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ABSTRACT

Sooner Rather Than Later: Social Networks and Technology Adoption*

Using data from a randomised experiment in Kenya, we estimate the causal effect of social networks on technology adoption. In this experiment, farmers were invited to information sessions about the use of Tissue Culture Banana (TCB), an in vitro banana cultivation technology. We find that an additional social connection with a treated farmer causes an untreated farmer to be 2.25 pp more likely to adopt TCB 6-18 months post-intervention, but not in the longer term. We provide evidence that the adoption of TCB by those social connections is the mechanism driving the effect; therefore, treated connections are significant because treated farmers are more likely to adopt. We also find that indirect social network effects, proxied for by eigenvector centrality, influence adoption at both the village level and the farmer level.

JEL Classification: O12, P36, Z13

Keywords: networks, social connections, agricultural technology adoption, Kenya

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1 Introduction

The strength of agricultural production is a crucial factor in increasing economic opportunities for people living in developing countries. Agriculture creates employment, increases food security, and disproportionately benefits those belonging to the lower end of the income distribution (Ligon and Sadoulet, 2018). However, agricultural productivity in countries such as Kenya has been persistently sluggish. One report has estimated that production would need to increase by 75%, relative to 2015 levels, in order to meet consumption needs in 2030 (Cilliers et al., 2018).

One of the key barriers to increased production is the low adoption rate of more productive agricultural technologies (Suri, 2011; World Bank, 2017). Economists have proposed a number of explanations accounting for this phenomenon. These include informational barriers (Bloom et al., 2013; Magruder, 2018); the fact that the benefits of adoption are not equally distributed, being greater for some and lower for others (Munshi, 2004; Suri, 2011), high costs (Omotilewa et al., 2019; Michler et al., 2019); and risk preferences (Liu, 2013; Mao et al., 2019).[1]

One important consideration is that humans are social creatures. Our decisions are rarely made in a vacuum; we decide on which products to buy, which businesses to support and which technologies we adopt based on the decisions of the people around us. The importance of social connections— our social networks—is the focus of a burgeoning literature which has found that they influence a wide variety of economic decisions such as whether to utilize financial services (Banerjee et al., 2013; Cai et al., 2015); the sustained use of health products (Oster and Thornton, 2012), and the spread of information about government policies (Banerjee et al., 2018; Alatas et al., 2019).[2]

The question we pose is whether social networks also affect decisions to adopt agricultural technology. Answering this question using the data that is typically available to economists is not easy. Moreover, even if one has access to detailed social network data, the identification of social network effects is made difficult because of unobservables that are correlated with both the choice of who an individual is connected with, as well as their adoption decisions (Manski, 1993; Maertens and Barrett, 2012). Indeed, whilst previous studies have found relationships between social networks and technology adoption, these have not produced causal estimates. Rather, these papers have focussed on different issues, such as on how networks can be used to optimally identify which individuals should receive treatment (Foster and Rosenzweig, 1995;

[1] Foster and Rosenzweig (2010) provide a review of barriers to technology adoption in developing countries.
[2] For a detailed review see Breza et al. (2019).

Bandiera and Rasul, 2006; Beaman et al., 2018; BenYishay and Mobarak, 2018a; Bandiera et al., 2020).[3]

Our paper fits into a growing literature that has sought to use randomised experiments to pin down the causal effect of social networks (Oster and Thornton, 2012; Miller and Mobarak, 2014; Cai et al., 2015). An additional contribution we make is to provide evidence on whether these effects are sustained in the longer term (up to 32 months post-intervention). To provide an example, the paper most closely related to ours, Cai et al. (2015), utilizes a randomised experiment to investigate the effect of an additional treated social connection on the take-up of agricultural insurance in China. The authors find that treated social connections increase the likelihood that an untreated farmer adopts an insurance product 3 days post-intervention. Whilst our approach to identification is very similar, our study considers whether adoption is affected for a much longer period of time (up to 32 months after the intervention).[4] Another contribution we make is to investigate whether there is any evidence of a causal relationship between social networks and agricultural technology adoption. Given the attention paid to how social network data might be used to more effectively allocate treatment (Banerjee et al., 2013, 2018; Beaman et al., 2018; Beaman and Dillon, 2018), it is pertinent to establish whether or not focusing attention on social networks might be a tenable way to improve adoption outcomes.

We utilise a randomised experiment centred around *Tissue Culture Banana* (TCB), an in vitro banana cultivation technology. Banana is the second most consumed fruit in the world, primarily due to their nutritional value and relative ease of cultivation (Singh et al., 2016). They provide over 25% of the daily carbohydrate requirement for at least 70 million people worldwide, and are a major source of income and employment in places such as Kenya (Muthee et al., 2019). Technologies such as TCB increase crop yields by mitigating the risk of pests and disease. Since as early as 1996, Kenyan authorities (e.g., the Kenyan Agricultural Research Institute (KARI)) have attempted to disseminate TCB amongst farming communities, but have had little success (Kabunga et al., 2012). The intervention that we evaluated was designed to provide farmers with knowledge about TCB in the hopes that it would be more widely adopted among farmers.

We found that an untreated farmer with an additional treated social connection (treated link) is 2.25 pp more likely to adopt TCB 6-18 months post-intervention. The size of the

[3]Noteably, Islam et al. (2018) utilizes an experimental design very similar to ours, however whilst providing some evidence of peer effects, does not consider how these are related to the social connections between individuals.

[4]Noteably, Oster and Thornton (2012) have a study timeline spanning 18 months. However, this is still only half of the time we consider in our study.

effect is large in relative terms: the effect of having an additional treated link is about 32% as large as the direct effect of treatment. However, treated links do not significantly increase the likelihood that an untreated farmer adopts TCB over the entire study period (0-32 months post intervention), nor are they related to new adoption in the immediate short term (0-6 months post-intervention) or in the longer term (18-32 months post-intervention). Hence, we conclude that while treated links do not seem to make farmers more likely to adopt overall, they do influence the farmers to adopt 6-18 months post-intervention, implying that the effect of social networks in this setting is to induce farmers to adopt 'sooner rather than later'.

Turning then to the potential mechanisms, we find evidence that the number of social connections who adopt TCB (adopter links) drives the relationship between treated links and a farmer's own adoption. Since the number of adopter links is potentially endogenous, we implement a two-stage least squares estimation using the treated links as an instrument, and find evidence that it can explain our main causal effect. This suggests that treated links are important for adoption decisions because those who are treated are themselves more likely to adopt.

We also consider the role of eigenvector centrality which is a network measure that is a weighted sum of direct and indirect social connections (Bonacich, 2007). The insight that motivates this analysis is that a farmer might be influenced by "friends of friends", (i.e. their indirect social connections). Thus, both the nature of connections (*whom* one connects with) and the number of connections matter. For example, suppose a farmer has two social connections: one with an individual with many social connections and the other with an individual who has very few. The one with more connections is more likely to have access to information about TCB from their own social connections. Indeed, this idea has been central to a strand of the literature focused on using network data to optimize which network members should receive treatment (Banerjee et al., 2013, 2018; Beaman et al., 2018; Beaman and Dillon, 2018; Porter et al., 2018). In this paper, we find that eigenvector centrality matters for aggregate village-wide adoption, indicating that the social network structure of the village matters for the adoption of TCB.[5] We then turn to individual farmers and find that farmers who are more "central" in the network are more likely to adopt TCB. In other words, conditional on the number of friends, farmers who have friends who relatively well connected (i.e. have many social connections) are more likely to adopt TCB, suggesting that both direct and indirect social networks play a role in TCB adoption.

[5] Recently, Bandiera et al. (2020) has considered the implications for village wide social structures on policy implementation in Uganda.

The rest of the paper is organised as follows: Section 2 describes the data and experimental design; Section 3 contains our main results, Section 4 teases out causative factors and provides an overall discussion of our results; and Section 5 concludes.

