Do Hypothetical Experiences Affect Real Financial Decisions? Evidence from Insurance Take-up

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DO HYPOTHETICAL EXPERIENCES AFFECT REAL FINANCIAL DECISIONS? EVIDENCE FROM INSURANCE TAKE-UP*

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Abstract

This paper uses a novel experimental design to study the effect of hypothetical personal experience on the adoption of a new insurance product in rural China. Specifically, we conduct a set of insurance games with a random subset of farmers. Our findings show that playing insurance games improves insurance take-up in real life by 48%. Exploring the mechanism behind this effect, we show that the effect is not driven by changes in risk attitudes, changes in perceived probability of disasters, or learning of insurance benefits, but is driven mainly by the experience acquired in playing the insurance game. Moreover, we find that, compared with experience with real disasters in the previous year, the hypothetical experience gained in the insurance game has a stronger effect on insurance take-up, implying that the impact of personal experience displays a strong recency effect.

Keywords: Insurance, Take-up, Game, Experience, Learning
JEL Classification Numbers: D03, D14, G22, M31, O16, O33, Q12

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1 Introduction

Recent literature in economics shows that personal experience plays an important role in decision-making[1]. This finding has implications for the adoption of a variety of products, including insurance policies. However, in many cases, it may take a long time for potential insurance adopters to experience shocks. One example is the adoption of weather insurance. As large weather disasters are not frequent, potential insurance adopters may perceive weather disasters as less salient in their decision-making processes, and they will not be able to experience the benefits of the product even after the insurance is offered. This lack of experience may contribute to the documented low adoption rate for weather insurance in both developed and developing countries[2]. In such cases, can real experience be substituted with hypothetical experience? In this paper, we investigate whether hypothetical experience can influence decision-making processes in the context of weather insurance adoption.

To study the effect of hypothetical experience on real adoption of weather insurance, we conduct a randomized experiment in rural China. The product we are studying is a new rice insurance policy offered by the People's Insurance Company of China (PICC). The farmers in our study have no previous experience with similar agricultural insurance products. Our experimental design allows us not only to identify and measure the causal effect of hypothetical experience on adoption rates, but also to distinguish this effect from other potentially confounding effects, including changes in risk aversion, changes in the perceived probability of disasters, and changes in knowledge regarding the benefits of insurance. Furthermore, using information about the farmers' real experience with disasters, we are able to compare the

[1] Evidence from lab experiments suggests that personal experience, especially recent experience, has a greater influence on personal decisions than exposure to described or observed information (Simonsohn et al. (2008); Hertwig et al. (2004)). In addition, field studies have provided evidence for the effect of experience on both consumer (Haselhuhn et al. (2009)) and investor behavior (Malmendier and Nagel (2011); Kaustia and Knüpfer (2008)).

[2] For example, in a study of farmers in rural India, Giné et al. (2008) find a low take-up rate (4.6%) for a standard rainfall insurance policy. Similarly, Cole et al. (2013) also find a low take-up rate (5%-10%) for a rainfall insurance policy in rural India. In rural China, the take-up rate for a new rice insurance product is less than 20% even when a 70% government subsidy is provided, while the optimal take-up should be more than 70% (discussed in Appendix B).

magnitude of the impact of hypothetical vs. real personal experience on insurance take-up.

In our experiment, we provide hypothetical experience with weather shocks and insurance benefits by playing an insurance game with a set of randomly-selected farmers in each of our sample village. During this game, we first ask a household head whether he/she would like to buy rice insurance in a hypothetical future year. We then play a lottery to see whether there is a weather-related disaster in that year. After the lottery, we help participants calculate their hypothetical income for the year based on whether they had chosen to purchase insurance. We play the game with each participant for 10 rounds to establish a base of hypothetical experience of weather shock and insurance.

Next, one or three days after the game, we visit each participant and ask whether he/she would like to purchase weather insurance. Our results show that participating in the game increases the actual insurance take-up by 9.6 percentage points, a 48% increase relative to the baseline take-up of 20 percentage points. In other words, we find that this effect is roughly equivalent to experiencing a 45 percent greater loss in yield in the past year, or a 45 percent increase in the perceived probability of future disasters.

After observing a large and significant effect of playing insurance games on take-up, it is natural to ask what are the mechanisms that drive the effect. Is the effect due to changes in risk aversion? Is the effect driven by new information acquired during the game, such as, probability of disasters and insurance benefits? Or is it mainly driven by the hypothetical experience with disasters and insurance benefits during the game? Our findings show that that the main mechanism of the game effect is the hypothetical experience itself. This result is obtained in the following manner.

First, to test whether this effect is due to the changes in risk attitudes or perceived probability of future disasters, we elicit subjects' risk attitudes and perceptions of disaster probability after playing the insurance game. We then test whether playing the insurance game affects either risk aversion or the perceived probability of future disasters. Our results indicate that neither one increases by an amount which could generate the observed 9.6

percentage points increase in insurance take-up.

Second, to test whether this effect is from learning about the benefits of insurance, we randomly assign a different set of participants to a group that receives only instruction on how to calculate the payoff of an insurance policy under different situations. However, this treatment only increased the real insurance take-up by 2.7 percentage points, and the effect was statistically insignificant. More importantly, playing insurance games has a larger effect than just receiving a calculation illustration, a difference which is significant at the 5% level. Furthermore, there is no evidence that the Game treatment increased the knowledge of insurance benefit. These results suggest that learning the objective benefits of insurance cannot fully explain the increase in insurance take-up.

Third, to test whether this effect is driven by the hypothetical experience with disasters created by our insurance game, we explore a second source of exogenous variation: the number of hypothetical disasters experienced during the game. Interestingly, we find that, although the total number of disasters does not increase take-up significantly, the number of disasters in last few rounds does increase adoption rates. Specifically, experiencing one additional hypothetic disaster in the last five rounds increases insurance take-up by 6.7 percentage points. This suggests that more recent experience with disasters might be the mechanism that influences the insurance purchase decisions.

Finally, we compare the magnitude of the impact of hypothetical vs. real personal experience on insurance take-up. Specifically, we find that, compared with experience with real disasters in the previous year, experience gained in the insurance game has a stronger effect on actual insurance take-up, implying that learning from experience displays a strong recency effect. To explain this result, we develop a simple model in which agents give less weight to disasters and benefits which they experience infrequently. The structural estimation of our model matches our empirical results (See Appendix B for the details of our model).

Note that our results are particularly relevant to the early stages of diffusion of new technologies that involve risk and uncertainty, such as insurance policies, as well as new fertilizers or hybrid seeds. We find that hypothetical experience obtained during the game does not impact insurance adoption in the long run. However, we do find suggestive evidence that real experience with receiving insurance payouts does impact long-run adoption. Our results thus do not apply to the adoption of products or technologies existed for a long time, when individuals already have real experiences with the product.

This paper contributes to existing literature in several ways. First, our results contribute to the literature on the effect of personal experience on individual decision-making. In particular, this paper is among the first to use a field experiment to disentangle the effects of hypothetical experience from the effects of learning new information. Moreover, although existing studies have shown the effect of experience on consumer (Haselhuhn et al. (2009)) and investor behavior in financial market, (Malmendier and Nagel (2011); Kaustia and Knüpfer (2008)), the effect of simulated experience on household behavior has been unexplored. The closest research to our work is that of Gaurav et al. (2011), who study the impact of financial education obtained via an insurance game on insurance take-up rates in India. The key difference between their study and ours is that we test individual-level variation in hypothetical experience and disentangle other potentially confounding effects from the effects of hypothetical personal experience.

Second, this paper sheds light on the puzzle of why weather insurance has low adoption rates. Although existing research has tested a number of explanations (Giné et al. (2008); Cole et al. (2013)), a lack of experience with disasters and insurance products remains relatively less explored as a possible explanation. To address this gap in the research, we provide evidence showing how hypothetical personal experience can affect the take-up rate for weather insurance.

Third, this paper provides a new perspective to the literature on the use of laboratory experiments to study economic behavior. Laboratory experiments provide controlled institutional contexts which are otherwise exceptionally difficult to obtain and can generate deep insights about economic theories and policy applications (Holt (2005); Plott (2001)). However, one potential limitation of laboratory experiments is that the behavior observed in the laboratory might not be representative of behavior in the field (Levitt and List (2007)). In our study, we demonstrate that laboratory experiments can serve as interventional mechanisms in obtaining field results. We do so by testing the causal effect of the laboratory experiment itself on actual behavior in the field. This design differs from the more commonly used design of having all subjects participate in both a laboratory experiment and a field intervention, and correlating behaviors across the two (Ashraf et al. (2006); Gazzale et al. (2009); Fehr and Goette (2007)).

Note that one difference between our study and most laboratory experiments is that we pay all households in our study a flat fee to eliminate any confounding effects due to income [3]. In our study, it is interesting to note that, even without a financial incentive, we still observe a large treatment effect. Future research could explore whether experiments that provide monetary incentives for insurance adoption provide similar results.

The rest of the paper is organized 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, where we present the main treatment effect of the insurance game on actual insurance take-up and analyze the possible mechanism driving this effect. In Section 5, we discuss the dynamics of the take-up decision during the hypothetical game. Section 6 concludes.

2 Background

Nearly 50 percent of farmers in China produce rice, which represents the staple crop for more than 60 percent of the Chinese population. In 2009, The People's Insurance Company of China designed the first rice insurance program and offered it to rural households in 31

[3] The literature on financial incentives in experiments suggests that, when there is no clear standard of performance in experiments that involve risk-based choices, incentives often cause subjects to move away from social desirable behavior toward more realistic choices. If social desirability depends on subject-experimenter interaction, then households might buy more insurance during the games because of a demand effect.

pilot counties[4].

To study the effect of hypothetical experience on the adoption of rice insurance, we conduct our experiment across 16 natural villages within two of the rice production counties included in the government's first round pilot of the insurance program. Our sample counties are located in Jiangxi province, which is one of China's major rice bowls[5]. All households in these villages were offered the formal rice insurance product. Since the product was new at that time, no household had previously heard of such insurance.

The insurance contract is depicted in Figure I. The full insurance premium is 12 RMB per mu per season[6]. Since the government subsidizes 70 percent of the premium, households are required to pay only 3.6 RMB[7]. The policy stipulates that the policyholder is eligible to receive a payment if he/she experiences a 30 percent or greater yield loss due to any of the following reasons: heavy rain, floods, windstorms, extremely high or low temperatures, or drought [8]. The loss rate in yield is determined through an investigation by a group of insurance agents and agricultural experts. The payout amount increases linearly with the size of the loss in yield.

To illustrate this policy, let us consider the case of a farmer growing rice within a two mu area. The normal yield per mu is 500kg; however, a wind disaster has reduced this year's yield to 300kg per mu. Since this represents a 40% yield loss, the farmer will receive 200*40% = 80 RMB per mu from the insurance company. Note that the maximum insurance payout

[4]Although there was no insurance before 2009, there were other mechanisms by which the consequences of weather shocks on farmers and their crop yields were mitigated. For example, if major natural disasters occurred, the government made payments to households whose production had been seriously hurt. However, the level of transfer in these cases was usually very limited and far from sufficient to help farmers resume production. In addition, households within villages would sometimes participate in informal risk-sharing in the case of non-aggregate weather shocks.

[5]Note that "natural village" refers to the actual village, whereas "administrative village" refers to a bureaucratic entity that contains several natural villages.

[6]1 RMB = 0.15 USD; 1 mu = 0.165 acre. Farmers produce two or three seasons of rice each year. The average annual gross income per capita in the study region is around 5000 RMB.

[7]Since a typical household has around 1000 RMB saved at home, liquidity constraints are not a problem in this case.

