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Networks, incentives and technology adoption: evidence from a randomised experiment in Uganda

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Abstract

We use data from a randomised experiment in Uganda to examine effects of incentives on the decision to adopt drought-tolerant maize varieties (DTMVs) and mechanisms through which effects occur. We find that social recognition (SR) incentives to a random subset of trained farmers – disseminating farmers (DFs) – increase knowledge transmission from DFs to their co-villagers and change information networks of both DFs and their neighbours. SR also increases DFs' likelihood of adopting DTMVs. However, the corresponding results for private material rewards are not conclusively strong. We find no evidence that incentives for knowledge diffusion increase the likelihood of co-villagers adopting DTMVs.

Keywords: social networks, incentives, adoption, risk-mitigating technologies, Uganda

JEL classification: D83, D85, O33, Q16

1. Introduction

Modern agricultural technologies hold huge potential for improving productivity and reducing poverty in developing countries. However, many agricultural technologies with demonstrated productivity gains have not been adopted as widely as one hoped in these countries, particularly so in many sub-Saharan African countries where the aggregate technology adoption remains strikingly low (Duflo, Kremer and Robinson, 2011; Rashid *et al.*, 2013; Sheahan and Barrett, 2014). Substantial efforts and investments have been made to promote the adoption and diffusion of improved agricultural technologies. The role of disseminating farmers (DFs) as 'injection points' for new technologies in this process has long been emphasised (Lukuyu *et al.*, 2012a; Kiptot and Franzel,

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2015). Trainings and demonstrations about new agricultural technologies target these DFs with the expectation that they will disseminate the new information to their co-villagers (Krishnan and Patnam, 2013; Kondylis, Mueller and Zhu, 2017). The DFs' approach is championed for its cost-effectiveness and its room to account for local contexts – social and cultural aspects of communication (Anderson and Feder, 2004).

The role of social learning in networks inspires the DFs' approach. A body of literature exists on the process of social network formation and underlying incentives (Bala and Goyal, 2000; Goyal, van der Leij and Moraga-González, 2006; Fafchamps and Gubert, 2007; Santos and Barrett, 2010). Equally, the role of social networks in technology diffusion has been extensively documented in empirical studies.[1] Recent efforts studying network effects on technology adoption have focused on documenting the existence of automatic and passive social learning from a few starting points (seed nodes) to the larger target population using careful empirical strategies.[2] However, our understanding of how this actually happens is limited. Existing studies have largely taken networks as exogenous and do not address how existing networks respond to interventions, like training of a random node in the network (Breza, 2015). As a result, most studies do not indicate the underlying mechanisms through which information and technology dissemination takes place within social networks. Furthermore, the emphasis in the literature is often on settings in which seed nodes' effort to communicate to their neighbours about a new technology is voluntary. Social learning may be suboptimal, suggesting that training alone to DFs might not optimally influence knowledge and adoption decision of peers (Kondylis, Mueller, and Zhu, 2017; BenYishay and Mobarak, 2018). The relevant question from a policy perspective is then whether we can leverage social learning within networks to nudge optimal information sharing and adoption behaviour. Particularly, the role and nature of incentives in the dissemination of information and behaviour in social networks are not well understood. What type of incentives facilitates social learning in networks and ultimately improves new technology adoption is still an active area of enquiry.

This paper studies whether social learning and adoption of a new technology can be improved by providing incentives to DFs as injection points. A novel contribution is the analysis of impacts of different incentive schemes for prosocial tasks on DFs' and other farmers' networks, knowledge and adoption. A randomised controlled trial (RCT) is conducted in which we compare conventional training of representative farmers to incentivised training across 126 villages in northern Uganda. A random sample of households – the DFs – is invited to receive training on growing of new drought-tolerant maize varieties

1 Early contributions to the literature on social learning and technology adoption include Bikhchandani, Hirshleifer and Welch (1992, 1998)); Banerjee (1992); Ellison and Fudenberg (1993); Besley and Case (1994); Foster and Rosenzweig (1995); Bala and Goyal (1998); Udry and Conley (2001); Munshi (2004); Acemoglu *et al.* (2011).

2 See, for example, Kim *et al.* (2015) and Chami *et al.* (2018) for health-improving technologies; Cai, de Janvry and Sadoulet (2015) for insurance products; and Kondylis, Mueller and Zhu (2017) and Vasilaky and Leonard (2018) for agricultural technologies.

(DTMVs), which are increasingly seen as important interventions that can help boost yields, while reducing downside risk associated with droughts (Wossen *et al.,* 2017). A random subsample of the DFs then receives incentives to encourage them to expend effort to share information with their co-villagers. The experiment varies whether DFs receive a private material reward (PR) or social recognition (SR) for their effort to share the knowledge learnt.

Theoretically, several reasons can motivate why the effect of social networks on adoption of a new technology may be mediated by incentives. When a task is prosocial, meaning that its benefits are enjoyed by those other than the DFs themselves, incentives may encourage efforts to reach out to more co-villagers with information about a new technology (Ashraf, Bandiera, and Jack, 2014). Incentives may, therefore, increase the DF's degree – the number of people with whom information about a technology is discussed. Furthermore, incentives may induce DFs to experiment with and adopt the new technology themselves, producing a demonstration effect (BenYishay and Mobarak, 2018). Several studies have shown increased propensity of a neighbour to adopt a technology when his or her network comprises adopters (Bandiera and Rasul, 2006; Matuschke and Qaim, 2009; Conley and Udry, 2010; Krishnan and Patnam, 2013). In the presence of an adopter DF, social networks can influence adoption decision of neighbours by sending information about the adoption decision of the DF or through diffusion of knowledge (Cai, de Janvry, and Sadoulet, 2015).

Further, we examine the main mechanisms through which incentives' effects occur. Specifically, we endogenise networks and assess how incentives provided to DFs affect information networks as important conduits to new technology diffusion. Subsequently, we assess network effects on knowledge about and adoption of DTMVs. We observe (i) whether a trained DF i is mentioned among co-villager j's contacts for crop production advice and (ii) the frequency of interaction via the information exchange link. The former allows us to measure network effects at the extensive margin, whereas the latter captures the intensive margin. We then test whether social networks diffuse knowledge about DTMVs or transfer adoption decisions of DFs to co-villagers. Such understanding is important to identify strategies for nudging adoption of optimal behaviour and designing incentives for better communication within networks, with direct implication for the design of agricultural extension and training programs in developing countries. Overall, we aim to answer three main research questions: (i) Does providing incentives to DFs influence their information networks and adoption behaviour? (ii) Does incentivised training of DFs affect their co-villagers' information networks, knowledge and adoption of DTMVs? (iii) Do information networks in rural Uganda influence technology adoption?

Our design has different distinctive aspects that sharpen the analysis of the paper. The technology (i.e. DTMV) that we use in our experiment is a recently introduced one, and people in the study communities have not formed their own experiences, subjective opinions and beliefs about the technology that might play a confounding role. This is corroborated by quite low adoption and knowledge of farmers at the baseline. Our social network is uniquely defined.

We define the relevant social network as the farmers from whom the respondent seeks advice on crop production. These informational networks are further defined based on unidirectional links from DFs to the co-villagers, because information is more likely to flow from the DFs rather than in the opposite direction, especially since the technology is a new one and directly 'injected' into the DFs but not the co-villagers.

The main results of the paper are as follows. Incentives, both private material rewards and SR, increase the knowledge of co-villagers about DTMVs and change the networks of DFs and their co-villagers. However, evidence on the effects of incentives on adoption behaviour is weak. SR increases the likelihood of DFs' growing DTMVs, while private material rewards increase co-villagers' adoption of the technology. Social networks significantly influence co-villagers' knowledge, at the extensive and intensive margins, suggesting that networks do transfer information that confer better knowledge and understanding of DTMVs. However, controlling for endogeneity of social networks, we do not find evidence that having an adopter DF within a co-villager's contacts for crop production advice significantly influences his or her adoption decision.

This paper contributes to the existing literature on social networks in several important ways. First, it explicitly shows how different types of incentives influence knowledge and technology diffusion within social networks. Lack of incentives may explain why direct training of DFs might not improve knowledge and adoption behaviour of neighbours (Kondylis, Mueller, and Zhu, 2017). One notable recent effort is BenYishay and Mobarak (2018), who show that communicators' own adoption and effort are susceptible to small private incentives. However, BenYishay and Mobarak do not make a distinction between private and social incentives.

Second, the paper contributes to the small but growing literature examining how social networks change in response to an external stimulus. For example, Vasilaky and Leonard (2018) matched female farmers in Uganda and encouraged them to exchange agricultural information. They observed continued interaction between the matched pairs, which resulted in greater increases in crop yields compared to conventional government extension. Feigenberg, Field, and Pande (2013) randomised microfinance borrowers into either weekly or monthly group meetings and showed that borrowers randomised into the weekly group meetings continued interacting more even outside of group meetings, suggesting persistent changes in the strength of ties through microfinance. While these studies generally suggest that networks respond to external stimulus, they provide evidence in very specific contexts, where prosocial behaviour is limited. We provide evidence about how information networks change in response to an agricultural extension intervention that provided direct training to a random subset of seed nodes and in a setting where individuals are expected to engage in a prosocial task to disseminate agricultural information to their co-villagers.

Third, the paper also contributes to the literature studying the effects of social networks on adoption of agricultural technologies. Several studies have shown that a critical mass of adopters in a neighbour's existing network – suggesting passive learning – influences technology diffusion (Bandiera and Rasul, 2006; Matuschke and Qaim, 2009; Conley and Udry, 2010; Krishnan and Patnam, 2013). We study the effect of having a directly trained and adopter DF in a co-villager's network for crop production advice on the likelihood of adopting a technology. Moreover, previous literature does not distinguish between the roles of the extensive and intensive margins in social networks.

The paper is organised as follows. Section 2 provides background on the research context. Section 3 describes the experimental design and data collection. In Section 4, we examine the evidence for network change, improved knowledge and adoption. We then examine potential mechanisms through which the intervention may have affected outcomes. Section 5 concludes.

