INVESTMENT BEHAVIOUR, RISK SHARING AND SOCIAL DISTANCE*

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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 (Collier and Gunning, 1999). 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-subistence 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

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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 year, only about a third of farmers report having ever used fertiliser.
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; di Falco and Bulte, 2011; Jakiela and Ozier, 2012). 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 dampens 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 (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 in 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).

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 article 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 to which the investment may give rise. 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 article 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 (Barr and Genicot, 2008; Attanasio et al., 2012; Barr et al., 2012). 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.

The rest of the article is organised as follows. In Section 1, we present the research design, including the experimental game and procedures used as well as some theoretical considerations and hypotheses. In Section 2, we describe the data and present the empirical analyses. Section 3 reviews plausible explanations of our findings. Section 4 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 pay-off $E + (r - 1)x$, or unsuccessful in which case the investment is lost and the pay-off is $E - x$. The likelihood of a successful investment is $s$, which is given, with $0 < s < 1$. As a result, each participant chooses his preferred prospect
\[ F = (s, E + (r - 1)x; (1 - s), E - x) \] by setting \( x \). The decisions made in Part 1 are used 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, he 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 subsection, 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 influence of sharing norms on the treatment effects.

In Part 1, the optimisation 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 maximisation problem:

\[
\text{Max } \mathbb{E} U(x) = su[E + (r - 1)x] + (1 - s)u(E - x)
\]  

with \( r > 1 \), being the investment return, and \( u(\cdot) \) 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), his optimisation problem is changed in the following way. With profit sharing (treatments T1 and T2) the first term is replaced by $su[E + (r - p - 1)x]$, 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 online Appendix D).

**Lemma 1.** With standard preferences, absence of sharing norms and $x_1^s, x_2^s > 0$, 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 him for any losses (i.e. $l_1 = l_2 = 0$) makes that the sharing terms in the optimisation functions disappear, and the optimisation function of each treatment becomes equal to (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 optimisation function. If sharing norms make that $l > 0$ and profit sharing is orthogonal to the option of loss sharing (i.e. $p_1 = p_2$), it can be shown that Player 1 will invest at least as much if loss sharing is an option as he would if it was not, that is $x_2^* \leq x_1^*$. Proposition 1 formally demonstrates this effect (for proofs see the online Appendix D).

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

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 his 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 online 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

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

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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 $E = 6,000$ Ugandan Shillings (UGX) and choices are limited to $x \in \{0, 1,000, 2,000, 3,000, 4,000, 5,000, 6,000\}$. Also, $r = 2$ so that investments are doubled if successful, and both profits and losses are equal to the amount invested. Finally, we set $s = 0.8$ so that Player 1 chooses her preferred prospect $F = (0.8, E + x; 0.2, E - x)$ by setting $x$. This translates into the pay-offs presented in Table 1.

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 five 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. 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 total, 360

Table 1

| Investment | Successful investment | Unsuccessful investment |
|------------|-----------------------|-------------------------|
|            | Profits   | Payoff    | Losses   | Payoff    | EV         |
| 0          | 0         | 6,000     | 0        | 6,000     | 6,000      |
| 1,000      | 1,000     | 7,000     | 1,000    | 5,000     | 6,600      |
| 2,000      | 2,000     | 8,000     | 2,000    | 4,000     | 7,200      |
| 3,000      | 3,000     | 9,000     | 3,000    | 3,000     | 7,800      |
| 4,000      | 4,000     | 10,000    | 4,000    | 2,000     | 8,400      |
| 5,000      | 5,000     | 11,000    | 5,000    | 1,000     | 9,000      |
| 6,000      | 6,000     | 12,000    | 6,000    | 0         | 9,600      |

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.

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.

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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.\textsuperscript{11} We organised eight different sessions of treatment T1 and five sessions of treatments T2 and T3 each. Average earnings were 9,892 UGX (including a 2,000 UGX show up fee), equal to 3.81 US$, that is 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 read numbers easily (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 at least three of the four control questions correctly, representing 83.9\% of the participants. We assume that participants who answered less 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 online Appendix E.

