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SOCIAL NETWORKS AND INSURANCE TAKE-UP: EVIDENCE FROM A RANDOMIZED EXPERIMENT IN CHINA[1]

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ABSTRACT

In this paper, we estimate the role of information in insurance take-up using data from a randomized experiment in rural China where information was either offered directly through financial education or accessed indirectly through social networks. Unlike previous studies, the experimental design allows to not only identify the causal effect of social networks, but also to differentiate the various channels through which they operate, including improvement of negotiating power, imitation, and social learning of insurance benefits. The results show that social networks have a large and significant effect on insurance take-up decisions. This is evidenced by the fact that households are more likely to buy the product if they have more strongly related friends who attended a village meeting that introduced the insurance contract and the benefits of purchasing it, and if their social networks include village leaders and influential farmers who attended the meeting. Moreover, we show that this effect is mainly driven by social learning of insurance benefits. The policy implication is that offering financial education to a subset of households in a village community selected for their strong friendship links with others, their recognized farming skills, and leadership roles, and relying on social networks to extend its effect on more farmers through social learning, is an effective way of improving insurance take-up.

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INTRODUCTION

When a new profitable service or technology is made available, it usually takes time for high adoption rates to occur because its characteristics and expected benefits are not easily understood by potential adopters (Evenson and Westphal, 1995). Learning needs to happen, and this can occur individually or through others. The latter can occur when the new service or technology is available to multiple people in similar circumstances, allowing people to learn its characteristics and expected benefits from each others. Individual decisions can be influenced by other people's behavior through social network effects (Foster and Rosenzweig, 1995).

There exists a vast literature on the role of social networks and social interactions in driving the adoption of technologies and financial products (Duflo and Saez, 2003; Hong et al., 2004; Conley and Udry, 2010). Identifying the social network effect on adoption is, however, challenging because it is hard to distinguish it from other factors that may give rise to similar observed outcomes such as correlated unobservable characteristics between friends (Manski, 1993). Several papers have attempted to use a variety of non-experimental econometric techniques to resolve this problem (Foster and Rosenzweig, 1995; Munshi, 2003; Bandiera and Rasul, 2006; Conley and Udry, 2010), or used experimental designs to identify the causal effects (Duflo and Saez, 2003; Miguel and Kremer, 2004; Duflo, Kremer, and Robinson, 2010; Oster and Thornton, 2009). However, there is no conclusive result from these papers. Moreover, most of the above studies have not attempted to reveal the channels through which social network effects operate, when differentiating among pathways is crucial from a policy perspective. Our paper contributes to the social network literature by using a randomized control trial approach to study the causal effect of social networks on insurance take-up, and to disentangle three possible channels through which this can occur, namely improvement of negotiating power, imitation, and social learning of product benefits.

We study the process of insurance take-up in rural China. In 2009, the People's Insurance Company of

China (PICC) started to offer a new insurance product to rice farmers in selected pilot counties. In most pilot areas, no such products had ever been offered before, so farmers and government officials at the village level had very limited understanding of how insurance works and what may be the expected benefits of purchasing one. Moreover, most households had never interacted with PICC before. Access to information and learning about the new product are thus key to adoption. In such context, social networks can play an important role. For example, farmers may learn about insurance from others who had access to more information or who have a better understanding of such products than them, or they may be influenced by other people's decisions. To test these hypotheses, we chose two pilot counties as experimental sites. The experiment was conducted in two parts. In Experiment #1, conducted in the Summer 2009, we studied the effect of social networks on insurance take-up. In Experiment #2, carried out in the Spring 2010, we analyzed the mechanisms through which social networks operate. Experiments #1 and #2 used different sets of villages that were randomly assigned to the two experiments.

Experiment #1 was designed to estimate the role of social networks in driving insurance take-up. Among the 52 experimental villages[3], we randomly selected 30 treatment villages. Within each of these villages we randomly invited a subset of households to attend village meetings at which we introduced the rice insurance program and explained the insurance contract. Several days after the village meeting, we visited door-to-door the remaining households. In control villages, all households were visited door-to-door. First, we expect that households who attended village meetings were exposed to more information and can better understand the program and the contract, and thus were more likely to purchase the product relative to households who were visited door-to-door.

Second, in a household survey, each household was asked to list the five closest friends with whom it discusses rice production or financial related matters, allowing us to identify social networks. Since invitation to village meetings was randomized at the household level, the fraction of friends invited to village meetings was random, allowing us to estimate the causal effect of social networks on take-up behavior. Moreover, we identify two types of social networks: strong social networks where two households reciprocally list the other as a friend, and weak social networks where only one household lists the other as a friend. We find that attending village meetings raises the take-up rate by around 12%, and that it has a significant spillover effect on non-invited households, which is around 7.7% and captures 70% of the meeting effect. Social networks have large and significant effects on driving adoption: having one additional listed (strongly connected) friend attending a village meeting increases your own take-up by around 4% (5.5%), which catches around 33% (50%) of the meeting effect.

Having established a role for both information and social networks in improving the insurance take-up, we then attempt to identify in Experiment #2 the channels through which social network effects operate. There are at least four possible mechanisms that drive social network effects: imitation (which can be blind or rational, whereby individuals want to act like their friends), improvement of negotiating power (farmers' expectation that they will have more negotiating power with the insurance company if they are not satisfied with payouts when more households purchased it together), informal risk sharing (individuals may be less likely to buy insurance if just a few households or most households purchased it because of existence of an informal risk-sharing network in the village), and social learning of insurance benefits (diffusion of knowledge and benefits of insurance among farmers through their social networks).

With the exception of Miguel and Kremer (2004) and Oster and Thornton (2009), most of the literature has not attempted to separate the channels through which social networks operate. However, differentiating along channels is crucial to make policy recommendations. In our case, for example, if network effects exist because farmers imitate each others, then using some marketing strategies to guarantee a high adoption rate in pilot areas could significantly improve take-up in follow-up areas; if a lack of trust in the program is the

[3] In China, the village is the smallest administrative unit. In this paper, by "village" we mean "natural village", which is a smaller unit than the administrative village. Usually a village includes around 5 to 10 natural villages and there are 30 to 50 households in each natural village.

constraining factor, improving farmers' negotiating power with the insurance company would be important; if insufficient knowledge or understanding of insurance impairs adoption, then providing financial education would be crucial; and if risk-sharing is the key mechanism of network effects, then establishing a well-developed rural financial system would be essential. In this paper, we do not consider the risk-sharing mechanism because, according to the informal risk-sharing data from the household survey, farmers usually borrow from richer relatives in urban area, rather than from households in the same village when they are hurt by natural disasters and have liquidity problems. So we do not think that this is an important driver of social network effects in this particular case.

In order to separately identify the three possible drivers of social network effects--imitation, improvement of negotiating power, and social learning of insurance benefits--, we designed Experiment #2 which includes around 170 natural villages. First, imitation includes both "blind" imitation which means that individuals just want to mimic each others, and "rational" imitation which means that individuals update their beliefs of product benefits according to other people's decisions. To identify either of these two types of imitations, we estimate the effect of other villagers' behavior--decisions made by friends within your network, influential farmers, and village leaders--, on your own take-up decision. Second, the negotiating power mechanism also means that farmers are influenced by other villager's decisions. However, in this case, farmers should only care about the total number of take-ups among other villagers, so we can identify this channel by estimating the effect of the overall take-up rate among other villagers on your own behavior. Third, we identify the role of social learning of insurance benefits by looking at whether farmers' understanding of insurance benefits and take-up rates increase after they interact with villagers who were exposed to intensive information and financial education about how insurance works and the benefits of purchasing it.

Results provide strong support to the claim that the main mechanism of social network effect in our case study is social learning of insurance benefits. Although other villagers' decisions, both the overall take-up rate in the village and decisions made by close friends, influence farmers' take-up decisions significantly if we disseminate such information to them, it made no difference if we did not explicitly reveal that information. This means that farmers could not learn about other individuals' decisions through communication with friends, allowing us to rule out a role for the imitation and negotiating power channels. In contrast, farmers' level of understanding of insurance benefits and take-up rates were significantly higher when they have more friends exposed to high levels of financial education. This suggests that social networks help increase insurance take-up through the diffusion of learning of insurance benefits. Farmers thus want to understand for themselves in deciding to adopt a new, and complex, financial product. Providing intensive financial education to a subset of households, and depending on social networks to extend its effect through the village community, thus appears to be an effective way of enhancing insurance take-up.

