Insurance Take-up in Rural China: Learning from Hypothetical Experience[1]

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Abstract

This paper uses a novel experimental design to test the role of experience and information in insurance take-up in rural China, where weather insurance is a new and highly subsidized product. We randomly selected a group of poor households to play insurance games and find that it increases the actual insurance take-up by roughly 48%. To pinpoint mechanisms, we test whether the result is due to: (1) changes in risk attitudes, (2) changes in the perceived probability of future disasters, (3) learning the objective benefits of insurance, or (4) the experience of hypothetical disaster. We show that the overall effect is unlikely to be fully explained by mechanisms (1) to (3), and that the experience acquired in playing the insurance game matters. To explain these findings, we develop a descriptive model in which agents give less weight to disasters and benefits which they experienced infrequently. Our estimation also suggests that experience acquired in the recent insurance game has a stronger effect on the actual insurance take-up than that of real disasters in the previous year, implying that learning from experience displays a strong recency effect.

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1. Introduction

Poor households in rural areas are vulnerable to losses from negative weather shocks (Banerjee 2003). To protect themselves from these shocks, they engage in costly ex ante risk-mitigation strategies, such as avoidance of high-risk and high-return agricultural activities, high levels of precautionary saving and insufficient investment in production (Rosenzweig *et al.* 1993) and human capital (Jesen 2000). The negative shock, the loss of profitable opportunities and the reduction of human capital accumulation can lead to persistent poverty.

A potential way to shield farmers from risks and to reduce poverty is to provide formal weather insurance products. In many cases, such insurance products are available but are not widely used.[4] In 2009, a rice insurance policy was first offered to rural households in Jiangxi Province of China. Under certain reasonable assumptions (discussed in Section 5), calibration suggests that more than 70 % of rural households should buy the weather insurance. However, the baseline take-up in our sample was only around 20%.These findings suggest a puzzle: why do so few households participate in weather insurance markets, given the potentially large benefit?

In this paper, we apply a novel method of financial education to test the role of experience and information in influencing weather insurance take-up, using a randomized experiment in rural China. Such insurance products are new to most farmers and large disasters are relatively uncommon.[5] Therefore, improving farmers' understanding of insurance benefits is important in this context.[6]

We offered financial education about weather insurance to a randomly selected group of households by playing insurance games with them. During the game, household heads were asked whether they would like to buy insurance for the hypothetical future year and then played a lottery to see whether there is disaster in that year. After the lottery results were

[4]For example, Gine, Townsend and Vickery (2008) find relatively low take-up (4.6%) of a standard rainfall insurance policy among farmers in rural India in 2004. Cole *et al*. (2008) also found relatively low take-up (5%-10%) of standard rainfall insurance in two regions of India in 2006. The take-up is higher (20%-30%) with door-to-door household visits.

[5] According to the private communication with local government officials, the actual probability of relatively large disaster in a year is around 10%.

[6] For example, in Gine et al. (2008), farmers who were asked why they did not buy weather insurance often responded that they "do not understand the product." This suggests that financial education might be important to help increase the use of insurance product.

revealed, the enumerator helped them to calculate the income from that year according to their insurance purchase decisions and the insurance contract. The game was played for 10 rounds. One or three days later, we visited sample households again to ask for their actual purchase decisions.

We find that playing insurance games increased the actual insurance take-up by 9.6 percentage points, a 48% increase relative to the baseline take-up of 20 percentage points. The effect is roughly equivalent to experiencing a 45 percentage point higher loss in yield in the previous year, or a 45 percentage point increase in the perceived probability of future disasters.

There are at least four possible mechanisms through which this effect could work: changes in risk attitude, changes in the perceived probability of future disasters, learning the benefits of insurance, and changes in experience of disasters and insurance benefits. We investigate each of them below.

After playing the insurance games, we elicited the subjects' risk attitudes and the perceived probability of future disasters. We then test whether playing insurance games increases either risk aversion or the perceived probability of future disasters by an amount that could generate the observed 9.6 percentage points increase in take-up. Our results show that it's not the case.

We also test whether this effect is due to learning the benefits of insurance by randomly assigning households to a group in which we explained the benefits of insurance. For these people, we calculated the payoff of the policy under different situations, but did not play insurance games. This treatment increases the actual take-up by only 2.7 percentage points, and the increase is not statistically significant. In fact, playing insurance games has a larger effect than just receiving the calculations, a difference which is significant at the 5% level. This suggests that learning the objective benefits of insurance is unlikely to fully explain the increased take-up.

To test whether this effect is driven by the experience of hypothetical disasters, we explore a second source of exogenous variation: the number of hypothetical disasters experienced during the game. We find that the total number of disaster increases take-up significantly and it is mainly driven by the number of disasters in last few rounds. Specifically, experiencing one more hypothetical disaster in the last five rounds increased the actual take-up by 6.7 percentage points. This suggests that the experience of recent disasters, even if hypothetical, might be the mechanism to influence the actual insurance decisions.

This paper contributes to the existing literature in the following ways. First, it sheds light on the puzzle of low weather insurance demand. Although existing research has tested a number of explanations (Gine *et al.* 2008; Cole *et al.* 2011), lack of experience remains less explored as a possible explanation. We provide evidence that the lack of experience of disasters and insurance contributes to the low take-up rate of weather insurance.

Second, this paper demonstrates a new method of financial education and shows that Although there is correlational evidence suggesting that individuals with low levels of financial literacy are less likely to participate in financial markets (Lusardi and Tufano 2008; Lusardi and Mitchell 2007; Stango and Zinman 2009), the experimental evidence of financial education is mixed.[7] We show that the novel method we used in this paper has a large and significant effect on improving insurance demand and it is more effective than the traditional method of financial education, which simply involves explaining the benefits.

Our results also contribute to the literature on the effect of direct experience. Existing work has shown the effect of actual experience in areas including consumer behavior (Haselhuhn *et al.* 2009), financial markets (Choi *et al.* 2009; Agarwal *et al.* 2011; Malmendier and Nagel 2010) and charitable giving (Small *et al.* 2006). This paper analyzes the effect of hypothetical experience on poor households' insurance take-up and disentangles the effects of learning new information from the effects of personal experience. Results suggest that we can influence individual decisions by simulating experiences, as even hypothetical experience has an impact on household behaviors.

Fourth, this paper provides a new perspective on the role of laboratory experiments. Laboratory experiments provide controlled institutional contexts which are otherwise exceptionally difficult to obtain; they can generate deep insights about economic theories and policy applications (Holt 2005; Plott 2001). However, the behavior observed in the laboratory might not be a good indicator for behavior in the field under certain conditions (Levitt and

[7] Some find small or no effects of financial education on individual decisions (Duflo and Saez 2003; Cole *et al.* 2011; Carter *et al.* 2008), while others find positive and significant effects (Cole *et al.* 2010; Gaurav *et al.* 2011; Cai. 2011).

List 2007). We demonstrate that laboratory experiments can serve as interventions in field experiments, by testing the causal effect of the laboratory experiment itself on actual behavior in the field. This differs from the more commonly used design of having all subjects participate in both a laboratory experiment and a field intervention, and correlating behaviors in the two (Ashraf *et al.* 2006; Gazzale *et al.* 2009; Fehr and Götte 2007). Unlike these studies, our random assignment procedure allows us to make a causal interpretation of the laboratory exposure. A difference from most laboratory experiments is that we paid all households a flat fee to eliminate confounding due to income effects.[8] It is interesting that, even when there is no incentive, we still observe a large treatment effect. Follow-up work will tell whether experiments with monetary incentives provide similar results.

The paper proceeds as follows. In section 2, we provide background information on rice insurance in China. In section 3, we describe the experimental design and survey data. The main empirical results are discussed in section 4. There, we present the main treatment effect of playing games on actual insurance take-up, analyze the possible channels of this effect and then show the dynamics of the take-up decision during the hypothetical games. Finally, in section 5, we develop a simple model to explain the results.

2. Rice Insurance in China

Nearly 50 percent of farmers in China produce rice, and rice is the staple crop for more than 60 percent of Chinese consumers. In 2009, The People's Insurance Company of China designed the first rice insurance program and offered it to rural households in 31 pilot counties. Our experimental sites are 16 natural villages within two rice production counties that were included in the first round pilots in Jiangxi province, which is one of China's major rice bowls.[9] All households in these villages were provided with the formal rice insurance product. Since the product was new at

[8] The literature on financial incentives in experiments suggests that when there is no clear standard of performance in experiments, such as risky choices, incentives often cause subjects to move away from social desirable behavior toward more realistic choices (Camerer and Hogarth 1999). If social desirability depends on subject-experimenter interaction, households might buy more insurance during the games because of demand effects. In our data, the take-up during the games is around 75% and the actual take-up is around 27%.

[9] "Natural village" refers to the actual villages, "administrative village" refers to a bureaucratic entity that contains several natural villages.

that time, no households had heard of or bought such insurance before.

The insurance contract is depicted in figure 1.

[Insert Figure 1 Insurance contract]

The full insurance premium is 12 RMB per mu per season.[10]The government subsidizes 70 percent of the premium so that the households only pay 3.6 RMB. The policyholder is eligible to receive a payment if there are disasters that cause 30 percent or more loss in yield for one of the following reasons: heavy rain, floods, windstorms, extremely high or low temperatures, or drought. Losses in yield are determined by investigation by a group of insurance agents and agricultural experts. The payout amount increases linearly with the size of the loss in yield. For example, consider a farmer growing rice with an area of 2 mu. The normal yield per mu is 500kg but this year a wind disaster happens to reduce the yield to 300kg per mu. In that case, since the loss in yield is 40%, the farmer is supposed to get 200*40% = 80 RMB per mu from the insurance company. Note that the insurance is partial: payout is capped at 200 RMB, but the medium gross income in our sample is around 855 RMB per mu so the insurance covers at most 25 percent of income.

