

Formal Insurance and Informal Risk Sharing Dynamics

Abstract: This paper investigates whether and how the crowding-out effect of formal insurance on informal risk sharing is mitigated by social preference change. We design a lab experiment in which formal insurance is introduced and removed unexpectedly in a repeated risk-sharing game. We find evidence of social preference change by showing that informal risk sharing is significantly improved after the removal of formal insurance, and the pattern mainly occurs when one subject obtains insurance but the other does not. Findings suggest that it is the insurance purchasers who take the initiative to share more risk for their partners. However, there is no significant improvement in informal risk sharing when insurance purchasing decisions are randomly computer generated. We propose a model based on guilt aversion to explain our findings.

Key words: insurance, risk sharing, crowding-out, social preferences, guilt

JEL Classification: C91 D81 O17

1. Introduction

Life in developing countries carries numerous and varied sources of risk. The near absence of formal insurance markets means that households in many developing countries depend primarily on the strength of their informal risk sharing networks to mitigate the myriad sources of risk they face (Rosenzweig, 1988, Ligon et al., 2002). This bleak risk management landscape may be changing thanks to the recent introduction of formal insurance by researchers and development institutions. However, the availability of formal insurance raises questions about its impact on informal risk sharing. Whether formal insurance will “crowd out” informal risk sharing structures is an important issue, because formal insurance may fail to provide perfect risk coverage due to incomplete information or idiosyncratic risk. In fact, neoclassical models predict that formal insurance will over-crowd out informal risk sharing (e.g. Di Tella and MacCulloch, 2002; Thomas and Worrall, 2007; Broer, 2011; Krueger and Perri, 2011), so the introduction of formal insurance may lead to a negative welfare effect.

Economists investigate the issue of “crowding-out” using either field or laboratory experiments (e.g., Schneider, 2005; Lin et al. 2014). While theoretical models that assume informal risk sharing behavior is unambiguously self-interested predict a strong crowding-out effect, the literature has mixed empirical results. Most studies find that formal insurance indeed crowds out the informal risk sharing, but the magnitude of the crowding-out effect is not as large as the theoretical prediction indicates (e.g. Lin et al. 2014). Some paper even documents a “crowding-in” effect (Takahashi et al., 2017; Landmann et al., 2018). Social preferences are one possible explanation for these mixed results. For instance, Lin et al. (2014) find that about one third of risk transfer is motivated by altruism. In this paper, we would like to further study whether and how the crowding-out effect of formal insurance on informal risk sharing is mitigated by change in social preferences, also seeking to understand how these social preferences are impacted by the introduction of formal insurance.

This paper uses an experimental model based on Charness and Genicot’s (2009) repeated risk sharing game to interrogate this question. In the game, two subjects are

randomly paired and receive equal fixed incomes during each test period. One of the subjects randomly receives additional income (with equal probabilities), and then must choose whether to transfer money to his/her partner. Subjects are informed that the same game would be repeated for at least 45 periods and end stochastically thereafter. The payoff of a single period is randomly selected as the final payment amount so that the subject is motivated to smooth the realized income across periods.

The formal insurance is introduced exogenously and unexpectedly in a once-for-all manner. It is offered for purchase in the 16th period. The subject can decide to purchase the insurance or not. Once the decision is made, it will be carried out through between the 16th and 30th periods, and be removed at 31st period. Formal insurance is designed as a partial substitute for informal risk sharing, so that individuals engage in risk sharing while having formal insurance. We ran one control session in which *no* formal insurance is introduced, which we use to control for the natural risk transferring trends over time, and then carried out two different treatment sessions. In the 25V (voluntary) treatment, the insurance contract offered for voluntary purchase is actuarially fair and covers 25% of total risk. We also run a 25F (forced) treatment, which is identical to the 25V treatment except that the insurance purchase decisions are randomly computer generated, a feature that of which subjects are fully aware.

Our design has two advantages. First, removing formal insurance allows us to clearly identify changes in social preferences towards cooperation. Without changes to social preferences, the level of informal risk sharing should be the same before the introduction and after the removal of the formal insurance, assuming the experiment controls for natural tendencies.¹ Second, inclusion of both voluntary and forced insurance purchase allows us to understand if the decision to voluntarily purchase insurance is crucial for the transfer decisions. Voluntary purchase decision often

¹ Previous models of repeated risk sharing games often employ a constrained optimal agreement (Coate and Ravallion, 1993; Kocherlakota, 1996; Charness and Genicot, 2009) hence it predicts no difference in the choices in Phase 1 and Phase 3 if the introduction and removal of the insurance is unexpected. Of course, due to equilibrium multiplicity in a repeated game, relationship improvement can be also caused by the switch of equilibrium. However, it is hard to reconcile why equilibrium switches occur in the manner we observed in the data.

reveals pairing trust levels or subject intentions, which also often leads to change in social preferences. Therefore, the two treatments should not have different results without a change in preferences.

We find, quite surprisingly, that the introduction of formal insurance positively affects people's inclination towards cooperation. In particular, after creating baseline controls for natural tendencies, informal risk sharing is not crowded out dramatically by the implementation of formal insurance, and informal risk sharing levels are even improved significantly after the insurance is removed. These patterns occur primarily when the paired subjects make voluntary and asymmetric formal insurance adoption decisions.

More specifically, the estimated treatment effects are obtained through a diff-in-diff method in which we controlled for natural end-of-game effects. We refer to periods 1-15 as Phase 1, periods 16-30 as Phase 2, and periods 31-45 as Phase 3. In the 25V treatment, after controlling for the baseline declining trends, the level of private transfers improves significantly in Phase 3 compared to Phase 1. Interestingly, the results depend on whether insurance purchasing decisions are symmetric or asymmetric. When both subjects purchase insurance (symmetric case), Phase 2 transfers decline significantly and there is no significant change in Phase 3 transfers compared to Phase 1.² When only one subject purchases insurance (asymmetric case), Phase 2 transfers do *not* decline significantly, i.e., there is no crowding-out effect, and Phase 3 transfers increase substantially for both subjects. These results suggest that private relationships are affected intrinsically and differentially by the introduction of formal insurance.

We further investigated the mechanism driving our findings. First, we find that intentions towards the partner revealed by the insurance purchasing decisions are crucial for transfers. In the 25F treatment when insurance assignments are randomly generated (and subjects are thus unable to express preferences), there is no significant difference in transfers between Phase 1 and 3 after controlling for natural trends. Second, we find an interesting informal risk sharing dynamic. Using 25F as the

² The data is insufficient for us to analyze cases when none of the subjects purchase insurance.

baseline for 25V to control for the crowding-out effect of formal insurance on private transfers when there is no preference change, we show that it is the insurance purchasers in the asymmetric case who initiate the increase in transfers to their partners, and their partners reciprocate subsequently by increasing their transfers near the end of Phase 2. These results suggest that the opportunities and actions of the insurance purchasers are critical to restoring and enhancing relationships. This result is consistent with Cecchi et al.'s (2016) finding that crowding out is *not* due to insurance adopters reducing the frequency of transfers.

The above results suggest that subjects' the introduction of formal insurance has a direct impact on cooperation preferences. To understand this dynamic, we discuss several aspects in which preferences can be changed. The first is guilt aversion. The intention-based guilt aversion literature (see, e.g., Charness and Dufwenberg, 2006) assumes that one feels guilty when one's actions fall short of others' expectations. For example when one individual purchases formal insurance and the other does not, the purchaser may feel guilty. This guilt is created because the non-purchaser's choice may signal his expectation of the purchaser's behavior, hence purchasing insurance is viewed as a violation of relational expectations. However, if the two players make the same insurance purchase decisions, there is no sense of guilt involved and their outcomes do not change in Phase 3. Although we do not explicitly elicit first-order and second-order expectations, two findings provide support for this dynamic: First, the comparison between 25V and 25F treatments suggests that the intentions behind insurance purchase are critical. Second, in the asymmetric case, insurance purchasers are the party who take initiative to restore the relationship. We considered the possibility that preferences shift due to changes in the relative value placed on partner utility, but this theory holds less predictive value and does not explain the unique role of insurance purchasers.

We carefully excluded other alternatives, including the effects of learning, restarting, and selection. The learning effect suggests that individuals learn the benefits of risk reduction through their experience with formal insurance, and hence, are more likely to cooperate in Phase 3. However, the same learning effect should be

present in the 25F treatment, yet we do not observe improved informal risk sharing after increased experience with formal insurance. The restarting effect refers to the possibility that the introduction and removal of insurance in Phases 2 and 3 induce a reset, which may help restore cooperation. In the control treatment such a restart does not exist, so the declining trend in transfers in control treatment is not representative of the trends in other treatments. However, in comparison to the 25F treatment (which included the restart effect) we find that subjects in the asymmetric case in the 25V treatment still display significantly better risk sharing behavior in Phase 3. The selection effect may exist because the insurance purchase decisions are endogenous such that pairs with different insurance purchase decisions have different natural tendencies. We try to account for this issue in part by predicting insurance purchasing decisions for the control group using the 25V treatment relationship structure. We obtained empirically similar results when comparing treatment and control sessions with the same predicted insurance purchasing behavior.

In general, we contribute to existing literature by demonstrating that the crowding-out effect of formal insurance on informal risk sharing can be affected by the change of social preferences.³ As a result, the crowding-out effect can depend on whether governments universally institutes formal insurance or if purchase is voluntary, as well as whether the purchase decisions are relationally symmetric. Our findings can therefore help understand the current mixed empirical findings, providing an improved framework for further research.

This paper is relevant to several areas of research. The first focuses on how the introduction of formal insurance affects existing informal risk sharing.⁴ Many empirical studies find that random and unexpected introduction of formal arrangements, such as formal insurance or government-sponsored welfare programs, tend to crowd out existing informal transfers (e.g. Attanasio et al., 2000; Di Tella and

³ Previous models of repeated risk sharing matrixes often assume maximization of one's own material payoffs (Coate and Ravallion, 1993; Kocherlakota, 1996; Charness and Genicot, 2009).

⁴ The literature on informal risk sharing also focuses on investigating, both theoretically and empirically, whether perfect risk sharing can be achieved via the informal risk sharing mechanism (e.g., Rosenzweig, 1988; Rosenzweig and Stark, 1989; Ligon et al., 2002). In terms of the interaction between formal and informal risk sharing schemes, some studies focus on understanding how existing informal risk sharing affects the decision to purchase formal insurance (Dercon et al., 2014; Vasilaky et al., 2014).

MacCulloch, 2002; Albarran and Attanasio, 2003; Dercon and Krishnan, 2003; Thomas and Worrall, 2007; Broer, 2011; Krueger and Perri, 2011; Klohn and Strupat, 2013). Interestingly, some recent studies such as Takahashi et al. (2017) show that index insurance uptake positively affects informal risk sharing provision, i.e. the crowding-in effect may exist under some conditions.⁵ Charness and Genicot (2009) were the first to examine informal risk sharing in a laboratory experiment. They find imperfect, yet significant, risk sharing via private transfers and prove the existence of rational risk sharing behavior. Lin et al. (2014) finds that the crowding-out effect based on standard preferences alone is not as large as the theoretical predicted, proving that altruism can play an important role. Landmann et al. (2018) finds that whether to keep anonymity and allow communication can even affect whether we observe crowd-in or crowd-out effect. However, due to data limitations, most other studies do not explore the underlying dynamics and motivations of the behavioral change.

Our study is also related to the much broader literature on the role of social preferences in repeated interactions (see Sobel, 2005 for an excellent literature review). To understand individuals' observed reciprocal behavior, Sobel (2005) offers two kinds of explanations: the *intrinsic reciprocity* model, which relates behavioral choices to social preferences, and the *instrumental reciprocity* model, which views behavior as the result of optimization from future-oriented, self-interested agents. Both models have some experimental support.⁶ Our explanation points to the potential role of guilt in motivating individuals' pro-social behavior. Charness and Dufwenberg (2006) models guilt as disutility caused when individuals do not fulfill others' expectations. In a repeated game setting, Ketelaar and Au (2003) discover that individuals who experience feelings of guilt cooperate more in repeated social

⁵ According to Boucher and Delpierre (2014), index insurance differs from conventional indemnity-based insurance because the latter is based on an external index and hence is immune to both the moral hazard and adverse selection problems. This property is consistent with features of the formal insurance designed in our experiment.

⁶ Experiments that investigate repeated interactions and support an instrumental reciprocity model include Reuben and Suetens (2012), Cabral et al. (2014), and Dreber et al. (2014); experimental evidence from Falk et al. (1999) and Fischbacher and Gächter (2010) supports the intrinsic reciprocity model.

bargaining games (prisoner’s dilemma and ultimatum game). Reuben and Winden (2010) consider a two-period power-to-take game, finding that players who feel guilt reduce their claims. We provide new evidence that social preferences can have a pronounced impact on the interaction between formal and informal risk sharing.

The remainder of this paper is organized as follows. Section 2 provides a detailed description of experimental design. Section 3 analyzes experimental results. Section 4 proposes theoretical explanations. Section 5 offers conclusions.

2. Experimental Design

In our experiment, two subjects are matched randomly to play a repeated risk sharing game (Charness and Genicot, 2009; Lin et al., 2014); the pairing remains the same throughout the experiment. There are three treatments in this design: Baseline, 25V, and 25F. Table 1 describes the basic design of the three treatments.

Table 1. Experimental Design

| | Baseline | 25V | 25F |
|---------------|--------------|---|--|
| Periods 1-15 | No insurance | No insurance | No insurance |
| Periods 16-30 | No insurance | Introduce insurance with 25% coverage. Decision is made in period 16 and remains stable to period 30. Decision is made by the subjects. | Introduce insurance with 25% coverage. Decision is made in period 16 and remains stable to period 30. Decision is randomly made by the computer. |
| Periods 31-45 | No insurance | No insurance | No insurance |

In each period in the Baseline, both subjects have fixed incomes of 125 units of experimental currency. Subjects were told that 10 units of experimental currency could be exchanged for 1 RMB, approximately 0.15 USD. In addition to this fixed income, one is equally likely to receive a random income of 0 or 200 units; if one receives 200 units, the partner receives 0. After disclosing the randomly assigned income, subjects who receive 200 units can choose to transfer a given amount to their partners, but for simplicity, subjects who receive 0 units are not allowed to make any transfers. Before making the transfer decision in each period, subjects are provided

with a history of the pair's random income allocations and transfers, but such information is not revealed to other pairs. Before the experiment, subjects are informed that the same game will be repeated for at least 45 periods and end stochastically after 45 periods. However, they are not told the exact probability that the game will end after 45 periods.⁷ The Baseline provides information about the natural trend of transfers, providing control information for our primary analysis.

