The Monetary Value of Externalities: Experimental Evidence from Ugandan Farmers*

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March 9, 2024

Abstract

Optimal targeting often requires externalities to be priced correctly. I design an experiment to measure the monetary value of the externalities of an agricultural pest-control technology; I elicit a farmer's willingness-to-pay to induce another farmer to adopt the technology, a measure of the externalities generated by the induced farmer. Externalities are large, as mean willingness-to-pay for others is equal to two days' wage, half the willingness-to-pay for oneself. A randomized intervention shows willingness-to-pay for others depends on how much they affect a farmer's pest-infection status. Willingness-to-pay is higher for farmers who are likely to disseminate knowledge of the technology.

JEL Codes: O12, Q12, Q16.

Keywords: externalities, technology adoption, agriculture, Becker-DeGroot-Marschak, field experiments, social networks.

*I thank the Handelsbankens Research Foundation and Vetenskapsrådet for generous financial support; Charles Angebault and Apollo Tumusiime for their collaborative efforts throughout the project; seminar participants at Bocconi University, Oxford University, Paris School of Economics, Stockholm University, University of California Berkeley, and World Bank for providing useful comments; Ingvild Almas, Tessa Bold, Konrad Burchardi, Jonathan de Quidt, Dahyeon Jeong, Florence Kondylis, John Loeser, Anna Tompsett, Jakob Svensson, Hannah Uckat for their helpful feedback. The experiment was preregistered on the AEA RCT registry. All errors and omissions are the responsibility of the author alone. The findings, interpretations, and conclusions expressed in this paper are entirely those of the author. They do not necessarily represent the views of the World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

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1 Introduction

Technology adoption can generate externalities whose monetary value is difficult to measure: since they are not sold on the market, they lack a price tag. Yet, knowing the value of externalities is crucial for two reasons. First, it helps governments decide *whether* to intervene: an intervention may be worth it only once its social benefit exceeds a certain threshold. Second, if the threshold is met, knowing how externalities are produced helps governments decide *how* to intervene: typically governments have a menu of policies to choose from, and may want to prioritize those with the highest social benefits.

Externalities are commonplace, and notably in agriculture. Many of the agricultural technologies available to farmers generate positive externalities, like pest control or wildfire prevention, that cause equilibrium adoption rates to be lower than socially optimal if agents do not internalize the social benefit of adoption (Pigou, 1920); this may contribute to explaining the persistent low adoption rates observed in many lower income countries (Bandiera and Rasul, 2006; Foster and Rosenzweig, 1995; World Bank, 2007).

In this paper I design a field experiment to uncover how much the externalities of one technology are worth and what governments should do to harness their benefits. Specifically, I study the externalities of a new agricultural pest-control technology in Uganda, consisting of a training on pest-control practices for a new pest. I measure the externalities of the training by asking farmers how much they are willing to pay for *only one specific farmer* in their village to be trained on pest-control practices for the pest fall armyworm. The willingness-to-pay elicitation mechanism, a variant of the Becker-deGroot-Marschak or BDM (Becker et al., 1964), is incentive-compatible and yields a measure of the externalities generated by one trained farmer keeping others' actions constant.[1] The feature of keeping others' actions constant is achieved by allowing a maximum of one farmer to receive training per village through a lottery; it allows to shut down complementarities and substitutabilities in demand that would confound the observed willingness-to-pay for others. The upside of such a design choice is that it ensures I measure the externalities produced by one farmer (at the margin) neatly, although at the expense of measuring how externalities change when more than one farmer is trained in a village. I find farmers anticipate substantial positive spillovers from others: the median value a farmer attaches to another farmer receiving the training is equivalent to the wage received from two days of agricultural labor (or $8.6), about half the median willingness-to-pay for themselves (equal to $17.3).

I use my valuation data to show that targeting choices can produce large and quantifiable social gains if they account for externalities. I identify which targeting criterion for the training produces a higher social benefit in two steps. First, I calculate the per capita externality generated by training one farmer - the average willingness-to-pay of

[1] This setup rests on the assumption, supported by the data, that farmers are self-interested and do not engage in altruistic behavior in this setting.

others for that farmer - and find it is heterogeneous, hence the data suggests the identity of the trained farmer matters.[2] On average, within a village, the farmer with the highest per capita externality generates double the amount than the farmer with the lowest per capita externality ($7.9 vs $4.5). Second, I use the per capita externality to calculate the social benefit generated by training one farmer[3] and horserace two targeting criteria for the training: one in which the farmer with the highest willingness-to-pay for self receives the training, and one in which the farmer with the highest per capita externality receives the training. I find that the latter criterion outperforms the former; providing training to the highest-externality farmer in a village generates $455 in social benefit, 26% more on average than providing training to the highest-willingness-to-pay farmer (which generates $362).

I implement two randomized interventions to illustrate that the ways externalities originate - their sources - cause some farmers to be more socially valuable than others; innovatively, I embed the interventions in my willingness-to-pay elicitation exercise. I study how two sources of positive externalities, contagion and knowledge, affect willingness-to-pay for others. I introduce randomized variation in farmers' beliefs over the two sources of externalities and observe how they adjust their willingness-to-pay for others in response. *Contagion externalities* occur when others face a lower risk of infection due to a farmer reducing the pest population in her own farm; they are a function of distance between plots (the shorter the distance, the higher the contagion externalities (Li et al., 2019; Pannuti et al., 2016). *Knowledge externalities* occur when knowledge spills over from a trained farmer to others in the village; they are a function of how much farmers interact as the more farmers interact, the likelier the knowledge transfer (Bandiera and Rasul, 2006; Conley and Udry, 2010; Foster and Rosenzweig, 1995; Munshi, 2004).

To identify contagion externalities I introduce a random shock to farmers' beliefs about plot distance; I randomize whether or not a farmer receives truthful information about the walking distance between own plot and the plot of another farmer before formulating her valuation for the other farmer. In 95% of the cases farmers are wrong about the true distance, typically overestimating it (when farmers overestimate, they do so by 14 minutes on average; when they underestimate, they do so by 8 minutes on average). To identify knowledge externalities I introduce a random shock to farmers' beliefs about future social interactions; I randomize whether or not a farmer receives an invitation to a one-on-one meeting with another farmer before formulating their valuation for the other farmer. I expect this to induce the participant to update own beliefs over future knowledge spillovers

[2]In the data heterogeneity is both demand-driven (different farmers value training the same person differently) and supply-driven (the same farmer values training different people differently). The coefficient of variation of farmers' willingness-to-pay for the same person is 74%, while the coefficient of variation of a farmer's willingness-to-pay for different people is 35%, showing that demand-driven heterogeneity plays a larger role than supply-driven heterogeneity.

[3]Which I define as the per capita externality multiplied by the number of farmers in a community, plus the farmer's willingness-to-pay for self.

between them.

I find participants respond to contagion externalities. When they learn that someone else's plot is further away - so contagion externalities are lower than believed - they decrease their valuation for that farmer by 11% of a daily wage. This indicates that the spatial distribution of plots indeed matters for targeting. My intervention is instead not conclusive about knowledge externalities, but I provide suggestive evidence that participants place a higher value on training farmers who are more likely to transfer knowledge to them. This indicates that social connections matter for knowledge diffusion, lending further evidence to policies targeting socially central individuals. Taken together, my results indicate that policies aimed at fostering technology adoption should be targeted towards socially central farmers with centrally located plots.

Note that farmers may have many neighbors, and the *direct* contagion risk[4] represented by one neighbor is a small share of the overall contagion risk a farmer is exposed to; as such, one may expect the direct contagion externalities generated by one neighbor to be low, and may consider the impact I find on valuations to be high. It is plausible this impact represents an upper bound of the true effect of contagion externalities on willingness to pay, and that it either incorporates *indirect* contagion externalities or some knowledge spillovers - due to, for example, higher anticipated learning costs once a participant has learned the other farmer's plots is further away than initially believed. Shutting down all "organic" knowledge spillovers is infeasible in my experiment; I would argue it is also undesirable from a policy standpoint, as it would exclusively generalize to interventions taking place in societies without communication. Further, indirect effects are likely to be small for contagion externalities and larger for knowledge externalities; the design choice of letting participants know that one farmer per village will be trained at best aims at controlling the impact of anticipated second order knowledge spillovers on valuations.[5]

This paper makes six contributions. First, I devise a technique to credibly price externalities, a key economic quantity that is not marketed by definition. Studies on the measurement of the monetary value of non-marketed goods have largely relied on contingent valuations. Contingent valuations have been at the center of a long academic debate, mainly due to their vulnerability to *hypothetical response bias*, which leads respondents to overestimate the value of hypothetical items they have no market experience of (Diamond and Hausman, 1994; Hanemann, 1994; Portney, 1994; Carson, 2012; Hausman, 2012; Kling et al., 2012). My paper overcomes this bias by eliciting valuations that are incentive-compatible and have real stakes. The elicitation technique I adopt is a multiple-price-list (MPL) variant of the standard BDM (Becker et al., 1964)[6], shown to perform

[4]As opposed to the indirect or second-order contagion risk, coming from direct impacts on other farmers.

[5]Beaman et al. (2021) find that information on agricultural technology is less likely to diffuse when only one farmer is informed.

[6]Standard BDM requires a participant to formulate her maximum willingness-to-pay for an item and

just as well as BDM for demand estimation in the field (Berry et al., 2020; Burchardi et al., 2021). In my elicitation exercise I collect valuations from six to nine sampled farmers per village in 103 villages in the Eastern region of Uganda (780 farmers in total), and each sampled farmer formulates one valuation for herself and one for every other sampled farmer in her village[7]. I use a random lottery incentive to ensure that the valuations formulated by a farmer are independent from each other. I inform farmers that only one among all of the valuations in their village will be randomized for implementation, resulting in maximum one trainee per village. The random lottery incentive allows me to measure the willingness-to-pay of a participant to change one farmer's actions at a time, *ceteris paribus*; it rules out complementarities or substitutabilities in demand that would arise if more than one farmer could be trained or the trainee were not selected randomly.[8]

Second, I quantify the importance of two types of externalities: contagion and knowledge externalities. Contagion externalities have been shown to affect take up of health products such as deworming medications or vaccinations (Miguel and Kremer, 2004; Boulier et al., 2007; White, 2019) and agricultural practices such as wildfire prevention (Crowley et al., 2009); knowledge externalities have been shown to affect take-up of agricultural technology as farmers do not fully internalize the positive effects of their learning on others' learning when making adoption decisions (Bandiera and Rasul, 2006; Conley and Udry, 2010; Foster and Rosenzweig, 1995; Munshi, 2004). My work complements these findings by measuring how much farmers value contagion and knowledge externalities in monetary terms, and by showing farmers take the identity of the adopter into account. Further, I confirm knowledge externalities are important in this setting by comparing the adoption rate of trained and untrained farmers in the same village; if training increases adoption by a similar amount among trained and untrained farmers, it means knowledge spills over from trained to untrained farmers. In my setting the random selection of one person to receive the training in a village means training is randomly assigned at the village level, conditional on the willingness-to-pay of the farmer whose valuation is selected. Training takes place in about two thirds of the villages (64 out of 103), and it results in a 15% increase in adoption among the trained farmers and a 14% increase in adoption among the untrained farmers in the same village, suggesting knowledge diffuses.

Third, I estimate the social benefit of alternative targeting policies and the losses in-

then the researcher to draw a random price. If the willingness-to-pay is at least as large as the random price, the participant has to buy the item paying the random price; if it is lower, the participant cannot buy the item. Just like in a second price auction, it is in the best interest of the participant to state her maximum willingness-to-pay truthfully. In addition, valuations obtained with this technique are equivalent to revealed preferences because they have real stakes.

[7]When formulating willingness-to-pay for another farmer, participants know which other farmers they are making the valuation for.

[8]Complementarities or substitutabilities in demand are central concepts in the study of externalities, and a key element to guide policies such as optimal subsidies. Gautam (2023) and Guiteras et al. (2019) provide recent evidence on the impact of providing subsidized access to sanitation, a technology with high contagion externalities.

curred by society if program targeting disregards externalities. A consistent finding in the social learning literature is that targeting matters for diffusion and, in some cases, for adoption; Beaman et al. (2021) show that targeting farmers using a social network model with complex contagion[9] performs better for diffusion and adoption than having extension workers choose who to target in Malawi; targeting farmers who are similar to the average farmer increase diffusion and adoption more than targeting wealthier farmers in Malawi and Uganda (BenYishay and Mobarak, 2018; Vasilaky and Leonard, 2018), but not in Mozambique (Kondylis et al., 2017); targeting seeds identified through a peer nomination process can diffuse simple bits of information in small communities (Banerjee et al., 2019; Kim et al., 2015). The social learning literature mostly focusses on identifying targeting strategies that maximize diffusion and adoption; my contribution consists in identifying whether a program is worth investing in or not depending on the social benefits, including those accruing to diffusion, of the chosen targeting strategy. I confirm targeting policies are key determinants of social outcomes and show the measurement of externalities via willingness-to-pay enhances government interventions and targeting in three ways: (i) by providing an intuitive tool to aggregate individual preferences over a social program; (ii) by identifying the monetary value attached by society to a social program with a specific targeting approach; (iii) by allowing the comparison and ranking of different targeting approaches at once.

Fourth, I show that in-person agricultural extension services are valuable to, and valued by, farmers. The traditional agricultural extension approach of "Training and Visit" (T&V) has been criticized for being financially unsustainable and causing at best modest improvements in adoption (Gautam, 2000; Krishnan and Patnam, 2014; Udry, 2010), spurring the growth of more cost-effective phone-based extension platforms (Fabregas et al., 2019). Cole and Fernando (2020) and Hidrobo et al. (2022) estimate farmers' monthly willingness-to-pay for phone-based extension services to be about \$0.22 in India and \$0.4 in Ghana respectively, noting how positive externalities may depress willingness to pay in their setting as farmers free-ride on others. My project backs out demand for in-person extension services at different price points, finding the service pays for itself only when positive externalities generate large social benefits because farmers value individual training highly but below cost (training one farmers costs \$29.4 in my setting, much less than the average willingness-to-pay).

Fifth, I use an incentive-compatible mechanism in the field to identify the ideal beneficiaries of a policy. A growing literature on mechanism design exploits truthful reporting mechanisms to target development interventions. Both Rigol and Roth (2016) and Hussam et al. (2021) study the feasibility of truth-telling mechanisms in the field, specifically peer prediction methods, and show they yield accurate reports and are empirically

[9]Complex contagion requires a farmer to be connected to at least two knowledgeable farmers to start a diffusion process, while one connection is sufficient for a diffusion process with simple contagion.

incentive-compatible. Similarly, I exploit a lab-in-the-field methodology to identify the farmers whose adoption decisions generate the highest social benefit.

Lastly, I connect a key issue in economics, the measurement of externalities, with a prominent contemporary policy challenge, the low agricultural technology adoption in lower income agrarian economies. The puzzle of underadoption in developing countries has been widely studied and shown to be tied to multiple failures, including input market failures (Bold et al., 2017), uninsured risk and liquidity constraints (Karlan et al., 2014), behavioral biases (Duflo et al., 2011), moral hazard (Burchardi et al., 2019). In the context of the underadoption puzzle, this paper shows how policy makers can leverage positive externalities to modernize agricultural production.

2 Setting

The America-native fall armyworm (*Spodoptera Frugiperda*) was first recorded in Africa in 2016 and in Uganda in early 2017. The pest feeds on several crops in the Americas and Europe, but in sub-Saharan Africa it mainly affects maize. FAO estimates the fall armyworm caused maize losses of about 1% of national GDP in Uganda in the first cropping season of 2017[10]. Maize is the most cultivated cereal and third-most cultivated crop in the country, and the Eastern region (one of the four regions of Uganda) produces over half of the total yearly maize (UBOS, 2010). Tackling the fall armyworm infestation has been a policy priority among both international and local organizations working in agriculture since the pest was detected in the country.

The fall armyworm is more destructive in tropical rather than temperate climates because of a shorter life cycle allowing the pest to reproduce earlier and its population to grow larger than in cooler climates. It has five development stages: egg, larva, caterpillar, pupa, and moth. The larva and caterpillar stages are the most damaging for the host plant as the pest feeds on both leaves and cob, while in the moth stage the pest spreads geographically (as female moths fly during the night to lay eggs).