This paper contributes to the literature in the following ways. First, as discussed before, it contributes to the social networks literature by using randomized experiment methods to estimate the causal effects of social networks on adoption and to identify the different mechanisms through which networks operate. Second, it contributes to the insurance adoption literature. In order to reduce fluctuations in income and consumption due to negative weather shocks, rural households engage in costly ex-ante risk management strategies, such as foregoing high risk-high return agricultural activities and maintaining high levels of precautionary savings. Self-insurance through risk management is known to be a major source of continuing poverty (Morduch 1990; Rosenzweig and Binswanger 1993; Dercon 2005; Dercon and Christiaensen 2007; Elbers et al. 2007). An efficient way of reducing poverty should thus be to provide them with access to formal insurance products. However, in many countries, the use of such products is not widespread even when available (Gine et al., 2007, 2008; Cole et al., 2009). This suggests a puzzle: Why don't more households participate when formal insurance markets are available? Studying this question is crucial because the increased demand of individuals is a prerequisite for scaling up insurance markets. We provide evidence that households' lack of understanding contributes to the low demand for

insurance products. Third, this paper contributes to the financial education literature. The existing literature on financial education shows that it can affect individual decisions in developing country settings where understanding of financial products is low. For the United States, Duflo and Saez (2003) found that a benefits information fair increased enrollment in retirement plans by 1.25 percentage points after 11 months, a small effect in absolute terms. By contrast, Hastings and Tejeda-Ashton (2008) find that helping Mexican workers gain a better understanding of management fees charged by investment funds allows them to make better choices among funds in the newly privatized social security system. In a context where insurance is new, and farmers have relatively low levels of general education, our results show that lack of financial education is a major constraint on the demand for insurance, and that moderate financial training can significantly improve take-up rates. We also show that, in village environments, understanding of financial products can be acquired not only directly through formal training, but also indirectly through learning from friends and leading personalities in social networks.

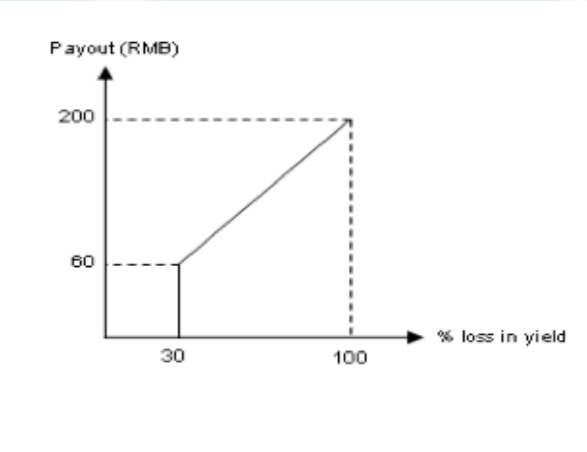
The rest of the paper is organized as follows. Section 2 describes the background and the insurance contract. Section 3 presents the experimental design and the results of experiment #1, which identifies the social network effect on insurance take-up. In section 4, we show the design and results of experiment #2 which aims at distinguishing different channels of the social network effect. Section 5 discusses policy implications and concludes.

BACKGROUND

Rice is the most important food crop in China. Nearly 50% of the farmers produce rice, and more than 60% of the Chinese people consume rice as their staple food. In order to increase food security and shield farmers from negative weather shocks, the Chinese government charged PICC in 2009 to design and offer rural households the first rice production insurance program against climatic events[4]. Our experimental sites are two rice production counties included in the first round pilots in the Jiangxi province, which is one of China's major rice bowls. In these two counties, above 80% of farmers produce rice and make it their main source of income. No households had ever purchased or heard of rice production insurance before since no such product had previously been offered. As a result, farmers had very limited knowledge of agricultural insurance products and most of them had never interacted with PICC before.

The insurance contract is as follows. The full price is 12 RMB per mu per season[5]. The government gives a 70% subsidy on the premium, so farmers only pay 3.6 RMB per mu. The insurance covers natural disasters including heavy rain, flood, windstorm, extremely high or low temperatures, and drought. If any of these disasters happened and led to 30% or more loss in yield, farmers are eligible to receive payments from the insurance company. The indemnity rule is illustrated in Figure 1 below.

Figure 1: The insurance indemnity rule



The payout amount increases linearly with the loss rate in yield, with a maximum payout of 200 RMB. The rate of loss in yield is assessed by a group of insurance agents and agricultural experts who come to the village to estimate the rice yield in different plots and calculate the loss rate[6]. Since the average gross income from

[4] Before 2009, although there was no insurance, if big natural disasters happened, governments issued subsidies to households whose production was seriously hurt. However, the level of subsidy was usually very limited and far from sufficient for farmers to restart production.

[5] 1 RMB = 0.15 US$, 1 mu = 0.067 hectare. Each year, farmers produce two or three rice crops.

[6] For example, consider a farmer whose normal yield per mu is 500kg. If, because of a windstorm, his yield decreased to 250kg per mu, then the loss rate is 50% and he is supposed to get 200*50% = 100 RMB per mu from the insurance company.

cultivating rice is between 700 RMB to 800 RMB per mu, and the production cost is around 300 RMB to 400 RMB per mu, this insurance program provides a partial insurance which covers 25 to 30% of the gross income or 50 to 70% of the production cost.

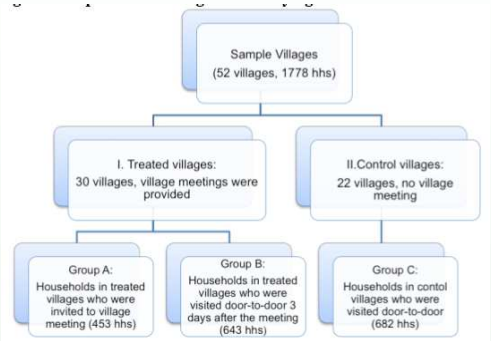
Based on historical weather data, the actual probability of disasters which can cause 30% or more loss in yield is estimated to be around 12%, so the fair price of this product, which is the price that makes the insurance company break even, should be higher than the 3.6 RMB/mu paid by farmers and lower than the 12 RMB/mu received by the insurance company[7]. As a result, PICC can earn profit and survive if the fixed cost of operating the insurance scheme is not too large, and the expected benefit of purchasing insurance is positive for farmers, implying that it is optimal for all farmers who cultivate rice to purchase it.

EXPERIMENT #¦: IDENTIFY THE EFFECT OF SOCIAL NETWORKS ON INSURANCE TAKE-UP

EXPERIMENTAL DESIGN

This experiment includes 52 villages with 1778 households[8]. The objective is to identify causal effects of social networks on insurance take-up. The experimental design is shown in Figure 2. The experiment contains two randomizations. The first is at the village level. We randomly divided villages into two groups[9]. In treated villages, we organized a village meeting to introduce the insurance program and explain the contract.

Figure 2: Experimental design to identify the effect of social networks



In control villages, a village meeting was not offered. There are 30 villages in the treatment group and 22 villages in the control group. The second randomization is at the household level[10] and is only within treated villages. In each village, we randomly invited 30% or 50% of the households to attend a village meeting[11], during which we distributed the insurance flyer, introduced the rice insurance program, explained the contract, and then asked participants to make take-up decisions. Three days after we finished the village meeting, we visited door-to-door the remaining households in treated villages who had not been invited to the village meeting. During the visit, we distributed insurance flyers, briefly introduced the contract, and then asked them to make purchase decisions. In control villages where there was no village meeting, all households were visited door-to-door.

In summary, we have three categories of households in this experiment: households in group A are those who were invited to the village meeting in treated villages, and they made decisions directly after the meeting; households in group B live in treated villages, were not invited to the meeting, were visited individually three days after the meeting, and made decisions at the end of the household visit; households in control villages belong to group C and were also visited door-to-door. For all three types of households, decisions were made separately rather than in group.

[7] The insurance company's profit from insuring 1 mu of rice equals: premium - probability of disaster * indemnity - fixed cost. In our case, probability of 30% disaster * indemnity = 12% * 200 * 30% = 7.2 RMB.

[8] Before the experiment, we first approached the village leaders to review with them the list of names we obtained from the agricultural department of the local government. Households who no longer grew rice were excluded from the sample. Those are households who abandoned the land and are working in urban areas or raising livestock for a living.

[9] The sample was stratified according to village size (total number of households), average rice production per households in the most recent year, and past disaster frequencies before we did the randomization.

[10] We stratified the sample according to village, household size, and average rice production per member in the most recent year before randomization.

[11] Village leaders were in charge of inviting farmers to the village meeting. During each meeting, a team member was responsible to record meeting attendance.

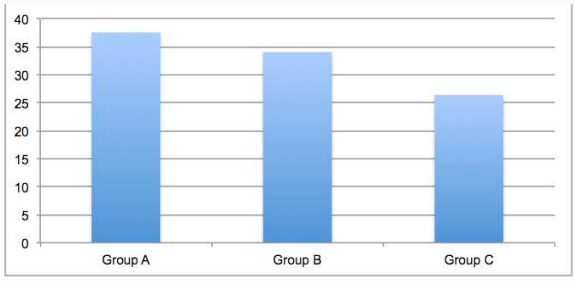
Before we started marketing the insurance, all households were asked to complete a household survey. The survey is composed of four parts: first, household background including household size, age and education of the household head, rice production, household income, etc.; second, natural disasters experienced in recent years and loss rate in yield; third, experience in purchasing any insurance and reimbursements received; fourth, social network questions which asked each household to list the five closest friends with whom they frequently discuss rice production and financial related problems.