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 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 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:

\textsuperscript{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.

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(i) the assumption that Player 2 is expected to share some of the losses; and
(ii) 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.

Fig. 1. Profit and Loss Sharing

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

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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.\textsuperscript{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 his risky investment that led to failure, or if he has to share half of the profits if the investment was successful (for experimental evidence on this see, for example Cappelen \textit{et al.}, 2013).\textsuperscript{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 1 not share any profits and in only 8–14% 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).

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 T1 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 subsection will test whether that is indeed the case.\textsuperscript{14}

2.2. \textit{Do Profit and Loss Sharing Matter for Investment?}

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

\textsuperscript{12} Gowdy \textit{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) and Henrich \textit{et al.} (2006).

\textsuperscript{13} Figure B1 in the online 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.

\textsuperscript{14} Online 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 T3. In particular, we find that the two highest investment levels (5,000 and 6,000) 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 A1 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

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Fig. 3. Distribution of Investment Decisions by Treatment and Type of Pair; (a) Different Village – Anonymous; (b) Same Village – Anonymous; (c) Different Village – Non-anonymous and (d) Same Village – Non-anonymous
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 1,000) 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.

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 A2 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.

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).

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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 3,539.1 and 4,362.5 in T1 and T2 respectively and 3,558.0 and 4,422.4 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 6,000 (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 Table 3

|               | Anonymous |          | Non-anonymous |          |
|---------------|-----------|----------|---------------|----------|
|               | Different village | Same village | Different village | Same village |
| 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      | 2,467.4***| 1,575.4**| 2,981.4***    | 2,512.8***|
|               | (675.4)   | (599.5)  | (637.2)       | (724.9)  |
| Observations  | 134       | 134      | 134           | 267      |
| R²            | 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.

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).
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 B1, B2 and B3 in the online Appendix B and are not qualitatively different from the OLS estimates presented here. We summarise the main observations in a first result.

**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

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:

1. **Anonymity versus identity revealed:**
2. **Co-villager versus residing in different villages.**

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

1. A dummy that indicates whether the pairs of players are anonymous (model 1) and
2. 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 $\times$ T1 and 0.108 for the coefficient of Anonymous $\times$ 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 B4 in the online 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.\textsuperscript{16} We summarise the observations in a second result.

\textbf{RESULT 2.} \textit{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.}

\textbf{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

\textsuperscript{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.

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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 were 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 A3, 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).\(^\text{17}\)

To analyse the interaction between real-life social distance and the treatment effects, we follow a similar approach as in the previous subsection. 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 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, that is 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 co-efficient on Friend × T3 is of similar size as the co-efficient 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 co-efficient on the negative difference variable together with a positive co-efficient (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 co-efficient 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.

\(^{17}\) 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.

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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, is quantified in the following table:

