The Crowding-out Effect of Formal Insurance on Informal Risk Sharing: An Experimental Study*

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Abstract: This paper investigates the crowding-out effect of formal insurance on informal risk-sharing arrangements using laboratory experiment. Our model predicts that formal insurance can lead to more than one-for-one crowding out on informal risk-sharing and decreases total risk coverage, even when altruism is taken into consideration. The experiment suggests that the crowding-out effect is *not* more than one-for-one. However, the risk coverage is increased when *ex-ante* income is equal but not changed when income inequality exists. Finally, the existence of altruistic motive is shown to contribute to the crowding-out effect.

JEL Classification: C91 D81 O17

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1. Introduction

Life is plagued by risks, especially in areas where formal insurance is not readily available. As a result, there have been significant efforts to promote the standardized insurance contract in the underdeveloped areas either by government or private institutions (Schneider, 2005). We call such a contract *formal insurance* because it is provided by formal institutions and enforced by law.

The promotion of formal insurance is based on the intuitive belief that formal insurance provides better risk coverage and improves the insured's welfare. However, an important issue that has been often ignored is the possible crowding-out effect of formal mechanisms on preexisting informal arrangements. Many studies show that people rely on private transfers to share risk and smooth consumption (Deaton, 1992; Townsend, 1994; Udry, 1994; Grimard, 1997; Foster and Rosenzweig, 2002). Due to limited enforcement problem these informal risk-sharing activities often cannot eliminate all the income fluctuations (Coate and Ravallion, 1993; Ravallion and Chaudhuri, 1997; Ligon et al., 2002; Maccini and Young, 2009). However, if formal insurance also fails to provide perfect risk coverage, which is often the case in reality due to incomplete information or idiosyncratic risk, the introduction of formal insurance may crowd out informal risk-sharing activities so much so that total risk coverage is not change or even decreased.

This paper presents theoretical models and laboratory experiment on the crowding-out effect of formal insurance on informal risk-sharing arrangement. We ask the following questions: First, does the introduction of formal insurance lead to excessive crowding out of informal risk-sharing activities that lowers total risk coverage? Second, whether the presence of altruism affects the crowding-out effect? Third, how does income inequality change the crowding out of private transfers?

To answer these questions, we first built theoretical models that adapt Charness and Genicot's (2009) characterization of the constrained optimal private risk-sharing scheme to a setting with formal insurance. The model focuses on idiosyncratic risk, because informal risk sharing mechanism can only cope with idiosyncratic risk so there is a fair comparison between the two forms of insurance. The conclusions in this paper therefore understate the advantage of formal insurance when individuals face mainly systematic risk. If the degree of risk coverage from formal insurance is not higher than the level provided by informal risk sharing, the model predicts a more than one-for-one crowding-out of private transfer, which implies a lower degree of risk coverage. Intuitively, the crowding-out effect comes from two sources. First, the introduction of formal insurance improves the autarky value, which makes the punishment (staying in autarky) in the informal activities less severe and the original high level of private transfers not sustainable. Second, the actual purchase of formal insurance introduces an additional substitution effect between formal and informal mechanisms for the risk-sharing purpose. The first channel exists regardless of whether formal insurance is purchased or not, and so the crowding-out of private transfers is often too large to be compensated by formal insurance.

We also build a model of crowding-out effect with altruism. Altruistic preferences interact with standard risk-sharing motive in a complex way as altruism affects both the punishment path and the equilibrium path. As long as the degree of altruism is not too high, the model suggests that the conclusions in the previous paragraph still hold. For low level of altruism, the model predicts larger crowding out of private transfers when there is altruistic concern than when there is not. However, the prediction is reversed for sufficiently high level of altruism.

To test these predictions, we design a laboratory experiment based on Charness and Genicot's (2009) repeated risk-sharing game. Two subjects were randomly paired throughout the experiment. Depending on the sessions, their fixed income was either equal (125 and 125) or unequal (100 and 150). In addition to the fixed income, one of the two subject can receive 200 random income with equal probabilities each period. Those who received the random income can choose whether to make a transfer to her partner for mutual risk-sharing purpose. We run control sessions in which no formal insurance was introduced and treatment sessions in which formal insurance was offered for purchase exogenously as a shock in the middle of the experiment. A single period was randomly selected to calculate the final payments to the subjects so they were incentivized to smooth their consumption over all periods.

The formal insurance we introduced is actuarially fair but only covers half of the risk. Introducing partial insurance serves as a good test of the theory, because full insurance will always improve total risk coverage regardless of how large the crowding-out effect is. Full insurance is also less likely in reality since the standardized insurance contracts cannot cover all idiosyncratic risks. A given type of insurance also only covers one aspect of the total risks in life, whereas informal risk-sharing arrangements can provide comprehensive coverage of all kinds of risks in life due to their flexibility.

We find significant crowding out of private transfers after introducing formal insurance. Unlike the theoretical prediction, we do *not* observe more than one-for-one crowding-out of private transfers. Splitting the sample by different combinations of insurance purchase decisions, we find that when no one purchases insurance, there is no significant crowding-out effect regardless of whether fixed income is equal or not. The change in autarky value alone (the first channel) does not seem to affect private transfers in our setting, and so the key intuition for the more than one-for-one crowding-out of private transfers is not supported.

We find that unequal fixed income generates larger crowding out of private transfers hence no significant change in the level of risk coverage is observed in this case. Analysis using overtime variations suggests that the more a given subject gave relative to the amount received in the history, the more likely she was to purchase insurance and transfer less controlling for the purchase decision. It is possible that the same transfer imbalance affects the cross-sectional difference in the crowding-out effect generated by fixed income structure. Because there appears to be a larger transfer imbalance in the absence of insurance when fixed income inequality exists, due to high-income subjects transferred consistently more than low-income subjects.

To test the theoretical predictions regarding altruism, the experimental design also includes a within-subject dictator game to facilitates a distinction between transfers out of altruistic and the risk-sharing motives. The altruistic transfers account for about one-third of the total transfers. Surprisingly, the existence of altruistic transfers is shown to exaggerate the crowding-out effect. This evidence indicates that the level of altruism may not be too high, consistent with the fact that altruism, even exists, does not help achieve perfect risk sharing in the absence of insurance.

Many theoretical papers study the substitution effect between formal and informal institutions in general (Kranton, 1996 and Dixit, 2003).[1] Other studies derive the excessive crowding-out effect in settings such as government insurance programs, redistribution programs, and taxation on private transfers (Di Tella and MacCulloch, 2002; Thomas and Worrall, 2006; Broer, 2011; Krueger and Perri, 2011). Empirically, Attanasio et al. (1999) and Albarran and Attanasio (2003) identify significant

[1] There are also studies that characterize conditions under which formal and informal institutions can be complements (Baker, Gibbons, and Murphy, 2004; Broer, 2011).

crowding out of private transfers by a randomized welfare program in Mexico.[2] However, the observed crowding-out effect in the related field studies may be affected by unobserved effects, such as social preferences and income effect. Laboratory experiments have the advantage of controlling the environment to a high degree that allows a clean test of the theory and the separation of confounding effects.

Charness and Genicot (2009) is the first paper to examine informal risk sharing using laboratory experiment. This paper follows their theoretical analysis and experimental design closely except that we focus on the crowding-out effect. Using a clever design that varies the probability of ending the game in a repeated interaction, they find imperfect, yet significant risk sharing via private transfers. The fact that continuation probability and risk aversion affect the level of transfers convincingly proves the existence of rational risk-sharing motive. They introduce fixed income inequality and observe a negative impact on transfer. The data also suggest that the first period's transfer and expected transfer matter for future giving. Chandrasekhar et al. (2012) implement laboratory experiment in the villages of India to test the effect of introducing saving technology. They fail to find significant crowing out of private transfers. Gronberg et al. (2012) use a laboratory experiment to test whether taxes crowd out voluntary contribution in public good game. Similar to this paper, they find incomplete crowding-out effect, but the theoretical reasoning of the crowding-out effect is different from our paper.

The remainder of this paper is organized as follows. Section 2 presents a stylized model deriving the crowding-out effect with and without altruism. Section 3 provides a detailed description of the experimental design. Section 4 analyzes the experiment results, and Section 5 concludes the paper.

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[2] Also see Cox and Jimenez (1998), Jensen (2004), Reil-Held (2006), and Schoeni (2002) for similar finding on public transfer programs in various countries.

2. A Theoretical Model

2.1. Model setup

The model setup closely follows that of Charness and Genicot (2009). Based on their characterization of optimal private transfers, we further analyze the effect of introducing formal insurance on private transfers and overall risk reduction. The model considers a risk-sharing group composed of two individuals, $i = 1, 2$. In each period $t = 1, 2, ...$ individual i receives income $y_i\left(s_t\right) \geq 0$, where s_t is an i.i.d. state of nature that takes value 1 or 2 with equal probability. Income follows the process:

$$y_i\left(s_t\right) = \begin{cases} f_i + h & if\, s_t = i \\ f_i & otherwise \end{cases}, \tag{1}$$

where f_i stands for fixed income, and h is random income that only one of the individuals will receive. Total group income is $f_1 + f_2 + h$ for each period, and so there is no aggregate risk. However, it should be clarified that, in reality, formal insurance often has an advantage in coping with systematic aggregate risk. The conclusions in this paper therefore understate the advantage of formal insurance when individuals face mainly systematic risk.

Individuals have the same per period von Neumann-Morgenstern utility of the consumption function $u\left(c_i\left(s_t\right)\right)$, with $u'\left(c_i\right) > 0$ and $u''\left(c_i\right) < 0$ for all $c_i \geq 0$. Individuals are infinitely lived, and the future is discounted with a common discount factor $0 < \beta < 1$. In a given period, a risk-sharing agreement is characterized by τ_i, the transfer made from individual i to her partner when i receives the random income h (We assume that individual i does not make transfer when she receives zero random income). Assuming no technology of savings, consumption in a given period is

$$c_i(s) = \begin{cases} f_i + h - \tau_i & if\ s = i \\ f_i + \tau_{-i} & otherwise \end{cases} . \tag{2}$$

Because the problem is stationary, Charness and Genicot (2009) focus on the time-independent and constrained optimal transfer schemes. Under this solution concept, the problem can be simplified to

$$\max_{c_1(s)} E\sum_{j=0}^{\infty}\beta^j u\left(c_1(s_{t+j})\right) = \frac{1}{1\text{-}\beta} Eu(c_1(s))$$

s.t.

$$Eu(c_2(s)) \geq v_2 \tag{3}$$

$$(1\text{-}\beta)u(c_i(s_t)) + \beta Eu(c_i(s)) \geq (1\text{-}\beta)u(y_i(s_t)) + \beta Eu(y_i(s)), i = 1,2 \tag{4}$$

Equation (4) characterizes individuals' incentive constraints. To be incentive compatible, a risk-sharing agreement must be such that the *ex-post* expected utility from participating in the agreement is larger than that from defection. The punishment of defection is to leave the subject in autarky forever.

Ligon, Thomas, and Worrall (2002), Genicot (2006) and Charness and Genicot (2009) derive the constrained optimal solution. Their results show that, if the incentive constraints are binding we can only achieve partial risk sharing. Let $\tau^*_{i,0}, i = 1,2$ be the optimal transfer when there is no formal insurance available. Equations (5) and (6) give the binding incentive constraints for each individual, and these two conditions together characterize the constrained optimal risk-sharing agreement in Charness and Genicot (2009).

$$(1-\frac{\beta}{2})u(f_1 + h - \tau^*_{1,0}) + \frac{\beta}{2}u(f_1 + \tau^*_{2,0}) = (1-\frac{\beta}{2})u(f_1 + h) + \frac{\beta}{2}u(f_1) \tag{5}$$

$$(1-\frac{\beta}{2})u(f_2 + h - \tau^*_{2,0}) + \frac{\beta}{2}u(f_2 + \tau^*_{1,0}) = (1-\frac{\beta}{2})u(f_2 + h) + \frac{\beta}{2}u(f_2) \tag{6}$$

2.2 The crowding-out effect without altruism

This section analyzes the insurance take-up decision and its effect on private risk sharing when formal insurance is available, with the assumption that individuals do not demonstrate altruistic motives. In our setting, formal insurance costs the individual ah and pays $2ah$ ($0 \le a \le \frac{1}{2}$) when no random income is received, and nothing otherwise. Since this insurance is actuarially fair, a risk-averse individual will always want to purchase it in the absence of private transfers. The autarky value is therefore improved when formal insurance is available. This change is the key channel that drives the more than one-for-one crowding-out of private transfers. When $a < \frac{1}{2}$, this insurance does not offer full coverage of risk and there is some room left for private risk-sharing mechanisms.

To simplify the analysis, we focus on the stationary equilibrium path in which individuals maintain the same insurance purchase decision throughout the repeated interaction. We first take the insurance purchase decision as given and analyze the optimal level of private transfers. Later, we analyze the optimal insurance purchase decision given the subsequent optimal private transfers.

Case A: None of the two individuals purchases formal insurance.

