Can Network Theory-based Targeting Increase Technology Adoption?

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Abstract

In order to induce farmers to adopt a new agricultural technology, we use predictions from the threshold model of diffusion to target information to key individuals within villages in Malawi. We combine social network data and model simulations to *ex ante* determine who is treated in our field experiment. We observe adoption decisions in 200 villages over 3 years. Our results are consistent with a model in which many farmers need to learn from multiple people before they adopt themselves. This means that without proper targeting of information, the diffusion process can stall and technology adoption remains perpetually low.

JEL Codes: O16, O13

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1. Introduction

Technology diffusion is critical for growth and development (Alvarez et al. 2013, Perla and Tonetti 2014). Information frictions are potential constraints to technology adoption, and social relationships can serve as important vectors through which individuals learn about, and are then convinced to adopt, new technologies.[1] With a better understanding of the diffusion process and how people choose to adopt new technologies, we could potentially manipulate social learning and identify strategies that would maximize diffusion. In this paper, we implement a field experiment in which we choose entry points of information into a social network and introduce a productive new agricultural technology via those entry points across 200 villages in Malawi.

Our experiment explores whether agricultural extension services can be improved to induce technology adoption. Agricultural productivity growth in Africa has stalled (World Bank 2008), in part because of a slow adoption rate of new technologies. Extension is the key policy tool governments use to promote technology adoption (Anderson and Feder 2007), and it often relies on social learning. A large literature has established that social learning about agricultural practices influences the uptake of new technologies among farmers (Griliches 1957, Foster and Rosenzweig 1995, Munshi 2004, Bandiera and Rasul 2006, Conley and Udry 2010, Burlig and Stephens 2019, Islam et al 2019). We implement insights from the "threshold model" of diffusion (e.g. Granovetter 1978, Centola and Macy 2007, Acemoglu *et al* 2011) - which postulates that individuals adopt a behavior only if they are connected to at least a threshold number of adopters – in a field experiment on agricultural extension. It is an attractive model to test in the context of agricultural technology adoption for several reasons. First, different thresholds in adoption are naturally micro-founded through a naive Bayesian learning model, as we demonstrate in section 2. Second, it has clear policy relevance: if farmers need more

[1] Large literatures in economics (Duflo and Saez 2003, Munshi 2008, Magruder 2010, Beaman 2012), finance (Bursztyn et al. 2013), sociology (Rogers 1962), and medicine and public health (Coleman et al 1957, Doumit et al. 2007, Oster and Thornton 2012) show that information and behaviors spread through inter-personal ties.

than one connection who has adopted before they themselves adopt (what this literature calls *complex contagion*), this would generate a very slow and in many cases permanently stalled adoption pattern. We anticipate that learning about a new agricultural technology in a developing country is precisely a context in which agents may have a high threshold. Third, in threshold models, the choice of network entry points used to influence diffusion becomes crucial, in that you cannot easily replicate the diffusion gains of strategic targeting by simply training a few additional farmers (Akbarpour, Malladi and Saberi 2018).

We partner with the Ministry of Agriculture in Malawi to run experiments that could potentially enhance the effectiveness of its extension services by asking extension agents to target farmers in the village who will induce widespread social learning. Suppose an extension agency can train two farmers per village on a new technology. Whom should it train to maximize diffusion? If knowing only one farmer who has adopted is sufficient for most farmers to adopt—a threshold known as *simple contagion* (Centola and Macy 2007)—the extension agent would optimally spread the entry points far apart to minimize repetition and redundancy in the same part of the network. Instead, if farmers need to know multiple adopters to be persuaded to adopt (*complex contagion)*, it is critical that the entry points are clustered together and share connections, in order to improve the chances that some recipients will learn from multiple sources simultaneously. The design of our field experiment is based on this insight.

Our experiment proceeded in the following steps. We first collected social network census data on agricultural learning relationships in 200 villages in Malawi. We then conducted simulations on those data to identify the two theoretically optimal entry points ("seeds") that would maximize diffusion of information about a new technology, assuming the diffusion process is characterized by either simple contagion or complex contagion. In 50 villages, the two seed farmers were chosen based on the simple contagion model and in another 50 villages according to complex contagion. Ministry of Agriculture extension agents trained the selected seeds. The specific technology promoted, 'pit planting', has the potential to significantly improve maize yields in arid areas of rural Africa.[2] It is a practice that was largely unknown in Malawi, and learning is therefore crucial for the diffusion of this technology. After being trained, seeds were asked to disseminate information about pit planting. We then trace adoption patterns in these villages over the next 3 years.

We compare the adoption in these network theory-based treatment villages against a benchmark treatment of 50 randomly selected villages, where agricultural extension agents use their local knowledge to select seeds.[3] As another comparison, we implement a fourth arm in which we choose seeds as we did in complex contagion villages but where we proxied social network ties with geographic proximity (we call this arm the 'Geo' treatment). Unlike social network relationships, geographic location is easy for extension agents to observe, so we view this as a first step towards a policy-relevant alternative to data intensive approaches.

A key insight from the threshold model is that poor targeting could lead to a complete failure of adoption within the village. We observe no diffusion of pit planting in 45% of the 'benchmark' villages after 3 years. In villages where seeds were selected using the complex contagion model, there was a 56% greater likelihood ($p<0.01$) that at least one person other than the seeds adopts in the village, relative to the benchmark. The results suggest that simply changing *who* is trained in a village on a technology based on social network theory can increase the adoption of new technologies compared to the Ministry's existing extension strategy.

[2] It has been shown to increase productivity by 40-100% in tests conducted under controlled conditions (Haggblade and Tembo 2003); in large-sample field tests conducted under realistic "as implemented by government" conditions (BenYishay and Mobarak 2019), and using experimental variation among villagers in the present study.

[3] Extension workers may be able to select influential partners based on specialized knowledge such as her eagerness to try the new technology, or the trust other villagers place in their opinions. As such, this benchmark provides a demanding test for network-based diffusion theory: our theoretically optimal partners were selected only by their position in the network, without the advantage of this additional local information.

During the 3-year period of the experiment, pit planting adoption grew from 0% to about 11% in the complex contagion villages. This rate of increase in adoption is comparable to the spread of some very profitable new agricultural technologies (e.g. Munshi 2007). Ryan and Gross (1943) show that it took 10 years for hybrid seed corn to be adopted in Iowa in the 1930s, and there was often 5 years between when a farmer heard of the technology and adopted it. The adoption rate is 3 percentage points lower in benchmark villages in years 2 and 3, though only the year 2 differences are statistically significant. We use our micro data on exactly which farmers adopt to provide more direct evidence in favor of the learning model we postulate. For example, we document larger and more sustained gains in adoption in complex contagion villages for the subset of farmers for whom returns to this technology are likely high (given their land-type), and in villages where farmers were initially uninformed about the technology, as predicted by the micro-foundation of the theory. By testing multiple predictions of the threshold model, we argue that - taken together – the evidence suggests there is a meaningful number of farmers who face a threshold above one, and for whom targeting information is important.

Even the low-cost geography-based targeting strategy generates some gains in adoption relative to the benchmark. However, physical proximity does not appear to be a good proxy for social connections in this context. Developing other low-cost proxies for social network structure would be a useful avenue for future research.[4] As a first step, we develop an intuitive algorithm to identify productive extension partners that can be implemented with a small number of interviews, and simulations on our data show that this method would generate large gains in technology adoption.[5]

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[4] For example, promising results in Banerjee et al. (2019b) imply that households know who is central in their village, and this type of information may be easily elicited from a random sample of people. Kim et al. (2015) use a related elicitation mechanism based on friends-of-friends, and conduct an experiment to distribute public health coupons in a sample of 32 villages. Breza et al (2019) provide a method for collecting aggregated relational data from a sample of individuals to feasibly estimate network statistics.

[5] A variety of other papers test the ability of local institutions, such as nominations or focus groups, to identify useful partners: Kremer et al (2011) identify and recruit 'ambassadors' to promote water chlorination in rural Kenya, Miller and Mobarak (2015) first markets improved cookstoves to 'opinion leaders' in Bangladeshi villages before marketing to

The literature has shown that more extensive diffusion takes place when entry points are more central (Banerjee et al. 2013 in the context of microfinance in India; Banerjee et al. 2019b in the context of immunization in India; Kim et al. 2015 looking at health behaviors in Honduras). Our experiment examines whether network theory has predictive power[6] to speed up the diffusion of a policy-relevant technology, and features a test of threshold models that improve our understanding of the diffusion process.

The rest of the paper is organized as follows. We present the theoretical model, and its micro-foundation, on which the experimental design is based in Section 2. Section 3 explains the experimental setting and design, along with details on the implementation of the intervention. Section 4 describes the data. Section 5 discusses the empirical results, including the village-level experimental results and heterogeneity analysis. Section 6 uses simulations on our data to explore cost-effective and policy-relevant alternatives to the data-intensive network-theory based procedures we experiment with in this paper. Section 7 concludes.

2. Theoretical model

The intuition for the importance of the threshold model can be seen using the example network below. In this thought experiment, we train two seed farmers in period 0 such that they are fully informed about a new technology. Diffusion occurs as farmers become informed in subsequent periods.

others, and BenYishay and Mobarak (2018) incentivize 'lead farmers' and 'peer farmers' to partner with agricultural extension officers in Malawi.

[6] In contrast, Carrel et al (2013) offers a cautionary tale that data-driven attempts to manipulate social interactions do not have predictive power to design optimal classrooms.

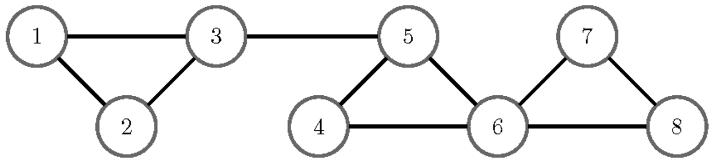

Figure 1: An example network

Suppose that farmers in this network become fully informed of a new technology if anyone they are connected to has been fully informed. This is what we call simple contagion. In this network, the ideal seed farmers will be farmer 6 and then either farmer 1, 2 or 3. With any of these configurations, all farmers are informed in period 1. Essentially, farmer 6 is central in his part of the network, and he will inform farmers 4, 5, 7 and 8 all in the first period. Since farmers 1, 2 and 3 are all connected to each other, training any of them will be sufficient to guarantee they are all informed at the end of the first period. In general, quickly diffusing information about the new technology will be easy: in 70% of all possible seed pairings, all farmers will be fully informed by the end of the second period.

However, if farmers need to know two other farmers before they have sufficient information to be fully informed, the diffusion process looks very different. Consider seeding farmers 5 and 8. During the first period, farmer 6 will become informed. In the second round, farmers 4 and 7 are informed. The diffusion process then stops with 3 out of a possible 6 non-seed farmers informed. There are 4 seed pairings which can achieve this 50% adoption rate, but it is not possible to get any higher. Moreover, without a focus on targeting, there is a good probability that there is no diffusion: in 40% of seed pairings, there is no diffusion whatsoever.

2.1 A micro-foundation for the threshold model of diffusion

The linear threshold model (Granovetter 1978, Acemoglu et al. 2011) is one of the seminal descriptions of diffusion processes. This model posits that an agent will adopt a new behavior once at least λ of his connections adopt the technology. We base our experimental design on this class of models for three reasons. First, the threshold model is built on a very natural insight about how social learning might affect adoption decisions: a farmer learns from the behavior of each connection she has, and depending on the farmer's priors, it may take more or fewer connections to motivate her to change behavior. The threshold formulation is therefore more naturally micro-founded with a model of learning, relative to other canonical diffusion models such as the SI (Susceptible-Infected) model.[7] Second, the threshold model has served as an important building block for diffusion theory. The original paper that introduced this formulation (Granovetter 1978) has been cited about 5000 times on Google Scholar. Third, the formulation is consistent with some key empirical patterns about technology diffusion in agriculture. For example, the number of contacts acting as a key driver of adoption decisions can explain the well-known S-shaped diffusion pattern for new technologies (e.g. Griliches 1957).[8]

There is little consensus on the underlying behavioral mechanisms generating thresholds in the model. This section therefore formally derives the threshold model as the outcome of optimizing behavior of microeconomic agents, so that we can take some clear predictions to the experiment and the micro data. We develop this micro-foundation by extending a framework presented in Banerjee et al. (2016) (hence: BBCM). One key insight in BBCM is that the majority of members of a social network may not have access to *any* useful signal when they are confronted with an entirely new

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[7] Both simple and complex contagion formulations are related to Bayesian learning models. In simple contagion, a single contact can induce adoption, suggesting that a person's prior (to not adopt) is not very strong. In contrast, complex contagion suggests that additional observations of adoption are necessary to move most people's priors. We will use this simpler version rather than a formal Bayesian learning model as those models quickly become intractable in real world networks (Chandrasekhar, Larreguy and Xandri 2019).

