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ABSTRACT

Does Voluntary Risk Taking Affect Solidarity? Experimental Evidence from Kenya*

In this study we experimentally investigate whether solidarity, which is a crucial base for informal insurance arrangements in developing countries, is sensitive to the extent to which individuals can influence their risk exposure. With slum dwellers of Nairobi our design measures subjects' willingness to share income with a worse-off partner both in a setting where participants could either deliberately choose or were randomly assigned to a safe or a risky project. We find that when risk exposure is a choice, willingness to give is roughly 9 percentage points lower compared to when it is exogenously assigned to subjects. The reduction of solidarity is driven by a change in giving behaviour of persons with the risky project. Compared to their counterparts in the random treatment, voluntary risk takers are seemingly less motivated to share their high payoff with their partner, especially if this person failed after choosing the risky project. This suggests that the willingness to show solidarity is influenced by both the desire for own compensation and attributions of responsibility. Our findings have important implications for policies that interact with existing informal insurance arrangements.

JEL Classification: D81, C91, O12, D63
Keywords: solidarity, risk taking, Kenya

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* We would like to thank the team of the Busara Center of Behavioral Economics in Nairobi, in particular Chaning Jang, Lucy Rimmington, Arun Varghese and all involved research assistants for helping to realize this project and their support. Moreover, we thank seminar participants at the Universities of Basel, Lucerne, Neuchatel, Mannheim and Munich for helpful discussions and comments.

1 Introduction

1.1 Motivation

Given that formal insurance markets in developing countries are very limited, poor households typically rely on the help of family or friends in times of economic hardship. These informal exchanges of gifts, loans or labour, which are motivated by social preferences or strategic incentives, serve de facto as risk pooling devices and are an important, though not complete source for households to cope with negative income shocks.[1] A large body of literature investigated forms, motives and constraints of such informal risk sharing arrangements (see Fafchamps, 2011 for a review). However, little attention has been paid to the relationship between mutual support and the extent to which individuals can control their risk exposure. This issue refers to the fact that (positive or negative) income shocks can either be the consequences of risky choices (e.g. investments) or completely random events (e.g. accidents which affect work capacity), a distinction which might be quite relevant for solidary behaviour for mainly two reasons. Firstly, evidence from the Western world suggests that a considerable proportion of individuals favour redistribution when inequalities are caused by exogenous circumstances rather than by factors of personal responsibility (e.g. Krawczyk, 2010; Le Clainche and Wittwer, 2015; Roemer and Trannoy, 2015; Schokkaert and Devooght, 2003). Moreover, in line with the responsibility argument, experimental studies with students from high-income countries find that subjects who exposed themselves to less risk 'punished' needy partners that had taken higher risks by transferring or distributing less money (Bolle and Costard, 2013; Cappelen et al., 2013; Cettolin and Tausch, 2015). Secondly, while the first point focuses on the behaviour of the *beneficiary* of solidarity, there is also reason to believe that it matters for the willingness to give whether the *donor* deliberately accepts risk for earning income or whether he can earn the same income by pure luck. In the former case he may view part of the earned income as compensation for bearing risk and choice costs resulting from foregoing safer opportunities which may induce him to share less. This is in line with experimental evidence showing that subjects in the US transfer less money to other persons if they earned income by effort rather than pure luck, therefore rewarding themselves for their workload (Jakiela, 2015).

Direct evidence on the question whether the extent to which individuals can control their risk exposure affects solidarity is scarce and mainly based on two experimental studies conducted in high-income Western countries (Trhal and Radermacher, 2009; Cettolin and Tausch, 2015).

[1]Informal insurance arrangements are shown to fail to provide full insurance, even against idiosyncratic shocks (Fafchamps and Lund, 2003; Kinnan, 2014; Townsend, 1994). Explanations for incomplete informal insurance are *limited commitment*, i.e. households with positive income shocks have incentives to leave the not legally enforceable insurance arrangement, or *limited information*, i.e. information asymmetries offer the possibility of shirking (moral hazard) or of pretending a negative shock in order to claim support or to escape payment obligations towards group members (hidden income).

These studies contrast the situation where subjects are exposed to exogenous income risk with the situation where subjects can choose freely between a risky and a safe(r) income option. Both studies find supporting evidence for the hypothesis that individuals are less generous towards those whose bad outcome is a result of their own risk-taking action compared to just bad luck. However, these findings are not necessarily transferable to developing countries. The countries in which the studies have been conducted (Germany and the Netherlands) have comprehensive social security systems that strongly limit the extent to which individuals need to rely on other people's solidarity. In contrast, in developing countries, where public social security nets are absent, mutual voluntary help is an important source for households to cope with negative income shocks. This is supported, for example, by Jakiela (2015), who finds that Kenyan villagers make virtually no difference in their allocation decisions with respect to whether income was earned by exerting real effort or the result of pure luck, while the contrary was the case for US students which seemingly rewarded themselves and others for their effort. Schokkaert and Devooght (2003) compare students in Belgium, Burkina Faso and Indonesia regarding answers to hypothetical questions about the fair distribution of ex post tax income and subsidies for health expenditures in different scenarios. When participants think that individuals in the case studies are responsible for their behaviour (e.g. in the case of smoking and low effort) the majority favours not to compensate for the consequences or even to punish the responsible person. This opinion is particularly strong in the Burkinese sample, which points to relevant differences in fairness perceptions.

Our study is - to the best of our knowledge - the first to investigate in a developing country whether individuals condition their giving behaviour on the extent to which they and their partners can influence own risk exposure using an incentivized experimental approach. Experimental evidence from middle- and low-income countries on the relationship between control of exposedness to risk and solidarity is so far limited to a strand of literature that investigates whether the introduction of voluntary formal insurance has a crowding-out effect on informal mutual support. All three existing experiments which have been conducted in the Philippines (Landmann et al., 2012), Cambodia (Lenel and Steiner, 2017) and China (Lin et al., 2014) find that the availability of formal insurance reduces informal transfers. The experimental designs have in common that they exogenously expose participants to a risky outcome in one treatment and allow them to reduce this level of risk exposure by choosing an insurance option in a second treatment. However, in focusing on insurance purchase decisions, these studies deal with a special case of risk avoidance. In particular, the validity of the measured impact on solidarity critically hinges on a proper understanding of, and some familiarity with the concept of insurance which is, however, typically not given for the majority of people in less developed countries (cf. Lenel and Steiner, 2017).

As the first key contribution we, therefore, test whether poor individuals' solidarity is sensitive to the degree of control subjects have over their risk exposure in a context without public social safety nets based on a laboratory experiment we conducted with slum dwellers in Nairobi. In a between-subject design with two different randomized treatments similar to Cettolin and Tausch (2015), each participant could either choose (treatment CHOICE) or was randomly assigned (treatment RANDOM) to a safe or a risky project. The risky project involved a one-half probability to end up with a zero payoff. After being randomly matched with another person, subjects could make voluntary transfers to their partner. We find that when risk exposure is a choice, the share of subjects picking the risky project is 30 percentage points lower and that overall willingness to give is roughly 9 percentage points lower compared to when risk is exogenously assigned to subjects which is in line with the evidence for developed countries. However, we also find some interesting differences to these studies. For example, in contrast to Cettolin and Tausch (2015) we find that lucky winners with the risky project show a particularly high degree of solidarity with unlucky losers when both face risk for exogenous reasons. This suggests that the willingness to share unexpectedly high income with individuals with unexpectedly low incomes is higher in developing countries where mutual aid is voluntary and has a strong tradition compared to industrialized countries where mutual aid is enforced by social insurance systems.

As the second key contribution we show that the average effect of CHOICE on giving is not informative about the behavioural effects we are interested in if giving depends on the level of risk faced by donors and CHOICE leads to largely different distributions of risk than RANDOM. Behavioural effects occur if individuals show different willingness to give in CHOICE than in RANDOM if they are exposed to the same level of risk. However, even if there are no such effects differential risk taking under CHOICE can produce a mechanical effect on the average willingness to give. To see this assume that risk takers exhibit higher willingness to give than non-risk takers and that the treatment itself has no impact on the behaviour of both groups. Because the share of risk takers in CHOICE is much smaller than in RANDOM the average willingness to give in CHOICE will be lower than in RANDOM because the group with higher willingness to give receives a smaller weight. As a result, we would measure a negative overall treatment effect although risk takers and non-risk takers do not behave differently under CHOICE than under RANDOM, i.e. although there is no behavioural effect. We propose to estimate the so-called average controlled direct effect (Pearl, 2001; Robins, 2003) to test whether the average effect on the willingness to give we measure is entirely caused by differential risk taking under CHOICE against the alternative of the existence of a behavioural effect (Acharya et al., 2016). This effect can be obtained by estimating the effect of CHOICE conditional on risk exposure. This, however, requires taking into account that donors who self-select into a specific project in CHOICE differ systematically from donors who are randomly assigned to the same project in RANDOM. Hence, the observed differences in transfers conditional on project provide biased estimates of the treatment effect. We address the issue of selection bias by drawing on a rich data set collected within our experiment.[2]

When studying giving behaviour conditional on risk taking, we find that donors with the safe project exhibit the same willingness to give independent of whether they have avoided risk deliberately or by pure chance which highlights another interesting difference to the findings from high-income countries. In contrast, donors who have deliberately chosen the risky project are significantly less willing to give than those exposed to the risky project for exogenous reason. This result implies that we can reject the hypothesis that the average effect on the willingness to give we measure is entirely caused by differential risk taking under CHOICE compared to RANDOM in favour of the alternative of the existence of a direct behavioural effect. Our findings show, though, that the behavioural response is limited to risk takers. This asymmetry compared to holders of the safe project supports the hypothesis that willingness to show solidarity is influenced by the desire for own compensation. Moreover, as donors who have chosen the risky project show particularly low solidarity with beneficiaries that self-select into the risky project we also find support for the hypothesis of attributions of responsibility. These findings have important implications for policies that possibly interact with existing informal insurance arrangements, such as the promotion of profitable but riskier innovative technologies or formal insurance to reduce risk exposure, which may crowd out solidarity.

The remainder of this article is organized as follows. The next section describes in detail the experiment we conducted including the data we collected within the experiment. Section 3 explains our empirical strategy and discusses its empirical implementation, estimation, and plausibility. In Section 4 we present and discuss results. The last section concludes. An appendix contains supplementary information and estimation results.

2 The experiment

2.1 Experimental context

We conducted a laboratory experiment at the Busara Center of Behavioral Economics in Nairobi, Kenya. The centre provides a state-of-the-art lab infrastructure, including 20 computer-supported workplaces. It maintains a subject pool with currently around 5,000 registered individuals, mainly

[2] Cettolin and Tausch (2015) also report the observed differences between treatments conditional on project but they do this to assess effect heterogeneity without acknowledging the problem of the mechanical effect. Moreover, they do not take into account self-selection into projects.

recruited from two different Nairobi informal settlements, the Kibera and Viwandani slum. The living situation in these slum communities is characterized by extreme poverty and insecurity due to the lack of property rights and high criminality. Housing and hygiene conditions are very poor since the government does not provide water, electricity, sanitation systems or other infrastructure (The Economist, 2012). Most of the slum residents work as small-scale entrepreneurs and casual workers in the informal sector, therefore relying on uncertain and irregular income streams. Related to the lack of formal employment, most of the slum dwellers have no formal risk protection such as health insurance (Kimani et al., 2012). Many households are, however, member in some kind of social network, such as *merry-go-rounds,* which allow saving and borrowing and implicitly provide an informal safety net (Amendah et al., 2014).

