SOCIAL INCENTIVES, DELIVERY AGENTS AND THE EFFECTIVENESS OF DEVELOPMENT INTERVENTIONS*

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Abstract

There has been a dramatic rise in the use of the local delivery model for development interventions, where local agents are hired as intermediaries to target interventions to potential beneficiaries. We study this model in the context of a standard agricultural extension intervention in Uganda, in a setting where communities are highly politically polarized. We use a two-stage field experimental design. In the first stage, we randomize the delivery of the intervention across communities. In the second stage, in each community we randomly choose one delivery agent out of two potential candidates. This design yields exogenous variation in social ties to the actual delivery agent as well as to her counterfactual. We reveal the key role the relationship between the actual and counterfactual delivery agents plays for how the intervention unfolds in these communities. The number of farmers targeted by the delivery agent, whether their own social ties are targeted, and whether they engage in propoor targeting all depend on whether actual and counterfactual delivery agents are aligned or divided in their political identities. As counterfactual agents play no formal role in the intervention, we interpret their influence as a social incentive provided to the delivery agent, varying with the political alignment between the two. We document the impact social incentives have for resource allocation, inequality and welfare, and narrow down the structure of social incentives consistent with all aspects of delivery agent behavior. Finally, we discuss the implications of our findings for the design of the local delivery model. *JEL: D78, O12.*

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1 Introduction

A silent revolution taking place in development policy since the 1990s is the shift from the centralized provision of interventions by the state, towards NGOs delivering anti-poverty programs. This shift is driven by limited state capacity of low-income governments, and the desire of foreign donors to bypass governments and use NGOs to deliver aid and emergency assistance [Werker and Ahmed 2008, Aldashev and Navarra 2018]. In the face of such increasing demand, the delivery model used by NGOs has adapted. A cornerstone of the modern approach is to use locally hired agents to deliver interventions to households in the communities from which they are recruited.

A central feature of the local delivery model is that the provision of monetary incentives to delivery agents is limited, because it is hard to monitor either their behavior or the economic outcomes of households. However, as local delivery agents are embedded in the social structure of communities from which they are recruited, their behavior will be shaped by the social incentives they face when serving their community. Social incentives cover the plethora of non-monetary motivations driving behavior, including positive and negative preferences for the welfare of others such as altruism and warm glow, identity and mission, fairness, ingroup-outgroup biases, spite, social status, cooperation and rivalry etc. [Ashraf and Bandiera 2018].

Little is currently known about how social incentives determine the behavior of these key players in the local delivery model of development interventions. This is partly because social incentives are hard to detect. Our contribution is to document their existence and structure in a highly policy relevant context.

By providing evidence on how social incentives influence the behavior of delivery agents, we advance a nascent empirical literature examining how social networks impact targeting behavior in the context of pro-poor interventions [Alatas *et al.* 2016, Beaman *et al.* 2021], and provide insights for the emerging theoretical literature on targeting interventions in networks [Ballester *et al.* 2006, Banerjee *et al.* 2013, Galeotti *et al.* 2020]. We document the consequent impacts social incentives have for intervention effectiveness in terms of resource allocation, economic welfare and inequality. We use our full set of findings to narrow down the structure of social incentives delivery agents face, consistent with all aspects of their behavior.

Overall, we show how social incentives can be critical for the way in which locally delivered development interventions unfold in rural communities.

In viewing the motivation of local delivery agents through the lens of social incentives, we revisit a classic question in public economics on the effective targeting of benefits to households when need is hard to observe [Zeckhauser 1971, Akerlof 1978, Besley and Coate 1992]. In low-income contexts the standard trade-off is: (i) local agents have private information that can be leveraged to target interventions towards the needy; (ii) local agents might engage in nepotism, favoritism or be subject to elite capture [Dreze and Sen 1989, Bardhan and Mookherjee 2000]. We bring a new third dimension to this long standing debate: social incentives can create a wedge between local delivery agent's desire to stick to any original pro-poor intent of interventions, versus other motivations they have because of these social incentives.

Our analysis is founded on two elements. The first is the network of social ties delivery agents have with potential beneficiaries: this element of delivery has been the focus of earlier work [Banerjee *et al.* 2013, Beaman and Dillon 2017, Beaman *et al.* 2021]. The second builds on the fact that communities in our study are deeply polarized along political lines: political affiliation is reported by the vast majority of elders to be the salient marker of individual identities and the basis of community conflict. Hence, we consider how the behavior of delivery agents is shaped by their alignment in political identity with another individual in their community that could have potentially served as the local delivery agent: a counterfactual delivery agent.

We use a two-stage field experiment, in the first stage we randomize the delivery of an agricultural extension intervention across 120 communities, in the second stage we randomly choose one delivery agent out of two potential candidates. This design yields exogenous variation in social ties to the actual delivery agent as well as to her counterfactual.

The counterfactual is irrelevant for delivering the program. We interpret any influence they have on the behavior of actual delivery agents as operating through the social incentives their presence and identity provides. *A priori*, these social incentives might be beneficial or detrimental to the original intent of the NGO or principal. For example, social incentives might be harnessed to induce cooperation between the actual and counterfactual agents, say by sharing information on the needy in the community and so ensuring the intervention reaches a wider group of needy farmers beyond just the social ties of the actual delivery agent. On the other hand, social incentives might induce rivalry and conflict between the actual and counterfactual delivery agents, with delivery agents becoming motivated to advance the economic interests of their own social ties rather than those of needy households.

The local delivery model is utilized in many anti-poverty programs related to agriculture, health, early childhood development, credit and insurance. We study the model in the context of the pilot phase of a standard agricultural extension intervention in rural Uganda.

The intervention is implemented by the NGO BRAC in South Western Uganda. Constraints on agricultural yields and incomes in this context are twofold: a lemons problem in the market for improved seed varieties [Bold *et al.* 2017], and a lack of information on agricultural techniques. The intervention relaxes both constraints by offering farmers BRAC-certified HYV seeds for different crops, and training them in modern techniques. Local delivery agents are recruited, trained and tasked to provide seeds and training to their communities. Delivery agents (DAs) are selected by BRAC using standard criteria for such 'model' farmers: they must be engaged in commercial agriculture, own large plots, be profitable, and be well known – thus firmly embedded in the social structure of their communities. The intervention is intended as an anti-poverty program to be targeted to the poorest farmers.

Our field experiment exploits a two-stage randomization. The first stage follows a standard village level randomization between treatment and control villages. This allows us to evaluate the two-year impacts of the intervention on the likelihood that a farmer is targeted by the DA (defined as whether the farmer reports ever receiving seeds or training from the DA), and ITT impacts on their economic outcomes. The likelihood of being targeted by the DA is 3.9pp higher for farmers in treated villages than in controls, and this magnitude is in line with the informal rate of targeting suggesting by BRAC to DAs during the pilot phase expansion of the intervention that our study period covers. The availability of the extension intervention – seeds and training – significantly increases farmer's profits from the last cropping season by over 40%. These large impacts are partly driven by changes on the extensive margin as the intervention pulls farmers out of subsistence, enabling them to start growing marketable crops and engage with agricultural supply chains.

These treatment effects mask considerable heterogeneity in the delivery of the intervention. The majority of treated villages have near zero delivery by DAs. At the same time, 15% of the villages have targeting rates far exceeding the informal target DAs are set during this pilot phase of the intervention. The intervention thus causes inequality across villages to increase. At the same time, we find equally significant within village variation in the degree of pro-poor targeting achieved by DAs.

The second stage randomization is designed to establish the extent to which these inequalities across and within villages can be explained by the behavior of DAs. This stage of the experiment takes place entirely within treated communities. In each, BRAC shortlists two potential candidates to serve as the delivery agent (out of typically a very low number of suitably eligible individuals in each community). We then rapidly survey farmers to establish their social ties to each of the shortlisted candidates, and survey each candidate to establish the nature of their relationship with each other. To identify the dimension that best defines identities and potential divisions in these communities, we ask each candidate, "Besides being a citizen of Uganda, which specific group do you feel you belong to first and foremost?" Politics is stated by 95% of them to be the most salient form of identity, and the majority describe it as the most important source of disagreement in the community. Communities in our study sample are politically polarized, with near equal support for the two main parties in Uganda (the NRM and FDC). These divisions are reflected within our shortlisted candidate pairings. In half our communities, the two potential candidates are politically aligned. In half they are not, and their relationship is better characterized as one of division in terms of political alignment.

Central to our identification strategy is the intuition that whether the shortlisted candidates are aligned or divided in their political identities is a characteristic *specific* to that finally selected pair – it is not a marker of community division more generally. To see this note that had another candidate for delivery agent been shortlisted into the final two, then the political alignment between them would have been different approximately half the time. As a result, we document that communities where the candidates are politically aligned and those where they are divided, are similar on observables including on political, religious or ethnic polarization.

We then randomly select one of the final two candidates to be the actual delivery agent (DA). The other serves as a counterfactual delivery agent (CA): a shadow individual in the same community that could have been tasked to deliver the agricultural extension intervention. The actual DA is the sole intermediary tasked to implement the program locally. CAs play no role in the delivery of the agricultural extension intervention: any influence they have over the DAs behavior is a form of social incentive that determines how the intervention is targeted to potential beneficiaries.

Our design partitions potential beneficiary farmers into those: (i) exclusively tied to the DA (and so not to the CA); (ii) exclusively tied to the CA; (iii) tied to both; (iv) tied to neither. This second stage randomization generates: (i) experimental variation in whether farmers have an exclusive tie with the DA, or with the CA, so allowing us to causally identify the targeting behavior of DAs towards their exclusive ties relative to the exclusive ties of the CA; (ii) non-experimental variation in whether the relationship between the actual and counterfactual delivery agent is such that they are aligned or divided in terms of their political identities. We identify the impact of these two features – social ties to potential beneficiaries and the relationship between candidate delivery agents – on DA behavior exploiting variation across farmers within the same community, so holding constant all other fixed aspects of social structure (such as features of the aggregate social network of farmers).

The core attraction of the local delivery model is that it can, in theory, harness social incentives to lower costs and improve delivery. Our findings both support and run counter to this claim. Our main results are as follows.

First, we find the DA treats more farmers if she has more social ties in the community. This is supportive of a presumption of the local delivery model, and the magnitude of the effect we find is in line with reduced form and structural estimates of information diffusion in social networks [Banerjee *et al.* 2013, Beaman and Dillon 2017, Beaman *et al.* 2021]. Our novel insight is to document this is driven by the scenario in which the DA and CA hold opposing political identities: in that case for each additional social tie the DA has, coverage increases by .179 farmers, while there is no precise relationship between ties and total coverage when the DA and CA are aligned.

This last result runs counter to the hypothesis that social incentives matter because when the DA and CA are aligned, they share information to ensure a greater number of farmers are targeted overall, not just the social ties of the DA. Rather, our findings suggest communities with near zero targeting – in which the intervention is largely ineffectual – are those in which: (i) the DA and CA are politically aligned; (ii) the DA has few social ties.

We next zoom in on precisely which farmers are targeted by the DA: in line with existing evidence, we find those exclusively tied to the DA are 6.2pp more likely to be targeted relative to those exclusively tied to the CA. As has been much discussed in the literature, this differential targeting probability could be due to the DA having lower screening costs of targeting her own social ties – say because they are better able to transmit information to them or persuade them to adopt.[1] However, we find this result again hinges on the nature of the relationship between the DA and CA. When the DA and CA are aligned in political identities, social ties of the DA are no more likely be targeted than social ties of the CA. In sharp contrast, when the DA and CA are divided, ties of the DA are 9.7pp more likely be targeted than ties of the CA.

If the mechanism causing DAs to target their ties is some match-specific factor with those farmers, this should be at play irrespective of the pre-intervention alignment of DA-CA identities. In contrast, the social incentives the DA faces can be influenced by the nature of their relationship to this counterfactual. If division between the DA and CA causes the DA to exert more effort to acquire information about their ties, this is part of the *ex post* endogenous response caused by social incentives, and results in different patterns of targeting.

On pro-poor targeting, when the DA and CA are aligned, social ties have little predictive power for who the DA targets. The poor and non-poor are equally likely to be targeted, but with relatively low probability: neither group has more than a 4% likelihood of being targeted. This in sharp contrast to the targeting behavior of DAs when they and the CA hold opposing political identities. In that case, non-poor ties of the DA are 12pp more likely to be targeted than the non-poor ties of the CA. Among farmers exclusively tied to the DA relative to those exclusively tied to the CA, the non-poor are significantly more likely to be targeted ($p = .009$).

Overall, the results thus suggest the behavior of the DA is explicitly shaped by her relationship with the CA, despite the CA playing no formal role in the delivery of the intervention. In short, social incentives matter for understanding the effort and targeting behavior of delivery agents.

Viewing the local delivery model through the lens of the social incentives reveals a basic tension: DAs are prompted to exert effort to target more farmers when there is a divisive relationship between themselves and their counterfactual. Hence social incentives driven by such division can be leveraged to increase the total number of farmers targeted. However, this comes at the cost of DAs then being more likely to concentrate their targeting among non-poor farmers they are socially tied to. This goes against the anti-poverty intent of the intervention.

We study the implications of social incentives for resource allocation using estimated impacts on agricultural profits. The null is that whatever the exact structure of social incentives delivery agents are subject to, their targeting behavior is efficient so the average returns to targeting her own social ties and those of the CA are equalized: (i) for poor and non-poor households; (ii) in both states – when the DA and CA are aligned or when they are politically divided. The underlying assumption is that the information available to DAs and production possibilities across farmers are independent of the nature of the DA-CA relationship.

We reject this null: there are significant differences in returns across targeted farmers, suggest-

[1] It is well documented that valuable information related to targeting can be held by community members. This is so in the context of anti-poverty interventions [Alatas *et al.* 2012, Basurto *et al.* 2017], labor markets [Beaman and Magruder 2012], credit markets [Maitra *et al.* 2017] or capital markets [Hussam *et al.* 2021]. In agriculture, a large literature has established such match-specific factors driving adoption such as information flows or enforcement of implicit agreements [Foster and Rosenzweig 1995, Conley and Udry 2010, BenYishay and Mobarak 2019].

ing a loss in potential surplus. We bolster this claim using a more agnostic approach to identifying surplus losses by assessing the distributional consequences of the intervention. Estimating quantile treatment effects (QTEs) for profits reveals a divergence in the distributional impacts of the intervention between when the DA and CA are aligned to when they are divided. This difference in dispersion (variance in profits) is again indicative of resource misallocation in at least one of these states. However social incentives are motivating DAs, efficient targeting cannot be achieved in the two states of the world given these divergent distributional impacts.

We quantify the potential surplus loss from the DA's targeting behavior, benchmarking this value by the implied gains to the intervention comparing treatment and control farmers. The implied output loss is 10.1%. In line with Alatas *et al.* [2019] and Basurto *et al.* [2020], we find evidence of nepotism, but given the small number of family ties of DAs, this is not the major source of allocative inefficiency.

We combine our findings on targeting and allocation to establish the welfare consequences of delivery agent's behavior. This is important to do given the countervailing forces revealed: delivery agents exert effort to target farmers only when there is a divide with their counterfactual. However, delivery agents are then also more likely to target non-needy farmers. Following the approach of Maitra *et al.* [2017] and using a CRRA utility function, our baseline calculation suggests the implied impact of the intervention is to increase welfare by 15%, falling to 6% as we place more welfare weight on the poor. Welfare overall rises because the coverage effect more than offsets the mistargeting effect.

Our final stage of analysis aims to narrow down the structure of social incentives provided by the presence and identity of the counterfactual delivery agent.

We propose a group-based explanation whereby individuals have social preferences over their group of social ties and other outgroups. When the DA is selected, it is salient to the DA's group they can get ahead of the CA's group of social ties if the DA exerts effort in targeting her own ties [Shayo 2020]. This can explain our coverage results and provides reasons why the DA prefers to target non-poor members of her group. First, she might do so to curry favor with elites within her group to raise her own social status. A second reason stems from the DA holding *unique* power in being able to advance her group. There are no alternative DAs within the group that can conduct her role with the NGO. This provides DAs the possibility to extract rents from ties she treats, and she can extract greater rents from non-poor ties, all else equal. This builds on the idea that development brokers in local interventions engage in rent seeking behavior [Platteau and Gaspart 2003, Maitra *et al.* 2021].

We test the idea that DAs extract rents from their ties when the DA and CA are divided, borrowing ideas from the tax evasion literature to examine whether the actual asset accumulation of DAs between baseline and endline is significantly greater than predicted based on the observed asset accumulation of potential delivery agents in control villages [Pissarides and Weber 1989]. We find this is so. Using our estimates to back out the value of rent extraction by the delivery agent to advance her group in the scenario of DA-CA division, the value of rent extracted is around three times the average gains to individual farmers from the intervention. This is the collective cost the delivery agent's group of social ties need to pay her to advance them as a whole relative to the group of social ties of the counterfactual agent.

Despite their pivotal role for delivering interventions, the behavior of delivery agents is relatively understudied. While there is a long literature examining decentralization, only recently have experimental approaches been applied to compare local versus centralized delivery of interventions [BenYishay and Mobarak 2019], or to study the targeting behavior of local delivery agents as the selection process for those agents is varied [BenYishay and Mobarak 2019, BenYishay *et al.* 2020, Maitra *et al.* 2021].[2] Our work also builds naturally on work that has emphasized information diffusion among farmers [Foster and Rosenzweig 1995, Conley and Udry 2010], among social networks as a whole [Banerjee *et al.* 2013, 2019, Beaman and Dillon 2017, Beaman *et al.* 2020], or among exogenously engineered social ties [Feigenberg *et al.* 2013, Brooks *et al.* 2018, Cai and Szeidl 2018, Vasilaky and Leonard 2018].

Our focus is different from these dimensions, and allows us to make two substantive contributions to the wider literature.

First, we add to important recent work on allocative distortions caused by social ties between delivery agents and potential beneficiaries [Alatas *et al.* 2019, Banerjee *et al.* 2019, BenYishay and Mobarak 2019, Maitra *et al.* 2021]. We build on this by going beyond the targeting behavior of delivery agents to try and understand the fundamental drivers of what motivates their behavior in the first place. We do so by providing novel insights on how social incentives drive outcomes – revealing the importance of the relationship between shortlisted candidates in shaping the social incentives of chosen delivery agents, and thus how locally delivered development policies unfold within rural economies in terms of targeting, coverage, misallocation, welfare and inequality.[3]

Second, while the earlier literature has emphasized demand side networks – how information or resources flow within potential beneficiaries, we instead focus on the networks and relationships of selected delivery agents. We thus start to recognize the importance of supply side networks for

[2] BenYishay and Mobarak [2019] show that the social identity of extension agents matters, and that their effort is influenced by the provision of small financial incentives. They compare the choice of lead farmers to peer farmers, with and without incentive provision. While their design allows for multiple extension agents to be identified within each territory, they do not study the role of the relationship between these agents in determining outcomes. BenYishay *et al.* [2020] provide evidence from Malawi on how randomly assigning the task of delivery agent to men or women affects their learning about a new agricultural technology and communicating it to others to convince them to adopt. Maitra *et al.* [2021] compare two models of appointing local commission agents as intermediary for a credit program in India: random selection versus being chosen via village council elections. They show how randomly selected agents led to more loans being made (greater coverage) with borrower outcomes being no worse in terms of repayment rates and better in terms of incomes.

[3] Ashraf and Bandiera [2018] overview the theoretical and empirical literature on social incentives. In models of principal-intermediary-beneficiary hierarchies, the selection of intermediaries is not normally considered. Even among those models that take this into account, they typically leave no role for the non-selected intermediary. Empirically, few studies have identified counterfactual agents or explored how the behavior of intervention intermediaries is shaped by their connection to a counterfactual.

development interventions [Galeotti *et al.* 2020].[4] Our approach sheds light on whether among those not socially tied to the delivery agent, it matters who *else* those non-ties are linked to. This allows us to reveal how agents that are seemingly irrelevant for an intervention can still critically impact outcomes through their relationship with those formally tasked to deliver the intervention. Moreover, this perspective allows us to go beyond considering non-compliance with the offer of treatment as being a take-up issue driven by a lack of demand. Rather non-compliance reflects supply-side biases in how treatment assignment by delivery agents within villages takes place.

Ultimately, viewing the local delivery model through the lens of social incentives provides insights to the classic question of how to provide private benefits to the poor through policy interventions when need is hard to observe. Our analysis shows social incentives have both up and down sides from the perspective of the NGO or principal, creating new trade-offs to be considered for the local delivery model. This new perspective complements the long standing literature on decentralization, that has emphasized the importance of elite capture or clientelism in driving the effectiveness of interventions[Galasso and Ravallion 2005, Bardhan and Mookherjee 2006].

Section 2 describes the intervention, data and the first stage randomization used to evaluate the intervention comparing treatment and control villages. Section 3 describes the selection of delivery agents and second stage randomization design. Section 4 presents findings on the number of farmers targeted, who is targeted, resource allocation and welfare. Section 5 narrows down the structure of social incentives counterfactual agents provide. Section 6 discusses design implications for the local delivery model, external validity and a broader research agenda. Section 7 concludes. The Appendix discusses further data details, results and research ethics.

2 Intervention, Data and Evaluation

2.1 The Agricultural Extension Program

Raising agricultural productivity has long been seen as a route to driving subsequent industrialization and economic development. The macro literature has emphasized how productivity differences in agriculture across countries can help explain cross country income differences [Restuccia *et al.* 2008, Gollin *et al.* 2014]. Yet agricultural productivity remains low in many developing regions, especially in Sub Saharan Africa. Some persistent causes of this are the low adoption rates of improved seed varieties and limited use of modern agricultural techniques [Evenson and Gollin 2003, World Bank 2008].[5]

[4] Galeotti *et al.* [2020] theoretically analyze the planner's problem of targeting interventions in social networks, considering strategic responses of individuals to others being directly targeted in their network, as well as externalities across beneficiaries.

[5] The Green Revolution – the adoption of high-yielding seeds and chemical fertilizers – has been a key factor behind the increase in yields in Asia and South America, with no such increase in Sub-Saharan Africa [Bridle *et al.* 2019]. Gollin *et al.* [2021] show using panel data from 84 countries just how important the adoption of high yielding

A common policy response has been the provision of agricultural extension services throughout the region, whereby local extension agents provide improved seeds and training to farmers. However, the evidence for extension services having positive returns in Sub Saharan Africa is mixed [Anderson and Feder 2007, Udry 2010]. Our study brings new insights to this debate: by focusing on the social incentives that locally hired agents are subject to, we shed light on interventions can be successful in some communities and fail in others. This links to the external validity of intervention evaluations, where program implementation has been highlighted as a potential driver of heterogeneity [Allcott and Mullainathan 2015, Meager 2019].

We study an agricultural extension program delivered by the NGO BRAC in Uganda. Our evaluation takes place during the pilot expansion of the intervention from 2012-15 into two districts in South Western Uganda: Kabale and Rukungiri. The vast majority of rural households in these districts are employed in subsistence agriculture. Two constraints on agricultural yields and incomes in this region are a lemons problem in the market for improved seed varieties, and a lack of information on the use of modern agricultural techniques.[6]

The intervention simultaneously relaxes both constraints by offering farmers BRAC-certified HYV seeds for various crops, and training them in six modern techniques. Improved seed varieties are sold (at below market price) for crops cultivated for market sale (potato, eggplant, cabbage), and those grown for home consumption (maize and beans).[7] As an indication of the lemons problem pre-intervention, we note that 93% of surveyed farmers know about improved seeds at baseline, and 73% believe they would have positive returns if adopted, yet only 33% have ever tried improved seeds because of the lack of certified supply, and the excessive cost of such seeds. The training component of the intervention teaches farmers to use techniques such as crop rotation, zero tillage, intercropping, line sowing and weeding, and avoid the use of mixed cropping. Two of these techniques are actually widely adopted pre-intervention (crop rotation and weeding are employed by more than 90% of farmers at baseline), while the others are less widely known: intercropping (62%), zero tillage (12%), line sowing (44%), and only 10% of farmers report avoiding mixed cropping. This is the practice whereby farmers simultaneously grow different crops on the same plot of land, without adequate spacing between plants: this is a significant drag on crop yields.

variety seeds are for economic development: they estimate an elasticity of GDP per capita to adoption rates for such improved seed varieties being around one, with the mechanisms being a combination of higher crop yields, factor adjustment and structural transformation. Of course there are other important frictions driving agricultural productivity gaps between rich and poor countries. At the macro level, those related to security of tenure and the functioning of land markets are notable [Restuccia and Santaeulalia-Llopis 2017]. At the micro level, frictions within households have been documented to cause the misallocation of inputs across plots of land [Udry 1995, Gollin and Udry 2020]. We later contrast the extent of resource misallocation caused by social incentives, to these macro and micro estimates.