It's also important to note that the post-subsidy price is below the actuarially fair price according to our calculations. The profit of the insurance company is revenue minus payment to households and fixed cost.

$$\pi = N \cdot premium - N \cdot p \cdot indemnity - FC$$

where p is the probability of future disasters, N is the number of households who buy insurance and the indemnity is the payment to households when there is a disaster. According to private communications with local government officials, the actual probability of a disaster that leads to 30 percent or more loss is around 10 percent. Since $N \cdot 3.6 < N \cdot 10\% \cdot 60$, the post subsidy price is below fair price. However, because the pre-subsidy price is higher than the fair price, the insurance company earns a profit if its fixed costs are not large.

[10] 1 USD≈6.35 RMB or 3.95 RMB in PPP; 1 mu≈666.7 m^2; 1 mu≈0.165 acre; Farmers produce two or three seasons of rice every year.

3. Experimental Design and Survey Data

3.1 Experimental Design

In 2009 and 2010, we randomly selected 16 natural villages as our experiment sites. Nine hired enumerators consisting of government officials and primary school teachers, together with the two authors, visited each village and conducted surveys of 885 households before the beginning of the growing season. Randomization is conducted at the household level. There were two rounds of interviews for each household. The timeline is presented in the figure below.

[Insert Figure 2 Timeline]

We implemented the baseline survey and intervention in round 1. The procedure is as follows: the enumerators first gave households flyers with information about the insurance contract, including liability, period and premium. Households were then asked questions about their socioeconomic background. If the households were assigned to the game treatment, the enumerators played the insurance games with them (discussed below). After the games, we elicited risk attitudes and the perceived probability of future disasters for all households (discussed below). If the households were assigned to the information treatment (discussed below), the enumerators informed them of the actual probability of a large disaster.[11] At the end of round 1, households were also told to think about whether they would like to buy the rice insurance and that enumerators would come back to ask them to make a decision in round 2.

Round 2 was conducted one to three days later. In round 2, the enumerators asked sample households to indicate their purchase decisions. The decisions would be passed to insurance company who would collect the premium later.[12]

Round 2 was conducted one to three days later. In round 2, the enumerators asked sample households to indicate their purchase decisions. The decisions would be passed on to the insurance company, which would collect the premium later.[13]

[11] As estimated by government officials.
[12] Note that in round 1 the enumerators were randomly assigned to households while in round 2 one enumerator visited one or more villages. In our data, 22 percent of households (196 households) were visited twice by the same enumerator.
[13]Note that the enumerators were randomly assigned to households in round 1, while in round 2 one

At the end of round 1, we paid each household 5 RMB to compensate for the participant’s time. As discussed in the introduction, we did not incentivize decisions in order to eliminate confounding due to income effects.

We first approached the leaders of the villages and obtained a list that included the names of villagers and basic information about them[14].Then we stratified the households by their natural villages, ages of household heads, and area of rice production. In each stratum, households were randomly assigned to one of eight interventions. We randomized the treatments in two dimensions: how the contract was explained to the households (four groups) and whether the true disaster risk was revealed to the households (two groups). Figure 3 summarizes our design with eight groups in round 1.

[Insert Figure 3 Interventions]

The contract was explained in the following four ways. In the *Control* group, the enumerators gave households rice insurance flyers and went through the information about the contract. Then household heads were asked to fill out a short survey about their age, education, insurance experience, disasters experienced in recent years, production, social networks, risk attitudes and perception of the probability of future disasters.

In the *Calculation* group, the enumerators followed the same procedure as in the control group but additionally calculated the expected benefit of buying insurance if zero, one, two or three disasters were to happen in the following ten years. Enumerators went through the calculation with households and told them the summary: “According to our calculations, if there is no large disaster in next 10 years, it is better to not buy any insurance in the following 10 year. If there is at least one relatively large disaster, it is better to always buy insurance in the following 10 years.”

In the *Game 20%* (and Game 10%) groups, the enumerators followed the same procedure as in the control group and then played the hypothetical insurance games with 20% (or 10%) probability of disaster for ten rounds. The game was played in the following way. Household heads were first asked whether they would hypothetically like to buy insurance in

enumerator visited one or more villages. In our data, 22 percent of households (196 households) were visited twice by the same enumerator.

[14]We excluded households that did not grow rice. Those were households that were raising livestock or who had abandoned the land and were looking for jobs in urban areas.

2011 and then played a lottery with 20% (10%) probability of a disaster. We implemented the lottery by drawing randomly from a stack of cards; for example, in the *Game 20%*case, two out of ten cards signified disaster. After the lottery results were revealed, enumerators helped the household heads calculate the income from that year based on the expected income per acre and insurance payments. The game was then played for another nine rounds from hypothetical year 2012 to year 2020.[15]At the end of the game, we gave households the same information as in the *Calculation* group. Note that the game treatment provided not only financial education but also the second source of randomization: the number of the hypothetical disasters experienced during the games is randomized.

In a crossed randomization, we also randomized whether households were informed at the end of round 1 of the actual probability of disaster, which local government officials estimate at 10%. This randomization is interacted with how the contract is explained; thus, we have eight groups in total.

To summarize, the *Calculation* treatment provides households with information about the expected benefits of insurance. The *Game* treatment makes households acquire (hypothetical) disaster experience and provides households with information about the benefits of insurance. The (crossed) *Information* treatment provides households with information about the risk of disaster.

Risk attitudes and the perceived probability of future disasters were elicited for all households. For those who were assigned to play games, the above two measures were elicited after playing the insurance games. Comparing these measures between the game group and the other groups allows us to test whether playing games changes these parameters and further changes the actual insurance take-up.[16]Risk attitudes were elicited by asking households to choose between increasing amounts of certain money (riskless option A) and risky gambles (risky option B) in Appendix TableA1. We use the number of riskless options as a measurement of risk aversion.

[15]The setup implies that 89 percent of households in the *Game 20%* group and 65 percent of the households in the *Game 10%* group were expected to experience at least one disaster. In our data, 82 percent of households in the *Game 20%* group and 66 percent of households in the *Game 10%* group experienced at least one disaster.

[16]We did not ask these questions before the games; if players had decided to act consistently with their answers, this would have obscured the treatment effects.

The perceived probability of future disasters was elicited by asking households “what do you think is the probability of a disaster that leads to more than 30 percent yield loss next year?” We used a simple mechanism to illustrate probability, which might be a difficult concept for households with limited education.[17]

3.2 Survey Data

We implemented the survey in three waves. In the first wave (181 households, August 2009), we implemented only the control and *Game 20%*in the no information treatment. In the second wave (379 households, early March 2010), we implemented the control, the *Calculation*, and *Game 20%*in the no information treatment. In the third wave (325 households, late March 2010), we implemented all eight interventions. Because the *Game 10%* group and the information treatment were only conducted in the third wave, we oversample the *Game 10%* group; the total sample sizes of the *Game 10%* group and the information treatment are smaller than in the other groups.

[Insert Table 1 Summary statistics]

Table 1 presents summary statistics and balance checks separately for each wave. In total, we visited 885 households in round 1 and 816households in round 2. The overall attrition rate between round 1 and round 2was 7.8 percent. While the attrition was slightly higher in the game group, 9.8 percent, than in the control and calculations groups, respectively 6.2 and 5.6 percent, the difference in attrition between groups is not statistically significant. Attrition was 11.8 percent in the information group, which is not significantly different from the 10.4 percent attrition in the no information group in wave 3.

The summary statistics show that household heads are almost exclusively male. The average education level is between primary school and secondary school. The average individual is risk averse. The randomization check shows that most control variables are balanced. The only exception is that in wave 1, the households in the game group are older than those in the control group. However, the regressions in the next section show that the

[17]The enumerators gave sample individuals 10 small paper balls and asked them to put these paper balls into two areas: (1) no disaster reducing yield more than 30% next year and (2) disaster reducing yield more than 30% next year. If households put two paper balls into area (2) and eight paper balls into area (1), their perceived probability of future disaster is around 20%.

relationship between take-up and age is in any case insignificant.

4. Empirical Result

4.1 The Impact of Hypothetical Experience on Actual Take-up

In what follows, "Game" refers to households who were assigned to the *Game 20%* group or the *Game 10%* group. As shown in Figure 4, the take-up rate of the control group is 19.8 percent, while that of the calculation group is 24.7 percent. In the game group, the take-up is 32.3 percent. Thus, both the game and the calculation treatment increase take-up, but the game treatment is more effective.

[Insert Figure 4 Treatment effect]

Figure 5 shows the treatment effects of the game treatment and the calculation treatment when interacted with the information treatment. In the no information group, the pattern is similar to Figure 4. However, the game treatment increases the take-up and is more effective than the calculation treatment. In the information group, the take-up rates of three groups are similar. This suggests that the game treatment and the calculation treatment are not as effective with the interaction of information treatment.

[Insert Figure 5 Treatment effect by the information treatment]

We estimate the treatment effect on the take-up decision through a logit regression in (1):

$$buy_{ij} = \alpha_j + \alpha_k + \beta_g Tg_{ij} + \beta_c Tc_{ij} + \phi X_{ij} + \varepsilon_{ij} \tag{1}$$

where buy_{ij} is an indicator that takes on a value of one if household *i* in natural village *j* buys the insurance. Tg_{ij} is an indicator for the game treatment and Tc_{ij} is an indicator for the calculation treatment. Random assignment implies that β_g is an unbiased estimate of the reduced-form intention-to-treat (ITT) game treatment effect and β_c is an unbiased estimate of the ITT calculation treatment effect. X_{ij} are household characteristics (e.g. , gender, age, years of education, household size, land for production, whether they own a car, etc) and α_j and α_k are village fixed effects and enumerator fixed effects, respectively. ε_{ij} is type I extreme value error term. Since our roll-out design has three waves, it is important to control for potential confounding variables such as the covariates (X) and fixed effects. We report marginal effects in Table 2.