2. Context

The experiment was implemented in Nwoya district, northern Uganda, a predominantly agrarian region characterised by low agricultural productivity. The region's poverty level is the highest in the country – about 44 per cent of the population lives on less than one US dollar per day (Republic of Uganda, 2015). The region suffers from frequent weather shocks, including prolonged dry spells and uncertainty about the onset and cessation of rainfall (Mwongera *et al.,* 2014). Damage to agricultural output due to weather shocks amounted to more than 900 million US dollars in 2010, or 77 per cent of total damage across all sectors of the country's economy (Republic of Uganda, 2012, 2016). Although households tend to engage in off-farm activities, such as weeding neighbours' plots, brick making and small businesses, diversification to non-farm activities in rural parts of northern Uganda remains minimal due to limited employment opportunities outside agriculture.

Efforts to sustain agricultural production in the region have focused on promoting adoption of climate-smart agricultural (CSA) technologies. The government of Uganda has identified CSA as an effective means of addressing challenges related to weather shocks. However, farmers lack knowledge about CSA technologies and perceive this as a major constraint to widespread adoption (Shikuku *et al.,* 2015). Current efforts to restructure the extension system recognise the importance of working with DFs at the sub-county and village level to enhance dissemination of improved technologies (Ministry of Agriculture, Animal Industry, and Fisheries [MAAIF], 2016). Our study is part of these efforts, and we focus on the performance of DFs who are more or less 'representative' of the target population.

3. Experimental Design and Data

3.1. Experimental design

We first generated a list of 310 sub-villages, from which we randomly selected 132 sub-villages for our study. A census of all households and household heads was compiled for these selected sub-villages, and we randomly sampled 10 households from each sub-village. We then randomly picked one potential DF from this subsample and organised a meeting with co-villagers to discuss whether the thus selected candidate was 'not too different' (especially in terms of wealth and landholdings) from the rest of the village and potentially interested to experiment with new technologies. In more than 75 per cent of the cases, the first candidate was selected as a DF. In the other villages, we randomly picked another candidate and repeated the process. In one village, we had to go through three iterations until we selected a candidate that was endorsed by his or her co-villagers.

The 132 sub-villages were randomly assigned to one of three experimental arms of 44 sub-villages each: (i) training only ('conventional' control), (ii) training plus a private material reward (PR) and (iii) training plus SR.[3] Target farmers in the first experimental arm received training about DTMVs and were subsequently asked to share the information with their co-villagers. Target farmers in the second experimental arm received the same training but were informed after the training they could earn a private reward (PR). They were promised a weighing scale if they managed to share sufficient knowledge with their peers – to be established during a surprise visit at some unknown date in the future. They would earn the weighing scale in case the knowledge score of one randomly sampled co-villager exceeded a threshold[4]. They were told the reward was private, that the weighing scale was theirs to keep, and that they were free to decide how to use it. Disseminating farmers in the third experimental arm also received the training and were informed their community would receive a weighing scale if they managed to share sufficient knowledge with their peers – to be evaluated the same way as in the previous treatment arm. We announced that, in case of sufficient knowledge diffusion, there would be a public celebration during which the 'good performance' of the DF was publicly announced, and the weighing scale would be handed over to the village chief in the presence of other villagers.

Interventions were rolled-out in March 2016. We partnered with researchers from the National Agricultural Research Organisation (NARO) and Tillers International – an NGO working with NARO to promote conservation farming in Uganda. We provided a 3-day training session to the selected DFs. This training lasted 5 hours per training day.

The trainings were organised in central locations, and DFs were invited to travel to these sites. Training sessions were organised per sub-county, with

3 While a pure control group was not included because the prime objective was to study the impact of private and social incentives as compared to the standard training approach in extension systems, this has its own limitations. Impact estimates do not capture the full impacts of training with private material rewards and training with social recognition on experimentation and diffusion effort since the comparison group is not a pure control.

4 A weighing scale was worth UGX 60,000, equivalent to US$20. This was a valuable asset to rural farmers in Uganda as among other purposes, they could use it to weigh farm produce before selling to traders or other buyers hence reducing the likelihood of being cheated about quantity. In total 19 farmers in the second experimental arm and 28 farmers in the third experimental arm actually received the incentive.

11 farmers per session.[5] In each sub-county, DFs from different treatment arms were trained in separate venues to minimise contamination. The cost of transport to the training venue and back was refunded, (USD 4, on average) and tea and lunch were provided during the training. Of the 132 DFs who we invited, 126 attended the full training.

3.2. Data and summary statistics

Data were collected during two household survey waves. A detailed baseline survey was conducted between September and December 2015 covering 132 sub-villages. In every sub-village, the DF as well as nine randomly selected co-villagers was interviewed. In total we visited 1320 households and collected information on household demographics, crop and livestock production, off-farm income, assets ownership, exposure to weather shocks, sources of agricultural information, knowledge about farming practices and food security. The second survey wave was conducted in February–May 2017. During the follow-up survey, 126 sub-villages whose selected DFs had actually attended the training were revisited. Effort was made to interview the same respondents that had been interviewed at the baseline. In total, 1036 respondents (122 DFs and 914 other farmers) were interviewed in the follow-up survey. Of the 914 co-villagers, 694 respondents were interviewed at both baseline and endline. This shows that attrition is non-negligible, and we turn to addressing it below. We administered a similar questionnaire to that used at baseline.

Panel A in Table 1 presents summary statistics of selected household characteristics at baseline (2015) and endline (2017). Household heads were predominantly male. On average, a household head was 45 years old and had completed 6 years of formal education. The average household size was six. The main source of livelihood for most households was farming. Households cultivated on average one-half of a hectare under maize. Less than 3 per cent of the sample households, both at baseline and endline, had access to formal government extension.

In both survey waves, a specific module collected data on social networks (an example of the social network modules is available in Appendix D). Previous studies have defined social networks in different ways. For example, some earlier studies defined social networks as comprising the entire village (e.g. Besley and Case, 1994; Foster and Rosenzweig, 1995; Munshi, 2004). The advantage of using the village as the relevant social network is that many of a farmer's contacts would be captured. The limitation, however, is that many who are not in the farmer's contacts are also included (Maertens and Barrett, 2012). Therefore, other studies have recently elicited farmer network links directly (Maertens and Barrett, 2012; Krishnan and Patnam, 2013; Cai, de Janvry, and Sadoulet, 2015; Magnan *et al.*, 2015). Respondents are asked about

5 A sub-county is the second administrative unit in Uganda, after the district. At the time of the study, Nwoya district had four sub-counties including Anaka, Alero, Purongo and Koch Goma. Below the sub-county there are parishes, villages, sub-villages and households.

Table 1. Summary statistics

	2015 (baseline)		2017 (endline)	
	Mean	SD	Mean	SD
Panel A: Household characteristics				
Sex of the household head (1 = male, 0 = female)	0.815	0.388	0.806	0.396
Age of the household head (years)	44.610	15.189	44.510	14.883
Household size (number of resident members)	5.789	2.374	6.346	2.623
Main activity of household head is farming (1 = yes, 0 = no)	0.913	0.282	0.954	0.210
Education of household head (years)	5.621	3.360	5.593	3.390
Area of maize production (hectares)	0.451	0.883	0.477	0.757
Received credit (1 = yes, 0 = no)	0.683	0.466	0.526	0.500
Own a radio (1 = yes, 0 = no)	0.505	0.500	0.525	0.500
Own a phone (1 = yes, 0 = no)	0.535	0.499	0.566	0.496
Received advice from government extension (1 = yes, 0 = no)	0.025	0.157	0.010	0.099
Panel B: Social networks				
Mentioned a DF as contact (1 = yes, 0 = no)	0.014	0.119	0.159	0.366
Mentioned DF is an adopter (1 = yes, 0 = no)	0.000	0.000	0.075	0.264
Frequency of interaction with DF (0 = no interaction, 1 = rarely, 2 = at least monthly, 3 = daily)	0.023	0.216	0.352	0.889
Neighbour's out-degree (size of information network)	0.767	1.169	2.014	1.109
Risk-sharing network (1 = DF is a member)	0.073	0.260	0.098	0.298
Weak ties (1 = household has a second-order link)	0.004	0.066	0.050	0.217
Other information network (1 = DF is a member)	0.020	0.140	0.024	0.154
Panel C: Knowledge and adoption				
Knowledge about DTMVs (score)	3.340	1.831	2.485	2.783
Adopt DTMV – Longe maize (1 = yes, 0 = no)	0.122	0.327	0.165	0.371
Observations	905		905	

the people with whom they interact for a specific purpose, such as information exchange, risk sharing and friendship. Once each individual's connections are determined, links can be classified as unidirectional (i is in j's network if j claims i), bidirectional (i is in j's network if j claims i or i claims j) or reciprocal (i is in j's network if j claims i and i claims j).

We elicit network links directly along several dimensions: names of individuals from whom the respondent gets advice about crop production, those to whom the respondent gives advice about crop production, those from whom the respondent gets advice about livestock production, those to whom the respondent gives advice about livestock production, those from whom the respondent would borrow money, those to whom the respondent would lend money, those from whom the respondent would borrow material goods (e.g. kerosene and salt), those to whom the respondent would lend material goods, those who visit the respondent's home regularly, those whose homes the respondent visits regularly, relatives in the village, nonrelatives with whom the respondent socialises, those from whom the respondent receives medical advice, those to whom the respondent would go if hit with a disaster, those considered as neighbours and those with whom they belong to the same farmers' group.

We required the respondent to mention a fixed number of names (i.e. five names) in a specific network type. The advantage of this approach is that it helps respondents to understand what is required of them and to consider only very relevant nodes of their specific network (Newman, 2010). The drawback is that imposing a threshold limits the out-degree – the number of people nominated by the respondent (Cai, de Janvry, and Sadoulet, 2015). Our pilot study before the survey, however, showed that none of the respondents named more than five people for all networks when the number was not limited. Similar results were also observed at the baseline, with the average household effectively consulting only another partner (Table 1).