By the time of my elicitation exercise farmers in my sample had been exposed to the fall armyworm for three cropping seasons, and reported 41% of their plots had been infected in the latest agricultural season (56% if we restrict the sample to maize plots). The high pest prevalence can be partly explained by the fact that practices to handle the fall armyworm are pest-specific and farmers would not be able to infer them without guidance from agronomists; at the same time, their execution is mostly labor-intensive and farmers would be able to carry them out on their own once trained. The main practices recommended by the Ugandan government are: plant early and uniformly, scout the field every three days, handpick and crush the eggs and caterpillars, pour sand and ash on infected plants, spray pesticide two to three times per season, use fertilizer, deep bury the infected plant residue in soil. Informational interviews carried out before the elicitation suggest

[10] www.fao.org

farmers did not know which practices would be effective to handle the fall armyworm and had not been in contact with a government extension officer about the issue;[11] some farmers reported adopting practices that had not been recommended by the government, such as spraying their plants with laundry detergent or paraffin.

3 Conceptual Framework

This section develops a simple model to highlight the parameters that determine willingness-to-pay for self and for others. A farmer i chooses whether to take or not a binary action $l_i \in \{0, 1\}$ to maximize net utility of avoiding a pest infestation. In an economy with no externalities farmer i solves

$$\arg\max_{l_i} \Pi_1 = (U_i - c - k_i)l_i, \tag{1}$$

where U_i is the utility of avoiding infection, c is the cost of acquiring the knowledge-based technology and k_i is the individual cost of implementing the recommended pest-management techniques. Information and contagion externalities allow i to learn from other farmers and face lower infection risk. All farmers in the village take adoption decisions simultaneously.

The parameter governing *knowledge* externalities is p^L, the probability that i learns from at least another farmer in the village,

$$p^L = 1 - \sum_{j \neq i} (1 - p^l_{ij})l_j \tag{2}$$

where p^l_{ij} is the probability that i learns from j. The utility value of the knowledge externality is measured by $(U_i - k_i)p^L$, the product of the net utility of avoidance times the probability of learning.

The parameter governing *contagion* externalities is $(1 - p^T)$, the probability that i is not infected by any other farmer j,

$$(1 - p^T) = \prod_{j \neq i} (1 - p^t_{ij}(1 - l_j)) \tag{3}$$

where p^t_{ij} is the reduced-form probability that i is infected by j, and trivially (1-p^t_{ij}) is the probability that i is not infected by j. Every adopting farmer j generates a positive contagion externality for i by reducing the pest load of the environment and increasing the probability that i avoids infection (or equivalently, decreasing the probability that i is infected). The utility value of the contagion externality is measured by $U_i(1 - p^T)$. It

[11] In general farmers reported they have seldom contact with government extension officers, whose activities had almost exclusively consisted of seeds distribution in their area.

is the product of avoidance utility U_i times the probability that farmer i does not get infected by any other farmer j. If $l_j = 0$, i avoids infection from j with probability $(1 - p_{ij}^t)$. If $l_j = 1$, i avoids infection from j with probability 1.
I make the two following assumptions. First, the technology works perfectly.

A1: If a farmer implements the technology, her plots are immune from the pest.

Second, once a farmer has learned the technology from another farmer, the utility from implementing the technology is higher than the utility from contagion externalities.

A2: The utility of implementing the technology is larger than the utility of contagion externalities

$$U_i - k_i \geq U_i(1 - p^T)$$

Trivially, if no $j \neq i$ farmer adopts the technology in the village, $\sum_{j \neq i} l_j = 0$, there are no externalities and farmer i's problem is just (1). If at least another farmer in the village chooses to adopt, such that $\sum_{j \neq i} l_j \geq 1$, farmer i's problem is

$$\max_{l_i} \Pi_2 = l_i(U_i - c - k_i) + (1 - l_i)[(U_i - k_i)p^L + (1 - p^L)U_i(1 - p^T)], \tag{4}$$

where the farmer can choose to adopt $l_i = 1$ and have profit $(U_i - c - k_i)$, or not adopt. If i does not adopt she can learn from others at no cost with probability p^L and not benefit from contagion externalities, yielding profit $(U_i - k_i)$, or not learn and benefit from contagion externalities with probability $(1 - p^T)$, yielding profit U_i.

In the experiment, the setting above is modified in the following ways:

1. There is a set of farmers $F = (i, j, \ldots, N)$
2. A maximum of 1 farmer can acquire the technology.
3. A participant i is either in a state of the world in which only i can acquire the technology $(l_{-i} = 0)$, or in a state of the world in which only j can acquire it $(l_{-j} = 0)$,
4. A participant i decides whether to acquire technology for herself in the state of the world $l_{-i} = 0$; she decides whether to acquire technology for j in the state of the world $l_{-j} = 0$,
5. A participant chooses her maximum willingness-to-pay for herself to adopt, w_{ii}, in

the state of the world $l_{-i} = 0$, or chooses her maximum willingness-to-pay for j to adopt, w_{ij}, in the state of the world $l_{-j} = 0$.

6. There is no cost to acquiring the technology, $c = 0$

Then equations 1 and 4 become

$$\max_{w_{ii}} \Pi_1 = U_i - k_i, \quad \text{if } l_{-i} = 0 \tag{5}$$

$$\max_{w_{ij}} \Pi_2 = (U_i - k_i)p_{ij}^l + (1 - p_{ij}^l)U_i(1 - p^T) \text{ if } l_{-j} = 0 \tag{6}$$

Suppose the state of the world is $l_{-i} = 0$. Then, a rational farmer will set her w_{ii} equal to the difference between the profits she would obtain if she chooses to adopt $\Pi_1(l_i = 1|l_j = 0)$ and the profits she would obtain if she chooses not to adopt $\Pi_1(l_i = 0|l_j = 0)$, yielding

$$w_{ii} = \Pi_1(l_i = 1|l_{-i} = 0) - \Pi_1(l_i = 0|l_{-i} = 0) = U_i - k_i, \tag{7}$$

which shows w_{ii} is equal to the utility of avoiding infection minus the cost of implementing the techniques.
Suppose the state of the world is $l_{-j} = 0$. Then a rational farmer will set her w_{ij} equal to the difference in profits if she chooses that j should adopt versus not adopt,

$$\begin{aligned} w_{ij} &= \Pi_2(l_j = 1|l_{-j} = 0) - \Pi_2(l_j = 0|l_{-j} = 0) \\ &= (U_i - k_i)p_{ij}^l + (1 - p_{ij}^l)U_i[\prod_{m \neq i,j}(1 - p_{im}^t) - (1 - p_{ij}^t)\prod_{m \neq i,j}(1 - p_{im}^t)] \\ &= (U_i - k_i)p_{ij}^l + (1 - p_{ij}^l)U_i \prod_{m \neq i,j}(1 - p_{im}^t)p_{ij}^t, \end{aligned} \tag{8}$$

where $\prod_{m \neq i,j}(1-p_{im}^t)$ describes the background infection environment. Setting $\prod_{m \neq i,j}(1-p_{im}^t) = E$ and substituting (7) into (8) yields

$$w_{ij} = w_{ii}p_{ij}^l + (1 - p_{ij}^l)(w_{ii} + k_i)Ep_{ij}^t \tag{9}$$

Which shows that willingness-to-pay of i for j depends on the parameters governing knowledge and contagion externalities, respectively p_{ij}^l and p_{ij}^t, willingness-to-pay for self, implementation cost k_i, and the background infection environment E. My empirical approach is designed to shed light on four key parameters in (9); the willingness-to-pay elicitation exercise measures w_{ii} and w_{ij}, while the belief update interventions identify the impact of p_{ij}^l and p_{ij}^t on w_{ij}.

Finally, it is important to notice that (9) embeds a selfish assumption: agents are only willing to pay for others to acquire the technology as long as it benefits them. I interpret a participants' willingness-to-pay for another farmer as the total value to the participant

of the externalities produced by the other farmer; as a result, the selfish assumption is crucial to identify spillovers. If participants had altruistic preferences, their willingness-to-pay would also include the value a participant attaches to providing the technology to others. I will further discuss the selfish assumption in the results section to provide evidence in its favor.

4 Data and Sample

The sample includes 780 farmers from 103 villages located in the districts of Manafa, Mbale, and Tororo in Eastern Uganda. This sample had been selected for a field experiment carried out in previous years by a research team that included the author (Burchardi et al., 2022). The three contiguous districts were selected for their high suitability to maize cultivation according to the FAO GAEZ data[12]. The 103 villages[13] were selected by a randomization algorithm from the pool of villages in the 2010 national census with a population density lower than 100 households per square kilometer (to ensure they were in rural areas), stratifying by population density. In each village, we enlisted all residing households through a researcher-run census survey. We selected six to nine research participants (maximum one per enlisted household, always the head of the household) according to the following criteria: *i)* engaging in commercial maize farming; *ii)* having cultivated or planning to cultivate maize; *iii)* reporting land holdings from two to six acres; *iv)* having a mobile money account. The participants in the Burchardi et al. (2022) experiment were enrolled at the end of 2016 and had participated in up to four survey interviews and one willingness-to-pay elicitation by the time the field work for the experiment in this article started; all research activities connected to Burchardi et al. (2022) were completed by 2017.

The data I use come from four surveys: an in-person baseline survey, an in-person willingness-to-pay elicitation survey, a phone followup survey, an in-person follow-up survey. The baseline, willingness-to-pay elicitation, and in-person follow-up surveys include all 780 participants, while the phone follow-up survey includes 744 out of the 780 participants (36 participants did not pick up their phone at the time of the phone survey). All surveys have been written by the research team and approved by the Ugandan ethics authorities. The enumerators who administered the survey have at least a high school diploma, speak the Eastern local languages (Ateso, Japadhola, Luganda, Lugisho, Swahili) and were recruited through a local research firm. The timeline of data collection is summarized in Figure A.1, while Appendix F provides details about each variable used in the analysis.

[12] www.gaez.fao.org

[13] Villages are commonly referred to as LC1 (Local Council 1) in Uganda, and they are the smallest administrative unit, governed by a chairperson (LC1 chairperson) and nine executive committee members.

5 Experimental Design

5.1 Willingness-To-Pay Elicitation

Enumerators carried out home visits to elicit participants' willingness-to-pay for a three-hour training session about Fall Armyworm. During the home visit, each participant formulates her willingness-to-pay to receive training herself, and also her willingness-to-pay for each other sampled farmer in her village to receive the training. These valuations are elicited in a way that makes it clear they are mutually exclusive events: only one farmer can actually receive training.

The elicitation technique I employ is a multiple-price list variant of that proposed by Becker et al. (1964), in which a participant formulates a bid b for an item and the bid is compared with a random price draw p. If the bid is equal to or larger than the random price, the participant has to buy the item; if the bid is lower than the random price, the participant cannot buy the item.

Before the exercise, enumerators ask participants if they know what the Fall Armyworm is and inform them it spreads by proximity, so that the closer a plot is to pest-infected plants, the likelier it is that the pest spreads to the plot.[14] We also inform participants that one decision maker would be randomly selected in their village. The decision maker is the one who actually gets the opportunity to purchase the training, and participants do not know the identity of the decision maker before the elicitation. We inform participants they will make N bids, $N = [6, 9]$, choosing their willingness-to-pay for themselves and for up to eight other farmers to receive the training in their village, but only one of these N choices will be actually implemented if they are the decision maker. If they are not the decision maker, none of their choices will be implemented.

In the multiple-price list variant I employ, the participant is presented with 21 amounts, ranging from 0 to 40,000 UGX ($ 34.6) and increasing in steps of 2,000 UGX ($ 1.7): (0, 2,000, 4,000, ..., 40,000)[15]. At every step, the participant is asked if she would be willing to pay amount x to purchase the training. If the respondent accepts to pay x, she is asked if she would be willing to pay $x + 2,000$. If she does not accept to pay x, the enumerator stops and records her bid as $b = x - 2,000$. Note that negative prices and prices above 40,000 are not allowed in the experiment. There are no participants who refuse to pay 0, and if a participant is willing to pay 40,000 or higher the enumerator records $b = 40,000$.

The enumerator follows this procedure to obtain a participant's willingness-to-pay for herself to receive the training, b_1, and then to obtain a participant's willingness-to-pay for each of the other sampled farmers in the village to receive the training, $b_2, b_3, \ldots, b_N$. Af-

[14] Figure A.2 shows that, one year after this exercise, farmers believe that the closer a plot is to an infected plot, the likelier it is to be infected. Farmers' beliefs are dispersed but decrease linearly with distance.

[15] All US Dollar amounts are expressed in PPP terms. I calculate amounts in $PPP using the 2018 conversion rate of $1=1157.27 LCU (Local Currency Units), retrieved by the World Bank Databank. Numbers in $PPP are rounded to the first decimal digit.

ter the respondent has made N willingness-to-pay bids, the enumerator gives her a sealed plastic card. The card contains a random price p drawn from a uniform distribution, $p \sim \mathcal{U}(0, 40,000)$. The card also indicates if the participant is the decision maker or not. If the respondent is the decision maker, the card also indicates which of the N choices made binds, so which of the $b_1, \ldots, b_N$ bids matters. The enumerator does not know p or any other information included in the card. Suppose a participant is the decision maker and her card says that choice 3 is the one that binds. Then if her bid for farmer 3 is larger than the random price, $b_3 \geq p$, she buys the training for farmer 3 at price p. If $b_3 < p$, she does not buy the training. Note that if the decision maker does not manage to buy the training, no other farmer can purchase the training for anyone else in the village. All participants from the same village are interviewed over the course of one day to minimize the chance that the identity of the decision maker becomes known in the village before the elicitation exercise is completed.

Design Rationale of the Elicitation In the experiment I exploit a random-lottery design and perform three lotteries,

1. a *price lottery* that assigns a price $p \in \{0, 2,000, \ldots, 40,000\}$ to each participant;
2. a *choice lottery* that determines which of the N choices should be implemented;
3. a *decision maker lottery* that determines which of the N participants in the village has the opportunity to buy the training for the choice selected in 2).

This design implies that a maximum of one farmer can receive the training in each village.

The *price* lottery ensures that a participant's valuation is truthful; the *choice* and *decision maker* lotteries rule out strategic complementarities or substitutabilities between farmers. To further clarify why strategic complementarities and substitutabilities need to be ruled out in this setting, assume it was possible for a participant to purchase training for two or more farmers. Then, when making a choice, the participant would have to weigh the pros and cons of each n-person combination of farmers (unobservable to the econometrician), their joint decision processes, and how their efforts would complement or subtitute each other. Such considerations could distort a participant's valuations and add a layer of complexity that compromises the tractability of the problem for both the participant and the researcher. The same reasoning applies if the experiment allowed for two or more decision makers. If another person's choice were to be implemented together with her own in a two decision maker scenario, a participant would have to consider a much larger set of states of the world than if she was the only decision maker, potentially distorting her choices to exploit the complementarity or substitutability with the other decision maker's choices (or expectations over them). Obviously, if information is made available outside of a controlled experimental setting, more than one person per village

can obtain it and demand complementarities or substitutabilities kick in. This makes the decision over the value of others' learning much more complex. While my experiment cannot speak to this by design, it also provides a scenario in which adoption is bounded from below and the valuation decision has high stakes. If none of the participants buys the training, no one in the village will adopt the techniques.

5.2 Belief Update Intervention

My willingness-to-pay elicitation survey included a belief update experiment to understand how concerns for *i)* knowledge externalities and *ii)* contagion externalities affect how much a farmer values training one other farmer.

Knowledge externalities between two farmers depend on the probability that the two farmers share agricultural information with each other p^L (I restrict my analysis to direct communication between farmers and exclude indirect communication that involves other members of the community). The enumerator induces the participant to update her belief over p^L by informing her that the research team would organize a meeting between the participant and that specific farmer after training had taken place, if the participant is the decision maker and their valuation for that specific farmer is selected by the choice lottery. I call this the "meeting treatment".

Contagion externalities between two farmers depend on the physical distance between their plots, because the pest spreads by proximity. The enumerator induces the participant to update her belief over p^T_{ij} following a two-step procedure. First the enumerator asks the participant how long it would take to walk from her largest maize plot to the closest plot of a specific farmer j, d^P_{ij}. Then the enumerator informs the participant i of the true as-the-crow-flies walking distance between those plots, d^T_{ij} (based on plot GPS location collected at baseline). I call this the "distance" treatment. Appendix G reports the exact wording used in the experiment. Figure A.4 shows the joint distribution of perceived distance d^P_{ij} and true distance d^T_{ij} in walking minutes. From the figure one can see that in most cases the true and perceived distances are within 20 walking minutes, and never above 100. This means that the plots considered lay within a distance of five miles or eight kilometers from each other. Figure A.5 shows the distribution of the difference between true and perceived distance in the control group, $d^T_{ij} - d^P_{ij}$. The median difference is close to zero (equal to -4), so farmers' beliefs are roughly correct on average. In addition, the distribution is well-behaved. This is encouraging, as it suggests that farmers are familiar with plot distances. It also suggests that farmers are putting effort into answering plot distance questions that were not incentivized.

Both types of belief update interventions are a within-individual experiment; I generate random variation in contagion and knowledge externalities *within-participant* at the *valuation level*. For each farmer, I assign each of her five to eight valuations for others to one of three experimental conditions: control, meeting treatment, distance treatment.