In Kenya, in general, there is a strong spirit of *harambee* (the Swahili term for 'pulling together') which encloses ideas of mutual support, self-help and cooperative effort. Harambee takes various forms, such as local fundraising activities to help persons in need or the joint implementation of community projects (e.g. building schools or health centers). While being an indigenous tradition in many Kenyan communities, the concept became a national movement since Kenya's first president Komo Kenyatta used it as slogan for mobilizing local participation in the country's development (Jakiela and Ozier, 2016; Mathauer et al., 2008; Ngau, 1987). In the light of this strong tradition of solidarity and seemingly well-established informal security nets it is therefore particular interesting and important to understand which behavioural mechanisms drive willingness to support others.

2.2 Experimental design

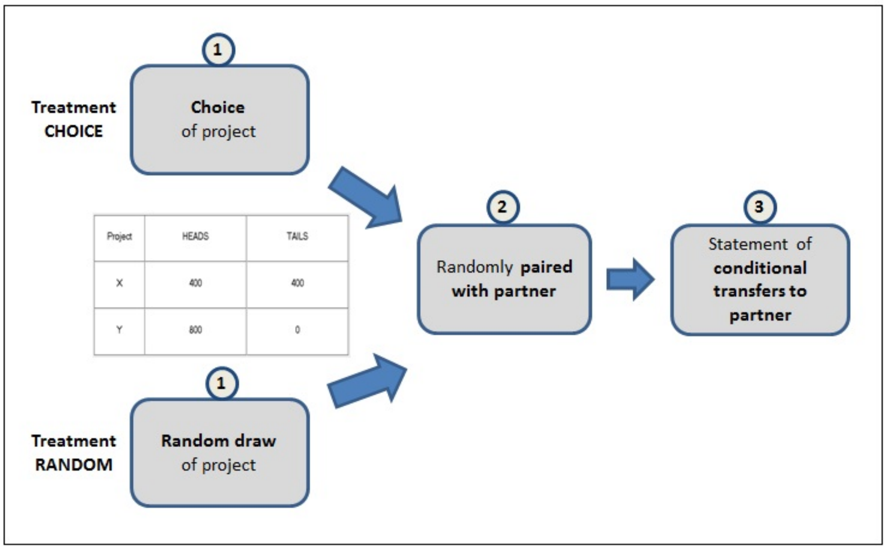
2.2.1 Risk solidarity game

The core game of the study aims at measuring solidarity behaviour in situations where subjects either can choose or are exogenously assigned to certain risk exposure. Figure 1 gives an overview on the sequence of steps in the game. At the beginning, two projects were presented to each subject: a safe option offering 400 KSh and a risky alternative yielding either 800 or 0 KSh with equal probability. Depending on the treatment, subjects could either choose (treatment CHOICE) or were randomly assigned (treatment RANDOM) to one option. After the choice or random draw, each participant was randomly and anonymously matched with another person. Using the *strategy method*[3] (Selten, 1967), subjects were then asked how much money they wanted to transfer to their partner in case of winning the 'high' (HEADS) payoff of their option (i.e. 400 or 800 KSh). Hence, before revealing their partners' choice (or assignment) and earnings, participants stated

[3]Brandts and Charness (2011) show that the strategy method is a valid alternative to the direct-response method, with both leading to similar results.

their gift for every possible payoff of their partner (i.e. 400, 800 or 0 KSh).[4] At the end of the session, lottery outcomes were randomly determined and transfers effected according to the actually realizing states. The stakes of the game represented considerable amounts for the mainly very poor participants who reported on average a daily income of 161 KSh (∼1.50 USD).

Figure 1: Sequence of steps in the risk solidarity game



The design implies that in the random treatment, subject's income is determined purely by chance, while in the other treatment, it can be influenced by the participant's choice. In particular, becoming a needy person, i.e. earning the zero income from the lottery, is just bad luck in RANDOM but involves a voluntary decision for the risky lottery in CHOICE. The imposed trade-off between a safe and a risky option thereby ensures that risk taking is salient to the participants. Moreover, since the payoffs of the two alternatives both equals 400 KSh in expectation, the risky option reflects a mean-preserving spread of the safe alternative implying that taking the risk is not compensated by higher expected income. Hence, choosing the lottery is not utility maximizing for risk averse individuals and possibly unnecessary in the risk-sharing partner's view since avoiding the risk is not costly. This case has also been studied in the related experimental literature (e.g. Bolle and Costard, 2013; Cettolin and Tausch, 2015; Trhal and Radermacher, 2009) since it provides an important benchmark for the effect of risk exposure choice on solidarity in alternative scenarios in which risk taking is either beneficial or even unfavorable in terms of expected income.

The design as an anonymous one-shot game deviates from conditions of real-world solidarity in developing countries which typically takes place among persons within the family or neighbourhood in repeated exchanges. Keeping subjects' identity confidential is, however, necessary

[4] As last step, the subjects were also asked to indicate how much they expected to receive from their partner, whereby these statements were not incentivized. This information is, however, not used this study.

in order to avoid that possible real-life relationships or fear of sanctions outside the lab bias behaviour of participants. Further, by restricting the game to one single round we implicitly rule out that subjects base their risk-taking and sharing decisions on strategic considerations induced by repeated interactions. This isolates the effect of risk taking on giving behaviour motivated by (social) preferences, such as altruism or distributive preferences (cf. Charness and Genicot, 2009), which represents an important reference case since it avoids that possibly interacting intrinsic and extrinsic motivations blur the measured impact. Overall, since our design excludes issues of social pressure and reciprocity considerations that probably would have reduced the participants' incentives to punish a risk-taking partner our experiment is likely to measure an upper bound of the behavioural effect of free project choice on solidarity.

2.2.2 Procedures

For recruitment, subjects were randomly chosen from the Kibera and Viwandani subject pool registered at Busara and then invited by SMS. A precondition for being selected was an education level of at least primary school to ensure some familiarity with numerical values as necessary in our study. The recruited persons were randomly assigned to treatments, therefore resulting in a between-subject design. The entire experiment was run within 13 sessions in the period of August to October 2014. Seven sessions were conducted of the RANDOM treatment and six of the CHOICE treatment. In total, 228 subjects participated in our study, thereof 102 in the RANDOM and 126 in the CHOICE. Of the 228 experimental subjects 51% are female, 40% are married and 42% live in the Kibera slum. On average, the participants are 31 years old and have a schooling level of 12 years.

Upon arrival, subjects were identified by fingerprint and randomly assigned to a computer station. The instructions were then read out in Swahili by a research assistant, while simultaneously, some corresponding illustrations and screenshots were displayed on the computer screens (see Appendix B for an English version of the instructions, exemplarily for CHOICE).[5] For the entire experiment the z-Tree software code (Fischbacher, 2007) was programmed to enable an operation per touchscreen which eases the use for subjects with limited literacy or computer experience. Subsequently, some test questions verified the participants' comprehension of the game rules. In case of a wrong answer, the subject was blocked to proceed to the following question. A research assistant then unlocked the program and gave some clarifying explanations if needed. This guaranteed that all participants fully understood the games and did not simply answer the test questions by trial

[5] In general, all verbal explanations of the research assistant were made in Swahili whereas information on the computer screens was written in English. This combination has proven to be useful for facilitating comprehension (Haushofer et al., 2014).

and error. After the comprehension test, the participants performed the actual experimental task. The experiment involved, firstly, a risk preference game which aimed at measuring subjects risk attitudes (see Section 2.3.2 for a detailed description of this game) and, secondly, the risk solidarity game explained in detail in the previous section. Importantly, the subjects completed the decisions in these two games without learning the realized payoff in the precedent game. Moreover, after randomly determining the game payoffs at the end of the experiment, only the result of one randomly selected game was relevant for real payment.[6] These two design features avoid that results are biased due to any strategic behaviour, expectation forming or income effects across games.

At the end of the session, participants completed a questionnaire covering important individual and household characteristics. After the session, subjects received 200 KSh in cash as show-up fee.[7] The earnings of the incentivized games, which amounted on average to 412 KSh per person, were transferred cashless to the respondents' MPesa accounts.[8]

2.3 The data collected within the experiment

2.3.1 Survey data

In the post-experimental survey we collected all individual and household-related data which are important for the validity of our empirical strategy (see Section 3.3 for more details). Besides basic demographics this includes information on health, occupation, income, asset ownership, financial risk exposure as well as social preferences. Table 3 provides an overview of the retrieved variables.

2.3.2 A measure of risk preferences

Since subjects' risk attitudes are an important determinant of risk-taking behaviour in the risk solidarity game and therefore a key variable to deal with selectivity under CHOICE, we elicitated an experimental measure of risk preferences which is comparable across both treatments. Prior to the risk solidarity game we ran a risk preference game which was incentivized and designed as an *ordered lottery selection* procedure (Harrison and Rutstroem, 2008). Originally developed by Binswanger (1980) for an experiment with Indian farmers, the method is commonly used to elicit risk attitudes in developing country settings since it is relatively simple to demonstrate and easy to understand. Other standard elicitation procedures, such as the approach of Holt and Laury

[6]Laury (2006) shows that experimentally measured risk behaviour is not sensitive to whether subjects are paid for all choices or only one randomly selected decision (at the same payoff level). The random-choice payment is therefore a valid method that allows to increase the number of observations and to maximize the salience of payoffs for a given budget.

[7]Participants coming from Viwandani received additionally 200 KSh as reimbursement of (higher) cost of transport. Moreover, for all respondents, arriving on time was awarded with 50 KSh.

[8]MPesa is a mobile-phone based money transfer service. It allows to deposit, withdraw and transfer money in a easy and safe manner with help of a cell phone. Its use is very widespread in Nairobi slums where around 90% of the residents have access to this service (Haushofer et al., 2014).