The 25V treatment is identical to the Baseline, except for the following differences. First, beginning in the 16th period, subjects are provided with an external option, i.e., they have access to formal insurance. At a cost of 25 units of experimental currency, the insurance pays out 50 units if the insured receives 0 random income and nothing otherwise. This insurance contract is actuarially fair and covers 25% of total risk, leaving some room for informal risk sharing activities. Subjects are not informed about this opportunity until the 16th period, and their insurance purchasing decisions at that time are applied automatically to the subsequent periods. After the random income is allocated, and insurance payments are made. If insurance is purchased, subjects who receive 200 units in random income again choose how much to transfer to their partner. Both partners are aware of all decisions. Second, in the 31st period, subjects are told that formal insurance is no longer available, making the external option short term. Beginning in the 31st period, the game design is the same as in periods 1-15. We call this treatment 25V, because subjects purchase insurance voluntarily.

To test further whether the decision to purchase insurance affect long-term outcomes primarily by revealing individuals' motivations, we run an additional session referred to as the 25F treatment. The 25F treatment is identical to 25V, except that subjects' insurance purchasing decisions are randomly computer generated and remain the same from the 16th to the 30th period. Subjects are fully aware of this random assignment procedure.⁸

⁷ More specifically, the probability of ending the game in period 1-47 periods was 0, but it became 78% in periods 48 and 49. The game was forced to finish in period 50 if it had not yet ended. We did not include in our analysis observations after 45 periods.

⁸ Because there are almost no cases in which neither subject purchases the insurance in the voluntary treatments, the computer also does not assign this scenario in the 25F Treatment.

We employ two settings to ensure that subjects are motivated to cooperate by sharing risk even if they are risk averse. First, the same two subjects interact repeatedly for at least 45 periods, and this repeated play makes risk sharing possible, even if the subjects are motivated solely by self-interest. Second, a single period is selected randomly to calculate subjects' final payments so that they are motivated to moderate their consumption over time. This payment method also guarantees that the subjects' choices are unaffected by differing income levels due to variance in income allocation.

We recruited a total of 108 undergraduate students online through the Bulletin Board System at Peking University. The sessions were run on computers on November 11 (Baseline, two sessions, 9:00-10:00am; 10:30-11:30 am), November 24 (25V, two sessions, the same time as above), and December 29, 2012 (25F, two sessions, the same time as above). Before the experiment, subjects were required to read the instructions, after which they were asked to solve several test questions to check their understanding of the game. After all the subjects answered the test questions correctly, the formal experiment began and subjects received further instructions onscreen during play (see Appendix C for experimental instructions).⁹ Their earnings consisted of a 10 RMB participation fee and an average of 30 RMB per game, approximately 6.5 USD in total for approximately an hour. This payment is higher than the average market wage for a student, which during the experiment period was about 30 RMB per hour.

3. Experimental Results

3.1. Data summary

Figure 1 summarizes transfer findings, providing raw data for the average transfer by treatment and period. Table 2 further provides statistics on the average private transfer in periods 1–15, 16–30, and 31–45 separately for the three treatments. In the Baseline, in which no insurance is introduced, the average transfers fall from 64

⁹ The experiment was implemented using “z-Tree” developed by Fischbacher (2007).

in period 1-15 to 59 in period 16-30, to 52 in period 31-45, indicating some natural tendency towards transfer decline, possibly attributable to an end-of-the-game effect. Therefore, it is important to use the Baseline to control for this time trend when analyzing the data in other treatments. In the remaining treatments, we observe a large crowding-out effect of private transfers when insurance is available and their partial recovery when insurance is removed. For example, in the 25V treatment, the average transfers fall from 65 in period 1-15 to 48 in period 16-30, to 59 in period 31-45. The drop-in transfer in the 25F treatment seems to be more moderate than in treatments with voluntary insurance purchase decisions.

Figure 1. The Average Transfer by Treatment and Period

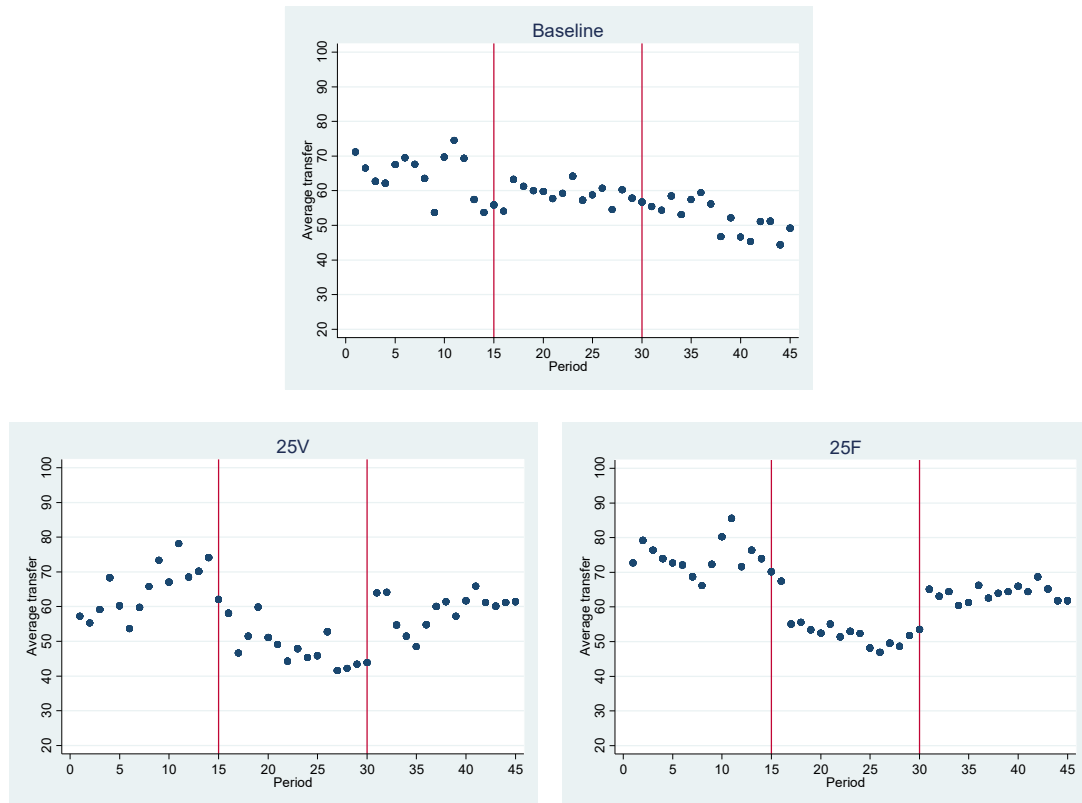


Table 2. Data Summary: Transfers

| Treatment | Insurance purchase rate | Number of subjects | Average Transfers (SD) | | |
|-----------|-------------------------|--------------------|------------------------|-------------|-------------|
| | | | Round 1-15 | Round 16-30 | Round 31-45 |
| Baseline | N/A | 32 | 64 (35) | 59 (41) | 52 (42) |
| 25V | 72% | 36 | 65 (44) | 48 (39) | 59 (43) |
| 25F | 70% | 40 | 78 (34) | 60 (36) | 72 (39) |

2.2. 25V treatment

- **Transfer Changes**

We begin by analyzing the 25V treatment transfer changes. Table 3 reports the formal estimates of changes in private transfers attributable to the introduction and removal of formal insurance. The regressions adopt a difference-in-difference approach, controlled for the underlying time trend using Baseline results, to identify the change in private transfers in other treatments. Observations from both the Baseline and 25V treatment are included in these regressions. For each subject, we also include only those observations in which the subject receives 200 units in random income and thus has to make a transfer decision. The dependent variable is private transfers. The key independent variables include the following: “Treat” is a dummy variable indicating that the observations are from treatments in the middle 15 periods during which insurance was available. “Phase 2” and “Phase 3” are also dummy variables indicating observations during periods 16-30 and 31-45, respectively. Therefore, the interaction terms, “Phase 2*treat” and “Phase 3*treat,” estimate the change in private transfers relative to the first 15 periods, controlling for the natural time trend during Baseline tests. We use subject fixed effect to account for the unobservable subject heterogeneity. Given this approach, the dummy variable “treat” is not identifiable because different treatments consist of different subjects. Standard errors are clustered at session level.¹⁰

Column 1 shows the overall sample regression. The estimates of “Phase 2” and “Phase 2*treat” suggest that the average transfer in Phase 2 declines about 5.46 units relative to Phase 1 in the Baseline. However, the corresponding decline is 11.06 units more in 25V treatment than in Baseline, about two times of the effect in Baseline. This is not surprising given the strong substitution of the informal risk sharing

¹⁰ We face a potential technical problem of small number of clusters when we cluster errors on sessions (or pairs). To check the robustness of the results, we also use wild cluster bootstrap (Cameron, et al. 2008). The overall results remain, i.e. there is significant improvement in risk sharing after insurance is removed, regardless of whether Baseline or 25F are as trend controls, and both the economic magnitude and the significance levels of the estimates are still sharply different across symmetric and asymmetric cases. But some of the estimates in the asymmetric case indeed become marginally significant (with p-values ranging from 0.10 to 0.13).

mechanism and formal insurance in terms of risk coverage. It is more interesting to see the effect after formal insurance is removed. The estimates of “Phase 3” and “Phase 3*treat” suggest that the average transfer in Phase 3 also declines by 11.92 relative to Phase 1 in the Baseline, but the decline is 6.3 units (about 50%) less in 25V treatment and this difference is significant at 5% level. This finding indicates, quite surprisingly, that the availability of formal insurance significantly improves private risk sharing practices after its removal, despite the general trend of declining private transfers.

Table 3. Analysis of the Change in Private Transfers in the 25V Treatment using Baseline Results as a Comparison Group

| | 1 Overall Sample | 2 Both purchase insurance | 3 Subjects who purchase insurance, but whose partners do not | 4 Subjects who do not purchase insurance, but whose partners do |
|----------------------|---------------------|---------------------------------|--|---|
| Percentage | | 50% | 22% | 22% |
| Phase 2 | -5.46*** (0.09) | -5.46*** (0.09) | -5.46*** (0.09) | -5.46*** (0.09) |
| Phase 3 | -11.92*** (1.94) | -11.92*** (1.94) | -11.92*** (1.94) | -11.92*** (1.94) |
| Phase 2*25V | -11.06*** (1.55) | -20.01*** (2.62) | -4.70 (2.68) | -2.05 (1.96) |
| Phase 3*25V | 6.30** (1.96) | 0.85 (2.92) | 11.31** (2.95) | 11.53** (2.29) |
| Constant | 64.55*** (0.44) | 61.05*** (0.40) | 66.13*** (0.49) | 65.25*** (0.50) |
| Subject fixed effect | Yes | Yes | Yes | Yes |
| R-squared | 0.06 | 0.08 | 0.04 | 0.04 |
| Observations | 1,530 | 1,125 | 903 | 897 |

Note: The regressions in this table include only observations of those who receive 200 units random income (and thus have to make the transfer decision). These regressions include observations from Baseline and the 25V treatment. We use subject fixed effect to account for unobserved individual heterogeneity. Therefore, the dummy variable “25V” is not identifiable. Standard errors clustered at session levels are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Columns 2-4 show the impact of insurance purchasing decisions on the treatment effect. The estimated pattern in each case is distinct from the overall sample

estimation, suggesting the existence of significant heterogeneity. In these regressions, we always include all observations from the Baseline to control for declining trends in transfers over time. It is possible that subjects with different insurance purchasing decisions have different natural tendencies over time, an issue we address later in the robustness check.

In fewer than 6% of the pairs, both subjects choose not to purchase insurance.¹¹ Therefore, we lack sufficient data to example this scenario. Column 2 begins with the pairs in which both subjects choose to purchase insurance in the 16th period (all periods in 16-30). Approximately 50% of the pairs are in this category. In this case, private transfers decline significantly by 20.01 units more than the Baseline with the introduction of formal insurance in Phase 2. However, transfer decline in Phase 3 after the formal insurance is removed is not significantly different from that in the Baseline.

Approximately 44% of pairs make asymmetric insurance purchasing decisions. Column 3 shows subjects who purchase insurance but whose partners do not, while Column 4 shows subjects who do not purchase insurance but whose partners do. Interestingly, compared to Baseline results, in both cases there is no significant reduction in private transfers when formal insurance is available in Phase 2. However, the significant estimates of “phase 3*25V” in both columns (3) and (4) suggest that the transfers increase by more than 11 units after formal insurance is removed in Phase 3, nearly offsetting the corresponding declining trend in Baseline findings. There are sharp differences in terms of the magnitude and significance levels of the estimates “phase 3*25V” across the symmetric and asymmetric cases, suggesting that the overall improvement in risk sharing in Phase 3 mainly comes from the asymmetric case.

Finding 1 (transfers in 25V): Comparing Phase 3 to Phase 1 and after controlling for the natural declining trend measured in Baseline tests, we can see that: (1) (symmetric case) when both subjects purchase insurance, private transfers do not

¹¹ Subjects in this case often cooperate pretty well early on to achieve perfect insurance. As a result, there is no demand for formal insurance, and the perfect risk sharing persists to the end of the experiment.

change significantly when insurance is removed, while in (2) (asymmetric case) when only one subject purchases insurance, the private transfers of both subjects increase significantly when insurance is discontinued.

