Risk Pooling, Commitment, and Information:

An experimental test of two fundamental assumptions*

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Abstract

This paper presents rigorous and direct tests of two assumptions relating to limited commitment and asymmetric information that underpin current models of risk pooling. A specially designed economic experiment involving 678 subjects across 23 Zimbabwean villages is used to solve the problems of observability and quantification that have frustrated previous attempts to conduct such tests. I find that more extrinsic commitment is associated with more risk pooling, but that more information is associated with less risk pooling. The first of these results accords with our expectations and assumptions. The second does not. I offer two explanations as to the origin of the second result and discuss their implications for how we view the assumptions made elsewhere in the literature. I also conduct a test of the relevance or external validity of the experimental results to our understanding of *real* risk pooling behaviour. In four out of the five villages for which the test could be conducted the networks of risk pooling contracts constructed during the experiment and the networks existing in real life were significantly correlated.

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1. Introduction

Over four fifths of the world's population do not have access to formal insurance against income and consumption shocks (Holtzmann, Packard & Cuesta, (2000)). Recent insights into how these people prepare for and cope with such shocks are based on the application of game and contract theory to informal risk pooling arrangements, informal credit, and reciprocal gift giving (e.g., Coate and Ravallion (1993), Fafchamps (1998)) and on the increasingly innovative empirical studies motivated by the resulting theoretical models. From these empirical studies we have learnt that full risk pooling is rare (e.g., Ravallion and Dearden (1988), Morduch (1991), Ravallion and Chaudhuri (1991), Alderman and Paxson (1992), Townsend (1994), Ligon (1998)), that some types of shocks are more likely to be informally insured than others, that risk pooling is associated with certain forms of relationship (e.g., Grimaud (1997), Fafchamps and Lund (1997), De Weerdt (2002), Dekker (2003)), and that some theoretical models explain the data more effectively than others (Ligon, Thomas and Worrall (2002)).

But are our insights well founded? There are a series of assumptions underpinning all of this work that have yet to be explored and tested directly. These relate to: the origins and extent of commitment and its effects on risk pooling; and the degree to which information is asymmetric and the effects of this asymmetry on risk pooling. If these assumptions are ill founded so may be the insights and conclusions we have drawn from the analyses cited above.

The assumption that limited commitment leads to less risk pooling is true by definition if, following Platteau (1994a, 1994b), Fafchamps (1992, 1996), Posner and Rasmusen (1999), Kreps (1997) and others, we acknowledge that both extrinsic incentives, especially the sanctions that can credibly be threatened, and intrinsic motivations, altruism, inequality aversion and reciprocal kindness, can act as bases for commitment. However, theoretical models of risk pooling focus exclusively on the former and we have yet to distinguish effectively between the two empirically. Kinship, co-ethnicity, shared clan membership, and religious co-affiliation are all statistically associated with flows of assistance (Grimaud (1997), Fafchamps and Lund (1997), De Weerdt (2002), Dekker (2003)), but is this because such relationships facilitate the effective use of extrinsic incentives or because they are associated with relation-specific forms of intrinsic motivation?

This ambiguity is problematic. For policy-makers the relative importance of extrinsic incentives and intrinsic motivations as determinants of behaviour has implications for the potential success of certain interventions. A carefully planned reform of an informal, grass-roots institution, that is predicted to improve welfare under the assumption that people respond to extrinsic incentives, could crowd out cooperative behaviour and lead to reductions in welfare if, in fact, people are primarily intrinsically motivated (Cardenas, Stranlund, and Willis (2000), Bohnet, Frey, and Huck (2001)). For theorists and empirical researchers the relative importance of extrinsic and intrinsic bases for commitment may also have implications for the effect of information asymmetries on the extent to which risk is pooled. If flows of assistance in times of need are intrinsically motivated is irrelevant and information asymmetries will have no bearing on levels of risk pooling.

Even if extrinsic incentives are required to support risk pooling arrangements, it is possible to conceive of scenarios under which more rather than less information leads to less risk pooling. Suppose that, as the experiments of Falk, Fehr, and Fischbacher (2000), Bowles, Carpenter and Gintis (2001), and Barr (2002) show, social or shame-based sanctions can be used to enhance commitment and that such sanctions are commonly threatened and applied. Also suppose that people can be and know that they can be tempted into behaving in a manner that will attract such sanctions when the immediate individual returns to doing so are high enough, but also know that they will suffer regret if they are tempted, exposed, and shamed in this way. Then, an increased likelihood of detection or exposure could lead to less risk pooling as it is only by *not* entering into such arrangements that people can ensure that they will not be tempted to renege and suffer the shameful consequences. Using Elster's (1984) terminology, choosing not to enter into risk pooling arrangements could be a form of pre-commitment.

Alternatively suppose that, instead of questioning their own willpower and fearing shame and regret, people are conflict averse, in much the same way that Harowitz (2001) assumes countries to be. They fear reneging by others because they suffer not only the material consequences of being cheated but also the psychological consequences of conflict. If the second concern dominates, an increased likelihood of detection could lead to less risk pooling as *not* entering into such arrangements is the only way to ensure that one will not end up in conflict with a renegade. Such conflict aversion might also cause people to put less effort into gathering information even when it is relatively easy or costless to do so. In other words, they might choose to 'turn a blind eye'. But this, along with the recent work of Boozer and Goldstein (2003) on information asymmetries within households, calls into question the assumption often made by theorists, that within the tightly knit social groups that are likely to engage in risk pooling, information is unlimited.