(2002) as well as non-incentivized survey questions (Dohmen et al., 2011), have turned out to be less successful in creating reasonable results in low-income settings, seemingly being too complex or abstract for the typically low-educated populations (Charness and Viceisza, 2011; Fischer, 2011). In the game, each subject was asked to choose one out of eight different lotteries (see Table 1,

Table 1: Risk preference game: payoffs, expected values, risk and levels of risk aversion

Lottery number	Lottery	High payoff HEADS (p=0.5)	Low payoff TAILS (p=0.5)	Expected value	Standard deviation	Risk aversion range (CRRA)[a]	Fraction of subjects (%)
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	K	320	320	320	0	2.46 to infinity	36.8
2	L	400	280	340	60	1.32 to 2.46	10.5
3	M	480	240	360	120	0.81 to 1.32	6.1
4	N	560	200	380	180	0.57 to 0.81	14.1
5	O	640	160	400	240	0.44 to 0.57	2.7
6	P	720	120	420	300	0.34 to 0.44	7.0
7	Q	800	80	440	360	0 to 0.34	14.5
8	R	880	0	440	440	-infinity to 0	8.3

Note: [a] As common in literature, we assume the individual's utility function $u(x) = \frac{x^{1-\gamma}}{1-\gamma}$, where γ is the CRRA parameter describing the degree of relative risk aversion. The intervals for the CRRA parameter were determined by computing γ where the expected utility from one option equals the expected utility from the next option, i.e. where the individual is indifferent between two neighbouring lotteries.

columns 2 to 4). The first alternative offers a certain amount of 320 Kenyan Shillings (KSh). The subsequent lotteries yield either a high (HEADS) or a low (TAILS) payoff with probability 0.5. While the first six lotteries are increasing in expected values and variances of payoffs, the last lottery R has the same expected payoff as Q, but implies a higher variance. Hence, only risk-neutral or risk-loving subjects should choose this dominated gamble (Binswanger, 1980).

Typically, the lottery numbers that subjects choose in ordered lottery designs (here: 1 to 8) are directly used as risk preference indicator (e.g. Eckel and Grossman, 2002).[9] In order to check whether they represent a plausible measure in our setting we test with the help of a regression analysis that is shown in Table A1 in the Appendix A how they are related to individual and household characteristics.[10] We include covariates reflecting subjects' socio-demographic situations as well as their 'real-life' background risk exposure, since this might influence their risk-taking behaviour with respect to the 'foreground risk' introduced by the experiment (Harrison et al., 2010). In particular, we use proxies for health risk exposure (past and expected future health

[9]The lottery numbers (LN) can be regarded as a parametric index of risk preference, since they are linearly related to the lotteries' expected payoffs (EP) and standard deviations (SD) (Eckel and Grossman, 2002). In our game the lottery number can be calculated as $LN = EP/20 - 15$ and the expected payoff as $EP = 320 + \frac{1}{3}SD$ (cf. Eckel and Grossman, 2002, p.7). In fact, this is only the case for the first seven options, as the last lottery is the dominated gamble with EP equal to the seventh lottery. Strictly speaking, the lottery number is therefore an ordinal rather than a metric variable. However, in summary statistics and regression analyses, we nevertheless use this indicator as risk preference measure and treat it therefore as metric, since it is more intuitive to interpret than alternative indicators for risk taking, such as the (continuous) standard deviation of lotteries. Moreover, it makes virtually no difference for estimation results whether lottery numbers or SDs are used.

[10]In Strobl (2016) we also statistically test whether the scenarios of the precedent investment game influenced the lottery choices in the risk preference game. However, we do not find evidence for such a bias.

shocks, health insurance enrolment) and for the ability to informally cope with shocks (wealth, household composition). Moreover, a proxy for perceived social capital in the society (GSS index)[11] is included in view of the empirical observation that people invest higher proportions in risky assets in areas with higher levels of social capital (Guiso et al., 2004). The study finds that the effect of social capital is particularly strong where education levels are low and law enforcement is weak, which is the typical situation in developing countries. Finally, we add two dummies that measure inequality aversion since evidence suggests that inequality aversion is positively correlated with risk aversion (Ferrer-i-Carbonell and Ramos, 2010).[12]

We find that being employed in a paid occupation is associated with higher risk aversion, an observation similarly made by Falco (2014). He shows that more risk averse workers in Ghana are more likely to search for formal employment than being engaged in the informal sector, seemingly in order to avoid the volatile income streams from informal work. Most of the coefficients, namely that from the variables reflecting health insurance enrolment, health care utilization, wealth and social capital, have the expected signs but are not statistically significant, which is, however, not an implausible finding given our small sample size. Overall, given the encouraging results of the plausibility test, we will use the lottery numbers as an indicator for subjects' risk preference in the following empirical analyses.

Table 1, column 8 reports the distribution of lottery choices made in the game. According to these results, we conclude that a majority of participants is risk averse since 77.2% of the subjects selected one of the first six lotteries. 14.5% and 8.3% of the respondents chose the 7th and 8th lottery, respectively, and exhibit therefore risk-neutral and respectively risk-seeking behaviour.

To compare the level of risk aversion from our sample with that from other low-income settings, we determine the average degree of risk aversion. For this, we follow Dave et al. (2010) and adapt the estimation procedure initially used for the Holt-Laury approach to our lottery choice task. Assuming the CRRA utility function $u(x) = \frac{x^{1-r}}{1-r}$ and using a maximum likelihood method, we estimate the average risk parameter r for our sample.[13] The estimated $\hat{r}$ is 0.72 (p=0.000), implying

[11] As common in literature we included the following three General Social Survey (GSS) questions in our questionnaire which claim to measure social capital: 1. Fairness: "Do you think that most people would try to take advantage of you if they got the chance, or would they try to be fair?" (1="Would try to be fair"; 0="Would take advantage"); 2. Trust: "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?" (1="Most people can be trusted"; 0="You can never be too careful in dealing with people"); 3. Helpfulness: "Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?" (1="Try to be helpful"; 0="Just look out for themselves"). The GSS Index represents the sum of answers to the three questions (i.e. it takes discrete values between 0 and 3).

[12] In order to measure inequality aversion we use the following questions: 1. Inequality 1 (disadvantageous): "How much do you agree/disagree with the following statement? "Other people should NOT own much MORE than I do."; 2. Inequality 2 (advantageous): ""Other people should NOT own much LESS than I do." (1=Strongly disagree; 2= Disagree; 3=Undecided; 4=Agree; 5=Strongly Agree). We create two dummies for the two types of inequality aversion which take each the value 1 when the subject answered with 4 or 5, and 0 otherwise.

[13] The detailed procedures to estimate the risk parameter are described in Harrison (2008) and Harrison and Rutstroem (2008). In brief, it is assumed that for each choice between two lotteries, the individual calculates the

that the mean participant is "very risk averse" according to the classification scheme of Holt and Laury (2002). Therefore, our study subjects reveal on average a higher degree of risk aversion than reported in other studies. For example, the estimated CRRA coefficient was 0.54 in a three-country experiment in India, Ethiopia and Uganda (Harrison et al., 2010), 0.45 in Peru (Galarza, 2009) and 0.39 in South Africa (Brick et al., 2012). This difference compared to other settings might be explained by the relatively riskier environment (as described in Section 2.1) and therefore due to the higher real life background risk that subjects face in the Nairobi slums. Moreover, social capital, as mentioned above as an motivating factor for financial risk taking, seems to be lower in Nairobi slums than in other regions. In a five-country (Armenia, Guatemala, Kenya/Kibera, India, the Philippines) group lending experiment, Cassar and Wydick (2010) find dramatically lower individual contributions rates to public goods in Kibera than in the other country samples, a result which is driven by the lack of confidence in other members. Greig and Bohnet (2008) find in their Nairobi slum experiment one of the lowest levels of trust ever reported from a Trust Game.

2.3.3 Outcome of interest

The most important source of data stems from the risk solidarity game which measured transfer behaviour under the CHOICE and RANDOM treatment. Figure 2 displays the distributions of our major outcome of interest, the stated amounts of transfers (in KSh), by treatment and sender's project. We only consider transfers from subjects with higher payoffs to partners with lower payoffs, i.e. from safe project owners to partners with zero income (400→0) and from lucky risky project holders to partners with safe or zero earnings (800→400 and 800→0).[14] The reason for this restriction is that we are interested in solidarity which is necessary for mutual aid arrangements to work, implying redistribution of income from better-off to worse-off subjects (and not the other way around). Figure 2 shows that the majority of subjects decided to give nothing to their partner. The cases of zero transfers range roughly between 60% to 75% depending on the treatment and which project the sender was assigned to. Moreover, the observed transfers are concentrated on few marked values. Given this unbalanced distribution, which is difficult to model empirically with the relatively small sample sizes in the experiment, we focus in the following on the outcome variable *willingness to make a transfer* rather than on the absolute amount of money. Thus, we

index $\nabla EU = EU_R - EU_L$, where the expected utility for lottery i with k different outcomes is $EU_i = \sum_k (p_k \times U_k)$ and the subscripts R and L refer to the 'right' and the 'left' lottery, denoting two neighbouring lotteries in the menu. Transforming ∇EU into the ratio $\nabla EU = \frac{EU_R^{1/\mu}}{EU_R^{1/\mu} + EU_L^{1/\mu}}$ yields a probabilistic choice function that expresses the probability of choosing the right lottery. Moreover, the noise parameter μ allows us to account for any behavioural errors (e.g. due to inattentiveness or a lack of understanding). The ratio builds the base of a conditional log-likelihood function that can be maximised with regard to μ and the CRRA risk coefficient r.

[14]This implies excluding 33 observations where persons transferred money to partners with equal income (400→400 [n=18] or 800→800 [n=3]) or even with higher income (400→800 [n=12]).

use as outcome of interest an indicator variable which takes the value of 1 if the participant makes a positive transfer to his partner and 0 otherwise.

Figure 2: Distribution of transfers (in KSh) by treatment and sender's project

3 Empirical strategy

3.1 Causal effects of interest

3.1.1 Overall treatment effects

Firstly, we are interested in the question how the possibility of choosing freely between a risky and a safe project (treatment CHOICE, $C = 1$) as opposed to random assignment of projects (treatment RANDOM, $C = 0$) affects risk taking (R), on the one hand, and the willingness to make transfers to worse-off individuals (outcome Y), on the other hand. Using the potential outcome framework typically applied in the statistical evaluation literature with a binary treatment variable C, we denote by Y^c potential willingness to make transfers given regime $C = c$ where $c \in \{0, 1\}$. Similarly, we denote by R^c potential risk exposure given regime $c \in \{0, 1\}$. Moreover, we set $R = 0$ if the safe project has been chosen or randomly assigned, and $R = 1$ for the risky project. The overall effect of free project choice on risk taking, which we denote as θ_R , is hence given by

$$\theta_R \equiv E[R^1 - R^0] = Pr(R^1 = r) - Pr(R^0 = 1),$$

and the overall effect of free project choice on willingness to make transfers, denoted as θ_Y, by

$$\theta_Y \equiv E[Y^1 - Y^0] = E[Y^1(R^1) - Y^0(R^0)],$$

respectively. Both overall effects are identified because the treatment status C has been randomized in the experiment, where randomization of C is equivalent to the assumption

$$(A1) : Y^1, Y^0, R^1, R^0 \perp C.$$

While it is natural to be interested in the overall effect of CHOICE on the willingness to make transfers we show in the following that this effect is not informative about the behavioural effect we are interested in. The overall effect θ_Y can be decomposed as follows:

$$\theta_Y = E[Y^1(R^1) - Y^0(R^0)] = \sum_{r \in \{0,1\}} E[Y^1(r)]Pr(R^1 = r) - E[Y^0(r)]Pr(R^0 = r).$$