- **Welfare analysis: Estimating the change in risk coverage**

In addition to the change in private transfers, it is important to understand the change in the total degree of risk coverage in each phase to have a clear understanding of its implications on welfare. In the following analysis, the degree of risk coverage is measured by the changes in final income in response to receiving randomly assigned income.

Table 4. Analysis of Risk Reduction in 25V Treatment using Baseline as Comparison Group

| | 1 | 2 | 3 | 4 |
|------------------|---------------------|-------------------------|--|---|
| | Overall Sample | Both purchase insurance | Subjects who purchase insurance, but whose partners do not | Subjects who do not purchase insurance, but whose partners do |
| R-income | 0.36*** (0.05) | 0.36*** (0.05) | 0.36*** (0.05) | 0.36*** (0.05) |
| Phase2 | -5.10*** (0.06) | -5.10*** (0.06) | -5.10*** (0.06) | -5.10*** (0.06) |
| Phase3 | -12.05*** (1.94) | -12.05*** (1.94) | -12.05*** (1.94) | -12.05*** (1.94) |
| Phase2*25V | 6.44*** (0.23) | 4.25 (2.55) | 20.28*** (2.66) | -3.30 (2.17) |
| Phase3*25V | 6.11* (1.95) | 0.35 (3.00) | 8.71** (2.23) | 13.64** (2.82) |
| R-income*25V | -0.01 (0.12) | 0.09 (0.16) | -0.07 (0.12) | -0.07 (0.12) |
| Phase 2*R-income | 0.05*** (0.00) | 0.05*** (0.00) | 0.05*** (0.00) | 0.05*** (0.00) |
| Phase 3*R-income | 0.12*** (0.02) | 0.12*** (0.02) | 0.12*** (0.02) | 0.12*** (0.02) |
| Phase | -0.06*** (0.00) | -0.04 (0.03) | -0.21*** (0.02) | 0.04 (0.02) |
| 2*R-income*25V | -0.06* (0.02) | -0.00 (0.03) | -0.11** (0.02) | -0.11** (0.02) |
| 3*R-income*25V | | | | |
| Constant | 189.45*** (6.03) | 185.93*** (6.34) | 190.51*** (4.56) | 190.75*** (4.57) |

| | | | | |
|---------------|-------|-------|-------|-------|
| Subject fixed | | | | |
| effect | Yes | Yes | Yes | Yes |
| R-squared | 0.49 | 0.57 | 0.49 | 0.50 |
| Observations | 3,060 | 2,250 | 1,800 | 1,800 |

Note: The regressions in this table include all observations for a given subject regardless of whether or not they receive 200 units of random income. We use subject fixed effect to account for unobserved individual heterogeneity. Standard errors clustered at session levels are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4 reports the estimated change in the degree of risk coverage by treatment and the resulting insurance purchasing decision. In the following analysis, we include all observations for a given subject, regardless of whether or not she receives random income. We refer to final income as the income subjects receive at the end of each period, i.e., after obtaining random income, purchasing insurance (if any), receiving insurance repayments (if any), and receiving the private transfers. We regress subjects' final income based on the amount of random income received (represented by the variable "R-income" in Table 4) and other variables. If random income has no effect on final income, then the subjects face no risk. If random income has a significant effect on final income, that demonstrates that risk coverage is imperfect. The change in the effect of random income is captured by the coefficient of the triple interaction terms, "Phase2*R-income*25V" and "Phase3*R-income*25V." Negative signs for these coefficients represent an increase in the degree of risk coverage after controlling for baseline trends.

Focusing on overall sample estimates (Column 1), we can see that the effect of random income on final income increases significantly by 0.05 from Phase 1 to Phase 2 in Baseline results (estimate of "Phase 2*R-income"). However, in the 25V treatment the corresponding change is 0.06 less than the Baseline trend suggested by the estimate of "Phase 2*R-income*25V". These results imply that the total degree of risk coverage is significantly increased in the 25V treatment relative to the Baseline. The estimate of "Phase 3*R-income*25V" is -0.06 and also significant at 10% level, suggesting that following the removal of formal insurance the degree of risk coverage also improved significantly relative to Phase 1.

With respect to insurance purchasing decisions, the results show that when both subjects purchase insurance (Column 2) in both Phases 2 and 3, there is no significant change in the total risk coverage after controlling for the Baseline trend. Therefore, improvements in informal risk sharing in the sample overall are driven primarily by the asymmetric case (in which only one subject purchases insurance). In the asymmetric case, the effect of random income on final income increases by 0.12 from Phase 1 to Phase 3 in the Baseline test, but the corresponding change is 0.11 less than the Baseline, almost offsetting natural trends. These estimates suggest that relative to Baseline trends, both subjects experience significant improvement in the degree of risk coverage in Phase 3, regardless of their insurance purchasing decisions in Phase 2.

Finding 2 (risk coverage in 25V treatment): Comparing Phase 3 to Phase 1 and after controlling for the natural declining trend measured in Baseline test, we can see that: (1) (Symmetric case) the degree of risk coverage does not change significantly in both Phases 2 and 3, and (2) (Asymmetric case) the risk coverage increases significantly in Phase 3 for both individuals.

3.3. Mechanism

What is the mechanism through which informal risk sharing improves after the removal of formal insurance? This section investigates the mechanism and dynamics of such improvement.

- **The Role of Revealing Intentions**

In the 25F treatment, insurance purchasing decisions are randomly computer generated, meaning that subjects have no opportunity to express personal preferences or motivations. The subjects are fully aware of this random structure. This treatment is crucial in understanding the relationship between private risk sharing and the motivations underlying insurance purchase.

Following the same empirical strategy as in Table 3, and using Baseline observations to control for the natural declining trend in transfer, Table 5 reports estimated change in private transfers during the 25F treatment. Relative to Phase 1

and after controlling for the natural trend in Baseline tests, the transfers decline significantly in Phase 2 by 15.58 in the overall sample. Significant declines of approximately the same magnitude occur in both the symmetric and asymmetric cases. This finding is attributable to the crowding-out effect of the formal insurance. However, the transfer rate in Phase 3 remains essentially the same as in the first 15 periods in both the overall sample regression and the subsample regressions for different insurance purchase combinations. These patterns indicate clearly that the unexpected results in 25V are driven primarily by the ability of insurance purchasing decisions to reveal information that affects interpersonal relationships.

Table 5. Analysis of the Change in Private Transfers in the 25F Treatment using Baseline as the Comparison Group

| | 1 Overall Sample | 2 Both purchase insurance | 3 Subjects who purchase insurance, but whose partners do not | 4 Subjects who do not purchase insurance, but whose partners do |
|----------------------|---------------------|---------------------------------|--|---|
| Percentage | | 70% | 15% | 15% |
| Phase 2 | -5.46*** (0.09) | -5.46*** (0.09) | -5.46*** (0.09) | -5.46*** (0.09) |
| Phase 3 | -11.92*** (1.94) | -11.92*** (1.94) | -11.92** (2.06) | -11.92** (2.06) |
| Phase 2*25F | -15.58*** (1.45) | -19.78*** (0.10) | -19.02*** (0.09) | -18.75*** (0.09) |
| Phase 3*25F | 1.89 (2.09) | -0.05 (2.10) | -1.40 (2.06) | 0.82 (2.06) |
| Constant | 70.63*** (0.53) | 69.34*** (0.33) | 64.63*** (0.52) | 64.81*** (0.52) |
| Subject fixed effect | Yes | Yes | Yes | Yes |
| R-squared | 0.10 | 0.10 | 0.06 | 0.06 |
| Observations | 2,070 | 1,530 | 900 | 900 |

Note: The regressions in this table include only observations of those who receive 200 units random income (and thus have to make a transfer decision). The regressions include observations from Baseline tests and the 25F treatment. We use subject fixed effect to account for unobserved individual heterogeneity. Therefore, the dummy variable “25F” is not identifiable. Standard errors clustered at session levels are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Finding 3 (25F treatment): When the computer determines insurance purchasing decisions randomly, compared to Phase 1 and after controlling for natural Baseline trends, private transfers decline significantly when insurance is available in Phase 3, but do not change when insurance is removed.

- **Dynamics of Informal Risk Sharing**

This section explains the dynamics of improvements to informal risk sharing displayed in Part 2. In our conceptual framework, the effect of formal insurance on transfer rates can come from three sources: (1) natural declining trends; (2) natural substitution between formal insurance and private transfer, keeping preferences the same; (3) the effects of preference change. The Baseline contains part (1); 25F treatment contains parts (1) and (2) (when insurance choice is randomly computer assigned, removing the possibility of preference change, i.e. the partner's intentions are unknown); 25V treatment contains parts (1), (2) and (3) (when insurance purchasing decisions can reveal a partner's motivations or trust level). We seek to separate part (3) from parts (1) and (2), so we used 25F treatment as the control group to perform regression analysis similar to the structure reported in Table 3. We reported our results in Table 6.

Table 6. Analysis of 25V Treatment using 25F Treatment as the Comparison Group

| | 1 | 2 | 3 | 4 |
|-------------|---------------------|-------------------------|--|---|
| | Overall Sample | Both purchase insurance | Subjects who purchase insurance, but whose partners do not | Subjects who do not purchase insurance, but whose partners do |
| Percentage | | 50% | 22% | 22% |
| Phase 2 | -21.04*** (1.45) | -25.24*** (0.05) | -24.48*** (0.00) | -24.22*** (0.00) |
| Phase 3 | -10.03*** (0.77) | -11.96*** (0.80) | -13.32*** (0.00) | -11.10*** (0.00) |
| Phase 2*25V | 4.52 (2.12) | -0.23 (2.62) | 14.32** (2.85) | 16.70** (2.09) |
| Phase 3*25V | 4.41** (0.82) | 0.90 (2.32) | 12.71** (2.36) | 10.71** (1.30) |
| Constant | 70.56*** (0.52) | 67.68*** (0.19) | 69.79*** (0.09) | 68.08*** (0.13) |

| | | | | |
|----------------------|-------|-------|------|------|
| Subject fixed effect | Yes | Yes | Yes | Yes |
| R-squared | 0.11 | 0.14 | 0.11 | 0.14 |
| Observations | 2,160 | 1,215 | 363 | 357 |

Note: The regressions in this table include only observations of those who receive the 200 unit random income (and thus have to make transfer decisions). The regressions include observations from the 25V and 25F treatments, with 25F serving as the control group. We use subject fixed effect to account for unobserved individual heterogeneity. Therefore, the dummy variable “25V” is not identifiable. Standard errors clustered at session levels are reported in parentheses.

From the estimates of the interaction term “Phase2*25V” in column 1, we can see that in the overall sample, the decrease of transfer in Phase 2 of 25V treatment is about 21% less than the corresponding decline in the 25F treatment. Columns 2-4 indicate that the asymmetric dynamic drives this overall effect entirely. While the transfer decline in Phase 2 does not differ significantly between the 25F and 25V treatments when both subjects purchase the insurance, in the asymmetric case, both the insurance purchasers and non-purchasers in the 25V treatment become more generous in Phase 2 compared to their counterparts in 25F. The estimates of “Phase 2*25V” are 14.32 and 16.70, suggesting that in Phase 2 of the asymmetric case, transfers decline 58% (14.32/24.48) and 69% (16.7/24.22) less than the corresponding decline in the 25F treatment, respectively.

This positive relationship continues in Phase 3. The estimates of “Phase 3*25V” are 12.71 and 10.71, suggesting that the transfers of both the insurance purchasers and non-purchasers decline 95% (12.71/13.32) and 96% (10.71/11.10) less than the corresponding decline in the 25F treatment, respectively. These changes are also significant and nearly offset the declining trend in 25F treatment.

We further explore Phase 2 dynamics by examining the change in periods 16-22 and 23-30 relative to Phase 1 separately, using the 25F treatment as the control group. Because the asymmetric case is our focus, and the symmetric case does not differ significantly across periods, Table 7 reports only the results of the asymmetric case.

Table 7 provides some suggestive evidence that it is the insurance purchasers who initially make very generous transfers in 25V: in periods 16-22, compared to Phase 1, the insurance purchasers’ transfers decline 17.81 units significantly less than

those of their counterparts in the 25F treatment, and the same estimate is 14.20 for the non-purchasers and not significantly different from zero. However, in periods 23-30, the non-purchasers significantly increase their transfers in response to the purchasers' relative increase in transfers. These results provide some suggestive evidence that a better long-term relationship is established because the insurance purchasers display initial kindness and non-purchasers reciprocate.

Finding 4: Subjects have a significantly lower transfer decline in Phase 2 of the 25V treatment compared to their counterparts in the 25F treatment. The pattern is mainly driven by the asymmetric case. In Phase 2, the insurance purchasers seem to make initial generous transfers, after which their partners reciprocate by increasing transfers near the end of the phase.

Table 7. Analysis of 25V Treatment using 25F as the Comparison Group (Dynamics in Phase 2)

| | Those who purchase the insurance | | Those who do <i>not</i> purchase the insurance | |
|----------------------|----------------------------------|---------------------|--|---------------------|
| | 1 16-22 | 2 23-30 | 4 16-22 | 5 23-30 |
| Phase 2 | -24.31*** (0.00) | -27.13*** (0.00) | -18.01*** (0.00) | -28.35*** (0.00) |
| Phase 2*25V | 17.81* (5.72) | 12.46*** (0.36) | 14.20 (5.94) | 18.45*** (0.60) |
| Constant | 69.11*** (1.00) | 71.29*** (0.06) | 69.10*** (0.85) | 66.36*** (0.12) |
| Subject fixed effect | Yes | Yes | Yes | Yes |
| R-squared | 0.10 | 0.15 | 0.08 | 0.19 |
| Observations | 184 | 184 | 168 | 184 |

Note: The regressions in this table include only observations of those who receive 200 units of random income (and thus have to make transfer decisions). These regressions include observations from the 25V and 25F treatments, with 25F serving as the control group. In all columns, observations from Phase 1 are included. We use subject fixed effect to account for unobserved individual heterogeneity. Therefore, the dummy variable "25V" is not identifiable. Standard errors clustered at session levels are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In Appendix Table B1 we also provide some suggestive evidence that subjects in the asymmetric case perform significantly better in Phase 3 if the insurance purchasers have more periods receiving 200 random income in Phase 2 hence more

exogenous opportunities to improve partner relations. These evidence al suggest that the intentions revealed by the insurance purchasing decisions matter, and the insurance purchasers play a crucial role in enhancing their long-term relationships.