[6] The lemons problem for high yielding seeds in rural Uganda is well documented. Bold *et al.* [2017], in a study spanning 120 local shops/markets in rural Uganda, find that the most popular HYV maize seeds contain less than 50% authentic seeds, and that such low quality results in negative average returns.

[7] For example, maize seeds are bought from BRAC at UGX2000/kg, and sold by agents at UGX2300/kg. A pre-study survey of 71 markets in our study area found the median price for non-certified seeds to be UGX2500/kg.

Seeds and techniques are complementary, but either can increase crop yield on its own.

The intervention is implemented through locally recruited delivery agents (DAs). It is intended as an anti-poverty program, that should be targeted to the poorest farmers. All DAs are women.[8] DAs are recruited (and then trained) by BRAC using criteria that lead DAs to be positively selected relative to the average farmer: they must be engaged in commercial agriculture, own large plots, and be well known, and so firmly embedded in the social structure of the communities they serve. It is common practice to deliver agricultural interventions through such 'model' farmers, and indeed, the recruitment of positively selected locals to serve as intermediaries between organizations/the state and intended program beneficiaries is typical of how locally delivery model interventions are designed in spheres as diverse as agriculture, credit and health.

A single DA is chosen for each territory – a community that typically comprises two adjacent villages – and they are given an informal target to provide seeds and training to around 20 farmers (as this is the pilot phase of the intervention). Effective extension requires adequate and timely access by farmers to advice. Hence, DAs are tasked to visit farmers daily to provide agricultural advice. The contractual structure for DAs is homogenous across communities. Typical to the design of the local delivery model, DAs are provided weak monetary incentives, earning a small commission on seeds sales, that in total is valued at 3% of their annual consumption if they reach their target number of farmers. They are provided free seeds for their own use and receive further monthly training from BRAC. They are hired on open-ended contracts and so might also be motivated by career concerns and the possibility to shift to a permanent contract with BRAC.

As with all interventions delivered by local intermediaries, there is a basic moral hazard problem in that BRAC has limited ability to observe the actions of DAs. Although DAs are supervised weekly by BRAC, this still gives them leeway in deciding how many and which farmers to target.

2.2 Design

This study is part of a wider project on the determinants of agricultural productivity in Uganda. We evaluated two interventions: agricultural extension services and the provision of microfinance using a 2×2 factorial design. The interventions are implemented entirely independently of each other. Microfinance is delivered by centrally located BRAC program officers, not local hires or DAs. For the purposes of this study, we do not utilize the microfinance only treatment arm. Our evaluation sample thus uses three of the four cells in the 2×2 factorial design, covering 167 villages. Random assignment takes place at the village level, with 59 villages being randomly assigned as controls, and 108 villages being assigned the agricultural extension program (of which

[8] The motivation for this is twofold. First, it is well documented that despite women supplying a significant share of all agricultural labor, there exist large gender productivity gaps in agriculture [Udry 1996, Baffes 2009]. Second, traditional government extension services typically bypass women [Lecoutere *et al.* 2020]. If women DAs are more likely to target women farmers, this can both help close the gender productivity gap and raise overall output [BenYishay *et al.* 2020].

51 also receive microfinance). In the Appendix we document that there is no interaction between the provision of extensions services and microfinance for our key outcomes.[9]

Timeline Figure 1 shows the study timeline, indicating the timing of surveys, agricultural cycle, and implementation of the intervention. We first conducted a listing in all 167 villages, covering 25,000 households. A sample of 4,741 households primarily engaged in agriculture is drawn from our baseline survey fielded from May to July 2012 (so close to 20% of all households in each village): 3,064 households reside in treated villages, 1,677 reside in control. As the intervention targets women farmers, we interview female heads of household. The endline survey takes place two years later. There are two six month cropping cycles per year in this region, and our baseline and endline surveys are timed to take place close to the end of the first cycle in each year.

Balance and Attrition Table 1 shows balance on village characteristics. Panel A shows that villages are small and have around 180 households in them, 79% of which have agriculture as their main income source. Treatment and control villages have similar levels and average wealth and inequality.[10] Panel B shows that on aspects of social structure related to political, religious or ethnic fractionalization, treatment and control villages are balanced, both being evenly polarized in terms of politics and religion (but not ethnicity): both fractionalization indices are around .45, with the largest political grouping having a 55% vote share in the last election (the incumbent NRM party), and around 58% of households belonging to the largest religious group (Protestants).

Table 2 shows balance on household characteristics. Panel A documents women farmers have low levels of human capital, consumption, and operate close to subsistence.[11] Panel B focuses on respondent's pre-intervention exposure to improved seeds and modern techniques. The majority are aware of improved seeds and believe them to generate positive returns, yet only a third have ever adopted improved seeds: in part because of the lemons problem described earlier. Similarly, farmers are aware of modern techniques and believe them to have positive returns if adopted correctly, but on average, only half of them have actually been adopted.[12]

[9] We evaluate the microfinance intervention in a separate analysis using two of the 2×2 cells, comparing household outcomes in the 59 control villages to those in 62 villages offered only microfinance [Bandiera *et al.* 2021].

[10] The household wealth score uses information on ten indicators, providing weighted scores that range from 1 to 100. The higher the score, the lower the likelihood that the household has expenditure below a given poverty line. The indicators are household size, enrolment rates of school aged children, the highest education level of the female head of household, the construction materials for the roof, the construction material for walls, the main source of lighting, the type of toilet, use of household electrical appliances, family members each having at least two sets of clothes, and family members each having at least one pair of shoes.

[11] To construct the measure of consumption, respondents were asked to report the weekly value of consumption for 22 items (matoke, potatoes, cassava, rice, maize, other cereals and vegetables, bread, beans and nuts, meat, fish, eggs, milk, butter, other in this category, oil, fruits, salt, non-alcoholic beverages, alcoholic, cigarettes, food in restaurants, and any other food). We take the total value of food consumption over the week (across all items) and divide it by the equivalent number of adults in the household, where adults are given a weight of one and members below 18 are given a weight of .5.

[12] Farmers are not so uncertain on the returns to adopting new seeds or techniques. This is despite profits being skewed suggesting returns can be very heterogeneous. Of course, delivery agents might be able to help farmers

Panel C shows household characteristics related to agriculture: they work around six hours per day, grow multiple crops, the majority of which are for home consumption. Around half of all output is sold. The use of mixed cropping means that yields are not a useful outcome measure to consider (depending on the crop types being mixed). Hence we focus on profit as the main agricultural outcome of interest.[13]

Table A1 shows correlates of household attrition from baseline to endline. Attrition is low (7%), uncorrelated to treatment, and not differential by characteristics of households in treatment and control villages: the p-value on the joint significance of baseline household characteristics interacted with the treatment dummy is .309.

2.3 Estimation

We measure ITT outcomes two years post-intervention using the following ANCOVA specification for household i in village v:

$$y_{iv1} = \alpha + \beta T_v + \delta X_v + \gamma y_{iv0} + u_{iv}, \tag{1}$$

where y_{iv1} is the outcome of interest at endline ($t = 1$), $T_v = 1$ for villages assigned to treatment, X_v includes indicators for the BRAC branch (of which there are four across the two study districts) and y_{iv0} is the outcome of interest at baseline ($t = 0$). We estimate standard errors clustered by village, and report p-value corrections for randomization inference and multiple hypothesis testing [Young 2019].[14] The former is especially important given that profits from agriculture are typically right skewed, and the treatment can have distributional impacts on profits.

2.4 Results

Treatment Effects Table 3 shows the estimates from (1). We first consider whether farmer i is targeted by the DA, defined as whether the farmer reports ever receiving seeds or training from the DA. Column 1 shows the likelihood of being targeted by the DA is 3.9pp higher than

understand with more precision the true returns to adoption. Suri [2011] uses data from Kenya to study the problem of technology adoption when farmers are uncertain over returns due to such skewness, and De Falco [2019] presents evidence from a field experiment in Tanzania that shows that improved seeds increase profits, and that these benefits are attenuated when farmers are uncertain about the gains from adoption.

[13] The measure of profits (in thousand UGX) is the value of output minus the value of agricultural expenditures. Output is the price times quantity sold across 61 agricultural products, including maize, beans, potatoes, bananas, nuts and cabbage. We impute the value of crops held for home consumption using median sales price in the village. Agricultural expenditures include the input cost of hired labor, seeds, manure, chemical fertilizer, pesticides and other expenses. For both profits and consumption, we drop observations above or below two standard deviations of the mean (corresponding to around 4% of observations for both variables).

[14] The randomization strata are BRAC branch, village size, the share of households primarily engaged in farming, and distance to the local market, and results are robust to including controls for all randomization strata. We note the average travel time between treatment and control villages is around 90 minutes, ameliorating concerns over spillovers into controls (that would in any case lie beyond the territory of each delivery agent).

for controls. Columns 2 and 3 show each element of DA targeting: in most cases, DAs bundle the provision of seeds and training to farmers.

There are two alternative sources of seeds in our study setting (while there is no market for training in modern techniques). Column 4 shows farmers in treated villages are 4.3pp more likely to obtain certified seeds from BRAC branches directly. Column 5 shows farmers might be more likely to obtain seeds from non-BRAC sources – the impact is significant once we adjust for randomization inference ($p = .033$). This hints that in treated villages seeds can diffuse among farmers – a point we return to later.

The remaining Columns of Table 3 document treatment effects on agricultural outcomes from the availability of the extension intervention. Column 6 shows that profits rise by 44% over controls, in part driven by an extensive margin increase in the number of marketable crops. Monthly food expenditures rise by 26% and the value of productive assets rises by 15%.

Taking into consideration that this is the pilot phase of the intervention and so only a small overall share of farmers are targeted, the implied TOT estimates on profits are far higher than is found in field trials for HYV seeds.[15] There are three reasons for this. First, being targeted by the DA often implies the combined receipt of seeds and training (Columns 2 and 3). Hence our estimates are not directly comparable to field trials that restrict attention to the return to the adoption of modern seeds alone. Second, these impacts occur partly through changes on the extension margin, as the intervention pulls farmers out of subsistence and they start to grow new marketable crops (Column 7), and begin engaging in agricultural markets (and so not just replacing traditional seeds with modern ones for the same crop). Third, as shown in Table 2, pre-intervention profits are very low with most farmers operating close to subsistence. This naturally leads to very large percentage impacts on profits: the absolute increase in profits of UGX$34,000$ corresponds to US$13 and is more plausible.

Taken together, the results imply the intervention provides substantial economic gains to the average farmer, given their pre-intervention economic standing. Hence there is unlikely to be a lack of demand for seeds/training from farmers, so non-compliance is unlikely to stem from a lack of demand-side take-up. Rather it reflects a lack of supply-side targeting or treatment assignment by DAs to potential beneficiaries.[16]

Inequality The documented treatment effects mask considerable heterogeneity in the delivery of the intervention across villages. The extent of these cross village differences becomes clear in Figure 2. Panel A shows the share of farmers targeted by the DA in each of the 109 treated

[15] In field trials in Kenya, hybrid maize and fertilizers have been found to increase profits by 40% to 100%. Suri [2011] finds heterogenous returns across farmers, with mean gross returns of 60%, but some farmers having returns as high as 150%. De Falco [2019] shows evidence from an RCT in rural Tanzania that the adoption of improved maize seeds led to between 40-50% increases in profits.

[16] Table A2 shows these baseline impacts on the likelihood of being targeted, and household outcomes, are all of similar magnitude in villages with and without the independently delivered microfinance program.

villages (where targeting is defined as in Column 1 of Table 3, so a farmer having received seeds or training from the DA). We see that 72 villages – two thirds of all treated villages – have near zero delivery of the intervention. At the same time, 17 villages have at least 10% of all farmers treated, so in absolute number far exceeding the informal target DAs are set during this pilot phase of the intervention expansion into new districts. This heterogeneity fits with the wider evidence. As Anderson and Feder [2007] note in their meta-analysis of agricultural extension, the overriding lesson is that the economic impacts vary widely – many programs have been highly effective, many others have not.

From the standpoint of program evaluation, the skewness in the delivery of the same intervention across villages means that ITT effects and subsequent cost benefit analyses will be fragile – any small change in the composition of treated villages in the sample (especially among those with positive targeting rates), would lead to non-negligible changes in estimated treatment effects and hence cost effectiveness.

Panel B shows equally significant within-village variation in the extent of pro-poor targeting. We use food consumption to classify household need as this is a better predictor than other measures such as income [Deaton 1997]. Panel B shows for the 37 villages with a strictly positive share of targeted farmers, how targeted farmers split into those in the lowest quartile of food consumption, the middle two quartiles, and the top quartile. By construction, 25% of farmers actually belong to the lowest quartile, and as only five villages have targeting rates above 25%, in all the remaining villages it is feasible for *only* farmers in the lowest quartile to have been targeted. The actual extent of pro-poor targeting by DAs falls well below this threshold. In only 3 villages are all targeted farmers in the lowest quartile of food expenditures. In the majority at least one farmer from the top quartile of food consumption is targeted by the DA.[17]

The literature has recognized factors such as elite capture or clientelism as causing interventions to drive within and across village inequality [Galasso and Ravallion 2005, Bardhan and Mookherjee 2006]. However the role of social incentives driving inequality, for interventions channelled through local delivery agents has not been studied. For the remainder of the paper we establish how the behavior of DAs drives this heterogeneity in program implementation and effectiveness. The second stage of our randomization design allows us to investigate precisely this issue.

[17] In the Appendix we show the full extent of cross village variation cannot be explained by sampling bias and measurement error in recorded targeting. Figure A1 summarizes our simulated findings. This rules out the null that targeting rates are homogeneous across villages, because if so, it would be impossible to simultaneously explain why a mass of villages have zero share of targeted farmers, and also explain why a non-trivial share have targeting rates above 10%.

3 Delivery Agents

3.1 Shortlisting and Selection

The second stage of our experimental design lies entirely within treated villages. Among these we first define 60 communities, each covered by a single delivery agent. Communities bundle together small and contiguous villages. The modal delivery agent covers two contiguous villages in their community. Delivery agents are thus recruited from within the communities they serve.

Delivery agents do not self-select for the role, rather they are recruited by BRAC. The recruitment process follows three steps. First, BRAC identifies a handful of potential candidates in each community, using the following criteria: they must be female, aged between 24 and 45, engaged in commercial agriculture, own at least one acre of land, be literate and be well known within their communities. These criteria positively select farmers as potential delivery agents, and only a handful of individuals in any given community meet all the criteria. BRAC then narrows down this potential candidate set to a shortlist of two.

As indicated on Figure 1, we then rapidly implement two surveys in each community: (i) to farmers; (ii) to both potential candidates. From farmers we collect information on their ties to these candidates. To the shortlisted candidates we field a survey with the purpose of establishing the relationship between them.

To measure ties between individuals we ask, "Do you know who [name] is?" and if so we then ask, "What is your relationship with her?" where responses can indicate a family tie, a friendship tie, or talking about agriculture with each other. Fieldwork for both surveys is completed with a few days of the delivery agents being shortlisted. The rapid timing of data collection, and the fact that the actual delivery agent is not yet known, helps avoid strategic reporting of ties.

The final recruitment stage is that we randomly select one of the shortlisted candidates to be the actual delivery agent (DA). The non-selected candidate serves as a counterfactual delivery agent (CA) from within the same community: namely a shadow individual that also meets all the selection criteria. Candidates are informed that out of the eligible candidates, the DA would be selected by lottery. It is not formally revealed who the CA is, but it is reasonable to expect this information to diffuse within communities over time, including to the actual DA.

Columns 1 and 2 of Table 4 confirm the second stage randomization: DA and CA characteristics are not statistically different to each other in terms of their human capital, land ownership, pre-intervention use of improved seeds, modern techniques, and agricultural outcomes. Column 3 shows how positively selected these candidates are relative to our main sample: for example, on agricultural profits, the average DA lies at the 94th percentile of agricultural profits in their community.

3.2 Ties Between Farmers and Candidates

Throughout our analysis, we define a farmer to be socially tied to a candidate if they report being linked either through friendship, family or because they discuss agriculture with each other. Panel A of Figure 4 shows the extent of different sub-types of tie between DAs, CAs and farmers: 5% are friends or family of the DA, 7% are friends or family of the CA; 11% discuss agriculture with the DA, 14% do so with the CA.

Figure 3 graphically represents the second stage design. This partitions potential beneficiary farmers into: (i) those exclusively socially tied to the DA (and so not to the CA) – corresponding to 10% of all farmers; (ii) those exclusively tied to the CA (15%); (iii) those tied to both (53%); (iv) those tied to neither (22%).

Our second stage randomization generates experimental variation in whether farmers are socially tied to the DA or the CA. Our focus is thus on these two groups of farmer (highlighted in Figure 3) because among farmers tied to either one of the two potential candidates, whether they are tied to the actual DA or the counterfactual agent is randomly assigned. In the average community, around 55 farmers are tied to either the DA or CA, and it is this set of farmers that determine our core results. Although all farmers are used in our empirical estimation, nowhere in our analysis do we focus on how social incentives impact targeting behavior towards those tied to both the DA *and* CA, or those tied to neither. The reason is that there might be unobservables that simultaneously determine their network position and agricultural outcomes. Our research design only allows us to exploit an experimental comparison between those exclusively tied either to the DA or to the CA.[18]

Columns 4 and 5 in Table 4 confirm balance on observables between those two groups of farmer. They do not differ in terms of background characteristics (Panel A), previous use of improved seeds and modern techniques (Panel B), and agricultural outcomes in the last season (Panel C). Importantly, the neediness of farmers – being in the bottom quartile of food consumption – is the same among those tied to the DA and those tied to the CA.

Panel D provides information on the distances between the homes of the farmer, DA and CA. There is some geographic sorting within communities so that those tied to the DA reside slightly closer to them, although physical distance between households is not always a good proxy for their social distance [Beaman *et al.* 2020].

Our second stage randomization eliminates endogenous tie formation between farmers and potential candidates. Hence we take social ties as exogenous. This is in contrast to the well established literature on clientelism, that emphasizes how beneficiaries can endogenously form ties with elites to gain access to distributed benefits. There is no doubt such endogenous network formation can be kickstarted by the intervention, but our analysis is based on pre-existing ties.

[18] Table A1 confirms that there is no differential attrition of farmers based on their tie to the DA, or to the CA (Columns 4 and 5). Nor is there evidence of there being differential attrition on observables of those with exclusive ties to either the DA or CA (Column 6), where the p-value on the null of zero interactions is .600.

3.3 Ties Between Candidates

The social incentives our design allows us to pin point stem entirely from the relationship between the DA and CA. To identify the dimension that best defines identities and thus potential cooperation or conflict in this setting, we follow Berge *et al.* [2018] and ask the DA, CA and village elders in each community, "Besides being a citizen of Uganda, which specific group do you feel you belong to first and foremost?" Politics is by far the most salient form of identity: 95% of respondents state they identify first with a political party (with others stating religion or their occupational group, and none stating ethnicity). As a follow up we asked, "In your village, what do people usually mostly disagree on?" Over 60% responded politics, 30% stated religion and the remainder said land (none responded with tribe/ethnicity). Recall that potential DAs are recruited according to strict eligibility criteria, but these do not include their political, religious or ethnic affiliation.[19]

The salience and sensitivity of discussing political ideologies was revealed in our pilot fieldwork: individuals were often wary of reporting their political affiliation to enumerators. To get around this and construct a measure of potential alignment/division between the DA and CA, we asked each separately whether they belong to the same political party as the other or not (and so did not directly ask them about their own political affiliation). For the same reason we did not collect data on the political affiliations of individual farmers.

Figure 4B shows various measures of ties between the actual and counterfactual delivery agents. In 51% of communities, they belong to the same political party (i.e. they both report belonging to the same party as the other), and in 49% of communities they belong to rival parties. To validate this, we designed an implicit association test (IAT) to measure the political views of DAs and CAs, and then assign whether they belong to the same or different parties based on their test scores. In the Appendix we describe the construction of this measure. Using the IAT, we find 49% of DA-CA pairs belong to the same party, almost identical to that inferred from self-reports (and in 64% of communities they exactly coincide). Even with such delicacy, in seven communities both candidates refused to answer either question.[20]

DA-CA pairings are equally split by whether they are of the same or different religions (and we reiterate that in no community was ethnicity reported as the foremost source of division). The majority of DA-CA pairings are either friends or family and in all cases they know each other. The baseline measure of ties we use is whether the DA-CA pair self-report belonging to the same party or not. We later present evidence to rule out that our measures of ties pick up religion

[19] There are two main political parties in Uganda: the incumbent NRM and FDC. The NRM's leader Yoweri Museveni has been in power continuously for 30 years. A historic North-South divide has led to traditionally lower support for Southwesterner Museveni and his NRM. Neither party has a strong ideology and both run more on personnel than on policy [Conroy-Krutz *et al.* 2016]. Our study period sits between two general elections (2011 and 2016), ameliorating concerns that political identity is especially salient in the run up to elections.

[20] IAT tasks entail the respondent engaging in a sorting task to measure individual's implicit attitudes towards specific targets. While such tests have traditionally been developed to measure attitudes towards race and gender, they have now been extended to political and ethnic preferences [Lowes *et al.* 2015, Berge *et al.* 2018].

or ethnic similarity/difference across individuals, and that they are robust to alternatively using IATs to measure the political alignment of the DA and CA.

Central to our identification strategy is the intuition that whether the political identities of the DA and CA are aligned or divided is a characteristic *specific* to the pair of shortlisted candidates – it is not a marker of division in the community more generally. To see this note that had another one of the potential candidates for delivery agent been selected into the final two, then the tie between DA and CA would have been different approximately half the time. As a result, communities where the DA-CA are aligned and those where they belong to opposing sides of community divisions, are similar on observables.

We confirm this in Table 5: this shows how community characteristics vary by whether the DA and CA are aligned or divided. Communities are balanced in terms of the share of farmers connected to the DA, to the CA, or to both (Panel A). It is thus not the case that in communities where the DA-CA are aligned, more farmers are connected to both. This follows from the above argument, that DA-CA ties are specific to that pair of potential candidates, and unrelated to community characteristics.

Similarly, we see that communities where the DA-CA are aligned or divided are balanced on their political, ethnic and religious fractionalization (Panels B and C). Hence, the DA and CA are not more likely to have conflicting political affiliations in more politically polarized communities.

Panel D shows that among farmers from the top decile of the wealth index, the degree of fractionalization is the same as for all farmers: ethnicity and religion do not vary with wealth. Hence the set of farmers that potential candidates are drawn from, are as divided as other farmers in the community.[21]

Panel E confirms that communities are similar on observables on characteristics beyond those related to social structure, such as their size, location and level of wealth.

A final concern for identification is that the characteristics of farmers exclusively tied to one of the candidates differ depending on whether the DA-CA are aligned or divided. For example, when the DA and CA are politically aligned, farmers connected to either might be more similar to each other than when the DA and CA are of conflicting political identities. As a result, it might be easier for the DA and CA to share information about their ties when they are aligned, with subsequent impacts on who is targeted and the aggregate number of farmers targeted. We check for this in Columns 6 to 9 of Table 4. This shows the characteristics of farmers exclusively connected to the DA or CA, by whether the DA-CA are themselves aligned or divided. On nearly all dimensions we find that among those exclusively tied to the DA (or CA), they are balanced on observables across communities where the DA and CA are aligned or divided.

[21] In some contexts, being a lead farmer and occupying a position in the local political hierarchy go together or confer benefits in terms of having more secure tenure [Goldstein and Udry 2008]. In Northern Nigeria, a common office is *sarkin noma* (chief farmer) which is often awarded to a successful farmer. In Malawi, local Chiefs adjudicate matters related to customary land and often play a role in local development projects [Basurto *et al.* 2020].

4 Results

We sequence our results as follows. We first document how social ties determine the total number of farmers targeted by the DA, and then consider which farmers are specifically targeted. On each margin we consider: (i) DA's behavior toward their social ties, exploiting the experimental variation our research design induces in whether farmers are tied to the DA or the CA; (ii) how the DA-CA relationship shapes DA behavior, a social incentive identified using non-experimental variation in this relationship that our research design creates. We then consider the consequent impacts on resource allocation and economic welfare.