2 Data and Experimental Design

We utilised a 'cluster' RCT with randomisation at both the village level and the farmer level. In this trial, the treatment was an invitation to an information session carried out by the East Africa Market Development Associates (EAMDA).[6] Briefly, the information sessions were led by an EAMDA instructor who provided information about the practical implementation and the benefits of planting Tissue Culture Banana (TCB) (e.g., its resistance to damage from pests and disease common to conventional banana crops). Control farmers were not invited and thus did not have access to these information sessions.[7]

At the first stage of the randomisation, 30 of the 90 sample villages were assigned to the pure control group; no farmers within these villages were invited to treatment.[8] Of the remaining villages, 15 were allocated to each of the four treatment intensity groups, in which either 20%, 40%, 60%, or 80% of the farmers were treated. Within each of these treatment villages, a second round of randomisation took place to determine which farmers would be invited to the information sessions. The number of invitees within each village corresponded with which of the treatment intensity groups the village was assigned to in the first round of randomisation. For example, in a village containing 50 eligible farmers, 10 farmers would be invited to an information session if the village was assigned to the 20% treatment group, whereas 20 farmers would be invited if the village was part of the 40% group. Figure 1 illustrates our experimental design.

We refer to a farmer who resides in a treatment village and was invited to the information session as a *treated* farmer, and an untreated farmer within a treatment village as a *spillover* farmer. Farmers in control village are referred to as *control* farmers. In sum, the farmers in our data set can be placed into three mutually exclusive categories:

Control: Farmers in villages that did not receive any treatment and were untreated;

Treated: Farmers in treated villages who were invited to the EAMDA's training session;

[6] The EAMDA is a consulting firm that offers business coaching and enterprise development in Eastern Africa. The EAMDA implemented the interventions with financial support from the Alliance for a Green Revolution (AGRA)

[7] Further details about the intervention can be found in Chowdhury et al. (2019).

[8] Although the control group farmers didn't have access to the treatment, it was still possible for them to learn about and adopt TCB given regional breeders also sell TCB seedlings.

Spillover: Farmers in treated villages who were not invited to the EAMDA's training session;

Finally, we use *untreated* to refer to both control farmers (in control villages) spillover farmers (in treatment villages).

A key aspect of our experimental design is that both treated and untreated farmers can reside in the same treatment village. Thus, an untreated farmer in our experiment can have social connections to treated farmers that received treatment, enabling us to identify the effect of social networks on the adoption of TCB. A program evaluation based on this data is available in Chowdhury et al. (2019).

2.1 Data Collection

Four rounds of surveys were carried out: baseline (May-June 2016), midline I (Oct-Nov 2017), midline II (Oct-Nov 2018) and endline (Oct-Nov 2019). Farmers were asked about their social networks and agricultural practices. Although we collected information about adoption outcomes in all four rounds, social network characteristics were only elicited in baseline. Figure 2 illustrates the survey timeline. The baseline survey collected information on a farmer's access to land, farming practices, crop production, income, consumption as well as demographic features such as age and education.

Complete midline I, midline II and endline, information is available for about 87% of farmers who were surveyed at baseline. Summary statistics provided in Table 1 display baseline characteristics. In Table A4 in the Appendix, we test for differential attrition across the control, treated and spillover groups. Besides testing for differential attrition across these groups, we also test whether there are interactions between group allocation and baseline characteristics in order to determine whether the types of farmers that attrited from each group were similar. We find that whilst there is no evidence for differences between the groups, there is evidence that baseline characteristics interact with treatment group allocation to create differential attrition.[9] In order to address this, we utilise inverse probability weighting to provide robust estimates of our main results in Table A5 in the Appendix. Overall, there is little difference between these results and our main results.

[9]The p-values for the joint significance of these baseline characteristics and their interactions are: $p = 0.066$ and $p = 0.062$ for differences between the control and treatment groups and the control and spillover groups, respectively

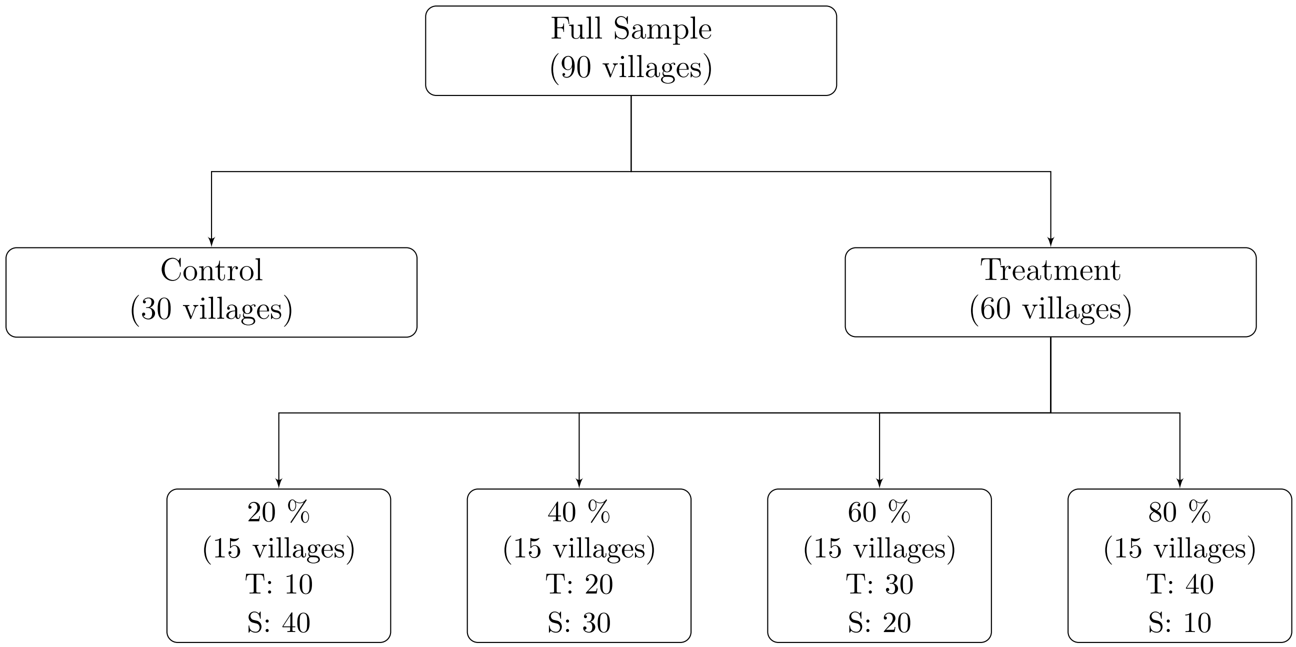

Figure 1: Experimental Design

Notes: We provide number of farmers that we surveyed at different levels of treatment saturation. T: number of treated farmers surveyed; S: number of spillover farmers surveyed. Number of farmers who are actually in each group varies by village. Control villages would have no treatment farmers.

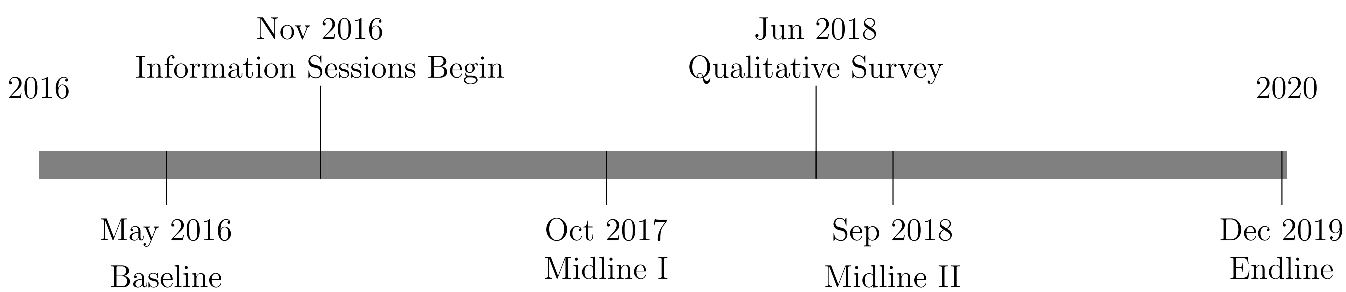

Figure 2: Timeline

Notes: Number of farmers surveyed in each period: Baseline: 4,719 (C=1,586, S=1,633, T=1,500); Midline I: 4,344 (C=1,454, S=1,497 T=1,393); Midline II: 4,345 (C=1,457, S=1,505, T=1,383); Endline: 4,190 (C=1,408, S=1,461, T=1,321).