[8]The options for reporting losses are stated on the insurance contract. Farmers can report a loss either by calling the insurance company directly or by reporting the loss to village leaders, who then inform the insurance company.

is capped at 200 RMB, but the medium gross income for farmers in our sample is around 855 RMB per mu. Consequently, the insurance policy covers, at most, 25 percent of the rice production income [9].

It is also important to note that the post-subsidy premium is below the actuarially fair price. Specifically, the profit of the insurance company equals revenue minus the payment to households and the fixed cost:

$$\pi = N * Premium - N * P * Payout - FC$$

, where P is the probability of future disasters, N is the number of households who buy insurance, and indemnity is the payment to households when there is a disaster. Furthermore, the actual probability of a disaster that leads to a 30 percent or greater yield loss is around 10 percent, according to local government sources. Since N*3.6 $<$ N*10%*60, the post-subsidy price is below the fair price. However, since the pre-subsidy price is higher than the fair price, the insurance company earns a profit when the fixed costs are relatively low.

3 Experimental Design

We use a randomized experiment to measure the role of hypothetical experience in influencing insurance adoption and to separate it from other confounding factors. As mentioned, our experiment includes 16 randomly selected natural villages in Jiangxi province of China[10]. The experiment was conducted in the Summer of 2009 and the Spring of 2010. 9 hired enumerators consisting of government officials and primary school teachers, together with the two authors, conducted surveys of 885 households across 16 villages prior to the beginning of the rice growing season.

[9] Given that the daily wage in rural China is around 20 RMB per day, the maximum amount of payout per mu is equivalent to 10 days' earnings.

[10] In these villages, the most recent large weather disaster was flood that occurred in 1998. This disaster caused a more than 80% loss in yield, on average. By comparison, the average annual loss rate in yield in the nine years from the disaster to the study was around 9%.

The timeline for our experiment is presented in Figure II. The experiment consists of two rounds of interviews for each household, with either one or three days between the two rounds. In round 1, the procedure is as follows: the enumerators provide the household with a flyer detailing the information about the insurance contract, including liability, period and premium information. We then administer the baseline survey. After that, we randomly assign each household to one of four interventions, explained below. At the end of round 1, households are asked to think about whether they would like to buy the rice insurance or not, and that we would come back a few days later to ask them to make a decision. We then conduct round 2 either one or three days later. During this round, the enumerators ask the farmers to indicate their purchase decisions. The decisions are conveyed to the insurance company, which then collects the premium[11]. We pay each household in the study 5 RMB to compensate for participant time.

The experimental design is illustrated in Figure III. Our experiment has a 4 by 2 design, which consists of 4 groups that differ on how the contract is explained to them and 2 groups that differ on whether they receive information on the true disaster probability[12].

As mentioned, we randomly assign the households in our group into one of four intervention groups. These interventions differ in how the insurance program is explained to the participants. These groups consist of the control group, the calculation-only group, the game 20%-disaster-probability group, and the game 10%-disaster-probability group. We use two different disaster probability conditions so that we can study insurance adoption behavior when the probability is either similar to or greater than the real probability of a weather disaster. The detailed procedure for each group is as follows.

[11]Note that, in round 1, the enumerators are randomly assigned to households while in round 2, a single enumerator visits one or more villages. In our sample, 22 percent of the households (196 households) are visited by the same enumerator in round 1 and round 2.

[12]Before the randomization, we first approached the leaders of the villages and obtained a list that included the names of villagers and basic information about them. In determining our sample, we excluded households that did not grow rice but instead had heads of households who had job aspirations in urban areas or who were using their land for livestock. We stratified our remaining households according to natural village, age of head of household, and total area of rice production. In each stratum, households were randomly assigned to one of eight interventions.

In the Control group, the enumerators give the household a flyer with information about the rice insurance program and explain the insurance contract briefly. The household head is then asked to fill out a short survey regarding age, education, experience with insurance, experience with weather-related disasters, rice production level, extent of social network, risk attitudes and perceptions of the probability of future weather disasters.

In the Calculation group, the enumerators follow the same procedure as in the control group, but also demonstrate how to calculate the expected benefit of buying insurance if zero, one, two or three disasters were to occur at any time in the following ten years. The details of the calculation examples provided to participants are illustrated in Table A1. In addition to demonstrating the calculations, the enumerators provide the following statement to each participant: "According to our calculations, if there is no large disaster in the next 10 years, it is better to not buy any insurance in the following 10 years. If there is at least 1 relatively large disaster, it is better to always buy insurance in the following 10 years."

In the Game 20% (respectively, 10%) group, the enumerators follow the same procedure as in the control group, but then play a hypothetical insurance game with the participant. The game includes ten rounds, representing the years 2011-2020, respectively, with the same procedure repeated in each round.

The following illustrates the structure of the game. The household head is first asked whether he or she would like to purchase insurance in the year 2011. After indicating this decision, the participant then plays a lottery which reveals whether a disaster occurs in that year. In the lottery, the participant draws a card from a stack of ten cards. In the Game 20% (10%) case, two (one) out of ten cards signifies a disaster. After the lottery result is revealed, the enumerator and the participant calculate the income from that year based on the assumed expected income per acre and any insurance payment (as shown in Table A2). The game is then played for another nine rounds, representing years 2012 to year 2020, respectively. In each round, the participant draws from a deck of ten cards to determine whether a disaster occurs in that year[13]. At the end of the game, the participant receives the same statement as the Calculation group. Note that the game treatment provides not only explanations of the benefits of insurance products, but also a randomized number of hypothetical disasters.

In a crossed randomization, we randomized whether households were informed of the actual probability of disaster at the end of round 1, which is 10% according to the private communication with local government officials. The objective of providing this randomization is to help us test whether the insurance game conveys more information than the probability of disasters. This randomization is interacted with how the contract is explained and thus we have eight groups in total.

To summarize, the Calculation treatment provides households with only information about the expected benefits of insurance. By contrast, the Game treatment makes households acquire (hypothetical) disaster experience and thus provides households with (hypothetical) disaster experience as well as information about the benefits of insurance. The (crossed) Information treatment provides households with information about the risk of disaster. Note that, as the Game treatment takes longer than the calculation and control group, to control for the time effect, we add some irrelevant survey questions in the latter two groups to control for any possible time effect.

To study whether our effects are due to changes in risk attitudes or perceptions of future disaster probabilities, we obtain information on these measures in round 1. For those participants assigned to either of the Game groups, we obtain this information after participants have played the insurance game, while for the Calculation group, we obtain this information before the intervention[14]. We elicit risk attitudes by asking participants to make a hypothetical choice between a sure amount of monetary offer (riskless option A) and a risky gamble

[13]Our experimental set-up implies that 89 percent of participants in the Game 20% group and 65 percent of the participants in the Game 10% group are expected to experience at least one disaster across the 10 rounds of the game. Our results indicate that 82 percent of households in the Game 20% group and 66 percent of households in the Game 10% group experience at least one disaster.

[14]We do not ask them the same questions before the game as their answers may reflect participant consistency rather than any potential treatment effect.

(risky option B). These options are outlined in Table A3. We use the number of riskless choices as a measure of risk aversion. The perceived probability of future disasters is elicited by asking participants the following question: "what do you think is the probability of a disaster that leads to a more than 30 percent yield loss next year?" We clarify this question with a simple example to illustrate the concept of probability to the participants[15].

To test whether the Game treatment effects are due to changes in knowledge about insurance benefits, we asked a few questions to test farmers' understanding about probability and insurance benefits in round 1 visit. For those participants assigned to either of the Game groups, we obtain this information after playing the insurance game, while for the the other groups, we ask for this information before the intervention. In this way, we can test whether the Game treatment increase farmers' insurance knowledge. Specifically, probability question is "If you roll a six-side dice for 100 times, how many times will you see number 6?"; Insurance Benefit Question is "Suppose your gross income is 1000 RMB per mu, the loss from disaster is 400 RMB, insurance premium is 3.6 RMB, you get 80 RMB from insurance company if there is a disaster and you buy the insurance. What is your income per mu if there is a disaster but you did not buy insurance? What is your income per mu if there is a disaster and you bought the insurance?"

4 Empirical Results

4.1 The Impact of Hypothetical Experience on Actual Insurance Take-up

In this section, we discuss the empirical results we obtain from our experiment. Note that we use the term "Game" to refer to the combined Game 20% and Game 10% groups. As shown

[15]The enumerators gave participants 10 small paper balls and asked them to put these paper balls into two areas: (1) no disaster resulting in yield loss of more than 30% for the next year and (2) a disaster resulting in yield loss of more than 30% for the next year. If households put 2 paper balls into (2) and 8 paper balls into (1), their perceived probability of future disaster is around 20%.

in Figure IV, we find that the insurance take-up rate for the control group is 19.8 percent, while that of the calculation group is 24.7 percent. By contrast, the insurance take-up rate for the game group is 32.3 percent. To see whether these effects are statistically significant, we run the following logit regression in (1):

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

where buy_{ij} is an indicator that takes on a value of one if household i in natural village j buys the insurance. Furthermore, 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} represent head of household characteristics (e.g., gender, age, years of education, household size, area of production, car ownership, etc) and α_j and α_k represent village fixed effects and enumerator fixed effects, respectively. ϵ_{ij} is the 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 the marginal effects obtained from our analysis in Table II. Column (1) presents the results from the simplest possible specification, where the only right-side variables are the indicators for the game treatment, the calculation treatment, the village fixed effects, and the enumerator fixed effects. These results show that the marginal effect of the game treatment (0.096) is positive and significant at the 5% level. This result means that participation in the game treatment increases the insurance take-up rate by 9.6 percentage points, representing a 48 percent increase relative to the baseline take-up rate of 20 percentage points. Furthermore, the results in column (1) indicate that the marginal effect of the calculation treatment (0.027) is insignificantly positive[16].

[16]Since there is one to three days between the intervention and the decision-making, there might be spillover effects on insurance take-up. Thus, our estimated treatment effects are likely to be a lower bound of true treatment effects

In column (2), we present the results when we include the previous year's self-reported yield loss and a dummy for missing values. The results are similar to those reported in column (1). That is, we find that the marginal effect of the previous year's yield loss is 0.22%, which is significant at the 10% level. Therefore, we conclude that the effect of the insurance game treatment on insurance take-up rates is of roughly the same magnitude as the effect of a 45 percentage point increase in actual yield loss in the previous year.

In column (3), we present the regression results when we include a variety of other control variables and dummies for missing values. The results are similar to those reported in column (1). Furthermore, we find that education level is positively correlated with insurance take-up while household size is negatively correlated with take-up. In column (4), we present the game effects in both the Game 10% group and the Game 20% group, separately. These results show that the effect of the insurance game treatment on insurance take-up is higher, albeit insignificantly, for the Game 20% group vs. the Game 10% group. This finding provides suggestive evidence that the game treatment is more effective when it more closely reflects previous disaster probability (20% for our sample of households) or when people experience more hypothetical disasters in the game. We discuss these findings in greater detail in Section 4.2.4.

The treatment effect (Game/Calculation) can be also different depending on whether farmers were provided with information about the actual probability of disasters. The results in Figure V indicate different findings for those groups that receive information on the true probability of a disaster and those that do not receive such information. To test this finding explicitly, we re-run estimation (1), restricting each sample to those households in the no information and information groups, respectively. The results, reported in columns (5) and (6) in Table II, show that, when no information regarding true probability is provided, the marginal effect of the game treatment on insurance adoption (0.126) is positively significant. However, for the information group, we find that the marginal effects of the game and calculation treatments are each negatively insignificant. The difference in marginal effects between the information group and the no information group is significant at the 10% level. This finding suggests that the game and calculation treatments are not as effective once farmers know the actual probability of disasters.