This implies that each participant is exposed to all three conditions. The randomized assignment rule specifies that a maximum of four valuations should be assigned to control, a maximum of two to the meeting treatment, and a maximum of two to the distance treatment.

Rationale of Belief Update Intervention The ideal experiment to study knowledge externalities consists in randomly adding a new node - a person - to a participant's social network, where the new node is a farmer who has either received training or not, and study how much the participant values information held by the new node. This would exploit the *extensive margin* of a participant's social network. The context of my experiment does not allow me to add a new node to a participant's network, but I can exploit the *intensive margin* of a social network manipulation by increasing the likelihood that the participant interacts with another farmer. The meeting treatment tries to achieve exactly that. *Ceteris paribus*, I expect the meeting treatment to increase a participant's willingness-to-pay for the farmer she is going to meet.

For the meeting treatment to affect willingness-to-pay for another farmer, the participant needs to know that she is going to meet the other farmer before formulating her willingness-to-pay. Additionally, for the meeting treatment to be exogenous, whether the meeting happens needs to be independent from a participant's willingness-to-pay for that farmer[16]. Hence, before the elicitation, the enumerator informs the participant that the meeting will take place i) regardless of her willingness-to-pay; ii) if the participant is the decision maker; iii) if that choice is selected by the choice lottery.

The ideal experiment to study contagion externalitie consists in randomly adding a new node - a plot - to a participant plot's network, where the new plot is at random distance. By comparing the valuation of participants for farmers with identical plots at a different random distance, one could gauge the importance of contagion spillovers. This would exploit the *extensive margin* of a participant plot's network. The context of my experiment does not allow me to add a new node to a participant plot's network, but I can exploit the *intensive margin* of the current plot network by increasing the perceived likelihood that a plot is infected by another plot. This is what my distance treatment achieves by revealing the true distance to randomly selected plots. *Ceteris paribus*, I expect the distance treatment to increase a participant's willingness-to-pay for a farmer whose plot is surprisingly closer and decrease a participant's willingness-to-pay for a farmer whose plot is surprisingly further away.

Similarly to the meeting treatment, for the distance treatment to affect the willingness-to-pay for another farmer, the participant needs to learn the true distance to a farmer's plot before formulating her willingness-to-pay for that farmer. Hence, before the elicita-

[16]If the participant could reduce the meeting probability by reducing her willingness-to-pay for that farmer, treatment would be endogenous.

tion, the enumerator asks the participant what she thinks is the walking distance between her plot and the other farmer's plot, and then informs her of the true distance. An important caveat is that the enumerator reveals the true as-the-crow-flies distance between any two plots in the experiment - the most relevant because the pest spreads by flying during the moth stage - and not the true walking distance.

5.2.1 Training and Researcher-Organized Meeting

One week after the elicitation exercise, a team of three agronomists visited the villages in which the decision maker had managed to buy the training. In each village, one agronomist collected payment from the decision maker and trained the farmer for whom training was bought. The agronomists were recruited for the project and had not had previous interactions with the participating farmers before this project. Training was carried out in 64 out of 103 experimental villages; all 64 farmers who committed to pay (five for themselves and 59 for another farmer) did, in fact, pay. Each individual training session lasted about three hours and took place at a respondent's homestead. A training session included a theory module and a practice module. Agronomists taught the theory module with the aid of a color-printed leaflet (available upon request) that they developed together with the author. The leaflet contains basic information about how to recognize, prevent, monitor and control a Fall Armyworm infestation, and the trainee is encouraged to keep it for future reference. The practice module was typically taught in the respondent's closest maize plot. The (marginal) cost of providing training to one farmer in the experiment is 34,000 UGX or $29.4. If the government were to implement the training, marginal costs would most likely be lower (government wages are typically lower than in the experiment, and government operations have larger economies of scale). Fixed costs of training are negligible in the experiment and would be so if a larger organization were to implement the training as well; the only fixed costs in my experiment consist in a 3-day training of the agronomists.

Once all training sessions had been carried out, the research team arranged meetings for the farmers who had been selected to be decision makers and whose randomly selected valuation involved a meeting treatment (this was the case for 24 farmers). Meetings took place at a location agreed upon by both farmers, and a member of the research team attended the meeting as a facilitator.

6 Results

6.1 The Training Program is Beneficial

I start by providing evidence that the training increases adoption of pest-control practices. To this end, I exploit the random variation in training provision at the village level generated by the elicitation exercise.

Whether training takes place in a village is a random event, conditional on the willingness-to-pay of the decision maker D for randomly selected potential trainee z. The *price*, *choice*, and *decision maker* lotteries are such that a decision maker cannot affect the price she pays for training, the identity of the recipient, and the likelihood that she herself is the decision maker. But her willingness-to-pay choice can affect the probability that a recipient z, selected via *choice-lottery*, receives training; the higher the willingness-to-pay of a decision maker for z, the more likely it is that z receives training. Whether training takes place in a village is therefore a random event, conditional on the willingness-to-pay of the decision maker D for randomly selected potential trainee z. This allows me to employ the following empirical specification to evaluate the impact of training one farmer on the adoption of other sampled farmers in the village[17]:

$$\begin{aligned} Adoption_{iv} = \beta_0 + \beta_1\ Training_v \times Trained\ Farmer_i \\ + \beta_2\ Training_v \times Spillover\ Farmer_i + \sum_p \delta_p \mathbb{1}(w_{Dz,v} = p) + \varepsilon_{iv} \end{aligned} \tag{10}$$

The dependent variable is a dummy equal to 1 if farmer i in village v has adopted the following recommended practices to control the Fall Armyworm: spraying pesticides, pouring ash (or sand or soil) on infected plants, crushing eggs or caterpillars. $Training_v$ is a dummy equal to 1 if training has taken place in village v. $Trained\ Farmer_i$ is equal to 1 if i has received training. $Spillover\ Farmer_i$ is equal to 1 if $Trained\ Farmer_i$ is zero. Indicator $\mathbb{1}(w_{Dz,v} = p)$ is equal to 1 if the willingness-to-pay of decision maker D for potential trainee z is equal to p, where $p \in \{0, 2\ 000, \ldots, 40\ 000\}$ [18].

Results from estimating 10 are presented in Table 1 and based on responses to a phone follow-up survey carried out in December 2018. In the survey we only ask adoption-related questions to farmers who say at least one of their plots had a Fall Armyworm infection during the current season; for this reason, the analysis in Table 1 focuses on practices included in the training module and only applicable once a plot is infected; the table includes fewer than 780 observations (the sample size of the study) because some participants did not respond to the phone survey. Before getting to the discussion of

[17] An alternative way to investigate the relationship between training and adoption is to use the random price assigned by the *price* lottery to the decision maker as an instrument for training. When I implement this IV specification (not reported) I find that coefficient magnitudes are quite similar to those obtained estimating Eq. 10 but, predictably, more noisy.

[18] The OLS estimator β_1 in this case is a weighted estimator, where weights are given by the conditional variance of the treatment variable. The conditional variance of $Training_v$ (conditional on each possible value of $w_{Dz,v}$) is highest when, within a bin of $w_{Dz,v}$, the share of treated and untreated is equal. So bins of $w_{Dz,v}$ in which everyone is treated or everyone is not treated are going to receive lower weights than bins in which treated and non treated have similar shares, and hence higher conditional variance (see Chapter 3 in Angrist and Pischke (2009)). In the data, bins such that $w_{Dz,v} \in [6,000, 12,000]$ are the ones with the most similar treatment shares and hence are weighted more; if $w_{Dz,v} < 6,000$ most or all villages are untreated; if $w_{Dz,v} > 12,000$ most or all villages are treated.

Table 1: Effects on Adoption

	(1) Any Action	(2) Spray	(3) Pour Ash	(4) Crush Eggs/Worm
Trained Farmer	0.15	0.13	-0.01	0.03
	(0.08)	(0.09)	(0.03)	(0.03)
Spillover Farmer	0.14	0.12	-0.00	0.01
	(0.06)	(0.06)	(0.02)	(0.01)
Control Mean	0.42	0.40	0.04	0.01
Observations	739	739	739	739

Notes: The table reports estimates based on specification (10). *Trained Farmer* is a dummy variable equal to 1 if a farmer has received training on the Fall Armyworm as part of the experiment. *Spillover Farmer* is a dummy variable equal to 1 if a farmer lives in a village where training was provided to another farmer. The dependent variable in Column (1) is a dummy equal to 1 if the farmer reported having adopted any of the following recommended practices: spraying their field, pouring ash (or sand or soil) on infected plants, crushing the eggs or caterpillars. In the remaining columns the dependent variable is a dummy for each recommended practice. All specifications include fixed effects for the willingness to pay of the Decision Maker for the potential trainee in the village. Robust standard errors are clustered at the village level and given in round brackets.

results, the reader should have an important caveat in mind with respect to estimates in Table 1. The training is randomized at the village level among 103 villages and takes place in 64 villages (translating into 64 trained farmers). Under standard assumptions, this sample size is not adequate to detect effects of the training on adoption. Powering this part of my experiment was not a first order concern since adoption is not the main outcome of my analysis, but an identical program could fail to replicate these results.

The first column of Table 1 shows adoption of any of the three recommended practices, and the remaining columns show the contribution of each individual practice to overall adoption. The omitted comparison group in composed by spillover farmers in villages where training did not take place. Adoption increases by 14% among spillover farmers in villages in which training took place, while it increases by 15% among farmers who received training themselves. This result is driven by higher take-up of pesticide as shown in column 2, which increases by a similar magnitude among trained and spillovers farmers [19]. The fourth column suggests that trained farmers are more likely to adopt the practice of crushing eggs and caterpillars by hand but the practice does not transfer to spillover farmers, as the point estimate for trained farmers is noisy but much larger than the point estimate for spillover farmers (who crush eggs and worms at the same rate as control farmers). I conclude that only the practice of spraying pesticides diffuses; the diffusion pattern suggested in Table 1 is qualitatively confirmed by two metrics I collected at follow-up. First, when spillovers farmers are asked which practices the trained farmer

[19]During the training agronomists listed eight recommended pesticides and specific dosages, but most farmers report using one type only, commercially known as Rocket - a broad-spectrum pesticide for caterpillars, aphids, and other pests. Rocket is the best known pesticide among farmers in this area.

recommended, most of them mention spraying pesticides and few of them mention any other practice (see top panel of Figure A.9). Second, in July 2019 I administered an incentivized knowledge test to all farmers in the sample[20] and find that while trained farmers are overall not more knowledgeable than spillover farmers or farmers in control villages (see Figure A.8), they are more likely to know that crushing eggs or caterpillars is advisable (the bottom panel of Figure A.9 shows the share of farmers who answered correctly in the knowledge test in each group).

The estimates reported in Table 1 provide causal evidence that when one farmer learns about the technology, others adopt it. This behavior could be triggered by several factors, including lower adoption costs due to a decrease in information barriers or larger value of adoption due to the decrease in pest population generated by adopters. Evidence of the effects of the training program, by itself, is not informative of whether farmers value neighbors' information. Investigating the value placed by farmers on neighbors' knowledge, and understanding which factors affect it, is the objective of the next section.

6.2 Farmers Value Training Others

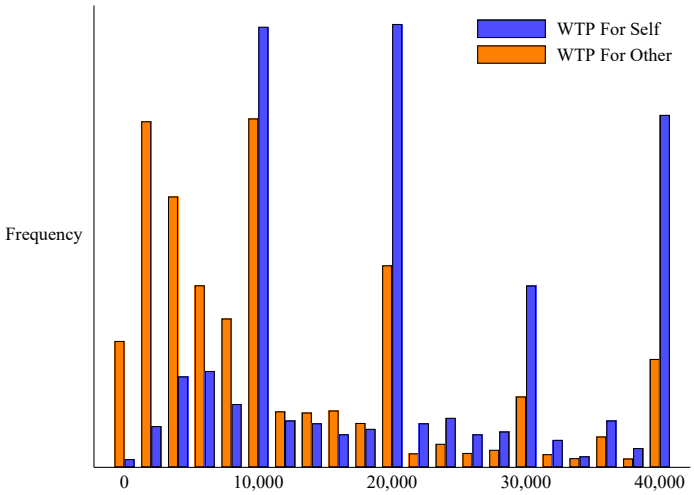
After having established that training is beneficial to village adoption, the next step is to verify whether farmers are aware of the benefits of the training, both for themselves and for others. To do so, I collect farmers' valuations for themselves to receive the training, and for others to receive the training. I elicit willingness-to-pay for the training using an incentive-compatible mechanism that farmers have already experienced in the past and that they understand well, as shown by measures of optimal bidding behavior and overall comprehension in Appendix B. Figure 1 shows the distribution of willingness-to-pay for self (blue) and one other farmer (orange).

Figure 2 shows the demand for training if the recipient is oneself (blue) or one other farmer (orange). In both cases, demand for training is non-negligible. A useful benchmark to interpret these curves is the median agricultural daily laborer wage in local markets, UGX 5,000 or $ 4.3. At the daily wage price, more than 90% of farmers would buy the training for themselves and more than 60% would buy the training for another farmer.

In the sample median willingness-to-pay for self w_{ii} is UGX 20,000 or $ 17.3, so about four days of work as a daily farm laborer. The median willingness-to-pay for another farmer w_{ij} is UGX 10,000 or $ 8.6, the equivalent of two days of work as a farm laborer. Figure 3 plots the joint distribution of willingness-to-pay for self and others; w_{ii} on the y-axis against w_{ij} on the x-axis, and the 45-degree line is added for reference. Bubble size

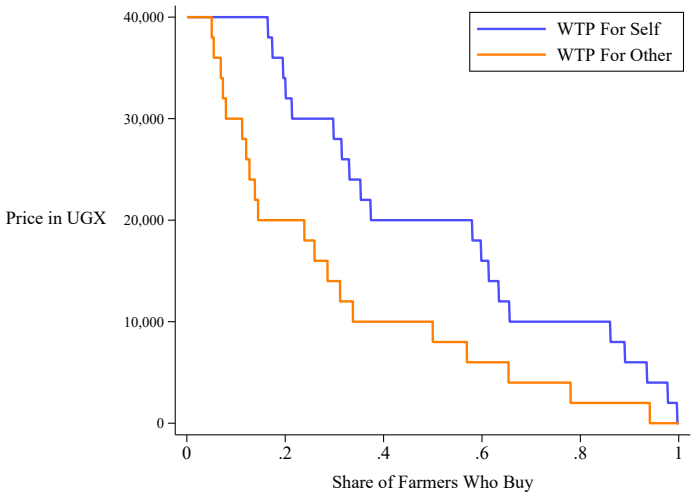
[20]The test consists of 15 multiple-choice questions about the FAW and how to prevent an infestation. After the respondent has answered all questions, one of them is randomly selected and if the answer is correct, the respondent is awarded a small prize (a pack of salt). On average, respondents answer correctly nine times out of 15, with no difference if they received training themselves (left bar, *Trained Farmer*), someone else in the village received training (center bar, *Spillover Farmer*), or no training took place in their village (right bar, *Non-trained Village*).

Figure 1: WTP Distribution



Notes: The figures depicts the distribution of willingness-to-pay for self and others in the sample among the 21 possible prices; blue bars indicate willingness-to-pay for self and orange bars indicate willingness-to-pay for another farmer.

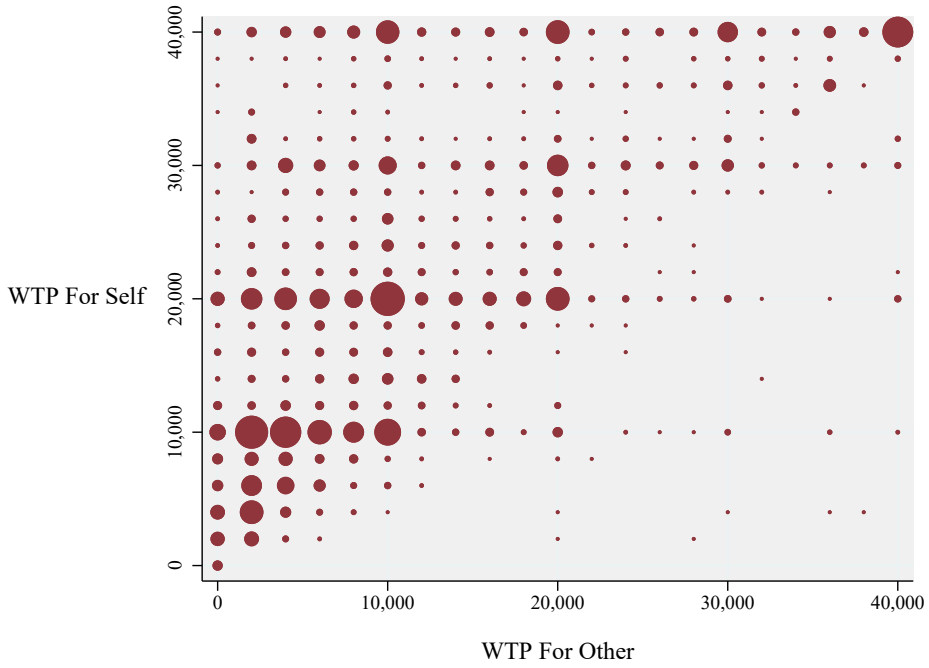
Figure 2: Training Demand Curves



Notes: The figures depicts the demand curves for training, showing the share of participants (on the x-axis) that would purchase training for each of the 21 possible prices (on the y-axis) for themselves and for another farmer in blue and orange respectively.

is proportional to frequencies. As Figure 3 shows, nearly all farmers display $w_{ii} > w_{ij}$. This is in line with an implication of the main assumption of the model. In the model, a farmer values learning positively and always prefers to learn than benefit from contagion externalities, which imply that a farmer always prefers to learn for sure than benefit from externalities. The average value of the ratio $\frac{w_{ij}}{w_{ii}}$ is 58%, so a farmer values another farmer's learning almost two-thirds of her own.