In principle, we produce six types of household-level social network measures for our main analysis. The first measure is a dummy variable equal to one if the household mentions a trained DF among its network of crop production advice and zero if otherwise. The second social network variable is based on the intensity of the link between households (Granovetter, 1973) and measures the frequency of interaction of a household with the trained DFs through a bilateral link. This measure ranges from 0 (no interaction at all between the neighbour and a DF) to 3 (daily interaction with the DF). We use unidirectional links because information is more likely to flow from the DF to the farmer claiming him or her (Magnan *et al.,* 2015). The third measure captures weak ties and is defined as a dummy variable equal to 1 if a household is connected to the DF for agricultural advice through second-order links and 0 if otherwise. A second-order linked household is one that is named as a contact by a given household's neighbour if that neighbour is linked to the DF (Cai, de Janvry, and Sadoulet, 2015).[6] The fourth measure is the co-villager's out-degree – a structural characteristic of the social network defined as the number of listed agricultural advice contacts for a household. We, however, acknowledge that the training might have induced more discussions (and hence greater network

6 The relevant information-sharing networks are defined as farmers from whom the respondent seeks advice on crop production, and further operationalised based on unidirectional links from DFs to the co-villagers, because the technology is relatively new and we believe that relevant information is more likely to flow from DFs (point of 'injection' for the new information) rather than from co-villagers to DFs. Therefore, our measurement of networks does not confirm to the standard and the full-spectrum definition of networks in the literature. We primarily focus on 'strong ties' because the information is relatively new and the time between the introduction of the information and our measurement is relatively shorter for weak ties to play a key role in diffusion of the new information. We do not claim that we mapped out the full network of individuals. On the other hand, we recognise the non-trivial importance of weak ties in several social networks and examine how the likelihood of information diffusion by weak ties is affected by incentives.

size) as people might have become more curious to discuss topics covered in the training, even with less intention of using the information immediately. This would be true if people had contacted others to clarify some doubts and questions following the training. The fifth social network variable is based on membership to financial or risk-sharing neighbourhood. Two farmers – the DF and the co-villager – belong to the same financial or risk-sharing neighbourhood if they lend to, borrow from or exchange material goods in common with each other at any point during the 2-year survey period. The sixth variable captures non-crop production advice networks and is based on co-membership of a DF and a co-villager to networks for medical or livestock advice. Our main analysis relies on the first four measures, while we employ the last two network measures for a sensitivity analysis. Baseline household social network variables are reported in panel B of Table 1.

Panel C of Table 1 gives our main outcome variables. Knowledge is measured as a sum of correct responses on a 10-question knowledge exam. The details of the questions are presented in the appendix. The questions included general awareness of improved varieties, names of improved varieties of maize and the benefits of growing improved varieties of maize. Questions included in the knowledge exam allowed for multiple responses/choices. Farmers were free to mention as many correct answers as they thought. All response categories were considered in the calculation of the knowledge score. Each response was, however, converted to a binary variable for inclusion in the calculation of the knowledge score. All questions carried the same weight. However, because DTMVs are adopted as a package of technologies, the training covered additional aspects related to intercropping with beans and groundnuts, correct sowing depth and spacing when intercropping and the number of seeds to be planted in a hole. Adoption is a dummy variable equal to 1 if a household grew a DTMV on any of its farming plots between the baseline and the endline.

Only 1.4 per cent of the households mentioned a DF among its contacts for agricultural advice at baseline compared to 15.9 per cent at endline. The average out-degree for neighbours was 0.77 at baseline and 2.01 at endline. Whereas there was no adopter DF at baseline, 7.5 per cent of the sample households reported having an adopter DF in their contacts at endline. The frequency of interaction between a mentioned DF and the neighbour was 0.33 points higher at endline compared to the baseline (0.02). Only 7 and 10 per cent of the sample households mentioned a DF as a contact for risk sharing at baseline and endline, respectively. The proportion of farmers connected via second-order links to the DFs was only 0.4 per cent at baseline. This increased to 5 per cent at endline. The proportion of farmers who are linked to the DFs for information other than crop production advice was 2 per cent at baseline; this number did not change much at endline.

3.3. Attrition, balance and spillovers

Unfortunately, attrition in our sample is considerable as outlined above. Six out of the selected 132 DFs did not attend the training. This means that 6 sub-villages representing 60 households or 4.5 per cent of the original total sample dropped from the study. Attrition as a consequence of DFs not attending training was not concentrated in a particular treatment arm. Because DFs were only informed about the incentives (for those in the material reward and SR groups) at the end of the training, attrition ought not to be related to treatment assignment. Four more DFs (0.3 per cent of the total sample) were not available for interviews during the endline survey: two had separated with their husbands and we could not track them; one had migrated to the neighbouring Gulu town; and another one had been hospitalised. These four DFs were not concentrated in one experimental arm once again.

Finally, we were unable to administer the endline survey to some households (220 households, or about 17 per cent of the original sample), as these farmers were absent even after three callback visits. We have no particular reason to believe that potential causes of attrition are systematically linked to specific treatments (something that is confirmed by the data). Attrition rates are rather equal across the three experimental arms. High attrition is potentially problematic, as it could introduce selection bias in our randomised experiment.

We examine the implication of this attrition for our results in several ways. First, we test whether our remaining sample is (still) balanced along key observable dimensions – 18 variables in total. Using the 'orth_out' command in STATA, pretreatment covariates are regressed on treatment dummies: an F-test that all treatment arm coefficients equal zero failed to reject existence of balance. In addition, we perform randomisation checks comparing each treatment arm to the other. As presented in Table 2 (for a selection of the variables), there was pretreatment balance across the randomly assigned groups for all but three variables, namely, age and education of the household head and household income. But, even for the three variables, differences are small: education of the household head is 6.32 in the conventional control group and 5.80 in the private material reward group; age of the household is 44.79 in the private material reward group and 42.30 in the SR group.

The second approach is to explain attrition with observable household characteristics. Appendix Table A2 presents the results of a probit regression where we regress attrition status on the treatment dummies and household characteristics. As shown, treatment assignment is not correlated with attrition. An F-test (p-value $= 0.632$) rejected the null hypothesis that the treatment dummies jointly influenced attrition. Most of the other variables are not correlated with our attrition dummy, while those that showed a significant correlation were mostly not significantly different across our three groups. Nevertheless, we cannot completely rule out the possibility that external validity of the impact analysis might be compromised by non-random attrition. For example, when attrition is based on unobservables like ability, we could perhaps systematically over- or underestimate the effect of incentives.

As a third approach, therefore, we attempt to control for potential selection concerns by a weighting procedure as a robustness analysis (Gerber and Green, 2012; Bulte *et al.,* 2014). Specifically, we follow a two-stage procedure. In the

Table 2. Randomisation balance on observables at baseline

	Whole sample (1)	Training only (2)	Private reward (PR) (3)	Social recognition (SR) (4)	F-test (p-value) (5)	p-value (2) = (3) (6)	p-value (2) = (4) (7)	p-value (3) = (4) (8)
Household head is male	0.82	0.84	0.80	0.82	0.528	0.261	0.629	0.482
Age of household head	43.49	43.41	44.79	42.30	0.105	0.246	0.328	0.035**
Household size	5.85	5.68	5.93	5.93	0.320	0.195	0.170	0.991
Dependency ratio	0.55	0.55	0.54	0.54	0.846	0.664	0.591	0.924
Education of household head	5.99	6.32	5.80	5.86	0.203	0.094*	0.155	0.847
Main activity is farming	0.91	0.89	0.92	0.92	0.553	0.300	0.367	0.837
Household income per AE	467,504.00	471,500.00	425,300.00	505,334.00	0.172	0.270	0.483	0.072*
Participation in casual job	0.29	0.31	0.29	0.28	0.529	0.497	0.269	0.644
Participation in self-employment	0.10	0.10	0.10	0.11	0.493	0.802	0.250	0.433
Credit amount received (UGX)	86,000.00	82,200.00	82,700.00	92,800.00	0.738	0.975	0.477	0.549
Agricultural assets index	0.01	0.15	−0.05	−0.06	0.690	0.570	0.409	0.965
Non-agricultural assets index	0.08	0.14	−0.08	0.18	0.731	0.586	0.910	0.444
Contacts for crop advice	2.00	2.00	2.00	2.00	0.885	0.781	0.629	0.806
Kinship network	1.73	1.76	1.70	1.73	0.841	0.559	0.839	0.759
Experienced floods	0.03	0.04	0.02	0.02	0.301	0.127	0.509	0.412
Experienced droughts	0.68	0.66	0.66	0.73	0.346	0.354	0.773	0.154
Variability in rainfall during study period (CoV in rainfall)	0.25	0.25	0.25	0.25	0.964	0.690	0.644	0.882
Mean monthly rainfall (mm)	140.39	139.16	141.03	140.91	0.192	0.151	0.928	0.284
Distance to nearest agro-dealer shop (km)	11.67	11.30	12.62	11.03	0.830	0.616	0.883	0.546
Distance to market (minutes)	43.90	40.03	46.97	44.34	0.274	0.134	0.265	0.571
Observations	1025	330	345	350				

*$p < 0.1$.
**$p < 0.05$.

first stage, a logit regression is used to estimate the predicted probability of having non-missing measures for our outcomes given treatment assignment and a vector of observable covariates (see Table 2). In the second stage, we weight each observation using the inverse of the thus estimated probability of having a non-missing measure of our outcomes. Our main results remain robust to all these robustness tests.

Random assignment to treatment implies that identification is satisfied, unless there are substantial spillover effects (so that the SUTVA is violated). This might happen if DFs in the conventional training group changed their behaviour as a result of knowing that others had been offered rewards. Yet, spillover effects are shown to not pose a serious threat to our identification effort. First, several design features help to minimise this risk: (i) we selected only one DF from each sub-village, and hence there was only one treatment per sub-village;[7] (ii) DFs attended the training with others who were assigned to the same experimental arm (even if this was not announced to the DFs before the training); (iii) training sessions for different treatment arms were organised at different venues; and (iv) sub-villages in northern Uganda, specifically in Nwoya district, are geographically dispersed.

