Incentives and the Diffusion of Agricultural Knowledge: Experimental Evidence from Northern Uganda

Kelvin Mashisia Shikuku, Janneke Pieters, Erwin Bulte, and Peter Läderach

We present results of a randomized evaluation that assesses the effects of different incentives for diffusion of agricultural knowledge by smallholders in northern Uganda. Randomly-selected disseminating farmers (DFs) from a large sample of villages are assigned to one of three experimental arms: (*a*) training about climate smart agriculture, (*b*) training plus a material reward for knowledge diffusion, and (*c*) training plus a reputational gain for knowledge diffusion. We find that leveraging somebody's reputation (or social recognition) has a positive impact on DFs' experimentation and diffusion effort. This impact is stronger than that measured in the private material rewards treatment.

Key words: Incentives, pro-social behavior, social learning, technology transfer, climate smart agriculture.

JEL codes: H5, O13, O33, Q12.

Transforming smallholder agriculture in order to lift the majority of the population in sub-Saharan Africa out of poverty requires boosting agricultural productivity under increasingly volatile conditions. This requires diffusion of modern technologies (e.g., Evenson and Gollin 2003; Minten and Barrett 2008), but in many African countries adoption rates of innovations remain low (Pamuk, Bulte, and Adekunle 2014). Several well-known reasons help to explain this. Benefits may be heterogeneous, reflecting variety in growing conditions and other factors, so adoption may be unprofitable for some smallholders (e.g., Suri 2011; Magnan et al. 2015). Costs associated with innovations such as improved seeds or fertilizer may be an impediment to adoption if capital markets are imperfect. The low quality of agricultural inputs may help explain low take up (Bold et al. 2017), as does a lack of information about the existence and proper implementation of agricultural innovations (e.g., Foster and Rosenzweig 1995).

In this paper we focus on the diffusion of information. Development organizations and policy makers have long believed that information "travels easily" within social networks. Interventions reaching small target groups are expected to reach much larger populations as information diffuses from

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"treated individuals" to their peers. Interventions based on the assumption of automatic and extensive spreading of information, such as traditional extension efforts, have by and large produced unsatisfactory results and failed to reach large parts of the intended population (de Janvry, Sadoulet, and Rao 2016). In some countries, such as Uganda, disappointing outcomes have led to the disbandment of national agricultural advisory services systems. Current efforts to strengthen national extension systems in developing countries recognize the need to search for cost-effective complementary actions (Godtland et al. 2004).

Recent evidence suggests that knowledge does not diffuse automatically. Diffusion of information requires time and effort of agents on both the "supply" and the "demand" side. Allocation of effort to teaching and learning is akin to an investment by smallholders, so it makes sense for development agents to consider complementary measures to facilitate such investments. There are several dimensions to this issue. The first one is who to select as the "disseminating farmer (DF)"—the first individual in the target population to receive the technology (Banerjee et al. 2013, 2019). Not all individuals are equally likely to reach large numbers of co-villagers, or be in a position to convince others to follow their behavior. Traditionally, extension efforts targeted better-off farmers, who typically are well-connected and expected to be role models for their peers. However, since such farmers may not be representative of their co-villagers, their experiences may be of limited value to others (e.g., Munshi 2004; Conley and Udry 2010; BenYishay and Mobarak 2018). Although it is beyond the scope of the current study to identify optimal DFs or map the network structure, recent literature focuses on exploiting (social) network theory, and proposes targeting individuals who occupy either a central (e.g., Kim et al. 2015) or clustered position in the network (Chami et al. 2018; Beaman et al. 2018). In our study, we select as DF a farmer who is comparable to his or her fellow villagers in terms of wealth and education.

A second dimension—which is the focus of our study—concerns how to motivate DFs to inform their peers and encourage them to adopt the technology (BenYishay and Mobarak 2018). Following Benabou and Tirole (2006), we distinguish between three motives for why farmers may invest time and effort in educating their peers. First, they may be altruistic and intrinsically motivated to help their co-villagers. Second, they may gain status and social recognition by helping others. Finally, they may engage in diffusion if there are private tangible rewards associated with knowledge diffusion. This could happen if there are externalities in adoption or the use of new technologies (e.g., pest management), or if external rewards for diffusion are introduced. BenYishay and Mobarak (2018) demonstrated that incentivizing DFs via material rewards was an effective approach for promoting diffusion.[1] These authors trained DFs in Malawi to use new technologies (pit planting and composting), and promised some of them a bag of seeds in the event that knowledge and adoption of new technologies increased sufficiently among other farmers. They find that only with the incentive, DFs experiment with and communicate about the technologies, leading to increased adoption among other farmers. These findings underscore the importance of understanding the motives for farmers to spread information to others.

The objectives of this paper are twofold. First, we use an experimental approach to evaluate the effectiveness of approaches based on the above-mentioned motives for knowledge diffusion: altruism or intrinsic motivation, social recognition, and private rewards. We ask whether social recognition and private rewards for diffusion affect DFs' effort to learn about the benefits of the new technology and subsequently diffuse information. Second, we probe whether the impact of social recognition and private reward incentives varies with DFs' pro-social preferences. We measure social preferences of DFs with an auxiliary lab-in-the-field game—an augmented dictator game with a local charity as the receiver.

We designed a field experiment in northern Uganda with two treatment arms and an "information only" arm: (*a*) a basic (status quo) arm where DFs receive training about specific climate smart agricultural technologies (i.e., the information-only arm); (*b*) another arm where they receive the same

[1] For related evidence in another context, namely the diffusion of financial knowledge, refer to Sseruyange and Bulte (2018).

training plus a private reward (a weighing scale) in the event of a sufficient increase in knowledge among other farmers (to be specified below); and (*c*) a final arm that combines the training with social recognition in the case of a sufficient increase in knowledge among other farmers. Specifically, in the case a threshold level was reached, a public ceremony was organized in which the DF's contribution was highlighted and a weighing scale was given "to the community." We consider the information-only arm as the control group, even though it is not a pure control, and results should be interpreted accordingly. As dependent variables we use *experimentation* with the new technologies by the DFs and other farmers, *effort* devoted by DFs towards training other farmers, and the *knowledge* gained by other farmers. Our main results are that (*a*) incentivizing DFs by providing them with social recognition has a significant and large effect on diffusion effort and levels of knowledge diffusion; (*b*) the effects of providing a private material reward are small; and (*c*) the effect of both types of incentives is not mediated by pro-social preferences (see also Ashraf, Bandiera, and Jack 2014).

