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Abstract

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 only a subgroup of subjects reduces willingness to give when risk exposure is a choice. Responses are limited to donors in the risky project, whereas donors in the safe project do not adjust their willingness to give. This difference in behaviour can be explained by differential giving in the absence of choice. Lucky winners with the risky project show a particularly high degree of solidarity with unlucky losers compared to donors and partners assigned to the safe project when they face risk for exogenous reasons. The possibility of free project choice removes these differences in generosity and we show that this is driven by attributions of responsibility for neediness. Our results suggest that crowding out of informal support might be less severe than suggested by the studies from Western countries and the evidence on formal insurance from developing countries.

JEL-Codes: D810, C910, O120, D630.

Keywords: solidarity, risk taking, Kenya.

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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 the following reason. 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 less money (Bolle and Costard, 2013; Cappelen et al., 2013).

Direct evidence on the question whether the extent to which individuals can control their risk exposure affects solidarity is scarce and mainly based on three experimental studies conducted in high-income Western countries (Trhal and Radermacher, 2009; Cettolin and Tausch, 2015; Akbas et al., 2016). 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. All three 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. Yet, these findings are not necessarily transferable to developing countries. The countries in which the studies have been conducted (Germany, the Netherlands, the US) 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, public social security nets are absent and mutual voluntary help is an essential risk-pooling source for households. 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

[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).

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 are responsible for their behaviour (e.g. in the case of smoking or low effort) the majority favours not to compensate for the consequences or even to punish the responsible person. However, responses differ strongly by country, 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 four existing experiments which have been conducted in Cambodia (Lenel and Steiner, 2017), China (Lin et al., 2014), Ethiopia (Morsink, 2017) and the Philippines (Landmann et al., 2012) find that the availability of formal insurance reduces informal transfers.[2] 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). Moreover, insurance is a device to opt out of risk while we are interested in more general situations that include opting into risk. The latter is particularly important in developing countries which are characterized by under-investment in profitable business opportunities, leaving much of the earnings and growth potential unexploited (De Mel et al., 2008; Dodlova et al., 2015; Fafchamps et al., 2014; Grimm et al., 2011, 2012; Kremer et al., 2016; McKenzie and Woodruff, 2008).

As a first 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 latter involved a one-half probability to end up with nothing. After being randomly matched with another person, subjects could make voluntary transfers to their partner. Using the strategy method, we elicit transfers for all possible choices and situations of the partner independent of the realized states. This allows us to compare transfers to partners with different projects both across and within treatments.

[2] Another related literature studies risk sharing under different enforcement mechanisms in microcredit contracts in developing countries, see e.g. Fischer (2013) and Kono (2006).

As a second contribution, our design allows us to disentangle changes in the willingness to give due to attributions of responsibility for neediness from other reasons behind changes in the solidarity norm. We exploit that transfers to partners in the safe project should be unaffected by free choice if attributions of responsibility are the driver behind reduced solidarity. Our design allows us to compare transfers to partners in the safe project not only relative to partners in the risky project within treatments as in the three previous studies, but also across treatments.

The third contribution is a methodological one. We show that the average effect of CHOICE on giving, i.e. the comparison of mean giving across treatments, does not measure the behavioural effects we are interested in and, most importantly, that it is not possible to ensure by the experimental design that the average treatment effect equals the behavioural effect. We show that the two effects differ if giving depends on the project faced by donors within treatments. This is likely to be the case as income risk and payoffs differ by project, which affects donors' utility. Differential giving by project is problematic because the CHOICE treatment changes the distribution of donors' projects at the same time as it changes the process by which transfer recipients become needy (bad luck versus choice). Both changes can affect giving but only the latter is what we are interested in. Moreover, the nature of the treatment is such that the former cannot be avoided. Hence, heterogeneous responses to projects within treatments bias the average treatment effect away from the behavioural effect of interest. This is also true for the designs used by Trhal and Radermacher (2009) and Cettolin and Tausch (2015), which implies that the treatment effects reported in these studies may differ from the behavioural effects of interest.[3]

The experimental design allows testing for different giving behaviour across projects within treatment. In our case, we do find evidence for heterogeneous giving behaviour across projects, which implies that the average treatment effect would be biased. We show that comparing giving across treatments conditional on project isolates the behavioural effect. This, however, requires accounting for self-selection into projects in CHOICE. We apply three approaches that differ in the assumptions we impose to solve the selection problem: non-parametric bounds, identification based on unconfoundedness that exploits the rich data on risk preferences and other drivers of risk taking collected within our experiment, and an approach that allows for some types of selection based on unobserved factors. As the results from all three approaches support each other, we are confident that our estimates do not suffer from selection bias.

We find that only a subgroup of subjects reduces willingness to give when risk exposure is a choice. Responses are limited to donors in the risky project. In contrast, donors in the safe project do not adjust their willingness to give. Within-subject and within treatment comparisons suggest that this difference

[3]In contrast, Akbas et al. (2016) focus on a design where only a subgroup of individuals, the so-called stakeholders, are randomized into the treatments while the redistribution decisions are being made by a third group of so-called observers. As observers are not part of the treatments, they are unaffected by different distributions of projects within treatments. Akbas et al. (2016) also elicit redistribution decisions by stakeholders and show that they differ from observers' decisions with the latter favouring more equal distributions and less severe punishment of risk taking. Hence, distinguishing between observer and stakeholder decisions is important. As the stakeholders are the ones who redistribute income in reality, we follow Trhal and Radermacher (2009) and Cettolin and Tausch (2015) and study redistribution decisions by stakeholders.

in behaviour can be explained by different behaviour in the RANDOM treatment. Lucky winners with the risky project show a particularly high degree of solidarity with unlucky losers compared to donors and partners assigned to the safe project when they 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. The possibility of free project choice removes the differences in generosity. In CHOICE, holders of the risky project no longer make a difference between partners with the safe or the risky project and they no longer give more than holders of the safe project. Our results also show that attributions of responsibility for neediness seem to drive the reduction in willingness to give we observe for holders of the risky project as we find no effect of CHOICE for transfers to partners with the safe project.

The difference in behaviour of holders of the risky and the safe project is an important finding with respect to the literature on the crowding out of informal insurance by the availability of formal insurance. In this literature, everybody is exposed to risk in the baseline state by construction. Hence, we expect larger effects on solidarity compared to more general situations, where only a subset of individuals is exposed to risk in the baseline state. Our findings show that if the share of individuals opting into risk is relatively small, then the overall effect on solidarity is also small and can even become negligible. This is important for policies that aim to encourage investments into new but risky technologies or new business opportunities to reduce poverty and foster economic growth in developing countries. Our results suggest that crowding out of informal support might be less severe than suggested by the studies from Western countries and the evidence on formal insurance from developing countries.

The remainder of this article is organized as follows. The next section describes the experiment we conducted as well as the data we collected within the experiment. In Section 3 we discuss theoretical considerations and derive hypotheses for the empirical analysis. Section 4 contains our methodological contribution and describes our empirical strategy. In Section 5 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 25 computer-supported workplaces. At the time of our experiment, it maintained a subject pool of around 5,000 registered individuals, mainly 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.

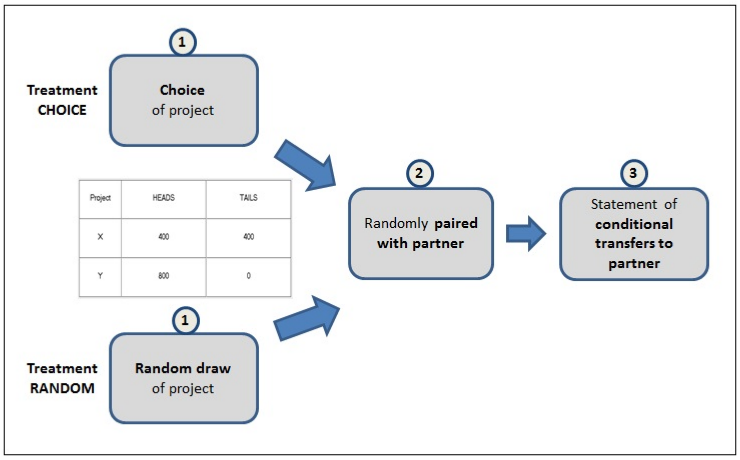
2.2 Experimental design

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, 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 their gift for every possible payoff of their partner (i.e. 400, 800 or 0 KSh). 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 ($\sim$1.60 USD).