3.4. Alternative Explanations

- **Learning effect**

Before resorting to a preference-based explanation of our results, we discuss and exclude several alternative explanations. The first is the learning effect, which refers to the possibility that the experience with formal insurance reinforces subjects' tendency to engage in informal risk sharing even after the insurance is removed, as subjects now understand the benefits of risk sharing. The learning effect can accurately explain asymmetric purchasing decisions, but it cannot account for the fact that risk-sharing behavior does not increase significantly when both subjects purchase insurance. The results based on the 25F treatment also help exclude the learning effect. This effect still exists when the computer makes insurance purchasing decisions, but the 25F treatment does not generate the same pattern as the 25V treatment.

- **Restart effect**

The second alternative is the restart effect proposed by Andreoni (1988). One may argue that introducing and removing formal insurance in Phases 2 and 3 introduces a restart, which may help restore cooperation. In this logic, in Baseline tests no such restart occurs, so Baseline trends are not universally representative. The 25F treatment should mimic the same trend, as it also includes the restart effect. However, Table 6 suggests that subjects in the 25V treatment's asymmetric case still play significantly better in terms of risk sharing in Phase 3. Thus, the restart effect cannot explain our primary findings.

- **Selection effect**

The third alternative explanation is the selection issue. Because insurance purchase decisions are endogenous, the regressions using 25V treatment insurance purchasing decisions in may suffer from selection bias. For example, it is likely that those who purchase insurance tend to make fewer transfers in all periods. However,

we controlled for selection bias through the use of the individual dummy variables and based on the understanding that estimated changes in private transfers are relative to one's own transfer history over the first 15 periods. The only issue remaining is the question of how to adjust natural Baseline time tendencies. In the previous regressions, we use the entire Baseline sample to control for time trends, recognizing that previous estimates could be biased those who make different insurance purchasing decisions have different natural time tendencies due of the selection effect.

**Table 8. Analysis of the Treatment Effect in the 25V Treatment
using Predicted Insurance Purchase Decisions as the Comparison Group**

| Sample | 1 Both purchase insurance | 2 Subjects who purchase insurance, but whose partners do not | 3 Subjects who do not purchase insurance, but whose partners do |
|----------------------|------------------------------|---|--|
| Phase2 | -5.81*** (0.71) | -5.81*** (0.71) | -4.23 (3.74) |
| Phase3 | -11.77** (2.62) | -11.77** (2.62) | -11.95*** (0.78) |
| Phase 2*25V | -19.66*** (2.71) | -4.34 (2.77) | -3.28 (4.23) |
| Phase 3*25V | 0.71 (3.41) | 11.16** (3.43) | 11.56*** (1.45) |
| Constant | 56.67*** (0.58) | 61.68*** (0.76) | 76.59*** (0.81) |
| Subject fixed effect | Yes | Yes | Yes |
| R-squared | 0.08 | 0.04 | 0.05 |
| Observations | 965 | 743 | 337 |

Note: The regressions in this table include only observations of those who receive 200 units of random income (and thus have to make transfer decisions). These regressions include observations from Baseline tests and the 25V treatment. Baseline insurance purchasing decisions are predicted by a regression modeled on the transfer history in the first 1-15 periods for treatments with voluntary insurance purchase. Subjects with the 25% lowest predicted probabilities of purchasing insurance are defined as having no insurance purchase in the Baseline. The Baseline test observations are included in Columns 1 and 2 when the subjects are predicted to purchase insurance and in Column 3 when the subjects are predicted not to purchase insurance. We use subject fixed effect to control for unobservable individual heterogeneity. Standard errors clustered at session levels are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

We try to control for this potential bias by using Baseline tests to predict insurance purchasing decisions. First, we estimate the determinants of insurance purchasing decisions in the treatments with voluntary insurance purchase (see Appendix B for the estimates). Our estimates suggest that pairs with lower average transfers are indeed more likely to purchase insurance. We then use the results to predict Baseline insurance purchasing decisions. Finally, we assign different time trends to different insurance purchase combinations in Table 3. This method is by no means accurate for our small sample, but at least it helps evaluate to what extent selection effect affects our estimates.

Based on “predicted” insurance purchasing decisions, we re-estimate the change in private transfers for pairs who both purchase insurance and pairs with asymmetric insurance purchasing decisions. These results are reported in Table 8. In each case, Baseline observations are included in the regressions only when the subjects belong to the corresponding insurance purchasing combination according to the predicted insurance purchase decisions. We can see that the estimates essentially are the same as those in Table 3. This suggests that, even if natural time trends attributable to endogenous selection of insurance purchasing decisions exist, they are not primary drivers of our results.

4. Theoretical Explanations

In this section, we provide a simple framework to explain our experimental findings. A standard repeated interaction model based on purely selfish preferences does not fully explain our results, because the decision problems exhibited in Phases 1 and 3 are identical to those with standard preferences. We therefore resort to explanations based on changes in social preference. We discuss two possible mechanisms in which preferences can be affected that can explain the relationship improvement: one based on guilt aversion and the other based on reciprocity. We base each model on reduced-form behavioral assumptions to illustrate the logic driving each mechanism, not to build more sophisticated modeling structures.

4.1. Model based on Guilt Aversion

We first propose an explanation based on guilt aversion (Charness and Dufwenberg, 2006). The idea is that in the asymmetric case, the insurance purchaser feels guilty when he finds that his partner has not purchased insurance. The underlying assumption is that insurance purchasing decisions also signals one's expectations of another's choices. To avoid guilt, the insurance purchaser acts first to help share risk with the non-purchaser, and in equilibrium the non-purchaser's transfers also increase. In the symmetric case, however, no guilt is involved as no one falls short of another's expectations. Because there are no changes in preferences, the level of risk sharing remains the same after the insurance is removed. This theory provides a satisfactory explanation for all experiment findings.

To put it more concretely, consider a pair of individuals with consumptions (c_i, c_{-i}) and transfers (τ_i, τ_{-i}) . Then, individual i 's utility is:¹²

$$v(c_i, c_{-i}) = (1 - \gamma)u(c_i) + \gamma u(c_{-i}) + \alpha(\tau_i; g). \quad (3)$$

The above utility function is quite standard within existing literature. On the one hand, we assume that individual i attaches a welfare weight $\gamma > 0$ to her partner's consumption utility in addition to her own as in Foster and Rosenzweig's (2001).¹³ On the other hand, we also include an additional term to capture the effect of guilt on the utility derived from individual's transfer, τ_i , following the specifications in the work of Li et al. (2010).

The key assumption is that guilt level changes based on insurance purchasing decisions. The literature on guilt aversion has indicated that "Feeling guilty [is] associated with...recognizing how a relationship partner's standards and expectations differ from one's own" (Baumeister, Stillwell, and Heatherton, 1995; Charness and Dufwenberg, 2006). Therefore, we assume that both individuals begin without feeling guilty. If both individuals purchase or do not purchase insurance, then there is still no guilt, as the partner's standards and expectations are the same as one's own. However,

¹² We assume that individuals have the same utility functions. Lin, Liu, and Meng (2014) consider a more general model in which γ_i can differ across individuals; adding that possibility does not affect the main results of the paper.

¹³ It is not essential to include the altruistic preferences in our model: our main results still hold even if the welfare weight is zero.

the level of guilt will change when an asymmetric insurance purchase is made. In particular, we assume that the insurance purchaser will feel guilty because she let her partner down; however, the non-purchaser does not feel guilty.

We follow the approach created by Charness and Genicot (2009) to solve the time-independent and constrained optimal transfer problem. The analytical framework is detailed in Appendix A.1. Our main prediction (Proposition 1 below) compares steady-state transfers in Phase 1 (before the introduction of formal insurance) and transfers in Phase 3 (when the formal insurance is removed) in the case of asymmetric insurance purchases:

Proposition 1: Suppose that full insurance coverage is not achieved, and the individuals make asymmetric insurance purchasing decisions. Then, for any $g_0 > 0$, it is always the case that equilibrium transfers for both individuals will be higher after the insurance is removed compared to cases without any formal insurance. Moreover, equilibrium transfers will increase as g_0 increases.

All formal proofs are presented in Appendix A. Proposition 1 states that when both individuals make asymmetric insurance purchasing decisions, equilibrium transfers for both individuals will be higher after the insurance is removed compared to scenarios without any formal insurance. It is not surprising that the individual who purchases insurance will be motivated by guilt to make more transfers to her partner. Surprisingly, the non-purchaser (who does not experience guilt) will also transfer more, because the non-purchaser will respond to the purchaser's kindness by making more transfers as well in equilibrium.

It is worthwhile to emphasize that the proposition above applies only to the asymmetric purchasing case; when both subjects make the same insurance purchasing decision (symmetric case), there is no change in the utility functions in our framework, and hence we do not expect to observe any systematic difference between the transfers in Phases 1 and 2.

The analysis of equilibrium transfers in Phase 2 is more complicated. On the one hand, compared to the transfers in Phases 1 and 3, those in Phase 2 will be lower because of the crowding-out effect of formal insurance on private transfers (see

Appendix A.3 and Lin et al., 2014). On the other hand, in the case of asymmetric insurance purchases, the transfers in Phase 2 also will be affected by guilt-induced preference changes.

4.2. Positive and Negative Reciprocity

Theories of social preferences may include other alternatives to explain our results besides guilt aversion. One possibility is positive and negative reciprocity. If purchasing insurance is viewed as relational betrayal and not doing so signals trust, then the insurance purchasers/non-purchasers can demonstrate positive/negative reciprocity by increasing/decreasing their transfers (Malmendier and Schmidt, 2016). Specifically, we can model these changes by changing the weight attached to another's utility in Equation (3), as shown in the Online Appendix. If positive reciprocity dominates negative reciprocity, we will observe an improvement in the relationship.

This theory does not generate clear predictions: the relationship can be either improved or ruined depending on whether positive or negative reciprocity dominates. Instead, guilt aversion predicts only that the insurance purchasers are affected by preference change, and hence enhancing the relationship. The empirical patterns also are less consistent with the theory of reciprocity. Table 7 suggests that those who do not purchase insurance in asymmetric cases actually do not reduce their transfers significantly compared to those at beginning of Phase 2 in the 25F treatment, so there is no evidence of negative reciprocity.

5. Conclusions

This lab study investigates how informal risk sharing is affected by formal insurance and whether these effects are driven by changes in social preferences. Our design involves both the introduction and removal of formal insurance to clearly identify whether changes in preference play a role in the crowding-out effect of formal insurance on informal risk sharing. By implementing both voluntary and forced formal insurance adoption decisions we are also able to understand the role of intentions. Quite surprisingly, we find that the introduction of formal insurance not

only leads to a very weak crowding-out effect, but also enhances informal risk sharing when the formal insurance is removed. This occurs primarily when the parties have voluntarily determined, asymmetric decisions when obtaining formal insurance. The exploration of these mechanisms and dynamics suggests that two elements are crucial for restoring and enhancing private relationships: intentions revealed by the voluntary purchase of insurance and the role of the insurance purchasers in asymmetric cases. A standard model of repeated risk sharing that only considers selfish preference cannot provide a satisfactory rationale for our results. Instead, we offer guilt aversion as an explanation for these findings.

This paper suggests that when asking how formal insurance affects informal risk sharing, it is important to incorporate consideration of social preferences. Our results suggest that the change in social preferences leads to change in informal risk sharing, which not only generates patterns counter to predictions from standard models, but also introduces a variety of heterogeneity not previously considered but perhaps empirically relevant. Moreover, our results are also relevant in other economic settings where relationships are subject to unexpected external introduction of formal insurance. For example, people in a business partnership may find other investment opportunities to which they can divert money. Similarly, research coauthors on a given project may be tempted to work on other projects as opportunities arise. Future studies are needed to test whether our results extend to these field settings.

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Appendix A. Theoretical Model

We set up a simple model that closely follows that of Charness and Genicot (2009). We consider a risk sharing group composed of two individuals, $i = 1, 2$. In each period $t = 1, 2, \dots$ individual, i receives income $y_i(s_t) \geq 0$, where s_t is an i.i.d. state of nature that takes the value of 1 or 2 with equal probability. Income follows the process:

$$y_i(s_t) = \begin{cases} f_i + h & \text{if } s_t = i \\ f_i & \text{otherwise} \end{cases} \quad (1)$$

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, so that there is no aggregate risk (in the experimental design, we assume that $f_1 = f_2 = f$).

In each 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 assumed that individual i will not make a transfer when she receives zero random income). Assuming no technology of savings, consumption in a given period is:

$$c_i(s) = \begin{cases} f_i + h - \tau & \text{if } s = i \\ f_i + \tau_{-i} & \text{otherwise} \end{cases} \quad (2)$$

We make some standard assumptions about our utility functions. $u(c_i)$ is the standard consumption utility that is increasing and concave in c_i , and individual i also

derives guilt-related utility $\alpha(\tau_i; g)$ if she makes a transfer, τ_i , to her partner. As in the assumption of u , we assume that α is increasing and concave in τ_i . With g representing the level of guilt, we assume that individual i derives more positive utility from making transfer τ_i if she feels guiltier, which implies that $\frac{\partial \alpha(\tau_i; g)}{\partial g} > 0$. Finally, individuals live infinitely, and the future is discounted with a common discount factor $0 < \beta < 1$.