There are three hypotheses we can test through this experimental design. First, we expect that attending a village meeting helps people better understand the program and can thus increase the take-up rate. We can test this by checking whether households in group A have a higher adoption rate than households in group C. Second, since group B and group A are people living in the same village, it is easy for those in group B to learn information from households in group A even though those in group B were not invited to the village meeting. We test the hypothesis that village meetings have a spillover effect on group B by comparing the take-up rate of those in group B to that of those in group C. [12] Third, to test the social network effect, we focus on households in group B and group C and test whether those with more friends attending village meetings are more likely to buy the insurance.

ESTIMATION STRATEGY AND RESULTS

In figure 3 below, we compare the average insurance take-up rate in the three groups of households. It shows that households in control villages (group C) have the lowest take-up rate, which is around 26%. Attending the village meeting (group A) raises the average adoption rate by 11 % points to 37%. Moreover, although households in groups B and C were provided with the same door-to-door visit, households in group B are 7% more likely to buy the insurance. This provides evidence that, in treated villages, uninvited households can obtain information from invited households, which improved their take-up rate. In other words, there is a positive spillover effect of the village meeting on uninvited households in treated villages.

Figure 3: Average take-up rate in different groups of households



In order to take into account village fixed effects and other household level controls, we test the treatment and spillover effect of village meetings by estimating the following two regressions[13]:

$$Takeup_{ij} = \alpha_0 + \alpha_1 Invitation_{ij} + \eta_j + \varepsilon_{ij} \quad (1)$$

$$Takeup_{ij} = \gamma_0 + \gamma_1 Vilmeeting_j + \varepsilon_{ij} \quad (2)$$

$Takeup_{ij}$ is an indicator of the purchase decision made by household *i* in village *j*, which takes a value of one if the household decided to buy the insurance and zero otherwise. $Invitation_{ij}$ is a dummy variable, which equals one if household *i* was invited to the meeting in village *j*.[14] $Vilmeeting_{ij}$ is also a dummy variable, which takes the value of one if a village meeting was offered in village *j*. η_j includes village dummies. Equation (1) estimates the effect of attending village meetings on insurance take-up. We expect that this can help people understand the program better and thus can improve the take-up rate, which means $\alpha_1 > 0$. Equation (2) restricts the sample to households in groups B and C to test the spillover effect of village meetings. We anticipate a positive spillover effect, which suggests $\gamma_1 > 0$.

[12] In this paper, we only consider spillover effects within villages but not across villages because usually there is a moderate distance between villages, and farmers in different villages do not interact as frequently as with farmers within the same village.

[13] We did not include household controls in these two regressions because questions about household characteristics were included in only 40% of the whole sample, since we did not start to ask these questions at the beginning of the experiment.

[14] Here we use "*invitation to the meeting*" as a proxy for "*attending the meeting*" because while invitation is randomized, households decide by themselves whether to attend it or not, which is endogenous. For most treatment villages, we had high meeting attendance rates. The average rate of attendance was around 80%.

Estimation results are given in Table 1:

Table 1: Effect of village meeting on insurance take-up

VARIABLES	Insurance take-up (1 = Yes, 0 = No)		
	(1)	(2)	(3)
Invitation to meeting	0.121**	0.0523	
(1 = Yes, 0 = No)	(0.0503)	(0.0402)	
Availability of meeting			0.0767*
(1 = Yes, 0 = No)			(0.0419)
Observations	1,135	1,096	1,325
Region fixed effects	Yes	Yes	No
R-squared	0.064	0.052	0.007

Notes: Robust clustered standard errors in parentheses. Columns (1) and (2) test the effect of attending village meetings on insurance take-up, column (1) includes group A and group C, while column (2) compares group A and group B; Column (3) restricts to households who receive door-to-door visit (group B and group C) and studies the spillover effect of village meetings to control farmers in treated villages.
*** p<0.01, ** p<0.05, * p<0.1

According to column (1), which estimates the meeting effect by restricting the sample to households in groups A and C, a village meeting has a positive effect on insurance take-up: households are 12% more likely to buy insurance if they were invited to the village meeting. However, as shown in column (2), attending village meeting does not make a significant difference within villages where a meeting was offered, by comparing treated (group A) and control (group B) households in treated villages. This is due to what column (3) tells us: there is a positive spillover effect of village meetings in treated villages. Based on the sample of households in groups B and C, the magnitude of the spillover effect is around 7.7%, which catches about 70% of the village meeting effect.

Turning to estimations of social network effects, we use the following two equations[15]:

$$Takeup_{i,j} = \beta_0 + \beta_1 Network_{i,j} + \eta_j + \varepsilon_{i,j} \quad (3)$$

$$Takeup_{i,j} = \beta'_0 + \beta'_1 Network\_strong_{i,j} + \eta_j + \varepsilon_{i,j} \quad (4)$$

Table 2.1 on the next page gives estimation results for equations (3) and (4). Columns (1) and (2) tell us that there is no significant network effect during the meeting However, according to columns (3) and (4), households who were visited door-to-door are significantly influenced by the number of close friends who were invited to the meeting. Look at column (3) first: the coefficient of the network measure is 0.196, which means having one additional closely related friend attending the village meeting (which raises the network measure by 20% because each household lists five friends) increases your own take-up rate by 0.196*20%, which is around 4%. Similarly, according to column (4), having one additional strongly connected friend attending a village meeting raises your own take-up rate by around 6%, which catches around 50% of the effect of having yourself attended a village meeting (equal to 12.1% in column (1) of Table 1).

In addition, we consider whether the magnitude of the social network effect varies with the social status of your friends. Here we consider two types of social status: village leaders, and influential farmers who are the opinion leaders and most respected persons in the village. Results are given in Table 2.2. It is clear that village leaders and influential farmers can affect uptake decisions more than friends, with the strongest influence coming from village leaders. This result is important in using social networks to maximize adoption spillover effects as it indicates the benefit of including these personalities in the village meetings where intensive training is provided.

[15] In the estimation of social network effects, we do not need to control for the size of the network because each household was asked to list 5 friends only. A very small proportion of people (around 3%) listed less than 5.

Table 2.1: Effect of social network on insurance take-up

VARIABLES	Insurance take-up (1 = Yes, 0 = No)			
	Invited households		Door-to-Door visit households	
	(1)	(2)	(3)	(4)
Fraction of network in village meeting	0.00371		0.196***	
	(0.103)		(0.0720)	
Fraction of network in village meeting (strong)		0.0466		0.314**
		(0.183)		(0.153)
Observations	418	418	1,360	1,360
Region fixed effects	Yes	Yes	Yes	Yes
R-squared	0.095	0.095	0.062	0.060

Notes: Robust clustered standard errors in parentheses. Columns (1) and (2) use households who were invited to village meetings, and columns (3) and (4) are based on sample of households who were visited door-to-door and estimate the social network effect. Columns (1)(3) and columns (2) (4) differs in the definition of social network: column (1) (3) defines social network as the fraction of listed closely related friends who were invited to the village meeting, while clumn (2) (4) defines it as the fraction of mutually listed friends who were invited to the village meeting.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.2 Effect of social network on insurance take-up -- Farmers with special social status

VARIABLES	Insurance take-up (1 = Yes, 0 = No)	
	(1)	(2)
Fraction of village leaders in social network	0.963***	
& have been invited to village meeting	(0.243)	
Fraction of influential farmers in social network		0.476***
& have been invited to village meeting		(0.153)
Observations	1,360	1,360
Region fixed effects	Yes	Yes
R-squared	0.066	0.062

Notes: Robust clustered standard errors in parentheses. Estimations are based on the sample of households who were not invited to village meetings. Independent variable of column (1) is the fraction of a household's friends who are village leaders and have been invited to the meeting, and that of column (2) is the fraction of a household's friends who are opinion leaders of the village and have been invited to the meeting.
*** p<0.01, ** p<0.05, * p<0.1

EXPERIMENT #2: IDENTIFY THE CHANNELS OF SOCIAL NETWORK EFFECTS

The results above show that information and social networks have large and significant effects on insurance take-up. But what are the mechanisms through which social networks operate? As discussed before, separating different channels of network effects is crucial from a policy perspective. In general, social networks may matter for technology or financial product adoption because individuals tend to imitate each other's[16](Banerjee, 1992; Rogers, 1995; Ellison and Fudenberg, 1993; Bandiera and Rasul, 2006); because people learn about how to use the technology from their friends (Duflo and Saez 2003; Munshi and Myaux 2006; Miguel and Kremer 2007, Oster and Thornton 2009); or because social networks affect individual perceptions about the values or benefits of a product (Kohler, Behrman, and Watkins, 2001; Miguel and Kremer, 2007; Oster and Thornton, 2009).