Table 1: Summary Statistics

	Full Sample		Control	Treatment	Spillover	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	SD	Mean	Mean	Mean	p-value
A. Baseline Demographics						
Household Head Educated (=1)	0.49	0.50	0.50	0.47	0.49	0.59
Male Respondent (=1)	0.20	0.39	0.21	0.21	0.20	0.85
Number of Household Members	2.95	1.39	2.90	2.99	2.96	0.50
Land Size (Acres)	1.43	1.07	1.42	1.45	1.40	0.62
Electricity Access (=1)	0.42	0.49	0.47	0.38	0.40	0.19
ln(Monthly HH Income)	9.62	2.82	9.82	9.51	9.53	0.37
B. Network Variables						
Total Links	4.41	3.00	4.35	5.15	3.78	0.00
Treated Links	1.65	2.21	0.00	3.48	1.55	
Midline I Adopter Links	0.65	1.00	0.59	0.77	0.61	
Midline II Adopter Links	0.79	1.15	0.66	1.07	0.67	
Endline Adopter Links	0.96	1.22	0.65	1.39	0.86	
Eigenvector Centrality	0.10	0.09	0.09	0.11	0.09	
C. Adoption Outcomes						
Adopted in Midline I (0-6 months)	0.12	0.32	0.10	0.14	0.12	
Adopted in Midline II (6-18 months)	0.17	0.38	0.15	0.22	0.14	
Adopted in Endline (18-32 months)	0.22	0.41	0.15	0.32	0.19	
Adopted in Any Period (0-32 months)	0.51	0.50	0.40	0.68	0.45	
D. Sample Sizes						
Baseline	4,719		1,586	1,500	1,633	
Midline I	4,344		1,454	1,393	1,497	
Midline II	4,345		1,457	1,383	1,505	
Endline	4,190		1,408	1,321	1,461	

Notes: Columns 1, 3, 4 and 5 report means. Column 2 reports standard deviations. The *p*-values reported in Column 6 are for the joint significance of treatment allocation in a regression of treatment dummies on the variable of interest, with standard errors clustered at the village level. The months reported in Panel C refer to months post-intervention. The implications of the imbalance of total links (Panel B) on our main results are discussed in Section 3.3. The land size variable was winsorised at the 95th percentile. Income is reported in Kenyan Shillings (KES). 1 USD = 109.78 KES as of 10th Jan 2021.

2.2 The Social Network

Social networks are made up of nodes (agents) and links (defined as the social connections between nodes). We define nodes as the farmers who were sampled and asked survey questions. For links, we adopt a definition whereby a pair of farmers is connected if one of the respondents responds affirmatively to a survey question asking whether they know the other respondent. Our definition of links implies that we are working with *unweighted* and *undirected* networks. We adopt the convention of assuming that the networks are defined at the village level, meaning farmers may be connected with other farmers from the same village, but not farmers from other villages. This definition is consistent with much of the literature exploring the role of social networks on development outcomes (Conley and Udry, 2010; Banerjee et al., 2013; Miller and Mobarak, 2014; BenYishay and Mobarak, 2018b; Beaman et al., 2018). We provide the technical definition of the network and explain how we constructed networks from the survey questions in Section B in the Appendix.

2.2.1 Explanatory Variables

We work with network measures that are widely used in the social network literature. We provide a brief overview here, but more thorough explanations of these measures can be found in Jackson (2010, 2019).

Links: *Total Links* are the total number of farmers an individual is connected with. *Treated Links* are the number of treated farmers an individual is connected with. By construction, treated links are 0 for all control farmers. *Adopter Links* are the number of TCB adopters a farmer is connected with. Since adoption decisions can be different in different time periods, we define a farmer as having an adopter link in time t if they are linked with an individual who adopts TCB in that time period.

Centrality: In light of the growing literature focused on the role of network targeting and the importance of centrality in the diffusion of technology, we also include a discussion about the role of *Eigenvector Centrality* in Section 4.1. Further technical details are available in Section B in the Appendix.

2.3 Outcome Variables: TCB Adoption

Banana is a 'perennial crop', and farmers typically expect to harvest banana crops for years into the future. The crops are traditionally propagated using suckers, making them susceptible to pests and the spread of disease (Kabunga et al., 2014). TCB uses an in vitro technique that creates pathogen free plantlets in a laboratory (Murugi, 2008). In the surveys we conducted at midline I, midline II, and endline, farmers were asked whether they had planted TCB plantlets on their farms in the previous cropping season. We define 'adoption' as occurring in time t if a farmer responds affirmatively to this question at time t but not in any previous periods (except for midline I since we do not observe any adoption data prior to this):

$$Adoption_{it} = \mathbb{1}\Big\{Adopted\ TCB\ in\ period\ t\ and\ not\ in\ the\ previous\ periods\Big\}. \tag{1}$$

It is unlikely that many farmers would plant TCB in multiple periods despite being current users of the technology.[10] Moreover, since many farmers who adopted TCB only once throughout the study period could go on to use the technology over multiple periods, using an outcome variable that encodes information about multiple instances of TCB adoption could lead to misleading results. We summarise adoption rates of control, treated, and spillover farmers in Panel C of Table 1. Note that in order to ensure that coefficients could be interpreted in terms of percentage points, this outcome variable is scaled by 100 in our reported results.

3 Identification and Results

A roadmap for our main results is as follows. First, we establish that there is a 'direct effect' of the treatment – those who were invited to the treatment were more likely to adopt TCB. Then, we present our main results on the effect of the social networks, illustrating that treated links had an effect on TCB adoption for the spillover group. We complete by providing robustness checks and additional analyses.

Was the intervention effective for those that were treated?

The first step in our analysis is to provide evidence for the direct effect of the treatment. This is an important part of our analysis, as we need to verify that treatment was effective for those it was directly targeting before investigating the effect of the treated links. Since we consider the effect of an invitation to the treatment as opposed to actual treatment attendance, our estimation results should be interpreted as an 'Intention to Treat' (ITT)

[10] Amongst farmers who planted a TCB plantlet at least once, 74% planted once, 22% planted twice and 6% planted three times

effect. And we use the following specification:

$$Adoption_{itv} = \beta_0 + \beta_1 Treated_{iv} + \gamma' \mathbf{x}_{iv} + u_{iv}. \tag{2}$$

The outcome variable $Adoption_{itv}$ equals 100 (percent) if individual i in village v adopted TCB in time t, and 0 otherwise. $Treated_{iv}$ is an indicator variable that equals to 1 if the farmer was invited to the information sessions and $\mathbf{x}_{iv}$ is a vector of controls (household head's education and gender, the number of household members, land size, access to electricity and monthly household income). We report the estimation results in Table A1 in the Appendix.

Our results demonstrate that the treatment increased the likelihood that farmers adopted TCB in all time periods. Compared to the untreated farmers (control and spillover), treated farmers are 3.6 pp more likely to adopt TCB by midline I, 7.9 pp more likely by midline II, and 15.5 pp more likely by endline.