For simplicity, we consider two distinct levels of guilt. If an individual does not feel guilty, we normalize her level of guilt to $g = 0$; otherwise, we let the level of guilt be $g = g_0 > 0$. We simplify the analysis further by assuming $\alpha(\tau_i; 0) = 0$: when an individual does not feel guilty, she derives no additional utility from her transfer except for the consumption and altruistic utilities. This enables us to focus only on the interesting case in which at least one individual feels guilty. The literature on guilt aversion has indicated that “Feeling guilty [is] associated with...recognizing how a relationship partner’s standards and expectations differ from one’s own” (Baumeister, Stillwell, and Heatherton, 1995; Charness and Dufwenberg, 2006). Therefore, we assume that both individuals begin with $g = 0$. If both individuals purchase or do not purchase insurance, then the level of guilt remains $g = 0$: there is no guilt, as the partner’s standards and expectations are the same as one’s own. However, the level of guilt will change when an asymmetric insurance purchase decision is made. In particular, we assume that the insurance purchaser will feel guilty and will have $g = g_0 > 0$ because she disappointed her partner; however, the non-purchaser does not feel guilty and still has $g = 0$.

We analyze the way in which the insurance purchasing decisions affects equilibrium transfers by changing the individuals’ guilt-related preferences. In our setting, formal insurance costs the individual ah and pays $2ah$ ($0 \leq a \leq \frac{1}{2}$) when no random income is received and nothing otherwise. When $a < \frac{1}{2}$, this insurance does not offer full coverage of risk and there is some room left for informal risk sharing mechanisms.

A.1. Steady State Equilibrium Analysis

We follow models of Charness and Genicot (2009) to solve the time-independent and constrained optimal transfer schemes. One important condition that determines the optimal transfer agreement are incentive constraints. To be incentive compatible, a risk sharing agreement must be such that the *ex-post* (after the random income is realized in a given period t) expected utility from participating in the agreement is larger than that from defection. When there is no altruistic concern, the most severe punishment for defection is to leave the subject in autarky forever. Ligon et al. (2002), Genicot (2006), and Charness and Genicot (2009) derive the constrained optimal solution when there are no altruistic preferences. Their results show that, if the incentive constraints are binding, only partial risk sharing can be achieved.

When the welfare weight is high enough, perfect risk-sharing can be achieved easily, even in the absence of repeated interactions. However, empirical evidence has shown that perfect risk sharing is seldom observed. Therefore, our analysis focus on the case in which an altruistic motive is not too strong and the binding incentive constraint still determines the optimal solution. We also want to avoid the unreasonable and largely unrealistic situation in which an agent hurts herself to punish the deviator if the deviator's level of altruism is extremely high. Thus, we make the following assumption throughout our analysis:

$$\textbf{Assumption. } \frac{\gamma}{1-\gamma} < \frac{u'(f_i+h)}{u'(f_{-i})}.$$

As we assume that $f_1 = f_2 = f$ in the experimental design, the above assumption implies that $\gamma < \frac{1}{2}$.

As shown by Lin et al. (2014), the most severe punishment individual i can receive is still the autarky value, i.e., no private transfers occur in the punishment path. Hence, the individual's problem in the absence of formal insurance can be written as:

$$\max_{\tau_i} \frac{1}{2} \sum_j \beta^j v(c_{ij}, c_{-ij}) = \frac{1}{1-\beta} Ev(c_i, c_{-i})$$

s.t.

$$Ev(c_{-i}, c_i) \geq \bar{v}_{-i} \quad (5)$$

$$(1 - \beta)v(c_{it}, c_{-it}) + \beta Ev(c_i, c_{-i}) \geq (1 - \beta)v(y_{it}, y_{-it}) + \beta Ev(y_i, y_{-i}) \quad (6)$$

Equation (5) ensures that equilibrium transfer leaves the other individual at a utility level of no less than a reservation value, \bar{v}_{-i} . This is the Pareto efficient condition. Equation (6) characterizes the individual's incentive constraint, which requires that the *ex-post* expected utility from the risk sharing agreement must be no lower than the expected utility in autarky. Let $\tau_{i,0}^*, i = 1, 2$ be the optimal transfer when there is no formal insurance available. As shown by Charness and Genicot (2009), the constrained optimal risk sharing agreements are determined simultaneously by the binding incentive constraints of *both* individuals in Equation (7), and these incentive constraints can only be binding when individuals receive the random income:

$$\begin{aligned} & \left(1 - \frac{\beta}{2}\right) \left[(1 - \gamma)u(f_i + h - \tau_{i,0}^*) + \gamma u(f_{-i} + \tau_{i,0}^*) \right] \\ & \quad + \frac{\beta}{2} \left[(1 - \gamma)u(f_i + \tau_{-i,0}^*) + \gamma u(f_{-i} + h - \tau_{-i,0}^*) \right] \\ & = \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f_i + h) + \gamma u(f_{-i})] + \frac{\beta}{2} [(1 - \gamma)u(f_i) + \gamma u(f_{-i} + h)], \\ & i = 1, 2. \end{aligned} \quad (7)$$

When an individual i receives the random income, by transferring $\tau_{i,0}^*$ to her partner, she receives a current utility $(1 - \beta)[(1 - \gamma)u(f_i + h - \tau_{i,0}^*) + \gamma u(f_{-i} + \tau_{i,0}^*)]$, and her future value is $\frac{\beta}{2}[(1 - \gamma)u(f_i + h - \tau_{i,0}^*) + \gamma u(f_{-i} + \tau_{i,0}^*)] + \frac{\beta}{2}[(1 - \gamma)u(f_i + \tau_{-i,0}^*) + \gamma u(f_{-i} + h - \tau_{-i,0}^*)]$. If i instead does not make any transfer to her partner, she receives a current utility $(1 - \beta)[(1 - \gamma)u(f_i + h) + \gamma u(f_{-i})]$, and her future value is $\frac{\beta}{2}[(1 - \gamma)u(f_i + h) + \gamma u(f_{-i})] + \frac{\beta}{2}[(1 - \gamma)u(f_i) + \gamma u(f_{-i} + h)]$, the expected utility from autarky. Since nobody feels guilty in this scenario, the equilibrium transfers are purely driven by preferences for risk-sharing and altruism.

After the insurance is removed, both individuals face a standard infinite horizon risk sharing game. Obviously, if both individuals purchase or do not purchase the insurance, their utility functions are the same as in the case of no formal insurance. As a result, the equilibrium transfers will not be affected. An interesting case occurs when the individuals make asymmetric insurance purchase decisions. Suppose that individual 1 purchases the insurance, while individual 2 does not. Then, the equilibrium transfers satisfy the following two equations:

$$\begin{aligned}
& \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h - \tau_{1,N}^*) + \gamma u(f + \tau_{1,N}^*) + \alpha(\tau_{1,N}^*; g_0)] \\
& \quad + \frac{\beta}{2} [(1 - \gamma)u(f + \tau_{2,N}^*) + \gamma u(f + h - \tau_{2,N}^*)] \\
& = \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h) + \gamma u(f)] + \frac{\beta}{2} [(1 - \gamma)u(f) + \gamma u(f + h)];
\end{aligned} \tag{8}$$

and

$$\begin{aligned}
& \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h - \tau_{2,N}^*) + \gamma u(f + \tau_{2,N}^*)] \\
& \quad + \frac{\beta}{2} [(1 - \gamma)u(f + \tau_{1,N}^*) + \gamma u(f + h - \tau_{1,N}^*)] \\
& = \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h) + \gamma u(f)] + \frac{\beta}{2} [(1 - \gamma)u(f) + \gamma u(f + h)].
\end{aligned} \tag{9}$$

Equations (8) and (9) are derived in an analogous way to equation (7). If a given individual reneges during this period by not making the transfer upon receiving random income, she will receive a temporarily higher utility today, but will have to rely solely on formal insurance to reduce the risk beginning in the next period. Equilibrium transfers are fixed, such that no individual has an incentive to deviate. Equation (9) is the same as equation (7); the only difference between equations (8) and (7) is that the level of guilt is $g = g_0 > 0$ in equation (8) while it is 0 in equation (7).

A.2. Proof of Proposition 1

The proof is to establish $\tau_{i,N}^* > \tau_{i,0}^*$, where these variables are defined in equations (7)-(9). The key of our proof is to show that both $\tau_{1,N}^*$ and $\tau_{2,N}^*$ are increasing in the insurance purchaser's level of guilt g_0 . The proof proceeds in two steps: in the first step, we will use equation (9) to show that the signs of $\frac{\partial \tau_{1,N}^*}{\partial g}$ and $\frac{\partial \tau_{2,N}^*}{\partial g}$ must be the same; in the second step, we will show by contradiction that it must be the case that both $\frac{\partial \tau_{1,N}^*}{\partial g}$ and $\frac{\partial \tau_{2,N}^*}{\partial g}$ are strictly positive.

Step 1. Given equation (9), total differentiation with respect to g yields:

$$0 = \left(1 - \frac{\beta}{2}\right) \left[-(1 - \gamma)u'(f + h - \tau_{2,N}^*) \frac{\partial \tau_{2,N}^*}{\partial g} + \gamma u'(f + \tau_{2,N}^*) \frac{\partial \tau_{2,N}^*}{\partial g} \right] + \frac{\beta}{2} \left[(1 - \gamma)u'(f + \tau_{1,N}^*) \frac{\partial \tau_{1,N}^*}{\partial g} - \gamma u'(f + h - \tau_{1,N}^*) \frac{\partial \tau_{1,N}^*}{\partial g} \right]. \quad (12)$$

From the assumption $\frac{\gamma}{1-\gamma} < \frac{u'(f+h)}{u'(f)}$, we have $\frac{\gamma}{1-\gamma} < \frac{u'(f+h)}{u'(f)} < \frac{u'(f+h-\tau_{2,N}^*)}{u'(f+\tau_{2,N}^*)}$, which

implies that $-(1 - \gamma)u'(f + h - \tau_{2,N}^*) + \gamma u'(f + \tau_{2,N}^*) < 0$. As $\gamma < \frac{1}{2}$ and $f + \tau_{1,N}^* < f + h - \tau_{1,N}^*$, we have $(1 - \gamma)u'(f + \tau_{1,N}^*) - \gamma u'(f + h - \tau_{1,N}^*) > 0$.

Therefore, it must be the case that $\text{sgn}\left(\frac{\partial \tau_{1,N}^*}{\partial g}\right) = \text{sgn}\left(\frac{\partial \tau_{2,N}^*}{\partial g}\right)$ from equation (12).

Step 2. Given equation (8), total differentiation with respect to g yields:

$$\begin{aligned} & \left(1 - \frac{\beta}{2}\right) \left[-(1 - \gamma)u'(f + h - \tau_{1,N}^*) \frac{\partial \tau_{1,N}^*}{\partial g} + \gamma u'(f + \tau_{1,N}^*) \frac{\partial \tau_{1,N}^*}{\partial g} + \frac{\partial \alpha(\tau_{1,N}^*; g)}{\partial \tau_{1,N}^*} \frac{\partial \tau_{1,N}^*}{\partial g} \right] \\ & + \frac{\beta}{2} \left[(1 - \gamma)u'(f + \tau_{2,N}^*) \frac{\partial \tau_{2,N}^*}{\partial g} - \gamma u'(f + h - \tau_{2,N}^*) \frac{\partial \tau_{2,N}^*}{\partial g} \right] \\ & = - \left(1 - \frac{\beta}{2}\right) \frac{\partial \alpha(\tau_{1,N}^*; g)}{\partial g} < 0. \end{aligned} \quad (13)$$

Obviously, it is impossible to have $\frac{\partial \tau_{1,N}^*}{\partial g} = \frac{\partial \tau_{2,N}^*}{\partial g} = 0$, because equation (13) is

violated. So, we only need to exclude the possibility that $\frac{\partial \tau_{1,N}^*}{\partial g}, \frac{\partial \tau_{2,N}^*}{\partial g} < 0$. Assume on

the contrary that $\frac{\partial \tau_{1,N}^*}{\partial g}, \frac{\partial \tau_{2,N}^*}{\partial g} < 0$ occurs for some g . Then consider another pair of transfers $\tilde{\tau}_{1,N} = \tau_{1,N}^* - d\tau_1$, $\tilde{\tau}_{2,N} = \tau_{2,N}^* - d\tau_2$ where $d\tau_1 = \frac{\partial \tau_{1,N}^*}{\partial g} dg$ and $d\tau_2 = (\frac{\partial \tau_{2,N}^*}{\partial g} + \epsilon \frac{\partial \alpha(\tau_{1,N}^*; g)}{\partial g}) dg$ with sufficiently small $dg > 0$ and $\epsilon > 0$. We will first show that under the new transfers, the non-purchaser's incentive constraint is still satisfied:

$$\begin{aligned} & \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h - \tilde{\tau}_{2,N}) + \gamma u(f + \tilde{\tau}_{2,N})] \\ & + \frac{\beta}{2} [(1 - \gamma)u(f + \tilde{\tau}_{1,N}) + \gamma u(f + h - \tilde{\tau}_{1,N})] \\ & \geq \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h) + \gamma u(f)] + \frac{\beta}{2} [(1 - \gamma)u(f) + \gamma u(f + h)]. \end{aligned}$$

By the Taylor expansion, the left-hand side (LHS) of the above inequality can be written as:

$$\begin{aligned} & \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h - \tau_{2,N}^*) + \gamma u(f + \tau_{2,N}^*)] \\ & + \frac{\beta}{2} [(1 - \gamma)u(f + \tau_{1,N}^*) + \gamma u(f + h - \tau_{1,N}^*)] \\ & - \left(1 - \frac{\beta}{2}\right) [-(1 - \gamma)u'(f + h - \tau_{2,N}^*) + \gamma u'(f + \tau_{2,N}^*)] d\tau_2 \\ & - \frac{\beta}{2} \left[(1 - \gamma)u'(f + \tau_{1,N}^*) - \gamma u'(f + h - \tau_{1,N}^*) \frac{\partial \tau_{1,N}^*}{\partial g} \right] d\tau_1 + o(dg), \end{aligned}$$

where $o(dg)$ satisfies $\lim_{dg \rightarrow 0} \frac{o(dg)}{dg} = 0$. Obviously, for any fixed $\epsilon > 0$, the new transfers increase the LHS of the above inequality when dg is sufficiently small, and hence the non-purchaser's incentive constraint is still satisfied.