It should be noted that our information group sample size is small. Consequently, our tests may not have enough power to estimate the effect precisely. Given that we do not have a sufficiently large sample in the information group, and neither the game nor the calculation effect is significant for the information group, we use the results for the whole sample and for the no-information group when discussing possible mechanisms for the game effect in the next section. [17]

We next test the heterogeneity of the game treatment effect. According to Table III, the results in columns (1)-(5) suggest that the magnitude of the game effect does not change by participant age, education, household size, or production scale. Moreover, column (6) of Table III indicates that our findings are similar for participants regardless of the level of their perceived probability of disasters.

In sum, our results show that the game treatment increases the insurance take-up rate by 9 to 10 percentage points, resulting in an increase of around 45 to 50 percent relative to the baseline take-up rate of 20 percentage points [18] [19]

[17] Another possible explanation for the insignificant results in the information group is that participants care more about the true probability of disaster. If this is the case, then we should see a change in perceived probability as the main mechanism driving the results for the no-information group. However, this is not the case, as we will show in Section 4.2.2.

[18] In Table A4, we test whether this treatment effect diminishes over time. The results in column (4) indicate that the longer the gap between round 1 and round 2, the lower the take-up rate. Interestingly, we find that the calculation effect decreases over time while the game effect remains. This suggests that people retain the game experience longer than they retain the calculation illustration.

[19] Although playing an insurance game has a large and positive effect on insurance take-up, even with this intervention, the overall take-up rate is still relatively low (30%). This suggests that there are other barriers to insurance adoption beyond experience level. One such barrier may be limited trust in the insurance company. Indeed, according to the results in Table A9, those who have purchased life insurance or asset insurance before are less likely to buy rice insurance. The reason for this finding could be that these households have had a negative experience and, as a result, have lost their trust in insurance companies. To further test this possibility, we construct several measures of trust, relating these to rice insurance adoption decisions. The results in column (1) of Table A10 show that the self-reported level of trust in insurance companies is positively correlated with rice insurance take-up. Column (2) reports the results when we measure trust by a dummy variable which equals one if a household has received a payout from another insurance contract and zero otherwise. These results show that those who have received an insurance payout in the past are more likely to buy rice insurance. Together, these results support the suggestion that the

4.2 Possible Mechanisms Driving the Game Effect on Insurance Take-Up

After observing a large and significant effect of the game intervention on insurance adoption, it is natural to ask what the underlying mechanism driving this effect is. In this study, we consider four possible mechanisms: (1) changes in risk attitudes, (2) changes in the perceived probability of future disasters, (3) changes in knowledge levels regarding the benefits of insurance, and (4) changes in hypothetical experience with disasters. In this section, we consider each mechanism in turn.

4.2.1 Changes in Risk Attitudes

First, we consider the possibility that the game increases insurance adoption because it changes participants' attitudes toward risk. To test this possibility, we ask whether the game treatment can change people's risk attitudes to the extent that it can generate an impact on insurance take-up that is as large as the game effect. To do so, we stack three equations, generate indicators for each equation, and then estimate the regression system. Our equations are indicated as follows:

$$buy_{ij} = \alpha_j + \alpha_k + \beta_{risk} risk_{ij} + \beta_{prob} prob_{ij} + \phi X_{ij} + \delta_{ij} \tag{2}$$

$$risk_{ij} = \alpha_j + \alpha_k + \gamma_{gr} Tg_{ij} + \gamma_{cr} Tc_{ij} + \phi X_{ij} + \eta_{ij} \tag{3}$$

$$risk_{ij} = \alpha_j + \alpha_k + \beta_{dr} disaster_{ij} + \phi X_{ij} + \omega_{ij} \tag{4}$$

where $risk_{ij}$ is a measure of risk aversion and $disaster_{ij}$ is the number of hypothetical disasters that a participant experiences during the games. Equation (2) represents the correlation between insurance take-up and risk attitudes. Note that we restrict the sample in Equation (2) to the control and calculation groups, as these are the groups that received a

level of trust in insurance companies impacts rice insurance take-up decisions.

pre-intervention survey on their risk attitudes. In Equations (3) and (4), we estimate the effects of the insurance game and disaster experiences in the game, respectively. In our analysis, we assume that there is no measurement error in risk attitudes and that the estimation in Equation (2) is unbiased. To account for the correlation of error terms between each equation, we cluster standard errors by the 16 natural villages in our sample[20].

Using the above equations, we next conduct our regressions and present the results in Table IV. The results in column (1) indicate a positive coefficient of risk aversion (0.032), which is significant at the 5% level. Furthermore, the results show a positive coefficient of perceived probability of future disasters (0.0214), significant at the 10% level. Column (2) presents our estimates for equation (3), including various controls and dummies for missing values, while column (3) presents the results when we restrict our sample to participants in the game intervention group. Overall, the results show that the game treatment has no effect on risk aversion. Consequently, we conclude that changes in risk aversion cannot be the explanation of the game effect on insurance take-up. To further confirm this result, we test the following two hypotheses:

$$\beta_{risk}\gamma_{gr} = \beta_g \tag{5}$$

$$1.48\beta_{risk}\gamma_{gr} = \beta_g \text{ [21]} \tag{6}$$

The first hypothesis is rejected at the 5% level (p=0.039), with a 95% confidence interval of [-0.013, 0.011], while the second hypothesis is also rejected at the 5% level (p=0.044), with a 95% confidence interval of $1.48\beta_{risk}\gamma_{gr}$ ranging in [-0.004, 0.004]. Overall, these results suggest that changes in risk attitudes are unlikely to explain the game effect.

4.2.2 The Perceived Probability of Future Disaster

In this section, we explore the possibility that the game intervention condition increases participants' perceived probability of future disasters. We use a similar strategy as in 4.2.1

[20] This procedure is discussed in greater detail in the Appendix.
[21] 1.48 is average number of hypothetical disasters they experience during the games.

to test for this possibility. Specifically, we run the following regressions:

$$prob_{ij} = \alpha_j + \alpha_k + \gamma_{gp} Tg_{ij} + \gamma_{cp} Tc_{ij} + \phi X_{ij} + \eta_{ij} \tag{7}$$

$$prob_{ij} = \alpha_j + \alpha_k + \beta_{gp} disaster_{ij} + \phi X_{ij} + \omega_{ij} \tag{8}$$

where $prob_{ij}$ is the perceived probability of a future disaster. In Equations (7) and (8), we estimate the effects of the game intervention and previous experience with disasters, respectively. The results of (7) and (8) are presented in columns (4) and (5) in Table IV, respectively.

The results in column (4) indicate that the game treatment has an overall negative effect on the perceived probability of future disasters. We further see that the coefficient for the number of hypothetical disasters is not significant. To test whether the influence of the game treatment on the perceived probability of a disaster is the main mechanism driving our game effect, we test the following two hypotheses:

$$\beta_{prob} \gamma_{gp} = \beta_g \tag{9}$$

$$1.48 \beta_{dp} \gamma_{gp} = \beta_g \tag{10}$$

Both hypotheses are rejected at the 5% level. As a result, we conclude that our game effect is unlikely to be driven by a change in participants' perceived probability of future disasters.

Finally, to determine whether combined changes in risk attitudes and perceived probability of future disasters drive our results, we test the following two hypotheses:

$$\beta_{risk} \gamma_{gr} + \beta_{prob} \gamma_{gp} = \beta_g \tag{11}$$

$$1.48 \beta_{dr} \gamma_{gr} + 1.48 \beta_{dp} \gamma_{gp} = \beta_g \tag{12}$$

We reject both hypotheses at the 5% level. Overall, these results suggest that changes in risk attitudes and the perceived probability of future disasters are unlikely to explain our main treatment effect.

4.2.3 Changes in Knowledge Regarding the Benefits of Insurance

It is also possible that playing insurance games improves knowledge about the benefits of insurance, which then impacts the decision to adopt insurance. We use two strategies to test this possibility.

First, we compare the effects of the game and calculation treatments. If a change in knowledge regarding insurance benefits is the main driver behind the game effect, then we should see no significant difference in insurance take-up between the game and calculation treatments, as each provides the same information about insurance benefits. To compare the two treatments, we run various regressions with (1) and report the p-value of a Ward test $\beta_g = \beta_c$ in Table II. Columns (1)-(4) present our results across the whole sample. These results show an insignificant difference of around 7 percentage points between β_g and β_c (with a p-value for the Ward test between 0.13 and 0.16). Column (5) presents the results when we restrict the sample to the no information group. In this case, the difference between β_g and β_c is around 11 percentage points and is significant at the 5% level.

Second, we asked a few question to test farmers' understanding about probability and insurance benefits after the Game treatment. We run the following regression to test whether the Game treatment affects knowledge:

$$knowledge_{ij} = \alpha_j + \alpha_k + \beta_g Tg_{ij} + \phi X_{ij} + \epsilon_{ij} \tag{13}$$

where $knowledge_{ij}$ is an indicator that takes on a value of one if household i in natural village j answer the question correctly [22]. We report the results for Probability Question

[22] We measure the correctness of the answer by whether the answer is exactly correct. We also use a fuzzy measurement in which the answer is regarded as correct if it is close to the exact number. The results are similar to those using the exact measurement and it is not reported

and two Insurance Benefit Questions in Column (1), (3) and (5) in Table V. The coefficients of the Game treatment are small and insignificant. However, while the overall effect of game treatment on knowledge is insignificant, people who experienced more disasters during the game might have learned insurance knowledge better. As a result, we further test the impact of the number of hypothetical disasters experienced on insurance knowledge:

$$knowledge_{ij} = \alpha_j + \alpha_k + \beta_g Tg_{ij} + \beta_{disaster} disaster_{ij} + \phi X_{ij} + \epsilon_{ij} \tag{14}$$

We report the results in Column (2), (4) and (6) in Table V. The coefficients of Number of Hypothetical Disasters are negative, insignificant and close to zero. Thus, we conclude that there's no evidence that the Game treatment increase the knowledge of insurance benefit.

In sum, our analyses show that changes in knowledge about the benefits of insurance are unlikely to explain the treatment effect of an insurance game for those participants who do not receive information about the true probability of a disaster. By contrast, for those participants who receive information on the true probability of a disaster, the results are only suggestive in discounting changes in knowledge as a driver of the game effect. Given that neither the game nor the calculation effect is significant in the information group in the first place, we conclude that changes in knowledge about insurance benefits cannot explain the game effect for either our overall sample or our no-information subsample.

4.2.4 Changes in Hypothetical Experience

The final possible explanation that we consider is that the hypothetical experience gained during the game is the driver behind the effect of the game on insurance adoption rates. To test this hypothesis, we take advantage of the randomization of the number of hypothetical disasters during the game to test the effect of that on real insurance purchase decisions.

In Figure VI, we plot the average actual insurance take-up across groups of households with different numbers of hypothetical disasters experienced during the games. In the Game

20% group, we find that the take-up rate among participants who experienced two or more disasters during the game is higher than that among those who experienced zero or one disaster. In the Game 10% group, we find that the take-up rate among participants who experienced one disaster during the game is higher than that among those who experienced zero or two and more disasters. However, we also note a relatively large standard deviation. Consequently, Figure IV provides only suggestive evidence that the take-up rate is increasing in the number of hypothetical disasters experienced or that the take-up rate for the group with no hypothetical disasters is greater than that of the control group. To further test these findings, we estimate the marginal effect in equation (15):

$$buy_{ij} = \alpha_j + \alpha_k + \gamma_{gr} Tg_{ij} + \gamma_{cr} Tc_{ij} + \beta_{disaster} disaster_{ij} + \phi X_{ij} + \delta_{ij} \tag{15}$$

where $disaster_{ij}$ is the number of hypothetical disasters experienced during the games. We present the results in columns (1) and (5) of Table VI. From column (1), we see that the coefficient (0.059) is positive and statistically significant at the 10% level. By contrast, the coefficient for the no information group (0.055) is insignificant (p=0.127). However, the magnitude of this coefficient is similar to that for the whole sample, so the insignificant result may reflect an insufficient sample size. Overall, we conclude that the observed treatment effects are driven mainly by those who experienced more hypothetical disasters during the game [23].