Figure 3: Joint Distribution of WTP for Self and Other



Notes: The figures depicts a scatterplot of the joint distribution of the willingness-to-pay for self against willingness-to-pay for another farmer to receive the training, for each of the 21 possible training prices.

Altruistic behavior and warm-glow In the conceptual framework I noted how a selfish assumption is necessary to interpret a participant's willingness-to-pay for others as the total value to the participant of the externalities produced by the other farmer. In line with this assumption, the willingness-to-pay data suggest that participants do not internalize the social benefit of training. If they did, we would expect to observe two patterns in the data. First, valuations would be uniform within villages - both within and across participants. The previous paragraph disproves that valuations are uniform within participant, as it shows that willingness-to-pay for self is substantially higher than willingness-to-pay for others; in addition, the average coefficient of variation of willingness-to-pay for others at the participant level is 35%. Having uniform valuations across participants means that different participants have a similar willingness to pay for the same farmer; in the data, the average coefficient of variation of willingness-to-pay for the same farmer is equal to 74%, pointing to high dispersion. Second, if participants internalized the social benefit of the training, their willingness-to-pay would increase with village size or with the number of maize farmers in a village. I find that willingness-to-pay for others is not positively correlated with village size and number of maize farmers in a village (the correlation is negative, -11% and -7% respectively), and willingness to pay for self is not meaningfully correlated with either (0 and 2% respectively). Figure A.7 plots willingness to pay (for self or others) against number of households engaged in maize farming in the village at baseline, and bubble size measures frequency. Participants appear to have similar valua-

tions irrespective of the number of households engaged in maize farming in their village. My experiment is not designed to identify or rule out altruism, but I can quantify warm glow (Andreoni, 1990) as the willingness-to-pay for another farmer whom the participant does not know and who produces no spillovers from the participant's standpoint. I assume that spillovers related to either contagion or knowledge externalities decay to zero beyond a certain plot or home distance threshold respectively. At the 95th percentile in plot and home distance (for a home distance larger than 1.7 miles and an average plot distance larger than 35 walking minutes), the median willingness-to-pay for a farmer whom the participant does not know is zero, pointing to a lack of warm-glow giving behavior; the same holds true at the 90th percentile.

6.3 The Social Benefit of the Training is Large and Heterogeneous

The willingness-to-pay data allow me to evaluate three policy-relevant objects: the value of the per capita externality generated by training one farmer; the social benefit of the training; and how different targeting criteria for the training generate different levels of social benefit. In what follows I will show how to calculate the per capita externality and social benefit of training one farmer, and then compare two targeting criteria. The first allocates the training according to a *price criterion*: the farmer with the highest willingness-to-pay gets the training. The second allocates the training according to an *externality criterion*: the farmer producing the highest per capita externality gets the training. While it is trivial to foresee that the externality criterion will produce a higher social value than the price criterion, farmers' valuations allow us to fine tune this intuition by quantifying the change in social benefit when externalities are accounted for in targeting decisions.

I use farmers' willingness-to-pay for others to calculate the externality generated by a trained farmer t in village v:

$$Per\ Capita\ Externality_t = \frac{\sum_{j \in v} w_{jt}}{N_j} \tag{11}$$

Where N_j is the number of sampled farmers $j \neq t$ in village v.

Column (2) in Table 2 shows the mean, standard deviation, median, maximum and minimum value of the per capita externality, calculated using equation (11); it ranges from 500 to 24,250 UGX and has a median value of 6,750 UGX.

The social value of t's training is then the *Per Capita Externality* in (11) scaled up by the relevant population size (excluding t) plus t's willingness-to-pay for self; note that I do not make assumptions on how village scale affects social value since my willingness-to-pay data come from a random draw of participants at the village level and thus village

Table 2: Social Value Generated by Different Targeting Criteria

	(1)	(2)	(3)	(4)
			Social Value if Targeting Criterion is:	
	WTP for Self	Per Capita Ext.	Highest WTP	Highest Ext.
Mean	15,226	7,392	419,174	526,434
	(8,721)	(4,469)	(535,303)	(630,738)
Median	12,000	6,750	246,857	359,714
Max	38,000	24,250	3,252,250	3,786,750
Min	0	500	59,500	103,333
N	274	274	37	37

Notes: The table shows summary statistics for four quantities indicated by columns (1)-(4). The first is willingness-to-pay for self, calculated at the farmer level. The second is the per-capita externality calculated at the farmer level using equation 11. The third is the social value produced by the farmer with the highest willingness-to-pay in a village, calculated at the village level using equation 12. The fourth is the social value produced by the farmer with the highest per-capita externality in a village, calculated at the village level using equation 12. The figure only includes villages where farmers reported willingness-to-pay values ranging from 0 to 38,000 UGX.

size dynamics are incorporated in the level of the per capita externality. Indicating the relevant population in t's village as N_v yields

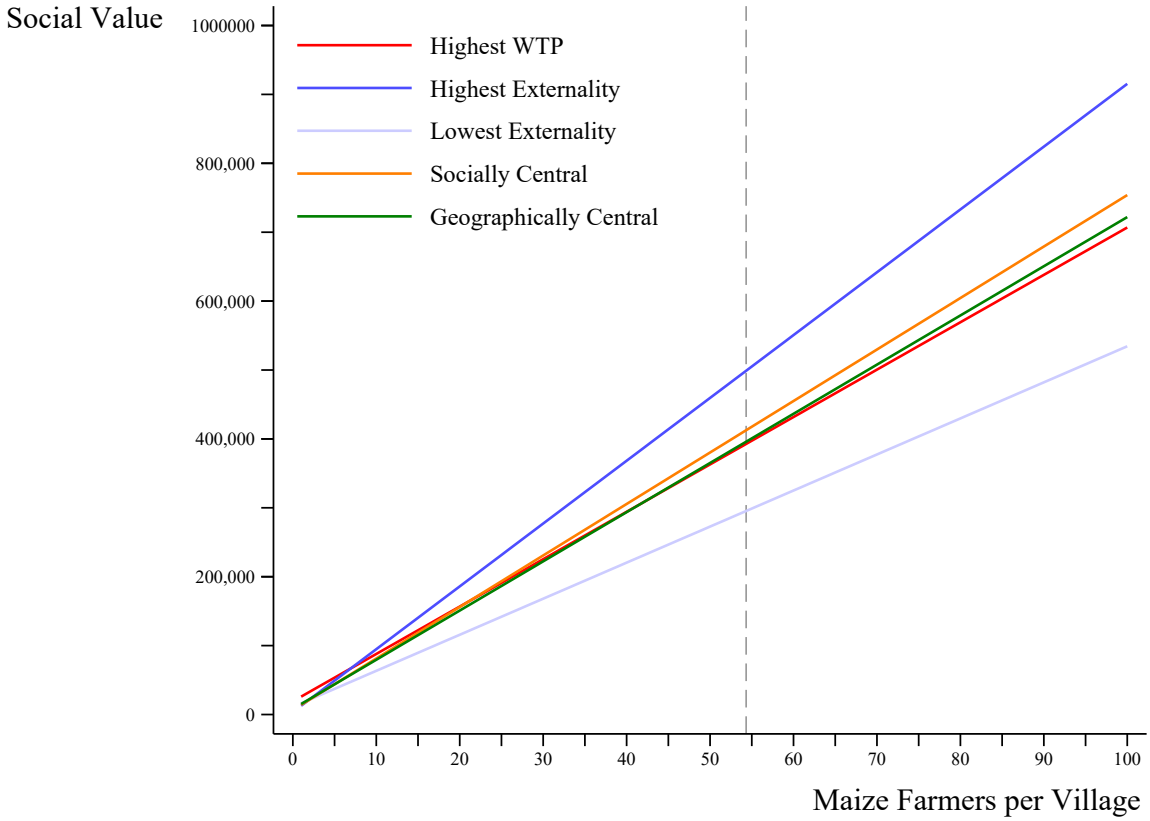
$$Social\ Value_t = w_{tt} + [Per\ Capita\ Externality \times (N_v - 1)] \tag{12}$$

Columns (3) and (4) in Table 2 display statistics for the social value generated by training the farmer willing to pay the highest price (column 3) or the farmer producing the highest externality (column 4). The table excludes villages in which any farmer reports willingness-to-pay equal to 40,000 UGX, the maximum price, because in that case the true willingness-to-pay may have been higher than the maximum price. The social values are calculated at the village level, assuming commercial maize farmers are the relevant population in each village - this seems reasonable since the farmers in my sample are a random draw from the population of commercial maize farmers in a village and almost all of them are willing to pay a positive price to train another farmer in their village. Typically the two targeting criteria select different farmers: in 85% of the villages the farmer with the highest willingness to pay is not the farmer producing the highest per capita externality. The table shows that targeting the training according to the externality criterion leads to a 107,000 UGX gain in mean social value (26%) and a 113,000 UGX gain in median social value (46%) relative to the price criterion. Table E.1 replicates the analysis including villages in which farmers report willingness-to-pay values for self or others equal the maximum (40,000 UGX).

While the assumption on the relevant population affects my estimates in columns (3)-(4), the general pattern that the social value generated by the highest-externality farmer is higher than that generated by the highest-willingness-to-pay farmer holds across the range

of the distribution of population sizes. In Figure 4 I estimate the social value of the two targeting policies at different population sizes using the per capita externality calculated in the data; I also include two counterfactual analyses: the social value generated by training the most socially connected farmer and the social value generated by training the farmer with the most central plot. Finally, I include the social benefits generated by the farmer producing the lowest per capita externality to illustrate the cost of wrong targeting. Like Table 2, the figure excludes villages in which any farmer reported willingness-to-pay values for self or others equal the maximum (40,000 UGX).

Figure 4: Social Value By Population Size and Targeting Criterion



Notes: The figure depicts the mean social value produced by five types of farmers when the population of commercial maize farmers ranges from 1 to 100. The social value is calculated at the village level using equation 12 and averaging across villages. The five types of farmers are: the farmer with the highest willingness-to-pay in the village (red line), the farmer producing the highest per capita externality in the village (dark blue line); the farmer producing the lowest per capita externality in the village (light blue line); the most socially connected farmer (orange line), or the farmer with the highest *Connectedness* (see Appendix F); the farmer with the most geographically central maize plot (green line). The mean value of the commercial farmer population size in my sample is equal to 54 and marked by a dashed vertical gray line. All valuations equal to 40,000 UGX are excluded from the sample.

The figure shows that training the highest-externality farmer (dark blue line) always generates a higher social value than training the highest-willingness-to-pay farmer (red line) for a farmer population size larger than four (a threshold that all villages in my

sample meet, as the commercial maize farmer population ranges from 11 to 164). At the maize farmer population sample mean, equal to 54 and marked by the vertical dashed line, the values of the lines in the figure correspond to the values reported in the first line of Table 2. The figure also anticipates the findings I will discuss in the next section on the importance of knowledge and contagion externalities. The orange line reports the social value of targeting the most socially connected farmer (the farmer with the highest *Connectedness*, see Appendix F), while the green line reports the social value of targeting the farmer with the most centrally located maize plot in the village. Interestingly, both lines lay between the red and the dark blue line, illustrating that farmers value more socially and geographically central farmers rather than farmers who have a large willingness to pay for themselves. The same pattern emerges if we include villages in which any farmer reported WTP values of 40,000 UGX for themselves or others (Figure E.2).

6.4 Valuations Respond to Changes in Externalities

This section first presents the empirical strategy to evaluate the impacts of the belief update intervention on willingness-to-pay for others, and then comments on results.
The outcome of interest is the willingness-to-pay of farmer i for farmer j, w_{ij}. A farmer's willingness-to-pay can take 21 monetary values that are multiples of 2,000 Ugandan Shillings, $w_{ij} \in \{0, 2\ 000, 4\ 000, \ldots, 40\ 000\}$.

I conjecture that the distance treatment should negatively affect w_{ij} if the distance information is *good news* for i, meaning that j's plot is surprisingly *further away* from i's plot. Conversely, the distance treatment should positively affect w_{ij} if the distance information is *bad news*, meaning that j's plot is suprisingly closer than expected. The rationale is that if j's plot is further away than i expected, the benefit accruing to i of j's adoption is lower, and the other way around. The key metrics are

$$\begin{aligned} Potential\ Good\ News_{ij} &= |d_{ij}^T - d_{ij}^P| \text{ if } d_{ij}^T > d_{ij}^P \\ Good\ News_{ij} &= Potential\ Good\ News_{ij} \times T_{ij}^d \end{aligned}$$

when i learns that j is surprisingly further away, and

$$\begin{aligned} Potential\ Bad\ News_{ij} &= |d_{ij}^T - d_{ij}^P| \text{ if } d_{ij}^T < d_{ij}^P \\ Bad\ News_{ij} &= Potential\ Bad\ News_{ij} \times T_{ij}^d \end{aligned}$$

when i learns that j is surprisingly closer. Variable d_{ij}^T is the true distance between i's largest plot and j's closest plot in walking minutes. Specifically, d_{ij}^T is the as-the-crow-flies distance in miles between the GPS coordinates of the centroids [21] of the two plots,

[21] If a plot of land is a two-dimensional plane, its centroid is the arithmetic mean position of all points

converted to walking minutes by assuming that the average adult takes 20 minutes to walk one mile. The variable d_{ij}^P is a participant i's answer when asked how long it takes to walk from her largest plot to j's closest plot (exact question wording can be found in Appendix G). Variable T_{ij}^d is a dummy equal to 1 if participant i is told the true distance to farmer j's plot. The estimating equation for the distance treatment is:

$$w_{ij} = \beta_1 Good\ News + \beta_2 Bad\ News + \\ + \beta_3 Potential\ Good\ News + \beta_4 Potential\ Bad\ News + \alpha_i + \varepsilon_{ij} \tag{13}$$

The coefficients of interest are β_1, for which I expect a negative sign, and β_2, for which I expect a positive sign. The coefficient of β_1 measures the change in w_{ij} for every minute that j's plot is surprisingly further away from i's plot; it proxies the change in the value that i attaches to j's adoption when j's plot is less important in determining i's infection status. The opposite is the case for β_2, it measures the change in w_{ij} for every minute that j's plot is surprisingly closer to i's plot. Specification 13 includes fixed effects α_i for participant i. Notice how, for clarity of exposition, (13) does not include the coefficient of T_{ij}^d alone: this implicitly assumes that treatment effects are null for farmers with correct beliefs about distance (I present an alternative specification that includes T_{ij}^d in appendix).

The distance treatment I implement allows me to estimate the local average treatment effect (LATE) of a change in i's perceived plot distance between farmer i and j and i's willingness-to-pay for j to receive training. The size of the treatment, which is the difference between true and perceived distance in absolute value $|d_{ij}^T - d_{ij}^P|$, is a function of perceived distance d_{ij}^P. The more a farmer's perception d_{ij}^P is off, the larger the treatment she receives. This implies that there is self-selection in the treatment and I can only estimate the effect on those who have wrong beliefs.

The purpose of the meeting treatment is to *ceteris paribus* increase the perceived probability that i learns the information from j. During the elicitation exercise, participant i is told that the research team is going to organize a meeting with j, to take place after j has received training. Participant i is also told that the meeting takes place regardless of whether j receives training or not, to keep the meeting treatment truly exogenous (if the participant could determine whether the meeting happens or not through her willingness-to-pay decision, the meeting treatment would be endogenous). The estimating equation for the meeting treatment is:

$$w_{ij} = \delta T_{ij}^m + \alpha_i + \varepsilon_{ij} \tag{14}$$

Where T_{ij}^m is a dummy equal to 1 if participant i is invited to a researcher-organized meet-

of the plane.

ing [22] with j. The exact words used to administer the meeting treatment are reported in Appendix G.

