Social Incentives, Delivery Agents, and the Effectiveness of Development Interventions

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There has been a rise in the use of the local delivery model for development interventions, where local agents are hired as intermediaries to target benefits to potential beneficiaries. We study this model in the context of a standard agricultural extension intervention in Uganda. We document a trade-off between coverage and targeting: delivery agents treat more farmers when they have a greater number of social ties, but they are significantly more likely to target their nonpoor ties. We conclude by discussing the implications of our findings for the design of the local delivery model for antipoverty interventions.

I. Introduction

A silent revolution taking place in development policy since the 1990s is the shift from the centralized provision of antipoverty interventions from the state, toward NGOs (nongovernmental organizations; Werker and Ahmed 2008; Aldashev and Navarra 2018). Given 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 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 observe the behavior of delivery agents or household outcomes. Moreover, delivery agents are typically not employees of the NGO, and because NGOs are resource constrained, the level of monetary reward is also typically low. A consequence is that because local delivery agents are embedded in the social fabric of communities from which they are recruited, their behavior will then be shaped to a greater extent by the social incentives they face when serving their community.

Delivery agents being subject to social incentives means their behavior is determined by the presence and identity of others in their community. NGOs often hold the belief that such social incentives can be harnessed for the greater good and in line with the antipoverty aims of interventions. However, in reality social incentives cover the plethora of nonmonetary motivations linked to others, including positive and negative concerns such as altruism and warm glow, identity, fairness, in-group/out-group biases, spite, social status, and implicit cooperative agreements that can be enforced through transfers, side payments, or kickbacks (Ashraf and Bandiera 2018).

Whether the incentives of local delivery agents are aligned with the implementing antipoverty organizations that engage them in terms of

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whether they target the poor is unknown. We present evidence using a two-stage experiment designed to shed light on how social incentives determine the behavior of local delivery agents.

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). The standard trade-off is (i) local agents have private information that can be leveraged to target interventions toward the needy; (ii) local agents might engage in nepotism, favoritism, or be subject to elite capture (Dreze and Sen 1989; Bardhan and Mookherjee 2020). We bring a third dimension to this debate: social incentives can create a wedge between the original propoor intent of NGO programs and the actual behavior of local delivery agents.

The local delivery model is utilized in programs related to agriculture, health, early childhood development, credit, and insurance. We draw general lessons for the model from the specific context of the pilot phase of a standard agricultural extension intervention in rural Uganda. The intervention is implemented by the NGO BRAC in southwestern 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 high yielding variety (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 farmers in their communities. The intervention is intended as an antipoverty program to be targeted to the poorest farmers. 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.

We use a two-stage experimental design to study how social incentives shape the behavior of delivery agents. The first stage follows a standard randomization of the agricultural extension intervention across communities. We use this to evaluate 2-year impacts of the intervention and establish its effectiveness during the pilot phase expansion of the intervention that our study period covers. In this pilot phase BRAC sets an informal goal for DAs to target 10%–15% of farmers in their village. In line with this, farmers in treated villages are 9 percentage points more likely to receive improved seeds through any source relative to those in control villages. The likelihood a farmer is targeted by the DA with both improved seeds and training in techniques is 3.9 percentage points higher for those in treated villages than controls (with zero farmers being targeted in controls).

The availability of the extension intervention—seeds and training—significantly increases farmer's profits from the last cropping season by

USD 13, corresponding to a 43.7% rise (albeit starting from a low base). These 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.

Overall, this stage of randomization shows the intervention to be effective. However, this masks how social incentives shape the behavior of DAs and thus how the intervention unfolds within communities. The second-stage randomization is designed to examine this issue.

This second layer of the experiment takes place within treated communities. In each community, BRAC short-lists two potential candidates to serve as the delivery agent (out of typically a very low number of suitably eligible individuals). We then rapidly survey farmers to establish their social ties to each short-listed candidate. Finally, we randomly select one of the two candidates to be the actual delivery agent (DA). The other serves as a counterfactual 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 intervention locally. CAs play no role in its delivery.

This design partitions potential beneficiary farmers into those (i) exclusively tied to the DA (and not to the CA); (ii) exclusively tied to the CA; (iii) tied to both; (iv) tied to neither. There are three key features of this second-stage-randomization design.

First, it eliminates endogenous tie formation between candidate delivery agents and potential beneficiaries. This is similar to designs that exogenously engineer new social ties (Feigenberg, Field, and Pande 2013; Brooks, Donovan, and Johnson 2018; Cai and Szeidl 2018; Vasilaky and Leonard 2018), except that our approach utilizes naturally formed and preexisting ties in the field. Outside of settings involving antipoverty interventions, a strand of literature has identified the impacts of social ties/patronage. Prominent examples include Bandiera, Barankay, and Rasul (2009), Hjort (2014), and Xu (2018). These papers leverage within-person variation in the presence of ties over time. Our research design differs from these in that it uses experimental variation to create exogenous variation *across* individuals in whether they are socially tied to actual delivery agents or not. It is closer to the approach of Beaman et al. (2021) who similarly randomize within a pool of candidate delivery agents to examine effects of being connected to a delivery agent while holding network position constant. Finally, our approach is in contrast to the well established literature on clientelism, which 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 kick-started by the intervention, but our analysis is based on preexisting ties.

Second, it allows us to causally estimate how the number of social ties affects coverage—the total number of farmers targeted by the DA in their community. To identify how social ties determine coverage 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.

Third, it ensures groups of farmers exclusively tied to the DA and CA are similar on observables. This enables us to build on work identifying distortions caused by social ties between delivery agents and potential beneficiaries (Banerjee et al. 2018; Alatas et al. 2019; BenYishay and Mobarak 2019; Beaman et al. 2021; Maitra et al. 2021). Specifically, we use experimental variation to identify whether farmers with a specific characteristic—say, being poor—are differentially likely to be targeted if they are tied to the DA relative to observationally equivalent farmers that are tied to the CA. In being able to make an experimental comparison between farmers all of whom share a given characteristic but who exogenously vary in their ties to the DA and CA, we can (i) shed light on the extent to which DAs engage in propoor targeting, and (ii) rule out that such behaviors are driven by demand-side factors related to a specific farmer characteristic unrelated to their social ties (such as their ability to pay for seeds or likelihood of adoption).

Finally, we identify the impact of social ties to potential beneficiaries on DA behavior exploiting variation across farmers within the same community, thus controlling for community fixed effects and holding constant all other aspects of social structure (such as features of the aggregate social network of farmers).

Our main results are as follows: On coverage—the total number of farmers targeted by the DA in their community—we find the DA treats more farmers if she has more social ties in the community. Zooming in on which farmers are targeted, we find those exclusively tied to the DA are 6.2 percentage points more likely to be targeted relative to those exclusively tied to the CA. Among those exclusively tied to the CA, 1.9% are targeted. The DA thus does not entirely ignore the exclusive ties of the CA, but there is a threefold increase in the likelihood of her own social ties being targeted relative to them.

This targeting of social ties 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 where the evidence typically supports using agents more central within social networks (and so with more social ties) as injection points into communities for intervention delivery (Banerjee et al. 2013; Beaman and Dillon 2018; Beaman et al. 2021).

Second, we examine the extent of propoor targeting by DAs. We find poor social ties of the DA are as likely to be targeted than poor ties of the CA: the baseline probability of the latter being targeted is 3.6% and this hardly changes among the poor ties of the DA. However, nonpoor social ties of the DA are significantly more likely to be targeted than nonpoor ties of the CA. The difference in targeting probabilities is 7.7 percentage points: the baseline probability that nonpoor ties of the CA are targeted is 1.4%. Hence, nonpoor ties of the DA are more than 5 times more likely to be targeted than farmers with similar observables but who are exclusively tied to the CA.

The differential likelihood the DA targets her poor (nonpoor) social ties more than those of the CA is experimentally identified using the second stage of our design. The difference between them thus causally pins down whether the DA engages in propoor targeting. In line with an absence of propoor targeting, we find DAs are significantly more likely to target their nonpoor ties than their poor ties, relative to comparable ties of the CA ($p = .043$).

Our two-stage design thus reveals a basic tension at the heart of the local delivery model commonly used by NGOs across contexts and types of poverty alleviation intervention in agriculture, health, credit, and so forth. While the local delivery model implicitly assumes that NGOs can harness social incentives for the greater good, we identify a basic coverage-targeting trade-off at the heart of the model. On the one hand, DAs are induced to exert greater effort to treat more farmers when they have more social ties in the community they are recruited from and serve. However, when exerting more effort, DAs are also more likely to target nonpoor farmers they are tied to. This goes against the antipoverty intentions of the intervention.

Our final stage of analysis explores *why* DAs target nonpoor social ties. Our research design rules out simple explanations for this, such as complementarities in adopting new seeds between DAs and their social ties, or explanations based on demand-side factors that are common to all nonpoor farmers irrespective of whether they are tied to the DA and/or the CA—for example, that the nonpoor are more willing to pay the (below market) price for the modern seeds, or that positively selected DAs have better information on the practices of nonpoor farmers. Rather, we seek explanations for why there exists an interaction between social ties and the poverty status of farmers, causing DAs to target their exclusive nonpoor social ties with modern seeds and techniques.

We consider two main explanations. We first use the data to rule out the possibility that nonpoor ties are targeted because this maximizes total surplus, which would be the case if (i) the returns to the intervention were higher for the nonpoor than the poor, and (ii) communities engage in ex post redistribution to the poor.

An alternative possibility is that there is redistribution of some of the surplus generated back to delivery agents. This hypothesis builds on the idea that development brokers in local interventions engage in rent-seeking behavior (Platteau and Gaspart 2003; Voors et al. 2018; Maitra et al. 2021). More precisely, assume delivery agents can more easily form an implicit agreement among their own social ties (rather than ties of the CA) of the following kind: if they target them, they provide the DA some rent—or kickback—from the gains generated. The possibility to form and enforce such implicit agreements is only possible among social ties, much as in the literature on implicit risk-sharing agreements within social networks. Finally, the nonpoor might be more willing and able to provide such kickbacks given the higher levels of economic well-being to begin with. This possibility to extract rents is reinforced by the fact that the DA holds unique power in communities: there is no alternative individual that can play this intermediary role with the NGO.

We test this using 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 counterfactual delivery agents in control villages (Pissarides and Weber 1989). We find this is so, and entirely driven by the presence of nonpoor social ties of the DA. We use our estimates to back out the value of rent extraction by the delivery agent: this is equivalent to 7 times the average gains to the average farmer from the intervention, as identified from the standard first-stage randomization. Overall, the evidence suggests targeting by delivery agents of their social ties is not driven by altruism, nor informational advantages, but because social incentives allow DAs and their social ties to enforce an implicit cooperative agreement whereby delivery agents target benefits toward their nonpoor social ties, but then extract some rent or side payments in return.

Despite their pivotal role for delivering interventions, the behavior of delivery agents is relatively understudied. Our analysis positions within this literature as follows.

While the earlier literature has emphasized demand-side networks—how information or resources flow within potential beneficiaries, say because of social learning between farmers (Foster and Rosenzweig 1995; Conley and Udry 2010)—we instead focus on the networks and relationships of selected delivery agents. We thus start to recognize the importance of supply-side networks for development interventions. This perspective allows us to go beyond considering noncompliance with the offer of treatment as being a take-up issue driven by a lack of demand. Rather, noncompliance reflects supply-side biases in how treatment assignment by delivery agents within villages takes place.

Narrowing in on the literature on delivery agents, some of this builds on the theory of targeting interventions in networks (Ballester, Calvó-Armengol, and Zenou 2006; Banerjee et al. 2013; Galeotti, Golub, and Goyal 2020). Empirical work examining how social networks affect targeting behavior in the context of propoor interventions includes

Banerjee et al. (2013, 2018), Alatas et al. (2016, 2019), Beaman and Dillon (2018), BenYishay and Mobarak 2019, Beaman et al. (2021), and Maitra et al. (2021). A separate strand of literature has focused on identifying the optimal delivery agent, either by contrasting local versus centralized delivery of interventions (BenYishay and Mobarak 2019) or by studying 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).[1] These are not issues our data or research design address.

Rather, we study how social incentives shape the core behaviors of delivery agents in terms of coverage, targeting, and propoor targeting. Our novel identification strategy allows us to identify a key coverage-targeting trade-off for the local delivery model, and shed light on the fundamental social incentives motivating local delivery agents. 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 downsides from the perspective of the NGO or principal, creating new trade-offs to be considered for the local delivery model. This new perspective provides an important complement to the long-standing literature on decentralization, which has emphasized the importance of elite capture or clientelism in driving intervention effectiveness (Galasso and Ravallion 2005; Bardhan and Mookherjee 2006).