In further exploring the effect of hypothetical experience on insurance adoption decisions, we consider the effect of number of hypothetical disasters experienced during the game on take-up, using the following regression:

$$buy_{ij} = \alpha_j + \alpha_k + \beta_0 disaster0_{ij} + \beta_1 disaster1_{ij} + \beta_2 disaster2_{ij} + \beta_3 disaster3_{ij} + \phi X_{ij} + \epsilon_{ij} \tag{16}$$

[23] In Table A5, we provide the results of a comparison of the observable characteristics of participants who experienced relatively more or fewer disasters in the game. We find that most characteristics are balanced across hypothetical disaster experience, with the exception of rice income as a proportion of overall income in the wave 3 sample.

where $disasterK_{ij}$ is an indicator that takes on a value of one if a participant experiences K disasters during the game. Furthermore, β_0 captures the understanding effect and the difference between β_0 and other coefficients captures the experience effect.

The results for the marginal effect of Equation (16) are presented in columns (2) and (6) in Table VI. Specifically, we find that the coefficients for $disaster0_{ij}$ and $disaster1_{ij}$ are each positive but not statistically significant. By contrast, the indicators for a greater number of disasters are positive and statistically significant. For the no information subsample, the coefficients (presented in column (6)) are positive, significant, and relatively larger than those for the entire sample. 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, this estimation provides suggestive evidence that the results are consistent with a experience effect.

To further explore how hypothetical experience influences take-up, we next distinguish between when participants experience a disaster across the 10 rounds of the insurance game. Specifically, we explore the effect of disaster experience on insurance take-up when participants experience a disaster in the first five vs. last five rounds of the game. The results, presented in Figure VII, suggest that the number of hypothetical disasters experienced in the last five rounds of the game appears to have a larger effect than the effect of that experienced in the first five rounds on insurance adoption. To test this finding further, we create two new variables: the number of hypothetical disasters in the first five rounds and the number of hypothetical disasters in the last five rounds. We then run the following regression:

$$buy_{ij} = \alpha_j + \alpha_k + \gamma_{gr} Tg_{ij} + \gamma_{cr} Tc_{ij} + \beta_{f5} disasterfirst5_{ij} + \beta_{15} disasterlast5_{ij} + \phi X_{ij} + \delta_{ij} \quad (17)$$

As seen in column (3) of Table VI, the coefficient for "disaster experience in the first half of the game" is negative and insignificant. By contrast, the coefficient for "disaster experience in the last half of the game" is positive and significant at the 5% level[24]. The latter coeffi-

[24]See Appendix Table A6 for further detail.

cient suggests that experiencing an additional disaster in the last half of the game increases insurance take-up by 7.0 percentage points. Examining the results for the no information subsample (presented in column (7)), we find that the coefficient for the last five rounds is also positive and significant at the 5% level. Indeed, this pattern is robust to different measures of disaster experience in the first and last rounds. Specifically, the results in columns (4) and (8) represent those when we add an interaction term between $disasterfirst5_{ij}$ and $disasterlast5_{ij}$. Although the effect of disaster experience in the last five rounds of the game decreases with the number of disasters experienced in the first five rounds, $disasterlast5_{ij}$ is still jointly significant with the interaction term.

Furthermore, if we regress insurance 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 for the last n rounds are positive and significant at the 5% level (Table A7). These results are consistent with those in the literature related to experienced utility and recency effects (Fredrickson and Kahneman (1993); Schreiber and Kahneman (2000)). In this literature, studies show that the affect experienced during the last moments of an experiment has a privileged role in subsequent evaluations, and that late moments in an experiment are assigned greater weight by participants than are earlier ones.

To summarize, we find that both the total number of disasters experienced as well as the number of disasters experienced in last few rounds increase subsequent insurance take-up significantly. These results suggest that recent experience with hypothetical disasters may be the mechanism by which an insurance game influences insurance decisions.

4.2.5 Other Possible Mechanisms Influencing Insurance Take-Up

In addition to the four mechanisms discussed above, we consider a further additional possible explanation for our game effect: participants may learn information from the game about the average disaster yield loss, which is assumed to be 40%. To explore this explanation, we estimate the heterogeneous treatment effect based on participants' average loss rate in yield in previous years. The intuition behind this test is that those who experienced lower losses in recent years are more likely to learn the average loss in yield from the game than those who experienced higher losses. As a result, if learning about the average yield loss is the main driver of our results, we should see a smaller effect of the game treatment for those who experienced higher losses in recent years. As the results in column (1) of Table A8 show, the coefficient of the interaction between the game treatment and losses in recent years is negative, but it is insignificant and very close to zero [25]. Thus we conclude that learning about the average loss rate during the game is unlikely to explain our game effect.

5 Dynamics of Insurance Take-up during the Game

During each round of the game, participants make new take-up decisions based on their experience and information received in previous rounds of the game. In this section, we explore the dynamics of their insurance take-up decisions during the hypothetical game.

To study the dynamic behavior during the game, we create two indexes. The first one is an S index which measures the switching behavior during the game. To construct the S index, we first calculate the switching rate C_t, conditioned on whether a participant experienced a disaster in the previous round. A switch is defined to be 1 (-1) if a participant switches from not buy (buy) to buy (not buy), and zero otherwise. We next calculate the switching rate C_t, which measures the switching rate across all participants for that round. This helps to capture the fixed effect of the year on switching decisions. The S index thus measures the normalized switching rate conditional on whether participants experienced a disaster in the previous round.

$$C_t | disaster_{t-1} = \frac{Switch, d_{t-1}}{N, d_{t-1}} \tag{18}$$

$$\bar{C}_t = \frac{Switch}{N_{t-1}} \tag{19}$$

[25] One explanation for this result could be that households that experienced greater losses paid greater attention during the game.

$$S_t = C_t - \bar{C}_t \tag{20}$$

According to the results in Figure VIII, conditioned on experiencing a disaster in a previous round, the S index is positive in eight out of nine rounds, with the first two rounds exhibiting the largest S indexes [26]. Since both take-up and switching behaviors are endogenous, this suggests that experiencing a disaster increases the likelihood of switching, with a greater effect of disaster experience in the first two rounds of the insurance game.

Our second index is an L index that measures the insurance take-up rate conditional on disaster experience in the first round of the game. The formula for our L index is given by the following:

$$L_t = \frac{E(buy_t|disaster_{2011})}{E(buy_t)} = \frac{Prob(takeup_t|disaster_{2011})}{Prob(takeup_t)} \tag{21}$$

The results in Figure IX show the L index across different rounds of the game. Conditioning on experiencing a disaster in the first round of the game (year 2011), we find a positive L index across the subsequent nine rounds of the game. Furthermore, a two-sample t-test shows that, in each round, participants who experience a disaster in the first period are more likely to buy insurance; this effect is significant at 10% level for five of the nine subsequent rounds. A logistics regression shows that experiencing a disaster in the first round increases insurance take-up in the second period (significant at the 5% level) as well as in subsequent periods (significant at the 1% level). This suggests that experiencing a disaster in the first round of the game is effective in increasing follow-up insurance take-up throughout the rest of the game.

In sum, we find that experiencing hypothetical disasters increases insurance take-up rates during the game. These results are consistent with our explanation in Section 4.2.4 that hypothetical experience also increases actual insurance take-up. That is, if hypothetical

[26] A two-sample t-test shows that, in the first two rounds, the S index is larger for participants who experienced a disaster in the previous round, significant at the 5% level. Furthermore, an ordered logistics regression shows that experiencing a disaster in first two rounds increases the probability of switching from not buying to buying; this result is significant at the 1% level

experience drives our main treatment effects, then we should see an effect of hypothetical experience on both hypothetical and real insurance adoption behavior. Thus, these results about dynamics of take-up during games are consistent with our experience story.

6 Conclusions

Despite the important role of personal experience in affecting decision-making, in many cases it takes a long time for people to acquire real experiences. The lack of experience can be an important reason of the slow diffusion of new technologies and policies. To better understand how to remove this barrier, we apply a unique experimental design to examine whether hypothetical experience can substitute real experiences to influence households' decision-making. We answer this question in the context of new weather insurance adoption. Overall, we find that playing an insurance game increases the actual insurance take-up rate by 9.6 percentage points, a 48% increase relative to the baseline take-up rate of 20 percentage points after being offered such insurance. After investigating possible mechanisms that could be driving this effect, we find that exposure to hypothetical disasters is the likely explanation for our findings.

While our results show a significant effect of the insurance game in the short term, it is interesting to see if the game has long-term effects. To answer this question, we followed up with a subsample of households one year later, and estimated the effect of the game treatment on actual insurance take-up in the second year. The results in Table VII show no significant effect of game treatment on insurance adoption one year later. This finding could be due to several reasons. First, the saliency of the hypothetical experience could diminish over time. Second, real life experience in the year following the game could dictate the insurance adoption decision (Cai (2012), Karlan et al. (2012)). Finally, it is possible that our sample is of insufficient size to capture any potential long-term effects of the game treatment[27].

[27] We obtain information for only 191 households in the second year as the rest of the households in our

From the policy perspective, this paper suggests that using a subsidy policy alone may not be effective in encouraging adoption of beneficial new financial products. Our calibration (in Appendix B) shows that, although the rice insurance product in our study is highly subsidized, rural households still buy less insurance than they should when they voluntarily make their decisions. One barrier to the slow diffusion of new insurance products that policy makers should consider is the lack of personal experience of disasters and the product benefits.

Finally, from a methodological standpoint, this paper is among the first to use a laboratory experiment as an intervention in a field experiment. Specifically, we find that exposure to a condition in the laboratory experiment influences subsequent field behavior in our setting. By using a laboratory experiment, we can explicitly manipulate more variables which are endogenous or otherwise difficult to manipulate. For example, Malmendier and Nagel (2011) find that individuals who have experienced low stock-market returns are less likely to participate in the stock market. However, such experience is difficult to manipulate in a field setting. By contrast, in our study, we use a laboratory experiment to simulate outside experience and thus influence participant field behaviors. It would be interesting to consider other settings in which laboratory experiments can lead to effects on field behavior to better understand what factors influence individual decision making.

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sample were required by the government to purchase insurance in the second year.

Appendices

A Supplementary Figures and Tables

Table A1. Description of the Calculation Treatment

Number of disasters in 10 years	Total ten years' income if you purchased insurance every year	Total ten years' income if you did not purchase insurance in any year
0	99640=10000-3.6*10mu*10year	100000=1000*10mu*10year
1	96440=96000-360+200*40%*10mu*1year	96000=100000-400*10mu*1year
2	93240=92000-360+200*40%*10mu*2year	92000=100000-400*10mu*2year
3	90040=88000-360+200*40%*10mu*3year	88000=100000-400*10mu*3year

Table A2. Description of the Game Treatment

Up-take	Disaster	Income (RMB)	Note
NO	NO	10000=1000*10 mu	Assume when there's no disaster, the gross income per mu is 1000 RMB
NO	YES	6000=600*10	Assume if a 40% disaster happened, the gross income per mu is 600 RMB
YES	NO	9964=1000*10-3.6*10	Assume when there's no disaster, the gross income per mu is 1000 RMB, and the premium is 36 RMB in total.
YES	YES	6764=600*10-3.6*10+200*40%*10	Assume if a 40% disaster happened, the gross income per mu is 600 RMB, and the premium is 36 RMB in total. The payout per mu is 200*40%=80 RMB.