The reason why these assumptions have not been fully explored and directly tested relates to problems of observability and quantification. Such an analysis requires data on how much risk is pooled, the degree of asymmetry in information, the extent to which extrinsic commitment is limited, and maybe even the incidence and content of intrinsic motivations. It has proven very difficult to generate these data, especially relating to commitment, using a survey-based approach. Here, I solve these problems by running an economic experiment that is novel in two regards. First, it is designed specifically for the task, making it the first risk pooling experiment to be run anywhere. And second, it is designed to support a formal test of its own external validity, i.e., its relevance to our understanding of *real* risk pooling behaviour.

Within the experiment, subjects were invited to pool risk under three treatments that varied with respect to the degree to which information is asymmetric and extrinsic commitment is limited. (Section 2 provides a detailed description of the experimental design.) The data generated during the experiment was used to test two hypotheses: (1) that when extrinsic commitment is limited less risk pooling occurs; and (2) that when information is asymmetric less risk pooling occurs. The results of the experiment support hypothesis (1). However, even though there is more reneging on pooling agreements when information is asymmetric, hypothesis (2) is rejected. Information asymmetries lead to more rather than less risk pooling. (Section 3 presents the analysis.)

To maximize the external validity of the experiment two unusual steps were taken: first, the experimental subjects were sub-Saharan villagers rather than students at a western university; and second, the risk pooling decisions within the experiment were made not anonymously with strangers via computer terminals or pencils and paper but face-to-face with fellow villagers. These steps also prepared the way for the test of external validity, to the author's knowledge, the first formal test of external validity ever to be carried out. The test, which was conducted in five out of the 23 villages, involved a comparison of the networks of risk pooling contracts formed within the context of the game with the networks of risk pooling contracts in real life. Using a technique developed by social network analysts I found that in four out of the five villages the experimental and real risk pooling networks were significantly correlated. (Section 4 describes the test, the data used, and the results.)

The paper concludes with a review of the assumptions commonly made by researchers working on risk pooling in the light of the experimental results (section 5).

2. Experimental Design

The experimental design has four components; the first is the choice of the subjects; the second involves the creation of a risky decision-making environment; the third is the provision of an opportunity to pool risk; and the fourth involves varying the degrees to which information and commitment are limited across subjects.

Choice of subjects

Most economic experiments are conducted in specially designed laboratories with university students as subjects. Usually, the subjects are assumed not to know one another and, often, their identity is kept private throughout the experiment. But here my objective is to simulate key aspects of the situations within which people in developing countries often but not always choose to pool risk. This objective can best be met by working with people in developing countries within the social groupings that are most relevant to their usual risk pooling endeavours. Thus, instead of university students my subjects are Zimbabwean villagers and instead of calling the subjects to a laboratory I take the experiment to their village meeting places. The results presented below relate to experimental sessions in 23 villages. A total of 678 subjects were involved in the experiment, although only 642 took part in the second, more interesting round and only 618 took part in both the first and second round. This attrition and replacement occurred because in each village the experiment took place over two days and some of the subjects were called away on unforeseen business on the second day.

Creating a risky decision-making environment

I created a risky decision-making environment by conducting a near-replication of Binswanger's (1980) risk aversion experiment. Each subject was confronted with a choice of six gambles. Every gamble yielded either a high or low payoff each with probability 0.5. Whichever gamble was chosen, the payoff was determined by playing a game that involved guessing which of the researcher's hands contained a blue rather than a yellow counter.[1] If the subject found the blue counter he or she received the high payoff associated with the gamble of his or her choice. If he or she found the yellow counter, he or she received the low payoff associated with that gamble. The six gambles, drawn from Binswanger's set of eight, are presented in Table 1.[2] Each is associated with a letter used to identify it during both play and the analysis. In Table 1, the column labelled 'Blue' shows the high payoffs associated

[1] Binswanger used coin tossing as the randomising device. However, in Zimbabwe women did not know how to toss and many men thought they could cheat and focused on this rather than on other aspects of the game. The which-hand-is-it-in game, known as Chigigaro in Shona, is played by grandmothers with their grandchildren and by children together throughout Zimbabwe. Thus, we can reasonably assume that the probabilities in the game are well understood.

[2] The two inefficient gambles, introduced by Binswanger in order to explore subject's understanding of the game, were left out.

with each gamble in Zimbabwean dollars. The column labelled ‘Yellow’ shows the corresponding low payoffs, also in Zimbabwean dollars. The expected values, risk aversion classes as defined by Binswanger and ranges of partial relative risk aversion coefficients associated with each gamble are also shown. The gambles were presented to the subjects, many of whom were illiterate, as pictures on a specially designed card (see Figure 1). Each gamble was depicted as two piles of money, one (the high payoff) on a blue background and the other (the low payoff) on a yellow background. The subject had to pick the hand that had the blue counter in order to secure the high payoff on the blue background.

At the time of the experiment, one day of casual labour would have earned a villager approximately 200 Zimbabwean dollars, although, such work opportunities were scarce. The official exchange rate was 50 Zimbabwean dollars to one US dollar.

In each village the subjects were called to a meeting, having been told that this was to be the first of two meetings to be held on consecutive days. Once the research team had been introduced, each subject met one of four research assistants in private. They were taught the gamble choice game, their comprehension was tested, and then they played. They were paid and then asked to sit separately from those who had not yet played to await further instructions. Tea and snacks were served. Once everyone had played, the next stage of the experiment was explained.

Introducing the possibility of risk pooling

The instructions then issued to the subjects varied across the villages (see Table 2). In three of the villages the subjects were told that during the meeting scheduled for the next day, they would be playing the same game once again. They were given copies of Figure 1 to aid any discussions they wished to hold and told the arrangements for the following day. This treatment is referred to as T1 below.