Similarly, we can show that the purchaser's incentive constraint is also satisfied:

$$\begin{aligned} & \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h - \tilde{\tau}_{1,N}) + \gamma u(f + \tilde{\tau}_{1,N}) + \alpha(\tilde{\tau}_{1,N}; g_0)] \\ & + \frac{\beta}{2} [(1 - \gamma)u(f + \tilde{\tau}_{2,N}) + \gamma u(f + h - \tilde{\tau}_{2,N})] \\ & \geq \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h) + \gamma u(f)] + \frac{\beta}{2} [(1 - \gamma)u(f) + \gamma u(f + h)]. \end{aligned}$$

By the Taylor expansion, this is also true when $\epsilon > 0$ is sufficiently small.

Second, we will compute the change of everybody's expected utility under these new transfers. From the construction of $\tilde{\tau}_{i,N}$, the change of individual 1's expected utility is:

$$dEv_1 = \frac{1}{2} \left[(1-\gamma)u'(f+h-\tau_{1,N}^*)d\tau_1 - \gamma u'(f+\tau_{1,N}^*)d\tau_1 - \frac{\partial \alpha(\tau_{1,N}^*;g)}{\partial \tau_{1,N}^*}d\tau_1 \right] \\ + \frac{1}{2} \left[-(1-\gamma)u'(f+\tau_{2,N}^*)d\tau_2 + \gamma u'(f+h-\tau_{2,N}^*)d\tau_2 \right] + o(dg).$$

From the definition of $d\tau_i$, we can rewrite dEv_1 as

$$dEv_1 = \frac{1}{2} \left[(1-\gamma)u'(f+h-\tau_{1,N}^*) - \gamma u'(f+\tau_{1,N}^*) - \frac{\partial \alpha(\tau_{1,N}^*;g)}{\partial \tau_{1,N}^*} \right] \frac{\partial \tau_{1,N}^*}{\partial g} dg \\ + \frac{1}{2} \left[-(1-\gamma)u'(f+\tau_{2,N}^*) + \gamma u'(f+h-\tau_{2,N}^*) \right] \frac{\partial \tau_{2,N}^*}{\partial g} dg \\ - \frac{1}{2} \left[(1-\gamma)u'(f+\tau_{2,N}^*) - \gamma u'(f+h-\tau_{2,N}^*) \right] \epsilon \frac{\partial \alpha(\tau_{1,N}^*;g)}{\partial g} dg + o(dg).$$

Using equation (13), we obtain:

$$dEv_1 = \frac{1}{2} \left\{ 1 - \left[(1-\gamma)u'(f+\tau_{2,N}^*) - \gamma u'(f+h-\tau_{2,N}^*) \right] \epsilon \right\} \frac{\partial \alpha(\tau_{1,N}^*;g)}{\partial g} dg \\ + \frac{1}{2} \left[-(1-\gamma)u'(f+\tau_{2,N}^*) + \gamma u'(f+h-\tau_{2,N}^*) \right] \frac{1-\beta}{1-\beta/2} \frac{\partial \tau_{2,N}^*}{\partial g} dg + o(dg).$$

The second term of the above expression is strictly positive when $\frac{\partial \tau_{2,N}^*}{\partial g} > 0$. Then, obviously, $dEv_1 > 0$ when $\epsilon > 0$ is sufficiently small. Similarly, it is straightforward to show that individual 2's expected utility also increases under these new transfers. But this contradicts with the optimality of $\tau_{i,N}^*$! Therefore, we conclude that it is impossible to have $\frac{\partial \tau_{1,N}^*}{\partial g}, \frac{\partial \tau_{2,N}^*}{\partial g} < 0$. As a result, equilibrium transfers should strictly increase as g increases.

It is also straightforward to show that $\tau_{1,N}^* > \tau_{2,N}^*$ because equations (8) and (9) together imply:

$$\left(1 - \frac{\beta}{2}\right) [(1-\gamma)u(f+h-\tau_{1,N}^*) + \gamma u(f+\tau_{1,N}^*) + \alpha(\tau_{1,N}^*;g_0)] \\ + \frac{\beta}{2} [(1-\gamma)u(f+\tau_{2,N}^*) + \gamma u(f+h-\tau_{2,N}^*)] \\ = \left(1 - \frac{\beta}{2}\right) [(1-\gamma)u(f+h-\tau_{2,N}^*) + \gamma u(f+\tau_{2,N}^*)] \\ + \frac{\beta}{2} [(1-\gamma)u(f+\tau_{1,N}^*) + \gamma u(f+h-\tau_{1,N}^*)]. \quad (14)$$

If $\tau_{1,N}^* \leq \tau_{2,N}^*$, we obtain $(1 - \gamma)u(f + h - \tau_{1,N}^*) + \gamma u(f + \tau_{1,N}^*) \geq (1 - \gamma)u(f + h - \tau_{2,N}^*) + \gamma u(f + \tau_{2,N}^*)$, because the function $(1 - \gamma)u(f + h - \tau) + \gamma u(f + \tau)$ is decreasing in τ . Moreover, $(1 - \gamma)u(f + \tau_{2,N}^*) + \gamma u(f + h - \tau_{2,N}^*) \geq (1 - \gamma)u(f + \tau_{1,N}^*) + \gamma u(f + h - \tau_{1,N}^*)$, because the function $(1 - \gamma)u(f + \tau) + \gamma u(f + h - \tau)$ is increasing in τ . Therefore, equation (14) cannot hold if $\tau_{1,N}^* \leq \tau_{2,N}^*$, and we conclude that $\tau_{1,N}^* > \tau_{2,N}^*$. This completes the proof of Proposition 1.

A.3. Analysis of the Impact of the Crowding-Out Effect on Formal Insurance

In this section, we analyze the crowding-out effect of formal insurance in Phase 2. Our steady-state analysis implies that when the insurance is still in place, the equilibrium transfers satisfy the following two equations in the asymmetric case:

$$\begin{aligned}
& \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h - ah - \tau_{1,I}^*) + \gamma u(f + \tau_{1,I}^*) + \alpha(\tau_{1,I}^*; g_0)] \\
& \quad + \frac{\beta}{2} [(1 - \gamma)u(f + ah + \tau_{2,I}^*) + \gamma u(f + h - \tau_{2,I}^*)] \\
& = \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h - ah) + \gamma u(f)] + \frac{\beta}{2} [(1 - \gamma)u(f + ah) + \gamma u(f + h)]
\end{aligned} \tag{15}$$

and

$$\begin{aligned}
& \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h - \tau_{2,I}^*) + \gamma u(f + ah + \tau_{2,I}^*)] \\
& \quad + \frac{\beta}{2} [(1 - \gamma)u(f + \tau_{1,I}^*) + \gamma u(f + h - ah - \tau_{1,I}^*)] \\
& = \left(1 - \frac{\beta}{2}\right) [(1 - \gamma)u(f + h) + \gamma u(f + ah)] + \frac{\beta}{2} [(1 - \gamma)u(f) + \gamma u(f + h - ah)].
\end{aligned} \tag{16}$$

Proposition 2: Suppose that full insurance is not implemented and the individuals make asymmetric insurance purchase decisions. Then, for any $g_0 > 0$, it is always

the case that equilibrium transfers will be crowded out by formal insurance: $\tau_{1,N}^* > \tau_{1,I}^*$, and $\tau_{2,N}^* > \tau_{2,I}^*$. Moreover, equilibrium transfers will increase as g_0 increases.

Proof of Proposition 2

Proposition 2 works similarly to the proof of Proposition 1 to show that equilibrium transfers will increase as g_0 increases. Hence the proof of that statement is omitted. And we only need to show the crowding-out results. Given equations (15) and (16), total differentiation with respect to a yields:

$$\begin{aligned}
& \left(1 - \frac{\beta}{2}\right) \left[- (1 - \gamma) u' (f + h - ah - \tau_{1,I}^*) \frac{\partial \tau_{1,I}^*}{\partial a} + \gamma u' (f + \tau_{1,I}^*) \frac{\partial \tau_{1,I}^*}{\partial a} \right. \\
& \quad \left. + \frac{\partial \alpha(\tau_{1,I}^*; g)}{\partial \tau_{1,I}^*} \frac{\partial \tau_{1,I}^*}{\partial a} \right] \\
& + \frac{\beta}{2} \left[(1 - \gamma) u' (f + ah + \tau_{2,I}^*) \frac{\partial \tau_{2,I}^*}{\partial a} - \gamma u' (f + h - \tau_{2,I}^*) \frac{\partial \tau_{2,I}^*}{\partial a} \right] \\
& = h(1 - \gamma) \left(1 - \frac{\beta}{2}\right) \left[u' (f + h - ah - \tau_{1,I}^*) - u' (f + h - ah) \right] \\
& + h(1 - \gamma) \frac{\beta}{2} \left[u' (f + ah) - u' (f + ah + \tau_{2,I}^*) \right], \tag{17}
\end{aligned}$$

and

$$\begin{aligned}
& \left(1 - \frac{\beta}{2}\right) \left[- (1 - \gamma) u' (f + h - \tau_{2,I}^*) \frac{\partial \tau_{2,I}^*}{\partial a} + \gamma u' (f + ah + \tau_{2,I}^*) \frac{\partial \tau_{2,I}^*}{\partial a} \right] \\
& + \frac{\beta}{2} \left[(1 - \gamma) u' (f + \tau_{1,I}^*) \frac{\partial \tau_{1,I}^*}{\partial a} - \gamma u' (f + h - ah - \tau_{1,I}^*) \frac{\partial \tau_{1,I}^*}{\partial a} \right] \\
& = h\gamma \left(1 - \frac{\beta}{2}\right) \left[u' (f + ah) - u' (f + ah + \tau_{2,I}^*) \right] \\
& + h\gamma \frac{\beta}{2} \left[u' (f + h - ah - \tau_{1,I}^*) - u' (f + h - ah) \right]. \tag{18}
\end{aligned}$$

Obviously, the RHS of equations (17) and (18) are both positive. As shown by the proof of Proposition 1 and also implied by the assumption, we obtain:

$$-(1-\gamma)u'(f+h-ah-\tau_{1,I}^*)+\gamma u'(f+\tau_{1,I}^*)+\frac{\partial\alpha(\tau_{1,I}^*,g)}{\partial\tau_{1,I}^*}<0,$$

$$(1-\gamma)u'(f+ah+\tau_{2,I}^*)-\gamma u'(f+h-\tau_{2,I}^*)>0,$$

$$-(1-\gamma)u'(f+h-\tau_{2,I}^*)+\gamma u'(f+ah+\tau_{2,I}^*)<0,$$

and

$$(1-\gamma)u'(f+\tau_{1,I}^*)-\gamma u'(f+h-ah-\tau_{1,I}^*)>0.$$

Therefore, $\frac{\partial\tau_{1,I}^*}{\partial a}$ and $\frac{\partial\tau_{2,I}^*}{\partial a}$ has to be both positive or negative. However, $\frac{\partial\tau_{1,I}^*}{\partial a}$ and $\frac{\partial\tau_{2,I}^*}{\partial a}$ cannot be both positive since otherwise, we can find another pair of transfers which will lead to strictly higher expected utilities. As a result, equilibrium transfers must decrease in a . Notice that $\tau_{1,N}^*$ and $\tau_{2,N}^*$ satisfy equations (8) and (9) when $a = 0$. Hence, it must be the case that $\tau_{1,N}^* > \tau_{1,I}^*$, and $\tau_{2,N}^* > \tau_{2,I}^*$ for any $a > 0$.

Appendix B. Omitted Regression Results

- **The Role of Random Transfer Opportunities**

It is cleaner to test the role of insurance purchasers by analyzing how the number of exogenous realizations of random income they receive in Phase 2 (hence creating opportunities to transfer) affects the outcomes in Phase 3. Specifically, we focus on the sample of asymmetric insurance purchases, further dividing the sample depending on whether the insurance purchasers receive random income more than 8 times during the 15 periods in Phase 2.

Table B1. How the Opportunity to Receive Transfers in Phase 2 Affects Phase 3

| | Insurance purchasers receive more random income in Phase 2 | | Insurance purchasers receive less random income in Phase 2 | |
|---|---|---------------------|---|---------------------|
| | 1 purchasers | 2 non-purchaser | 3 purchasers | 4 non-purchaser |
| <i>Panel A: 25V treatment + Baseline treatment</i> | | | | |
| Phase 3 | -11.99*** (1.97) | -11.99*** (1.97) | -11.99*** (1.97) | -11.99*** (1.97) |
| Phase 3*25V | 13.40*** (2.24) | 12.39** (2.41) | 5.32 (4.36) | 11.21** (2.01) |
| Constant | 64.98*** (0.84) | 64.13*** (0.87) | 65.52*** (0.92) | 65.39*** (0.89) |
| Subject fixed effect | Yes | Yes | Yes | Yes |
| R-square | 0.04 | 0.05 | 0.05 | 0.05 |
| Observations | 566 | 544 | 517 | 533 |
| <i>Panel B: 25V treatment + 25F treatment</i> | | | | |
| Phase 3 | -26.38*** (0.00) | -24.10*** (0.00) | -1.05*** (0.00) | 3.00*** (0.00) |
| Phase 3*25V | 27.79*** (1.13) | 24.50*** (1.47) | -5.62 (4.16) | -3.78** (0.44) |
| Constant | 60.97*** (0.33) | 59.26*** (0.39) | 84.41*** (0.73) | 76.35*** (0.11) |
| Subject fixed effect | Yes | Yes | Yes | Yes |
| R-square | 0.09 | 0.16 | 0.02 | 0.00 |
| Observations | 146 | 124 | 97 | 113 |

Note: The regressions in this table include all observations for a given subject regardless of whether or not she receives the 200 units of random income. Panel A includes observations from the 25V treatment and Baseline tests. Panel B includes observations from the 25V and 25F treatments. Only observations in Phases 1 and 3 are included, but the samples are divided according to whether the insurance purchasers receive more opportunities to make transfers in Phase 2. Panel A includes all subjects' observations in the corresponding periods of Baseline treatment because they do not have

choices around insurance purchase. However, in Panel B, we include only observations from the 25F treatment, in which the same insurance purchase decisions are made as are those in the 25V treatment. We use subject fixed effect to account for unobserved individual heterogeneity. Therefore, the dummy variable “25V” is not identifiable. Standard errors clustered at session levels are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B1 reports the estimated results of Phases 1 and 3. To check the robustness of our results, Panel A uses the Baseline treatment and Panel B uses the 25F treatments as control groups. In Panel A, all of the observations in the corresponding periods of the Baseline treatment are included as a control. In Panel B, we include only observations from individuals in the 25F treatment who make asymmetric insurance purchasing decisions as a control group to match their counterparts in the 25V treatment. Regardless of which control groups were used, the results are consistent: pairs in which the insurance purchasers receive more opportunities to make transfers in Phase 2 (hence restoring the pair’s relationship) perform better in Phase 3 than those for whom such opportunities are limited.