3.1 Identification of a Social Network Effect

The challenge for the identification of an association between network variables and outcomes is well described in Manski (1993). Essentially, this challenge stems from unobservable characteristics that contribute to both the adoption of TCB and the structure of one's social network (e.g. the number of total links). For example, behavioural traits such as openness might be associated with one's willingness to seek out new technologies as well as the number of people they are friends with.[11] The practical implication of this is that it is difficult to isolate exogenous variation in network characteristics to provide causal evidence for social network effects.

However, our experimental design allows us to use a specification in which treated links can be considered as good as randomly assigned. To illustrate this point, consider an subset of the untreated farmers within a village who have the same number of social connections. The number of treated links each of these farmers has is then random, since the treatment is randomly assigned and which of their links receives the treatment is random. Thus, conditioning on the number of total links, the number of treated links is no longer correlated with unobservables related to the structure of one's social network and TCB adoption. Therefore, we are able to leverage exogenous variation in the treated links to provide causal evidence.

[11] This is supported empirically: He and Veronesi (2017) finds that personality traits such as openness contribute to the adoption of biogas on farms in China. Separately, Giulietti et al. (2011) provides evidence that personality traits affect networks and labour market outcomes; they find that outgoing individuals are both more likely to have more friends as well as higher wages.

3.2 Main Results: The Effect of Social Networks on TCB Adoption

Our main estimation results are obtained from the following equation:

$$Adoption_{itv} = \beta_0 \ + \ \beta_1 Treated\ Links_{iv} + \beta_2 Total\ Links_{iv} + \eta Intensity_v + \gamma' \mathbf{x}_{iv} + u_{iv}, \quad (3)$$

where $Adoption_{itv}$ equals 100 (percent) if individual i in village v adopted TCB in time t and 0 otherwise. Our parameter of interest, β_1, identifies the causal relationship between an additional treated link and the likelihood of adopting TCB. We estimate this equation with data on the untreated, a group that comprises of both spillover and control farmers. In all specifications, we include a vector of controls, $\mathbf{x}_{iv}$ (education and gender of the household head, number of household members, land size (winsorized) access to electricity and the natural log of monthly household income), and control for village-level treatment intensity. Results are reported in Table 2.

Treatment intensity (or simply, "Intensity") is defined as the proportion of treated farmers to total farmers in each village. We control for treatment intensity in order to account for possible influences of the treatment that do not stem from social connections. To illustrate just one scenario where this might occur: a farmer goes to the market and overhears a conversation between people, who the farmer does not know, about the use of TCB.[12]

Results in Column 1, Table 2 show that total links are associated with adoption in the immediate short term (0-6 months post-intervention), whilst treated links are not. This result should be interpreted with caution. Since total links are a control variable, β_2 does not itself imply any causal relation between social networks and TCB adoption. [13]

The key findings of our main regression come from Column 2. These results indicate that an untreated farmer who has an additional social connection that is invited to treatment is 2.25 pp more likely to adopt TCB 6-18 months post-intervention. This effect size is large given that the direct effect of invitation to treatment in the same time period is 7.9 pp (Column 2, Table A1). The effect of an additional treated link is equivalent to about $\frac{2.25}{7.9} \times 100 \approx 32\%$ of the direct effect, which is large in relative terms.

[12] We note that treated links and our intensity measure appear to be highly correlated ($\rho = 0.67$)

[13] To be more concrete, suppose that the error term in our regression can be modelled as: $u = \alpha + \delta W + V$, where W is a variable representing the social network endogeneity and V is an exogenous error term. If we assume that $\mathbb{E}[V|Treated \quad links, W] = \mathbb{E}[V|W]$ (conditional exogeneity), that W is included in our regression, and that $\mathbb{E}[V|W]$ is a linear function of W, then the estimated coefficient on the treated links is consistent for β_1, the causal effect of the treated links, while the estimated coefficient on the total links may not capture the causal effect of the total links β_2.

Table 2: Main Results: Treated Links Influence TCB Adoption

	Adopted TCB ×100			
Months since treatment:	Midline I (0-6) (1)	Midline II (6-18) (2)	Endline (18-32) (3)	Any Month (0-32) (4)
Social Network Variables				
Total Links	1.169***	-0.052	0.074	1.133*
	(0.411)	(0.311)	(0.389)	(0.574)
Treated Links	-0.840	2.250***	-0.204	1.244
	(0.683)	(0.809)	(0.996)	(1.417)
Controls				
Household Head Educated (=1)	3.310**	-1.217	0.705	2.457
	(1.571)	(1.552)	(1.738)	(2.221)
Male Respondent (=1)	-2.768*	-1.079	4.294**	-0.240
	(1.420)	(1.820)	(1.958)	(2.554)
Number of Household Members	-0.368	-0.500	1.611**	0.161
	(0.447)	(0.557)	(0.629)	(0.739)
Land Size (Acres)	2.899***	0.315	-0.334	1.720
	(0.906)	(0.733)	(0.861)	(1.170)
Electricity Access (=1)	4.404**	-1.204	-0.182	2.878
	(1.848)	(1.559)	(1.981)	(2.574)
ln(Monthly HH Income)	0.115	0.328	-0.127	0.336
	(0.294)	(0.260)	(0.360)	(0.468)
Treatment Intensity				
Intensity	5.809	-9.188	11.962	7.797
	(4.026)	(5.571)	(7.525)	(9.099)
Observations	2951	2791	2646	2951

Notes: Constant omitted from output. Standard errors clustered at the village level. We control for treatment intensity in all specifications. Household size refers to number of occupants. The outcome variable is a binary variable that has been multiplied by 100. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

However, Column 3 indicates no social network effect in the longer term between 18-32 months post-intervention. In Column 4, of Table 2 we test whether social networks contribute to the likelihood of adoption in *any* of the 32 months spanning the study. We find little evidence of this being the case; total links are significant but the coefficient is practically small (it is about 50% the size of the causal treated links estimate). This suggests that farmers with more social connections are not much more likely to adopt TCB overall over the course of the study.

The lack of a longer-term social network effect can be explained by the actual dissipation of these effects rather than by an overall decline in TCB adoption. The summary statistics in Table 1 suggest that the highest levels of adoption for all groups (control, spillover, treated) are seen in the period between 18-32 months post-intervention. Given the fact there is no evidence of a causal relationship between social networks and TCB adoption in 18-32 months post-intervention, and a very small effect on the overall likelihood of adoption across the 32 months of the study, we conclude that the effect of social networks is to induce farmers to adopt TCB "sooner rather than later" between 0-18 months post-intervention.

3.3 Additional Analyses and Robustness

3.3.1 Evidence of a Non-linear Effect?

A natural question arising from our causal estimates is whether the treated link effect is non-linear. We attempt to answer this question using two approaches which provide complementary results.

In the first approach, we re-estimate Equation (3) with the inclusion of a quadratic term of the treated links:

$$\begin{aligned} Adoption_{itv} = \beta_0 \ + \ \beta_1 Treated\ Links_{iv} + \ \beta_2 Treated\ Links_{iv}^2 + \beta_3 Total\ Links_{iv} \\ + \eta Intensity_v + \ \gamma' \mathbf{x}_{iv} + u_{iv}. \end{aligned} \tag{4}$$

Our second approach utilises the fact that the absence of non-linear effects would imply that the effect of having at least one treated link would be equal to the effect of an additional treated link conditional on having at least one. In order to test whether this is true, we use two specifications: (i) one that looks at the 'extensive margin' – the effect of having at least one treated link on the full sample; and (ii) one which looks at the 'intensive margin' – the effect of an additional treated link on the sample of farmers that have at least one treated link. The intensive margin specification is identical to Equation (3) but is estimated using a subset of the sample, whilst our extensive margin specification is:

$$Adoption_{itv} = \beta_0 + \beta_1 \mathbf{1}\{Treated\ Links > 0\}_{iv} + \beta_2 Total\ Links_{iv} + \eta Intensity_v + \gamma' \mathbf{x}_{iv} + u_{iv}. \tag{5}$$