In eight of the villages, having been told that they would be playing the same game once again, they were informed that they could, if they wished, form 'sharing groups' within which all winnings from the game would be shared equally. It was left up to the subjects whether or not they joined a group and, if they did, how large the group was to be. The monetary implications of sharing-group-formation were explained with the aid of simple examples. Once again the subjects were given copies of Figure 1 to aid any discussions they wished to hold and told the arrangements for the following day. This treatment is referred to as T2 below.

Varying the asymmetry of information and the limits on extrinsic commitment

Under T2 the subjects could not renege on the risk pooling arrangements they made amongst themselves prior to the second round of games, i.e., extrinsic commitment was perfect. In contrast, in treatments T3 and T4 reneging is possible, i.e., extrinsic commitment was limited, and information was variably asymmetric. In six villages, having been told that they could form sharing groups, the subjects were told that they could, if they wished, opt out of their sharing groups in secret after they knew the outcome of their own gamble. Once again the subjects were given copies of Figure 1 and told the arrangements for the following day. Under this treatment, T3 below, commitment is limited and information is asymmetric. In the remaining six villages, the subjects were told that they could opt out of their sharing groups after they knew the outcome of their own gamble, but only if they were prepared to do so by raising their hand in front of the whole meeting when the question was asked. Under this treatment, T4 below, information is less asymmetric than under T3.

The data

The experiment generates two indicators of the amount of risk pooling that the subjects are undertaking. One relates to the subject's decisions about group formation. If a subject joins a group containing n subjects in total, he or she is making n-1 risk pooling contracts. The other relates to their choice of gamble. *Ceteris paribus*, if the members of a sharing group select more risky gambles, they can be said to be pooling more risk in anticipation of a higher expected return. By comparing these two indicators across treatments we can investigate the effects of limited extrinsic commitment and asymmetric information on risk pooling.

3. Results

Do subjects renege when commitment is limited?

Before turning to the analysis of risk pooling, it is useful to look at reneging behaviour in the two treatments under which it is possible. Caution is required here as subjects could only renege if they were socially selected into a sharing group: a subject might not be selected if he or she is expected to renege. Thus, the statistics presented will be subject to selection bias.

Figure 2 describes the decisions made by those subjects that chose to join groups under T3 and T4. Under T3, 64 of 157 subjects joined sharing groups. Of these, 32 won high payoffs on the gambles of their choosing and of these 7 (22 percent) reneged by opting out of their sharing group in private. All of the subjects who won low payoffs stayed in their groups. Under T4, 66 of 183 subjects joined sharing groups. Of these 34 won high payoffs on the gambles of their choosing and of these only one (three percent) reneged by opting out of her sharing group in public.

All of the subjects who won low payoffs stayed in their groups. The proportion of renegades among high payoff winning group members differs significantly (five percent level) between the treatments, being lower when the reneging has to be done publicly. This suggests that subjects do fear social sanctioning when information is less limited.

Group formation

Figure 3 graphs the size of groups that each of the subjects in T2, T3, and T4 joined prior to the second round of the game. To aid comparisons between the treatments, relative frequencies are shown. A group size of one indicates that the subject played solo, i.e., not in a sharing group. Thus, a direct comparison of the bars relating to each of the treatments on the far left of the figure is highly informative. Only 31 percent of subjects did not join sharing groups under T2 as compared to 59 and 68 percent under T3 and T4 respectively. Turning to group size, it is in T2 that we see the largest group, a group of twelve. Groups of ten are a focal point due to the examples given during the instruction of the subjects. We see groups of 10 under both T2 and T3. It is under T4 that group size is most restricted. No groups of more than five were formed under this treatment.

In order to test the statistical significance of these variations, while controlling for other factors that may have influenced the subjects' decisions, I conduct a regression analysis. Table 3 presents the results of a probit analysis of whether subjects joined a group and then a linear regression analysis of group size conditional on group membership. In each case the standard errors are corrected to take account of the potential non-independence of errors within villages due to group formation being a social process.

In the first column of the table, the dichotomous variable that takes the value one if the subject joined a sharing group and zero otherwise is regressed on the following: two treatment dummies (T2 is the basis for comparison); five dummies relating to the choice of gamble that the subject made in the first round, included to control for how risk averse the subject is; a dummy indicating whether the subject won the high payoff in the first round; the number of households in the village, which corresponds to the maximum number of subjects attending the session; two dummies relating to the geographical area within which the subjects' village falls, and one dummy distinguishing resettled villages from non-resettled villages. Only the coefficients on the two treatment dummies are significant and only these survive a general to specific process of elimination taking the ten percent level of significance as a cut-off. Under both T3 and T4 subjects are significantly less likely to join a group than under T2.

If the size of the group that the subject joins is regressed on the same set of explanatory variables only one of the treatment dummies, the one relating to T4, is significant. The coefficient on this dummy variable is also significantly different from the one on the treatment dummy relating to T3. In addition, three of the other variables are significant in this model. First, subjects who won the high payoff associated with their choice of gamble in the first round joined smaller groups.[3] Second, subjects in resettled villages formed larger groups. And third, subjects in Area 2, which has the poorest soil and least reliable rainfall, form larger groups,

[3] This is not an income effect. When used in place of the high payoff dummy, actual winnings from the first round were not significant in the group formation regressions. Many of those who won high payoffs in the first round described how they avoided forming groups with those who received only low payoffs, while many of those who won low payoffs in the first round expressed a desire to be in groups with high payoff winners and disappointment at having to settle for being in a group of 'loosers' even though this was preferable to playing solo. This suggests that the subjects did not see the two rounds of gambles as independent probabilistic events. Such failures of expected utility theory have been observed in many contexts and should not come as a surprise here.

although the area dummies did not survive in the general to specific process of variable elimination.