[8] The slow rate of diffusion in early stages can be explained by not many people in a network having multiple contacts who have adopted when a technology is new, but the probability of having multiple adopter contacts increases more rapidly as the technology spreads through the network. In general, both this intuition and examples of threshold modeling have been unspecific as to whether the threshold is in the number of contacts, or the fraction of contacts. The micro-foundation we develop below produces a threshold in the number of contacts.

technology. Thus, there are two parts to the learning problem for new technologies: acquiring a signal in the first place (becoming informed) which may be costly, and forming a revised belief on the profitability of the new technology based on the signals received from informed connections. Optimizing farmers adopt a new technology only if their beliefs change, and they are convinced by others that this would be more profitable than alternatives.[9]

There are three key phases of decision-making in our model: (1) the farmer has to decide whether to acquire information[10], (2) she has to combine the new information with her priors, and (3) she then decides whether to adopt the new technology. We will present and solve the model backwards, starting with the third phase.

The farmer will choose to adopt the new technology in phase 3 if she believes that adoption will be profitable. Suppose farmer *j* knows the technology will cost her c_j to adopt and believes the new technology has either profit $\bar{\pi}$ or $\underline{\pi}$ $(\underline{\pi} < c_j < \bar{\pi})$.[11] Since the technology is new and farmer *j* is initially uninformed, she has a uniform prior as to whether the technology is profitable or not. She can aggregate signals given by her connections to update her prior and make an informed adoption decision.

We adopt the same learning environment modeled in BBCM: first, informed farmer *i* disseminates a binary signal, $x_i \in \{\underline{\pi}, \bar{\pi}\}$, which is accurate with probability $\alpha > \frac{1}{2}$. Uninformed

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9 A very different micro-foundation for a similar model is explored in Jackson and Storms (2018). In that model, thresholds become relevant as individuals face greater payoffs from conforming to the behavior of their connections. Since coordination incentives for smallholder adoption of new agricultural technologies adoption seem likely to be low, we pursue instead a model based on learning and individual optimization.

10 There is a growing literature on how agents decide whether to seek out information. Banerjee et al. (2019a) – which builds on theoretical work by Chandrasekhar, Golub and Yang (2019) – demonstrate in the context of India's demonetrization that some agents choose to remain uninformed in order to avoid shame. BenYishay et al. (2020) show that agents may choose not to receive agricultural information if the sender is a woman.

11 Here for simplicity we follow BBCM in assuming that the distribution of profits is binary and known. In practice, there will be uncertainty over a wider range of profits due to the potential performance of the technology under different agroclimatic conditions and different weather realizations. While posterior distributions will be much more complicated under more realistic depictions of uncertainty, the key intuition driving the threshold model will be unchanged.

farmers do not disseminate a signal. Second, farmers follow DeGroot learning (DeMarzo et al. 2003). DeGroot learning can be interpreted as a boundedly rational version of Bayes learning, and suggests that farmers aggregate signals from their connections without attempting to calculate the inherent correlation structure between those signals. That is, if farmer *j* sees a signal of $\bar{\pi}$ from both farmers *i* and *k*, she interprets that as two positive signals without decomposing the likelihood that farmer *i* and *k* are disseminating information obtained from the same source.[12] Once farmers have observed signals from their informed connections, they aggregate those signals via Bayes' rule.

This framework suggests the following for the second phase of the farmer's learning problem: suppose farmer *j* has D_j informed contacts. If farmer *j* decides to learn about the new technology from her informed contacts, and if *H* of those contacts provide the signal $x = \bar{\pi}$, then the farmer's posterior probability that $\pi = \bar{\pi}$ is given by[13]

$$E_j[\pi = \bar{\pi}] = \frac{\alpha^{2H-D_j}}{\alpha^{2H-D_j} + (1-\alpha)^{2H-D_j}}$$

Denote $\tilde{\bar{\pi}} = \bar{\pi} - \underline{\pi}$ and $\tilde{c_j} = c_j - \underline{\pi}$. With that posterior, the farmer would adopt the technology if

$$\frac{\tilde{c}_j}{\tilde{\bar{\pi}}} \leq \frac{\alpha^{2H-D_j}}{\alpha^{2H-D_j}+(1-\alpha)^{2H-D_j}} \leq \frac{\alpha^{D_j}}{\alpha^{D_j}+(1-\alpha)^{D_j}} \tag{1}$$

This model highlights a potential challenge to diffusing new technologies: when few other farmers are informed, then there is a ceiling on how much a new farmer's priors would move even if they receive unanimously positive signals from the informed. At early stages in the diffusion process, D_j may be small for most farmers.

[12] Chandrasekhar, Larreguy and Xandri (2019) provide laboratory evidence in support of DeGroot learning over Bayes learning in India. Additional citations in favor of this boundedly-rational approximation can be found in BBCM.

[13] A simple proof is given in BBCM.

Last, we consider the first phase of the farmer's learning problem, which is her decision to acquire signals and become informed. Here, we depart from BBCM to suggest that there may be a small cost to receiving a signal η. This cost could be interpreted as "shoe leather" costs of acquiring information (which are not necessarily trivial in villages in rural Malawi as households may be fairly far apart), or as stigma from seeking information (e.g. Banerjee et al. 2019a).

Thus, the farmer *j* with informed degree D_j has an objective given by

$$\max_{d \le D_j} \sum_{h \le d} \frac{1}{2} \left(\left[\alpha^h (1-\alpha)^{d-h} \left(\overline{\pi} - c_j \right) + (1-\alpha)^h \alpha^{d-h} \left(\underline{\pi} - c_j \right) \right] \left(I \left(\frac{\alpha^{2h-d}}{\alpha^{2h-d} + (1-\alpha^{2h-d})} > \frac{\tilde{c}}{\tilde{\tilde{\pi}}} \right) \right) \right) - \eta d$$

When $\eta = 0$, the dynamics of learning are explored by BBCM. However, when $\eta > 0$ the dynamics are slightly different. In that case (for small η), farmers will only become informed if

$$\frac{\alpha^{D_j}}{\alpha^{D_j} + (1-\alpha)^{D_j}} > \frac{\tilde{c}_j}{\tilde{\tilde{\pi}}} \tag{2}$$

In other words, farmers only choose to seek information if they have a large enough number of informed connections, such that it is possible that an informed decision would lead them to adopt. In this case (and for small η), farmers will choose to seek information when they have only one informed connection if

$$\frac{\alpha}{\alpha + (1-\alpha)} > \frac{\tilde{c}_j}{\tilde{\tilde{\pi}}} \tag{3}$$

In general, they will choose to become informed with λ informed connections if

$$\frac{\alpha^{\lambda}}{\alpha^{\lambda} + (1-\alpha)^{\lambda}} > \frac{\tilde{c}_j}{\tilde{\tilde{\pi}}} \tag{4}$$

This implies that farmers choose to become informed about new technologies if expectations about the net benefits of technology are high (i.e., low costs and high potential gains), or if signals from individual other farmers are highly accurate. Under certain parameter values, just a single informed contact may be sufficient to induce farmers to seek information. That is the diffusion process that Centola and Macy (2007) refer to as a "simple contagion." They demonstrate that some types of information – for example, job opportunities – spread in this way. On the other hand, if the expected upside of the technology is more modest relative to costs, or if signals from other farmers have low accuracy, then farmers may only be persuaded to seek information when there is sufficient information to be gained from their network.[14] In that case, for many farmers the lowest λ satisfying equation (4) may be larger than 1, and information diffusion follows a process termed "complex contagion" in the literature.[15]

Our interpretation of the microeconomics of the threshold theory is that the thresholds result from an underlying process of farmers deciding *whether* to learn, given their information environment. This motivates an experimental design in which we seed new information in a network to improve the overall information environment, which can change incentives to learn and jump-start the technology diffusion process.

Given that the econometrician is unlikely to observe signal accuracy (α), the threshold required for adoption of a specific new technology is an empirical question. As a numerical example, consider a technology with 30% potential returns (so that $\tilde{\bar{\pi}} = 1.3\ \tilde{c}_j$). If signals are more than 77% accurate, farmers will choose to become informed if they have a single informed connection, and diffusion will follow a simple contagion. If signal accuracy falls in the range 65% - 77% accurate, then farmers will only become informed if they have 2 informed connections, and learning will follow a complex contagion. If signals are less than 65% accurate, then farmers will need at least 3 informed

[14] Though not explicitly considered here, minimal thresholds for learning will also be higher if η (the cost of information acquisition) is larger.

[15] Several theory papers have explored the implications of this model. In contrast to the "strength of weak ties" in labor markets proposed by Granovetter (1978), strong ties may be important for the diffusion of behaviors that require reinforcement from multiple peers. Centola (2010) provides experimental evidence that health behaviors diffuse more quickly through networks where links are clustered, consistent with complex contagion. Acemoglu et al. (2011) highlights that when contagion is complex, highly clustered communities will need a seed placed in the community in order to induce adoption. Finally, Monsted et al. (2017) provide experimental evidence generated by twitter-bots that twitter hashtag retweets follow a process which more closely resembles complex than simple contagion.

connections to make an adoption decision. In general, agents will face higher thresholds in contexts where signals are noisier, a point with implications for external validity which we return to in the concluding remarks.

2.2 Model predictions and implications for the experiment

The micro-foundation of the threshold model suggests that the model would need to be tested using the diffusion of a truly new technology, where would-be adopters are *ex ante* uninformed about the technology and face an important adoption decision. A corollary is that the threshold model should fit the data better in locations where the technology is more novel. A good empirical setting to test the model is also one in which agents are receiving noisy signals from the network.

If thresholds exist and are above one, then seeding the network with multiple sources of information who are clustered in the same part of the network will achieve very different diffusion patterns than seeding the network with the same number of information sources spread more diffusely. Our experimental design will take advantage of this insight. When thresholds are above one, the information environment only induces learning when initial nodes share some connections, which we test using micro data on technology diffusion patterns.

The model highlights that farmers will become informed when they have sufficiently many informed contacts. However, conditional on being informed, they will only adopt the technology if the realization of signals from their connections are sufficiently positive. These two facts suggest two different tests of the model.

PREDICTION 1: If most farmers in a village have a threshold $\bar{\lambda}$, then people who are connected to at least $\bar{\lambda}$ informed farmers should become informed themselves.

PREDICTION 2: Adoption should increase most strongly among farmers who have high net benefits of adoption, who would adopt with a broader range of received signals.[16]

3. Field experiment

3.1 Setting

Our experiment on technology diffusion via an agricultural extension system takes place in 200 villages randomly sampled from 3 Malawian districts with largely semi-arid climates (Machinga, Mwanza, and Nkhotakota). Approximately 80% of Malawi's population lives in rural areas (World Bank 2011), and agricultural production in these areas is dominated by maize: 97% of farmers grow maize, and over half of households grow no other crop (Lea and Hanmer 2009). Technology adoption and productivity in maize is thus closely tied to welfare.

The existing agricultural extension system in Malawi relies on Agricultural Extension Development Officers, henceforth extension agents, who are employed by the Ministry of Agriculture and Food Security (MoAFS). Many extension agents are responsible for upwards of 30-50 villages, which implies that direct contact with villagers is rare. According to the 2006/2007 Malawi National Agricultural and Livestock Census, only 18% of farmers participate in any type of extension activity. Extension agents cope with these staff shortages by relying on a small number of lead farmers, who are trained but not incentivized to disseminate knowledge via social learning.[17] Against this backdrop of staff shortages, maximizing the reach of social learning in the diffusion process may be a cost-effective way to improve the effectiveness of extension.