In our case, since insurance is a financial product rather than a technology, people do not need to learn how to use it, and thus we do not have to consider the "learn-to-use" channel. However, there are two additional mechanisms we need to consider in the insurance context. First, social networks may influence individual choices by affecting households' negotiating power with the insurance company in case they are not

[16] Imitation includes both blind imitation, which means that individuals just want to act like the others, and rational imitation, which suggests that individuals update their beliefs of product benefits according to other people's behavior. In this paper, we do not attempt to distinguish between these two types of imitations.

satisfied with the loss rate determined or the payout received. This mechanism is especially important in our case because farmers are a relatively weak social group in China, and it is in general very difficult for a single farmer to argue with a big insurance company or a government agency. As a result, farmers are less likely to buy the insurance if no one else in the village purchased it. Second, social networks may affect farmers' decisions because they provide informal risk sharing, which means that farmers can borrow from each other's if they have liquidity problems. From this perspective, individuals are less likely to buy the insurance if only a few others purchased it because they may worry that, if disaster happened and they got a payout, other uninsured farmers would ask to borrow from them; similarly, individuals are less likely to buy the insurance if most other villagers purchased it because they could borrow from insured farmers in case disaster happened. However, we do not consider the risk sharing mechanism here because, according to our household survey, there is not much informal risk sharing within villages. Farmers usually borrow from their richer relatives in urban areas if they are short of money[17].

In summary, we consider the following three important mechanisms of social network effect: imitation, negotiating power, and social learning of insurance benefits. Before we go on to the experimental design, we discuss the strategies used to identify each of these three mechanisms.

First, if it is imitation that drives the network effect, then it means that farmers' decisions are affected by other villagers' behavior, especially by that of those within their own social network,[18] or by influential farmers and village leaders. In order to identify this channel, we need to estimate the effects of four types of take-up rates on your own adoption decision: the overall take-up rate among other villagers, the take-up rate of friends listed in your social network, and the take-up rate of influential farmers and village leaders.

Second, if social networks operate through improving farmers' negotiating power with the insurance company, then we should see that farmers are more likely to buy the insurance if they observe that more other villagers purchased it. In other words, in this case, what farmers care about is the total number or the fraction of households in their village that purchased the product. Consequently, we can identify this channel by estimating the effect of the overall take-up rate among other villagers on your own behavior[19].

Third, if social networks are important when farmers make decisions because individuals improved their understandings of how insurance works and the benefits of purchasing it by learning from people who were exposed to more intensive financial training than them, then it should be reflected in a better understanding of insurance benefits among farmers who have more interactions with people who received more information and have greater insurance knowledge. Empirically, we can identify this effect by testing whether farmers' understanding of insurance benefits is higher after they have interacted with villagers who received more financial education.

EXPERIMENTAL DESIGN

This experiment includes 173 villages and around 5,000 households. The objective is to separately identify each of the three channels of social network effect discussed above. The experimental design is shown in Figure 4.1 and 4.2. In each village, we held two rounds of sessions to introduce the rice insurance program, S1 and S2. The second round sessions were held 1 to 3 days after we finished the first round. During each round, there were two sessions, one simple (T1 and T3) and one intensive (T2 and T4).The simple session took around 20 minutes, during which we only introduced the contract, including information

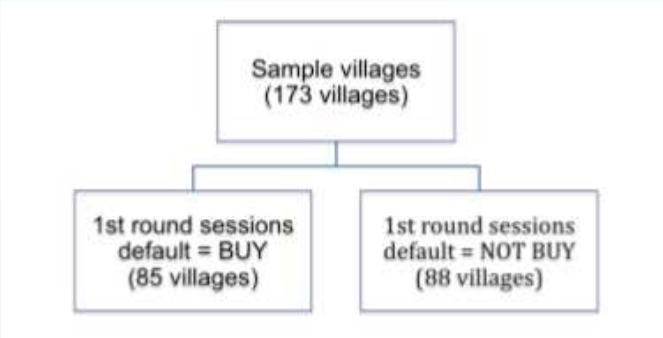
[17] We randomly selected around 50 households in the sample households in experiment #2 to ask the following question: Who are the five persons that you will first approach to borrow from if you are short of money for agricultural production, investment, etc.? List their names, addresses, and relationships with you. We found that 86% of persons that they listed are not within the same village, while 75% of them are living in the county or city.

[18] The existing literature suggests that people are more likely to imitate individuals who have characteristics similar to theirs or who are experts in related areas.

[19] We need to estimate the effect of the overall take-up rate among other villagers on your own behavior when identifying either the imitation or the negotiating power channels. However, this does not mean that we cannot separate these two channels. To support the imitation channel, we need significant effects of both the overall take-up rate among other farmers and the take-up rate of special farmers or people in your social network, while for the negotiating power channel we only need to verify whether the overall take-up has a significant effect on your own decision.

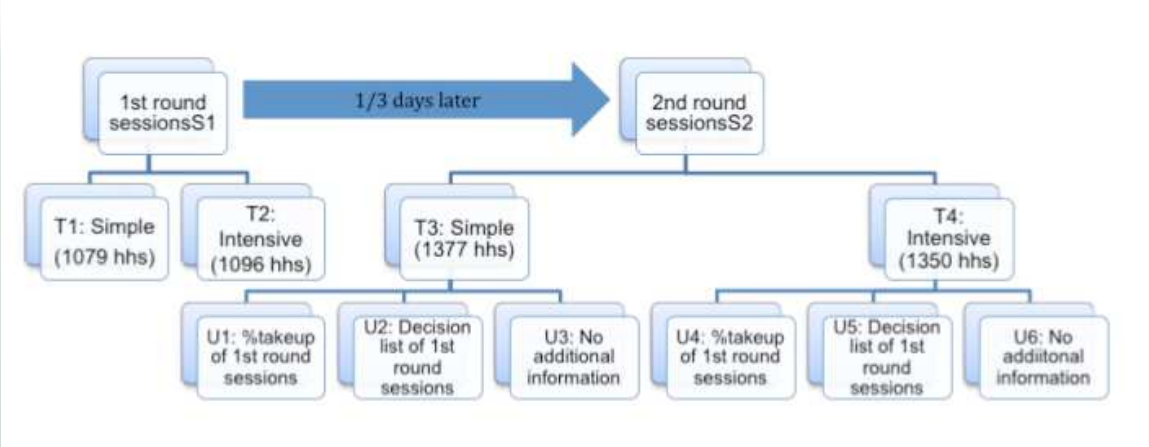
on the insurance premium, the amount of subsidy provided by the government, the responsibility of the insurance company, the maximum pay-out, the period of responsibility, the rules of loss checking, and the procedures for making pay-outs.

Figure 4.1 Experimental design to identify the channels of social network effects (village level randomization)



The intensive session took around 45 minutes and covered all information provided during simple sessions, plus several concrete examples to explain the following questions: What is a policy-oriented agricultural insurance product? How does it differ from commercial insurance products? How does this insurance program differ from a government subsidy? How to calculate the pay-out that you can get under different situations? What benefits can farmers get from purchasing insurance? How to calculate the expected benefit of taking the insurance to see if you can gain or lose from purchasing it, etc.

Figure 4.2 Experimental design to identify the channels of social network effects (household level randomization)



There are three randomizations in this experiment as shown in Figures 4.1 and 4.2. First, within each village, households were randomly assigned to one of the four sessions.[20] Second, at the village level, we randomized

[20] In each village, we invited household heads to attend one of the four sessions. No one could attend more than one session. For all household-level randomizations in this experiment, we stratified the sample according to village size, household size, and average rice production per member in the most recent year before randomization. Similar to what we did in Experiment #1, we asked village leaders to inform and invite household heads to attend these sessions in order to achieve a high attendance rate.

the default options in first-round sessions.[21] If the default is BUY, then you needed to sign off if you did not want to purchase the insurance,[22] while if the default was NOT BUY, then you had to sign on if you decided to buy the insurance. Default options were the same in the two first-round sessions within each village. According to the existing literature (Laibson et al., 2008), default options can significantly influence households' financial decisions[23]. The objective of using different default options was thus to generate exogenous variations in the take-up rate in first-round sessions across villages. Third, for each second-round session, after the presentation and before participants made final decisions, we randomly divided them into three groups, led different groups to separate rooms, and then disseminated different additional information to the different groups of participants. As shown in Figure 4.2, we told farmers in groups U1 and U4 what had been the overall take-up rate at the two first-round sessions held in their village, so they knew the number of people who attended previous sessions and how many of them had purchased the insurance. In groups U2 and U5, we told farmers the overall take-up rate and showed them the detailed decision list at the two first-round sessions, so they knew specifically who purchased insurance and who did not., In groups U3 and U6, we did not give farmers any additional information and directly asked them to make decisions. In all cases, households made take-up decisions separately rather than in group.

Similar to what we did in Experiment #1, all households were asked to respond to a household survey. The survey is mainly composed of six parts: first, household background including household size, age and education of the household head, rice production and sales, household income, borrowing, etc.; second, natural disasters experienced in recent years and loss rate in yield; third, experience in purchasing any insurance and reimbursement received; fourth, risk attitude and perception of future disasters; fifth, ten questions which test farmers' understanding of information provided during sessions; and sixth, general and detailed social network questions which ask each household to rank and list five of their most closely related friends with whom they frequently discuss rice production and financial related problems, and ask what specific topics do they usually discuss with each of these five friends.

