent. On the one hand, the former is intent on enhancing the output-maximizing and poverty-minimizing potential of its program. This necessitates seeking out the most deserving beneficiaries who are best able to make use of treatment, which it might only hope to achieve by mobilizing the insider information of a local delivery agent. By contrast, our findings suggest that this agent is focused on maximizing the share of program benefits that are returned to it. This involves targeting rich villagers to whom it is directly tied, in addition to those whom it is indirectly tied to via a shared group affiliation with the counterfactual agent.

A key implication of this dissonance is that local delivery may increase rather than decrease distortions in resource allocations within villages. Though the choice of better-connected delivery agents can promote wider program coverage, our evidence suggests that this is driven by the treatment of those for whom the program was not initially intended. In particular, the agent's prioritization of not only her own ties, but rich ones, is consistent with the bias being driven by private rents rather than match-specific productivity. As a result within-village inequality both of consumption and profits per acre increasing in treatment versus control villages. This echoes recent findings on community-driven development programs leading to divisions that reduced network-based economic activities in Gambia (Heß et al. 2018). However, favor exchange is at the network level, so when agents themselves belong to the same group, these distortions disappear (Jackson et al. 2012).

Our paper therefore indicates we should be more sanguine about the advantages of local delivery of development programs. The local delivery model intends to exploit the social networks in which agents are embedded, mobilizing insider knowledge of deserving beneficiaries, and harnessing the motivation of local people to help those around them. This approach has also been upheld as a means of upskilling locals to enhance their agency in the development process by creating a professional cadre of treatment providers within the village. Moreover, by removing the need to hire qualified and highly paid workers from outside the village, localization may also reduce turnover and improve the financial viability of development programs. This is especially critical in the context of developing countries where state capacity is particularly weak. Indeed, it has been precisely in these countries that the expansion of the local delivery model has been most rapid. However, these potential benefits must set against the risks associated with affording a local agent ultimate discretion as to whom a program's benefits should be conferred upon.

Our findings therefore should inform the choice of the mode of delivery of development programs. When training costs are low, one option is to hire several agents or even target several beneficiaries directly and to rely on their connections to ensure diffusion. In simple contagion models, this has been shown to yield the same adoption rates as targeting the optimal seed without having to pay the cost of identifying such seed (Akbarpour et al. 2018). The influence of social motives is also likely to interact with other motives such as financial incentives. Evidence from the private sector indicates that sufficiently strong monetary incentives mute social incentives, and such crowd-out is desirable when social incentives lead to an inferior outcome (Ashraf and Bandiera 2018; Bandiera et al. 2009). Moreover, evidence suggests that even small financial rewards can motivate all but the richest agents (BenYishay and Mobarak 2019). This can help alleviate concerns that organizations may be unable to afford the required level of financial incentives. Furthermore, any costs incurred may be low relative to the lost benefits that we identify in this paper.

At the heart of what we have uncovered is that relying solely on social motives renders the success of development programs dependent on pre-existing social structures and divisions that can potentially exacerbate existing inequalities in resource allocations within villages, against all best intentions. We are left with the open question of whether it is possible to create a professional cadre of local agents that retains the desirable features of the local model – better information, lower turnover, lower cost – while aligning the interests of the agents with those of the development agency. This model might combine some features of professional centralized bureaucracies – meritocratic selection, common training, common mission, structured careers, and regular compensation – with the virtues of local delivery. Understanding whether and at what cost this can be achieved is a prerequisite for choosing the optimal delivery mode and achieving the stated goal of helping the poor.