Second, we formally test for evidence of spillovers across neighbouring sub-villages using Global Positioning System (GPS) coordinates of the DFs. We test whether the presence of a DF from another experimental arm in a neighbouring sub-village affects information networks of the neighbour. Figure B1 in the appendix (top panel) shows graphically the random assignment of treatments, whereas the lower panel shows sub-villages receiving different treatments but neighbouring each other. To check whether information networks of the control group neighbours were affected by spillovers, we compare information networks of control group neighbours who are close to a treated neighbour and control group neighbours further away from treated units. According to our estimates, summarised in Table A3 in the appendix, there are no spillovers. Using a border-to-treatment dummy variable, a *t*-test also indicates that control group neighbours' information networks were not significantly affected by the presence of a neighbour from another experimental arm.

4. Results

4.1. Effects of incentives on adoption of DTMVs

Incentives can matter for agricultural technology diffusion for different reasons. In Appendix E, we provide a simple theoretical framework to formally organise these arguments. DFs may be motivated to communicate about the new technology and experiment with the technology to achieve the critical mass of peer farmers who know about the new technology to ensure getting the

7 Only one of our DFs migrated after the training, and none moved to another sub-village with a different treatment.

reward. Alternatively, neighbours, including those who were not in the DFs' networks at the baseline, may realise that DFs become potentially important nodes as a source of information about a highly relevant new technology in the context of rural Uganda and actively seek to be connected with DFs.

We first estimate Equation (1) using the sample of DFs to assess the effect of incentives on their decisions to grow DTMVs:

$$y_{ivc} = \beta_0 + \beta_1 \text{private}_{ivc} + \beta_2 \text{social}_{ivc} + \gamma_i X_{ivc} + C_c + \varepsilon_{ivc} \tag{1}$$

where y_{ivc} indicates whether a DF i in sub-village v and sub-county c grew a DTMV or not; private_{ivc} and social_{ivc} are dummy treatment variables: private_{ivc} is equal to 1 if the DF was randomly assigned to receive a private material reward and 0 if otherwise and social_{ivc} is equal to 1 if the DF was randomly assigned to receive SR and 0 if otherwise. X_{ivc} includes farmer and household characteristics, and C_c captures sub-county fixed effects. Our selection of the covariates X_{ivc} was guided by literature on adoption analysis and the role of incentives in technology diffusion (e.g. BenYishay and Mobarak, 2018). Generally, the literature on adoption considers farmer and farm characteristics; human, social and financial capital; and institutional factors as important determinants of agricultural technology adoption. We estimate Equation (1) using probit and report robust standard errors clustered at the sub-village level. The coefficients β_1 and β_2 in Equation (1) measure the causal effect of incentive treatments on the DFs' adoption decisions, under the identifying assumption that private_{ivc} and social_{ivc} are orthogonal to ε_{ivc}. Results in Table 3, columns 3 and 4, show that SR increased the likelihood of a DF adopting a DTMV by 14 percentage points relative to conventional training. The effect was significantly larger than that of PRs (2.5 percentage points). That incentives affect adoption decisions of DFs is consistent with the findings of BenYishay and Mobarak (2018), who examined the responsiveness of DFs – selected using a criterion similar to the one used in the current study – to incentives for technology diffusion. These authors showed that material rewards motivate DFs to experiment with new technologies. In our context, however, we distinguish between PRs and SR and find that only the latter significantly influenced adoption decision of DFs.

Next, we test incentive effects on knowledge and adoption of DTMVs by DFs' co-villagers (neighbours). Formally, we estimate Equation (1) using the sample of co-villagers. In this estimation, y_{ivc} measures (i) knowledge scores of co-villagers and (ii) a dummy variable equal to 1 if a co-villager i in sub-village v and sub-county c grew a DTMV and 0 if otherwise. Results reported in columns 5 and 6 indicate a significantly positive effect of SR on co-villagers' knowledge. The effect of private material rewards on co-villagers' knowledge is significant, but only when we control for additional covariates (column 6). Knowledge score increased 0.77 points higher for SR (p-value = 0.005) and 0.44 for private material rewards (p-value = 0.093). Without controlling for additional covariates, the effect of both incentive types on adoption of DTMVs by co-villagers is not statistically significant. After controlling for additional

Table 3. Incentive effect on adoption and knowledge

	DFs' knowledge		DFs' adoption		Co-villagers' knowledge		Co-villagers' adoption	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Training plus private reward (PR)	−0.168	−0.118	0.015	0.025	0.209	0.436*	0.039	0.064*
	(0.236)	(0.231)	(0.071)	(0.073)	(0.262)	(0.275)	(0.039)	(0.034)
Training plus social recognition (SR)	−0.089	−0.064	0.140**	0.136**	0.629***	0.769***	0.015	0.042
	(0.225)	(0.231)	(0.058)	(0.057)	(0.245)	(0.270)	(0.035)	(0.034)
Household controls	No	Yes	No	Yes	No	Yes	No	Yes
Sub-county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.028	0.037	0.263	0.340	0.024	0.209	0.005	0.140
Observations	123	123	123	123	694	694	694	694
Mean of dependent variable for control group	0.054	0.054	0.024	0.025	2.320	2.320	0.162	0.162
	(0.071)	(0.071)	(0.154)	(0.158)	(2.672)	(2.672)	(0.369)	(0.369)
PR = SR (*p*-value)	0.716	0.814	0.026	0.067	0.116	0.171	0.484	0.468

Notes. Dependent variables are as follows: columns 1–2 measure the knowledge scores of co-villagers; columns 3–4 are a dummy variable equal to 1 if DF tried out the technology on at least one of the household's plots and 0 otherwise; columns 5–6 measure the knowledge scores of co-villagers; columns 7–8 are a dummy variable equal to 1 if co-villager tried out the technology on at least one of the household's plots and 0 otherwise. Robust standard errors corrected for sub-village level clustering are reported in parentheses. Square parentheses are the standard deviations of the control group means. Baseline household controls include sex, age and education of household head; agriculture as the main source of livelihood of the household head; size of land cultivated with maize; size of land cultivated with groundnuts; ownership of radio, bicycle and solar panel; access to information from a neighbour and friend; and agricultural assets index. In the probit regression for adoption analysis, we include rainfall and temperature variables measuring coefficient of variation in rainfall based on historical georeferenced rainfall data during the survey period and self-reported perceptions about frequent drought and less rainfall. We also control for characteristics of the DFs including sex, education, age, dependency ratio, access to credit and area under maize. Columns 1, 2, 5 and 6 report OLS estimates. Columns 3, 4, 7 and 8 report average marginal effects from probit regression.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

covariates, we find a 6.4 percentage point increase in the likelihood of adopting DTMVs for the private material rewards (p-value = 0.063).

Results in Table 3 need a special attention. PRs do not lead to DFs' adoption. But the effect estimates of PRs on neighbours' knowledge and adoption are shaky. They are weakly significant only when covariates are included in the regression models. SR leads to adoption by DFs and knowledge by neighbours (both precisely estimated) but no adoption by neighbours. One possible explanation is that SR may motivate DFs to react more strongly to incentives. If this is true, DFs in this treatment arm not only expend communication effort but also adopt the technology themselves to produce a demonstration effect for fellow villagers. Concurrently, our results show that DFs in the SR treatment arm are more likely to adopt the new technology. An alternative explanation is that the public good (the scale to be given for the village based on the performance of the village DF) might have created incentives for neighbours to learn about the technology (in order to get the scale that was to be shared with everyone in the village) even if they did not use that knowledge immediately through adoption, possibly because of the short time between training and adoption.[8] This suggests that incentivising farmers by offering a public good would induce more interest for learning, and this provides relevant insight to a wider literature on how to encourage learning. The implication is that programs and policies would benefit more by taking into account other barriers to agricultural technology adoption beyond incentives for knowledge transfer. Having said this, we would like to warn readers that, though meaningfully plausible, this later explanation remains speculative, since we do not have data on the strategies DFs use to communicate the information to fellow villagers, particularly how DFs communicate the incentive rewards to other farmers.

4.2. Identifying mechanisms for the effects of incentives

How do incentives operate? We hypothesise that incentives changed the social networks of DFs and their co-villagers. Subsequently, and consistent with previous studies, we identify two main channels through which social networks may influence the adoption of a new technology: (i) co-villagers may gain knowledge about the availability and benefits of the technology (e.g. Conley and Udry, 2010), and (ii) people may be influenced by the decisions of others (e.g. Bandiera and Rasul, 2006; Matuschke and Qaim, 2009). We, therefore, ask: (i) suppose that incentives change networks of trained DFs and the co-villagers, and that (ii) DFs respond to incentives by adopting DTMVs, do networks transfer adoption decision of the DFs or help to diffuse knowledge or both?

To assess the effect of incentives on the social networks of the DFs, we estimate Equation (1) where y_{ivc} now measures the DFs' in-degree – the number of neighbours with who the DF shared crop production advice. To test the effect

8 We thank an anonymous referee for suggesting this insight.

of incentives on the networks of co-villagers, we causally estimate the intent-to-treat (ITT) effects of a sub-village being assigned to incentivised DF training (relative to conventional DF training) using Equation (2):

$$\text{Network}_{ivc} = \alpha_0 + \alpha_1 \text{private}_{ivc} + \alpha_2 \text{social}_{ivc} + \delta_i X_{ivc} + C_c + \varepsilon_{ivc} \qquad (2)$$

where the outcomes of interest Network_{ivc} in Equation (2) measure whether or not a DF is mentioned in a neighbour's contacts for crop production advice, the frequency of interaction between a DF and his or her co-villagers and a weak tie defined as an indirect link between a DF and a neighbour through another co-villager. The rest of the variables are as defined in Equation (1).

Results in Table 4 show that both private material reward and SR increase the likelihood of mentioning DFs as contacts for crop production advice (col. 2). Specifically, the probability of a neighbour mentioning a DF as a contact for crop production advice increased by 8 percentage points more, for both private material reward and SR, compared with conventional training of DFs. The corresponding increase in the intensity of information exchange link was 0.67 points more for SR and 0.65 points more for private material rewards (col. 3).[9] Neither PRs nor SR significantly influenced formation of weak ties (col. 4). Throughout, we find that the effect of PR and SR on networks is not statistically different.