The three channels through which social network effects occur were identified as follows. First, to identify the negotiating power channel, we estimated the effect of the first-round overall take-up rate on second-round decisions. Specifically, if the first-round take-up rate influences second-round decisions significantly when we disseminate information about it (groups U1, U2, U4, and U5), then it means that farmers actually care about other people's behavior when they make their own decisions; if the effect is still significant when we do not explicitly disseminate that information (groups U3 and U6), it means that such information can be diffused through everyday communication among farmers, so that we cannot rule out negotiating power as one of the network effect mechanisms.

Second, to identify the imitation channel, we focus on groups U2, U3, U5, and U6 to estimate the effect of the first-round overall take-up rate, first round take-up rate of friends in their social network, and take-up rate by influential farmers and village leaders on second round individuals' take-up behavior. If at least the behavior of friends within social networks or of individuals with special status matter, regardless of whether we explicitly reveal their take-up information or not, then we cannot rule out the existence of an imitation channel.

Third, to test whether the effect of social networks operates through social learning of insurance benefits, we first compare levels of understanding derived from sessions between groups T1 and T2: if T2 performs

[21] The sample was stratified according to village size (total number of households), average rice production per household in the most recent year, and past disaster frequencies before we did the randomization.

[22] During sessions where default = BUY, before we asked farmers to make decisions, we told them the following: "We think that this is a very good insurance product, and we believe that most farmers will choose to buy it, so it is more convenient for us to record who does not buy it rather than who buys it. So if you decided to buy it, there is nothing you need to do, the premium will be deducted from your agricultural card automatically; if you do not want to buy it, then please come here and sign."

[23] The reason why default options influence households' financial decisions can be because households found it too complex to make a decision by themselves, or they think the option is set as the default because it is a good choice. For more details, see Laibson et al. (2008).

significantly better than T1, then it means that the extra financial education we provided in T2 works. Then we compare the same thing between groups T3 and T4: if financial education still matters a lot in second-round sessions, then financial knowledge cannot be sufficiently transmitted through social networks, which rules out social learning as the main channel of network effects. Otherwise, if financial education makes no difference in T3 and T4, and performance in answering understanding questions of T3 and T4 is very similar to that of T1 and T2 then it suggests that, during the time interval between the two rounds of sessions, farmers were able to learn from early round participants the information provided during intensive sessions and we cannot rule out the third mechanism.

ESTIMATION STRATEGIES AND RESULTS

Chanel 1: Negotiating power

We estimate the following equation to test the negotiating power channel:

$$Takeup_{ij} = \beta_0 + \beta_1 TakeupRate_j + \beta_2 X_{ij} + \eta_j + \epsilon_{ij} \quad (5)$$

$Takeup_{ij}$ is an indicator of the purchase decision made by household i in village j, which takes a value of one if the household decided to buy the insurance and zero otherwise. $TakeupRate_j$ is the rate of take-up in first-round sessions in village j, which is a continuous variable ranging from 0 to 1. X_{ij} and η_j are defined as before.

The hypothesis here is that individuals are more likely to purchase insurance if they see higher take-up rates in previous sessions, which implies $\beta_1 > 0$. However, OLS estimation cannot give us a consistent estimate of β_1 because $TakeupRate_j$ is clearly endogenous. Unobservable variables such as social norms may affect both $TakeupRate_j$ and $Takeup_{ij}$. Since we expect that the randomization of default options in first-round sessions should induce some exogenous variation in take-up rate, we use it as an instrumental variable for the first round take-up rate after verifying its validity by estimating the following equation based on the sample of first round participants:

$$Takeup_{ij} = \beta_0' + \beta_1' Default_j + \beta_2' X_{ij} + \epsilon_{ij}, \quad (6)$$

where $Default_j$ is a dummy variable which equals one if the default option assigned to first-round sessions in village j is BUY and zero otherwise.

The results of estimating equation (6) are given in Table 3. According to column (1), offering default options creates substantial exogenous variation in the take-up rate of first-round sessions. The average take-up rate of "default = BUY" sessions is around 12% higher than that of "default = NOT BUY" sessions. As seen in column (2), adding additional household controls does not influence the magnitude and significance of this estimate. As a result, we can use default options in first-round sessions as the IV for first-round take-up rates. The OLS and IV estimation results of equation (5) are presented in Table 4. Columns (1) and (2) are based on second-round participants to whom we disseminated first-round take-up information (groups U1, U2, U4, and U5). They show that the first-round take-up rate has a significantly positive effect on second-round decisions if we disseminate this information.

Specifically, increasing the first-round take-up rate by 10% and revealing it to second-round participants can raise their take-up rate by around 4.5%, which is almost half of the first-order effect. However, if we look at the second-round individuals to whom we did not reveal that information, as in columns (3) and (4), the decisions made by early participants do not have any significant effect on second-round behavior. This tells us that, although previous take-up information is important for later participants to make their own decisions, they cannot get this information through normal communication with each other's. Consequently, it is not likely that individuals are able to judge whether they have enough negotiating power with the insurance company by observing at the number of other villagers who took the insurance. This means that we can rule out negotiating power as a channel for social network effects.

Table 3: Effect of default options on 1st round insurance take-up

VARIABLES	Insurance take-up (1 = Yes, 0 = No)	
	(1)	(2)
Default (1 = Buy, 0 = Not buy)	0.121***	0.121***
	(0.0328)	(0.0326)
Male		0.0370
		(0.0490)
Age		0.00202*
		(0.00107)
Household Size		-0.00434
		(0.00515)
Rice production area (mu)		0.00159
		(0.000972)
Illiteracy		-0.0868***
		(0.0265)
No. of Observation	2,175	2,137
Region fixed effects	Yes	Yes
R-squared	0.110	0.120

Notes: Robust clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Effect of disseminating 1st round take-up information on 2nd round decision

VARIABLES	Insurance take-up (1 = Yes, 0 = No)			
	(1)	(2)	(3)	(4)
	Info = 1st round takeup info		Info = none	
	OLS	IV	OLS	IV
1st round take-up rate	0.503***	0.445*	0.0278	0.0599
	(0.0579)	(0.238)	(0.0855)	(0.328)
Male	0.0412	0.0412	0.0260	0.0263
	(0.0535)	(0.0536)	(0.0688)	(0.0694)
Age	0.00484***	0.00484***	0.00406***	0.00406***
	(0.00133)	(0.00132)	(0.00122)	(0.00122)
Household Size	-0.00206	-0.00206	-0.00964	-0.00975
	(0.00723)	(0.00715)	(0.00713)	(0.00725)
Rice production area	0.00146***	0.00146***	0.00166	0.00163
	(0.000549)	(0.000556)	(0.00137)	(0.00138)
Illiteracy	-0.0919**	-0.0919**	-0.0971***	-0.0966***
	(0.0366)	(0.0365)	(0.0331)	(0.0331)
Intensive	0.00877	0.00878	0.0102	0.0103
(1 =Yes, 0 = No)	(0.0276)	(0.0272)	(0.0314)	(0.0314)
No. of Observation	1,378	1,378	1,296	1,296
Region fixed effects	Yes	Yes	Yes	Yes
R-squared	0.116	0.116	0.075	0.075

Notes: Robust clustered standard errors in parentheses.
Estimations in this table are based on the sample of 2nd round session participants. Columns (1) and (2) are based on the subgroup of households to whom we desseminate the first round take-up information; Columns (3) and (4) are based on the sub-sample who receive no extra information in addition to the presentation. In IV estimations, Default options are used as the instrumental variable for the first round take-up rate. *** p<0.01, ** p<0.05, * p<0.1

CHANNEL 2: IMITATION

To identify the imitation mechanism empirically, we not only need to test the effect of overall previous take-up rates on follow-up participants, but also the effect of first-round take-up among special groups of people, including friends in people's network, influential farmers, and village leaders. We have already shown in the last section that there was no natural diffusion of the overall first-round take-up rate, so here we only need to test the influence of decisions made by one's friends, influential farmers, and village leaders on your own behavior.

Consider first the impact of decisions made by friends in your social networks first. We use two specifications to estimate this channel. The first is:

$$Takeup_{ij} = \gamma_0 + \gamma_1 TakeupRate_j + \gamma_2 TakeupRateNetwork_{ij} + \gamma_3 X_{ij} + \eta_j + \epsilon_{ij} \quad (7)$$

While $TakeupRate_j$ is the rate of take-up in first-round sessions in village j, the variable $TakeupRateNetwork_{ij}$ represents the take-up rate among friends listed by household i who attended first-round sessions in village j. The hypothesis is that individuals tend to imitate their closely related friends when they make purchase decisions.