The binding incentive constraints can be written as follows for individual i [3]

$$(1-\frac{\beta}{2})u(f_i+h-\tau^*_{i,A})+\frac{\beta}{2}u(f_i+\tau^*_{-i,A})=(1-\beta)u(f_i+h)+\frac{\beta}{2}(u(f_i+h-ah)+u(f_i+ah)) \quad (7)$$

The left-hand side of equation (7) represents the ex-post expected utility of the risk-sharing agreement conditional on receiving random income this period. The

[3] If incentive constraints are binding before the introduction of formal insurance, they continue to be binding after formal insurance is available. This is because formal insurance does not change individuals' unwillingness to make transfers beyond the level implied by the constrained optimal solution when called upon.

right-hand side delivers the expected value of reneging. If a given individual reneges this period by not making the transfer when receiving random income, she can receive higher utility today ($u(f_i+h)$), but she will have to rely solely on formal insurance to reduce the risk starting from the next period. In case A, individuals do not purchase insurance, so the introduction of formal insurance affects private transfers only through its effect on the autarky value.

Case B: Both individuals purchase formal insurance.

The binding incentive constraints can be written as, for individual i:

$$(1-\frac{\beta}{2})u(f_i+h-ah-\tau^*_{i,B})+\frac{\beta}{2}u(f_i+ah+\tau^*_{-i,B})=(1-\frac{\beta}{2})u(f_i+h-ah)+\frac{\beta}{2}u(f_i+ah)$$

(8)

When both individuals purchase formal insurance, the insurance not only changes the autarky value (the right-hand side), but also becomes a direct substitute to the private transfers (the left-hand side) for risk-sharing purpose.

Case C: Only individual 1 purchases formal insurance.

The binding incentive constraints in case C are a combination of case A and B.

$$(1-\frac{\beta}{2})u(f_1+h-ah-\tau^*_{1,C})+\frac{\beta}{2}u(f_1+ah+\tau^*_{2,C})=(1-\frac{\beta}{2})u(f_1+h-ah)+\frac{\beta}{2}u(f_1+ah) \quad (9)$$

$$(1-\frac{\beta}{2})u(f_2+h-\tau^*_{2,C})+\frac{\beta}{2}u(f_2+\tau^*_{1,C})=(1-\beta)u(f_2+h)+\frac{\beta}{2}(u(f_2+h-ah)+u(f_2+ah)) \quad (10)$$

The second-best self-enforcing transfers under each insurance purchase decision combination are the maximum transfers that satisfy the new incentive constraints.

Proposition 1: The availability of formal insurance crowds out private transfers of both individuals regardless of whether formal insurance is purchased or not.

Proof: See Appendix A.

Proposition 1 implies existence of the crowding-out effect on private transfers.

The surprising conclusion is that the crowding-out effect exists even when none of the individuals purchases formal insurance. The intuition is the following.

For risk-averse individuals, higher levels of private transfers always increase *ex-ante* expected utility before full risk sharing is achieved. However, the *ex-post* expected utility conditioning on receiving random income (the left-hand sides of the incentive constraints) is decreasing in private transfers around the optimal solutions. Because if not, individuals can improve *ex-ante* expected utility by increasing private transfers while still satisfying the incentive constraints. This fact contradicts the original solution being optimal.

The introduction of formal insurance affects private risk sharing both through its effect on the autarky value and through the substitution between formal and informal mechanisms for risk-sharing purpose. First, the availability of formal insurance enhances the autarky value regardless of whether individuals choose to purchase formal insurance or not. Since the punishment for deviation is less severe, the original high levels of private transfers are no longer incentive compatible. Second, when individuals purchase formal insurance, to achieve the same level of risk reduction the substitution between the two forms of insurance leads to less demand for private transfers. Both channels contribute to the crowding-out effect. The existence of the first channel is directly responsible for the more than one-for-one crowding-out. This intuition is the same as in Di Tella and MacCulloch (2002), Krueger and Perri (2011), and Baker, Gibbons and Murphy (2004).

Define a_i^* to be a threshold value of risk coverage of formal insurance so that relying solely on formal insurance generates the same level of *ex-ante* expected utility as when formal insurance is not available, i.e.

$$u(f_i + h - a_i^* h) + u(f_i + a_i^* h) = u(f_i + h - \tau_{i,0}^*) + u(f_i + \tau_{-i,0}^*) . \tag{11}$$

Corollary 1. Compared to the case when formal insurance is not available,

(a) When $a \le a_i^*$, individual i 's total degree of risk coverage is lower regardless of whether formal insurance is purchased or not.

(b) When $a > a_i^*$, private transfers are completely crowded out and individual i 's total degree of risk coverage is higher when formal insurance is purchased.

Proof: see Appendix A.

Introducing formal insurance has both positive and negative impact. The risk reduction from formal insurance obviously contributes to risk coverage. But the crowding out on private transfers raises exposure to risk. It can be shown that the degree of risk coverage first decreases with a due to the more than one-for-one crowding-out effect, and then increases with a after all private transfers are crowded out. It finally returns to its original level at $a = a_i^*$. Corollary 1 summarizes the welfare change relative to the level without formal insurance in this process.

The above analysis leads to the conclusion that purchasing formal insurance is the dominant choice for both individuals. It is because even when one does not purchase formal insurance, the original level of private transfers is not sustainable anyway. Purchasing formal insurance can at least ensure some degree of risk coverage.

Proposition 2. Purchasing formal insurance is the dominant choice for both individuals.

Proof: see Appendix A.

Since our model does not restrict fixed income to be the same for the two individuals, the above conclusions apply to both the cases of equal and unequal fixed income. It is nonetheless interesting to understand how the degree of crowding out varies with income inequality. For income inequality generated by transferring fixed income between two equally rich subjects, Genicot (2006) shows that the utility functions demonstrating hyperbolic absolute risk aversion (HARA) will demonstrate higher private transfers, mainly because low-income subjects tend to increase transfers more than the reduction in high-income subjects' transfers. However, under which case the crowding-out effect is larger is generally ambiguous because the answer involves the third order of utility function.[4]

2.3 The crowding-out effect with altruism

Private transfers may be motivated by risk-sharing purpose as well as altruistic concern. Fehr and Schmidt (1999), Bolton and Ockenfels (2000) and Charness and Rabin (2002) have all proposed theories of altruism, and Cox et al. (1998) and Foster and Rosenzweig (2001) both analyze the role of altruism specifically in risk-sharing setting. Foster and Rosenzweig's (2001) empirical study further provides evidence that higher degree of altruism leads to higher level of risk sharing. This section incorporates altruism to analyze the crowding-out effect. We are interested in whether the presence of altruism alters the more than one-for-one crowding-out result, and whether altruism increases or decreases the level of the crowding out.

Following Foster and Rosenzweig's (2001) specification of altruistic preferences, we assume that individual i attach a welfare weight $\gamma_i > 0$ to her partner's consumption utility.

$$v_i = u(c_i) + \gamma_i u(c_{-i}) \tag{12}$$

When the welfare weight γ_i is high enough, perfect risk sharing can be easily

[4] For instance, assume that the degree of risk aversion declines as income rises. In this case, formal insurance leads to smaller improvements in autarky value for higher income levels, which potentially requires a smaller reduction in private transfers to regain balance. But at the same time, the ex-post marginal disutility of making additional private transfers is also lower, so that to achieve a given amount of utility change, we need larger reductions in private transfers. The tradeoff between the two effects leaves us with an ambiguous change in the magnitude of the crowding-out effect. An unreported simulation using CRRA utility function confirms this observation.

achieved, even in the absence of repeated interaction. However, empirical evidence has shown that perfect risk sharing is seldom observed in reality. Theoretically speaking it is possible that an individual will hurt herself to punish the deviator if the deviator’s level of altruism is extremely high. We also want to avoid this unreasonable and largely unrealistic situation. For these two reasons our analysis focuses on the case that altruistic motive is not too strong and the binding incentive constraint determines the optimal solution.

We assume that individual will always purchase insurance in the punishment path.[5] Under this assumption, it can be shown that when $\gamma_i < \frac{u'(f_i + h)}{u'(f_{-i})}$, the most severe punishment individual i can receive is still the autarky value, i.e. no private transfers occur in the punishment path.[6]

Proposition 3: Assume that perfect risk sharing is not achieved in the absence of formal insurance, and in the punishment path both individuals purchase insurance.

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[5] If the deviator cares enough about other individuals' welfare, it is theoretically possible that the punisher will hurt herself to punish the deviator by not purchasing insurance. This punishment can lead to possible crowding in of private transfers when both individuals purchase insurance in equilibrium, because in this case the availability of insurance increases the value of the equilibrium path more than that of the punishment path. However, punishing oneself to hurt others is weird and unrealistic. This strategy is also not likely to be subgame perfect for the punisher. We therefore focus on the more realistic case that both individuals purchase insurance in the punishment path.

[6] In the absence of formal insurance, when $\gamma_i < \frac{u'(f_i + h)}{u'(f_{-i})}$ no private transfers are made in i ’s punishment path; when $\frac{u'(f_i + h)}{u'(f_{-i})} < \gamma_i < \frac{u'(f_i)}{u'(f_{-i} + h)}$, individual i will transfer some income to his partner but his partner will not make any transfer so as to lower i 's utility; when $\gamma_i > \frac{u'(f_i)}{u'(f_{-i} + h)}$, individual i cares so much about his partner that ironically, his partner will consume nothing (by giving all the income to individual i) to hurt i . When formal insurance is available, the situation of the punishment path is similar, only with the cutoff points becoming $\frac{u'(f_i + h - ah)}{u'(f_{-i} + ah)}$ and $\frac{u'(f_i + ah)}{u'(f_{-i} + h - ah)}$.

When $\gamma_i < \dfrac{u'(f_i+h)}{u'(f_{-i})}, i=1,2$ we have [7]

(a) crowding-out effect exists regardless of insurance purchase decision.

(b) the degree of risk coverage is lower than the case without formal insurance when private transfers are not completely crowded out.

Proof: See Appendix A.

Proposition 3 suggests that incorporating altruistic preferences does not alter the previous conclusions on the crowding-out effect as long as the degree of altruism is realistically low. This is because the moderate altruism only weakens the incentive of not giving when random income is received but not reverses it. Appendix A provides the mathematic expression of the incentive constraints with altruism and proves Proposition 3.

Given that the crowding-out effect exists with and without altruism, it is more interesting to explore how the level of crowing out changes with altruistic preferences.

Proposition 4: Assume that perfect risk sharing is not achieved in the absence of formal insurance, and in the punishment path both individuals purchase insurance. When $\gamma_i < \dfrac{u'(f_i+h)}{u'(f_{-i})}, i=1,2$, there exists a cutoff point $\bar{\gamma}_{-i}$ such that for $\gamma_{-i} < \bar{\gamma}_{-i}$, the level of individual i's crowding-out effect increases with the increase of γ_i; and for $\gamma_{-i} \geq \bar{\gamma}_{-i}$, the level of individual i's crowding-out effect decreases with the increase of γ_i.

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[7] When $\gamma_i < \dfrac{u'(f_i+h)}{u'(f_{-i})}, i=1,2$ is not satisfied and $|\gamma_i - \gamma_{-i}|$ is large enough, it is possible to observe crowding in of private transfers. Because the availability of insurance can make the less altruistic individual lower his/her transfers, which actually increases the ex-post utility of the more altruistic individual who cares too much about the partner's welfare. This individual can then afford to increase transfers more and still keep the incentive constraints binding.

Proof: See Appendix A.

It may be intuitive to think that altruism will reduce the crowding-out effect based on the notion that altruism generates higher level of private transfers. However, Proposition 4 suggests that the conclusion depends on the other individual’s degree of altruism. The intuition is as follows. The increase in autarky value induced by the availability of formal insurance is larger when there is altruism, because altruistic individual also takes the other player's welfare gain into account. This fact alone requires larger reduction in private transfers to balance the incentive constraints if the ex-post marginal disutility of making transfers is not changed by altruism.

However, the ex-post marginal disutility is affected by altruism, and the direction of the impact depends on the degree of the partner's altruism. First, keeping the level of transfer constant, the ex-post marginal disutility of making additional transfers after receiving random income is lower if there is altruism. Second, it can be shown that the ex-post marginal disutility of making transfers is larger when the level of transfer is higher, and altruism often increases the transfer level, which makes disutility of making transfer higher. Mathematical analysis in Appendix A shows that when γ_{-i} is low, the increase in equilibrium transfers of both individuals relative to the case of no altruism is small, and so the first effect dominates. In this case, the equilibrium transfers should be reduced more to make the risk-sharing plan attractive. Combined with the larger increase in autarky value, we obtain a larger crowding-out effect with the increase of one's altruism. When γ_{-i} is high, however, the level of private transfers can increase so much with the presence of altruism that the ex-post marginal disutility becomes larger. Even with larger increase in autarky value, only small amount of decrease in private transfers is needed to restore the balance in the incentive constraints. The crowding-out effect is then smaller when there is altruism.

3. Experiment Design

Our experimental design is based on Charness and Genicot's (2009) repeated risk-sharing game. There are four treatments. Treatment 1 and 2 analyze the case of equal fixed income. Treatment 1 is the baseline in which no formal insurance was available. In Treatment 2, we introduced an actuarially fair formal insurance that covers 50% of the risk at the middle point of the repeated interaction. Treatment 3 and Treatment 4 play the similar roles but focus on the case of unequal fixed income.

