INVESTMENT BEHAVIOUR, RISK SHARING AND SOCIAL DISTANCE

short title: INVESTMENT BEHAVIOUR AND RISK SHARING*

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Using a lab-in-the-field experiment in Uganda we study how risk sharing influences investment behaviour. Depending on the treatment, an investor may decide to share profits with a paired person, and/or the paired person may compensate the investor for investment losses. Following sharing norms in African societies, predicted investment is higher if loss sharing is possible, and/or profit sharing is not possible. Contrary to these predictions, we find that investment is higher when losses may not be shared or when profits may be shared with friends. A combination of directed altruism and expected reciprocity appears most plausible to explain these results.

For sustained economic growth to be achieved in a society, conditions need to be in place that ensure that risk taking by entrepreneurs is balanced. In particular, incentives entrepreneurs face need to be such that risk taking is stimulated, but not excessively so, since excessive risk taking burdens society with economic shocks and losses. An important way in which balanced risk taking may be encouraged is through the redistribution of profits and losses resulting from investments between entrepreneurs and the rest of society. Society's role and the incentives it provides have been at the centre of a debate in the aftermath of the 2008 financial crisis, in which *excessive* risk taking has been blamed for the crisis and it has been questioned whether society should assume the fallout of irresponsible risk taking.

By contrast, in African societies, it has been *too little* risk-taking investment that has kept growth subdued and poverty persistent, especially in rural areas

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(Collier and Gunning, 1999).[1] Similar questions to the ones recently posed in the West can be asked in rural Africa about the incentives society provides to entrepreneurs by redistributing the profits and losses resulting from investment. There are, however, important differences between western economies and rural societies in developing countries. In particular, formal state institutions are less present in African villages and society's influence is of a more informal nature, mostly through social networks in which semi-subsistence farmers are typically embedded. Fafchamps (2003) synthesises the evidence on the myriad ways in which risk coping mechanisms in such societies lead to underinvestment. One key way in which they do so is through sharing obligations. In sub-Saharan Africa, risk sharing is predominately organised through informal networks. The strong equality and redistributive norms present in these networks may hold back investment and accumulation, thereby contributing to the region's poor growth performance (Baland *et al.*, 2011; Jakiela and Ozier, 2012; di Falco and Bulte, 2011). Platteau (2009, p. 671) holds a 'universe of personalised relationships [...] throughout the region, with its attendant obligations and solidarity ties' responsible for corroding incentives to invest.

The literature on informal risk sharing in sub-Saharan Africa thus suggests that sharing obligations dampen incentives to invest; profits need to be shared with others. On the other hand, there is a potential positive effect of risk sharing on investment too, through the sharing of losses (or the promise thereof). Risk sharing in rural communities in the developing world has primarily been thought of and tested as enabling consumption smoothing when households are affected by income fluctuations that do not co-vary with fluctuations in the average income in the community, so-called idiosyncratic risk, so that only collective risk remains (e.g. Ravallion and Dearden, 1988; Townsend, 1994; Udry, 1994 and many subsequent related studies). Typically, such risk sharing is found to lead to an incomplete but substantial reduction of the sensitivity of the consumption of households to idiosyncratic risk, with recent research focusing on membership of risk-sharing networks, rather than community membership per se (Fafchamps and Lund, 2003; Kinnan and Townsend, 2012; Chiappori *et al.*, 2014). Kinship networks have also been found to facilitate access to finance, presumably because relatives borrow on an investor's behalf and/or kinship ties are strong enough for relatives to be credible guarantors (Kinnan and Townsend, 2012).

[1] A striking example of the level of underinvestment in rural sub-Saharan Africa is provided for Western Kenya by Duflo *et al.* (2008): whereas the average return on investment in fertiliser is close to 70 per cent per year, only about a third of farmers report having ever used fertiliser.

Importantly, when risks are pooled both profits and losses tend to be shared, and profit and loss sharing may, as mentioned, have different effects on investment propensities. These effects are at work simultaneously and the contribution of our paper is to isolate them. To disentangle the effects of profit and loss sharing we make use of an artefactual field experiment in rural Uganda, in which participants decide how much of an initial endowment to invest in a risky activity, simulated by a lottery game. Depending on the treatment, profits that may result from the investment can be shared with a paired person, and/or the paired person can compensate the investor for losses that the investment may give rise to. To analyse the role of social distance, we add within-subject variation by comparing pairs of anonymous and non-anonymous players, pairs of players from the same village and from different villages, and non-anonymous pairs from the same village that vary in real-life social ties and socio-economic characteristics.

With standard preferences, no treatment differences are predicted. When considering the strong sharing norms in African societies, investment is predicted to be higher if loss sharing is possible and/or profit sharing is not possible. Contrary to these predictions, we find that investment levels are actually higher when loss sharing is ruled out. This effect is particularly strong when the identity of the paired participants is revealed to each other, and (conditional on non-anonymity) stronger when an investor is paired with a wealthier or less risk averse person. We also find that investment levels are higher when profit sharing is possible and one is paired with a friend. Our findings are paradoxical as they contradict the predictions of the literature on informal risk sharing, reviewed above, on the respective effects of loss sharing and profit sharing on investment. We conclude the paper by pointing out that directed altruism and expected reciprocity may help explain these paradoxical findings.

Several studies are related to ours. A large number of studies have examined risky choice in developing countries with the use of experimental games (Binswanger (1980, 1981) in India; Barr and Genicot (2008) in Zimbabwe, Tanaka *et al.* (2010) in Vietnam, Liu (2013) in China, Cardenas and Carpenter (2013) in six Latin American capitals, and Humphrey and Verschoor (2004a,b); Mosley and Verschoor (2005); Harrison *et al.* (2010) in India, Ethiopia and Uganda). While most have treated risky choice as if taken in social isolation, in reality, people are embedded in social networks in which risks are pooled.

How this is done may influence people's propensity to undertake an investment. While a few recent studies have studied risky choice in lottery games with the option of risk pooling, the focus of these studies was primarily on the decision to join risk sharing groups and not on the risky choice itself (see, e.g., Barr and Genicot, 2008; Barr *et al.*, 2012; Attanasio *et al.*, 2012).[2] The focus of our study is on people's decision to expose themselves to risk and how that decision is influenced by different risk pooling options. An important innovation of our design is that we disentangle profit and loss sharing, by varying the options of profit sharing and loss sharing in a systematic way. We also look at interactions with social distance either induced experimentally or by relying on natural variation in socio-economic differences and social ties within our sample.[3]

The rest of the article is organised as follows. In Section 2, we present the research design, including the experimental game and procedures used as well as some theoretical considerations and hypotheses. In Section 3, we describe the data and present the empirical analyses. Section 4 reviews plausible explanations of our findings. Section 5 concludes the article.

1. Research Design

To answer the research questions, we make use of an artefactual field experiment with different treatments. In this section, we present the game as well as the theoretical predictions of treatment differences. We also present the procedures followed to implement the experiment.

1.1. The Game

The experiment consists of two parts. In Part 1, all participants receive an endowment E and decide how much of this endowment to invest in a risky asset x, with $x \in [0, E]$. The investment can either be successful, in which case the investment gets a return $r > 1$, leading to a payoff $E + (r-1).x$, or unsuccessful in which case the investment is lost and the payoff is $E - x$. The likelihood of a successful investment is s, which is given, with $0 < s < 1$. As a result, each participant chooses her preferred prospect $F = (s, E + (r-1).x; (1-s), E - x)$ by setting x. The decisions made in Part 1 are used

[2]Other studies have combined lottery games with a redistribution stage to study fairness views about risk-taking (e.g. Cappelen *et al.*, 2013), but here again the focus was not on the risky choice decision itself.

[3]Some studies have studied the effect of social distance on resource sharing, but have not linked it to risky choice (cf. Leider *et al.*, 2009; Goeree *et al.*, 2010; Brañas-Garza *et al.*, 2010)

to elicit individual risk preferences.[4]

In Part 2 all participants again receive an endowment E and are paired, with each pair having a Player 1 and a Player 2.[5] Player 1 again makes a decision about a risky investment with the same parameters as in Part 1. If the investment is successful, she has the option to share the gains with Player 2; if unsuccessful, Player 2 has the option to share his endowment with Player 1 up to the losses incurred. Both sharing decisions are taken simultaneously; hence neither player is informed about the other's sharing decision. Sharing decisions are also made before the resolution of the lottery, so that we can capture sharing decisions for both the scenario of a successful and that of an unsuccessful investment. Several treatments are organised. Whereas in treatment T1, profits can be shared by Player 1 and Player 2 can compensate Player 1 for some of the losses, in treatment T2 only profits can be shared and in treatment T3 only losses can be shared.[6]

In addition to this between-subject design, we add within-subject variation in social distance by pairing Players 1 successively with five different Players 2. Whereas in the first two pairs the identities of Players 1 and 2 are not revealed to each other, in the last three pairs the identities of both players are revealed. For the latter, we not only reveal the name but we also show a photograph of Player 1 to Player 2, and vice versa. Moreover, to study the influence of being matched with somebody from the same village, participants are paired with a co-villager in one of the first two pairs and two of the last three pairs. In the other pairs, matched players come from different villages.[7] Whether or not the person they are paired with is a co-villager is revealed to all players. To avoid order effects, it is randomly determined which of the pairs come from the same village.

1.2. Theoretical Considerations and Hypotheses

In this section, we develop our main behavioural hypotheses. We start with the assumption of standard preferences and absence of sharing norms. Thereafter, we reflect on the possible

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[4]For a similar approach see Gneezy and Potters (1997) and Charness and Gneezy (2012).

[5]It is randomly determined who assumes the roles of Player 1 and Player 2. These roles are assumed for the entire Part 2 of the experiment.

[6]In treatment T2 Player 2, while not making any decision, is still informed about the investment decision of Player 1.

[7]To maximise social distance, we made sure that participants from different villages in the same session came from different parishes, so that the villages were not neighbouring.

influence of sharing norms on the treatment effects.

In Part 1 the optimization problem of Player 1 would be to choose investment level x up to a maximum of E such that her expected utility is maximised. In classical expected utility theory (assuming risk aversion or risk neutrality), this translates into the following maximization problem:

$$\text{Max } EU(x) = s.u(E + (r-1).x) + (1-s).u(E-x) \tag{1}$$

with $r > 1$, being the investment return, and u(.) being a utility function with $u' > 0$ and $u'' \leq 0$ (which rules out risk loving).

In Part 2 participants are assigned the roles of Players 1 and 2 and they are paired. Assuming Player 1 has standard preferences (i.e. characterised by narrow material self-interest), her optimization problem is changed in the following way. With profit sharing (treatments T1 and T2) the first term is replaced by $s.u(E+(r-p-1)x)$, with $0 \leq p \leq 1$, p being the share of the profit given to Player 2, and $(r-1)-p \geq 0$ as the profits shared cannot be larger than the total profits. With loss sharing (treatments T1 and T3) the second term is replaced by $(1-s).u(E-(1-l)x)$, with $0 \leq l \leq 1$, l being the share of the losses that Player 1 expects to be compensated for by Player 2.

With standard preferences and absence of sharing norms, it can be shown that Player 1 will invest the same amount in all treatments. To prove this we first need to demonstrate that Player 1 would not share any profits if she makes a non-zero investment. This is demonstrated in Lemma 1 (for proofs see the on-line Appendix D).

LEMMA 1. *With standard preferences, absence of sharing norms and* $x_1^*, x_2^* > 0$, *the optimal profit sharing in T1 and T2 will be such that* $p_1^* = p_2^* = 0$.

Lemma 1 together with Player 1 not expecting Player 2 to compensate her for any losses (that is $l_1 = l_3 = 0$) makes that the sharing terms in the optimization functions disappear, and the optimization function of each treatment becomes equal to equation (1), so that $x_1^* = x_2^* = x_3^*$. This would be our natural null hypothesis H_0.

With sharing norms, however, the range of sharing options of Players 1 and 2 becomes more constrained and no longer includes zero sharing. In the extreme case discretion about this variable is lost, so that it becomes a given parameter in the optimization function. If sharing norms make that $l > 0$ and profit sharing is orthogonal to the option of loss sharing (that is $p_1 = p_2$), it can be shown that Player 1 will invest at least as much if loss sharing is an option as she would if it was not, that is $x_2^* \leq x_1^*$. Proposition 1 formally demonstrates this effect (for proofs see the on-line Appendix D).

PROPOSITION 1. *If $l > 0$ and $p_1 = p_2$, optimal investment levels will be such that $x_2^* \leq x_1^*$.* [8]

In a similar way, it can be shown that once profit sharing becomes an option and sharing norms make profit sharing sufficiently high, Player 1 will lower her investment. In other words, investment levels will be at most as high in T1 as they are in T3. The prediction of such behaviour is formally demonstrated by proposition 2 (for proofs see the on-line Appendix D).

PROPOSITION 2. *There exists a minimum $p_{min} > 0$ above which $x_1^* \leq x_3^*$.*

Together propositions 1 and 2 support the alternative hypothesis H_1: $x_2^* \leq x_1^* \leq x_3^*$.