4.1 Coverage

Empirical Method To identify how social ties and social incentives determine coverage – the total number of farmers targeted by the DA in their community – we use the intuition that conditional on the total number of farmers exclusively tied to either the DA or the CA, the exact number exclusively tied to the DA is exogenous. Figure A2 shows the variation used: the number of farmers exclusively socially tied to the DA ranges from zero to over 20 in other communities. We estimate the following specification for community c:

$$coverage_c = \alpha + \beta_{DA} \left(\textstyle\sum_i ST_{i,DAc}\right) + \beta_{DA+CA} \left(\textstyle\sum_i ST_{i,DA,c} + \sum_i ST_{i,CA,c}\right) + \vartheta R_c + \gamma X_c + u_c. \quad (2)$$

$coverage_c$ is the total number of farmers targeted in the community by the DA, among those exclusively socially tied to the DA or CA. $ST_{ijc} = 1$ if i has social tie of type j, where $j \in \{DA, CA, both, neither\}$ indicates being exclusively tied to the DA, exclusively tied to the CA, tied to both or to neither. The total number of farmers exclusively tied to the DA or CA is $(\sum_i ST_{i,DA,c} + \sum_i ST_{i,CA,c})$, and $(\sum_i ST_{i,DAc})$ is the number of farmers exclusively tied to the DA. $R_c = k \in \{0, 1\}$ indicates the relationship between the actual delivery agent and her counterfactual in the community. $k = 0$ when they are aligned to the same political party, and $k = 1$ when they are not politically aligned and so have a more divisive relationship. In X_c we control for BRAC branch and report robust standard errors.[22]

β_{DA} is the parameter of interest: the responsiveness of coverage to the number of social ties the actual delivery agent has. A presumption of the local delivery model is that β_{DA} is large, as reflected in the common usage of selection criteria for potential delivery agents requiring them to be well known or central in the social network of their community [Banerjee *et al.* 2013, 2019, Beaman and Dillon 2017, Galeotti *et al.* 2020].

[22] In line with the rest of our analysis, we note that our second stage randomization design does not allow us to estimate the level effects on total coverage of the other three types of vertical tie (being exclusively tied to the CA, being tied to both the DA and CA, or being tied to neither).

Results Table 6 presents the results. Column 1 focuses only on social ties and so uses a simplified version of (2): $\widehat{\beta}_{DA} = .138$ and is statistically different from zero. Hence, conditional on the total number of farmers exclusively tied to the DA or CA, the DA treats more farmers if she has more social ties in the community. However, the responsiveness of coverage to ties is also far from one: for every seven social ties the DA has, she targets one additional farmer among the ties of the DA and CA. Column 2 checks for any non-linearity in the relationship between the number of ties of the DA and coverage (say because of convex costs of screening more ties). We find no evidence of any such non-linearity.

Column 3 estimates (2) in full to explore how social incentives determine coverage. The previous result that social ties determine coverage is largely driven by the scenario in which the DA and CA are politically divided: for each additional tie the DA has, coverage increases by .179 farmers, while there is no precise relationship between ties and coverage when the DA and CA are aligned. However, given our small sample, we cannot reject equality of these effects ($p = .437$).

The results run counter to the hypothesis that social incentives matter because when the DA and CA are aligned, they share information on the needy to ensure the intervention reaches a wider group of farmers beyond only the social ties of the actual delivery agent.[23]

$\widehat{\beta}_{DA} > 0$ is supportive of a presumption of the local delivery model, and given its standard error, the magnitude of the effect we find is in line with reduced form and structural estimates of information diffusion in social networks. Our novel insight is to document the process of targeting depends critically on elite division as embodied in the DA-CA relationship.[24]

To quantify how much of the cross-village variation in coverage is explained by the social ties of the DA, we note from Column 3 that: (i) the partial R-squared for the number of ties of the DA is .366 (so more than half the R-squared); (ii) using the Shapley approach to decompose the R-squared suggests 56% of the variation is explained by ties of the DA. Linking back to Figure 2A, our findings suggest that communities with near zero targeting are those in which, all else equal: (i) the DA and CA are politically aligned; (ii) the DA has few social ties to farmers.

[23] A similar hypothesis for why CAs matter for targeting is the idea that targeting requires signals about farmers. When the DA and CA are aligned, they have more correlated information about the need etc. of farmers in the community, and hence more precise signals of whether they should be targeted. Therefore, the set of targeted farmers looks more similar when the DA-CA are aligned relative to when the DA-CA are divided. This explanation also predicts that the total number of farmers targeted, among those exclusively tied to the DA or CA, should be higher when the DA-CA are aligned, which is ruled out by these results.

[24] In the context of information diffusion about a new product (microfinance), Banerjee *et al.* [2013] show the likelihood information is passed along to social ties is .350 (they also highlight the role that non-participants play for information diffusion). In the case of a new agricultural technology in Malawi, Beaman and Dillon [2017] show that social ties directly connected to a treated individual have a .300 probability of receiving the information. In another agricultural intervention, Beaman *et al.* [2020] show that respondents with two connections to entry points are 7.2pp more likely to have new information, corresponding to a 33% increase in knowledge relative to those unconnected to entry nodes.

4.2 Targeting

Empirical Method To study how social ties and social incentives interact to shape the precise targeting behavior of delivery agents, we estimate the following specification for farmer i in community c:

$$target_{ic} = \alpha + \sum_j \beta_j ST_{ijc} + \sum_j \sum_k \gamma_j^k (ST_{ijc} \times R_c) + \sum_{j \in \{DA,CA\}} \rho_j dist_{ij} + \lambda_c + u_{ic}. \quad (3)$$

$target_{ic} = 1$ if i is targeted by the delivery agent (so they receive seeds or training from them). $ST_{ijc} = 1$ if i has social tie of type j, where $j \in \{DA, CA, both, neither\}$ indicates being exclusively tied to the DA, exclusively tied to the CA, tied to both or to neither. All four groups are in the estimation sample, and the omitted group are those exclusively connected only to the CA ($ST_{i,CA,c}$). Thus β_{DA} measures the differential likelihood of being targeted between those exclusively tied to the DA and those exclusively tied to the CA, and is identified exploiting the second stage experimental variation. The earlier balancing checks showed that among those exclusively tied to the DA (or CA), they are balanced on observables across communities where the DA and CA are aligned or divided (Table 4).

$R_c = k \in \{0, 1\}$ again indicates the relationship between the actual delivery agent and her counterfactual in the community. As documented above, this captures a feature specific to the shortlisted candidates, and is uncorrelated to other community characteristics (Table 5). We check our core results to be robust to additionally controlling for a set of community characteristics (X_c) as well as their interactions with social ties ($ST_{ijc} \times X_c$).

All specifications control for the distance between farmer j's residence and the DA's and CA's residence ($dist_{ij}$), and community fixed effects (λ_c). We thus identify the causal impact on targeting of social ties and social incentives holding constant all other relevant aspects of community social networks in λ_c. For example, Alatas *et al.* [2016] show that community network characteristics such as the largest eigenvalue of the adjacency matrix are correlated with the ability of the network to target resources effectively – such features are captured in λ_c.

We report standard errors clustered by community-tie status (jc).

Results Column 1 of Table 7 first estimates a restricted version of (3) focusing only on how social ties are targeted. This shows that relative to farmers exclusively tied to the counterfactual agent, those exclusively tied to the delivery agent are 6.2pp more likely to be targeted by the DA. At the foot of Column 1 we report the share of those exclusively tied to the CA and targeted: 1.9%. The DA thus does not entirely ignore the exclusive ties of the CA, but there is a threefold likelihood in her own social ties being targeted relative to them. As much of the earlier literature has emphasized, this differential targeting probability can capture the return to the DA having lower screening costs of targeting her own ties – say because of better knowledge of their need, or being able to transmit information to them more effectively. The fact that $\widehat{\beta}_{DA} > 0$ re-confirms a central presumption of the local delivery model.

Column 2 estimates the full specification in (3), allowing the targeting of social ties to vary by social incentives embodied in the DA-CA relationship. When the DA and CA are aligned ($k = 0$), social ties of the DA are no more likely to be targeted than social ties of the CA: $\widehat{\gamma}^0_{DA}$ is precisely zero in this scenario. In sharp contrast, when the DA and CA have different political affiliations, social ties of the DA are 9.7pp more likely to be targeted than ties of the CA. This is a statistically and economically significant fivefold increase in the likelihood to be targeted over the baseline probability for the ties of the CA. Column 3 shows this result to be strengthened if we additionally control for all community characteristics shown in Table 5 (X_c) and interactions with social ties ($ST_{ijc} \times X_c$).

Two points are of note. First, if the mechanism causing DAs to target their own ties more than those of the CA is due to them having lower screening costs or other match-specific factors with those farmers, then this mechanism should be at play irrespective of whether the DA and CA are divided or not: such division pre-dates the intervention, and such *ex ante* screening costs between the DA and her ties are the same irrespective of the nature of the relationship between the actual and counterfactual delivery agent. Hence any interactive effect between social ties and the DA-CA relationship (γ^k_j) purely captures the social incentive provided by the identities of the DA and CA. If division between the DA and CA causes the DA to exert more effort to acquire information about their social ties, then this is part of the *ex post* endogenous response caused by social incentives, and results in different patterns of targeting.

Second, like the findings on coverage above, these results run entirely counter to the hypothesis that CAs matter because they allow the DA to target farmers exclusively tied to the CA, say because of information sharing between the CA and DA. To see this note that at the foot of Table 6, Columns 2 and 3, we report the likelihood that exclusive ties of the CA are targeted: when the DA-CA are aligned this is 4.1%, and DA ties are no more likely to be targeted ($\widehat{\beta}_{DA} = -.000$). When the DA-CA are divided this falls to 1% and DA ties are 14.3pp more likely to be targeted. Given the number of exclusive ties to the DA or CA are not statistically different, the total number of farmers targeted, among those exclusively tied to the DA or CA, is *lower* when the DA-CA are aligned, in line with the results on coverage reported in Table 6.

Pro-Poor Targeting The NGO's objective is that the intervention be used as an anti-poverty program, with DAs being instructed this is so. We thus next examine the extent to which social incentives impact whether DAs adhere to this objective, or behave according to some other motivation. We do so by extending the earlier specification to estimate:

$$\begin{aligned} target_{ic} \;=\; & \alpha + \textstyle\sum_j \beta_j ST_{ijc} + \sum_j \sum_k \gamma^k_j \left(ST_{ijc} \times R_c\right) + \sum_j \sum_k \tau^{kp}_j \left(ST_{ijc} \times R_c \times P_i\right) \\ & + \textstyle\sum_j \zeta_j \left(ST_{ijc} \times P_i\right) + \vartheta \left(R_c \times P_i\right) + \kappa P_i \\ & + \textstyle\sum_{j \in \{DA,CA\}} \rho_j dist_{ij} + \lambda_c + u_{ic}. \end{aligned} \quad (4)$$

We use food consumption to classify neediness, so $P_i = 1$ if the household of farmer i is in the lowest quartile of food consumption at baseline, and zero otherwise. The coefficients of interest are the τ_j^{kp}'s that capture the differential likelihood poor farmers tied to the DA are treated relative to those exclusively tied to the CA, given the nature of the DA-CA relationship $R_c = k \in \{0, 1\}$. Recall that we earlier documented that the second stage randomization ensures the levels of need are the same among the ties of the DA and CA (Table 4).

The result in Column 4 of Table 7 shows that when the DA-CA are aligned, poor social ties of the DA are as likely to be targeted as poor ties of the CA: the baseline probability of the latter being targeted is 3.6%. Moreover, in this scenario when the DA-CA are aligned, the non-poor ties of the DAs are as likely to be targeted as the non-poor ties of the CA. In short, social ties have little predictive power for who the DA targets in their community when the DA and CA are aligned. At the foot of Column 4 we show the p-value on the difference in probability of being treated for poor and non-poor farmers when the DA and CA are politically aligned ($p = .576$).

These findings are in sharp contrast to the targeting behavior of DAs when the DA and CA identify with different political parties. In that case we find that although the poor ties of the DA are not more likely to be treated than the poor ties of the CA, the non-poor ties of the DA are 12pp more likely to be treated than the non-poor ties of the CA. Moreover, among those farmers tied to the DA (relative to those tied to the CA), the non-poor are significantly more likely to be targeted than the poor ($p = .009$). For the poor, the DA-CA tie makes no difference to the likelihood they are targeted ($p = .375$). In contrast, the non-poor are significantly more likely to be targeted when the DA-CA are divided ($p = .007$).

As Column 5 shows, all these conclusions are robust to controlling for community characteristics (X_c) and their interactions with social ties ($ST_{ijc} \times X_c$).

To reiterate, our underlying identifying assumption is that whatever private information or match-specific costs the DA faces with regards to which farmers should be targeted, this does not vary with the nature of their relationship with the counterfactual agent. For example, the DA's ability to observe the neediness of their social ties should be the same irrespective of whether they are aligned or divided with respect to the CA.

Our findings thus reveal a basic tension at the heart of the local delivery model: DAs are induced to exert more effort to treat more farmers when there is a divisive relationship between themselves and the counterfactual delivery agent (Table 6). Such division can be leveraged to increase the intervention coverage of farmers in the community. However, when exerting more effort in this scenario, DAs are most likely to target non-poor farmers they are tied to (Table 7). This goes against the anti-poverty intentions of the intervention.

It is useful to contrast our findings with the established literature on elite capture, that has long been a concern for locally delivered programs [Bardhan and Mookherjee 2000, Mansuri and Rao 2013, Mookherjee 2015]. The classic trade-off emphasized there has been the valuable private information held locally that can help local interventions be better targeted, versus concerns over the accountability of local institutions or intermediaries to be accountable to the poor. As Dreze and Sen [1989] and Galasso and Ravallion [2005] emphasize, the accountability argument is more persuasive in settings where there is little distributional conflict at the local level.

Our results highlight that *elite division* between the DA and CA – separate from elite capture – acts as a social incentive driving the behavior of delivery agents and impacting the pro-poor targeting of the intervention. In our context, DAs either do not target the poor over the non-poor (when the DA and CA are aligned) or they target the non-poor (when the DA and CA are divided). Ignoring social incentives provided by the DA-CA relationship can lead to false impression of there being elite capture, when in fact division with the CA shapes the targeting behavior of DAs.

Robustness Tables A3 to A7 show our core findings on targeting from Table 7 to be robust to: (i) p-value corrections for randomization inference (which is important given the second stage randomization occurs within two candidates per community) and multiple hypothesis testing; (ii) narrower measures of ties between farmers and the DA and CA – such as whether they are friends/family, or talk about agriculture with each other; (iii) using the IAT score of political preferences to define the alignment/division between the DA and CA; (iv) alternative measures of the neediness of households, to assess the extent of pro-poor targeting – such as the wealth score of households at baseline, the number of modern techniques that had been adopted pre-intervention, and their baseline agricultural profits. Using these alternatives addresses concerns that neediness might be assessed by DAs using a more holistic measure, or that neediness might be harder to observe on some dimensions such as food consumption [Alatas *et al.* 2016, Kinnan 2021].[25]

Irrespective of the precise definitions along these three margins of data, social incentives embodied in the DA-CA relationship matter for the targeting behavior of DAs. Whenever the DA-CA are aligned, the DA does not favor her own social ties over those of the CA, but whenever they are divided the DA is significantly more likely to target: (i) her non-needy ties than those of the CA; (ii) within her social ties, she is significantly more likely to target her non-needy ties.[26]

Religious or Ethnic Ties? We have so far measured social incentives being driven by the DA-CA alignment or division along the lines of political identity. We have done so because this is the most salient form of self-identity and basis of disagreement in these communities. We next establish this is not just picking up other characteristics unrelated to identity and division such as religion and ethnicity. To do so we estimate (3) and (4) but defining ties using these dimensions, so for example, whether farmers are exclusively of the same religion as the DA (and not of the CA), exclusively of the same religion as the CA (and not of the DA) and so forth, and whether

[25] We also note all these conclusions on targeting are robust to clustering by community (j) throughout.

[26] The fact that the results are robust across alternative definitions of social tie suggests that targeting behavior of DAs is not driven by a lack of demand from farmers or trust towards the DA. For example, even poor farmers that report talking to the DA about agriculture pre-intervention, are significantly less likely to be targeted by the DA than her non-poor ties if the DA and CA hold opposing political identities.

the DA-CA belong to the same religion or not.

The results in Table 8 show a common pattern of null effects: (i) ties of religion or ethnicity do not predict the targeting behavior of DAs (Columns 1 and 4); (ii) the identities of DA and CA along lines of religion and ethnicity do not predict the targeting behavior of DAs (Columns 2 and 5); (iii) they also largely do not predict the extent to which DAs engage in pro-poor targeting (Columns 3 and 6).[27] Table A3 shows these null effects hold when p-value corrections for randomization inference and multiple hypothesis testing are made.

Diffusion, Surplus Maximization and Ex Post Transfers The results show that DA behavior is not consistent with them engaging in pro-poor targeting: the poor are no more likely to be targeted when the DA and CA are aligned, and they are more likely to target the non-poor when there is political division with the CA. This however does not tell us anything about the underlying motivations of DAs in the presence of social incentives. They might provide DAs with alternative objectives – such as to maximize the surplus through targeting, in the knowledge that communities can engage in *ex post* redistribution to the poor. This is especially the case for an agricultural intervention, unlike the targeting of basic food items or cash transfers (as in Alatas *et al.* 2012, 2019).

To shed light on this we examine two mechanisms through which the targeting behavior of DAs could be offset or exacerbated by communities: diffusion of the new technologies among farmers [Foster and Rosenzweig 1995, Conley and Udry 2010], and *ex post* transfers within communities [Basurto *et al.* 2020]. The results, in Table A7, show the diffusion of seeds among farmers actually exacerbates any initial targeting bias of DAs. More precisely, when the DA-CA are divided, non-poor ties of the DA (those most likely to be targeted) are also significantly more likely to report obtaining seeds from non-BRAC sources. Assuming they do not resort to buying seeds from the market (that are subject to the lemons problem), this suggests non-poor ties of the DA diffuse seeds among themselves.

The magnitude of this effect is large: non-poor ties of the DA are 29pp more likely than non-poor ties of the CA to report obtaining seeds from some source when the DA and CA are divided, that given the share of non-poor CA ties obtaining seeds in this scenario, implies around one third of DA social ties obtain modern seeds overall.

Second, as detailed in Appendix A.3, we use data on informal transfers between households to document that the pattern of *ex post* transfers does not change in response to DA behavior. Hence this channel does not ameliorate any targeting biases of DAs.

[27] We noted earlier that communities have one dominant ethnic group in them, with an ethnic fractionalization index of .1 (Table 1). Unlike for politics or religion, there are thus not two large rival groups in this dimension. As a result there are insufficient observations of some groups g to estimate all coefficients.

4.3 Allocation

Empirical Method However social incentives shape the underlying objective of DAs, we can exploit the second stage randomization to shed light on whether they act in a way consistent with maximizing surplus from their targeting behavior. To do so we estimate a specification analogous to (4) but where the outcome is profits of household i in community c:

$$\begin{aligned} profit_{ic} \quad = \quad & \alpha + \sum_j \beta_j ST_{ijc} + \sum_j \sum_k \gamma_j^k \left(ST_{ijc} \times R_c\right) + \sum_j \sum_k \pi_j^{kp} \left(ST_{ijc} \times R_c \times P_i\right) \\ & + \sum_j \zeta_j \left(R_c \times P_i\right) + \vartheta \left(R_c \times P_i\right) + \kappa P_i \\ & + \sum_{j \in \{DA,CA\}} \rho_j dist_{ij} + \lambda_c + u_{ic}. \end{aligned} \tag{5}$$

All right hand side terms are as defined earlier, and the omitted set of ties j are those farmers exclusively tied to the CA. The null is that whatever the objective of the DA determining her targeting behavior, she does so efficiently so $\pi_{DA}^{kp} = 0$ and the return to targeting her own ties and those of the CA are equalized: (i) for poor and non-poor households; (ii) in both states – when the DA and CA are aligned ($k = 0$) or when they are divided ($k = 1$). As before, the underlying assumption is that the information available to DAs and production possibilities of farmers are independent of the nature of the DA-CA relationship.[28]

Results We begin with a simpler specification that exploits only the experimental variation in exclusive ties of the DA and CA: in Column 1 of Table 9 we see that profits of DA ties are lower than CA ties but this difference is not statistically significant. In Column 2 we allow these impacts to vary with social incentives: in the case of DA-CA division in political identities, the profit returns to DA ties are significantly lower than those to CA ties. Column 3 estimates the specification in full: there are significant differences in profit returns across farmers so $\widehat{\pi}_j^{kp} \neq 0$ for all (j, k, p). More precisely when the DA and CA are aligned, the profit returns to poor farmers that are tied to the DA are significantly higher than those to poor farmers exclusively tied to the CA. At the foot of Column 3 we show that returns vary across the neediness status of households (holding constant the DA-CA relationship), and with the nature of the DA-CA alignment (holding constant the poverty status of households).

The $\widehat{\pi}_j^{kp}$ estimates show there is a wedge in average returns across farmers between those socially tied to the DA and to the CA. The targeting behavior of the DA leads to a resource misallocation if the marginal returns between these groups of farmers differ. We use three strategies to further demonstrate that DA targeting behavior leads to resource misallocation.

First, we compare differences in the likelihood of being targeted, $\widehat{\tau}_j^{kp}$ (from Column 5 of Table 7) to the profit returns $\widehat{\pi}_j^{kp}$ (from Column 3 of Table 9). Figure 5 plots these two sets of estimates

[28] This approach is similar to that used to test for favoritism in the allocation of credit, by computing differences in *ex post* returns to loans from alternative lenders [Khwaja and Mian 2005, Vera-Cossio 2020].

against one another, each with their associated 95% confidence interval. Panel A uses our baseline measure of need: whether the household lies in the bottom quartile of food consumption. We see a clear negative relationship between the likelihood a farmer in any given group is targeted ($\widehat{\tau}_j^{kp}$), and the average returns to that group ($\widehat{\pi}_j^{kp}$). The other panels of Figure 5 show broadly similar patterns of trade-off between profit gains/losses and targeting probabilities using alternative measures of neediness at baseline, related to wealth, the use of modern techniques, and baseline profit.

Observing higher average returns in groups with lower targeting rates is in line with there being diminishing returns to being targeted within each group. The fact that the highest average return is between poor farmers tied to the DA relative to those tied to the CA, when the DA and CA are politically aligned, matches with those farmers being least likely to be targeted by the DA. In other words, too few poor ties of the DAs are treated when the DA-CA are aligned, so there remains a large differential surplus from targeting them relative to identical ties of the CA. Similarly, the fact that the most negative average return differential between ties of the DA and CA is between non-poor farmers when the DA-CA are divided is because these farmers are most likely to be targeted. The DA over targets these farmers causing their average profit returns to be lower than counterfactual farmers tied to the CA.

Our second approach reflects the insight of Gollin and Udry [2020], that productivity dispersion is not always due to misallocation, but can be driven by measurement error, unobserved technology shocks or late season production shocks, adjustment costs or land quality. Unlike Gollin and Udry [2020], our estimates are not derived from estimating a production function to test whether marginal productivities across targeted and non-targeted farmers are equated. Rather we exploit the experimental variation in our design and assume the underlying production possibilities are the same for ties of the DA and CA.

However, a step we can take in this direction is to control for a rich set of baseline production characteristics correlated to farmer productivity. Doing so yields the estimates in Column 4 of Table 9. As expected, these $\widehat{\pi}_{DA}^{kp}$ estimates are generally smaller in absolute value. Nevertheless, we continue to reject the null that all four are equal to zero.[29]

We have so far used a specific measure of poverty or need (being in the lowest quartile of food consumption) and then quantify the extent of misallocation based on this. Although we find similar patterns of targeting bias using alternative measures of need (Table A6), we cannot be precisely sure which markers of need the DA actually uses to target her ties. Our third strategy uses a more agnostic approach to quantifying misallocation by assessing the distributional consequences of the intervention across households. We do so estimating quantile treatment effects (QTEs) for profits and present our results in Figure 6. To begin with, Panel A shows QTEs using the first stage of

[29] The production characteristics controlled for are the number of hours worked in agriculture, the number of acres cultivated, the number of marketable crops grown, whether the household has ever used improved seeds, dummies for whether it engages in intercropping, line sowing, proper weeding, single cropping, crop rotation, zero tillage, uses inorganic/chemical fertilizers, pesticides, herbicides, fungicides, manure or compost.

randomization and so only comparing treatment and control households. These QTEs show how skewed impacts are on profits, as is often the case for adoption of new technologies in agriculture [Suri *et al.* 2011].

In Panel B, we just use the second stage randomization in treated communities only, and then estimate quantile treatment effects of being exclusively tied to the DA (relative to being exclusively tied to the CA) in two cases: (i) when the DA and CA are aligned in political identities; (ii) when they are divided. The resulting estimates show a divergence in the distributional impacts of the intervention across these two states. Given our underlying assumptions that the information available to DAs and production possibilities across farmers are independent of the nature of the DA-CA relationship, these differences in dispersion (variance in profits) are again indicative of misallocation or distortions in at least one of these states.

However social incentives determine the underlying objective of the DA, this cannot be achieved in both states of the world given these divergent distributional impacts. This confirms social incentives shape targeting behavior and lead to a loss in potential surplus from the intervention.