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Figure 1: Research Design

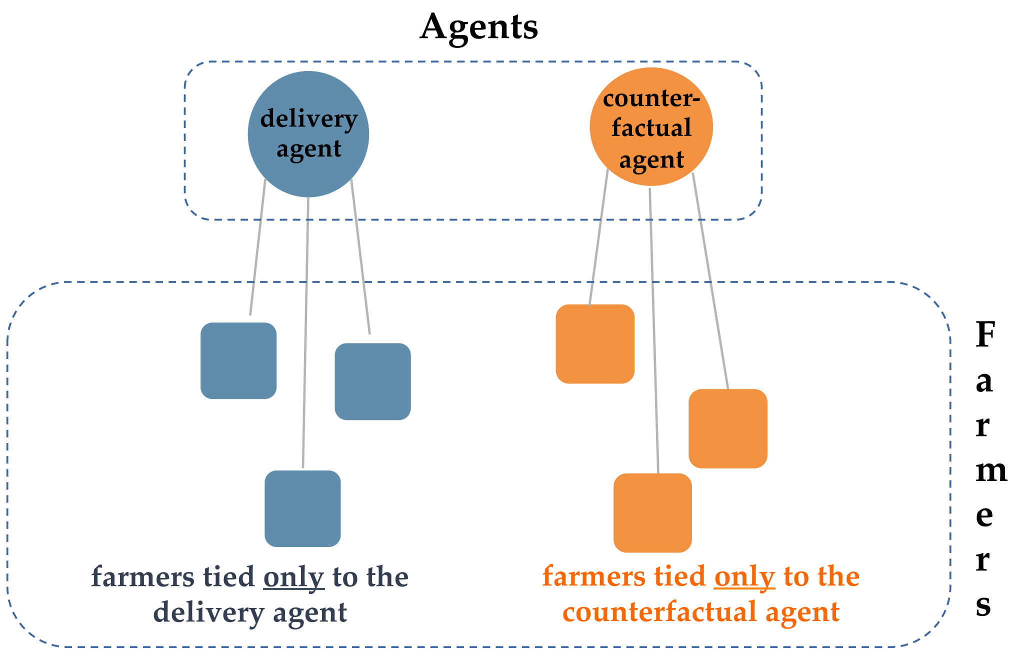


Figure 2: Social Ties Between Farmers and Agents

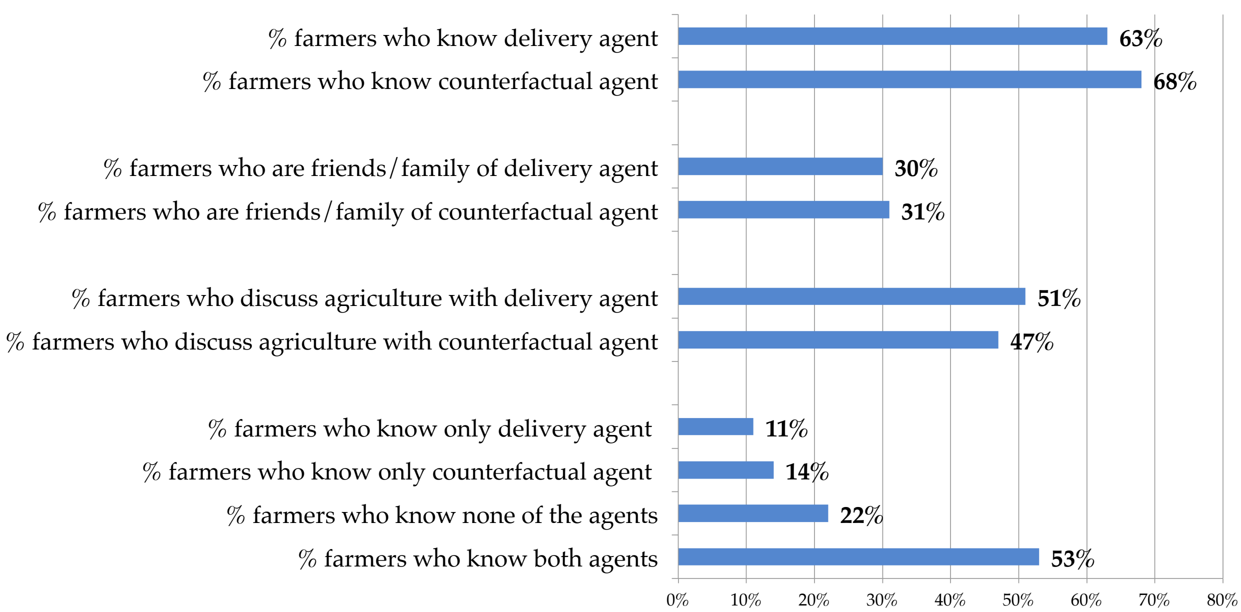
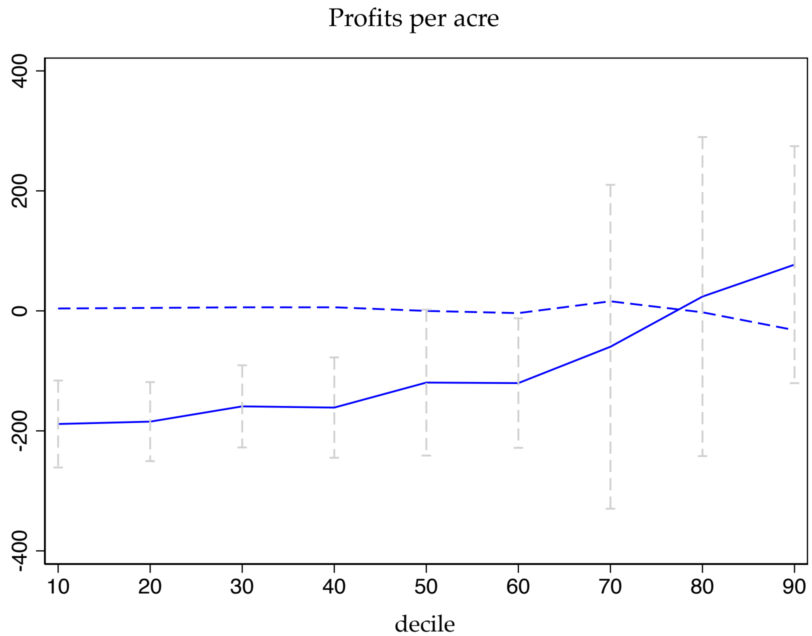
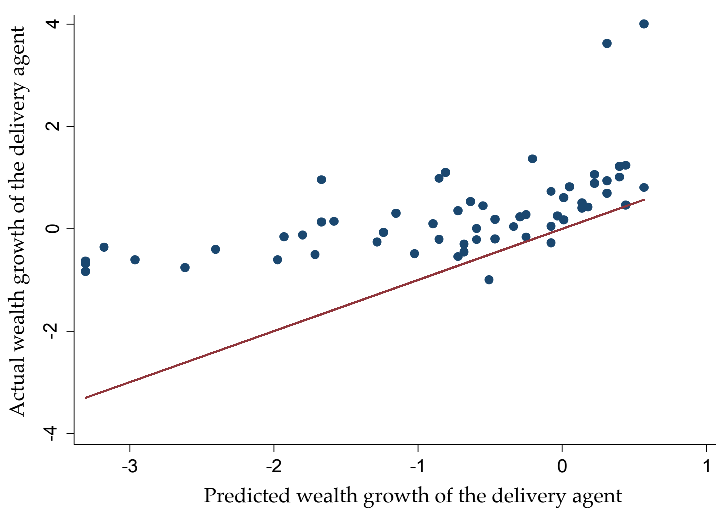


Figure 3: Misallocation



Notes: The solid line plots the difference in endline profits per acre between the treated farmers who are tied to the delivery agent and the treated farmers who are tied to the counterfactual agent controlling for baseline profits per acre, at each decile between 10 and 90. Vertical dashed lines are 95% confidence intervals. The dashed line plots the difference in profits per acre between farmers tied to the delivery agent and farmers tied to the counterfactual agent at baseline. "Profits per acre" are equal to revenues (or imputed revenues if self-consumption) minus expenses divided by acres cultivated in the last season (in thousand of UGX).

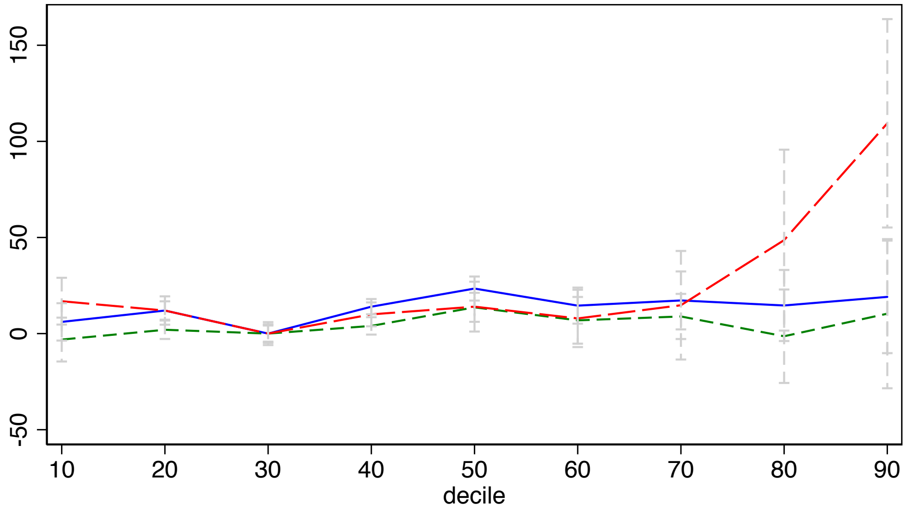
Figure 4: Predicted vs. Actual Wealth Growth of the Delivery Agents



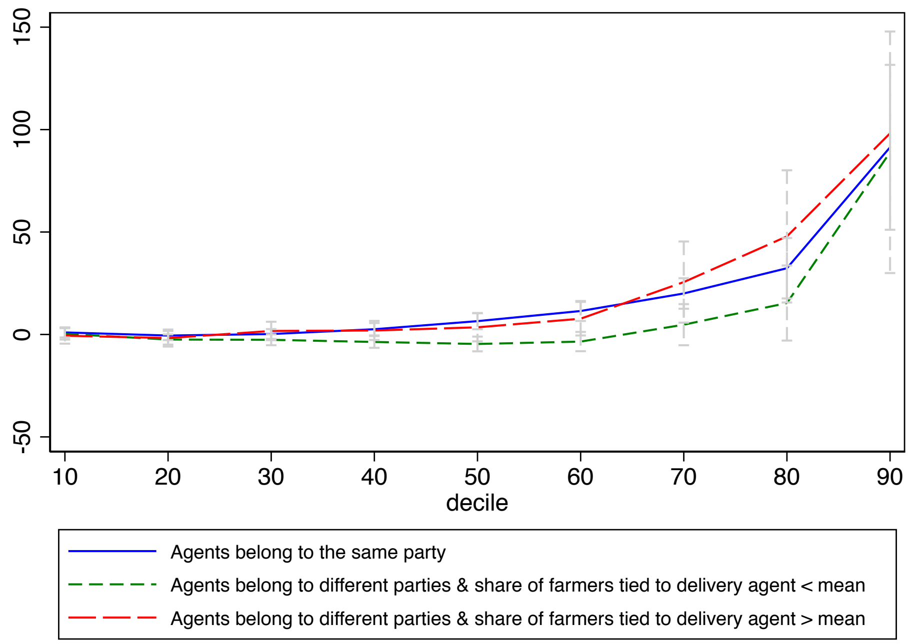
Notes: The figure plots actual wealth growth between baseline and endline against the predicted wealth growth for each of the 60 delivery agents. "Actual wealth growth" is the growth in the number of assets owned by a delivery agent between baseline and endline (in percentage). "Predicted wealth growth" is the predicted value obtained by regressing the actual wealth growth on baseline wealth for the sample of farmers eligible for the delivery agent position in control villages.