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.

Figure 1: Sequence of steps in the risk solidarity game



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 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 E for an English version of the instructions, exemplarily for CHOICE).[4] 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 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. 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.[5] The earnings of the incentivized games, which amounted on average to 412 KSh per person, were transferred cashless to the respondents' MPesa accounts.[6]

[4]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).

[5]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.

[6]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).

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 4 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 (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).

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.

In the game, each subject was asked to choose one out of eight different lotteries (see Table 1, 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).[7] 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. 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 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)[8] 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; Müller and Rau, 2016).[9]

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 enrollment, 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.

[7] 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.

[8] 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).

[9] 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.

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.[10] The estimated $\hat{r}$ is 0.72 (p-value=.00), implying 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

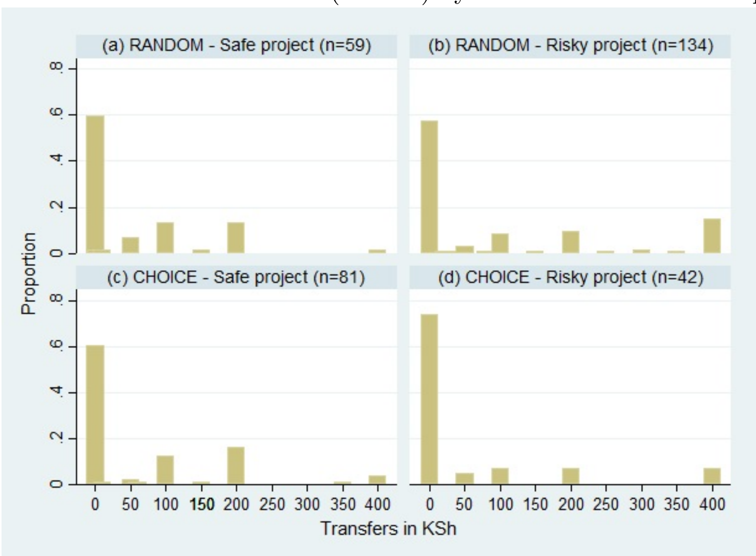
The most important source of data stems from the risk solidarity game which measured transfer behaviour under the CHOICE and RANDOM treatment. Figure 2 displays the distributions of our major outcome of interest, the stated amounts of transfers (in KSh), by treatment and sender's project. We only consider 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).[11] The reason for this restriction is that we are interested in solidarity

[10] 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 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.

[11] 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]).

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 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 Theoretical considerations and effects of interest

We assume that individual transfer decisions are motivated by own income and by the desire to behave in line with one's own solidarity norm, η_i. Subjects make transfer decisions once they know in which project they are for all possible situations of the partner where the assigned partner is worse off. Hence, all payoff combinations for which transfer decisions have to be made are known. Thus, the income of subject i is determined by the payoff from the project, x_i, and the transfer T_i made to the assigned partner. Utility takes the following form:

$$U(x_i, T_i, \eta_i) = u(x_i - T_i) - v(\eta_i - T_i) \tag{1}$$

where $u(\cdot)$ is subject i's utility from the net payoff after deducting one's own transfer to the assigned partner if she is worse off than i, with $u'(\cdot) > 0$ and $u''(\cdot) < 0$. The solidarity norm, η_i, specifies the transfer amount to the assigned partner that is perceived to be adequate by donor i. We do not restrict the way in which these norms come about. Hence, they can depend on social norms, individual perceptions of fairness, preferences regarding redistribution such as inequality aversion or different combinations of them. The function $v(\cdot)$ resembles the utility cost subject i has to bear when the transfer T_i deviates from the norm η_i. Following the literature (Cappelen et al., 2007; Konow, 2010; Cappelen et al., 2013; Lenel and Steiner, 2017), we assume that $v'(\eta_i - T_i)(\eta_i - T_i) > 0$ for $\eta_i \neq T_i$ and $v''(\cdot) > 0$. Subject i maximizes utility with respect to the transfer T_i. Konow (2010) shows that the optimal transfer T_i^* increases with η_i for a given payoff x_i with $0 < dT_i^*/d\eta_i < 1$. In the empirical analysis we are interested in the willingness to make a transfer, i.e. in a binary variable $Y_i = \underline{1}(T_i > 0)$ where $\underline{1}(\cdot)$ is the indicator function. We assume that the solidarity norm η_i not only affects the transfer level if positive but also the probability for positive transfers, i.e. that $dPr(T_i^* > 0)/d\eta_i > 0$. In the following we allow the solidarity norm and, hence, willingness to give to depend on the following factors:

- treatment C_i, where $C_i = 0$ denotes RANDOM and $C_i = 1$ denotes CHOICE
- donor's risk exposure R_i where $R_i = 0$ denotes the safe project and $R_i = 1$ the risky project
- partner's risk exposure r where $r = 0$ denotes the safe project and $r = 1$ the risky project.

Using the potential outcome framework typically applied in the statistical evaluation literature, we denote by $Y_r^C(R)$ potential willingness to give of a subject in project $R \in \{0, 1\}$ to a partner in project $r \in \{0, 1\}$ given treatment $C \in \{0, 1\}$. Moreover, we account for the fact that a given subject may end up in different projects in RANDOM than in CHOICE by using potential outcomes for the projects as well, $R^C \in \{0, 1\}$ for $C \in \{0, 1\}$.

Our main hypothesis takes up theoretical arguments and empirical findings from previous studies that argue that responsibility for neediness affects solidarity negatively. In our paper, we are interested in the question whether this is also true in the context of a developing country with a strong social norm for solidarity and heavy dependence on informal support due to the unavailability of generous social security systems and other types of formal insurance. The arguments brought forward by existing studies imply that responsibility for neediness changes the solidarity norm that determines transfers such that lower transfers are more acceptable. The specific channels differ, though. Trhal and Radermacher (2009), Akbas et al. (2016) and Lenel and Steiner (2017) argue that individuals have different fairness views depending on the process that generates inequality. Morsink (2017) explains the same prediction by a shared norm about low risk taking. Cettolin and Tausch (2015) argue that inequality aversion is lower when neediness is self-inflicted. Lenel and Steiner (2017) also provide an alternative explanation for lower solidarity with risk takers. They argue that choosing the risky option reveals their risk preference and signals to donors that they do not suffer a utility loss from being exposed to risk. When risk is exogenous, though, some safety choosers will also be exposed to risk. These subjects suffer a utility loss compared to their preferred option, and donors may find it more adequate to give because they know that this will compensate some safety choosers for having to bear risk. All of the above arguments imply that transfers should be lower in CHOICE than in RANDOM which is summarized in the following hypothesis:

Hypothesis 1: Subjects who self-select into the risky project in CHOICE on average receive lower transfers than subjects who end up in the risky project for exogenous reasons in RANDOM.

To test hypothesis 1 we need to make sure that we compare donors who are in different treatments but in the same project. Otherwise, we confound the behavioural treatment effect of interest with the effect of being in a different project because the CHOICE treatment changes the distribution of donors' projects at the same time as it changes the process by which transfer recipients become needy (bad luck versus choice), where only the latter is what we are interested in. Hence, the statistical equivalent of this hypothesis is $\beta_{1,1}(R) \equiv E[Y_1^1(R) - Y_1^0(R)] < 0$ for $R \in \{0,1\}$, where the notation $\beta_{1,1}(R)$ indicates that we compare willingness to give to partners with the risky project, $r = 1$, in both treatments for donors in project R. In the next section, we show formally why fixing projects is necessary to isolate the behavioural effect of interest. Hypothesis 1 can also be tested based on the average behavioural effects, which can be obtained by aggregating the conditional (on project) behavioural effects using the distribution of projects in RANDOM, R^0, or in CHOICE, R^1:

$$\begin{aligned}
\beta_{1,1}(R^0) &= E[\beta_{1,1}(0)|R^0 = 0]Pr(R^0 = 0) + E[\beta_{1,1}(1)|R^0 = 1]Pr(R^0 = 1) \\
&= \beta_{1,1}(0)Pr(R^0 = 0) + \beta_{1,1}(1)Pr(R^0 = 1) \\
\beta_{1,1}(R^1) &= E[\beta_{1,1}(0)|R^1 = 0]Pr(R^1 = 0) + E[\beta_{1,1}(1)|R^1 = 1]Pr(R^1 = 1) \\
&= \beta_{1,1}(0|R^1 = 0)Pr(R^1 = 0) + \beta_{1,1}(1|R^1 = 1)Pr(R^1 = 1).
\end{aligned}$$