[16] For clarity, the model assumed that the potential net benefits of production were known to the farmer before deciding whether to become informed about the technology. In practice, farmers may or may not be aware that their private net benefits to adoption are high before becoming informed. Only when a farmer is *ex ante* aware that she has relatively high net benefits will we see greater adoption associated with a greater propensity to become informed.

[17] The lead farmer model may additionally help farmers learn by facilitating frequent conversations. Banerjee et al. (2019a) show that informing a subsample of individuals may lead to greater diffusion compared to broadcasting general information, because it creates opportunities for follow-up conversations.

3.2 Experimental design

We partner with the Malawi Ministry of Agriculture to select the appropriate technologies to promote and engage extension staff to train exactly two seed farmers in each study village. Our experimental variation only changes how those seed farmers are chosen and holds all other aspects of the training constant. We identified the farmers in each of the study villages who would be the "theoretically optimal" choices as seeds under alternative formulations of the threshold model, where our objective is to maximize diffusion in the village over a 4-year horizon. Our four treatment arms randomly vary which theoretically optimal pair of seeds is trained in each village, as follows:[18]

1. Simple Contagion: Simple diffusion (λ=1) model applied to the network relationship data
2. Complex Contagion: Complex diffusion (λ=2) model applied to network relationship data
3. Geo Treatment: Complex diffusion (λ=2) model applied to network data constructed using only geographic proximity
4. Status Quo Benchmark: Extension worker selects the seed farmers based on his local knowledge

To implement this procedure, we first collected social network relationships data (to be described in detail in section 4) on the census of households in all study villages. The social network structures observed in these data allow us to construct network adjacency matrices for each of the 200 villages. Next, we conduct technology diffusion simulations for all villages using these matrices, where each individual in the village draws an adoption threshold τ from the data, which is normally

[18] In other words, we randomly assign "theories" or "threshold model formulations" to different villages. Randomization was stratified by district, and implemented using a re-randomization procedure which checked balance on the following covariates: percent of village using compost at baseline; percent village using fertilizer at baseline, and percent of village using pit planting at baseline. Randomization was implemented in each district separately.

distributed[19] N(λ, 0.5) but truncated to be strictly positive. We conduct simulations with $\lambda=1$ and $\lambda=2$ in all villages to evaluate simple and complex contagion respectively.

In the simulations, when an individual is connected to at least τ individuals who are informed, she becomes informed in the next period. Once an individual is informed, we assume that all other household members are immediately also informed. We also assume that becoming informed is an absorbing state. As seed farmers are trained by extension agents, we assume all assigned seed farmers become informed.

We run the model for four periods.[20] Given the randomness built into the model, we simulate the model 2000 times for each potential pair of seeds in the village, and create a measure of the average information rate induced by each pair. We designate the pair that yields the highest average three-period information rate in our simulations as the two "*optimal seeds*" for each village for that particular model (simple contagion, $\lambda=1$ or complex contagion, $\lambda=2$). Armed with the identities of the optimal seeds under each model, we then randomly assign different villages in the sample to the treatment arms. The optimal seeds identified through the simple contagion ($\lambda=1$) simulation are trained on the technology in some randomly chosen villages assigned to treatment 1. Optimal seeds identified through the complex contagion ($\lambda=2$) simulation are instead trained in other villages that were randomly assigned to treatment 2.

To determine seeds for villages in the Geo treatment arm, the simulation steps are the same as in the Complex Contagion case, except that we apply the procedure to a different adjacency matrix. To capture the idea that geography may be an easy way to capture key features of a social network, we

[19] Heterogeneity in the model comes from variation across individuals in the net benefits realized by adopting pit planting. This affects the threshold number of connections an individual would need to have in order to get enough signals to be induced to adopt.

[20] We collected data for up to three agricultural seasons after the interventions were implemented, so our theoretical set-up matches our empirical research design. With knowledge of the value of λ, a policymaker could use the model to maximize adoption over any timeframe they cared about, either more short-term or more long-term.

generate an alternative adjacency matrix by making the assumption that two individuals are connected if their plots are located within 0.05 miles of each other in our geo-coded location data. We chose a radius of 0.05 miles because this characterization produces similar values for network degree measures in our villages as using the actual network connections measures.

The fourth group is the status-quo benchmark, where extension agents were asked to select two seed farmers as they normally would in settings outside the experiment. This benchmark constitutes a meaningful and challenging test for the simple and complex contagion treatments since the extension agents were able to use valuable information not available to researchers, such as the individual's motivation to take on the role. The benchmark treatment is similar to what the Malawi Ministry of Agriculture and other policymakers would normally do, so this is the most relevant counterfactual.[21]

Note that the Simple, Complex, and Geo seed farmer selection strategies were simulated in all 200 villages, so we know – for example – who the optimal simple contagion seed farmers would have been in a village randomly assigned to the complex contagion or the geo treatment. We label the counterfactual optimal farmers as "shadow seeds" or "shadow farmers".

3.3 Agricultural technologies

In this section, we describe the two technologies introduced to seed farmers and in appendix section A1 we analyze data on crop yields to give further insights into the benefits of the technologies.

[21] Normally the Ministry only trains one "Lead Farmer" per village, not two. In most villages, the Lead Farmer will already be established, except for villages in which there hasn't been an extension officer assigned to the village for a long time. The extension agents would have had to select a second seed farmer in benchmark villages due to the experiment.

Pit planting

Maize farmers in Malawi traditionally plant seeds in either flat land or after preparing ridges. Ridging has been shown to deplete soil fertility and decrease agricultural productivity over time (Derpsch 2001, 2004). In contrast, pit planting involves planting seeds in a shallow pit in the ground, in order to retain greater moisture for the plant in an arid environment, while minimizing soil disturbance. In our sample, pit planting was not widely practiced at baseline: 9/4,004 farmers (0.22%) planted with pits the year prior to treatment. The technique is practiced more widely in the Sahel, and has been shown to greatly enhance maize yields both in controlled trials and in field settings in East Africa, with estimated gains of 50-113% in yields (Haggblade and Tembo 2003, BenYishay and Mobarak 2019). In appendix section A1, we present evidence that pit planting increased yields by 44% (a treatment on the treated estimate) for our trained seed farmers. The enhanced productivity is thought to derive from three mechanisms: (1) reduced tillage of topsoil, which allows nutrients to remain fixed in the soil rather than eroding, (2) concentration of water around the plants, which aids in plant growth during poor rainfall conditions, and (3) improved fertilizer retention.

Practicing pit planting may involve some additional costs. First, only a small portion of the surface is tilled with pit planting, and hand weeding or herbicide requirements may increase, though focus groups undertaken by the authors suggest that weeding demands were reduced substantially relative to ridging. Second, digging pits is a labor-intensive task with large up-front costs. However, land preparation becomes easier over time, since pits should be excavated in the same places each year, and estimates suggest that land preparation time falls by 50% within 5 years (Haggblade and Tembo 2003). BenYishay and Mobarak (2019) find that in Malawi, labor time decreases while the change in other input costs are negligible in comparison. Labor costs are minimized when pit planting is used on flat land.

Crop residue management

Seed farmers were also trained in crop residue management (CRM), a set of farming practices which largely focus on retention of crop residues in fields for use as mulch. Alternative practices commonly used by farmers include burning the crop residues in the fields and removing them for use as livestock feed and compost. The trainings emphasized the value of retaining crop residues as mulch to protect topsoil, reduce erosion, limit weed growth, and improve soil nutrient content and water retention. There is little experimental evidence on the impacts of CRM on soil fertility, water retention, and yields in similar settings.

3.4 Seed farmers: descriptive statistics, training, and take up

The procedure described in section 3.2 generated a list of two seed farmers to train in the simple, complex and geo villages, plus shadow farmers. Extension agents chose the seed farmers in the benchmark villages. In 50% of villages, there was at least one seed who was selected as optimal in more than one (simple, complex or geo) model. Appendix table A1 provides some summary statistics describing how the chosen seeds differ along observable characteristics. The analysis includes both actual seeds and counterfactual seeds (i.e. shadow farmers) to maximize sample size.[22] The most striking pattern in Table A2 is that the farmers selected as seeds under the geographic treatment are significantly poorer than other seeds. This is because many households live on one of their plots in Malawi. Households who are geographically close to lots of people will mechanically have less land, and these households tend to be poorer overall. In terms of position within the network, seed farmers selected through the complex contagion simulations are the most "central" across all measures of network centrality we compute, including degree, between-ness and eigenvector centrality (columns

[22] Table 1 is not demonstrating balance in the randomization of villages across treatment arms. Note that there are only 100 benchmark farmers since we never observe shadow benchmark farmers.

3-5).[23] Simple seeds have similar betweenness centrality as complex seeds, but lower eigenvector centrality.

Essential to the experimental design, we observe that there are more households connected to both seeds in complex contagion villages than in other treatment arms. 35% of our random household sample has a connection to a simple seed, and 6% are connected to both simple seeds. By contrast, 18% of households are connected to two complex seeds. For the geo-based seeds, 10% of households are connected to two seeds. Appendix table A3 displays the distribution of how far - in social distance - households are from the seeds in the different treatment arms.

In addition to the names of the two seed farmers, we provided extension agents in experimental arms 1-3 with replacement names if either of the first two refused to participate. Refusal was uncommon: extension agents trained[24] 93% of the selected seeds or their spouses. We conduct intent-to-treat analysis using the original seed assignment.

The seed farmers received a small in-kind gift (valued at US$8) if they themselves adopted pit planting in the first year. There was no gift or incentive provided on the basis of others' adoption in the village or the seeds' own adoption in subsequent years. Appendix table A4 demonstrates that the training (and incentive) was effective at inducing adoption, but not perfectly. Seed farmers, relative to the shadow farmers, are more likely to know how to do pit planting and more likely to adopt pit planting during the first agricultural year.[25] 30% of seed farmers adopted pit planting during year 1,

[23] Eigenvector Centrality is weighted sum of connections, where each connection's weight is determined by its own eigenvector centrality (like Google page-rank). Betweenness centrality captures that a person is important if one has to go through him to connect to other people. Therefore it is calculated as the fraction of shortest paths between individuals in the network that passes through that individual. See Jackson (2008) for more details.

[24] As the technologies themselves were new, the extension agents were themselves trained by staff from the Ministry's Department of Land Conservation.

[25] Seed farmers are also more likely to adopt crop residue management (CRM) in year 1. However, by year 2 there is no longer a meaningful gap in the CRM adoption rate, and in fact the adoption rate among shadow farmers is declining

compared to 5% of shadow farmers (p<.01). Moreover, the adoption rate among seed farmers is the same across all treatment arms: complex, simple, geographic, and benchmark.

Knowledge and adoption rates of pit planting increase among the shadow farmers over time while it remains more or less constant among seed farmers. This reduces the size of the knowledge and adoption gap with trained seed farmers in years 2 and 3, but there remains a statistically significant difference. The appendix A.2 and the notes to appendix table A4 provide the details on the econometric specification used for these results.

4 Data

After training the seed farmers, we collected up to three rounds of household survey data. Appendix Figure A1 shows the timeline of these data collection activities. We describe each major data source in turn.

Social Network Census Data

Targeting based on different network characteristics requires relatively complete information on network relationships within the village (Chandrasekhar and Lewis 2016). We reached more than 80% of households participating in the census in every sample village.[26]

The main focus of the social network census was to elicit the names of people each respondent consults when making agricultural decisions. General information on household composition, socioeconomic characteristics of the household, general agriculture information, and work group membership was also collected. Agricultural contacts were solicited through several prompts.[27] These

over time. Given this pattern, and the fact that CRM was not a "new" technology in this area, we focus our analysis on the adoption of pit planting. We include CRM adoption results in Appendix table A7.

[26] We interviewed at least one household member from 89.1% of households in Nkhotakota, 81.4% in Mwanza and 88.6% in Machinga. We interviewed both a man and a woman in about 30% of households.

[27] We first asked in general terms about farmers with whom they discuss agriculture. To probe more deeply, we also asked them to recall over the last five years if they had: (i) changed planting practices; (ii) tried a new variety of seed, for

responses were matched to the village listing to identify links. Individuals are considered linked if either party named each other (undirected graph), and all individuals within a household are considered linked.