Now assume that there are no behavioural effects. Behavioural effects occur if individuals with the same project under CHOICE as under RANDOM show different willingness to give in the two treatments. For example, the willingness to support partners that have exposed themselves to the risky project by their own choice may be lower than the willingness to support partners who end up in a risky situation for exogenous reasons that lie outside their own power due to attributions of responsibility. Also, individuals that have been lucky in a lottery may be more willing to give than individuals who have exposed themselves to a certain risk (and return) for a good reason and at some cost for which they want to be compensated. The absence of behavioural effects implies that $E[Y^1(r)] = E[Y^0(r)]$ for $r \in \{0, 1\}$. In this case we can write the overall effect as

$$\theta_Y = E[Y^0(1) - Y^0(0)][Pr(R^1 = r) - Pr(R^0 = 1)] = E[Y^0(1) - Y^0(0)]\theta_R.$$

As a consequence, whenever $E[Y^0(1)] \neq E[Y^0(0)]$, i.e. when subjects with the safe project under RANDOM show different average willingness to give than subjects with the risky project under RANDOM, and $\theta_R \neq 0$, i.e. there is differential risk taking under CHOICE than under RANDOM, then we will have a non-zero overall effect $\theta_Y \neq 0$ even if the behavioural effect $E[Y^1(r)] - E[Y^0(r)]$ is zero for every group. Therefore, we need to estimate the behavioural effects $E[Y^1(r)] - E[Y^0(r)]$ for $r \in \{0, 1\}$ directly. These effects are called the average controlled direct effects in the literature (Pearl, 2001; Robins, 2003). If we can reject the hypothesis that these are zero for at least one $r \in \{0, 1\}$, i.e. for subjects with either the safe or the risky project or both, then we can reject the hypothesis that the overall effect θ_Y is entirely caused by differential risk taking under CHOICE, i.e. by the treatment effect on risk exposure θ_R, and provide evidence for the existence and the size of behavioural effects (Acharya et al., 2016).[15]

3.1.2 Treatment effects conditional on risk exposure

Identification of the behavioural or average controlled direct effects $\theta_{Y,r} \equiv E[Y^1(r)] - E[Y^0(r)]$ for $r \in \{0,1\}$ is more challenging because we need to take into account that subjects have self-selected into the project $R = r$ under CHOICE which implies that $E[Y^1(r)]$ does not equal the expected observed willingness to give of holders of project r under CHOICE, $E[Y|R = r, C = 1]$. In the RANDOM treatment, risk exposure is randomly assigned, where randomization is equivalent to the assumption

$$(A2): \; Y^0 \perp R | C = 0$$

which implies that $E[Y^0(r)]$ equals the expected observed willingness to give of holders of project r under RANDOM, $E[Y|R = r, C = 0]$, for $r \in \{0,1\}$. To identify $E[Y^1(r)]$ we impose the assumption that we observe all factors X that determine both the endogenous choice of the risky versus the safe project and the willingness to make transfers, i.e. that the following unconfoundedness assumption holds:

$$(A3): Y^1 \perp R | X = x, C = 1.$$

If this assumption is satisfied we have $E[Y^1(r)] = \int E[Y|R = r, X = x, C = 1] dF_X(x)$, i.e. we can reweigh the observations with $R = r, X = x$ under CHOICE according to the distribution of characteristics X in the population $F_X(x)$ which is equal to the distribution in the randomized samples $F_{X|C=c}(x)$ for $c \in \{0,1\}$. Unconfoundedness cannot be tested and hence needs to be plausibly justified. Whether this justification is convincing crucially depends on the richness of available data which should contain information on all relevant confounding variables. Additionally, we need to ensure that there is no combination of risk exposure R and covariates X that perfectly predicts treatment status C, i.e. that there is common support in the covariate distributions of RANDOM and CHOICE conditional on R:

$$(A4): \; 0 < Pr(C = 1|R = r, X = x) < 1, \quad r \in \{0,1\}.$$

In other words, we need to make sure that for each individual with $R = r, X = x$ in RANDOM there is a comparable individual with $R = r, X = x$ in CHOICE. The common support assumption is testable in the data.

[15] In a companion paper (Wunsch and Strobl, 2017) that makes a methodological contribution to the literature on the identification and estimation of causal mechanisms we propose and apply different methods to decompose the overall effect into the mechanical effect and the behavioural effect. We find that the behavioural effect that does not go via the change in risk-taking explains at least 75 percent of the total effect.

3.2 Empirical implementation

We apply both parametric and semi-parametric methods to estimate the overall and conditional treatment effects. For the parametric estimations we use probit models given that all outcomes are binary. Additionally, we use semi-parametric inverse probability weighting (IPW) which has the advantage of allowing for heterogeneity in treatment effects. This takes recent evidence into account which suggests that parametric methods such as OLS or probit that implicitly assume effect homogeneity might yield biased estimates if this assumption is in fact violated (Sloczynski, 2016). Table 2 summarizes the different estimations we run in terms of the effect of interest, the dependent variable, the treatment variable and the sample we use for the estimation.

Table 2: Effect estimations

Effect of interest	Dependent variable	Treatment	Sample
Overall treatment effects			
Effect θ_R	R_i	C_i	all
Effect θ_Y	Y_i	C_i	all
Treatment effects conditional on donor's risk exposure			
Effect $\theta_{Y,0}$	Y_i	C_i	$R_i = 0$
Effect $\theta_{Y,1}$	Y_i	C_i	$R_i = 1$

Parametric estimators of the overall effects are obtained by (probit) regressing the outcome of interest (risk exposure R or willingness to make transfers Y) on the treatment dummy C. Covariates X are not necessary in these regressions if randomization worked but may be included to increase precision. The choice of control variables for all estimations is discussed below in Section 3.4. For the effect of CHOICE conditional on risk exposure, $\theta_{Y,r}$, we regress willingness to make transfers on CHOICE C and the set of covariates required for selection correction (see the detailed discussion in Section 3.4) within the subsamples with $R = 0$ and $R = 1$, respectively. We also check common support (A4) and run all estimations with and without enforcing common support. The latter excludes 12.7% of all observations. Moreover, to assess possible heterogeneity in giving behaviour with respect to the choices made by the recipients of transfers we reestimate everything restricting the sample to transfers to partners with the risky and safe project, respectively.

The IPW coefficients for the overall and conditional treatment effects are estimated in a standard two-stage approach which, first, uses probit regressions to predict the treatment status and to derive inverse probability weights and, second, contrasts the weighted mean outcomes of both treatment groups to estimate the average treatment effects (ATE). The covariates and samples used in the IPW approach are the same as in the parametric (probit) models.

3.3 Plausibility of the empirical strategy

3.3.1 Did randomization work?

As a first step, we check whether randomization in the first and second stage of the experiment was successful in creating comparable groups in terms of individual and household characteristics. To assess assumption (A1) - randomization of CHOICE C - Table 3 displays in column a) mean characteristics for the RANDOM and CHOICE sample, respectively, as well as their differences. It shows that the two samples are balanced well in terms of most characteristics. Statistically significant differences are observed, though, for age, income, some aspects household composition, ethnicity, residence in the Kibera slum and one of the two measures of inequality aversion. A closer look at these differences reveals, however, that most of them are driven by the difference in residency shares in the Kibera slum because Kibera residents are younger on average and have on average smaller households and lower household income. Moreover, we will show below that our results are robust to including those covariates with imbalances.

To assess assumption (A2) - randomization of projects R in the RANDOM sample - Table 3 displays in column b) mean characteristics for individuals with the safe and risky project randomly assigned, respectively, as well as their differences. The large majority of characteristics are balanced well. The only exceptions with statistically significant differences occur for the characteristics married, Nubian ethnicity, the fairness measure and the risk aversion measure. However, by including these variables as control variables, we are again able to show below that our results are not driven by these imbalances.

Table 3: Means of variables by treatment and project

	(a) RANDOM and CHOICE			(b) RANDOM			(c) CHOICE		
	RANDOM	CHOICE	Diff.[a]	safe	risky	Diff[a]	safe	risky	Diff[a]
	(1)	(2)	(2)-(1)	(1)	(2)	(2)-(1)	(1)	(2)	(2)-(1)
A. Individual characteristics									
Socio-economic characteristics									
Age	29.57	32.28	2.71**	30.8	28.49	-2.3	31.95	33.57	1.62
Male	0.52	0.49	-0.03	0.47	0.57	0.09	0.52	0.38	-0.14
Education (years compl.)	12.06	11.98	-0.08	11.97	12.13	0.17	11.98	12	0.02
Married	0.39	0.42	0.03	0.49	0.3	-0.19**	0.4	0.52	0.13
Household (HH) head	0.5	0.52	0.02	0.54	0.46	-0.08	0.52	0.52	0.01
Monthly income	3,811	6,038	2,227***	4,239	3,434	-804	5,749	7,152	1,403
Kibera slum	0.48	0.34	-0.13**	0.49	0.46	-0.03	0.32	0.43	0.11
Occupational status:									
Employed	0.11	0.16	0.05	0.14	0.09	-0.05	0.15	0.19	0.04
Self-employed	0.25	0.31	0.07	0.27	0.22	-0.05	0.32	0.29	-0.04
Work without payment	0.06	0.05	-0.01	0.05	0.07	0.02	0.04	0.1	0.06
Student	0.19	0.13	-0.06	0.2	0.18	-0.02	0.12	0.14	0.02
Unemployed	0.33	0.29	-0.03	0.27	0.37	0.1	0.32	0.19	-0.13
Other	0.06	0.06	0	0.07	0.06	-0.01	0.05	0.1	0.05
Main occupation:									
Selling goods	0.19	0.23	0.04	0.24	0.15	-0.09	0.2	0.33	0.14
Manufacturing/repairing goods	0.05	0.04	-0.01	0.05	0.04	-0.01	0.05	0	-0.05**