Next, we assess the influence of social networks on knowledge of co-villagers, by estimating:

$$\text{Knowledge}_{ivc} = \theta_0 + \theta_1 \text{Network}_{ivc} + \vartheta_i X_{ivc} + C_c + \varepsilon_{ivc} \qquad (3)$$

At the extensive margin, Network_{ivc} measures whether a DF is mentioned among a co-villager's contacts for crop production advice, whereas at the intensive margin, the variable measures the frequent interaction of co-villagers with DFs. To control for endogeneity of Network_{ivc}, we exploit the exogenous variation generated by the random assignment of incentive treatments to

9 The relati onship between village chiefs and DFs may affect DFs' incentives to share information about DTMVs as the weight scales are given to the village chiefs. Our criteria for selecting the DFs excluded village chiefs. We also avoided selecting DFs who were kins with the village chief. We cannot, however, rule out completely that the selected DFs may still have some other indirect relationship with village chiefs. In that case, there may be an incentive effect other than social recognition in the SR group if DFs expect that they would finally get the scale once we hand it over to the village chief. We went back to the field in May 2018 to collect additional data on how the weighing scales were being used. We found that in both groups, the weighing scales still existed and were in a working condition. In the private arm, the DFs mostly used the weighing scales for weighing their own produce (mainly maize), rarely allowing others to access it – in very few cases, access was allowed to close relatives and neighbours. Whereas relatives did not pay, neighbours were typically charged a small fee for using the scale. In the social recognition arm, we found that the village chiefs were still in charge of keeping and maintaining the weighing scales – ruling out the possibility that the weighing scale ended up with the DFs. Second, co-villagers were allowed to access the weighing scale at no fee, but with strict instructions to handle the scale with care. Finally, we found that there were a few other individuals – in both the private and social recognition arms – who owned weighing scales. For these privately owned weighing scales, access by co-villagers was limited.

Table 4. Incentive effects on co-villagers' social networks

	DF's in-degree (1)	DF is mentioned (2)	Frequency of interaction (3)	Weak link (4)
Private reward (PR)	0.689**	0.086*	0.669**	0.008
	(0.282)	(0.044)	(0.296)	(0.021)
Social recognition (SR)	0.908***	0.085*	0.649**	−0.002
	(0.300)	(0.047)	(0.313)	(0.023)
Household controls	Yes	Yes	Yes	Yes
Sub-county effects	Yes	Yes	Yes	Yes
Constant			−2.066***	
			(0.713)	
Observations	123	694	694	694
R-squared	0.169	0.052		0.095
Control group mean	1.225	0.122	0.234	0.045
	[1.050]	[0.328]	[0.718]	[0.207]
p-value: PR = SR	0.752	0.978	0.946	0.580

Notes. Column 1 is OLS regression estimates; columns 2 and 4 are average marginal effects from probit regression; column 3 is Poisson regression estimates. Dependent variables are as follows: column 1 measures number of co-villagers mentioning a DF as contact for crop production advice; column 2 is a binary variable equal to 1 if a DF is mentioned among the co-villager's contacts for crop production advice, 0 if otherwise; column 3 measures the frequency of interaction between a neighbour and the mentioned DF; column 4 is a binary variable equal to 1 if a DF is not linked to the neighbour directly for crop production advice but through another co-villager, 0 if otherwise. In parentheses are robust standard errors clustered at the sub-village level. In square parentheses are the standard deviations for the control group. Control group comprises households in sub-villages in which a DF was not incentivised. Baseline household controls include sex, age and education of household head; agriculture as the main source of livelihood of the household head; size of land cultivated with maize; size of land cultivated with groundnuts; ownership of radio, bicycle and solar panel; access to information from a neighbour and friend; and agricultural assets index.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

estimate an instrumental variable regression. OLS and IV estimation results are reported in Table 5, columns 1 and 2. They show that social networks improved the knowledge scores of co-villagers both at the extensive and intensive margins. These results suggest that networks do transfer information that confer better knowledge and understanding of DTMVs. The findings that social networks increased knowledge of co-villagers are consistent with those found in literature (e.g. Vasilaky and Leonard, 2018).

Throughout, the effects of the intensive margin are generally found to be smaller than the extensive margin. This is a bit contrary to our expectation. Our measure of intensive margin captures whether the DFs and their co-villagers never interacted or interacted daily, at least weekly, at least monthly or less often. First – with a caveat that the current study cannot definitively explain this effect – we speculate that if neighbours who interacted with their DFs at the extensive margin discussed for longer hours within one visit, it is plausible that the effect could be greater compared to less hours of interaction during

Table 5. Effect of information networks on co-villagers' knowledge

	Extensive		Intensive	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Mentioned DF as contact for advice	2.200***	7.208*	0.799***	2.620**
	(0.280)	(3.713)	(0.111)	(1.308)
Household controls	Yes	Yes	Yes	Yes
Sub-county effects	Yes	Yes	Yes	Yes
Constant	1.796***	1.342	1.898	1.674
	(0.592)	(0.783)	(0.601)	(0.713)
R-squared	0.280	0.367	0.262	0.407
Observations	694	694	694	694

Notes. In parentheses are robust standard errors clustered at the sub-village level. *Extensive* (columns 1 and 2) indicates that an adopter DF is mentioned in the neighbour's network, whereas *Intensive* (columns 3 and 4) indicates the frequency of interaction with an adopter DF. Baseline household controls include sex, age and education of household head; agriculture as the main source of livelihood of the household head; size of land cultivated with maize; size of land cultivated with groundnuts; ownership of radio, bicycle and solar panel; access to information from a neighbour and friend; and agricultural assets index.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

several period of discussion at the intensive margin. Second, while our network questions about the frequency of interactions were very specific and clearly intended to reflect only information about crop production, social networks in developing countries are multitasked so that (some) interactions might also capture other aspects. Future research can benefit from explicitly addressing these caveats.

In order to test whether social networks transfer adoption decisions of the DFs, we estimate Equation (4) via instrumental variable approach using the randomised incentive treatments as instruments:

$$\text{Adopt}_{ivc} = \varphi_0 + \varphi_1 \text{DFadopt}_{ivc} + \rho_i X_{ivc} + C_c + \varepsilon_{ivc} \tag{4}$$

where Adopt_{ivc} is a dummy variable equal to 1 if a co-villager grew a DTMV and 0 if otherwise. DFadopt_{ivc} measures whether a co-villager has an adopter DF in his or her contacts for crop advice. Results are presented in Table 6. Average marginal effects from probit regression (col. 1) show that having an adopter DF in a co-villager's network increased the likelihood of him or her growing a DTMV. This correlation, however, disappears when we control for endogeneity using IV approach (col. 2).

4.3. Robustness checks

To bolster further confidence in our incentives and network effects, we perform a number of placebo tests by regressing (i) co-villagers' baseline knowledge,

Table 6. Networks and co-villagers' adoption decisions

	Probit (1)	IV (2)
Adopter DF in co-villager's network for crop production advice	0.247* (0.037)	1.457 (1.310)
Household controls	Yes	Yes
Sub-county effects	Yes	Yes
Constant		0.037 (0.107)
R-squared	0.179	
Observations	694	694

Notes. In parentheses are robust standard errors clustered at the sub-village level. Baseline household controls include sex, age and education of household head; agriculture as the main source of livelihood of the household head; size of land cultivated with maize; size of land cultivated with groundnuts; ownership of radio, bicycle and solar panel; access to information from a neighbour and friend; and agricultural assets index.

*$p < 0.01$.

Table 7. Placebo test for spurious incentive effects

Explanatory variables	Knowledge (1)	Adopt DTMV (2)	DF as contact (3)
Private reward (PR)	0.294	0.030	0.006
	(0.182)	(0.033)	(0.012)
Social recognition (SR)	0.099	0.006	0.014
	(0.175)	(0.029)	(0.016)
Household controls	Yes	Yes	Yes
Sub-county fixed effects	Yes	Yes	Yes
R-squared	0.246	0.142	0.030
Observations	694	694	694

Notes. Columns 1 and 3 report OLS estimates. Column 2 reports average marginal effects from probit regression. In parentheses are robust standard errors clustered at the sub-village level. Baseline household controls include sex, age and education of household head; agriculture as the main source of livelihood of the household head; size of land cultivated with maize; size of land cultivated with groundnuts; ownership of radio, bicycle and solar panel; access to information from a neighbour and friend; and agricultural assets index.

networks and adoption decisions – revealed before training of DFs – on the (1) incentive treatment dummies and (ii) co-villagers' baseline knowledge on information network variable at endline. If the coefficients on the incentive treatment dummies or information network variable are significantly positive (or negative), it would indicate the presence of unobserved heterogeneity, which could introduce bias (see also Magnan *et al.*, 2015 for a similar approach). Results in Table 7 indicate no statistically significant effect of incentive knowledge, adoption and social networks of co-villagers before DFs were trained, suggesting that our estimates are not affected by such a bias. Similarly, results in Table 8 show no significant network effects on baseline co-villagers' knowledge and adoption.

Table 8. Placebo test for spurious network effects

	Co-villager's knowledge (1)	Co-villager's adoption (2)
Mentioned DF as contact for advice	2.281 (1.992)	
Adopter DF in co-villager's network for crop production advice		0.475 (1.009)
Household controls	Yes	Yes
Sub-county effects	Yes	Yes
R-squared	0.798	0.123
Observations	694	694

Notes. In parentheses are robust standard errors clustered at the sub-village level. Baseline household controls include sex, age and education of household head; agriculture as the main source of livelihood of the household head; size of land cultivated with maize; size of land cultivated with groundnuts; ownership of radio, bicycle and solar panel; access to information from a neighbour and friend; and agricultural assets index.

In addition to the placebo tests, we test whether co-membership of adopter DFs and co-villagers in networks other than the crop production advice affects adoption decisions of neighbours. Two additional network variables are constructed. The first variable is based on membership to financial or risk-sharing neighbourhood. Two farmers – the DF and the neighbour – belong to the same financial or risk-sharing neighbourhood if they lend to, borrow from or exchange material goods in common with each other at any point during the 2-year survey period. The second variable captures non-crop production advice networks and is based on co-membership of a DF and a neighbour to networks for medical or livestock advice. Our focus on these two additional network variables is motivated by alternative explanations that would suggest that a significant effect of agricultural advice network on uptake might be caused by omitted variable bias because of information that neighbours share from common access to other arrangements (Conley and Udry, 2010). As shown in Table 9, we do not find significant effects of alternative networks on adoption of DTMVs, and hence ruling out that neighbours may have changed their adoption decision because they shared membership to other arrangements with DFs.