Table 5

| Interaction with Real-life Social Distance | (1)       | (2)       | (3)       | (4)       |
|------------------------------------------|-----------|-----------|-----------|-----------|
| T2: profit sharing                       | 644.1**   | 622.8     | 833.5**   | \( \text{-35.24} \) |
|                                           | (296.7)   | (384.0)   | (417.3)   | (295.9)   |
| T3: loss sharing                         | \(-650.1**\) | \(-192.8\) | \(-322.5\) | \(-1,188.6**\) |
|                                           | (316.4)   | (368.4)   | (464.2)   | (555.5)   |
| Related                                  | \(-81.77\) | 359.8     | 474.1**   | 192.4     |
|                                           | (292.3)   | (237.1)   | (235.6)   | (194.5)   |
| Related \( \times \) T2                  | 598.6     | 616.8     | 292.7     | 313.2     |
|                                           | (464.5)   | (645.6)   | (579.1)   | (522.7)   |
| Related \( \times \) T3                  | 588.1     | 122.8     | 2353      | 337.3     |
|                                           | (620.0)   | (604.8)   | (622.0)   | (635.6)   |
| Friend                                    | 899.4**   | 944.3**   | 782.3**   | (326.2)   |
|                                           | (365.8)   | (330.6)   | (326.2)   |           |
| T2: profit sharing                       | \(-189.7\) | \(-438.7\) | \(-554.7\) |           |
| T3: loss sharing                         | \(-588.9\) | (508.0)   | (495.4)   |           |
| Related \( \times \) T2                  | \(-939.5**\) | \(-909.1**\) | \(-953.5^*\) |           |
| T2: profit sharing                       | (423.3)   | (451.0)   | (517.3)   |           |
| Wealth (pos. dif.)                       | 51.47     | 32.04     |           |           |
|                                           | (113.7)   | (102.3)   |           |           |
| Wealth (abs. neg. dif.)                  | \(-179.4**\) | \(88.96\) |           |           |
| Wealth (pos. dif.) \( \times \) T2      | \(-76.81\) | (160.2)   | (132.3)   |           |
| Wealth (pos. dif.) \( \times \) T3      | 8.586     | 83.63     |           |           |
| Wealth (abs. neg. dif.) \( \times \) T2 | 196.4*    | 134.2     |           |           |
| Wealth (abs. neg. dif.) \( \times \) T3 | 103.5     | 106.8     |           |           |
| Risk pref. (pos. dif.)                   | 0.062     |           |           |           |
| Risk pref. (abs. neg. dif.)              | \(-0.345**\) | \(0.92\) |           |           |
| Risk pref. (pos. dif.) \( \times \) T2  | 0.176     |           |           |           |
| Risk pref. (pos. dif.) \( \times \) T3  | \(-0.041\) |           |           |           |
| Risk pref. (abs. neg. dif.) \( \times \) T2 | 0.663**   | \(0.195\) |           |           |
| Risk pref. (abs. neg. dif.) \( \times \) T3 | 0.699**   | \(0.354\) |           |           |
| Constant                                 | 2,518.9*** | 2,060.4** | 1,847.5*  | 2,557.1*** |
|                                           | (848.1)   | (817.6)   | (975.6)   | (941.1)   |
| Observations                             | 245       | 245       | 241       | 241       |

Notes. Tobit regression with higher censoring set at 6,000. *** ** * 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 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 co-efficient 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 B5 in the online Appendix B). We summarise the main observations in a third result.

**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.*

### 3. Discussion of Plausible Explanations

Summarising our results, we found that:

(i) 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;

(ii) 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

(iii) 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. As a first step, we 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.

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 co-efficient of the investment level (0.406***, two-sided $p = 0.001$) and a (marginally) significant negative co-efficient 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 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 us first assume risk neutrality. With a 1,000 increase in investment we have an expected increase in gains of $0.8 \times 1,000 = 800$. For Player 1 to make the decision to increase investment by 1,000, 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 4,000 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 online 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 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 if the investment fails. If Player 1 has altruistic preferences he 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

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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.
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 subsection, we 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 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 him not to implicate Player 2 in his 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

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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 ≤ T1 and T2 ≤ 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.

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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 (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, 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. 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.

In fact, Leider et al. (2009) demonstrated that both altruism and future enforced reciprocity play a role in sharing with friends.
We argued that altruism may help explain these paradoxical findings, because an 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.