Figure 5: Program Evaluation – Quantile Treatment Effects

Panel A. Profits per acre



Panel B. Consumption



Notes: Quantile treatment effects on profits per acre (Panel A) and consumption (Panel B), for three types of villages: villages in which the agents belong to the same party, villages in which the agents belong to different parties and the share of farmers in the village who know only the delivery agents is below or above the mean. Vertical dashed lines are 90% confidence intervals. "Profits per acre" are equal to revenues (or imputed revenues if self-consumption) minus expenses divided by acres cultivated in the last season (in thousand UGX). "Consumption" is the total household consumption per adult equivalent (in thousand UGX).

Table 1: Descriptives and Balance Checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Farmers			Agents		
Sample:	Farmers tied (only) to the delivery agent	Farmers tied (only) to the counterfactual agent	*p-value (1)=(2)*	Delivery agent	Counterfactual agent	Percentile of the delivery agent within her own village
Completed primary education	0.221	0.260	*0.378*	0.617	0.533	88.83
	(0.42)	(0.44)		(0.49)	(0.50)	(4.90)
Acres owned	2.470	2.547	*0.648*	2.949	2.873	94.41
	(4.57)	(5.15)		(2.51)	(2.31)	(5.01)
Number of assets owned	17.977	17.895	*0.302*	42.817	39.550	98.83
	(8.08)	(8.29)		(32.33)	(29.67)	(2.13)
Consumption	182.72	150.59	*0.398*	-	-	-
	(271.05)	(226.39)		-	-	-
Ever received improved seeds	0.224	0.230	*0.192*	0.843	0.800	86.57
	(0.42)	(0.42)		(0.37)	(0.40)	(3.67)
Number of techniques ever adopted	1.344	1.125	*0.276*	1.592	1.674	94.39
	(0.96)	(0.93)		(0.79)	(0.63)	(2.42)
Acres of land cultivated	1.22	1.26	*0.682*	1.58	1.76	95.00
	(0.94)	(1.06)		(1.09)	(1.36)	(4.22)
Profits	82.922	77.625	*0.720*	471.875	585.875	98.75
	(314.03)	(266.86)		(327.63)	(708.68)	(2.31)
Profits per acre	71.734	72.837	*0.750*	219.760	221.656	95.63
	(330.31)	(350.91)		(166.96)	(241.53)	(1.77)
Not interviewed at endline	0.080	0.073	*0.846*	-	-	-
	(0.27)	(0.26)		-	-	-
Observations	*662 farmers*			*120 agents*		

Notes: The table presents summary statistics for farmers who know the delivery agent only (Column 1), farmers who know the counterfactual agent only (Column 2), delivery agents (Column 4) and counterfactual agents (Column 5). Standard errors are presented in parentheses. The p-values reported in Column 3 are estimated with randomization inference using 500 random permutations and controlling for village fixed effects. Column 6 presents summary statistics for the percentile of delivery agent trait within her own village (example: the delivery agent belongs to the 90th percentile if her trait is higher than 90% of the sample farmers in her village). "Consumption" is the total household consumption (food+semi-durables) per adult equivalent (in thousand UGX). "Number techniques ever adopted" calculates the number of good techniques ever adopted (out of 3: inter cropping, line sowing, zero tillage) and the number of bad techniques never adopted (out of 1: mixed cropping). "Acres of land cultivated" are the number of acres cultivated by the household in the last season. "Profits" are equal to revenues (or imputed revenues if self-consumption) minus expenses in the last season (in thousand of UGX). "Profits per acre" are profits divided by acres cultivated. All monetary values are truncated above and below two standard deviations from the mean. "Not interviewed at endline" is an indicator for attrition between baseline and endline.

Table 2: The Effect of Social Structure on Delivery

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Trained by the delivery agent	Received seeds from the delivery agent	Received seeds from BRAC branch office	Received seeds from non-BRAC source	Number of techniques adopted *(measured)*	Number of techniques adopted *(self-reported)*
Farmer is tied to delivery agent	0.0608***	0.0510***	0.0078	0.0108	0.0794***	0.0501*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Observations	2,423	2,430	2,430	2,448	1,327	1,327
R-squared	0.126	0.133	0.076	0.174	0.404	0.385
Mean dep.var. for farmers not tied	0.016	0.008	0.057	0.051	0.088	0.118
Randomization inference						
p-value (Farmer tied=0)	*0.000*	*0.000*	*0.738*	*0.392*	*0.006*	*0.038*

Notes: Farmer-level OLS regressions with robust standard errors clustered at the connection status*village level. "Farmer is tied to the delivery agent" equals 1 if the farmer knows only the delivery agent. The omitted group ("farmers not tied") are farmers who know only the counterfactual agent. All regressions control for village fixed effects, walking distance to delivery agent's home, whether the farmer knows both agents, whether the farmer knows none of the agents. "Trained by the delivery agent" equals 1 if the respondent was trained by the delivery agent in the past year. "Received seeds from the delivery agent" equals 1 if the respondent received seeds from the delivery agent in the past year. "Received seeds from the delivery agent (BRAC branch office/non-BRAC source)" equals 1 if the respondent received seeds from the delivery agent (bought seeds from the BRAC branch office/non-BRAC source) in the past year. "Number of techniques adopted" is measured in two ways: (1) by asking enumerators to go check the plot of land of a random sub-sample of 60% of our endline respondents and report whether the technique was adopted ("measured"), (2) by asking respondents to self-report the number of techniques adopted. Columns 5 and 6 are restricted to the sample of farmers for whom the plot of land was "measured." *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3: The Effect of Social Structure on Delivery – Continued

	(1)	(2)
Dependent variable	Trained by the delivery agent	Received seeds from the delivery agent
(1) Farmer is tied to delivery agent & agents belong to the same party	-0.0025	0.0036
	(0.02)	(0.02)
(2) Farmer is tied to delivery agent & agents belong to different parties	0.0975***	0.0878***
	(0.03)	(0.02)
Observations	2,218	2,225
R-squared	0.137	0.142
Mean dep.var.	0.038	0.034
Mean dep.var. for farmers not tied & agents belong to the same party	0.010	0.005
Mean dep.var. for farmers not tied & agents belong to different parties	0.033	0.016
p-value (1) = (2)	0.007	0.006
Randomization inference		
p-value (Farmer tied=0) if agents belong to the same party	*0.926*	*0.804*
p-value (Farmer tied=0) if agents belong to different parties	*0.000*	*0.000*

Notes: Farmer-level OLS regressions with robust standard errors clustered at the connection status*village level. "Farmer is tied to the delivery agent" equals 1 if the farmer knows only the delivery agent. The omitted group ("farmers not tied") are farmers who know only the counterfactual agent. All regressions control for village fixed effects, walking distance to delivery agent's home, whether the farmer knows both agents, whether the farmer knows none of the agents, and the interaction of the latter two variables with "agents belong to the same party." *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: The Effect of Social Structure on Pro-Poor Targeting

	(1)	(2)	(3)	(4)
Sample of villages	**Agents belong to different parties**		**Agents belong to the same party**	
Dependent variable	Trained by the delivery agent	Received seeds from the delivery agent	Trained by the delivery agent	Received seeds from the delivery agent
Farmer is in bottom 25% of consumption	-0.0003	-0.0054	0.0005	0.0001
	(0.01)	(0.01)	(0.01)	(0.01)
Farmer is tied to the delivery agent & in bottom 25% of consumption	0.0418*	0.0562***	0.0235	-0.0140
	(0.02)	(0.02)	(0.05)	(0.01)
Farmer is tied to the delivery agent & not in bottom 25% of consumption	0.1182***	0.1002***	-0.0081	0.0074
	(0.03)	(0.03)	(0.02)	(0.02)
Observations	1,024	1,025	1,194	1,200
R-squared	0.193	0.225	0.082	0.068
Mean for farmers not tied & not bottom	0.006	0.006	0.031	0.010
p-value (tied & not bottom = tied & bottom)	0.001	0.000	0.454	0.216
p-value (tied & not bottom = not tied & bottom)	0.000	0.000	0.687	0.740
Randomization inference				
p-value (Farmer tied & bottom=0)	*0.586*	*0.008*	*0.530*	*0.472*
p-value (Farmer tied & not bottom=0)	*0.000*	*0.000*	*0.772*	*0.368*