So far we abstracted from variation in social distance between Players 1 and 2. In reality, however, there is substantial variation in social distance, which may interact with the treatment effects. For example, the sharing norms may be stronger among groups of people who are socially more proximate. To test the importance of social distance, we incorporated within-subject variation in social distance in the design by varying social distance over successive pairs along two dimensions: whether or not the paired players come from the same village, and whether or not they get to know the identity of the person they are paired with. For the non-anonymous pairs we also have natural variation in real-life social relations and important socio-economic differences, which we captured with a survey.

1.3. Procedures

We choose parameters such that $E = 6000$ Ugandan Shillings (UGX) and choices are limited to $x \in \{0, 1000, 2000, 3000, 4000, 5000, 6000\}$. Also, $r = 2$ so that investments are doubled if successful, and both profits and losses are equal to the amount invested. Finally,

[8] Whether $p_1 = p_2$ holds is ultimately an empirical question that will be tested in the analytical section.

we set $s = 0.8$ so that Player 1 chooses her preferred prospect $F = (0.8, E + x; 0.2, E - x)$ by setting x.[9] This translates into the payoffs presented in Table 1.

Table 1: *Possible Payoffs in the Experiment*

	Successful investment		Unsuccessful investment		
Investment	Profits	Payoff	Losses	Payoff	EV
0	0	6000	0	6000	6000
1000	1000	7000	1000	5000	6600
2000	2000	8000	2000	4000	7200
3000	3000	9000	3000	3000	7800
4000	4000	10000	4000	2000	8400
5000	5000	11000	5000	1000	9000
6000	6000	12000	6000	0	9600

For this study we selected the district of Sironko, which is located in eastern Uganda. It is a densely populated area where around 90% of the population live in rural areas. Most households' livelihoods depend on farming, with the most important crops being beans, groundnuts, maize, soya or potatoes (Ministry of Water and Environment, 2010). To select the participants in our study a multi-stage cluster sampling procedure was used to make our results representative for Sironko district. Two months before the experimental sessions, we randomly selected 10 villages in each of 5 randomly selected sub-counties. In each selected village we took a random sample of households, and we invited one (randomly selected) adult household member from each selected household to participate.[10] Of each invited person who agreed to participate, we took a photograph for three reasons: to make sure the correct persons would participate in the experiment, to facilitate the administration of a social tie questionnaire and to organise the non-anonymous pairing in the experiment. Two weeks before the experimental session, a questionnaire was administered to capture important socio-economic characteristics of the participants and their households, as well as their social ties with other participants from the same village. In

[9] When pre-testing the design, we calibrated r and s to induce sufficient variation in investment decisions in the sample. We piloted several combinations of r and s and among the ones we tried, the combination of $r = 2$ and $s = 0.8$ gave us maximum variation in investment decisions.

[10] For this study we used a subgroup of the described sample. The subjects in the full sample were randomly allocated to one of three unrelated experiments. All experiments in the same sub-county were organised on the same day, most of them simultaneously in different class rooms of the same school.

total, 360 participants from five districts were invited in 18 sessions, with 324 (90%) of them showing up. In each session, we had up to 20 participants.[11] We organised 8 different sessions of treatment T1, and 5 sessions of treatments T2 and T3 each. Average earnings were 9892 UGX (including a 2000 UGX show up fee), equal to 3.81 US$, i.e., around two days' average income.

Each experimental session began with a welcoming act, after which the experimenters explained the experimental instructions and procedures. In particular, it was made clear that participation was voluntary, and that the decisions the participants would take would be dealt with in a confidential way. To help ensure this, communication was not allowed and questions could only be asked in private. Any money the participants earned would be paid out privately and confidentially after the exercise. It was explained that in the exercise the participants would be asked to make several choices, and only one of them would be randomly selected to determine the money they would be paid. Because of the low literacy of some of the participants, no written instructions were given. The instructions had been pretested and adapted to make sure that they would be understandable to participants with low literacy. All participants were able to easily read numbers (because that is what they do when they go shopping) and handling a pen for simple operations such as marking an option.

After the explanation of the instructions, each participant was privately asked a series of control questions. This allowed us to identify participants who were struggling with the instructions and help them by providing additional explanations in private. In all analyses, we only include participants who answered correctly at least three of the four control questions, representing 83.9% of the participants. We assume that participants who answered were than three control questions correctly were less likely to understand the full details of the experimental game, even after our additional explanations in private. For more details on the experimental instructions and procedures see the on-line Appendix E.

[11]To guarantee anonymity we made sure that sessions were large enough, that each session had participants from exactly two villages and that the number of participants in each session was equally divided between the two villages. As a result, with most sessions having 18-20 participants, most sessions had two batches of 9-10 participants from each of two villages. The lowest number of co-villagers in a session was six. As participants were paired with three co-villagers, they were never able to infer the identity of the anonymous players.

2. Analyses

In this section, we present the main empirical results. To test our research hypotheses we compare investment levels across the three treatments. We will also analyse the role of social distance among the pairs, generated by our within-subject experimental design and the natural variation in real-life social distance among the non-anonymous pairs from the same village.

2.1. How Common Is Sharing?

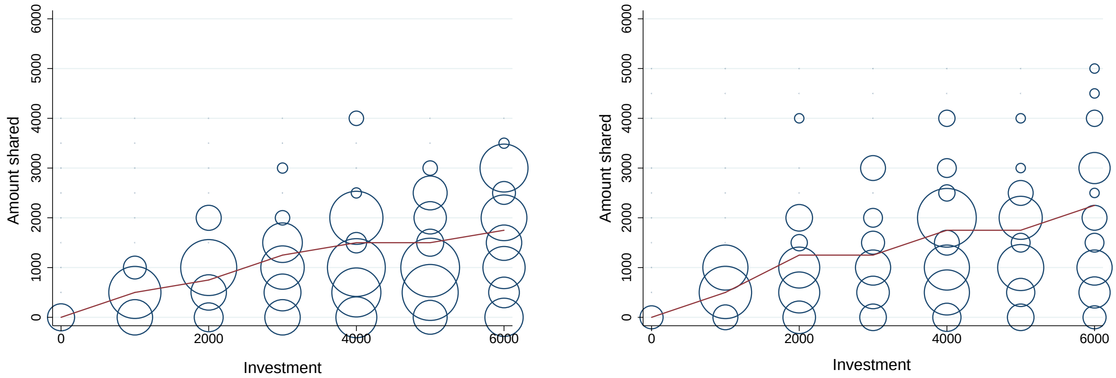
Before these analyses, however, we will undertake a descriptive analysis of profit and loss sharing. As demonstrated in the theoretical section, the predictions behind the alternative hypothesis H_1 rely on (1) the assumption that Player 2 is expected to share some of the losses, and (2) the assumption that Player 1 does not share profits differently in treatments T1 and T2.

To get a first idea of the variation in profit and loss sharing we plot the distribution of profit sharing and loss sharing for each investment level, as presented in Figure 1. We observe that most people do share some profits or losses when possible, and that in most cases the proportion that is shared is lower than 50%. This contrasts with the strong focal point of equal sharing in African societies.[12] That a lower proportion of profits and losses is shared is probably due to the interaction between risk-taking and what people find a fair distribution of economic resources. In particular, an investor may consider it to be unfair if other people share half of the loss of her risky investment that led to failure, or if she has to share half of the profits if the investment was successful (for experimental evidence on this see, e.g., Cappelen *et al.*, 2013).[13]

To obtain a better idea of any treatment differences in proportional sharing Figure 2 plots the cumulative distribution of the proportion of profits and losses shared, for each treatment separately. In only 15-16 % of the pairs in treatments T1 and T2 does Player

[12] Gowdy *et al.* (2003), for example, found that the mode in a dictator game in rural Nigeria was 50% and the mean offer was 42%. For other dictator game studies in African countries see Ensminger (2000); Henrich *et al.* (2006).

[13] Figure B.1 in the on-line Appendix B plots sharing as a proportion of the investment made. We observe that there is a small negative correlation suggesting that an increase in investment is not accompanied by an equally large increase in sharing.



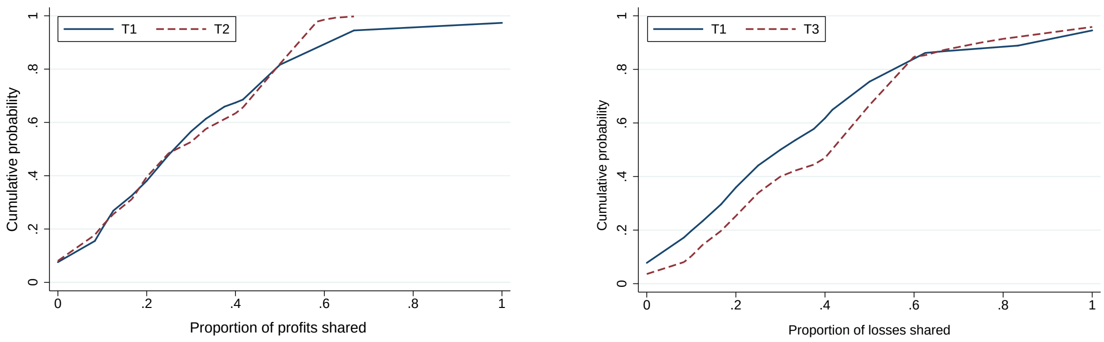
(a) Profit sharing

(b) Loss sharing

Figure 1: *Profit and Loss Sharing*

Note: The line represents median sharing at different investment levels.

1 not share any profits, and in only 8 to 14 % of the pairs in treatments T1 and T3 does Player 2 not share any of the losses. Also, we do not observe any strong difference in profit sharing between treatments T1 and T2 or in loss sharing between treatments T1 and T3. Applying a t-test to compare proportional sharing between the different treatments for the four different types of pairs (that vary in terms of anonymity and co-locality), we do not find any statistically significant differences in proportional profit sharing between treatments T1 and T2 (t-tests have two-sided p-values larger than 0.195). Nor do we find any significant differences in proportional loss sharing between treatments T1 and T3 (t-tests have two-sided p-values larger than 0.293).



(a) Profit sharing

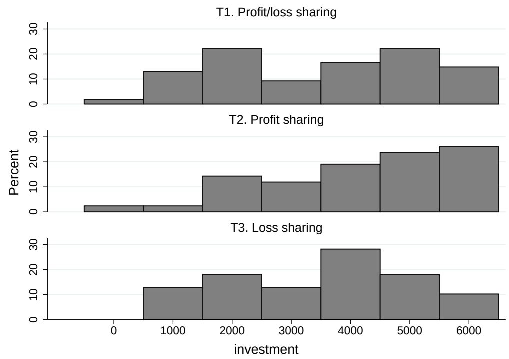
(b) Loss sharing

Figure 2: *Cumulative Distributions of Proportion of Profits and Losses Shared*

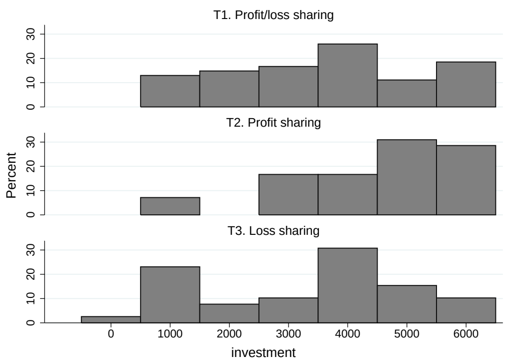
In sum, there is a considerable amount of profit and loss sharing, which confirms the existence of strong sharing norms. The assumption of equal profit sharing in T1 and T2 is confirmed, that is $p_1 = p_2$. We also found considerable loss sharing, which suggests that Players 1 have every reason to expect to be compensated for some of their losses if their investment is unsuccessful, that is $l > 0$. That two important assumptions behind propositions 1 and 2 are empirically confirmed makes us expect that the null hypothesis H_0 will be rejected in favour of the alternative hypothesis H_1. The next section will test whether that is indeed the case.[14]

2.2. *Do Profit and Loss Sharing Matter for Investment?*

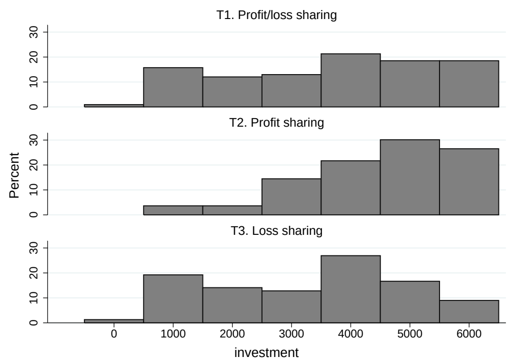
(a) Different village - anonymous



(b) Same village - anonymous



(c) Different village - non-anonymous



(d) Same village - non-anonymous

Figure 3: *Distribution of Investment Decisions by Treatment and Type of Pair*

[14]On-line Appendix C also shows that correlations between the proportion of profits or losses shared and the social distance between the paired persons are very small and mostly statistically insignificant. This suggests that sharing is not used as a decision variable to be optimised, as we assumed in the theoretical section, which may be the result of the strong sharing norms.