Appendix A. Descriptives

Table A1

Distribution of Investment Decisions (in percentage)

|               | (a) Different village | (b) Same village |
|---------------|-----------------------|------------------|
|               | T1        | T2        | T3        | Total    | T1        | T2        | T3        | Total    |
| Anonymous pairs |          |          |          |          |          |          |          |          |
| 0             | 5.56     | 2.38     | 0.00     | 2.96     | 1.85     | 2.38     | 0.00     | 1.48     |
| 1,000         | 9.26     | 0.00     | 12.82    | 7.41     | 12.96    | 2.58     | 12.82    | 9.63     |
| 2,000         | 16.67    | 14.29    | 12.82    | 14.41    | 22.22    | 14.29    | 17.95    | 18.52    |
| 3,000         | 9.26     | 9.52     | 10.26    | 9.63     | 9.26     | 2.58     | 12.82    | 11.11    |
| 4,000         | 25.93    | 30.95    | 35.90    | 30.37    | 16.67    | 19.05    | 28.21    | 20.74    |
| 5,000         | 18.52    | 21.43    | 17.95    | 19.26    | 22.22    | 23.81    | 17.95    | 21.48    |
| 6,000         | 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   |

|               | (a) Different village | (b) Same village |
|---------------|-----------------------|------------------|
| Non-anonymous pairs |          |          |          |          |          |          |          |          |
| 0             | 0.00     | 0.00     | 2.56     | 0.74     | 0.95     | 0.00     | 1.28     | 0.74     |
| 1,000         | 12.96    | 7.14     | 23.08    | 14.07    | 15.74    | 3.61     | 19.23    | 13.01    |
| 2,000         | 14.81    | 0.00     | 7.69     | 8.15     | 12.04    | 3.61     | 14.1     | 10.04    |
| 3,000         | 16.67    | 16.67    | 10.26    | 14.81    | 12.96    | 14.66    | 12.82    | 13.38    |
| 4,000         | 25.93    | 16.67    | 30.77    | 24.44    | 21.3     | 21.69    | 26.92    | 23.05    |
| 5,000         | 11.11    | 30.95    | 15.38    | 18.52    | 18.52    | 30.12    | 16.67    | 21.56    |
| 6,000         | 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 A2

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  | 4,051.28 | 3,877.19 | 4,282.35 | 4,054.05 | 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.

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Table A3

Descriptive Statistics – Pairs

|                  | N  | Mean | SD   |
|------------------|----|------|------|
| Know the other person | 246 | 92.7 |      |
| Kinship          | 246 | 42.7 |      |
| Friend           | 246 | 31.3 |      |

|                  | N  | Mean  | SD   |
|------------------|----|-------|------|
| Wealth (pos. dev.) | 242 | 1.097 | 1.616 |
| Wealth (abs. neg. dev.) | 242 | 0.976 | 2.109 |
| Risk pref. (pos. dev.) | 247 | 793.522 | 1.223.864 |
| Risk pref. (abs. neg. dev.) | 247 | 878.543 | 1.334.892 |

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

Table A4

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 |                   |                       |                      |
| Wealth (pos. dev.) | -0.028 | -0.065 | 1.000             |                       |                      |
|                   | (0.642) | (0.275) |                   |                       |                      |
| Wealth (abs. neg. dev.) | 0.039 | 0.025 | -0.306*** | 1.000             |
|                   | (0.513) | (0.673) | (0.000)           |                       |                      |
| Risk pref. (pos. dev.) | -0.022 | 0.048 | 0.055             | -0.0004              | 1.000               |
|                   | (0.711) | (0.416) | (0.357)           | (0.995)              | (0.372)             |
| Risk pref. (abs. neg. dev.) | -0.036 | 0.048 | -0.081            | 0.053               | -0.430***           |
|                   | (0.539) | (0.420) | (0.170)           | (0.372)              | (0.000)             |

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

Additional Supporting Information may be found in the online version of this article:

Appendix B. Additional Analyses.
Appendix C. Correlation between Sharing and Social Distance.
Appendix D. Theoretical Predictions.
Appendix E. Instructions and Procedures.
Appendix F. Kuhn–Tucker Conditions.
Appendix G. Simulation of the Effect of Declining Loss Sharing.
Appendix H. Analysis of Potential Selection Bias.
Data S1.

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