Table 3 presents results from estimating specification 13. Estimates are at the dyad level, a dyad is composed by a farmer i stating her valuation and a farmer j for which the valuation is made. Out of 3,566 dyads for which the distance variables are not missing, the last line of column (1) shows that in 939 dyads farmer i would potentially receive good news and in 2,435 dyads she would receive bad news (in 192 dyads she would receive no news because she has correct beliefs). The coefficients of interest are *Good News* and *Bad News*, which measure the change in willingness-to-pay of i for j for a 1-minute increase in absolute net distance, or $|d_{ij}^T - d_{ij}^P|$. The coefficient on *Good News* in column (1) is negative as expected. It should be interpreted as the decrease in i's willingness-to-pay for j's information for every extra minute that j's plot is surprisingly further away than i believed. The coefficient size is 68 UGX and at the mean of *Potential Good News* (equal to 8) it implies a decrease of 544 UGX, \$0.14 or about 11% of the daily wage of a local agricultural laborer. The coefficient on *Bad News* is surprisingly negative but small and insignificant at conventional levels. Overall, results in column (1) show that farmers reduce their willingness-to-pay for farmers that generate lower contagion externalities.

Theory does not predict the difference between the coefficient size of *Bad News* and *Good News* that column (1) in Table 3 displays. An experimental feature that would explain this difference is that a *Good News* treatment is *actually* good news, while a *Bad News* treatment is not necessarily bad news, for the following reason. Farmers are asked about the walking distance between plots, so their answer may take into account rural paths, obstacles or differences in altitude. The distance the enumerator treats them with, d_{ij}^T, is the as-the-crow-flies walking distance (between plot centroids obtained at baseline) assuming a farmer walks at three miles per hour (20 minutes per mile). As-the-crow-flies is the relevant distance metric for pest spread because the pest spreads by flying to other plots during the moth stage. Hence, the walking distance stated by farmers d_{ij}^P can only be larger than the true walking distance between plots if the farmer has perfect information and walks at a speed of three miles/hour. For this reason, when the true distance is larger than the perceived distance,$d_{ij}^T > d_{ij}^P$, the farmer is undoubtedly receiving a *Good News* type of shock. When instead $d_{ij}^T < d_{ij}^P$, it may be that the farmer does not actually receive bad news because she knows that the walking path is actually much longer than the as-the-crow-flies path. I address this issue by making four different assumptions about the relationship between true and perceived distance in the data, and re-estimating specification 13. In columns (2)-(5) I replace the perceived distance d_{ij}^P, used

[22]The meeting treatment is similar to Lowe (2021) since both treatments are aimed at increasing the probability that individuals interact with each other. But Lowe (2021) introduces economic incentives to communicate with cricket players of a different social caste and looks at how this type of contact affects discrimination, while the meeting treatment of my study is not (and should not) be incentivized.

Table 3: Effects of Belief Update Intervention

	Unscaled	Scaled			
		Equality of		Path Shape	
	(1)	Means (2)	Medians (3)	Triangle (4)	Half Circle (5)
Potential Good News	-10.74	-34.24	-30.59	-28.65	-32.48
	(20.97)	(20.43)*	(20.57)	(20.65)	(20.53)
Good News	-68.01	-54.30	-56.22	-58.10	-55.43
	(36.80)*	(26.85)**	(28.40)**	(29.44)**	(27.69)**
Potential Bad News	-10.14	-21.60	-18.07	-16.53	-19.68
	(8.82)	(16.79)	(14.65)	(13.55)	(15.60)
Bad News	-14.55	-27.82	-24.61	-23.06	-26.14
	(9.66)	(18.09)	(15.86)	(14.70)	(16.85)
Scaling Factor λ	-	$E(d^T)/E(d^P)$	$M(d^T)/M(d^P)$	$1/\sqrt{2}$	$2/\pi$
Control Means					
w_{ij}	11,772	11,772	11,772	11,772	11,772
Potential Good News	8	7	7	7	7
Potential Bad News	14	8	9	9	9
Observations	3,566	3,566	3,566	3,566	3,566
Of Which Pot. Good/Bad News	939/2435	1764/1802	1596/1824	1566/2000	1759/1807

Notes: The table reports ordinary least squares estimates based on specification (13). The dependent variable w_{ij} is the willingness-to-pay of the participant i for another farmer j. *Potential Good News* is equal to the absolute value of the difference between true and rescaled perceived distance in walking minutes if the difference is positive, $|d^T_{ij} - \lambda d^P_{ij}|$ if $d^T_{ij}\lambda - d^P_{ij} > 0$. *Potential Bad News* is equal to the absolute value of the difference between true and rescaled perceived distance in walking minutes if the difference is negative, $|d^T_{ij} - \lambda d^P_{ij}|$ if $d^T_{ij} - \lambda d^P_{ij} < 0$. *Good (Bad) News* is equal to *Potential Good (Bad) News* multiplied by a treatment indicator T_d equal to 1 if the true distance was revealed to the participant. Column (1) reports unscaled estimates (or $\lambda = 1$). In column (2) λ solves $E(d^T_{ij}) = \lambda E(d^P_{ij})$; so the perceived distance is rescaled to have mean equal to the true distance. In column (3) λ solves $Median(d^T_{ij}) = \lambda Median(d^P_{ij})$; so the perceived distance is rescaled to have median equal to the true distance. In column (4) λ solves $d^T_{ij} = \lambda d^P_{ij}$, d^T_{ij} is the hypotenuse of an isosceles right triangle and d^P_{ij} is twice each cathetus, $(d^T_{ij})^2 = (\frac{1}{2}d^P_{ij})^2 + (\frac{1}{2}d^P_{ij})^2$. In column (5) λ solves $d^T_{ij} = \lambda d^P_{ij}$, d^T_{ij} is the diameter of a circle and d^P_{ij} is half of a circumference, $2d^P_{ij} = 2\pi\frac{d^T_{ij}}{2}$. Then $\lambda = 2/\pi$. Values are in Ugandan Shillings. All specifications include participant fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level.

to calculate *Bad News*, *Good News*, *Potential Bad News* and *Potential Good News*, with λd^P_{ij}, where $0 \leq \lambda \leq 1$ is a scaling factor. In column (2) I set λ such that the means of the distance variables equalize, $\lambda = {}^{E(d^T)}/_{E(d^P)}$, while in column (3) I set λ so that medians equalize, $\lambda = {}^{Med(d^T)}/_{Med(d^P)}$. In column (4) λ is set such that true distance is the hypothenuse of an isosceles right triangle and the perceived distance d^P_{ij} is twice each cathetus: $\lambda = 1/\sqrt{2}$. This allows me to examine the scenario depicted in Figure A.3, where the solid line between A and B is the as-the-crow-flies distance and the dotted line is the walking path a farmer has in mind. Similarly, in column (5) I set λ such that the true

distance is the diameter of a circle and the perceived distance d_{ij}^P is half a circumference: $\lambda = 2/\pi$. In this case, the farmer has in mind a semi-circular walking path between two plots, while the as-the-crow-flies distance is the diameter.

Scaling results in more dyads having potential good news and fewer dyads having potential bad news relative to the unscaled analysis; in the last row of columns (2) - (5) the number of dyads with potential good news is much closer to the number of dyads with potential bad news. As a result, the point estimates of *Good News* are smaller and have tighter confidence intervals, while those of *Bad News* are larger and have wider confidence intervals. These results further confirm that farmers decrease their willingness-to-pay with lower contagion externalities, but do not increase their willingness-to-pay with higher contagion externalities.

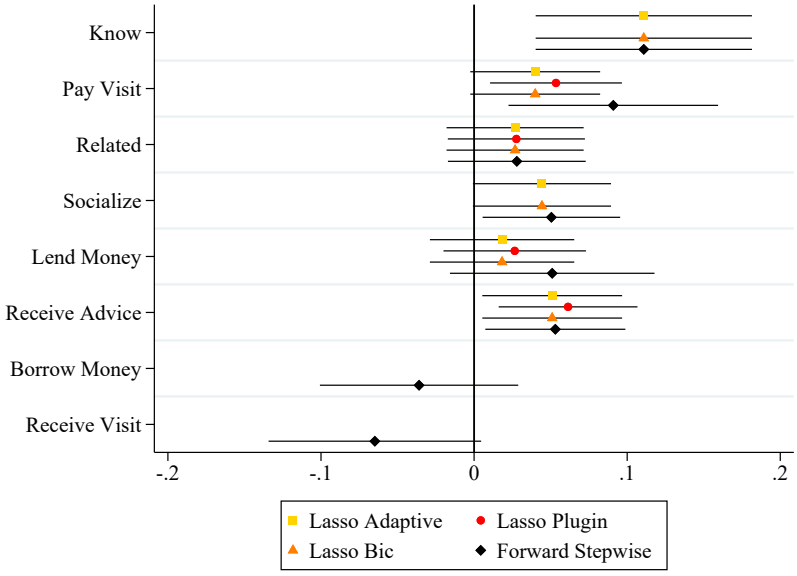
As noted earlier, specification (13) does not include T_{ij}^d alone, assuming implicitly that telling a farmer her beliefs are correct will not have any effect on her w_{ij}. About 5% of the dyads have a distance error equal to zero, and half of those are treated. Table D.1 in appendix B estimates specification (13) including T_{ij}^d; it shows the effect of telling a farmer her beliefs are correct is statistically indistinguishable from zero, albeit with a positive point estimate, and other estimates are not affected by the inclusion of T_{ij}^d.

Figure A.6 displays the average willingness-to-pay for another farmer if a participant is invited to meet the other farmer (right bar) or not (left bar). The willingness-to-pay for the other farmer is not significantly affected by the meeting treatment: coefficient δ in specification (14) is negative and insignificant (the point estimate is -230.8, the standard error 153.9). The meeting treatment did not seem to work as intended by design, overall, which is suprising because encouraging farmers to interact is a common approach to disseminate information in rural areas in lower-income countries. Previous work has shown that it has some positive effects on adoption and yields (BenYishay and Mobarak, 2018; Vasilaky and Leonard, 2018). If the behavior observed in my experiment replicates in other contexts, uncovering its causes will help identify when we can and cannot leverage human interaction to disseminate knowledge. One element that emerged during the elicitation exercise, and that is relevant here, is that 92% of all farmer dyads know each other already. It may be that farmers prefer to exchange information on their own, outside of a structured social interaction mediated by an enumerator; or it may be that farmers anticipate costs in term of lost labor or transport without any added benefits, given that they are anyway able to reach the recipient independently from the research team. Another option is that farmers are particularly adverse to meeting farmers that they do not know very well, while attaching a positive value to them receiving training. Or still, farmers may be unwilling to meet someone that is more knowledgeable than them for reputation purposes or because they anticipate feeling shameful. They may also be willing to pay for someone's training for reciprocity purposes (to give back to someone, or to trigger a symmetric reaction from them) which are orthogonal to the information

content of the training. Finally, another potential explanation is that a participant infers that, because meetings will happen, the information content of the training is going to circulate in the village. This realization can trigger a free-riding behavior, where the participant decreases her willingness-to-pay for all the farmers she is asked about after learning that researchers will organize meetings.

While the belief shock on knowledge externalities did not affect valuations as anticipated, it is possible to exploit the richness of the elicitation data to shed some light on what drives farmers' valuations for others. To do so, I let four parsimonious model selection algorithms choose which variables out of 16 best explain w_{ij}. The results of this exercise are displayed in Figure 5. The 16 variables I feed to the algorithms are: true plot distance, perceived plot distance, home distance, whether i knows j, whether i visits (or receives visits from) j, whether i and j are relative, whether i and j socialize, whether i borrows (or lends) money from j, whether i borrows (or lends) goods from j, whether i receives (or gives) advice to j, whether they speak about agriculture, whether they pray together (see Appendix F for variable definitions). These variables measure the extent of social connection between two individuals in different life domains, and are identical to the measures used by Banerjee et al. (2013) to quantify social connection between Indian villagers (but adapted to the Ugandan context). The model selection algorithms used in this exercise are three Lasso (adaptive, plugin, and minimum BIC) and Forward Stepwise. I adopt the following procedure: i) standardize regressors; ii) residualize both w_{ij} and each regressor on fixed effects for i and j, such that the remaining variation is specific to the ij dyad; iii) run the algorithms such that each selects the model with the highest explanatory power; iv) regress w_{ij} on the four selected models. Figure 5 shows the coefficient sizes of regressors in each selected model, expressed in standard deviations. Whether farmers know each other is the predictor with the largest coefficient and is picked up by three out of four selection algorithms; it is followed by receiving advice from, paying visits to, and lending money to the other farmer, which are picked up by all four algorithms. Socializing also matters but is only picked up by the Adaptive and BIC Lasso, and borrowing money and receive visit are only picked up by the Forward Stepwise. I interpret the selected coefficients as providing some suggesting evidence that farmers value training another farmer if that person is accessible, trustworthy, and has a record of sharing information.

Figure 5: Correlates of Willingness To Pay



Notes: The figure depicts the standardized coefficients of a regression of w_{ij} on subsets of regressors generated by Lasso and Forward Stepwise. In each case, the algorithm is fed a set of 16 variables: $True\ Distance_{ij}$, $Perceived\ Distance_{ij}$, $Home\ Distance_{ij}$, $Know_{ij}$, $Receive\ Visit_{ij}$, $Pay\ Visit_{ij}$, $Relative_{ij}$, $Socialize_{ij}$, $Borrow\ Money_{ij}$, $Lend\ Money_{ij}$, $Borrow\ Goods_{ij}$, $Lend\ Goods_{ij}$, $Receive\ Advice_{ij}$, $Give\ Advice_{ij}$, $Speak\ Agriculture_{ij}$, $Pray_{ij}$. Each algorithm returns a subset of the 16 variables. The willingness-to-pay w_{ij} is then regressed on each subset, including fixed effects for participant i and recipient j.

7 Conclusion

This article investigates how externalities affect farmers' decisions to adopt agricultural technology. I study a new technology, namely a training on pest-control techniques to manage the outbreak of a new pest in Uganda; the training is beneficial and increases adoption of the practices by 15% among training participants. I set out to measure the monetary value of the externalities of the training, calculate the social benefit of the training if it is targeted to farmers producing high externalities versus not, and identify where externalities originate from.

To measure externalities, I carry out a lab-in-the-field experiment and elicit how much participants are willing to pay for a farmer in their village to receive the training, conditional on only one of them being able to. Willingness to pay for others measures the net spillovers that each participant anticipates as a consequence of training another farmer in their community. I find that farmers anticipate substantial spillovers from the training, as they are willing to pay two days' wage on average ($8.6) to have a farmer in their community receive the training.

To calculate the social benefit of training a farmer, I calculate the per capita externality the farmer produces (how much others are willing to pay for that farmer on average) and scale it up by community size. This allows me to compare the social benefit obtained from training a farmer others value highly (a farmer producing high positive externalities) to the social benefit obtained from training a farmer with high individual valuation of the training (high willingness to pay for self). Results show that the social benefit gain from targeting farmers who produce high positive externalities rather than farmers with high private valuation of the technology is substantial, a 26% difference on the mean and 46% on the median, which demonstrates externalities should be taken into account when defining targeting criteria.

To understand where externalities originate from, I examine two sources that would matter theoretically in this context - *contagion* and *knowledge* externalities - and evaluate whether they affect farmer's valuations. I introduce randomized variation in externalities by inducing some farmers to update (i) their belief over the probability of infection from another farmer's plot to theirs (the perceived *contagion* externality between the two farmers), (ii) their beliefs over the probability that knowledge of the pest-control techniques spills over from another farmer to them (the perceived *knowledge* externality between them). I find that farmers respond to contagion externalities by decreasing their willingness-to-pay for farmers whose plots are further away. My experimental approach does not pick up knowledge externalities, but I provide evidence that farmers value training those who are more accessible to them and from whom they have received advice.

Taken together, my results show the importance of accounting for externalities to optimize social policies. In my specific context, externality considerations indicate the ideal

adopters are farmers who are socially central in the social network and whose plots are centrally located. Adapting these conclusions to other contexts will require incorporating a careful analysis of the positive and negative externalities connected to a technology before a targeting decision is made, and weighing the maximization of social benefits against alternative considerations such as equity or inclusivity.