In a first analysis, we compare investment levels across the three treatments for the four different types of pairs. Figure 3 shows the distribution of investment levels for the three treatments, separately for each of the four different types of pairs. In each sub-panel we observe that investment levels tend to be highest in treatment T2 and lowest in T3. In particular, we find that the two highest investment levels (5000 and 6000) are chosen in 42-59% of cases in T2, 29-37% in T1 and 25-28% in T3 (for details on the distribution see Table A.1 in Appendix A). Moreover, these differences between treatments become more pronounced with lower social distance between the paired players (i.e. giving up anonymity and matching players from the same village). More specifically, the percentage of cases in which one of the two highest investment levels was chosen rises from 42% to 59% in T2 when moving from panel (a) to panel (d) while the percentage of cases in which the two lowest investment levels were chosen (0 and 1000) in T3 goes up from 12-13% to 20-25%.

To test whether the differences between treatments are statistically significant, we use pairwise t-tests, the results of which are reported in Table 2. The results indicate that average investment levels are higher in treatment T2 compared with both treatments T1 and T3, but investment levels do not differ between treatments T1 and T3. These differences are highly significant for non-anonymous pairs while only marginally significant for anonymous pairs.

Table 2: *Mean Differences of Pairwise Treatment Comparisons*

	Anonymous		Non-anonymous	
	Different village	Same village	Different village	Same village [a]
Comparison T2-T1	611.111*	671.958*	870.370***	824.074***
	(0.074)	(0.057)	(0.008)	(0.007)
Comparison T3-T1	85.470	-5.698	-321.937	-368.234
	(0.808)	(0.987)	(0.366)	(0.275)
Comparison T2-T3	525.641	677.656*	1192.308***	1192.308***
	(0.116)	(0.058)	(0.001)	(0.000)

Note: Two-sided p-values of a two-sample t-test between parentheses; ***, **, * indicate significance levels at 1, 5, and 10 %, respectively; [a] as investors took two decisions for two different pairs, we took the average investment.

Although the implementation of the treatments was randomised across sessions, the limited number of sessions per treatment may affect the orthogonality of treatment assignment to regional characteristics. While we do not find any strong imbalances in individual characteristics across the three treatments (see Table A.2 in Appendix A), we run a regression as it allows us to correct statistical inference for possible within-session correlations by clustering standard errors at the session level. In the model we add sub-county fixed effects as well as a control for individual risk preferences as elicited with the investment decision in Part 1 of the experiment. As all sessions were run by one of two different experimenter teams, we also add controls for the experimenters who organised the session. Finally, we include controls for gender, age and wealth (as measured by the first factor of a principal component analysis using a list of assets) of Player 1. Table 3 presents the results.

Table 3: *Treatment Comparisons: Regression Analysis*

	Anonymous		Non-anonymous	
	Different village	Same village	Different village	Same village
	(1)	(2)	(3)	(4)
T2: Profit sharing	471.5	434.1*	823.4**	864.4***
	(381.2)	(231.1)	(309.1)	(269.3)
T3: Loss sharing	8.206	15.72	-276.1	-243.6
	(291.4)	(199.3)	(210.6)	(220.5)
Constant	2467.4***	1575.4**	2981.4***	2512.8***
	(675.4)	(599.5)	(637.2)	(724.9)
Observations	134	134	134	267
R^2	0.192	0.298	0.181	0.283

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.

In Model 1, which only uses anonymous pairs from different villages, we do not find any statistically significant differences between treatment T1 (the reference category) and treatments T2 and T3. For anonymous pairs from the same village (Model 2) we observe a marginally significant difference between T1 and T2. On average investors invest around 434.1 UGX more in T2 than in T1. Comparing the coefficients on T2 and T3 using an F-test we cannot reject the null hypothesis that the coefficients are the same for pairs from different villages (two-sided p of F-test = 0.143) but we can reject this hypothesis for pairs from the same village (two-sided p of F-test = 0.024).

Models 3 and 4 estimate the same model for non-anonymous pairs from different villages and the same village, respectively. The results demonstrate that Players 1 tend to invest significantly more if losses cannot be shared (T2) compared to when both profits and losses can be shared (T1). The difference is on average 823.4 UGX for pairs from different villages (Model 3) and 864.4 UGX for pairs from the same village (Model 4). They also invest more in T2 compared to when only losses can be shared (T3). Differences between T2 and T3 are statistically significant for non-anonymous pairs from a different village (two-sided p of F-test = 0.000) and from the same village (two-sided p of F-test = 0.000). Estimating predicted investment decisions for T2 and T1, we find that treatment effects are sizeable. In Model 3, the predicted investment levels are 3539.1 and 4362.5 in T1 and T2, respectively, and 3558.0 and 4422.4 in Model 4. Put differently, removing the option of loss sharing leads to an increase in investment levels of 23.3% and 24.3%, respectively.[15]

To test robustness of these results, we run a tobit model, as the dependent variable shows some censoring at 6000 (see the distribution of investment decisions in Figure 3). Furthermore, as clustered standard errors may be inaccurate with a low number of experimental sessions, we run a regression model with bootstrapped standard errors that provides a better method to correct standard errors for non-independencies within sessions when the number of sessions is low (on this see Cameron *et al.* (2008)). Finally, to test whether the functional form of a control for risk preferences matters, we run a model in which we control for risk preferences using dummy variables for each of the possible investment decisions in Part 1 of the experiment. The results of these estimations are presented in Tables B.1, B.2 and B.3 in the on-line Appendix B and are not qualitatively different from the OLS estimates presented here. We summarise the main observations in a first result.

[15]To test whether there are any heterogeneous effects by risk aversion we add interaction terms between T2 and T3 and the investment decision in Part 1, which we use as a measure of risk preference. The coefficients of the interaction terms are negative and significant in Model 3 (-0.326* and -0.481 for T2 and T3 respectively) and especially Model 4 (-0.527*** and -0.627** for T2 and T3 respectively). These effects are driven by the fact that the closer one gets to the highest investment level the smaller the scope to differentiate between the three treatments. This effect is weaker at the lower end of the range as the distribution of investment levels is skewed to the higher end (see the descriptive statistics of the investment decision in Part 1, in the Appendix).

Result 1. Among anonymous pairs from the same village, investment is slightly higher if losses cannot be shared by the paired person. Among non-anonymous pairs, investment is substantially higher if losses cannot be shared by the paired person, irrespective of whether the paired players live in the same village.

2.3. Interactions with Experimental Social Distance

Table 4: *Interaction with Experimental Social Distance*

	(1)	(2)	(3)	(4)
Anonymous	-171.6	-101.2	280.9	-101.2
	(185.1)	(155.3)	(225.4)	(155.3)
Same village	-4.608	35.32	-4.608	-120.1
	(93.11)	(127.6)	(93.11)	(227.7)
T1: Profit/loss sharing			391.1	117.2
			(238.2)	(286.0)
T2: Profit sharing	875.9**	776.4**	1267.0***	893.6***
	(340.2)	(317.6)	(309.9)	(250.2)
T3: Loss sharing	-391.1	-117.2		
	(238.2)	(286.0)		
Anonymous * T1			-452.5	
			(289.5)	
Anonymous * T2	-214.9		-667.4	
	(397.5)		(414.5)	
Anonymous * T3	452.5			
	(289.5)			
Same village * T1				155.5
				(258.0)
Same village * T2		20.82		176.3
		(181.4)		(256.9)
Same village * T3		-155.5		
		(258.0)		
Constant	2369.3***	2317.2***	1978.3***	2200.0***
	(597.7)	(572.8)	(684.1)	(682.6)
Observations	669	669	669	669

Notes. Tobit regression with higher censoring set at 6000. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.

To recap, we can study the effects of social distance of two types: experimentally generated and that found in real-life social ties. The former varies along two dimensions: anonymity vs. identity revealed (1), and co-villager vs. residing in different villages (2). While we found interesting differences in treatment effects across the pairs varying on these two social distance dimensions, a more formal test is needed to examine whether the interaction between social distance and the treatment effects is statistically significant. To test whether the treatment effects depend on whether the pairs come from the same village and whether the pairs are anonymous, we pool all observations and estimate a regression model with interaction terms between each of the sharing treatments and (a) a dummy that indicates whether the pairs of players are anonymous (Model 1) and (b) a dummy that indicates whether the paired players belong to the same village (Model 2).

As shown in Table 4 none of the coefficients in Models 1 and 2 is statistically significant at conventional significance levels. However, when using T3 as a reference category we find that the effect of anonymity (Model 3) is marginally significant (two-sided p is 0.119 for the coefficient of Anonymous * T1 and 0.108 for the coefficient of Anonymous * T2). This indicates that giving up anonymity increases the difference in investment levels with T3. As both treatments differ from T3 both in terms of profit and in terms of loss sharing, it is impossible to know how much of these effect are due to the option of profit sharing and how much to the option of loss sharing. Using wild bootstrapped standard errors as reported in Table B.4 in the on-line Appendix B does not qualitatively change the results.

That we did not find any statistically significant effect of experimental social distance is not surprising as we obtained only limited within-subject variation on investment behaviour. In treatment T1 31.5% of the participants did not change their investment decision over the five different pairs, while the corresponding figure is 16.7% for T2 and 23.08% for T3.[16] We summarise the observations in a second result.

Result 2. The treatment effects do not depend on whether the players in a pair belong to the same village. Giving up anonymity has a marginally significant positive effect on investment differences between treatments T2 and T3.

[16]While the anonymity condition was not randomised we do not expect the effect of anonymity to be confounded by order effects. Regressing investment levels on the order of the pair separately for anonymous pairs (the first two pairs) and non-anonymous pairs (the last three pairs) indicates that order effects are limited.

2.4. Interactions with Real-Life Social Distance

Based on the analyses so far, we found weak evidence for the role of social distance. While we observed that differences between T1 and T2 were particularly apparent when the identities of the pairs were revealed, we were unable to reject the hypothesis that these treatment differences are the same for pairs that vary on anonymity and belonging to the same village. An additional source of variation in social distance that may influence investment propensities corresponds to variation in real-life social ties and socio-economic differences among people from the same village. Such variation may influence the investment behaviour of Players 1 when they are non-anonymously paired with Players 2 from the same village.

Data on social ties was collected using a social tie questionnaire two weeks before the experiment. In particular, we asked all sampled people about their relations with all other sampled people in the same village. We assume a social tie exists if Player 1 reported having a connection with Player 2. As reported in Table A.3, of all non-anonymous pairs from the same village used in the analysis, 42.7% have a kin relation, and 31.3% are close friends (and not kins).[17]

To analyse the interaction between real-life social distance and the treatment effects we follow a similar approach as in the previous section. In particular, we interact the treatment dummy variables with variables that capture important real-life variation in social distance, generated through social ties and socio-economic differences. In Model 1, we add a dummy variable equal to one if Player 1 reports being related to Player 2, as well as interaction variables with treatments T2 and T3. In Model 2, we do the same for friendship ties using a dummy variable equal to 1 if Player 1 reports being a close friend of Player 2 (but not kin). Variation in social distance may not only be the result of variation in real-life social relations. It may also result from differences in socio-economic characteristics, such as wealth and risk preferences. To analyse the effect of these socio-economic differences, we add interaction terms with these socio-economic differences. More specifically, in Model 3 we control for wealth differences and their interaction with the T2

[17]As the participants come from different households, kinship ties do not include intra-household ties, but only refer to ties among people from different households.

and T3 treatments. As the effect of wealth differences may be different for positive and negative differences, we use two variables, measuring positive wealth differences (being zero when the wealth difference is negative, i.e. Player 1 has lower wealth than Player 2) and absolute negative differences (being zero when the difference is positive). Model 4 does the same for differences in individual risk preferences as measured by the participants' investment decision in Part 1.

The results are presented in Table 5. The results of Model 1 indicate that there are no significant interaction effects with kinship ties. In Model 2, in which we add interaction terms with friendship ties, we find that friendship ties increase investment levels in treatments T1 and T2, but not in treatment T3. Note that the coefficient on Friend * T3 is of similar size as the coefficient on the Friend dummy but has the opposite sign. This indicates that when paired with a friend, having the option of sharing profits (Treatments T1 and T2) increases investment levels.

In Model 3 we add interaction terms with wealth differences. The negative coefficient on the negative difference variable together with a positive coefficient (of similar size) of the interaction between this difference variable and T2 indicates that Players 1 with lower wealth than the paired person are less likely to invest unless loss sharing is not an option. As the coefficient on T2 is also significant (and positive) we can conclude that everyone is sensitive to the option of loss sharing, but that this effect is stronger when investors are matched with a wealthier person.