Our results speak to several bodies of literature. First, and as mentioned above, it relates to the rapidly growing literature on social learning.[2] Learning from others facilitates aggregation of dispersed information (Acemoglu et al. 2011; Alatas et al. 2016) and can generate social multiplier effects in diffusion of innovations (Hogset and Barrett 2010). Social learning can, therefore, contribute to increased agricultural productivity (Vasilaky 2012; Vasilaky and Leonard 2018). Second, we contribute to the literature on incentives for pro-social behavior or contributions to the common good.[3] This literature has benefitted from recent insights in the field of behavioral economics, highlighting the potential interaction between motives (e.g., Benabou and Tirole 2006; Ariely Bracha, and Meier 2009; Gneezy, Meier, and Rey-Biel 2011). For example, the provision of private rewards for pro-social behavior may crowd out altruism or social recognition motives by obscuring the (self-)signal that someone is doing "good"—instead of simply doing "well". Diffusion of agricultural knowledge is a pro-social task; the direct benefits created by the task are enjoyed by those other than the person who expends the effort (Ashraf, Bandiera, and Jack 2014). Our inclusion of a social recognition incentive and analysis of the role of altruism further differentiates the current paper from that of BenYishay and Mobarak (2018), who focus on the effect of private reward incentives. To our knowledge, we provide the first evidence about the effects of social recognition on the diffusion of agricultural knowledge.

The paper is organized as follows. The next section describes the agricultural context, experimental design, and data. The subsequent section discusses the empirical estimation, followed by a section that presents the findings. The final section concludes.

Context, Experimental Design, and Data

The experiment was implemented in Nwoya district, northern Uganda, a predominantly agrarian region characterized by low agricultural productivity. The region's poverty level is the highest in the country—about 44% of the population lives on less than one U.S. dollar per day (Republic of Uganda 2015). The region is expected to suffer more frequently from weather shocks in the future, including prolonged dry spells and uncertainty about the onset and cessation of rainfall (Mwongera et al. 2014). Damages to agricultural output due to weather shocks amounted to more than $900 million in 2010, or 77% of total damages across all sectors of the country's economy (Republic of Uganda 2012, 2016). 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 the 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

[2] Examples include Munshi (2004), Bandiera and Rasul (2006), Conley and Udry (2010), Maertens and Barrett (2012), Krishnan and Patnam (2013), Genius et al. (2013), Magnan et al. (2015), Beaman et al. (2018), and BenYishay and Mobarak (2018).

[3] Examples include Gneezy and Rustichini (2000), Carpenter and Myers (2010), Lacetera, Macis, and Slonim (2011), Duflo, Hanna, and Ryan (2012), Glewwe, Ilias, and Kremer (2010), Ashraf, Bandiera, and Jack (2014), Ashraf, Bandiera, and Lee (2014), Barile, Cullis, and Jones (2015), and DellaVigna and Pope (2016).

about CSA technologies and perceive this as a major constraint to widespread adoption (Shikuku et al. 2015). Current efforts to restructure the extension system recognize the importance of working with DFs at the sub-county and village levels to enhance dissemination of improved technologies (Ministry of Agriculture, Animal Industry and Fisheries 2016; MAAIF). Our study is part of this effort, and we focus on the performance of DFs that are more or less representative of the target population.

Sampling and Intervention

We first generated a list of 310 sub-villages in Nwoya district, from which we randomly selected 132 sub-villages for our study.[4] 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 organized a meeting with co-villagers to discuss whether the selected candidate was "not too different" (especially in terms of wealth and landholdings, and education) from the rest of the village, and potentially interested to experiment with new technologies. We did not collect data on individual characteristics during the meeting. In more than 75% 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 the co-villagers.

Selected DFs were trained and had to decide whether or not to experiment with the new CSA technologies on their own farms. Importantly, the new technologies were not subsidized or "offered for free" to encourage farmers to try them out. Instead, farmers had to decide whether or not to purchase certain inputs from local agro-dealers, and whether or not to allocate labor (effort) to the construction of structures recommended during the training.[5] They also had to decide about the level of effort devoted to the diffusion of information. The main technologies, described below, were new and unfamiliar to the farmers, so DFs had to spend time explaining their implementation and potential benefits.

The 132 sub-villages were randomly assigned to one of three experimental arms of 44 sub-villages each: (*a*) training only, (*b*) training plus a private material reward, and (*c*) training plus social recognition. Disseminating farmers in the first experimental arm (control group) received training about drought-tolerant maize varieties and conservation farming basins and were subsequently asked to share the information with their co-villagers. Disseminating farmers in the second experimental arm received the same training, but after the training were informed they could earn a private reward. 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. These farmers 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 the 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.

We do not have information on what the DFs told other farmers about the potential rewards. We therefore acknowledge that in both the social recognition and the private reward treatment, it is possible that DFs told other farmers about the potential for getting access to a scale. If so, both the social recognition and private reward treatments may have also had an incentivizing effect on other farmers (in addition to the DF).

The decision to use the training-only group as a comparison group instead of including a fourth arm (pure control) was made to increase statistical power, especially because the randomization was done at the sub-village level and not the individual level. Our

[4] A sub-village is equivalent to a hamlet, and is the lowest administrative unit in Uganda. The 132 sub-villages in our sample are located within four sub-counties.

[5] We verified that inputs that had to be purchased were actually available in local agro-dealers. This was invariably the case.

experiment, therefore, provided training to all DFs, but varied the incentive received to disseminate knowledge. The comparison between either incentive treatment and the information-only group is more relevant than the comparison between either treatment and a pure control because it isolates the effect of incentives.[6]

Observe that DFs were not informed about the (private or social) reward until *after* completing the training. This design, therefore, deviates from BenYishay and Mobarak (2018), who informed their subjects about the potential reward *before* the training. Informing DFs after the training rules out the potential impact of incentives on two intermediate outcomes. First, incentives may change the composition of the group of DFs who attend the complete training. Incentives may potentially stimulate invitees with low intrinsic motivation to attend (see Finan, Olken, and Pande (2017), on financial incentives and recruitment of public sector workers). Second, for a given pool of participating DFs, the incentives may affect their level of learning effort and hence the knowledge they accumulate during the training (Sseruyange and Bulte 2018). Since we are primarily interested in the effect of incentives on DFs' knowledge diffusion efforts (and not selection effects or learning effort), we opted for a design in which the type of DF and his or her knowledge accumulation during the training is orthogonal to treatment status, that is, by informing DFs of their potential rewards *after* the training. Our approach, however, implies that we potentially underestimate the full effect of incentives, so that our estimate is a lower bound of the full treatment effect of incentives on diffusion effort (that is, attendance and effort during training and effort during follow-up activities).