The second equality holds because projects have been randomized in RANDOM. In the last line we define $E[\beta_{1,1}(R)|R^1 = R] \equiv \beta_{1,1}(R|R^1 = R)$. Testing hypothesis 1 corresponds to testing $\beta_{1,1}(R) < 0$ for $R \in \{0,1\}$ and $\beta_{1,1}(R^C) < 0$ for $C \in \{0,1\}$. If the data support hypothesis 1, than this is evidence that the solidarity norm in RANDOM differs from the norm in CHOICE such that giving nothing is considered more acceptable. However, it does not allow distinguishing between lower solidarity due to attributions of responsibility for neediness and other explanations for reduced solidarity such as the utility loss compensation motive brought forward by Lenel and Steiner (2017). Our design allows us to test whether attributions of responsibility are the driver behind reduced solidarity. On the one hand, if responsibility matters, then, ceteris paribus, willingness to give to partners who self-select into the risky project in CHOICE should be lower than with partners who self-select into the safe project. This is summarized in the following hypothesis:

Hypothesis 2: Subjects who self-select into the risky project in CHOICE on average receive lower transfers than subjects who self-select into the safe project.

Noting that only donors in the risky project make transfers to partners in the safe project, the statistical equivalent of this hypothesis is $\beta_{1,0}(1|R^1 = 1) \equiv E[Y_1^1(1) - Y_0^1(1)|R^1 = 1] < 0$, where the notation $\beta_{1,0}(1|R^1 = 1)$ indicates that we compare average willingness to give of donors who self-selected into the risky project in CHOICE, $R = R^1 = 1$, to partners in the risky project, $r = 1$, with their willingness to give to partners in the safe project, $r = 0$. On the other hand, willingness to give to partners with the safe project should not differ across treatments if responsibility for neediness is the only driver behind different solidarity in CHOICE. This is summarized in the following hypothesis:

Hypothesis 3: Subjects who self-select into the safe project in CHOICE on average receive the same transfers as subjects who end up in the safe project for exogenous reasons in RANDOM.

Noting again that only donors in the risky project make transfers to partners in the safe project, the statistical equivalent of this hypothesis is $\beta_{0,0}(1) \equiv E[Y_0^1(1) - Y_0^0(1)] = 0$, where the notation $\beta_{0,0}(1)$ indicates that we compare willingness to give to partners with the safe project, $r = 0$, in both treatments for donors in the risky project, $R = 1$. In the following we describe how we can obtain the empirical counterparts of the behavioural effects (the β's) we need to estimate to test hypotheses 1 and 2.

4 Empirical strategy

4.1 The identification problem

Table 2 summarizes the quantities that need to be estimated to obtain the behavioural effects $\beta_{1,1}(r)$ and $\beta_{1,1}(r|R^1 = r)$ for $r \in \{0,1\}$ to test hypothesis 1, $\beta_{1,0}(1|R^1 = 1)$ to test hypothesis 2, and $\beta_{0,0}(1)$ to test hypothesis 3. The aggregate effects $\beta_{1,1}(R^0)$ and $\beta_{1,1}(R^1)$ can be obtained from the former two, respectively. Additionally, we need the share in project $r \in \{0,1\}$, $Pr(R^C = r)$, for each treatment $C \in \{0,1\}$, which is known to be 0.5 for RANDOM and given by $E[R = r|C = 1]$ for CHOICE since treatments have been randomized. Table 2 also shows the quantities we can obtain directly from the data without any assumptions. To describe observed quantities as opposed to potential outcomes, we denote by Y_r observed willingness to give to a partner with project $r \in \{0,1\}$.

Table 2: Required and observed quantities

Hypothesis	Effect	Quantity required	Quantity observed	Assumptions required
1	$\beta_{1,1}(r)$	$E[Y_1^1(r)]$	$E[Y_1|C = 1, R = r] = E[Y_1^1(r)|R^1 = r]$	yes
		$E[Y_1^0(r)]$	$E[Y_1|C = 0, R = r] = E[Y_1^0(r)]$	no
1	$\beta_{1,1}(r|R^1 = r)$	$E[Y_1^1(r)|R^1 = r]$	$E[Y_1|C = 1, R = r] = E[Y_1^1(r)|R^1 = r]$	no
		$E[Y_1^0(r)|R^1 = r]$	$E[Y_1|C = 0, R = r] = E[Y_1^1(r)]$	yes
2	$\beta_{1,0}(1|R^1 = 1)$	$E[Y_1^1(1)|R^1 = 1]$	$E[Y_1|C = 1, R = 1] = E[Y_1^1(1)|R^1 = 1]$	no
		$E[Y_0^1(1)|R^1 = 1]$	$E[Y_0|C = 1, R = 1] = E[Y_0^1(1)|R^1 = 1]$	no
3	$\beta_{0,0}(r)$	$E[Y_0^1(1)]$	$E[Y_0|C = 1, R = 1] = E[Y_0^1(1)|R^1 = 1]$	yes
		$E[Y_0^0(1)]$	$E[Y_0|C = 0, R = 1] = E[Y_0^0(1)]$	no

The main lesson from Table 2 is that only hypothesis 2 can be tested without further assumptions because only $\beta_{1,0}(1|R^1 = 1)$ can be identified from observed objects. All other behavioural effects of interest are not identified from the data without additional assumptions because we need to condition on projects in these cases and subjects self-select into projects in CHOICE. Conditioning on the treatment is unproblematic because they are randomized. Similarly, conditioning on projects R^0 within RANDOM is unproblematic because they are randomized as well. However, average willingness to give conditional on project is not identified in CHOICE because we only observe this for the sub-population who self-selects into the project. Also, we do not observe average willingness to give in CHOICE conditional on the project one would have obtained in RANDOM and vice versa.

While it is intuitive that we need to condition on projects to test hypothesis 1, there still remains the question whether we cannot just compare average willingness to give across treatments to measure the overall effect on solidarity. This average treatment effect compares willingness to give to partners with the risky project across treatments for the observed distribution of projects within treatment. In CHOICE this distribution is R^1 while in RANDOM it is R^0. Hence, the average treatment effect is given by $\theta_1 \equiv E[Y_1^1(R^1) - Y_1^0(R^0)]$. Since subjects have been randomized into treatments, this can be calculated from the difference in mean observed willingness to give across treatment, $E[Y_1|C = 1] - E[Y_1|C = 0]$. What is important to note here is that the endogenous risk treatment CHOICE changes two things at the same time compared to the exogenous risk treatment RANDOM. Firstly, it changes the process by which subjects end up in the risky project, which may affect willingness to give and which corresponds to the behavioural effect we are interested in (Y^1 versus Y^0). Secondly, it changes who ends up in the risky project, i.e. it changes the distribution of projects from R^0 to R^1.

In the following we show that it is not possible to ensure by the experimental design that the average treatment effect corresponds to the average behavioural effects $\beta_{1,1}(R^0)$ or $\beta_{1,1}(R^1)$. To see this, note that the average treatment effect θ_1 can be decomposed as follows:

$$\begin{aligned}
\theta_1 \quad &\equiv \quad E[Y_1^1(0)|R^1 = 0]Pr(R^1 = 0) + E[Y_1^1(1)|R^1 = 1]Pr(R^1 = 1) \\
&\quad -[E[Y_1^0(0)|R^0 = 0]Pr(R^0 = 0) + E[Y_1^0(1)|R^0 = 1]Pr(R^0 = 1)]
\end{aligned}$$