The remainder of the paper is organized as follows. Section II describes the intervention, data, and first-stage randomization. Section III describes the selection of delivery agents and second-stage randomization. Section IV presents findings on the number of farmers targeted and propoor targeting, and narrows down the structure of social incentives motivating delivery agents. Section V discusses design implications for the local delivery model, external validity, and a broader research agenda. Section VI concludes. The appendix discusses further data details, results, and research ethics.

[1] 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. 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 vs. 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.

II. Intervention, Data, and Evaluation

A. *The Agricultural Extension Program*

Productivity differences in agriculture across countries can help explain their differences in income (Restuccia, Yang, and Zhu 2008; Gollin, Lagakos, and Waugh 2014). Agricultural productivity remains especially low in sub-Saharan Africa. Some persistent causes are the low adoption rates of improved seed varieties and limited use of modern agricultural techniques (Evenson and Gollin 2003; World Bank 2008).[2]

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). By focusing on the social incentives that locally hired agents are subject to, our study brings new insights to this debate. We shed light on why interventions can be successful in some communities and fail to fully live up to their promise in others. This links to wider debates on the external validity of interventions, where program implementation has been highlighted as a driver of heterogeneity of effectiveness (Allcott and Mullainathan 2012; 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 to 2015 into two districts in southwestern Uganda: Kabale and Rukungiri. The vast majority of rural households in these districts are employed in subsistence agriculture. Two fundamental 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.[3]

The intervention we study relaxes both constraints by offering farmers BRAC-certified HYV seeds for various crops, and training them in six

[2] 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. 2020). Gollin, Hansen, and Wingender (2021) show using panel data from 84 countries just how important the adoption of high yielding variety seeds are for economic development: they estimate an elasticity of GDP per capita to adoption rates for such improved seed varieties being around 1, 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 the security of tenure and the functioning of land markets are notable (Chen, 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 2021).

[3] 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.

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).[4] As an indication of the lemons problem before intervention, we note that 93% of surveyed farmers know about improved seeds at baseline and more than 70% believe they would have positive returns if adopted, yet only 33% have ever tried improved seeds because of the lack of certified supply, and their excessive cost. The training component of the intervention teaches farmers to use techniques such as crop rotation, zero tillage, intercropping, line sowing, and weeding, and to avoid the use of mixed cropping. Two of these techniques are actually widely adopted before 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%), and 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. Seeds and techniques are complementary, but either can increase crop yield on its own.

The intervention is implemented through locally recruited delivery agents (DAs). All DAs are women.[5] 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 delivered 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), corresponding to 10%–15% of all farmers. Effective extension requires adequate and timely access by farmers to

[4] For example, maize seeds are bought from BRAC at UGX 2,000/kg, and sold by agents at UGX 2,300/kg. A prestudy survey of 71 markets in our study area found the median price for noncertified seeds to be UGX 2,500/kg (UGX = Ugandan shilling).

[5] 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 1995). Second, traditional government extension services typically bypass women (Lecoutere, Spielman, and Van Campenhout 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).

advice. Hence, DAs are tasked to visit farmers daily to provide agricultural advice.

The intervention is intended as an antipoverty program, which should be targeted to the poorest farmers. During the training of DAs, BRAC emphasizes to them that poverty reduction is a core objective of the intervention, and suggests some ways to operationalize targeting the poor—say, using farm size as a proxy for poverty status of the household.

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, 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. Given these relatively low powered financial incentives, it is stressed to DAs that they are essentially being recruited as volunteers to help fellow farmers – thus the intent of the program is to find socially motivated agents. DAs 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, where they might be tasked to become more regular commercial distributors of the seeds.

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.

B. Design

This study is part of a wider project on the determinants of agricultural productivity in Uganda. The project evaluates 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 168 villages. Random assignment takes place at the village level, with 59 villages being randomly assigned as controls, and 109 villages being assigned the agricultural extension program (of which 51 also receive microfinance). We later document that there is no interaction between the provision of extensions services and microfinance for our key outcomes.[6]

[6] 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. 2022a).

Table A1 shows balance on village characteristics. 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 of average wealth and wealth inequality.[7]

1. 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 households. The endline survey takes place 2 years later. There are two 6 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.

2. Balance and Attrition

Table 1 shows balance on household characteristics. Panel A documents that women farmers have low levels of human capital and reside close to subsistence.[8]

Panel B focuses on respondents' preintervention exposure to improved seeds and modern techniques. The majority are aware of improved seeds and believe them to have positive returns, yet only a third have ever adopted them, partly because of the lemons problem in the market for improved seeds. Similarly, farmers are aware of modern techniques and believe them

[7] The household wealth score uses information on 10 indicators, providing weighted scores that range from 1 to 100. The higher the score, the lower the likelihood that the household has expenditures below a given poverty line. The indicators are household size, enrollment 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.

[8] 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, nonalcoholic beverages, alcoholic beverages, cigarettes, food in restaurants, and any other food). Nonmarketed own consumption is included in our measure of food consumption. We impute the value of crops held for home consumption using median sales price in the village. 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 1 and members below 18 are given a weight of 0.5.

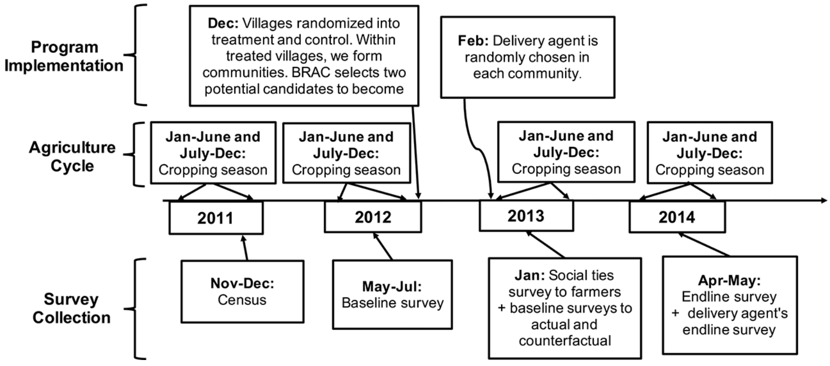

Fig. 1.—Study timeline.

to have positive returns if adopted correctly, but on average, only half of these techniques have ever been used.[9] Panel C shows household characteristics related to agriculture: they work 6 hours per day and grow multiple crops, most 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, even though this is noisy and likely to be measured with some error.[10]

Columns 1 to 3 of table A2 show 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 .324.

[9] 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 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 Di 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.

[10] The measure of profits (in thousands of 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. 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 2 standard deviations of the mean (corresponding to around 4% of observations for both variables).

TABLE 1
BALANCE ON HOUSEHOLD CHARACTERISTICS

	Control (1)	Treated: Agriculture Extension Program (2)	p-Value (1) = (2) (3)
Number of households	1,677	3,064	
	A. Socioeconomic Background		
Household head completed primary education	.431	.459	[.393]
Acres of land owned	2.030	2.169	[.704]
	(3.728)	(3.378)	
Wealth score (0–100)	59.55	60.10	[.954]
	(12.93)	(13.57)	
Food expenditure in last week (000 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 six)	4.643	4.660	[.383]
	(.954)	(.922)	
Number of techniques believed to have positive returns (out of six)	3.380	3.485	[.078]
	(1.156)	(1.086)	
Number of techniques ever used (out of six)	3.174	3.162	[.244]
	(.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 (000 UGX)	74.40	82.89	[.260]
	(313.9)	(304.1)	

NOTE.—Means, with standard deviation in parentheses. Household-level summary statistics for households in control villages (col. 1) and treatment villages (col. 2). The p-values are obtained from regressing each of the reported baseline variables 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 10 poverty indicators into a score going from 0 to 100. Food expenditure in last week is the total household expenditure on food, beverages, and tobacco per week per adult equivalent. Number of techniques ever adopted (out of six) calculates the number of techniques ever used (the six are the following: intercropping, line sowing, zero tillage, proper weeding, crop rotation, avoid mixed cropping). Number of marketable crops grown counts the number of vegetable, root, and fruit 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 are the total output value minus total expenditure value in the last season. All monetary values are expressed in thousands of UGX and are truncated above and below 2 standard deviations from the mean. Exchange rate: 1 USD = 2,519.6 UGX (March 2014).

C. *Aggregate Impacts*

1. Empirical Method

The standard first stage of randomization allows us to measure intention-to-treat (ITT) outcomes 2 years after intervention using the following ANCOVA (analysis of covariance) 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).[11] The former is especially important given that profits from agriculture are typically right skewed, and the treatment can have distributional impacts on profits.

2. Results

Table 2 shows 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 that the likelihood of being targeted by the DA is 3.9 percentage points higher in treated villages than in controls. Columns 2 and 3 show each element of 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 that farmers in treated villages are 4.3 percentage points more likely to obtain certified seeds from BRAC branches directly. Column 5 shows that farmers might be more likely to obtain seeds from non-BRAC sources; the impact is significant once we adjust for randomization inference ($p = .031$), suggesting seeds can diffuse among farmers. We do not find any evidence of an aggregate improvement in techniques used by farmers (col. 6); this might reflect the relatively high number of techniques already known about or used at baseline, as shown in table 1.

Combining all sources of seeds suggests farmers in treated villages are around 9 percentage points more likely to receive improved seeds than those in control villages.

[11] 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 (which would in any case lie beyond the territory of each delivery agent).

TABLE 2

Aggregate Impacts

	Targeting			Other Sources and Techniques			Agriculture in Last Season		Consumption and Assets		
	Targeted by the Delivery Agent: Received Seeds or Training in Last Year (1)	Received Seeds from the Delivery Agent in Last Year (2)	Trained by the Delivery Agent in Last Year (3)	Received Seeds from BRAC Branch Office in Last Year (4)	Received Seeds from Non-BRAC Source in Last Year (5)	Share of Techniques Adopted (Observed) (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)	Total Consumption in Last Month (000 UGX) (10)	Productive Assets (000 UGX) (11)
Treated village: agricultural extension intervention	.039***	.031***	.037***	.043***	.019	.004	33.66**	.228**	6.424**	27.058**	3.069**
	(.007)	(.006)	(.007)	(.007)	(.015)	(.033)	(14.06)	(.106)	(3.117)	(13.561)	(1.474)
	{.001, .006}	{.001, .006}	{.001, .006}	{.001, .006}	{.031, .375}	{.837, .916}	{.001, .052}	{.001, .052}	{.001, .066}	{.001, .066}	{.051, .066}
Mean in control	.001	.001	.000	.001	.094	.213	77.0	1.24	24.7	119	20.1
Observations	4,378	4,390	4,381	4,390	4,410	2,346	3,968	4,410	4,395	4,333	4,339

Note.—ITT estimates, with standard errors in parentheses (clustered by village). The p-values adjusted for randomization inference and multiple hypothesis testing are in braces. Table shows farmer-level OLS regressions. All regressions control for branch fixed effects and for the baseline value of the outcome variable. In braces, we report randomization inference p-values computed following the Young (2019) approach, and p-values adjusted for multiple hypothesis testing computed using the Romano and Wolf (2016) stepdown procedure, using 500 iterations. Share of techniques adopted (col. 6) calculates the share of techniques used (out of six: intercropping, line sowing, zero tillage, proper weeding, crop rotation, avoid mixed cropping) as observed by enumerators (by checking the plot of land of a random sample of households). Profits (col. 7) are the total output value minus total expenditure value in the last season. Number of marketable crops grown (col. 8) counts the number of vegetable, root, and fruit crops produced in the last season. Food expenditure in last week (col. 9) is the total household expenditure on food, beverages, and tobacco per week per adult equivalent. Total consumption (col. 10) in the last month includes food, durables, and semidurables. Productive assets (col. 11) is the total value of agriculture assets owned by the household. All monetary values are expressed in thousands of UGX and are truncated above and below 2 standard deviations from the mean. Exchange rate: 1 USD = 2,519.6 UGX (March 2014).

** Significant at the 5% level.

*** Significant at the 1% level.

The remaining columns document treatment effects on agricultural outcomes. Column 7 shows that profits rise by 43.7%, partly driven by an extensive margin increase in the number of marketable crops. Weekly food expenditures rise by 26%, total monthly consumption rises by 23%, and the value of productive assets rises by 15%. These gains to the average farmer underscore that there is likely high demand to receive seeds and training in this context.