[Insert Table 2]

Column 1 presents results from the simplest possible specification, where the only right hand side variables are the indicators for the game treatment, the calculation treatment, and the village and enumerators fixed effects. The marginal effect of the game treatment (0.096) is positive and significant at the 5% level. Thus, the game treatment increases the take-up by 9.6 percentage points, which is about a 48 percent increase relative to the baseline take-up of 20 percentage points. The marginal effect of the calculation treatment (0.027) is positive but it is not statistically significant.

In column 2, we restrict the sample to households in the no information group. The marginal effect of the game treatment (0.126) increases and the pattern is similar to column 1.

In column 3, we restrict the sample to households in the information group. The marginal effects of the game and calculation treatment are imprecisely estimated; they are negative and not statistically significant. The difference in marginal effects between the information group and the no information group is significant at the 10% level.

In column 4, the self reported percentage of loss last year and a dummy for missing values are included in the regression with all samples. The pattern is similar to column 1.The marginal effect of the percentage of loss last year is 0.22%; this is significant at the 10% level. Thus, the effect of playing games is roughly of the same magnitude as the effect of a 45 percentage point increase in actual loss last year.

In column 5, a variety of other control variables and dummies for missing values are additionally included in the regression with all samples. The pattern is still similar to column 1.Education level is positively correlated with take-up and household size is negatively correlated with the take-up.

In sum, the game treatment increases the insurance take-up by 9 to 10 percentage points, resulting in an increase of around 45 to 50 percent relative to baseline take-up of 20 percentage points. The effect of playing games is roughly of the same magnitude as a 45 percentage point increase in actual loss during the previous year.

4.2 Possible Channels

In order for these findings to illustrate channels, more information is needed to analyze the mechanisms through which this effect could work. Possible explanations include: (1) changes in risk attitudes, (2) changes in the perceived probability of future disasters, (3) learning about the benefits of insurance, or (4) changes in hypothetical experience of disasters.

4.2.1 Risk Attitudes

First, it is possible that the treatment increases take-up by changing risk attitudes. To determine whether the game treatment changes risk attitudes and increases take-up, we run the follow regressions to test it:

$$buy_{ij} = \alpha_{2j} + \beta_{risk}\, risk_{ij} + \beta_{prob}\, prob_{ij} + \delta_{ij} \quad (2)$$

$$risk_{ij} = \alpha_{3j} + \gamma_{gr} Tg_{ij} + \gamma_{cr} Tc_{ij} + \eta_{ij} \quad (3)$$

$$risk_{ij} = \alpha_{4j} + \beta_{dr}\, disaster_{ij} + \omega_{ij} \quad (4)$$

where $risk_{ij}$ is an increasing measurement of risk aversion and $disaster_{ij}$ is the number of hypothetical disasters households experienced during the games. Equation (2) analyzes the correlation between take-up and risk attitudes. We restrict the sample to the control group and the calculation group in Equation (2) because we asked them questions about their risk attitudes and the perceived probability of future disasters before any intervention took place. In Equation (3) and (4), we estimate the effects of playing games and experiencing disasters, respectively. We assume that there is no measurement error as to risk attitudes and perceived probability of future disaster, and that the estimation in Equation (2) is unbiased.

[Insert Table 3]

In column 1 of Table 3, estimates from (2) are presented. The coefficient of risk aversion (0.032) is positive and significant at the 5% level. The coefficient of perceived probability of future disasters (0.0214) is positive and significant at the 10% level. Column 2 presents the estimates of (3), including various controls and dummies for missing values. Column 3 restricts the sample to households who played the hypothetical games and presents the estimates of (4). The results show that the treatment has no effect on risk aversion and the coefficient of the number of hypothetical disasters is not statistically significant.

To determine whether the game treatment changes risk attitudes and increases take-up, we stack Equation (1), (2), and (3), generate indicators for each equation, and estimate the regression system. To account for the correlation of error terms between each equation, standard errors are clustered by 16 natural villages. We test the hypothesis: $\beta_{risk}\gamma_{gr} = \beta_g$. We reject the hypothesis at the 5% level (p=0.039), with the 95% confidence interval ranging in [-0.013, 0.011]. To determine whether the number of hypothetical disasters changes risk attitudes and increases take-up, we stack Equation (1), (2), and (4) and estimate the regression system. We test the hypothesis: $1.48\beta_{dr}\gamma_{gr} = \beta_g$, where 1.48 is average number of hypothetical disasters experienced during the games. We reject the hypothesis at the 5% level (p=0.044), with the 95% confidence interval of $1.48\beta_{dr}\gamma_{gr}$ ranging in [-0.004, 0.004]. These results suggest that changes in our measurement of risk attitudes are unlikely to explain our main treatment effect.

4.2.2 The Perceived Probability of Future Disaster

Demand for insurance also depends on the perceived probability of future disasters. It is possible that the games increase take-up by changing the perceived probability of future disasters. To test this channel, we run the following regressions:

$$prob_{ij} = \alpha_{3j} + \gamma_{gp} Tg_{ij} + \gamma_{cp} Tc_{ij} + \eta_{ij} \qquad (5)$$

$$prob_{ij} = \alpha_{4j} + \beta_{dp}\, disaster_{ij} + \omega_{ij} \qquad (6)$$

where $prob_{ij}$ is the perceived probability of future disaster. In Equation (5) and (6), we estimate the effects of playing games and experiencing disasters, respectively. The results of (5) and (6) are presented in column 4 and 5 in Table 3, respectively.

The treatment has a negative effect on the perceived probability of future disasters in columns 4. The coefficient of the number of hypothetical disasters is not significant. Following a similar procedure as in section 4.2.1, we test the hypothesis $\beta_{prob}\gamma_{gp} = \beta_g$ and

$1.48\beta_{dp}\gamma_{gp} = \beta_g$ to determine whether changes in the perceived probability of future disasters is the channel. We reject that at the 5% level.

To determine whether the total effects of changing risk attitudes and the perceived probability of future disasters are the channel through which the observed effects operate, we follow a similar procedure as in section 4.2.1 and test the following two hypothesis: $\beta_{risk}\gamma_{gr} + \beta_{prob}\gamma_{gp} = \beta_g$ and $1.48\beta_{dr}\gamma_{gr} + 1.48\beta_{dp}\gamma_{gp} = \beta_g$.We reject the hypothesis at the 5% level. These results suggest that the total effects of changes in risk attitudes and the perceived probability of future disasters are unlikely to explain our main treatment effect.

4.2.3 Learning the Benefits of Insurance

It is also possible that playing insurance games provides direct information about the benefits of insurance. To test that, we compare the treatment effect of the game and calculation treatment; the difference between those two interventions should indicate whether households acquire disaster experiences during the games.

We run various regressions with (1) and report the p-value of Ward test $\beta_g = \beta_c$ in Table 2. In columns 1, 4 and 5, we use the whole sample. The difference between β_g and β_c is around 7 percentage points and it is not statistically significant (p-value of Ward test is between 0.13 and 0.16). In columns 2, we restrict the sample to the no information group. The difference between β_g and β_c is around 11 percentage points and is significant at the 5% level.

In sum, when we consider the channel of the game treatment effect without the interaction effect of the information treatment, we conclude that learning about the benefits of insurance is unlikely to explain the treatment effect of playing games. When we consider the channel of the game treatment effect and interaction effect of the information treatment, there is suggestive evidence that learning about the benefit is unlikely to explain the game treatment effect.

4.2.4 The Experience of Hypothetical Disasters

Another hypothesis is that hypothetical experience matters. To test this hypothesis, we explore the randomization of the number of hypothetical disasters during the game. We present Figure 6 about actual take-up and the hypothetical disasters experienced during the games.

[Insert Figure 6]

In the *Game 20%* group, the take-up among households who experienced two or more disasters is higher than that among those who experienced zero or one disaster. In the *Game 10%* group, the take-up of households who experienced one disaster is higher than those who experienced either zero or two and more disasters. However, given the relatively large standard deviation, Figure 6 provides only suggestive evidence that the take-up rate is increasing in the number of hypothetical disasters experienced and that the take-up in the group with no hypothetical disasters is greater than that in the control group.

To understand this further, we run the following regression:

$$buy_{ij} = \alpha_j + \beta_{disaster}\ disaster_{ij} + \delta_{ij} \qquad (7)$$

where $disaster_{ij}$ is the number of hypothetical disasters experienced during the games.

[Insert Table 4]

The marginal effect estimated in (7) is presented in column 1 and 4 of Table 4. In column 1, the coefficient (0.059) is positive and statistically significant at the 10% level. In the no information group (column 4), the coefficient (0.055) is positive but not significant (p=0.127). Therefore, the results suggest that the treatment effects were driven mainly by those who experienced more hypothetical disasters during the games.

Hypothetical experience might change two things: understanding about insurance and vividness. We run regression in Equation (8) to analyze these two effects:

$$buy_{ij} = \alpha_j + \beta_0 disaster0_{ij} + \beta_1 disaster1_{ij} + \beta_2 disaster2_{ij} + \beta_3 disaster3_{ij} + \varepsilon_{ij} \quad (8)$$

where $disasterK_{ij}$ is an indicator that takes on a value of one if households experience K disasters during the games. β_0 captures the understanding effect; the difference between β_0 and other coefficients captures the vividness effect.

The marginal effect of (8) is presented in column 2 and 5 in Table 4. The coefficients of

$disaster0_{ij}$ and $disaster1_{ij}$ are positive but not statistically significant. Indicators for more disasters are positive, statistically significant and relatively large in magnitude. In the no information group (column 4), the coefficients are relatively larger, which is similar to what we have seen in Table 2. The difference between β_1 and β_2 is statistically significant at the 10% level. However, we cannot reject the hypothesis that β_0 and β_1 are the same. Therefore, we cannot distinguish between the understanding effect and the vividness effect.