Specifically, the first two columns of Panel A demonstrate that, when insurance purchasers happen to have more opportunities to make transfers to their partners in Phase 2, the difference between transfers in Phases 3 and 1 is approximately 13.40 (for purchasers) and 12.39 (for non-purchasers) higher in the 25V treatment than in the Baseline treatment. These numbers increase even more—27.79 (for purchasers) and 24.50 (for non-purchasers)—when we use the 25F as the control group. However, when insurance purchasers have fewer opportunities to make transfers in Phase 2, the corresponding estimates become smaller and even negative in Panel B.

- **Analysis of Insurance Purchasing Decisions**

Table B2 reports a logit regression on insurance purchasing decisions in the 16th period, while pooling the corresponding observations from the 25V and 75V treatments.

The outcome variable is a dummy variable of value 1 if insurance is purchased in the 16th period, and zero otherwise. Across columns, explanatory variables include: the pair’s average transfers in the first 15 periods, the subject’s and the partner’s

average transfers in the first 15 periods, the subject's and the partner's transfers in the 15th period, and the number of 200 random income received in the first 15 periods. Although the significance levels vary, past transfers appear to be negatively associated with insurance purchasing decisions. We use estimates in Column 4 to predict the probability of purchasing insurance among the Baseline subjects. However, the results are not affected by the column structure selected. For those with the 25% lowest probability to purchase insurance, a decision not to purchase insurance is assigned. This percentage was chosen because about 72% and 78% subjects purchase insurance in the 25V and 75V treatments.

Table B2. A Logit Model of Insurance Purchase Decisions in the 25V and 75V Treatments

| | 1 | 2 | 3 | 4 |
|--|-------------------|-------------------|-------------------|------------------|
| | Insurance | Insurance | Insurance | Insurance |
| Average pair transfer (period 1-15) | -0.02* (0.01) | | | |
| The subject's average transfer (period 1-15) | | -0.02 (0.02) | | -0.01 (0.04) |
| The partner's average transfer (period 1-15) | | -0.02 (0.02) | | -0.00 (0.04) |
| The subject's transfer in period 15 | | | -0.02** (0.01) | -0.02* (0.01) |
| The partner's transfer in period 15 | | | -0.01 (0.01) | -0.00 (0.01) |
| # of received 200 R-income | | | | 0.10 (0.16) |
| constant | 2.61*** (0.90) | 2.61*** (0.90) | 2.24*** (0.67) | 0.26 (3.70) |
| Observations | 72 | 72 | 72 | 72 |

Note: This table analyzes subject's insurance purchase decision in period 16 of 25V and 75V treatment using logit model. Robust standard errors are reported in parentheses with *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix C. Experiment Protocol

Below we provide details of our experiment protocol, using the images presented to subjects the 25V treatment as an example.

- **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 10 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.”

- **Risk Sharing Games**

The instructions subjects received read as follows:

“This experiment includes many periods, and the computer will randomly choose to end the experiment at some point after period 45. At the beginning of the first period, each participant will be randomly matched against another participant. This pairing will remain the same during the entire experiment.

During 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 then your partner will receive 0 points and vice versa. You have equal probabilities to receive 200 or 0 points each period.

If you receive 200 points, you may choose to transfer some money or no money to your partner. If you do make a transfer, this amount must be non-negative, and no more than the total income you receive during 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 the 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 10 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 receives 200 points of 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 the 25V treatment as an example, which can be divided into three phases: Phase 1 (periods 1 to 15), Phase 2 (periods 16 to 30) and Phase 3 (periods 31 to 45).

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

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

Step 1 of Phase 2: At the beginning of the 16th period, the screen in Figure C.4 suggests that each subject now has an option to purchase insurance. Each subject will be informed that purchasing decisions can only be made in the 16th period, and this decision will be replicated in later periods. This screen introduces the details of the formal insurance policy and asks for the subject's decision.

Step 2 of Phase 2: After the insurance purchasing decision is made, a screen similar to the Step 1 of Phase 1 is displayed (See Figure C.5 and Figure C.6, suppose one subject decided to purchase insurance and the opponent player decided not to) to ask for subjects' transfer decision.

Step 3 of Phase 2: Figure C.7 again shows current and historical information about the insurance purchasing decisions and transfers.

Step 1 of Phase 3: At the beginning of the 31 period, the screen in Figure C.8 is displayed to subjects, suggesting that the option to purchase insurance is no longer available.

Step 2 of Phase 3: From the end of period 30 till the conclusion of the game, subjects who receive 200 points of random income can see Figure C.9 and answer the question "How much are you willing to transfer to your partner?" Subjects with 0 points of random income see Figure C.10.

Step 2 of Phase 3: Figure C.11 again shows current and historical information about the insurance purchase decision and transfers.

Figure C.1

| | | |
|---|--|---|
| Period | | 3 |
| Your | | Your opponent player's |
| <p>Fixed income 125</p> <p>Random income 200</p> <p>Income before transfer 325</p> <p>How much are you willing to transfer to your opponent? <input type="text"/></p> <p>Please enter a nonnegative integer.</p> | | <p>Fixed income 125</p> <p>Random income 0</p> <p>Income before transfer 125</p> <p>No transfer of your opponent is required because of the low random income.</p> |
| OK | | |

Figure C.2

| | | |
|---|--|---|
| Period | | 3 |
| Your | | Your opponent player's |
| <p>Fixed income 125</p> <p>Random income 0</p> <p>Income before transfer 125</p> <p>Your random income is relatively low this round. You do not need to make a transfer decision this round.</p> | | <p>Fixed income 125</p> <p>Random income 200</p> <p>Income before transfer 325</p> <p>A transfer is required from your opponent because of the high random income.</p> |
| OK | | |

Figure C.3

Period
3

Your history

Your opponent player's history

Your fixed income is
125

Your opponent player's fixed income is
125

| Period | Random income | Income before transfer | Transfer | Transfer from opponent | Income after transfer | Random income | Income before transfer | Transfer | Transfer from opponent | Income after transfer |
|--------|---------------|------------------------|----------|------------------------|-----------------------|---------------|------------------------|----------|------------------------|-----------------------|
| 1 | 0 | 125 | 0 | 93 | 218 | 200 | 325 | 93 | 0 | 232 |
| 2 | 200 | 325 | 98 | 0 | 227 | 0 | 125 | 0 | 98 | 223 |
| 3 | 200 | 325 | 87 | 0 | 238 | 0 | 125 | 0 | 87 | 212 |

OK

Figure C.4

Period
16

From this period on, you can purchase an insurance contract before the income shock. The premium of this insurance is 25 points. If you purchase insurance in a round and get 0 points of random income in that round, you will be compensated 50 points. If you purchase this insurance and receive 200 points of random income, you will not be compensated.

You can make purchase decision only in this period. And in later periods your purchase decision would be replicated automatically.

Each period, you and your partner will receive the fixed income and a random income as before. Both incomes are independent of your purchase decision.

If you want to purchase insurance, press 'Yes'. If you don't want to purchase insurance, press 'no'.

Do you want to purchase the insurance

Yes
No

Figure C.5

| Period 16 | |
|---|--|
| 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 25 | Cost of insurance purchase 0 |
| Compensation of insurance 0 | Compensation of insurance 0 |
| Income before transfer 300 | Income before transfer 125 |
| How much are you willing to transfer to your opponent? <input type="text" value="1"/> | No transfer of your opponent is required because of the low random income. |
| Please enter a nonnegative integer. | |
| <input type="button" value="OK"/> | |

Figure C.6

| Period 16 | |
|--|--|
| 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 25 |
| Compensation of insurance 0 | Compensation of insurance 0 |
| Income before transfer 125 | Income before transfer 300 |
| Your random income is relatively low this round. You do not need to make a transfer decision this round. | A transfer is required from your opponent because of the high random income. |
| <input type="button" value="OK"/> | |

Figure C.7

| Period 18 | | | | | | | | | | | | | | | | |
|--------------------------|-------------------------------|--------------------|----------------------------|------------------------|------------------------|----------------------|-----------------------|-----|--|--------------------|----------------------------|------------------------|------------------------|----------------------|-----------------------|-----|
| Your history | | | | | | | | | Your opponent player's history | | | | | | | |
| Your fixed income is 125 | | | | | | | | | Your opponent player's fixed income is 125 | | | | | | | |
| Period | Random income before transfer | Insurance purchase | Cost of insurance purchase | Pensation of insurance | Transfer from opponent | Transfer to opponent | Income after transfer | | Random income before transfer | Insurance purchase | Cost of insurance purchase | Pensation of insurance | Transfer from opponent | Transfer to opponent | Income after transfer | |
| 2 | 200 | 325 | — | 0 | 0 | 98 | 0 | 227 | 0 | 125 | — | 0 | 0 | 0 | 98 | 223 |
| 3 | 200 | 325 | — | 0 | 0 | 87 | 0 | 238 | 0 | 125 | — | 0 | 0 | 0 | 87 | 212 |
| 4 | 0 | 125 | — | 0 | 0 | 0 | 120 | 245 | 200 | 325 | — | 0 | 0 | 120 | 0 | 205 |
| 5 | 200 | 325 | — | 0 | 0 | 110 | 0 | 215 | 0 | 125 | — | 0 | 0 | 0 | 110 | 235 |
| 6 | 0 | 125 | — | 0 | 0 | 0 | 100 | 225 | 200 | 325 | — | 0 | 0 | 100 | 0 | 225 |
| 7 | 200 | 325 | — | 0 | 0 | 102 | 0 | 223 | 0 | 125 | — | 0 | 0 | 0 | 102 | 227 |
| 8 | 0 | 125 | — | 0 | 0 | 0 | 96 | 221 | 200 | 325 | — | 0 | 0 | 96 | 0 | 229 |
| 9 | 0 | 125 | — | 0 | 0 | 0 | 106 | 231 | 200 | 325 | — | 0 | 0 | 106 | 0 | 219 |
| 10 | 0 | 125 | — | 0 | 0 | 0 | 108 | 233 | 200 | 325 | — | 0 | 0 | 108 | 0 | 217 |
| 11 | 200 | 325 | — | 0 | 0 | 138 | 0 | 187 | 0 | 125 | — | 0 | 0 | 0 | 138 | 263 |
| 12 | 200 | 325 | — | 0 | 0 | 126 | 0 | 199 | 0 | 125 | — | 0 | 0 | 0 | 126 | 251 |
| 13 | 0 | 125 | — | 0 | 0 | 0 | 150 | 275 | 200 | 325 | — | 0 | 0 | 150 | 0 | 175 |
| 14 | 200 | 325 | — | 0 | 0 | 142 | 0 | 183 | 0 | 125 | — | 0 | 0 | 0 | 142 | 267 |
| 15 | 200 | 325 | — | 0 | 0 | 123 | 0 | 202 | 0 | 125 | — | 0 | 0 | 0 | 123 | 248 |
| 16 | 200 | 300 | Yes | 25 | 0 | 88 | 0 | 212 | 0 | 125 | No | 0 | 0 | 0 | 88 | 213 |
| 17 | 200 | 300 | Yes | 25 | 0 | 78 | 0 | 222 | 0 | 125 | No | 0 | 0 | 0 | 78 | 203 |
| 18 | 200 | 300 | Yes | 25 | 0 | 143 | 0 | 157 | 0 | 125 | No | 0 | 0 | 0 | 143 | 268 |

Figure C.8

Period
31

From this period on, you cannot purchase an insurance contract anymore, regardless of the previous purchase decision. Each period, you need not pay the insurance premium, or get the compensation after realization of random income. You and your partner will receive the fixed income and a random income with equal probability as before.