Notes: Farmer-level OLS regressions with robust standard errors clustered at the connection status*village level. "Farmer is tied to the delivery agent" equals 1 if the farmer knows only the delivery agent. "Farmer is in the bottom 25% of consumption" equals 1 if the farmer is in the bottom quartile of the baseline distribution of food consumption per adult equivalent in her own village. The omitted group ("farmers not tied & not bottom") are farmers who know only the counterfactual agent and who are not in the bottom 25% of consumption. All regressions control for village fixed effects, the walking distance to delivery agent's home, whether the farmer knows both agents, whether the farmer knows none of the agents, and the interaction of the latter two variables with "Farmer is in the bottom 25% of consumption." *** p<0.01, ** p<0.05, * p<0.1.

Table 5: The Effect of Social Structure on Coverage

	(1)	(2)	(3)	(4)
Dependent variable	Number of farmers trained by the delivery agent	Number of farmers who received seeds from the delivery agent	Number of farmers trained by the delivery agent	Number of farmers who received seeds from the delivery agent
Number of farmers tied to delivery agent	0.2550***	0.2158***		
	(0.09)	(0.08)		
Agents belong to the same party	-0.1026	0.1288	0.1275	0.4147
	(0.57)	(0.55)	(0.78)	(0.78)
Number of farmers tied to delivery agent * agents belong to the same party			0.0872	0.1230
			(0.19)	(0.15)
Number of farmers tied to delivery agent * agents belong to different parties			0.2926***	0.2254**
			(0.11)	(0.10)
Observations (# villages)	53	53	53	53
R-squared	0.211	0.203	0.228	0.215
Mean dependent variable	1.550	1.367	1.550	1.367
Randomization inference				
p-value (Number tied=0)	*0.004*	*0.000*	-	-
p-value (Number tied=0) if agents belong to the same party	-	-	*0.994*	*0.788*
p-value (Number tied=0) if agents belong to different parties	-	-	*0.042*	*0.058*

Notes: Village-level OLS regressions with robust standard errors in parentheses. The number of farmers tied to the delivery agent is the number of sample farmers who know the delivery agent only. All regressions control for the number of exclusive ties (farmers who know one of the two agents only), the number of farmers in the village and branch fixed effects. The number of farmers trained by the delivery agent (who received seeds from the delivery agent) is the sum of the sample farmers who report being trained by (received seeds from) the delivery agent. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: The Effect of Social Structure on Program Evaluation

Dependent variable	(1) Trained by the delivery agent	(2) Received seeds	(3) Profits per acre	(4) Consump-tion	(5) Village inequality: Profits per acre	(6) Village inequality: Consump-tion
Panel A: Overall ITT						
Agri extension program	0.0365***	0.0831***	16.0041	24.9477*	26.0876**	40.1664**
	(0.01)	(0.01)	(11.27)	(13.42)	(10.62)	(15.46)
Observations	4,381	4,410	3,419	4,333	117	119
R-squared	0.017	0.043	0.010	0.027	0.124	0.261
Mean in control	0.000	0.003	64.61	110.6	115.4	103.5
Panel B: ITT by Social Structure						
(1) Agri extension program & agents belong to the same party	0.0343***	0.0844***	17.5900	33.3023*	30.3224**	50.0475**
	(0.01)	(0.01)	(14.29)	(19.62)	(12.58)	(22.20)
(2) Agri extension program & agents belong to different parties & share of farmers tied to delivery agent < mean	0.0239***	0.0709***	9.1705	19.6830	9.6180	33.8178*
	(0.01)	(0.02)	(14.10)	(21.72)	(15.94)	(19.38)
(3) Agri extension program & agents belong to different parties & share of farmers tied to delivery agent ≥ mean	0.0841***	0.1100***	31.3106*	37.3283**	67.2649**	61.2141*
	(0.03)	(0.03)	(16.37)	(18.12)	(31.16)	(35.01)
Observations	4,159	4,186	3,253	4,113	110	112
R-squared	0.029	0.047	0.012	0.030	0.188	0.283
Mean in control	0.000	0.003	64.612	110.640	115.365	103.487
p-value (1)=(2)	0.321	0.459	0.587	0.627	0.227	0.526
p-value (1)=(3)	0.099	0.436	0.431	0.868	0.258	0.774
p-value (2)=(3)	0.050	0.251	0.187	0.484	0.095	0.458

Notes: Farmer-level OLS regressions with robust standard errors clustered at the village level in Columns 1-4. Village-level OLS regressions with robust standard errors in Columns 5-6. Panel A presents the overall ITT by comparing treatment villages ("Agriculture extension program" =1) to control villages ("Agriculture extension program" =0). Panel B divides treatment villages into three types: (1) villages in which the agents belong to the same party, (2) villages in which the agents belong to different parties and the share of farmers who know only the delivery agent is below the mean, and (3) villages in which the agents belong to different parties and the share of farmers who know only the delivery agent is above the mean. "Profits per acre" are equal to revenues (or imputed revenues if self-consumption) minus expenses divided by acres cultivated in the last season. "Consumption" is the total household consumption per adult equivalent. Village inequality is measured as the within-village standard deviation of profits per acre or consumption.