To further understand how hypothetical experience influences take-up, we present the take-up conditioning on disaster in the first 5 rounds and in the last 5 rounds in Figure 7.

[Insert Figure 7]

The evidence in Figure 7 suggests that the number of hypothetical disasters experienced in the first 5 rounds does not influence take-up, whereas the number of disasters in the last 5 rounds appears to have a bigger effect.

We then create two new variables: the number of hypothetical disasters in the first 5 rounds and the number of hypothetical disasters in the last 5 rounds. We run the following regression:

$$buy_{ij} = \alpha_j + \beta_{f5} disaster\_first5_{ij} + \beta_{l5} disaster\_last5_{ij} + \delta_{ij} \quad (9)$$

As seen in column 4, the coefficient of "disasters in the first half" is negative and not statistically significant. However, the coefficient of "disasters in the last half" is positive and significant at the 5% level. The coefficient suggests that experiencing an additional disaster in the last half increases take-up by 7.0 percentage points. In the no information group (column 6), the coefficient of the last 5 rounds is also positive and significant at the 5% level. This pattern is robust to different measurement of the first and last few rounds. If we regress take-up on the number of hypothetical disasters in the first (10-n) rounds and that in the last n rounds, we find that when n equals 5,6,7,8 or 9, the coefficients of the last n rounds are positive and significant at the 5% level.[18] These results are consistent with the literature in experienced utility and recency effects (Fredrickson and Kahneman 1993; Schreiber and Kahneman, D. 2000), where they find that the affect experienced during the last moments of

[18] See Appendix Table A4 for detail

the experiment has a privileged role in subsequent evaluations, and late moments in the experiment are assigned greater weight than earlier ones.

To summarize, we find that both the total number of disasters and the number of disasters in last few rounds increase take-up significantly. These results suggest that the experience of recent hypothetical disasters might be the channel through which the games influence insurance decisions.

5. Models

The evidence so far implies that hypothetical experience influences the actual insurance decisions. In this section, we present a simple model to illustrate how such an effect could occur. In section 5.1, we show that standard constant absolute risk aversion (CARA) preferences and constant relative risk aversion (CRRA) preferences are unlikely to explain the data. In section 5.2, we add a weight parameter to the utility function to capture the influence of experience. Then we estimate the parameters through a maximum likelihood method (MLE).

5.1 Standard Model

We first consider a simple model with CARA preferences commonly used in the insurance literature (Einav *et al.* 2010).

$$u(x) = -\frac{\exp(-\alpha x)}{\alpha} \qquad (11)$$

With CARA preferences, the consumer's wealth does not affect his insurance choices. Therefore, the take-up decisions should be determined by the joint distribution of risk attitudes and perceived probability of future disasters.

Let $U(a)$ denote the household utility as a function of the insurance decision. $a=1$ if the household buys the insurance and $a=0$ if the household does not buy the insurance. Let (b,τ) denote the insurance contract in which b is the repayment of insurance if there is a disaster and τ is the premium. Let x be the gross income of rice production and p the perceived probability of future disasters. Let l denote the loss in yield. The expected utility of not buying the insurance is:

$$U(a=0) = (1-\mathrm{p})u(x) + \mathrm{p}u(x-l) \qquad (12)$$

If a household buys insurance, it should earn its normal income and pay the premium when there is no disaster; it should have a loss and receive payment from the insurance company when there is a disaster. The utility of buying the insurance is:

$$U(a=1) = (1-\mathrm{p})u(x-\tau) + \mathrm{p}u(x-\tau-l+b) \qquad (13)$$

The condition for the household to buy the insurance is

$$U(a=1) >= U(a=0) \qquad (14)$$

It is straightforward to show that the households who are more risk averse and whose perceived probabilities of future disasters are larger are more likely to buy the insurance.

To test whether the standard CARA preferences could explain our data, one way is to use the parameter as measured, calibrate individual decisions and compare the calibrated decisions with actual decisions. We assume that there is no measurement error for risk aversion (α) or for the perceived probability of future disasters (p). Although we do not observe parameter α, we can make use of the choices in Table 1 to estimate the intervals of their α in the utility function. The intervals of α under CARA and CRRA are presented in Table 5. If a household takes two riskless options, α should be greater than zero and less than 0.0041 under CARA preferences. The details of the simulation procedures are discussed in Appendix A.

[Insert Table 5]

We find that the mean of simulated take-up is 81.08% and the standard deviation is 0.0049. This contradicts our actual data that the take-up in the sample is 26.84%. This suggests that standard CARA and CRRA preferences are unlikely to explain our data.

Another route is to ignore α and p as elicited. Suppose that we had not elicited measures for risk aversion and perceived probability of future disasters. Then we estimate α and p in the logit formula (15) through MLE:

$$P(a=1) = \frac{\exp(U(a=1))}{\exp(U(a=1)) + \exp(U(a=0))} \qquad (15)$$

We find that the model is not identifiable. The log-likelihood function reaches a flat region and the combination of α and p falls into the following two categories: (1) negative

α (risk seeking) and p greater 17% (2) positive α (risk averse) and p less than 5%. This contradicts our data that average risk attitude implies risk aversion and that the average perceived probability of future disasters is around 20%.

In sum, both the calibrated decisions and the estimated parameters contradict our data under standard CARA and CRRA preferences. These results suggest that standard CARA and CRRA preferences are unlikely to explain the observed take-up rates in the presence of the perceived probability of future disasters which our questions elicited.

5.2 Model Based on Experience

We have shown that standard CARA and CRRA preferences are unlikely to explain the data. In order to develop a model that fits our data, we add a weight parameter to capture the effect of experience. It is possible that households buy more insurance because they pay more attention to disasters and benefits after they experience the hypothetical disasters during the games. We develop a simple model in the following.

$$U(a=0) = (1 - \mathrm{p})u(x) + \mathrm{p}u(x - \mu l) \qquad (16)$$

$$U(a=1) = (1 - \mathrm{p})u(x - \tau) + \mathrm{p}u(x - \tau - \mu l + \mu b) \qquad (17)$$

where μ is a parameter that measures the weight of disaster loss and insurance benefits. The idea is that households might give less weight to disasters and benefits which they experience infrequently. When they are treated with games, they experience disasters and insurance benefits during the hypothetical games. These hypothetical disasters draw their attention to disaster loss and insurance benefits and increase the weight parameter μ.

It is straightforward to show that, under the assumption of CARA preferences with inattention parameter μ, if $\alpha > 0$, then $\frac{\partial P(buy==1)}{\partial \mu} > 0$. To the extent that playing games increases μ, it would increase the insurance take-up. To test this, we allow μ in the group who do not play games (μ_1) to be different from μ in the group who play games (μ_2). Then we estimate μ_1 and μ_2 with MLE and simulation. The details of the estimation procedures are discussed in Appendix A.

[Insert Table 6]

The result is presented in column 2 table 6. We find that the estimated mean of μ_1 is around 0.21 and that μ_2 is around 0.37. The T-test and Kolmogorov-Smirnov test show that the mean and the distribution are significantly different at the 1% level. Column 6 presents the result with CRRA preferences. Although the point estimates are different, the key pattern is similar. These results are consistent with our hypothesis that playing games increases μ and thus increases insurance take-up.

Hypothetical experience might have two effects: changes in understanding and changes in vividness. We add another parameter δ in (17):

$$U\ (a\ =1) = (1\ \text{-}\ \mathrm{p})\, u\,(x - \tau) + \mathrm{p}\, u\,(x - \tau - \mu l + \mu b) + \delta \quad (18)$$

where δ measures the utility of understanding insurance if they buy the insurance. The intuition is that households would be less happy if they buy something they do not understand than something they understand. It might capture ambiguity aversion and it is a reduced form in our model. We normalize δ to be zero in the game treatment so that the estimated δ is the difference of the utility of understanding. We estimate μ_1, μ_2 and δ with the same procedure to estimate μ_1 and μ_2. The results are presented in column 3.

The estimated mean of μ_1 is about 20.4% and μ_2 is about 33.9%. The T-test and Kolmogorov-Smirnov test show that the mean and the distribution are significantly different at the 1% level. The estimated mean of δ is -1.097 and the t-test shows that the mean is significantly different from zero at the 1% level. Since we normalize δ to be zero in the game treatment, this means that the utility of understanding is higher in the game treatment. Column 7 presents the result with CRRA preference. Although the point estimates are different, the key pattern is similar. These results are consistent with our hypothesis that playing games increases the understanding and vividness and thus increases the insurance take-up.

In order to understand the empirical relationship between experience and the weight parameter, we model μ following the lead of Agarwal *et al.* (2011).

$$\mu = C + D(1 - \exp(-k_a f_a - k_h f_h))$$

where $k_a, k_h, C, D > 0$, and $C + D \le 1$.

f_a is actual experience, measured by percentage of disasters reducing yield more than 30% in the past 3, 2 or 1 years. f_h is hypothetical experience, measured by percentage of disasters during 10 rounds of games. k_a and k_h capture the rate of learning from actual experience and hypothetical experience. With enough experiences, attention saturates to C + D. If $C+D=1$, attention is perfect in the long run, but if $C+D<1$, attention is imperfect, even in the long run. Here, we assume $C+D=1$. Then we could estimate k_a and k_h and compare the effect of actual and hypothetical experience.

In column 4, we estimate the learning effect of both actual experience (k_a) and hypothetical experience (k_h) under CARA preferences. f_a is measured by percentage of disasters reducing yield more than 30% in the past 3 years. The mean of k_a is 0.075 and the mean of k_h is 4.254; they are significantly different at the 1% level. Column 8 presents the result with CRRA preferences. Although the point estimates are different, the key pattern is similar. These results suggest that both actual and hypothetical experience matter. Moreover, experience acquired in the recent insurance game has a stronger effect on the actual insurance take-up than that of real disasters in the previous year.