Now assume that there are no behavioural effects, i.e. that $E[Y_{1,i}^1(R)] = E[Y_{1,i}^0(R)]$ for all i and $R \in \{0, 1\}$. If the average treatment effect corresponds to the average behavioural effect, then we must have $\theta_1 = 0$ in this case. This is not true, though. Instead, we are left with a remainder term

$$\begin{aligned}
\rho^C \quad &= \quad E[Y_1^C(0)|R^1 = 0]Pr(R^1 = 0) + E[Y_1^C(1)|R^1 = 1]Pr(R^1 = 1) \\
&\quad -[E[Y_1^C(0)|R^0 = 0]Pr(R^0 = 0) + E[Y_1^C(1)|R^0 = 1]Pr(R^0 = 1)]
\end{aligned}$$

for $C \in \{0,1\}$ with $\rho^0 = \rho^1 \equiv \rho$ if there are no behavioural effects. This is not equal to zero for two reasons. Firstly, the probability to be in the risky project differs across treatments, $Pr(R^0 = 1) \neq Pr(R^1 = 1)$. This can be avoided, though, by an experimental design that assigns the same share to the risky project in RANDOM as the share who self-selects into the risky project in CHOICE. However, this is neither necessary nor sufficient to ensure that $\rho = 0$. The reason is that, secondly, the subjects who self-select into project R in CHOICE systematically differ from the subjects randomly assigned to project R in RANDOM. As a result, $E[Y_1^C(R)|R^1 = R]$ averages over a different population than $E[Y_1^C(R)|R^0 = R]$. Thus, the nature of the treatment is such that we cannot ensure by the experimental design that $\rho = 0 \Leftrightarrow \theta_1 = \beta_{1,1}(R^0) = \beta_{1,1}(R^1)$. For the remainder term to be zero we would have to impose the assumption that subjects who are assigned to different projects in treatment $C \in \{0,1\}$ do not differ in their willingness to give, i.e. that $Y_{1,i}^C(1) = Y_{1,i}^C(0)$ for all i. This would be a very strong assumption, which can be tested in the data, though. If we find significant differences in giving behaviour across projects within RANDOM or CHOICE for some group of subjects, then the average treatment effect θ_1 is not informative about the behavioural effect of interest. In this case, differential willingness to give across projects within treatments biases the average treatment effect away from the behavioural effect of interest. Hence, we need to estimate the treatment effects conditional on projects, $\beta_{1,1}(\cdot)$, as described above to isolate the behavioural effect.

4.2 Identifying assumptions

To solve the identification problem we use three approaches that differ in the assumptions we impose. This allows us to assess the robustness of results with respect to the assumptions we impose. The first approach calculates nonparametric bounds for the unobserved parts in $E[Y_r^1(R)]$. They have the advantage that we do not impose any assumptions. Instead, we exploit that willingness to give is bounded between 0 and 1. This disadvantage of this approach is that we cannot point-identify the effects of interest and that the bounds may be relatively wide. To obtain the bounds we proceed as follows. We observe transfers of donors in project R in CHOICE, $Y_r^1(R)$, for subjects who self-select into project R but not for those who select into the other project. Assumption-free lower and upper bounds are obtained by setting $Y_{r,l}^1(R) = 0$ and respectively $Y_{r,u}^1(R) = 1$ for all subjects not in project R in CHOICE. For subjects with project R in CHOICE we use observed transfers and set $Y_{r,l}^1(R) = Y_{r,h}^1(R) = Y_r^1(R)$. With this approach we can estimate the, respectively, lower and upper bound on the conditional treatment effect $\beta_{r,r}(R)$ as

$$\beta_{r,r}^b(R) = E[Y_{r,b}^1(R)] - E[Y_r^0(R)] = E[Y_{r,b}^1(R)|C = 1] - E[Y_r|R = R, C = 0]. \tag{2}$$

for $r, R \in \{0,1\}$ and $b \in \{l,u\}$. For $\beta_{r,r}(R|R^1 = R)$ we cannot use this approach, though, because we need to condition on projects as under CHOICE, R^1, which is unknown for subjects in RANDOM.

The second approach imposes the assumption that we are able to observe and, hence, control for all factors X that jointly determine self-selection into projects and willingness to give, i.e. that the following unconfoundedness assumption holds:

$$(A1) : Y_r^1 \perp R | X = x, C = 1.$$

If this assumption is satisfied, then we can construct

$$E[Y_r^1(R)] = \int E[Y_r | R = R, X = x, C = 1] dF_{X|C=1}(x), \tag{3}$$

i.e. we can reweigh the observations with $R = R, X = x$ in CHOICE according to the distribution of characteristics X in the population, $F_X(x)$, which is equal to the distribution in CHOICE, $F_{X|C=1}(x)$, because treatments have been randomized. Similarly, we can construct

$$E[Y_r^0(R) | R^1 = R] = \int E[Y_r | R = R, X = x, C = 0] dF_{X|R=R,C=1}(x) \tag{4}$$

by reweighing the observations with $R = R, X = x$ in RANDOM according to the distribution of characteristics X in the group who self-selects into project R in CHOICE, $F_{X|R=R,C=1}(x)$.

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. We will discuss this in detail in Section 4.3.2. 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 project R:

$$(A2) : \; 0 < Pr(C = 1 | R = R, X = x) < 1, \quad R \in \{0, 1\}.$$

This means that 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. We implement the approach based on assumptions (A1) and (A2) using inverse probability weighting (IPW). First, we estimate the probability to be observed in CHOICE conditional on project R, $Pr(C = 1 | R = R)$, as a function of observed drivers of selection X based on a probit model. Second, we predict this probability for all subjects with $R = R$ using their observed characteristics and the estimated coefficients. Third, we construct weights based on the predicted probabilities that can be used for reweighing subjects to resemble the distribution of characteristics of interest (see Imbens and Wooldridge 2009 and Section 4.4 for details).

Since unconfoundedness is a strong assumption, we use a third approach that imposes weaker assumptions than approach 2 because it allows for certain types of unobserved drivers of selection into projects. Here, we exploit that in RANDOM, the subjects in project R who would also self-select into this project are a strict subset of those randomly assigned to project R. If one could identify those subjects, one would give those who would self-select into the assigned project weight 1 and those who would prefer the other project weight 0 to mimic the distribution of projects in CHOICE. For approach 3, we use this idea and impute counterfactual projects under CHOICE, R^1, for subjects in RANDOM based on their predicted probability to choose the risky project. For this, we estimate the probability to choose the risky project in CHOICE as a function of observed characteristics using a probit model. Next, we predict this probability for all subjects in RANDOM based on their observed characteristics and the estimated coefficients. Then, we assign a fraction φ of the subjects in RANDOM with the highest predicted probabilities counterfactual project $\hat{R}^1_i = 1$ and the remaining subjects $\hat{R}^1_i = 0$. The fraction φ is chosen to equal the observed share of subjects choosing the risky project in CHOICE, $\varphi = Pr(R = 1|C = 1) = Pr(R^1 = 1)$.

Approach 3 imposes the assumption that the predicted probability to choose the risky project places subjects in RANDOM in the same part of the distribution as the true distribution of choice probabilities. This allows, for example, for unobserved drivers of selection into the risky project that lead to a different spacing of choice probabilities but not to a different order. It even allows for a different order within the parts of the distribution that separate risk choosers from safety choosers but not across these parts. This mainly rules out unobserved drivers that are completely orthogonal to the observed drivers. With this approach we can estimate

$$\beta_{r,r}(R|R^1 = R) = E[Y^1_r(R) - Y^0_r(R)|R^1 = R] = E[Y_r|R = R, C = 1] - E[Y_r|\hat{R}^1 = R, C = 0] \quad (5)$$

for $r, R \in \{0, 1\}$ which serves as an input for $\beta_{r,r}(R^1)$. Approach 3 cannot be used to obtain $\beta_{r,r}(R)$, though.

4.3 Empirical implementation

4.3.1 Assessing imbalances across randomized samples

Approaches 1 and 3 require simple comparisons of mean outcomes across randomized samples if randomization was successful in creating comparable groups across treatments and across projects within the RANDOM treatment. Therefore, we check whether observed characteristics are balanced across these samples. In column a) of Table 3 we report 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. We will show below that our results are robust to including those covariates with imbalances. In column b) of Table 3 we present 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. Again, we will show below that our results are not driven by these imbalances by including all variables with imbalances as control variables.

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**
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

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)
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 marked as follows: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$; [b]in the past 3 months; [c]Inequality aversion 1 (disadv.): Dummy=1 if respondent thinks that others should not own much more than herself; Inequality aversion 2 (adv.): dto. ..not own much less.. (Section 2.3.2); [d]No. of GSS questions positively answered (Section 2.3.2); [e]Index bases on ownership of 11 household items (house, land, poultry, goats, sheep, cows/bullocks, refrigerator, radio, bicycle, motorcycle, car), weights generated by principal component analysis; [f]Expected likelihood of unaffordable HH health expenditures within next year; [g]No. of lottery the subject chose out of 8 different lotteries: 1=safe income to 8=riskiest lottery (Section 2.3.2); [h]Average number of trials needed to answer the comprehension test questions correctly.