Interventions were rolled out in March 2016. We partnered with researchers from the National Agricultural Research Organization (NARO) and Tillers International—an NGO working with NARO to promote conservation farming in Uganda. We provided a three-day training session to the selected DFs, which lasted five hours per training day. In addition to learning about the benefits and cultivation of drought-tolerant (DT) maize (Longe 10H), selected farmers learned how to construct so-called conservation farming (CF) basins, and how to sow seeds of the improved varieties in these basins. Basins retain soil moisture, improve water infiltration (reducing surface water run-off) and minimize soil disturbance—similar to the "pit planting" technology studied in Malawi by BenYishay and Mobarak (2018). Experimental evidence suggests the existence of yield gains associated with this technology (Otim et al. 2015; see also Haggblade and Tembo 2003; Gatere et al. 2013). The training also included crop management practices such as correct spacing, row planting, and timely weeding. While the technology requires an upfront labor investment, the labor burden decreases in subsequent periods as the constructed basins are "permanent" (Haggblade and Tembo 2003).

The trainings were organized in central locations, and DFs were invited to travel to these sites. Training sessions were organized per sub-county, with 11 farmers per session. In each sub-county, DFs from different treatment arms were trained in separate venues to minimize 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 farmers that we invited, 126 attended the full training.

Data and Summary Statistics

The data we use were collected during two household survey waves. We conducted a detailed baseline survey between September and December 2015. During this time, we visited 132 sub-villages and in every village surveyed the DFs, as well as nine randomly selected co-villagers. In total we visited 1,320 households, and collected information on household demographics, crop and livestock production, off-farm income, assets ownership, exposure to weather shocks, sources of agricultural information and knowledge about farming practices, social networks, and food security. The "random villager" that was later used to evaluate the extent of knowledge diffusion was randomly drawn from this sub-sample (enabling us to control for *ex ante* knowledge levels in regression models to increase precision of our estimates), but this was not communicated to DFs. It is possible that DFs suspected that we would interview the same co-villagers visited at baseline, so they might target diffusion efforts towards these individuals. If so,

[6] Our design is not suitable to assess the impact of knowledge provision on diffusion, or the synergies between incentives and knowledge provision, relative to a pure control group.

this may bias our estimates of treatment effects on knowledge diffusion.[7]

Table 1 presents summary statistics of baseline data per treatment group, including demographic information, social network variables, exposure to weather shocks, and sources of agricultural information. Differences across the three groups are small in magnitude. Using a regression of pre-treatment covariates on treatment dummies, an *F*-test that all treatment arm coefficients equal zero failed to reject. In addition, we perform an *F*-test of joint orthogonality using a multinomial logit, which tests whether the observable characteristics in table 1 are jointly unrelated to treatment status. We cannot reject this null hypothesis (p-value = 0.227), suggesting that the randomization succeeded in achieving balance across the experimental arms.

Most sample households are male-headed with an average age of 43 and six years of completed formal education. The average size of a household is six with a dependency ratio of 54%. Ownership of both agricultural and livestock assets is very low. On average, a household has two people from whom it seeks agricultural advice and two relatives in the village. More than 90% of the sample households reported to have experienced drought. Access to government extension is very low: only 2% of the sample respondents had received agricultural advice from government extension.

The second survey wave was conducted in September 2016, after the first post-experimental cropping season, to measure performance of the DFs. We visited 246 farmers: 123 DFs (of the initial sample of 132 selected DFs, six did not attend their training and three were not available for interview at the time of the survey) and a random subsample of 123 "other farmers" (sampled from the original list of farmers at baseline).[8] We measured three types of dependent variables: *knowledge* levels (of the DFs and their co-villagers), on-farm *experimentation* (by the DFs and their co-villagers) and diffusion *effort* by the DFs.

To gauge knowledge levels we administered a simple test focusing on the content of the CSA training. Such exams are an effective approach of assessing knowledge retention by subjects (Kondylis, Mueller, and Zhu 2015), picking up effort during the training as well as effort to memorize the training content afterwards. We weigh correct answers by the inverse probability of a correct response so that difficult questions carry more weight in the final outcome (see online supplementary appendix B for the questions). Knowledge scores for DFs ranged between 0 and 33.0, with a mean of 20.0 (the mean knowledge score for "other villagers" was only 13.2).

To measure on-farm experimentation with the new technologies, we verified whether the DF and the co-villager had planted the DT maize variety (Longe 10H), and had constructed the CF basins on at least one household plot. We also measured uptake of other Longe maize varieties, also discussed during the trainings and more familiar to the farmers in our sample. About 8.3% of the DFs had tried out the Longe 10 H maize variety, and 22% had constructed CF basins. In addition, about one-third had planted another Longe maize variety. Not surprisingly, experimentation by co-villagers was much lower: about 2% tried out Longe 10 H maize, another 2% tried out CF basins, and 6.5% grew a different Longe maize variety.

To measure diffusion effort by DFs we use a binary outcome capturing whether the DF organized at least one activity in the sub-village intended to train co-villagers. Specifically, we asked the other farmer whether he or she knew of (or had attended) any agricultural technology training organized by another farmer in their sub-village during the previous season. If they answered affirmatively, we asked the name of the farmer who had organized the activity. We also asked about the content of the activity. On average, 18% of the other farmers indicated the DF from their sub-village had organized at least one meeting to train co-villagers. It is also possible that DFs communicated with their neighbors via word of mouth. To capture this, we include an additional effort variable measuring the number of people with whom the DF communicated about improved farming methods (based on

[7] This would be especially problematic if DFs were informed about the time of the evaluation visit or the content of the knowledge exam. However, DFs neither knew the date of the visit nor details of the knowledge exam.