6. Conclusion

It is important to understand why the take-up for weather insurance is low even when farmers face substantial natural risks. We apply a novel method of financial education and test for the role of experience and information in weather insurance take-up in rural China. We find that playing insurance games increases the actual insurance take-up by 9.6 percentage points, a 48% increase relative to the baseline take-up of 20 percentage points. We investigate the possible mechanisms through which this effect could work, and find that changes in experience of disasters and insurance benefits are very likely to be the channel.

There is mixed evidence in the literature as to whether financial education is effective to change individual decisions. Why is financial education effective sometimes but not others? Under what circumstances is financial education effective? This paper shows that financial education with simulated experiences can help increase insurance take-up in rural areas.

Gaurav et al. (2011) finds similar results in India. Song (2011) finds that learning the concept of compound interest has a positive and significant effect on weather insurance adoption in rural China. These suggest that we should first identify the barriers to individual participation and then apply specific financial education to remove the barriers. This seems to work better than general financial education.

From a policy perspective, this paper suggests that policy makers should take into account the individuals' biases when design policies, especially in rural areas where most people are less educated. In particular, policy makers can provide cheap financial education to overcome individual constraints and thus improve individual welfares.

From a methodological standpoint, this paper is among the first to use a laboratory experiment as an intervention in the field experiment.[19] We find that the laboratory experiment influenced the field behavior in our setting. By using laboratory experiments, researchers can explicitly manipulate more variables which are endogenous or are otherwise difficult to manipulate. For example, Malmendier and Nagel (2010) find that individuals who have experienced low stock-market returns throughout their lives so far are less likely to participate in the stock market. However, it is difficult to manipulate experience in order to influence individual decisions. In this paper, we use a laboratory experiment to simulate experiences and influence field behaviors. We hope to explore in future research whether this can apply to other settings.

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Table 1. Summary Statitics and Randomization Check

	Wave 1			Wave 2				Wave 3				
	Control	**Game 20%**	**p-value***	**Control**	**Calculation**	**Game 20%**	**p-value****	**Control**	**Calculation**	**Game 20%**	**Game 10%**	**p-value****
Panel A:before playing the game												
Age	46.90	50.44	0.05	51.43	50.86	52.99	0.34	50.64	48.27	52.10	48.53	0.23
	(11.33)	(12.37)		(11.41)	(11.67)	(12.32)		(12.28)	(11.47)	(12.24)	(12.17)	
Education***	1.38	1.32	0.57	1.30	1.30	1.35	0.84	1.45	1.37	1.41	1.44	0.94
	(0.75)	(0.82)		(0.78)	(0.71)	(0.82)		(0.78)	(0.85)	0.93)	(0.90)	
Household Size	4.80	5.04	0.62	5.05	5.25	5.26	0.80	4.48	4.60	4.31	4.58	0.75
	(1.79)	(2.30)		(2.52)	(2.84)	(2.89)		(1.29)	(1.39)	(1.69)	(1.51)	
Area of Rice Production (mu)	12.14	12.08	0.97	8.90	9.20	8.90	0.94	10.28	11.91	10.46	11.25	0.69
	(9.58)	(7.56)		(7.51)	(7.90)	(7.79)		(5.42)	(13.57)	(10.25)	(7.37)	
Share of Rice Income in Total Income (%)	84.00	85.05	0.76	64.30	63.13	60.24	0.50	90.8	89.45	87.34	87.38	0.52
	(21.16)	(24.19)		(28.2)	(27.07)	(28.04)		(14.79)	(15.58)	(18.70)	(16.99)	
Loss in Last Year (%) (self-report)	6.72	6.98	0.92	24.29	22.96	23.01	0.79	31.60	29.38	26.94	29.37	0.53
	(15.14)	(16.91)		(15.41)	(15.12)	(15.33)		(18.02)	(15.30)	(13.65)	(17.51)	
Panel B:after playing the game												
Risk Aversion				4.13	4.16	4.10	0.95	3.20	3.23	3.04	3.11	0.90
				(1.45)	(1.44)	(1.43)		(1.52)	(1.44)	(1.59)	(1.71)	
Perceived Probability of Future Disaster				23.10	22.33	21.64	0.76	24.10	23.15	21.38	23.80	0.30
				(15.77)	(15.52)	(14.53)		(9.83)	(9.26)	(9.26)	(9.38)	
Take-up(%)	0.19	0.24	0.42	0.17	0.17	0.32	0.01	0.28	0.39	0.37	0.36	0.61
	(0.39)	(0.43)		(0.38)	(0.38)	(0.47)		(0.45)	(0.49)	(0.49)	(0.48)	
Observations	86	95		121	124	134		52	73	49	151	

Note: standard deviations are in the parentheses.

*P-value in wave 1 is for F test of equal means of two groups

** P-values in wave 2 and 3 are for Wald test of equal means of three and four groups

***Education is coded as follows: 0-illitracy; 1-primary school; 2-secondary school; 3-high school; 4-College

Table 2. The Effect of Game and Calculation on Insurance Take-up

Specification:	**Logistic regression**				
Dep. Var.:	**Individual adoption of insurance**				
Sample:	**All Sample**	**No Information**	**Information**	**All Sample**	
	1	2	3	4	5
Game	0.092	0.119	-0.086	0.096	0.092
	(0.039)**	(0.034)***	(0.172)	(0.037)***	(0.038)**
Calculation	0.025	0.012	-0.009	0.029	0.031
	(0.043)	(0.047)	(0.189)	(0.042)	(0.040)
%Loss Last Year (self report)				0.207	0.200
				(0.104)**	(0.110)*
Age					0.008
					(0.011)
Education					0.039
					(0.017)**
Household Size					-0.015
					(0.005)***
Land of Rice Production					0.002
					(0.014)
Wald Test: $\beta_g=\beta_c$					
p-value	0.1376	0.0117**	0.5376	0.1328	0.1568
Obs.	816	674	132	816	816
Omitted Treatment			Control		
Mean of Dep. Var. for Omitted Treatment:			0.198		
Fixed Effects for Village and Enumerator	Y	Y	Y	Y	Y
Log Likelihood	-431	-335	-86	-429	-424
Pseudo R-square	0.0918	0.1057	0.0323	0.0962	0.1065

Notes: Dependent variable is individual adoption; standard errors are clustered by 16 natural villages. Robust clustered standard errors are in the parentheses. *** significant at 1% level; ** significant at 5% level; * significant at 10% level. In column 2, we restrict the sample to households in the no information group. In column 3, we restrict the sample to households with the information treatment. In column 4 to 5, we add dummies for missing values of control variables in the regression. In column 4, the self reported percentage of loss in last year is included in the regression. In column 5, additional control variables are age group of household head, education of household head, household size and area of rice production. We lose ten observations in column 3 because one independent variable predicts not buying perfectly and the logistic regression drops them.

Table 3. The Decomposition Effect of Game and Calculation

Specification:	**OLS Regression**				
Dep. Var.:	**Individual Adoption of Insurance**	**Risk Aversion**		**Perceived Probability of Future Disaster**	
Sample:	**Control & Calculation**	**All Sample**	**Game**	**All Sample**	**Game**
	1	2	3	4	5
Risk Aversion	0.035	-0.024			
	(0.016)**	(0.182)			
Perceived Probability of Future Disaster	0.215	0.055			
	(0.110)*	(0.165)			
Game				-0.015	
				(0.008)*	
Calulation				-0.011	
				(0.009)	
Number of Hypothetical Disasters			0.080		0.003
			(0.138)		(0.008)
Obs.	329	697	320	667	310
Omitted Treatment			Control		
Mean of Dep. Var. for Omitted Treatment:	0.198	Y	Y	Y	Y
Social-economic Variables	Y	Y	Y	Y	Y
Fixed Effects for Village and Enumerator	Y	Y	Y	Y	Y
R-square	0.1397	0.1932	0.2022	0.0990	0.1896

Notes: Standard errors are clustered by 16 natural villages. Robust clustered standard errors are in the parentheses. *** significant on 1% level; ** significant on 5% level, * significant on 10% level. In column 1, we restrict the sample to the control group and the calculation group and regress adoption on risk attitude. In column 2 to 3, we regress risk attitude on treatment indicator and controls. In column 4 to 5, we regress the perceived probability of future disasters on treatment indicator and controls.

Table 4. the Effect of Hypothetical Games on Actual Insurance Take-up

Specification:	**Logistic Regression**					
Dep. Var.:	**Individual Adoption of Insurance**					
Sample:	**All sample**			**No information**		
	1	2	3	4	5	6
Game	0.010		0.047	0.037		0.085
	(0.059)		(0.046)	(0.067)		(0.047)
Calculation	0.042		0.044	0.032		0.037
	(0.046)		(0.045)	(0.051)		(0.050)
Number of Hypothetical Disasters	0.059			0.055		
	(0.031)*			(0.036)		
Game and No Disaster		0.030			0.060	
		(0.060)			(0.076)	
Game and One Disaster		0.046			0.064	
		(0.045)			(0.044)	
Game and Two Disasters		0.137			0.159	
		(0.043)***			(0.042)***	
Game and Three or More Disasters		0.133			0.143	
		(0.066)**			(0.062)**	
Number of Hypothetical Disasters in First Half of Game (2011-2015)			-0.019			-0.042
			(0.024)			(0.028)
Number of Hypothetical Disasters in Second Half of Game (2016-2020)			0.070			0.072
			(0.033)**			(0.034)**
Obs.	804	804	804	664	664	664
Omitted Treatment	Control					
Mean of Dep. Var. for Omitted Treatment:	0.198					
Social-economic Variables	Y	Y	Y	Y	Y	Y
Fixed effects for village and enumerator	Y	Y	Y	Y	Y	Y
Log Likelihood	-427	-427	-426	-333	-334	-331
Pseudo R-square	0.0599	0.0864	0.0884	0.0956	0.0965	0.1021

Notes: Dependent variable is individual adoption; standard errors are clustered by 16 natural villages. Robust clustered standard errors are in the parentheses. *** significant on 1% level; ** significant on 5% level, * significant on 10% level. In column 4 to 6, we restrict the sample to households in the no information treatment. In column 3 and 6, we regress the actual take-up on the number of hypothetical disasters in the first 5 rounds and the number of hypothetical disasters in the last 5 rounds.