In Model 4 we add differences in risk preferences. While the effects of friendship ties and wealth differences remain robust, we find interesting interaction effects with differences in risk preferences. Specifically, larger negative differences in risk preferences lower investment levels, as indicated by the negative and significant coefficient on the variable measuring such negative differences. This effect, however, disappears once loss sharing is not an option, as indicated by the significant and positive coefficient on the interaction between T2 and the negative difference in risk preferences. We also find a positive interaction effect with T3, but this effect disappears with wild bootstrapped standard errors (see Table B.5 in the on-line Appendix B).[18] We summarise the main observations in a third result.

[18]Note that the wealth and risk preferences of the paired person were not made explicit by us to the participants. When they were taken into account, this must have been due to prior knowledge.

Result 3. Being linked with a friend stimulates investment if profit sharing is an option. When loss sharing is an option, investment levels are lower when an investor is matched with a wealthier and/or less risk averse person, and the more so the larger the differences are between the paired players in wealth and/or risk preferences.

Table 5: *Interaction with Real-Life Social Distance*

	(1)		(2)		(3)		(4)	
T2: Profit sharing	644.1**	(296.7)	622.8	(384.0)	833.5**	(417.3)	-35.24	(395.9)
T3: Loss sharing	-650.1**	(316.4)	-192.8	(368.4)	-322.5	(464.2)	-1188.6**	(553.5)
Related	-81.77	(292.3)	359.8	(237.1)	474.1**	(235.6)	192.4	(194.5)
Related * T2	598.6	(464.5)	616.8	(645.6)	232.7	(579.1)	331.2	(522.7)
Related * T3	588.1	(620.0)	122.8	(604.8)	2.353	(622.0)	337.3	(635.6)
Friend			899.4**	(365.8)	944.3***	(330.6)	782.3**	(326.2)
Friend * T2			-189.7	(588.9)	-438.7	(508.0)	-554.7	(495.4)
Friend * T3			-939.5**	(423.3)	-909.1**	(451.0)	-953.5*	(517.3)
Wealth (pos. dif.)					51.47	(113.7)	32.04	(102.3)
Wealth (abs. neg. dif.)					-179.4**	(83.45)	-160.3*	(86.96)
Wealth (pos. dif.) * T2					-76.81	(160.2)	3.894	(132.3)
Wealth (pos. dif.) * T3					8.586	(150.4)	83.63	(134.2)
Wealth (abs. neg. dif.) * T2					196.4*	(103.5)	190.4*	(106.8)
Wealth (abs. neg. dif.) * T3					148.1	(149.7)	164.3	(177.4)
Risk pref. (pos. dif.)							0.062	(0.158)
Risk pref. (abs. neg. dif.)							-0.345***	(0.092)
Risk pref. (pos. dif.) * T2							0.176	(0.200)
Risk pref. (pos. dif.) * T3							-0.041	(0.301)
Risk pref. (abs. neg. dif.) * T2							0.663***	(0.195)
Risk pref. (abs. neg. dif.) * T3							0.699**	(0.354)
Constant	2518.9***	(848.1)	2060.4**	(817.6)	1847.5*	(975.6)	2557.1***	(941.1)
Observations	245		245		241		241	

Notes. Tobit regression with higher censoring set at 6000. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.

3. Discussion of Plausible Explanations

Summarizing our results, we found that (1) investment levels are lower when paired others have the option to compensate the investor for losses, which is particularly apparent if the identities of the paired persons are revealed to each other; (2) the effect of loss sharing on investment among non-anonymous pairs from the same village is stronger when the investor is matched with a wealthier or less risk averse person, and the more so the larger the differences in wealth and/or risk preferences; and (3) investment levels are higher when profits can be shared with friends. These results go against the hypotheses based on standard preferences that either predict no treatment differences (without sharing norms) or predict treatment differences in the opposite direction to the ones we find (with sharing norms). In this section we will discuss plausible explanations for these treatment differences. In a first step, we will try to explain them by focusing on the potential role of declining loss sharing as investment rises. Thereafter, we look for clues as to whether, and if so, which, non-standard preferences may explain the observed treatment effects.

3.1. Declining Loss Sharing

The observed lower investment when loss sharing is possible may be the result of lower loss sharing at higher investment levels. The paired person may be more reluctant to share losses if the investor takes a lot of risk. If the expected decline in loss sharing with higher investment levels is stronger than the expected increase in gains, the investor may prefer not to increase investment levels. As this effect is only possible with loss sharing being an option, investment levels may be lower compared to when loss sharing is not an option, that is in T2.[19]

From Figure 1 we observe that the increase in loss sharing weakens with higher investment levels. This is confirmed by a regression analysis with both investment and investment squared as explanatory variables. When treatments T1 and T3 are pooled we obtain a highly significant positive coefficient of the investment level (0.406***, two-sided P = 0.001) and a (marginally) significant negative coefficient of the squared investment level (-0.00003*, two-sided P = 0.081). These results taken together show that the increase in loss sharing becomes weaker with higher investment levels, but based on the predicted

[19] We thank an anonymous referee for suggesting this explanation.

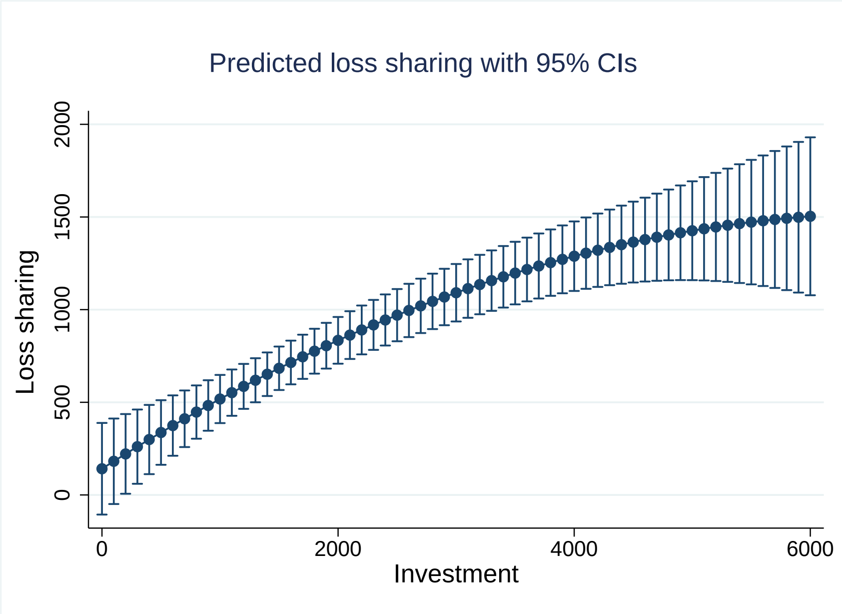


Figure 4: *Predicted Loss Sharing*

values in Figure 4 loss sharing never actually decreases. To test whether the existence of an inverted U relation varies across the social distance treatments, we add interaction terms between two dummies equal to one for anonymous pairs and pairs from the same village, respectively, and the investment and investment square variables. Figure 5 plots predicted loss sharing for pairs that vary in social distance. For non-anonymous pairs and pairs from a different village we do observe a slight decline in loss sharing at higher investment levels.

While for some types of pairs there can indeed be a decline in loss sharing, we will demonstrate that the observed decline will not be large enough to make people prefer lower investment levels. Let's first assume risk neutrality. With a 1000 increase in investment we have an expected increase in gains of $0.8 * 1000 = 800$. For Player 1 to make the decision to increase investment by 1000, the final expected earnings (including expected loss sharing) have to increase. For this to be the case the change in expected loss sharing needs to be smaller than 800. As the probability of a loss is only 0.2, we would need a decline of loss sharing of more than 4000 for the change in the expected loss sharing to outweigh the change in expected gains; such a drastic decline is not observed in Figures 1, 4 and 5. For the case with risk aversion we refer to the simulation presented in the on-line Appendix G, which again confirms that the required declines in loss sharing are much higher than the ones we empirically observed. As a result, an anticipated decline in loss

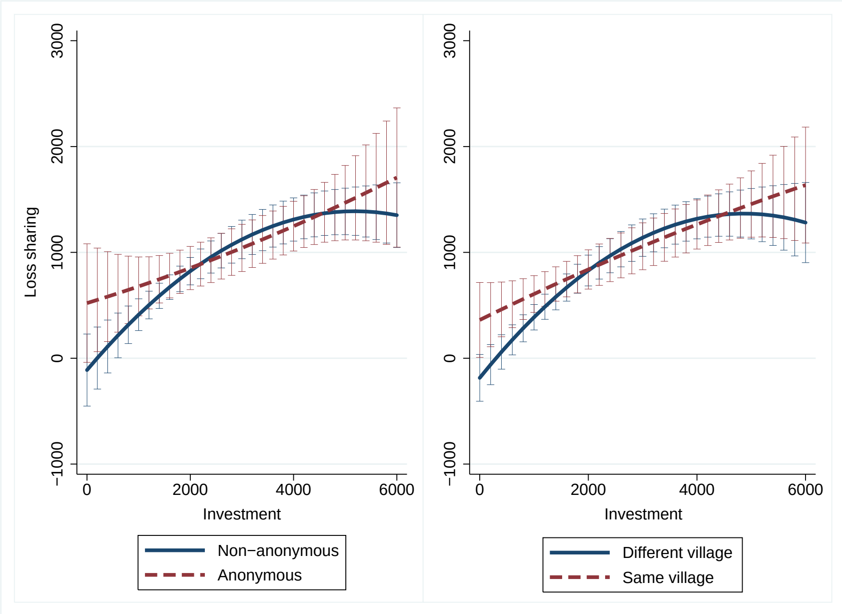


Figure 5: *Predicted Loss Sharing by Social Distance*

sharing is not likely to explain the lower investment levels when loss sharing is an option.

3.2. Altruism and Expected Reciprocity

If we cannot explain the observed treatment differences with a theoretical model that assumes standard preferences, it is likely that the assumption of standard preferences needs to be revised. In particular, altruistic preferences may explain our finding of lower investment levels when loss sharing is possible. Given strong sharing norms, Player 2 will be socially expected to compensate Player 1 for some of the losses Player 1 may incur. Player 1 is aware of these sharing norms, so knows that Player 2 will probably share the losses in case the investment fails. If Player 1 has altruistic preferences she would care if the income of Player 2 were to be decreased; to limit the extent to which this may happen, she may lower investment levels.

A second explanation might be that loss sharing by Player 2 incurs a future obligation on Player 1 to help Player 2 when the latter is in need. The literature on risk-sharing networks, reviewed in Section 1, implies that mutual insurance is beneficial for all its members. In such settings, a mutualistic approach to morality would predict that people have internalised norms that sharing needs to be reciprocated: one person insuring another implies an obligation on the latter to provide assistance to the former in the future (Baumard *et al.*, 2013). In the absence of formal insurance markets—which is the case in most rural areas in developing countries—people may thus try to build up their individual social capital by making known people reciprocally indebted to them and/or reduce their reciprocal debts to others.[20] This may make people reluctant to accept help from others while eager to help others as giving help generates an entitlement to others' help when needed.[21] Once behaviour outside the frame of our experiment is taken into account, anticipated reciprocity may thus become an important driver for behaviour inside the experiment. One could interpret Player 1's reducing investment when losses may be shared as a reluctance to take on the (full) demands of expectations of reciprocity.

We thus have altruism and expected reciprocity as two possible explanations for investment being lower when loss sharing is possible. An extensive experimental literature has demonstrated that both motivations may be important. First, studies with dictator games have demonstrated that altruism may be an important behavioural motive (Forsythe *et al.*, 1994; Bohnet and Frey, 1999), with plenty of experimental evidence on altruism found in African societies (Ensminger, 2000; Henrich *et al.*, 2001; Gowdy *et al.*, 2003; Ligon and Schechter, 2012). Second, studies using trust and gift-exchange games have demonstrated that any transfer of resources to others with the possibility of a future return is largely driven by (expected) reciprocity (Berg *et al.* (1995); Ostrom and Walker (2003); on gift-exchange games see Fehr *et al.* (1998); Brandts and Charness (2004)). There is also extensive experimental evidence on trust and reciprocity in developing countries and in African societies where these games have been used (Ensminger, 2000; Henrich *et al.*, 2001, 2006; Gowdy *et al.*, 2003; Etang *et al.*, 2011; Binzel and Fehr, 2013). Given the abundant evidence on the behavioural importance of both motivations in a variety of settings, we would also expect them to play a role in a setting where profits or losses resulting from risky choice are shared.

In the rest of this section, we will exploit the variation in social distance in our data, either generated by our experimental design or the natural variation in our sample, to say something more about the extent to which each of these two behavioural motives drives

[20]The assumption is that social capital consists of a set of social ties that people form because they conceive of them as investments from which they expect a return (Lin, 2001).