Choice of gamble

Figure 4 depicts the cumulative frequencies for the subjects' choices of gamble under T1, T2, T3, and T4. The most striking feature of these plots is the greater tendency for subjects under T2 to choose the riskier gambles. Over one quarter of the subjects under T2 chose gambles E or F as compared to between 10 and 13 percent of subjects under the other treatments. Further, only seven percent of subjects under T2 chose gambles A or B as compared to between 14 and 17 of subjects under the other treatments.

A more careful analysis of gamble choice under the four treatments must take account of each subject's decision in the first round, thereby allowing us to focus on the extra risk that the subjects take on when pooling is an option. Figure 5 contains four transition matrices, one for each of the treatments. The numbers within the matrices are percentages. The *ij*th element of a matrix is the percentage of those subjects who chose gamble *i* in the first round and gamble *j* in the second round. The frequencies associated with the first round choices are shown beside the matrices. To aid the reader the cells of the matrices have been shaded in accordance with the percentage they contain. Darker cells contain higher percentages. When the high percentages are concentrated down the principle diagonal it indicates that most subjects chose the same gamble in both rounds. When the high percentages are concentrated in the middle two columns it indicates that the subjects tended towards the middle-of-the-range gambles in the second round regardless of their choices in the first round. If there are medium to high percentages in the bottom left-hand corner of a matrix it indicates that subjects chose less risky gambles in the second round. And if there are medium to high percentages in the top right-hand corner of a matrix it indicates that subjects chose riskier gambles in the second round.

The matrices for T1, T3, and T4 display quite dominant principle diagonals combined, to varying degrees, with dominant middle columns. The option to risk pool under T3 and T4 appears not to be causing subjects to choose riskier gambles. In contrast, the matrix for T2 is shaded in the top right-hand corner indicating that a considerable proportion of subjects chose riskier gambles in the second round. In Figure 6 the matrix for T2 is further sub-divided into those who did and did not join groups. It is in the former that we see the concentration in the top right-hand corner.

Once again, we can use a regression analysis to test the significance of these regularities in the data, while controlling for other factors that might have influenced the subjects' decisions. Here, I run a tobit regression that takes account of the fact that the gamble choice sorts the subjects into intervals according to how much risk they are prepared to take on. The tobit was set up with reference to the bounds on the risk aversion coefficients associated with each gamble and presented in Table 1. Thus, if an explanatory variable bears a significant negative coefficient it indicates that an increase in that variable is associated with an increase in the riskiness of the chosen gamble. The results of the analysis are presented in Table 4. In the first column the subjects' choices in the second round are regressed on a set of five dummy variables indicating their choices of gamble in the first round (A is the base for comparison), their winnings from the first round,[4] and a set of six dummies indicating which treatment the decision was made under and whether they opted to be in a group or play solo (T1 is the base for comparison). All standard errors are adjusted to take

[4] The large coefficient on the winnings variable may be indicative of an income effect or may relate to the perceived non-independence of gambles described in footnote 3.

account of the potential non-independence of errors due to the social context within which the decisions were made.

The dummies relating to choice of gamble in the first round are jointly, highly significant. Those who won more in the first round assume greater risks in the second. The dummies relating to being in a group under T2, T3, or T4 are jointly significant, while those relating to playing solo under T2, T3, or T4 are not significant. A careful general to specific process yields the regression presented in the second column of Table 4. In this regression, of the treatment-group dummies, only the one identifying those who belonged to a group under T2 survives. The choices made under T3 and T4 or by solo players under T2 were indistinguishable from those made under T1.

4. A Test of External Validity

While this experiment was underway, Dekker (2002) was conducting an in-depth study of flows of assistance between households in five of the 23 villages. As part of her fieldwork Dekker asked every household in these villages to name the households in their village to whom they had given assistance and from whom they had received assistance. Here, I use Dekker's data to investigate whether the groups formed during the experiment bear any resemblance to real risk pooling networks within these five villages.[5]

Dekker's data takes the form of relational matrices. The *ij*th element in the 'assistance given' matrix for a particular village takes the value one if household *i* reported giving assistance to household *j* and zero otherwise. Similarly, the *ij*th

[5] There is no reason to expect that the five villages in Dekker's study represent a biased sample of the 23 included in the experiment. In her choice of villages, Dekker's aim was to capture as much social diversity as possible while controlling for access to roads, other infrastructure and social services. There is a possibility that Dekker presence in the villages could have biased behaviour in the experiments. However, Dekker had not yet started work in two out of the three villages at the time when the experiments were conducted.

element in the 'assistance received' matrix for a particular village takes the value one if household *i* reported receiving assistance from household *j* and zero otherwise. With no measurement error, one of these matrices should equal the transpose of the other. This is not the case and, consistent with there being a stigma associated with needing or receiving assistance and/or high status associate with giving assistance, many more relations involving the giving of assistance are reported, i.e., there are many more ones in the 'assistance given' matrix than in the 'assistance received' matrix. For this reason, and because there is no way of telling which of the two matrices is a better reflection of the actual pattern of transfers, the procedure described below is repeated twice for each village, once using the 'assistance received' matrix, and once using the 'assistance given' matrix.