continued on the next page

	(a) RANDOM and CHOICE			(b) RANDOM			(c) CHOICE		
	RANDOM	CHOICE	Diff.[a]	safe	risky	Diff[a]	safe	risky	Diff[a]
	(1)	(2)	(2)-(1)	(1)	(2)	(2)-(1)	(1)	(2)	(2)-(1)
Offering services	0.14	0.17	0.02	0.14	0.15	0.01	0.16	0.19	0.03
Domestic work	0.17	0.14	-0.04	0.14	0.21	0.07	0.12	0.19	0.07
Farming	0.06	0.08	0.02	0.05	0.06	0.01	0.1	0	-0.10***
Other	0.39	0.35	-0.04	0.39	0.39	0	0.37	0.29	-0.08
Religion (1=christian)	0.9	0.85	-0.04	0.86	0.93	0.06	0.85	0.86	0.01
Ethnicity:									
Kamba	0.09	0.2	0.11**	0.05	0.12	0.07	0.19	0.24	0.05
Kikuyu	0.31	0.37	0.06	0.32	0.3	-0.02	0.36	0.43	0.07
Kisii	0.13	0.08	-0.06	0.15	0.12	-0.03	0.09	0.05	-0.04
Luhya	0.25	0.15	-0.10*	0.25	0.24	-0.02	0.15	0.14	-0.01
Luo	0.14	0.12	-0.03	0.12	0.16	0.05	0.12	0.1	-0.03
Nubian	0.05	0.05	0	0.08	0.01	-0.07*	0.05	0.05	0
Other	0.03	0.04	0.01	0.02	0.04	0.03	0.05	0	-0.05**
Health-related characteristics									
Health problem[b]	0.44	0.51	0.07	0.46	0.42	-0.04	0.53	0.43	-0.1
Chronical health problem	0.13	0.15	0.01	0.19	0.09	-0.1	0.11	0.29	0.17
Visited health care provider[b]	0.45	0.54	0.09	0.47	0.43	-0.04	0.54	0.52	-0.02
Health expenditures[b]	1,104	901	-203	1,283	946	-336	879	986	107
Enrolled in health insurance (HI)	0.23	0.23	0	0.29	0.18	-0.11	0.19	0.38	0.2
Enrolled in other insurance	0.1	0.07	-0.03	0.14	0.07	-0.06	0.05	0.14	0.09
Social preferences									
Inequality aversion 1 (disadv.)[c]	0.26	0.32	0.06	0.29	0.24	-0.05	0.3	0.43	0.13
Inequality aversion 2 (adv.)[c]	0.28	0.38	0.10*	0.25	0.3	0.04	0.33	0.57	0.24*
GSS questions:									
Fairness	0.31	0.28	-0.03	0.39	0.24	-0.15*	0.28	0.29	0
Trust	0.19	0.18	-0.01	0.15	0.22	0.07	0.19	0.14	-0.04
Helpfulness	0.25	0.26	0.01	0.27	0.24	-0.03	0.27	0.24	-0.03
GSS Index[d]	0.75	0.73	-0.03	0.81	0.7	-0.11	0.74	0.67	-0.07
B. Household characteristics									
Socio-economic characteristics									
No. of adults	2.7	3.69	0.99***	2.68	2.72	0.04	3.75	3.43	-0.32
No. of children	1.9	2.17	0.27	2.02	1.79	-0.23	2.26	1.81	-0.45
Monthly per capita (p.c.) income	3,312	2,773	-539	3,744	2,933	-811	2,700	3,058	358
No. of other earners	1.02	1	-0.02	0.95	1.07	0.13	1.11	0.57	-0.54**
No. of dependent HH members	2.23	2.83	0.60*	2.47	2.01	-0.46	2.78	3.05	0.27
HH is in wealth index quintile[e]:									
Poorest quintile	0.34	0.31	-0.03	0.36	0.33	-0.03	0.32	0.29	-0.04
Poorer quintile	0.1	0.07	-0.03	0.08	0.1	0.02	0.06	0.1	0.03
Middle quintile	0.17	0.23	0.06	0.2	0.13	-0.07	0.22	0.24	0.02
Richer quintile	0.22	0.17	-0.06	0.17	0.27	0.1	0.17	0.14	-0.03
Richest quintile	0.17	0.23	0.05	0.19	0.16	-0.02	0.22	0.24	0.02
Health-related characteristics									
Health expenditures (p.c.)[b]	550	612	62	639	473	-166	548	863	315
Expected future health shock[f]	3.62	3.19	-0.43	3.61	3.63	0.02	3.17	3.24	0.07
Foregone health care[b]	0.49	0.51	0.02	0.47	0.51	0.03	0.49	0.57	0.08
Prop. of HH members enrolled in HI	0.31	0.24	-0.07	0.37	0.26	-0.11	0.18	0.45	0.27
C. Experimental outcomes									
Risk preference[g]	3.56	3.56	0	4.07	3.1	-0.96**	3.04	5.57	2.53***
Understanding of instructions[h]	1.17	1.17	0	1.18	1.17	-0.01	1.16	1.21	0.04
Observations	*126*	*102*		*59*	*67*		*81*	*21*	

Note: [a]Statistically significant mean differences are marked as follows: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.; [b]in the past 3 months; [c]Inequality aversion 1 (disadvantageous): Dummy which takes the value 1 if respondent thinks that others should not own much more than herself ; Inequality aversion 2 (advantageous): dto. ...not own much less... (see Section 2.3.2 for the exact wording of questions); [d]No. of GSS questions positively answered (see Section 3.3.2); [e]The wealth index bases on the ownership of 11 household items (house, land, poultry, goats, sheep, cows/bullocks, refrigerator, radio, bicycle, motorcycle, car) and is constructed by using weights generated by principal component analysis; [f]Expected likelihood of unaffordable HH health expenditures within next year; [g]Number of lottery the subject has chosen out of 8 different lotteries with an increasing degree of riskiness, with 1(=safe income) to 8 (=riskiest lottery) (see Section 2.3.2); [h]Average number of trials needed to answer the comprehension test questions correctly.

3.3.2 Plausibility of the unconfoundedness assumption and common support

As discussed in Section 3.1.2 the identification strategy for the treatment effects conditional on risk exposure requires controlling for all factors that determine both subjects' project choice and the willingness to make transfers. Within the experiment we collected all information suggested to be important by theory and the empirical literature to render the unconfoundedness assumption (A3) plausible. We expect that risk preference, which we measure with the risk preference game, is one of the most important determinants of project choice. Moreover, background risk theory (e.g. Gollier and Pratt, 1996) suggests that individuals reduce financial risk taking in the presence of other, even independent risks. Therefore, subjects' risk exposure in their real life might influence their decisions in the lab (Harrison et al., 2010). Moreover, individuals may also be less willing to make transfers in the presence of other risks because they want to preserve a certain capacity to cope with negative shocks with their own resources. Thanks to our rich data set, we can draw on a broad range of variables reflecting exposure to the main sources of risk, such as income risk (occupation in paid employment, type of main occupation) and health and health expenditure risk (past and expected future health shocks, health insurance enrolment). Additionally, we have measures of the capacity to cope with negative shocks (wealth, household composition). Proxies for social capital and inequality aversion may also be relevant for predicting both project choice and the willingness to make transfers. Higher levels of trust and cooperation as well as inequality aversion in a society can encourage greater informal risk-sharing among community members and therefore provide better risk coping possibilities (Narayan and Pritchett, 1999). Moreover, higher social capital is found to promote financial risk-taking (Guiso et al., 2004). We observe five variables which are typically used to measure these factors (e.g. Giné et al., 2010; Karlan, 2005): trust, fairness, helpfulness and two measures of inequality aversion (see Section 2.3.2 for a detailed description of these variables).

Table 3, column c), which compares the characteristics of risk and safety choosing persons under CHOICE, shows indeed systematic differences with respect to several of the just mentioned characteristics. In particular, we observe a lower average degree of risk aversion and a higher average degree of inequality aversion among risk takers as well as some other differences that are related to background risk and ability to cope with negative shocks, such as type of main occupation and number of other earners in the household. Table 3, column c), also shows lack of common support for three variables: main occupation farming, main occupation manufacturing/repairing of goods and the residual ethnicity category. We estimate all of our results with and without enforcing common support and show that they are robust.

3.4 Choice of control variables

Having discussed imbalances across the randomized samples as well as the variables required for selection correction, we now discuss the choice of specific control variables for the estimations specified in Table 2. In general, we estimate one version with and one without covariates for all estimations. Whenever we use controls, four variables are always included: a dummy for males, for residence in the Kibera slum, and one for inequality aversion (2), as well as the risk preference measure. The latter three are important both for balancing the randomized samples, and for determining project choice and the willingness to make transfers under CHOICE. The male dummy turned out to be important in omitted variable tests for most of the estimations. Additionally, we add control variables on a case-by-case basis. Due to the relatively small number of observations we started with parsimonious specifications motivated by the descriptive evidence in Table 3, theory, and previous empirical research discussed in the last section. We then added covariates stepwise based on omitted variables test. For continous variables (such as age, income, education etc.), we tested both the continuous variable as well as derived dummy variables to account for possible non-linear dependencies. As many of the variables are highly correlated, the number of additional covariates is not too large, ranging between 5-7. The exact specifications together with the corresponding estimation results for the covariates are reported in Appendix A in Tables A2 and A3. To correct for imbalances across the RANDOM and CHOICE samples visible in Table 3, we additionally include the variables age, monthly income, two dummy variables for household composition as well as an indicator for ethnicity. To correct for selectivity in risk taking in the CHOICE treatment we include, in addition to the four baseline covariates, controls for income, occupational status, household composition, insurance enrolment, ethnicity and fairness.

4 Results

4.1 Descriptive evidence

The first experimental outcome that we are interested in is actual project choice. Table 4 displays how safe and risky projects are distributed within and across treatments. In the RANDOM group, the randomization created relatively similar proportions of safe (46.8%) and risky (53.2%) project holders as intended. When being able to choose the project freely in the CHOICE group, however, only a minority of the subjects preferred the risky lottery. Specifically, we observe 32.6 percentage points fewer persons with the risky project in CHOICE than in RANDOM, a difference which is highly statistically significant. Given that 77.2% of the participants can be classified as risk averse according to the risk preference game (see Table 1), this finding is not surprising since the lottery involves a high chance of earning nothing without offering a higher expected payoff than the safe option in return.

Table 4: Distribution of projects by treatment

	RANDOM (1)		CHOICE (2)		Difference (2)-(1)
	n	%	*n*	%	
Safe project	59	46.83	81	79.41	
Risky project	67	53.17	21	20.59	-32.59***
Observations	*126*		*102*		
Note:	Statistically significant mean differences: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.				

Second, we are interested in whether the possibility to choose freely between projects affects individuals' giving behaviour. Table 5 displays the share of individuals making positive transfers by treatment in rows (1) and (4), as well as by project within treatment in rows (2) and (3) for RANDOM and in rows (5) and (6) for CHOICE. The lower part of Table 5 shows differences between those shares by treatment in row (7), by treatment conditional on projects in rows (8) and (9) as well as by project within treatment in rows (10) and (11). These difference provide a preview on possible effects before accounting for selectivity in risk taking under CHOICE.