[21]Baumard *et al.* (2013, p.74) review the literature on ultimatum games in traditional societies and interpret the evidence that in some of these societies very high offers are refused as consistent with such a mutualistic approach to morality: these offers place high demands of reciprocity on those who accept them. We thank an anonymous referee for suggesting this line of interpretation.

the observed treatment effects. The reciprocity interpretation relies on future interactions between players that necessitate the honouring of the obligation incurred during the experiment if Player 2 finds himself in need of assistance. This is the case for non-anonymous pairs from the same village, but less so for players from different villages (who as noted come from different parishes, which as key informants told us would make future interaction in most cases non-existent). Only when non-anonymously paired with someone from the same village can any repayments be claimed after the game when needed. Since we find, for non-anonymous pairs, an effect of the same order of magnitude for players from different villages and players from the same village, reciprocity cannot be the only explanation. Our finding that the investment-reducing effect of loss sharing is particularly strong for non-anonymous pairs (regardless of whether or not they are from the same village) is consistent with altruistic preferences: the person insuring Player 1 now has a face, and Player 1's altruistic preferences may induce her not to implicate Player 2 in her willingness to take risk. This is consistent with Bohnet and Frey (1999) who found that identification increases generosity in dictator games.[22]

While the results support the altruistic preferences explanation we have evidence that suggests that reciprocity plays a role as well. First, the reciprocity explanation was supported by interviews with agricultural extension officers and others who advise farmers in the study area, of whom we interviewed 16 to assist us in the interpretation of our findings. They suggested that loss sharing in the experiment may have been thought of by subjects as giving rise to future sharing obligations on the current beneficiary. Second, the result that investment levels are lower when the investor is matched with a wealthier or less risk averse person (and loss sharing is an option) is consistent with the reciprocity explanation but not the altruism explanation. The mutualistic approach to morality discussed above suggests that reciprocity does not imply equivalent assistance to assistance previously received, but is defined in relation to the need that has arisen (cf. Baumard *et al.*, 2013). When matched with a less risk averse person, the likelihood of needing to reciprocate assistance previously received is higher, since the other person takes more risk. Similarly,

[22]Another frequently documented non-standard preference is inequality aversion. Our results, however, do not support inequality aversion as a main driver of investment decisions. If inequality aversion influenced investment decisions, we would expect T3 $\leq$ T1 and T2 $\leq$ T1, as in both T2 and T3 expected risk sharing would be imbalanced and lead to an expected increase in inequality. However, we find that T2 $>$ T1.

reciprocation may be costlier towards a wealthier person, since that person may invest larger amounts and thus incur larger losses. Conversely, with altruism being higher towards socially close others—people value social proximity by giving a higher value to the income of others (Bohnet and Frey, 1999)—we would expect the investor's altruism to be lower when she is paired with a wealthier and/or less risk-averse person, which would actually *weaken* the treatment effects in these pairs. It follows that expected reciprocity helps explain our findings.

The observation that investment levels are higher when profits can be shared with friends may be driven by either or both of the two motivations.[23] First, altruistic preferences, which may be stronger towards friends, may induce people to increase investment levels, as this increases the potential profits that can be shared. Second, reciprocal motives should increase investment as it allows one to increase the profits that can be shared to reciprocate past favours or to build up debts that can be reclaimed when needed.

4. Conclusion

In rural societies in developing countries, where society's influence is of an informal nature and insurance markets are largely absent, one important way of dealing with the hazards of life is through risk-sharing networks. The literature on informal risk sharing in village economies, reviewed in the Introduction, predicts effects on investment in two directions: loss sharing facilitates investment, whereas profit sharing dampens incentives to invest. We designed an economic experiment to disentangle these two effects. We also made use of real-life and experimentally varied social distance to obtain clues as to the drivers of these effects.

Paradoxically, we find that profit sharing does not lower investment; among friends, it even raises investment considerably. Perhaps even more strikingly, loss sharing lowers investment. This finding runs counter to a key prediction of the literature on informal risk sharing. The effect is particularly strong in the non-anonymous treatments, and then especially so when the investor is paired with somebody wealthier or less risk averse than she is herself.

We argued that altruism may help explain these paradoxical findings, because an

[23] In fact, Leider *et al.* (2009) demonstrated that both altruism and future enforced reciprocity play a role in sharing with friends.

altruistic person would want others to benefit—perhaps especially friends—and would not want others to suffer—perhaps especially when these ”have a face”—from the consequences of her investment. Expected reciprocity could provide a second part of the explanation. Non-anonymous others may ask for or be asked for assistance in future, when these have respectively provided or been given help in the experiment. If future help is defined in relation to the need that will by then have arisen, which a mutualistic approach to morality suggests, an explanation is provided for why subjects in the experiment are reluctant to take on such an obligation.

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Appendix

A. Descriptives

Table A.1: *Distribution of Investment Decisions (in percentage)*

	Anonymous pairs							
	(a) Different village				(b) Same village			
	T1	T2	T3	Total	T1	T2	T3	Total
0	5.56	2.38	0.00	2.96	1.85	2.38	0.00	1.48
1000	9.26	0.00	12.82	7.41	12.96	2.38	12.82	9.63
2000	16.67	14.29	12.82	14.81	22.22	14.29	17.95	18.52
3000	9.26	9.52	10.26	9.63	9.26	11.90	12.82	11.11
4000	25.93	30.95	35.90	30.37	16.67	19.05	28.21	20.74
5000	18.52	21.43	17.95	19.26	22.22	23.81	17.95	21.48
6000	14.81	21.43	10.26	15.56	14.81	26.19	10.26	17.04
Total	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00
	Non-anonymous pairs							
	(a) Different village				(b) Same village			
	T1	T2	T3	Total	T1	T2	T3	Total
0	0.00	0.00	2.56	0.74	0.93	0.00	1.28	0.74
1000	12.96	7.14	23.08	14.07	15.74	3.61	19.23	13.01
2000	14.81	0.00	7.69	8.15	12.04	3.61	14.1	10.04
3000	16.67	16.67	10.26	14.81	12.96	14.46	12.82	13.38
4000	25.93	16.67	30.77	24.44	21.3	21.69	26.92	23.05
5000	11.11	30.95	15.38	18.52	18.52	30.12	16.67	21.56
6000	18.52	28.57	10.26	19.26	18.52	26.51	8.97	18.22
Total	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00

Table A.2: *Descriptive Statistics*

	Total	T1	T2	T3	p-value
Gender (male)	52.59%	57.02%	45.12%	54.05%	0.247
Age	40.19	40.63	39.49	40.28	1.000
Wealth	0.243	0.664	-0.079	-0.048	0.150
Risk preference	4051.28	3877.19	4282.35	4054.05	0.244

Notes. N = 270. For continuous variables we report the lowest two-sided p-value of a Bonferroni multiple-comparison test, while for binary variables we report the two-sided p-value of a chi-square test. To measure individual risk preference we used the investment choice in part 1 of the experiment.

Table A.3: *Descriptive Statistics - Pairs*

	N	%	
Know the other person	246	92.7%	
Kinship	246	42.7%	
Friend	246	31.3%	
	N	Mean	St.dev.
Wealth (pos. dev.)	242	1.097	1.616
Wealth (abs.neg. dev.)	242	0.976	2.109
Risk pref. (pos.dev.)	247	793.522	1223.864
Risk pref. (abs.neg.dev.)	247	878.543	1334.892

Note. Only non-anonymous dyads from the same village selected, with player 1 having answered at least three control questions correctly.

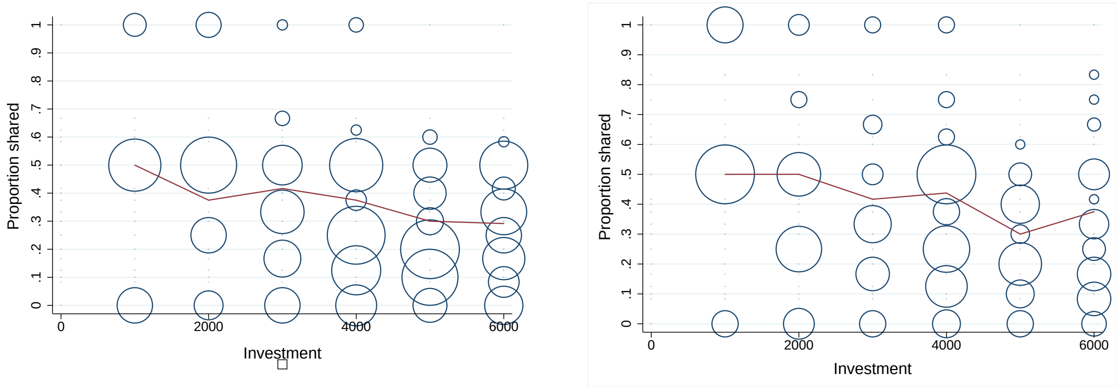
Table A.4: *Correlations between Explanatory Variables*

	Related	Friend	Wealth (pos.dev.)	Wealth (abs.neg.dev.)	Risk pref.(pos.dev.)
Related	1.000				
Friend	-0.556***	1.000			
	(.000)				
Wealth (pos.dev.)	-0.028	-0.065	1.000		
	(.642)	(.275)			
Wealth (abs.neg.dev.)	0.039	0.025	-0.306***	1.000	
	(.513)	(.673)	(.000)		
Risk pref. (pos.dev.)	-0.022	0.048	0.055	-0.0004	1.000
	(.711)	(.416)	(.357)	(.995)	
Risk pref. (abs.neg.dev.)	-0.036	0.048	-0.081	0.053	-0.430***
	(.539)	(.420)	(.170)	(.372)	(.000)

Note. Two-sided p-values reported between parentheses. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively.

On-line Appendix

B. Additional Analyses



(a) Profit sharing

(b) Loss sharing

Figure B.1: *Proportional Profit and Loss Sharing*

Note: The line represents median sharing at different investment levels.

Table B.1: *Treatment Comparisons (Tobit Regression)*

	Anonymous		Non-anonymous	
	Different village	Same village	Different village	Same village
	(1)	(2)	(3)	(4)
T2: Profit sharing	538.1	514.4*	910.5**	985.5***
	(431.2)	(288.8)	(366.1)	(331.6)
T3: Loss sharing	-37.22	-14.15	-371.8	-313.0
	(320.4)	(244.6)	(251.2)	(249.9)
Constant	2396.7***	1370.0**	2921.9***	2411.9***
	(750.6)	(658.5)	(732.4)	(798.1)
Observations	134	134	134	267

Notes. Tobit regression with higher censoring set at 6000. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.

Table B.2: *Treatment Comparisons (Bootstrapped Standard Errors)*

	Anonymous		Non-anonymous	
	Different village	Same village	Different village	Same village
	(1)	(2)	(3)	(4)
T2: Profit sharing	471.5	434.1	823.4**	864.4**
	(502.6)	(275.7)	(415.9)	(375.1)
T3: Loss sharing	8.206	15.72	-276.1	-243.6
	(142.0)	(148.8)	(252.3)	(254.9)
Constant	2467.4***	1575.4***	2981.4***	2512.8***
	(7.67e-20)	(602.0)	(7.67e-20)	(804.7)
Observations	134	134	134	267
R^2	0.192	0.298	0.181	0.283

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; wild bootstrapped standard errors (in parentheses) to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.

Table B.3: *Treatment Comparisons (Control for Risk Preferences with Dummy Variables)*

	Anonymous		Non-anonymous	
	Different village	Same village	Different village	Same village
	(1)	(2)	(3)	(4)
T2: Profit sharing	387.0	370.6	711.8**	813.0**
	(361.5)	(214.0)	(330.4)	(284.6)
T3: Loss sharing	-26.37	-28.96	-332.1	-195.9
	(293.7)	(216.3)	(238.3)	(228.1)
Constant	2928.8	1085.9	3231.0***	2703.5***
	(2750.9)	(1567.2)	(978.3)	(909.8)
Observations	134	134	134	267
R^2	0.229	0.345	0.211	0.314

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.

Table B.4: *Interaction with Experimental Social Distance*

	(1)	(2)	(3)	(4)
Anonymous	-124.8	-73.67	267.9	-73.67
	(167.4)	(122.0)	(330.5)	(122.0)
Same village	-8.248	18.59	-8.248	-110.6
	(86.42)	(106.9)	(86.42)	(226.8)
T1: Profit/loss sharing			304.9	70.34
			(236.6)	(289.4)
T2: Profit sharing	775.1**	671.2*	1080.0**	741.5**
	(384.1)	(359.0)	(428.7)	(287.0)
T3: Loss sharing	-304.9	-70.34		
	(237.6)	(292.5)		
Anonymous * T1			-392.7	
			(279.2)	
Anonymous * T2	-206.9		-599.6	
	(347.7)		(397.6)	
Anonymous * T3	392.7			
	(277.8)			
Same village * T1				129.2
				(235.8)
Same village * T2		34.60		163.8
		(150.3)		(236.0)
Same village * T3		-129.2		
		(237.0)		
Constant	2464.8***	2428.3***	2160.0***	2357.9***
	(7.67e-20)	(7.67e-20)	(7.67e-20)	(7.67e-20)
Observations	669	669	669	669
R^2	0.216	0.211	0.216	0.211

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; standard errors (in parentheses) obtained with wild bootstrapping to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.