[8] The selected disseminating farmers who did not attend the training were spread across the three experimental arms. Of the three DFs who were not available for interviews during data collection, one had gotten a temporary job at an electricity dam constructed by the government, another had migrated to a neighboring Gulu town, and the third had been hospitalized. These three DFs were also from three different treatment arms.

Table 1. Baseline Characteristics by Treatment Group

	Training Only	Private Reward	Social Recognition
Panel A: Baseline individual and household characteristics			
Household head is male	0.820	0.791	0.817
	(0.384)	(0.407)	(0.387)
Age of household head (years)	44.084	44.548	42.778
	(16.080)	(15.644)	(14.216)
Household head's number of years of formal education	6.336	6.032	5.808
	(3.336)	(4.167)	(4.022)
Number of resident household members	5.603	5.870	5.841
	(2.317)	(2.576)	(2.331)
Dependency ratio	0.551	0.539	0.545
	(0.233)	(0.226)	(0.211)
The main activity of household head is farming	0.881	0.926	0.904
	(0.324)	(0.262)	(0.295)
Per capita household income	564,217	519,178	579,632
	(752,677)	(782,057)	(871,267)
Household dietary diversity score	6.813	6.608	6.679
	(1.522)	(1.554)	(1.553)
Agricultural assets index	0.064	−0.010	−0.063
	(4.200)	(4.513)	(4.326)
Livestock ownership (tropical livestock units)	0.779	0.661	0.640
	1.953)	(1.328)	(1.251)
Access to credit (1=yes; 0=no)	0.647	0.638	0.719
	(0.478)	(0.481)	(0.450)
Median social distance in education in the sub-village	2.938	3.269	3.409
	(1.272)	(1.307)	(1.382)
Median social distance in wealth index in the sub-village	3.023	3.052	3.377
	(0.908)	(0.952)	(0.991)
Panel B: Baseline social networks			
Number of agricultural information network links	2.018	1.907	1.857
	(1.009)	(1.066)	(1.445)
Number of kinship links outside the household but within the same sub-village	1.752	1.722	1.724
	(0.972)	(1.051)	(1.111)
Panel C: Baseline exposure to weather shocks			
Household has experienced droughts	0.956	0.944	0.953
	(0.206)	(0.230)	(0.212)
Panel D: Baseline sources of information			
Government extension	0.028	0.023	0.023
	(0.165)	(0.151)	(0.151)
Number of sub-villages (total = 132)	44	44	44
Number of observations	428	431	427
p-value for joint orthogonality test		0.227	

Note: Standard deviations appear in parentheses. The *p*-value for the joint orthogonality test is obtained from a multinomial logit regression of the treatment arms on the variables with robust standard errors clustered at the sub-village level.

survey data provided by co-villagers, not the DFs).

Finally, we organized an artefactual field experiment to measure altruism. As mentioned, intrinsic motivation may interact with extrinsic and reputation motives. Following Ashraf, Bandiera, and Jack (2014), we implemented a dictator game to elicit an incentive-compatible measure of pro-social motives. Each disseminating farmer received 5,000 Ugandan shillings, of which a fraction could be donated to a charity organization helping farmers to increase agricultural productivity and improve their lives.[9,10] We interpret the amount donated as a proxy for the DF's

[9] The exact script used in the adapted dictator game is available in online supplementary appendix C.

[10] USD 1 = UGX 3,000 during the time of our experiment.

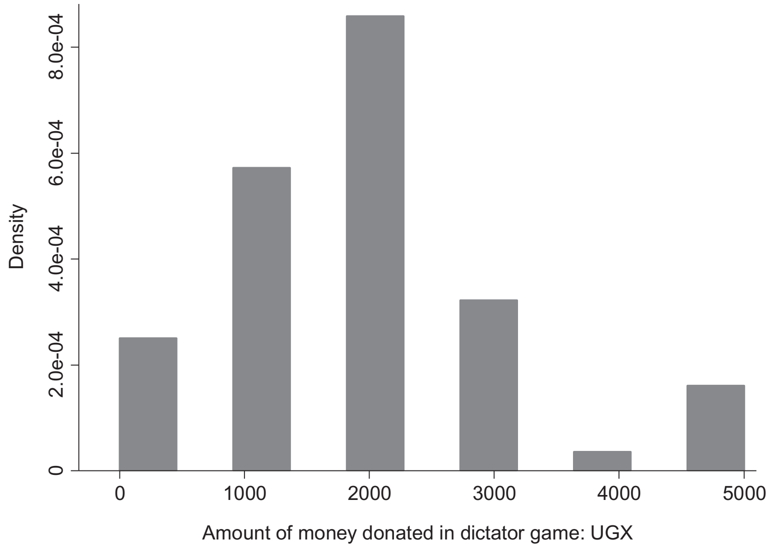

Figure 1. Distribution of the amount of money donated by disseminating farmers in the augmented dictator game

intrinsic motivation for the cause (see also Carpenter and Myers 2010). The average donation was UGX 1,900, with a median of UGX 2,000 (figure 1 shows the distribution of donation amounts). We assume that pro-social preferences are exogenous and do not vary with exposure to the training or experiment. A formal test (see online supplementary table A1) was performed to check whether the experiment affected the outcome of the pro-social preferences game. We cannot reject the null hypothesis that treatment did not affect the outcome of the games.

Empirical Estimation

We first examine the effect of incentives on the main outcomes of interest, using the following equation:

$$(1) \quad y_{ivc} = \alpha + \beta_1 private_{ivc} + \beta_2 social_{ivc} + \gamma_i W_{ivc} + \xi_c + \varepsilon_{ivc}$$

where y_{ivc} represents the outcome of interest for farmer i in sub-village v and sub-county c: the above-mentioned measures of *knowledge*, *experimentation*, or diffusion *effort*. The variables $private_{ivc}$ and $social_{ivc}$ denote the two treatment dummies, private material reward and social recognition, respectively, with the training-only group as comparison group. Next, W_{ivc} is a vector of individual characteristics, and ξ_c captures sub-county fixed effects. We use OLS to explain variation in knowledge (by DFs and other farmers), and use a probit model to analyze the DF's and other farmer's on-farm experimentation. For DF's training effort, we use a probit model for the dummy effort variable and OLS for the number of people with whom the DF communicated. Throughout we cluster standard errors at the sub-village level.