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 (treatment effect on the treated) estimates on profits are higher than is found in field trials for HYV seeds.[12] There are three potential reasons for this. First, being targeted by the DA often implies the combined receipt of seeds and training (cols. 2 and 3). Hence our estimates are not directly comparable to field trials that only estimate the return to adopting modern seeds. 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 (col. 7), and begin engaging in agricultural markets (and so not just replacing traditional seeds with modern ones for the same crop). Third, preintervention 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 USD 13 and is more plausible.[13]

Taken together, the results imply that the intervention provides substantial economic gains to the average farmer, given their preintervention economic standing. Hence there is unlikely to be a lack of demand for seeds/training from farmers, so noncompliance 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.[14]

For the remainder of the paper we seek to understand the behavior of DAs in detail and so shed new light on how development interventions unfold within communities. To be clear, whatever biases are revealed

[12] 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%. Di Falco (2019) shows evidence from a randomized control trial in rural Tanzania that the adoption of improved maize seeds led to between 40% and 50% increases in profits.

[13] As mentioned earlier, all DAs are women, and we consider outcomes for women heads of household. Table A3 exploits the first stage of randomization to explore intrahousehold dynamics along two margins: (i) whether household members are engaged in agriculture on their own plots; (ii) what share of land owned by the household is cultivated by women. We see the intervention significantly increases the second outcome, consistent with women in treated households having a greater role in agricultural production of the household.

[14] Table A4 shows that 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.

due to the social incentives of DAs, it remains the case that the average impact of the intervention between treatment and control communities is positive. This is noteworthy in itself, given the poor track record of agricultural extension interventions increasing welfare in similar contexts (Anderson and Feder 2007). The second stage of our randomization design allows us to open the black box of the drivers of behavior of DAs.

III. Delivery Agents

A. Short-Listing and Selection

The second stage of our experimental design lies entirely within the 109 treated villages, and is thus based on the 3,064 households surveyed in these villages. Among these villages 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 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 short list of two.

Once this short list of two is established, we then rapidly implement two surveys in each community. From farmers we collect information on their ties to these candidates. To measure social ties between farmers and each candidate 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. From potential candidates we collect more information on their characteristics. Fieldwork for both surveys is completed within a few days of the delivery agents being short-listed. The rapid timing of data collection and the fact that the actual delivery agent is not yet known help avoid strategic reporting of ties.

In the third and final step, we randomly select one of the short-listed candidates to be the actual delivery agent (DA). The nonselected candidate serves as a counterfactual delivery agent (CA) from within the same community, namely, a shadow individual that also meets all the selection criteria and has a similar network of social ties within the same community. 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 3 confirm the second-stage randomization: DA and CA characteristics are not different from each other in terms of their human capital, land ownership, preintervention 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 96th percentile of agricultural profits in their community.

B. *Social 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.

Figure 2 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%). In the average community, around 55 farmers are tied to either the DA or CA. On the different subtypes of ties between DAs, CAs, and farmers, while 29% of farmers are friends/family of at least one candidate, 5% are exclusively friends or family of the DA (and not the CA), and 7% are exclusive friends or family of the CA. While 62% of farmers discuss agriculture with at least candidate, 11% exclusively discuss agriculture with the DA (and not with the CA), and 14% do so exclusively with the CA.[15]

Our second-stage randomization generates experimental variation in whether farmers are exclusively socially tied to the DA or the CA. Our focus is thus on these two groups of farmers, highlighted in figure 2. 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. Although all farmers are used in our empirical estimation, nowhere in our analysis do we focus on how social incentives affect targeting behavior toward 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.

[15] This separation in exclusive ties of the DA and exclusive ties of the CA occurs despite the fact that the two candidates themselves might be socially tied: three-quarters of candidate pairs report being friends or belonging to the same extended family as each other.

Our research design only allows us to exploit an experimental comparison between those exclusively tied either to the DA or to the CA.[16]

Columns 4 and 5 in table 3 confirm balance on observables between those two groups of farmers. 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 shows there is some geographic sorting within communities so that those tied to the DA reside slightly closer to them. We account for this in our empirical approach described below. For completeness, columns 6 and 7 of table 3 show descriptives on farmers tied to both the DA and CA and those tied to neither candidate, although these two groups of farmers play a less central role in our analysis.

There are four key features of our second-stage-randomization design. First, it eliminates endogenous tie formation between candidate delivery agents and potential beneficiaries. This is similar to designs that exogenously engineer new social ties (Feigenberg, Field, and Pande 2013; Brooks, Donovan, and Johnson 2018; Cai and Szeidl 2018; Vasilaky and Leonard 2018), except that our approach utilizes naturally formed and preexisting ties in the field. Our design is in contrast to the literature identifying impacts of social ties/patronage that leverage within-person variation in the presence of ties over time (Bandiera, Barankay, and Rasul 2009; Hjort 2014; Xu 2018). Finally, our approach is also in contrast to the well established literature on clientelism, which 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 kick-started by the intervention, but our analysis is based on preexisting ties.

Second, it allows us to causally estimate how the number of social ties affects coverage—the total number of farmers targeted by the DA in their community. To identify how social ties determine coverage 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.

Third, it ensures groups of farmers exclusively tied to the DA and CA are similar on observables. This enables us to build on work identifying distortions caused by social ties between delivery agents and potential

[16] Table A2 confirms that there is no differential attrition in the endline survey of farmers based on their tie to the DA, or to the CA (cols. 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 (col. 6), where the p-value on the null of zero interactions is .628.

TABLE 3
BALANCE ON SOCIAL TIES TO ACTUAL AND COUNTERFACTUAL DELIVERY AGENTS

	Actual and Counterfactual Delivery Agents				Farmers Socially Tied to						
	Delivery Agent (1)	Counterfactual Agent (2)	Percentile of the Delivery Agent within Community (3)	p-Value (1) = (2) (4)	Delivery Agent Exclusively (5)	Counterfactual Agent Exclusively (6)	Both Agents (7)	Neither Agent (8)	p-Value (5) = (6) (9)	p-Value (5) = (7) (10)	p-Value (5) = (8) (11)
					A. Socioeconomic Background						
Household head has primary education	.617	.533	89	[.358]	.416	.472	.443	.479	[.146]	[.237]	[.328]
Acres of land owned	2.949	2.873	94	[.886]	2.470	2.547	1.972	2.202	[.933]	[.120]	[.223]
	(2.508)	(2.313)			(4.573)	(5.151)	(2.289)	(2.209)			
Wealth score (0–100)					60.01	59.24	58.92	62.29	[.688]	[.851]	[.687]
					(12.67)	(13.68)	(13.66)	(13.47)			
Food expenditure in last week (000 UGX)					32.17	24.03	22.28	33.12	[.256]	[.350]	[.926]
					(60.75)	(48.90)	(56.96)	(61.13)			
In first quartile of distribution of food expenditure					.237	.220	.251	.235	[.650]	[.559]	[.943]
					B. Seeds and Modern Techniques						
Ever adopted improved seeds	.843	.800	87	[.569]	.224	.230	.380	.234	[.392]	[.417]	[.896]
Number of techniques ever adopted (out of six)	3.583	3.652	94	[.456]	3.255	3.020	3.240	3.080	[.089]	[.266]	[.123]
	(.821)	(.640)			(1.021)	(.996)	(.946)	(.929)			

	C. Agriculture in Last Season										
Hours in agriculture per day	6.596 (2.043)	6.088 (1.515)	94	[.136]	5.607 (1.559)	5.586 (1.476)	6.228 (1.832)	5.342 (1.410)	[.472]	[.557]	[.878]
Acres of land cultivated	1.583 (1.086)	1.763 (1.359)	95	[.414]	1.152 (.954)	1.190 (1.070)	1.122 (1.024)	1.164 (.963)	[.897]	[.937]	[.810]
Profits (000 UGX)	206.9 (176.0)	267.1 (220.9)	96	[.736]	82.92 (314.0)	77.62 (266.9)	81.97 (316.0)	81.57 (307.2)	[.781]	[.515]	[.863]
	D. Distance										
Distance from home of the delivery agent (minutes walking)					1.431 (3.336)	2.169 (6.837)	1.538 (5.616)	1.660 (5.100)	[.051]	[.406]	[.286]
Distance from home of the counterfactual agent (minutes walking)					1.918 (5.041)	2.171 (7.742)	1.715 (6.443)	1.891 (5.983)	[.554]	[.868]	[.727]
Resides in the same village as delivery agent					.450 (.498)	.327 (.470)	.502 (.500)	.283 (.451)	[.324]	[.949]	[.172]
Resides in the same village as counterfactual agent					.347 (.477)	.495 (.501)	.494 (.500)	.235 (.425)	[.167]	[.357]	[.260]

NOTE.—Means, with standard deviation in parentheses. Summary statistics are presented for delivery agents (col. 1), counterfactual agents (col. 2), farmers who know only the delivery agent at baseline (col. 5), farmers who know only the counterfactual agent at baseline (col. 6), farmers who know both agents at baseline (col. 7), and farmers who know neither agent (col. 8). The p-values for (1) = (2) ((5) = (6)) are obtained from regressing each of the reported baseline variable on the dummy for being the delivery agent (being tied to the delivery agent) with robust standard errors (standard errors clustered at the village level) and controlling for branch fixed effects. The percentile of the delivery agent within community in col. 3 presents the percentile of delivery agent trait within her own village (e.g., 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 10 poverty indicators into a score going from 0 to 100. Food expenditure in last week is the total consumption of food, beverages, and tobacco per week per adult equivalent. Number of techniques ever adopted (out of six) calculates the number of techniques ever used (the six are the following: intercropping, line sowing, zero tillage, proper weeding, crop rotation, avoid mixed cropping). Profits are the total output value minus total expenditure value in the last season. All monetary values are expressed in thousands of UGX and are truncated above and below 2 standard deviations from the mean. Exchange rate: 1 USD = 2,519.6 UGX (March 2014).

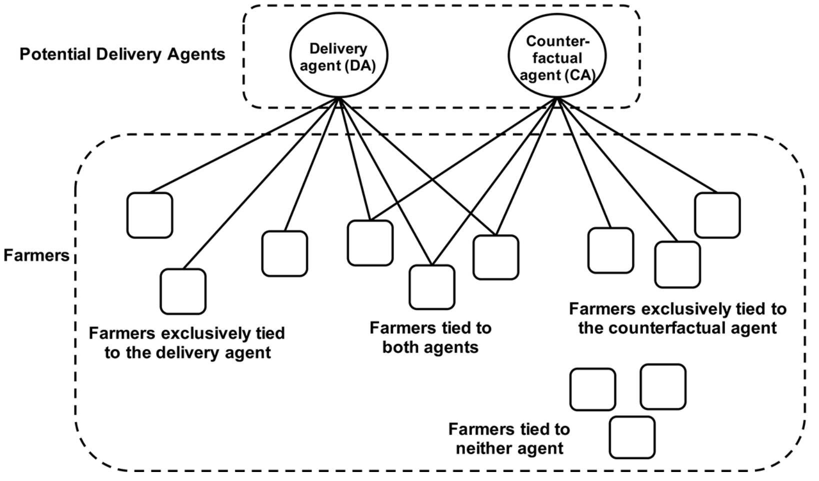

FIG. 2.—Second stage of randomization.

beneficiaries (Banerjee et al. 2018; Alatas et al. 2019; BenYishay and Mobarak 2019; Beaman et al. 2021; Maitra et al. 2021). Specifically, we use experimental variation to identify whether farmers with a specific characteristic—say, being poor—are differentially likely to be targeted if they are tied to the DA relative to observationally equivalent farmers that are tied to the CA. In being able to make an experimental comparison between farmers all of whom share a given characteristic but who exogenously vary in their ties to the DA and CA, we can (i) shed light on the extent to which DAs engage in propoor targeting, and (ii) rule out that such behaviors are driven by demand-side factors related to farmers' behavior (such as their ability to pay for seeds and likelihood of adoption).

Finally, we identify the impact of social ties to potential beneficiaries on DA behavior exploiting variation across farmers within the same community, controlling for community fixed effects and so holding constant all other fixed aspects of social structure (such as features of the aggregate social network of farmers).

IV. The Behavior of Delivery Agents

We sequence our results as follows. We first document how social ties determine the total number of farmers targeted by the DA (coverage), and then consider the extent to which DAs engage in propoor targeting, in line with the original intent of the NGO BRAC. This ultimately sheds light on whether social incentives can be harnessed for the greater good and in line with the antipoverty objectives of the intervention.