Sample Household Survey Data

We collected survey data on farming techniques, input use, yields, assets, and other characteristics for a sample of approximately 5,600 households in the 200 sample villages. We attempted to survey all seed and shadow farmers in each village, as well as a random sample of 24 other individuals, for a total of about 30 households in each village.[28] In villages with fewer than 30 households, all households were surveyed. Three survey rounds were conducted in Machinga and Mwanza in 2011, 2012 and 2013, and two survey rounds were conducted in Nkhotakota in 2012 and 2013.[29] The first round asked about agricultural production in the preceding year—thus capturing some baseline characteristics—as well as current knowledge of the technologies, which could reflect the effects of training. Since the data was collected at the start of a given agricultural season, but after land preparation was complete, we observe three adoption decisions for pit planting for farmers in Mwanza and Machinga, and two decisions for farmers in Nkhotakota. Since crop residue management

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any crop; (iii) tried a new way of composting; (iv) changed the amount of fertilizer being used for any crop; (v) tried a new crop, such as paprika, tobacco, soya, cotton, or sugar cane; or (vi) started using any other new agricultural technology. If they responded affirmatively, we asked respondents to name individuals they knew had previously used the technique in the past and whether they had consulted these individuals. Finally, we asked them if they discussed farming with any relatives, fellow church or mosque members, or farmers whose fields they pass by on a regular basis, or if there are any others with whom they jointly perform farming activities. We also elicited their close friends and contacts with whom they share food, though we did not include these contacts as agricultural connections for the purposes of our network mapping.

[28] In Simple, Complex and Geo villages there were 6 (2x3) seed and shadow farmers to interview, while in Benchmark villages there were 8 (2x4) seeds and shadows. Recall we do not observe Benchmark farmers in Simple, Complex and Geo villages.

[29] Unanticipated delays in project funding required us to start training of extension agents and seed farmers in Nkhotakota in 2012 instead of 2011 as we did in Mwanza and Machinga.

(CRM) decisions are made the end of an agricultural season after harvest, we observe CRM decisions for two agricultural seasons in Mwanza and Machinga, and one in Nkhotakota.

Randomization and Balance

Appendix Table A5 shows how observable baseline characteristics from the social network census vary with the treatment status of the village. The table also shows p-values from the joint test of all treatment groups. The table notes provide details on the specification used. Few differences across treatment groups are statistically significant. Overall, the joint test reveals no differences for 10 out of 12 variables. Farm size is the most concerning: farmers in the benchmark villages have larger farm sizes on average than farmers in simple and complex villages, and the joint test across the network treatment variables is significant at the 10% level. Additional analysis available from the authors controls for this variable in all specifications and finds that all results are robust to this control.

5 Empirical Results

A strength of our novel methodological approach is that we have a *set of predictions* on the behavior of villages under all four arms of the experiment. The following is a roadmap of how we present our empirical evidence. We start by providing evidence that our experiment induced diffusion within the social networks: first, we analyze whether individual farmers are more likely to have conversations about pit planting with trained seed farmers; and second, we ask if farmers who are socially closest to the seeds have more knowledge and are more likely to adopt pit planting in the initial years of the experiment. We then analyze village-level differences in adoption rates by treatment status. We present treatment effect estimates in the spirit of the standard approach in an impact evaluation. We also simulate the entire distribution of treatment effects in *all four* experimental arms under each theory, and compare the predictions against the distribution of treatment effects observed in the data. A key implication of the model is that targeting is only critical if the learning environment is complex. Under simple learning differences between treatments (and the benchmark) are small.[30] The overall speed of diffusion, and in particular the percentage of villages with no diffusion, is an important data point in assessing the relevance of the complex contagion model. We finish the section with heterogeneity analysis, examining two dimensions of heterogeneity motivated by the theory. We argue that the data fits complex contagion theory best, after aggregating across the full range of this evidence, and not just on the basis of any single program evaluation result.

5.1 Communication about pit planting

In this sub-section, we use data on conversations about pit planting that respondents had with others in the village. These conversations may arise from either the seed pushing information or from villagers seeking information. Each respondent was asked questions about seven other individuals in their village: whether they knew them, and what they had discussed. The seven individuals comprised of the two seed farmers, randomly selected shadow farmers, and a random sample of other village residents. To study whether the experiment led to social learning between the seeds and fellow villagers, we exploit the random variation from the experiment: for example, we compare the frequency of conversations with the complex seed farmers in complex contagion villages, to the frequency of conversations with complex shadow farmers in other villages.[31] With this design, if the experiment was successful in activating social learning, then we should observe statistically significant increases in conversations in the diagonal elements of Table 1.

[30] Our simulated results, presented in Table A5, suggest that geographic targeting should have generated the least diffusion under simple contagion, followed by the benchmark, followed by complex and simple contagion targeting who produce weakly more diffusion in short time intervals. These distinctions are all small and often insignificant, in contrast to simulated results under complex contagion.

[31] We estimate regressions where the dependent variable is talking about pit planting with the simple (complex, geo) farmer. While all sample respondents in Simple treatment villages were asked about simple farmers, not all respondents in the remaining villages were, since we chose a random subset of shadow farmers. This is analogously true for complex and geo villages. Therefore in the analysis we flexibly control for the number of simple (complex, geo) farmers we asked respondents about.

Table 1 shows that the experiment indeed induced seed farmers to discuss pit planting with fellow villagers. Columns (1)-(3) show that there are more conversations with trained seeds than with shadows.[32] The simple contagion treatment led to more conversations with the simple partner, the complex contagion treatment led to significantly more conversations with the complex partner, and so forth.

Column (4) examines whether the treatments increased the total number of conversations about pit planting in the village. The dependent variable is equal to 1 if a respondent discussed pit planting with either seed farmers or one of the randomly selected individuals within the village. We find that respondents in Complex villages have a slightly higher likelihood of a having at least 1 conversation about pit planting compared to Benchmark or Geo villages. We cannot statistically distinguish between Simple and Complex villages.

5.2 Knowledge and adoption of farmers by social distance to seeds

If adoption is a social contagion, individuals close to the seeds should be first to become informed and then adopt. To explore this, we estimate the following equation:

$$Y_{iv} = \alpha + \beta_1 1TSeeds_{iv} + \beta_2 2TSeeds_{iv} + \beta_3 1Simple_{iv} + \beta_4 2Simple_{iv} + \beta_5 1Complex_{iv} + \beta_6 2Complex_{iv} + \beta_7 1Geo_{iv} + \beta_8 2Geo_{iv} + \theta_v + \varepsilon_{iv}$$

$1TSeeds$ is an indicator for the respondent being directly connected to exactly one seed farmer, and $2TSeeds$ indicates the respondent was directly connected to two seefd farmers. Seeds and shadows are removed from the analysis. Since network position is endogenous, we also control for whether an individual is connected to one or two Simple, Complex or Geo (actual or shadow) seeds, but these coefficients are not displayed in the table. Identification therefore comes from variation in closeness

[32] We may observe a treatment effect on conversations with the Simple partner in Complex villages and conversations with the Complex partner in Simple villages for one of two reasons: (i) as mentioned above, often there is one individual who would be chosen as a seed in both the simple and complex versions of the model but (ii) this may also be the outcome of the diffusion process. It is challenging to disentangle these two alternatives.

to the seed generated by the experiment. As an example, we can compare two farmers who are both connected to two 'Simple seeds', but where one farmer is in a village randomly assigned to the Simple treatment and his friend is trained, while the other was not.

In the theoretical model, individuals have to become informed prior to adopting. As an empirical matter, it is unclear what level of knowledge is associated with "being informed" as used in the model. In table 2, we therefore consider three variables which represent increasing levels of information: whether the respondent has heard of pit planting; whether the respondent knows how to implement pit planting; and whether the respondent adopted pit planting (which implies not only knowledge but also that the signals that the respondent received were sufficiently positive). In season 1, the training led to more information transmission to those directly connected to seeds. In particular, those who have a direct connection to both seed farmers had the most knowledge. This is true for both measures of "knowledge": whether the respondent had heard of pit planting and whether they reported being capable of implementing it. Respondents with two connections are 8.4 percentage points more likely to have heard of pit planting than those with no connection to a seed. This represents a 33% increase in knowledge relative to the mean familiarity among unconnected individuals. This effect is also statistically significantly different from the effect of being connected to one seed (p=.02). They are also 6.2 percentage points more likely to report knowing how to pit plant, a 108% increase over unconnected individuals and again significantly different from the effect of being connected to one seed (p = 0.072). These knowledge effects are suggestive – but not conclusive – of a complex contagion process ($\lambda = 2$) rather than simple contagion. The increased awareness of pit planting and knowledge of pit planting among households connected to two seeds persists into season 2 (columns 2 and 5), and two connections is again significantly more advantageous than one connection (p=.04 and .095, respectively).

We see no effect on adoption in the first year (column 7) among individuals directly connected to either one or two seeds. However, we do observe an adoption effect in year 2. This temporal pattern of results is consistent with the set-up of our theoretical model: individuals become informed in year 1 and then some choose to adopt in year 2. Column (8) shows that households with two connections to trained seeds are 3.9 percentage points more likely to adopt in the second season than those with no connections, which represents a 90% increase in adoption propensity. Though the point estimate of the effect of 2 connections is considerably larger than the effect of a connection to one seed (3.9 pp compared to 1.2 pp), we cannot statistically reject that households with a connection to only one treated seed adopt less frequently (p=.16). We also observe that individuals who are within path length 2 of at least one seed (that is, a friend of a friend) are 2.2 percentage points more likely to adopt.

The predictions of the model for which individuals learn about pit planting are weakened as time passes and knowledge diffuses through the network. In all three of our dependent variables, this diffusion can be observed through large increases in knowledge and adoption over time in our reference category: individuals with no direct connections to a seed. Among this group awareness increases from 22% to 39% from year one to three, while "knowing how" to pit plant increases from 6% to 15% and adoption increases from 1% to 4%. In principle, this diffusion should reduce power on our exogenous variation, as the number of connections to informed individuals becomes less correlated with the number of signals available to farmers. In practice, by year 3 we still see significance on the effects of two direct connections on one of our two knowledge variables ("knowing how" to pit plant, column 6), but we no longer see significant differences from direct connections in adoption or awareness of pit planting. Consistent with the hypothesis that this loss in precision is due to diffusion in the network, we see that adoption increases among those at moderate distance to the seeds in year 3: column (9) shows that households within path length 2 are more likely (3.7 pp) to have adopted over those who are socially more distant.[33]

In summary, analysis using individual-level data demonstrates that individuals who are initially close to the trained seeds are more likely to adopt than individuals with no direct connections – as one would expect if the experiment is inducing social network-based diffusion. The data also suggest that having two direct connections – and not just one – is important for diffusion. This is suggestive evidence in favor of the complex contagion model: farmers may need to know multiple informed connections before becoming informed, and then subsequently adopting, themselves.

5.3 The advent of diffusion under simple and complex contagion

In this section, we report experimental results on village-level outcomes across the four treatment arms. We measure technology adoption in our surveys, because (1) this is ultimately of most interest to policy and (2) adoption can be observed and measured more precisely than being informed.

One key feature of the threshold model that helps distinguish complex from simple contagion is that for almost *any* choice of seed farmer, the diffusion process will start under simple contagion. However, if the diffusion process is complex, then many potential pairs of seeds would never generate any diffusion. This is because when two seeds are not proximate to each other in the network map and they don't share any common connection, then no other individual is connected to multiple informed seeds, and the technology never diffuses. This leads us to focus on the *advent* of diffusion in our sample villages as a key outcome. We define 'any adoption' as an indicator for villages which have at least one household (other than the seeds) that adopted pit planting. Our models actually simulate being "informed" and not adoption directly, but in order to be parsimonious and tractable we compare

[33] This is a lower power test of the model than the direct connections test as it is imperfectly correlated with the number of informed, indirect connections to seeds (which is unobserved). We do not see a significant effect of this variable on knowledge outcomes, though coefficients are positive.

the rates of being informed from the simulations to adoption rates in the data.[34] The focus on 'any adoption' yields a sharp prediction to distinguish complex contagion from the other treatments: if complex contagion is the correct description of the diffusion process, then the indicator 'any adoption' should be significantly higher under the complex treatment than all other treatments.