Table 5: Effects of 1st round result on 2nd round take-up (network, revealed 1st round info)

VARIABLES	Network 1st round take-up%	Insurance take-up (1 = Yes, 0 = No)			
	(1)	(2)	(3)	(4)	(5)
		OLS	IV	OLS	IV
1st round take-up rate		0.786***	0.460	0.721***	-0.555
		(0.120)	(0.790)	(0.171)	(0.937)
1st round take-up rate (network)		-0.0162	0.969**		
		(0.0536)	(0.383)		
1st round take-up rate				0.0122	2.295*
* Fraction of network in 1st round				(0.305)	(1.335)
Fraction of network in 1st round				0.212	-0.875
				(0.188)	(0.665)
Default * Fraction of network in 1st round	0.377***				
	(0.0598)				
Fraction of 1st round network	0.0923*				
in intensive session	(0.0483)				
Male	0.0285	0.0699	0.0293	0.0473	0.0392
	(0.0559)	(0.0802)	(0.0732)	(0.0784)	(0.0781)
Age	-0.000422	0.00511**	0.00652***	0.00453**	0.00503**
	(0.00135)	(0.00197)	(0.00244)	(0.00190)	(0.00205)
Household Size	0.00511	0.00529	-0.00793	0.00186	0.00163
	(0.00727)	(0.00987)	(0.0108)	(0.00971)	(0.0102)
Rice production area (mu)	0.00153	0.000743	0.00178	0.000882	0.00119
	(0.00102)	(0.00169)	(0.00162)	(0.00170)	(0.00191)
Illiteracy	0.0354	-0.0702	-0.102	-0.0796	-0.0745
	(0.0438)	(0.0546)	(0.0628)	(0.0520)	(0.0547)
Intensive (1 =Yes, 0 = No)	-0.0148	0.0412	0.0556	0.0387	0.0533
	(0.0326)	(0.0395)	(0.0511)	(0.0390)	(0.0413)
P-value of joint significance: 1st round take-up rate & 1st round take-up rate * Fraction of network in 1st round				0	0.0325
No. of Observation	610	660	610	689	689
Region fixed effects	Yes	Yes	Yes	Yes	Yes
R-squared	0.228	0.172		0.176	0.102

Notes: Robust clustered standard errors in parentheses.
Estimations in this table are based on a sub-sample of the second round participants who were provided with the decision list of 1st round session. Column (1) verifies whether variables Default * network% in 1st round and fraction of friends in 1st round who were assigned to the intensive sessions can work as a valid IV for the 1st round take-up rate among social network; Columns (2)-(3) and columns (4)-(5) use two different specifications to test whether farmers are more influenced by people in their social network. Column (3) uses Default as IV for 1st round take-up rate, and uses Default*network% in 1st round and fraction of 1st round network in intensive sessions as IV for 1st round take-up rate among social network; Column (5) uses Default * network% in 1st round as IV for 1st round take-up rate * network% in 1st round.
*** p<0.01, ** p<0.05, * p<0.1

Similar to what we discussed in the last section, both $TakeupRate_j$ and $TakeupRateNetwork_{ij}$ are endogenous here. Since we have already proved that default options in first-round sessions can be good IVs for $TakeupRate_j$, we only need to find IVs for the take-up rate among network members. We propose two IVs:

Default*Fraction of network in first-round sessions (first round take-up rates matter more to you if more of your friends are included), and Fraction of first-round network in intensive sessions (since we expect that intensive sessions raise the take-up rate relative to simple sessions, your friends included in first-round sessions should have a higher overall take-up rate if more of them were assigned to the intensive session).

Table 6: Effect of 1st round result on 2nd round decisions (network, no info revealed)

VARIABLES	Network 1st round take-up%	Insurance take-up (1 = Yes, 0 = No)			
	(1)	(2)	(3)	(4)	(5)
		OLS	IV	OLS	IV
1st round take-up rate		-0.0492	0.136	0.147	0.0728
		(0.0981)	(0.425)	(0.114)	(0.375)
1st round take-up rate (network)		0.0157	0.104		
		(0.0478)	(0.253)		
1st round take-up rate				-0.429	0.677
* Fraction of network in 1st round				(0.285)	(1.185)
Fraction of network in 1st round				0.279*	-0.235
				(0.163)	(0.575)
Default * Fraction of network in 1st round	0.137*				
	(0.0795)				
Fraction of 1st round network	0.147***				
in intensive session	(0.0466)				
Male	-0.0407	-0.0110	-0.0124	0.0208	0.0252
	(0.0621)	(0.0874)	(0.0935)	(0.0688)	(0.0700)
Age	-0.000669	0.00473***	0.00559***	0.00342***	0.00351***
	(0.00130)	(0.00134)	(0.00134)	(0.00122)	(0.00126)
Household Size	2.34e-06	-0.00890	-0.0116	-0.00820	-0.00941
	(0.00654)	(0.00729)	(0.00774)	(0.00716)	(0.00743)
Rice production area (mu)	-9.23e-05	0.000977	0.00408***	0.00161	0.00151
	(0.00104)	(0.00158)	(0.00128)	(0.00137)	(0.00141)
Illiteracy	-0.0223	-0.110***	-0.107***	-0.102***	-0.0994***
	(0.0373)	(0.0368)	(0.0364)	(0.0324)	(0.0330)
Intensive (1 =Yes, 0 = No)	-0.00303	0.00925	0.0215	0.0170	0.0145
	(0.0247)	(0.0341)	(0.0348)	(0.0312)	(0.0311)
P-value of joint significance: 1st round take-up rate & 1st round take-up rate * Fraction of network in 1st round				0.3017	0.7537
No. of Observation	920	983	920	1,280	1,280
Region fixed effects	Yes	Yes	Yes	Yes	Yes
R-squared	0.188	0.077	0.111	0.078	0.059

Notes: Robust clustered standard errors in parentheses.
Estimations in this table are based on a sub-sample of the second round participants who received no additional information except for the presentation. Column (1) verifies whether variables Default * network% in 1st round and fraction of friends in 1st round who were assigned to the intensive sessions can work as a valid IV for the 1st round take-up rate among social network; Columns (2)-(3) and columns (4)-(5) use two different specifications to test whether farmers are more influenced by people in their social network. Column (3) uses Default as IV for 1st round take-up rate, and uses Default*network% in 1st round and fraction of 1st round network in intensive sessions as IV for 1st round take-up rate among social network; Column (5) uses Default * network% in 1st round as IV for 1st round take-up rate * network% in 1st round.
*** p<0.01, ** p<0.05, * p<0.1

To further support these results, we use a second specification to test that it is friends' decisions that matter . To further support these results, we use a second specification to test that it is friends' decisions that matter:

$$Takeup_{ij} = \sigma_0 + \sigma_1 TakeupRate_j + \sigma_2 Network_{ij} + \sigma_3 Takeuprate_j * Network_{ij} + \sigma_4 X_{ij} + \eta_j + \epsilon_{ij}, \quad (8)$$

where $Network_{ij}$ is the fraction of the five friends listed by household i who were assigned to first-round sessions. If the coefficient of the interaction term is positively significant, it supports the argument that farmers care more about decisions made by their own friends. Estimation results are shown in columns (4) and (5) of Tables 5 and 6, and they are consistent with the results obtained using the first specification. Similarly, we estimate the effect of influential farmers' decisions by the following equation:

$$Takeup_{ij} = \gamma_0 + \gamma_1 TakeupRate_j + \gamma_2 TakeupRateNetwork_{ij} + \gamma_3 TakeupRateInfluential_j + \gamma_4 X_{ij} + \eta_j + \epsilon_{ij} \quad (9)$$

where $TakupRateInfluential_j$ is the take-up rate among influential farmers in village j who attended one of the first-round sessions. We use the same instrumental variables for $TakeupRate_j$ and $TakeupRateNetwork_{ij}$ as when we estimated equation (7). For the variable $TakeupRateInfluential_j$, we use Default*% of influential farmers in 1st round sessions as the IV.