Table 5: Proportion of subjects making positive transfers

		Project of partner							
		All		Safe		Risky		Difference	
Project of donor				(a)		(b)		(b)-(a)	
RANDOM		%	*n*	%	*n*	%	*n*	%	P-value
(1)	All	42.0	*193*	35.8	*67*	45.2	*126*	9.4	0.20
(2)	Safe[+]	40.7	*59*	-	-	40.7	*59*	-	-
(3)	Risky	42.5	*134*	35.8	*67*	49.3	*67*	13.4	0.12
CHOICE		%	*n*	%	*n*	%	*n*	%	P-value
(4)	All	34.9	*123*	28.6	*21*	36.3	*102*	7.7	0.50
(5)	Safe[+]	39.5	*81*	-	-	39.5	*81*	-	-
(6)	Risky	26.2	*42*	28.6	*21*	23.8	*21*	-4.7	0.73
Differences across treatments			P-value		P-value		P-value		P-value
(7)	All: (4)-(1)	-7.0	0.21	-7.3	0.54	-9.0	0.17	-1.7	0.88
(8)	Safe: (5)-(2)	-1.2	0.89	-	-	-1.2	0.9	-	-
(9)	Risky: (6)-(3)	-16.4**	0.05	-7.3	0.54	-25.4**	0.03	-18.2*	0.09
Differences within treatments			P-value		P-value		P-value		P-value
(10)	RANDOM: (3)-(2)	1.9	0.81	-	-	8.6	0.34	-	-
(11)	CHOICE: (6)-(5)	-13.3	0.13	-	-	-15.7	0.16	-	-

Note: Statistically significant mean differences: * p<0.10 , ** p<0.05 , *** p<0.01. [+]Holders of safe projects only make transfers to worse-off partners holding the risky project because partners with the safe project are always equally well off.

In the RANDOM treatment about 40% of subjects are willing to make transfers with only small differences between safe and risky project holders. For subjects assigned to the risky project we observe a notable difference in the willingness to share income depending on the level of risk faced by beneficiaries, though. Subjects who have been lucky in the risky lottery are 13 percentage points more likely to share their payoff with partners who have been unlucky in the same lottery than with partners ending up with the safe outcome. With a p-value of 0.12 this difference is close to reaching statistical significance on the 10% level. This result points to particularly high degrees of solidarity when facing risk for exogenous reasons in environments such as the slums of Nairobi where our subjects are from where mutual aid has a strong tradition and is an important risk pooling device.

Under CHOICE we observe similar shares of about 40% of subjects willing to give for those who choose the safe project. However, of those individuals who choose the risky project a significantly smaller share is willing to make transfers in case they are lucky, especially to partners who have also self-selected into the risky project but have been unlucky. For the latter the difference to the RANDOM treatment is highly statistically significant with 25 percentage points compared to insignificant 7 percentage points for partners who choose the safe project. Together this yields a statistically significant average reduction the willingness to give of 16 percentage points. Hence, giving behaviour conditional on risk exposure seems to differ systematically between the RANDOM and the CHOICE treatment suggesting that there is a direct behavioural effect which is negative and driven by a change in behaviour of subjects who self-select into the risky project. In the next section we will assess whether this finding is confounded by selection bias resulting from self-selection into projects under CHOICE.

4.2 Econometric analyses

In the following we present and discuss the results of the econometric analyses we conducted as described in Section 3.2. We always present estimates with and without conditioning on covariates as discussed in Section 3.4 allowing us to assess whether imbalances across randomized samples and self-selection into projects in the CHOICE treatment affect the results. When using covariates, we report probit estimates, expressed as average marginal effects (AME), as well as inverse probability weighting (IPW) estimates which provide a better approximation to the average treatment effect compared to probit in the presence of heterogeneous treatment effects. Moreover, we assess effect heterogeneity with respect to the project choice of the partners who are assigned to our subjects and who are therefore the recipients of possible transfers. Further, we estimate everything separately for the full sample and for the sample in which we impose common support with respect to risk taking under CHOICE. Since applying the IPW approach and focusing on the common support sample produces the most reliable estimates, we will mainly concentrate on these results in the following discussion.

Table 6: Overall treatment effects and treatment effects conditional on risk exposure

			Full sample			Common support		
Covariates			No	Yes	Yes	No	Yes	Yes
			OLS	Probit	IPW	OLS	Probit	IPW
Effect	Project of donor	Project of partner	Coeff.	AME	Coeff.	Coeff.	AME	Coeff.
Overall treatment effects								
θ_R	-	-	-.326***	-.327***	-.329***	-.286***	-.309***	-.306***
			(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
θ_Y	All	All	-.070	-.051	-.039	-.126*	-.100$^+$	-.088
			(.258)	(.441)	(.529)	(.056)	(.132)	(.164)
	All	Risky §	-.090	-.081	-.085	-.142**	-.122*	-.126*
			(.171)	(.239)	(.212)	(.042)	(.081)	(.072)
Treatment effects conditional on risk exposure								
$\theta_{Y,0}$	Safe	Risky ‡	-.012	-.014	.014	-.036	-.052	-.032
			(.890)	(.871)	(.873)	(.689)	(.545)	(.712)
$\theta_{Y,1}$	Risky	All	-.163$^+$	-.162	-.111	-.204*	-.229*	-.177**
			(.108)	(.174)	(.212)	(.051)	(.050)	(.045)
	Risky	Risky	-.254**	-.264*	-.223**	-.296**	-.322**	-.278**
			(.026)	(.067)	(.037)	(.012)	(.023)	(.014)
	Risky	Safe	-.072	-.066	.002	-.111	-.142	-.076
			(.534)	(.626)	(.989)	(.355)	(.295)	(.451)

Note: Covariates are indicated in Table A2 and A3 in Appendix A. Probit coefficients are expressed as average marginal effects (AME). Standard errors are robust (Inverse probability weighting, IPW) or clustered at the individual level (OLS, Probit). Statistically significant coefficients: $^+$ p<0.15 , * p<0.10 , ** p<0.05 , *** p<0.01. P-values in parentheses. §The overall effect θ_Y for the subsample of transfers to partners with the *safe* project corresponds to the conditional effect $\theta_{Y,1}$ for this subsample (reported in the last row of this table) since only risky project holders make transfers to safe project holders. ‡Holders of the safe projects only make transfers to worse-off partners holding the risky project because partners with the safe project are always equally well off.

4.2.1 Overall treatment effects

Table 6 reports in the upper panel the estimation results for the overall effects of CHOICE on risk taking (θ_R) and on the willingness to make transfers (θ_Y). In line with the descriptive results, we find that the possibility to choose freely between projects reduces the share of subjects with the risky project significantly by about 30 percentage points. This finding is clearly linked to the fact that the majority of the participants in the experiment is risk averse. Moreover, the strength of the effect underlines the importance of considering this mechanism as a separate impact channel on solidarity behaviour.

In line with the negative effect on risk taking we find that the overall effect of CHOICE on the willingness to make transfers is also negative. The magnitude of the negative effect is around 9 percentage points in the common support sample with statistical significance nearly on the 15% level. When focusing on recipients of transfers who hold the risky project the magnitude of the effects is even larger by 4 percentage points with statistical significance also increasing to the 10% level. This suggests that there are, as hypothesized, some factors other than risk taking at work through which free project choice influences solidarity.

When comparing different estimates of the effects in the different columns of Table 6 we find that they are robust to both including covariates with small sample imbalances across the randomized samples and the estimator used. Moreover, the effects in the full and common support sample are of similar magnitude for risk taking and larger in absolute value for the willingness to make transfers. The latter points to some effect heterogeneity with respect to the characteristics without common support.

4.2.2 Treatment effects conditional on risk exposure

Given the observed overall negative effect of free project choice on the willingness to make transfers we now statistically test whether this effect, as discussed in Section 3.1.2, is driven by a change in giving behaviour and not simply the result of different levels of risk exposure across treatments. The coefficients $\theta_{Y,0}$ and $\theta_{Y,1}$ in the lower panel of Table 6 capture the effects of free choice on willingness to make transfers when holding risk exposure constant. Hence, they are informative about possible behavioural effects not resulting from changes in risk taking behaviour. As project choice is not random under CHOICE the results without covariates (columns 4 and 7 in Table 6) are potentially affected by selection bias. Interestingly, though, the estimated effects remain virtually unchanged when including the covariates needed for selection correction. However, it is dangerous to conjecture from this that selection bias is not an issue. Quite to the contrary, as we have shown in Section 3.3.2, a highly selective group of individuals chooses the risky project under CHOICE. Leaving out important covariates does change the results (not reported). It just happens that, when we appropriately account for selectivity, the estimated eects are very close to the ones without accounting for selectivity.

Confirming the descriptive results of Table 5, persons with the safe project do not condition their giving behaviour on whether project choice is free or not: the coefficient $\theta_{Y,0}$ is close to zero and p-values are quite high. In contrast, persons with risky projects give significantly less when projects can be selected. For these individuals the willingness to make transfers is around 18 percentage points lower under CHOICE than under RANDOM. This result implies that we can reject the hypothesis that the average effect on the willingness to give we measure is entirely caused by differential risk taking under CHOICE compared to RANDOM in favour of the alternative of the existence of a direct behavioural effect. Our findings show, though, that the behavioural response is limited to risk takers. This asymmetry compared to holders of the safe project suggests that compensation motives matter: risky project holders seem to be less willing to share their good-state lottery outcome when everyone can freely determine his or her risk exposure, i.e. when everyone had the chance - through project choice - to receive the high payoff. When comparing the estimates of $\theta_{Y,1}$ for the samples with transfers to partners with both types of projects and those with risky projects only, we find that the willingness to make transfers to the latter is even lower by an additional 10 percentage points. Hence, having a partner who failed after choosing the risky project deliberately additionally reduces solidarity. This suggests that attributions of responsibility also matter. Correspondingly, we find that the coefficient $\theta_{Y,1}$ for the subsample of transfers to partners who selected the riskless option and hence did not provoke neediness is statistically insignificant.

To sum up, the evidence presented in Table 6 regarding the effects of free choice conditional on project type ($\theta_{Y,r}$) shows that free project choice induces a change in giving behaviour which is characterized both by a generally higher reluctance to share high but risky payoffs and by an even higher reluctance to support individuals who have exposed themselves deliberately to high risk but have suffered the negative outcome. As we do not find any behavioural changes for individuals who choose the safe project, these findings suggest that individuals may consider the high payoff in the risky project as a well-deserved compensation for accepting the risk which they are less willing to share than money they can earn safely, especially with unlucky risk takers because they could have avoided their loss at no cost by choosing the safe project. They seem to feel less of an obligation to show solidarity in this case compared to the situation where sharing partners face unfavourable outcomes not because of their own doing but because of pure bad luck. This interpretation is in line with our finding that lucky winners in a lottery they face by pure chance show particularly high solidarity with unlucky losers in the same lottery, as indicated in Table 5.

5 Conclusion

In this study we experimentally investigate whether solidarity, which is a crucial base for informal insurance arrangements in developing countries, is sensitive to the extent to which individuals can influence their risk exposure. With slum dwellers of Nairobi our design measures subjects' willingness to share income with a worse-off partner both in a setting where participants could either deliberately choose or were randomly assigned to a safe or a risky project.

We find that the overall willingness to support others is significantly lower when the level of income risk can be chosen freely compared to when it is completely exogenous. Comparisons of transfers across treatments conditional on subjects' risk exposure reveal that solidarity is affected by a change in giving behaviour of risky project holders under free choice, who are less generous than their counterparts in the random treatment. Our evidence supports the hypothesis that this finding is driven by behavioural mechanisms which are linked to both the donor's choice as well as the recipient's choice. Firstly, since persons with safe income in contrast to risk takers, do not condition their transfer behaviour on whether project choice is free or not, this suggests that risk-taking individuals want to preserve their good-state outcome as merited reward for bearing the risk. Secondly, given that the willingness to share is especially low when partners end up empty-handed as a result of their risky choice instead of purely bad luck, this indicates that attributions of responsibility dampen solidarity.