Table 5. Range of Risk Aversion Parameter

Number of Riskless Options Taken	Range α of for CARA $u(x)=-\exp(-\alpha x)/\alpha$	Range α of for CRRA $u(x)=x^{1-\alpha}/(1-\alpha)$
0	$\alpha<-0.0121$	$\alpha<-1.4$
1	$-0.0121<\alpha<-0.0041$	$-1.4<\alpha<-0.35$
2	$-0.0041<\alpha<0$	$-0.35<\alpha<0$
3	$0<\alpha<0.0041$	$0<\alpha<0.25$
4	$0.0041<\alpha<0.0121$	$0.25<\alpha<0.5$
5	$\alpha>0.0121$	$\alpha>0.5$

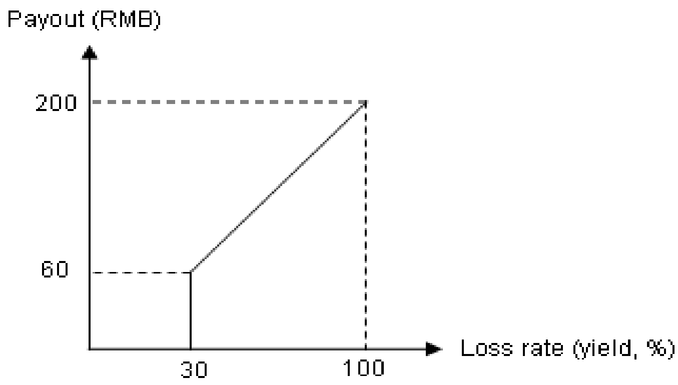
Notes: Calculation of range of risk aversion parameter is based on the number of riskless options taken in table A1.

Table 6. Maximum Likelihood Estimation of Utility Function

	CARA			CRRA		
	1	2	3	4	5	6
μ_1	0.208	0.204		0.152	0.149	
μ_2	0.370	0.339		0.269	0.262	
δ		-1.097			-0.689	
c			0.203			0.205
k_a			0.075			0.012
k_h			4.254			0.735
90% CI for μ_1 or k_a	[0.106,0.391]	[0.121,0.395]	[0.000,0.450]	[0.121,0.203]	[0.121,0.174]	[0.000,0.082]
90% CI for μ_2 or k_h	[0.152,0.645]	[0.152,0.546]	[0.000,32.689]	[0.152,0.645]	[0.174,0.311]	[0.000,2.326]
t test p-value	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***
k-s test p-value	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***
Obs.	613	613	344	613	613	344
Number of Draws for α	100	100	100	100	100	100

Notes: we estimate parameters in CARA utility function $u(x) = -\frac{\exp(-\alpha x)}{\alpha}$ and CRRA utility function $u(x) = \frac{x^{1-\alpha}}{1-\alpha}$ through MLE. In all columns, we constrain α to be uniform draws from the intervals of their risk attitudes and constrain p to be the perceived probability of future disasters from our survey data. We present the mean of coefficients from 100 draws of α. In column 1 and 4, we allow the weight parameter in the group who do not play games (μ_1) to be different from weight parameter in the group who play games (μ_2). In column 2 and 5, we add δ to measure the utility of understanding the insurance if they buy the insurance. We normalize δ to be zero in the game treatment so that the estimated δ is the difference of the utility of understanding. In column 3 and 6, we assume that the weight parameter has the following structure $\mu = C + D(1 - \exp(-k_a f_a - k_h f_h))$. Then we estimate both the learning effect of actual experience (k_a) and hypothetical experience (k_h) with different measurement of actual disaster.

Figure 1 Insurance contract



Note: The original premium of insurance is RMB 12 per mu. The government will subsidize 70% of the premium so the households only pay the remaining 30%, i.e. RMB 3.6. The policyholder is eligible to receive a payment if there are disasters that cause 30% or more loss in yield by the following reasons: heavy rain, flood, windstorm, extremely high or low temperature and drought. The payout amount increases linearly with the size of the loss in yield, reaching a maximum payout at 200 RMB. The losses in yield will be determined by the investigation of a group of agricultural experts. They will come to the village to sample the rice in different plot and calculate the loss in yield.