Table 7: Effect of desseminating 1st round results on 2nd round decisions (influential farmers)

	Info = 1st round decision list			Info = none		
VARIABLES	Influential farmers 1st round take-up%	Insurance take-up (1 = Yes, 0 = No)		Influential farmers 1st round take-up%	Insurance take-up (1 = Yes, 0 = No)	
	(1)	(2) OLS	(3) IV	(4)	(5) OLS	(6) IV
1st round take-up rate		0.717***	0.707		0.0178	0.637
		(0.167)	(1.220)		(0.135)	(0.722)
1st round take-up rate		-0.00669	0.950**		0.0185	0.108
(Social networks)		(0.0580)	(0.404)		(0.0507)	(0.348)
1st round take-up rate		-0.00679	0.369		-0.0642	-0.0319
(Influential farmers)		(0.0874)	(0.388)		(0.0724)	(0.336)
Default*% of influential	0.418***			0.418***		
farmers in 1st round	(0.127)			(0.113)		
Male	-0.0727	0.0635	-0.00404	-0.00552	0.0205	0.0512
	(0.0699)	(0.0995)	(0.0927)	(0.0698)	(0.0927)	(0.121)
Age	0.000207	0.00474**	0.00554*	0.000276	0.00544***	0.00586***
	(0.00160)	(0.00221)	(0.00282)	(0.00129)	(0.00164)	(0.00202)
Household Size	-0.00247	-0.00267	-0.0182	0.0118*	-0.0186**	-0.0198**
	(0.00998)	(0.0111)	(0.0137)	(0.00681)	(0.00777)	(0.00972)
Rice production area (mu)	0.00148	0.000370	0.00212	0.00389**	0.00231	0.00463***
	(0.00186)	(0.00242)	(0.00238)	(0.00152)	(0.00159)	(0.00152)
Illiteracy	-0.0885	-0.0491	-0.0547	-0.00379	-0.111***	-0.106**
	(0.0588)	(0.0635)	(0.0809)	(0.0337)	(0.0414)	(0.0420)
Intensive (1 =Yes, 0 = No)	0.0300	0.0184	-0.0301	-0.0115	0.0437	0.0518
	(0.0238)	(0.0450)	(0.0602)	(0.0166)	(0.0383)	(0.0406)
No. of Observation	523	504	463	985	761	718
R-squared	0.098	0.180	0.063	0.113	0.099	0.078

Notes: Robust clustered standard errors in parentheses.
Estimations in columns (1) to (3) are based on a sub-sample of the second round participants who were provided with the decision list of 1st round session, while those in columns (4) to (6) are based on a sub-sample of the second round participants who received no additional information except for the presentation. Column (1) and (4) verifies whether variables Default* influential farmers% in 1st round can work as a valid IV for the 1st round take-up rate of influential farmers; Columns (2) and (5) use OLS estimation, and columns (3) and (6) use IV estimation, using Default as IV for 1st round take-up rate, and uses Default*influential farmers% in 1st round as IV for 1st round take-up rate among influential farmers.
*** p<0.01, ** p<0.05, * p<0.1

Finally, we check the influence of village leaders' behaviors in equation 10 above:

$$Takeup_{ij} = \gamma'_0 + \gamma'_1 TakeupRate_j + \gamma'_2 TakeupRateLeaders_j + \gamma'_4 X_{ij} + \eta_j + \epsilon_{ij} \quad (10)$$

Where $TakupRateLeaders_j$ is the take-up rate among village leaders in village j who attended one of the first-round sessions. We use Default*% of village leaders in 1st round sessions and % of village leaders in 1st round intensive sessions as the IVs. Results are reported in Table 8.

Table 8: Effect of desseminating 1st round result on 2nd round decision (village lenders)

	Info = 1st round decision list			Info = none		
VARIABLES	Village leaders 1st round take-up%	Insurance take-up (1 = Yes, 0 = No)		Village leaders 1st round take-up%	Insurance take-up (1 = Yes, 0 = No)	
	(1)	(2)	(3)	(4)	(5)	(6)
		OLS	IV		OLS	IV
1st round take-up rate		0.652***	-0.0747		0.157	0.316
		(0.122)	(0.844)		(0.102)	(0.690)
1st round take-up rate		0.107	0.441*		0.0686	-0.266
(Village leaders)		(0.0729)	(0.230)		(0.0488)	(0.255)
Default * % of village leaders	0.296**			0.290**		
in 1st round	(0.145)			(0.144)		
% of village leaders	0.303**			0.226*		
in 1st round intensive session	(0.127)			(0.132)		
Male	0.00790	0.0210	0.00210	0.0461	0.0463	0.0662
	(0.0747)	(0.0998)	(0.112)	(0.0773)	(0.0665)	(0.0923)
Age	-0.000343	0.00633***	0.00631**	-0.00114	0.00316**	0.00304*
	(0.00172)	(0.00215)	(0.00249)	(0.00166)	(0.00152)	(0.00175)
Household Size	0.00104	-0.00386	-0.00622	-0.00203	-0.0181*	-0.0181*
	(0.0106)	(0.0110)	(0.0114)	(0.00760)	(0.00940)	(0.0105)
Rice production area (mu)	-0.00127	0.00370**	0.00467***	0.000621	0.00373***	0.00359**
	(0.00213)	(0.00161)	(0.00169)	(0.00209)	(0.00130)	(0.00152)
Illiteracy	0.0617	-0.0832	-0.0809	0.0732**	-0.0668	-0.0446
	(0.0663)	(0.0695)	(0.0843)	(0.0353)	(0.0467)	(0.0535)
Intensive (1 =Yes, 0 = No)	-0.00423	0.00955	0.0270	-0.0278	0.0475	0.0386
	(0.0258)	(0.0503)	(0.0528)	(0.0196)	(0.0395)	(0.0418)
Observations	385	385	385	722	722	722
R-squared	0.110	0.146	0.014	0.071	0.035	

Notes: Robust clustered standard errors in parentheses.
Estimations in columns (1) to (3) are based on a sub-sample of the second round participants who were provided with the decision list of 1st round session, while those in columns (4) to (6) are based on a sub-sample of the second round participants who received no additional information except for the presentation. Column (1) and (4) verifies whether variables Default* leaders% in 1st round and % of village leaders in 1st round intensive session can work as valid IVs for the 1st round take-up rate of village leaders; Columns (2) and (5) use OLS estimation, and columns (3) and (6) use IV estimation, using Default as IV for 1st round take-up rate, and uses Default*village leaders% in 1st round and % of village leaders in 1st round intensive session as IV for 1st round take-up rate among them.
*** p<0.01, ** p<0.05, * p<0.1

Note that because of the missing values in $TakupRateLeaders_j$, we dropped almost half of the observations, so this set of result is only presented as suggestive evidence. It shows that the impact of first-round decisions made by village leaders is more significant and larger than that of overall first-round take-up rates (column (3)). However, as shown in columns (5) and (6), the effect is not significant if we did not explicitly reveal such information.

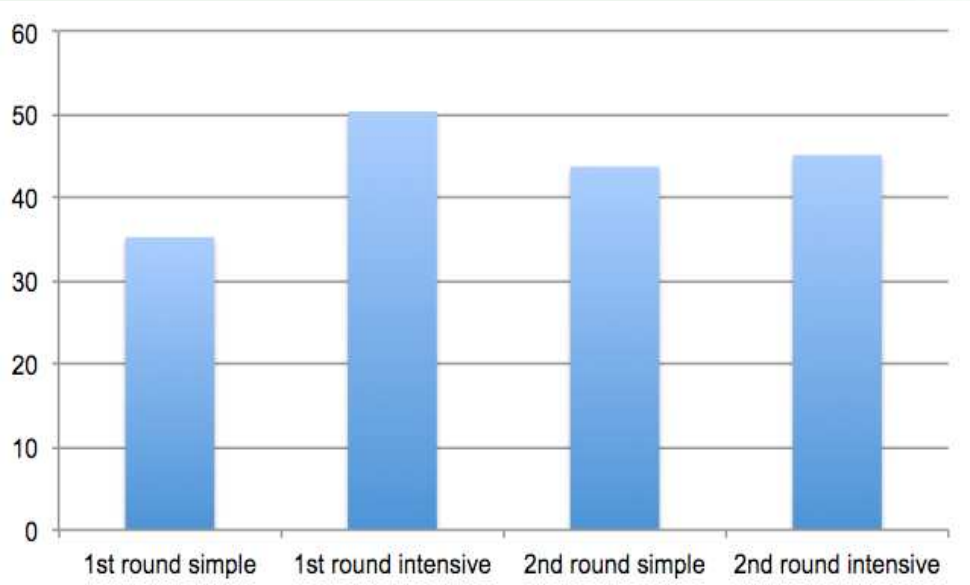
The above results tell us that farmers imitate early participants, especially those within their own social network, when they are given information about these

people's take-up decisions. However, these effects do not hold when such information was not revealed directly. This suggests that farmers cannot learn about other people's take-up decisions, even their own friends' decisions, simply through talking with eachother. This result rules out imitation as a possible channel of social network effects.

CHANNEL 3: SOCIAL LEARNING OF INSURANCE BENEFITS

Figure 5 shows the average take-up rate in different sessions. There are three messages we can get from it. First, comparing the take-up rate of the two first-round sessions shows that the financial education provided in intensive sessions raises the take-up rate from around 35% to 50%; second, there is not much difference in the impact of intensive and simple sessions in the second round; third, while the average take-up rate of second-round sessions is higher than that of first-round simple sessions, it is lower than that of first-round intensive sessions.

Figure 5: Average take-up rate in different sessions



To test whether these differences are statistically significant, we estimate the following equation:

$$Takeup_{ij} = \delta_0 + \delta_1 Intensive_{ij} + \delta_2 Delay_{ij} + \delta_3 Intensive_{ij} * Delay_{ij} + \delta_4 X_{ij} + \eta_j + \epsilon_{ij} \quad (11)$$

where $Intensive_{ij}$ is a dummy variable which takes a value of one if household i was invited to one of the two intensive sessions in village j and zero otherwise, and $Delay_{ij}$ is also a dummy which equals 1 if household i was invited to one of the two second-round sessions in village j and zero otherwise. The interaction term is included to test whether financial education has different size effects in different rounds.