Here, $\mathbf{1}\{Treated\ Links > 0\}$ is an indicator for a farmer having at least one treated link. Results are shown in Tables A2 and A3, respectively. Both approaches produce complementary results, and the overarching message is that there is little evidence of non-linear effect of treated links in our data. Both find some evidence of non-linear effect in midline I (0-6 months post-intervention), however the results from (4) show that the coefficient on the on the quadratic term is relatively small.[14] Moreover, in both specifications, in midline II (6-18 months post-intervention) — the time period relevant for our main results — we find no evidence of non-linear effect. In Column 2 of Table A2, we find that the quadratic term is not statistically significant. In addition, the point estimates in Column 2 and Column 5 in Table A3 show that the effect of having at least one treated link is very similar to the effect of having an additional treated link, their difference being less than 0.5pp.[15]

3.3.2 Addressing Attrition

To determine the potential effects of differential attrition rates, we tested: (a) whether treatment allocation predicted attrition; and (b) whether the interaction between treatment allocation and the baseline controls in our main regressions predicted attrition.[16] The latter is an important part of our analysis because finding no evidence of a relationship between treatment allocation and attrition does not necessarily imply that the types of farmers that attrited in each group are the same. For example, it could be the case that higher-income farmers were more likely to drop out in the treatment group compared to those who were untreated. The results of our analysis are shown in Table A4; treatment allocation does not predict attrition, however the interaction between our baseline controls and treatment allocation is statistically significant. In order to allay concerns that differential attrition

[14] The magnitude of the non-linear effect with the intensive vs. extensive approach is harder to interpret since it is determined by estimates from two separate specifications

[15] Although the coefficient on the indicator in Column 2 is not significant, this is unsurprising because using a dummy variable means we are not utilising all the variation in the data. What is of interest is the value of the point estimate itself.

[16] Recall these were: education and gender of the household head, number of household members, land size (winsorized) access to electricity and the natural log of monthly household income .

might be driving our results, we re-estimate Equation (3) using inverse probability weights as suggested by Wooldridge (2007). We obtain the probability weights by using the fitted values of a Probit model, and then run our main estimation equation (with adoption as the outcome variable) weighting each observation by the inverse of the predicted probability. The results obtained from this procedure are displayed in Table A5 in the Appendix, and, reassuringly, we find that these are very similar to our main results.

3.3.3 Imbalance of Total Links Across Treatment Groups

Another concern that is relevant for the robustness of our main results is whether the fact that, despite randomization, total links appear imbalanced across treatment groups might be affecting our results (see Table 1). We provide two pieces of evidence which indicate that this does not seem to be the case. Firstly, if we only consider the sample of untreated farmers (i.e., the sample that we use to estimate our main results) and consider the difference between the control and the spillover group, we find that total links are not different statistically across these two groups ($p = 0.143$). Whilst this suggests that treated farmers have more treated links on average, it tells us these differences do not contribute to our main results which only use the sample of untreated farmers. Secondly, whilst there may be differences across individual farmers that are treated and untreated, we show that on average, there is no evidence that network characteristics are different across treatment and control villages (see Table B1 in Section B of the Appendix). Putting these two together, it seems unlikely that differences in the total links across treatment groups is driving our main results.

3.3.4 Supporting Evidence from Qualitative Survey

A qualitative survey was conducted in June 2018 between midline I and midline II (see Figure 2). It included both Focus Group Discussions (FGDs) and Key Informant Interviews (KIIs). 15 of the 90 total villages were randomly selected for the qualitative study; 4 control and 11 treatment. In total, 15 FGDs were held and 30 key informants were interviewed: 15 farming group leaders, 5 extension officers, 5 local chiefs, and 5 banana traders.

The FGDs had open-ended questions to explore issues such as the reasons for and the barriers against adopting TCB and the factors influencing gender and youth participation in the production and marketing of bananas. KIIs were more focussed and aimed at collecting in-depth information from individuals who could offer expert knowledge about the geographical area and TCB adoption.

Responses from our KIIs and FGDs revealed some key reasons for non-adoption of TCB, such as the lack of access to proper irrigation and poor road conditions when wet (which would impede market access). This can help explain why social networks do not contribute to overall or long-term adoption; once information on TCB is diffused to the farmers, their adoption decisions are more likely to be determined by structural factors outside of what can be influenced by a farmer's social network. This insight is also supported by other studies of banana production in Kenya that have suggested structural factors such as land size and access to irrigation are deterrents of TCB adoption (Mbaka et al., 2008; Muthee et al., 2019).

The qualitative survey also offers an insight into the temporal nature of the social network effect we observed. Recall, one of our main results was that treated links did not affect adoption either between 0-6 months post-intervention, and were only significant between 6-18 months post-intervention. When we asked farmers about whether they would be willing to plant TCB if the structural barriers discussed above were minimized, they responded affirmatively, but it appeared they'd prefer to wait until knowing the costs involved before adopting. To quote one farmer: *"We will wait in order to understand the cost incurred in the management of TC varieties by observing from adopters if TC bananas have extra benefits compared to conventional varieties"*. This provides a reasonable explanation for why treated links did not influence adoption in the short term; farmers might have been waiting to understand the costs involved.

4 Possible Mechanisms and Discussions

4.1 Adoption decisions as a social network mechanism

The idea that one's own adoption decisions might be influenced by the adoption decisions of their social connections has been explored by the previous technology adoption literature. Foster and Rosenzweig (1995), for example, introduced the classic target-input model for technology diffusion where adopters are more influential than non-adopters. Empirically, these predictions have been shown to hold in a variety of contexts: the take-up of stoves in Bangladesh (Miller and Mobarak, 2014); adoption of biochar (a soil amendment) in Kenya (Crane-Droesch, 2018); and pineapple fertilizer in Ghana (Conley and Udry, 2010).

However, much of the aforementioned evidence is descriptive and only shows a correlation between being connected with adopters and technology adoption, and there is a dearth of causal evidence. The problem for identification here is an example of the 'reflection problem' (Manski, 1993). It is not possible to disentangle whether links with farmers who adopted in a particular period are influencing the adoption of the untreated farmers in that same period, or whether those untreated farmers' adoption decisions are influencing the decisions of their social connections.Note that this problem is not present in our main results because the number of treated links a farmer has does not depend on the adoption status of those links. As we have argued previously, conditional on total links, treated links are exogenous.

We try to overcome this issue by instrumenting for adopter links with treated links. As we argued in our main results, since treatment is randomly allocated, our instrument is exogenous conditional on total links. We argue that it is unlikely that treated links, after conditioning on total links, affect adoption through other channels, because treatment was randomly assigned. As with our main results, we restrict our analysis to untreated farmers. Our main equation of interest is:

$$Adoption_{itv} = \beta_0 + \beta_1 Adopter\ Links_{itv} + \beta_2 Total\ Links_{iv} + \gamma' \mathbf{x}_{iv} + \eta Intensity_v + u_{itv}. \quad (6)$$

We report (possibly biased) OLS estimates of this equation in Columns 1-3 of Table 3. We find that the number of links with adopters are correlated with adoption in all periods. We then estimate (6) by 2SLS, using the treated links as an instrument for the adoptor links. More specifically, the first stage is modelled as:

$$Adopter\ Links_{itv} = \beta_0 + \beta_1 Treated\ Links_{itv} + \beta_2 Total\ Links_{iv} + \gamma' \mathbf{x}_{iv} + \eta Intensity_v + u_{itv}. \quad (7)$$

The key explanatory variable in Equation (6), *Adopter Links*, varies over time (since adoption decisions vary over time). We report contemporaneous effects, that is, the association between adoption and this variable in the same time period.[17] We include the same controls that were used in Equation (3), which is the model of our main results.