Quantifying Misallocation To quantify the loss in potential surplus arising from social incentives, we take the absolute value of the coefficients of interest from (5), $\left|\widehat{\pi}_{DA}^{kp}\right|$, multiply each by the number of farmers in the same groups (n_{DA}^{kp}), so the total surplus loss is:

$$\sum_k \sum_p n_{DA}^{kp} \left|\widehat{\pi}_{DA}^{kp}\right|. \tag{6}$$

We benchmark this value by the implied gains to the intervention comparing treatment and control farmers. Taking the estimate on profits from Column 6 of Table 3 ($\widehat{\beta}$), assuming homogenous gains across all n farmers in treated communities, we estimate the total surplus generated by the intervention to be $n\widehat{\pi}$. Hence using this benchmark, the implied surplus loss is:

$$\frac{1}{n\widehat{\beta}} \sum_k \sum_p n_{DA}^{kp} \left|\widehat{\pi}_{DA}^{kp}\right| = 10.1\%. \tag{7}$$

We can alternatively calculate the implied misallocation from the earlier specification that controlled for a rich set of baseline production characteristics correlated to farmer productivity (Column 4 of Table 9). Doing so reveals a potential surplus loss of 10.0%. Hence even conditioning out these initial differences in agricultural productivity (that naturally make it harder to pick up diminishing returns to targeting more farmers within a group), we still document sizeable surplus losses caused by the targeting behavior of DAs being impacted by social incentives.

Three further points are of note. First, this method isolates misallocation occurring through targeting by the DA of her exclusive ties relative to those of the CA. This is a *lower bound* on total surplus loss because it assumes no mistargeting of DA effort among the 75% of farmers that are tied to both the DA and CA or tied to neither.

Second, we can conduct two counterfactual exercises based on alternative scenarios for the tie between the DA and CA. Assuming that the two are always aligned and recalculating using the approach above, the implied surplus loss is 10.5%. If we assume the two are always of opposing political identities, the implied loss is only slightly lower at 8.5%. The reason is that although the profit loss/gain is larger for the poor when aligned than for the non-poor when they are divided ($\left|\widehat{\pi}_{DA}^{k=0,p=1}\right| > \left|\widehat{\pi}_{DA}^{k=1,p=0}\right|$), the number of ties go in the opposite direction – with there being many more non-poor ties of the DA when the DA-CA are divided than poor ties when they are aligned ($n_{DA}^{k=1,p=0} > n_{DA}^{k=0,p=1}$). These two almost offset each other in (7), leaving the lost surplus almost equal between the two scenarios.

Third, we can compare the extent of misallocation arising from social incentives with other relevant estimates. At one extreme, the macro-orientated work has documented agricultural productivity gaps between rich and poor countries arising from tenure insecurity and imperfect land markets. Restuccia and Santaeulalia-Llopis [2017] present evidence that liberalizing land markets and input markets would lead to a potential threefold increase in agricultural output in the developing world. At the other extreme, micro-orientated work has shown the extent to which inputs across household plots can be misallocated because of intrahousehold inefficiencies. For example, Udry [1995] shows for rural households in Burkino Faso that plots controlled by women have significantly lower yields than similar plots controlled by men with a 6% loss in output due to input misallocation, aggregating up to a 13% loss in potential output at the village level that could be resolved through reallocating factors of production across households. The fundamental drivers of these inefficiencies relate to the absence of functioning land rental and labor markets (with moral hazard restricting labor transactions), but also due to asymmetric information and limited commitment.

Nepotism A much-raised concern with the local delivery model is that intermediaries can skew resources towards their family. This concern has driven the move to more bottom-up participatory approaches to development interventions, that have led to their own complications and still been subject to elite capture [Mansuri and Rao 2013]. We can assess the relative importance of nepotism in our context by estimating (5) using only family ties of the DA and CA. We then use the derived $\widehat{\pi}_{DA}^{kp}$'s to calculate the surplus loss due to these specific types of social tie. The result is in the final Column of Table 9: we see some evidence of a divergence in average returns among farmers that are family members of the DA versus those that are family members of the CA, but these estimates are not precisely estimated given the small sample sizes.

The implied consequence for output losses are second order because there are few family members of the DA (relative to the number of ties the DA has), as noted in Figure 4. When we use the estimates from Column 5 of Table 9 to make an analogous calculation to that in (7), we find a potential surplus loss of only 2.7% due to nepotism. Hence, as in Alatas *et al.* [2019] and Basurto *et al.* [2020], nepotism might occur but this is not the major source of allocative inefficiency.

4.4 Welfare

Method Combining our findings on coverage and targeting reveals countervailing effects social incentives can have on welfare: delivery agents exert more effort to target farmers when they and their counterfactual have opposing political identities. However, delivery agents are then also more likely to target non-needy farmers. To establish the distributional and net effects on welfare in treated communities from baseline to endline, we follow the approach of Atkinson [1970] and Maitra *et al.* [2019].

We divide treated communities into G groups of households, indexed $g = 1, ...G$ and defined along three dimensions: (i) whether they are exclusive ties of the DA or CA ($j \in \{DA, CA\}$); (ii) whether they reside in a community where the DA and CA are aligned or divided ($k \in \{0, 1\}$); (iii) whether they are poor or not ($p \in \{0, 1\}$). Our sample thus comprises eight groups g of type jkp. Group g comprises n_g farmers indexed $i = 1, ...n_g$ and $N = \sum_{g=1}^{G} n_g$ is the total number of households in treated communities. Pre-intervention agricultural profits of farmer i in group g are y_{ig0}. We assume households share a common CRRA utility function so the pre-intervention welfare of household i can be written as:

$$V(y_{ig0}) = \left\{ \begin{array}{c} \frac{y_{ig0}^{1-\theta}}{1-\theta} \text{ if } \theta > 0, \theta \neq 1 \\ \log(y_{ig0}) \text{ if } \theta = 1. \end{array} \right\} \tag{8}$$

The average welfare of the community at baseline is:

$$w = \frac{1}{N} \sum_{g=1}^{G} \sum_{i=1}^{n_g} V(y_{ig0}). \tag{9}$$

We assume households in group g experience the same treatment effect on profits, so have endline profits $y_{ig1} = T_g + y_{ig0}$. We take estimates of T_g from (5). These ITT estimates embody targeting probabilities (τ_g) as estimated from (4), as well as average treatment effects on profits (Π_g) that we cannot directly estimate. Hence $T_g = T_g(\tau_g, \Pi_g)$. The change in welfare for a member of group g is:[30]

$$\frac{1}{1-\theta}[(T_g(\tau_g, \Pi_g) + y_{ig0})^{1-\theta} - (y_{ig0})^{1-\theta}]. \tag{10}$$

The change in community welfare in treated communities then is:

$$\Delta w = \left\{ \begin{array}{c} \sum_{g=1}^{G} \alpha_g [\frac{1}{1-\theta}[(T_g(\tau_g, \Pi_g) + y_{ig0})^{1-\theta} - (y_{ig0})^{1-\theta}]] \text{ if } \theta \neq 1 \\ \sum_{g=1}^{G} \alpha_g [\log[T_g(\tau_g, \Pi_g) + y_{ig0}] - \log(y_{ig0})] \text{ if } \theta = 1, \end{array} \right\} \tag{11}$$

where α_g is the population share of group g at baseline. The proportionate change in welfare from

[30] More precisely, the estimates from (5) provide $T_g(\tau_g, \Pi_g)$ for all groups that are tied to the delivery agent, $g \in \{DA, k, p\}$. For groups tied to the CA and all baseline measures of profits for all groups (y_{ig0}) we take the mean profits in the group at endline and baseline respectively.

baseline to endline is:

$$\frac{\Delta w}{w}. \tag{12}$$

The CRRA function allows us to estimate welfare impacts for different degrees of inequality aversion θ. If $\theta = 0$, the intervention has same impact on welfare as on profits ($V(y_{ig}) = y_{ig}$). If $\theta = 1$ welfare is logarithmic so impacts are proportional to those on profits. As θ increases the welfare function becomes more concave and the poor receive greater weight.[31]

Results Figure 7 summarizes the results, with details shown in Table A7. Starting with $\theta = 0$ the intervention increases welfare by 14.6%. Placing more weight on the poor, say by setting $\theta = 3$, the implied welfare impact falls to 6.0%. Social incentives cause welfare impacts to differ across groups g. However, the welfare of all groups rises relative to baseline except for poor DA ties when the DA-CA hold different political identities.

We conduct two counterfactual exercises. In the first we only consider households in communities in which the DA and CA are aligned ($k = 0$). The welfare impacts of the intervention are smaller than in the overall sample: with $\theta = 0$ welfare rises by 11.1%, and when $\theta = 3$ the welfare impact falls to 4.2%. This is in contrast to our second counterfactual in which we only consider communities in which the DA and CA are divided ($k = 1$). In this scenario, if $\theta = 0$ welfare rises by 16.9%, and when $\theta = 3$ the welfare impact falls to 7.2%.

In short, welfare overall is higher when the DA and CA are divided because the impact on coverage more than offsets the mistargeting effect. This is true for any (plausible) degree of inequality aversion θ.

5 The Structure of Social Incentives

It is impossible to understand the behavior of delivery agents without accounting for the social incentives provided by the nature of their relationship to a counterfactual delivery agent identified in the same community. This is despite the fact that counterfactual agents play no formal role in intervention delivery. We have so far been able to rule out that delivery agents always maximize social surplus, and also rule out one simple explanation of why counterfactual agents matter: they can better transmit information to/from their exclusive ties when aligned with the DA. We thus propose two further explanations on the structure of social incentives to DAs: one individual-based and one group-based.

[31] To allow cross θ comparisons to be made, we normalize profits in all groups and time periods to lie between one and two. In other words we set $\min_g\{T_g(\tau_g, \Pi_g) + y_{ig0}, y_{ig0}\} = 1$ and $\max_g\{T_g(\tau_g, \Pi_g) + y_{ig0}, y_{ig0}\} = 2$.

5.1 Implicit Incentives

A first explanation builds on the idea that the selection procedure – choosing the delivery agent from a final shortlist of two – makes salient the next best alternative to the DA. Identifying a counterfactual might provide social incentives to the DA if she perceives the counterfactual will replace her if she underperforms. The DA might be especially motivated to exert effort to target her ties if she fears being replaced by another individual with opposing political identity. This helps explain our coverage results (Table 6). However, this hypothesis does not provide an obvious reason why the DA would target her non-poor ties in the face of this division and implicit incentive (Table 7). Skewing targeting towards her non-poor ties when the DA and CA are divided seems only to increase the risk of being replaced by the CA given the explicit objective of the NGO to use the intervention as an anti-poverty program.

5.2 Group Advancement

We thus focus attention on a group-based explanation for social incentives in this context. We follow an established literature and assume individuals hold social preferences over their social ties and other outgroups. When the DA is selected, if the DA and CA are divided in political identities, it is then salient to the DA's social group they can advance economically over the CA's social group if the DA exerts effort in targeting her own ties. This can explain our coverage results. In turn, it could strengthen identification with the ingroup and animosity towards the outgroup, in line with parochial altruism [Bowles and Choi 2007, Shayo 2020].[32]

There are two reasons why the DA then prefers to also target her non-poor ties. First, she might do so to curry favor with elites in her group to raise her social status [Shayo 2020]. A second reason builds on the idea that development brokers in local interventions engage in rent seeking behavior [Platteau and Gaspart 2003, Maitra *et al.* 2021]. The DA holds *unique* power in being able to advance her social group – there is no alternative to the DA that can play this role with the NGO. This however provides the DA the possibility to extract rents from ties she treats, and she can extract greater rents from the non-poor than the poor.

Rent Extraction The ability of DAs to extract rents from their ties when the DA and CA are divided can be tested. Borrowing ideas from the tax evasion literature, we examine whether the actual asset accumulation of DAs between baseline and endline is significantly greater than predicted based on the observed asset accumulation of potential delivery agent candidates in control villages [Pissarides and Weber 1989]. This is the outcome in Table 10, where the excess

[32] As long emphasized in the social psychology of identity [Tajfel and Turner 1979, Turner *et al.* 1987], when a social cleavage becomes salient, individuals identify with one of the relevant groups. This identity can then anchor beliefs about oneself and others, shape behavior and yield positive self-esteem if one identifies with a high status group [Bonomi *et al.* 2020].

asset accumulation of delivery agents is the log difference between their actual and predicted wealth at endline.[33]

Using the number of assets owned as the simplest measure of wealth, we see that when the DA has more social ties, she has significantly higher excess wealth than predicted (Column 1). However, this effect occurs only when the DA and CA are divided (Column 2). Columns 3 and 4 confirm exactly the same pattern of results when we use the value of assets as the measure of asset accumulation.[34] In short, the evidence suggests excess asset accumulation of delivery agents occurs in exactly those circumstances this hypothesis predicts: when the DA has more ties to farmers and when the DA and CA are of divided political identities.

We use the estimates in Column 4 to back out the value of rent extracted by the delivery agent to advance her social group when there is division with her counterfactual. Given the baseline average value of assets owned by DAs is UGX531,000, a 20.5% excess wealth accumulation corresponds to UGX108,855. As a benchmark, from Table 3 we see the ITT impact on net profits is UGX33,660, so the rent extraction of delivery agents is approximately three times the average gains to individual farmers from the intervention. This is the collective monetary cost the delivery agents's social group need to pay her to advance them relative to the social group of the counterfactual agent.[35]

6 Discussion

6.1 Policy Implications

Social incentives cause local delivery agents tasked to deliver a standard development intervention to skew its delivery towards their social ties. Although this still increases welfare overall, it leads

[33] We construct this measure in three steps. First, we apply the eligibility criteria used to select potential delivery agents to farmers in control villages. This identifies a small set of potential DAs in controls. Second, we regress the endline wealth of these farmers in controls on their baseline wealth, conditional on BRAC branch fixed effects, age, acres of land owned, number of marketable crops grown, and baseline profits. This provides a conditional expectation for asset accumulation among potential delivery agents between baseline and endline. Third, we apply this prediction model to the asset accumulation of actual delivery agents in treated communities.

[34] To reduce prediction noise, we use those assets that are owned most frequently and for which we have reliable price information across villages. These cover the following types of household and agricultural asset: furniture, furnishings (carpet, mat, mattress, etc.), bednets, household appliances, radio/cassette, bicycles, jewelry and watches, mobile phones, hoes, pangas/slashers etc, advances paid for rented shop premises, business furniture and fixings, and other business equipment. These asset categories have relatively low price dispersion across our control villages, and we use median prices to construct asset values.

[35] An alternative group-based explanation is that the DA acts as a conduit of the political party, to advance its political position. A consequence is that DAs are more likely to target swing voters. They could plausibly be those farmers connected to both the DA and CA, or those connected to neither. We do not find evidence that these two groups of farmers are more likely to be targeted than those exclusively tied to the DA, neither when the horizontal tie between the DA and the CA means they are politically aligned or when they are political rivals. Moreover, the vote buying literature nearly always suggests poor swing voters are more likely to be targeted, that again runs counter to our findings.

to a substantial loss in potential surplus and increases inequality within and across villages. We discuss modifications to the design of the local delivery model that could ameliorate these concerns.

Modifying the Local Delivery Model The process of shortlisting *per se* is not the fundamental problem. For example, our results suggest that when the DA-CA are aligned in political identities, the mere fact that they were the shortlisted two does not endogenously create rivalry between them: rather they exacerbate existing divisions in these communities that pre-date the intervention. This was also confirmed in the earlier robustness check that confirmed ties of religion or ethnicity do not predict which farmers are targeted by the DA. These are dimensions along which there is little conflict pre-intervention in these communities. Shortlisting candidates and selecting one does not endogenously engineer division between them on these dimensions.

However, a first obvious design change to consider is to select two delivery agents in each community in cases where the shortlisted two have rival identities, assuming this in itself nullifies the adverse effects of social incentives. The fixed costs of using two delivery agents are small compared to the surplus loss from only using one delivery agent per community.

A second set of responses emerge from the literature on elite capture. This has emphasized providing information to eligible households about the availability of treatment, and making treatment offers public within the community. These design adjustments provide forms of bottom-up monitoring of DAs or enable the poor to improve their negotiating position with regards to elites [Bjorkman and Svensson 2009, Banerjee *et al.* 2019].[36]

Selecting Delivery Agents As BRAC has scaled up the intervention through rural Uganda, engaging more than 800 delivery agents and reaching over 40,000 women farmers, their response to our findings has been to alter the eligibility criteria for delivery agents, making it easier for non-elites to be selected. This increases the costs of training DAs, but the hope is that it leads to more pro-poor targeting. Counter to this is the concern that it might also lead to more elite capture as chosen DAs seek to curry favor with elites or gain social esteem by targeting the non-poor.

The political economy literature on decentralization has emphasized democratic incentives can discipline local agents. The selection and retention mechanisms for delivery agents do not currently embody such incentives (beyond reputation): they face no oversight or formal accountability to locals nor any notion of re-election/re-appointment, that is surprising given farmers are well placed to evaluate the effectiveness of these agents. Recent experimental evidence shows the promise of using forms of direct democracy to select intermediaries [Deserranno *et al.* 2019].

[36] Olken [2007] discusses the limits of bottom up monitoring, stemming from free riding, or the inability of the poor to detect misallocation on technical projects. Hence it can be more effective to provide information to the poor when the benefits are private. Attanassova *et al.* [2013] show that the response to mistargeting is not necessarily to tighten up the eligibility criteria for the poor: conditioning on additional poverty indicators can strictly worsen targeting because the additional indicator affects not only who is eligible but also how costly verification of the (in)eligibility of other households is. If the latter is sufficiently negative then targeting worsens as a result of imposing stricter criteria.

By providing clear indications for career paths to posts outside of their community, development organizations might be able to harness individual career concerns and help offset the immediate social incentives that delivery agents otherwise face from within their communities [Dal Bo *et al.* 2013, Ashraf *et al.* 2020].

Another natural response is to suggest professionalizing a cadre of delivery agents. Such an approach runs into familiar problems of program scale-up: as labor supply curves slope upward, average costs must increase if program quality is to be held constant. Such labor supply constraints are first order in the context of agricultural extension interventions, where a key reason why such programs have limited impact is the lack of qualified personnel [Anderson and Feder 2007, Udry 2010, BenYishay and Mobarak 2019].[37] Deserranno *et al.* [2020] present evidence from a field experiment that vividly illustrates these labor supply constraints in Uganda: they find the entry of a health-orientated NGO reduces government provision of similar services because the NGO often hires the government worker, worsening health outcomes in villages where the NGO poaches the government agent from.

Incentivizing Delivery Agents Local delivery agents are hard to monitor, hence the limited use of monetary incentives in the standard local delivery model and the greater scope for social incentives to drive behavior. It is however natural to ask whether providing more high powered incentives would better align the interests of delivery agents to the pro-poor interests of the NGO, BRAC. One concern is that the offer of greater financial incentives impacts the pool of applicants, discouraging the most pro-social to apply [Deserranno 2019]. Conditional on selection, BenYishay and Mobarak [2019] show the effort of extension agents is positively influenced even by small incentives. Similarly Berg *et al.* [2019] find incentivizing local agents tasked to deliver information about a public health insurance program increases their effort, and reduces the importance of social ties for who they target. Whether the provision of monetary incentives would weaken social incentives in the context of local development interventions remains unknown.[38]

6.2 External Validity

The local delivery model is used for a raft of development interventions in agriculture, health, insurance and microcredit. We view our findings as being informative beyond agricultural extension interventions. Whenever delivery agents face weak monetary incentives and serve communities from which they are recruited, social incentives can play a first order role in determining their behavior and the effectiveness of the intervention they are tasked to deliver. To appreciate how

[37] Bridle *et al.* [2019] document that in Mozambique, extension coverage is as low as 1.3 agents per 10,000 rural individuals. BenYishay and Mobarak [2019] note that in Malawi, approximately half the government extension positions remain unfilled.

[38] A mechanism weakening the effect of monetary incentives is that they can act as signals to communities served, weakening the ability of delivery agents to conduct their work. The emerging evidence on this remains mixed [BenYishay and Mobarak 2019, Deserranno *et al.* 2019].

our results might apply more widely, it is important to be clear on the key structural features of our setting.

First, in our context there is one salient form of identity and disagreement (politics). This is the basis of DA-CA division that drives the targeting biases of DAs. Other characteristics can be salient drivers of identity and division across contexts. There is of course an established literature on ethnic fractionalization pinning back economic growth in Sub Saharan Africa [Easterly and Levine 1997, Alesina and La Ferrara 2005, Burgess *et al.* 2015, Hess *et al.* 2020], and mounting evidence from around the developing world of dimensions of identity related to religion, kinship structures, segmentary lineages and political networks all determining resource allocations [Fisman *et al.* 2017, Lowes *et al.* 2017, Enke 2019, Cruz *et al.* 2020, Moscona *et al.* 2020].

Second, the intervention we study is one in which it is possible for farmers to bypass delivery agents and receive seeds from others (diffusion) or BRAC directly. In principle such substitutes can offset targeting bias of DAs (although in our context we find these routes actually exacerbate this bias). Community wide *ex post* transfers could also be used to offset any initial targeting bias. This did not occur in our study setting, again perhaps these are polarized communities to begin with. Finally, with large enough interventions there is the possibility for market responses to offset distortions caused by the social incentives of delivery agents [Vera-Cossio 2020, Bjorkman Nyqvist *et al.* 2021].[39]

Third, the benefits distributed by delivery agents to farmers are private. Individual gains from being targeted are noticeable, enabling delivery agents to extract rents from targeted individuals. Such attribution is harder for more complex interventions, those requiring complementary actions or where benefits are spread over time, such as in health.

Finally, our research design allows us to study the social incentives provided to DAs by social structures of their communities taking these ties as exogenous to the intervention. This is in contrast to the literature of clientelism that has emphasized how beneficiaries can be incentivized to endogenously form ties to elites to gain access to distributed benefits [Vicente and Wantchekon 2009]. We would therefore expect the local delivery of interventions to gradually cause endogenous changes in social structure, the dynamics of which should be part of a future research agenda.

Understanding the effectiveness of the local delivery model as we vary these aspects – the salience of societal divisions, the availability of market and non-market substitutes for delivery agents, the extent to which the project delivers a private or (excludable) public good, and dynamic network formation – are all important comparative statics to take forward in future research.

[39] Vera-Cossio [2020] studies the provision of credit in Thai villages by local leaders under the Million Baht Village Fund. He finds they allocate credit towards richer, less productive and elite connected households. These impacts are however partially corrected by informal markets, with the net effect being a reduction in village output of 2.4%. Bjorkman Nyqvist *et al.* [2021] study the market for drugs in Uganda – that is subject to similar lemons problem as that for seeds. They show that competition from a reputable entrant (an NGO) has equilibrium effects in the market, raising the quality of drugs supplied by others. Such a market mechanism is unlikely to operate in our setting given the pilot scale of the intervention during our study period.

7 Conclusion

Given limited state capacity of low-income governments, and increased demands from foreign donors to use NGOs to bypass those same governments and deliver development interventions on the ground, the local delivery model is here to stay. The model intends to leverage the social networks in which agents are embedded, mobilizing insider knowledge of deserving beneficiaries, and harnessing the intrinsic motivation of locals to help their community. This approach has 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.

Our results indicate a need to be more sanguine about the advantages of local delivery of development programs, especially if delivery agents face weak monetary incentives. This is because social incentives then drive the behavior of delivery agents, creating a wedge between their motivations and any pro-poor intent of the principal or planner. By recognizing the critical role that social incentives play in determining the effectiveness of this model, we can begin to understand the circumstances in which interventions drive inequality between and within villages. While much remains to be understood, replicated and generalized, we hope that with further research and widening of the issues raised, a model of localized delivery that is robust to personal divisions and rivalries can be forged.

A Appendix

A.1 Sampling Bias and Measurement Error

To understand whether the variation in targeting rates across villages in Figure 2A can be explained by sampling variation and measurement error in which farmers are targeted, we run simulations under alternative assumptions on true targeting rates and the extent of measurement error. Our null is that targeting rates are homogeneous across villages, so the variation is spurious. We assume homogeneous targeting rates from 1 to 10%. For each we take 1000 sample draws following the same sample stratification as in our field experiment. We plot the empirical p-value, that is the share of draws that yield 50% of villages having a zero targeting rate, plus or minus 2.5% (red dashed line) to account for potential measurement error in targeting. We also plot the share of draws from the same simulation that yield a maximum targeting rate larger than 25%. Figure A1 shows the results. For all homogenous targeting rates the probability of drawing the observed distribution of targeting rates is close to zero: we cannot simultaneously generate near zero targeting rates in the majority of villages and high targeting rates in a small subset of villages.