To facilitate the analysis another matrix relating to group formation within the experiment is constructed for each of the five villages. In these matrices the *ij*th element takes the value one if the representative from household *i* in the experiment was in the same group as the representative from household *j* and zero otherwise. Then, the statistical correlations between the experimental group formation matrix and the corresponding 'assistance given' and 'assistance received' matrices are calculated. A standard correlation assuming independence is not appropriate as each matrix represents a fully enumerated and interrelated social system. Instead, the following two-stage procedure was used. First, a simple matching coefficient between corresponding cells in the two data matrices is calculated. Second, the rows and columns of one matrix are randomly but synchronously permutated and the correlation recomputed every time. 10,000 permutations are carried out and the proportion of times that the match between the one observed and one permutated matrix is greater than or equal to the match between both observed matrices is calculated. This proportion is reported as the level of significance or p-value of the correlation.

The test is subject to two sources of bias both of which may suppress the significance of a correlation and thereby render the test conservative. First, in Dekker's data an actual flow of assistance is observed only if one household is in need and another is both willing and able to respond, whereas in the experiment only the condition of willingness needs to be met in order for two subjects to become linked by group membership. Dekker's data implies an *ex post* flows of assistance while experimental co-group membership implies an *ex ante* informal contract relating to a flow of assistance if unequal payoffs arise. Thus, the experimental group matrices are likely to contain many more ones than either of the 'assistance' matrices. This suppresses the significance of the correlations and renders it more likely that the denser 'assistance given' matrices will yield the significant results.

The second potential source of bias arises if the subjects in the experiment and those making real life risk pooling decisions are distinct. Out of the five villages for which the test was conducted there was one in which this bias may have arisen. In Pedzanhamo a large proportion of the experimental subjects were young dependents rather than senior household members.

The results of the correlation exercise are reported in Table 5. In three out of the five villages, Moturamhepo, Zvataida, and Mudzinge, the correlation between group formation in the game and patterns of assistance reported as given is significant. For one of the other villages, Muringamombe, the correlation between group formation in the game and patterns of assistance reported as received is significant. For the fifth village, Pedzanhamo, no correlation is found between reported assistance, given or received, and group formation in the game.

5. Discussion

The results indicate that more risk pooling takes place when extrinsic commitment is unlimited; under T2 more subjects join sharing groups and group members take on greater risk. However, even then a considerable proportion of subjects choose not to join groups. One possible explanation is that these subjects are risk loving, although if this were the case we would expect to see significant coefficients on the choice of gamble dummies in the probit regressions. An alternative explanation is that they are being punished by exclusion for previous misdemeanours.

When extrinsic commitment is limited and information is asymmetric less risk pooling takes place; in T3 and T4 a smaller proportion of subjects join groups and group members take on no more risk than solo players. This result supports the commonly made assumption that limited extrinsic commitment reduces risk pooling. However, note that some subjects are not deterred from forming groups under such conditions suggesting that intrinsic motivations may be a sufficient basis for commitment for some agent dyads.

Not in accordance with commonly made assumptions is the finding that asymmetric information is associated with more rather than less risk pooling; the information asymmetries are greatest in T3 but it is under T4 that the lowest proportion of subjects join groups and that the smallest groups are formed. This finding suggests that individual responses to socially sensitive information might be more complex than we usually acknowledge. People may be conflict averse or may be highly motivated by anticipated shame and may not always trust themselves to act in way that minimises their likelihood of attracting shame. While the current experiment was not designed to distinguish between these hypotheses, it suggests that they might be usefully explored. Further, the result suggests that the full information within social groups such as villages is ill founded, that given the opportunity people may actually shy away from information, and that there might be a negative relationship between information and commitment. Knowing when people renege may make relations too difficult, there may be value in being able to give people the benefit of the doubt in some circumstances. This challenges the verisimilitude of many of our theoretical models and the conclusions based on the related empirical work.

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Table 1: First round of the risk pooling game: Choices, payoffs, expected values, and risk aversion classes

Choice	Blue	Yellow	EV	RA class	RA coeff.
A	100	100	100	Extreme	infinity to 7.51
B	190	90	140	Severe	7.51 to 1.74
C	240	80	160	Intermediate	1.74 to 0.81
D	300	60	180	Moderate	0.81 to 0.32
E	380	20	200	Slight-neutral	0.32 to 0.00
F	400	0	200	Neutral-negative	0 to -ve infinity

Table 2: Second round treatments in the risk pooling game

Treatment	Description	Villages	Subjects
T1	Repeat of first round after aproximately 24 hours for players' discussions.	3	86
T2	Players told that they could form sharing groups if they wished and given approximately 24 hours to do so.	8	216
T3	Like T2 but players also told that once they had played they could opt out of their groups in secret if they wished.	6	157
T4	Like T3 but players must declare their decision to opt out of their group in public.	6	183
Total number of players involved in Round 2		23	642

Table 3: Group membership and group size

Dependent variables = dummy taking the value 1 if in a group and number of group members

	Probit of group formation				Group size conditional on formation			
	Coef.	s.e.	Coef.	s.e.	Coef.	s.e.	Coef.	s.e.
Constant	0.430	0.670	0.625	0.165 ***	2.934	1.556 *	4.514	0.811 ***
T3	-0.891	0.417 **	-0.859	0.391 **	-0.428	0.623	-0.244	0.776
T4	-0.984	0.204 ***	-0.982	0.212 ***	-3.737	0.515 ***	-3.287	0.575 ***
B (round 1)	0.297	0.257			-0.266	0.386		
C (round 1)	-0.102	0.255			-0.003	0.462		
D (round 1)	0.182	0.266			0.239	0.426		
E (round 1)	0.375	0.411			1.040	0.978		
F (round 1)	0.034	0.353			0.239	0.980		
Blue in round 1	-0.166	0.146			-0.457	0.244 *	-0.400	0.234 *
H'holds in village	-0.002	0.012			0.042	0.031		
Area 2	0.450	0.276			1.811	0.857 *		
Area 3	0.389	0.260			0.964	0.687		
Resettled	0.053	0.371			1.686	0.432 ***	1.396	0.444 *
Joint significance of round 1 choice	0.155				0.766			
Joint significance of areas	0.200				0.120			
R^2	0.117		0.091		0.350		0.328	
Obs.	568		592		260		260	