Figure 2 Timeline

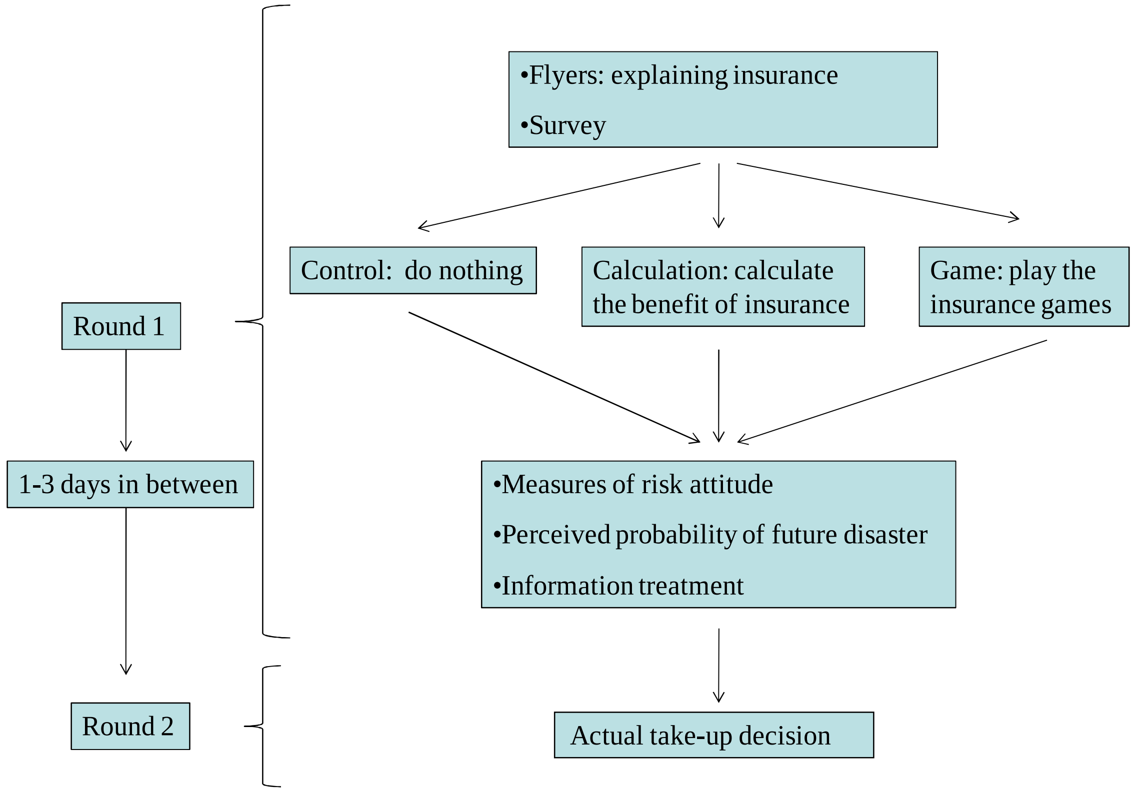


Figure 3 Interventions

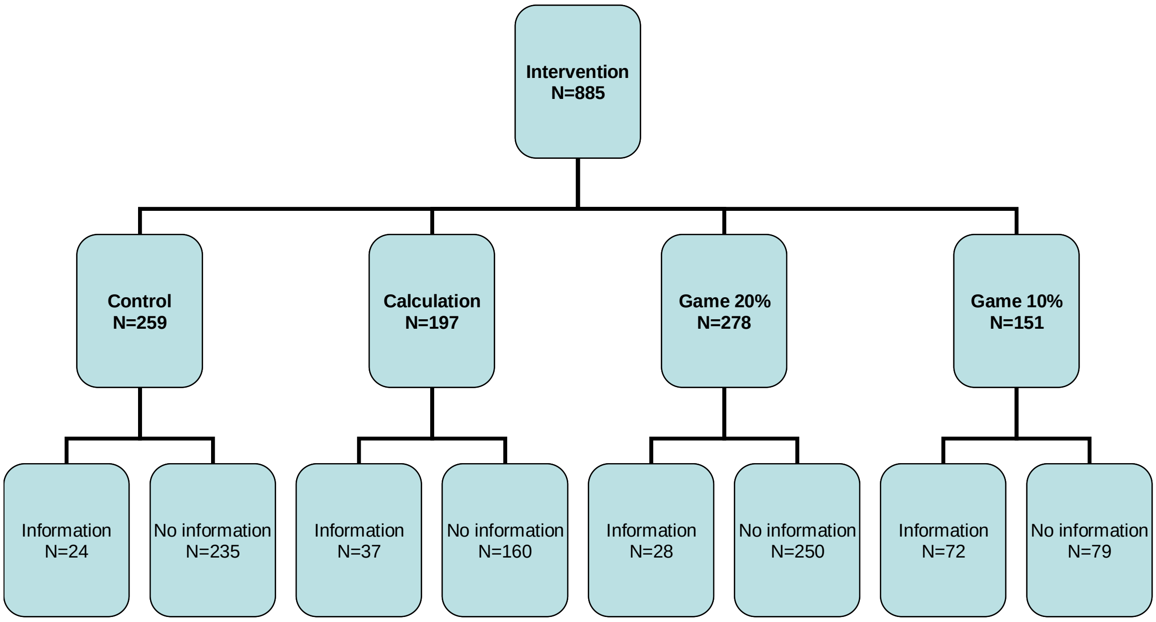
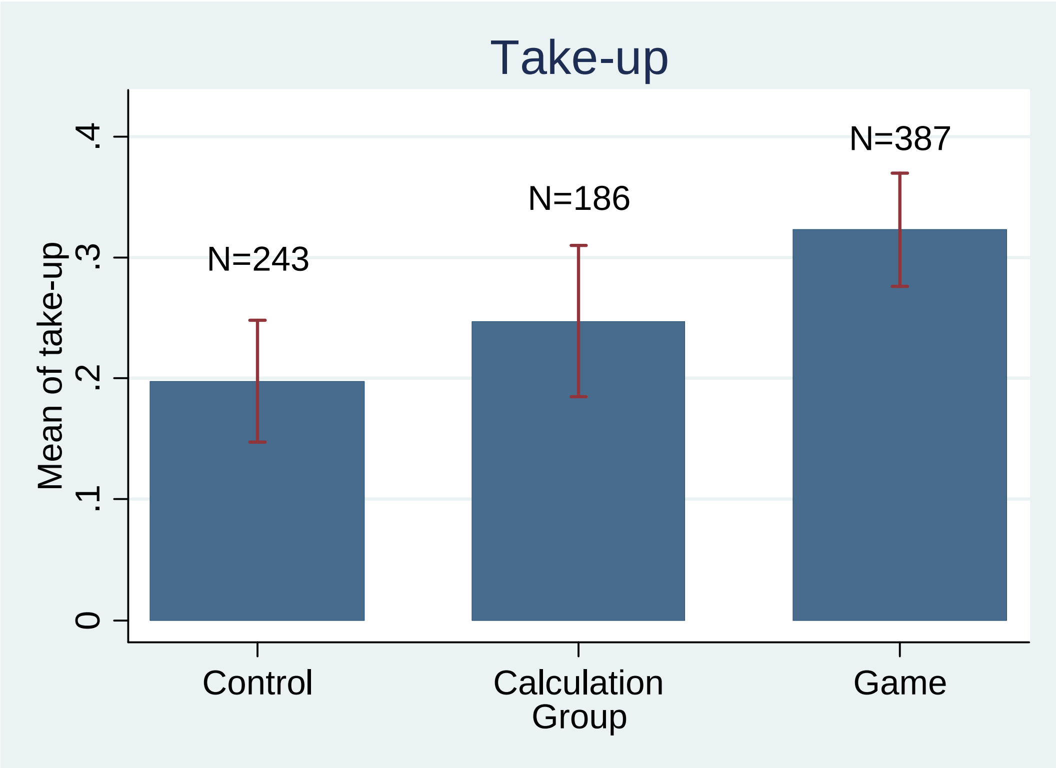
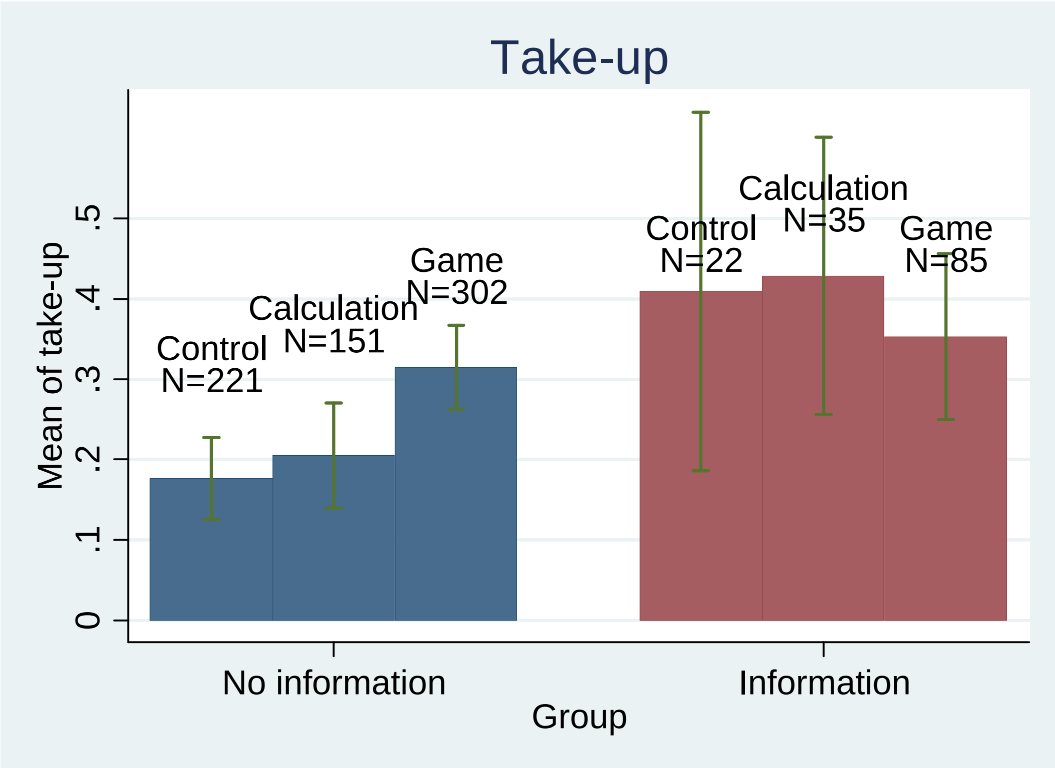


Figure 4 Treatment effect



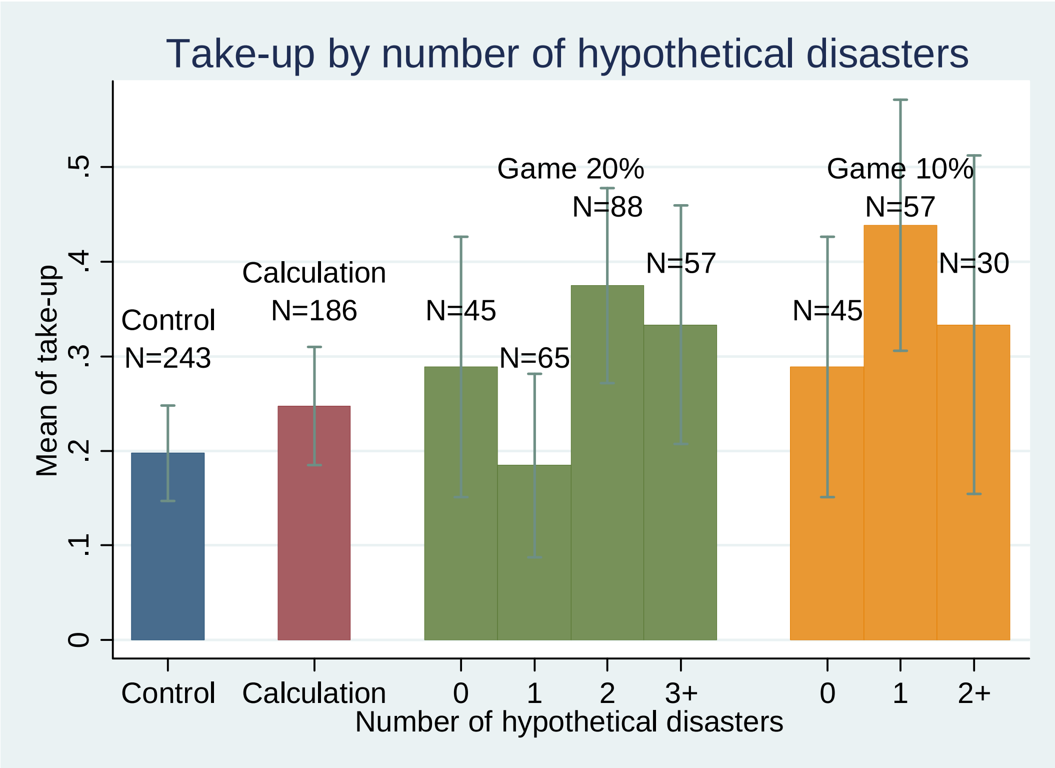
Note: This figure shows the treatment effect of the calculation group and the game group. In the control group, the take-up is 19.8%. In the calculation group, the take-up increases to 24.7%.In the game group, the take-up increases to 32.3%. It suggests that both the game treatment and the calculation treatment increase the actual take-up and the game treatment is more effective.

Figure 5 Treatment effect by the information treatment



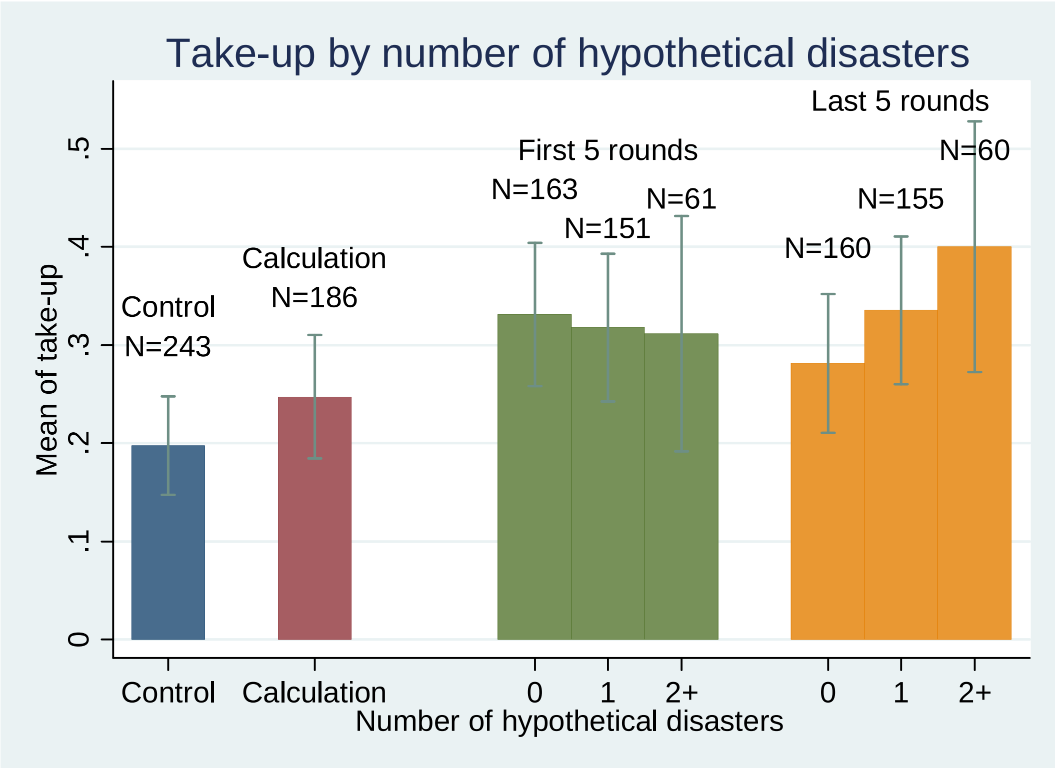
Note: This figure shows the treatment effect by the information treatment. Without the information treatment, the game treatment is more effective than the calculation treatment. With the information treatment, the game treatment and the calculation treatment is not effective.