The left part of Figure 2 shows the *predicted* fraction of villages with 'any adoption' from simulating the model for all sample villages when $\lambda=1$ (Simple contagion) and $\lambda=2$ (Complex contagion).[35] Since the goal is to compare these simulations to the actual data, we design the simulations to reflect the fact that we only observe a random sample of households in these villages.[36] The right part of Figure 2 shows the empirical counterpart: 'any adoption' rates in the data in years 2 and 3.

When the threshold is set to $\lambda=1$, diffusion is predicted to be widespread. In year 2, 85% of villages where Geo and Benchmark partners were trained are predicted to have some sampled diffusion, and that rate goes up to 94% with Simple and Complex partners. The predicted rates of 'any diffusion' are even higher in year 3.

The risk of no diffusion increases if the diffusion process is characterized by complex contagion. In that case, the model predicts that more than half of the villages assigned Simple, Geo

34 Simulating adoption in the model would require a number of additional assumptions, including estimates of signal accuracy, the distribution of net benefits, and any heterogeneity in prior beliefs which may exist. Being informed is necessary but not sufficient to adopt.

35 These simulations exclude 12 villages where at least one of the extension worker chosen seeds (benchmark) was not observed in our social network census. This occurred because the spatial boundaries of villages are not always clearly delineated in Nkhotakota.

36 The simulations use the full social network to predict becoming informed, measured here through adoption. We then sample from the full network to better mimic our data. In the model, the rate of any adoption is identical in years 2 and years 3. If there was no adoption by year 2, there is no way there will be any additional adoption taking place in year 3. The sampling process, however, generates the increase over time observed in the figure. If the rate of adoption is low, as is empirically the case, then a random sample may miss all adopters. As the number of adopters increases over time, the random sample is more likely to pick up an adopter and hence the rate of any adoption increases over time in the figure.

or Benchmark partners will not see any sampled diffusion at all in year 2. In contrast, when complex seeds are trained, 70% of villages are predicted to experience some diffusion.

Comparing the theoretical simulations to the data on the right side of Figure 2 shows that the data are more consistent with the patterns generated by a complex (rather than simple) learning environment in three distinct ways. First, the simple contagion simulations suggest that we should observe a much higher fraction of villages with some adoption than is true in the data. Second, simple contagion predicts that the 'any adoption' outcome should not be very sensitive to the identity of the seed farmer who is initially trained. In contrast, the identity of the seed farmer dramatically alters this outcome in the data. Finally, the complex contagion simulations predict that the complex partners will maximize the fraction of villages with some adoption, which is exactly what we observe in the data.

The first two columns of Table 3 replicate the data panels on the right side of figure 2 in a regression framework. The propensity for 'any adoption' in season 2 in statistically significantly larger in villages assigned to the complex contagion treatment relative to Benchmark villages. The 25 percentage point gap is large relative to the 'any adoption' rate of 42% in our Benchmark villages. The 'any adoption' rate in complex villages is also 15 percentage points larger than in Geo villages (p-value = 0.10) and 10 percentage points larger compared to villages assigned to the simple contagion treatment (p-value = 0.30). In season 3, Simple, Complex and Geo villages all attain a statistically higher rate of 'any adoption' than Benchmark villages. 85% of Complex villages had at least one non-seed adopter, compared to 73% of Simple and Geo villages and 54% of Benchmark villages.

5.4 Adoption rates across treatment arms

We also look at the speed of diffusion, captured by the adoption rate. Columns (3) and (4) in Table 3 document treatment effects on the adoption rate, which is defined as the proportion of non-seed farmers who adopted pit planting in each agricultural season. Both simple and complex contagion villages have higher adoption rates relative to the benchmark in season 2. Compared to the benchmark rate of 3.8%, complex and simple villages both experience a 3.6 percentage point higher adoption rate. We cannot reject that the adoption rates are the same in Simple, Complex and Geo villages. The adoption rate increases across all four types of villages in season 3. The adoption rate increases in the benchmark villages, the reference category, from 3.8% to 7.5% from season 2 to 3. With the smaller sample size of 141 villages in season 3, we cannot reject that the adoption rate is the same across all treatment types, though the point estimate on Complex remains the largest, and is equal in magnitude to the effect size observed in season 2. The adoption rate in complex villages in year 3 is 11%.

Appendix Table A6 shows the results of analogous regressions on "data" generated from the theoretical simulations we conducted to create the left panels of Figure 2. Note that the simulated rate of becoming 'informed' about the technology is a worse proxy for the rate of technology adoption than what we used for 'any adoption'.[37] We therefore need to be cautious about comparing columns 3 and 4 across Table 3 (the data) and Appendix Table A6 (the simulations). The simulations in Appendix Table A6 predict that the complex treatment should perform best both in terms of 'any adoption' and the adoption rate if the learning environment in reality is complex. If the learning environment is instead simple, then we should expect to see few statistical differences in diffusion across targeting strategies by season 3, since the choice of seed partners is relatively unimportant if the technology diffuses easily.

In the data, we observe that the diffusion process is far too slow to be consistent with simple contagion. However, our parameterization of λ=2 does not provide a perfect fit for the data. For example, the simulations in Appendix Table A6, columns 3 and 4 suggest that the complex treatment

[37] The adoption rate is (# households informed/# households)* P(adopt|informed). The 'any adoption' rate is 1-(1-P(adopt|informed))^(# informed). As long as the number of informed households is sufficiently high, the latter will be informative of the true 'any adoption' rate. However, the adoption rate will always be scaled by a constant probability.

should produce a larger adoption rate than the simple treatment if the learning environment is complex. In Table 3, we cannot statistically distinguish between these two treatments. Overall, however, the empirical results in Tables 3 and in Figure 2 appear more consistent with a complex learning environment than with simple contagion.

5.5 Heterogeneity in the learning environment

Our theoretical micro-foundation suggests that the threshold model describes diffusion as a learning process where farmers need to aggregate signals and ultimately adopt if those signals are sufficiently positive. Thus, we anticipate that our treatments will be most effective in inspiring adoption for farmers who are likely to receive positive signals. We use two different approaches to identify subsets of sample farmers for whom the signal about the technology's profitability is more likely to have been positive, and we use such farmers to construct empirical tests. First, the Ministry of Agriculture recommends pit planting only for flat land, and labor costs of pit planting are lower on flat land.[38] Focus group discussions in our sample villages confirmed that villagers thought pit planting was more suitable for flat rather than sloped land. We therefore expect more positive treatment effects for farmers who possess land that is flat and not sloped.

Second, pit planting is in general a new technology in Malawi, but there is heterogeneity across villages in how novel it is. In the median village, 4.3% of farmers reported having ever tried pit planting at baseline while 0.2% were currently practicing pit planting across all villages. The information environment should be most affected by our treatments when the technology is truly novel, both because each piece of new information will have a larger effect on posterior beliefs, and because the differences between treatments may become less stark if some farmers in the network are

[38] Pit planting is possible on land with some slope, but in those cases, the pits need to be constructed differently, and our extension workers were not trained on that technique.

already informed about pit planting.[39] For these reasons, we anticipate larger treatment effects in villages where the technology is truly novel.

Table 4 explores the heterogeneity in treatment effects across these two dimensions, by interacting the randomized treatments with an indicator for "Farmer likely to receive a Good Signal". This "Good Signal" variable is first defined as the farmer having flat land in columns (1) and (2), and then re-defined as "Village with lower-than-median familiarity with the technology at baseline" in columns (3) and (4). "Bad signal" refers to the converse of these characteristics. The equation estimated:

$$y_{ivt} = \beta_0 + \beta_1 Simple_v * Bad\ Signal + \beta_2 Complex_v * Bad\ Signal + \beta_3 Geo_v * Bad\ Signal + \beta_4 Good\ Signal + \beta_5 Simple_v * Good\ Signal + \beta_6 Complex_v * Good\ Signal + \beta_7 Geo_v * Good\ Signal + \delta X_v + \epsilon_{ivt}$$

The reference group comprises of farmers who are likely to receive a bad signal in Benchmark villages. Our hypothesis is that among those who receive a positive signal, we will observe more diffusion in Complex villages if the true model is Complex.

Columns (1) and (2) show that adoption in year 2 is higher for farmers who have flat land in Simple, Complex and Geo villages compared to farmers with flat land in Benchmark villages. In year 3, we see that Complex villages continue to have a larger adoption rate than Benchmark villages for farmers with flat land.

Columns (3) and (4) show that the complex treatment performs best in villages where the technology was relatively novel. In this sub-sample, the adoption rate is statistically significantly higher in Complex Contagion treatment villages compared to both the Simple contagion and the benchmark treatments in year 3.

[39] Given that there is approximately 0% adoption at Baseline, it is additionally unlikely that previously informed farmers are disseminating a positive signal about this technology.

To summarize, these heterogeneity tests indicate that targeting based on complex contagion is most effective precisely in the types of villages and for the types of farmers where we theoretically expect it to perform well. Just as importantly, we do not observe that targeting simple and complex seeds affects adoption patterns for farmers who are not likely to be influenced by additional information in the network. We interpret these tests as strongly suggesting that the social learning environment about agriculture in rural Malawi is well characterized by complex contagion. The policy implications that stem from that observation are (a) the network position of who you initially target with the new technology matters, and (b) complex contagion-theory-based targeting can improve the speed and scope of technology diffusion relative to other forms of targeting.

6. Cost-effective, Policy-Relevant Alternatives to Data-Intensive Targeting Methods

While targeting based on the threshold model improves technology diffusion, eliciting the social network map to achieve these gains is expensive. Our geography-based treatment arm was an attempt to assess how much of the diffusion benefit derived from applying network theory could be achieved without having to resort to expensive data collection methods (since each household's physical location is much easier to observe than network relationships). This specific approach was not an unqualified success. Table A1 showed that geo seeds tended to have less land and were therefore poorer. Therefore, while the idea of using geography as a proxy for one's network may be intuitive, the implications of geographic centrality may be context-specific, and inappropriate as a network-based targeting proxy in some cases. Even though the two Geo seeds are often clustered together, in this setting the seeds are poorer, and have fewer connections to others in the network. This limits the pace of diffusion.

Combining our experimental results with research on other inexpensive procedures to identify the optimal seeds under complex contagion theory would make network-based targeting more policy relevant and scalable. In some contexts, relevant groups within the village may be well-known by policy-makers, and our result would suggest that the critical goal for policy is to saturate individual groups with a few trained seeds. For other contexts, we may need to infer more about the network, and a few recent papers have suggested promising, less expensive methods for inferring network characteristics. Banerjee et al. (2019b) suggests that despite the implicit challenges in learning about network structure, the simple question of "if we want to spread information about a new loan product to everyone in your village, to whom do you suggest we speak?" is successful in identifying individuals with high eigenvector centrality and diffusion centrality, who ultimately improve the diffusion process. Breza et al. (2019) suggest that aggregate relational data collected from a smaller sample combined with a census can yield accurate estimates of network characteristics.

While we cannot test the viability of either approach empirically, we can explore via simulations some alternate strategies that extension officers could use to identify useful partners. We make use of the fact that – unlike other network statistics – "degree" of a network node (i.e. simply the number of direct connections it has to other nodes) can be estimated from a single interview (Chandrasekhar and Lewis 2016). In this section, we simulate the effects of several potentially low-cost strategies in our data, assuming a complex contagion learning environment.

We suppose that an extension agent enters a village and randomly selects a small number of farmers to interview, and only asks one question from our social network census: "Do you discuss agriculture frequently with anyone in the village? What is the name of the person you speak with about agriculture frequently?" The response to this question generates a small list of names. The extension agent can then use the responses to the initial interviews to select any follow-up interviews.