The coefficients β_1 and β_2 in equation (1) measure the causal effect of the incentive treatments on knowledge scores, experimentation and effort, under the identifying assumption that $private_{ivc}$ and $social_{ivc}$ are orthogonal to ε_{ivc}. Random assignment to treatment implies the identifying assumption is satisfied, unless there are substantial spillover effects (so that the SUTVA is violated). This might happen if DFs in the training-only group changed their behavior as a result of knowing that others had been offered rewards. Two design features were employed to minimize this risk: (*a*) we selected only one DF from each sub-village and hence there was only one treatment per sub-village; and (*b*) 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).[11] Training sessions for different treatment arms were organized

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

at different venues. Furthermore, sub-villages in northern Uganda, and Nwoya district specifically, are geographically dispersed. Still, we use Global Positioning System (GPS) coordinates of the sub-villages to test for evidence of spillovers across neighboring sub-villages. Online supplementary figure A1 (top panel) graphically shows the random assignment of treatments, whereas the lower panel shows sub-villages receiving different treatments but neighboring each other. Using a border-to-treatment dummy variable, a *t*-test indicates that control group DF effort was not significantly affected by the presence of a neighbor from another experimental arm (see online supplementary table A2).

Note that because we lack follow-up data in the villages where DFs did not attend training, our estimates are not intent-to-treat (ITT) effects. They should be close to the true ITT effects, however, since only six farmers dropped out and these were spread across the three experimental groups. Indeed, results from estimating Lee's lower bounds (Lee 2005) are very similar to the results we report below (see online supplementary table A3).[12]

Finally, we assess heterogeneity in the treatment effect of incentives. To evaluate the mediating effect of altruism on DFs' diffusion effort, we use donations in the dictator game to construct a dummy variable equal to one if the DF donated above the median amount, and zero if otherwise. The *pro-social* variable π is interacted with the treatment dummies and included in the DF effort equation:

$$(2)\quad effort_{ivc} = \alpha + \vartheta_1 private_{ivc} + \vartheta_2 social_{ivc} + \sigma_1 private_{ivc} * \pi_i + \sigma_2 social_{ivc} * \pi_i + \lambda \pi_i + \rho W_{ivc} + \xi_c + \varsigma_{ivc}.$$

Results and Discussion

This section presents the results of our empirical analysis. The section begins with findings about the effect of incentives on DFs' experimentation with the technologies, knowledge, and effort to train co-villagers. It then proceeds to present results of incentives on the knowledge of co-villagers and their adoption behavior. Finally, results of heterogeneous effects of incentive types by pro-social preferences and social distance are presented and discussed.

Incentives and DFs' Experimentation, Knowledge, and Diffusion Effort

Table 2 presents results of a series of OLS and probit regressions assessing the effect of incentives on DFs' experimentation with the technologies (columns 1–3), their retained knowledge six months after the training (column 4), and their diffusion effort (columns 5–6).

Considering on-farm experimentation with the new technologies, we find that the social recognition treatment increases the propensity to experiment with DT maize (column 1)—compared to the mean (0.025) for control group farmers, DFs incentivized with social recognition are 14 percentage points more likely to experiment with DT maize on their own farm. The impact of the private material reward is null. Disseminating farmers in this group are as likely as un-incentivized DFs to grow DT maize.

Social recognition also increases the likelihood of using improved maize varieties (other than DT maize, column 2) and CF basins (column 3). On average, the probability of growing improved maize varieties increases by 17 percentage points more for the social recognition reward arm relative to the mean (0.150) for the control group. Similarly, social recognition increases the probability of using CF basins by around 15 percentage points as compared to the comparison group (mean = 0.125). For these experimentation outcomes there are no differences between the private material reward and social recognition treatment, but again we observe that the effect of the private material reward incentive does not significantly differ from zero either.

Results in column 4 show that the incentive treatments did not affect DFs' level of knowledge. Remember that DFs were informed about the treatments after they completed their training, ruling out any impact on their knowledge accumulation during training. These results further indicate that knowledge

[12] It is important to mention that the problem we are dealing with here is non-compliance. Lack of data about the drop outs means that we are unable to estimate real ITT or LATE (using a random assignment as an instrument for treatment). Fortunately, the number of dropouts is very small: 6 out of 132 farmers. Lee's "lower bounds" for the impact (Lee 2005) were estimated by excluding the "worst realization" from the control group and the best ones from the "social recognition group".

Table 2. Incentives and Disseminating Farmers' Knowledge, On-farm Experimentation, and Diffusion Effort

Incentive type	On-farm Experimentation			Knowledge	Effort	
	DT maize	Improved maize	CF basin		Organized activity	Information exchange
	(1)	(2)	(3)	(4)	(5)	(6)
Training plus private reward (PR)	0.025	0.153	0.133	−0.118	0.209**	0.689**
	(0.073)	(0.097)	(0.085)	(0.231)	(0.089)	(0.282)
Training plus social recognition (SR)	0.136**	0.171*	0.147*	−0.064	0.244***	0.908***
	(0.057)	(0.096)	(0.082)	(0.231)	(0.083)	(0.300)
Household controls	Yes	Yes	Yes	Yes	Yes	Yes
Sub-county effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.340	0.168	0.158	0.037	0.139	0.169
Observations	123	123	123	123	123	123
Mean of dependent variable for non-incentivized DFs	0.025	0.150	0.125	0.054	0.075	1.225
	[0.158]	[0.362]	[0.335]	[1.071]	[0.267]	[1.050]
PR = SR (*p*-value)	0.067	0.836	0.857	0.814	0.674	0.513

Note: DF means disseminating farmer. Dependent variables are as follows: columns (1), (2), and (3) are dummy variables equal to one if the disseminating farmer (DF) tried out the technology on at least one of the household's plots, and zero otherwise; column (4) is the standardized knowledge score of the DF; column (5) is a dummy equal to one if the DF held at least one meeting or activity to train other farmers, and zero otherwise; column (6) measures the number of people in the sub-village with whom the DF communicated about improved farming methods. Robust standard errors corrected for sub-village level clustering are reported in parentheses. Square parentheses are the standard deviations of the control group means. Asterisks indicate the following: ***= $p < 0.01$, **= $p < 0.05$, and *= $p < 0.1$. Household controls include household head being male, age, education, and main economic activity of the household head, dependency ratio, livestock ownership, agricultural assets ownership, and access to credit. Columns (4) and (6) are OLS estimates. Columns (1), (2), (3), and (5) report average marginal effects from probit regression. DT maize means drought-tolerant maize; CF basin means conservation farming basin.

levels did not change differentially during the subsequent six months. The training included a practical session where, for example, spacing, number of seeds to sow in a hole, and length, width, and height of the CF basins was demonstrated in the field. The knowledge questions in the test focused on this sort of information, not on the practical knowledge that farmers acquire through on-farm experimentation. Hence, it is not surprising that test scores did not vary across treatment arms (i.e., did not improve with own on-farm experimentation).