Note: Reported standard errors have been adjusted to take account of potential non-independence within villages/sessions. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 4: Choice of gamble in second round

Dependent variable = riskiness of gamble as indicated by corresponding coefficient of partial relative risk aversion

	Coef.	s.e.		Coef.	s.e.	
Constant	1.700	0.318	***	1.942	0.368	***
Winnings in round 1	-0.003	$6.02e^{-4}$	***	-0.003	$5.84e^{-4}$	***
B (game 1)	-0.267	0.318		-0.269	0.317	
C (game 1)	-0.287	0.334		-0.306	0.332	
D (game 1)	-0.058	0.368		-0.061	0.372	
E (game 1)	-0.911	0.350	***	-0.864	0.334	***
F (game 1)	-0.530	0.527		-0.571	0.539	
Treatment 2 & in group	-0.346	0.236		-0.579	0.268	**
Treatment 3 & in group	0.308	0.194				
Treatment 4 & in group	0.597	0.402				
Treatment 2 & solo	0.110	0.251				
Treatment 3 & solo	0.197	0.181				
Treatment 4 & solo	0.260	0.160				
σ	1.512	0.142		1.521	0.145	
Joint sig of game 1 choice	0.000			0.000		
Joint sig of treatment & in a group dummies	0.056					
Joint sig of treatment & solo dummies	0.364					
Wald χ^2	70.59			64.32		
Obs.	618			618		

Note: Reported standard errors have been adjusted to take account of potential non-independence within villages/sessions. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 5: Correlations between experimental and real risk pooling networks

Village	Correlation with reported assistance given (p-value)	Correlation with reported assistance received (p-value)
Pedzanhamo	0.786	0.631
Moturamhepo	0.056	0.956
Zvataida	0.053	0.310
Muringamombe	0.151	0.099
Mudzinge	0.065	0.268