Alternatively, it is known that communication effectiveness critically depends on the proximity or similarity of the source of information and its recipient. As such, the social distance between DFs and their co-villagers is likely to matter for the credibility and relevance of the information being shared and hence for adoption behaviour. Farmers who receive information update their beliefs about the technology under their own conditions. Usually, it is expected that farmers would obtain more precise information when the DF is more proximate to them. However, evidence on social distance as a determinant of information exchange and adoption behaviour in agricultural settings is scant (Munshi, 2004; BenYishay and Mobarak, 2018). Here, we present heterogeneous treatment effects by social distance between DFs and neighbours.

Table 9. Effects of alternative networks on co-villagers' adoption decision

	Co-villager's adoption (1)
Adopter DF in risk-sharing network	0.103
	(0.105)
Adopter DF in medical or livestock advice network	0.117
	(0.167)
Household controls	Yes
Sub-county effects	Yes
R-squared	0.112
Observations	694

Notes. OLS estimates. In parentheses are robust standard errors clustered at the sub-village level. Baseline household controls include sex, age and education of household head; agriculture as the main source of livelihood of the household head; size of land cultivated with maize; size of land cultivated with groundnuts; ownership of radio, bicycle and solar panel; access to information from a neighbour and friend; and agricultural assets index.

Recently, Santos and Barrett (2010) and Shikuku (2019) provide some guidance on a potentially useful measurement of social distance. The following steps were followed in constructing the social distance variables. In step 1, dyadic pairs were generated for each of the respondent interviewed at baseline. Step 2 involved computing (for each dyadic pair) the absolute difference in the continuous variable (education, age, agricultural assets endowment and non-agricultural assets endowment). In step 3, the median village distance was obtained for each variable. Step 4 then calculated the distance between the village median and the absolute difference (for each variable) between the DF and the neighbour. Social distance between DF i and neighbour j was measured for gender types by a set of dummy variables that consider the several possible characterisations of the match (Santos and Barrett, 2010). Results of heterogeneous effects of incentives by social distance are presented in Table 10.

As shown in Table 10, the positive effect of incentives on knowledge scores and social networks was higher when the DF was female and the co-villager was male. A larger distance between the DF and the co-villager in agricultural asset endowment and education relative to the village median increased the likelihood of information exchange links, but did not improve the knowledge scores of co-villagers. This may indicate that although co-villagers were receptive of the messages of the DFs, it is possible that such messages were not viewed as relevant to their own decision-making. This finding, therefore, supports Bandiera and Rasul (2006).

Finally, robustness analysis of the effect of incentives on knowledge, adoption and networks was performed using an inverse probability score weighting procedure to formally assess the sensitivity of the main results to the attrition problem as described in Section III. Results are presented in Table 11. As shown, the attrition-weighted estimates remain robust and are similar to those reported in Tables 3 and 4, suggesting the robustness of our results to the level of attrition in our sample.

Table 10. Heterogeneous effects of incentives by social distance

	Knowledge and adoption		Co-villagers' networks	
	Co-villagers' knowledge (1)	Co-villagers' adoption (2)	Mentioned D F as contact (3)	Intensity of link with DF (4)
Private reward (PR)	−0.077	0.046	0.054	0.030
	(0.465)	(0.072)	(0.065)	(0.157)
Social recognition (SR)	0.541	0.023	0.025	0.051
	(0.524)	(0.069)	(0.077)	(0.191)
Female DF_female co-villager	−0.553	−0.042	0.004	−0.037
	(0.451)	(0.060)	(0.059)	(0.135)
Female DF_male co-villager	−0.546	0.014	0.044	−0.053
	(0.527)	(0.064)	(0.061)	(0.129)
Distance in education	0.065	0.002	−0.002	−0.014
	(0.105)	(0.012)	(0.010)	(0.021)
Distance in agricultural assets endowment	−0.053	−0.016	0.143**	0.369**
	(0.572)	(0.086)	(0.113)	(0.161)
Private* female DF_female co-villager	0.456	−0.046	0.006	0.230
	(0.568)	(0.093)	(0.106)	(0.287)
Private* female DF_male co-villager	1.744***	0.086	0.100	0.444*
	(0.635)	(0.086)	(0.103)	(0.260)
Private* distance in education	−0.038	−0.005	0.027	0.067*
	(0.134)	(0.019)	(0.016)	(0.035)
Private* distance in agricultural assets endowment	0.143	0.041	−0.157	−0.331
	(0.741)	(0.107)	(0.097)	(0.240)
Social* female DF_female co-villager	0.900	0.098	0.143	0.482
	(0.591)	(0.077)	(0.113)	(0.324)
Social* female DF_male co-villager	1.960***	0.064	0.216**	0.534**
	(0.629)	(0.075)	(0.095)	(0.229)
Social* distance in education	−0.168	−0.071	0.001	0.002
	(0.145)	(0.121)	(0.018)	(0.045)
Social* distance in agricultural assets endowment	−0.358	−0.012	−0.045	−0.144
	(0.789)	(0.063)	(0.109)	(0.274)
Household controls	Yes	Yes	Yes	Yes
Sub-county effects	Yes	Yes	Yes	Yes
Constant	1.918***	—	0.022	−0.040
	(0.726)		(0.094)	(0.229)
R-squared	0.230	0.139	0.086	0.091
Observations	694	694	694	694

Notes. In parentheses are robust standard errors clustered at the sub-village level. Columns 1, 3 and 4 are OLS estimates. Column 2 is average marginal effects from probit regression. Baseline household controls include sex, age and education of household head; agriculture as the main source of livelihood of the household head; size of land cultivated with maize; size of land cultivated with groundnuts; ownership of radio, bicycle and solar panel; access to information from a neighbour and friend; and agricultural assets index.
*$p < 0.1$.
**$p < 0.05$.
***$p < 0.01$.

5. Conclusion and Discussion

The central role of social learning in the transmission of information and behaviours has been convincingly demonstrated. In many developing countries, social learning in networks offers a unique opportunity to augment national extension systems for new technology diffusion. This paper studies how incentives can harness social learning for adoption of drought-tolerant

Table 11. Attrition-weighted incentive effects on co-villagers' knowledge, adoption and networks

	Knowledge (1)	Adoption (2)	Networks (3)
Private reward (PR)	0.450*	0.068*	0.093**
	(0.485)	(0.038)	(0.044)
Social recognition (SR)	0.735***	0.031	0.086*
	(0.267)	(0.035)	(0.045)
Household controls	Yes	Yes	Yes
Sub-county fixed effects	Yes	Yes	Yes
Constant	1.603**	0.035	0.046
	(0.700)	(0.100)	(0.102)
Observations	694	694	694
R-squared	0.197	0.107	0.047
Control group mean	2.320	0.162	0.122
	[2.672]	[0.369]	[0.328]
p-value: PR = SR	0.244	0.277	0.890

Notes. OLS regression estimates. The outcomes are as follows: in column 1 are knowledge scores, in column 2 is an indicator variable equal to 1 if a co-villager adopted a drought-tolerant variety and 0 if otherwise and in column (3) is an indicator variable equal to 1 if DF is mentioned among the co-villager's contacts for agricultural advice, 0 if otherwise. In parentheses are robust standard errors clustered at the sub-village level. In square parentheses are the standard deviations for the control group. Control group comprises households in sub-villages in which a DF was not incentivised. Household controls include sex, age and education of household head, household size, size of land cultivated with maize, ownership of radio, ownership of mobile phone, access to government extension and access to credit.
$^{*}p < 0.1$.
$^{**}p < 0.05$.
$^{***}p < 0.01$.

maize varieties (DTMVs) in northern Uganda and mechanisms through which effects occur. In our experiment, a random subsample of trained individuals receives incentives for sharing the knowledge learnt with their neighbours. We distinguish between private material reward and SR incentive schemes.

Incentives increase the likelihood of DFs to experiment with DTMVs, the knowledge of co-villagers about DTMVs and the number of people with whom DFs discussed about farming, suggesting a change in the networks of both DFS and neighbours. Relative to conventional training, incentivised training increased the likelihood of a neighbour mentioning a DF in his or her own contacts for agricultural advice and the frequency of interaction with a DF for information exchange, but not second-order linkages through friends of neighbours. Having an adopter DF in a farmer's own network further improved knowledge transmission about the new technology. We also document weak evidence on the effects of incentives on adoption behaviour of peer farmers. The results are robust to several robustness checks and controlling for spillover effects and the problem of attrition in our sample.

Our results generate several important implications for policy. Providing direct training alone to contact farmers might not significantly influence knowledge and adoption decision of neighbours owing to the lack of appropriate incentives to share knowledge (Kondylis, Mueller, and Zhu, 2017). Here, we demonstrate that incentives matter for technology diffusion within social networks. Recently, BenYishay and Mobarak (2018) showed that PRs influenced social learning. In addition to PRs, we find that SR by announcing 'good' performance of DFs in public plays an important role to substantially improve social learning. Our findings demonstrate that PR and SR are equally important in affecting information networks. Overall, the fact that incentives matter and social transmission is not always automatic suggests that social learning models can likely be enriched by incentives that govern whether (and how) people experiment with new technologies and communicate about them with their peers.