More specifically, in Treatment 1, two subjects played a repeated risk-sharing game. Subjects were told that the game would end stochastically after 30 periods.[8] In each period, both subjects had fixed income of 125 units of experimental currency, which was unchanged throughout the experiment. Subjects were told that 7.5 units of experimental currency can be exchanged for 1 RMB, approximately 0.15 dollar. In addition to this fixed income, one was equally likely to receive a random income of 0 or 200. If one received 200, the partner would receive 0. After the realization of random income, subjects who received 200 can choose to transfer some amount to their partners. For simplicity, we did not allow subjects who received 0 to make any transfers.[9]

Treatment 2 is identical to Treatment 1, except that starting from the 16th period, subjects had access to a formal insurance before the realization of the random income. At a cost of 50 units of experimental currency, the insurance pays out 100 if the

[8] We increase the probability of ending from 90% to 100% after 30 periods to ensure that most game ends before 35 periods. We do not include observations after 30 periods in our analysis. There are also no substantial differences in behavior relative to that in periods 16–30.

[9] This design feature is slightly different from the one used by Charness and Genicot (2009). In their experiment, all subjects can transfer to their partners. They found that subjects with low random income also transferred, which is not consistent with the theory and is hard to explain. It is possible that subjects with low random income employ transfers as a strategy to encourage their partners to transfer more. To make the analysis simple, we eliminated the possibility of low random income subjects transferring to their partners.

insurer receives 0 random income, and nothing otherwise. This insurance contract covers 50% of the total risk so that there is some room left for private risk-sharing activities. Subjects were not informed that they could purchase formal insurance until the 16th period, and then they had to make a purchase decision each period thereafter. This design feature is more flexible than the stationary strategy assumed in the model. However, this flexibility does not change the conclusion that purchasing formal insurance is the dominant choice. The insurance purchase decision was made before the realization of random income and became common knowledge within the pair. After random income was realized, subjects who purchased insurance and received 0 random income got payment from the insurance, and subjects who received 200 made private transfers.

Treatments 3 and 4 are the same as Treatments 1 and 2 respectively, except that the fixed income was changed to (100,150) to introduce income inequality.

We employed two settings to ensure that subjects were motivated to share risk if they were risk averse. First, the same two subjects interacted repeatedly for least 30 periods, and this repeated play makes risk sharing possible even if they are selfish. Second, a single period was randomly selected to calculate the final payments to the subjects so they were incentivized to smooth consumption over periods. The random selection of payment period also guarantees that subjects' choices were unaffected by income effect due to different histories of income realization.

Private transfers may be motivated by risk-sharing motive as well as altruistic motive. In order to separate the altruistic transfers from the risk-sharing transfers, we also designed a within-subject dictator game to measure the magnitude of the altruistic giving (e.g. Cox and Deck, 2005; Ashraf et al., 2006). At the end of the risk-sharing game, we asked the same group of subjects to play a one-shot dictator game for multiple rounds. We randomly matched two subjects each period and assigned them the roles of senders and receivers. These pairs dissolved immediately at the end of each period. We endowed the subjects with income in all possible before-transfer outcomes in the risk-sharing game.[10] The senders were endowed with the income from those who received 200 random income in the main experiment and could decide how much to transfer to the receivers. To prevent learning in the repeated one-shot game, the receivers were not able to observe the amount they received each period. We did the random rematch for enough periods so that every subject experienced all possible income structures and revealed their altruistic giving. Since the dictator game is a one-shot game there is no incentive to make transfers beyond the altruistic motive.

Our design has a few deviations from Charness and Genicot (2009). First, to simplify the analysis we do not allow subjects receiving no random income to make transfers while Charness and Genicot (2009) do. Second, a given subject in our design only experiences one single fixed income level but their subjects played multiple matches with different fixed income levels. Third, since we need a fixed number of periods to observe choices without and with formal insurance, and to make sample periods comparable across treatments, we stochastically end the game only after 30 periods. While Charness and Genicot's (2009) design that ends the game with either 80% or 90% each period allows a clean test of the existence of rational risk-sharing motive.

We recruited 80 undergraduate students from Peking University to participate

[10] For example, in Treatment 2, if none subjects of a pair purchased insurance, their income before private transfers was (325,125). If they both purchased insurance, their income before private transfers became (275,175). If only one of them purchased insurance, their income before transfers was either (325,175) or (275,125). Therefore, there are four income structures possible in Treatment 2 and we included all of them in designing the parameters of the dictator game to gather each subject's altruistic giving under all possible cases.

the experiment. They were randomly assigned to one of the four treatments, which were run using computers on either May 21 or May 22, 2012. Before the experiment, subjects were required to read the instructions, after which they were asked to solve several testing questions to check their understanding of the game. After all the subjects answered the testing question correctly, the experiment formally began and subjects received further instructions on screen during their play (see Appendix B).[11] Their earnings consisted of a 10 RMB show-up fee and an average of 30 RMB from the games, approximately 6.5 USD in total for about an hour's play.

4. Experiment Result

4.1 Data summary

Figure I graphs the average private transfers across period 1-30 for the four treatments (only including those who received the 200 random income each period). Compared to Treatment 1 and Treatment 3, we see a clear drop in private transfers in Treatment 2 and Treatment 4 in the later 15 periods following the introduction of formal insurance.

[Insert Figure I about here]

Table I summarizes the average private transfers in periods 1–15 and periods 16–30 separately. From these summary statistics, we observe large crowing out of private transfers regardless of whether fixed income is equal or not. In Treatment 2 in which fixed income is equal, with the introduction of formal insurance, the average transfers fall from 64.2 in the former 15 periods to 44.7 in the latter 15 periods. However, the average transfers also fall from 66.8 to 56.8 in Treatment 1, indicating some natural trend of declining in transfers, possibly due to end-of-the-game effect. In Treatment 4 in which *ex-ante* income inequality exists, the average private transfers

[11] The experiment was implemented using "z-Tree" developed by Fischbacher (2007).

go from 76.1 to 38.1 after the introduction of formal insurance. This decrease is much larger than the crowding-out effect under equal income even after controlling for time trend demonstrated in Treatment 3 (from 77.3 to 73.9)

The average transfers in the absence of formal insurance is higher in the unequal fixed income treatments than equal fixed income treatments. This finding is inconsistent with Genicot's (2006) theoretical prediction that for most utility functions the low-income subjects should make higher transfers than the high-income subjects. It is also the opposite of the finding in Charness and Genicot (2009) that average transfer with unequal fixed income is lower, mainly due to low-income subjects giving significantly less than with the equal fixed income. A close look suggests that high-income subjects in our sample gave significantly more than low-income subjects, and the additional transfers are almost entirely driven by altruistic transfers. [12] A couple of design differences from Charness and Genicot (2009), including our restricting those who received zero random income to make transfers and presenting each subject with only one fixed income level, may account for the observed difference. [13]

[Insert Table I about here]

Despite the theoretical prediction that purchasing insurance is the dominant

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[12] After subtracting the altruistic giving measured in the dictator game, the non-altruistic transfers in the first 15 periods of equal treatment is 50, that of the low-income subjects is 55 and high-income subjects is 50. This result is more consistent with the theoretical prediction.

[13] First, those who received zero random income were not allowed to make transfers in our design but could do so in Charness and Genicot (2009). This simplification may increase transfers in our sample, because their opportunity of giving is limited. This effect should be especially strong for high-income subjects if they are partially motivated by altruistic concern. Second, our subjects played with only one fixed income level but not multiple fixed income levels as in Charness and Genicot (2009). If one receives different levels of fixed income, the degree of altruism when receiving high fixed income may not as strong as when one always receives high income. Experiencing the contrast between equal and unequal income structure may also dilute the sense of identification and cooperation in the unequal fixed income treatments, but this effect is smaller in our design due to the lack of contrast.

choice, the insurance take-up rates in the later 15 periods of Treatments 2 and 4 are not very high. In Treatment 2, subjects purchased insurance only 48% of the time. With income inequality, this rate rises to 73%. Low-income subjects are 77% more likely to purchase insurance, slightly higher than the 70% with high-income subjects. Low take-up rate is a puzzling phenomenon both in the laboratory and in the field (Giné et al., 2007; Cai et al., 2012; Cole et al., 2011). For instance, in Trhal and Radermacher's (2009) laboratory experiment, only 51.4% of the subjects chose an actuarially fair insurance that provides full coverage. Cole et al. (2011) find that take-up rate of rainfall insurance in India is only about 5%–10%. The literature proposed many reasons for the low take-up rate in the field, such as liquidity constraint and lack of trust and knowledge (Karlan and Morduch, 2009; Cole et al., 2011), but none of them should matter in the laboratory environment. Later analysis suggests that at least in our experimental context, transfer imbalance has a significant influence on the insurance purchase decision.

4.2 Estimating the crowding-out effect and the welfare impact

Table II reports the formal estimates of the crowding-out effect. Our regression adopts a difference-in-difference approach in which observations from Treatment 1 and 3 are used to control for the underlying time trend in identifying the reduction in private transfers for the equal and unequal fixed income treatments, respectively. In Table II, "Treatment" is a dummy variable indicating that the observations are from Treatments 2 or 4 in which insurance was available in the later 15 periods. "Phase 2" is a dummy variable indicating observations in the later 15 periods of each treatment. The interaction term of the two dummy variables estimates the crowding-out effect. We use a fixed effect model to account for the unobservable subject heterogeneity. Given this approach, the dummy variable "Treatment" is not identifiable because different treatments consist of different subjects. The regressions in this table include only observations of those who received the 200 random income (thus had to make the transfer decision). In all regressions reported in this paper, observations in the first five periods of each treatment are dropped to avoid inaccuracy from the subjects' inexperienced play at the beginning of the game. There are in total five observations in the first 15 periods of Treatment 1 or Treatment 2 that show transfers equal or larger than 200. We drop these outliers to avoid overestimating the crowding-out effect.

[Insert Table II about here]

Column (1) of Table II includes the observations from Treatments 1 and 2 only to identify the crowding-out effect under equal fixed income. The estimated reduction of private transfers is about 13.6 out of 64.2 (the average transfers in the first 15 periods of Treatment 2), which is significantly different from zero. Column (2) looks at the case of unequal fixed income and includes observations only from Treatments 3 and 4. The estimated crowding-out effect is 31.5 out of 77.3, also significantly different from zero. This estimate is much larger than the one in Column (1) in terms of both the absolute and percentage changes. In Columns (3) and (4), we further split the sample of unequal income by low-income and high-income subjects. The estimated crowding-out effect of low-income subjects is 34.4, slightly larger than the 28.1 estimate for the high-income subjects. These findings can be summarized as:

Finding 1: There exists significant crowding-out effect, but the magnitude is much larger when there is ex-ante fixed-income inequality.

It is not surprising to observe significant crowding out of private transfers when formal insurance was introduced. The key question is whether such crowding out is large enough to prevent any significant welfare improvement. There are three aspects to be analyzed for a comprehensive welfare analysis: change in the degree of risk coverage, change in the final income level and altruism. In this study, we focus on the first aspect, to be measured by how sensitive final income is to the arrival of random income. The increase in risk coverage is viewed as welfare improving, with the implicit assumptions that individuals are risk averse.[14] We cannot discuss the second aspect because the total income of the two subjects was fixed and the introduction of actuarially fair insurance makes no change in the expected final income across treatments and phases. To keep the analysis simple, we also do not consider the complex aspect of altruism when doing the following welfare analysis.

Table III reports the estimated change in the degree of risk coverage before and after the introduction of formal insurance. When estimating the crowding-out effect, we only includes observations with 200 random income. However, in the welfare analysis, we include a given subject's entire observations, regardless of whether random income was received or not. We refer to final income as the income after the insurance purchase decision (if any), the realization of random income, and the private transfers. We regress subjects' final income on the amount of random income received (represented by the variable "Random" in Table III) and other variables. If random income has no impact on final income, then there is no risk. If random income has a significant impact on final income, then risk is not perfectly covered.

[14] We have tested subjects' degree of risk aversion using Charness and Genicot's (2009) investment question in our pilot study. We endowed the subjects with 100 units and asked them how much they want to invest in a risky asset that has 50% of making 2.5 times the amount invested and 50% of having zero left. About 81% of the subjects invested less than 100 and so they are for sure risk averse. About 19% invested exactly 100 and they could be either risk averse, risk neutral or risk loving. Since the pilot study and the main study randomly draw subjects from the same population, we are confident that the majority of the population is risk aversion, a conclusion also consistent with Holt and Laury (2002) and many other studies. However, because the use of subject fixed effect makes the impact of risk aversion unidentifiable, and because risk aversion affects the level of informal risk-sharing but not necessary the level of crowding-out (which should depend on the third order of the utility function), we did not include risk averse in the main analysis.

The change in the impact of random income is captured by the coefficient of the triple interaction term "Random*phase2*treatment". A negative sign of this coefficient represents an increase in the degree of risk coverage.

[Insert Table III about here]

When fixed income is equal (Column (1)), we observe a significant reduction in risk exposure. The change in final income induced by one unit increase in random income was reduced by 0.11 units after the introduction of formal insurance. The introduction of formal insurance brings a positive welfare impact in terms of the reduced degree of risk exposure. However, when there is inequality in fixed income (Column (2)), the crowding-out effect is so large that we do not observe a significant change in the degree of risk coverage. This finding is true regardless of whether subjects have low or high fixed income (see Columns (3) and (4)). [15]

The estimates also suggest that perfect risk-sharing is *not* achieved both before and after formal insurance became available. Because the estimates of random income ("Random") are significantly positive in all columns and larger than the reduction in risk exposure after formal insurance was introduced. These findings confirm the assumption that incentive constraints are binding.

Finding 2: The introduction of formal insurance leads to significantly more risk coverage if fixed income is equal, but there is not significant change in the degree of risk coverage if there is fixed income inequality. [16]

[15] Albarran and Attanasio (2003) simulate and confirm the pattern that high income variance leads to lower crowding-out effect. However, they look at public transfer program, which affects the income level and potentially degree of risk aversion of the recipients, but not the risk coverage itself. So the reason for the crowding-out effect is different from that in this paper.