Columns 1-3 of Table 3 contain our OLS estimates of Equation (6). We find that adoption decisions are correlated with links with adopters in all time periods. These results are purely illustrative because of the endogeneity concerns expressed above. Our 2SLS results in Columns 4-6 are telling; being connected with more adopters increases the likelihood of a farmer's own adoption of TCB 6-18 months post-intervention. As in our main results, the effect is not present in the immediate short term (0-6 months after the intervention) nor is it present in the longer term (18-32 months after the intervention). This result suggests that

[17] Although results are not presented here, we also tried specifications with lagged effects (i.e., with the explanatory variables being links with those who adopted in the previous period). We found no evidence of lagged effects.

Table 3: Links With Adopters are a Mechanism for the Social Network Effect

	Adopted TCB ×100					
	OLS Estimates			2SLS Estimates		
Months since treatment:	Midline I (0-6) (1)	Midline II (6-18) (2)	Endline (18-32) (3)	Midline I (0-6) (4)	Midline II (6-18) (5)	Endline (18-32) (6)
Midline I Adopter Links	4.671***			-24.05		
	(1.214)			(46.63)		
Midline II Adopter Links		7.187***			18.86**	
		(1.230)			(9.521)	
Endline II Adopter Links			5.645***			-1.396
			(1.451)			(6.831)
Total Links	0.209	-0.862***	-1.053**	5.208	-2.820*	0.308
	(0.363)	(0.315)	(0.431)	(8.761)	(1.561)	(1.371)
Intensity	1.615	-2.820	6.520	7.142	-6.956*	12.30
	(2.938)	(3.708)	(5.750)	(8.281)	(4.048)	(8.637)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-Test	-	-	-	0.151	7.74	0.04
Anderson Rubin p-values	-	-	-	0.222	0.007	0.838
Observations	2951	2791	2646	2951	2791	2646

Notes: Constant omitted from output. Standard errors clustered at the village level. The sample in all specifications is untreated farmers. Columns 1-3 report results of Equation (6) using OLS whilst columns 4-6 report results using 2SLS with a first stage specified as in Equation (7). The outcome variable is a binary variable that has been multiplied by 100. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

the likely mechanism underpinning the main causal effect we estimated in Section 3 are links with adopters. Being connected to treated farmers increases the amount of adopter links since there is a direct effect of treatment (as illustrated in Table A1).

The inference results in 3 do not account for the possibility of a weak instrument. To address this issue, we follow Olea and Pflueger (2013) and provide the effective first-stage F-statistic. The effective first-stage F-statistic for our midline II specification is 3.121 and is well below the rule-of-thumb critical value 10. As a result, we use weak instrument robust inference and find that the Anderson-Rubin test statistic is 7.74 ($p = 0.0066$). Our conclusions remain the same under weak instrument robust inference.

4.2 Eigenvector Centrality and Indirect Network Effects

A farmer's direct social connections (i.e., their links) are one channel through which a social network might affect behaviour. But how might a farmer be affected by their indirect social connections? Suppose a farmer has two friends: one who is very well-connected (i.e. has many social connections) and one who is not well-connected (i.e. has very few connections). The friend who is very well-connected is more likely to transmit important information about TCB than the less connected one because they have access to a greater amount of information from their own links. Moreover, the well-connected friend might have more treated links and/or links who have adopted TCB. Therefore, being linked with this farmer is more likely to induce one's own adoption compared to being linked with the less well-connected farmer.

The idea that direct social connections do not paint the full picture of how social networks operate is at the core of a literature that has provided methods for how to optimally target treatment to influential agents within networks (Banerjee et al., 2013; Beaman et al., 2018; Beaman and Dillon, 2018; BenYishay and Mobarak, 2018b). A measure that takes into account of the connectedness of a farmer's own social connections is the eigenvector centrality.

Eigenvector centrality is defined recursively; a node's centrality depends on the centrality of its social connections. One key feature of the eigenvector centrality is that it depends on indirect, as well as direct, social connections (Bonacich, 2007).[18] Eigenvector centrality has been shown to contribute to social learning processes (Bloch et al., 2019) and is considered in closely related studies such as Cai et al. (2015), thus we also include it in our analysis and study its effect.

First, we provide a set of village-level results which establish that eigenvector centrality

[18]In fact, eigenvector centrality is a weighted sum of direct and indirect social connections, as explained in Bonacich (2007)

is positively associated with overall village level adoption. We estimate:

$$Prop.\ Adopted_{tv} = \beta_0 \ + \beta_1 \ Mean\ Eigenvector\ Centrality_v + \eta Intensity_v \ + \ u_{tv}, \quad (8)$$

where the outcome variable is the proportion of farmers in village v that adopted TCB in time t. Results are reported in Table A6 in the Appendix. We find that the results of the village level regressions follow the same pattern as our main causal estimates. Eigenvector centrality matters for adoption 6-18 months after the intervention, but not before or after this. The magnitude of this effect is relatively large: a 1 s.d. increase in average eigenvector centrality (s.d. = 0.098)[19] is associated with an 8 pp increase in aggregate adoption within a village. This result accords with previous literature which has found that village-level social structures influence aggregate behaviour (Foster and Rosenzweig, 1995; Banerjee et al., 2013; Bandiera et al., 2020).

Then, to investigate whether a farmer's centrality influences their own adoption, we estimate:

$$\begin{aligned} Adoption_{itv} = \beta_0 \ + \beta_1 \ Eigenvector\ Centrality_{iv} + \beta_2 \ Treated\ Links_{iv} + \\ + \beta_3 \ Total\ Links_{iv} \ + \eta Intensity_v + \gamma' \mathbf{x}_{iv} + u_{iv}. \end{aligned} \quad (9)$$

Results are reported in Columns 1-4 of Panel B, Table A6 in the Appendix. Columns 5-8 are from the same specification with treated links omitted.

Our results reveal two facts: (a) our previous causal results are robust to the inclusion of the eigenvector centrality; and (b) the indirect effect of the social network—caused by the influence on TCB adoption of the links of a farmer's links—is separate from the direct effect (i.e., the effect of an additional treated link). Comparing the midline II results from Column 2, Table 2 and Column 2, Panel B, Table A6, we find that the point estimates of the coefficients on the treated links are similar in magnitude to specifications with and without eigenvector centrality. In midline II (6-18 months post-intervention), the effect of a 1 s.d. increase in eigenvector centrality (s.d. = 0.097) is equal to a 2.08 pp increase in the likelihood of TCB adoption. This result is interesting because it exists even after controlling for the direct effect of treated links. This indicates that the indirect social network effects, proxied for by eigenvector centrality, exist independently of the direct effects account for by

[19]This is the mean of average eigenvector centrality at the village level.

treated links. To support this further, we included results from a version of the specification without treated links in Columns 5-8 and find that they are strikingly similar.

These results differ from the study most closely related to ours, Cai et al. (2015), which finds that eigenvector centrality *does not* contribute to insurance adoption either at the village[20] or the farmer level. However, one result—consistent with (Cai et al., 2015)—is that eigenvector centrality had no effect on adoption between 0-6 months. Therefore, it is possible that the indirect social network effects explained by eigenvector centrality take time to manifest, and that differences in our findings might be explained by differences in the durations of our respective studies.

5 Conclusion

We estimate the causal effect of social networks on the adoption of TCB, an agricultural technology, amongst farmers in Kenya. Our work fits into to a growing literature that has sought to use randomised experiments to identify the causal effect of social networks. We make two further contributions by: (a) providing evidence for longer term (that is, up to 32 months post-intervention) social network effects; and (b) investigating whether causal social network effects are a key determinant for agricultural technology adoption.