Finally, certain qualifications warrant duly consideration in interpreting our results. First, using incentives to harness social learning may produce unexpected consequences. For example, it is possible that provision of private incentives could undermine the credibility of DFs as communicators. Peer farmers could become less interested in DFs' advice about the positive attributes of the new technology once they realise that the DFs are being paid an incentive to deliver that information. In our context, this would be less of a concern with the SR incentive scheme. Second, our incentives are based on knowledge, but actual adoption may matter more than knowledge for policy, as knowledge about a technology is an intermediate outcome. Our decision to base incentives on knowledge was because, while DFs can hold meetings or move from house to house to train the neighbours, the decision by neighbours to actually adopt or not might be beyond the DFs' effort. Furthermore, whether DFs actually receive the incentive was assessed based on knowledge of a randomly selected fellow villager. We acknowledge that this might introduce some level of noise to the expected incentive for DFs and affect their efforts to spread information. Third, we do not have information on the strategies DFs used to communicate the information and specifically what they told other farmers about the potential rewards. However, this concern can affect results on both directions. For example, it is possible that the DFs communicate co-villagers that the reward (weighting scale) can be used as a public good (even in the private arms). On the other hand, it is also possible that provision of incentives (especially private ones) could undermine the credibility of DFs as communicators. We, therefore, acknowledge that in both the SR and the PR treatment, it is possible that DFs told other farmers about the potential for getting access to a scale. If so, both the SR and PR treatments may have also had an incentivising or disincentivising effect on other farmers (in addition to the DF). Fourth, although efforts were made to minimise strategic interactions by DFs, we are unable to rule out completely that DFs may have targeted only those co-villagers who were visited for the baseline survey. Fifth, although our pilot study showed that none of the respondents mentioned more than five contacts for agricultural advice, it is still possible that DFs shared information about DTMVs, but their names were not mentioned among the top five. Lastly, adoption is a process that largely follows procedures of awareness and knowledge exposure about the technology, try-out of the technology and continued use – sometimes with considerable time lags. We, therefore, interpret our results broadly as early uptake and not adoption/diffusion as defined in a strict sense. We, therefore, acknowledge that the short time between introduction of the technology and our adoption measurement may partially account for insignificant adoption reports. These are relevant considerations for future research.

5. Supplementary data

Supplementary data are available at *ERAE* online.

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Conflict of interest

The authors declare there is no conflict of interest in this paper.

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Appendix A: Additional Tables

Table A1. Determinants of attrition: probit regression

Variable	Coefficient	Standard error	p-value
Private material reward (PR)	0.115	0.288	0.690
SR	−0.163	0.250	0.514
Household head is male	−0.476	0.220	0.030**
Age of household head	−0.002	0.006	0.736
Size of the household	−0.058	0.045	0.192
Education of the household head	0.039	0.020	0.049**
Household income (natural log)	−0.064	0.104	0.538
Participation in casual employment	−0.434	0.456	0.341
Participation in self-employment	0.299	0.532	0.575
Amount of credit received (natural log)	−0.091	0.085	0.285
Agricultural assets index	0.026	0.021	0.224
Non-agricultural assets index	0.025	0.027	0.363
Housing index	−0.224	0.233	0.336
Crop production advice network	−0.056	0.082	0.497
Kinship network	0.010	0.074	0.891
Experience with floods	−0.318	0.186	0.088*
Experience with droughts	0.601	0.417	0.150
Constant	0.148	0.694	0.832
p-value of test: PR + SR = 0	0.632		
Wald Chi-square (17)	49.33***		
Pseudo R-squared	0.029		
Observations	1286		

*$p < 0.1$.
**$p < 0.05$.
***$p < 0.01$.

Table A2. Test for spillover effects: t-test using a border-to-treatment dummy variable

	Without potential spillover	With potential spillover	p-value of difference in means
DF is mentioned	0.092	0.088	0.773
	(0.011)	(0.009)	
	[0.288]	[0.283]	
Frequency of interaction	0.179	0.205	0.417
	(0.023)	(0.023)	
	[0.634]	[0.710]	
Weak link	0.032	0.024	0.339
	(0.006)	(0.005)	
	[0.176]	[0.154]	
Observations			

Notes. Standard errors are reported in parentheses. In square parentheses are standard deviations.

Table A3. Differences in maize importance and preferences across treatment groups: whole sample

	Whole sample (1)	Training only (2)	PR (3)	SR (4)	*F*-test (*p*-value) (5)	*p*-value (2) = (3) (6)	*p*-value (2) = (4) (7)	*p*-value (3) = (4) (8)
Per cent of land under maize	10.26	12.12	10.43	10.26	0.453	0.311	0.240	0.915
Yield of maize (kg/ha)	709.54	767.92	630.24	731.07	0.347	0.184	0.756	0.326
Per capita maize income (US$)	28.00	45.03	13.48	25.60	0.177	0.127	0.399	0.271
	1286	428	431	427				

Table A4. Differences in maize importance and preferences across treatment groups: DFs' sample

	Whole sample (1)	Training only (2)	PR (3)	SR (4)	*F*-test (*p*-value) (5)	*p*-value (2) = (3) (6)	*p*-value (2) = (4) (7)	*p*-value (3) = (4) (8)
Per cent of land under maize	12.28	13.64	13.32	10.08	0.460	0.932	0.280	0.33
Yield of maize (kg/ha)	851.28	990.11	694.35	875.12	0.488	0.253	0.672	0.443
Per capita maize income (US$)	22.54	32.64	18.04	17.68	0.633	0.413	0.352	0.977
	120	38	40	42				

Table A5. Differences in maize importance and preferences across treatment groups: co-villagers' sample

	Whole sample (1)	Training only (2)	PR (3)	SR (4)	*F*-test (*p*-value) (5)	*p*-value (2) = (3) (6)	*p*-value (2) = (4) (7)	*p*-value (3) = (4) (8)
Per cent of land under maize	10.78	11.97	10.13	10.28	0.481	0.289	0.300	0.926
Yield of maize (kg/ha)	694.95	746.27	623.68	715.34	0.456	0.246	0.794	0.389
Per capita maize income (US$)	28.57	46.24	13.01	26.46	0.190	0.141	0.433	0.269
	1166	390	391	385				

Table A6. Differences in maize importance and preferences between DFs and co-villagers: *t*-tests

	Co-villagers (1)	DFs (2)	Difference (3)	*t*-statistic (3) = (4) (8)
Per cent of land under maize	11.21	12.28	−1.07	−0.714
Yield of maize (kg/ha)	699.50	851.28	−151.78	−1.399
Per capita maize income (US$)	33.01	22.54	10.46	1.131
	1026	1026	1026	

Notes. *t*-statistics are reported.

Table A7. Differences in characteristics among disseminating farmers

	Training only (1)	PR (2)	SR (3)	F-test (p-value) (4)	p-value (1) = (2) (5)	p-value (1) = (3) (6)	p-value (2) = (3) (7)
Household head is male	0.87	0.82	0.88	0.770	0.600	0.868	0.481
Household head has primary level education	0.55	0.53	0.55	0.967	0.810	0.965	0.840
Age of household head	44.11	44.03	41.88	0.620	0.975	0.386	0.413
Household size	6.08	6.68	6.17	0.390	0.201	0.847	0.279
Area under maize	0.34	0.43	0.56	0.073	0.417	0.023	0.229
Dependency ratio	0.50	0.56	0.50	0.372	0.233	0.995	0.216
Participation in farmer group	0.79	0.88	0.71	0.184	0.320	0.442	0.072
Access to credit	0.71	0.68	0.79	0.508	0.738	0.447	0.265
Friendship network	1.71	2.03	1.91	0.392	0.177	0.418	0.614
Kinship network	2.05	1.63	1.74	0.224	0.088	0.210	0.589
Observations	38	40	42				

Table A8. Differences in characteristics between disseminating farmers and their co-villagers

	Co-villagers (1)	DFs (2)	Difference (3)	p-value (4)
Household head is male	0.82	0.86	−0.04	0.254
Household head has primary level education	0.42	0.54	−0.12	0.012
Age of household head	43.68	43.3	0.38	0.747
Household size	5.80	6.31	−0.51	0.015
Area under maize	0.45	0.45	0.01	0.840
Dependency ratio	0.52	0.52	0.00	0.914
Participation in farmer group	0.80	0.79	0.01	0.880
Access to credit	0.68	0.73	−0.04	0.328
Friendship network	2.03	1.88	0.14	0.160
Kinship network	1.73	1.80	−0.07	0.488
Observations	855	120		

Appendix B

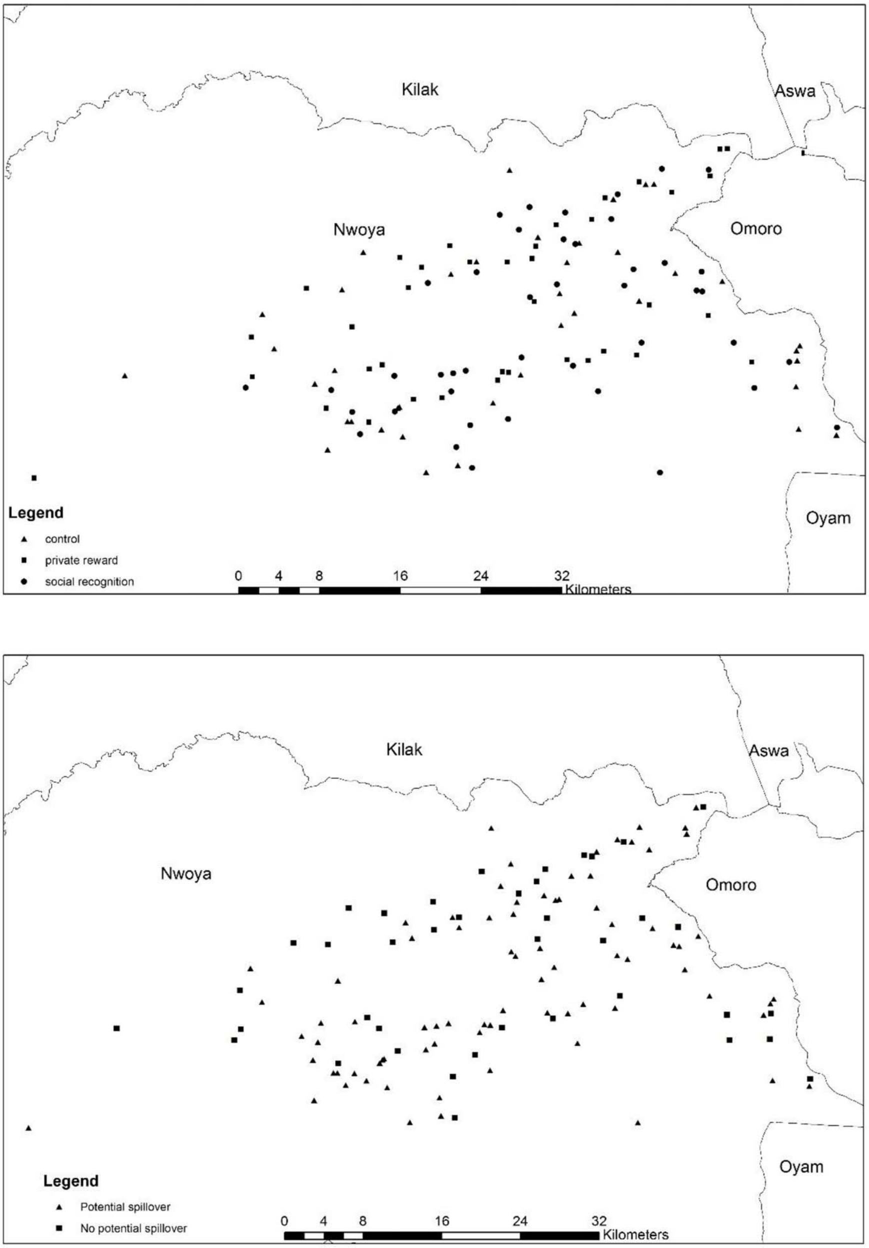

Figure B1. Location of treatment and control sub-villages (top panel) and potential for spillover (bottom panel)