Year	Do you buy the insurance?	Why do you buy/not buy the insurance	Have you experienced disaster in this year?	Income in this year
2011				
2012				
…				
2020				

Table A3. Questions to Test Risk Aversion

	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.	

Notes: 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 A4. The Effect of the Day Gap Between Treatment and Decision-making on Insurance Take-up

Specification:	Logistic regression			
Dep. Var.:	Individual adoption of insurance			
Sample:	All Sample	Information	Information	All Sample
	(1)	(2)	(3)	(4)
Game	0.087	0.143	-0.076	0.093
	(0.065)	(0.055)***	(0.212)	(0.063)
Calculation	0.073	0.054	0.112	0.080
	(0.060)	(0.064)	(0.198)	(0.059)
Day Gap (1 or 3)	-0.326	-0.289	-0.063	-0.325
	(0.099)***	(0.090)***	(0.179)	(0.105)***
Game*Day Gap	0.019	-0.033	-0.045	0.010
	(0.082)	(0.073)	(0.214)	(0.082)
Calculation*Day Gap	-0.130	-0.084	-0.576	-0.131
	(0.074)*	(0.090)	(0.200)***	(0.075)*
%Loss Last Year (self report)				0.210
				(0.111)*
Age				0.009
				(0.011)
Education				0.034
				(0.017)**
Household Size				-0.015
				(0.005)***
Land of Rice Production				0.004
				(0.013)
Obs.	816	674	132	816
Omitted Treatment	Control			
Mean of Dep. Var. for Omitted Treatment:	0.198			
Fixed Effects for Village and Enumerator	Y	Y	Y	Y
Log Likelihood	-425	-330	-84	-418
Pseudo R-square	0.1052	0.1196	0.0596	0.1199

Notes: Dependent variable is individual adoption; Robust clustered standard errors are in the bracket; *** significant on 1% level, ** significant on 5% level, * significant on 10% level.

Table A5. Check Balance Across Households Experienced Different Number of Disasters in the Game

	Wave 1				Wave 2				Wave 3			
	No Disaster	1 Disaster	2 Disaster	p-value**	No Disaster	1 Disaster	2 Disaster	p-value**	No Disaster	1 Disaster	2 Disaster	p-value**
Panel A: Before Playing the Game												
Age	46.96	49.52	50.55	0.20	51.55	52.52	52.53	0.77	48.98	50.01	49.19	0.80
	(11.47)	(11.47)	(13.07)		(11.98)	(10.87)	(11.86)		(12.56)	(11.24)	(11.67)	
Education***	1.39	1.36	1.31	0.85	1.28	1.30	1.45	0.25	1.43	1.48	1.29	0.46
	(0.72)	(0.91)	(0.81)		(0.73)	(0.86)	(0.81)		(0.84)	(0.87)	(0.92)	
Household Size	4.88	4.52	5.15	0.44	5.30	4.92	4.96	0.52	4.44	4.53	4.77	0.37
	(1.87)	(1.87)	(2.42)		(2.84)	(2.80)	(2.41)		(1.36)	(1.67)	(1.58)	
Area of Rice Production (mu)	12.15	13.47	11.5	0.57	9.06	10.05	8.57	0.74	11.35	11.46	10.12	0.38
	(9.33)	(7.78)	(7.77)		(7.64)	(11.43)	(6.09)		(11.17)	(6.51)	(5.52)	
Share of Rice Income in Total Income (%)	83.41	87.8	84.05	0.70	62.67	60.86	64.03	0.87	86.99	91.76	90.09	0.02**
	(21.37)	(23.01)	(25.10)		(27.44)	(28.54)	(28.65)		(18.50)	(8.17)	(15.31)	
Loss in Last Year (%) (self-report)	8.92	7.9	10.1	0.82	23.73	22.05	22.36	0.62	28.60	31.46	31.49	0.11
	(14.78)	(12.69)	(18.74)		(13.79)	(11.42)	(14.37)		(16.73)	(15.45)	(16.09)	
Panel B: After Playing the Game					4.16	4.26	3.99	0.59	3.12	3.05	3.45	0.23
Risk Aversion					(1.43)	(1.39)	(1.51)		(1.71)	(1.42)	(1.37)	
					22.84	17.91	22.39	0.14	23.43	23.51	22.32	0.69
Perceived Probability of Future Disaster (%)					(16.76)	(12.07)	(12.04)		(9.38)	(9.82)	(8.89)	
	0.19	0.39	0.30	0.13	0.17	0.21	0.39	0.00**	0.34	0.41	0.36	0.62
Take-up(%)	0.39	(0.34)	(0.46)		(0.38)	(0.41)	(0.49)		(0.47)	(0.49)	(0.48)	
Observations	85	24	56		256	29	69		166	69	50	

Notes: Standard deviations are in the parentheses. *P-value in wave 1 is for F test of equal means of two groups. **P-value in wave 2 and 3 are for Wald test of equal means of three and four groups. ***Education is coded as follows: 0-illiteracy; 1-primary school; 2-secondary school; 3-high school; 4-college

Table A6. Effect of the Number of Hypothetical Disasters Experienced During Games on Actual 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 A7. The Effect of Hypothetical Disasters Experienced in Earlier vs. Later Rounds of the Game 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

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 A8. The Heterogeneity Effect of Game and Calculation Interventions

Specification:	Logistic Regression							
Dep. Var.:	Individual Adoption of Insurance							
Sample	No information				Information			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Game	0.123	0.054	0.179	0.073	-0.211	-0.166	-0.182	-0.176
	(0.056)**	(0.066)	(0.044)***	(0.080)	(0.384)	(0.138)	(0.455)	(0.150)
Calculation	-0.060	0.016	0.129	0.042	0.187	0.0003	-0.047	-0.014
	(0.050)	(0.045)	(0.059)**	(0.055)	(0.462)	(0.178)	(0.544)	(0.174)
No. of Disaster		0.069		0.015		-0.105		0.2007
		(0.050)		(0.043)		(0.174)		(0.151)
%Loss Last Year	0.211	0.354			-0.137	-0.249		
	(0.196)	(0.118)***			(0.918)	(0.376)		
Perceived Probability of Future Disaster			0.426	0.216			-0.867	-0.326
			(0.129)***	(0.117)*			(1.510)	(0.495)
Game × %Loss Last Year	-0.011				0.368			
	(0.260)				(1.096)			
Calculation × %Loss Last Year	0.340				-0.621			
	(0.170)**				(1.227)			
No. of Disaster × %Loss Last Year		-0.120				0.529		
		(0.125)				(0.435)		
Game × Perceived Probability of Future Disaster			-0.110				0.280	
			(0.110)				(1.657)	
Calculation × Perceived Probability of Future Disaster			-0.342				0.160	
			(0.181)*				(2.216)	
No. of Disaster × Perceived Probability of Future Disaster				0.161				-0.766
				(0.140)				(0.697)
p-value for Game × %Loss Last Year=Calculation × %Loss Last Year	0.1070				0.1812			
p-value for Game × Perceived Probability of Future Disaster=Calculation × Perceived Probability of Future Disaster			0.1636				0.8809	
Obs.	674	664	468	474	126	124	123	125
Omitted Treatment	Control and No Information				Control and Information			
Mean of Dep. Var. for Omitted Treatment:								
Social-economic Variables	Y	Y	Y	Y	Y	Y	Y	Y
Fixed effects for village and enumerator	Y				Y			
Log Likelihood	-328	-322	-232	-236	-78	-76	-76	-78
Pseudo R-square	0.1253	0.1267	0.1262	0.1218	0.0752	0.0792	0.0789	0.0707

Notes: Dependent variable is individual adoption; Robust clustered standard errors are in the bracket;*** significant on 1% level, ** significant on 5% level, * significant on 10% level.

Table A9. The Correlation Between Weather Insurance Take-up and Other Insurance Take-up

Specification:	Linear Regression					
Dep. Var.:	Individual adoption of insurance					
Sample:	All Sample					
	(1)	(2)	(3)	(4)	(5)	(6)
Game	0.104	0.094	0.104	0.094	0.103	0.093
	(0.029)***	(0.041)***	(0.030)***	(0.029)***	(0.030)***	(0.030)***
Health Insurance	0.121	0.103				
	(0.077)	(0.072)				
Life Insurance			-0.170	-0.135		
			(0.053)***	(0.043)***		
Asset Insurance					-0.094	-0.086
					(0.045)*	(0.041)*
%Loss Last Year (self report)		0.245		0.236		0.247
		(0.097)**		(0.095)**		(0.097)**
Age		0.005		0.006		0.007
		(0.010)		(0.010)		(0.010)
Education		0.052		0.054		0.053
		(0.019)**		(0.018)***		(0.018)***
Household Size		-0.024		-0.022		-0.023
		(0.006)***		(0.007)***		(0.007)***
Land of Rice Production		0.005		-0.004		-0.004
		(0.013)		(0.013)		(0.013)
Obs.	816	816	816	816	816	816
Fixed Effects for Village and Enumerator	N	N	N	N	N	N
R-square	0.0160	0.0489	0.0186	0.0503	0.0171	0.0501

Notes: Dependent variable is individual adoption; Robust clustered standard errors are in the bracket;*** significant on 1% level, ** significant on 5% level, * significant on 10% level;

Table A10. The Correlation Between Insurance Take-up and Trust Indicators

Specification:	Linear Regression		
Dep. Var.:	Individual adoption of weather insurance		
Sample:	All Sample		
	(1)	(2)	(3)
Game	0.118	0.114	0.111
	(0.035)***	(0.037)***	(0.037)***
Calculation	0.028	0.051	0.047
	(0.039)	(0.044)	(0.046)
Age	-0.003	0.003	-0.005
	(0.011)	(0.011)	(0.011)
Education	0.025	0.052	0.053
	(0.019)	(0.019)**	(0.018)**
Household Size	-0.041	-0.024	-0.027
	(0.010)***	(0.006)***	(0.008)***
Land of Rice Production	0.033	0.004	0.009
	(0.015)*	(0.014)	(0.013)
Self-Report Positive Trust Indicator	0.472		
	(0.073)***		
Self-Report Negative Trust Indicator	-0.425		
	(0.076)***		
Self-Claim Positive Trust Indicator		0.118	
		(0.065)*	
Self-Claim Negative Trust Indicator		0.024	
		(0.041)	
Other-Claim Positive Trust Indicator			-0.039
			(0.044)
Other-Claim Negative Trust Indicator			0.026
			(0.050)
Obs.	816	816	816
Village and Enumerator Fixed Effects	N	N	N
R-square	0.2341	0.0489	0.0421

Notes: Dependent variable is individual adoption; Self-report trust indicator is positive if a household purchased the weather insurance and the reason is trust the program and negative otherwise; Self-claim trust indicator is positive if they got payout from other insurance products purchased previously and negative otherwise; Other-claim trust indicator is positive if they observed other people receiving payout from purchasing other insurance products and negative otherwise. Robust clustered standard errors are in the bracket;*** significant on 1% level, ** significant on 5% level, * significant on 10% level;

B Insurance Demand Models

The evidence discussed in our paper implies that hypothetical experience influences actual insurance decisions. In this section, we present a simple model to illustrate how such an effect could occur. In Section B.1, we show that neither a standard constant absolute risk aversion (CARA) preference and nor a constant relative risk aversion (CRRA) preference is likely to explain the data. In Section B.2, we add a weight parameter in the utility function to capture the influence of experience. We then estimate the parameters through a maximum likelihood method (MLE).