[16] In this paper, we focus on analyzing the crowding-out effect of the formal insurance that provides only partial coverage of the total risks because we think this case is more relevant both for our research purpose and the reality. However, we also carried out experiments to test the effect of introducing full insurance. We find that the insurance take-up rate is up to 70%, but still far from 100%. The crowding-out effect is also much larger than with partial

Corollary 1 predicts that the total risk coverage is decreased when $a \le a_i^*$ and increased when $a > a_i^*$. To have a more accurate test of these predictions, we divide the sample by whether the level of risk-sharing in the absence of insurance is higher than the 50% risk coverage provided by the formal insurance. The average income difference across states is calculated as a measure of risk exposure. The income difference generated by purchasing formal insurance alone (100) is then used as the cutoff point to divide the sample. Table IV reports the regression results.

[Insert Table IV about here]

We first split the sample by average level of risk-sharing in the first 15 periods. We can see that when average level of private risk-sharing is higher than that provided by formal insurance (Column (1)), there is no significant change in level of risk coverage. However, when formal insurance provides better risk coverage than private risk-sharing (Column (2)), there is an overall improvement in total risk coverage. To avoid the possible endogeneity problem of using the same data to do sample splitting and estimate the crowding-out effect, we also split the sample based on the average level of risk-sharing in the first 5 periods (these observations are not used to estimate the crowding-out effect) and report the results in Column (3) and (4). The estimates are essentially unchanged. These results are only partially consistent with Corollary 1, because there is no increase in risk exposure when the introduced formal insurance provides less risk coverage than the level of private risk sharing.

The theoretical analysis suggests that the change in autarky value is the main reason driving the more than one-for-one crowding out result and the negative welfare impact. To understand the observed inconsistency with the theoretical prediction,

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insurance, and we observe significantly increased degree of risk coverage under both equal and unequal fixed income. The regression results of the experiments with full insurance are available upon request.

Table V reports the crowding-out effect and change in risk coverage under different insurance purchase decisions (no one purchased insurance, both purchased insurance, and only one purchased insurance). The effect of autarky value can be directly observed in the case when no one purchases insurance.

[Insert Table V about here]

To make the classification of the insurance purchase combination accurate, observations in the last 15 periods of Treatment 2 or 4 are included to a specific column only when the corresponding insurance purchase combination occurred more than two periods within the same pair. [17] When a given subject has observations included into certain insurance purchase combination, his entire observations in the first 15 periods are also selected into the regression. Observations from the baseline (Treatment 1 or 3) are always included in the regressions to control for the time trend.

There are two possible endogeneity problems in this set of regressions: selection effect and signaling effect. First, it is likely that those who have low level of transfers tend to purchase insurance and also give less in the later 15 periods than other subjects. However, the use of individual fixed effect and the fact that the estimated crowding-out effect is relative to one's own historical transfer should eliminate most of the problem. Additionally, unreported regressions using Treatment 1 and Treatment 3 show that there is no different time trend on transfers for different level of transfers in the first 15 periods. Overall, sample selection is not likely to be a big concern. Second, the decision to purchase insurance can signal a lack of trust that reinforces to the crowding-out effect but this effect is not considered by our simple model. A complete analysis of signaling effect is beyond the scope of this paper. However, in a

[17] For example, if subject A and subject B of a given pair both purchased insurance from period 16 to period 29, but none of them purchased insurance in period 30, their observations from periods 16 to 29 are included in case B, but the observations from period 29 are not included in case A.

related study, we do find evidence that signaling effect contributes to the crowding-out effect when both individuals purchased insurance but weakens this effect when there is asymmetric insurance purchase decision.[18]

For simplicity, we only report the estimates of the interaction terms that identify the crowding-out effect and the change in the degree of risk coverage after insurance was introduced. Panel A analyzes the case of equal fixed income. There is a large percentage of subjects (31.3%) both chose *not* to purchase insurance. Surprisingly, the estimate is positive, which even suggests some degree of crowding in, except that the effect is not significant. In the remaining cases, the estimates all indicate significant crowding-out effect. The effect is strongest when both subjects purchased insurance (Column (2), 27.3% of the sample), with transfer reduced by 30.8. In the case of asymmetric insurance purchase decision (20.7% of the sample), the amount of transfer reduction for those who purchased insurance is 19.7 (Column (3)) and for those who did not purchase insurance is 24.7 (Column (4)). There is significant increase in risk coverage due to the insurance introduction in all cases except for subjects who did not purchase insurance but whose partner did, in which case there is significantly more risk exposure.

Panel B reports the case of unequal fixed income. In only 15.3% of the occasions no subjects chose to purchase insurance. In this case there is no significant crowding out and also no significant change in risk coverage. In 62.0% of the occasions, both subjects chose to purchase insurance, and the private transfers were

[18] In a complementary paper, we include a treatment that uses computer to randomly select the insurance purchase decision for the subjects, and then make the outcome common knowledge within the pair. Under fair insurance that provides 25% risk coverage, we find that the level of crowding-out effect goes from 20 (self-selected insurance) to 14 (randomly-selected insurance) when both subjects purchased insurance, but there is no significant crowding-out effect in both cases when no subjects purchased insurance. When there is asymmetric insurance purchase decision, the crowding-out effect is smaller under self-selected insurance than under randomly-selected insurance.

crowded out by 42 units of experimental currency, the largest among all cases in Table V. Other findings are the same as in Panel A.

Finding 3: When no one purchased insurance, there is no significant crowding-out of private transfers. When both purchased insurance, the crowding-out effect is significant and the largest among all cases.

Estimates in Column (1) of Table V suggest that the change in autarky value alone does not seem to influence subjects' transfer decision. The key channel of the more than one-for-one crowding out of private transfers is therefore not supported. Compare to the direct substitution effect between formal and informal risk-sharing, the impact of the autarky value change requires more complex dynamic consideration, which may be easily ignored by individuals if they are bounded rational.

4.3 Fixed income inequality and transfer imbalance

The existence of ex-ante income inequality is shown to induce larger crowding-out effect and insignificant improvement in risk coverage. This finding is potentially important for policy purpose. However, our simple model cannot offer satisfactory prediction on the impact of income inequality. Charness and Genicot (2009) suggest that "In another sense equal fixed payoffs make it easier for one to identify with the other". Following the same spirit, we hypothesize that historical transfer imbalance is an important determinant of the crowding-out effect. In the analysis below, transfer imbalance is measured by cumulative net transfers, calculated as the difference between the cumulative transfers made and received by the subject.

We first analyze how cumulative net transfers affect insurance purchase decision as well as the level of transfers after controlling for the insurance status, and then use the estimated relationship to explain why unequal fixed income could possibly lead to higher level of crowding out. Because of the individual fixed effect, the estimated impact is essentially identified through overtime variations of the same individual. Therefore it is more convincing to use the estimated results to explain the cross-sectional difference between the equal and unequal fixed income treatments.

[Insert Table VI about here]

Table VI reports the logit regression of the insurance purchase choice. Observations in the latter 15 periods of Treatment 2 and Treatment 4 are pooled in the regression. Cumulative net transfers has a significant positive effect on insurance purchase decision across columns. In other words, the more a given subject gave relative to the amount received, the more likely this subject was to purchase insurance. Columns (4) further control for whether the subject received random income last period, the insurance purchase situations last period, as well as the period dummy variables indicating periods 21–25 and 25–30. However, only one's own insurance purchase decision has a significant positive impact.

Table VII reports how private transfers depend on cumulative net transfers in periods 1–15 and periods 16–30, using observations from Treatment 2 and Treatment 4. For periods 16–30, we report results both with and without insurance purchase decision as the control variables. Interestingly, cumulative net transfers do not affect subsequent transfer levels in the first 15 periods, but has strong negative impact in the later 15 periods, even after controlling for insurance purchase decision. Overall, subjects valued a balanced private relationship. Interestingly, they seemed to be far more sensitive to this notation of fairness after formal insurance was introduced.

[Insert Table VII about here]

The estimates from the overtime variations shed light on the impact of fixed income inequality across individuals. It is obvious that inequality in fixed income leads to more imbalanced transfers in the first 15 periods. The absolute value of the average cumulative net transfers in the first 15 periods is 7.53 in Treatment 2 and 16.53 in Treatment 4. This high level of imbalance could in principle lead to higher insurance purchase rate and lower transfers in the later 15 periods, both contributing to the crowding-out effect. Naturally, the large imbalance is driven by high-income subjects giving significantly more than what they received, which in turn is almost entirely due to altruistic transfers. The average non-altruistic transfers in the first 15 periods of Treatment 4 are 55 and 50 for low-income and high-income subjects, respectively. Overall, the existence of *ex-ante* income inequality introduces a natural barrier for a balanced high-quality relationship, perhaps due to altruism, that is vulnerable to introduction of formal insurance.

Finding 4: Transfer imbalance stimulates more insurance purchase and lower transfers after controlling for insurance status. These effects could in principle explain why unequal fixed income generates larger crowding-out effect.

4.4 The effect of altruism

The within-subject dictator game allows us to distinguish altruistic transfers from risk-sharing transfers in the laboratory for the first time. When the level of altruism is relatively low, the predictions on the existence of crowding out and its impact on risk coverage are not substantially affected by the presence of altruism. However, Proposition 4 does suggest that altruism could contribute to the crowding-out effect when the level of altruism is not too high. This section tests this prediction.

Figure II draws the average transfers in the dictator game at different income gaps designed to mimic all the possible outcomes in the risk-sharing game. There is a reasonable monotonic relationship, with more income inequality generating more altruistic giving. We subtract this altruistic giving from private transfers in the risk-sharing game to obtain the non-altruistic transfers. Altruistic transfers account for about one-third of the total transfers. As Charness and Genicot (2009) point out, the obtained non-altruistic transfers are only approximate, because altruism may interact with risk-sharing motive in a nonlinear way. Recognizing this limitation, we nonetheless report the results to provide some first-time evidence.

[Insert Figure II about here]

Table VIII reports the estimates of the crowding-out effect and the degree of risk reduction using non-altruistic transfers. There are still significant crowding out of private transfers in all columns. However, the level of the crowding out is reduced by almost one-third from the estimates in Table II. Correspondingly, the total risk coverage is increased much more than that with altruism, so that there is significant reduction in risk exposure even for the case with unequal fixed income. Compared to the estimates with altruism, we also find that the crowding-out of altruistic transfers is more severe on high-income subjects than low-income subjects in Treatment 4. This is natural because high-income subjects are more likely to be motivated by altruistic concern in the first place, leading to a more salient effect. Overall, we find evidence conforming to the case of low altruism in Proposition 4 that altruism contributes to the crowding-out effect. The implied low level of altruism is also consistent with the fact that perfect risk-sharing is not achieved in the absence of formal insurance.

[Insert Table VIII about here]

Finding 5: The existence of altruism contributes to the crowding-out effect and exaggerates the degree of risk exposure after introducing formal insurance.

5. Conclusion

It is intuitive to think that the introduction of formal insurance will increase the degree of risk coverage in areas that lack formal institutions to cover risks. This is the main reason underlying the recent promotion of formal insurance by policy makers.

However, this simple view ignores the important interaction between formal and informal institutions.

To the best of our knowledge, this paper performs the first experimental test on the crowding-out effect of formal insurance on informal risk-sharing activities. We investigate the change in total risk coverage, the impact of income inequality and the altruistic motive on the crowding-out effect theoretically and test the model predictions using a clean experiment design.

By introducing a fair formal insurance that provides half risk coverage, we observe significant crowding out of private transfers. However, the reduction in transfers is less than one-for-one, implying no significant decline in risk coverage. We find it likely that subjects were bounded rational in the sense that they responded more to the direct substitution between formal and informal risk-sharing mechanisms but considered less the dynamic impact of the change in autarky value.

Our data suggests that fixed income inequality leads to larger reduction in transfers and so the increase in risk coverage is not as significant as the case of equal fixed income. We present evidence that prior transfer imbalance can lead to higher insurance purchase rate and lower transfers under the same insurance decision. With fixed income inequality, high-income subjects gave more than low-income subjects due to altruistic concern. The resulting high transfer imbalance can potentially explain the larger crowding out of private transfers. We also find that the presence of altruism contributes to the overall crowding-out effect.

Overall, the findings are not as pessimistic as the theoretical predictions, because we observe significantly more risk coverage when subjects purchased insurance and when the fixed income is equal. However, the empirical findings are also not optimistic as the policy makers want. Because the low insurance take-up rate, the existence of income inequality and existence of altruism could all exaggerate the crowding-out effect. Our findings have important policy implications regarding when and where it is more appropriate to promote formal insurance. Further study—especially field evidence—is needed to confirm our results.

Acknowledgments

We would like to thank Abhijit Banerjee, Gary Charness, Janet Currie, Robert Gibbons, Roger Gordon, James Heckman, Jin Li, David Ong, Mark Rosenzweig, and Lixin Xu for their helpful comments and suggestions. Wanchuan Lin and Juanjuan Meng want to acknowledge the financial support from the National Natural Science Foundation of China (Grant No. 70903003, No. 71073002, and No. 71103003) and Key Laboratory of Mathematical Economics and Quantitative Finance (Peking University), Ministry of Education. All comments and suggestions are welcome.