A.2 IATs

We validate our self-reports from the actual and counterfactual delivery agent on the political tie between them using an Implicit Association Test (IAT). Our IAT constructs the degree to which agents have a bias in favor of the incumbent party (NRM) or the runner-up party (FDC) in the 2011 presidential elections, by measuring an agent's automatic associations with each political party (NRM or FDC). The IAT was divided into six blocks, with the first four being trial blocks to allow agents to become familiar with the test. In trial blocks 1 and 3, agents were timed to assess how quickly they were able to associate happy or sad faces with "good" versus "sad". In trial blocks 2 and 4, agents were timed to assess how quickly they were able to associate pictures of NRM or FDC logos/leaders with "NRM" versus "FDC". In block 5, agents were timed to assess how quickly they were able to associate faces or political pictures with "FDC or good" versus "NRM or bad". In block 6, agents were timed to assess how quickly they were able to associate faces or political pictures with "FDC or bad" versus "NRM or good".

A faster response time in block 5 compared to block 6 implies the agent feels more comfortable associating "good" pictures to FDC than to NRM, and we then assume that the agent has a more positive attitude toward FDC. A faster response time in block 6 compared to block 5 instead implies that the agent has a more positive attitude toward NRM. We drop observations with response time above $10,000$ milliseconds and all agents with at least 10% of response times less than 100 milliseconds (this dropped one agent).

As a measure of agents' bias towards FDC or NRM, we construct the IAT D-score. This is the within-agent normalized difference in average reaction times (ART) between block 6 (in which FDC is paired with good words) and block 5 (in which NRM is paired with good words):

$$D_i = \frac{ART_i^{Block6} - ART_i^{Block5}}{\sigma_i}, \tag{13}$$

where σ_i is the standard deviation of an agent's reaction times across both blocks. In our sample the average D-score is .053 and the standard deviation of the D-score is .589. We code agents as holding the same political identity if both of them have a positive or a negative D-score.

A.3 Diffusion and *Ex Post* Transfers

Any *ex post* diffusion of seeds among farmers might soften any *ex ante* targeting bias of the DA. To study this, in Column 1 of Table A7 we consider whether farmers report obtaining seeds from non-BRAC sources, including other farmers. We see the ties of the DA are no more likely than those of the CA to report doing so when the DA-CA are aligned. However, when the DA-CA are divided, the non-poor ties of the DA (those who are most likely to be targeted by the DA) are also significantly more likely to report obtaining seeds from non-BRAC sources. Assuming they do not resort to buying seeds from the market (that are subject to the lemons problem), this suggests non-poor ties of the DA diffuse seeds among themselves. This further *exacerbates* – rather than offsets – any targeting bias of the DA.[40]

Aggregating across all sources that farmers can obtain seeds (delivery agents, other farmers and BRAC branch offices), Column 2 shows the overall likelihood impact on farmers obtaining seeds: this confirms that non-poor DA ties are most likely to obtain seeds when the DA and CA hold opposing political identities.

An established literature shows the importance of informal transfers in rural economies to insure households against idiosyncratic income risk. Informal transfers can interlink with the targeting behavior of DAs so driving a wedge between poverty targeting and poverty reduction. Specifically, DAs could seek to target farmers in order to maximize total surplus in the knowledge that the community engages in *ex post* informal transfers towards the poor [Basurto *et al.* 2020]. If returns to adoption are rising in initial wealth, DAs will find it optimal to target non-poor farmers to first maximize the social surplus. We probe this interpretation using two strategies.[41]

First, we have seen that targeting patterns of the DAs differ depending on their political alignment with the CA. Given the underlying assumption that the return to being targeted for any given farmer is independent of the nature of the DA-CA relationship, this already rules out that they behave according to such an objective in *all* states.

Second, we can examine reports of informal transfers received and given by households and check whether they match a pattern that aligns with the targeting results. We construct measures on the extensive and intensive margin of informal net transfers: whether households report on net receiving more or fewer informal transfers, and the amount of net transfers they report informally receiving/giving. We then estimate a specification analogous to (4) but where the outcome is net transfers on the extensive or intensive margins.[42]

The results are in Columns 3 and 4 of Table A7. We see there is no differential change in net transfers on either margin for farmers exclusively tied to the DA relative to those exclusively tied to the CA. This is so irrespective of the nature of the relationship between the DA and CA ($\widehat{\tau}_{DA}^{kp} = 0$ for three out of four k, p combinations).

[40] This concentration of diffusion within non-poor farmers is in line with models of complex diffusion whereby farmers need to observe multiple members of their network adopting a new technology before becoming convinced to themselves adopt [Centola and Macy 2007]. Beaman *et al.* [2020] present evidence from a field experiment to exactly test between simple and complex diffusion models for a new agricultural technology. Their structural estimates suggest that 65% of farmers require multiple connections to adopt a new technology.

[41] Basurto *et al.* [2020] study elite capture and targeting in the context of a subsidy program administered by local chiefs in Malawi. They find that chiefs target households with higher returns, generating an allocation that is more productively efficient than what would have been achieved through strict poverty-targeting.

[42] Net transfers are defined as the total value of gifts received + total value of other transfers received, minus the total value of gifts sent + total value of other transfers sent. We do not include remittances in these transfers as they are far more likely to originate from outside the community.

A.4 Research Ethics

Following Asiedu *et al.* [2021] we detail key aspects of research ethics. On policy equipoise and scarcity, there was uncertainty regarding the net benefits from treatment for any given farmer. The interventions under study did not pose any potential harm to participants and non-participants. The program implementation was coordinated with the randomization protocol so that after the study was completed, the control group also received the treatment. As randomization was conducted at the village level, all study participants in treated villages could potentially access the intervention. Accessing any of the intervention services were voluntary for study subjects.

The researchers coordinated throughout with the implementing organization, BRAC. The program rollout took place according to the evaluation protocol. The researchers did not have any influence in the way programs were implemented or potential delivery agents shortlisted. We obtained informed consent from all participants prior to the study. The informed consent included an explanation of the agricultural extension and microfinance programs. The consent form also described the research team, and met IRB requirements of explaining the purpose of the study, the participants' risks and rights, confidentiality, and contact information. Research staff and enumerator teams were not subject to additional risks in the data collection process. None of the researchers have financial or reputational conflicts of interest with regard to the research results. No contractual restrictions were imposed on the researchers limiting their ability to report the study findings.

On potential harms to participants or nonparticipants, our data collection and research procedures adhered to protocols around privacy, confidentiality, risk-management, and informed consent. Regardless of their access to the interventions, participants were not considered particularly vulnerable (beyond residing in poverty). Participants capacity to access future services or policies is not reduced by their participation in the study.

Besides individual consent from study participants, consultations were conducted with local representatives at the district and community levels. In the four study districts, separate Memorandum of Understanding were signed, and the Local Council Chairperson (LC1) in each village was consulted before any data collection took place. All the enumerators involved in data collection were recruited from the study districts to ensure they are aware about implicit social norms in these communities. The salience and sensitivity of discussing political ideologies was revealed in our pilot fieldwork: individuals were often wary of reporting their political affiliation to enumerators. Hence this is never asked to respondents.

Summary findings from the project have been presented to district level authorities and policy briefs were distributed to the national and district level stakeholders. However, no activity for sharing results to participants in each study village is planned due to resource constraints. We do not foresee risks of the misuse of research findings.

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Table 1: Balance on Village Characteristics

Means and standard deviation in parentheses

	(1) Control	(2) Treated: Agriculture Extension Program	p-value (1)=(2)
Number of villages	59	108	
A. Village Characteristics			
Number of households	182.2	180.2	[.837]
	(74.09)	(81.73)	
Share of households engaged in agriculture	.785	.789	[.856]
	(.211)	(.214)	
Distance to a control/treated village (miles)	5.801	5.200	[.622]
	(4.271)	(3.743)	
Distance to BRAC branch (minutes walking)	98.91	104.0	[.671]
	(57.95)	(59.64)	
Average HH wealth score (0-100)	61.91	62.01	[.709]
	(4.754)	(5.319)	
Standard deviation of HH wealth score	12.95	12.94	[.851]
	(1.516)	(1.584)	
B. Social Structure			
Political fractionalization index	.462	.470	[.605]
	(.047)	(.044)	
Share of votes for incumbent party (NRM)	.561	.549	[.858]
	(.133)	(.122)	
Religious fractionalization index	.432	.436	[.930]
	(.137)	(.126)	
Share of Protestants (vs. Catholics or others)	.589	.569	[.510]
	(.216)	(.218)	
Ethnic fractionalization index	.087	.104	[.377]
	(.100)	(.111)	
Share of Banyankole (vs. Bakiga or others)	.586	.634	[.310]
	(.463)	(.438)	

Notes: Village-level summary statistics for control villages (Column 1) and treated villages (Column 2). The p-values are obtained from regressing each of the reported baseline variable on the dummy for treatment with robust standard errors and controlling for branch fixed effects. Shortest distance to a control/treated village (miles) is the distance from the control village to the closest treated village in Column 1 and the distance from the treated village to the closest control village in Column 2. The household wealth score is measured for all households in our census survey by aggregating ten poverty indicators into a score going from 0 to 100. Average HH wealth score (0-100) and standard deviation of HH wealth score calculate the average and the standard deviation of household's wealth score in the village. The fractionalization indices are based on Alesina et al. [2003]. The political fractionalization index equals 1 minus the sum of the square of the share of votes in the village for the incumbent NRM, the runner-up FDC, and other parties (from the 2011 Ugandan presidential results by polling station). The religious/ethnic fractionalization index is measured in the same way as the political fractionalization index using the share of households in our census survey belonging to each religion/ethnicity.

Table 2: Balance on Household Characteristics

Means and standard deviation in parentheses

	(1) Control	(2) Treated: Agriculture Extension Program	p-value (1)=(2)
Number of households	1,677	3,064	
A. Socio-economic background			
Household head completed primary education	.431	.459	[.393]
Wealth score (0-100)	59.55	60.10	[.954]
	(12.93)	(13.57)	
Food expenditure in last month (thousand UGX)	27.49	27.52	[.533]
	(66.36)	(63.70)	
B. Seeds and modern techniques			
Knows improved seeds	.947	.928	[.583]
Believes improved seeds have positive returns	.760	.700	[.422]
Ever adopted improved seeds	.372	.297	[.954]
Number of techniques known (out of 6)	4.640	4.660	[.383]
	(.954)	(.922)	
Number of techniques believed to have positive returns (out of 6)	3.380	3.485	[.078]
	(1.156)	(1.086)	
Number of techniques ever adopted (out of 6)	3.174	3.162	[.370]
	(.970)	(.957)	
Ever adopted mixed cropping	.915	.897	[.546]
C. Agriculture in last season			
Hours in agriculture per day	6.224	5.853	[.252]
	(1.826)	(1.697)	
Acres of land cultivated	1.050	1.151	[.088]
	(.968)	(1.027)	
Number of crops grown	3.672	3.734	[.456]
	(1.402)	(1.442)	
Number of marketable crops grown	1.247	1.236	[.552]
	(.903)	(.891)	
Share of output sold	.494	.581	[.229]
	(2.399)	(4.218)	
Profits (thousand UGX)	74.40	82.89	[.260]
	(313.9)	(304.1)	

Notes: Household-level summary statistics for households in control villages (Column 1) and treatment villages (Column 2). The p-values are obtained from regressing each of the reported baseline variable on the dummy for Treatment with standard errors clustered at the village level and controlling for branch fixed effects. The wealth score (0-100) is measured by aggregating ten poverty indicators into a score going from 0 to 100. Food expenditure in last month (thousand UGX) is the total household expenditure on food, beverage and tobacco per month per adult equivalent. Number of techniques ever adopted (out of 6) calculates the number of techniques ever adopted (out of 6: intercropping, line sowing, zero tillage, proper weeding, crop rotation, avoid mixed cropping). Number of marketable crops grown counts the number of vegetables, roots and fruits crops produced in the last season. Share of output sold is the share of the total output quantity produced by the household in the last season that is sold rather than consumed. Profits (thousand UGX) is the total output value minus total expenditures value in the last season. All monetary values are expressed in thousand UGX and are truncated above and below two standard deviations from the mean. Exchange rate: 1 USD = 2519.6 UGX (March 2014).

Table 3: Program Evaluation

ITT estimates and standard errors in parentheses (clustered by village)
p-values adjusted for randomization inference and multiple hypothesis testing in braces

	Targeting			Other Sources		Agriculture in Last Season		Consumption and Assets	
	(1) Targeted by the delivery agent: Received seeds or training in last year	(2) Received seeds from the delivery agent in last year	(3) Trained by the delivery agent in last year	(4) Received seeds from BRAC branch office in last year	(5) Received seeds from non-BRAC source in last year	(6) Profits in last season (000 UGX)	(7) Number of marketable crops grown in last season	(8) Food expenditure in last week (000 UGX)	(9) Productive assets (000 UGX)
Treated Village: Agricultural Extension Intervention	.039***	.031***	.037***	.043***	.019	33.66**	.228**	6.424**	3.069**
	(.007)	(.006)	(.007)	(.007)	(.015)	(14.06)	(.106)	(3.117)	(1.474)
	{.001,.002}	{.001,.004}	{.001,.002}	{.001,.006}	{.033,.202}	{.001,.052}	{.001,.052}	{.001,.078}	{.057,.078}
Mean in control	.001	.001	.000	.001	.094	76.96	1.243	24.69	20.12
Observations	4,378	4,390	4,381	4,390	4,410	3,968	4,410	4,395	4,339

Notes: Household (farmer)-level OLS regressions. All regressions control for branch fixed effects and for the baseline value of the outcome variable. In parentheses, we report standard errors clustered at the village level. In brackets, we report randomization inference p-values computed following Young [2019] approach, and p-values adjusted for multiple hypothesis testing computed using Romano and Wolf [2016] step-down procedure. Profits (000 UGX) are the total output value minus total expenditures value in the last season. Number of marketable crops grown counts the number of vegetables, root and fruit crops produced in the last season. Food expenditure in last month (000 UGX) is the total household expenditure on food, beverage and tobacco per month per adult equivalent. Productive assets (000 UGX) is the total value of agriculture assets owned by the household. All monetary values are expressed in thousand UGX and are truncated above and below two standard deviations from the mean. Exchange rate: 1 USD = 2519.6 UGX (March 2014). ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 4: Balance on Social Ties to Actual and Counterfactual Delivery Agents

Means and standard deviation in parentheses

	Actual and Counterfactual Delivery Agents				Farmers Exclusively Tied to the…			Farmers Exclusively Tied to the…		Farmers Exclusively Tied to the…			
	(1) Delivery Agent	(2) Counterfactual Agent	(3) Percentile of the Delivery Agent Within Community	p-value (1)=(2)	(4) Delivery Agent	(5) Counterfactual Agent	p-value (4)=(5)	(6) Delivery Agent	(7) Counterfactual Agent	(8) Delivery Agent	(9) Counterfactual Agent	p-value (6)=(8)	p-value (7)=(9)
								DA-CA Aligned		DA-CA Divided			
A. Socio-economic background													
Household head has primary educ.	.617	.533	78	[.358]	.416	.472	[.146]	.427	.504	.392	.450	[.537]	[.583]
Acres of land owned	2.949	2.873	70	[.886]	2.470	2.547	[.933]	2.692	2.303	2.360	2.846	[.563]	[.416]
	(2.508)	(2.313)			(4.573)	(5.151)		(3.505)	(3.565)	(5.359)	(6.199)		
Wealth score (0-100)					60.01	59.24	[.688]	63.02	59.58	57.46	59.26	[.196]	[.687]
					(12.67)	(13.68)		(11.82)	(13.48)	(12.95)	(13.40)		
Food expenditure in last month (thousand UGX)					32.17	24.03	[.256]	30.13	30.34	35.14	21.66	[.323]	[.917]
					(60.75)	(48.90)		(42.23)	(73.81)	(73.54)	(28.53)		
In 1st quartile of distribution of food expenditure					.237	.220	[.650]	.182	.207	.262	.219	[.376]	[.891]
B. Seeds and modern techniques													
Ever adopted improved seeds	.843	.800	94	[.569]	.224	.230	[.392]	.260	.233	.213	.197	[.655]	[.700]
Number of techniques ever adopted (out of 6)	3.583	3.652	67	[.456]	3.255	3.020	[.089]	3.340	3.021	3.229	3.035	[.495]	[.730]
	(.821)	(.640)			(1.021)	(.996)		(1.060)	(1.021)	(1.002)	(.993)		
C. Agriculture in last season													
Hours in agriculture per day	6.596	6.088	71	[.136]	5.607	5.586	[.472]	5.750	5.698	5.565	5.537	[.394]	[.579]
	(2.043)	(1.515)			(1.559)	(1.476)		(1.680)	(1.397)	(1.450)	(1.517)		
Acres of land cultivated	1.583	1.763	72	[.414]	1.152	1.190	[.897]	1.158	1.253	1.205	1.180	[.858]	[.562]
	(1.086)	(1.359)			(.954)	(1.070)		(1.046)	(1.177)	(.913)	(1.059)		
Profits (thousand UGX)	471.9	585.9	94	[.463]	82.92	77.62	[.781]	99.26	82.02	80.94	80.76	[.818]	[.596]
	(327.6)	(708.7)			(314.0)	(266.9)		(414.8)	(292.3)	(244.4)	(264.0)		
D. Distance													
Distance from home of the delivery agent (minutes walking)					1.431	2.169	[.051]	1.601	1.490	1.322	2.788	[.202]	[.225]
					(3.336)	(6.837)		(3.240)	(1.697)	(3.599)	(8.912)		
Distance from home of the counterfactual agent (minutes walking)					1.918	2.171	[.554]	1.814	1.821	1.924	2.541	[1.000]	[.383]
					(5.041)	(7.742)		(5.283)	(3.062)	(5.196)	(9.959)		
Resides in the same village as delivery agent					.450	.327	[.324]	.232	.479	.614	.246	[.012]	[.286]
					(.498)	(.470)		(.424)	(.501)	(.489)	(.431)		
Resides in the same village as counterfactual agent					.347	.495	[.167]	.364	.371	.303	.610	[.862]	[.065]
					(.477)	(.501)		(.483)	(.485)	(.461)	(.489)		

Notes: Summary statistics are presented for delivery agents (Column 1), counterfactual agents (Column 2), farmers who know only the delivery agent at baseline (Columns 4, 6 and 8), farmers who know only the counterfactual agent at baseline (Columns 5, 7 and 9). The p-values for (1)=(2) [resp., (4)=(5)] are obtained from regressing each of the reported baseline variable on the dummy for treatment with robust standard errors (resp., standard errors clustered at the village level) and controlling for branch fixed effects. The p-values (6)=(8) and (7)=(9) are similar to those comparing (4)=(5) with the difference that the former is restricted to the sample of communities in which the agents are aligned (have the same political identity) and the latter to the sample in which the agents are divided (have different political identities). The percentile of the delivery agent within community in Column 3 presents 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). The wealth score (0-100) is measured by aggregating ten poverty indicators into a score going from 0 to 100. Food consumption in last month (thousand UGX) is the total consumption of food, beverage and tobacco per month per adult equivalent. Number of techniques ever adopted (out of 6) calculates the number of techniques ever adopted (out of 6: intercropping, line sowing, zero tillage, proper weeding, crop rotation, avoid mixed cropping). Profits (thousand UGX) is the total output value minus total expenditures value in the last season. All monetary values are expressed in thousand UGX and are truncated above and below two standard deviations from the mean. Exchange rate: 1 USD = 2519.6 UGX (March 2014).

Table 5: Community Characteristics, by Tie between the Actual and Counterfactual Delivery Agents

Means and standard deviation in parentheses

	Actual and Counterfactual Delivery Agent are...		
	(1) Aligned	(2) Divided	p-value (1)=(2)
Number of communities	27	26	
A. Social Ties			
Share of farmers exclusively tied to actual delivery agent	.068	.118	[.378]
	(.086)	(.165)	
Share of farmers exclusively tied to counterfactual agent	.099	.178	[.343]
	(.155)	(.195)	
Share of farmers tied to both agents	.596	.582	[.792]
	(.336)	(.336)	
B. Politics			
Politics fractionalization index	.466	.463	[.297]
	(.045)	(.046)	
Share of votes for incumbent party (NRM)	.583	.594	[.249]
	(.114)	(.110)	
C. Religion and Ethnicity			
Religious fractionalization index	.446	.429	[.589]
	(.137)	(.076)	
Share of Protestants (vs. Catholics or others)	.543	.538	[.765]
	(.202)	(.213)	
Ethnic fractionalization index	.104	.077	[.133]
	(.105)	(.063)	
Share of Banyankole (vs. Bakiga or others)	.461	.558	[.670]
	(.467)	(.473)	
D. Religion and Ethnicity Among Households in Top Decile of Wealth Score			
Religious fractionalization index	.444	.427	[.618]
	(.140)	(.085)	
Ethnic fractionalization index	.100	.084	[.363]
	(.108)	(.086)	
E. Community Characteristics			
Number of households	309.4	270.5	[.148]
	(185.5)	(138.6)	
Share of households engaged in agriculture	.748	.803	[.220]
	(.252)	(.170)	
Distance to BRAC branch (minutes walking)	95.45	107.1	[.759]
	(56.83)	(60.14)	
Average HH wealth score (0-100)	61.24	61.54	[.884]
	(6.225)	(3.690)	
Standard deviation of HH wealth score	12.90	13.37	[.081]
	(1.502)	(1.116)	

Notes: Community-level summary statistics for communities in which the agents have the same political identity (Column 1) or different parties (Column 2), as reported by the agents. Information on the political identify of potential delivery agents is missing in 7 out of 60 communities. The p-values are obtained from regressing each of the reported baseline variable on the dummy for treatment with robust standard errors and controlling for for branch fixed effects. Share of farmers tied to actual delivery (resp., counterfactual) agent is the share of farmers in our baseline survey who know only the delivery agent (resp., counterfactual agent). The fractionalization indices are based on Alesina et al. [2003]. The political fractionalization index equals 1 minus the sum of the square of the share of votes in the village for the incumbent NRM, the runner-up FDC, and other parties (from the 2011 Ugandan presidential results by polling station). The religious/ethnic fractionalization index is measured in the same way as the political fractionalization index using the share of households in our census survey belonging to each religion/ethnicity. The household wealth score is measured for all households in our census survey by aggregating ten poverty indicators into a score going from 0 to 100. Average HH wealth score (0-100) and standard deviation of HH wealth score calculate the average and the standard deviation of the wealth score in the community.