Figure 1: Presentation of the gambles to the experimental subjects

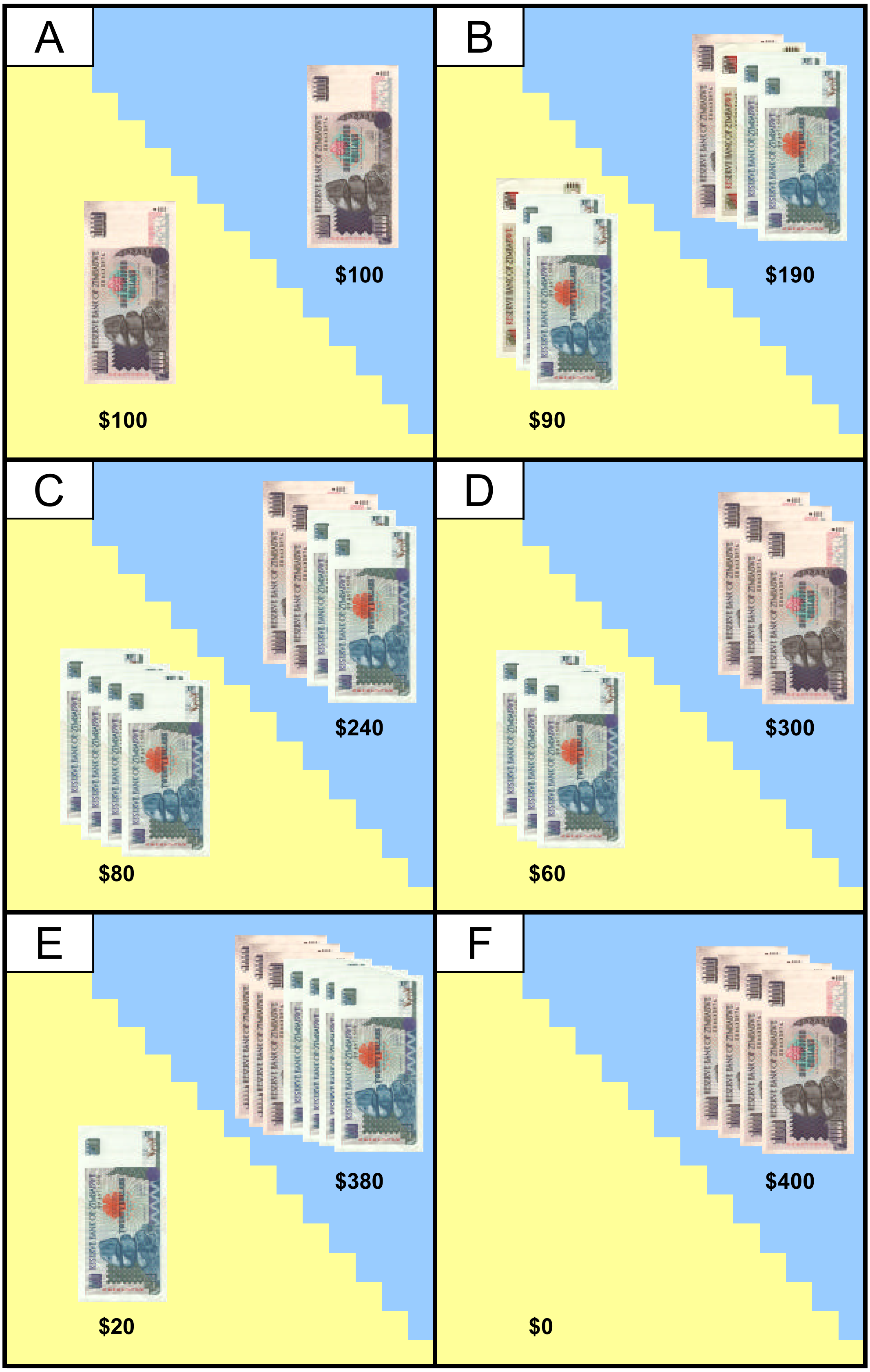

Figure 2: Limited commitment and renegades in Treatments 3 and 4

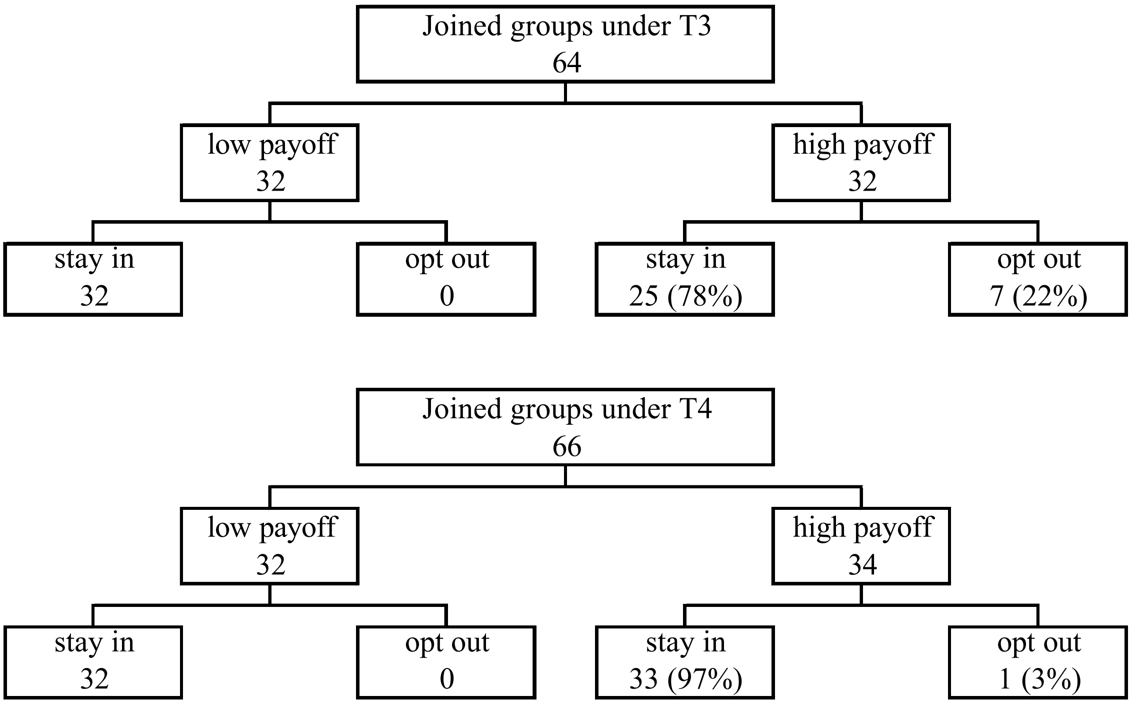

Figure 3: Group formation in second round of the risk pooling game

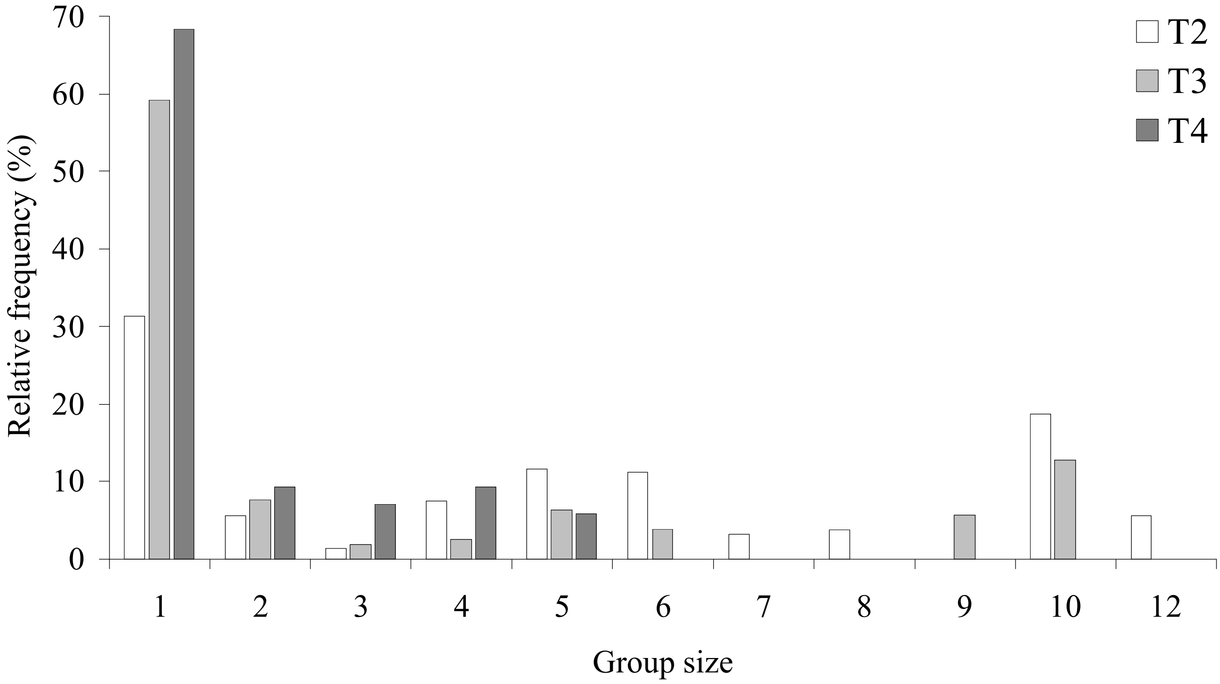

Figure 4: Choice of gamble in second round of the risk pooling game

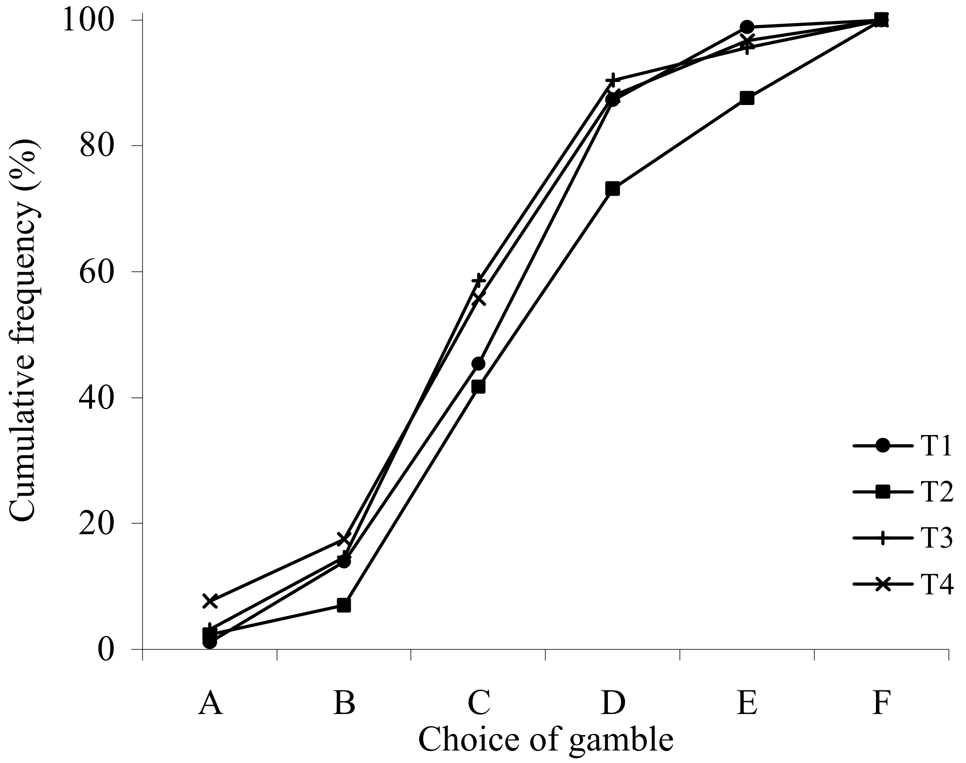

Figure 5: Comparison of gambles chosen in first and second rounds

Treatment 1 (T1)

Choice in round 1 \ Choice in round 2	A	B	C	D	E	F	Subs.
A	0	20	20	40	20	0	10
B	0	13	27	53	7	0	15
C	0	8	42	46	4	0	24
D	3	14	31	34	14	3	29
E	0	0	0	50	50	0	2
F	0	17	33	33	17	0	6

Treatment 2 (T2)

Choice in round 1 \ Choice in round 2	A	B	C	D	E	F	Subs.
A	18	12	12	18	12	29	17
B	0	0	41	27	14	18	22
C	1	6	40	31	13	9	80
D	1	4	37	37	14	7	73
E	0	0	20	30	30	20	10
F	0	0	0	38	13	50	8