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Table I

Data Summary-Transfer

	Fixed income	Periods with insurance	Mean of Transfer		Insurance take-up rate
			Round 1-15	Round16-30	Round16-30
			(1)	(2)	(3)
Treatment 1	(125,125)	None	66.8	56.8	N/A
Treatment 2	(125,125)	16-30	64.2	44.7	48.0%
Treatment 3	(100 150)	None	77.3	73.9	N/A
Low income	100	None	64.0	61.2	N/A
High income	150	None	92.6	85.8	N/A
Treatment 4	(100,150)	16-30	76.1	38.1	73.3%
Low income	100	16-30	64.0	26.1	76.7%
High income	150	16-30	87.0	49.2	70.0%

Table II

Analysis of the Crowding-out Effect under Equal and Unequal Fixed Income

	Equal fixed income (Treatments 1 and 2)	Unequal fixed income (Treatments 3 and 4)		
	(1)	(2)	(3)	(4)
	Private transfers (overall sample)	Private transfers (overall sample)	Private transfers (low income)	Private transfers (high income)
Phase 2	-8.60***	-3.79	-1.21	-6.77*
	(3.00)	(2.31)	(2.61)	(3.91)
Treatment*phase2	-13.59***	-31.47***	-34.44***	-28.09***
	(4.19)	(3.26)	(3.74)	(5.44)
Constant	66.16***	74.87***	62.10***	88.12***
	(1.62)	(1.26)	(1.41)	(2.16)
Fixed effect	Yes	Yes	Yes	Yes
R-squared	0.05	0.17	0.34	0.15
Observations	495	500	250	250

Note: The regressions in this table include only observations of those who received the 200 random income (thus had to make the transfer decision). Observations in the first 5 periods of each treatment are dropped to avoid inaccuracy from subjects' inexperienced play at the beginning of the game. Column (1) includes observations from Treatment 1 and Treatment 2. Outliers with transfers no less than 200 (only occur in period 1-15) are also dropped to avoid overestimating the crowding-out effect. Column (2)-(4) include observations from Treatment 3 and Treatment 4 only. We use fixed effect model to account for unobserved individual heterogeneity. Therefore the dummy variable "Treatment" is not identifiable. Standard errors are reported in parentheses with *** p<0.01, ** p<0.05, * p<0.1.

Table III

Analysis of the Risk Reduction under Equal and Unequal Fixed Income

	Equal fixed income (Treatments 1 and 2)	Unequal fixed income (Treatments 3 and 4)		
	(1)	(2)	(3)	(4)
	Final income (overall sample)	Final income (overall sample)	Final income (low income)	Final income (high income)
Phase 2	-8.88*	-4.01	-1.55	-6.84
	(5.22)	(3.79)	(5.08)	(5.70)
Treatment*phase2	12.71*	5.91	1.04	11.02
	(7.33)	(5.36)	(7.28)	(7.93)
Random	0.34***	0.23***	0.23***	0.23***
	(0.03)	(0.02)	(0.03)	(0.03)
Random*treatment	-0.00	0.04	0.04	0.04
	(0.04)	(0.03)	(0.04)	(0.04)
Random*phase 2	0.09**	0.04	0.04	0.04
	(0.04)	(0.03)	(0.04)	(0.04)
Random*phase 2	-0.11**	-0.05	-0.04	-0.07
*treatment	(0.05)	(0.04)	(0.05)	(0.06)
Constant	191.24***	200.02***	212.13***	188.12***
	(2.90)	(2.08)	(2.76)	(3.16)
Fixed Effect	Yes	Yes	Yes	Yes
R-squared	0.45	0.37	0.42	0.40
Observations	990	1,000	500	500

Note: The regressions in this table include the complete observations of a given subject regardless of whether the 200 random income was received. Observations in the first 5 periods of each treatment are dropped to avoid inaccuracy from subjects' inexperienced play at the beginning of the game. Outliers with transfers no less than 200 (only occur in period 1-15) are also dropped to avoid overestimating the crowding-out effect. We use fixed effect model to account for unobserved individual heterogeneity. Therefore the dummy variable "Treatment" is not identifiable. Standard errors are reported in parentheses with *** p<0.01, ** p<0.05, * p<0.1.

Table IV

Analysis of the Risk Reduction by Level of Private Risk Sharing in Phase 1

	Divide by average private risk sharing in the first 15 periods		Divide by average private risk sharing in the first 5periods	
	(1)	(2)	(3)	(4)
	Final income	Final income	Final income	Final income
	($a \le a_i^*$)	($a > a_i^*$)	($a \le a_i^*$)	($a > a_i^*$)
Phase 2	-2.59	-12.72***	-3.21	-8.28*
	(3.47)	(3.78)	(4.33)	(4.78)
Treatment*phase2	-0.03	19.84***	2.31	15.33**
	(4.50)	(5.73)	(6.24)	(6.59)
Random	0.05***	0.43***	0.20***	0.33***
	(0.02)	(0.02)	(0.03)	(0.02)
Random*treatment	0.04*	0.09***	0.05	0.02
	(0.02)	(0.03)	(0.04)	(0.03)
Random*phase 2	0.00	0.11***	0.07**	0.08**
	(0.02)	(0.03)	(0.04)	(0.03)
Random*phase 2	0.03	-0.17***	-0.06	-0.12***
*treatment	(0.03)	(0.04)	(0.05)	(0.04)
Constant	218.62***	179.50***	203.38***	188.50***
	(1.76)	(2.20)	(2.35)	(2.67)
Fixed Effect	Yes	Yes	Yes	Yes
R-squared	0.15	0.70	0.35	0.50
Observations	921	1,069	870	1,120

Note: This table pools all observations from Treatment 1 to Treatment 4. The regressions include the complete observations of a given subject regardless of whether the 200 random income was received. Observations in the first 5 periods of each treatment are dropped to avoid inaccuracy from subjects' inexperienced play at the beginning of the game. Outliers with transfers no less than 200 (only occur in period 1-15) are also dropped to avoid overestimating the crowding-out effect. We use fixed effect model to account for unobserved individual heterogeneity. Therefore the dummy variable "Treatment" is not identifiable. Standard errors are reported in parentheses with *** p<0.01, ** p<0.05, * p<0.1.

Table V

Analysis of the Crowding-out Effect and Risk Reduction under Various Insurance Purchase Decision

	(1)	(2)	(3)	(4)
Sample	No one purchased insurance	Both purchased insurance	Subjects who purchased insurance but whose partner did not	Subjects who did not purchase insurance but whose partner did
Panel A: Equal fixed income (Treatment 1 and 2)				
Percentage	31.3%	27.3%	20.7%	20.7%
Crowding-out				
Phase 2*treatment	9.20	-30.83***	-19.68***	-24.73***
	(5.82)	(4.61)	(5.65)	(5.21)
Risk reduction				
Random*phase 2	-0.21***	-0.17***	-0.22***	0.28***
*treatment	(0.07)	(0.06)	(0.07)	(0.07)
Panel B: Unequal fixed income (Treatment 3 and 4)				
Percentage	15.3%	62.0%	11.3%	11.3%
Crowding-out				
Phase 2*treatment	-1.71	-41.82***	-17.96***	-24.80***
	(5.19)	(3.41)	(6.85)	(6.35)
Risk reduction				
Random*phase 2	-0.02	-0.07*	-0.34***	0.16*
*treatment	(0.07)	(0.04)	(0.08)	(0.08)

Note: In each insurance purchase combination, observations from the baseline treatments (no insurance is available) are always included. Observations in the last 15 periods of Treatment 2 or Treatment 4 are included only if the corresponding insurance decision combination was made more than two periods by the same pair. When a subject has observations selected into an insurance purchase combination, his/her entire observations in the first 15 periods are also selected into the regression. We use fixed effect model to account for unobserved individual heterogeneity. Therefore the dummy variable “Treatment” is not identifiable. Standard errors are reported in parentheses with *** p<0.01, ** p<0.05, * p<0.1.

Table VI

A Logit Model of Insurance Purchase Decision in Treatment 2 and Treatment 4

	(1)	(2)	(3)	(4)
	Insurance	Insurance	Insurance	Insurance
Cumulative net transfers	0.15***	0.15***	0.11**	0.12**
	(0.05)	(0.05)	(0.05)	(0.05)
Received random income last		0.02	0.16	0.16
period		(0.25)	(0.26)	(0.26)
Purchased insurance last			1.10***	1.11***
period			(0.25)	(0.25)
Partner purchased insurance			0.40	0.42
last period			(0.29)	(0.29)
Period 21-25				-0.20
				(0.30)
Period 26-30				-0.01
				(0.30)
Fixed effect	Yes	Yes	Yes	Yes
Observations	450	450	450	450

Note: This table analyzes subject's insurance purchase decision in period 15-30 of Treatment 2 and Treatment 4 using logit model. "Cumulative net transfers" is the cumulative transfers made minus the cumulative transfers received. We use fixed effect model to account for unobserved individual heterogeneity. Standard errors are reported in parentheses with *** p<0.01, ** p<0.05, * p<0.1.

Table VII

Analysis of Transfers Before and After Introducing Insurance in Treatment 2 and Treatment 4

	Period 1-15	Period 16-30	
	(1)	(2)	(3)
	Private transfers	Private transfers	Private transfers
Cumulative net transfers	-0.01	-1.07***	-1.06***
	(0.07)	(0.39)	(0.38)
Insurance			-6.14*
			(3.19)
Other insurance			-13.78***
			(3.17)
Constant	69.76***	40.97***	53.24***
	(1.79)	(1.11)	(3.03)
Fixed effect	Yes	Yes	Yes
R-squared	0.00	0.03	0.10
Observations	280	300	300

Note: This table analyzes subject's transfer level before and after the introduction of formal insurance in Treatment 2 and Treatment 4. "Cumulative net transfers" is the cumulative transfers made minus the cumulative transfers received. "Insurance" and "other insurance" refer to purchasing insurance of the two subjects within a given pair respectively. We use fixed effect model to account for unobserved individual heterogeneity. Standard errors are reported in parentheses with *** p<0.01, ** p<0.05, * p<0.1.

Table VIII

Analysis of the Crowding-out Effect and Risk Reduction on Non-altruistic Giving

	Equal fixed income (Treatment 1 and 2)	Unequal fixed income (Treatment 3 and 4)		
	(1)	(2)	(3)	(4)
	Overall	Overall	Low income	High income
Panel A: the crowding-out effect				
Treatment*phase2	-7.63*	-23.12***	-29.88***	-15.89***
	(4.15)	(3.45)	(3.83)	(5.81)
Observations	495	500	253	247
Panel B: the risk reduction				
Random*treatment	-0.17***	-0.14***	-0.15**	-0.12**
*phase2	(0.05)	(0.04)	(0.06)	(0.06)
Observations	990	1,000	500	500
Fixed effect	Yes	Yes	Yes	Yes

Note: This table reports the estimated crowding-out effect and the degree of risk reduction without including altruistic transfers. We subtract the amount of altruistic transfers measured in the dictator game from the total private transfers and do the estimations based on this non-altruistic transfer. “Random” stands for the random income subjects received subjects. “Phase2” is the dummy variable for period 16-30. “Treatment” is the dummy variable indicating treatments with insurance available in the later 15 periods. We use fixed effect model to account for unobserved individual heterogeneity. Therefore the dummy variable “Treatment” is not identifiable. Standard errors are reported in parentheses with *** p<0.01, ** p<0.05, * p<0.1.

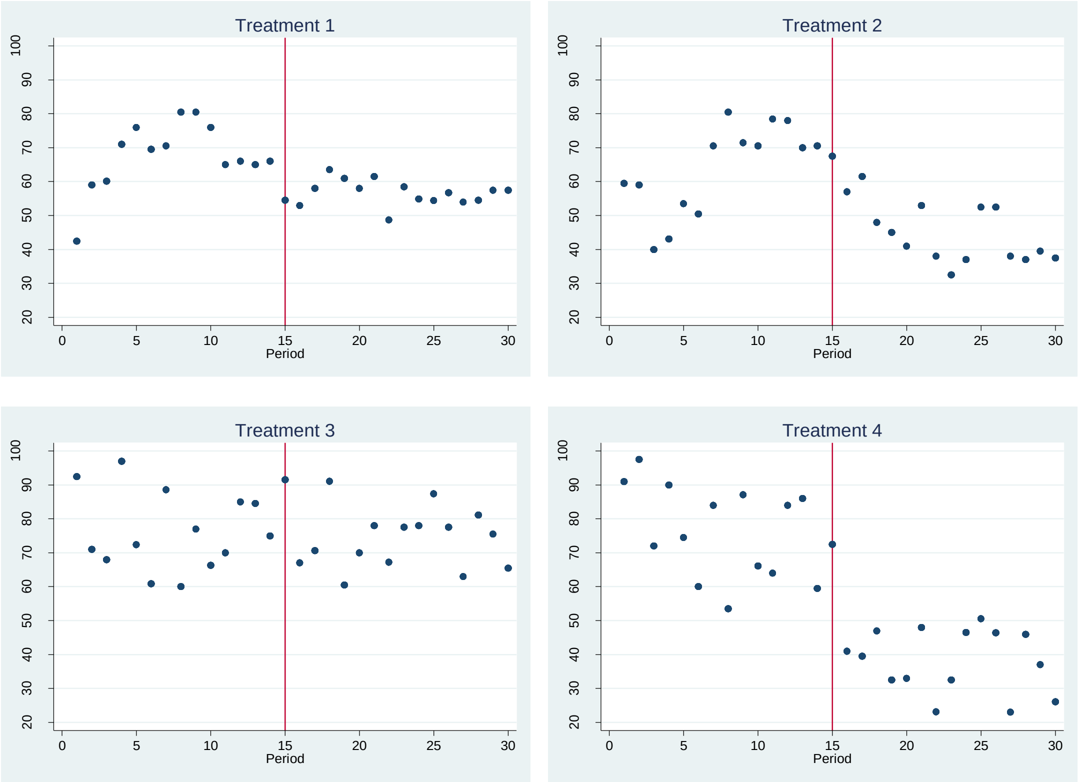

Figure I. The Average Transfer across Periods

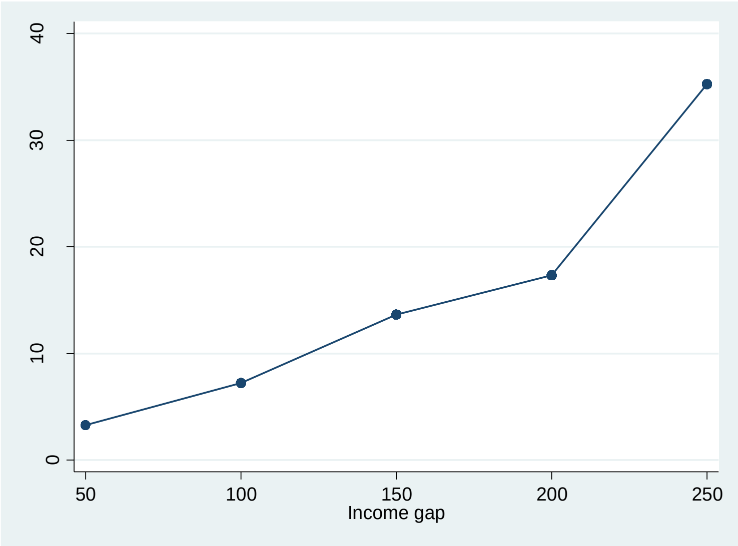

Figure II. The Average Altruistic Transfer

Appendix

(Not intended for publication)