Online Appendix – Not for Publication

A Appendix: Tables and Figures

Figure A1: Project Timeline

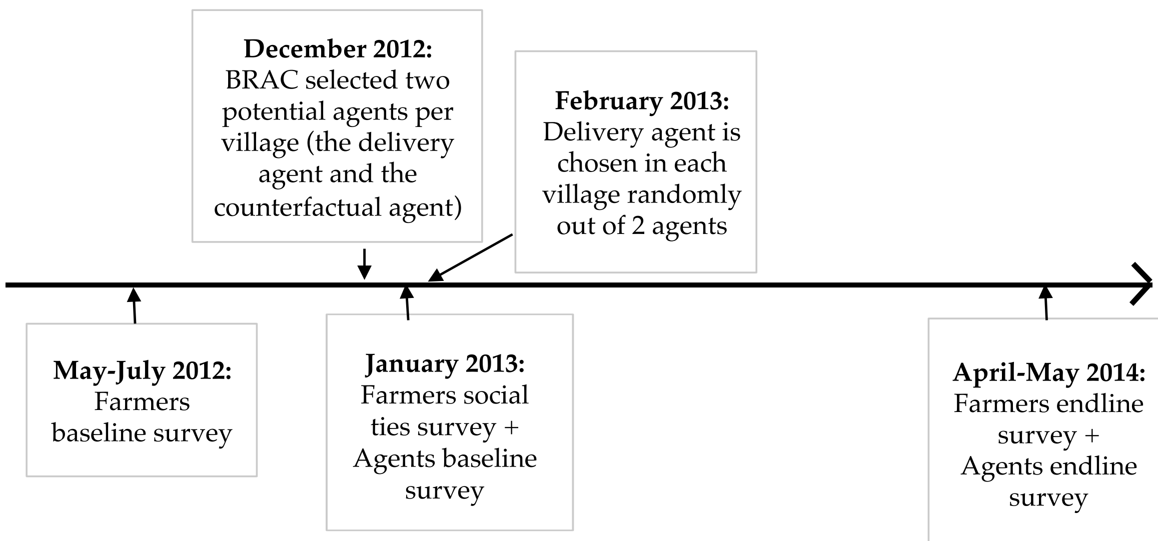
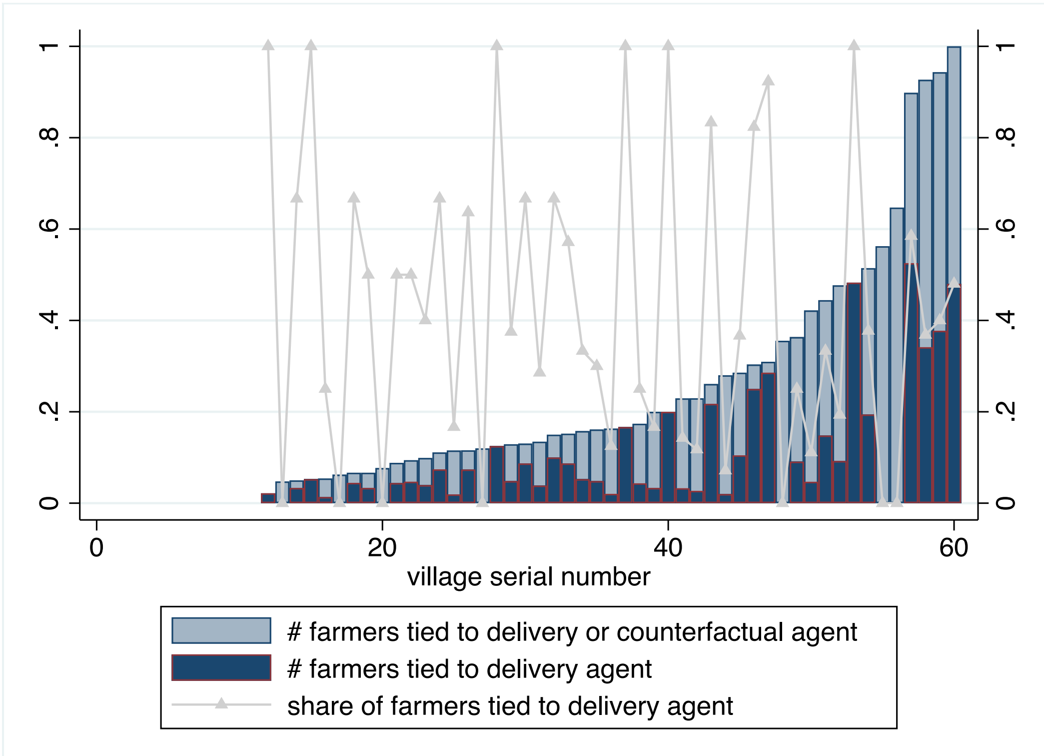


Figure A2: Variation in the Share of Farmers Tied to the Delivery Agent across Villages



Notes: The light blue histogram is the number of farmers in the village who know either only the delivery agent or only the counterfactual agent. The dark blue histogram is the number of farmers in the village who know only the delivery agent. The grey line is the fraction between the dark blue histogram and the light blue one. Villages are sorted based on the former.

Table A1: Balance Checks with Alternative Definitions of Farmer-Agent Ties

	(1)	(2)	(3)	(4)	(5)	(6)
Measure of Farmer-Agent Tie	**Friend or family**			**Discusses agriculture**		
Sample:	Farmers tied (only) to the delivery agent	Farmers tied (only) to the counterfactual agent	*p-value (1)=(2)*	Farmers tied (only) to the delivery agent	Farmers tied (only) to the counterfactual agent	*p-value (4)=(5)*
Completed primary education	0.223	0.196	*0.444*	0.220	0.260	*0.798*
	(0.42)	(0.40)		(0.42)	(0.44)	
Acres owned	2.158	2.663	*0.116*	2.179	2.536	*0.602*
	(2.82)	(5.54)		(4.17)	(5.49)	
Number of assets owned	17.719	17.233	*0.848*	18.693	17.895	*0.636*
	(8.22)	(7.56)		(8.40)	(7.94)	
Consumption	188.28	129.47	*0.122*	136.91	155.06	*0.672*
	(260.77)	(138.66)		(160.37)	(295.70)	
Ever received improved seeds	0.215	0.217	*0.404*	0.315	0.295	*0.634*
	(0.41)	(0.41)		(0.47)	(0.46)	
Number of techniques ever adopted	1.372	1.053	*0.450*	1.389	1.184	*0.342*
	(0.99)	(0.90)		(0.95)	(0.92)	
Acres of land cultivated	1.22	1.36	*0.322*	1.18	1.18	*0.924*
	(0.93)	(1.22)		(0.85)	(0.93)	
Profits	81.379	80.724	*0.546*	86.710	65.447	*0.546*
	(343.84)	(300.58)		(336.93)	(257.19)	
Profits per acre	50.891	73.854	*0.496*	88.308	70.968	*0.842*
	(250.74)	(388.10)		(407.80)	(383.01)	
Not interviewed at endline	0.086	0.063	*0.662*	0.054	0.073	*0.422*
	(0.28)	(0.24)		(0.23)	(0.26)	
Observations	*328 farmers*			*556 farmers*		

Notes: The table presents summary statistics for farmers who are friend or family member of the delivery agent only (Column 1), farmers who are friend or family member of the counterfactual agent only (Column 2), farmers who regularly discuss agriculture with the delivery agent only (Column 4), farmers who regularly discuss agriculture with the counterfactual agent only (Column 5), Standard errors are presented in parentheses. The p-values reported in Columns 3 and 6 are estimated with randomization inference using 500 random permutations and controlling for village fixed effects. "Consumption" is the total household consumption (food+semi-durables) per adult equivalent (in thousand UGX). "Number techniques ever adopted" calculates the number of good techniques ever adopted (out of 3: inter cropping, line sowing, zero tillage) and the number of bad techniques never adopted (out of 1: mixed cropping). "Acres of land cultivated" are the number of acres cultivated by the household in the last season. "Profits" are equal to revenues (or imputed revenues if self-consumption) minus expenses in the last season (in thousand of UGX). "Profits per acre" are profits divided by acres cultivated. All monetary values are truncated above and below two standard deviations from the mean. "Not interviewed at endline" is an indicator for attrition between baseline and endline.