4.3.2 Plausibility of unconfoundedness and common support

As discussed in Section 4.2, approach 2 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 (A1) 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 more details on 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.

4.4 Estimation

To estimate the behavioural effects of interest (the β's), we run regressions of the following type

$$Y_i = \alpha + \theta D_i + \gamma X_i + \varepsilon_i \tag{6}$$

where D_i is a dummy variable that indicates the groups we want to compare, which is CHOICE versus RANDOM when we test hypotheses 1 and 3, and partner in the risky versus the safe project when we test hypothesis 2. Thus, θ measures the behavioural effect of interest. In Table 4 we summarize the effects of interest and the corresponding definition of the group indicator D_i. This indicator is only defined for the groups used for the respective estimation, which is also indicated in Table 4.

Table 4: Summary of model specifications

Effect θ	Definition	Approach	$D_i = 0$	$D_i = 1$	Y_i
$\beta_{r,r}(R)$	$E[Y_r^1(R) - Y_r^0(R)]$	1	$C_i = 0, R_i = R, r_i = r$	$C_i = 1, r_i = r$	$Y_{i,b}$
		2	$C_i = 0, R_i = R, r_i = r$	$C_i = 1, R_i = R, r_i = r$	$\omega_i Y_i$
$\beta_{r,r}(R\|R^1 = R)$	$E[Y_r^1(R) - Y_r^0(R)\|R^1 = R]$	2	$C_i = 0, R_i = R, r_i = r$	$C_i = 1, R_i = R, r_i = r$	$\omega_i Y_i$
		3	$C_i = 0, \hat{R}_i^1 = R, r_i = r$	$C_i = 1, R_i = R, r_i = r$	Y_i
$\beta_{1,0}(1\|R^1 = 1)$	$E[Y_1^1(1) - Y_0^1(1)\|R^1 = 1]$	-	$C_i = 1, R_i = 1, r_i = 0$	$C_i = 1, R_i = 1, r_i = 1$	Y_i

Note: $b \in \{l, u\}$ for the lower and upper bound. R_i donor's project, r_i partner's project. ω_i inverse probability weight.

In Table 4, we also indicate the dependent variable we use, which depends on the approach we apply. For approach 1, we use the bounded outcome for the unobserved counterfactuals in CHOICE. For approach 2, we use the reweighed outcomes that account for selection into projects. We construct the weights based on the predicted probability to be observed in CHOICE conditional on project R, $p_R(x_i) \equiv Pr(C_i = 1|R_i = R, X_i = x_i)$. To obtain $\beta_{r,r}(R)$, we need to reweigh the subjects in CHOICE using the weight $\omega_i = \frac{1-p_R(x_i)}{p_R(x_i)}$ whereas the subjects in RANDOM receive $\omega_i = 1$. To obtain $\beta_{r,r}(R|R^1 = R)$, we need to reweigh the subjects in RANDOM using the weight $\omega_i = \frac{p_R(x_i)}{1-p_R(x_i)}$ whereas the subjects in CHOICE receive $\omega_i = 1$.[12] Approach 3 uses the observed outcomes but groups subjects in RANDOM according to their imputed project $\hat{R}_i^1$ under CHOICE where the imputation is based on the predicted probability to choose the risky project as described in Section 4.2. In Table A2 in Appendix B, we report the estimated probit models underlying the weights for approach 2 and the imputation for approach 3. As control variables we use all variables with significant differences in Table 3. Thus, they account for both imbalances across randomized samples, and self-selection into projects. To test hypothesis 2, we can use observed groups and outcomes as shown in Table 2.

Control variables X are not necessary when estimating (6) because they are either not required due to randomization of D, or because we run them after selection correction (i.e. on the reweighed data). However, to show robustness of our results with respect to small sample imbalances across randomized samples, we will run three different versions: one without covariates, one where we include all variables with small sample imbalances across treatments (age, income, Kibera, ethnicity Kamba, ethnicity Luhya, inequality 2, number of adults, number of dependents), and one where we additionally include all variables with small sample imbalances across projects within RANDOM (married, ethnicity Nubian, fairness, risk preference). Moreover, we estimate all of our results with and without enforcing common support (with respect to the variables main occupation farming, main occupation manufacturing/repairing of goods, residual ethnicity category), where the former excludes 12.7% of all observations. Inference is based on the wild bootstrap (Wu 1986) with null imposed, as recommended by Cameron, Gelbach, and Miller (2008) for estimates with clustered standard errors and few clusters. This accounts for the fact that randomization into treatments takes place on the session level. We use 999 boottrap replications.

5 Results

5.1 Descriptive evidence

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). We also report average willingness to give by partner's project in columns (a) and (b) as well as the difference between the two. Only some of the differences correspond to causal effects. This includes the overall comparison across treatments in line (7) due to randomization of treatments, the

[12] We exclude observations with extremely large weights > 10 to obtain more reliable results (see Huber, Lechner and Wunsch 2013). Descriptive statistics on the weights are reported in Table A3 in Appendix C.

comparison across projects within RANDOM in line (10) due to randomization of projects in RANDOM, and the within-subject comparisons across partners with different projects in the second last column.

Line (7) shows the average treatment effect which is negative but not statistically significant with a p-value of 17%. As shown in Section 4.1, this is not equal to the average behavioural effect on solidarity we need to estimate to test hypothesis 1 unless subjects in different projects do not show different willingness to give within treatments. Lines (10) and (11) in Table 5 show that there are no statistically significant differences in the average willingness to give across projects within treatments. However, this does not rule out that there are differences for subjects with certain characteristics. Therefore, we will test for heterogeneous responses more systematically in the next section.

Table 5: Proportion of subjects making positive transfers

		Project of partner					
		Safe		Risky		Difference	
Project of donor		(a)		(b)		(b)-(a)	
RANDOM		%	*N*	%	*N*	Diff.	P-value
(1)	All			45.2	*126*		
(2)	Safe+	-	-	40.7	*59*	-	-
(3)	Risky	35.8	*67*	49.3	*67*	13.4	0.12
CHOICE		%	*N*	%	*N*	Diff.	P-value
(4)	All			36.3	*102*		
(5)	Safe+	-	-	39.5	*81*	-	-
(6)	Risky	28.6	*21*	23.8	*21*	-4.7	0.73
Differences across treatments		Diff.	P-value	Diff.	P-value	Diff.	P-value
(7)	All: (4)-(1)			-9.0	0.17		
(8)	Safe: (5)-(2)	-	-	-1.2	0.90	-	-
(9)	Risky: (6)-(3)	-7.3	0.54	-25.4**	0.03	-18.2*	0.09
Differences within treatments		Diff.	P-value	Diff.	P-value	Diff.	P-value
(10)	RANDOM: (3)-(2)	-	-	8.6	0.34	-	-
(11)	CHOICE: (6)-(5)	-	-	-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.

Conditional on project, we find no reduction in willingness to give across treatments for subjects in the safe project in line (8) of Table 5. The difference is very small with -1.2 percentage points and not statistically significant with a high p-value of 90%. For subjects with the risky project, though, we find a large and statistically significant reduction in willingness to give of 25.4 percentage points with a p-value of 3%. This would suggest that hypothesis 1 is supported for subjects in the risky project but nor for those in the safe project. However, due to self-selection into projects in CHOICE, we cannot conclude that these findings represent causal effects due to selection bias.

In contrast, hypothesis 2 can be tested directly without further assumptions because we can make use of a within-subject comparison. Here, we need to compare willingness to give of subjects in the risky project in CHOICE across partners with different projects. This is given in line (6) of Table 5. Willingness to give to partners who have self-selected into the risky project is 4.7 percentage points lower compared to partners choosing the safe project. This difference is not statistically significant, though, with a p-value of 73%. Hence, attributions of responsibility do not seem to matter for solidarity. However, in the

RANDOM treatment, we see that in the absence of choice, willingness to give is 13.2 percentage points higher for partners in the risky than in the safe project with a p-value of 12% just short of statistical significance on the 10% level. This may be due to different payoff differences as partners in the safe project earn 400 KSh while those in the risky project earn 0 compared to the 800 KSh earned by the donors in the risky project. If we take this pre-existing difference into account in a difference-in-differences calculation, then we obtain a negative effect on willingness to give due to attributions of responsibility of 18.2 percentage points that is statistically significant with a p-value of 9% (see last two columns in line (9)). However, this estimate is again affected by selection bias because we compare effects conditional on project across treatments in this case. Therefore, we will also employ approaches 2 and 3 to assess how pre-existing differences affect our estimate of the effect of responsibility for neediness on solidarity.