Appendix A

Proof of Proposition 1

To analyze the crowding-out effect, we first characterize the relationship between the amount of insurance introduced (a) and the level of private transfers (τ_i^*), taking insurance purchase decision as given. Later we analyze the optimal insurance purchase decision given the subsequent optimal private transfers.

A: None of the individuals purchase formal insurance

Let $\tau_{2,A}(\tau_{1,A}, a)$ be the level of individual 2's transfer determined by individual 2's incentive constraint. We can show that

(A.1) $$\frac{d\tau_{2,A}(\tau_{1,A}, a)}{d\tau_{1,A}} = \frac{\frac{\beta}{2}u'(f_2 + \tau_{1,A})}{(1-\frac{\beta}{2})u'(f_2 + h - \tau_{2,A})} > 0$$

(A.2) $$\frac{d\tau_{2,A}(\tau_{1,A}, a)}{da} = \frac{\frac{\beta}{2}h(u'(f_2 + h - ah) - u'(f_2 + ah))}{(1-\frac{\beta}{2})u'(f_2 + h - \tau_{2,A})} < 0$$

Based on $\tau_{2,A}(\tau_{1,A}, a)$, equation (A3) defines $g_A(\tau_{1,A}, a)$ as the difference between the left and right hand sides of individual 1's incentive constraint, with

(A.3) $$\begin{aligned} g_A(\tau_{1,A}, \tau_{2,A}(\tau_{1,A}, a), a) = {} & (1-\frac{\beta}{2})u(f_1 + h - \tau_{1,A}) + \frac{\beta}{2}u(f_1 + \tau_{2,A}(\tau_{1,A}, a)) \\ & -(1-\beta)u(f_1 + h) - \frac{\beta}{2}(u(f_1 + h - ah) + u(f_1 + ah)) \end{aligned}$$

Next we show how $g_A(\tau_{1,A}, \tau_{2,A}(\tau_{1,A}, a), a)$ changes with $\tau_{1,A}$ and a.

$$\frac{\partial g_A(\tau_{1,A},\tau_{2,A}(\tau_{1,A},a),a)}{\partial \tau_{1,A}} =$$

(A.4) $$\frac{(\frac{\beta}{2})^2 u'(f_1+\tau_{2,A})u'(f_2+\tau_{1,A})-(1-\frac{\beta}{2})^2 u'(f_1+h-\tau_{1,A})u'(f_2+h-\tau_{2,A})}{(1-\frac{\beta}{2})u'(f_2+h-\tau_{2,A})}$$

(A.5) $$\frac{\partial g_A(\tau_{1,A},\tau_{2,A}(\tau_{1,A},a),a)}{\partial a} = \frac{\beta}{2}u'(f_1+\tau_{2,A})\frac{\frac{\beta}{2}h(u'(f_2+h-ah)-u'(f_2+ah))}{(1-\frac{\beta}{2})u'(f_2+h-\tau_{2,A})} + \frac{\beta}{2}h(u'(f_1+h-ah)-u'(f_1+ah))$$

If individuals are risk averse, purchasing a fair insurance that provides partial coverage always improves the autarky value by allowing better risk coverage in the future. To see this, notice that such an insurance contract makes $f_i + ah < f_i + h - ah, i = 1,2$, hence $u'(f_i + ah) > u'(f_i + h - ah)$. Therefore we always have $\frac{\partial g_A(\tau_{1,A},\tau_{2,A}(\tau_{1,A},a),a)}{\partial a} < 0$.

The sign of equation (A.4) depends on the numerator, which reflects the weighted difference between marginal utilities of income when there is no income shock and when there is positive income shock, and the weight is generated from time preferences. On one hand, because in most cases private transfers cannot completely eliminate income fluctuations, we have $f_i + h - \tau_{i,A} \geq f_i + \tau_{-i,A}$ and $u'(f_i + h - \tau_{i,A}) - u'(f_i + \tau_{-i,A}(\tau_{i,A})) \leq 0, i = 1,2.$ These facts tend to make the numerator positive. On the other hand, because $0 < \beta < 1$, we also have $1 - \frac{\beta}{2} > \frac{\beta}{2}$, which tends to make the numerator negative. The reason for this weight difference is that the incentive constraints can only be binding when there is positive random income. Since this case is current and not discounted, the resulting utility is relatively more important if individuals discount future consumption. Overall, the effects of time discounting and difference in marginal utility go against each other.

When $\tau_{1,A}=0$, which effect dominates is ambiguous. However, as $\tau_{1,A}$ rises ($\tau_{2,A}$ rises at the same time too), the difference between marginal utilities of the two states diminishes, the effect of time discounting is maintained, and the attractiveness of private risk sharing arrangement therefore declines. When the transferred amount is high enough such that there is little difference between income under the two states of the nature for both individuals, the effect of time discounting eventually dominates and $\frac{dg_A(\tau_{1,A},\tau_{2,A}(\tau_{1,A},a),a)}{d\tau_{1,A}}<0$. It is easy to see that the constrained optimal transfer must occur in a region where $\frac{\partial g_A(\tau_{1,A},\tau_{2,A}(\tau_{1,A},a),a)}{\partial \tau_{1,A}}|_{\tau_{1,A}=\tau^*_{1,A}}<0$. Because if not, we can always increase private transfers $\tau_{1,A}$ to achieve better risk sharing, but still satisfy the incentive constraint.

Let $\tau^*_{1,0}$ denote the constraint optimal transfer when formal insurance is not available. By definition we have $g_A(\tau^*_{1,0},\tau_{2,A}(\tau^*_{1,0},0),0)=0$. After the introduction of formal insurance ($a>0$), the autarky value is improved, which implies that $g_A(\tau^*_{1,0},\tau_{2,A}(\tau^*_{1,0},0),0)<0$. To make the balance that $g_A(\tau^*_{1,A},\tau_{2,A}(\tau^*_{1,A},a),a)=0$, the private transfers of individual 1 must be lower to make the ex-post expected utility along the equilibrium path higher. Since transfers of individual 1 and 2 are negatively related, $\tau^*_{2,A}$ must be lower as well. We therefore have $\tau^*_{i,0}>\tau^*_{i,A}$.

B: Both individuals purchase formal insurance

Similar to case A, we also define the difference between the left-hand side and the right-hand side of individual 1's incentive constraint expressed in Equation (11) as

$g_B(\tau_{1,B}, \tau_{2,B}(\tau_{1,B}, a), a)$, where $\tau_{2,B}(\tau_{1,B}, a)$ is defined by individual 2's incentive constraint.

(A.6)
$$g_B(\tau_{1,B}, \tau_{2,B}(\tau_{1,B}, a), a) = (1-\frac{\beta}{2})u(f_1 + h - ah - \tau_{1,B}) + \frac{\beta}{2}u(f_1 + ah + \tau_{2,B}(\tau_{1,B}, a)) - (1-\frac{\beta}{2})u(f_1 + h - ah) - \frac{\beta}{2}u(f_1 + ah)$$

Taking derivative with respect to $\tau_{1,B}^{*}$ and a respectively leads us to

(A.7)
$$\frac{\partial g_B(\tau_{1,B}, \tau_{2,B}(\tau_{1,B}, a), a)}{\partial \tau_{1,B}} = \frac{1}{(1-\frac{\beta}{2})u'(f_2 + h - ah - \tau_{2,B})}((\frac{\beta}{2})^2 u'(f_1 + ah + \tau_{2,B})u'(f_2 + ah + \tau_{1,B}) - (1-\frac{\beta}{2})^2 u'(f_1 + h - ah - \tau_{1,B})u'(f_2 + h - ah - \tau_{2,B}))$$

(A.8)
$$\frac{\partial g_B(\tau_{1,B}, \tau_{2,B}(\tau_{1,B}, a), a)}{\partial a} = h(((1-\frac{\beta}{2})u'(f_1 + h - ah) - \frac{\beta}{2}u'(f_1 + ah)) - ((1-\frac{\beta}{2})u'(f_1 + h - ah - \tau_{1,B}) - \frac{\beta}{2}u'(f_1 + ah + \tau_{2,B})) + \frac{\beta}{2}u'(f_1 + ah + \tau_{2,B})\frac{d\tau_{2,B}(\tau_{1,B}, a)}{da}$$

Similarly, before perfect risk sharing is achieved, we always have $f_i + ah + \tau_{-i,B} < f_i + h - ah - \tau_{i,B}$ as well as $f_i + ah < f_i + h - ah, i = 1, 2.$ Similar to case A, when the transfers are large enough the time discounting effect will dominate, and the constrained optimal solution must be operating in a region where $\frac{\partial g_B(\tau_{1,B}, \tau_{2,B}(\tau_{1,B}, a), a)}{\partial \tau_{1,B}}\Big|_{\tau_{1,B} = \tau_{1,B}^{*}} < 0$.

Regarding the sign of equation (A.8), first notice that the difference between the wealth levels in two states of the nature is smaller when there are both formal insurance and positive transfer, i.e. $f_1 + ah < f_1 + ah + \tau_{2,B} < f_1 + h - ah - \tau_{1,B} < f_1 + h - ah$. It can be easily shown that this relationship makes $\frac{d\tau_{2,B}(\tau_{1,B},a)}{da}<0$ hence $\frac{\partial g_B(\tau_{1,B},\tau_{2,B}(\tau_{1,B},a),a)}{\partial a}<0$.

Overall, we come to the same conclusion that when both individuals purchase formal insurance, private transfers will be lower to make the incentive constraints binding again.

C: Individual 1 purchases formal insurance but individual 2 does not.

The proof is similar to that in case A and case B.

As Equation (A.9) suggests, the ex-post autarky value (right-hand sides of the incentive constraints) in case A is higher than that in case B, because in case A individuals do not have to pay the insurance premium in the current period when there is a good income shock.

$$\begin{aligned}(1-\beta)u(f_i+h)+\frac{\beta}{2}(u(f_i+h-ah)+u(f_i+ah))\\ >(1-\frac{\beta}{2})u(f_i+h-ah)+\frac{\beta}{2}u(f_i+ah)\end{aligned} \tag{A.9}$$

To satisfy the binding incentive constraints, the amount of risk coverage from both the formal insurance and private transfers must be larger in case B. We therefore have $ah+\tau_{i,B}^*>\tau_{i,A}^*$. To compare with case C, we separately look at the situation when $\tau_{i,A}^*\geq\tau_{i,B}^*$ and $\tau_{i,A}^*<\tau_{i,B}^*$.

It is easy to see that $\tau_{2,C}(\tau_{1,A}^*,a)=\tau_{2,A}^*$, which implies that

$$\begin{aligned}g_C(\tau_{1,A}^*,\tau_{2,A}^*,a)=(1-\frac{\beta}{2})u(f_1+h-ah-\tau_{1,A}^*)+\frac{\beta}{2}u(f_1+ah+\tau_{2,A}^*)\\ -(1-\frac{\beta}{2})u(f_1+h-ah)-\frac{\beta}{2}u(f_1+ah)\end{aligned} \tag{A.10}$$

Assuming $\tau_{i,A}^*\geq\tau_{i,B}^*$, because $g_C(\tau_{1,B}^*,\tau_{2,B}^*,a)=g_B(\tau_{1,B}^*,\tau_{2,B}^*,a)=0$ we can see that $g_C(\tau_{1,A}^*,\tau_{2,A}^*,a)<0$. In order for $\tau_{i,C}^*$ to make the balance that $g_C(\tau_{1,C}^*,\tau_{2,C}^*,a)=0$, it must be that $\tau^*_{i,A} > \tau^*_{i,C}$. We can do similar analysis on individual 2 to get the conclusion that $\tau^*_{i,C} > \tau^*_{i,B}$.