Figure 6: Take-up by number of hypothetical disasters in the games



Note: the figure shows the insurance take-up conditioning on the number of disasters they experienced during the games. The left two bars show the take-up of the control group and the calculation group.

Figure 7: Take-up by number of hypothetical disasters in different rounds



Note: the left two bars show that the insurance take-up conditioning whether there is a disaster in the first round and last round.

The right two bars show the insurance take-up conditioning on the number of disasters in the first 5 rounds and last 5 rounds.

Appendix

A.1 Simulation of Insurance Take-up under Standard Model

We simulate the take-up decisions with the following steps:

1. Take a uniform draw of α from the interval according to each household's choices of riskless options
2. Take two extreme type I error term and difference them to get logistic error term
3. Use the draw of α,self-reported p and the error term to calculate the insurance decision of each household and the percentage of take-up in the simulated sample
4. Repeat 1 to 3 for 100 times and calculate the mean and standard deviation of take-up.

A.2 MLE Estimation of Weight Parameters

We estimate μ_1 and μ_2 with MLE and simulation with the following steps:

1. Take a uniform draw of α from the interval according to each household's choices of riskless options
2. Constrain α to be the draw value and p to be the perceived probability of future disasters from our survey data, then estimate μ_1 and μ_2 with MLE
3. Redo step 1 and 2 for 100 times to generate 100 μ_1 and μ_2
4. Compare the distribution of μ_1 and μ_2

Table A1. Risk Attitude Qustions

	Option A	Option B	Choice
1	50 RMB	Toss a coin. If it is heads, you get 200RMB. If it is tails, you get nothing.	
2	80 RMB	Toss a coin. If it is heads, you get 200RMB. If it is tails, you get nothing.	
3	100RMB	Toss a coin. If it is heads, you get 200RMB. If it is tails, you get nothing.	
4	120RMB	Toss a coin. If it is heads, you get 200RMB. If it is tails, you get nothing.	
5	150RMB	Toss a coin. If it is heads, you get 200RMB. If it is tails, you get nothing.	

Note: Risk attitudes were elicited for all the households with questions in table 1. For those who were assigned to play games, risk attitudes were elicited after playing insurance games. Households were asked to make five hypothetical decisions to choose between riskless option A and risky option B. We use the number of riskless options as a measurement of risk averse. The more the riskless options are chosen, the more the risk averse is.

Table A2. the Effect of Hypothetical Games on Actual Insurance Take-up

Specification:	**Logistic Regression**			
Dep. Var.:	**Individual Adoption of Insurance**			
Sample:	**All Sample**			
	1	2	3	4
Game 20%	0.108	0.119	-0.056	0.107
	(0.035)***	(0.034)***	-0.159	(0.036)***
Game 10%	0.045	0.120	-0.097	0.036
	(0.066)	(0.086)	(0.182)	(0.067)
Calculation	0.020	0.012	-0.008	0.020
	(0.045)	(0.048)	(0.189)	(0.043)
Wald test: β_{20}=β_{10}				
p-value	0.2548	0.9956	0.5877	0.2051
Obs.	816	674	132	816
Omitted Treatment	Control			
Mean of Dep. Var. for Omitted Treatment:	0.198			
Social-economic Variables	Y	Y	Y	Y
Fixed Effects for Village and Enumerator	Y	Y	Y	Y
Log Likelihood	-430	-335	-86	-425
Pseudo R-square	0.0933	0.1507	0.0329	0.1045

Notes: Dependent variable is individual adoption; standard errors are clustered by 16 natural villages. Robust clustered standard errors are in the parentheses. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table A3. the Effect of Hypothetical Games on Actual Insurance Take-up

Specification:	**Logistic Regression**							
Dep. Var.:	**Individual Adoption of Insurance**							
Sample:	**Game**				**Game with no Information**			
	1	2	3	4	5	6	7	8
Number of Hypothetical Disasters	0.072				0.083	-0.047	0.133	
	(0.037)*				(0.043)*	(0.038)	(0.067)**	
Number of Hypothetical Disasters in First Half of Game (2011-2015)		-0.026	0.107			0.085		
		(0.033)	(0.058)*			(0.038)**		
Number of Hypothetical Disasters in Second Half of Game (2016-2020)		0.075						
		(0.034)**						
Interaction Term (dd5)			-0.051				-0.075	
			(0.050)				(0.057)	
Hypothetical Disaster in 2011				-0.134				-0.140
				(0.082)				(0.105)
Hypothetical Disaster in 2012				-0.134				-0.155
				(0.048)***				(0.052)***
Hypothetical Disaster in 2013				-0.025				-0.030
				(0.071)				(0.080)
Hypothetical Disaster in 2014				0.004				-0.113
				(0.056)				(0.043)**
Hypothetical Disaster in 2015				0.073				0.096
				(0.058)				(0.057)*
Hypothetical Disaster in 2016				0.155				0.164
				(0.039)***				(0.044)***
Hypothetical Disaster in 2017				-0.053				-0.050
				(0.069)				(0.082)
Hypothetical Disaster in 2018				0.120				0.147
				(0.064)*				(0.081)*
Hypothetical Disaster in 2019				0.016				0.037
				(0.079)				(0.060)
Hypothetical Disaster in 2020				0.141				0.160
				(0.063)**				(0.064)**
Obs.	375	375	375	375	292	292	292	292
Omitted Treatment	Control							
Mean of Dep. Var. for Omitted Treatment:	0.198							
Social-economic Variables	Y	Y	Y	Y	Y	Y	Y	Y
Fixed Effects for Village and Enumerator	Y	Y	Y	Y	Y	Y	Y	Y
Log Likelihood	-214	-213	-213	-206	-160	-158	-158	-150
Pseudo R-square	0.0934	0.0966	0.0979	0.1251	0.1219	0.129	0.1307	0.1743

Notes: Dependent variable is individual adoption; standard errors are clustered by 16 natural villages. Robust clustered standard errors are in the parentheses. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table A4. the Effect of Hypothetical Games on Actual Insurance Take-up

Specification:	**Logistic Regression**								
Dep. Var.:	**Individual Adoption of Insurance**								
Sample:	**Game**								
	1	2	3	4	5	6	7	8	9
Hypothetical Disaster in 2011	-0.134								
	(0.079)*								
Number of Hypothetical Disasters in Last Nine Years (2012-2020)	0.032								
	(0.012)***								
Number of Hypothetical Disasters in First Two Years (2011-2012)		-0.102							
		(0.036)***							
Number of Hypothetical Disasters in Last Eight Years (2013-2020)		0.077							
		(0.026)***							
Number of Hypothetical Disasters in First Three Years (2011-2013)			-0.077						
			(0.034)**						
Number of Hypothetical Disasters in Last Seven Years (2014-2020)			0.080						
			(0.027)***						
Number of Hypothetical Disasters in First Four Years (2011-2014)				-0.053					
				(0.028)*					
Number of Hypothetical Disasters in Last Six Years (2015-2020)				0.084					
				(0.026)***					
Number of Hypothetical Disasters in First Five Years (2011-2015)					0.020				
					(0.033)				
Number of Hypothetical Disasters in Last Five Years (2016-2020)					0.067				
					(0.040)*				
Number of Hypothetical Disasters in First Six Years (2011-2016)						0.013			
						(0.028)			
Number of Hypothetical Disasters in Last Four Years (2017-2020)						0.039			
						(0.044)			
Number of Hypothetical Disasters in First Seven Years (2011-2017)							0.005		
							(0.026)		
Number of Hypothetical Disasters in Last Three Years (2018-2020)							0.068		
							(0.043)		
Number of Hypothetical Disasters in First Eight Years (2011-2018)								0.025	
								(0.027)	
Number of Hypothetical Disasters in Last Two Year (2019-2020)								0.057	
								(0.057)	
Number of Hypothetical Disasters in First Nine Years (2011-2019)									0.004
									(0.014)
Number of Hypothetical Disasters in Last Year (2020)									0.1108
									(0.070)
Obs.	375	375	375	375	375	375	375	375	375
Omitted Treatment				Control					
Mean of Dep. Var. for Omitted Treatment:				0.198					
Social-economic Variables	Y	Y	Y	Y	Y	Y	Y	Y	Y
Fixed effects for village and enumerator	Y	Y	Y	Y	Y	Y	Y	Y	Y
Log Likelihood	-221	-218	-218	-219	-221	-223	-222	-222	-222
Pseudo R-square	0.0628	0.0772	0.0763	0.0733	0.0631	0.0563	0.0601	0.0586	0.0586