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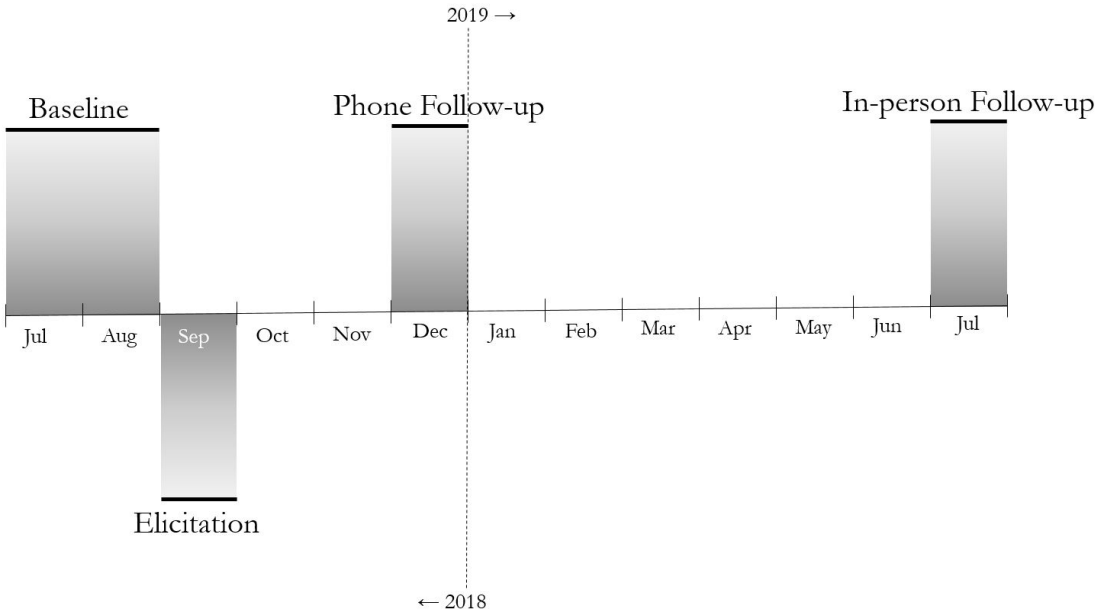
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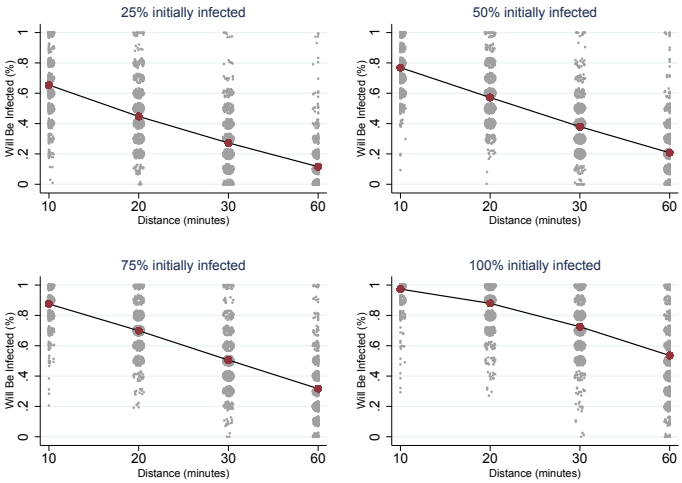
A Figure Appendix

Figure A.1: Data Collection Timeline



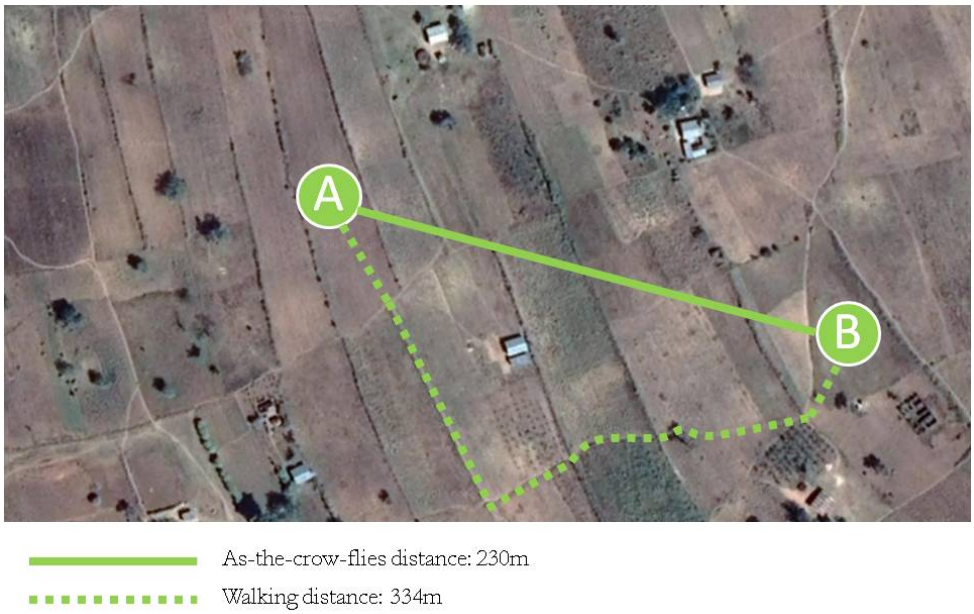
Notes: The figure shows the time schedule of the data collection for the project. Shaded bars indicate the survey name and when it was administered.

Figure A.2: Farmers' Beliefs over Infection Probability



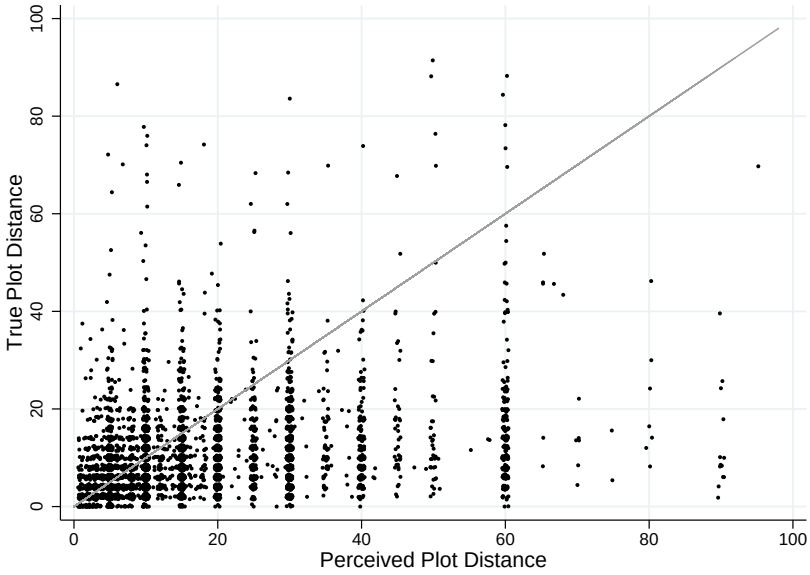
Notes: The figure depicts a scatterplot of the answers given by farmers to the question "How many plots at walking distance X will be infected if Y% of the plants in the central plot are affected by Fall Armyworm?". The question is hypothetical; participants are shown an illustration of a central infected plot surrounded by non-infected plots all located at distance X from the central plot. The x-axis reports four different distance values, 10, 20, 30 and 60 minutes while the y-axis reports the share of plots the farmer thinks will be infected if a plot situated in the middle is affected by Fall Armyworm. Each panel shows answers provided for a different level of initial plant infestation in the central plot, reported on top of each panel. Grey dots show the raw data, jittered to show more frequent answers. Red dots mark the average answer at each value of walking distance. Red dots are connected with a straight line to show, on average, the relationship between infection rate and distance.

Figure A.3: Example of As-The-Crow-Flies distance vs. Walking Distance



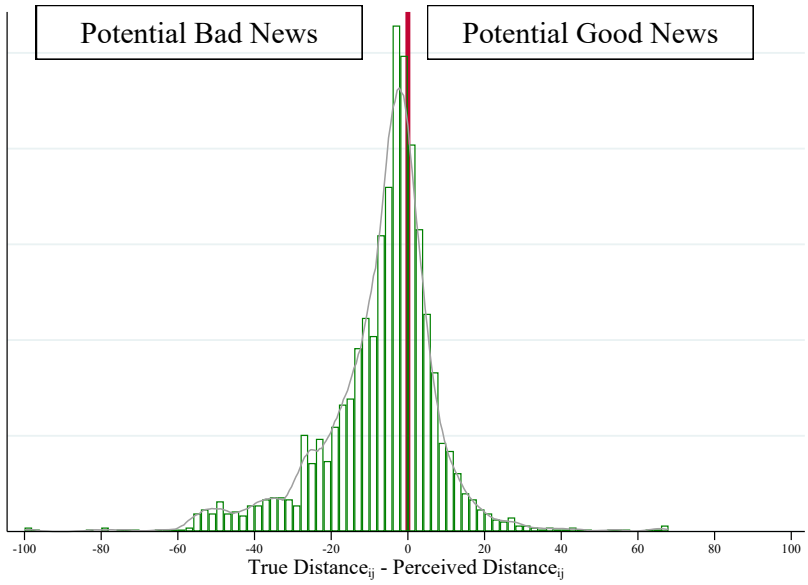
Notes: The figure depicts the distance between centroids of plots A and B if calculated as-the-crow-flies (solid line) or if taking into account rural paths (dotted line).

Figure A.4: Joint Distribution of True and Perceived Plot Distance



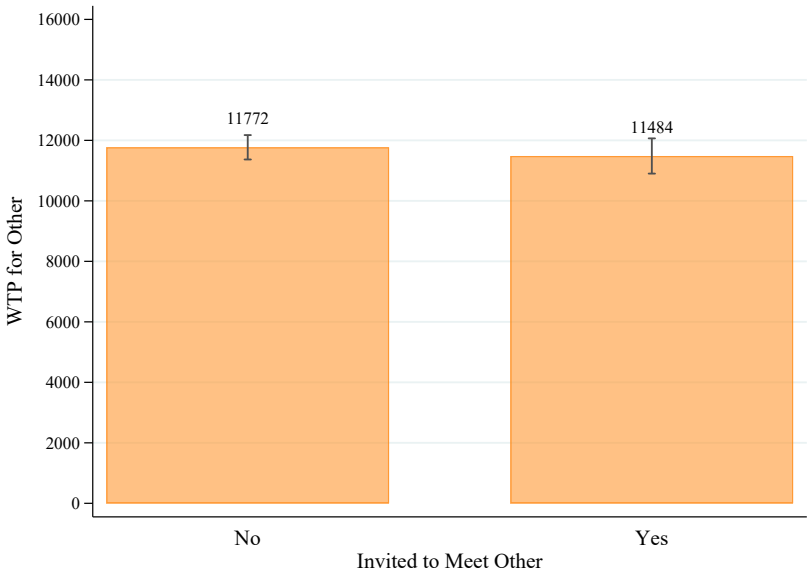
Notes: The figure depicts a scatterplot of the joint distribution of $True\ Distance_{ij}$ (d_{ij}^T) and $Perceived\ Distance_{ij}$ (d_{ij}^P). Both variables refer to the distance between i's largest maize plot and j's closest plot and are measured in walking minutes. $True\ Distance_{ij}$ is the as-the-crow-flies distance between coordinates of the plots' centroids, measured by a GPS tracker in miles and converted in walking distance assuming that the average adult takes 20 minutes to walk one mile. $Perceived\ Distance_{ij}$ is the answer provided by i when asked to estimate the distance between her largest plot and j's closest plot. The 45-degree line is added for reference.

Figure A.5: Distribution of Net Plot Distance



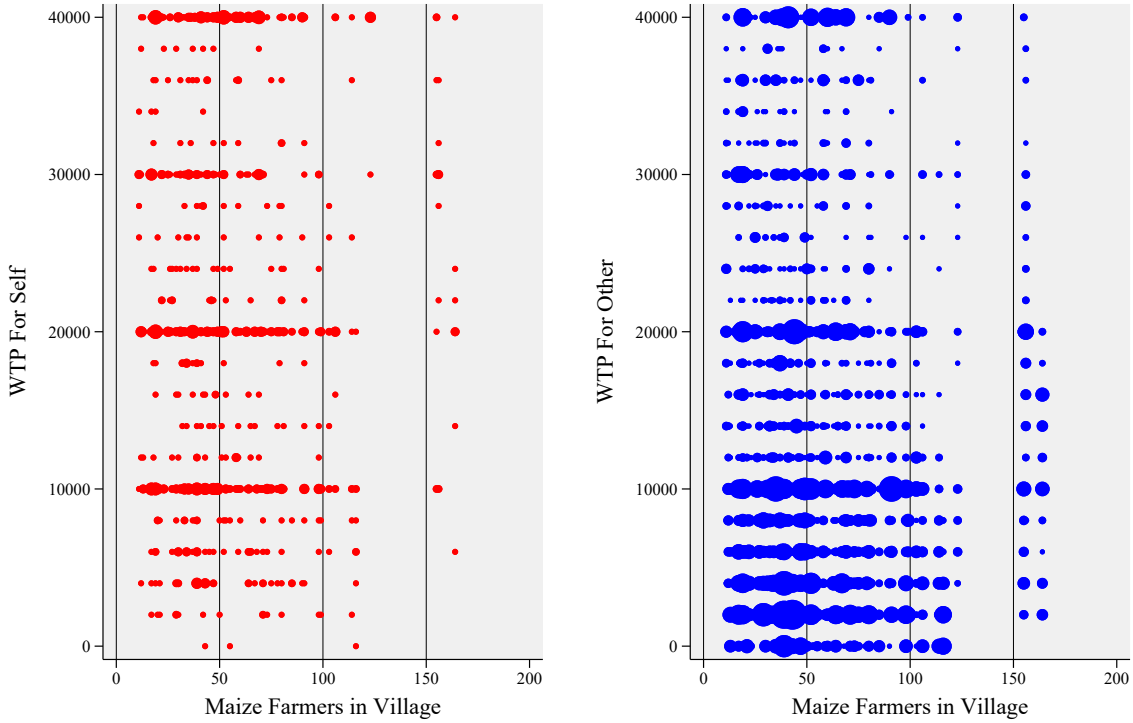
Notes: The figure depicts the probability density function of the difference between $True\ Distance_{ij}$ and $Perceived\ Distance_{ij}$ for each farmer dyad in the control group, where the control group is the set of dyads in which participant i had neither been exposed to the meeting treatment nor to the distance treatment with respect to farmer j. Both variables refer to the distance between i's largest maize plot and j's closest plot and are measured in walking minutes. $True\ Distance_{ij}$ is the as-the-crow-flies distance between coordinates of the plots' centroids, measured by a GPS tracker in miles at baseline and converted in walking distance assuming that the average adult takes 20 minutes to walk one mile. $Perceived\ Distance_{ij}$ is the answer provided by i when asked to estimate the distance between her largest plot and j's closest plot.

Figure A.6: Effects of Meeting Treatment



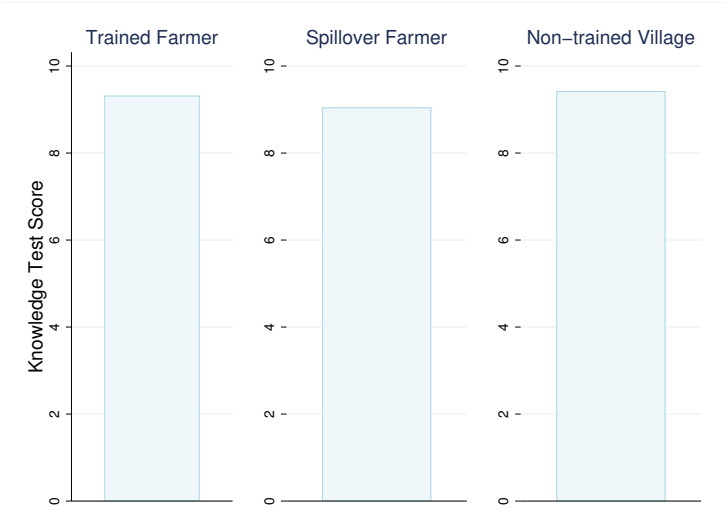
Notes: The figure depicts the mean and 95% confidence interval of willingness-to-pay for others w_{ij}. The right bar shows the mean w_{ij} if a participant i has been invited to meet j, the left bar shows the mean w_{ij} if a participant i has not been invited to meet j.