Table A2: Balance Checks – Village Infrastructure

Sample of villages	(1) Agents belong to different parties	(2) Agents belong to the same party	(3) *p-value (1)=(2)*	(4) Share of farmers tied to the delivery agent ≤ median	(5) Share of farmers tied to the delivery agent > median	(6) *p-value (4)=(5)*
Panel A: Village Infrastructure						
Share of votes to incumbent party in 2011 presidential election	0.594	0.583	*0.249*	0.621	0.562	*0.195*
	(0.11)	(0.11)		(0.10)	(0.12)	
Share of votes to main party in 2011 presidential election	0.624	0.615	*0.133*	0.636	0.612	*0.660*
	(0.08)	(0.08)		(0.08)	(0.07)	
Number of farmers in the village	88.593	84.631	*0.820*	81.223	85.821	*0.940*
	(34.33)	(36.13)		(39.93)	(28.29)	
Minutes to the BRAC branch (walking)	107.098	95.447	*0.759*	106.080	94.008	*0.141*
	(60.14)	(56.83)		(56.00)	(59.51)	
Minutes to closest market (walking)	69.381	77.761	*0.219*	77.905	68.470	*0.211*
	(50.09)	(48.82)		(45.28)	(50.04)	
Minutes to main road (walking)	1.844	2.479	*0.223*	1.493	2.358	*0.445*
	(3.36)	(6.02)		(3.24)	(5.70)	
Road usable during rainy season (1=yes)	0.584	0.487	*0.307*	0.617	0.528	*0.478*
	(0.39)	(0.41)		(0.41)	(0.40)	
Electricity (=1 if available)	0.460	0.409	*0.595*	0.406	0.461	*0.330*
	(0.43)	(0.43)		(0.44)	(0.44)	
Television broadcast (=1 if available)	0.665	0.687	*0.855*	0.643	0.729	*0.841*
	(0.46)	(0.46)		(0.47)	(0.43)	
Newspapers (=1 if available)	0.147	0.091	*0.518*	0.082	0.150	*0.329*
	(0.33)	(0.23)		(0.26)	(0.29)	
Mobile coverage (=1 if available)	0.789	0.658	*0.197*	0.781	0.709	*0.470*
	(0.39)	(0.48)		(0.41)	(0.43)	
Minutes from average farmer to delivery agent (walking)	1.969	1.479	*0.217*	1.528	1.790	*0.339*
	(1.08)	(1.15)		(1.03)	(1.16)	
Share of farmers tied to delivery agent	0.118	0.068	*0.378*	0.016	0.160	*0.000*
	(0.16)	(0.09)		(0.03)	(0.15)	
Share of farmers tied to counterfactual agent	0.178	0.099	*0.343*	0.141	0.124	*0.516*
	(0.20)	(0.16)		(0.17)	(0.18)	
Panel B: Characteristics of the selected delivery agents						
Completed primary education	0.600	0.633	*0.971*	0.577	0.667	*0.725*
	(0.50)	(0.49)		(0.50)	(0.48)	
Acres owned	2.603	3.283	*0.773*	2.760	3.389	*0.354*
	(2.18)	(2.79)		(2.13)	(2.92)	
Number of assets owned	41.967	43.667	*0.282*	39.000	46.000	*0.990*
	(29.86)	(35.13)		(30.16)	(35.42)	
Ever received improved seeds	0.821	0.870	*0.208*	0.857	0.792	*0.518*
	(0.39)	(0.34)		(0.36)	(0.41)	
Number of techniques ever adopted	1.680	1.500	*0.930*	1.444	1.680	*0.245*
	(0.75)	(0.83)		(0.78)	(0.75)	
Acres of land cultivated	1.517	1.650	*0.925*	1.481	1.833	*0.201*
	(1.05)	(1.13)		(0.93)	(1.22)	
Profits	618.667	383.800	*0.623*	537.600	362.333	*0.843*
	(305.67)	(339.52)		(246.07)	(475.18)	
Profits per acre	310.833	165.117	*0.050*	304.767	78.083	*0.236*
	(7.78)	(196.97)		(124.38)	(135.82)	
Number of villages	26	27		30	30	

Notes: This table presents village-level summary statistics on village infrastructure (Panel A) and delivery agents' characteristics (Panel B) for different sub-samples of villages: villages in which the agents belong to different parties (Column 1) vs. belong to the same party (Column 2), and share of farmers who know the delivery agent only is above the median (Column 3) vs. below the median (Column 4). All p-values are estimated with randomization inference using 500 random permutations and controlling for branch fixed effects.

Table A3: The Effect of Social Structure on Delivery (Coverage) with (without) Agents' Group Affiliation as Control

	(1)	(2)	(3)	(4)
	Delivery		**Coverage**	
Dependent variable	Trained by the delivery agent	Received seeds from the delivery agent	Number of farmers trained by the delivery agent	Number of farmers who received seeds from the delivery agent
Farmer is tied to delivery agent	0.0697***	0.0622***		
	(0.03)	(0.02)		
Agents belong to the same party	-0.0065	-0.0026		
	(0.01)	(0.01)		
Number of farmers tied to delivery agent			0.2478***	0.2310***
			(0.08)	(0.07)
Observations	2,218	2,225	60	60
R-squared	0.014	0.013	0.214	0.213
Mean dependent variable	0.016	0.008	1.550	1.367

Notes: **Columns 1-2**; Farmer-level OLS regressions with robust standard errors clustered at the connection status*village level. "Farmer is tied to the delivery agent" equals 1 if the farmer knows only the delivery agent. The omitted group are farmers who know only the counterfactual agent. Regressions control for village fixed effects, walking distance to delivery agent's home, whether the farmer knows both agents, whether the farmer knows none of the agents. **Columns 3-4**; Village-level OLS regressions with robust standard errors in parentheses. The number of farmers tied to the delivery agent is the number of sample farmers who know the delivery agent only. Regressions control for branch fixed effects, the number of exclusive ties (farmers who know one of the two agents only), the number of farmers in the village. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A4: The Effect of Social Structure on Delivery with Alternative Definitions of Farmer-Agent Ties

	(1)	(2)	(3)	(4)
Measure of Farmer-Agent Tie	**Friend or family**		**Discusses agriculture**	
Dependent variable	Trained by the delivery agent	Received seeds from the delivery agent	Trained by the delivery agent	Received seeds from the delivery agent
Farmer is tied to delivery agent	0.0666***	0.0456**	0.0311	0.0326**
	(0.03)	(0.02)	(0.02)	(0.02)
Observations	2,423	2,430	2,089	2,095
R-squared	0.124	0.130	0.120	0.135
Mean for farmers not tied	0.023	0.017	0.024	0.014

Notes: Farmer-level OLS regressions with robust standard errors clustered at the connection status*village level. "Farmer is tied to the delivery agent" equals 1 if the farmer is a friend or family member of the delivery agent only (Columns 1-2); if the farmer regularly discusses agriculture with the delivery agent only (Columns 3-4). The omitted group ("farmers not tied") are farmers tied only to the counterfactual agent. All regressions control for village fixed effects, for the walking distance to delivery agent's home, for whether the farmer is tied to both agents and whether the farmer is tied to none of the agents. The omitted group are farmers tied only to the counterfactual agent. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A5: The Effect of Social Structure on Delivery with Alternative Measures of Agents' Group Affiliation

	(1)	(2)	(3)	(4)
Measure of group affiliation	**Politics (IAT)**		**Friends or family**	
Dependent variable	Trained by the delivery agent	Received seeds from the delivery agent	Number of farmers trained by the delivery agent	Number of farmers who received seeds from the delivery agent
(1) Farmer is tied to delivery agent & agents belong to the same group	0.0144	0.0200	0.0175	0.0208
	(0.03)	(0.02)	(0.02)	(0.02)
(2) Farmer is tied to delivery agent & agents belong to different groups	0.1137***	0.0934***	0.1092***	0.1005***
	(0.03)	(0.03)	(0.04)	(0.04)
Observations	2,067	2,074	2,245	2,252
R-squared	0.143	0.141	0.139	0.142
Mean dep.var.	0.038	0.034	0.038	0.034
Mean dep.var. for farmers not tied & same group	0.014	0.007	0.006	0.000
Mean dep.var. for farmers not tied & different groups	0.021	0.010	0.025	0.019
p-value (1) = (2)	0.014	0.040	0.056	0.062

Notes: Farmer-level OLS regressions with robust standard errors clustered at the connection status*village level in parentheses. "Agents belong to the same group" indicates that agents share the same political affiliation in the implicit association test (Columns 1-2) or that they are friends or family members (Columns 3-4). "Farmer is tied to the delivery agent" equals 1 if the farmer knows only the delivery agent. The omitted group ("farmers not tied") are farmers who know only the counterfactual agent. All regressions control for village fixed effects, walking distance to delivery agent's home, whether the farmer knows both agents, whether the farmer knows none of the agents, and the interaction of the latter two variables with "agents belong to the same group." *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A6: The Effect of Social Structure on Delivery with Extra Village-Level Controls