A. Coverage

1. 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, our second-stage randomization ensures that the exact number exclusively tied to the DA is exogenous. Figure A1 shows the variation used: the number of farmers exclusively socially tied to the DA ranges from zero to over 20 per community. We estimate the following specification for community c:

$$\begin{aligned}\text{coverage}_c = \alpha + \beta_{\text{DA}}\left(\sum_i \text{ST}_{i,\text{DA},c}\right) \\ + \beta_{\text{DA+CA}}\left(\sum_i \text{ST}_{i,\text{DA},c} + \sum_i \text{ST}_{i,\text{CA},c}\right) + \gamma X_c + u_c.\end{aligned} \tag{2}$$

Here, 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. $\text{ST}_{ijc} = 1$ if i has social tie of type j, where $j \in \{\text{DA, CA}\}$ indicates being exclusively tied to the DA or exclusively tied to the CA. The total number of farmers exclusively tied to the DA or CA is $\Sigma_i \text{ST}_{i,\text{DA},c} + \Sigma_i \text{ST}_{i,\text{CA},c}$, and $\sum_i \text{ST}_{i,\text{DA},c}$ is the number of farmers exclusively tied to the DA. In X_c we control for BRAC branch and report robust standard errors.[17]

The parameter of interest is β_{DA}: the responsiveness of coverage to the number of exclusive social ties the 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, 2018; Beaman and Dillon 2018; Galeotti, Golub, and Goyal 2020).

2. Results

Table 4 presents the results; $\hat{\beta}_{\text{DA}} = .138$ 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 1: 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

[17] 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 ties (being exclusively tied to the CA, being tied to both the DA and CA, or being tied to neither).

TABLE 4
COVERAGE

	Social Ties	
	(1)	(2)
Number of exclusive ties to DA	.138***	.123
	(.041)	(.074)
Number of exclusive ties to DA squared		.001
		(.002)
Mean	.500	.500
R^2	.675	.676
Partial R^2 for number of exclusive ties to DA	.306	.121
Shapley decomposition of the R^2	.565	.695
Observations	60	60

NOTE.—Dependent variable: number of farmers targeted. OLS estimates, with robust standard errors in parentheses. Table shows community-level OLS regressions. All regressions control for branch fixed effects and for the number of exclusive ties (number of farmers tied to one of the two agents). Number of farmers targeted by the DA is the total number of sample farmers, among those exclusively tied to the actual or counterfactual DA, in the community who report having received seeds or training from the DA in the last year. Number exclusively tied to DA is the number of sample farmers in the community who know only the DA. The partial R^2 for number of ties to DA is the variation in the outcome variable that is explained by variation in the number of farmers tied to the DA. The Shapley decomposition of the R^2 reports the proportion of the R^2 that is contributed by the reported coefficients.

*** Significant at the 1% level.

any nonlinearity 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 nonlinearity.

The parameter $\hat{\beta}_{\mathrm{DA}} > 0$ 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.[18]

If we focus on coverage as the metric of intervention success, the results begin to shed light on why the first-stage intervention results are positive overall (in the comparison between treated and control villages), and also why social ties can lead interventions to be successful in some communities and fail in others. With the goal of quantifying how much of the cross-village variation in coverage is explained by exclusive social ties of the DA, we note from column 1 that (i) the partial R^2 for

[18] In the context of information diffusion about a new product (microfinance), Banerjee et al. (2013) show the likelihood that information is passed along to social ties is .350 (they also highlight the role that nonparticipants play for information diffusion). In the case of a new agricultural technology in Malawi, Beaman and Dillon (2018) 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. (2021) show that respondents with two connections to entry points are 7.2 percentage points more likely to have new information, corresponding to a 33% increase in knowledge relative to those unconnected to entry nodes.

the number of exclusive ties of the DA is .306 (so just under half the R^2), and (ii) using the Shapley approach to decompose the R^2, 57% of the variation is explained by exclusive ties of the DA.[19]

B. Targeting

1. Empirical Method

To study how social ties shape the targeting behavior of delivery agents, we estimate the following specification for farmer i in community c:

$$\text{target}_{ic} = \alpha + \sum_j \beta_j \text{ST}_{ijc} + \sum_{j \in \{\text{DA,CA}\}} \rho_j \text{dist}_{ij} + \lambda_c + u_{ic}. \tag{3}$$

Here, $\text{target}_{ic} = 1$ if i is targeted by the delivery agent (so they receive seeds or training from her). $\text{ST}_{ijc} = 1$ if i has social tie of type j, where $j \in \{\text{DA, CA, both, neither}\}$. All four groups are in the estimation sample, and the omitted group are those exclusively connected only to the CA ($\text{ST}_{i,\text{CA},c}$). 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 only the second-stage experimental variation. We earlier showed that among those exclusively tied to the DA (or CA), they are balanced on observables (table 3).

We noted earlier that there is some geographic sorting within communities so that those tied to the DA reside slightly closer to them (table 3, panel D). As documented in similar settings, physical distance between households is not always a good proxy for their social distance (Beaman et al. 2021); we account for any unobservables driving outcomes and correlated to geography by controlling for the distance between farmer j's residence and the DA's and CA's residence (dist_{ij}).

Our design also enables us to control for community fixed effects (λ_c) and identify the causal impact on targeting of social ties 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

[19] An alternative approach would be to regress coverage on the total number of ties of the DA, irrespective of whether they are also connected to the CA, as well as the total number of farmers exclusively tied to the DA or CA. The reasons we use our current approach are (i) to ensure no farmers are double counted in the empirical specification (which only reduces power to distinguish between those connected to either the DA or the CA relative to those connected to both), and (ii) that the specification we use narrows the focus on those sets of agents that the second stage of the experiment allows a clean comparison between (namely the exclusive ties of the DA relative to the exclusive ties of the CA). Notwithstanding these issues, estimating the alternative specification leaves the conclusions unchanged: coverage significantly increases in the number of social ties of the DA. The marginal effect is .235, so larger than that reported in our preferred specification, and statistically significant at the 1% level. We continue to find no evidence of any nonlinear relationship between social ties of the DA and coverage. (Results are available upon request.)

with the ability of the network to target resources effectively; such features are captured in λ_c.

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

2. Results

Column 1 of table 5 shows that farmers exclusively tied to the delivery agent are 6.2 percentage points more likely to be targeted by the DA relative to farmers exclusively tied to the CA. 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 increase in the likelihood of her own social ties being targeted relative to them. The fact that $\hat{\beta}_{\mathrm{DA}} > 0$ reconfirms a central presumption of the local delivery model.

As much of the literature has emphasized, this differential targeting probability can capture the DA having lower screening costs of targeting her own ties—say, because of better knowledge of their need—or being able to communicate or transmit information to them more effectively—say, because ties of the DA might be able to observe the plot of the DA more easily and hence trust the quality of the seeds to a great extent.[20]

To help tease apart these explanations, we use two approaches. First, the other rows in table 5 show that DAs are significantly more likely to target those exclusively tied to them relative to those tied to both the DA and CA ($p = .013$) as well as those tied to neither agent ($p = .001$). This pattern of results runs counter to the assumption that the targeting costs to DAs are the same for all their social ties (irrespective of whether those ties are also then tied to the CA). Rather it is in line with the behavior of DAs being driven by social incentives, whereby they target their exclusive social ties—over those farmers tied to both them and the CA, as well as those tied exclusively to the CA. Such targeting biases might be motivated by them not wanting any gains from the intervention ever diffusing to the CA or their social ties.

Second, we use 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, to examine whether the targeting by DAs depends on the nature of the social tie. We see that for friends/family, DAs are

[20] In line with this we note that we do find that the distance between farmer j's residence and the DA's residence ($\mathrm{dist}_{\mathrm{DA}_j}$) does predict the likelihood of being targeted. It is well documented that valuable information related to targeting can be held by community members. This is so in the context of antipoverty interventions (Alatas et al. 2012), labor markets (Beaman and Magruder 2012), credit markets (Maitra et al. 2017), or capital markets (Hussam, Rigol, and Roth 2022). 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).

TABLE 5
TARGETING

	Social Ties (1)	Friend or Family (2)	Discusses Agriculture (3)	Religion (4)	Ethnicity (5)
Exclusively tied to DA	.062***	.059**	.039*	−.023	−.010
	(.023)	(.027)	(.020)	(.018)	(.048)
Tied to both agents	.015	.011	.013	−.004	−.001
	(.011)	(.017)	(.014)	(.027)	(.039)
Tied to neither agent	−.006	−.003	−.009	−.035**	−.040
	(.013)	(.015)	(.015)	(.016)	(.028)
Community fixed effects	Yes	Yes	Yes	Yes	Yes
Mean outcome: exclusively tied to CA	.019	.029	.024	.046	.032
p-Value: exclusively tied to DA = tied to both	[.013]	[.043]	[.091]	[.440]	[.817]
p-Value: exclusively tied to DA = tied to neither	[.001]	[.006]	[.001]	[.292]	[.265]
Observations	2,421	2,421	2,087	2,420	2,413

NOTE.—Dependent variable: DA targets farmer (received seeds or training in last year). OLS estimates, with standard errors in parentheses (clustered by community and ties). Table shows farmer-level OLS regressions. All regressions control for community fixed effects, the walking distance to the DA's home, and the walking distance to the CA's home. Exclusively tied to DA equals 1 if the farmer knows the DA only in col. 1, is a friend or family of the DA only in col. 2, regularly discusses agriculture with the DA only in col. 3, has the same religion as the DA in col. 4, or has the same ethnicity as the DA in col. 5. The omitted group (exclusively tied to CA) is composed of farmers who are socially tied only to the CA.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

significantly more likely to target their link than similar exclusive links to the CA. The magnitude of the effect is smaller and less precise for ties related to discussing agriculture than for family/friend ties. Although there is existing evidence that social ties can lower the effort costs of communication (BenYishay and Mobarak 2019; Berg et al. 2019), in our context the evidence casts doubt on the hypothesis that the *only* reason DAs favor their ties is because they can convince them more easily, or that such farmers are more receptive to information that comes from the DA (because those already discussing agriculture with the DA are more likely to trust their advice). Similarly, the fact that the targeting behavior of DAs is not only concentrated among their friends/family casts doubt on the hypothesis that the *only* reason DAs favor their ties is because of pure altruism toward their closest ties.[21]

[21] We cannot split between friends and family so the results cannot rule out that kinship does or does not matter separately from friendship ties. We do have some other information

Given that the results show DAs target their social ties irrespective of the nature of the link, a natural concern is that such links are picking up some other characteristic, such as religion or ethnicity, that is common to the social group of the DA. To check for this we reestimate (3) by defining ties using these dimensions—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. The results in columns 4 and 5 show a common pattern of null effects: ties of religion or ethnicity do not predict the targeting behavior of DAs.[22]

Panel A of table A6 shows that correcting for randomization inference generally reduces *p*-values: all forms of social ties exclusively tied to the DA are significantly more likely to be targeted relative to the same type of exclusive ties of the CA, although the placebo check using ethnic ties continues to show this is not a marker of targeting behavior of DAs.

3. Propoor Targeting

The NGO's objective is that the intervention be used as an antipoverty tool, with DAs being clearly instructed to engage in propoor targeting. The fundamental moral hazard problem is that this cannot be monitored by the NGO. As monetary incentives cannot be designed to achieve this objective, there is a reliance on social incentives being harnessed to do so. We next examine the extent to which social incentives affect whether DAs adhere to this objective. We do so by extending the earlier specification to estimate

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from endline on the percentage of the total time engaged in labor activities that DAs devote to their work for BRAC. We find no significant correlation between this and the social ties they have to others in their community. This again is suggestive that communication costs are not driving the targeting behavior of DAs.