Table 6: Coverage

Dependent Variable: Number of farmers targeted, among those exclusively socially tied to the DA or CA

OLS estimates and robust standard errors in parentheses

	(1) Social Ties	(2) Social Ties	(3) Social Incentives
#Ties to Delivery Agent	.138***	.123	
	(.041)	(.074)	
#Ties to Delivery Agent Squared		.001	
		(.002)	
DA-CA Aligned			.171
			(.164)
#Ties to Delivery Agent x DA-CA Aligned			.030
			(.074)
#Ties to Delivery Agent x DA-CA Divided			.179***
			(.050)
Mean	.500	.500	.500
Test [p-value]			[.437]
R-squared	.675	.676	.742
Partial R-squared for #Ties to DA	.306	.121	.366
Shapley Decomposition of the R-squared	.565	.695	.557
Observations	60	60	53

Notes: Community-level OLS regressions. All regressions control for branch fixed effects and for the number of exclusive ties (number of farmers who know one of the two agents). In Column 3, we also control for the interaction between the number of exclusive ties and DA-CA Aligned. In parentheses, we report robust standard errors. Number of farmers targeted by the delivery agent is the total number of sample farmers, among those exclusively tied to the actual or counterfactual delivery agent, in the community who report having received seeds or training from the delivery agent in the last year. Number tied to delivery agent is the number of sample farmers in the community who know only the delivery agent. DA-CA aligned (resp., divided) equals 1 if agents report having the same (resp., different) political identity. Information on whether agents have the same political identity or not is missing in 7 out of 60 communities and this explains the smaller sample size. In Column 3 the p-value reports the test of equality between the number of ties to the delivery agent interactions when the DA-CA are aligned and divided. The partial R-squared for number of ties to delivery agent is the variation in the outcome variable that is explained by variation in the number of farmers tied to the delivery agent. The Shapley decomposition of the R-squared reports the proportion of the R-squared that is contributed by the reported coefficients. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 7: Targeting

Dependent Variable: Delivery agent targets farmer (received seeds or training in last year)

OLS estimates and standard errors in parentheses (clustered by community and ties)

	(1) Social Ties	(2) Social Incentives	(3) Interactions	(4) Pro-poor Targeting	(5) Interactions
Tied to Delivery Agent	.062***				
	(.023)				
Tied to Delivery Agent x DA-CA Aligned		-.000	-.000		
		(.028)	(.018)		
Tied to Delivery Agent x DA-CA Divided		.097***	.143***		
		(.028)	(.031)		
Tied to Delivery Agent x DA-CA Aligned x Poor				-.030	-.044
				(.067)	(.057)
Tied to Delivery Agent x DA-CA Aligned x Not Poor				.008	.012
				(.028)	(.021)
Tied to Delivery Agent x DA-CA Divided x Poor				.035	.075*
				(.030)	(.042)
Tied to Delivery Agent x DA-CA Divided x Not Poor				.120***	.158***
				(.030)	(.032)
Community Fixed Effects	Yes	Yes	Yes	Yes	Yes
Community Controls x DA-CA Aligned	No	No	Yes	No	Yes
Mean Outcome, Tied to CA	.019				
Mean Outcome, Tied to CA, DA-CA Aligned		.041	.041		
Mean Outcome, Tied to CA, DA-CA Divided		.010	.010		
Mean Outcome, Poor and Tied to CA				.036	.036
Mean Outcome, Not Poor and Tied to CA				.014	.014
Anti-poverty targeting:					
∆prob(targeted)/∆ Poor | DA-CA Aligned				[.576]	[.381]
∆prob(targeted)/∆ Poor | DA-CA Divided				[.009]	[.022]
Anti-poverty targeting and DA-CA horizontal tie:					
∆prob(targeted)/∆ Horizontal Tie | Poor				[.375]	[.096]
∆prob(targeted)/∆ Horizontal Tie | Not Poor				[.007]	[.000]
Observations	2,421	2,216	2,195	2,216	2,195

Notes: Farmer-level OLS regressions. All regressions control for community fixed effects, an indicator for whether the farmer is tied to both agents, an indicator for whether the farmer is tied to no agent, the walking distance to the delivery agent's home, and the walking distance to the counterfactual agent's home. Columns 3 and 5 also control for all community characteristics presented in Table 5 interacted with whether the DA and CA are aligned. In parentheses, we report standard errors clustered at the community and ties level. Tied to delivery agent equals 1 if the farmer is socially tied only to the delivery agent. The omitted group (tied to counterfactual agent) is composed of farmers who are socially tied only to the counterfactual agent. DA-CA aligned (resp., divided) equals 1 if the farmer resides in a community in which the agents report having the same (resp., different) political identities. Information on whether agents have the same political identity or not is missing in 7 out of 60 communities and this explains the smaller sample size in Columns 2-5. Poor (resp., not poor) equals 1 if the household belongs (resp., does not belong) to the bottom quartile of the within-communitydistribution of food expenditure. At the foot of Columns 4 and 5 we report (in order of appearance) p-values for: (i) Tied to Delivery Agent x DA-CA Aligned x Poor vs. Not Poor, (ii) Tied to Delivery Agent x DA-CA Divided x Poor vs. Not Poor, (iii) Tied to Delivery Agent x Poor x DA-CA Aligned vs. Divided, (iv) Tied to Delivery Agent x Not Poor x DA-CA Aligned vs. Divided. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 8: Targeting Other Groups of Farmer

Dependent Variable: Delivery agent targets farmer (received seeds or training in last year)

OLS estimates and standard errors in parentheses (clustered by community and ties)

	Religion			Ethnicity		
	(1) Social Ties	(2) Social Incentives	(3) Pro-poor Targeting	(4) Social Ties	(5) Social Incentives	(6) Pro-poor Targeting
Tied to Delivery Agent	-.023			-.010		
	(.018)			(.048)		
Tied to Delivery Agent x DA-CA Aligned		-.009			-	
		(.026)			-	
Tied to Delivery Agent x DA-CA Divided		-.039			-.013	
		(.028)			(.053)	
Tied to Delivery Agent x DA-CA Aligned x Poor			-.027			-
			(.043)			-
Tied to Delivery Agent x DA-CA Aligned x Not Poor			-.003			-
			(.035)			-
Tied to Delivery Agent x DA-CA Divided x Poor			-.067*			-.017
			(.034)			(.049)
Tied to Delivery Agent x DA-CA Divided x Not Poor			-.031			-.008
			(.034)			(.058)
Community Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Community Controls x DA-CA Aligned	No	Yes	Yes	No	Yes	Yes
Mean Outcome, Tied to Counterfactual Agent	.046	.046	.046	.032	.032	.032
Mean Outcome, Poor and Tied to Counterfactual Agent			.073			.037
Mean Outcome, Not Poor and Tied to Counterfactual Agent			.037			.031
Observations	2,420	2,194	2,194	2,413	2,187	2,187

Notes: Farmer-level OLS regressions. All regressions control for community fixed effects, an indicator for whether the farmer is tied to both agents, an indicator for whether the farmer is tied to no agent, the walking distance to the delivery agent's home, and the walking distance to the counterfactual agent's home. Columns 2-3 and 5-6 also control for all community characteristics presented in Table 5 interacted with whether the DA and CA are aligned. In parentheses, we report standard errors clustered at the community and ties level. The dependent variable equals one if the farmer reports having received seeds or training from the delivery agent in the last year. Tied to delivery agent equals 1 if the farmer has the same religion (resp., ethnicity) as the delivery agent only in Columns 1-3 (resp., Columns 4-6). The omitted group (tied to counterfactual agent) is composed of farmers who belong to the same religion (resp., ethnicity) in Columns 1-3 (resp., Columns 4-6) as the counterfactual agent only. DA-CA aligned equals 1 if the agents have the same religion (resp., ethnicity) in Columns 1-3 (resp., Columns 4-6). DA-CA divided equals 1 if the agents have different religions (resp., ethnicities) in Columns 1-3 (resp., Columns 4-6). Poor (resp., not poor) equals 1 if the household belongs (resp., does not belong) to the bottom quartile of the within-community distribution of food expenditure. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 9: Misallocation

Dependent Variable: Profits from agriculture in last season (thousand UGX)

OLS estimates and standard errors in parentheses (clustered by community and ties)

	(1) Social Ties	(2) Social Incentives	(3) Pro-poor	(4) Agricultural Controls	(5) Family Ties Only
Tied to Delivery Agent	-36.94				
	(24.97)				
Tied to Delivery Agent x DA-CA Aligned		35.66			
		(38.21)			
Tied to Delivery Agent x DA-CA Divided		-93.18***			
		(29.88)			
Tied to Delivery Agent x DA-CA Aligned x Poor			211.7**	205.9**	11.79
			(88.24)	(93.04)	(38.24)
Tied to Delivery Agent x DA-CA Aligned x Not Poor			63.91	53.59	-118.4
			(54.70)	(54.66)	(167.6)
Tied to Delivery Agent x DA-CA Divided x Poor			-23.23	-40.09	-209.9
			(72.749)	(80.225)	(176.6)
Tied to Delivery Agent x DA-CA Divided x Not Poor			-53.57	-55.45	-108.4
			(45.26)	(47.50)	(73.56)
Community Fixed Effects	Yes	Yes	Yes	Yes	Yes
Community Controls x DA-CA Aligned	No	No	Yes	Yes	Yes
Agriculture Controls	No	No	No	Yes	No
Mean in Control			114.1	114.1	113.4
Mean for Tied to CA	128.2		128.2	128.2	216.3
Mean for Tied to CA, DA-CA Aligned		89.40			
Mean for Tied to CA, DA-CA Divided		156.1			
∆Profits/∆ Poor | DA-CA Aligned			147.8*	152.3*	130.2
			(85.14)	(84.47)	(155.7)
∆Profits/∆ Poor | DA-CA Divided			30.34	15.36	-101.5
			(67.58)	(75.84)	(186.5)
∆Profits/∆ Horizontal Tie | Poor			234.9**	246.0*	221.7
			(116.8)	(125.9)	(185.2)
∆Profits/∆ Horizontal Tie | Not Poor			117.5*	109.0	-9.967
			(68.19)	(70.46)	(180.2)
Observations	2,337	2,142	2,122	2,010	2,122

Notes: Farmer-level OLS regressions. All regressions control for community fixed effects, an indicator for whether the farmer is tied to both agents, an indicator for whether the farmer is tied to no agent, the walking distance to the delivery agent's home, and the walking distance to the counterfactual agent's home. Regressions in Columns 3, 4 and 5 control for poor, DA-CA aligned*Poor, Tied to Both*DC-CA Aligned*Poor, Tied to Both *DC-CA Aligned*Not Poor, Tied to Both *DC-CA Divided* Poor, Tied to Both *DC-CA Divided*Not Poor, Tied to None *DC-CA Aligned*Poor, Tied to None *DC-CA Aligned*Not Poor, Tied to None *DC-CA Divided* Poor, Tied to None *DC-CA Divided*Not Poor. All regressions in columns 3-5 also control for all community characteristics presented in Table 5 interacted with ties. Column 4 additionally controls for hours in agriculture per day, acres of land cultivated, number of marketable crops grown, ever adopted seeds/ crop rotation/ intercropping/ proper weeding/ line sowing/ zero tillage/ mixed cropping/ pesticides/ fertilizers/ manure. In parentheses, we report standard errors clustered at the community and vertical ties. Profits are equal to total output value minus total expenditures value in the last season (thousand UGX), truncated above and below two standard deviations from the mean. Exchange rate: 1 USD = 2519.6 UGX (March 2014). Tied to delivery agent equals 1 if the farmer knows only the delivery agent in Columns 1-4, and is a family member of the the delivery agent only in Column 5. DA-CA aligned (resp., divided) equals 1 if the farmer resides in a community in which the agents report having the same (resp., different) political identity. Information on whether agents have the same political identity or not is missing in 7 out of 60 communities and this explains the smaller sample size. Poor (resp., not poor) equals 1 if the household belongs (resp., does not belong) to the bottom quartile of the within-community distribution of food expenditure. At the foot we report (in order of appearance) the coefficients for: (i) Tied to Delivery Agent x DA-CA Aligned x Poor vs. Not Poor, (ii) Tied to Delivery Agent x DA-CA Divided x Poor vs. Not Poor, (iii) Tied to Delivery Agent x Poor x DA-CA Aligned vs. Divided, (iv) Tied to Delivery Agent x Not Poor x DA-CA Aligned vs. Divided. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 10: Excess Asset Accumulation of Delivery Agents

Dependent variable: Excess wealth of the delivery agent (actual-predicted)

OLS estimates and robust standard errors in parentheses

	Number of Assets Owned		Value of Assets Owned	
	(1) Social Ties	(2) Social Incentives	(3) Social Ties	(4) Social Incentives
#Ties to Delivery Agent	.042**		.161***	
	(.020)		(.046)	
#Ties to Delivery Agent x DA-CA Aligned		.036		.061
		(.047)		(.113)
#Ties to Delivery Agent x DA-CA Divided		.063**		.205***
		(.026)		(.063)
Mean Outcome	.751	.751	-.022	-.022
R-squared	.153	.183	.181	.199
Observations	60	53	60	53

Notes: Community-level OLS regressions. The excess wealth growth of the delivery agent is measured as the log of the difference between the actual wealth of the delivery agent at endline and her predicted wealth. The predicted wealth is obtained by (1) regressing the endline wealth of farmers in control villages who satisfy all criteria to become a delivery agent on their baseline wealth, and (2) using the estimated coefficient to predict the delivery agent's endline wealth based on her baseline wealth. In predicting wealth, we control for branch fixed effects, age, acres of land owned, number of marketable crop grown, and profits in agriculture (step 1). Wealth is proxied with the number of assets owned or with the value of assets owned. The value of assets owned equals the number of each asset owned times the median price of that asset in the community. We consider 18 categories of assets for which there is relatively low variation in prices across villages. The number tied to delivery agent is the number of sample farmers in the community who know only the delivery agent. DA-CA aligned (resp., divided) equals 1 if agents report having the same (resp., different) political identities. Information on whether agents have the same political identity or not is missing in 7 out of 60 communities and this explains the smaller sample size. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Figure 1: Study Timeline

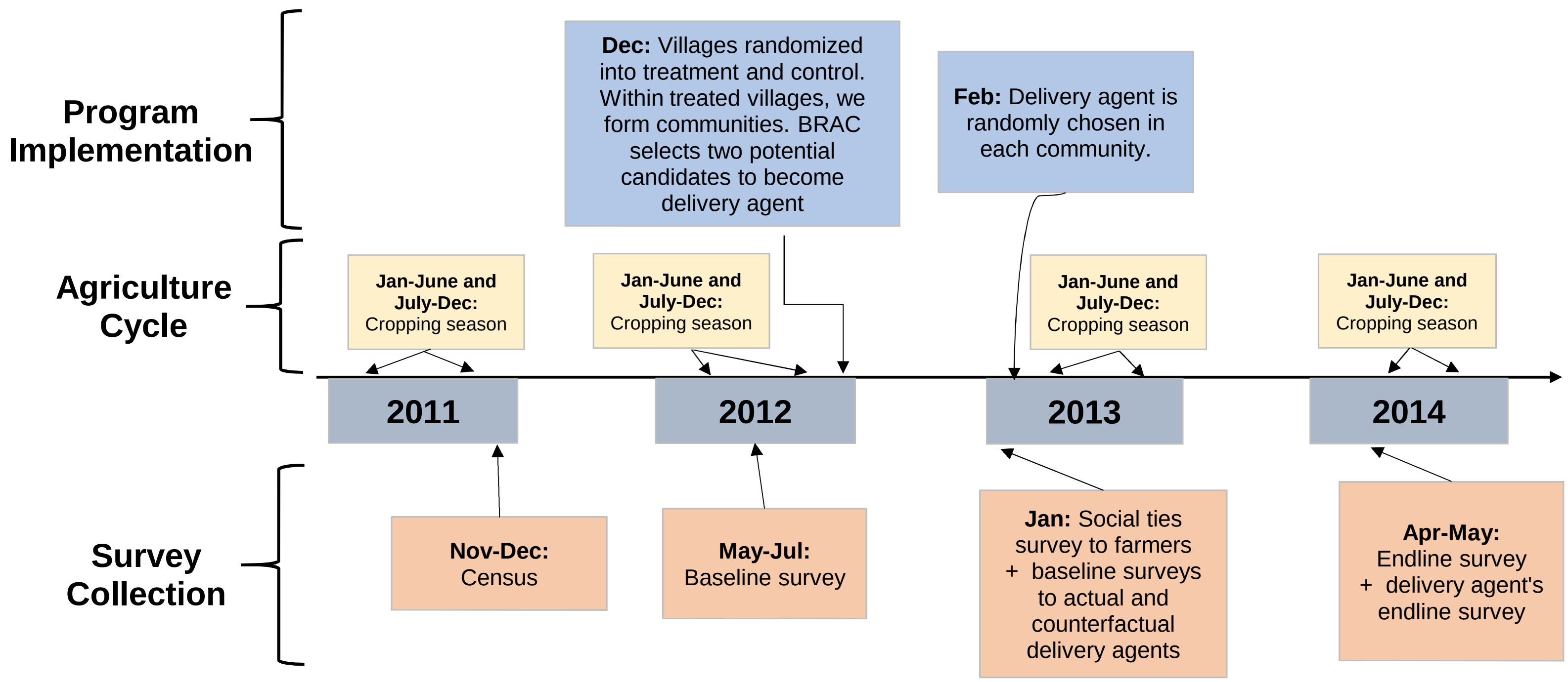
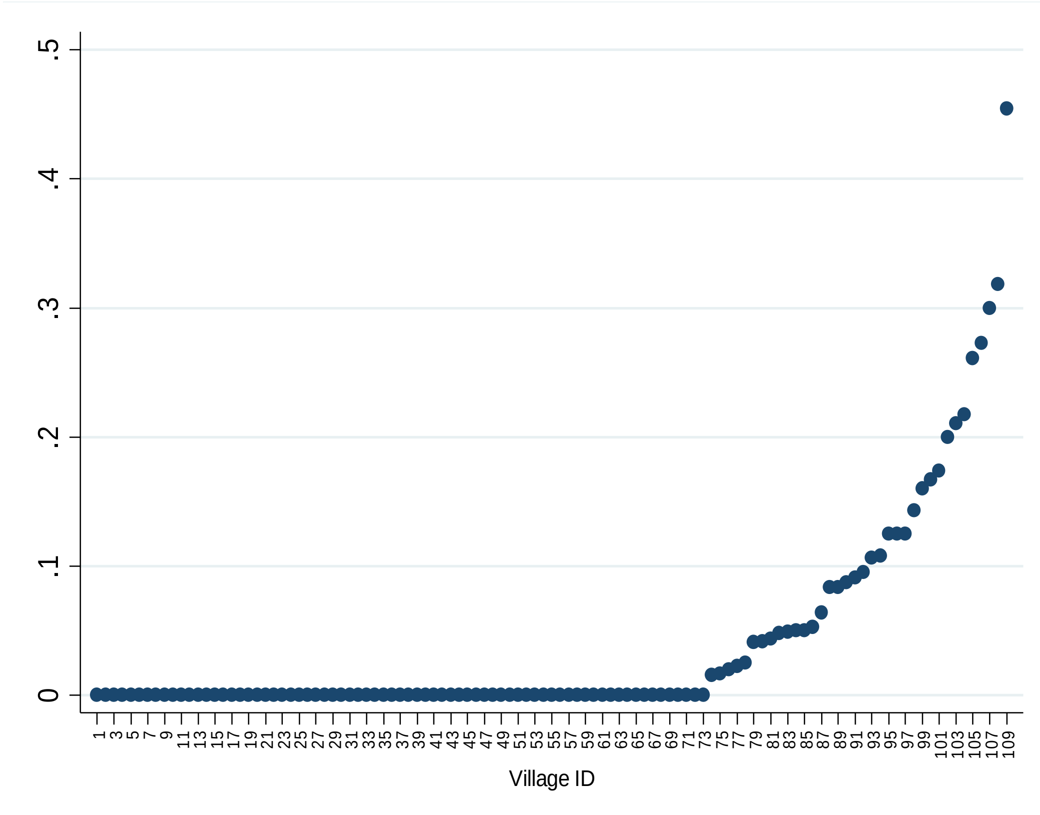
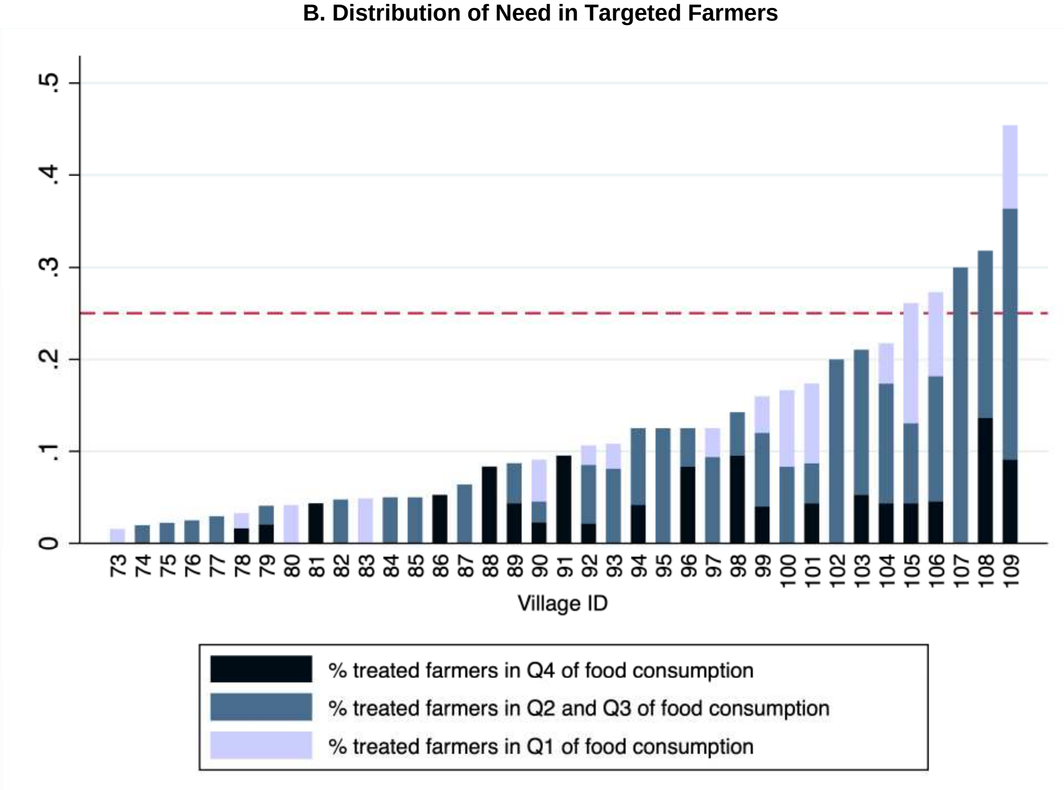


Figure 2: Heterogeneity Across Villages in Delivery

A. Share of Farmers Targeted by the Delivery Agent in Last Year



B. Distribution of Need in Targeted Farmers



Notes: Panel A presents the share of farmers in a village that are targeted (i.e., received seeds or training in the last year) by the delivery agent. Panel B presents the share of targeted farmers who are in the bottom quartile (Q4), in the second and third quartile (Q2 and Q3) and in the top quartile (Q1) of food consumption. Sample restricted to villages with a strictly positive targeting rate. Villages are sorted from lowest to highest targeting rates.