Table B.5: *Interaction with Real-Life Social Distance*

	(1)		(2)		(3)		(4)	
T2: Profit sharing	668.0**	(313.2)	600.8*	(337.0)	649.2	(431.6)	-82.36	(286.4)
T3: Loss sharing	-537.7*	(317.8)	-162.7	(333.5)	-351.2	(420.0)	-1134.0**	(543.1)
Related	-33.88	(197.6)	282.8	(252.6)	338.0	(252.4)	84.25	(168.7)
Related * T2	314.3	(331.5)	385.2	(495.8)	89.51	(384.2)	215.5	(474.2)
Related * T3	473.0	(590.5)	92.02	(589.1)	34.65	(1534.0)	338.1	(623.1)
Friend			625.9**	(283.0)	642.8**	(297.8)	499.6**	(222.8)
Friend * T2			-43.65	(361.5)	-190.9	(386.6)	-249.9	(375.1)
Friend * T3			-749.2*	(383.5)	-711.6*	(415.2)	-728.6	(454.0)
Wealth (pos. dif.)					-11.65	(159.9)	-15.48	(87.64)
Wealth (abs. neg. dif.)					-163.3	(113.0)	-145.3	(123.8)
Wealth (pos. dif.) * T2					42.97	(139.3)	95.95	(126.2)
Wealth (pos. dif.) * T3					45.40	(110.6)	102.4	(96.15)
Wealth (abs. neg. dif.) * T2					181.5	(113.7)	173.7	(119.1)
Wealth (abs. neg. dif.) * T3					150.3	(168.0)	158.4	(204.7)
Risk pref. (pos. dif.)							0.0427	(0.117)
Risk pref. (abs. neg. dif.)							-0.342***	(0.118)
Risk pref. (pos. dif.) * T2							0.127	(0.129)
Risk pref. (pos. dif.) * T3							-0.0115	(1.147)
Risk pref. (abs. neg. dif.) * T2							0.558***	(0.201)
Risk pref. (abs. neg. dif.) * T3							0.620	(0.513)
Constant	2613.4***	(836.0)	2291.7**	(904.9)	2245.0**	(1040.1)	2936.0***	(7.67e-20)
Observations	245		245		241		241	
R^2	0.298		0.311		0.344		0.398	

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; standard errors obtained with wild bootstrapping to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.

C. Correlation between Sharing and Social Distance

In this appendix we test whether the variation in sharing is correlated with experimental and real-life social distance. To analyse correlations between profit/loss sharing and social distance we first observe that within-subject variation in profit sharing is quite limited. A substantial proportion of participants do not change their profit sharing decision across the five different pairs: 22.2% in treatment T1 and 19.05% in treatment T2.

Comparing the proportion of profits and losses shared between pairs from the same village and pairs from a different village with a t-test, we do not find any statistically significant differences in the proportion of profits shared in anonymous pairs (two-sided $p = 0.917$) and non-anonymous pairs (two-sided $p = 0.220$). Doing the same for loss sharing we do not find any significant differences (respective two-sided p-values are 0.463 and 0.202). Comparing the proportion of profits and losses shared between anonymous and non-anonymous pairs, we do not find any statistically significant differences in the proportion of profits shared in pairs from the same village (two-sided $p = 0.638$) and from a different village (two-sided $p = 0.162$). Doing the same for loss sharing we do not find any significant differences (respective two-sided p-values are 0.296 and 0.204)

Finally, analysing the effect of natural variation in social distance among non-anonymous pairs from the same village, we do not find any significant differences between pairs of friends and pairs without a friendship relation (two-sided p-values for profit sharing is 0.434 and for loss sharing = 0.759). Analysing correlations between sharing and differences in wealth and risk preferences we find that all correlation coefficients are not statistically significant except the correlation between the positive difference in risk preferences and loss sharing which is marginally significant (two-sided $p = 0.073$). The coefficient (-0.139) is very small though.

D. Theoretical Predictions

Proof of Lemma 1

According to condition (5) of the Kuhn-Tucker conditions of treatment T1 (shown in Appendix F) the solutions for x^* and p^* need to be such that $[-s.x^*.u'(E + (r - 1 - p^*).x^*) - \lambda_2].p^* = 0$. Because of condition (6) $\lambda_2 \geq 0$. As we also have that $x^* > 0$, $s > 0$ and $u'(.) > 0$ we need that $p^* = 0$ for condition (5) to hold. As the Kuhn-Tucker conditions used are the same for treatment T2, we also have that $p^* = 0$ in treatment T2.

Proof of Proposition 1

If $x_1, x_2 \in]0, E[$ the first-order conditions for treatments T1 and T2 are (see Appendix F for the Kuhn-Tucker conditions):[24]

T1: $\Phi_1(x_1^*) = s.(r - p_1 - 1).u'(E + (r - p_1 - 1)x_1^*) - (1 - s).(1 - l).u'(E - (1 - l)x_1^*) = 0$

T2: $\Phi_2(x_2^*) = s.(r - p_2 - 1).u'(E + (r - p_2 - 1)x_2^*) - (1 - s).u'(E - x_2^*) = 0$

The concavity of u implies that $\Phi_2(.)$ is decreasing in x. Therefore, if we can show that $\Phi_2(x_1^*) < 0$, it must follow that $x_2^* < x_1^*$.

$\Phi_2(x_1^*) = s.(r - p_2 - 1).u'(E + (r - p_2 - 1)x_1^*) - (1 - s).u'(E - x_1^*)$

assuming $p_1 = p_2$ this becomes: $\Phi_2(x_1^*) = (1 - s).(1 - l).u'(E - (1 - l)x_1^*) - (1 - s).u'(E - x_1^*)$

With $l > 0$ and $u'' \leq 0$ the first term is smaller than the second. This is the case as with $l > 0$ we have $E - (1 - l)x \geq E - x$ and given that $u'' \leq 0$, u' remains equal or decreases with larger values so that we have $u'(E - (1 - l)x) \leq u'(E - x)$. This together with the fact that $(1 - s).(1 - l) < (1 - s)$ makes that the first term is smaller than the second. As a result, we have $\Phi_2(x_1^*) < 0$, and therefore $x_2^* < x_1^*$.

If x_2^* and/or x_1^* are corner solutions, we either have $x_2^* < x_1^*$ or $x_2^* = x_1^*$. The former is the case when $x_2^* = 0$ and/or $x_1^* = E$. The latter is the case when $x_1^* = x_2^* = 0$ or $x_1^* = x_2^* = E$. Taking all cases together we have that $x_2^* \leq x_1^*$.

Proof of Proposition 2

[24] The conditions presented in Appendix F apply to the general case where both p and x are decision variables. As with sharing norms we assume that only x is a decision variable we ignore conditions 3, 5, 8 and 10

If $x_1, x_3 \in]0, E[$, the first-order conditions for treatments T1 and T3 are:
T1: $\Phi_1(x_1^*) = s.(r - p_1 - 1).u'(E + (r - p_1 - 1)x_1^*) - (1 - s).(1 - l).u'(E - (1 - l)x_1^*)] = 0$
T3: $\Phi_3(x_3^*) = s.(r - 1).u'(E + (r - 1)x_3^*) - (1 - s).(1 - l).u'(E - (1 - l)x_3^*) = 0$

The concavity of u implies that $\Phi_1(.)$ is decreasing in x. Therefore, if we can demonstrate that $\Phi_1(x_3^*) < 0$, it must follow that $x_1^* < x_3^*$.

$$\Phi_1(x_3^*) = s.(r - p_1 - 1).u'(E + (r - p_1^* - 1)x_3^*) - (1 - s).(1 - l).u'(E - (1 - l)x_3^*)$$
$$\Phi_1(x_3^*) = s.(r - p_1 - 1).u'(E + (r - p_1^* - 1)x_3^*) - s.(r - 1).u'(E + (r - 1)x_3^*)$$

With $p_1 = 0$, $\Phi_1(x_3^*) = 0$ which implies that $x_1^* = x_3^*$. In the other extreme case where $p_1 = r - 1$, we have that $\Phi_1(x_3^*) < 0$, hence $x_1^* < x_3^*$. This together with the fact that $\frac{\partial \Phi_1(x_3^*)}{\partial p} < 0$ makes that there must be a p_{min} between 0 and $r - 1$ above which $\Phi_1(x_3^*)$ becomes negative and therefore $x_1^* < x_3^*$.

If x_1^* and/or x_3^* are corner solutions, we either have $x_1^* < x_3^*$ or $x_1^* = x_3^*$. The former is the case when $x_1^* = 0$ and/or $x_3^* = E$. The latter is the case when $x_1^* = x_3^* = 0$ or $x_1^* = x_3^* = E$. Taking all cases together we have that $x_1^* \leq x_3^*$.

E. Instructions and Procedures

[When people enter the meeting room, they are asked for their name. We have a list of invited candidates. Their name is marked and they are given a sticker with an identity number, which we ask them to stick on their shirt. It is explained that this identity number is unique and allows us to identify them during the exercise while guaranteeing complete confidentiality. This is important, as they are able to earn real money in the exercise.] [They are asked to take a seat in the meeting room. There are two rows of chairs/benches, placed orthogonal to the instruction table. Participants with ID 1-10 are seated on the left row (in increasing order), while participants with ID 11-20 are seated on the right row (in increasing order). Both rows of participants are seated back-to-back. The benches/chairs should be arranged so that no subject can see what another subject is looking at.]

The following instructions are given to all subjects simultaneously while they are seated in the experiment room.

INSTRUCTIONS

Welcome. Thank you for taking the time to come today. [Introduce Experimenters and Assistants]. Later, you can ask any of us questions during today's programme. For this raise your hand so that we can come and answer your question in private.

We have invited you here, today, because we want to learn about how people in this area make decisions. You are going to be asked to make decisions about money. The money that results from your decisions will be yours to keep.

What you need to do will be explained fully in a few minutes. But first we want to make a couple of things clear.

First of all, this is not our money. We belong to a university, and this money has been given to us for research.

Participation is voluntary. You may still choose not to participate in the exercise.

We also have to make clear that this is research about your decisions. Therefore you cannot talk with others. This is very important. I am afraid that if we find you talking with others, we will have to send you home, and you will not be able to earn any money here today. Of course, if you have questions, you can ask one of us. We also ask you to switch off your mobile phones.

Make sure that you listen carefully to us. You will be able to make a good amount of money here today, and it is important that you follow our instructions.

During today's programme, you will be asked to make one or more choices, which will be explained to you very clearly. Only one of your choices will be selected to determine the money you will be paid. At the end of the exercise, we will randomly select one of your decisions to be paid out. Any money you earn will be paid out to you privately and confidentially after all parts of the exercise are complete.

Now, before we explain what you need to do, it is really important to bear one more thing in mind. You will be asked to make decisions that are not a matter of getting it right or wrong; they are about what you prefer. However, it is important to think seriously about your choices because they will affect how much money you can take home.

Part 1

"In the first part of the task you will receive 6000 UGX which will be yours. You will be able to choose how much of these 6000 UGX to invest. You have seven options: invest 0 UGX, invest 1000 UGX, invest 2000 UGX, invest 3000 UGX, invest 4000 UGX, invest 5000 UGX, or invest 6000 UGX. If your investment is successful it is doubled, which means you get a profit equal to the amount of your investment. If your investment is unsuccessful you will lose it, so that you will have a loss equal to what you invested."

[Invite them to come forward around the table where the options are laid out. All option cards are laid out on the table in increasing order of riskiness (with the zero investment option laid out on the left end of the table). In front of each card there are four white counters and one green counter.]

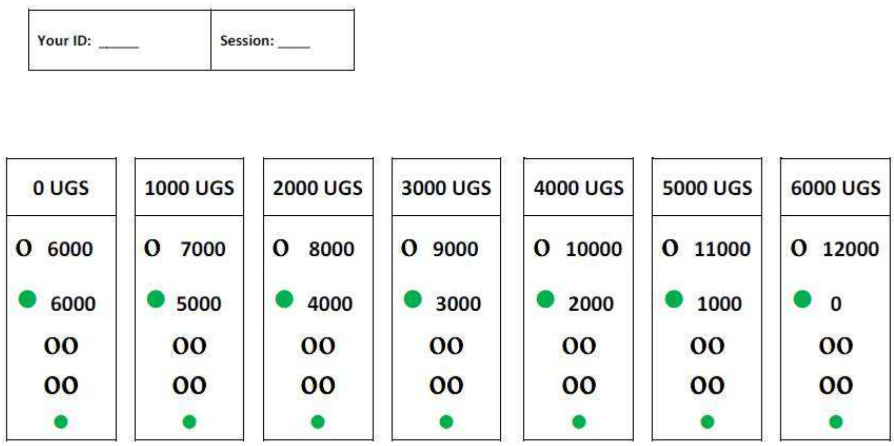


Figure E.1: *Investment Game*

"The choice is between the different options on the table in front of you. You can choose exactly one of these options. Each option consists of one group of 5 counters, where 4 counters are white and 1 is green. Next to each group of counters is a piece of paper which states how much each counter is worth in that option. A white counter indicates the amount you would get if your investment is successful, while a green counter indicates the value you would get if your investment is unsuccessful."