[22] We can also extend these targeting results to examine impacts on techniques. The result is in table A5: we see an improvement in enumerator-observed techniques among those exclusively tied to the DA relative to those exclusively tied to the CA (col. 1), and this improvement is significantly higher than for those farmers tied to both the DA and CA ($p = .012$). As with the main results on seed adoption, cols. 2 and 3 show that this improvement in techniques is concentrated among the family and friends of the DA. As for the analysis of coverage, an alternative approach to evaluating the targeting behavior of DAs is to control for the social ties of the DA irrespective of whether they are also tied to the CA, so effectively adjusting (3) to classify farmers into social ties of type $j \in \{\text{CA, DA, both, neither}\}$. This is not our preferred approach as this does not exploit the second-stage randomization, and does not allow us to distinguish between exclusive social ties of the DA and those ties to both the DA and CA—which the results reveal is an important distinction to make. Nevertheless, following this approach leads to a similar conclusion: social ties of the DA (irrespective of whether they are also tied to the CA) are significantly more likely to be targeted by DAs relative to those connected to both: the marginal effect is .030 and this is statistically significant at the 5% level. We continue to find this effect is concentrated among friends/family rather than those that talk to the DA about agriculture, and the placebo results related to religious or ethnic ties continue to hold.

$$\begin{aligned}\text{target}_{ic} = \alpha + \sum_j \beta_j\left[\text{ST}_{ijc} \times (1 - P_i)\right] + \sum_j \zeta_j\left(\text{ST}_{ijc} \times P_i\right) + \kappa P_i \\ + \sum_{j\in\{\text{DA,CA}\}} \rho_j \text{dist}_{ij} + \lambda_c + u_{ic}.\end{aligned} \tag{4}$$

This specification therefore includes a sequence of interactions between the type of social tie the DA has and the poverty status of the farmer ($\text{ST}_{ijc} \times (1 - P_i)$, $\text{ST}_{ijc} \times P_i$). 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 β_{DA}, which captures the differential likelihood that nonpoor farmers exclusively tied to the DA are treated relative to nonpoor farmers exclusively tied to the CA, and ζ_{DA}, the differential likelihood that poor farmers exclusively tied to the DA are treated relative to poor farmers exclusively tied to the CA. We earlier documented that the second-stage randomization ensures that neediness is the same among the ties of the DA and CA (table 3).

The result in column 1 of table 6 shows that poor ties of the DA are no more (or less) likely to be targeted than poor ties of the CA: the baseline probability of the latter being targeted is 3.6% and this hardly changes among the poor exclusive ties of the DA ($\hat{\zeta}_j = .009$). In other words, poor farmers are equally likely to be targeted irrespective of their social ties to the DA.

However, the nonpoor social ties of the DA are significantly more likely to be targeted than the nonpoor ties of the CA. The difference in targeting probabilities is 7.7 percentage points: the baseline probability that nonpoor ties of the CA are targeted is 1.4%. Hence, nonpoor ties of the DA are more than 5 times more likely to be targeted than farmers with similar observables but who are exclusively tied to the CA.

Columns 2 and 3 confirm that this pattern of targeting nonpoor social ties is replicated across types of ties. Among social ties based on friends/family, (i) poor friend/family ties of the DA members are no more likely to be targeted than poor friend/family ties of the CA, and (ii) DAs are nearly twice as likely to target their nonpoor friends/family members as to target the nonpoor friends/family ties of the CA (the latter group's baseline probability to be targeted is 8.3%, and this rises by another 8.4 percentage points for similar exclusive ties of the DA). Among social ties based on discussing agriculture, (i) such poor ties of the DA members are no more likely to be targeted by them than similar poor ties of the CA, and (ii) DAs are 3 times as likely to target nonpoor farmers whom they talk to about agriculture than similar ties of the CA (the latter group's baseline probability to be targeted is 2.3%, rising by another 4.3 percentage points for similar exclusive ties of the DA).

TABLE 6
Propoor Targeting

	Social Ties (1)	Friend or Family (2)	Discusses Agriculture (3)	Religion (4)	Ethnicity (5)
Exclusively tied to DA × poor	.009	−.030	.023	−.045	−.022
	(.033)	(.064)	(.034)	(.031)	(.052)
Exclusively tied to DA × not poor	.077***	.084***	.043**	−.015	−.008
	(.024)	(.029)	(.021)	(.020)	(.049)
Poor	.022	.070	.012	.038	.009
	(.026)	(.051)	(.023)	(.030)	(.012)
Community fixed effects	Yes	Yes	Yes	Yes	Yes
Mean outcome: poor and exclusively tied to CA	.036	.083	.030	.073	.037
Mean outcome: not poor and exclusively tied to CA	.014	.014	.023	.037	.031
p-Value: exclusively tied to DA × poor = exclusively tied to DA × not poor	[.043]	[.103]	[.566]	[.406]	[.666]
Observations	2,421	2,421	2,087	2,420	2,413

Note.—Dependent variable: DA targets farmer (received seeds or training in last year). OLS estimates, with standard errors in parentheses (clustered by community and ties). Table shows 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 neither agent, the walking distance to the DA's home, and the walking distance to the CA's home. Exclusively tied to DA equals 1 if the farmer knows the DA only in col. 1, is a friend or family of the DA only in col. 2, regularly discusses agriculture with the DA only in col. 3, has the same religion as the DA in col. 4, or has the same ethnicity as the DA in col. 5. The omitted group (exclusively tied to CA) is composed of farmers who are socially tied only to the CA. Poor (not poor) equals 1 if the household belongs (does not belong) to the bottom quartile of the within-community distribution of food expenditure.

** Significant at the 5% level.

*** Significant at the 1% level.

Columns 4 and 5 reconfirm that if we repeat the analysis using alternative measures of links, based on same religion or ethnicity, we find that neither characteristic predicts the degree of propoor targeting behavior of DAs.

The coefficients of interest ($\hat{\beta}_{\mathrm{DA}}, \hat{\zeta}_{\mathrm{DA}}$) on the differential likelihood the DA targets her nonpoor (poor) social ties more than those of the CA are both experimentally identified using the second stage of our research design. The difference between them, $\hat{\beta}_{\mathrm{DA}} - \hat{\zeta}_{\mathrm{DA}}$, is thus also experimentally identified and pins down the extent of propoor targeting by the DA. At the foot of each column in table 6 we show the p-value on the difference in probability of being targeted for poor and nonpoor social ties of the DAs (relative to those of the CA). Starting in column 1 with our core measure of social ties, we see that DAs are significantly less likely to target their poor ties than their nonpoor ties, relative to comparable ties of the CA (p = .043). Moreover, examining what drives this difference in differences, we see that nonpoor CA ties are no more likely to be targeted than poor CA ties ($\hat{\kappa}$ = .022, with standard error .026, so this is not statistically significant). Hence the difference in differences is driven by differential targeting probabilities of the DA within her own social ties.

Combining these treatment effects with the baseline probabilities of the poor and nonpoor exclusive ties of the CA being targeted (as reported at the foot of col. 1), we find that the ranking in targeting probabilities is as follows: exclusive nonpoor ties of the DA are most likely to be targeted (9.1%), followed by poor ties of the DA (4.5%), poor ties of the CA (3.6%), and finally the nonpoor ties of the CA (1.4%).[23]

In table A6 we show that all these results are robust to p-value corrections for randomization inference following the approach set out in Young (2019). In tables A7 and A8 we show that the findings are also robust to defining poverty based on slightly different thresholds on consumption, and using nonconsumption metrics of poverty, which might be easier for DAs to observe.[24]

From the full specification in (3) we can also examine whether the extent of DAs targeting the nonpoor toward their social ties varies by whether those social ties are exclusively tied to them, and those that are tied to both the DA and CA. We find there is a robust difference between the two: among the nonpoor, DAs are significantly more likely to target their exclusive ties rather than ties tied to both them and their counterfactual. This holds when using our baseline measure of social ties (p = .008), or narrowing the type of tie down to friends/family (p = .041) or those that discuss agriculture with the DA (p = .085). No such difference is found for the placebo ties based on religion or ethnicity (p = .260, .821, respectively).[25] These results build on the earlier suggestion that DAs do not favor all their social ties equally: they prefer to target the nonpoor that are exclusively tied to them rather than additionally tied to the CA.

[23] When considering specific types of ties we lose some power in this test. It remains the case that among CA ties, the poor and nonpoor are not differentially targeted.

[24] In our sample, the total value of food consumption at the 25th percentile is UGX 9,957 per month. This is twice as high as Uganda's current national poverty line. Hence all those classified in our sample as nonpoor are almost certainly nonpoor by that definition. In table A7 we show robustness of our main result to changing the threshold of poverty to above/below what is used in our main specification. Table A8 shows the robustness of the propoor targeting result to using a wide range of alternative measures of poor including wealth, asset values, never having adopted any modern agricultural techniques, and agricultural profits.

[25] Results are available upon request.

Our findings overall reveal a basic tension at the heart of the local delivery model commonly used by NGOs across contexts and types of poverty alleviation intervention in agriculture, health, credit, and so forth. While the local delivery model implicitly assumes that NGOs can harness social incentives for the greater good, we identify a basic coverage-targeting trade-off at the heart of the model. On the one hand, DAs are induced to exert greater effort to treat more farmers when they have more social ties in the community that they are recruited from and serve (table 4); hence on this dimension it is better to use better socially connected DAs, all else equal. However, when exerting more effort, while poor farmers are equally likely to be targeted irrespective of their social ties to the DA, DAs are also more likely to target nonpoor farmers they are exclusively tied to (table 6). This goes against the antipoverty intentions of the intervention. These targeting biases do not undo the overall positive effect of the intervention, as identified in the first stage of randomization. But they do suggest that social incentives of DAs can prevent some interventions from living up to their full promise, and variation in social ties of DAs across communities leads to heterogeneity in the efficacy of the exact same intervention across locations, even though DAs are offered the same set of monetary incentives and career concerns.

C. *Why Target Nonpoor Social Ties?*

The results are not consistent with DAs engaging only in propoor targeting. Our research design rules out simple explanations for this based on demand-side factors that are common to all nonpoor farmers irrespective of whether they are tied to the DA and/or the CA—for example, that the nonpoor are more willing to pay the (below market) price for the modern seeds, or that positively selected DAs have better information on the practices of nonpoor farmers. Rather, we seek explanations for why there exists an interaction between social ties and the poverty status of farmers, causing DAs to target their exclusive nonpoor social ties with modern seeds and techniques. Moreover, we note throughout that the evidence shows that DAs are significantly more likely to target those that are exclusively tied to them, relative to those that are tied to both the DA and CA. This is consistent with the social incentives of DAs being such that they want to favor some of their own ties, and not those who could be more likely to enable the intervention benefits to flow to the CA or their exclusive ties.

We consider two explanations for why social incentives drive such behavior: (i) this maximizes total surplus among their exclusive social ties, which is then redistributed using informal transfers among group members; (ii) they can more easily enforce an implicit cooperative agreement among their ties whereby delivery agents target benefits toward their nonpoor social ties in exchange for some side payment or kickback.

1. Surplus Maximization

Social incentives might provide DAs with the objective of maximizing total surplus through targeting their nonpoor ties because (i) the return to the intervention is higher for the nonpoor than the poor, and (ii) having maximized surplus, social groups then engage in ex post redistribution to the poor. Such mechanisms might be especially strong 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 possibility we proceed in two steps following (i) and (ii) above. On (i) we assess the differential returns to being targeted for poor and nonpoor farmers. We split households into poor and nonpoor based on our baseline consumption-based measure. In table A9 we then consider impacts on profits from the last season prior to endline either from being targeted directly by the DA (col. 1) or from having received improved seeds—although not necessarily training—from any source (col. 2). The results provide support that being targeted generates returns, but not that this is differentially so for the poor or nonpoor (p = .387, .899, respectively). However this interpretation is subject to the obvious caveats that these results do not exploit experimental variation in targeting, and profits are noisy.

An alternative approach to the issue is to nonparametrically estimate the relationship between consumption (our proxy for poverty) and net output (profits) from agriculture at baseline, and examine whether this is differential between high- and low-ability farmers as proxied by (i) the number of techniques they use at baseline, and (ii) whether they have previously adopted the seeds. These results are shown in figure A2. We do not find any evidence of a strong gradient between consumption and profits, nor does this differ across these types of farmers. This is again suggestive of relatively homogeneous impacts of potential adoption across farmers.

On (ii) 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, Dupas, and Robinson 2020). As detailed in the appendix and summarized in table A10, we find the diffusion of seeds among farmers does not depend on social ties to the DA (cols. 1 and 2). Second, 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 (cols. 3 and 4). Hence this channel does not ameliorate any targeting biases of DAs.

2. Rent Extraction

Rather than considering redistribution to the poor of the surplus generated from DAs targeting the nonpoor, an alternative possibility is that what motivates DAs is the redistribution of some of the surplus back to them. This hypothesis builds on the idea that development brokers in local interventions can engage in rent-seeking behavior (Platteau and Gaspart 2003; Voors et al. 2018; Maitra et al. 2021).