Figure A.7: Joint distribution of WTP and Number of Maize Farmers in the Village



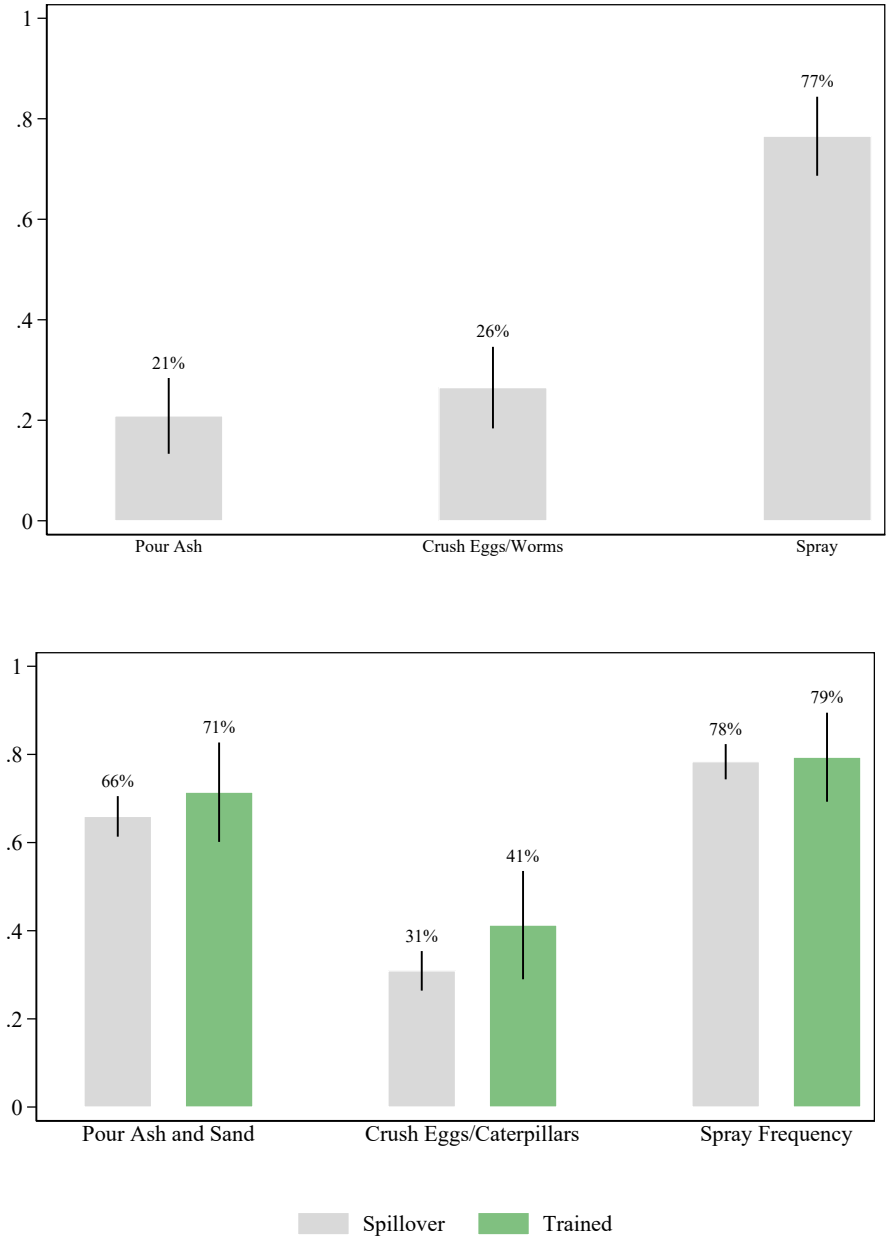
Notes: The figure depicts a scatterplot of the joint distribution of a participant's willingness-to-pay for self (left panel) or for another farmer (right panel) and the number of maize farmers recorded in the participant's village at census. Bubble size is proportional to frequency.

Figure A.8: Knowledge at Endline



Notes: The figure depicts the average score in a FAW knowledge test obtained by three categories of participants. The left bar shows the average score of trained farmers; the central bar shows the average score of spillover farmers (non-trained farmers in trained villages); the left bar shows the average score of farmers in non-trained villages.

Figure A.9: What Spillover Farmers Are Told vs What They Know



Notes: The top panel of the figure depicts the share of spillover farmers who reported the trained farmer had adviced them to adopt an agricultural practice (crushing eggs and caterpillars, pouring ash and sand on infected plants, spraying the field with pesticide). The bottom panel of the figure depicts the share of trained and spillover farmers who have a correct knowledge about each practice as measured by an incentivized knowledge test.

B Practice Rounds and Comprehension

Before eliciting farmers' willingness-to-pay for the agricultural training, I carry out three practice rounds of elicitations to familiarize farmers with the process. In the first round I elicit farmers' willingness-to-pay for an experimental voucher worth 1,200 UGX in the experiment and redeemable at face value at the end of the practice round. In the second round I elicit farmers' willingness-to-pay for a bar of laundry soap[23] In the third round I elicit farmers' willingness-to-pay for an information sheet about the history of the Fall Armyworm; I included this round so that farmers could practice formulating their willingness-to-pay for information without learning notions applicable to pest management (otherwise, their valuations for the training would be compromised).[24]

The three goods have different price supports. The price of the voucher ranges from 0 to 2,000 UGX in increments of 200 UGX. The price of the soap ranges from 0 to 4,000 UGX in increments of 400 UGX. The price of the history sheet ranges from 0 to 1,000 UGX in increments of 100UGX. Figure B.1 displays the distribution of the willingness-to-pay (left column) and demand curves (right column) for each good.

I measure participants' level of comprehension using (i) the share of optimal bids for voucher and soap and (ii) their answers to comprehension checks about voucher and soap purchases (Berry et al., 2020; Burchardi et al., 2021). I do not perform comprehension checks on the history sheet because there is no reference price to benchmark valuations. I code participants as bidding optimally for the voucher if their willingness-to-pay is equal to 1,000-1,200 UGX,[25] and as bidding optimally for the soap if they bid the self-reported market price or below. Over 780 respondents, 75% bid optimally for the voucher and 85% bid optimally for the soap.

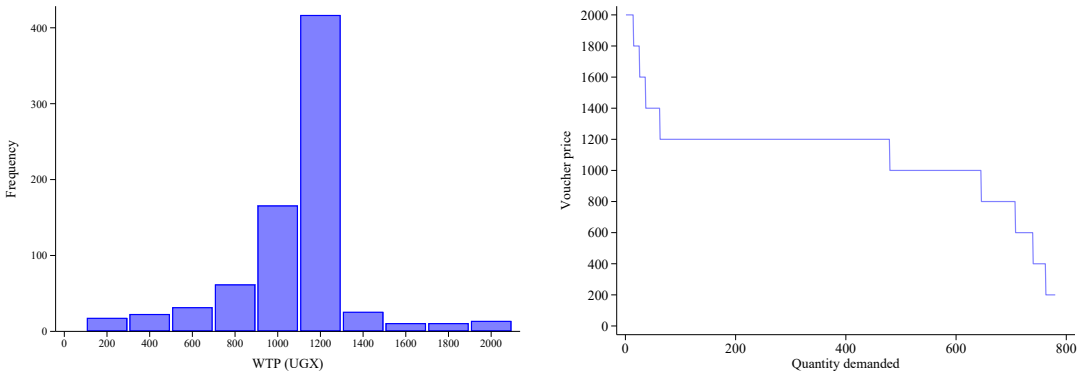
Comprehension checks are of three types. The first asks to list all the possible prices for both voucher and soap; 99% of participants list all the prices (only 3 and 5 out of 780 fail this check for the voucher and soap, respectively). For the second we draw two prices, one lower and one larger than a participant's willingness-to-pay, and ask whether they would have been able to buy had the price been equal to the lower draw, or to the higher draw, for both voucher and soap; all participants answered this check correctly for both voucher and soap, excluding one who failed one soap check. The third asks participants to calculate the potential profits from purchasing the voucher if a different price was drawn or they had stated a different willingness-to-pay; all but 10 answered this check correctly.

[23]The first two rounds are identical to those implemented in Burchardi et al. (2021), with the only difference being the face value of the voucher.

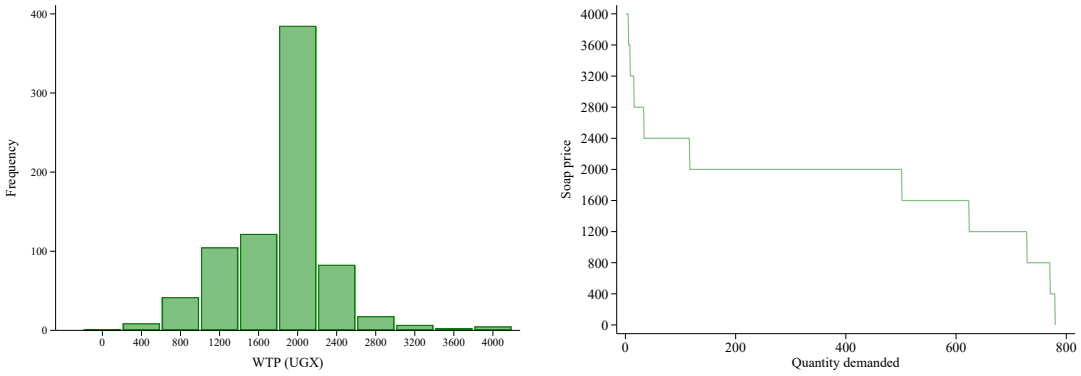
[24]Participants who purchased the history sheet were given an A4 sheet with the following text: "Fall Armyworm is an insect pest native to the Americas. It is new to Africa and was first detected in Central and West Africa in early 2016. The pest was first reported in Uganda in June 2016 in Kayunga, Kasese and Bukedea districts. By the end of 2017, the pest had spread to all the districts of Uganda including Manafa, Mbale and Tororo." The sheet included a map of Africa displaying the spread of the pest.

[25]For participants having less than 1,000 UGX on hand, they are coded as bidding optimally if they bid all they have on hand.

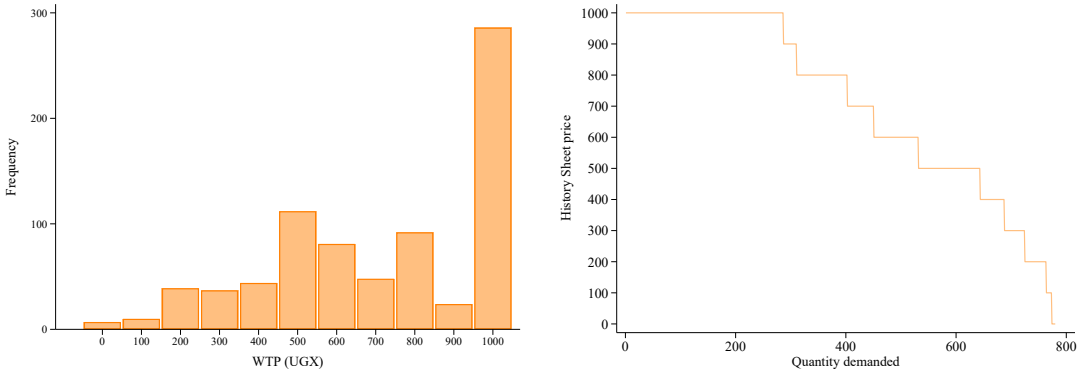
Figure B.1: Distribution (Left) and Demand Curve (Right) in Practice Rounds



(a) Voucher



(b) Soap



(c) History Sheet

Notes: The left column depicts the absolute number of participants (on the y-axis) that would purchase the voucher (a), soap (b), or history sheet (c) at each of the 21 possible prices (on the x-axis). The right column depicts the share of participants (on the x-axis) that would purchase the voucher (a), the soap (b), or history sheet (c) for each of the 21 possible prices (on the y-axis).

C Descriptives and Balance Tests

Table C.1: Dyad-Level Summary Statistics

Dyad-level Covariates	*Means*			*t-statistics*		*Normalized Diff.*	
	Control	Meeting	Distance				
	C	M	D	M – C	D – C	M – C	D – C
True Plot Distance	10.88	10.78	10.81	-0.26	-0.18	-0.01	-0.01
Perceived Plot Distance	18.02	18.32	17.54	0.46	-0.79	0.02	-0.03
Home Distance	15.09	15.30	16.05	0.25	1.00	0.01	0.03
Connection	7.43	7.35	7.50	-0.54	0.50	-0.02	0.02
Know Other	0.92	0.92	0.92	0.39	0.17	0.01	0.01
Receive Visit from Other	0.80	0.80	0.80	0.07	0.23	0.00	0.01
Pay Visit to Other	0.77	0.77	0.78	-0.43	0.25	-0.02	0.01
Related to Other	0.33	0.33	0.33	-0.50	-0.18	-0.02	-0.01
Socialize with Other	0.79	0.79	0.80	0.22	1.21	0.01	0.04
Borrow Money from Other	0.57	0.55	0.55	-0.89	-1.06	-0.03	-0.04
Lend Money to Other	0.56	0.55	0.55	-0.26	-0.20	-0.01	-0.01
Borrow Goods from Other	0.38	0.36	0.40	-1.05	1.33	-0.04	0.05
Lend Goods to Other	0.38	0.36	0.40	-1.10	1.14	-0.04	0.04
Receive Advice from Other	0.63	0.62	0.64	-0.63	0.56	-0.02	0.02
Give Advice to Other	0.67	0.65	0.67	-1.34	0.13	-0.05	0.00
Speak Agriculture with Other	0.80	0.79	0.81	-0.31	0.84	-0.01	0.03
Pray with Other	0.45	0.44	0.45	-0.66	-0.05	-0.02	-0.00
Other is Male	0.50	0.50	0.52	0.15	1.02	0.01	0.03
Other's Age	42.41	41.81	41.78	-1.30	-1.39	-0.04	-0.05
Other's Education	7.12	7.01	7.30	-0.94	1.43	-0.03	0.05
Other Can Read or Write	0.80	0.78	0.80	-1.53	-0.02	-0.05	-0.00
Other's Farm Area	2.20	2.12	2.22	-1.32	0.25	-0.04	0.01
Dyads per Group	2715	1313	1182				

Notes: The table shows the means and differences among groups (control C, meeting treatment M, distance treatment D) at the dyad level. The second, third, and fourth columns show the mean of each characteristic within each group. The fifth and sixth columns show the t-statistics for dyads in the meeting treatment vs. control, and for dyads in the distance treatment vs. control. The seventh and eightth column show the normalized difference for dyads in the meeting treatment vs. control, and for dyads in the distance treatment vs. control. Detailed variable definitions are provided in Appendix F.

D Alternative Specifications

Table C.2: Descriptives and Balance Tests - Choice Lottery

	Control Mean	*T-stats*	*Normalized Differences*	*Observations*
Decision Maker Lottery	(1)	(2)	(3)	(4)
Male	0.50	1.28	0.14	776
Age	42.09	0.45	0.05	776
Years of Education	7.02	1.64	0.18	776
Can Read or Write	0.79	0.06	0.01	776
Ever Involved in Farm Decisions	0.93	-1.30	-0.15	776
Ever Received Agro Training	0.13	0.47	0.05	774
Farm Area in Acres	2.14	0.47	0.05	778
Used Improved Seeds (past season)	0.41	0.70	0.08	757
Used Pesticides (past season)	0.35	-0.34	-0.04	778
Used Fertilizer (past season)	0.06	1.47	0.17	778
Choice Lottery	(1)	(2)	(3)	(4)
Male	0.50	0.43	0.05	776
Age	42.33	-0.83	-0.09	776
Years of Education	7.13	-0.54	-0.06	776
Can Read or Write	0.79	0.10	0.01	776
Ever Involved in Farm Decisions	0.93	-0.40	-0.04	776
Ever Received Agro Training	0.13	-0.30	-0.03	774
Farm Area in Acres	2.15	-0.14	-0.01	778
Used Improved Seeds (past season)	0.41	0.64	0.07	757
Used Pesticides (past season)	0.34	0.80	0.09	778
Used Fertilizer (past season)	0.07	0.24	0.03	778

Notes: The table shows the differences between farmers who have been selected by the decision maker lottery (top panel) or by the choice lottery (bottom panel) versus farmers who have not been selected. Column 1 reports the mean of each baseline characteristic among non-selected farmers. Column 2 reports the t-statistic of the mean difference between selected vs. non-selected farmers for each covariate. Column 3 reports the normalized difference between the two groups for each covariate. Column 4 reports the number of observations for each covariate. Detailed variable definitions are provided in Appendix F.