Figure 3: Second Stage of Randomization

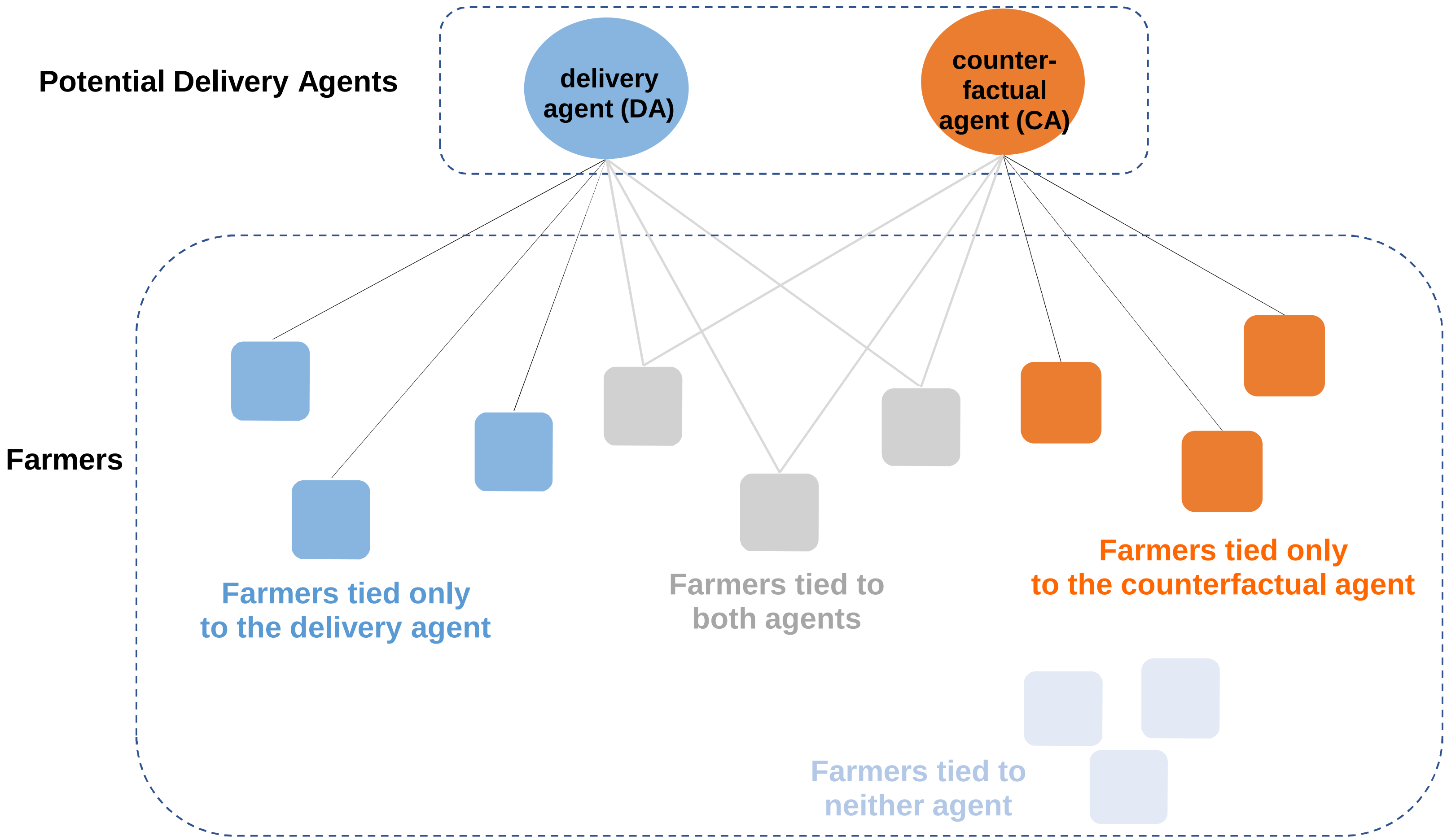
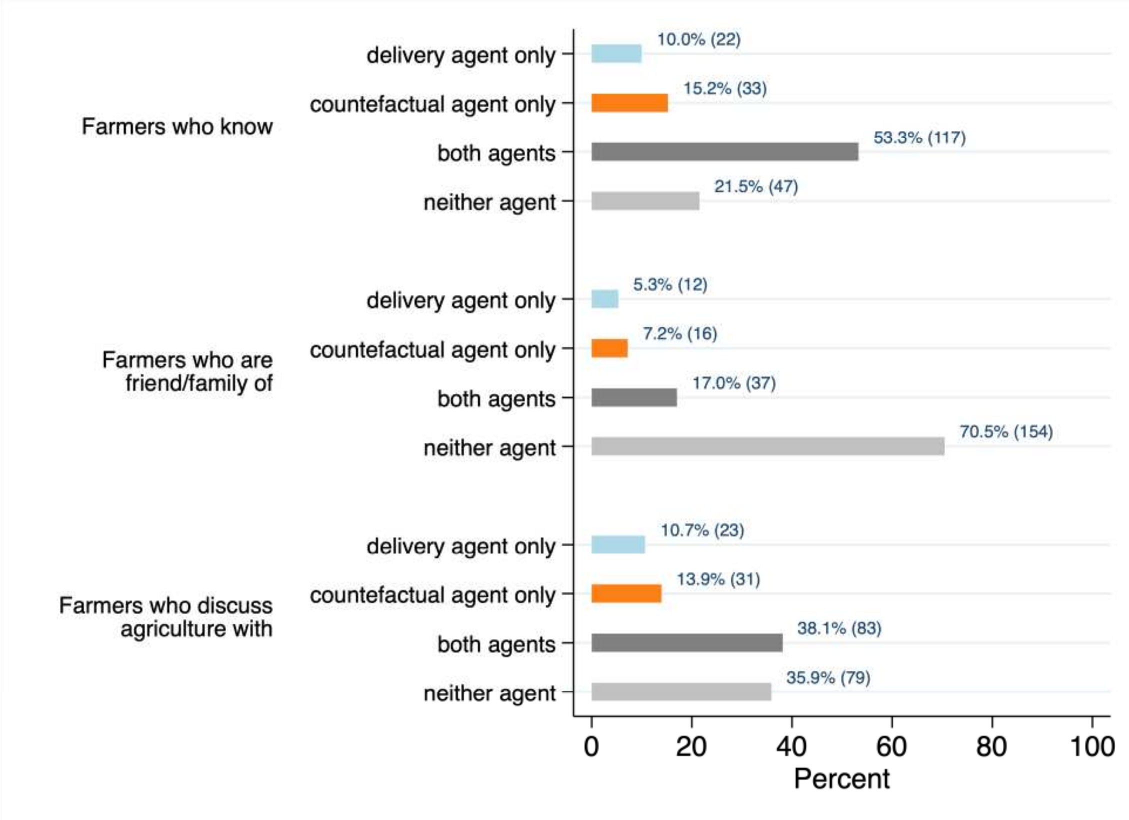
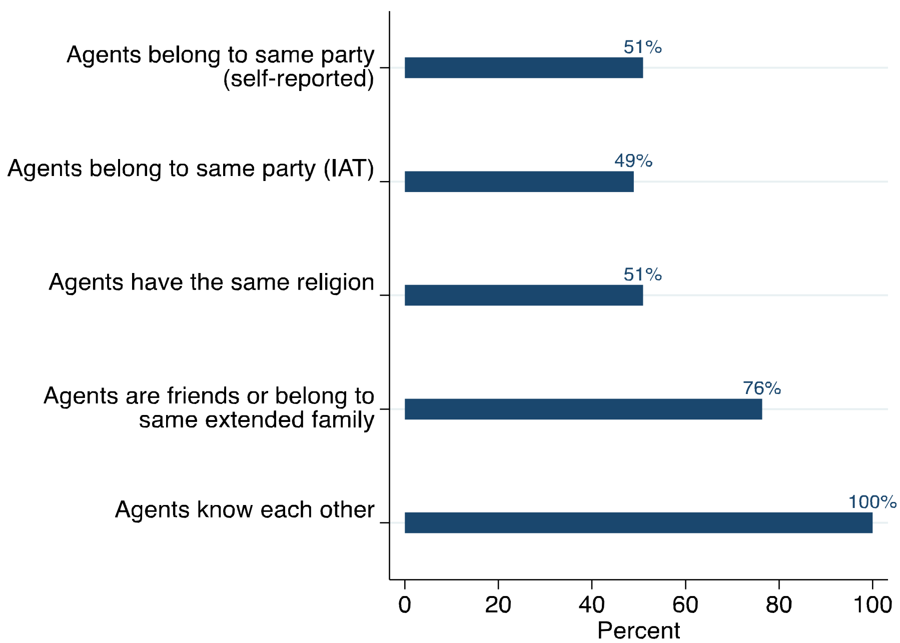


Figure 4: Ties

A. Ties Between Actual and Counterfactual Delivery Agents and Farmers

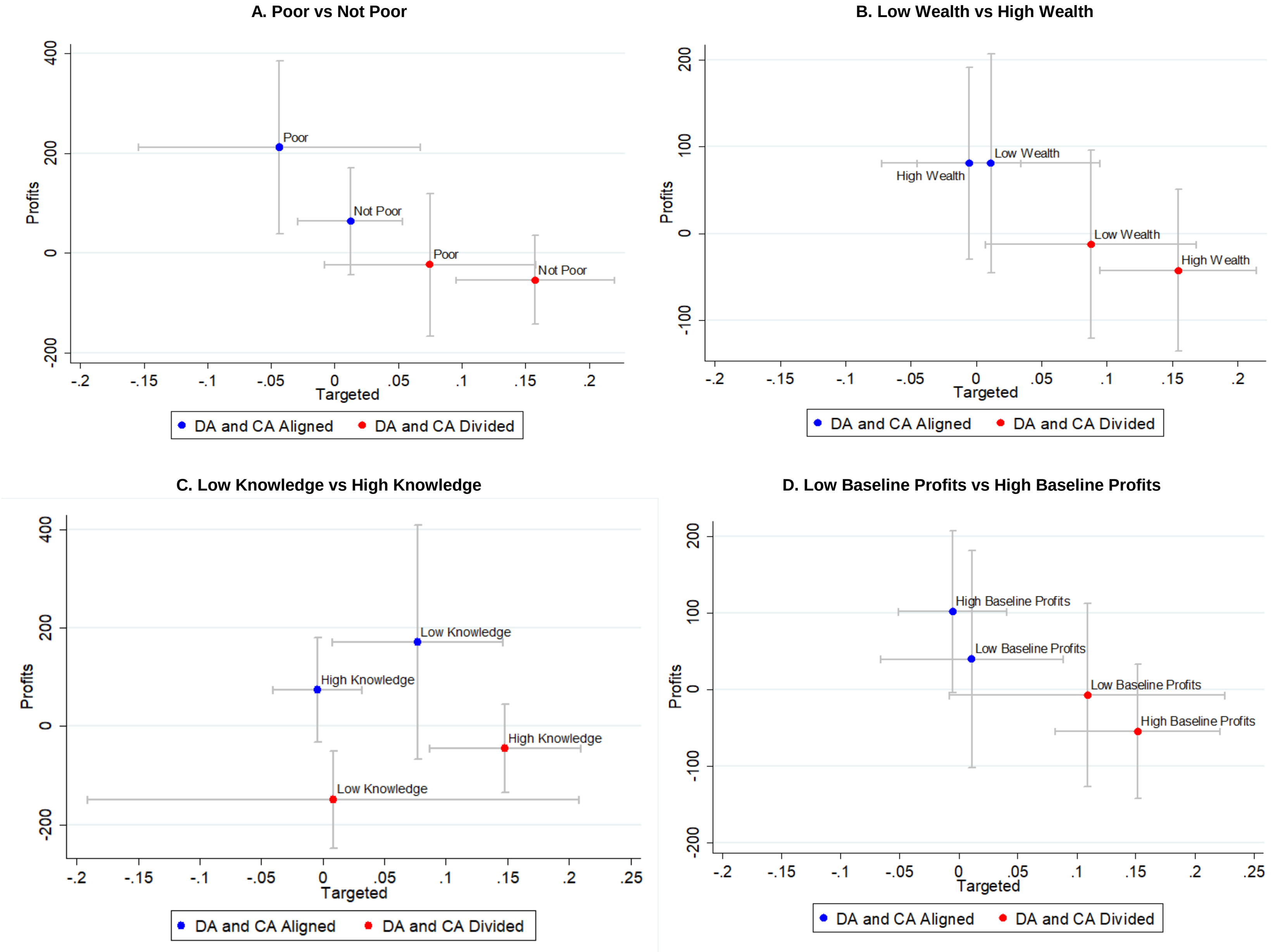


B. Ties Between Actual and Counterfactual Delivery Agents



Notes: Panel A presents the distribution of social ties within a community. In parentheses, we report the number of farmers per community sampled in our baseline survey by ties. Panel B presents the distribution of DA-CA ties across communities.

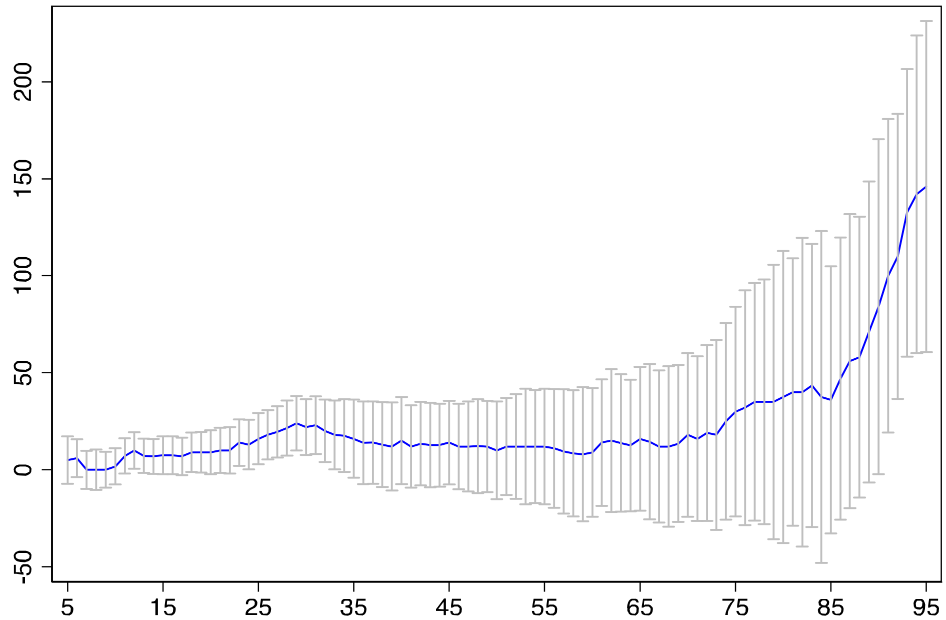
Figure 5: Targeting and Misallocation



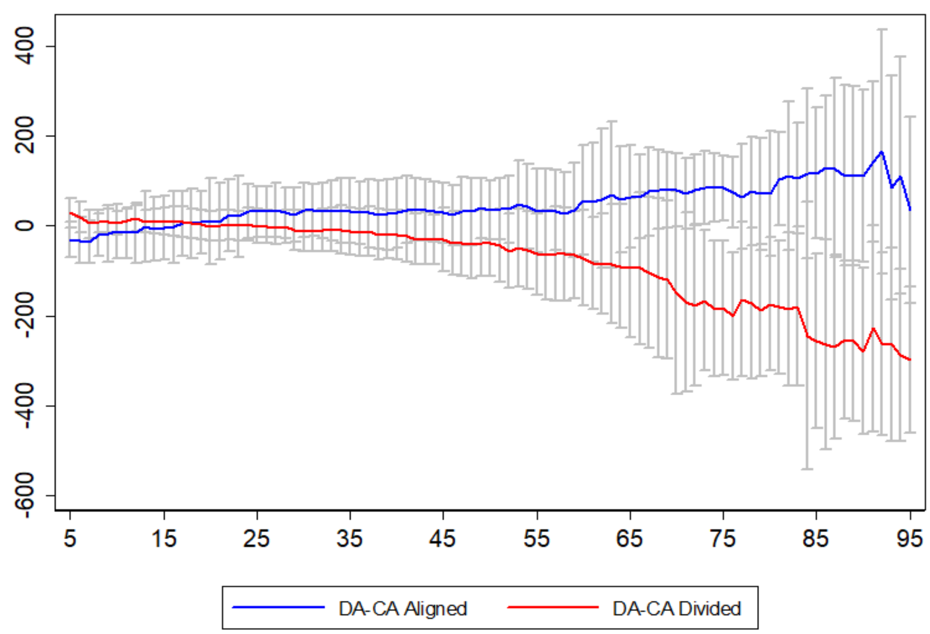
Notes: Panel A presents the difference in profits on the Y-axis and in targeting rates on the X-axis for farmers tied to the delivery agent vs. counterfactual agent only. The differences are presented separately for households in Q4 of X vs. not in Q4 of X, where X is baseline food consumption in Panel A, baseline wealth score in Panel B, baseline number of techniques ever adopted in Panel C and baseline profits in Panel D. The differences are also presented separately for villages in which the agents are aligned (report having the same political identity; blue dots) and villages which the agents are divided (report having different political identities; red dots). Vertical dashed lines are 95% confidence intervals. Targeting rates are the share of farmers who received seeds or training from the delivery agent in the last year. The household wealth score is measured by aggregating ten poverty indicators into a score going from 0 to 100.

Figure 6: Distributional Impacts - QTE on Profits

A. Treatment versus Control

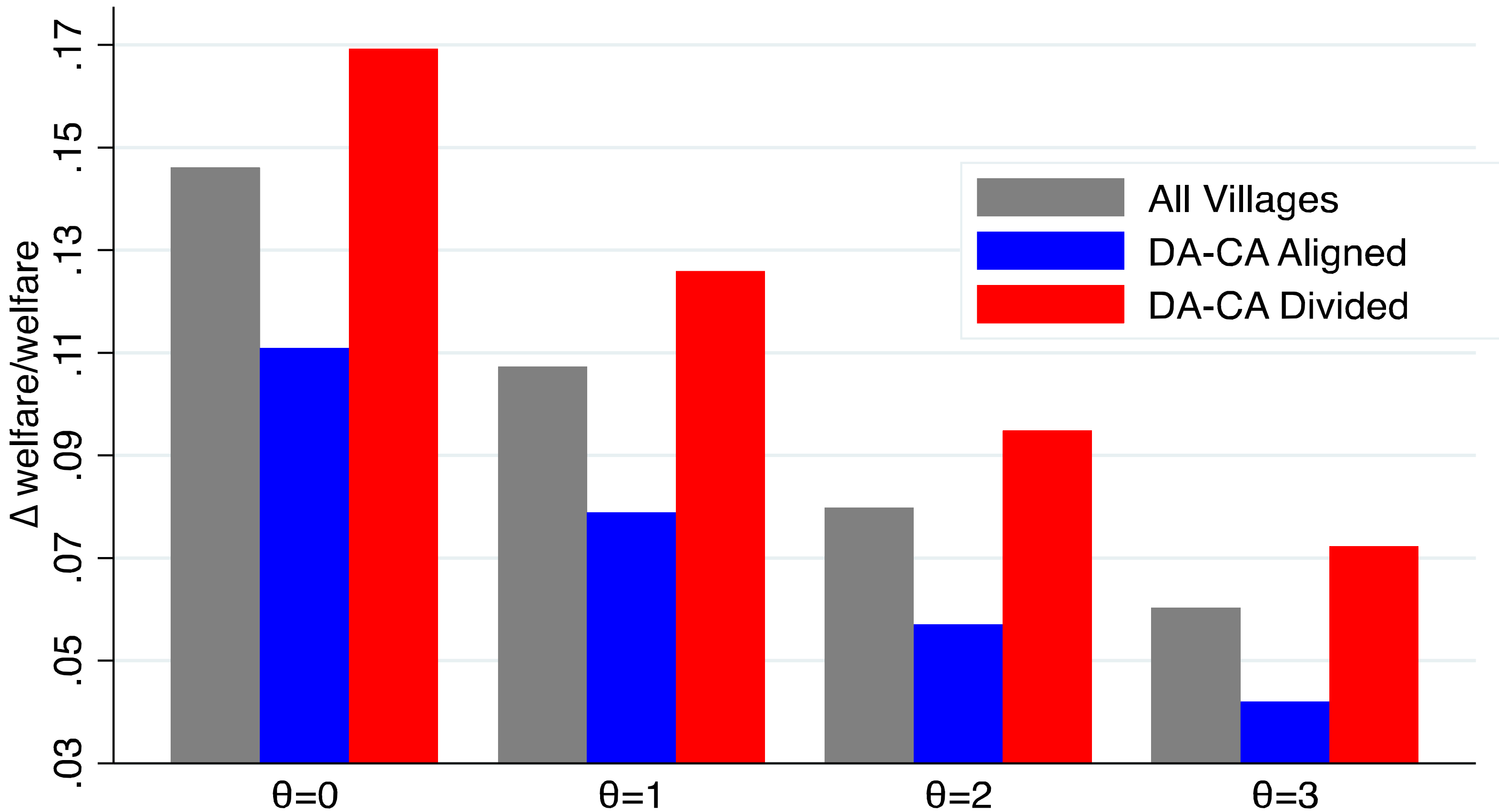


B. Within Treatment, Ties of the Delivery Agent vs. Ties of the Counterfactual Agent by DA-CA Horizontal Tie



Notes: Panel A presents quantile treatment effects of the treatment on profits, controlling for branch fixed effects and clustering standard errors at the village level. The sample includes farmers in the control and treated villages with positive profits. Panel B presents quantile treatment effects for farmers who know the delivery agent only vs. the counterfactual agent only in villages in which the agents are aligned (have the same political identity; blue line) and divided (have different political identities; red line), controlling for branch fixed effects, an indicator for whether the farmer is tied to both agents, an indicator for whether the farmer is tied to no agent, the walking distance to the delivery agent's home, and the walking distance to the counterfactual agent's home. Standard errors clustered at the community and tie level. The sample includes farmers in the treated villages with positive profits. Vertical dashed lines are 95% confidence intervals. Profits equal the total output value minus total expenditures value in the last season, expressed in thousand UGX and truncated above and below two standard deviations from the mean.

Figure 7: Welfare



Notes: Δwelfare (Δw) is the the weighted sum of the difference between endline and baseline profit for each group of farmers. Welfare (w) is the weighted sum of the baseline profits for each group of farmers. Δwelfare/welfare represents the proportionate change in welfare from baseline to endline. θ is a measure of the degree of inequality aversion. If θ=0, the intervention has same impact on welfare as on profits. If θ=1, welfare is logarithmic, so impacts are proportional to those on profits. In gray bars, farmers are divided in 8 groups depending on whether they are tied to the delivery agent or the counterfactual agent, whether they are poor or not, and whether they reside in a village where the agents belong to the same vs. different parties. In blue bars (resp., red bars), we restrict the analysis to villages in which the agents have the same political identity (resp., different political identities) and divide farmers in 4 groups depending on whether they are tied to the delivery agent or the counterfactual agent, and whether they are poor or not. Profits are equal to total output value minus total expenditures value in the last season (thousand UGX), truncated above and below two standard deviations from the mean. DA-CA aligned (resp., divided) equals 1 if the farmer resides in a community in which the agents report having the same (resp., different) political identity. Tied to delivery (resp., conterfactual) agent equals 1 if the farmer knows only the delivery (resp., conterfactual) agent. Poor (resp., not poor) equals 1 if the household belongs (resp., does not belong) to the bottom quartile of the within-community food consumption distribution.

Table A1: Attrition

OLS estimates and standard errors parentheses (clustered by community in Columns 1-3, and by community and ties in Columns 4-6)

Dependent variable =1 if respondent attrited at endline

	Agricultural Extension Program			Social Ties		
	(1) No Covariates	(2) Covariates	(3) Covariates plus their interaction with treatment	(4) No Covariates	(5) Covariates	(6) Covariates plus their interaction with treatment
Treated	.015	.017	.050			
	(.011)	(.011)	(.076)			
Treated x Tied to Delivery Agent				.019	.032	-.026
				(.023)	(.021)	(.172)
Treated x Tied to Counterfactual Agent				.015	.026	.080
				(.015)	(.016)	(.127)
Mean dependent variable	.070	.070	.070	.070	.070	.070
p-value on interactions	-	-	[.309]	-	-	-
p-value on interactions for Tied to Delivery Agent vs. Counterfactual Agent						[.600]
Observations	4,741	3,555	3,555	4,303	3,216	3,216

Notes: Household (farmer)-level OLS regressions. In Columns 1-3, we use the sample of households in the control and treated villages and cluster standard errors at the village level. In Columns 4-6, we use the sample of households in treated villages only and cluster standard errors at the community and ties level. All regressions control for branch fixed effects. Additionally, Columns 2 and 5 control for all household-level characteristics in Table 2; Column 3 controls for all household-level characteristics in Table 2 interacted with the treatment; Column 6 controls for all household-level characteristics in Table 2 interacted with tied to the delivery agent and tied to the counterfactual agent. Tied to delivery (counterfactual) agent equals 1 if the farmer knows only the delivery (counterfactual) agent. At the foot of Column 3 we report the p-value from a joint test of significance of all interactions. At the foot of Column 6 we report the p-value from a joint test of significance of all interactions with tied to the delivery agent vs. with tied to the counterfactual agent. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A2: Interaction with the Microfinance Program

ITT estimates and standard errors in parentheses (clustered by community)
p-values adjusted for randomization inference and multiple hypothesis testing in braces

	Targeting			Other Sources		Agriculture in Last Season		Consumption and Assets	
	(1) Targeted by the delivery agent: Received seeds or training in last year	(2) Received seeds from the delivery agent in last year	(3) Trained by the delivery agent in last year	(4) Received seeds from BRAC branch office in last year	(5) Received seeds from non-BRAC source in last year	(6) Profits in last season (000 UGX)	(7) Number of marketable crops grown in last season	(8) Food expenditure in last month (000 UGX)	(9) Productive assets (000 UGX)
(1) Agricultural Extension Intervention with Microfinance	.032***	.026***	.030***	.048***	.018	36.28**	.161	18.61	2.897
	(.008)	(.007)	(.007)	(.010)	(.018)	(16.38)	(.124)	(15.32)	(1.944)
	{.001,.006}	{.001,.006}	{.001,.006}	{.001,.006}	{.081,.349}	{.001,.084}	{.001,.182}	{.009.299}	{.109,.299}
(2) Agricultural Extension Intervention without Microfinance	.047***	.037***	.044***	.039***	.020	31.00*	.296**	37.08**	3.239*
	(.012)	(.011)	(.011)	(.008)	(.016)	(16.72)	(.137)	(18.51)	(1.847)
	{.001,.010}	{.001,.018}	{.001,.016}	{.001,004}	{.043,.188}	{.001,.080}	{.001,.064}	{.001,.110}	{.083,.110}
Mean in control	.001	.001	.000	.001	.094	76.96	1.243	107.0	20.12
p-value (1)=(2)	[.252]	[.373]	[.279]	[.461]	[.855]	[.762]	[.377]	[.380]	[.886]
Observations	4,378	4,390	4,381	4,390	4,410	3,968	4,410	4,395	4,339

Notes: Household (farmer)-level OLS regressions. All regressions control for branch fixed effects and for the baseline value of the outcome variable. In parentheses, we report standard errors clustered at the village level. In brackets, we report randomization inference p-values computed following Young [2019] approach, and p-values adjusted for multiple hypothesis testing computed using Romano and Wolf [2016] step-down procedure. Profits (000 UGX) are the total output value minus total expenditures value in the last season. Number of marketable crops grown counts the number of vegetables, roots and fruits crops produced in the last season. Food expenditure in last month (000 UGX) is the total household expenditure on food, beverage and tobacco per month per adult equivalent. Productive assets (000 UGX) is the total value of agriculture assets owned by the household. All monetary values are expressed in thousand UGX and are truncated above and below two standard deviations from the mean. Exchange rate: 1 USD = 2519.6 UGX (March 2014). ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A3: p-value Corrections for Randomization Inference and Multiple Hypothesis Testing

OLS estimates and standard errors in parentheses (clustered by community and ties)

p-values adjusted for randomization inference and multiple hypothesis testing in braces