More precisely, assume DAs can more easily form an implicit agreement among their own exclusive social ties (rather than exclusive ties of the CA or those tied to both) of the following kind: if they target them, they provide the DA some rent—or kickback—from the gains generated. The possibility to form and enforce such implicit agreements is only possible among close social ties, much as in the literature on implicit risk-sharing agreements within social networks. Finally, because of diminishing marginal utility of consumption, the nonpoor might be more willing and able to provide such kickbacks given their higher levels of economic well-being to begin with. This possibility to extract rents is reinforced by the fact that the DA holds unique power in communities: there is no alternative individual that can play this intermediary role with the NGO.

The ability of DAs to extract rents from their ties 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). The excess asset accumulation of delivery agents is the log difference between their actual and predicted wealth at endline. Of course we do not observe actual DAs in control villages, so we have to predict which farmers might have been eligible candidates based on the descriptive evidence in table 3. The excess asset accumulation measure, which differences across actual and predicted DAs, then should eliminate any common trends in wealth accumulation among those that can potentially serve as delivery agents.[26]

We regress this on the number of exclusive ties of the DA that are poor, the number of exclusive ties of the DA that are not poor, the number of ties to either the DA or the CA that are poor, and the number of ties to

[26] 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.

either the DA or CA that are not poor, and so estimate the following specification across DAs in treated communities c:

$$\begin{aligned}
\text{excess asset accumulation}_c = {} & \alpha + \beta_{\mathrm{DA}}\left[\sum_i \mathrm{ST}_{i,\mathrm{DA},c} \times (1 - P_i)\right] \\
& + \beta_{\mathrm{DA}}\left(\sum_i \mathrm{ST}_{i,\mathrm{DA},c} \times P_i\right) \\
& + \beta_{\mathrm{DA+CA}}\left\{\left[\sum_i \mathrm{ST}_{i,\mathrm{DA},c} \times (1 - P_i)\right] + \left[\sum_i \mathrm{ST}_{i,\mathrm{CA},c} \times (1 - P_i)\right]\right\} \qquad (5) \\
& + \beta_{\mathrm{DA+CA}}\left[\left(\sum_i \mathrm{ST}_{i,\mathrm{DA},c} \times P_i\right) + \left(\sum_i \mathrm{ST}_{i,\mathrm{CA},c} \times P_i\right)\right] \\
& + u_c.
\end{aligned}$$

To begin with we use the number of assets owned as the simplest measure of wealth. We see that when the DA has more exclusive social ties, she has significantly higher excess wealth than predicted (table 7, col. 1). Column 2 confirms the same pattern of results when we use the value of assets as the measure of asset accumulation.[27] Column 3 shows that this result is driven by the presence of exclusive nonpoor ties of the DA in the community, exactly in line with the hypothesis. Columns 4 to 6 break down total asset values into categories: we find excess accumulation is concentrated in agricultural assets, rather than household or other business assets. Finally, column 7 examines impacts on excess savings. As with assets, we see significant excess savings accumulation by DAs when they have more exclusive social ties to nonpoor farmers.[28]

We can use the estimates from column 3 to back out the value of rent extracted by the delivery agent. Given that the baseline average value of assets owned by DAs is UGX 531,000, a 46% excess wealth accumulation corresponds to UGX 244,260. As a benchmark, from table 2 we see that the ITT impact on net profits is UGX 33,660, so the rent extraction of delivery agents is approximately 7 times the average gains to individual farmers from the intervention. While some of this asset accumulation

[27] 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 assets: furniture, furnishings (carpet, mat, mattress, etc.), bed nets, 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.

[28] Breza and Chandrasekhar (2019) present experimental evidence that an agent's position in a social network and social incentives operating through reputational concerns can distort savings decisions in villages in India.

TABLE 7
EXCESS ASSET ACCUMULATION OF DAs

	Number of Assets Owned	Value of					
		Assets (Total)		Household Assets	Agriculture Assets	Business Assets	Savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of exclusive ties to DA	.042**	.161***					
	(.020)	(.046)					
Number of exclusive ties to DA and poor			.020	.047	−.047	.002	−.029
			(.067)	(.047)	(.047)	(.010)	(.057)
Number of exclusive ties to DA and not poor			.460***	.105	.176**	.016	.217**
			(.118)	(.082)	(.083)	(.017)	(.100)
Mean outcome	.751	−.022	−.022	−.132	.106	.025	.774
R^2	.153	.181	.288	.108	.112	.026	.098
Observations	60	60	60	60	60	60	60

NOTE.—Dependent variable: excess wealth of the DA (actual minus predicted). OLS estimates, with robust standard errors in parentheses. Table shows community-level OLS regressions. All regressions control for community fixed effects. They also control for the number of poor ties to DA or CA, and the number of nonpoor ties to DA or CA. The excess wealth growth of the DA is measured as the log of the difference between the actual wealth of the DA 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 DA on their baseline wealth, and (2) using the estimated coefficient to predict the DA'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 crops grown, and profits in agriculture (step 1). Wealth is proxied with the value of assets owned (by category). The value of assets owned equals the number of each asset owned times the median price of that asset in the community. In cols. 2 and 3, we consider 18 categories of assets for which there is relatively low variation in prices across villages. Columns 4, 5, and 6 are restricted to household-, agriculture-, and business-related assets, respectively.

** Significant at the 5% level.

*** Significant at the 1% level.

can be due to DAs themselves adopting seeds and techniques, this is unlikely to explain the majority of this excess given the earlier magnitude of the effect on the accumulation of productive assets of the average farmer (table 2, col. 9). Moreover, this static estimate might represent a lower bound on the net present value of kickbacks if DAs also receive future favors from the nonpoor; such dynamic considerations of course lie at the heart of implicit agreements related to risk sharing that have been much documented in rural economies.

3. Alternative Explanations

Of course this does not exhaust all the possible explanations for DA behavior. An alternative is that the gains to the DA adopting the new seeds and techniques are higher for DAs the greater the number of their social ties who also adopt, through a standard social learning externality. Three pieces of evidence mitigate against such an interpretation. First, we note that at baseline the vast majority of DAs (and CAs) have previously adopted the new seeds, and they use more modern techniques than regular farmers. As such they have less to learn about the new technologies than the kinds of poor farmers who are the intended beneficiaries of the intervention. Second, this does not easily explain the differential treatment by DAs of their exclusive social ties relative to those that are socially tied to them and the CA. Third, we can examine whether agricultural outcomes of DAs change excessively relative to those of predicted DAs in control villages, and whether this relates to the same structure of social ties in their communities that drive targeting decisions. Using the same methodology as in specification (5), these results are presented in table A11. They demonstrate that there is no association between the structure of social ties of the DAs and their agricultural outcomes, such as acres cultivated, hours worked, revenues, or expenditures. This runs counter to the idea that the targeting behavior of DAs is driven by potential complementarities in learning or gains from targeting other nonpoor farmers they are socially tied to.[29]

[29] A second alternative explanation is that they target nonpoor ties to curry favor with elites in their social group and raise their own social status (Shayo 2020). Although we lack qualitative data along these lines, in our endline survey of DAs we asked, "Compared to others in your community, how would you describe your financial status?" with potential answers being of a five-point Likert scale from "5 = better than many" to "1 = worse than many." We asked similar questions about their social status (these questions were not asked to farmers about their perceptions of the DA, though). We examine how these self-reports of DAs relate to the structure of their social ties, following an analogous specification to that in (5). We do not have the same information from controls, so these regressions are only exploiting data from the cross section of DAs in treated villages. The results are shown in table A12. The coefficients are of the expected sign but imprecisely estimated. For example, the self-reported financial status of the DA is positively correlated (conditional on other factors) to the number of ties they have to nonpoor farmers (col. 2). The same is true for their

V. Discussion

A. *Policy Implications*

Social incentives cause local delivery agents tasked to deliver a standard development intervention to skew its delivery toward their nonpoor social ties, counter to the original propoor intent of the intervention. We discuss modifications to the design of the local delivery model that could ameliorate these concerns.

1. Modifying the Local Delivery Model

A first set of responses emerges 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. 2018).[30]

2. 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 propoor targeting. Counter to this is the concern that it might strengthen incentives for DAs to seek rents from targeting the nonpoor, or lead to more elite capture as chosen DAs seek to curry favor with elites or gain social esteem by targeting the nonpoor.

The political economy literature on decentralization has emphasized that democratic incentives can discipline local agents. The selection and

social status in the community (col. 4), and for them reporting no financial problems related to agriculture (col. 6). In the first two cases, the sign of the coefficient for the number of ties the DA has to poor farmers is negative, and in all cases the magnitude of the coefficient of ties to nonpoor farmers is orders of magnitude higher than the coefficient on ties to poor farmers. Taken together, the evidence is suggestive of the financial and social status of DAs being raised through their behavior under the intervention.

[30] 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. Atanassova 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.

retention mechanisms for delivery agents do not currently embody such incentives (beyond reputation): they face no oversight or formal accountability to locals or any notion of reelection/reappointment, which is surprising given that 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, Stryjan, and Sulaiman 2019).

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 Bó, Finan, and Rossi 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).[31] Deserranno, Qian, and Nansamba (2020) present evidence from a field experiment that vividly illustrates these labor supply constraints in Uganda: they find that 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 from which the NGO poaches the government agent.

3. 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 propoor interests of the NGO, BRAC. One concern is that the offer of greater financial incentives affects the pool of applicants, discouraging the most prosocial from applying (Deserranno 2019). Conditional on selection, BenYishay and Mobarak (2019) show that 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

[31] Bridle et al. (2020) 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.

increases their effort, and reduces the importance of social ties regarding whom they target.

Whether the provision of monetary incentives would weaken social incentives in the context of local development interventions remains unknown. Our estimates suggest that the additional monetary value of these incentives would need to offset the rents that DAs currently appear to earn from targeting their nonpoor social ties.[32]

B. *External Validity*

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. The concern that social incentives can lead to a trade-off between coverage and mistargeting arises across contexts. Hence we view our findings as being potentially informative beyond the specifics of agricultural extension interventions, to other settings where the local delivery model is used. However, to appreciate precisely when our results might apply more widely, it is important to be clear on the key structural features of our setting.

First, 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 the targeting bias of DAs (although in our context we find these routes neither offset nor exacerbate this bias). Community-wide ex post transfers could also be used to offset any initial targeting bias. This also did not occur in our study setting. Finally, with large enough interventions there is the possibility for market responses to offset distortions caused by the social incentives of delivery agents (Bjorkman Nyqvist, Svensson, and Yanagizawa-Drott 2022; Vera-Cossio 2022).[33]

Second, the benefits distributed by delivery agents to farmers are private. Individual gains from being targeted are noticeable, enabling delivery

[32] 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 2019).

[33] Vera-Cossio (2022) studies the provision of credit in Thai villages by local leaders under the Million Baht Village Fund. He finds they allocate credit toward 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, Svensson, and Yanagizawa-Drott (2022) study the market for drugs in Uganda, which is subject to a lemons problem similar to 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.

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 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 the web of social ties.

Understanding the effectiveness of the local delivery model as we vary these aspects—the availability of market and nonmarket 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.

VI. 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 propoor 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 can harness the greater benefits of social incentives can be forged.

Appendix

A1. 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 A10 we consider whether farmers report obtaining seeds from non-BRAC sources, including other farmers. We see that the ties of the DA are no more likely than those of the CA to report doing so. Aggregating across all sources that farmers can obtain seeds from, column 2 shows the overall likelihood impact on farmers obtaining seeds: this confirms that nonpoor CA ties are no more likely to obtain seeds.

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, thus 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 toward the poor (Basurto, Dupas, and Robinson 2020). If returns to adoption are rising in initial wealth, DAs will find it optimal to target nonpoor farmers to first maximize the social surplus. We probe this interpretation using two strategies.[34]

First, 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.[35] The results are in columns 3 and 4 of table A10. We see that 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.

A2. 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 nonparticipants. The program implementation was coordinated with the randomization protocol so that after the

[34] Basurto, Dupas, and Robinson (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.

[35] 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.

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 was 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 short-listed. We obtained informed consent from all participants prior to the study. This included explanations of the agricultural extension and microfinance programs. This also described the research team, and met institutional review board 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 memoranda of understanding were signed, and the local council chairperson 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 were aware of 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 of 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 with participants in each study village is planned due to resource constraints. We do not foresee risks of the misuse of research findings.