B.1 A Standard Model

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

$$u(x) = -\frac{exp(-\alpha x)}{\alpha} \tag{22}$$

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

We first 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. We also 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. Finally, we let x indicate the gross income from rice production, p the perceived probability of future disasters and l the loss in yield. The expected utility of not buying insurance is thus represented by:

$$U(a = 0) = (1 - p)u(x) + pu(x - l) \tag{23}$$

If a household buys insurance, it should earn its normal income and pay the premium when there is no disaster. It will then receive a payment from the insurance company when there is a disaster. The utility of buying insurance is thus represented by:

$$U(a = 1) = (1 - p)u(x - \tau) + pu(x - \tau - l + b) \tag{24}$$

The condition for the household to buy the insurance is:

$$U(a = 1) \geq U(a = 0) \tag{25}$$

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

To test whether a standard CARA preference could explain our data, we can use the parameter as measured, calibrate individual decisions and compare the calibrated decisions with actual decisions. In this test, we assume that there is no measurement error for either risk aversion (α) or perceived probability of future disasters (p). Although we do not observe parameter α, we can make use of their 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 B1. If a household takes two riskless options, then α should be greater than zero and less than 0.0041 under a CARA preference[28].

[28] 1. Simulation of Insurance Take-up under Standard Model:

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.

2. MLE Estimation of Weight Parameters:

1. Take a uniform draw of α from the interval according to each householdâĂŹs 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; estimate μ_1 and μ_2 with MLE
3. Repeat steps 1 and 2 for 100 times to generate 100 μ_1 and μ_2

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

A second way to test whether our results reflect CARA preferences is to ignore α and p as elicited. In this case, suppose that we had not elicited measures for risk aversion and perceived probability of future disasters. We would then estimate α and p in the logit formula (26) through MLE:

$$P(a = 1) = \frac{exp(U(a = 0))}{exp(U(a = 1)) + exp(U(a = 0))} \tag{26}$$

However, in doing so, 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 than 17% (2) positive α (risk averse) and p less than 5%. This finding 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 estimated parameters methods provide results that contradict our data under standard CARA and CRRA preferences. These results suggest that standard CARA and CRRA preferences are unlikely to explain our observed increased insurance take-up and perceived probability of future disasters together.

B.2 A 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 now 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 have experienced a hypothetical

4. Compare the distributions of μ_1 and μ_2

disaster during the game. We develop a simple model in the following.

$$U(a = 0) = (1 - p)u(x) + pu(x - \mu l) \quad (27)$$

$$U(a = 1) = (1 - p)u(x - \tau) + pu(x - \tau - \mu l + \mu b) \quad (28)$$

where μ is a parameter that measures the weights of disaster loss and insurance benefit. The idea is that households might give less weight to disasters and benefits which they experience infrequently. By extension, when they experience disasters and insurance benefits during the hypothetical games, these events draw their attention to disaster loss and insurance benefit and increase the weight parameter μ.

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

The result of this estimation is presented in column (2) of Table B2. Specifically, we find the estimated mean of μ_1 is around 0.21 and μ_2 is around 0.37. We then use a t-test and Kolmogorov-Smirnov test to show that the mean and the distribution are significantly different at the 1% level. Column 6 presents the result with a CRRA preference. Although the point estimates are different, the key pattern is similar. These results are consistent with our hypothesis that playing the game increases μ and thus increases insurance take-up.

Finally, we consider that hypothetical experience might have two effects: a change in understanding and a change in vividness. We thus add another parameter δ in (29):

$$U(a = 1) = (1 - p)u(x - \tau) + pu(x - \tau - \mu l + \mu b) + \delta \quad (29)$$

where δ measures the utility of understanding insurance if the participant buys the insurance. The intuition is that households would be less happy if they buy something they do not understand as well. Thus this parameter might capture ambiguity aversion. We normalize δ to be zero in the game treatment so that the estimated δ is the difference in the utility of understanding. We estimate μ_1, μ_2 and δ with the same procedure to estimate μ_1 and μ_2 and present the result in column 3.

The estimated mean of μ_1 is around 20.4% and μ_2 is around 33.9%. A 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. 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 the utility of understanding is higher in the game treatment. Column 7 presents the result with a CRRA preference. Although the point estimates are different, the key pattern is similar. These results are consistent with our hypothesis that playing the game increases understanding and vividness and thus increases insurance take-up.

In order to understand the empirical relationship between experience and the weight parameter, we model μ in the following way:

$$\mu = C + D(1 - exp(-K_a f_a - K_h f_h) \tag{30}$$

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

Note that f_a is actual experience, measured as a greater than 30% probability of having experienced a disaster in the past 3, 2 or 1 year(s). f_h is hypothetical experience, measured as the percentage of disasters experienced during the 10 rounds of the game. k_a and k_h capture the rate of learning from actual experience and hypothetical experience, respectively. With enough experience, 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 $C + D = 1$. We can now estimate k_a, k_h to compare the effects of actual and hypothetical experience on insurance adoption.

In column (4), we present the results from estimating the learning effects of actual experience (k_a) and hypothetical experience (k_h) under a CARA preference. f_a is measured as a greater than 30% probability of having experienced a disaster in the past 3 years. The mean of k_a is 0.075 and the mean of k_h is 4.254; these are significantly different at the 1% level. Column 8 presents the results under a CRRA preference. Although the point estimates are different, the key pattern is similar. These results suggest that both actual and hypothetical experience matter in influencing insurance take-up decisions. Moreover, hypothetical experience a few days prior to the insurance decision has a stronger effect than actual experience one year prior to the decision, suggesting a strong recency effect.

Table B1. Range of the 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 A3.

Table B2. 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: This table estimates parameters in CARA utility function and CRRA utility function through MLE. In all columns, we constrain a 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 a . In Columns (1 and (4), we allow the weight parameter in the group who do not play games (μ1) to be different from weight parameter in the group who play games (μ2). 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 μ = C+D(1-exp(-kafa-khfh)) . Then we estimate both the learning effect of actual experience (ka) and hypothetical experience (kh) with different measurement of actual disaster.

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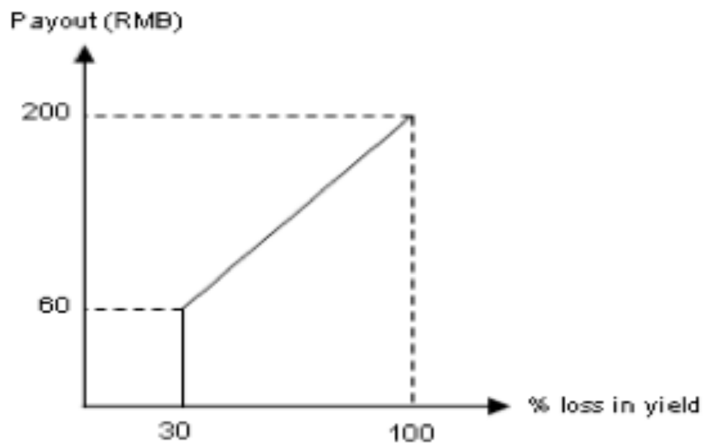
Figure I. Insurance Contract



Notes: The original premium of insurance is RMB 12 per mu. The government will subsidize 70% of the premium so the households pay only the remaining 30%, i.e. RMB 3.6. The policyholder is eligible to receive a payment if there are disasters that cause a 30% or more loss in yield for 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 through an investigation by a group of agricultural experts. They will come to the village to sample the rice in different plots and calculate the loss in yield.