Our findings have important implications for policies that possibly interact with existing informal insurance arrangements. One example is the promotion of innovative production technologies (e.g. new crops) which are typically more profitable but also riskier compared to traditional techniques. If such (observable) risk taking indeed crowds out solidarity, this impact must be taken into account when implementing these policies. Combining such interventions with formal insurance programs could then be a necessary option to compensate for reduced informal risk protection. The knowledge on behavioural patterns behind solidarity could also be exploited for an effective design of formal insurance interventions. On the one hand, the availability of formal insurance with voluntary take-up may reduce solidarity with individuals who choose not to insure themselves which may counteract the objective of increasing risk pooling and income equality (cf. Landmann et al., 2012; Lenel and Steiner, 2017; Lin et al., 2014). On the other hand, if subjects are not willing to support self-inflicted neediness, this might legitimate differentiated insurance premiums according to the riskiness of behaviour.[16] Such risk-adjusted contributions could be decisive in increasing enrolment in formal insurance schemes in developing countries and to improve financial sustainability of these schemes.

There are some possible limitations and extention possibilities regarding our study. Firstly, given that in the free choice treatment participants endogenously choose between a safe and a risky project we base the identification of the treatment effects conditional on risk exposure on a critical unconfoundedness assumption. While the detailed survey data retrieved during the experiment allows us to presume that we have all relevant information at our disposal to render this identification strategy plausible, the unconfoundedness assumption is not testable. However, in Wunsch and Strobl (2017) we discuss different identification strategies that exploit the double randomization nature of the experiment and that allow assessing the plausibility of identification based on the unconfoundedness assumption which is supported for the experiment and data we study here. Secondly, our experimental design does not allow us to distinguish between sharing

[16] Interestingly, the health insurance scheme *Gonoshasthaya Kendra* in rural Bangladesh applies premiums that are differentiated according to smoking habits (see http://www.munichre-foundation.org/dms/MRS/Documents/Microinsurance/2012_IMC/2012IMC_Presentations-and-papers/P4-MIC2012-Paper-Chowdhury/P4%20MIC2012%20Paper%20Chowdhury.pdf).

motives induced by own and partner's actions in the endogenous risk treatment. In a follow-up study we will therefore adjust the experimental set-up in a way that allows for this distinction in order to get further insights in the behavioural patterns behind solidarity. Thirdly, our anonymous one-shot game implicitly focuses on transfers motivated by intrinsic motivations such as altruism or distributive preferences. Allowing for repeated interactions and comparing the results to ours would allow investigating which role reciprocity and strategic incentives play in the relationship between risk taking and solidarity. Fourthly, our experiment investigates solely a situation where the outcomes of the safe and risky option are equal in expectation. However, solidarity might be affected differently when taking risk generates higher or lower expected returns than safe projects. While in the first case, the willingness to give might increase since risk taking is perceived as more profitable and acceptable, the contrary might be the case in the second scenario, in which risk takers are not adequately compensated for their risky choice and might be regarded as irresponsible. Our results can therefore be viewed as upper or lower bounds of these two scenarios regarding the impact of free risk exposure choice on solidarity (cf. Cettolin and Tausch, 2015).

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A Appendix: Estimation results

Table A1: Determinants of risk taking in the risk preference game

	Coeff.	(SE)
Age	0.004	(0.008)
Male	-0.011	(0.158)
Kibera	-0.042	(0.165)
No. of adults	-0.017	(0.040)
No. of children	-0.045	(0.041)
Employed in paid work	-0.547**	(0.236)
Wealth index quintile 2	0.241	(0.285)
Wealth index quintile 3	0.234	(0.213)
Wealth index quintile 4	0.011	(0.227)
Wealth index quintile 5	0.202	(0.225)
Enrolled in health insurance	0.144	(0.182)
Visited health care provider	-0.123	(0.153)
Expected future health shock	0.018	(0.026)
GSS index	0.114	(0.088)
Inequality aversion 1	0.096	(0.174)
Inequality aversion 2	0.187	(0.165)
Ordered probit constant 1	-0.160	(0.315)
Ordered probit constant 2	0.122	(0.313)
Ordered probit constant 3	0.283	(0.313)
Ordered probit constant 4	0.653**	(0.315)
Ordered probit constant 5	0.731**	(0.316)
Ordered probit constant 6	0.959***	(0.319)
Ordered probit constant 7	1.625***	(0.333)
Observations	228	

Note: Estimation method is ordered probit. Dependent variable: lottery number chosen in the risk preference game. Standard errors in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A2: Estimation results for unrestricted effects with covariates

	Full sample			Common support		
Dependent variable	Y_i	Y_i	R_i	Y_i	Y_i	R_i
Project of partner	Both	Risky		Both	Risky	
Choice	-0.136	-0.218	-0.966***	-0.277	-0.343*	-0.884***
	(0.176)	(0.187)	(0.198)	(0.186)	(0.201)	(0.204)
Male	0.192	0.276	0.061	0.371**	0.473**	0.051
	(0.164)	(0.180)	(0.189)	(0.176)	(0.195)	(0.199)
Kibera	0.149	0.162	0.0658	0.127	0.173	0.009
	(0.174)	(0.186)	(0.196)	(0.190)	(0.204)	(0.210)
Inequality aversion 2	-0.111	0.076	0.324*	-0.178	0.0172	0.384*
	(0.164)	(0.184)	(0.194)	(0.177)	(0.199)	(0.211)
Risk preference	-0.025	-0.041	0.027	-0.016	-0.036	0.035
	(0.032)	(0.034)	(0.035)	(0.036)	(0.038)	(0.038)
Ethnicity: Kamba	-0.226	-0.157	0.376	-0.148	-0.020	0.319
	(0.251)	(0.257)	(0.254)	(0.260)	(0.272)	(0.259)
Age	-0.00025	-0.0095	-0.0056	0.0048	-0.0076	-0.0082
	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)	(0.012)
Monthly income	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
No. of adults	0.033	0.049	-0.016	0.021	0.048	0.011
	(0.042)	(0.042)	(0.045)	(0.046)	(0.048)	(0.051)
No. of dependents	-0.036	-0.027	-0.013	-0.082*	-0.080*	0.011
	(0.039)	(0.039)	(0.041)	(0.045)	(0.045)	(0.043)
Constant	-0.273	-0.011	0.023	-0.369	-0.071	-0.062
	(0.335)	(0.368)	(0.394)	(0.363)	(0.409)	(0.408)
Observations	316	228	228	276	197	197

Note: Estimation method is probit. Standard errors clustered at the individual level in parentheses: *** p<0.01, ** p<0.05, * p<0.10

Table A3: Estimation results for treatment effects conditional on risk exposure (with covariates)

	Full sample				Common support			
Dependent variable	Y_i	Y_i	Y_i	Y_i	Y_i	Y_i	Y_i	Y_i
Project of subject	Safe	Risky	Risky	Risky	Safe	Risky	Risky	Risky
Project of partner	Risky	Both	Risky	Safe	Risky	Both	Risky	Safe
Choice	-0.039	-0.450	-0.751*	-0.197	-0.158	-0.641*	-0.941**	-0.423
	(0.241)	(0.337)	(0.428)	(0.406)	(0.262)	(0.336)	(0.443)	(0.409)
Male	0.390*	0.009	0.229	-0.234	0.566**	0.163	0.412	-0.076
	(0.236)	(0.252)	(0.302)	(0.309)	(0.258)	(0.266)	(0.324)	(0.331)
Kibera	0.345	0.102	0.194	0.0143	0.394	-0.00341	0.127	-0.140
	(0.235)	(0.253)	(0.305)	(0.320)	(0.259)	(0.260)	(0.322)	(0.331)
Inequality aversion 2	0.181	-0.169	0.076	-0.460	0.222	-0.187	0.0265	-0.431
	(0.244)	(0.249)	(0.326)	(0.336)	(0.279)	(0.269)	(0.352)	(0.361)
Risk preference	-0.014	-0.042	-0.076	-0.010	-0.043	-0.019	-0.048	0.011
	(0.045)	(0.054)	(0.064)	(0.065)	(0.052)	(0.058)	(0.068)	(0.070)
Unemployed	0.099	-0.034	0.025	-0.092	0.412	-0.047	-0.052	-0.024
	(0.259)	(0.275)	(0.324)	(0.325)	(0.295)	(0.296)	(0.352)	(0.352)
Prop. of HH members with HINS	0.105	0.037	0.215	-0.136	0.102	0.0278	0.239	-0.177
	(0.202)	(0.215)	(0.243)	(0.242)	(0.224)	(0.212)	(0.248)	(0.235)
GSS: Fairness	-0.465*	0.508*	0.309	0.738**	-0.601**	0.455	0.168	0.760**
	(0.243)	(0.299)	(0.358)	(0.367)	(0.282)	(0.297)	(0.372)	(0.369)
Monthly income	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
No. of adults		0.020	0.050	-0.001		-0.007	0.026	-0.038
		(0.071)	(0.081)	(0.087)		(0.074)	(0.087)	(0.093)
Ethnicity: Luhya		-0.200	-0.603*	0.170		-0.238	-0.723*	0.182
		(0.298)	(0.352)	(0.375)		(0.312)	(0.392)	(0.398)
No. of other earners	-0.000				0.098			
	(0.084)				(0.097)			
Constant	-0.458	-0.240	-0.203	-0.308	-0.659	-0.194	-0.123	-0.294
	(0.361)	(0.365)	(0.447)	(0.469)	(0.421)	(0.380)	(0.486)	(0.505)
Observations	140	176	88	88	118	158	79	79

Note: Estimation method is probit. Standard errors clustered at the individual level in parentheses: *** p<0.01, ** p<0.05, * p<0.10

B Appendix: Experimental instructions (exemplarily for CHOICE)

The entire experiment involved three games. Thereof, only two games are relevant for this study, with Game 2 corresponding to the risk preference game and Game 3 to the risk solidarity game. Also, in Game 3 we asked subjects to state their expectations on their partners' transfers, however, we do not use this information in this study. For the sake of simplicity, we therefore present a version of the original instructions shortened by the parts that are not relevant for this study.

General instructions

Welcome and thank you for participating in our study. You are now taking part in an experiment on economic decision-making.

Three Games:
In the following, you will play three short games, named [*Game 1*,] *Game 2* and *Game 3*. In each game, you will make one or several decisions. The result of your decision(s) will determine how much money you can finally earn in the respective game. We will explain later, how these three games work in detail.

Payment:
However, please note that we will only pay you according to the result in one of the three games.