Appendix C: Questions for Testing Knowledge

(1) Have you ever heard about improved varieties of maize? (1 mark)

(2) What varieties have you heard about?

1 mark if farmer mentions Longe 10H.

1 mark if farmer mentions Longe 7.

1 mark if farmer mentions Longe 5.

1 mark if farmer mentions any other Longe maize.

(3) What are the benefits of growing improved varieties of maize?

1 mark if farmer mentions drought-tolerance.

1 mark if farmer mentions disease-resistance.

1 mark if farmer mentions pest-resistance.

1 mark if farmer mentions high-yielding.

1 mark if farmer mentions early maturing

Appendix D: Example of Social Networks Questions

Sn01. Name the people from whom you seek advice on crop production

Name of the contact	Sub-village	Relationship	Sex 1 = male 0 = female	Age (Years)	Level of education (Codes)	Distance to contact (Walking minutes)	How often do you seek advice about crop production 1 = daily 2 = at least weekly 3 = less often

Sn02. Name the people who seek advice on crop production from you

Name of the contact	Sub-village	Relationship	Sex 1 = male 0 = female	Age (Years)	Level of education (Codes)	Distance to contact (Walking minutes)	How often do you seek advice about crop production 1 = daily 2 = at least weekly 3 = less often

Appendix E: Theoretical Model

To guide our empirical analysis, we summarise a theoretical framework that combines insights from the standard target input model commonly used in diffusion studies (e.g. Bardhan and Udry, 1999; Bandiera and Rasul, 2006) and a model of incentives for communication proposed by BenYishay and Mobarak (2018). The basic setup of the model is as follows. There is a continuum of farmers distributed on a line, with mean revenues equal to 0 and variance equal to 1. Farmers can produce output using a conventional technology, earning a certain level of profit q, or a new technology. While the basics of the new technology are observable and known to all farmers, we consider that one parameter is random and ex ante unknown. This parameter is the target level of a variable input (say labour), denoted by y^*.[10] Payoffs of the new technology for farmer i depend on the distance between the applied input level and the target: $Q_i = 1–(y_i–y^*)^2$. For simplicity we assume both the target value and productivity of the new technology are homogenous across farmers. Nevertheless, payoffs may depend on the location of farmers in the distribution. The reason is that farmers receive signals about the profitability and implementation of the new technology by observing their peers, but the signal of 'neighbouring farmers' is more informative than signals received from farmers further away in the distribution.

Assume there is an ex ante common belief about the target input level, which is normally distributed with mean 0 and variance σ^2. If farmers adopt the new technology, their expected payoff equals $1 - \sigma^2$, i.e. in the absence of additional information, farmers will choose not to adopt when $q > 1 - \sigma^2$. Next, assume there is one informed farmer, the disseminating farmer, who knows the target level y^*. This farmer is located at x in the distribution and can choose to send a signal with precision ρ to her peers at a cost $c(\rho)$. We assume these costs are increasing in the precision of the signal such that $c'(\rho) > 0$ and $c''(\rho) > 0$. Following BenYishay and Mobarak (2018), we assume that if the disseminating farmer sends a signal, farmer i receives a noisy message with the noise level increasing in the distance between x and i:

$$s_{xi} = y^* + \frac{| x - i |}{\rho} \tag{1}$$

After receiving signal s_{xi} the receiving farmer uses Bayesian updating to update his beliefs about the target level. The ex post mean and variance are now given by:

$$E\left[y^*|s_{xi}, \rho\right] = \frac{\sigma^2\rho^2 s_{xi}}{\sigma^2\rho^2 + (x-i)^2} \tag{2}$$

$$VAR\left[y^*|s_{xi}, \rho\right] = \frac{1}{\frac{1}{\sigma^2} + \frac{\rho^2}{(x-i)^2}} \tag{3}$$

10 The original diffusion model developed by Bardhan and Udry (1999) and Bandiera and Rasul (2006) assumes that the target level y^* varies across farms (i.e. y_i^*). This approach captures differences in agronomic conditions between farms. However, since our data do not enable quantification of 'proximity' (or similarity) between farmers, we ignore such heterogeneity in production in the theoretical model.

Farmers further away from the disseminating farmer receive a noisier signal; their updated beliefs are more biased and variable than the updated beliefs of farmers closer to the disseminating farmer. Since farmers will only adopt if their expected payoffs of the new technology are higher than their profits under the traditional technology, farmer i will adopt the new technology if the following condition is satisfied:

$$q < Q_i = 1 - \frac{1}{\frac{1}{\sigma^2} + \frac{\rho^2}{(x-i)^2}} \tag{4}$$

While diffusion of the new technology in the absence of signal-sending by the disseminating farmer only occurs if $q < 1 - \sigma^2$, the probability that adoption occurs increases after receiving a signal. Since the variance of the target level is decreasing in the distance between sending and receiving farmers (x–i), diffusion is most likely to occur among farmers within the information network of the signal-sending farmer (i.e. the DF). The variance is also decreasing in the precision of the signal, so disseminating farmers willing to incur greater signalling costs will also promote diffusion. The level of signal-sending chosen by the disseminating farmer will vary with marginal benefits and costs of increasing the precision of the signal. In the absence of any benefits, farmers will not invest in information diffusion and choose $\rho = 0$. We distinguish between two reasons why disseminating farmers may choose a precision level that is greater than 0 and incur positive signalling costs.

First, altruistic disseminating farmers may invest in signalling to increase the payoffs of their peers (e.g. Ashraf, Bandiera, and Jack, 2014). Specifically, assume farmer x internalises the payoffs of farmer i and knows that (i) adopting the technology would be welfare-increasing for farmer i and (ii) that sending a signal would convince that farmer to adopt the new technology. Farmer x's full payoff function reads as:

$$\pi_x = Q_x + \beta\left[Q_i - q\right] - c(\rho) \tag{5}$$

where Q_x are the own material payoffs for farmer x and $\beta \leq 1$ is the parameter used to weigh the payoffs of farmer i. For Q_i–$q > 0$, an altruistic disseminating farmer will set $\rho > 0$. For $Q_i > q$, the optimal precision level of the signal solves:

$$\frac{\frac{2\beta\rho}{(x-i)^2}}{\left(\frac{1}{\sigma^2} + \frac{\rho^2}{(x-i)^2}\right)^2} = c'(\rho)\,. \tag{6}$$

Importantly, altruistic disseminating farmers should *not* send a signal to their peers if they believe the distance to others is 'too large' so that the resulting signal for the receivers will be 'too noisy'.[11] Further, we have assumed that the new technology is equally productive for all farmers (upon applying the same level of

11 Observe that this finding depends on the assumption that disseminating farmers discount the future. Her neighbours, after receiving a precise signal about input use, may subsequently decide to send a signal to their own neighbours (located further away from the injection point). This would allow information about the new technology to gradually and accurately spread. We assume such second-order diffusion is ignored by the disseminating farmer.

input y). However, if heterogeneity in production conditions – say due to agronomic circumstances or farming skills – implies a range of payoffs from adoption (as documented by Suri (2011)), then an altruistic disseminating farmer may decide to *not* send a signal if he or she suspects a fraction of her peers will be worse off after adoption, even if they choose the optimal target y^*. Altruistic disseminating farmers should only work hard to diffuse knowledge if they believe the net payoffs of the new technology are positive for their peers.

Second, disseminating farmers may invest in signalling to secure private payoffs – either in the form of a private material reward (PR) or in the form of SR. Following BenYishay and Mobarak (2018), assume the disseminating farmer receives a reward PR (or SR) if a certain mass of peer farmers knows about the new technology or adopts the new technology. From Equation (4), adoption will occur by the mass of farmers i satisfying the following condition:

$$(x-i)^2 \le \frac{\rho^2}{\frac{1}{1-q}-\frac{1}{\sigma^2}}. \tag{7}$$

Suppose the reward is given if a mass z of farmers adopts. To obtain the reward, disseminating farmer x should send signal with precision ρ^* such that condition (7) is satisfied for all farmers located on the interval $[x - \frac{1}{2}z, x + \frac{1}{2}z]$. Of course, this signal will only be sent if $c(\rho^*)<$PR (or if $c(\rho^*) <$SR).

This theoretical model implies a potential for changes in information networks coming from two sources. First, DFs may be motivated to reach out to more neighbours either to optimise their altruistic behaviour or to achieve the critical mass of peer farmers who know about the new technology to secure getting the reward. In the end, this leads to an increase in the number of people with whom the DF shares information about the new technology. Second, providing training to DFs exogenously makes them potentially important nodes as a source of information about a highly relevant new technology in the context of rural Uganda. Neighbours, including those who were not in the DFs' networks at the baseline, may realise this and actively seek to be connected with DFs, ultimately implying changes in information networks of neighbours.