Figure II. Timeline of the Experiment

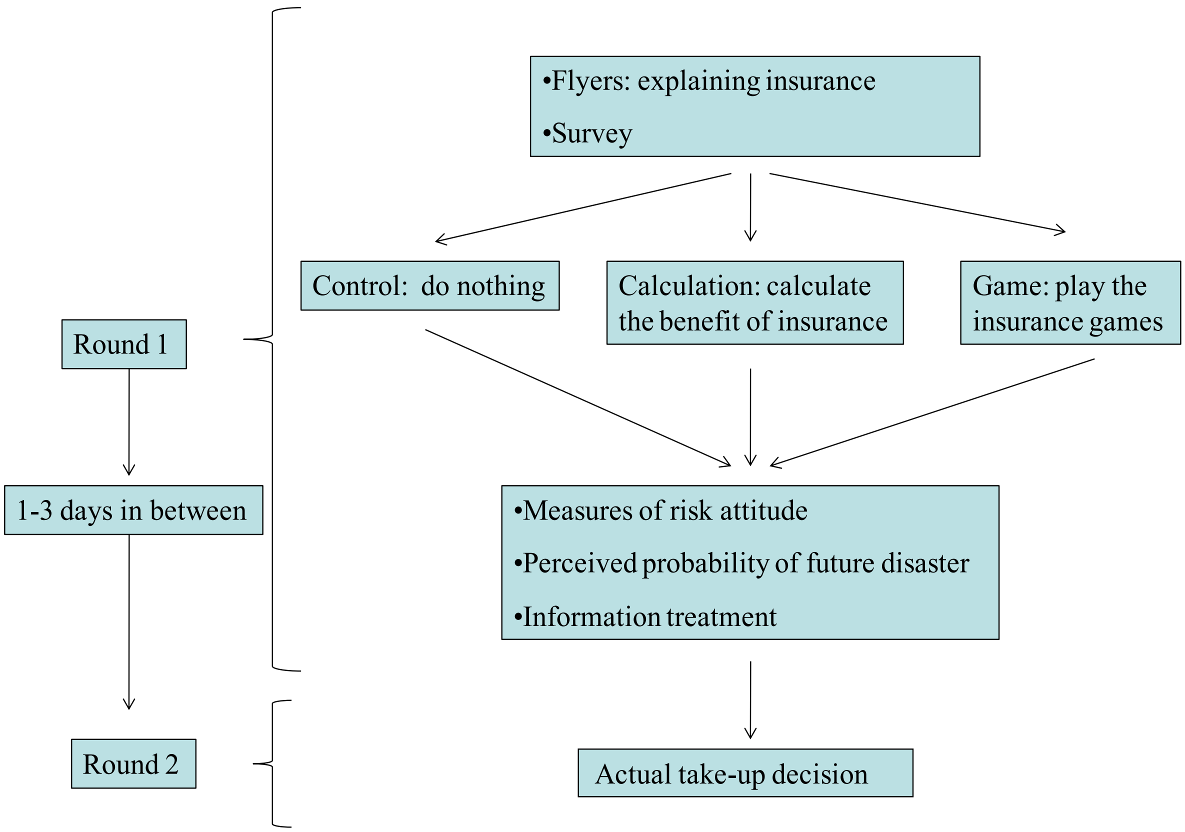


Figure III. Overview of Interventions

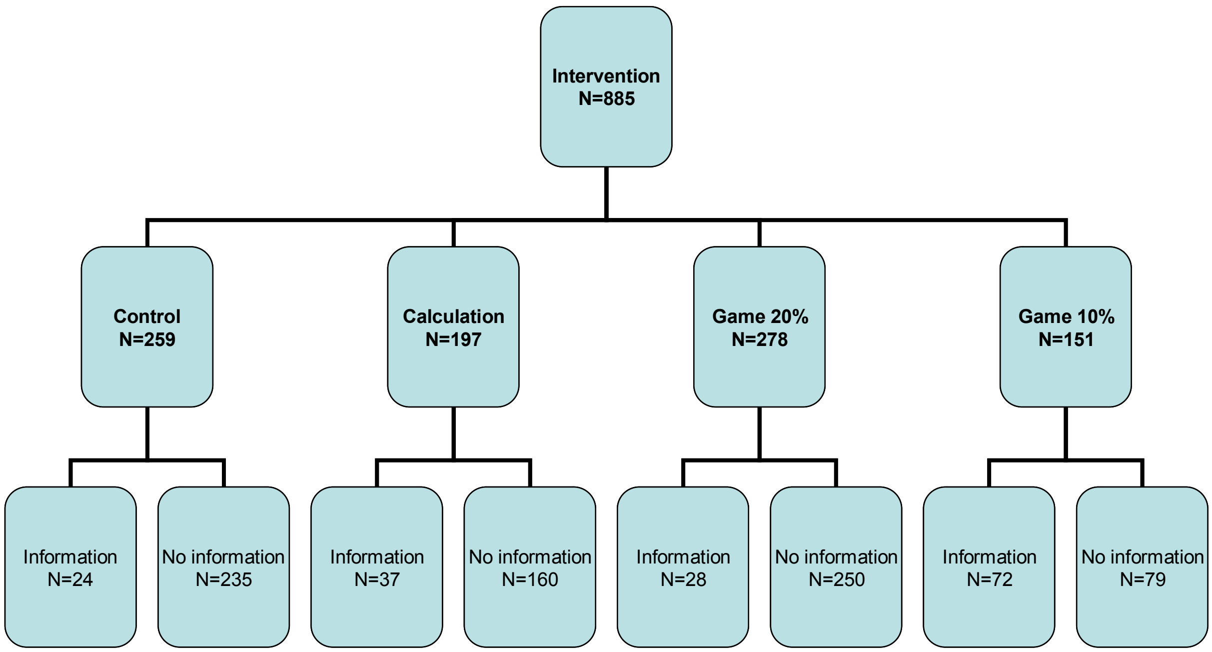
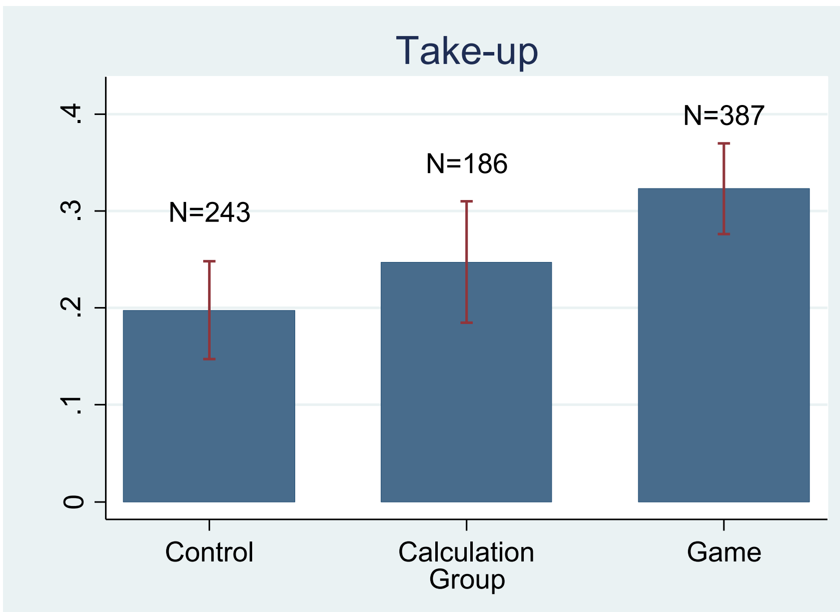
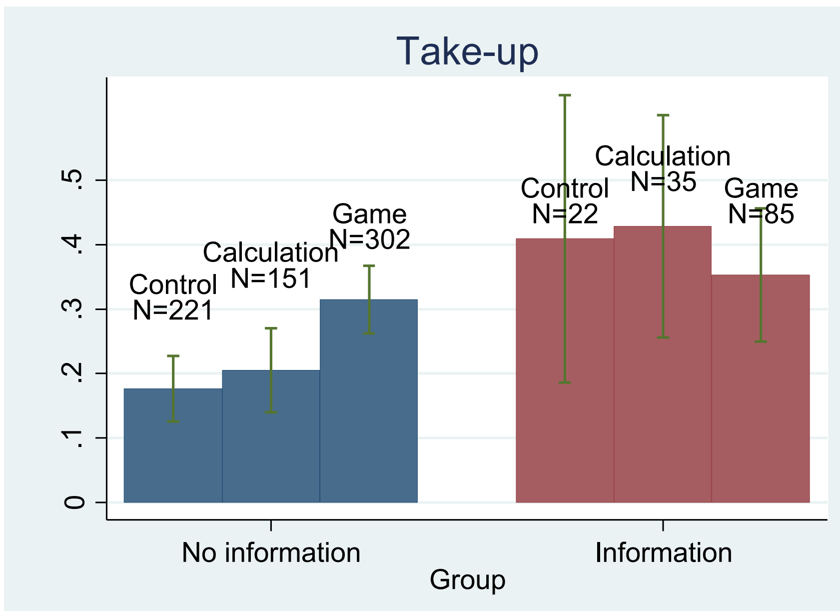


Figure IV. The Effect of Game and Calculation Treatments on Insurance Take-up



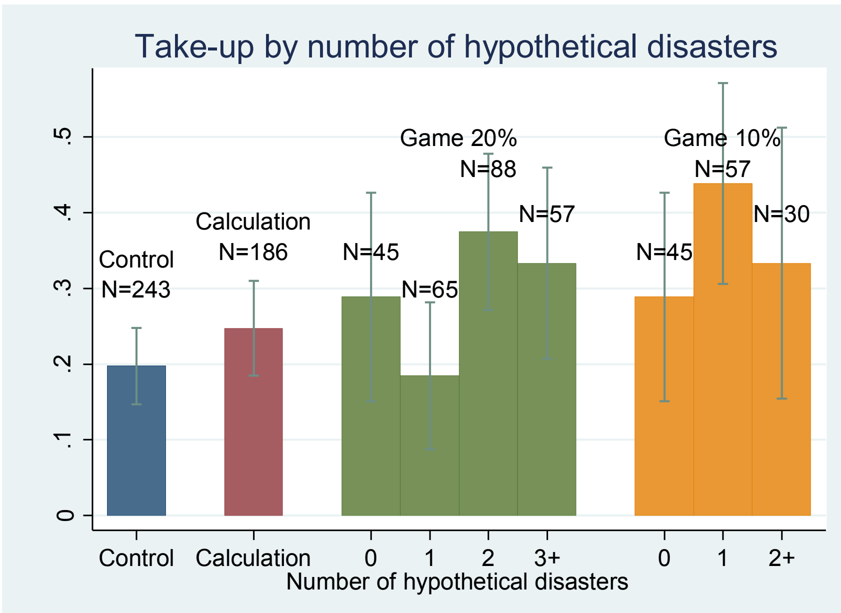
Notes: This figure shows the treatment effect for the calculation group and the game group, respectivley. In the control group, the take-up rate is 19.8%. In the calculation group, the take-up rate increases to 24.7%.In the game group, the take-up rate increases to 32.3%. These results suggest that both the game treatment and the calculation treatment increase the actual take-up and the game treatment is more effective.

Figure V. Treatment Effects by the Information Treatment



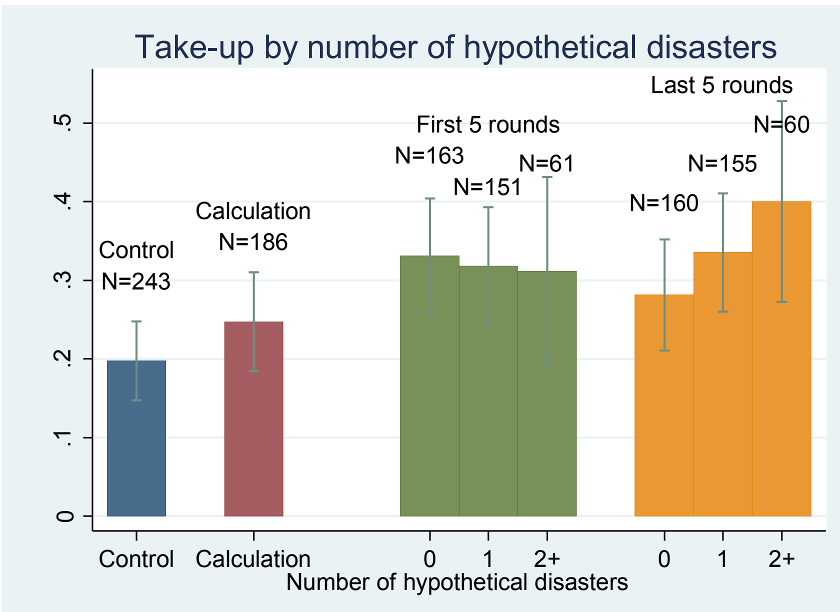
Notes: This figure shows the treatment effect by the information treatment. Without the information treatment, the game treatment is more effective than the calculation treatment. With the information treatment, neither the game treatment nor the calculation treatment is as effective.

Figure VI. Insurance Take-up by the Number of Hypothetical Disasters Experienced in Games



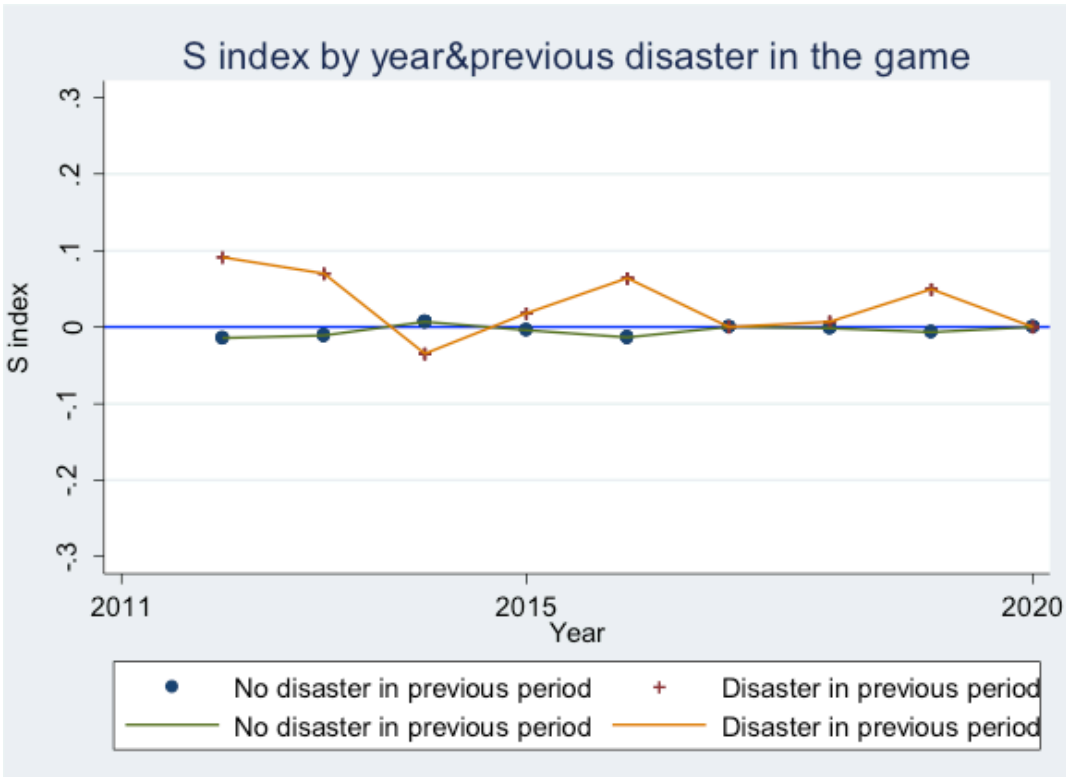
Notes: This figure shows the insurance take-up rate, conditional on the number of disasters participants experienced during the games. The left two bars show the take-up rates of the control group and the calculation group.

Figure VII. Insurance Take-up by the Number of Hypothetical Disasters in Earlier vs. Later Rounds



Notes: In this figure, the left two bars show insurance take-up conditional on whether there is a disaster in the first round and last round. The right two bars show insurance take-up conditional on the number of disasters in the first five rounds and last five rounds.