With respect to hypothesis 3, we find no statistically significant effect of CHOICE on transfers to partners with the safe project in line (9) and column (a) of Table 5. The difference is -7.3 percentage points with a p-value of 54%. This suggests that the reduction in willingness to give to partners with the risky projects we observe in the same line in column (b), indeed seems to be driven by attributions of responsibility. However, before we can draw final conclusions we need to account account for selection into projects in CHOICE.

5.2 Econometric analyses

5.2.1 Testing for heterogeneous willingness to give across projects

We could test hypothesis 1 without having to worry about self-selection into projects in CHOICE when the average treatment effect, $\theta_1 = E[Y_1^1(R^1) - Y_1^0(R^0)]$, is equal to the average behavioural effects $\beta_{1,1}(R^0) = E[Y_1^1(R^0) - Y_1^0(R^0)]$ and $\beta_{1,1}(R^1) = E[Y_1^1(R^1) - Y_1^0(R^1)]$. For this, we need to rule out that subjects show different willingness to give when assigned to different projects within treatment. We cannot test this without imposing assumptions for the CHOICE treatment due to self-selection into projects. However, in RANDOM assignment to projects is random such that we can compare willingness to give across projects free of selection bias.

In Table A4 in Appendix D we present the results from a regression of willingness to give on a dummy for the risky project in the RANDOM sample without additional control variables and with all covariates with small sample imbalances in Table 3. Additionally, we individually (i.e. one at a time) add all other observed variables together with an interaction term with the dummy for the risky project and test whether this interaction term is statistically significant. If so, this would provide evidence for heterogeneous responses across projects. In Table A4 in Appendix D we present the coefficients and p-values for these interaction terms. We run all regressions both on the full RANDOM sample, and the subsample that satisfies common support.

We find interaction terms that are statistically significant on the 10% level for 8 out of 40 variables (20%) in the full sample and for 9 out of 40 variables (22.5%) in the common support sample. Moreover, in both samples, there are 4 additional variables with p-values just above 10%. This leads to the conclusion that we must reject equality between the average treatment effect and the average behavioural effects as differential willingness to give across projects within RANDOM biases the average treatment effect away from the behavioural effect of interest. Therefore, we need to estimate the behavioural effects directly which requires accounting for selection into projects in CHOICE.

5.2.2 Effect of CHOICE on the willingness to give

In Table 6 we present the estimation results for the behavioural effects $\beta_{1,1}(\cdot)$ that we use to test hypothesis 1. Before we discuss substantive results, we comment on the robustness of the results with respect to imbalances across randomized samples and enforcing common support. Qualitatively, all results remain unchanged when we add covariates or exclude observations due to lack of support. Quantitatively, somewhat larger differences occur when we add covariates in columns (2) and (3) in the following two cases: when the sample is rather small, as for the group that either self-selects or is predicted to self-select into the risky project under CHOICE, and for some of the IPW estimates. For the latter, it is important to note that adding covariates is not necessary because the inverse probability weights already take imbalances across the randomized samples into account. Results with covariates added when estimating (6) are only reported for completeness in case of IPW. In the majority of cases where coefficients are similar, estimates often become more noisy when we add covariates. The effects estimated for the common support sample are in all cases quantitatively very similar to those for the full sample but the p-values are lower indicating that they are estimated with higher precision. In the following, we therefore focus on discussing the results for the common support sample in the lower part of Table 6 and for the case without covariates added in column (1).

The results reported in Table 6 confirm the descriptive evidence from Table 5. Willingness to give to partners in the risky project does not differ by treatment for donors in the safe project. The point estimates that correct for selection into the risky project in CHOICE are mostly negative but small and not statistically significantly different from zero on any conventional level. The non-parametric bounds, which do not impose any assumptions, are relatively narrow, symmetric around zero, and they rule out non-zero behavioural effects for holders of the safe project, $\beta_{1,1}(0)$. Thus, we can conclude that willingness to give is unaffected for holders of the safe project and that this is result is not driven by selection bias. In contrast, willingness to give to partners in the risky project is significantly lower in CHOICE than in RANDOM for holders of the risky project. The average behavioural effect $\beta_{1,1}(1)$ that is estimated using IPW is -32.1 percentage points with a p-value of 9%. The non-parametric bounds are relatively wide and they neither rule out negative nor positive effects. However, they show a strong asymmetry of about 20 percentage points towards a negative effect, which supports our findings based on IPW. The conditional behavioural effect for those who self-select into the risky project, $\beta_{1,1}(1|R^1 = 1)$, is somewhat smaller in the baseline regression in column (1) than the average effect but similar in most other specifications including the specifications that use imputed projects R^1 which shows that our IPW results are robust to certain forms of unobserved heterogeneity. However, the effect only reaches statistical significance on the 5% level when we use imputed projects R^1 and control for small sample imbalances in column (2). The lack of precision can be explained by the small number of subjects who self-select into the risky project which leads to imprecise estimates.

Table 6: Results for $\beta_{1,1}(\cdot)$ to test hypothesis 1

Effect	Donor's project	Partner's project	Method	N	No covariates		Imbalances treatments		Imbalances RANDOM	
					Coeff.	*Pval.*	Coeff.	*Pval.*	Coeff.	*Pval.*
Full sample					(1)		(2)		(3)	
$\beta_{1,1}(0)$	SAFE	RISKY	Lower bound	161	-.093	.390	-.075	.484	-.097	.300
	SAFE	RISKY	Upper bound	161	.113	.381	.121	.339	.125	.296
	SAFE	RISKY	IPW	140	-.067	.534	-.039	.714	-.006	.956
$\beta_{1,1}(0\|R^1 = 0)$	SAFE	RISKY	IPW	140	-.002	.983	-.019	.867	-.051	.647
	SAFE	RISKY	Imputed R^1	127	.004	.974	.007	.945	-.005	.964
$\beta_{1,1}(1)$	RISKY	RISKY	Lower bound	169	-.444***	.001	-.462***	.000	-.462***	.001
	RISKY	RISKY	Upper bound	169	.351***	.006	.343***	.006	.361***	.006
	RISKY	RISKY	IPW	88	-.279$^+$	.129	-.315$^+$	.117	-.241	.245
$\beta_{1,1}(1\|R^1 = 1)$	RISKY	RISKY	IPW	88	-.169	.411	-.226	.829	-.241	.894
	RISKY	RISKY	Imputed R^1	34	-.147	.393	-.314*	.077	-.344	.230
$\beta_{1,1}(R^0)$	ALL	RISKY	IPW		-.173*	.100	-.177$^+$	.117	-.124	.286
$\beta_{1,1}(R^1)$	ALL	RISKY	IPW		-.029	.759	-.052	.763	-.080	.804
Common support					(1)		(2)		(3)	
$\beta_{1,1}(0)$	SAFE	RISKY	Lower bound	139	-.120	.308	-.090	.425	-.106	.271
	SAFE	RISKY	Upper bound	139	.121	.344	.130	.330	.133	.291
	SAFE	RISKY	IPW	118	-.080	.474	-.042	.698	.002	.988
$\beta_{1,1}(0\|R^1 = 0)$	SAFE	RISKY	IPW	118	-.032	.786	-.050	.667	-.083	.461
	SAFE	RISKY	Imputed R^1	107	-.017	.872	-.008	.958	-.024	.828
$\beta_{1,1}(1)$	RISKY	RISKY	Lower bound	145	-.477***	.000	-.504***	.001	-.508***	.001
	RISKY	RISKY	Upper bound	145	.282**	.030	.287**	.022	.309***	.009
	RISKY	RISKY	IPW	79	-.321*	.090	-.342$^+$	.108	-.290	.192
$\beta_{1,1}(1\|R^1 = 1)$	RISKY	RISKY	IPW	79	-.249	.261	-.295	.779	-.332	.831
	RISKY	RISKY	Imputed R^1	32	-.216	.278	-.362**	.035	-.416$^+$	.137
$\beta_{1,1}(R^0)$	ALL	RISKY	IPW		-.201*	.067	-.192$^+$	.109	-.144	.245
$\beta_{1,1}(R^1)$	ALL	RISKY	IPW		-.066	.516	-.088	.616	-.120	.666

Note: ***/**/*/$^+$ indicates significance on the 1/5/10/15% level.