Column 5 shows that both incentive types increase the probability that a DF organized an activity to train other farmers, compared to the training-only group. Both types of incentives are effective in stimulating DFs' diffusion activity. Specifically, DFs incentivized by a private material reward are 21 percentage points more likely than unincentivized DFs to train other farmers, and DFs incentivized by social recognition are 24 percentage points more likely to train other farmers. These outcomes are statistically identical. Observe that the size of the treatment effect, relative to the mean experimentation or effort level of the control group (0.075) is large. We find similar evidence for the effect of the incentives on the number of people a DF communicated with about improved farming methods (column 6). Specifically, the DF's out degree—the number of people to whom information was communicated—increased by 0.9 more in the social recognition treatment arm and 0.7 more in the private material arm, compared to the control group (mean = 1.225).

These findings support and extend insights of BenYishay and Mobarak (2018). Disseminating farmers respond strongly to incentives for diffusion. The findings are also consistent with Ashraf, Bandiera, and Lee (2014), as well as Carpenter and Myers (2010), who found that social recognition incentives may be as effective as private material rewards for promoting pro-social behavior. If anything, we find that social recognition may matter even more than private material rewards.

Results of the effect of incentives on the knowledge of "other farmers" and experimentation with the technologies are presented in table 3. Compared to respondents from training-only sub-villages, knowledge scores (column 1) increased by 0.42 standard deviations in the social recognition treatment arm (significant at the 10% level) and by a statistically insignificant 0.27 standard deviations in the private material reward

Table 3. Incentives and Other Farmers' Knowledge and On-farm Experimentation

Incentive type	Other Farmers' Knowledge	On-farm Experimentation		
		DT Maize	Improved Maize	CF Basin
	(1)	(2)	(3)	(4)
Training plus private reward (PR)	0.267	0.033	0.031	−0.045
	(0.210)	(0.030)	(0.043)	(0.032)
Training plus social recognition (SR)	0.413*	0.052	0.102*	−0.046
	(0.232)	(0.033)	(0.055)	(0.033)
Baseline knowledge score	0.030	-	-	-
	(0.037)			
Household controls	Yes	No	No	No
Sub-county fixed effects	Yes	Yes	Yes	Yes
R-squared	0.100	0.065	0.040	0.043
Observations	123	123	123	123
Mean of dependent variable for other farmers in sub-villages where DFs were not incentivized	−0.211	0.000	0.025	0.050
	[0.729]	[0.000]	[0.158]	[0.221]
PR = SR (*p*-value)	0.529	0.617	0.236	0.908

Note: DF means disseminating farmer. Dependent variables are as follows: column (1) is standardized knowledge scores of the other farmer (not DF); columns (2), (3), and (4) are dummy variables equal to one if another farmer (not the DF) tried out the technology on at least one of the household's plots, and zero 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. Asterisks indicate the following: ***= $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. Linear probability model (LPM) estimates for column (1) and average marginal effects from probit regression for columns (2–4). DT means drought-tolerant variety of maize (Longe 10H); CF basin means conservation farming basin.

arm. We acknowledge that DFs in both treatment arms could try to "incentivize" their co-villagers by promising access to the weighing scale after the experiment, and that this is likely to be stronger in the social recognition arm. The estimates therefore pick up the incentive effect for the DF as well as (any) incentive effect for co-villagers, and the latter may contribute to the difference between private reward and social recognition effects.[13]

The effects of both treatments on "other farmers' experimentation" are smaller, as expected, and only marginally significant in the social recognition arm and for improved varieties of maize generally (column 3). While we are able to pick up as small as 0.2 SD effect sizes, the effects are smaller than that. We find no significant effects on DT maize (column 2) and CF basin (column 4). Compared to the actual adoption by neighbors, DF effort and knowledge as well as other farmers' knowledge are more proximate outcomes with less confounding factors. Hence, one is more likely to see impact for these variables than for actual adoption.[14]

Heterogeneous Treatment Effects of Incentives

It is plausible that not all DFs are equally responsive to incentives. For example, in their

[13] We went back to the field in May, 2018 to collect additional data on how the weighing scales were being used (we held personal interviews with the DFs in the private reward arm, and with village leaders in the social recognition arm). We found that in both groups, the weighing scales still existed and were in 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 neighbors. Whereas relatives did not pay, neighbors 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 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.

[14] *Ex-post* power calculation assuming a 5% level of significance, 0.8 (80%) power, and a sample size of 246 revealed that the minimum detectable effect for DF experimentation with DT maize (private=0.057; social recognition=0.099), CF basins (private=0.141; social recognition=0.143), likelihood to train others (private=0.124; social recognition=0.126), information exchange (private=0.286; social recognition=0.287), and co-villager knowledge (private=0.307; social recognition=0.305). are in most cases smaller than the ones picked up in this study, and in all cases smaller for social recognition. However, for other farmers' actual experimentation with DT maize, we are able to pick up effects as small as 0.040 and 0.054 for private and social recognition larger than the actual effects that we actually estimate. Similarly, the minimum detectable effects for other farmers' experimentation with improved maize (private=0.069; social recognition=0.091) and CF basins (private=0.056; social recognition=0.056) are generally larger than the actual effects that we actually estimate.