Figure VIII



S index by year&previous disaster in the game
S index
.3
.2
.1
0
-.1
-.2
-.3
2011
2015
2020
Year
No disaster in previous period
Disaster in previous period
No disaster in previous period
Disaster in previous period

Figure IX

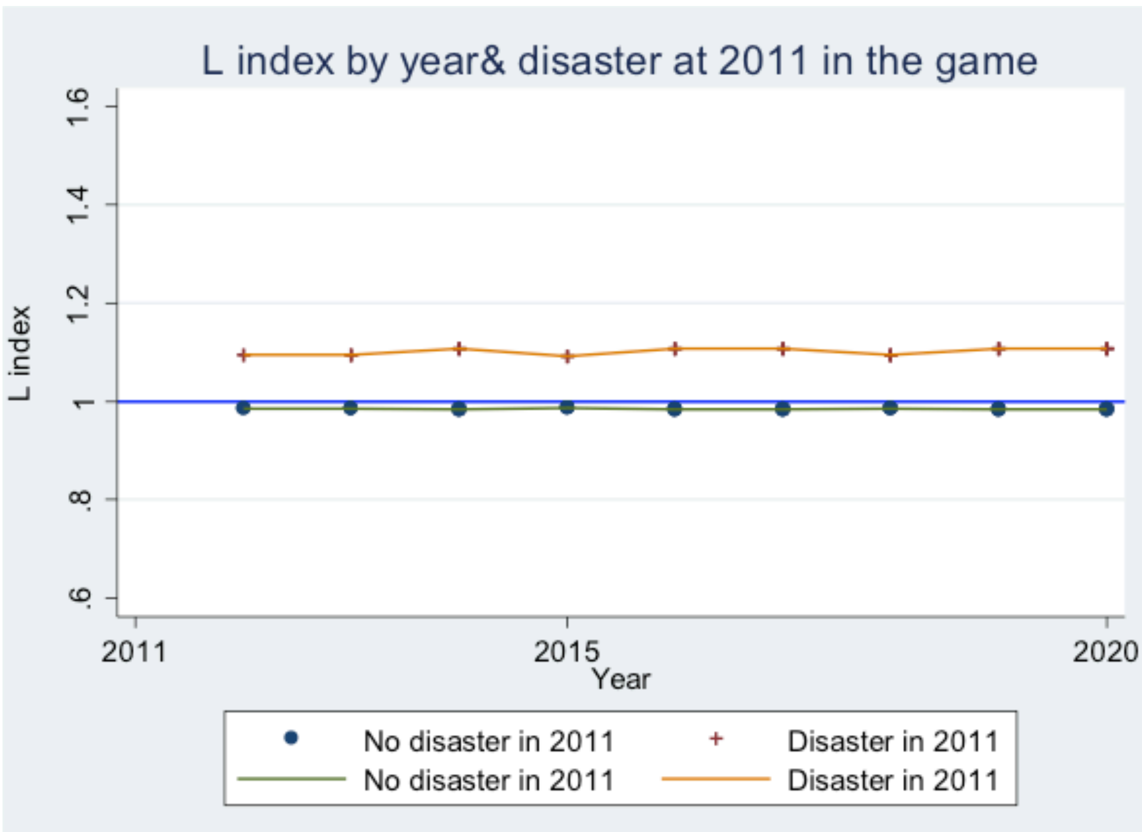

L index by year& disaster at 2011 in the game
L index
.6
.8
1
1.2
1.4
1.6
2011
2015
2020
Year
No disaster in 2011
Disaster in 2011
No disaster in 2011
Disaster in 2011

Table I. 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 ([0,1])	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	

Notes: Standard deviations are in the parentheses. *P-value in wave 1 is for F test of equal means of two groups. **P-value in wave 2 and 3 are for Wald test of equal means of three and four groups. ***Education is coded as follows: 0-illiteracy; 1-primary school; 2-secondary school; 3-high school; 4-college

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

Specification:	Logistic regression					
Dep. Var.:	Individual Adoption of Insurance					
Sample:	All Sample				No Information	Information
	(1)	(2)	(3)	(4)	(5)	(6)
Game (1=Yes, 0=No)	0.092	0.096	0.092		0.119	-0.086
	(0.039)**	(0.037)***	(0.038)**		(0.034)***	(0.172)
Game 20% (1=Yes, 0=No)				0.108		
				(0.035)***		
Game 10% (1=Yes, 0=No)				0.045		
				(0.066)		
Calculation (1=Yes, 0=No)	0.025	0.029	0.031	0.020	0.012	-0.009
	(0.043)	(0.042)	(0.040)	(0.045)	(0.047)	(0.189)
%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)***			
Area of Rice Production (mu)			0.002			
			(0.014)			
Wald Test:	βg=βc	βg=βc	βg=βc	$\beta_{20}=\beta_{10}$	βg=βc	βg=βc
p-value	0.1376	0.1328	0.1568	0.2548	0.0117**	0.5376
Obs.	816	816	816	816	674	132
Omitted Treatment	Control					
Mean of Dep. Var. for Omitted Treatment:	0.198					
Fixed Effects for Village and Enumerator	Y	Y	Y	Y	Y	Y
Log Likelihood	-431	-429	-424	-430	-335	-86
Pseudo R-square	0.0918	0.0962	0.1065	0.0933	0.1057	0.0323

Notes: Dependent variable is individual insurance 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. Columns 1-4 are based on the whole sample. In Columns 2 to 3, we add dummies for missing values of control variables in the regression. In Column 2, the self reported percentage of loss in last year is included in the regression. In Column 3, additional control variables are age group of household head, education of household head, household size and area of rice production. Column 4 compares between the two game groups with 20% and 10% probability of disasters. Columns 5 and 6 are based on the sample households in the no information group and the information group, respectively.

Table III. The Heterogeneity of the Game Effect on Insurance Take-up

Specification:	Logistic Regression					
Dep. Var.:	Individual Adoption of Insurance					
Sample	All Sample					
	(1)	(2)	(3)	(4)	(5)	(6)
Game (1=Yes, 0=No)	0.117	0.029	0.114	0.182	0.099	0.099
	(0.070)*	(0.039)	(0.066)*	(0.097)*	(0.057)*	(0.035)***
Calculation (1=Yes, 0=No)	0.024	0.037	0.028	0.026	-0.048	0.048
	(0.040)	(0.044)	(0.043)	(0.042)	(0.047)	(0.037)
Game × Age	-0.013					
	(0.019)					
Game × Education		0.044				
		(0.028)				
Game × Household Size			-0.010			
			(0.016)			
Game × Area of Rice Production (mu)				-0.027		
				(0.022)		
Game × %Loss Last Year					-0.034	
					(0.253)	
Game × Perceived Probability of Future Disaster						-0.057
						(0.137)
Obs.	808	799	813	807	816	816
Omitted Treatment	Control and No Information					
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	-421	-416	-425	-420	-423	-425
Pseudo R-square	0.0988	0.1061	0.1037	0.1037	0.1092	0.1050

Notes: Dependent variable is individual adoption; Robust clustered standard errors are in the bracket;*** significant on 1% level, ** significant on 5% level, * significant on 10% level.

Table IV. The Decomposition of Game and Calculation Effects: Changes in Risk Aversion and Perceived Probability of Future Disasters

Specification:	OLS Regression				
Dep. Var.:	Individual Adoption of Insurance	Risk Aversion		Perceived Probability of Future Disaster	
Sample:	Control & Calculation (1)	All Sample (2)	Game (3)	All Sample (4)	Game (5)
Risk Aversion	0.035				
	(0.016)**				
Perceived Probability of Future Disaster	0.215				
	(0.110)*				
Game (1=Yes, 0=No)		-0.024		-0.015	
		(0.182)		(0.008)*	
Calculation (1=Yes, 0=No)		0.055		-0.011	
		(0.165)		(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				
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: 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 (1), we restrict the sample to the control group and the calculation group and regress adoption on risk attitude. In Columns (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 V. the Effect of Game Treatment on Insurance Knowledge

Specification:	OLS Regression					
Sample	All Sample					
Dep. Var.:	Probability Question		Insurance Benefit Question 1		Insurance Benefit Question 2	
	(1)	(2)	(3)	(4)	(5)	(6)
Game (1=Yes, 0=No)	-0.017	-0.014	0.009	0.031	0.016	0.025
	(0.020)	(0.026)	(0.010)	(0.024)	(0.022)	(0.023)
Number of Hypothetical Disasters		-0.001		-0.018		-0.009
		(0.013)		(0.018)		(0.008)
Obs.	667	659	658	650	657	649
Omitted Treatment	Control and No Information					
Mean of Dep. Var. for Omitted Treatment:	0.072		0.416		0.265	
Social-economic Variables	Y	Y	Y	Y	Y	Y
Fixed effects for village and enumerator	Y	Y	Y	Y	Y	Y
R-square	0.0838	0.0751	0.7692	0.7589	0.6882	0.6757

Notes: Probability Question is "If you roll a six-side dice for 100 times, how many times will you see number 6?" Insurance Benefit Question 1 is "Suppose your gross income is 1000 RMB per mu, the loss from disaster is 400 RMB, insurance premium is 3.6 RMB, you get 80 RMB from insurance company if there is a disaster and you buy the insurance. What is your income per mu if there is a disaster but you did not buy insurance? " Insurance Benefit Question 2 is "What is your income per mu if there is a disaster and you bought the insurance?" Robust clustered standard errors are in the bracket;*** significant on 1% level, ** significant on 5% level, * significant on 10% level.

Table VI. The Effect of the Number of Hypothetical Disasters 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)	(7)	(8)
Game (1=Yes, 0=No)	0.010		0.047	0.002	0.037		0.085	0.021
	(0.059)		(0.046)	(0.057)	(0.067)		(0.047)	(0.065)
Calculation (1=Yes, 0=No)	0.042		0.044	0.044	0.032		0.037	0.035
	(0.046)		(0.045)	(0.046)	(0.051)		(0.050)	(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	0.037			-0.042	0.026
			(0.024)	(0.036)			(0.028)	(0.044)
Number of Hypothetical Disasters in Second Half of Game (2016-2020)			0.070	0.128			0.072	0.141
			(0.033)**	(0.048)**			(0.034)**	(0.045)**
Interation between First and Second Half of Game				-0.058				-0.066
				(0.025)**				(0.024)**
Obs.	804	804	804	804	664	664	664	664
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	-427	-427	-426	-424	-333	-334	-331	-329
Pseudo R-square	0.0599	0.0864	0.0884	0.0927	0.0956	0.0965	0.1021	0.1087

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 Columns (4) to (6), we restrict the sample to households in the no information treatment. In Columns (3) and (7), 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 VII. The Long-term Effect of Hypothetical Games on Actual Insurance Take-up

Specification:	Logistic Regression	
Dep. Var.:	Second-Year Individual Adoption of Insurance	
Sample:	All sample	
	(1)	(2)
Game (1=Yes, 0=No)	0.0155	
	(0.0238)	
Calculation (1=Yes, 0=No)	0.1425	0.1414
	(0.053)	(0.0639)
Payout (1=Yes, 0=No)	0.3147	0.3215
	(0.257)	(0.2487)
Game and No Disaster		-0.012
		(0.0587)
Game and One Disaster		-0.0437
		(0.0302)
Game and Two Disasters		0.0299
		(0.1279)
Game and Three or More Disasters		0.0665
		(0.0517)
Obs.	191	191
Social-economic Variables	Y	Y
Fixed effects for village and enumerator	Y	Y
R-Square	0.1603	0.1627

Notes: Dependent variable is second-year individual adoption; Robust clustered standard errors are in the bracket;*** significant on 1% level, ** significant on 5% level, * significant on 10% level;