OK

Figure C.9

| | |
|--|---|
| Period <input type="text" value="31"/> | |
| Your | Your opponent player's |
| <p>Fixed income 125</p> <p>Random income 200</p> <p>Income before transfer 325</p> <p>How much are you willing to transfer to your opponent? <input type="text" value="1"/></p> <p>Please enter a nonnegative integer.</p> <p style="text-align: center;">OK</p> | <p>Fixed income 125</p> <p>Random income 0</p> <p>Income before transfer 125</p> <p>No transfer of your opponent is required because of the low random income.</p> |

Figure C.10

| | |
|--|---|
| Period <input type="text" value="31"/> | |
| Your | Your opponent player's |
| <p>Fixed income 125</p> <p>Random income 0</p> <p>Income before transfer 125</p> <p>Your random income is relatively low this round. You do not need to make a transfer decision this round.</p> <p style="text-align: center;">OK</p> | <p>Fixed income 125</p> <p>Random income 200</p> <p>Income before transfer 325</p> <p>A transfer is required from your opponent because of the high random income.</p> |

Figure C.11

| Period 32 | | | | | | | | |
|--------------------------|-------------------------------|--------------------|----------------------------|---------------------|------------------------|-------------------------|-----------------------|-----|
| Your history | | | | | | | | |
| Your fixed income is 125 | | | | | | | | |
| Period | Random income before transfer | Insurance purchase | Cost of insurance purchase | Pensar of insurance | Transfer from opponent | Transfer after transfer | Income after transfer | |
| 16 | 0 | 125 | No | 0 | 0 | 0 | 88 | 213 |
| 17 | 0 | 125 | No | 0 | 0 | 0 | 78 | 203 |
| 18 | 0 | 125 | No | 0 | 0 | 0 | 143 | 268 |
| 19 | 0 | 125 | No | 0 | 0 | 0 | 65 | 190 |
| 20 | 200 | 325 | No | 0 | 0 | 139 | 0 | 186 |
| 21 | 0 | 125 | No | 0 | 0 | 0 | 86 | 211 |
| 22 | 200 | 325 | No | 0 | 0 | 82 | 0 | 243 |
| 23 | 0 | 125 | No | 0 | 0 | 0 | 136 | 261 |
| 24 | 0 | 125 | No | 0 | 0 | 0 | 157 | 282 |
| 25 | 200 | 325 | No | 0 | 0 | 144 | 0 | 181 |
| 26 | 0 | 125 | No | 0 | 0 | 0 | 152 | 277 |
| 27 | 0 | 125 | No | 0 | 0 | 0 | 162 | 287 |
| 28 | 200 | 325 | No | 0 | 0 | 146 | 0 | 179 |
| 29 | 200 | 325 | No | 0 | 0 | 168 | 0 | 157 |
| 30 | 0 | 125 | No | 0 | 0 | 0 | 84 | 209 |
| 31 | 200 | 325 | — | 0 | 0 | 111 | 0 | 214 |
| 32 | 200 | 325 | — | 0 | 0 | 125 | 0 | 200 |

| Your opponent player's history | | | | | | | | |
|--|-------------------------------|--------------------|----------------------------|---------------------|------------------------|-------------------------|-----------------------|-----|
| Your opponent player's fixed income is 125 | | | | | | | | |
| Period | Random income before transfer | Insurance purchase | Cost of insurance purchase | Pensar of insurance | Transfer from opponent | Transfer after transfer | Income after transfer | |
| 16 | 200 | 300 | Yes | 25 | 0 | 88 | 0 | 212 |
| 17 | 200 | 300 | Yes | 25 | 0 | 78 | 0 | 222 |
| 18 | 200 | 300 | Yes | 25 | 0 | 143 | 0 | 157 |
| 19 | 200 | 300 | Yes | 25 | 0 | 65 | 0 | 235 |
| 20 | 0 | 150 | Yes | 25 | 50 | 0 | 139 | 289 |
| 21 | 200 | 300 | Yes | 25 | 0 | 86 | 0 | 214 |
| 22 | 0 | 150 | Yes | 25 | 50 | 0 | 82 | 232 |
| 23 | 200 | 300 | Yes | 25 | 0 | 136 | 0 | 164 |
| 24 | 200 | 300 | Yes | 25 | 0 | 157 | 0 | 143 |
| 25 | 0 | 150 | Yes | 25 | 50 | 0 | 144 | 294 |
| 26 | 200 | 300 | Yes | 25 | 0 | 152 | 0 | 148 |
| 27 | 200 | 300 | Yes | 25 | 0 | 162 | 0 | 138 |
| 28 | 0 | 150 | Yes | 25 | 50 | 0 | 146 | 296 |
| 29 | 0 | 150 | Yes | 25 | 50 | 0 | 168 | 318 |
| 30 | 200 | 300 | Yes | 25 | 0 | 84 | 0 | 216 |
| 31 | 0 | 125 | — | 0 | 0 | 0 | 111 | 236 |
| 32 | 0 | 125 | — | 0 | 0 | 0 | 125 | 250 |

Online Appendix (not Intended for Publications)

A Theoretical Model of Both Positive and Negative Reciprocity

A.1. Model Design

The model design is identical to those details in the appendix, except for the modeling of social preferences. Let indicator function ϕ_i denote the insurance purchasing decisions of individual i : $\phi_i = 1$ if i purchases insurance and $\phi_i = 0$ otherwise. Individual i 's utility function then can be written as:

$$v(c_i, c_{-i}) = (1 - \gamma - \rho\phi_i(1 - \phi_{-i}) + \theta\phi_i(1 - \phi_{-i}))u(c_i) + (\gamma + \rho\phi_i(1 - \phi_{-i}) - \theta\phi_i(1 - \phi_{-i}))u(c_{-i}). \quad (4)$$

If both individuals purchase or do not purchase insurance, then the utility function $v(c_i, c_{-i})$ remains $(1 - \gamma)u(c_i) + \gamma u(c_{-i})$: there is no perceived kindness or meanness if individual i makes the same choice. However, the utility function $v(c_i, c_{-i})$ changes when an asymmetric insurance purchase decision occurs. In particular, we

assumed that $\rho \geq 0$ if individual i exhibits positive reciprocity when i purchases the insurance while the other individual does not, and $\theta \geq 0$ if individual i exhibits negative reciprocity when i does not purchase the insurance while the other individual does.

Similar to assumptions made in the appendix, we make the following assumption throughout our analysis:

$$\textbf{Assumption.} \quad \frac{\gamma + \rho}{1 - \gamma - \rho} < \frac{u'(f_i + h)}{u'(f_{-i})}.$$

As shown by Lin et al. (2014), the most severe punishment individual i can receive is still the autarky value, i.e., no private transfers occur in the punishment path. Hence, the individual's problem in the absence of formal insurance can be written as:

$$\max_{\tau_i} \frac{1}{2} \sum_j \beta^j v(c_{ij}, c_{-ij}) = \frac{1}{1 - \beta} Ev(c_i, c_{-i})$$

s.t.

$$Ev(c_{-i}, c_i) \geq \bar{v}_{-i} \quad (5)$$

$$(1 - \beta)v(c_{it}, c_{-it}) + \beta Ev(c_i, c_{-i}) \geq (1 - \beta)v(y_{it}, y_{-it}) + \beta Ev(y_i, y_{-i}). \quad (6)$$

Equation (5) ensures that the equilibrium transfer leaves the other individual at a utility level of no less than a reservation value, \bar{v}_{-i} . This is the Pareto efficient condition. Equation (6) characterizes the individual's incentive constraint, which requires that the *ex-post* expected utility from the risk sharing agreement must be no lower than the expected utility in autarky. Let $\tau_{i,0}^*, i = 1, 2$ be the optimal transfer when there is no formal insurance available. As shown by Charness and Genicot (2009), constrained optimal risk sharing agreements are determined simultaneously by the binding incentive constraints of *both* individuals in Equation (7), and these incentive constraints can only be binding when individuals receive random income:

$$\begin{aligned} & (1 - \frac{\beta}{2})[(1 - \gamma)u(f_i + h - \tau_{i,0}^*) + \gamma u(f_{-i} + \tau_{i,0}^*)] + \frac{\beta}{2}[(1 - \gamma)u(f_i + \tau_{-i,0}^*) + \gamma u(f_{-i} + h - \tau_{-i,0}^*)] \\ & = (1 - \frac{\beta}{2})[(1 - \gamma)u(f_i + h) + \gamma u(f_{-i})] + \frac{\beta}{2}[(1 - \gamma)u(f_i) + \gamma u(f_{-i} + h)], \quad i = 1, 2. \end{aligned}$$

(7)

A.2. The effect of insurance purchasing decisions

This section analyzes how the insurance purchasing decisions affects equilibrium transfers by changing the individuals' preferences. In our setting, formal insurance costs the individual ah and pays $2ah$ ($0 \leq a \leq \frac{1}{2}$) when no random income is received, and nothing otherwise. When $a < \frac{1}{2}$, this insurance does not offer full coverage of risk and there is some room left for informal risk sharing mechanisms. In this section, we analyze the effect of insurance purchasing decisions on subsequent risk sharing outcomes. We first analyze scenarios after formal insurance is removed and there is only a change of altruistic preferences, and then analyze cases when formal insurance is still in place. The second scenario is more complicated because of the coexistence of preference change and the crowding-out effect of formal insurance.

A.2.1 After insurance is removed

After insurance is removed, both individuals face a standard infinite horizon risk sharing game. Obviously, if both individuals purchase or do not purchase the insurance, their utility functions are the same as in the case of no formal insurance. As a result, equilibrium transfers will not be affected. An interesting case occurs when individuals make asymmetric insurance purchasing decisions. For example, suppose that individual 1 purchases insurance, while individual 2 does not. Then, the equilibrium transfers satisfy the following two equations:

$$\begin{aligned}
 & (1 - \frac{\beta}{2})[(1 - \gamma - \rho)u(f + h - \tau_{1,N}^*) + (\gamma + \rho)u(f + \tau_{1,N}^*)] \\
 & + \frac{\beta}{2}[(1 - \gamma - \rho)u(f + \tau_{2,N}^*) + (\gamma + \rho)u(f + h - \tau_{2,N}^*)] \\
 & = (1 - \frac{\beta}{2})[(1 - \gamma - \rho)u(f + h) + (\gamma + \rho)u(f)] \\
 & + \frac{\beta}{2}[(1 - \gamma - \rho)u(f) + (\gamma + \rho)u(f + h)];
 \end{aligned}$$

(8)

and:

$$\begin{aligned}
& (1 - \frac{\beta}{2})[(1 - \gamma + \theta)u(f + h - \tau_{2,N}^*) + (\gamma - \theta)u(f + \tau_{2,N}^*)] \\
& + \frac{\beta}{2}[(1 - \gamma + \theta)u(f + \tau_{1,N}^*) + (\gamma - \theta)u(f + h - \tau_{1,N}^*)] \\
& = (1 - \frac{\beta}{2})[(1 - \gamma + \theta)u(f + h) + (\gamma - \theta)u(f)] \\
& + \frac{\beta}{2}[(1 - \gamma + \theta)u(f) + (\gamma - \theta)u(f + h)].
\end{aligned} \tag{9}$$

Equations (8) and (9) are derived in a similar fashion as equation (7). If a given individual reneges during this period by not making the transfer while receiving random income, she will receive a temporarily higher utility, but will have to rely solely on formal insurance for risk reduction beginning in the next period. Equilibrium transfers are fixed, such that no individual has an incentive to deviate.

Proposition 1: Suppose that providing full insurance coverage is not achieved. Then, for any $\rho > 0$, there exist unique cutoffs, $\bar{\theta}_1$ and $\bar{\theta}_2$, such that equilibrium transfers for both individuals will be higher after the insurance is removed compared to those in the case of no formal insurance if $\theta < \bar{\theta}_1$; individual 1's equilibrium transfers will be higher, while individual 2's equilibrium transfers will be lower if $\bar{\theta}_1 < \theta < \bar{\theta}_2$, and equilibrium transfers will be lower if $\theta > \bar{\theta}_2$. Moreover, equilibrium transfers will decrease as θ increases.

All of the formal proofs can be found in Section A.4. Proposition 1 states that when both individuals make asymmetric insurance purchase decisions, equilibrium transfers after the insurance is removed decrease in θ . As θ increases, individual 2, who has not purchased insurance, reciprocates less. As a result, individual 2 will decrease her transfers, and individual 1's transfers will decrease as well to guarantee that they have no incentive to deviate. The same logic implies that equilibrium transfers after insurance is removed also increase in ρ : as individual 1 becomes more reciprocal, both individuals' transfers will increase in equilibrium.

Equilibrium transfers may be higher or lower after insurance is removed, in comparison to cases without any formal insurance. In particular, if $\rho > 0$ and $\theta = 0$ (individual 1 becomes more reciprocal while 2 stays the same), equilibrium transfers will be higher compared to those occurring in scenarios without formal insurance; if $\rho = 0$ and $\theta > 0$ (individual 2 becomes more reciprocal while 1 stays the same), equilibrium transfers will be lower compared to scenarios without formal insurance.

A.2.2 When insurance is still in effect

When insurance is still implemented, equilibrium transfers satisfy the following two equations for asymmetric cases:

$$\begin{aligned}
& (1 - \frac{\beta}{2})[(1 - \gamma - \rho)u(f + h - ah - \tau_{1,I}^*) + (\gamma + \rho)u(f + \tau_{1,I}^*)] \\
& + \frac{\beta}{2}[(1 - \gamma - \rho)u(f + ah + \tau_{2,I}^*) + (\gamma + \rho)u(f + h - \tau_{2,I}^*)] \\
& = (1 - \frac{\beta}{2})[(1 - \gamma - \rho)u(f + h - ah) + (\gamma + \rho)u(f)] \\
& + \frac{\beta}{2}[(1 - \gamma - \rho)u(f + ah) + (\gamma + \rho)u(f + h)];
\end{aligned} \tag{10}$$

and:

$$\begin{aligned}
& (1 - \frac{\beta}{2})[(1 - \gamma + \theta)u(f + h - \tau_{2,I}^*) + (\gamma - \theta)u(f + ah + \tau_{2,I}^*)] \\
& + \frac{\beta}{2}[(1 - \gamma + \theta)u(f + \tau_{1,I}^*) + (\gamma - \theta)u(f + h - ah - \tau_{1,I}^*)] \\
& = (1 - \frac{\beta}{2})[(1 - \gamma + \theta)u(f + h) + (\gamma - \theta)u(f + ah)] \\
& + \frac{\beta}{2}[(1 - \gamma + \theta)u(f) + (\gamma - \theta)u(f + h - ah)].
\end{aligned} \tag{11}$$

Proposition 2: Suppose that $\gamma > \theta$ and full insurance provision is not achieved. Then, when insurance is still provided, equilibrium transfers will decrease as θ or a increases, and increase as ρ increases. Moreover, equilibrium transfers will be crowded out by formal insurance: $\tau_{1,N}^* > \tau_{1,I}^*$, and $\tau_{2,N}^* > \tau_{2,I}^*$.

Proposition 2 implies the existence of the crowding-out effect when the insurance is in place, which is consistent with the findings of Lin et al. (2014). Because of this crowding-out effect, the comparison of $\tau_{i,I}^*$ and $\tau_{i,0}^*$ may be ambiguous. If $\tau_{i,0}^* \geq \tau_{i,N}^*$, then we know with certainty that $\tau_{i,0}^* > \tau_{i,I}^*$. However, if $\tau_{i,0}^* < \tau_{i,N}^*$, then $\tau_{i,I}^*$ can be larger or smaller than $\tau_{i,0}^*$, depending on the value of a . In particular, $\tau_{i,0}^* > \tau_{i,I}^*$ occurs when a is relatively large (the crowding-out effect is relatively large), and the opposite occurs when a is relatively small (the crowding-out effect is relatively small).