"If you invest 6000 UGX, each white counter is worth 12000 UGX and a green counter is worth 0 UGX. If you invest 5000 UGX, each white counter is worth 11000 UGX and a green counter is worth 1000 UGX. If you invest 4000 UGX, each white counter is worth 10000 UGX and a green counter is worth 2000 UGX. If you invest 3000 UGX, each white counter is worth 9000 UGX and a green counter is worth 3000 UGX. If you invest 2000 UGX, each white counter is worth 8000 UGX and a green counter is worth 4000 UGX. If you invest 1000 UGX, each white counter is worth 7000 UGX and a green counter is worth 5000 UGX. If you invest 0 UGX, both a white and green counter have a value of 6000 UGX."

"After you have made your choice, your earnings will be calculated in the following way. [Show the bag]. We will place the counters from the option you selected into a bag and pick one out without looking. The colour of this counter will determine the amount of money you will get. As there are 4 white counters and only 1 green counter, it is much more likely that you will pick a white counter, than a green counter." [Show how a green and white counter can be picked out of the bag; dont ask them to pick out a counter, as this example may influence their decisions!]

"Let me give you the following examples [show money with each example]. If you chose to invest 0 and picked a white counter, how much would be your profits? (0 UGX). How much would you go home with? (6000 UGX). If you picked a green counter, how much would be your losses? (0 UGX). How much would you go home with? (6000 UGX)."

"If you chose to invest 6000 and picked a white counter, how much would be your profits? (6000 UGX). How much would you go home with? (12,000 UGX). If you picked a green counter, how much would be your losses? (6000 UGX) How much you go home with? (0 UGX)."

"If you chose to invest 1000 and picked a green counter, how much would be your losses? (1000 UGX). How much would you go home with? (5000 UGX) If you picked a white counter, how much would be your profit? (1000 UGX). How much would you go home with? (7000 UGX, that is the investment of 1000 that is doubled and 5000 which you did not invest)."

"If you chose to invest 5000 and picked a white counter, how much would be your profit? (5000 UGX). How much would you go home with? (11000 UGX). If you picked a green counter, how much would be your losses? (5000 UGX). How much would you go home with? (1000 UGX)."

"If you chose to invest 2000 and picked a green counter, how much would be your losses? (2000 UGX). How much would you go home with? (4000 UGX). If you picked a white counter, how much would be your profit? (2000 UGX). How much would you go home with? (8000

UGX)."

"If you chose to invest 4000 and picked a white counter, how much would be your profit? (4000 UGX). How much would you go home with? (10000 UGX). If you picked a green counter, how much would be your loss? (4000 UGX). How much would you go home with? (2000 UGX)."

"If you chose to invest 3000 and picked a green counter, how much would be your losses? (3000 UGX). How much would you go home with? (3000 UGX). If you picked a white counter, how much would be your profit? (3000 UGX). How much would you go home with? (9000 UGX.)"

[Distribute decision cards] "To make your decision we will use the following decision card. It shows the same 7 options as the ones presented on the table. Out of these 7 options we ask you to select one." [Show the decision card, and indicate where they can find the different options and how they correspond to the options presented on the table. Explain where they have to indicate their investment choice.]

[Participants return to their seats]

CONTROL QUESTIONS

"We will now ask some questions to see whether you understood the instructions."

[The experimenter assistants call each participant one by one. First, they explain the different options again. Then, they ask the following four questions making reference to the decision cards that they carry with them.]

- If you chose to invest 2000 UGX, how much would be your profits if you picked a white counter out of the bag? (2000 UGX) How much would you go home with? (8000 UGX)
- If you chose to invest 5000 UGX, how much would be your losses if you picked a green counter out of the bag (5000 UGX)? How much would you go home with? (1000 UGX)

[For each of the questions, record on the control question card whether they answered it correctly. If the participant gave a wrong answer for at least one of the questions, ask him/her what was not clear. Answer their questions as clearly and accurately as possible. If necessary, clarify the instructions; but not more than once. Retain their decision cards.]

DECISIONS

[Give each participant a pen.] "If you have no further questions, we will now begin. Please indicate the investment option you choose. Remember, there are no wrong choices, so you should invest exactly as much as you prefer."

"We emphasise that it is important that you make your choice in private. Do not show your decision sheet to the other participants. If you need assistance, please raise your hand so that one of us can come to you to assist you. Once you have made your choice, please fold the decision sheet and raise your hand so that we can come and collect your decision card."

[The participants remain seated. Experimenter assistants give decision cards to the participants. IDs are already filled in by the experimenters. After the participants have made their choice, they fold their decision card, and we collect them. The central administrator enters investment decisions in an excel data sheet. When all participants have made their decision, Part 1 is complete.]

Part 2

INSTRUCTIONS *(These instructions are for treatment T1. Those for treatments T2 and T3 are identical apart from obvious modifications.)*

[stick posters of investment decision card, profit sharing card and loss sharing card to the wall]

[Participants remain seated.] "Thank you, you have now all completed the first part of the task. We will now explain the second part of the task. In this part, each of you will be paired with several other persons in the room."

"In each pair we have two persons: person 1 and person 2. Both persons receive an income of 6000 UGX. In addition, persons 1 have an investment opportunity. These persons will be asked to choose how much to invest of the 6000 UGX. In other words, they are asked to choose again between the different investment options. They may make the same decision that they made in the first part of the task, or they may decide to change their decision, and invest a different amount."

"If the investment of person 1 is successful (by picking out a white counter of the bag), she may share any of her profit with person 2."

"If person 1 loses her investment (by picking out a green counter of the bag) person 2 may share part of his income with person 1 to compensate any of the losses."

"For example, if person 1 chooses to invest 4000 UGX, and the investment is successful (white counter), then the investment is doubled and person 1 has a profit of 4000 UGX. He is then free to share any part of this profit with person 2."

"If the investment fails, then person 1 looses 4000 UGX and person 2 can choose to share any amount from his/her earnings of 6000 UGX to compensate any of the investment losses of person 1."

"The possibility to share profits or losses depends on the investment decision of person 1. The more he invests the more he might be able to share if the investment is successful. Also, the more he invests the more person 2 might be able to compensate if the investment is unsuccessful."

"To make decisions, we will proceed in the following way. [Distribute and explain investment decision card and profit sharing card]. For persons 1 to make an investment decision they use a similar decision card as before. They mark the option of their preference on their decision card." [Make use of the poster of the investment decision card to explain how to use it]

"After having made an investment decision, they will be asked whether they would be willing to redistribute part of their profit if their investment is successful. Importantly, this decision will be implemented if the investment is successful (that is, if he picks out the white counter out of the bag.). For this, they make use of the following decision sheet." [Make use of the poster of the profit sharing card to explain how to use it]. Before making their sharing decision, we ask them to fill in the amount of profits they would get if their investment is successful. This is the maximum amount that they are able to share with the other person. Then they decide how much of these profits they would like to share. They mark the option of their preference on their decision card. This decision will be implemented if they pick out a white counter. We will then ask persons 1 to fold both decision cards and raise their hand so that we can come and collect your decision card.

"The investment decision card (not the profit sharing card!) will then be passed to the persons 2 they are paired with. By doing so, persons 2 will be informed of the investment decision of the person 1 they are paired with. Persons 2 should then look carefully at the investment choice to find out what person 1 would get in case of a successful investment and what he would lose in case of an unsuccessful investment."

[Distribute and explain the loss sharing card. Make use of the poster of the loss sharing card to explain how to use it] "Persons 2 are then asked how much losses they would like to compensate in case the investment of person 1 is unsuccessful. For this, they first fill in the amount person 1 would lose in case her investment is unsuccessful. Then, they decide how much of these losses they would like to compensate by sharing part of their income, by marking the option of their preference. This decision will be implemented if the investment of person 1 unsuccessful (that is, if person 1 picks out the green counter out of the bag)."

[Role-play to explain decisions and procedures; making use of the poster]

"You will be paired with several persons in this room. You will never be linked twice with the same person."

"In some pairs the other person will be from the same village, in other pairs he/she will be from another village." [Show on the poster of the investment decision card where it will be indicated whether 'same/different village']

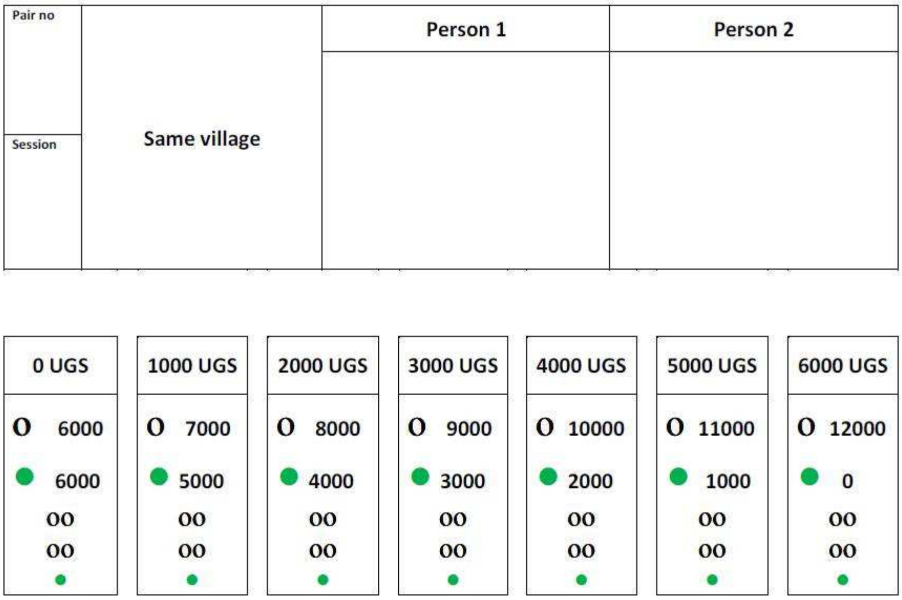


Figure E.2: *Investment Game*

”In some pairs you won’t know the identity of the person you are paired with, and neither will the other person get to know your identity. In other pairs, you will know the identity of the person you are paired with, and the person you are paired with will know your identity. In the latter case, we will communicate the name of the person you are paired with and show a photograph. We will also communicate your name to that person and show a photograph of you.” [Show on the poster of the investment decision card where they can find the names and photographs of both persons].

”For each of the pairs you are involved in you may have to make a decision. For each pair you will receive a new decision card. You may make the same decision or you may invest a different amount for each pair. Only one of the pairs will be selected for your payment.”

”Participants with ID 1-10 will be a person 1 in all the pairs in which they are involved. Participants with ID 11-20 will always be a person 2.”

QUESTIONS

”We will now give you the opportunity to ask questions in private. Please raise your hand if you have any questions.” [The 2 experimenter assistants and experimenter administrator invite participants with questions to come forward and discuss their questions in private. Answer any remaining questions as clearly and accurately as possible. If necessary, clarify the instructions.]

DECISIONS

”We will now begin. Remember, there are no right or wrong choices, so make the decision that you prefer.”

”Persons 1, we ask you to make an investment decision for the first pair, by marking the option of preference on your decision card.” [Assistants distribute the decision cards of pair no 1.]

”To give you complete privacy, we ask you to fold all your decision cards before returning them to one of the experimenter assistants. Raise your hand so that we can come and collect them.”

[Experimenter assistants collect decision cards, verify whether participants filled in the correct amount of profit, and did not share more than this amount. If a participant shares more than allowed, assistants will ask the participant to reconsider his choice.]

[When a decision card is collected the profit sharing card is torn off and put in the second partition of the sorting box with the corresponding ID of person 1.]

[The investment decision card is stapled together with a clean loss sharing card. The pair number is filled in, and the set of cards is placed in the sorting box of the corresponding person 2. They are passed to the person 2 if he is not busy with one of the previous pairs.]

[After collecting the set of decision cards from person 2 assistants verify whether the participant filled in the correct amount of losses, and did not compensate more than this amount. If a participant shares more than allowed, assistants will ask the participant to reconsider his choice. Assistants put the set of decision cards in the second partition of the sorting box with the corresponding ID of person 1.]

[Repeat this procedure for pairs 2, 3, 4, 5]

[After all decisions have been made and the experimenter administrator closes the session, the assistants staple the profit sharing cards back to the corresponding investment decision card of the paired person 1].

”Thank you, you have now all completed all parts of the task. Your earnings from all your decisions will now be determined in the following way. As we explained before, we will randomly select one of your 6 decisions to be paid out (1 individual decision, and 5 pairs). For this, we put 6 cards with numbers 1 to 6 on them in a bag, and pick out one card without looking. The number on the card that is picked out determines the decision which will be used to calculate your earnings. [Ask one volunteer to pick out a card] We now invite you to come forward, one by one, to determine your earnings.”

”Thank you for coming today, your participation has been greatly appreciated.”