Proof of Corollary 1

Based on the results from proposition 1, it is easy to analyze the welfare change of introducing formal insurance. Let $u_{i,j}, i = 1,2; j = 0, A, B, C$ represent the optimal ex-ante expected utility for each individual under each insurance purchase decision. When a is sufficiently small so that private transfers are not completely crowded out, we have $\tau_{i,0} > \tau_{i,A}, \tau_{i,0} > ah + \tau_{i,B}, \tau_{1,0} > ah + \tau_{1,C}$, and $\tau_{2,0} > \tau_{2,C}$. This is because the autarky value is always increased, and the total risk coverage that can be sustained in equilibrium (including both formal and private risk coverage) must be lower. Therefore the ex-ante expected utility is always lower regardless of whether individuals purchase insurance or not.

When risk coverage from formal insurance a is sufficiently high so that private transfers are entirely crowded out, incentive constraints can only be satisfied when both individuals purchase insurance. In this case, introducing additional degree of risk coverage from formal insurance only brings positive welfare impact. If a passes the threshold a^*_i determined by equation (14), then the introduction of formal insurance increases welfare.

Proof of Proposition 2

Let case D represent the situation that individual 1 does not purchase insurance but individual 2 does. The optimal solution under this case can be easily derived from case C because of the symmetry. We then present the strategic insurance purchase decision in the following 2 by 2 normal form game. The following analysis shows that $u_{1,C} > u_{1,A}$, $u_{2,B} > u_{2,C}$, $u_{1,B} > u_{1,D}$ and $u_{2,D} > u_{2,A}$. Given these inequalities, purchase formal insurance is the dominant choice for both individuals.

		Individual 2	
		Not Purchase	Purchase
Individual 1	Not Purchase	$u_{1,A}, u_{2,A}$	$u_{1,D}, u_{2,D}$
	Purchase	$u_{1,C}, u_{2,C}$	$u_{1,B}, u_{2,B}$

From the inequalities in Proposition 1 we can further see that when $\tau^*_{i,A} \geq \tau^*_{i,B}$ we have $ah + \tau^*_{1,C} > ah + \tau^*_{1,B} > \tau^*_{1,A}$ and $ah + \tau^*_{2,B} > \tau^*_{2,A} > \tau^*_{2,C}$, which implies that $u_{1,C} > u_{1,B} > u_{1,A}$ and $u_{2,B} > u_{2,A} > u_{2,C}$.

The analysis when $\tau^*_{i,A} < \tau^*_{i,B}$ is exactly the same, with the sign of the inequality reversed. Therefore we have $\tau^*_{1,A} < ah + \tau^*_{1,C} < ah + \tau^*_{1,B}$ and $\tau^*_{2,A} < \tau^*_{2,C} < ah + \tau^*_{2,B}$, which imply that $u_{1,B} > u_{1,C} > u_{1,A}$ and $u_{2,B} > u_{2,C} > u_{2,A}$.

Similarly, when $\tau^*_{i,A} \geq \tau^*_{i,B}$, we have $ah + \tau^*_{1,B} > \tau^*_{1,A} > \tau^*_{1,D}$ and $ah + \tau^*_{2,D} > ah + \tau^*_{2,B} > \tau^*_{2,A}$, based on which we can conclude that $u_{1,B} > u_{1,A} > u_{1,D}$ and $u_{2,D} > u_{2,B} > u_{2,A}$. When $\tau^*_{i,A} < \tau^*_{i,B}$, we have $\tau^*_{1,A} < \tau^*_{1,D} < ah + \tau^*_{1,B}$ and $\tau^*_{2,A} < ah + \tau^*_{2,D} < ah + \tau^*_{2,B}$, and therefore $u_{1,B} > u_{1,D} > u_{1,A}$ and $u_{2,B} > u_{2,D} > u_{2,A}$.

Proof of Proposition 3

When $\gamma_i < \dfrac{u'(f_i + h)}{u'(f_{-i})}, i = 1,2$ individuals will purchase insurance and make zero transfers in the punishment path. The binding incentive constraint in the absence of formal insurance is given by

$$(1-\frac{\beta}{2})(u(f_i+h-\tau_{i,0}^A)+\gamma_i u(f_{-i}+\tau_{i,0}^A))+\frac{\beta}{2}(u(f_i+\tau_{-i,0}^A)+\gamma_i u(f_{-i}+h-\tau_{-i,0}^A))$$

(A.11)

$$=(1-\frac{\beta}{2})(u(f_i+h)+\gamma_i u(f_{-i}))+\frac{\beta}{2}(u(f_i)+\gamma_i u(f_{-i}+h))$$

Case A: None of the two individuals purchases formal insurance.

(A.12)

$$(1-\frac{\beta}{2})(u(f_i+h-\tau_{i,A}^A)+\gamma_i u(f_{-i}+\tau_{i,A}^A))+\frac{\beta}{2}(u(f_i+\tau_{-i,A}^A)+\gamma_i u(f_{-i}+h-\tau_{-i,A}^A))$$
$$=(1-\beta)(u(f_i+h)+\gamma_i u(f_{-i}))+\frac{\beta}{2}(u(f_i+h-ah)+u(f_i+ah)+\gamma_i u(f_{-i}+ah)+\gamma_i u(f_{-i}+h-ah))$$

Case B: Both individuals purchase formal insurance.

(A.13)

$$(1-\frac{\beta}{2})(u(f_i+h-ah-\tau_{i,B}^A)+\gamma_i u(f_{-i}+ah+\tau_{i,B}^A))+\frac{\beta}{2}(u(f_i+ah+\tau_{-i,B}^A)+\gamma_i u(f_{-i}+h-ah-\tau_{-i,B}^A))$$
$$=(1-\frac{\beta}{2})(u(f_i+h-ah)+\gamma_i u(f_{-i}+ah))+\frac{\beta}{2}(u(f_i+ah)+\gamma_i u(f_{-i}+h-ah))$$

Case C: Only individual 1 purchases formal insurance.

(A.14)

$$(1-\frac{\beta}{2})(u(f_1+h-ah-\tau_{1,C}^A)+\gamma_i u(f_2+\tau_{1,C}^A))+\frac{\beta}{2}(u(f_1+ah+\tau_{2,C}^A)+\gamma_i u(f_2+h-\tau_{2,C}^A))$$
$$=(1-\beta)(u(f_1+h-ah)+\gamma_i u(f_2))+\frac{\beta}{2}(u(f_1+ah)+u(f_1+h-ah)+\gamma_i u(f_2+ah)+\gamma_i u(f_2+h-ah))$$

(A.15)

$$(1-\frac{\beta}{2})(u(f_2+h-\tau_{2,C}^A)+\gamma_i u(f_1+ah+\tau_{2,C}^A))+\frac{\beta}{2}(u(f_2+\tau_{1,C}^A)+\gamma_i u(f_1+h-ah-\tau_{1,C}^A))$$
$$=(1-\beta)(u(f_2+h)+\gamma_i u(f_1+ah))+\frac{\beta}{2}(u(f_2+h-ah)+u(f_2+ah)+\gamma_i u(f_1+h-ah)+\gamma_i u(f_1+ah))$$

Let's use case B as an example. The analysis of the crowding-out effect and the change in total risk coverage is essentially the same as in Proposition 1 and Corollary 1. Define the difference between the left-hand side and the right-hand side of Equation (A.13) as $g_B^A(\tau_{i,B}^A,\tau_{-i,B}^A(\tau_{i,B}^A,a),a)=g_B^A(\tau_{i,B}^A,a)$, where $\tau_{-i,B}^A(\tau_{i,B}^A,a)$ is a function of $\tau_{i,B}^A$ and a (the functional form is determined by the incentive constraint of individual $-i$). The crowding-out effect can be represented by

(A.16) $$\frac{\partial \tau_{i,B}^{A}}{\partial a} = -\frac{\partial g_{B}^{A}(\tau_{i,B}^{A}, a)}{\partial a} / \frac{\partial g_{B}^{A}(\tau_{i,B}^{A}, a)}{\partial \tau_{i,B}^{A}}$$

where

(A.17) $$\frac{\partial g_{B}^{A}(\tau_{i,B}^{A}, a)}{\partial \tau_{i,B}^{A}} = (1-\frac{\beta}{2})(\gamma_i u'(f_{-i}+ah+\tau_{i,B}^{A}) - u'(f_i + h - ah - \tau_{i,B}^{A})) \\ + \frac{\beta}{2}(u'(f_i + ah + \tau_{-i,B}^{A}) - \gamma_i u'(f_{-i} + h - ah - \tau_{-i,B}^{A}))(\partial \tau_{-i,B}^{A} / \partial \tau_{i,B}^{A})$$

(A.18)

$$\frac{\partial g_{B}^{A}(\tau_{i,B}^{A}, a)}{\partial a} \\ = (1-\frac{\beta}{2})h(\gamma_i u'(f_{-i}+ah+\tau_{i,B}^{A}) - \gamma_i u'(f_{-i}+ah) + u'(f_i + h - ah) - u'(f_i + h - ah - \tau_{i,B}^{A})) \\ + \frac{\beta}{2}h(u'(f_i + ah + \tau_{-i,B}^{A}) - u'(f_i + ah) + \gamma_i u'(f_{-i} + h - ah) - \gamma_i u'(f_{-i} + h - ah - \tau_{-i,B}^{A})) \\ + \frac{\beta}{2}(u'(f_i + ah + \tau_{-i,B}^{A}) - \gamma_i u'(f_{-i} + h - ah - \tau_{-i,B}^{A}))(\partial \tau_{-i,B}^{A} / \partial a)$$

and individual $-i$'s binding incentive constraints determines that

(A.19) $$\partial \tau_{-i,B}^{A} / \partial \tau_{i,B}^{A} = \frac{\frac{\beta}{2}(u'(f_{-i}+ah+\tau_{i,B}^{A}) - \gamma_{-i} u'(f_i + h - ah - \tau_{i,B}^{A}))}{(1-\frac{\beta}{2})(u'(f_{-i}+h-ah-\tau_{-i,B}^{A}) - \gamma_{-i} u'(f_i + ah + \tau_{-i,A}^{A}))}$$

(A.20)

$$\partial \tau_{-i,B}^{A} / \partial a = \\ ((1-\frac{\beta}{2})h(\gamma_{-i} u'(f_i + ah + \tau_{-i,B}^{A}) - \gamma_{-i} u'(f_i + ah) + u'(f_{-i} + h - ah) - u'(f_{-i} + h - ah - \tau_{-i,B}^{A})) \\ + \frac{\beta}{2}h(\gamma_{-i} u(f_i + h - ah) - \gamma_{-i} u'(f_i + h - ah - \tau_{i,B}^{A}) + u'(f_{-i} + ah + \tau_{i,B}^{A}) - u'(f_{-i} + ah))) \\ /(1-\frac{\beta}{2})(u'(f_{-i} + h - ah - \tau_{-i,B}^{A}) - \gamma_{-i} u'(f_i + ah + \tau_{-i,A}^{A}))$$

Similar to the case without altruism, we still have $\frac{\partial g_{B}^{A}(\tau_{i,B}^{A}, a)}{\partial \tau_{i,B}^{A}} < 0$. Intuitively, this is again because when perfect risk sharing is not achieved in the absence of formal insurance, individual i should not have incentive to increase transfers when he receives the random income. Otherwise one can increase transfers (hence the value of the objective function) yet still satisfy the incentive constraints. Mathematically, we can show that when $\gamma_i < \frac{u'(f_i+h)}{u'(f_{-i})}, i=1,2$, $\partial\tau_{-i,B}^A / \partial\tau_{i,B}^A > 0$. Also when $\tau_{i,B}^A$ and $\tau_{-i,B}^A$ are relatively high, the effect of time discounting dominates and the negative sign of $(\gamma_i u'(f_{-i}+ah+\tau_{i,B}^A) - u'(f_i+h-ah-\tau_{i,B}^A))$ will dominate, and so $\frac{\partial g_B^A(\tau_{i,B}^A, a)}{\partial\tau_{i,B}^A} < 0$.

The sign of $\frac{\partial g_B^A(\tau_{i,B}^A, a)}{\partial a}$ depends on the last term, because the addition of the first two parts is negative due to decreasing marginal utility. Some calculation suggests that

(A.19) $$sign(\partial\tau_{-i,B}^A / \partial a) = sign(\gamma_{-i} u'(f_i+ah+\tau_{-i,B}^A) - u'(f_{-i}+h-ah-\tau_{-i,B}^A))$$

When $\gamma_i < \frac{u'(f_i+h)}{u'(f_{-i})}, i=1,2$, it is easy to see that $\partial\tau_{-i,B}^A / \partial a < 0$ and $(u'(f_i+ah+\tau_{-i,B}^A) - \gamma_i u'(f_{-i}+h-ah-\tau_{-i,B}^A)) > 0$. We therefore have $\frac{\partial g_B^A(\tau_{i,B}^A, a)}{\partial a} < 0$.

In all, we can show that $\frac{\partial\tau_{i,A}^A}{\partial a} < 0$, i.e. adding altruism does not affect the two channels of the crowding-out effect as long as altruism does not lead to perfect risk sharing before insurance is available. Further, since the autarky value is improved, we know that $ah + \tau_{i,B}^A < \tau_{i,0}^A$, which implies that the total level of risk sharing is reduced before private transfers are not completely crowded out.