	Targeting			Profits from agriculture in last season (thousand UGX)		
	(1) Main Measure of SocialTie	(2) Religion	(3) Ethnicity	(4) Baseline	(5) Agricultural Controls	(6) Family Ties Only
	-.044	-.027	-	211.7**	205.9**	11.79
Tied to Delivery Agent x DA-CA Aligned x Poor	(.057)	(.043)	-	(88.24)	(93.04)	(38.24)
	{.306, .451}	{.345,.689}	-	{.000, .052}	{.000, .044}	{.918, .701}
	.012	-.003	-	63.91	53.59	-118.4
Tied to Delivery Agent x DA-CA Aligned x Not Poor	(.021)	(.035)	-	(54.70)	(54.66)	(167.6)
	{.594, .541}	{.850, .990}	-	{.017, .403}	{.037, .533}	{.345, .435}
	.075*	-.067*	-.017	-23.23	-40.09	-209.9
Tied to Delivery Agent x DA-CA Divided x Poor	(.042)	(.034)	(.049)	(72.749)	(80.225)	(176.6)
	{.032, .124}	{.005,.116}	{,422, .647}	{.508, .699}	{.318 .529}	{..018, .37}
	.158***	-.031	-.008	-53.57	-55.45	-108.4
Tied to Delivery Agent x DA-CA Divided x Not Poor	(.032)	(.034)	(.058)	(45.26)	(47.50)	(73.56)
	{.001, .002}	{.032, .469}	{.562, .856}	{.015, .218}	{.013, .275}	{.031, .222}
Community Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Community Controls x DA-CA Aligned	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,194	2,194	2,187	2,122	1,917	2,122

Notes: Farmer-level OLS regressions. In brackets, we report randomization inference p-values computed following Young [2019] approach, and p-values adjusted for multiple hypothesis testing computed using Romano and Wolf [2016] step-down procedure. All regressions control for community fixed effects, an indicator for whether the farmer is tied to both agents, an indicator for whether the farmer is tied to no agent, the walking distance to the delivery agent's home, the walking distance to the counterfactual agent's home, and for all community characteristics presented in Table 5 interacted with vertical ties. In parentheses, we report standard errors clustered at the community and ties level. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A4: Alternative Definitions of Social Ties

Dependent Variable: Delivery agent targets farmer (received seeds or training in last year)

OLS estimates and standard errors in parentheses (clustered by community and ties)

Measure of Tie:	Friend or family			Discusses agriculture		
	(1) Social Ties	(2) Social Incentives	(3) Pro-poor Targeting	(4) Social Ties	(5) Social Incentives	(6) Pro-poor Targeting
Tied to Delivery Agent	.059**			.039*		
	(.027)			(.020)		
Tied to Delivery Agent x DA-CA Aligned		-.056**			-.043	
		(.024)			(.028)	
Tied to Delivery Agent x DA-CA Divided		-.007			.042**	
		(.039)			(.017)	
Tied to Delivery Agent x DA-CA Aligned x Poor			-.084			-.067
			(.120)			(.042)
Tied to Delivery Agent x DA-CA Aligned x Not Poor			-.110			.036
			(.068)			(.045)
Tied to Delivery Agent x DA-CA Divided x Poor			-.050*			-.035
			(.027)			(.035)
Tied to Delivery Agent x DA-CA Divided x Not Poor			.023			.045**
			(.044)			(.018)
Community Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Community Controls x DA-CA Aligned	No	Yes	Yes	No	Yes	Yes
Mean Outcome, Tied to CA	.019	.019	.019	.019	.019	.019
p-values:						
Anti-poverty targeting:						
∆prob(targeted)/∆ Poor | DA-CA Aligned			.797			.549
∆prob(targeted)/∆ Poor | DA-CA Divided			.044			.861
Anti-poverty targeting and DA-CA horizontal tie:						
∆prob(targeted)/∆ Horizontal Tie | Poor			.859			.100
∆prob(targeted)/∆ Horizontal Tie | Not Poor			.147			.038
Observations	2,421	2,195	2,195	2,087	1,888	1,888

Notes: Farmer-level OLS regressions. All regressions control for community fixed effects, an indicator for whether the farmer is tied to both agents, an indicator for whether the farmer is tied to no agent, the walking distance to the delivery agent's home, and the walking distance to the counterfactual agent's home. Columns 2-3 and 5-6 also control for all community characteristics presented in Table 5 interacted with the DA-CA alignment. In parentheses, we report standard errors clustered at the community and vertical ties level. Tied to delivery agent equals 1 if the farmer is a friend or family of the delivery agent only in Columns 1-3, equals 1 if the household regularly discusses agriculture with the delivery agent only in Columns 4-6. DA-CA aligned (resp., divided) equals 1 if the farmer resides in a community in which the agents report having the same (resp., different) political identity. Information on whether agents have the same political identity or not is missing in 7 out of 60 communities and this explains the smaller sample size. Poor (resp., not poor) equals 1 if the household belongs (resp., does not belong) to the bottom quartile of the within-community distribution of food expenditure. At the foot of Columns 4 to 7 we report (in order of appearance) p-values for: (i) Tied to Delivery Agent x DA-CA Aligned x Poor vs. Not Poor, (ii) Tied to Delivery Agent x DA-CA Divided x Poor vs. Not Poor, (iii) Tied to Delivery Agent x Poor x DA-CA Aligned vs. Divided, (iv) Tied to Delivery Agent x Not Poor x DA-CA Aligned vs. Divided. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A5: Define DA-CA Ties Using the Implicit Association Test

Dependent Variable: Delivery agent targets farmer (received seeds or training in last year)

OLS estimates and standard errors in parentheses (clustered by community and ties)

	(1) Social Incentives	(2) Pro-poor Targeting
Tied to Delivery Agent x DA-CA Aligned	.034	
	(.027)	
Tied to Delivery Agent x DA-CA Divided	.065*	
	(.037)	
Tied to Delivery Agent x DA-CA Aligned x Poor		-.026
		(.057)
Tied to Delivery Agent x DA-CA Aligned x Not Poor		.045
		(.030)
Tied to Delivery Agent x DA-CA Divided x Poor		.011
		(.058)
Tied to Delivery Agent x DA-CA Divided x Not Poor		.072*
		(.038)
Community Fixed Effects	Yes	Yes
Community Controls x DA-CA Aligned	Yes	Yes
Mean Outcome, Tied to CA	.019	.019
Anti-poverty targeting:		
Δprob(targeted)/Δ Poor | DA-CA Aligned		[.242]
Δprob(targeted)/Δ Poor | DA-CA Divided		[.267]
Anti-poverty targeting and DA-CA horizontal tie:		
Δprob(targeted)/Δ Horizontal Tie | Poor		[.631]
Δprob(targeted)/Δ Horizontal Tie | Not Poor		[.623]
Observations	2,051	2,051

Notes: Farmer-level OLS regressions. All regressions control for community fixed effects, an indicator for whether the farmer is tied to both agents, an indicator for whether the farmer is tied to no agent, the walking distance to the delivery agent's home, and the walking distance to the counterfactual agent's home. They also control for all community characteristics presented in Table 5 interacted with the DA-CA alignment. In parentheses, we report standard errors clustered at the community and ties level. Tied to delivery agent equals 1 if the farmer knows only the delivery agent. The omitted group (tied to counterfactual agent) is composed of farmers who know only the counterfactual agent. DA-CA aligned (resp., divided) equals 1 if the farmer resides in a community in which the agents have to the same (resp., different) political identity as measured by an IAT test. Poor (resp., not poor) equals 1 if the household belongs (resp., does not belong) to the bottom quartile of the within-community distribution of food expenditure. At the foot of Column 2 we report (in order of appearance) p-values for: (i) Tied to Delivery Agent x DA-CA Aligned x Poor vs. Not Poor, (ii) Tied to Delivery Agent x DA-CA Divided x Poor vs. Not Poor, (iii) Tied to Delivery Agent x Poor x DA-CA Aligned vs. Divided, (iv) Tied to Delivery Agent x Not Poor x DA-CA Aligned vs. Divided. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A6: Dimensions of Targeting

Dependent Variable: Delivery agent targets farmer (received seeds or training in last year)
OLS estimates and standard errors in parentheses (clustered by community and ties)

Definition of Need (in bottom 25% of baseline):	(1) Wealth score	(2) Number of techniques ever adopted	(3) Profits
Tied to Delivery Agent x DA-CA Aligned x in Q4 of X	.011	.077**	.011
	(.043)	(.035)	(.039)
Tied to Delivery Agent x DA-CA Aligned x Not in Q4 of X	.088**	.009	.109*
	(.041)	(.102)	(.059)
Tied to Delivery Agent x DA-CA Divided x in Q4 of X	-.005	-.004	-.005
	(.020)	(.018)	(.023)
Tied to Delivery Agent x DA-CA Divided x Not in Q4 of X	.154***	.148***	.152***
	(.031)	(.031)	(.036)
Community Fixed Effects	Yes	Yes	Yes
Community Controls x DA-CA Aligned	Yes	Yes	Yes
Mean Outcome, Tied to CA	.019	.019	.019
p-values:			
∆prob(targeted)/∆ Q4 of X | DA-CA Aligned	[.713]	[.013]	[.744]
∆prob(targeted)/∆ Q4 of X | DA-CA Divided	[.062]	[.173]	[.545]
∆prob(targeted)/∆ Horizontal Tie | Poor	[.161]	[.534]	[.174]
∆prob(targeted)/∆ Horizontal Tie | Not Poor	[.000]	[.000]	[.000]
Observations	2,195	2,195	2,195

Notes: Farmer-level OLS regressions. All regressions control for community fixed effects, an indicator for whether the farmer is tied to both agents, an indicator for whether the farmer is tied to no agent, the walking distance to the delivery agent's home, and the walking distance to the counterfactual agent's home. All regressions control for all community characteristics presented in Table 5 interacted with ties. In parentheses, we report standard errors clustered at the community and ties level. Tied to delivery agent equals 1 if the farmer knows only the delivery agent. The omitted group (tied to counterfactual agent) is composed of farmers who know only the counterfactual agent. DA-CA aligned (resp., divided) equals 1 if the farmer resides in a community in which the agents report having the same (resp., different) political identity. Information on whether agents have the same political identity or not is missing in 7 out of 60 communities and this explains the smaller sample size. In Q4 of X (resp., not in Q4 of X) equals 1 if the household belongs (resp., does not belong) to the bottom quartile of the baseline within-community distribution of the wealth score (Column 1), number of techniques ever adopted (Column 2), profits (Column 3). The household wealth score is measured by aggregating ten poverty indicators into a score going from 0 to 100. At the foot of each Column we report (in order of appearance) p-values for: (i) Tied to Delivery Agent x DA-CA Aligned x in vs. not in Q4 of X, (ii) Tied to Delivery Agent x DA-CA Divided x in vs. not in Q4 of X (iii) Tied to Delivery Agent x in Q4 of X x DA-CA Aligned vs. Divided, (iv) Tied to Delivery Agent x Not in Q4 of X x DA-CA Aligned vs. Divided. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A7: Diffusion and Informal Transfers

OLS estimates and standard errors in parentheses (clustered by community and ties)

	(1) Diffusion: Received seeds from non-BRAC source in last year	(2) Received seeds from any source in last year	(3) Net transfers (extensive margin) in last year	(4) Net transfers (intensive margin) in last year (000 UGX)
Tied to Delivery Agent x DA-CA Aligned x Poor	-.019	.009	-.007	32.54*
	(.061)	(.085)	(.138)	(19.45)
Tied to Delivery Agent x DA-CA Aligned x Not Poor	.026	-.034	-.046	3.920
	(.032)	(.049)	(.064)	(8.906)
Tied to Delivery Agent x DA-CA Divided x Poor	.081	.116	.062	-5.560
	(.051)	(.089)	(.120)	(18.59)
Tied to Delivery Agent x DA-CA Divided x Not Poor	.114**	.293***	-.002	6.445
	(.044)	(.064)	(.105)	(16.96)
Community Fixed Effects	Yes	Yes	Yes	Yes
Community Controls x DA-CA Aligned	Yes	Yes	Yes	Yes
Mean Outcome, Tied to CA			.488	48.66
Mean Outcome, Poor and Tied to CA	.035	.106		
Mean Outcome, Not Poor and Tied to CA	.056	.140		
Anti-poverty targeting:				
∆prob(targeted)/∆ Poor | DA-CA Aligned	[.476]	[.623]		
∆prob(targeted)/∆ Poor | DA-CA Divided	[.469]	[.032]		
Anti-poverty targeting and DA-CA horizontal tie:				
∆prob(targeted)/∆ Horizontal Tie | Poor	[.208]	[.353]		
∆prob(targeted)/∆ Horizontal Tie | Not Poor	[.084]	[.000]		
Observations	2,220	2,220	2,220	2,141

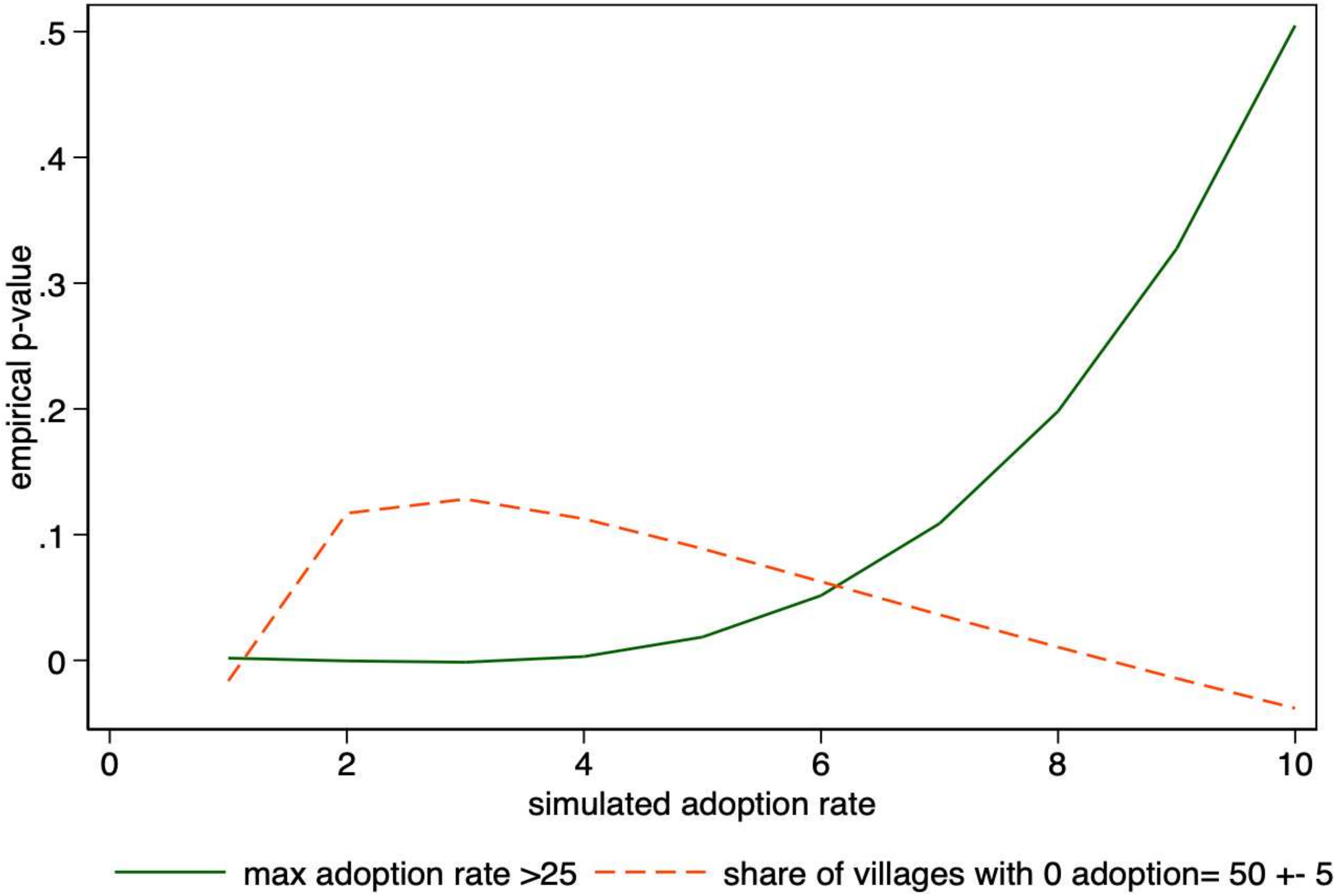
Notes: Farmer-level OLS regressions. All regressions control for community fixed effects, an indicator for whether the farmer is tied to both agents, an indicator for whether the farmer is tied to no agent, the walking distance to the delivery agent's home, and the walking distance to the counterfactual agent's home. All regressions also control for all community characteristics presented in Table 5 interacted with ties. In parentheses, we report standard errors clustered at the community and ties level. The dependent variable in Column 1 equals one if the household received seeds from non-BRAC source (market, friend, etc.) in the last year. The dependent variable in Column 2 equals one if the household received seeds from any source (BRAC or non-BRAC) in the last year. Net transfers extensive margin is if a household received a transfer minus if household sent a transfer (it ranges from -1 to 1). Net transter intensive margin (000 UGX) is the total transfer received minus total transfers sent (gifts, alimony, scholarship, etc.) in the last year. Tied to delivery agent equals 1 if the farmer knows only the delivery agent. The omitted group (tied to counterfactual agent) is composed of farmers who know only the counterfactual agent. DA-CA aligned (resp., divided) equals 1 if the farmer resides in a community in which the agents report having the same (resp., different) political identity. Information on whether agents have the same political identity or not is missing in 7 out of 60 communities and this explains the smaller sample size. Poor (resp., not poor) equals 1 if the household belongs (resp., does not belong) to the bottom quartile of the within-community distribution of food expenditure. At the foot of Columns 1 and 2 we report (in order of appearance) p-values for: (i) Tied to Delivery Agent x DA-CA Aligned x Poor vs. Not Poor, (ii) Tied to Delivery Agent x DA-CA Divided x Poor vs. Not Poor, (iii) Tied to Delivery Agent x Poor x DA-CA Aligned vs. Divided, (iv) Tied to Delivery Agent x Not Poor x DA-CA Aligned vs. Divided. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A8: Welfare Calculations

Farmer socially tied to the:	DA	DA	DA	DA	CA	CA	CA	CA
DA-CA tie:	Aligned		Divided		Aligned		Divided	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Aggregate Welfare								
Population share (α_g)	.029	.132	.062	.175	.047	.181	.082	.291
Baseline profits (yg0)	1.665	1.229	1.233	1.247	1.109	1.286	1.000	1.310
Endline profits (yg1)	2.000	1.518	1.229	1.377	1.222	1.284	1.315	1.574
∆ welfare per household								
θ=0	.347	.293	.001	.135	.115	.003	.315	.270
θ=1	.191	.215	.000	.103	.099	.002	.273	.188
θ=2	.105	.158	.000	.079	.085	.002	.239	.131
θ=3	.058	.116	.000	.060	.073	.001	.211	.092
B. Counterfactual 1: DA-CA Aligned								
Population share (α_g)	.075	.339			.121	.464		
Baseline profits (yg0)	1.665	1.229			1.109	1.286		
Endline profits (yg1)	2.000	1.518			1.222	1.284		
∆ welfare per household								
θ=0	.347	.293			.115	.003		
θ=1	.191	.215			.099	.002		
θ=2	.105	.158			.085	.002		
θ=3	.058	.116			.073	.001		
C. Counterfactual 2: DA-CA Divided								
Population share (α_g)			.102	.287			.121	.477
Baseline profits (yg0)			1.233	1.247			1.000	1.310
Endline profits (yg1)			1.229	1.377			1.315	1.574
∆ welfare per household								
θ=0			.001	.135			.315	.270
θ=1			.000	.103			.273	.188
θ=2			.000	.079			.239	.131
θ=3			.000	.060			.211	.092

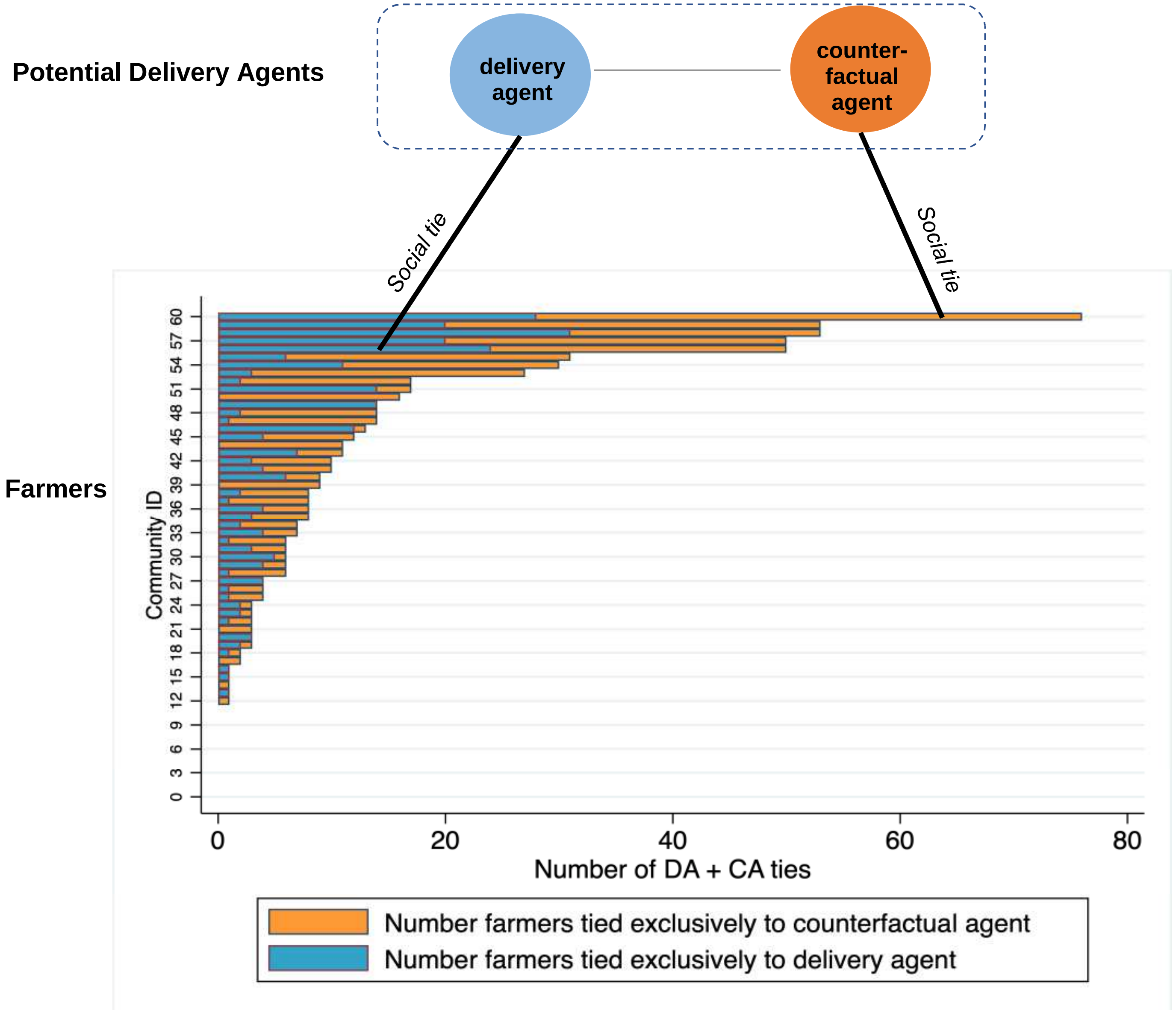
Notes: ∆welfare per household is the difference between endline and baseline CRRA utility based on profits. The population share of group g equals the share of farmers in each group (the sum across all groups equals to 1). Baseline profits are the average total output minus expenditures value for each group at baseline (thousand UGX). In Columns 1 to 4, endline profits are the average total output minus expenditures value at endline for the CA ties plus the corresponding coefficient from Table 9. In Columns 5 to 8, endline profits are the average total output minus expenditures value at endline (thousand UGX). We added a positive constant (c=.940) to the profits and multiply them by a positive constant (k=.004) to normalize the variable between 1 and 2. θ is a measure of the degree of inequality aversion. If θ=0, the intervention has same impact on welfare as on profits. If θ=1, welfare is logarithmic, so impacts are proportional to those on profits. DA-CA aligned (resp., divided) equals 1 if the farmer resides in a community in which the agents self-report having the same (resp., different) political identity. Tied to delivery (resp. conterfactual) agent equals 1 if the farmer knows only the delivery (resp. conterfactual) agent. Poor (resp. not poor) equals 1 if the household belongs (resp., does not belong) to the bottom quartile of the within-community distribution of food expenditure.

Figure A1: Simulated Adoption Rates Across Villages



Notes: For each "true" adoption rate from 1 to 10% we take 1000 sample draws following the same stratification as in the actual experiment. We then plot the empirical p-value, that is the share of draws, that yield a share of villages with zero adopters equal to 50% plus or minus 2.5% (red dashed line) and the share of draws that yield a maximum adoption rate larger than 25%. For all levels of adoption rates the probability of drawing the observed combination is close to zero.

Figure A2: Variation in Number of Social Ties



Notes: The blue (orange) histogram is the number of farmers in the community who know only the counterfactual (delivery) agent. Communities are sorted from the lowest to the highest number of farmers who know one of the two agents.