Table 4. Heterogeneous Treatment Effects: Pro-social Preferences

	Organized Activity (1)	Information Exchange (2)	DT Maize (3)	Improved Maize (4)	CF Basin (5)	Other Farmers' Knowledge (6)
Private reward (PR)	0.184**	0.754**	0.020	0.182	0.097	0.340
	(0.091)	(0.317)	(0.050)	(0.112)	(0.097)	(0.252)
Social recognition (SR)	0.188**	0.794**	0.156*	0.255**	0.090	0.277
	(0.087)	(0.340)	(0.084)	(0.108)	(0.093)	(0.241)
Pro-social	−0.938***	0.677*	−0.074	0.182	0.013	0.039
	(0.161)	(0.382)	(0.050)	(0.162)	(0.147)	(0.228)
PR × *pro-social*	0.900***	−0.117	0.040	−0.124	0.150	−0.311
	(0.220)	(0.714)	(0.073)	(0.230)	(0.196)	(0.417)
SR × *pro-social*	1.010***	0.268	0.034	−0.317	0.170	0.471
	(0.209)	(0.620)	(0.129)	(0.206)	(0.184)	(0.498)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sub-county effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.156	0.220	0.178	0.184	0.163	0.136
Observations	123	123	123	123	123	123
p-value (PR × pro-social) = (SR × pro-social)	0.565	0.629	0.967	0.352	0.188	0.169

Note: Average marginal effects. The variable *Pro-social* is a dummy variable equal to one if the DF's donation in the dictator game was above the median donation. Controls include household head is male, age, education, and main economic activity of the household head, dependency ratio, livestock ownership, agricultural assets ownership, and access to credit. Robust standard errors corrected for sub-village level clustering (123) reported in parentheses. Asterisks indicate the following: ***= p < 0.01, **= p < 0.05, *= p < 0.1. DT means drought-tolerant variety of maize (Longe 10H); CF basin means conservation farming basin.

study of promoting health-related pro-social behavior, Ashraf, Bandiera, and Jack (2014) found that the effects of private material rewards and social recognition are stronger for intrinsically altruistic subjects. We now analyze whether this result extends to the domain of agricultural knowledge diffusion. We first ask whether the impact of incentives on the propensity to invest effort in knowledge diffusion is mediated by pro-social preferences, and whether external incentives may "crowd out" altruism—as sometimes proposed in the literature. Specifically, if altruism leverages the impact of incentives then we expect the interaction of our altruism variable and the incentive (treatment) dummies to enter with a positive sign and significantly. Instead, if incentives crowd out altruism, then we expect that the altruism variable enters with a positive sign (level effect), but that the interaction between altruism and incentive dummies enters with negative signs.

Results are reported in table 4, where we use a dummy variable equal to one if the DF donated above the median amount of money, and zero if otherwise, as a proxy for pro-social preferences or altruism. We find that the interaction between pro-social preferences and incentives—consider the terms *PR* × *pro-social* and *SR* × *pro-social*—is not significant at the 10% level for DF's experimentation with the technologies (columns 3–5) and the knowledge of other farmers (column 6). Looking at effort expended by DFs to hold activities and train other farmers, the interaction between pro-social preferences and incentives is positive and statistically significant at the 1% level (column 1). However, we also find a significant and negative *level effect* of pro-social preferences (column 1). More altruistic farmers spend, on average, less effort organizing training activities. The interaction terms and level effect are statistically of the same magnitude, but have opposite signs, meaning that the positive effect of the interaction terms is cancelled out by the negative effect of the level pro-social variable. The effect of incentives on DF's effort and actual experimentation with the technologies does not, therefore, seem to be mediated by pro-social preferences.[15] The effect

[15] Lack of a differential effect should be regarded with caution as it is possible that the study did not have enough power to pick such effects. *Ex-post* power analysis indicates that except for the likelihood of a DF to train co-villagers, the study is under-powered for the rest of the outcomes. The minimum detectable effects results are as follows: likelihood to train co-villagers (private*pro-social=0.286; social recognition*pro-social=0.340); information exchange (private*pro-social=1.225; social recognition*pro-social=1.163); DT maize (private*pro-social=0.104; social

of the interaction terms on the number of people that the DF informed about the technologies is also not statistically significant at tje 10% level (column 2).

First, consider the negative effect of altruism: in the absence of incentives, why are more altruistic DFs less likely to invest effort in training their peers? This finding is consistent with our understanding of heterogeneity in farm productivity and the low quality of agricultural inputs in Africa. We speculate that altruistic farmers who lack confidence in the profitability of new technologies for their co-villagers should not diffuse information. Such a lack of confidence in overall profitability may follow from three reasons. (*a*) Heterogeneity in production conditions imply that the same technology will not be profitable for all farmers—even within the same village (Suri 2011)—this is especially likely for labor-intensive (or costly) innovations such as the construction of CF basins; (*b*) drought-tolerant seeds might not have a yield advantage over other improved varieties, or might even have a yield penalty in normal years (Holden and Fisher 2015); (*c*) there exists a major problem of counterfeit inputs in northern Uganda. In a recent study, Bold et al. (2017) find that 30% of nutrients are missing in chemical fertilizer, and samples of hybrid maize were estimated to contain less than 50% of improved seeds (presumably due to extensive adulteration). These authors find that, on average, low quality inputs result in near zero average rates of return in Uganda.

In light of these observations it seems reasonable for DFs to question whether adopting these innovations is actually welfare-improving for all co-villagers. Instead, it may be optimal to delay transmission of the relevant information until after additional information has become available. Such a cautionary response can, however, be overwhelmed by incentives. If DFs are incentivized to diffuse information they choose *not* to delay transmission, and behave like their non-altruistic peers. In an effort to gain the material reward or social recognition, they seem willing to take the risk of spreading information that is potentially not useful to their peers. Extrinsic and intrinsic motives therefore work in opposite directions if the net benefits of new technologies are uncertain, and can offset each other.

The finding of a positive effect of pro-social preferences on the number of people that the DF talked with about the technologies perhaps suggests that while altruistic DFs may be reluctant to demonstrate the use of new technologies to their peers, they may consider it harmless to make them aware of such technologies. Altruistic DFs may also talk to their peers about the new technologies because they enjoy interacting with them, or to "send a signal" that they are not withholding information that could potentially be relevant for them. The interaction terms are, however, not statistically different from zero.