Similar analysis and intuition can be easily extended to case A and case C.

Proof of Proposition 4

We again use case B as an example. To understand how the presence of altruism affects crowding-out effect, we take derivative with respect to γ_i on Equation (A.16).

(A.20) $sign(\frac{\partial(\partial\tau_{i,B}^A/\partial a)}{\partial\gamma_i})=sign(\frac{\partial g_B^A(\tau_{i,B}^A,a)}{\partial a}\frac{\partial^2 g_B^A(\tau_{i,B}^A,a)}{\partial\tau_{i,B}^A\partial\gamma_i}-\frac{\partial^2 g_B^A(\tau_{i,B}^A,a)}{\partial a\partial\gamma_i}\frac{\partial g_B^A(\tau_{i,B}^A,a)}{\partial\tau_{i,B}^A})$

(A.21)

$$\frac{\partial^2 g_B^A(\tau_{i,B}^A,a)}{\partial\tau_{i,B}^A\partial\gamma_i}=(1-\frac{\beta}{2})u'(f_{-i}+ah+\tau_{i,B}^A)-\frac{\beta}{2}u'(f_{-i}+h-ah-\tau_{-i,B}^A)(\partial\tau_{-i,B}^A/\partial\tau_{i,B}^A))$$
$$=sign((1-\frac{\beta}{2})^2((u'(f_{-i}+ah+\tau_{i,B}^A)(u'(f_{-i}+h-ah-\tau_{-i,B}^A)-\gamma_{-i}u'(f_i+ah+\tau_{-i,B}^A))$$
$$-(\frac{\beta}{2})^2((u'(f_{-i}+h-ah-\tau_{-i,B}^A)(u'(f_{-i}+ah+\tau_{i,B}^A)-\gamma_{-i}u'(f_i+h-ah-\tau_{i,B}^A))$$

When $\gamma_{-i}=0$, it is easy to see that $\frac{\partial^2 g_B^A(\tau_{i,B}^A,a)}{\partial\tau_{i,B}^A\partial\gamma_i}|_{\gamma_{-i}=0}>0$ because $1-\frac{\beta}{2}>\frac{\beta}{2}$. Further we can show that $\frac{\partial^2 g_B^A(\tau_{i,B}^A,a)}{\partial\tau_{i,B}^A\partial\gamma_i}$ is increasing in γ_{-i} again because $1-\frac{\beta}{2}>\frac{\beta}{2}$ and decreasing marginal utility. Therefore we know that $\frac{\partial^2 g_B^A(\tau_{i,B}^A,a)}{\partial\tau_{i,B}^A\partial\gamma_i}>0$.

(A.22)

$$\frac{\partial^2 g_B^A(\tau_{i,B}^A,a)}{\partial a\partial\gamma_i}=(1-\frac{\beta}{2})h(u'(f_{-i}+ah+\tau_{i,B}^A)-u'(f_{-i}+ah))$$
$$+\frac{\beta}{2}h(u'(f_{-i}+h-ah)-u'(f_{-i}+h-ah-\tau_{-i,B}^A))$$
$$-\frac{\beta}{2}u'(f_{-i}+h-ah-\tau_{-i,B}^A)(\partial\tau_{-i,B}^A/\partial a)$$

Substitute Equation (A.20) into Equation (A.22). After some calculation we can see that when $\gamma_{-i}=0$, we have

$$\frac{\partial^2 g_B^A(\tau_{i,B}^A,a)}{\partial a\partial\gamma_i}|_{\gamma_{-i}=0}=(h(u'(f_{-i}+ah+\tau_{i,B}^A)-u'(f_{-i}+ah))((1-\frac{\beta}{2})^2-(\frac{\beta}{2})^2)/(1-\frac{\beta}{2})<0 \quad .$$

Because $\gamma_i<\frac{u'(f_i+h)}{u'(f_{-i})}, i=1,2$, one can show that $\partial\tau_{-i,B}^A/\partial a$ is decreasing γ_{-i}, and so $\frac{\partial^2 g_B^A(\tau_{i,B}^A, a)}{\partial a \partial \gamma_i}$ is increasing in γ_{-i}.

So far we know that $\frac{\partial g_B^A(\tau_{i,B}^A, a)}{\partial a} < 0, \frac{\partial^2 g_B^A(\tau_{i,B}^A, a)}{\partial \tau_{i,B}^A \partial \gamma_i} > 0,$ and $\frac{\partial g_B^A(\tau_{i,B}^A, a)}{\partial \tau_{i,B}^A} < 0$.

Therefore there exists a cutoff point $\bar{\gamma}_{-i}$ such that for $\gamma_{-i} < \bar{\gamma}_{-i}$, $\frac{\partial^2 g_B^A(\tau_{i,B}^A, a)}{\partial a \partial \gamma_i}$ is small enough such that $\frac{\partial(\partial \tau_{i,B}^A / \partial a)}{\partial \gamma_i} < 0$, i.e. larger degree of altruism leads to larger crowding-out effect (note that the crowding-out effect $\partial \tau_{i,B}^A / \partial a < 0$). When $\gamma_{-i} > \bar{\gamma}_{-i}$, we have $\frac{\partial(\partial \tau_{i,B}^A / \partial a)}{\partial \gamma_i} > 0$, i.e. the level of the crowding out decreases with the increase in altruism.

Appendix B. Experimental Protocol

Our experiment contains two parts: the main risk sharing game and the dictator game. We provide the detail of the experimental protocol for each game and the screen subjects saw during the experiment.

1. General instructions

Our general instructions read as follows:

"Welcome to our experiment. You are now taking part in an economics experiment about choice in risk sharing arrangements. All subjects are anonymous, and your decisions will be kept private and used only for academic research. You will receive cash payments according to the result of the outcomes in the experiment session. Please read the following instructions very carefully. During the experiment, communication between participants is not allowed. Violation of this rule will lead to you being excluded from the experiment and from all payments. If you have questions, please raise your hand and we will answer your question in private.

The experiment uses points instead of Chinese Yuan (RMB) as currency. Your payoffs will therefore be calculated in points, and we will exchange your total points to RMB at the end of the experiment. The exchange rate will be 15 points to 1 RMB.

Upon completion of the experiment, you will be paid in cash in the amount equivalent to the points you have earned from participating in this experiment."

2. Risk-sharing games

The instructions subjects received read as follows:

"This experiment includes many periods, and the computer will randomly choose a period some time after period 30 to end the experiment. At the beginning of the first period, each participant will be randomly matched against another participant. This pairing will keep the same during the entire experiment.

Each period, you and your partner will receive points consisting of a fixed income and a random income. The fixed income is 125 points each, and the random income is 200 points or 0 points. If you receive 200 points the your partner will receive 0 points and vise versa. You have equal probabilities to receive 200 or 0 points each period.

If you receive the 200 points, you may choose to transfer some money to your partner, or choose not to transfer any. If you do make a transfer, this amount must be non-negative, and no more than the total income you receive that period. If you receive 0 points of random income, you will not be allowed to transfer money to your partner. After the transfer stage, you will be able to see you and your partner's final income as well as the transfer history on screen. The experiment consists of multiple periods, and we want to emphasize that only one of these periods will be chosen—randomly—for conversion to real RMB, at a rate of 15 points to 1 RMB.

Your income for each period can be calculated according to the following formula:

Your final income for one period = Fixed income + Random income – Money transferred to the other person (if random income = 200).

Your final income for one period = Fixed income + Random income + Money transferred to you (if random income = 0).

For example, assume that you receive a fixed income of 125 and your partner receive the 200 points random income (which means your random income in this period is zero). Your total income before the transfer is therefore 125, while your partner's is 325. Now, your partner decides to transfer X points to you. Your final income this period is then 125 + X, and your partner's final income this period is 125 + 200 - X."

In the following we show the computer screens that appeared during the experiment. We use Treatment 2 as an example, which can be divided into two phases: Phase 1 (periods 1 to 15) and Phase 2 (periods 16 to 30).

Step 1 of Phase 1: In each period of Phase 1, subjects who receive the 200 points of random income can see Figure (B.1) and answer the question "How much are you willing to transfer to your partner?" Subjects with 0 points of random income see Figure (B.2).

Figure (B.1)

Phase 1 screen 1（200 random income）

Period 3			
Your		Your opponent player's	
Fixed income	125	Fixed income	125
Random income	200	Random income	0
Income before transfer	**325**	**Income before transfer**	**125**
How much are you willing to transfer to your opponent?			
			OK

Figure (B.2)

Phase 1 screen 2(0 random income)

Period 3			
Your		Your opponent player's	
Fixed income	125	Fixed income	125
Random income	0	Random income	200
Income before transfer	**125**	**Income before transfer**	**325**
Your random income is relatively low this round. You do not need to make a transfer decision this round.			
			OK

Step 2 of Phase 1: After the transfer decision, the screen in Figure (B.3) is displayed to show subjects both their and their partner's current and past transfers.

Figure (B.3)

Phase 1 Screen 2

Period 3					Time remaining			10
Your					Your opponent player's			
Your fixed income is 125					Your opponent player's fixed income is 125			
Period	Random income	Income before transfer	Transfer	Income after transfer	Random income	Income before transfer	Transfer	Income after transfer
1	200	325	100	225	0	125	N/A	225
2	0	125	N/A	185	200	325	60	265
3	**200**	**325**	**100**	**225**	**0**	**125**	**N/A**	**225**
								OK

Stage 1 of Phase 2: At the beginning of the 16th period, the screen in Figure (B.4) suggests that each subject now has an option to purchase insurance. This screen introduces the details of formal insurance and asks for the subject's decision.

Figure (B.4)

Phase 2 screen 1 (Round 16)
Period 16
From this period on, you can purchase an insurance contract before the income shock. The premium of this insurance is 50 points. If you purchase insurance in a round and get 0 points of random income in that round, you will be compensated 100 points. If you purchase this insurance and receive 200 points of random income, you will not be compensated. If you want to purchase insurance, press 'Yes'. If you don't want to purchase insurance, press 'No'.
Are you willing to purchase insurance in this period? **Yes** **No**

Stage 2 of Phase 2: After the insurance purchase decision, a screen similar to the Step 1 of Phase 1 is displayed to ask for subjects' transfer decision.

Figure (B.5)

Phase 2 Screen 2 (200 random income)

Period 24			
Your		Your opponent player's	
Fixed income	125	Fixed income	125
Random income	200	Random income	0
Insurance purchase	Yes	Insurance purchase	NO
Cost of insurance purchase	50	Cost of insurance purchase	0
Compensation of insurance	0	Compensation of insurance	0
Income before transfer	**275**	**Income before transfer**	**125**
How much are you willing to transfer to your opponent?			
			OK

Figure (B.6)

Phase 2 Screen 2(0 random income)

Period 24			
Your		Your opponent player's	
Fixed income	125	Fixed income	125
Random income	0	Random income	200
Insurance purchase	NO	Insurance purchase	Yes
Cost of insurance purchase	0	Cost of insurance purchase	50
Compensation of insurance	0	Compensation of insurance	0
Income before transfer	**125**	**Income before transfer**	**275**
Your random income is relatively low this round. You do not need to make a transfer decision this round			
			OK

Step 3 of Phase 2: Figure (B.7) again shows current and historical information about the insurance purchase decision and transfers.

Figure (B.7)

Phase 2 Screen 3 Treatment 2

Period 24														Time remaining 10
Your								Your opponent player's						
Your fixed income is 125								Your opponent player's fixed income is 125						
Period	Random incom	Insurance purchase	Cost of insurance purchase	Compensation of Insurance	Income before transfer	Transfer	Income after transfer	Random income	Insurance purchase	Cost of insurance purchase	Compensation of Insurance	Income before transfer	Transfer	Income after transfer
21	Yes	No	0	N/A	325	100	225	No	No	0	N/A	125	N/A	225
22	Yes	Yes	50	0	275	50	225	No	Yes	50	100	175	N/A	225
23	No	Yes	50	100	175	N/A	235	Yes	No	0	N/A	325	60	265
24	No	No	0	N/A	125	N/A	185	Yes	Yes	50	0	275	60	215
													OK	

3. Dictator game

In this section, the same group of subjects plays the one-shot dictator game. The dictator game will be played 8 times to allow for 8 different income structures that match those appear in the risk sharing game. The instructions subjects received read as follows:

"For each period, you will be randomly matched with another subject. Different from the risk sharing game, this pair dissolves immediately at the end of each period. Each period, you and your partner will receive a certain amount of income. One will receive a higher income and the other a lower income. You can choose to transfer money to your partner if you received the higher income. You may also choose not to make a transfer. If you choose to make a transfer, the amount must be non-negative and no more than the income you receive at the beginning of that period. If you receive the lower income you will not be allowed to transfer any money to your partner. You will also not be able to observe the amount you receive from your partner.

At the end of the experiment, one period will be randomly chosen for payment, and the screen will state your earnings. You income for each period can be calculated according to the following formula:

Your final income for one period = Initial income – Money transferred to the other person (if income is higher than partner's)

Your final income for one period = Initial income + Money transferred to you (if income is lower than partner's)

For example, assume that you receive an initial income of 325 points and your partner receive 125 points. Suppose you transfer X to your partner. Your final income for this period is 325 – X, and your partner's final income is 125 + X."

Figure (B.9)

Altruism - A Dictator Game

Period 1	Time remaining 10
Your income is	Your opponent player's income is
325	125
How much are you willing to transfer to your opponent?	
	OK