Our experiment involved inviting farmers to information sessions about the technology, and we examine how the adoption decisions of untreated farmers are influenced by their treated social connections (treated links). We find that an untreated farmer with an additional treated link is 2.25 pp more likely to adopt TCB 6-18 months post-intervention. The magnitude of the effect is large in relative terms, comprising of about 32 % of the direct effect of being invited to treatment. We find that treated links do not influence untreated farmer adoption either before (0-6 months post-intervention) or after (18-32 months post-intervention) this time period. Moreover, since treated links do not appear to increase the likelihood of adoption over the 32 month course of the study (i.e., 0-32 months post-intervention), we conclude that the effect of social networks is to induce farmers to adopt TCB sooner rather than later.

We provide evidence that the adoption decisions of the treated links are a mechanism driving our main results. This effect would typically be difficult to identify due to the fact that the number of adopter links is endogenous. To overcome this issue, we utilise treated links as an instrument to estimate the adopter links effect. We find that links with adopters

[20] More precisely, the average eigenvector centrality of the village has no affect on individual farmer adoption outcomes

are significant for adoption between 6-18 months post-intervention, echoing our main causal results. It is possible that treated links contribute to adoption decisions because that those who are treated are themselves more likely to adopt and that their adoption decision would encourage their network neighbour to adopt as well.

Finally, in order to provide evidence for the indirect effects of links in the social network, we investigate whether eigenvector centrality influences adoption outcomes. We first establish that villages with higher average eigenvector centrality have higher adoption rates, suggesting that, apart from the number of direct links, other features of the structure of the social network may also affect adoption outcomes. We then provide evidence that farmers who have higher eigenvector centrality (and thus, have friends who are better connected) are more likely to adopt TCB. Interestingly, we show that this effect is separate from our causal treated link effect; farmers are influenced both by their direct social connections, and also indirectly through the links of their links.

Program evaluations of agricultural extension programs often assume or provide evidence for 'spillover effects' when providing cost-benefit analyses. Our results show that if the magnitude of social network effects are to be accounted for, the possibility that they take some time to manifest should also be considered. However, we have not necessarily found evidence that social networks are a key contributor to the alleviation of the low-adoption of agricultural technologies in Sub-Saharan Africa. Firstly, while our results show that social networks influence farmers to adopt sooner rather than later, we found that the overall likelihood of adoption is not significantly increased. If the goal of policy makers is to increase aggregate adoption of technologies such as TCB, this study has not provided particularly compelling evidence that social networks provide a clear pathway. Secondly, we provided evidence that a mechanism through which social networks operate was adoption decisions, one of the key outcomes that agricultural extension programs target in the first place. So, rather than "solving" the low-adoption problem, it appears as though social networks might enhance the effect of policies that are *already* successful at increasing adoption.

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A Appendix

Table A1: Treatment Directly Influenced Adoption

	Adopted TCB ×100			
Months since treatment:	Midline I (0-6) (1)	Midline II (6-18) (2)	Endline (18-32) (3)	Any Month (0-32) (4)
Treatment HH	3.642**	7.859***	15.47***	24.42***
	(1.446)	(2.364)	(2.896)	(2.757)
Household Head Educated (=1)	3.991***	0.280	0.369	4.840***
	(1.329)	(1.237)	(1.393)	(1.751)
Male Respondent (=1)	-1.055	-1.198	3.511**	1.296
	(1.394)	(1.555)	(1.749)	(2.129)
Number of Household Members	-0.253	-0.335	1.271**	0.0208
	(0.406)	(0.465)	(0.557)	(0.617)
Land Size (Acres)	3.037***	0.862	-1.134	0.921
	(0.710)	(0.676)	(0.759)	(0.979)
Electricity Access (=1)	6.261***	-0.601	-3.748*	1.892
	(1.590)	(1.516)	(1.891)	(2.125)
ln(Monthly HH Income)	-0.0590	0.426*	-0.0297	0.374
	(0.232)	(0.216)	(0.270)	(0.347)
Observations	4344	4345	4190	4719

Notes: Standard errors clustered at village level. Treatment intensity controlled in all regressions. Outcome variables are whether farmer used TCB in last season. The outcome variable is a binary variable that has been multiplied by 100. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

Table A2: Inclusion of a 2nd-order Polynomial Does Not Reveal Non-linearities

	Adopted TCB ×100			
Months since treatment:	Midline I (0-6) (1)	Midline II (6-18) (2)	Endline (18-32) (3)	Any Month (0-32) (4)
Social Network Variables				
Total Links	1.166***	-0.050	0.074	1.133*
	(0.410)	(0.312)	(0.390)	(0.574)
Treated Links	1.440	1.257	-0.753	1.512
	(1.225)	(1.506)	(1.823)	(2.042)
Treated Links^2	-0.411**	0.177	0.097	-0.048
	(0.164)	(0.217)	(0.268)	(0.294)
Controls				
Household Head Educated (=1)	3.349**	-1.230	0.691	2.462
	(1.575)	(1.554)	(1.740)	(2.223)
Male Respondent (=1)	-2.671*	-1.123	4.272**	-0.229
	(1.417)	(1.813)	(1.964)	(2.560)
Number of Household Members	-0.368	-0.501	1.610**	0.161
	(0.446)	(0.558)	(0.629)	(0.739)
Land Size (Acres)	2.874***	0.324	-0.330	1.717
	(0.900)	(0.733)	(0.861)	(1.169)
Electricity Access (=1)	4.483**	-1.235	-0.199	2.887
	(1.856)	(1.564)	(1.981)	(2.579)
ln(Monthly HH Income)	0.108	0.331	-0.125	0.335
	(0.293)	(0.260)	(0.360)	(0.468)
Treatment Intensity				
Intensity	2.964	-7.940	12.655	7.463
	(3.998)	(5.831)	(7.829)	(9.142)
Observations	2951	2791	2646	2951
p-value: Treated Links, Treated Links^2	0.011	0.013	0.918	0.660

Notes: Constant omitted from output. Standard errors clustered at the village level. We control for treatment intensity in all specifications. Household size refers to number of occupants. The outcome variable is a binary variable that has been multiplied by 100. The specification is identicial to Equation (3) except for the inclusion of a second-order polynomial in treated links. The p-value reported at the bottom of the table refers to the joint significance of the treated link terms. We also estimated a similar specification with the inclusion of a third-order polynomial as well and found that results were similar, however, these results have been omitted for brevity. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

Table A3: Extensive vs. Intensive Margins Do Not Reveal Non-linearities

	Adopted TCB ×100					
	Extensive Margin (Sample: Untreated)			Intensive Margin (Sample: Treated Links > 0)		
Months since treatment:	Midline I (0-6) (1)	Midline II (6-18) (2)	Endline (18-32) (3)	Midline I (0-6) (4)	Midline II (6-18) (5)	Endline (18-32) (6)
Total Links	0.978***	0.279	0.0417	1.082	-0.201	0.138
	(0.357)	(0.334)	(0.347)	(0.722)	(0.698)	(0.873)
1{Treated Links > 0 }	1.568	2.289	-0.139			
	(2.059)	(2.143)	(2.909)			
Treated Links				-1.292	2.762***	-0.156
				(1.138)	(1.001)	(1.625)
Intensity	0.219	-3.631	11.36	-2.131	-8.808	8.722
	(4.264)	(5.558)	(8.249)	(8.097)	(7.789)	(12.57)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2951	2791	2646	1043	987	933

Notes: Columns 1-3 report results of Equation (5). Columns 4-6 report results of Equation (3) estimated on the sample of farmers that have at least one treated link. $\mathbf{1}\{Treated\ Links > 0\}$ is an indicator variable for whether the farmer has at least one treated link. The outcome variable is a binary variable that has been multiplied by 100. Standard errors are clustered at the village level. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

Table A4: Evidence of Differential Attrition Across Treatment Groups

	Model 1	Model 2
Spillover	-0.00690	0.0292
	(0.0128)	(0.0582)
Treated	0.00710	0.0191
	(0.0145)	(0.0577)
Baseline Characteristics	No	Yes
Baseline Characteristics × treatment groups	No	Yes
p-value: Joint significance of treatment groups	0.450	0.742
p-value: F-Stat of interactions (Treatment)	-	0.066
p-value: F-Stat of interactions (Spillover)	-	0.062
Observations	4719	4719