Our analysis deviated from the American Economic Association preregistry in two ways. First, we prespecified that our main analysis would estimate the heterogeneous effects of the agriculture extension program with and without an additional microfinance program. These results are shown in table A4 but we do not discuss them in detail in the main body of the paper. Second, the sample size slightly changed since the preregistration: the number of villages that received the microfinance programs is 51 (rather than 52), and the total number of villages in the sample is 168 (rather than 166).

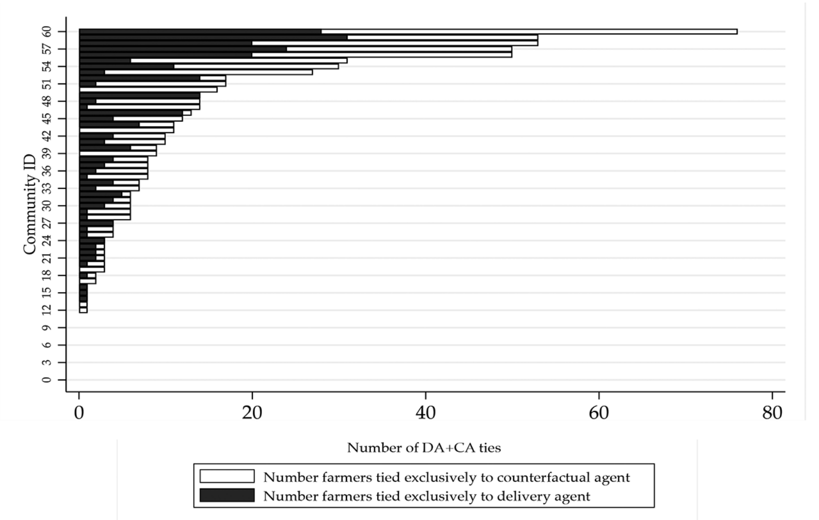

FIG. A1.—Variation in number of social ties. The black (white) histogram is the number of farmers in the community who know only the counterfactual (delivery) agent. Communities are sorted from the lowest to the highest number of farmers who know one of the two agents.

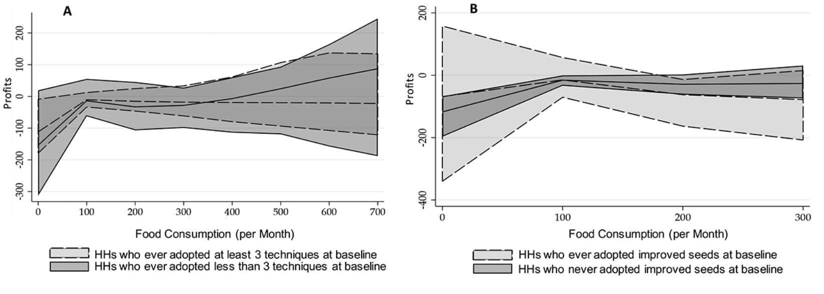

FIG. A2.—Consumption and agricultural profit. The plots present the nonparametric relationship (and the 95% confidence interval) between baseline profits and baseline expenditure on food consumption, for two categories of households (HHs): *A*, those that ever adopted at least three techniques at baseline versus those that adopted less than three techniques; *B*, those that ever adopted improved seeds at baseline versus those that did not.

TABLE A1
Balance on Village Characteristics

	Control (1)	Treated: Agriculture Extension Program (2)	p-Value (1) = (2) (3)
Number of villages	59	109	
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	103.9	[.671]
	(56.37)	(59.64)	
Average household wealth score (0–100)	61.91	62.01	[.709]
	(4.754)	(5.319)	
Standard deviation of household wealth score	12.95	12.94	[.851]
	(1.516)	(1.584)	

Note.—Means, with standard deviations in parentheses. Village-level summary statistics for control villages (col. 1) and treated villages (col. 2). The p-values (col. 3) are obtained from regressing each of the reported baseline variables 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 col. 1 and the distance from the treated village to the closest control village in col. 2. The household wealth score is measured for all households in our census survey by aggregating 10 poverty indicators into a score going from 0 to 100. Average household wealth score (0–100) and standard deviation of household wealth score calculate the average and the standard deviation of household's wealth score in the village.

TABLE A2
ATTRITION

	Agricultural Extension Program			Social Ties		
	No Covariates (1)	Covariates (2)	Covariates plus Their Interaction with Treatment (3)	No Covariates (4)	Covariates (5)	Covariates plus Their Interaction with Treatment (6)
Treated	.015	.017	−.017			
	(.011)	(.011)	(.072)			
Treated × exclusively tied to DA				.019	.032	.005
				(.023)	(.021)	(.178)
Treated × exclusively tied to CA				.015	.026*	.052
				(.015)	(.016)	(.133)
Mean dependent variable	.070	.070	.070	.070	.070	.070
p-value on interactions			[.324]			
p-value on interactions for exclusively tied to DA vs. CA						[.628]
Observations	4,741	3,555	3,555	4,303	3,216	3,216

NOTE.—OLS estimates, with standard errors in parentheses (clustered by community in cols. 1–3 and by community and ties in cols. 4–6). Dependent variable =1 if respondent attrited at endline. Table shows farmer-level OLS regressions. In cols. 1–3, we use the sample of households in the control and treated villages and cluster standard errors at the village level. In cols. 4–6, we also use the sample of households in the control and treated villages, but within treated villages we break down households into those tied to the DA only and those tied to the CA only, and use cluster standard errors at the community and ties level. All regressions control for branch fixed effects. Additionally, cols. 2 and 5 control for all household-level characteristics in table 1, col. 3 controls for all household-level characteristics in table 1 interacted with the treatment, and col. 6 controls for all household-level characteristics in table 1 interacted with tied to the DA and tied to the CA. Exclusively tied to DA (CA) equals 1 if the farmer knows only the DA (CA). At the foot of col. 3 we report the p-value from a joint test of significance of all interactions. At the foot of col. 6 we report the p-value from a joint test of significance of all interactions with tied to the DA vs. with tied to the CA.

* Significant at the 10% level.

TABLE A3

Aggregate Impacts on Intrahousehold Outcomes

	Household Members Get Involved in Agriculture and Work on Own Plots (1)	Shared Land Owned by Household Which Is Cultivated by the Female Household Head (2)
Treated village:		
Agricultural extension intervention	.016	.088***
	(.031)	(.022)
	{.271, .605}	{.001, .002}
Mean in control	.523	.589
Observations	4,230	4,100

Note.—ITT estimates, with standard errors in parentheses (clustered by village). The p-values adjusted for randomization inference and multiple hypothesis testing are in braces. Table shows household-level OLS regressions. All regressions control for branch fixed effects and for the baseline value of the outcome variable. In braces, we report randomization inference p-values computed following the Young (2019) approach, and p-values adjusted for multiple hypothesis testing computed using the Romano and Wolf (2016) step-down procedure, using 500 iterations. All monetary values are expressed in thousands of UGX and are truncated above and below 2 standard deviations from the mean. Exchange rate: 1 USD = 2,519.6 UGX (March 2014).

*** Significant at the 1% level.

TABLE A4
INTERACTION WITH THE MICROFINANCE PROGRAM

	Targeting			Other Sources and Techniques			Agriculture in Last Season		Consumption and Assets		
	Targeted by the Delivery-Agent: Received-Seeds or Training in Last Year	Received Seeds from the Delivery Agent in Last Year	Trained by the Delivery Agent in Last Year	Received Seeds from BRAC Branch Office in Last Year	Received Seeds from Non-BRAC Source in Last Year	Share of Techniques Adopted (Observed)	Profits in Last Season (000 UGX)	Number of Marketable Crops Grown in Last Season	Food Expenditure in Last Week (000 UGX)	Total Consumption in Last Month (000 UGX)	Productive Assets (000 UGX)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1) Agricultural extension intervention with microfinance	.032***	.026***	.030***	.048***	.018	.009	36.28**	.161	4.294	17.282	2.897
	(.008)	(.007)	(.007)	(.010)	(.018)	(.039)	(16.38)	(.124)	(3.535)	(15.187)	(1.944)
	{.001, .006}	{.001, .006}	{.001, .006}	{.001, .006}	{.065, .349}	{.666, .838}	{.001, .084}	{.003, .182}	{.011, .309}	{.015, .309}	{.127, .309}

(2) Agricultural extension intervention without microfinance	.047***	.037***	.044***	.039***	.020	−.001	31.00*	.296**	8.556**	36.871**	3.239*
	(.012)	(.011)	(.011)	(.008)	(.016)	(.032)	(16.72)	(.137)	(4.272)	(18.603)	(1.847)
	{.001, .012}	{.001, .018}	{.001, .016}	{.001, .012}	{.065, .188}	{.975, .976}	{.001, .080}	{.001, .064}	{.001, .116}	{.001, .116}	{.067, .116}
Mean in control	.001	.001	.000	.001	.094	.213	76.96	1.243	24.69	119	20.12
p-value (1) = (2)	[.252]	[.373]	[.279]	[.461]	[.855]	[.745]	[.762]	[.377]	[.380]	[.348]	[.886]
Observations	4,378	4,390	4,381	4,390	4,410	2,346	3,968	4,410	4,395	4,333	4,339

NOTE.—ITT estimates, with standard errors in parentheses (clustered by community). The p-values adjusted for randomization inference and multiple hypothesis testing are in braces. Table shows farmer-level OLS regressions. All regressions control for branch fixed effects and for the baseline value of the outcome variable. In braces, we report randomization inference p-values computed following the Young (2019) approach, and p-values adjusted for multiple hypothesis testing computed using the Romano and Wolf (2016) step-down procedure. Share of techniques adopted (col. 6) calculates the share of techniques used (out of six: intercropping, line sowing, zero tillage, proper weeding, crop rotation, avoid mixed cropping) as observed by the enumerators (by checking the plot of land of a random sample of households). Sample restricted to sample of households for whom plot of land was checked by the enumerator. Profits (col. 7) are the total output value minus total expenditure value in the last season. Number of marketable crops grown (col. 8) counts the number of vegetable, root, and fruit crops produced in the last season. Food expenditure in last week (col. 9) is the total household expenditure on food, beverages, and tobacco per week per adult equivalent. Total consumption (col. 10) in the last month includes food, durables, and semidurables. Productive assets (col. 11) is the total value of agriculture assets owned by the household. All monetary values are expressed in thousands of UGX and are truncated above and below 2 standard deviations from the mean. Exchange rate: 1 USD = 2,519.6 UGX (March 2014).

* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

TABLE A5

Targeting and Techniques

	Social Ties (1)	Friend or Family (2)	Discusses Agriculture (3)
Exclusively tied to DA	.071**	.060**	.032
	(.031)	(.029)	(.038)
Tied to both agents	−.030	.021	−.024
	(.026)	(.041)	(.031)
Tied to neither agent	.022	.033*	.012
	(.027)	(.018)	(.032)
Community fixed effects	Yes	Yes	Yes
Mean outcome, exclusively tied to CA	[.088]	[.030]	[.110]
p-Value: exclusively tied to DA = exclusively tied to both	[.012]	[.429]	[.148]
p-Value: exclusively tied to DA = exclusively tied to neither	[.182]	[.364]	[.581]
Observations	1,327	1,327	1,127

Note.—Dependent variable: share of techniques adopted (enumerator observed). OLS estimates, with standard errors in parentheses (clustered by community and ties). Table shows farmer-level OLS regressions. All regressions control for community fixed effects, the walking distance to the DA's home, and the walking distance to the CA's home. Exclusively tied to DA equals 1 if the farmer knows the DA only in col. 1, is a friend or family of the DA only in col. 2, or regularly discusses agriculture with the DA only in col. 3. The omitted group (exclusively tied to CA) is composed of farmers who are socially tied only to the CA.

* Significant at the 10% level.

** Significant at the 5% level.

TABLE A6
p-VALUE CORRECTIONS FOR RANDOMIZATION INFERENCE

	Social Ties (1)	Friend or Family (2)	Discusses Agriculture (3)	Religion (4)	Ethnicity (5)
			A. Targeting		
Exclusively tied to DA	{.000}	{.002}	{.002}	{.016}	{.361}
Community fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	2,421	2,421	2,087	2,420	2,413
			B. Propoor Targeting		
Exclusively tied to DA × poor	{.708}	{.401}	{.421}	{.007}	{.271}
Exclusively tied to DA × not poor	{.002}	{.002}	{.011}	{.132}	{.564}
Poor	{.001}	{.001}	{.000}	{.000}	{.001}
Community fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	2,421	2,421	2,087	2,420	2,413

NOTE.—Dependent variable: DA targets farmer (received seeds or training in last year). The *p*-values adjusted for randomization inference are in braces. Table shows farmer-level OLS regressions in both panels. In braces, we report randomization inference *p*-values computed following the Young (2019) approach using 500 iterations. 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 neither agent, the walking distance to the DA's home, and the walking distance to the CA's home. Exclusively tied to DA equals 1 if the farmer knows only the DA in col. 1, is a friend or family of the DA only in col. 2, regularly discusses agriculture with the DA only in col. 3, has the same religion as the DA in col. 4, or has the same ethnicity as the DA in col. 5. The omitted group (exclusively tied to CA) is composed of farmers who are socially tied only to the CA. Poor (not poor) equals 1 if the household belongs (does not belong) to the bottom quartile of the within-community distribution of food expenditure.