Treatment 3 (T3)

Choice in round 1 \ Choice in round 2	A	B	C	D	E	F	Subs.
A	0	6	50	28	6	11	18
B	5	19	38	29	5	5	21
C	0	10	51	36	3	0	39
D	5	16	39	30	5	4	56
E	0	0	38	38	13	13	8
F	11	0	33	33	11	11	9

Treatment 4 (T4)

Choice in round 1 \ Choice in round 2	A	B	C	D	E	F	Subs.
A	20	10	15	45	5	5	20
B	8	16	24	40	8	4	25
C	4	4	53	28	7	5	57
D	6	13	43	28	11	0	54
E	0	0	20	60	20	0	10
F	0	20	40	20	0	20	5

Figure 6: Comparison of gambles chosen in first round and second round under Treatment 2

Treatment 2 (T2) not in groups

Choice in round 1 \ Choice in round 2	A	B	C	D	E	F	Subs.
A	33	0	17	33	0	17	6
B	0	0	57	43	0	0	7
C	0	4	48	33	15	0	27
D	6	6	28	50	11	0	18
E	0	0	50	25	0	25	4
F	0	0	0	50	0	50	2

Treatment 2 (T2) in groups

Choice in round 1 \ Choice in round 2	A	B	C	D	E	F	Subs.
A	9	18	9	9	18	36	11
B	0	0	33	20	20	27	15
C	2	8	36	30	11	13	53
D	0	4	40	33	15	9	55
E	0	0	0	33	50	17	6
F	0	0	0	33	17	50	6