We simulate six candidate targeting strategies, discussed in more detail in appendix A.3. While most do not perform competitively with the optimal complex contagion targeting, we find that strategies which leverage the highest degree respondent from the random sample can approach the performance of the optimal targeting. More specifically, if we train any two connections of the highest degree respondent, we achieve 73% of the optimal adoption rate with just 2 total interviews. If the extension agent then identifies her two highest degree friends (which requires an additional 5 or so interviews to determine which connections are the highest degree), and trains those two, we simulated that those trained seeds would achieve 84-90% of the adoption under optimal complex. The intuition is that in a complex learning environment, it is most useful to identify two seeds who have at least one connection in common, and who are fairly central in the network so that their connections have many connections. Training two high degree friends of the highest degree farmer guarantees at least one high degree person will become informed, and generates a high likelihood of creating other connections in common.

These simulations therefore suggest that it is possible for us to learn about the relevant pieces of network structure to enhance technology diffusion under a complex contagion learning environment at modest cost.

7. Concluding Remarks

This paper seeks to use a theory of social learning to increase diffusion of a new technology. We first develop a theory-driven methodology to select seed farmers who are predicted to maximize diffusion of information about a productive new agricultural technique in Malawi under different theoretical assumptions about the process of diffusion. We then implement those selections using a field experiment on agricultural extension conducted in partnership with the Malawi Ministry of Agriculture. This allows us to test whether (a) theory-driven targeting using detailed social network data can increase technology adoption relative to the status quo approach to agricultural extension services; (b) a less data-intensive approach can generate similar gains; and (c) whether the diffusion we measure follows a learning process similar to the one suggested by the threshold theory.

We have provided evidence that technology adoption is well-described by complex contagion theory using a *set of predictions* from the threshold model on the behavior of households in villages under all four treatment arms. We also demonstrate that farmers who are connected to two seed farmers are also most likely to adopt pit planting in year 2, consistent with the fact that under complex learning, multiple connections to seeds should be predictive of adoption.

Now the question may be: why does the data not look exactly like the prediction simulated under complex contagion? Our experimental design tests two possible scenarios: where either the vast majority of farmers had a threshold of one (simple) or where most had a threshold of two (complex). If in reality there is a mix of low and high threshold farmers, then the empirical adoption patterns would be in between our two simulations. Our heterogeneity analysis, which is motivated by our micro-foundation of the threshold model, is consistent with the idea that farmers within the same village may have different thresholds.

Our paper suggests two key directions for future research. First, we provide evidence that many farmers have a high threshold for adoption in a context of agricultural learning in Malawi and future research should explore which contexts generate these high thresholds. Our micro-founded diffusion model suggests a key dimension to consider when assessing if contagion is likely to be simple or complex: the noise of the signal. Rosenzweig and Udry (2020) highlight the importance of aggregate stochastic shocks in distinguishing the returns to agricultural investment, microenterprise investment, and human capital from large-scale survey data. Farmers, entrepreneurs, and parents likely have access to far fewer data points then these large-scale surveys when they attempt to infer the returns to investments and schooling, which – together with our model – may suggest that high thresholds bind for a number of problems of interest to economists. However, in contexts in which agents are learning about concepts that are less noisy than returns – say the availability of microfinance, how to enroll in welfare, or whether a firm is hiring – simple contagion may be the right model. Characterizing which productive investments should diffuse easily through social networks - and which need extensive and targeted diffusion - is crucial but beyond the scope of this paper.

Second, while the methodological approach in this paper is not directly scalable for policy, given the high costs of collecting network data, our simulations suggest that with only about 10 interviews per village, it may be possible to identify individuals who can trigger the diffusion process. Further research is needed on best practices for engineering diffusion in the context of thresholds: when is systematic targeting the best approach, compared to attempting to manipulate farmers' thresholds or engineer new connections to overcome high learning thresholds?

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Appendix

A.1. Effect of Technology Adoption on Crop yields

In order to estimate the returns of adopting the new technologies on yields, we compare seed farmers to shadow farmers. Appendix table A4 demonstrates that there were large differences in adoption rates between seeds and shadow farmers. To estimate the impact of adoption on yields, we estimate an ITT specification exploiting that random difference in take-up:

$$y_{ivt} = \beta Seed_{ivt} + \gamma X_v + \delta_t + \epsilon_{ivt} \qquad (1)$$

where y_{ivt} is log maize yields for farmer *i* in village *v* at time *t*, $Seed_{ivt}$ is an indicator for being the selected seed farmer, X_v are control variables used during the re-randomization routine (see notes in table 3), village size, village size squared, district fixed effects plus baseline land size. δ_t are year dummies. We use data from seasons 2 and 3. In the intent-to-treat specification in column (1), maize yields among seed farmers are 13% greater than the yields experienced by the shadow seeds. The fact that the technologies we promoted led to an increase in output strongly suggests that the information about pit planting that diffused through the networks was likely positive on average.

Since only about 30% of seeds adopted pit planting, we also report the local average treatment effect using an IV regression in column (2) in which we instrument pit planting adoption with an indicator for being randomly assigned as the seed (rather than a shadow). In this specification, pit planting adoption is associated with a 44% increase in maize yield. However, we cannot rule out that CRM adoption also increased yields, potentially violating the exclusion restriction in the IV estimation.[40]

A.2. Adoption rates among seeds (compared to shadow farmers)

[40] We also cannot rule out any labor or other input use response to training which may have positively contributed to yields. Changes in other inputs makes it impossible for us to say that the yields increases map directly into increases in profits.

Appendix table A4 compares the technology adoption behavior of seed farmers to shadow farmers. We focus on this sub-sample because shadow farmers act as the correct experimental counter-factual for the seed farmers to capture the causal effect of the intervention, removing any bias due to the seeds' position within their networks. We estimate the following equation, and Panel A displays the results:

$$y_{ivt} = \beta Seed_{ivt} + \delta_v + \epsilon_{ivt} \qquad (1)$$

where the dependent variable is an indicator for adoption, and δ_v are village fixed effects. Column (1) shows that trained seeds are 52% more likely in year 1 to know how to pit plant than shadow farmers. Columns (4)-(6) show that seed farmers who are trained on pit planting adopt at a rate of 31-32% in all three years, compared to the low 5% adoption rate of shadow farmers in year 1.

Panel B of Table A3 restricts the sample to only seed farmers (and drops all shadow farmers) and compares knowledge and adoption among seeds across the four experimental arms as follows:

$$y_{ivt} = \beta_0 + \beta_1 Simple_v + \beta_2 Complex_v + \beta_3 Geo_v + \delta X_v + \epsilon_{ivt} \qquad (2)$$

where $\mathrm{X_v}$ include the re-randomization controls (listed in table notes), village size, the square of village size, and district fixed effects. Standard errors are clustered at the village level. Column (1) shows that in the first year, Benchmark seeds are most likely to say they know how to pit plant, while all other seeds are similar. The extension agents evidently chose seed farmers carefully to ensure that their chosen extension partners receive the initial training from them. However, in years 2 and 3, Simple and Complex seeds catch up and have similar levels of familiarity with pit planting as Benchmark seeds. Geo seeds continue to display lower familiarity in subsequent years.

Column (4) shows that there are no differences in adoption propensities across the four types of seeds in the first year. This implies that it is unlikely that any observed differences in village-wide adoption patterns across the four treatment arms that we will examine later, are driven by initial adoption differences inside the sub-sample of seed farmers. Columns (5) and (6) show that seed farmers in simple contagion villages become relatively more likely over time to adopt the technology. This could be due to the technology diffusion process, or in other words, a consequence of the experiment. Columns (7)-(8) show that there are no significant differences in adoption in seasons 1 or 2 for crop residue management.

A.3. Simulation of cost-effective targeting strategies

For our simulations, we suppose that our extension agent starts with a random sample of candidate respondents, and is able to screen out individuals with less than 2 connections. We suppose the extension agent starts with a list of 2-10 randomly selected farmers.

Starting from that random sample of farmers, we solicit each farmer's connections and calculate each random farmer's degree. We then focus on 6 candidate targeting strategies:

A. Trains two randomly selected people from that list (used as a benchmark)
B. Trains the two highest degree people from that list
C. Select two random friends of the highest degree person from that list
D. Trains the two highest degree connections of the highest degree farmer from the random sample (requires interviewing all connections of the highest degree respondent to determine their degree)
E. Selects two farmers from that list at random; interviews two of their connections (selected at random) and trains two of connections' connections[41]
F. Trains the highest degree respondent and one of his connections (at random).

For each of these five candidate strategies, we simulate adoption rates after 4 rounds of simulations against the seeds chosen by our complex contagion treatment. We find that Strategy A,

[41] This "friends of friends" approach to identifying central people was inspired by Feld (1991), Christakis and Fowler (2010), and Kim et al (2015), who note that randomly selected connections tend to be more central than randomly selected nodes in a network. We again assume that the extension agent is able to screen out potential trainees with less than two total connections.

selecting two farmers at random, achieves 57% of the adoption produced by the complex contagion treatment. We can then view the other targeting strategies in terms of their performance above the random benchmark. Strategy B is identical to random selection with only 2 initial interviews, and so similarly generates 57% adoption; however, as the extension agent interviews more people to identify these high degree individuals it performs somewhat better, achieving 70% of the complex contagion adoption with 10 total interviews. Strategies C and D both leverage the highest degree respondent from the initial random sample. These perform the best out of the strategies we consider. Strategy C achieves 73% of the optimized adoption with just two total interviews, which increases modestly to 76% of the optimized adoption as the number of interviews grows to 10 to better identify a high degree individual. Strategy D, our best performing strategy, achieves 84% of the optimized adoption with 2 initial interviews (necessitating 8 total interviews as the connections are interviewed), and up to 90% of the optimized adoption with 8 initial interviews (and 13 total interviews). Strategy E requires a total of 4 interviews, and achieves 69% of the optimized adoption. Strategy F achieves 60% of the optimized adoption with 2 interviews, and up to 67% of optimized adoption with up to 10 interviews.

Clearly the most effective strategies are those that identify a high degree farmer and train her connections. Given the nature of the complex contagion learning process, the intuition is clear: training two high degree friends of someone who is high degree means that three people with many connections in the same part of the network will become informed. With clustered networks, it is likely that others will as well.

Figure 2: % of villages where at least some non-seeds adopted in data and simulations

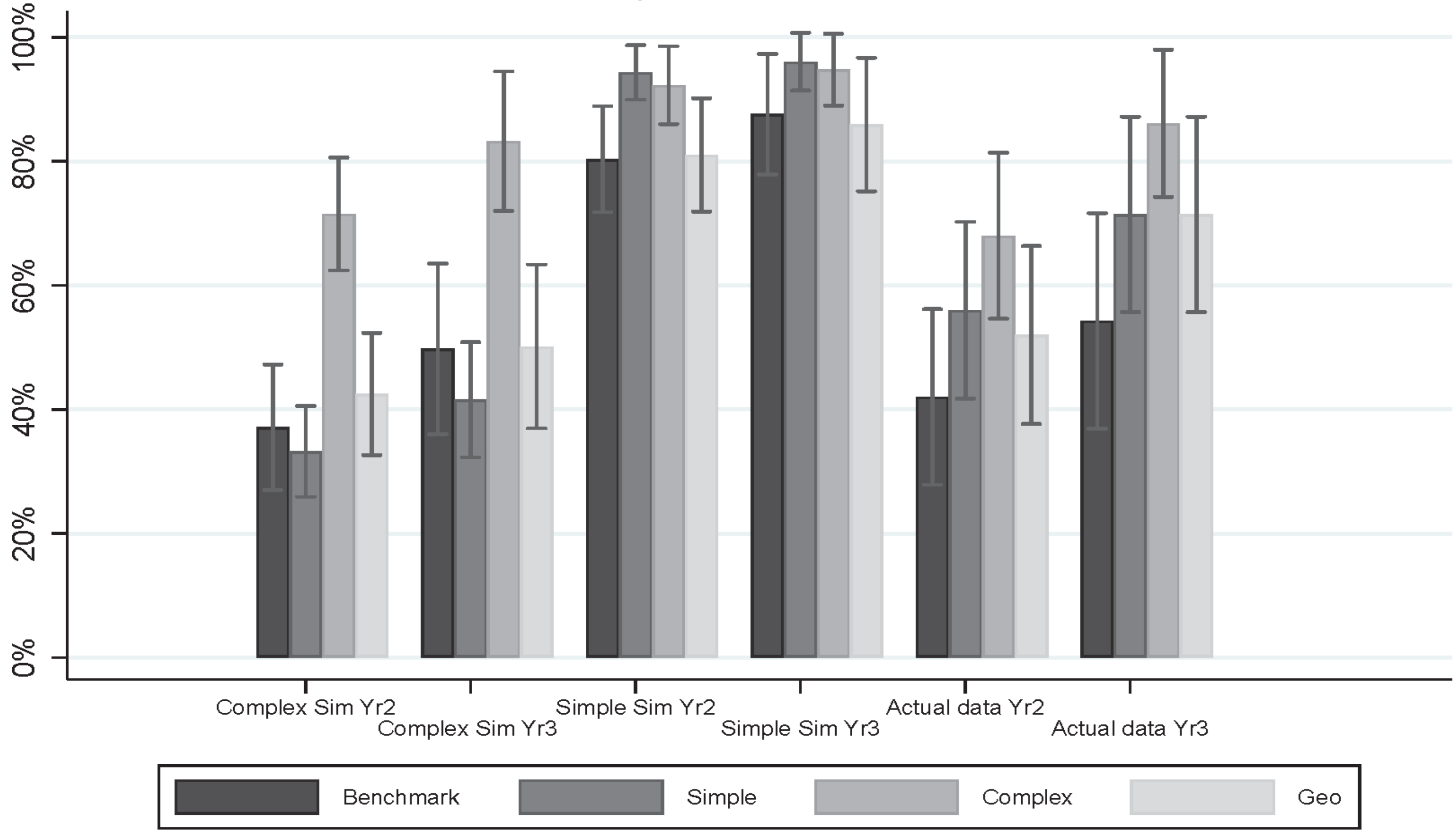

Table 1: Conversations other farmers report having about Pit Planting with Seed and Shadow Partners