How will we determine your payment?
The computer will record what you have finally earned [in Game 1,] in Game 2 and in Game 3. At the end of the experiment, the computer will randomly select [Game 1,] Game 2 or Game 3 with equal chance. We will pay you in shillings the final earnings you have made in this selected game. So, please remember that you will receive either your final earnings [from Game 1 or] from Game 2 or from Game 3, according to what game the computer will randomly select. Therefore, it is important to think carefully about the choice you make in each game.

Test Questions:
Before each game starts, we will ask you to answer a few test questions to check if the rules of the games are clear to you. Please note that you will not get money for your answers and decisions in these test questions.

Questionnaire:
After completing the three games, we will ask you to answer a few short questions about yourself and your household.

All your decisions and answers in this study will be kept confidential and only used for academic research purposes.

Instructions for Game 2

[Game 2 is very similar to the game before. But please note that it is completely independent from Game 1]. Here is how Game 2 works.

<u>Project Income:</u>

Assume that within your business, you have [again] a choice of 8 different income opportunities and you have to decide which one you want to realize. The table on your screen describes these income opportunities, named *Project K* to *R*:

[Screenshot 1]

Game 2

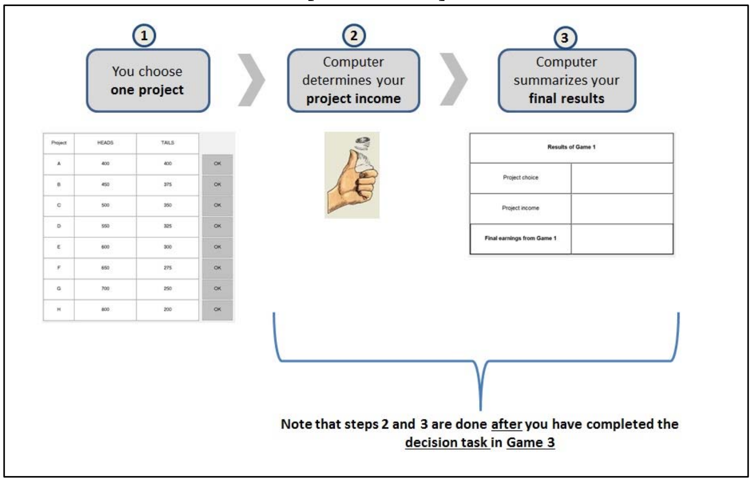
Project	HEADS	TAILS	
K	320	320	OK
L	400	280	OK
M	480	240	OK
N	560	200	OK
O	640	160	OK
P	720	120	OK
Q	800	80	OK
R	880	0	OK

We will ask you to choose 1 out of the 8 projects. How much money you can earn from a project is [again] based on flipping a coin. [As in the game before,] the computer flips a coin after you have chosen your preferred project. If the coin lands on heads, you earn the amount given in the column “HEADS” in the row of your chosen project. If the coin lands on tails, you earn the amount given in the column “TAILS” in the row of your chosen project. Please choose the project that you prefer the most. There is no right or wrong answer.

<u>Summary:</u>

The picture on your screen shows the sequence of events in Game 2.
Please note that steps 2 to 3 will be done after you have completed the decision task of GAME 3.

[Screenshot 2]



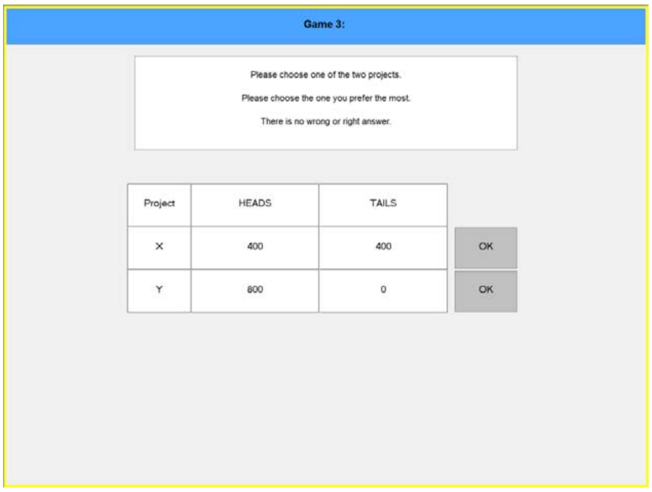
Instructions for Game 3

In this game, you will make decisions that will determine your earnings and the earnings of another participant. Please note that Game 3 is completely independent of [Game 1 and] Game 2. Here is how Game 3 works.

1) Project Choice

In this game, you have a choice of 2 different income opportunities, named Project X and Y. The table on your screen describes these two projects.

[Screenshot 3]



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With each of these projects you can earn some income. We will ask you to choose 1 of the 2 projects. The amount of money you can earn from a project is again based on flipping a coin, as in Game [1 and] 2. If the coin lands on heads, you earn the amount in the column “HEADS” for your chosen project. If the coin lands on tails, you earn the amount in the column “TAILS” for your chosen project. Please choose the project that you prefer the most. There is no right or wrong answer.

2) Partner

After you have chosen your preferred project, the computer will randomly pair you with another person in this room. However, you will not know which person your partner is. His or her identity will be not revealed either during or after the game. Your partner will also have already chosen either project X or Y. How much he/she will earn from the project is also determined by coin flip. Please note that another coin will be flipped for your partner, so that you both get individual results (i.e. heads or tails). Please also note that you will not know your partner’s project choice and project income until the end of Game 3.

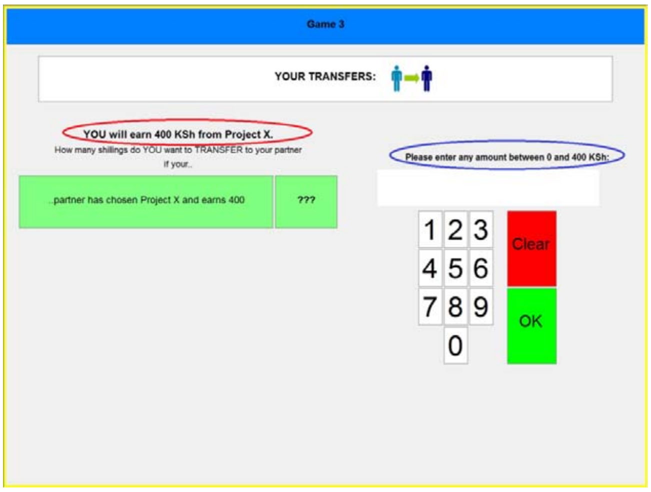
3) Transfers

In this game, you can give some of your project income to your partner if you want to. Please note that you can give some of your income to your partner, but you do not have to. The amount that you decide to transfer to your partner will be deducted from your project income and added to your partner’s project income. Just as you, your partner can give some of his/her income to you if he/she wants to, but he/she also does not have to. The amount that he/she decides to transfer to you will be deducted from his/her project income and added to your project income. Please note that you both will decide how much you want to transfer to your partner before both of your project incomes are determined by coin flip. So, we will ask you both to decide in advance on the amount you wish to transfer for every possible combination of incomes you both might earn. The next two examples will explain the possible cases.

Example 1 – You choose Project X

Please look at your screen.

[Screenshot 4]



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This screen appears, if you have chosen Project X. With Project X, you will earn 400 shillings, regardless of whether the coin lands on heads or on tails. We will ask you to decide how much you would like to transfer from your project income of 400 shillings to your partner. As the partner's income is not yet known, we will ask you to decide on your transfers for every possible amount that your partner might have earned with his/her chosen project. Therefore, the first question (in green) ask what amount you would like to transfer from your project income of 400 shillings to your partner if your partner has also chosen Project X and earns 400 shillings. Please enter the amount that you would like to give to your partner by using the number pad. You can enter any amount between 0 and your full project income, that is 400 shillings in this example.

[Screenshot 5]

Similarly, the second and third questions ask what amount you would like to transfer to your partner if you earn 400 shillings and your partner has chosen Project Y and earns 800 or 0 shillings. For each question, you can enter any amount between 0 and your full project income, that is 400 shillings. Your entered transfer amounts will appear in the small grey boxes (here on your screen, they are left empty). Please note that later only one of the three possible partner's incomes will be realized, depending on which project your partner has chosen and what the result of the partner's coin flip is. The transfer amount that you have stipulated for exactly this realized partner's income will be deducted from your project income afterwards.

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Example 2 – You choose Project Y

[Screenshot 6]

If you have chosen Project Y, you will earn 800 shillings if the coin lands on heads and 0 shillings if the coin lands on tails. If you earn 0 shillings, you cannot make any transfers to your partner. If you earn 800 shillings, you can transfer some money to your partner. So, we will ask you to decide how much you would like to transfer to your partner if you would earn 800 shillings. As in Example 1, we will ask you to enter your transfer amounts for each of your partner's possible project incomes, that is 400, 800 and 0 shillings. Again, you can enter any amount between 0 and your full project income, that is 800 shillings in this case. As already explained in Example 1, later only one of the three possible partner's incomes will be realized. The transfer amount that you have stipulated for exactly this realized partner's income will be deducted from your project income afterwards. Please note that you and your partner make the transfer decisions simultaneously. Please also note that you will not know how much your partner has decided to give to you until the end of Game 3. Also, your partner will not know your transfer decisions until the end of Game 3.

5) Coin flip

After you have entered the [transfer] amounts, the computer will determine your project income by flipping a coin. The computer will also determine your partner´s project income by flipping another coin. The computer will now credit you and your partner with the transfer amounts that you each stipulated for each other for exactly the now realized incomes.

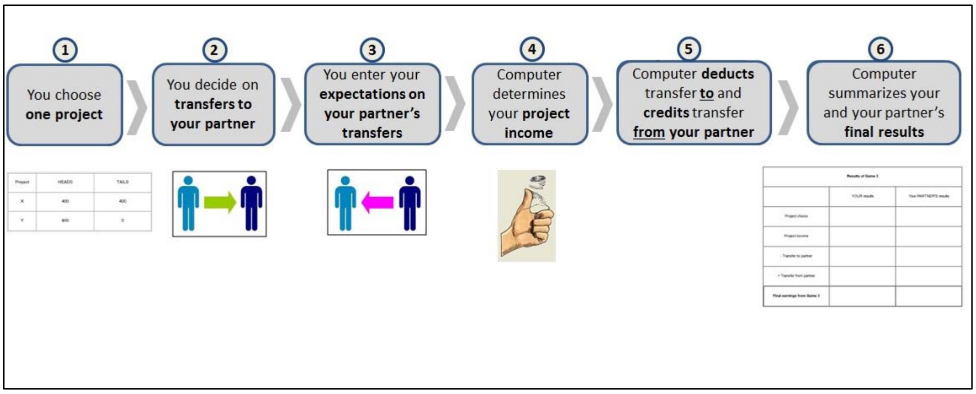
6) Final earnings of Game 3:

Your final earnings from Game 3 will be your project income MINUS the transfer that you made to your partner PLUS the transfer that your partner made to you.

Summary:

The picture on your screen shows the sequence of events in Game 3.

[Screenshot 7]



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