TABLE A7

Propoor Targeting: Robustness to Alternative Consumption Thresholds for Poverty

	Poor = Bottom 10% Food Consumption			Poor = Bottom 50% Food Consumption		
	Social Ties (1)	Friend or Family (2)	Discusses Agriculture (3)	Social Ties (4)	Friend or Family (5)	Discusses Agriculture (6)
Exclusively tied to DA × poor	−.026	−.086*	−.047	.018	.003	.015
	(.042)	(.045)	(.052)	(.017)	(.015)	(.020)
Exclusively tied to DA × not poor	.035**	.031***	.027*	.043***	.048***	.027*
	(.013)	(.011)	(.015)	(.013)	(.017)	(.014)
Poor	.044	.068	.059	.028**	.064***	.022
	(.038)	(.049)	(.048)	(.012)	(.020)	(.015)
Community fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome: poor and exclusively tied to CA	.032	.049	.039	.045	.068	.053
Mean outcome: not poor and exclusively tied to CA	.044	.057	.051	.041	.045	.046
p-Value: exclusively tied to DA × poor = exclusively tied to DA × not poor	[.138]	[.010]	[.154]	[.090]	[.045]	[.463]
Observations	2,421	2,421	2,129	2,421	2,421	2,129

Note.—Dependent variable: DA targets farmer (received seeds or training in last year). OLS estimates, with standard errors in parentheses (clustered by community and ties). Table shows 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 neither agent, the walking distance to the DA's home, and the walking distance to the CA's home. Exclusively tied to DA equals 1 if the farmer knows the DA only in col. 1, is a friend or family of the DA only in col. 2, or regularly discusses agriculture with the DA only in col. 3. The omitted group (exclusively tied to CA) is composed of farmers who are socially tied only to the CA. Poor (not poor) equals 1 if the household belongs (does not belong) to the bottom decile of the within-community distribution of food expenditure in cols. 1–3 and to the bottom half of the distribution in the rest of the columns.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

TABLE A8

Propoor Targeting: Robustness to Non-Consumption-Based Measures of Poverty

	Poor = Bottom 25% Wealth Index			Poor = Bottom 25% Asset Value			Poor = Never Adoption of Any Technique			Poor = Bottom 25% Agricultural Profits		
	Social Ties (1)	Friend or Family (2)	Discusses Agriculture (3)	Social Ties (4)	Friend or Family (5)	Discusses Agriculture (6)	Social Ties (7)	Friend or Family (8)	Discusses Agriculture (9)	Social Ties (10)	Friend or Family (11)	Discusses Agriculture (12)
Exclusively tied to DA × poor	.012	.005	.037**	.008	−.009	−.002	−.024	−.012	−.028	.031	−.019	−.001
	(.021)	(.019)	(.016)	(.018)	(.022)	(.023)	(.036)	(.042)	(.036)	(.022)	(.022)	(.032)
Exclusively tied to DA × not poor	.034**	.027**	.017	.037**	.033**	.030	.034**	.026**	.026	.029**	.034**	.028*
	(.014)	(.013)	(.017)	(.016)	(.013)	(.018)	(.015)	(.012)	(.017)	(.014)	(.014)	(.015)
Poor	−.000	−.012	−.034***	.016	.018	.025	.033	.009	.024	.007	.040	.038
	(.018)	(.023)	(.012)	(.017)	(.023)	(.023)	(.033)	(.039)	(.035)	(.017)	(.029)	(.027)
Community fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome: poor and exclusively tied to CA	.029	.030	.036	.039	.048	.040	.036	.048	.043	.052	.068	.058

TABLE A8 (*Continued*)

	Poor = Bottom 25% Wealth Index			Poor = Bottom 25% Asset Value			Poor = Never Adoption of Any Technique			Poor = Bottom 25% Agricultural Profits		
	Social Ties (1)	Friend or Family (2)	Discusses Agriculture (3)	Social Ties (4)	Friend or Family (5)	Discusses Agriculture (6)	Social Ties (7)	Friend or Family (8)	Discusses Agriculture (9)	Social Ties (10)	Friend or Family (11)	Discusses Agriculture (12)
Mean outcome: not poor and exclusively tied to CA	.047	.064	.053	.045	.059	.052	.044	.058	.050	.041	.053	.047
p-Value: exclusively tied to DA × poor = exclusively tied to DA × not poor	[.274]	[.332]	[.269]	[.181]	[.098]	[.254]	[.142]	[.366]	[.185]	[.934]	[.054]	[.345]
Observations	2,421	2,421	2,129	2,421	2,421	2,129	2,421	2,421	2,129	2,421	2,421	2,129

Note.—Dependent variable: DA targets farmer (received seeds or training in last year). OLS estimates, with standard errors in parentheses (clustered by community and ties). Table shows 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 neither agent, the walking distance to the DA's home, and the walking distance to the CA's home. Exclusively tied to DA equals 1 if the farmer knows the DA only in col. 1, is a friend or family of the DA only in col. 2, or regularly discusses agriculture with the DA only in col. 3. The omitted group (exclusively tied to CA) is composed of farmers who are socially tied only to the CA. Poor (not poor) equals 1 if the household belongs (does not belong) to the bottom quartile of the within-community distribution of the wealth index, total consumption, asset value, and an indicator for whether the farmer never adopted any technique at baseline.

* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

TABLE A9
PROFIT IMPACTS OF BEING TARGETED BY DAs OR RECEIVING SEEDS FROM ANY SOURCE

	Targeted by DA (1)	Received Seeds from Any Source (2)
Targeted farmer (DA) × poor	95.0	
	(62.7)	
Targeted farmer (DA) × not poor	42.4	
	(32.4)	
Targeted farmer (any source) × poor		46.7*
		(24.6)
Targeted farmer (any source) × not poor		43.0**
		(18.1)
Poor	2.34	3.25
	(7.87)	(7.94)
Mean in control	77	77
p-Value: treated × poor = treated × not poor	[.387]	[.899]
Observations	4,180	4,196

NOTE.—Dependent variable: profits in last season (thousands of UGX). Standard errors are in parentheses (clustered by village). Table shows farmer-level OLS regressions. All regressions control for branch fixed effects. Profits (thousands of UGX) are the total output value minus total expenditure value in the last season, truncated above and below 2 standard deviations from the mean. Targeted farmer (DA) is an indicator for whether the DA targets farmer (received seeds or training in last year at endline). Targeted farmer (any source) is an indicator for whether the farmer received seeds from any source (DA or other). Poor (not poor) equals 1 if the household belongs (does not belong) to the bottom quartile of the within-community distribution of food expenditure. Exchange rate: 1 USD = 2,519.6 UGX (March 2014).

* Significant at the 10% level.

** Significant at the 5% level.

TABLE A10
DIFFUSION AND INFORMAL TRANSFERS

	Diffusion: Received Seeds from Non-BRAC Source in Last Year (1)	Received Seeds from Any Source in Last Year (2)	Net Transfers (Extensive Margin) in Last Year (3)	Net Transfers (Intensive Margin) in Last Year (000 UGX) (4)
Exclusively tied to DA × poor	.005	−.003	−.045	−5.41
	(.034)	(.047)	(.074)	(10.57)
Exclusively tied to DA × not poor	.009	.042	.023	−1.129
	(.019)	(.038)	(.032)	(4.354)
Poor	−.016	−.033	.054	−.258
	(.023)	(.032)	(.054)	(6.435)
Community fixed effects	Yes	Yes	Yes	Yes
Mean outcome: exclusively tied to CA	.051	.132	.488	48.66
Mean outcome: poor and exclusively tied to CA	.035	.106	.553	51.98
Mean outcome: not poor and exclusively tied to CA	.056	.140	.469	47.67
p-Value: exclusively tied to DA × poor = exclusively tied to DA × not poor	[.919]	[.479]	[.395]	[.723]
Observations	2,448	2,448	2,448	2,364

NOTE.—OLS estimates, with standard errors in parentheses (clustered by community and ties). Table shows 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 neither agent, the walking distance to the DA's home, and the walking distance to the CA's home. The dependent variable in col. 1 equals 1 if the household received seeds from non-BRAC source (market, friend, etc.) in the last year. The dependent variable in col. 2 equals 1 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 transfers (intensive margin; thousands of UGX) is the total transfers received minus total transfers sent (gifts, alimony, scholarship, etc.) in the last year. Exclusively tied to DA equals 1 if the farmer knows only the DA. The omitted group (exclusively tied to CA) is composed of farmers who know only the CA. Poor (not poor) equals 1 if the household belongs (does not belong) to the bottom quartile of the within-community distribution of food expenditure.

TABLE A11
AGRICULTURAL OUTCOMES OF DAS

	Acres Cultivated		Daily Hours Worked in Agriculture		Agricultural Revenues		Agricultural Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of exclusive ties to DA	.034		−.008		−.016		.002	
	(.045)		(.020)		(.021)		(.034)	
Number of exclusive ties to DA and poor		.069		.014		.015		−.012
		(.070)		(.031)		(.033)		(.054)
Number of exclusive ties to DA and not poor		−.035		−.056		−.087		.026
		(.123)		(.056)		(.058)		(.095)
Mean outcome	−.301	−.301	.696	.696	.171	.171	.017	.017
R^2	.027	.040	.020	.035	.066	.107	.010	.024
Observations	60	60	60	60	60	60	60	60

NOTE.—Dependent variable: excess *y*-variable of the DA (actual minus predicted). OLS estimates, with robust standard errors in parentheses. Table shows community-level OLS regressions. All regressions control for community fixed effects. They also control for the number of poor ties to DA or CA, and the number of nonpoor ties to DA or CA. The excess *y*-variable growth of the DA is measured as the log of the difference between the actual *y*-variable of the DA at the endline and her predicted *y*-variable. The predicted *y*-variable is obtained by (1) regressing the endline *y*-variable of farmers in control villages who satisfy all criteria to become a DA on their baseline wealth, and (2) using the estimated coefficient to predict the DA's endline *y*-variable based on her baseline *y*-variable. In predicting the *y*-variable, we control for branch fixed effects, age, acres of land owned, number of marketable crops grown, and profits in agriculture (step 1). The number exclusively tied to DA is the number of sample farmers in the community who know the DA. Poor (not poor) indicates that a household belongs (does not belong) to the bottom quartile of the within-community distribution of food expenditure.

TABLE A12
Financial and Social Status of DAs

	Financial Status in Community (1 = lowest to 5 = highest)		Social Status in Community (1 = lowest to 5 = highest)		Had No Financial Problems Related to Agriculture	
	(1)	(2)	(3)	(4)	(5)	(6)
Number of exclusive ties to DA	.021		.003		.013	
	(.025)		(.033)		(.012)	
Number of exclusive ties to DA and poor		−.026		−.004		.006
		(.038)		(.040)		(.014)
Number of exclusive ties to DA and not poor		.130*		.020		.030
		(.074)		(.066)		(.022)
Mean outcome	2.47	2.47	2.46	2.46	.929	.929
R^2	.039	.074	.001	.002	.024	.030
Observations	57	57	56	56	56	56

Note.—Dependent variable: endline delivery agent financial/social status. OLS estimates, with robust standard errors in parentheses. Methodology: effect on endline DA characteristics (no possible comparison to counterfactual DAs in controls). Table shows community-level OLS regressions. The regression controls for community fixed effects, and the total number of ties to DA or CA. The outcome variable is reported by the DA at endline. Percent of time dedicated to DA job is the fraction of time that the DA reports dedicating to the DA job (vs. other jobs) at endline. Poor (not poor) indicates that a household belongs (does not belong) to the bottom quartile of the within-community distribution of food expenditure.
* Significant at the 10% level.

Data Availability

All data used in this project and the code replicating the article's results can be found in Bandiera et al. (2022b) in the Harvard Dataverse, https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/T2WBLK.

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