	(1)	(2)	(3)	(4)	(5)	(6)
Measure of group affiliation	**Politics (Main)**		**Politics (IAT)**		**Friends or family**	
Dependent variable	Trained by the delivery agent	Received seeds from the delivery agent	Trained by the delivery agent	Received seeds from the delivery agent	Trained by the delivery agent	Received seeds from the delivery agent
Farmer is tied to delivery agent & agents belong to the same group	-0.0018	0.0112	-0.0234	-0.0009	0.0226	0.0252
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Farmer is tied to delivery agent & agents belong to different groups	0.0751***	0.0654***	0.1024***	0.0753***	0.1145***	0.1181***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)
Observations	2,197	2,204	2,053	2,060	2,224	2,231
R-squared	0.161	0.158	0.166	0.159	0.161	0.159
Mean dependent variable	0.038	0.034	0.038	0.034	0.038	0.034
p-value (1)= (2)	0.008	0.035	0.000	0.040	0.015	0.020

Notes: Farmer-level OLS regressions with robust standard errors clustered at the connection status*village level. "Agents belong to the same group" is a village-level dummy for whether the delivery agent and the counterfactual agent belong to the same party as self-reported (Columns 1-2), have same political affiliations as measured in the implicit association test (Columns 3-4), or whether they are friends or family members (Columns 5-6). "Farmer is tied to the delivery agent" equals 1 if the farmer knows only the delivery agent. All regressions control for village controls (all demeaned and measured at baseline), each interacted with whether the farmer knows the delivery agent only, knows the counterfactual agent only, and knows both agents. The village controls are: polarization (share of votes to the main party in the 2011 presidential election), minutes to the BRAC branch, minutes to closest market, distance to the main road, the road is usable during the rainy season, access to electricity, access to the television broadcast, access to newspapers, mobile coverage, the average distance from the house of the average farmer and the house of the delivery agent. All regressions also control for village fixed effects, the walking distance to delivery agent's home, whether the farmer knows both agents, whether the farmer knows none of the agents and the interaction of the latter two variables with "agents belong to the same group."
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A7: The Effect of Social Structure on Pro-Poor Targeting with Alternative Measures of Wealth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample of villages	**Agents belong to different parties**				**Agents belong to the same party**			
Variable X	Value of assets owned		Food insecurity		Value of assets owned		Food insecurity	
Dependent variable	Trained by the delivery agent	Received seeds from the delivery agent	Trained by the delivery agent	Received seeds from the delivery agent	Trained by the delivery agent	Received seeds from the delivery agent	Trained by the delivery agent	Received seeds from the delivery agent
Farmer is in bottom 25% of X	-0.0031	-0.0016	-0.0188	-0.0204	0.0044	-0.0091	0.0128	-0.0156
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
Farmer is tied to the delivery agent & in bottom 25% of X	0.0379	0.0084	0.0135	0.0153	-0.0358**	0.0254	-0.0610**	0.1538
	(0.04)	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)	(0.14)
Farmer is tied to the delivery agent & not in bottom 25% of X	0.1176***	0.1147***	0.0989***	0.0892***	0.0089	-0.0041	0.0016	-0.0067
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.01)
Observations	1,024	1,025	1,024	1,025	1,194	1,200	1,194	1,200
R-squared	0.194	0.231	0.191	0.224	0.083	0.068	0.082	0.071
Mean for farmers not tied & not bottom	0.012	0.006	0.010	0.005	0.033	0.022	0.035	0.017
p-value (tied & not bottom=tied & bottom)	0.165	0.020	0.001	0.003	0.167	0.632	0.081	0.308
p-value (tied & not bottom=not tied & bottom)	0.001	0.000	0.000	0.000	0.885	0.774	0.721	0.730

Notes: Farmer-level OLS regressions with robust standard errors clustered at the connection status*village level. "Farmer is tied to the delivery agent" equals 1 if the farmer knows only the delivery agent. "Farmer is in the bottom 25% of X" equals 1 if the farmer is in the bottom quartile of the baseline distribution of variable X in her village; where variable X = {total value of assets owned; food security (i.e., skipping meals or eating reduced portions less than once a week)}. The omitted group ("farmers not tied & not bottom") are farmers who know only the counterfactual agent and who are not in the bottom 25% of X. All regressions control for village fixed effects, the walking distance to delivery agent's home, whether the farmer knows both agents, whether the farmer knows none of the agents, and the interaction of the latter two variables with "Farmer is in the bottom 25% of X." *** p<0.01, ** p<0.05, * p<0.1.

Table A8: The Effect of Social Structure on Agent Excess Wealth Growth

	(1)	(2)
Dependent variable	Excess wealth growth of the delivery agent (actual-predicted)	
Number of farmers tied to delivery agent	0.1081***	
	(0.04)	
Number of farmers tied to delivery agent & agents belong to the same party		0.0272
		(0.04)
Number of farmers tied to delivery agent & agents belong to different parties		0.1405***
		(0.05)
R-squared	0.169	0.187

Notes: Village-level OLS regressions with robust standard errors in parentheses. The dependent variable calculates, for each delivery agent, the log of excess wealth growth (actual wealth growth minus predicted wealth growth), where "actual wealth growth" is the growth in the number of assets owned by the delivery agent between baseline and endline (in percentage), and "predicted wealth growth" is the predicted value obtained by regressing the actual wealth growth on baseline wealth for the sample of farmers eligible for the delivery agent position in control villages. The number of farmers tied to the delivery agent is the number of sample farmers who know the delivery agent only. All regressions control for the total number of farmers who know one of the two agents only (total number of exclusive ties). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A9: The Effect of Social Structure on Coverage with Alternative Measures of Agents' Group Affiliation

	(1)	(2)	(3)	(4)
Measure of group affiliation	**Politics (IAT)**		**Friends or family**	
Dependent variable	Number of farmers trained by the delivery agent	Number of farmers who received seeds from the delivery agent	Number of farmers trained by the delivery agent	Number of farmers who received seeds from the delivery agent
Agents belong to the same group	-1.1386	-1.0591	-0.3844	-0.2442
	(0.96)	(1.02)	(0.93)	(0.96)
Number of farmers tied to delivery agent * agents belong to the same group	0.0410	0.0824	0.2318*	0.2321**
	(0.14)	(0.10)	(0.12)	(0.10)
Number of farmers tied to delivery agent * agents belong to different groups	0.3676**	0.3518**	0.3884**	0.4028**
	(0.16)	(0.15)	(0.16)	(0.15)
Observations	47	47	55	55
R-squared	0.270	0.243	0.243	0.244
Mean dependent variable	1.550	1.367	1.550	1.367

Notes: Village-level OLS regressions with robust standard errors in parentheses. "Agents belong to the same group" indicates that agents share the same political affiliation in the implicit association test (Columns 1-2) or that they are friends or family members (Columns 3-4). All regressions control for branch fixed effects, the number of exclusive ties (farmers who know one of the two agents only), the number of farmers in the village. The number of farmers trained by the delivery agent (who received seeds from the delivery agent) equals the sum of the sample farmers who report being trained by (received seeds from) the delivery agent. The number of farmers tied to the delivery agent is the number of sample farmers who know the delivery agent only. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A10: Program Evaluation – Balance Checks

	(1)	(2)	(3)
	Control villages	Treated villages: Agriculture Extension Program	*p-value (1)=(2)*
Completed primary education	0.218	0.247	*0.327*
	(0.41)	(0.43)	
Acres owned	2.030	2.169	*0.712*
	(3.73)	(3.38)	
Number of assets owned	(18.74)	(18.20)	*0.296*
	(8.31)	(8.46)	
Consumption	167.412	168.698	*0.624*
	(357.96)	(312.28)	
Ever received improved seeds	0.372	0.297	*0.954*
	(0.48)	(0.46)	
Number of techniques ever adopted	1.263	1.274	*0.155*
	(0.91)	(0.89)	
Acres of land cultivated	1.091	1.210	*0.062*
	(0.96)	(1.02)	
Profits	74.402	82.891	*0.272*
	(313.88)	(304.10)	
Profits per acre	72.228	83.668	*0.106*
	(482.60)	(456.37)	