[First, persons 1 are called in one by one. We explain them what investment decision they made (including the sharing decision in treatments T1 or T2) as well as the decision made by the person 2 they were paired with (in treatments T1 or T3, and if one of decisions 2-6 is selected), so that they understand how their earnings are calculated. Then, they pick out a counter from the bag to determine whether their investment is successful or unsuccessful.]

[After all persons 1 received their payment, persons 2 are called in one by one. We explain them what decision they made as well as the decision of the person 1 they were paired with (if one of decisions 2-6 is selected), so that they understand how their earnings are calculated. If decision 1 is selected, persons 2 are also asked to pick out a counter from the bag.]

[After the session, put all used decision cards in one big envelope, and write on it date, time and code of session. Close and seal the envelope.]

F. Kuhn-Tucker Conditions

T1: Max $EU(x) = s.u(E + (r - 1 - p).x) + (1 - s).u(E - (1 - l)x)$

s.t.

$0 \leq p$

$0 \leq x$

$0 \leq 1 - p$

$0 \leq 6000 - x$

$L(x, p, \lambda_1, \lambda_2) = s.u(E + (r - 1 - p).x) + (1 - s).u(E - (1 - l)x) + \lambda_1.(E - x) + \lambda_2.(1 - p)$

(1) $x, p \geq 0$

(2) $\frac{\partial L}{\partial x} = s.(r - 1 - p).u'(E + (r - 1 - p).x) - (1 - s).(1 - l).u'(E - (1 - l)x) - \lambda_1 \leq 0$

(3) $\frac{\partial L}{\partial p} = -s.x.u'(E + (r - 1 - p).x) - \lambda_2 \leq 0$

(4) $[s.(r - 1 - p).u'(E + (r - 1 - p).x) - (1 - s).(1 - l).u'(E - (1 - l)x) - \lambda_1].x = 0$

(5) $[-s.x.u'(E + (r - 1 - p).x) - \lambda_2].p = 0$

(6) $\lambda_1, \lambda_2 \geq 0$

(7) $E - x \geq 0$

(8) $1 - p \geq 0$

(9) $(E - x).\lambda_1 = 0$

(10) $(1 - p).\lambda_2 = 0$

T2: Max $EU(x) = s.u(E + (r - 1 - p).x) + (1 - s).u(E - x)$

s.t.

$0 \leq p$

$0 \leq x$

$0 \leq 1 - p$

$0 \leq E - x$

$L(x, p, \lambda_1, \lambda_2) = s.u(E + (r - 1 - p).x) + (1 - s).u(E - x) + \lambda_1.(E - x) + \lambda_2.(1 - p)$

(1) $x, p \geq 0$

(2) $\frac{\partial L}{\partial x} = s.(r - p - 1).u'(E + (r - 1 - p).x) - (1 - s).u'(E - x) - \lambda_1 \leq 0$

(3) $\frac{\partial L}{\partial p} = -s.x.u'(E + (r - 1 - p).x) - \lambda_2 \leq 0$

(4) $[s.(r - 1 - p).u'(E + (r - 1 - p).x) - (1 - s).u'(E - x) - \lambda_1].x = 0$

(5) $[-s.x.u'(E+(r-1-p).x)-\lambda_2].p=0$

(6) $\lambda_1,\lambda_2 \geq 0$

(7) $E-x \geq 0$

(8) $1-p \geq 0$

(9) $(E-x).\lambda_1 = 0$

(10) $(1-p).\lambda_2 = 0$

T3: Max $EU(x) = s.u(E+(r-1).x)) + (1-s).u(E-(1-l).x)$

s.t.

$0 \leq x$

$0 \leq E-x$

$L(x,\lambda_1) = s.u(E+(r-1).x)) + (1-s).u(E-(1-l)x) + \lambda_1.(E-x)$

(1) $x \geq 0$

(2) $\frac{\partial L}{\partial x} = s.(r-1).u'((rx+(E-x)) - (1-s).(1-l).u'(E-(1-l)x) - \lambda_1 \leq 0$

(3) $[s.(r-1).u'(rx+(E-x)) - (1-s).(1-l).u'(E-(1-l)x) - \lambda_1].x = 0$

(4) $\lambda_1, \geq 0$

(5) $E-x \geq 0$

(6) $(E-x).\lambda_1 = 0$

G. Simulation of the Effect of Declining Loss Sharing

To show that the observed decline in loss sharing will not be large enough to make people prefer lower investment levels under the assumption of risk aversion let's use the following utility function: $U(x) = x^{(1-r)}/(1-r)$

For $U(5000) > U(6000)$ we need that $0.8.(1000 + 10000)^{(1-r)}/(1-r) + 0.2(1000 + l(5000))^{(1-r)}/(1-r) > 0.8.(0+12000))^{(1-r)}/(1-r) + 0.2(0+l(6000))^{(1-r)}/(1-r)$. Let's set loss sharing $l(5000) = 2000$ and $l(6000) = 1000$. The table below shows the utility differences for different risk aversion levels. In none of the risk aversion levels does a negative sign of the utility difference change into a positive sign when moving from a situation without loss sharing (note we set l equal to a value close to zero but different from zero, to avoid division by zero when (1-r) is negative) to a situation with loss sharing. As a result, with the option of loss sharing (and the given loss sharing parameters) investment decisions would not change, hence lower loss sharing would not lower player 1's investment levels.

Table G.1: *Simulation*

	Without loss sharing			With loss sharing		
Investment	5000	6000		5000	6000	
Loss sharing	0.0000001	0.0000001		2000	1000	
	U(5000)	U(6000)	U(5000)-U(6000)	U(5000)	U(6000)	U(5000)-U(6000)
r						
0.3	806.81	819.26	-12.44	848.45	855.22	-6.77
0.4	375.66	373.64	2.03	395.29	394.67	0.62
0.5	180.46	175.27	5.19	189.72	187.92	1.80
0.7	48.79	44.65	4.14	50.85	49.94	0.92
0.9	24.28	20.86	3.41	24.74	24.46	0.29
1.1	-4.16	-13.15	8.99	-4.05	-4.13	0.08
1.3	-0.25	-84.09	83.84	-0.22	-0.24	0.02
1.5	-0.03	-1264.93	1264.90	-0.02	-0.03	0.005
2	-0.00027	-2000000	2000000	-0.00014	-0.00027	0.00013
3	-1.03E-07	-1E+13	1E+13	-1.44E-08	-1.03E-07	8.84E-08
4	-6.69E-11	-6.7E+19	6.67E+19	-2.67E-12	-6.68E-11	6.42E-11

As can be seen from the first columns (without loss sharing), the point at which the decision maker becomes indifferent between choosing 5000 and 6000 lies somewhere between a coefficient of CRRA of 0.3 and 0.4. It can therefore be concluded that only for subjects with low risk aversion, anticipated lower loss sharing can be relevant, as subjects with higher risk aversion would not invest 5000 or 6000 (the area where a possible decline in loss sharing could be observed).

H. Analysis of Potential Selection Bias

An important decision was made on the selection of the participants for the analysis. For all analyses we only included the participants who answered at least 3 out of the 4 control questions correctly. To test whether our results are valid for the whole population we need to be sure that our results do not suffer from selection bias. The table below shows the descriptive statistics for the excluded and non-excluded participants. Participants excluded from the analysis tend to be more likely female, older and poorer. However, their risk preferences are not different, as elicited by the decision in the first part of the experiment.

Table H.1: *Characteristics of Included and Excluded Participants*

	non-excluded	excluded	
	270	48	p-value
Gender (male)	52.59%	33.33%	0.014**
Age	40.19	44.23	0.057*
Wealth	0.24	-0.67	0.020**
Investment (part 1)	4051.28	3791.67	0.309

Note. For gender we report the two-sided p-value of a chi-square test, while for age, wealth and investment (part 1) we report the lowest two-sided p-value of a Bonferroni multiple-comparison test.

To test whether the results of Table 3 are robust to changing the cut-off point of the number of correctly answered control questions we estimated the same models with different selection criteria. When we include all subjects (see Table H.2 below) the coefficient of T2 becomes smaller (483.1* in Model 3 and 643.4*** in Model 4). If we become more selective (only including the participants that answered all 4 control questions correctly), the estimated coefficient of T2 in Models 3 and 4 becomes larger as well as its statistical significance (1050.5*** and 976.6***, respectively).

These differences in results would be a source of concern if they were caused by selection bias. For selection bias to exist the characteristics on which the excluded participants differ from the included participants should influence the treatment effects. To test whether the treatment effects are influenced by the gender, age or wealth of player 1 we run additional regression models in which we add an interaction term between each of the treatments T2 and T3 and the characteristics of player 1, being gender, age or wealth, and we only use the sample of participants who answered at least 3 control questions correctly. The tables below show the results. We do not find any strong interaction effects with T2 (except with gender, but only so in model 2; and a marginally significant interaction effect with wealth in model 1), on which our main result is

Table H.2: *Treatment Effects with Full Samples*

	Anonymous		Non-anonymous	
	Different village	Same village	Different village	Same village
	(1)	(2)	(3)	(4)
T2: Profit sharing	175.2	231.0	483.1*	643.4***
	(316.0)	(196.3)	(269.9)	(215.8)
T3: Loss sharing	-39.52	-38.68	-347.6*	-325.0
	(219.2)	(195.4)	(174.6)	(189.0)
Constant	2741.4***	1584.6***	3069.0***	3236.8***
	(587.7)	(463.3)	(517.4)	(704.1)
Observations	160	160	160	319
R^2	0.152	0.279	0.125	0.195

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.

based .

In sum, the fact that our main result (the treatment effect of T2) remains robust to including all subjects is reassuring, as is the fact that gender, age and wealth (the main characteristics on which the excluded participants differed from the included participants) do not strongly interact with this treatment effect.

Table H.3: *Interaction between Gender and Treatment Effects*

	Anonymous		Non-anonymous	
	Different village	Same village	Different village	Same village
	(1)	(2)	(3)	(4)
Male player 1 * T2	304.2	-1192.2**	-309.2	-64.11
	(641.7)	(541.8)	(713.0)	(352.5)
Male player 1 * T3	17.03	-874.1	-1534.2	-247.6
	(776.9)	(777.8)	(888.8)	(601.2)
T2: Profit sharing	310.1	1090.7**	1032.7	906.0***
	(564.2)	(404.8)	(639.8)	(303.9)
T3: Losses sharing	-9.045	527.6	576.2	-105.7
	(533.4)	(474.4)	(596.6)	(357.7)
Male player 1 (dummy)	-172.9	668.0**	829.6*	-95.26
	(453.6)	(234.8)	(408.1)	(303.6)
Constant	2524.5***	1211.5**	2659.9***	2458.4***
	(625.8)	(536.4)	(544.3)	(706.0)
Observations	134	134	134	267
R^2	0.194	0.322	0.217	0.284

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.

Table H.4: *Interaction between Age and Treatment Effects*

	Anonymous		Non-anonymous	
	Different village	Same village	Different village	Same village
	(1)	(2)	(3)	(4)
Age player 1 * T2	-6.348	13.17	-8.226	-19.38
	(21.54)	(18.80)	(21.25)	(20.00)
Age player 1 * T3	-12.68	-29.48**	-32.69	-27.23
	(24.73)	(13.89)	(20.71)	(18.31)
T2: Profit sharing	725.3	-61.76	1161.2	1633.4*
	(814.1)	(737.8)	(842.3)	(879.9)
T3: Losses sharing	539.4	1305.2*	1109.5	886.1
	(1055.7)	(636.9)	(898.4)	(837.0)
Age player 1	9.475	-6.588	0.871	6.006
	(13.85)	(10.60)	(18.66)	(9.212)
Constant	2236.5**	1418.8*	2496.9***	1938.6**
	(810.6)	(711.9)	(819.1)	(782.3)
Observations	134	134	134	267
R^2	0.194	0.315	0.191	0.291

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.

Table H.5: Interaction between Wealth and Treatment Effects

	Anonymous		Non-anonymous	
	Different village	Same village	Different village	Same village
	(1)	(2)	(3)	(4)
Wealth player 1 * T2	170.5*	93.66	38.91	-28.16
	(90.97)	(196.1)	(212.1)	(106.8)
Wealth player 1 * T3	159.1	211.0*	46.82	268.8**
	(131.7)	(105.0)	(100.4)	(114.2)
T2: Profit sharing	434.8	352.7	809.8**	726.3***
	(407.6)	(257.8)	(333.2)	(250.5)
T3: Losses sharing	-64.41	-70.95	-296.6	-343.1*
	(323.0)	(234.9)	(218.1)	(195.6)
Wealth player 1	-21.16	-18.21	4.652	18.43
	(61.44)	(92.98)	(85.53)	(82.02)
Constant	2518.1***	1591.1**	2991.9***	2475.3***
	(679.3)	(591.0)	(641.5)	(715.0)
Observations	134	134	134	267
R^2	0.203	0.313	0.181	0.310

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within experimental sessions; regional fixed effects were used, as well as controls for experimenter effects, gender, age, wealth and individual risk preferences.