Finally, we examine heterogeneous treatment effects on DF effort (whether they organized an activity and the number of people communicated with) by social distance. Motivated by the selection criteria for the DFs, we consider two social distance variables, namely wealth status and education. The social distance variables are measured based on baseline data as follows. First, we construct dyadic pairs for each of the respondents in a sub-village who were interviewed at baseline. Next, for each dyadic pair, we compute the absolute difference in wealth status (household assets index) and education. We then calculate the median distance for each sub-village and variable, and observe how close or far the absolute distance between the DFs and their neighbors is from the median distance in the sub-village. This allows us to capture heterogeneity in distance in the sub-village: in other words, we control for the possibility that in a sub-village, a wide social distance between the DF and the neighbor might simply reflect an existing wide median distance in the sub-village.

Results show that treatment effects do not vary with social distance in terms of wealth status, as interaction terms of both treatments with social distance are insignificant (table 5, columns 1 and 3). In terms of distance in education, we find that greater distance increases the effect of the private reward incentive on the probability of DFs organizing a training activity (table 5, column 2), but has no interaction with treatment effects on the other effort variable (columns 3 and 4). Overall,

recognition*pro-social=0.274); improved maize (private*pro-social=0.257; social recognition*pro-social=0.338); CF basin (private*pro-social=0.408; social recognition*pro-social=0.397); knowledge of co-villagers (private*pro-social=0.644; social recognition*pro-social=1.092).

Table 5. Heterogeneous Treatment Effects: Social Distance

	Organized Activity		Information Exchange	
	(1)	(2)	(3)	(4)
Private reward (PR)	0.200	0.076	1.149***	0.157
	(0.125)	(0.131)	(0.434)	(0.524)
Social recognition (SR)	0.215*	0.183*	0.960**	0.982*
	(0.110)	(0.108)	(0.421)	(0.506)
DistHHassets index	−0.019		−0.011	
	(0.022)		(0.087)	
PR × *DistHHassets index*	0.002		−0.174	
	(0.036)		(0.133)	
SR × *DistHHassets index*	0.007		−0.031	
	(0.035)		(0.146)	
DistHHHeduc		−0.048**		−0.164**
		(0.024)		(0.079)
PR × *DistHHHeduc*		0.063*		0.199
		(0.037)		(0.165)
SR × *DistHHHeduc*		0.039		0.013
		(0.027)		(0.102)
Controls	Yes	Yes	Yes	Yes
Sub-county fixed effects	Yes	Yes	Yes	Yes
R-squared	0.146	0.155	0.192	0.210
Observations	123	123	123	123
p-value (PR × social distance) = (SR × social distance)	0.911	0.488	0.363	0.258

Note: Average marginal effects. Dependent variable for column (1) and (2) is a dummy equal to one if the disseminating farmer (DF) held at least one meeting or activity to train other farmers, and zero otherwise, whereas in columns (3) and (4) the dependent variable measures the number of people in the sub-village with whom the DF communicated about improved farming methods *DistHHassets* index and *DistHHHeduc* measure social distance in terms of household assets (wealth status) and education, respectively. Household controls include household head being male, age, education, and main economic activity of the household head, dependency ratio, livestock ownership, agricultural assets ownership, and access to credit. Robust standard errors corrected for sub-village level clustering (123) reported in parentheses. Asterisks indicate the following: ***= $p < 0.01$, **= $p < 0.05$, *= $p < 0.1$.

therefore, we find no evidence that the effect of incentives increases with smaller social distance between DFs and their co-villagers.

Conclusion

Effective approaches to alleviate poverty in sub-Saharan Africa will require agricultural intensification, but a key concern is how to promote the adoption of modern production techniques that are more productive and resilient. Conventional extension efforts have by and large failed to reach large swaths of the rural population, and the search is on for innovative approaches to stimulate the diffusion of information about agricultural innovations. Social learning has long since been an important component of such efforts, but it is increasingly clear that diffusion of information within social networks may neither be easy nor “automatic.” In contexts where individual farmers stand to gain little from spreading information but expect to pay a positive (effort) cost, diffusion is often slow and imperfect. Incentivizing farmers to engage in diffusion represents one potential solution.

In this paper we use an experimental approach to study the effects of incentivizing farmers to allocate effort to the diffusion of information. Incentivizing can happen in different forms, and we consider two types of “extrinsic rewards” for effective information sharing: a private material reward for the disseminating farmer (DF), and an intervention that aims to build the reputation of the DF within his or her community (“social recognition”). As a material reward we used a weighing scale, and we focus on the diffusion of knowledge about climate-smart agricultural practices. We find that reputation building may be a particularly effective way to promote diffusion—while a private material reward had small effects on diffusion, the same reward given to “the community” in a public ceremony celebrating the efforts of the

DF effectively pushed up DFs' own experimentation, diffusion effort, and actual information transmission. We believe this result speaks to the importance of social recognition or status for rural livelihoods in Africa.

A large body of literature studies the interaction between different motives for prosocial behavior, and in particular asks whether extrinsic motives (private rewards or "reputation building") may interact with intrinsic motives. Indeed, in theory it would be possible that providing extrinsic rewards reduces the diffusion of information if the "crowding out effect" is sufficiently large and dominates the direct incentive effect. However, our data are not consistent with such outcomes.

We hope the results in this paper can guide thinking about effective ways to promote the diffusion of information. The main policy message is that including incentives in extension schemes may be welfare-enhancing. However, this begs the question about scalability—can extension approaches based on incentives be scaled across larger landscapes, and how can first-order beneficiaries in turn be incentivized to reach out to second-order beneficiaries, and so on? Additional experimentation with innovative approaches is presumably necessary for this. An auxiliary policy message concerns the perceived low quality of agricultural inputs. Bold et al. (2017) correctly identify that poor handling and adulteration reduce the rate of return of adopting these inputs. Our results suggest that low input quality may also attenuate incentives to share information in social networks. Addressing the issue of low-quality inputs may therefore have beneficial effects along multiple dimensions.

Two caveats to our experimental design and results are noteworthy. First, as mentioned earlier, while excluding a pure control group was necessary in order to increase statistical power, the pitfall is that our estimates do not capture the full impacts of training with private material rewards and training with social recognition on experimentation and diffusion effort. Second, we did not inform DFs about the nature of their potential reward until the end of the training. Doing so could have affected effort during the training (as documented by BenYishay and Mobarak 2018 and Sseruyange and Bulte 2018). Because of this reason, our approach implies we potentially underestimate the true effect of the incentives.

Supplementary Material

Supplementary materials are available at *American Journal of Agricultural Economics* online.

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