Notes: The table presents summary statistics for farmers in control villages (Column 1) and treatment villages (Column 2). Standard errors are presented in parentheses. The p-values reported in Columns 3 are obtained from regressing each of the reported baseline variables on the dummy for "treatment" with robust standard errors clustered at the village level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A11: Program Evaluation – Attrition

Dependent variable =1 if respondent was not surveyed at endline (attrition)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VAR:	-	Completed primary education	Acres owned	Number of assets owned	Consump-tion	Ever received improved seeds	Number of techniques ever adopted	Acres of land cultivated	Profits	Profits per acre
Agri extension program	0.0150	0.0087	0.0172	-0.0200	0.0168	0.0064	-0.0148	0.0123	0.0110	0.0099
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
VAR		0.0236**	-0.0001	-0.0007	0.0000	-0.0235**	-0.0152*	-0.0014	-0.0000	0.0000
		(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Agri extension program* VAR		0.0125	-0.0009	0.0019	-0.0000	0.0234	0.0236**	0.0019	0.0000	0.0000
		(0.02)	(0.00)	(0.00)	(0.00)	(0.02)	(0.01)	(0.01)	(0.00)	(0.00)
Observations	4,741	4,618	4,730	4,741	4,723	4,649	4,720	4,530	4,437	4,233
R-squared	0.014	0.018	0.015	0.016	0.014	0.016	0.016	0.011	0.010	0.007
Mean in Control	0.059	0.059	0.059	0.059	0.059	0.059	0.059	0.059	0.059	0.059

Notes: Farmer-level OLS regressions with robust standard errors clustered at the village level. The dependent variable equals one if the household female head was interviewed at baseline but not at endline. "Agri extension program" equals 1 for treatment villages and 0 for control villages. The variable is interacted with " VAR" in Columns 2-10 (see columns headings).

B Appendix: Model

B.1 Heterogeneous costs

This section extends the model by allowing the agent's cost of treating a farmer to be lower for ties than non-ties ($c(1, h_v) \leq c(0, h_v)$), e.g., because of lower communication costs. Consider the following modification of the agent's problem:

$$\max_{(T_i)_{i=1}^N \in \{0,1\}^N} \sum_{i=1}^{N} T_i \big[s(d_i, h_v) A(d_i) \theta_i - c(d_i, h_v) \big]$$

where the marginal cost of treating an additional person $c(t_i, h_v)$ is assumed to be multiplicatively separable into a constant and a heterogenous component: $f\big(\tilde{c}(d_i, h_v)\big) g(c)$.[39] We can rewrite the definition of the optimal cutoff as:

$$\widehat{\theta}_i^{DA*'}(d_i, h_v) = \frac{f\big(\tilde{c}(d_i, h_v)\big) g(c)}{s(d_i, h_v) A(t_i)} \Leftrightarrow \frac{s(d_i, h_v) A(t_i) \widehat{\theta}_i^{DA*'}(d_i, h_v)}{f\big(\tilde{c}(d_i, h_v)\big)} = g(c)$$

so that

$$\tilde{s}(0, h_v) A(0) \widehat{\theta}_i^{DA*'}(0, h_v) = \tilde{s}(1, h_v) A(1) \widehat{\theta}_i^{DA*'}(1, h_v).$$

where $\tilde{s}(d_i, h_v) = \frac{s(d_i, h_v)}{f\left(\tilde{c}(d_i, h_v)\right)}$. The intuition of equalizing the agent's net benefit at the margin remains the same as in the main text. Moreover, since the agent's optimal cutoffs are strictly decreasing in $\tilde{s}(d_i, h_v) A(d_i)$, the statement $\text{sign}\big\{G\big(\widehat{\theta}_i^{DA*}(1, h_v)\big) - G\big(\widehat{\theta}_i^{DA*}(0, h_v)\big)\big\} = \text{sign}\big\{\tilde{s}(1, h_v) A(1) - \tilde{s}(0, h_v) A(0)\big\}$ is still true.

B.2 Proof of Equation (2)

Consider the following redistribution problem:

$$\begin{aligned} &\max_{(T_i)_{i=1}^{N_v} \in \{0,1\}^{N_v}} \sum_{i=1}^{N_v} T_i A(d_i) \theta_i \\ &\text{subj. to} \quad \sum_{i=1}^{N_v} T_i = N_{DA}\, G\big(\widehat{\theta}^{DA*}(1, h_v)\big) + (N - N_{DA}) G\big(\widehat{\theta}^{DA*}(0, h_v)\big) \equiv N_v^T. \end{aligned} \tag{7}$$

Writing the objective function in its Lagrangian form and manipulating it, we can rewrite the maximization problem of the social planner as:

[39] We get similar results if we assume $c(d_i, h_v)$ to be multiplicative (instead of additively) separable.

$$\max_{(T_i)_{i=1}^{N_v} \in \{0,1\}^{N_v}} \sum_{i=1}^{N_v} T_i A(d_i)\theta_i - \lambda\big(\sum_{i=1}^{N_v} T_i - N_v^T\big)$$

$$\max_{(T_i)_{i=1}^{N_v} \in \{0,1\}^{N_v}} \sum_{i=1}^{N_v} T_i\big[A(d_i)\theta_i - \lambda\big] + \lambda N_v^T$$

where $\lambda \geq 0$ is the Lagrangian multiplier on the coverage constraint. Just as in the agent's problem in the main text, since the objective function is strictly increasing in each θ_i, the social planner will choose to treat any farmer whose value added is greater than the opportunity cost of treating them (λ). Therefore, the optimal allocation can be characterized by a set of cutoffs $\widehat{\theta}^{SP*}(d_i, h_v)$ for each pair (d_i, h_v) such that:

$$T_i^{SP*} = T_i^{SP*}(d_i, h_v, \theta_i) = \mathbf{1}\big\{A(d_i)\theta_i \geq \lambda\big\} = \mathbf{1}\big\{\theta_i \geq \widehat{\theta}^{SP*}(d_i, h_v)\big\}$$

where

$$\widehat{\theta}^{SP*}(d_i, h_v) = \frac{\lambda}{A(d_i)}$$

Since λ is constant across all social contexts (t_i, h_v), we obtain that:

$$A(1)\widehat{\theta}^{SP*}(1, h_v) = A(0)\widehat{\theta}^{SP*}(0, h_v)$$

which, taking into account that $\widehat{\theta}^{DA*}(d_i, h_v) = \frac{c}{s(d_i,h_v)A(d_i)}$, means that the agent's privately optimal allocation is socially optimal for a given level of coverage if and only if $s(0, h_v) = s(1, h_v)$.

In particular, if $s(0, h_v) > s(1, h_v)$, then $\widehat{\theta}^{DA*}(1, h_v) < \widehat{\theta}^{SP*}(1, h_v)$ and $\widehat{\theta}^{DA*}(0, h_v) > \widehat{\theta}^{SP*}(0, h_v)$. In other words, too many delivery agent ties and too few counterfactual agent ties are being treated with respect to the optimum. Expected total output can be increased by raising the threshold at which the delivery agent ties get treated (thus treating fewer delivery agent ties in expectation) and reducing the threshold at which the counterfactual agent ties get treated.

Note also that it is still possible that in the optimum $\widehat{\theta}^{SP*}(1, h_v) < \widehat{\theta}^{SP*}(0, h_v)$ if $A(1) > A(0)$. As we said above, both the productivity boost and the private benefit through rent-sharing tend to increase favoritism towards the agent's ties, but only the former motive is beneficial from the social point of view.