A possible explanation for the different effects we find for subjects in the safe and the risky project is provided by differences in willingness to give observed within RANDOM. In the absence of CHOICE, holders of the risky project are more generous to partners who receive nothing than holders of the safe project (see line (10) in Table 5), which may be explained by different payoffs (800 versus 400 KSh). They also give considerably more than to partners with the safe project (see line (3) in Table 5), which may also be explained by different payoffs (0 versus 400 KSh). The possibility of free project choice removes these differences in generosity. In CHOICE, holders of the risky project no longer make a difference between partners with the safe or the risky project (see line (6) in Table 5) and they no longer give more than holders of the safe project (see line (11) in Table 5).

In the last two lines of Table 6, we present the results for the aggregated behavioural effects $\beta_{1,1}(R^0)$ and $\beta_{1,1}(R^1)$. Since both the non-parametric bounds (approach 1), and the results from imputing projects R^1 (approach 3) support our findings from IPW (approach 2), we have calculated them based on IPW. When we use the distribution of projects as in RANDOM, R^0, we obtain a statistically significant reduction in willingness to give of 20.1 percentage points with a p-value of 6.7%. If we use the distribution of projects as in CHOICE, R^1, instead, the effect is still negative but small with 6.6 percentage points and not statistically significant with a p-value of 55%. This difference can be explained by two things. Firstly, only holders of the risky project respond to the treatment and this share is much smaller in CHOICE ($Pr(R^1 = 1) = 0.206$) than in RANDOM ($Pr(R^0 = 1) = 0.5$). Secondly, the effect for holders of the risky project is somewhat larger for the average population with $R^0 = 1$ than for those who self-select into the risky project with $R^1 = 1$. The fact that the two effects differ also shows that the average treatment effect, θ_1 is not equal to the aggregated behavioural effects. This is because we must have $\theta_1 = \beta_{1,1}(R^0) = \beta_{1,1}(R^1)$ if the average treatment effects corresponds to the behavioural effect. Moreover, the aggregated behavioural effects hide important effect heterogeneity which shows the importance of looking at the conditionaly behavioral effects $\beta_{1,1}(r)$ for each project $r \in \{0, 1\}$.

5.2.3 Isolating the effect of responsibility for neediness

Next, we test whether attributions of responsibility for neediness drives reduced willingness to give of holders of the risky project. In Table 7 we report the results of the within-subject comparisons of willingness to give to partners in the risky versus the safe project, $\beta_{1,0}(1|R^1 = 1)$, within the CHOICE treatment in line (1) and within the RANDOM treatment in lines (2) and (3). The latter two test for any pre-existing differences that we may have to subtract from the estimate in CHOICE to obtain the correct estimate of the effect of interest. They have been estimated based on IPW and imputed project R^1, respectively, in order to resemble the population that would self-select into the risky project.

Table 7: Results for $\beta_{1,0}(1|R^1 = 1)$ to test hypothesis 2

			Full sample			*Common support*		
		Method	N	Coeff.	*Pval.*	N	Coeff.	*Pval.*
(1)	CHOICE	OLS	42	-.048	.589	42	-.048	.589
(2)	RANDOM	IPW	134	.157	.360	116	.187	.357
(3)	RANDOM	Imputed R^1	26	.231	.651	22	.280	.656
(4)	Difference (1)-(2)	IPW		-.205	.274		-.235	.277
(5)	Difference (1)-(3)	Imputed R^1		-.278	.293		-.328	.295

Within CHOICE, willingness to give to partners choosing the risky project is only 4.8 percentage points lower than to partners choosing the safe project, and this difference is not statistically significant with a p-value of 59%. However, pre-existing differences within RANDOM are relatively large with about 20 percentage points although they do not reach statistical significance due to small numbers of observations. When we take the difference between the difference in CHOICE and in RANDOM to correct for pre-existing differences, we obtain effects that are similar to the conditional behavioural effect $\beta_{1,1}(1|R^1 = 1)$. This suggests that the reduction in willingness to give we observe is mainly driven by attributions of responsibility for neediness.

This is confirmed by the results in Table 8. When comparing willingness to give to partners with the safe project across treatments, we find no statistically significant effects as hypothesized. The conditional effects $\beta_{0,0}(1|R^1 = 1)$, which are the ones that are directly comparable to the results in Table 7, are small and have high p-values. The average effects $\beta_{0,0}(1)$ are considerably larger with about minus 20 percentage points but not statistically significant with p-values between 21-28%. This suggests that there might be some individuals who adjust their solidarity norm also for other reasons which may also explain why the average effects are more pronounced than the conditional effects. However, the data do not allow us to explore this further.

Table 8: Results for $\beta_{0,0}(\cdot)$ to test hypothesis 3

Effect	Donor's project	Partner's project	Method	N	No covariates		Imbalances treatments		Imbalances RANDOM	
					Coeff.	*Pval.*	Coeff.	*Pval.*	Coeff.	*Pval.*
Full sample					(1)		(2)		(3)	
$\beta_{0,0}(1)$	RISKY	SAFE	Lower bound	169	-.309***	.000	-.330***	.002	-.339***	.001
	RISKY	SAFE	Upper bound	169	.485***	.000	.475***	.000	.484***	.000
	RISKY	SAFE	IPW	88	-.213	.276	-.184	.348	-.190	.358
$\beta_{0,0}(1\|R^1 = 1)$	RISKY	SAFE	IPW	88	.036	.860	.037	.972	-.024	.989
	RISKY	SAFE	Imputed R^1	34	-.022	.886	-.055	.779	.021	.948
Common support					(1)		(2)		(3)	
$\beta_{0,0}(1)$	RISKY	SAFE	Lower bound	145	-.339***	.001	-.371***	.000	-.384***	.000
	RISKY	SAFE	Upper bound	145	.420***	.000	.419***	.002	.434***	.002
	RISKY	SAFE	IPW	79	-.252	.210	-.221	.259	-.220	.278
$\beta_{0,0}(1\|R^1 = 1)$	RISKY	SAFE	IPW	79	-.014	.948	-.044	.967	-.128	.935
	RISKY	SAFE	Imputed R^1	32	-.078	.684	-.099	.636	.000	1.000

Note: ***/**/*/$^+$ indicates significance on the 1/5/10/15% level.

6 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 only a subgroup of subjects reduces willingness to give when risk exposure is a choice. Responses are limited to donors in the risky project. In contrast, donors in the safe project do not adjust their willingness to give. Within-subject and within-treatment comparisons suggest that this difference in behaviour can be explained by differences in the RANDOM treatment. Lucky winners with the risky project show a particularly high degree of solidarity with unlucky losers compared to donors and partners assigned to the safe project when they 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. The possibility of free project choice removes these differences generosity. In CHOICE, holders of the risky project no longer make a difference between partners with the safe or the risky project and they no longer give more than holders of the safe project. Our results also show that attributions of responsibility for neediness seem to drive the reduction in willingness to give we observe for holders of the risky project as we find no effect of CHOICE for transfers to partners with the safe project.

The difference in behaviour of holders of the risky and the safe project is an important finding with respect to the literature on the crowding out of informal insurance by the availability of formal insurance. In this literature, everybody is exposed to risk in the baseline state by construction. Hence, we expect larger effects on solidarity compared to more general situations, where only a subset of individuals is exposed to risk in the baseline state. Our findings show that if the share of individuals opting into risk is relatively small, then the overall effect on solidarity is also small and can even become negligible. This is important for policies that aim to encourage investments into new but risky technologies or new business opportunities to reduce poverty and foster economic growth in developing countries. Our results suggest that crowding out of informal support might be less severe than suggested by the studies from Western countries and the evidence on formal insurance from developing countries.