Table 9: Effect of financial education on insurance take-up

VARIABLES	Insurance take-up (1 = Yes, 0 = No)					
	1st round		2nd round (Info = none)		Combined	
	(1)	(2)	(3)	(4)	(5)	(6)
Intensive (1 = Yes, 0 = No)	0.149***	0.140***	0.0139	0.0102	0.149***	0.140***
	(0.0261)	(0.0259)	(0.0311)	(0.0314)	(0.0262)	(0.0259)
Delay (1 = Yes, 0 = No)					0.0758**	0.0715**
					(0.0311)	(0.0308)
Intensive * Delay					-0.130***	-0.126***
					(0.0399)	(0.0398)
Male		0.0393		0.0256		0.0451
		(0.0476)		(0.0687)		(0.0339)
Age		0.00205*		0.00405***		0.00289***
		(0.00108)		(0.00122)		(0.000837)
Household Size		-0.00381		-0.00955		-0.00569
		(0.00514)		(0.00712)		(0.00425)
Rice production area (mu)		0.00161		0.00168		0.00168**
		(0.000993)		(0.00136)		(0.000801)
Illiteracy		-0.0823***		-0.0976***		-0.0851***
		(0.0269)		(0.0330)		(0.0197)
P-value of joint significance: Intensive & Intensive * Delay					0.0000	0.0000
No. of Observation	2,175	2,137	1,317	1,296	3,492	3,433
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.121	0.129	0.063	0.075	0.079	0.089

Notes: Robust standard errors in parentheses. Columns (1) and (2) are based on the sample of participants to the two first round sessions; Columns (3) and (4) are based on the subgroup of second round session participants who received no extra information in addition to the presentation; Column (5) and (6) are based on the whole sample. *** p<0.01, ** p<0.05, * p<0.1

According to estimation results reported in Table 9, columns (1) and (2) tell us that, in the first round, participating to an intensive rather than a simple session raises the take-up rate by around 14 % points, and that financial education in intensive sessions makes no difference in second-round sessions (columns (3) and (4)).[24] In addition, results in columns (5) and (6) suggest that the difference in the magnitude of the financial education effects between the two rounds is significant: according to the interaction term coefficient, the effect of financial education is around 13% smaller in the second round than that in the first round. These results support the argument that, during the time interval between first- and second-round sessions, farmers communicated the information they learned in sessions so that second-round participants already know what will be presented before they come to the session, and informal learning also increases the take-up rate significantly. However, the result also indicates that the second-round average adoption rate is 7.2% higher than the first-round average take-up rate, but it is 5% lower than the purchase rate in first-round intensive sessions. So we need additional evidence to support the social learning mechanism. For this, we first test the effect of financial education on farmers' understanding of insurance benefits and see whether the level of understanding is higher in the second round by estimating:

[24] Here we only use the subgroup of second-round participants who did not receive any additional information because only those people were exposed to the same information as first-round participants.

$$Understanding_{ij} = \omega_0 + \omega_1 Intensive_{ij} + \omega_2 Delay_{ij} + \omega_3 Intensive_{ij} * Delay_{ij} + \omega_4 X_{ij} + \eta_j + \varepsilon_{ij} \quad (12)$$

where $Understanding_{ij}$ is a variable constructed from ten questions we asked in the household survey to test farmers' understanding of benefits of this program. We measure it as the score farmers get which ranges from 0 to 1. Results are shown in Table 10.

Table 10: Effect of financial education on improving farmers` understanding of insurance benefits

VARIABLES	Insurance take-up (1 = Yes, 0 = No)	Understanding		
	All sample	First round	Second round	All sample
	(1)	(2)	(3)	(4)
Understanding	0.338***			
	(0.0296)			
Intensive (1 = Attend intensive session, 0 = Otherwise)		0.313***	0.00705	0.314***
		(0.0121)	(0.0107)	(0.0120)
Delay (1 = Attend later sessions, 0 = Otherwise)				0.230***
				(0.0114)
Intensive * Delay				-0.308***
				(0.0157)
Fraction of network in 1st round intensive				
Male	0.0270	0.0503**	0.0271	0.0403***
	(0.0297)	(0.0214)	(0.0201)	(0.0152)
Age	0.00457***	-0.00145**	-0.00175***	-0.00161***
	(0.000736)	(0.000580)	(0.000510)	(0.000354)
Household Size	-0.00678*	0.00262	0.00443	0.00384*
	(0.00392)	(0.00301)	(0.00303)	(0.00216)
Rice production area (mu)	0.001 15**	0.000805***	-0.000193	0.000320*
	(0.000516)	(0.000190)	(0.000386)	(0.000174)
Illiteracy	-0.0636***	-0.0961***	-0.0856***	-0.0904***
	(0.0184)	(0.0157)	(0.0153)	(0.0106)
No. of Observation	4,637	1,963	2,674	4,637
Region fixed effects	Yes	Yes	Yes	Yes
R-squared	0.109	0.329	0.060	0.180

Notes: Robust standard errors in parentheses.
Columns (1) tests the effect of understanding of insurance benefit on take-up decisions based on the whole sample; Columns (2) - (4) study the effect of intensive session on improving farmers' understanding of insurance benefits among first round session participants, second round participants, and all samples, respectvely.
*** p<0.01, ** p<0.05, * p<0.1

According to column (1), we can see that understanding insurance benefits is an important correlate of insurance take-up: successfully answering one additional question corresponds to a 3.4% increase in take-up rate. Columns (2) and (3) test the effect of providing financial education on improving farmers' understanding of insurance benefits. It shows that although first-round financial education can improve levels of understanding by around 31%, second-round education does not make any difference. However, the last column tells us that although second-round intensive financial education does not have any effect, the level of understanding is around 23% higher than that of first-round simple sessions, but it is 8% lower than that of first-round intensive sessions. Social learning thus works for understanding, though less powerfully than direct

intensive training.Second, we test whether the take-up rate and the understanding of insurance benefits are better when individuals have more friends exposed to financial education in the first round session by estimating:

$$Takeup_{ij} = \pi_0 + \pi_1 NetworkIntensive_{ij} + \pi_2 X_{ij} + \eta_j + \epsilon_{ij} \quad (13)$$

where $NetworkIntensive_{ij}$ is the fraction of the five friends listed by household i who were invited to the first-round intensive session in village j. The hypothesis is that if a household has more friends exposed to financial education, then it is more likely that its understanding of insurance benefits can be improved by social learning is thus more likely to buy the insurance. Results in Table 11 show that having one additional friend attending a first-round intensive session can raise the take-up rate by around 7%.

Table 11: Spillover effect of 1st round intensive sessions on 2nd round decisions

VARIABLES	Insurance take-up (1 = Yes, 0 = No)	
	(1)	(2)
Fraction of network in 1st round intensive	0.352***	0.364***
	(0.0828)	(0.0806)
Male		0.0252
		(0.0666)
Age		0.00423***
		(0.00118)
Household Size		-0.0104
		(0.00679)
Rice production area (mu)		0.00348***
		(0.00112)
Illiteracy		-0.100***
		(0.0321)
Intensive (1 =Yes, 0 = No)		0.0105
		(0.0322)
No. of Observation	1,270	1,251
Region fixed effects	Yes	Yes
R-squared	0.084	0.102

Notes: Robust standard errors in parentheses. Results in this table are based on the sample of participants in 2nd round sessions. Columns (1) and (2) estimates the effect of fraction of friends in early session on take-up rates, column (3) estimate the effect of that on understanding of insurance benefits. *** p<0.01, ** p<0.05, * p<0.1

These results confirm that it is social learning about insurance benefits that drives the network effect. The fact that second-round understanding is not quite as good as that obtained directly in first-round intensive sessions explains why the second-round take-up rate is lower than that of first-round intensive sessions.

CONCLUSIONS

This paper analyzed the role of information in the adoption of a new insurance product using data from a randomized experiment in rural China. We find strong evidence that financial education and social networks play important roles in insurance take-up. We also find that the main channel through which social networks affect insurance take-up is social learning of insurance benefits (learning from strongly connected friends, influential farmers, and village leaders), as opposed to expected gains in negotiating power (strength by the numbers) or imitation (acting like others). This suggests that farmers need to understand for themselves in order to decide on the adoption of a costly insurance product, as opposed to merely imitating others and counting on the mass of others. Providing intensive financial education through participation to village meetings is the most effective, but costly, instrument. The existence of social learning in traditional rice growing villages offers another option. Intensive financial education can be provided to a subset of households, and social networks relied upon to multiply its effects on understanding and uptake on others in the village community. Social learning can be made more effective by targeting financial education at individuals in the community most stringly connected to others, influential farmers, and village leaders. A cost effective diffusion strategy would thus consist in the direct training of carefully selected village members and reliance on their roles for the diffusion of social learning.

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