Table D.1: Effects of Belief Update Intervention, Including T^d

	Unscaled	Scaled			
		Equality of		Path Shape	
	(1)	Means (2)	Medians (3)	Triangle (4)	Half Circle (5)
Potential Good News	-9.03	-34.06	-29.98	-27.80	-32.05
	(21.86)	(21.19)	(21.34)	(21.43)	(21.29)
Good News	-70.44	-54.63	-57.37	-59.66	-56.23
	(37.73)*	(28.73)*	(30.10)*	(31.02)*	(29.48)*
Potential Bad News	-9.19	-21.42	-17.51	-15.80	-19.28
	(9.37)	(17.54)	(15.37)	(14.26)	(16.34)
Bad News	-16.18	-28.13	-25.59	-24.33	-26.85
	(10.64)	(19.86)	(17.40)	(16.13)	(18.50)
T^d	76.33	8.72	30.31	41.82	20.91
	(216.69)	(218.20)	(216.96)	(216.50)	(217.69)
Scaling Factor λ	-	$E(d^T)/E(d^P)$	$M(d^T)/M(d^P)$	$1/\sqrt{2}$	$2/\pi$
Control Means					
w_{ij}	11,772	11,772	11,772	11,772	11,772
Potential Good News	8	7	7	7	7
Potential Bad News	14	8	9	9	9
Observations	3,566	3,566	3,566	3,566	3,566
Of Which Pot. Good/Bad News	939/2435	1764/1802	1596/1824	1566/2000	1759/1807

Notes: The table reports ordinary least squares estimates based on specification (13). The dependent variable w_{ij} is the willingness-to-pay of the participant i for another farmer j. *Potential Good News* is equal to the absolute value of the difference between true and rescaled perceived distance in walking minutes if the difference is positive, $|d^T_{ij} - \lambda d^P_{ij}|$ if $d^T_{ij}\lambda - d^P_{ij} > 0$. *Potential Bad News* is equal to the absolute value of the difference between true and rescaled perceived distance in walking minutes if the difference is negative, $|d^T_{ij} - \lambda d^P_{ij}|$ if $d^T_{ij} - \lambda d^P_{ij} < 0$. *Good (Bad) News* is equal to *Potential Good (Bad) News* multiplied by a treatment indicator T_d equal to 1 if the true distance was revealed to the participant. Column (1) reports unscaled estimates (or $\lambda = 1$). In column (2) λ solves $E(d^T_{ij}) = \lambda E(d^P_{ij})$; so the perceived distance is rescaled to have mean equal to the true distance. In column (3) λ solves $Median(d^T_{ij}) = \lambda Median(d^P_{ij})$; so the perceived distance is rescaled to have median equal to the true distance. In column (4) λ solves $d^T_{ij} = \lambda d^P_{ij}$, d^T_{ij} is the hypotenuse of an isosceles right triangle and d^P_{ij} is twice each cathetus, $(d^T_{ij})^2 = (\frac{1}{2}d^P_{ij})^2 + (\frac{1}{2}d^P_{ij})^2$. In column (5) λ solves $d^T_{ij} = \lambda d^P_{ij}$, d^T_{ij} is the diameter of a circle and d^P_{ij} is half of a circumference, $2d^P_{ij} = 2\pi\frac{d^T_{ij}}{2}$. Then $\lambda = 2/\pi$. Values are in Ugandan Shillings. All specifications include participant fixed effects. Robust standard errors are given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level.

E Alternative Social Benefits Calculations

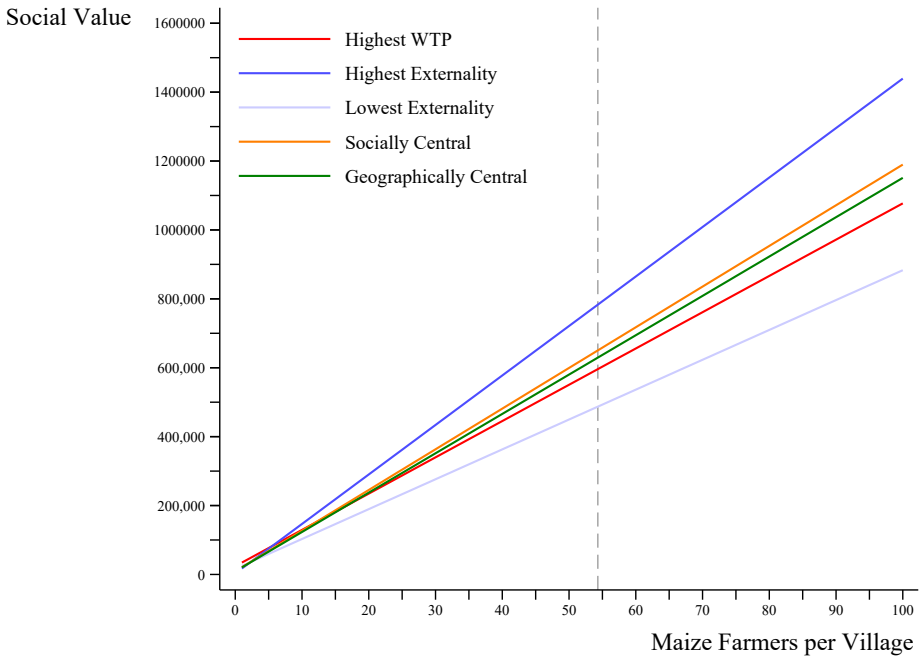
E.1 Including Villages Where Farmers Report the Maximum WTP

Table E.1: Social Value Generated by Different Targeting Criteria

	(1)	(2)	(3)	(4)
			Social Value if Targeting Criterion is:	
	WTP for Self	Per Capita Ext.	Highest WTP	Highest Ext.
Mean	20,695	11,595	584,024	775,075
	(11,909)	(6,941)	(507,119)	(656,516)
Median	20,000	10,000	445,429	580,571
Max	40,000	36,333	3,252,250	3,786,750
Min	0	500	59,500	98,000
N	780	780	103	103

Notes: The table shows summary statistics for four quantities indicated by columns (1)-(4). The first is willingness-to-pay for self, calculated at the farmer level. The second is the per-capita externality calculated at the farmer level using equation 11. The third is the social value produced by the farmer with the highest willingness-to-pay in a village, calculated at the village level using equation 12. The fourth is the social value produced by the farmer with the highest per-capita externality in a village, calculated at the village level using equation 12.

Figure E.2: Social Value By Population Size and Targeting Criterion



Notes: The figure depicts the mean social value produced by five types of farmers when the population of commercial maize farmers ranges from 1 to 100. The social value is calculated at the village level using equation 12 and averaging across villages. The five types of farmers are: the farmer with the highest willingness-to-pay in the village (red line), the farmer producing the highest per capita externality in the village (dark blue line); the farmer producing the lowest per capita externality in the village (light blue line); the most socially connected farmer (orange line), or the farmer with the highest *Connectedness* (see Appendix F); the farmer with the most geographically central maize plot (green line). The mean value of the commercial farmer population size in my sample is equal to 54 and marked by a dashed vertical gray line.

F Variable List

Outcome Variables	
Bad News	$T_d \times Potential\ Bad\ News$.
$Connectedness_i$	The average $Connection_{ji}$, or the connection with participant i reported by other farmers j's.
$Connection_{ij}$	The social connection between a participant i and the other farmer j as reported by i, ranging from 0 to 13. It is the sum of 13 dummies; • *Know Other*, i reports knowing j • *Receive Visit from Other*, i receives visits from j • *Pay Visit to Other*, i pays visits to j • *Related to Other*, i is related to j • *Socialize with Other*, i socializes with j • *Borrow Money from Other*, i would borrow money from j • *Lend Money to Other*, i would lend money to j • *Borrow Goods from Other*, i would borrow goods from j *Lend Goods to Other*, i would lend goods to j • *Receive Advice from Other*, i receives advice from j in matters about health, agriculture, school, personal, market prices. • *Give Advice to Other*, i gives advice to j in matters about health, agriculture, school, personal, market prices • *Speak Agriculture with Other*, i speaks to j about agriculture • *Pray with Other*, i goes to church or mosque with j
Good News	$T_d \times Potential\ Good\ News$.

Home Distance	The physical distance between the dwellings of the participant and the other farmer. The GPS-measured distance in miles is converted to walking distance in minutes assuming that it takes 20 minutes for an average adult to walk one mile.
Invited to Meet Other	A dummy variable taking the value of one if the participant was offered to meet with the other farmer.
Perceived Plot Distance (d_{ij}^P)	The respondent's guess on the time it takes to walk between the respondent's (i) largest plot and the other's (j) closest plot .
Potential Bad News	The absolute value of the difference between the true plot distance d_{ij}^T and the perceived plot distance d_{ij}^P, if the difference is negative.
Potential Good News	The absolute value of the difference between the true plot distance d_{ij}^T and the perceived plot distance d_{ij}^P, if the difference is positive.
Spillover Farmer	A dummy variable taking the value of one if the farmer did not receive training.
T_d	A dummy variable taking the value of one if the respondent was told the true distance between their largest plot and the other's closest plot, d_{ij}^T.
Trained Farmer	A dummy variable taking the value of one if the farmer received training.
Trained Village	A dummy variable taking the value of one if training took place in the village.
True Plot Distance (d_{ij}^T)	The physical distance between the centroids of the respondent's (i) largest plot and the other's (j) closest plot in walking minutes. The GPS-measured distance in miles is converted to walking distance in minutes assuming that it takes 20 minutes for an average adult to walk one mile.
Baseline Variables	
Can Read or Write	A dummy variable taking the value of one if the participant can read and write in any language.
Education	The number of completed years of education of the participant.
Farm Area	The area of the land cultivated by the participant's household, in acres.

Male	A dummy variable taking the value of one if the participant is male.
Other's Age	The age of the potential training recipient in completed years.
Other's Education	The number of completed years of education of the potential training recipient.
Other Can Read or Write	A dummy variable taking the value of one if the potential training recipient can read and write in any language.
Other's Farm Area	The total size of the land cultivated by the potential training recipient, in acres.
Other is Male	A dummy variable taking the value of one if the potential training recipient is male.
Used Fertilizer	A dummy variable taking the value of one if the participant's household used chemical fertilizer on their farm in the previous agricultural season.
Used Improved Seeds	A dummy variable taking the value of one if the participant's household used improved seeds on their farm in the previous agricultural season.
Used Pesticides	A dummy variable taking the value of one if the participant's household used pesticides on their farm in the previous agricultural season.

Table F.2: List of Variables

G Elicitation and Belief Update Survey Instrument

Instructions

1. Did the Fall Armyworm affect your farm during 2018? (Yes, No)

   I would like to tell you something about the Fall Armyworm. It spreads by proximity. This means that if someone else's plot is attacked by Fall Armyworm and your plot is close-by, the Fall Armyworm will likely spread to your plot. The closer an attacked plot is to your plot, the more likely it is that the Fall Armyworm spreads to your plot too.

2. Would you be interested in learning more about the Fall Armyworm and how if affects maize and other crops? (Yes, No)

   Stockholm University is organizing a training about the Fall Armyworm. The training is about 3 hours long, it is given to a farmer individually, and the teacher is an agriculture extension worker that speaks your language. The training includes a practice session in the field. During the training, the agriculture extension worker will teach: 1) how to recognize a Fall Armyworm infestation; 2) how to prevent an infestation; 3) how to tackle an infestation; 4) how to apply pesticide and which pesticide to use.

   Maximum one farmer can receive the training about Fall Armyworm from Stockholm University in this village.

   I am now going to ask for your willingness to pay for receiving this training yourself, in the same way as we did before [Reference to practice session].

   After that, I will ask for your willingness to pay for [Farmer 1] to receive the training. Then your willingness to pay for [Farmer 2] to receive the training, and then the same for[Farmer 3], [Farmer 4], [Farmer 5], [Farmer 6], [Farmer 7], [Farmer 8].

   So in total, you will choose N willingness-to-pay for training. For yourself, for[Farmer 1], [Farmer 2],[Farmer 3], [Farmer 4], [Farmer 5], [Farmer 6], [Farmer 7], [Farmer 8].

   You can choose from a list of 21 possible prices for the training and we will ask you whether you would be willing to pay each possible price for it. The prices range

from 0 to 40,000 UGX and increase by 2,000 UGX each time. For example we will ask: "Would you buy the training for 8,000 UGX?", "Would you buy the training for 10,000 UGX?", and so on.

After you have told me your N willingness-to-pay choices, for yourself and the other farmers, I will give you a price card with your price, like this one: [Enumerator Shows Card]

This price may be 0, 2000, 4000, ..., up to 40000 UGX. The price has been randomly selected by the computer and I DO NOT KNOW IT. Note that all training prices are equally likely.

Only one farmer in this village is selected to be a Decision Maker. If you are selected to be a Decision Maker, we will randomly select one of your N choices to be the one that matters for purchasing the training.

Let's suppose you are selected to be the Decision Maker. If for example your choice that matters is your willingness-to-pay for yourself, you will purchase the training ONLY if your willingness-to-pay for training is equal or larger than the price on the price card. If your choice that matters is your willingness to pay for [Farmer 1] , you will purchase the training ONLY if your willingness-to-pay for [Farmer 1] is equal or larger than the price on the price card. Any of your choices could be selected to be the choice that matters.

[...] we understand that you may not have enough money with you today to pay for the training. Instead, after opening the price, if you can purchase the training we will ask you to sign a document promising that you will pay for the training.

You can pay using cash on training day. We will contact you 3 days before training day to inform you that we are going to collect the money. We can also give you telephone numbers you can call if you have any concerns. Notice that buying is mandatory if you are selected to be the decision maker. If the price you agreed to pay is higher than the price on the price card, you HAVE TO purchase the training paying the price you opened.

Willingness-to-Pay for Self

1. Would you buy the training for yourself 0 UGX? (Yes, No)
2. If no: are you sure you would not buy the training for 0 UGX? (Yes, No)
3. If no: would you buy the training for 0 UGX? (Yes, No)

4. Would you buy the training for yourself for 2,000 UGX? (Yes, No)
5. If no: are you sure you would not buy the training for 2,000 UGX? (Yes, No)
6. If no: would you buy the training for 2,000 UGX? (Yes, No)
7. Would you buy the training for yourself for 4,000 UGX? (Yes, No)
8. If no: are you sure you would not buy the training for 4,000 UGX? (Yes, No)
9. If no: would you buy the training for 4,000 UGX? (Yes, No)

[These questions are asked for a training price up to 40,000 UGX]

Willingness-to-Pay for Other

Before we proceed, let me ask you some questions about your relationship with [Farmer 1].

1. Do you know [Farmer 1] ?
2. Does [Farmer 1] visit your home?
3. Do you visit [Farmer 1] 's home?
4. Is [Farmer 1] your relative?
5. Do you socialize with [Farmer 1] ?
6. Would you borrow money from [Farmer 1] ?
7. Would you lend money to [Farmer 1] ?
8. Would you borrow goods like fuel/paraffin, salt, hoes, etc. from [Farmer 1] ?
9. Would you lend goods like fuel/paraffin, salt, hoes, etc. to [Farmer 1] ?
10. Do you receive advice from [Farmer 1] (health, agriculture, school, personal, market prices)?
11. Do you give advice to [Farmer 1] (health, agriculture, school, personal, market prices)?
12. Do you speak to [Farmer 1] about agriculture?
13. Do you go to church/mosque with[Farmer 1] ?

14. I would like to ask you some information about the walking distance from your plot called [Plot Name] to the closest plot of [Farmer 1]. Think about the plot of [Farmer 1] that is closest to your plot [Plot Name].

    [ENUMERATOR: PAUSE HERE. LET THE RESPONDENT THINK. THIS IS A DIFFICULT QUESTION.]

    How long does it take to walk from your plot [Plot Name] to the closest plot of [Farmer 1] ?

15. (ONLY IF DISTANCE TREATMENT:) You said that it takes m minutes to walk from your plot [Plot Name] to [Farmer 1]'s closest plot. According to our information, [Farmer 1]'s closest plot is M Miles from [Plot Name], which means it actually takes about t minutes to walk from [Plot Name] to [Farmer 1]'s closest plot.

    I am now going to ask about your willingness-to-pay for [Farmer 1] to receive the training.

16. (ONLY IF MEETING TREATMENT:) Please bear in mind that if you are the Decision Maker and your choice about [Farmer 1] is selected, we will give you the opportunity to meet [Farmer 1]. If [Farmer 1] receives training, the meeting will take place after [Farmer 1] has received the training. At the meeting, you and [Farmer 1] will have the chance to discuss how to handle a Fall Armyworm infestation. Notice that the meeting will take place regardless of whether [Farmer 1] receives training or not.

17. Would you pay 0 for [Farmer 1] to receive the training? (Yes, No)
18. If no: are you sure you would not pay 0 for [Farmer 1] to receive the training? (Yes, No)
19. If no: would you pay 0 for [Farmer 1] to receive the training? (Yes, No)
20. Would you pay 2,000 for [Farmer 1] to receive the training? (Yes, No)
21. If no: are you sure you would not pay 2,000 for [Farmer 1] to receive the training? (Yes, No)
22. If no: would you pay 2,000 for [Farmer 1] to receive the training? (Yes, No)

23. Would you pay 4,000 for [Farmer 1] to receive the training? (Yes, No)

24. If no: are you sure you would not pay 4000 for [Farmer 1] to receive the training? (Yes, No)

25. If no: would you pay 4,000 for [Farmer 1] to receive the training? (Yes, No) [These questions are asked for a training price up to 40,000 UGX]

[These questions are asked for each other sampled farmer in the village]