Notes: Constant omitted from output. The outcome variable is a binary variable that has been multiplied by 100. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

Table A5: Main Results Robust to Accounting for Attrition (IPW Estimates)

	Adopted TCB ×100			
	Midline I	Midline II	Endline	Any Month
Months since treatment:	(0-6)	(6-18)	(18-32)	(0-32)
	(1)	(2)	(3)	(4)
Social Network Variables				
Total Links	1.199***	-0.0244	0.0616	1.271**
	(0.410)	(0.309)	(0.392)	(0.602)
Treated Links	-0.893	2.348***	-0.178	0.838
	(0.683)	(0.819)	(0.979)	(1.383)
Controls				
Household Head Educated (Y = 1, N = 0)	3.507**	-1.095	0.727	2.843
	(1.602)	(1.596)	(1.766)	(2.329)
Household Head Gender (M = 1, F = 0)	-2.828*	-1.010	4.374**	1.116
	(1.477)	(1.836)	(1.927)	(2.642)
Number of Household Members	-0.416	-0.486	1.799***	0.656
	(0.462)	(0.565)	(0.639)	(0.806)
Wins. Land Size (Acres)	2.824***	0.311	-0.293	3.013**
	(0.925)	(0.721)	(0.864)	(1.308)
Electricity Access (Y = 1, N = 0)	4.403**	-0.961	0.0892	2.657
	(1.941)	(1.545)	(2.004)	(2.756)
ln(Monthly HH Income)	0.158	0.350	-0.222	0.196
	(0.306)	(0.261)	(0.371)	(0.509)
Treatment Intensity				
Intensity	5.898	-9.483*	11.73	9.742
	(4.031)	(5.517)	(7.434)	(9.010)
Observations	2951	2791	2646	2646

Notes: We run the same specification as Equation (3) which gave us our main, causal results. Here, estimates are robust to differential attrition across treatment groups by using inverse probability weighting. For probability weights, we utilise the inverse fitted values of a Probit regression predicting attrition with baseline controls as RHS variables. Constant omitted from output. Standard errors clustered at the village level. We control for treatment intensity in all specifications. Household size refers to number of occupants. The outcome variable is a binary variable that has been multiplied by 100. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

Table A6: Eigenvector Centrality: Village and Farmer-level Results

Months since treatment:	Midline I (0-6) (1)	Midline II (6-18) (2)	Endline (18-32) (3)	Any Month (0-32) (4)	Midline I (0-6) (5)	Midline II (6-18) (6)	Endline (18-32) (7)	Any Month (0-32) (8)
A. Dependent Variable = Proportion Adopted TCB **×100**								
Mean Eigen. Centrality	5.861	88.66**	32.29	1.268**				
	(26.27)	(35.50)	(40.79)	(0.535)				
Intensity	Yes	Yes	Yes	Yes				
Controls	No	No	No	No				
Observations	90	90	90	90				
B. Dependent Variable = Adopted TCB **×100**								
Treated Links	-0.832	2.238***	-0.201	0.827				
	(0.686)	(0.805)	(0.998)	(1.412)				
Eigenvector Centrality	-9.155	21.68**	-4.752	10.84	-9.265	21.85**	-4.772	10.92
	(10.80)	(10.05)	(10.16)	(18.58)	(10.76)	(10.03)	(10.14)	(18.56)
Total Links	1.276***	-0.304	0.130	1.112*	1.133***	0.0883	0.0949	1.258**
	(0.406)	(0.327)	(0.387)	(0.569)	(0.333)	(0.347)	(0.350)	(0.503)
Intensity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2951	2791	2646	2646	2951	2791	2646	2646

Notes: Panel A presents results from the village-level regressions described in Equation (8). The dependent variable is the proportion of the village that adopted TCB in the particular time period (and is therefore between 0 and 1). Panel B presents results from the farmer-level regressions described in Equation (9). Standard errors are clustered at the village level and the outcome variable is a binary variable that has been multiplied by 100. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

B Social Network Appendix

Technical Definition

It will be helpful to provide a technical defintion of a social network in for some of the following defintions. A network is a pair $G = (V, E)$. where V is a set of *nodes* and E is a set of *edges* representing connections between them. Information encoded by the set (V, E) can be summarised using an 'adjacency matrix' $A := A(G)$, made up of 0s and 1s where $A_{ij} = \mathbf{1}\{ij \in E\}$, meaning that $A_{ij} = 1$ if individuals i and j are connected.[21] This implies that networks are unweighted (links either exist or they do not) and undirected (individual i responding that they know individual j is equivalent to individual j responding that they know individual i). The elements of the adjacency matrix are determined by answers to social network questions.

Social Network Questions

At the midline I survey, respondents were asked social network questions. Respondents were asked: *"Do you know [...]?"* about 25-30 other farmers (respondents from the baseline survey). Who they were asked about was determined according to the following method.

Question Allocation Method: Suppose we sample a set of n farmers $V = \{v_1, v_2, v_3 \dots v_n\}$ from the same village. We are interested in characterising the social connections between the three of them. Two farmers are linked if one household responds to knowing another. Now, there are $\frac{(n)(n-1)}{2}$ total combinations of pairs that can be defined within this set of households:

$$(v_1, v_2), (v_2, v_1), \dots (v_{n-1}, v_n)(v_n, v_{n-1}) \tag{10}$$

If we assume that the social network is 'undirected' (i.e., $v_i \overset{link}{\rightarrow} v_j$ implies $v_j \overset{link}{\rightarrow} v_i \ \forall \ i, j \in V$), we may observe the existence of a link or non-link between every pair within the network by restricting our attention to the *unique* pairs of farmers within the network:

$$(v_1, v_2), \cancel{(v_2, v_1)}, \dots (v_{n-1}, v_n)\cancel{(v_n, v_{n-1})} \tag{11}$$

For each village:

Step 1: Obtain all pairs of farmers that are sampled. That is: $\{(v_i, v_j) | i, j \in V, i \neq j\}$

Step 2: Sort this list such that (v_j, v_i) follows $(v_i, v_j) \ \forall \ i, j \in V$.

Step 3: Obtain all the unique pairs by dropping every second pair in this list.

[21] By convention, we set diagonal elements equal to 0.

Step 4: For each unique pair, ask the first farmer whether they know the second; e.g. if (v_i, v_j) is one of the unique pairs obtained in Step 3, then ask v_i whether they know v_j.[22].

Following these steps ensures that all farmers are only asked about 25-30 (depending on the size of their village) other households, and that the survey covers all possible links between farmers sampled from the same village. If there was no attrition, this would allow us to determine the existence of social connections between all pairs of farmers in sample villages. Our definition of networks being based at the village level, being unweighted and being undirected is consistent with much of the development literature (see Cai et al., 2015).

Eigenvector Centrality

Eigenvector Centrality is a recursively defined measure where a node's centrality is proportional to the centrality of it's neighbours' centralities. It captures how well a node connects different subgroups of the network. A farmer may only have 2 friends, but those friends may be members of distinct, unconnected cultural groups. The farmer therefore, would have a high eigenvector centrality but a relatively small number of links. It is related to information diffusion because farmers with a high eigenvector centrality are likely to transmit information to otherwise unconnected members of the network. Whilst we could construct many different network centrality measures, we focus on eigenvector centrality due to it's use in other field experiments focussed on diffusing information about agricultural products amongst farmers in developing countries (Cai et al., 2015; De Janvry et al., 2017; de Janvry et al., 2019).

Table B1: Network Statistics Are
Similar Across Treatment and Control Villages

	Control	Treatment	p-value
Ave. Total Links	4.41	4.29	0.747
Ave. Eigen. Centrality	0.10	0.09	0.173
Observations	60	30	

[22]In this case, v_j would not be asked about v_i