Conversation with:	Simple Partner	Complex Partner	Geo Partner	At least 1 conversation with seeds or randomly chosen villagers
	(1)	(2)	(3)	(4)
Simple Contagion Treatment	0.085	0.043	0.009	0.034
	(0.026)	(0.019)	(0.016)	(0.026)
Complex Contagion Treatment	0.055	0.097	0.011	0.052
	(0.020)	(0.024)	(0.016)	(0.026)
Geographic treatment	-0.003	0.008	0.050	-0.021
	(0.021)	(0.020)	(0.020)	(0.026)
N	10354	10712	10585	11606
Mean of Benchmark	0.176	0.189	0.152	0.370
SD of Benchmark	0.381	0.391	0.359	0.483
p-values for equality in coefficients:				
Simple = Complex	0.209	0.023	0.9	0.414
Complex = Geo	0.003	0	0.04	0.001
Simple = Geo	0	0.083	0.044	0.016
Season	All	All	All	All

Notes

1 Sample excludes seed and shadow farmers.

2 In columns (1)-(3), we also include controls for the number of partner farmers (of the type asked about in the respective column) we asked about in the questionnaire by including a dummy variable for each number of partner farmers from 0 to 4. In column (4) we also include controls for the number of seeds we asked respondents about and the number of randomly selected villagers. This varies by village treatment type, since we do not know observe shadow benchmark villages in non-Benchmark villages, and in those villages were asked about more randomly chosen villagers.

3 All columns include controls used in the re-randomization routine (percent of village using compost at baseline; percent village using fertilizer at baseline; percent of village using pit planting at baseline); village size and its square; and district fixed effects. Standard errors are clustered at the village level.

Table 2: Diffusion within the village

	Heard of PP			Knows how to PP			Adopts PP		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Connected to 1 seed	0.002	0.030	0.016	0.017	0.021	-0.031	0.008	0.012	0.004
	(0.024)	(0.022)	(0.029)	(0.016)	(0.017)	(0.023)	(0.011)	(0.015)	(0.017)
Connected to 2 seeds	0.084	0.124	0.064	0.062	0.068	0.110	0.016	0.039	0.014
	(0.038)	(0.040)	(0.064)	(0.028)	(0.029)	(0.051)	(0.014)	(0.019)	(0.035)
Within path length 2 of at least one seed	-0.018	0.016	0.067	0.005	0.022	0.028	0.013	0.022	0.037
	(0.028)	(0.027)	(0.042)	(0.018)	(0.021)	(0.028)	(0.008)	(0.013)	(0.021)
Season	1	2	3	1	2	3	1	2	3
N	4155	4532	3103	4155	4532	3103	4203	3931	2998
Mean of Reference Group (No connection to any seed)	0.223	0.286	0.391	0.057	0.095	0.147	0.013	0.044	0.043
SD of Reference Group	0.416	0.452	0.488	0.232	0.293	0.355	0.113	0.206	0.203
p-value for 2 connections = 1 connection	0.018	0.013	0.442	0.072	0.091	0.004	0.522	0.164	0.760

Notes

1 Sample excludes seed and shadow farmers. Only connections to simple, complex and geo seed farmers are considered (no connections to benchmark farmers included).

2 The dependent variable in columns (1)-(3) is an indicator for whether the respondent reported being aware of a plot preparation method other than ridging and then subsequently indicated awareness of pit planting in particular. In columns (4)-(6), the dependent variable is an indicator for whether the farmer reported knowing how to implement pit planting. The dependent variable in (7)-(9) is an indicator for the household having adopted pit planting in that season.

3 In all columns, additional controls include indicators for the respondent being connected to: one Simple partner, two Simple partners, one Complex partner, two Complex partners, one Geo partner, two Geo partners, within 2 path length of a Simple partner, within 2 path length of a Complex Partner, and within 2 path length of the geo partner. Also included are village fixed effects. Standard errors are clustered at the village level.

4 The reference group is comprised of individuals with no direct or 2-path-length connections to a seed farmer.

Table 3: Village-Level Regressions of Adoption Outcomes Across Treatment Arms

	Any Non-Seed Adopters		Adoption Rate	
	(1)	(2)	(3)	(4)
Simple Contagion Treatment	0.155	0.189	0.036	0.006
	(0.100)	(0.111)	(0.017)	(0.022)
Complex Contagion Treatment	0.252	0.304	0.036	0.036
	(0.093)	(0.101)	(0.016)	(0.026)
Geographic treatment	0.107	0.188	0.038	0.013
	(0.096)	(0.110)	(0.027)	(0.034)
Year	2	3	2	3
N	200	141	200	141
Mean of Benchmark Treatment (omitted category)	0.420	0.543	0.038	0.075
SD of Benchmark	0.499	0.505	0.073	0.109
p-values for equality in coefficients:				
Simple = Complex	0.300	0.240	0.981	0.173
Complex = Geo	0.102	0.220	0.937	0.491
Simple = Geo	0.623	0.990	0.950	0.783

Notes

1 The reference group is the Benchmark treatment.

2 The "Any non-seed adopters" indicator in columns (1)-(2) excludes seed farmers. The adoption rate in columns (3)-(4) include all randomly sampled farmers, excluding seed and shadow farmers.

3 Sample for year 3 (columns 2 and 4) excludes Nkhotakota district.

4 All columns include controls used in the re-randomization routine (percent of village using compost at baseline; percent village using fertilizer at baseline; percent of village using pit planting at baseline); village size and its square; and district fixed effects. Standard errors are clustered at the village level.

Table 4: Heterogeneity in Farmer-Level Adoption Decisions Across Treatment Arms

	(1)	(2)	(3)	(4)
Bad Signal*simple	-0.008	-0.036	0.019	-0.008
	(0.024)	(0.037)	(0.017)	(0.034)
Bad Signal* complex	0.006	-0.027	0.013	-0.045
	(0.024)	(0.036)	(0.015)	(0.033)
Bad Signal * geo	0.002	-0.068	0.031	-0.054
	(0.031)	(0.031)	(0.035)	(0.032)
Good Signal	-0.037	-0.062	-0.007	-0.064
	(0.017)	(0.024)	(0.022)	(0.038)
Good Signal * Simple	0.064	0.029	0.054	0.021
	(0.021)	(0.020)	(0.029)	(0.020)
Good Signal * Complex	0.059	0.067	0.054	0.083
	(0.018)	(0.025)	(0.024)	(0.030)
Good Signal * Geo	0.042	0.022	0.026	0.031
	(0.020)	(0.023)	(0.022)	(0.029)
Good Signal Type	Flat Land	Flat Land	Unfamilliar Tech	Unfamilliar Tech
Year	2	3	2	3
N	3546	2645	3954	3023
Mean of Bad Signal in Benchmark Treatment	0.066	0.123	0.046	0.104
SD	0.248	0.33	0.21	0.305
p-values for equality in coefficients:				
Simple, Good = Complex, Good	0.828	0.113	0.986	0.032
Complex, Good = Geo, Good	0.482	0.103	0.297	0.138
Simple, Good = Geo, Good	0.364	0.755	0.351	0.680

Notes

1 The reference group is Bad signal recipients in the Benchmark treatment.

2 In columns (1)-(2), households with any flat land are those who have Good Signal=1 and those with all sloped land have Good Signal=0. In columns (3)-(4), households in villages where less than 4.32% (the median) of households ever tried pit planting at baseline are those who have Good Signal=1.

3 Sample for year 3 (columns 2 and 4) excludes Nkhotakota district.

4 All columns include controls used in the re-randomization routine (percent of village using compost at baseline; percent village using fertilizer at baseline; percent of village using pit planting at baseline); village size and its square; and district fixed effects. Standard errors are clustered at the village level.

Figure A1: Project Timeline

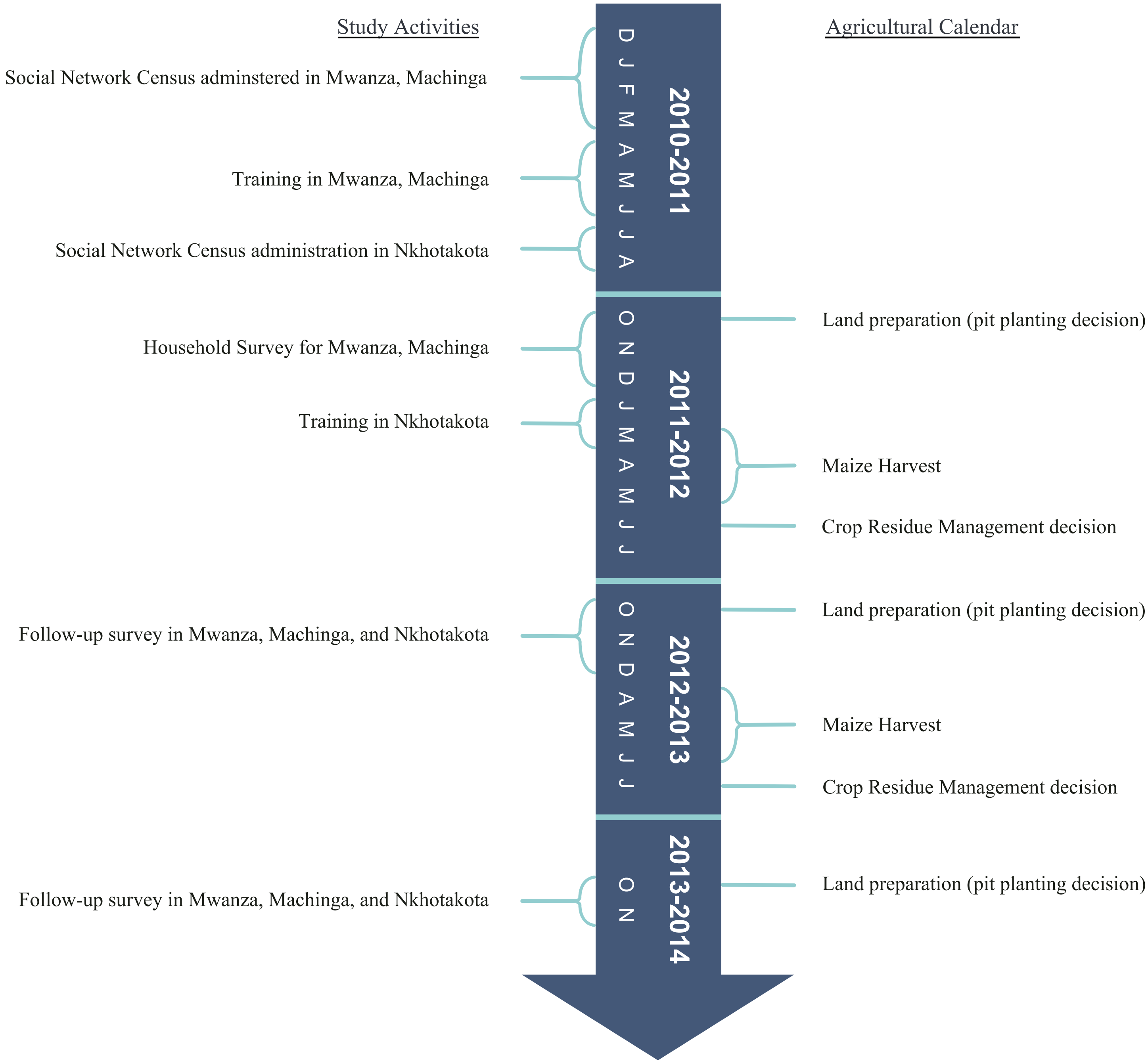

Table A1: Agricultural yields of Seeds Relative to Shadow (Counterfactual) Farmers

	Log of Agricultural Yields	
	(1)	(2)
Seed	0.126	
	(0.061)	
Adopted PP		0.443
		(0.210)
N	959	959
Mean of Shadows		
Season	2,3	2,3

Notes

1 Sample includes only seed and shadow farmers. Benchmark villages are excluded.

2 Agricultural yields were winsorized. The specification also controls for total farm size; controls used in the re-randomization routine (percent of village using compost at baseline; percent village using fertilizer at baseline; percent of village using pit planting at baseline); village size and its square; and district and season fixed effects. Standard errors are clustered at the village level.