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A Appendix: Risk preference game

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

B Appendix: Probit estimation results

Table A2: Probit estimation results for approaches 2 and 3

	(1)	(2)	(3)
Project	ALL	SAFE	RISKY
Treatment	CHOICE	ALL	ALL
Dependent variable	$R_i = 1$	$C_i = 1$	$C_i = 1$
Approach	3	2	2
Age	-.0126	-.0119	-.0116
	(.0219)	(.0157)	(.0287)
Monthly income	3.54e-05	1.81e-05	8.99e-05**
	(2.40e-05)	(2.46e-05)	(3.92e-05)
Kibera	.385	-.739**	-.131
	(.363)	(.292)	(.420)
Ethnicity: Kamba	.404	.715*	-.000422
	(.444)	(.433)	(.519)
Ethnicity: Luhya	-.0460	-.346	-.495
	(.505)	(.327)	(.494)
Ethnicity: Nubian	-.462	-.642	.710
	(.943)	(.542)	(1.812)
Inequality aversion 2	.575	.348	.208
	(.355)	(.269)	(.431)
No. of adults	-.00606	.309***	.224*
	(.0773)	(.0893)	(.124)
No. of dependents	-.0178	.0762	-.00617
	(.0693)	(.0570)	(.0935)
Married	.118	-.380	.408
	(.403)	(.295)	(.488)
Fairness	.280	-.253	-.0686
	(.439)	(.255)	(.479)
Risk preference	.270***	-.124**	.261***
	(.0722)	(.0497)	(.0832)
No. of other earners	-.130	-.184	-.528*
	(.165)	(.117)	(.274)
Constant	-2.175***	.426	-2.207***
	(.774)	(.501)	(.854)
Observations	102	140	88
Note:	Standard errors in parentheses.		
	*** p<.01, ** p<.05, * p<.1		

C Appendix: Inverse probability weights

Table A3: Distribution of inverse probability weights

Project	SAFE		RISKY	
Effect	$\beta_{1,1}(0)$	$\beta_{1,1}(0\|R^1 = 0)$	$\beta_{1,1}(1)$	$\beta_{1,1}(1\|R^1 = 1)$
Weight ω_i	$\frac{1-p_0(x_i)}{p_0(x_i)}$	$\frac{p_0(x_i)}{1-p_0(x_i)}$	$\frac{1-p_1(x_i)}{p_1(x_i)}$	$\frac{p_1(x_i)}{1-p_1(x_i)}$
Minimum	0.000	0.025	0.000	0.001
10% quantile	0.028	0.143	0.040	0.007
50% quantile	0.409	0.927	0.714	0.079
90% quantile	1.483	2.271	5.044	0.791
Maximum	4.746	3.485	8.697	2.955
Mean	0.652	1.091	1.689	0.265
Share excluded ($w > 10$)	0.012		0.048	
Observations	81	59	21	67

D Appendix: Differences in willingness to give across projects

Table A4: Effect of project on willingness to give within RANDOM

	Full sample		Common support	
	Coeff.	*Pval.*	Support	*Pval.*
Age	.010	.32	.017*	.10
Male	-.066	.72	-.039	.84
Schooling	-.061	.31	-.091$^+$	.13
Married	.195	.30	.151	.45
Head of household	.019	.91	.086	.65
Monthly income	.000	.51	.000	.26
Kibera	-.091	.61	-.186	.32
Christian	.090	.78	.343	.31
Ethnicity: Kamba	-.138	.70	-.267	.45
Ethnicity: Kikuyu	.269	.17	.326$^+$	.12
Ethnicity: Kisii	.397$^+$	.14	.346	.21
Ethnicity: Luhya	-.327$^+$	.12	-.445**	.05
Ethnicity: Luo	.089	.74	.021	.94
Ethnicity: Nubian	-.935*	.09	-1.004*	.06
Ethnicity: Other	-1.187**	.04		
Health problem	-.115	.53	-.060	.76
Chronical health problem	-.451*	.10	-.413	.16
Health care	-.186	.30	-.089	.64
Health expenditures	.000	.92	.000	.78
Health insurance	.144	.51	.297	.19
Other insurance	.544*	.07	.485$^+$	.11
Inequality aversion 1	.279	.17	.451**	.04
Inequality aversion 2	.024	.91	-.015	.95
Fairness	.148	.45	.177	.39
Trust	-.061	.80	-.081	.75
Helpfulness	-.355*	.08	-.373*	.09
GSS index	-.052	.63	-.063	.59
No. of adults	.002	.96	-.030	.60
No. of children	.044	.41	.006	.92
Household income (p.c.)	.000$^+$	.13	.000***	.00
No. of other earner	.167*	.08	.154$^+$	.12
No of dependents	-.004	.91	.000	1.00
Wealth index score	.042	.39	.047	.34
Household health expend.(p.c)	.000$^+$	.13	.000	.18
Expected health shock	-.069**	.02	-.083***	.01
Foregone health care	-.302*	.10	-.422**	.03
Household health insurance (prop.)	.226	.19	.353*	.06
Risk perference	-.012	.73	.001	.99
Risk aversion	.082	.71	.093	.69
Understanding instructions	.221	.75	.413	.58

Note: Coefficient on interaction term of R_i with respective variable.
***/**/*/$^+$ indicates significance on the 1/5/10/15% level.

E 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.

Project Income:

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

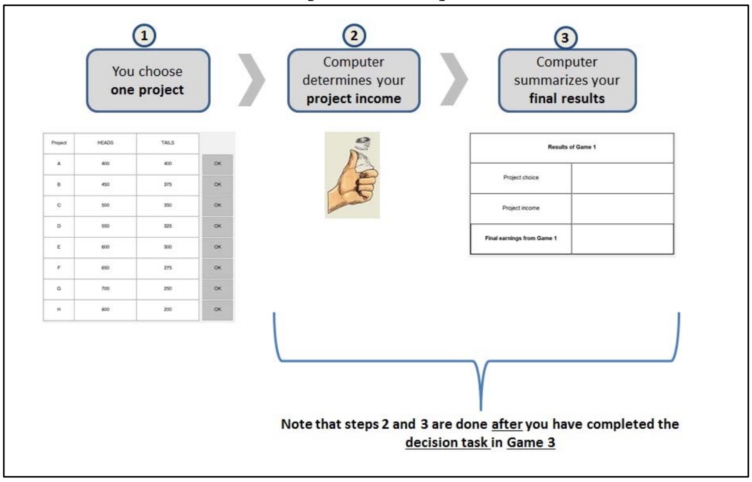
Project	HEADS	TAILS	
K	320	320	OK
L	400	280	OK
M	480	240	OK
N	560	200	OK
O	640	160	OK
P	720	120	OK
Q	800	80	OK
R	880	0	OK

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.

Summary:

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]

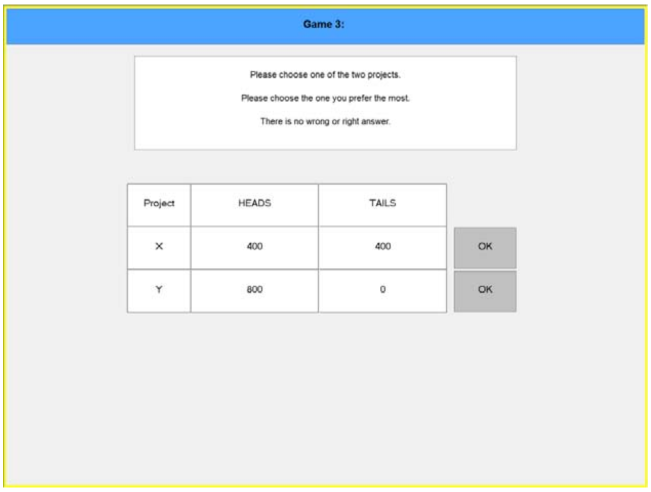


Instructions for Game 3

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]



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.

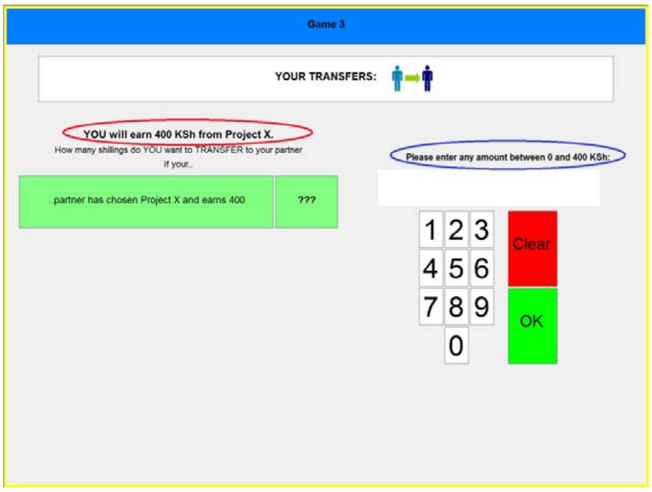
3) Transfers

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]



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.

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Summary:

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

[Screenshot 7]