Table A2: Characteristics of the Seeds Chosen by Each Treatment Arm

	Wealth Measures		Social Network Measures		
	Farm Size	Total Index (PCA)	Degree	Betweenness Centrality	Eigenvector Centrality
	(1)	(2)	(3)	(4)	(5)
Treatment arm:					
Simple Contagion	-0.152	0.113	0.455	156.009	0.009
	(0.19)	(0.23)	(1.03)	(67.93)	(0.01)
Complex Contagion	-0.037	0.380	3.725	146.733	0.064
	(0.19)	(0.23)	(1.02)	(67.74)	(0.01)
Geographic	-0.614	-0.740	-3.616	-90.204	-0.046
	(0.19)	(0.23)	(1.03)	(68.04)	(0.01)
p-values for tests of equality in seed characteristics					
Simple = Complex	0.335	0.067	0.000	0.815	0.000
Complex = Geographic	0.000	0.000	0.000	0.000	0.000
Simple = Complex = Geographic	0.000	0.000	0.000	0.000	0.000
N	1248	1248	1232	1232	1232
Mean Value for Seeds in Benchmark Treatment (omitted category)	2.06	0.626	11.9	169	0.173
SD for Seeds in Benchmark Treatment	2.97	1.7	6.77	343	0.0961

Notes

1. The sample includes all seeds and shadows. The sample frame includes 100 Benchmark farmers (2 partners in 50 villages), as we only observe Benchmark farmers in Benchmark treatment villages, and up to 6 additional partner farmers (2 Simple partners, 2 Complex partners, and 2 Geo partners) in all 200 villages.
2. Benchmark treatment seeds are the reference category.

Appendix table Table A3: Distribution of distance to Seeds

	(1)	(2)	(3)	(4)
Path Distance to Closest Seed	Simple Seed	Complex Seed	Geo Seed	Benchmark Seed
1	38%	42%	24%	33%
2	50%	41%	46%	44%
3	9%	10%	20%	14%
4 +	4%	6%	10%	9%
N	4856	4856	4856	922

Notes: Includes only randomly selected (non-seed and non-shadow) respondents as well as the 6.5% of households in our sample (419) with zero measured connections

Table A4: Seed knowledge and adoption

	Knows How to Pit Plant			Adopts Pit Planting			Adopts CRM	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
Seeds	0.518	0.367	0.245	0.258	0.230	0.182	0.137	0.047
	(0.04)	(0.04)	(0.05)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Seasons	1	2	3	1	2	3	1	2
N	659	735	503	686	672	489	686	467
Mean of Shadows	0.165	0.187	0.291	0.0541	0.0929	0.139	0.32	0.207
SD of Shadows	0.371	0.39	0.455	0.227	0.291	0.347	0.467	0.406
Panel B								
Simple Contagion	-0.133	-0.067	0.108	-0.006	0.129	0.176	0.078	-0.097
	(0.07)	(0.07)	(0.08)	(0.07)	(0.07)	(0.09)	(0.08)	(0.09)
Complex Contagion	-0.120	-0.058	0.007	-0.020	0.002	0.037	-0.001	-0.077
	(0.07)	(0.07)	(0.08)	(0.08)	(0.07)	(0.08)	(0.08)	(0.09)
Geographic	-0.193	-0.255	-0.150	-0.095	-0.064	-0.003	-0.011	-0.075
	(0.07)	(0.07)	(0.09)	(0.08)	(0.07)	(0.08)	(0.08)	(0.10)
Seasons	1	2	3	1	2	3	1	2
N	343	383	264	353	352	259	353	243
Mean of Benchmark	0.824	0.653	0.547	0.337	0.276	0.238	0.442	0.339
SD of Benchmark	0.383	0.479	0.502	0.476	0.45	0.429	0.5	0.478
p-value for tests of equality in adoption rates across treatment cells:								
Simple = Complex	0.872	0.904	0.242	0.862	0.0766	0.108	0.311	0.808
Complex = Geographic	0.377	0.0155	0.111	0.36	0.358	0.625	0.886	0.977
Joint test of 3 treatments	0.472	0.0206	0.0109	0.252	0.00846	0.049	0.235	0.795

Notes

1 In Panel A, all columns compare seed farmers to shadow farmers. Village fixed effects are included, and standard errors are clustered at the village level.

2 In Panel B, the sample includes only seed farmers, and the reference group is Benchmark seed farmers. The specification also includes controls which were used in the re-randomization routine (percent of village using compost at baseline; percent village using fertilizer at baseline; percent of village using pit planting at baseline); village size and its square; and district fixed effects. Standard errors are clustered at the village level.

Table A5: Test of Balance across Randomized Treatment Arms

	Simple	Complex	Geo	Benchmark	N	p value of joint test
	(1)	(2)	(3)	(4)	(5)	(6)
Housing (pca)	-0.159	-0.036	0.023	0.106	14089	0.052
	(0.05)	(0.09)	(0.21)	(0.08)		
Assets (pca)	-0.059	-0.034	-0.040	0.005	14346	0.855
	(0.07)	(0.05)	(0.06)	(0.08)		
Livestock (pca)	0.012	0.025	-0.087	0.014	14346	0.210
	(0.06)	(0.06)	(0.04)	(0.06)		
Basal fertiliser (kg)	51.98	53.11	50.92	50.94	10427	0.970
	(4.78)	(3.14)	(3.17)	(2.23)		
Top dressing fertiliser (kg)	49.82	49.49	50.28	52.11	10526	0.787
	(3.33)	(2.05)	(2.53)	(1.99)		
# of Adults	2.305	2.316	2.299	2.306	14103	0.987
	(0.02)	(0.02)	(0.03)	(0.02)		
# of Children	2.617	2.650	2.619	2.599	14346	0.847
	(0.04)	(0.05)	(0.05)	(0.04)		
Farm size (acres)	1.624	1.676	1.764	1.808	14083	0.064
	(0.08)	(0.06)	(0.09)	(0.08)		
Own land	0.904	0.907	0.903	0.913	14346	0.922
	(0.01)	(0.01)	(0.02)	(0.01)		
Yields	304.20	290.46	303.54	300.77	13500	0.842
	(18.63)	(21.65)	(20.71)	(25.43)		
Provided Ganyu	0.254	0.250	0.242	0.233	14078	0.599
	(0.01)	(0.02)	(0.02)	(0.02)		
Used Ganyu	0.123	0.134	0.150	0.142	14078	0.115
	(0.01)	(0.01)	(0.01)	(0.01)		

Notes

1 Housing, assets and livestock in the first three set of rows are pca scores. Housing includes information on: materials walls are made of, roof materials, floor materials and whether the household has a toilet. Assets includes the number of bycicles, radios and cell phones the household owns. Livestock is an index including the number of sheep, goats, chickens, cows, pigs guinea fowl, and doves.

2 Columns (1)-(4) give the means and standard errors of the variable listed in the title column in each of the treatment arms. The seeds and the shadow seeds are excluded from the sample. The data is from the social network census.

3 Column (6) shows the p value of a joint test of significance of all treatment arms. Also included in the specification used for the test are controls used in the re-randomization routine (percent of village using compost at baseline; percent village using fertilizer at baseline; percent of village using pit planting at baseline) and district fixed effects. Standard errors are clustered at the village level.

4 Ganyu is the term used in Malawi for hired wage labor on the farm.

Table A6: Simulation of Village Level Adoption Outcomes across all treatment cells, assuming Diffusion follows either Complex or Simple Contagion Pattern

	Simulated Any Adopters		Simulated Adoption Rate	
	(1)	(2)	(3)	(4)
Panel A: Simulations Assuming Farmers learn by Simple Contagion				
Simple Treatment	0.095	0.036	0.026	0.090
	(0.043)	(0.037)	(0.024)	(0.052)
Complex Treatment	0.060	0.013	0.087	0.072
	(0.048)	(0.045)	(0.029)	(0.063)
Geo treatment	-0.050	-0.070	-0.022	-0.113
	(0.053)	(0.054)	(0.027)	(0.057)
Year	2	3	2	3
N	187	138	187	138
Mean Benchmark Partners	0.845	0.927	0.182	0.504
SD Benchmark Partners	0.258	0.186	0.149	0.306
Test: Simple = Complex	0.384	0.559	0.013	0.733
Test: Complex = Geo	0.026	0.129	0.000	0.001
Test: Simple = Geo	0.001	0.030	0.035	0.000
Panel B: Simulations Assuming Farmers Learn by Complex Contagion				
Simple Treatment	-0.092	-0.109	0.001	-0.022
	(0.056)	(0.077)	(0.012)	(0.040)
Complex Treatment	0.257	0.275	0.047	0.162
	(0.061)	(0.081)	(0.012)	(0.046)
Geo treatment	-0.028	-0.048	0.008	-0.032
	(0.060)	(0.083)	(0.011)	(0.038)
Season	2	3	2	3
N	187	138	187	138
Mean Benchmark Partners	0.436	0.541	0.038	0.138
SD Benchmark Partners	0.341	0.39	0.0479	0.194
Test: Simple = Complex	0.000	0.000	0.000	0.000
Test: Complex = Geo	0.000	0.000	0.001	0.000
Test: Simple = Geo	0.192	0.370	0.533	0.777

Notes

1 Simulations only include control villages where we had both seeds in social network census.

Table A7: Village Level Adoption Outcomes for Crop Residue Management (CRM)

	Any Non-Seed Adopters	Adoption Rate
	(1)	(2)
Simple Contagion Treatment	-0.083	-0.037
	(0.062)	(0.027)
Complex Contagion Treatment	-0.064	-0.026
	(0.060)	(0.027)
Geographic treatment	-0.152	-0.054
	(0.070)	(0.029)
Year	2	2
N	141	141
Mean of Benchmark Treatment (omitted category)	0.971	0.204
SD of Benchmark	0.169	0.109
p-values for tests of equality of coefficients…		
Test: Simple = Complex	0.794	0.680
Test: Complex = Geo	0.258	0.366
Test: Simple = Geo	0.336	0.583

Notes

1 The "Any non-seed adopters" indicator in columns (1) excludes seed farmers. The adoption rate in column (2) include all randomly sampled farmers, excluding seed and shadow farmers.

2 Analysis restricted to data from Mwanza and Machinga.

3 All columns include controls used in the re-randomization routine (percent of village using compost at baseline; percent village using fertilizer at baseline; percent of village using pit planting at baseline); village size and its square; and district fixed effects. Standard errors are clustered at the village level.