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IZA DP No. 11641

Risky Choices and Solidarity: Why Experimental Design Matters

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Conny Wunsch
University of Basel, CEPR, CESifo and IZA

Renate Strobl
University of Basel

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Risky Choices and Solidarity: Why Experimental Design Matters*

Negative income shocks can either be the consequence of risky choices or random events. A growing literature analyzes the role of responsibility for neediness for informal financial support of individuals facing negative income shocks based on randomized experiments. In this paper, we show that studying this question involves a number of challenges that existing studies either have not been aware of, or have been unable to address satisfactorily. We show that the average effect of free choice of risk on sharing, i.e. the comparison of mean sharing across randomized treatments, is not informative about the behavioural effects and that it is not possible to ensure by the experimental design that the average treatment effect equals the behavioural effect. Instead, isolating the behavioural effect requires conditioning on risk exposure. We show that a design that measures subjects preferred level of risk in all treatments allows isolating this effect without additional assumptions. Another advantage of our design is that it allows disentangling changes in giving behaviour due to attributions of responsibility for neediness from other explanations. We implement our design in a lab experiment we conducted with slum dwellers in Nairobi that measures subjects' transfers to 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 free choice matters for giving and that the effects depend on donors' risk preferences but that attributions of responsibility play a negligible role in this context.

JEL Classification: C91, D63, D81, O12

Keywords: solidarity, risk taking, experimental design

Corresponding author:
Conny Wunsch
Faculty of Business and Economics
University of Basel
Peter-Merian-Weg 6
CH-4002 Basel
Switzerland

E-mail: conny.wunsch@unibas.ch

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1 Introduction

Negative income shocks can either be the consequence of risky choices (e.g. investments), or completely random events (e.g. accidents which affect work capacity). A growing literature analyzes the role of responsibility for neediness for informal financial support of individuals facing negative income shocks based on lab experiments (Trhal and Radermacher 2009, Lin et al. 2014, Cettolin and Tausch 2015, Akbas et al. 2016, Strobl and Wunsch 2018) or lab-in-the-field experiments (Landmann et al. 2012, Lenel and Steiner 2017, Morsink 2017). These experiments study redistribution between matched subjects with different incomes in two situations. In an *exogenous risk treatment*, subjects have no control over their level of risk exposure; they are exposed to risk for exogenous reasons. In an *endogenous risk treatment*, subjects have some or full control over their level of risk exposure, e.g. by buying insurance or by choosing between different options that involve different levels of risk.

The experiments differ in who makes the redistribution decision. In Akbas et al. (2016), Lenel and Steiner (2017), and Morsink (2017), a subgroup of subjects who are not exposed to the endogenous or exogenous risk treatment themselves is randomly assigned the role of redistributor. Cappelen et al. (2013) and Akbas et al. (2016) call these subjects spectators or observers. These studies are mainly interested in the question whether social norms or fairness views depend on the extent to which the beneficiaries of redistribution can control their risk exposure, and redistributors' decisions are used to measure these norms. In contrast, in the majority of studies,[1] the subjects that are randomly assigned to the endogenous or exogenous risk treatment themselves make the redistribution decision. Cappelen et al. (2013) and Akbas et al. (2016) call these subjects the stakeholders. These studies are directly interested in the sharing behaviour of stakeholders as they are the ones who redistribute income in reality. Cappelen et al. (2013) and Akbas et al. (2016) show that it matters whether observers or stakeholders make redistribution decisions. They find that observers favour more equal distributions than stakeholders when confronted with the same situation. Akbas et al. (2016) additionally find in their study that stakeholders are much less prone to punish self-selection into risky situations by giving less than observers.

In this paper, we reconsider the question whether responsibility for neediness matters for informal support of *stakeholders*. We show that answering this question involves a number of challenges that existing studies either have not been aware of, or have been unable to address without imposing strong assumptions. When stakeholders decide about sharing their income, then it matters which level of risk they face themselves because it affects their utility as well as their income relative to the income of the potential beneficiaries of redistribution. The key challenge is that the nature of the treatment is such that it affects the level of risk faced by stakeholders at the same time as it changes the process by which the beneficiaries of redistribution become needy (by making risky choices or as the result of

[1] Trhal and Radermacher (2009), Landmann et al. (2012), Lin et al. (2014), Cettolin and Tausch (2015), Strobl and Wunsch (2018). Akbas et al. (2016) also measure stakeholders' redistribution decisions but focus on spectators' decisions in the analysis.

exogenous income shocks). Both may affect sharing behaviour of stakeholders. But only the latter is the behavioural effect researchers are typically interested in, while the former is what we call a mechanical effect that cannot be avoided due to the nature of the treatment. Building on first ideas we developed in this direction in Strobl and Wunsch (2018), we show that in stakeholder designs, the average effect of free choice of risk on sharing, i.e. the comparison of mean sharing across randomized treatments, which is what previous studies report, does not measure the behavioural effect of interest. Most importantly, we show that it is not possible to ensure by the experimental design that the average treatment effect equals the behavioural effect. Instead, isolating the behavioural effect requires conditioning on stakeholders' risk exposure. This, however, requires accounting for self-selection of risk exposure under endogenous risk. Strobl and Wunsch (2018) impose relatively strong assumptions to point-identify the behavioural effect in this case. In contrast, we show that a design that measures subjects preferred level of risk in all treatments allows isolating this effect without additional assumptions.[2]

We conducted a laboratory experiment with slum dwellers in Nairobi, Kenya. In a between-subject design with two randomized treatments, each participant could either choose (treatment CHOICE) or was randomly assigned (treatment RANDOM) to a safe or a risky project. The risky project involved a one-half probability to end up with a zero payoff. After being randomly matched with another person, subjects could make voluntary transfers to their partner. Using the strategy method, we elicit transfers for all possible choices and situations of the partner independent of the realized states. By construction, all subjects in the CHOICE treatment receive their preferred option. In the RANDOM treatment, some subjects end up in projects they would not have chosen for themselves. The key innovation of our design is that we elicit preferred projects for all subjects in RANDOM. This allows us to compare giving behaviour of stakeholders who hold and prefer the same project across treatments. I.e., we can compare transfers of stakeholders who self-select into a given project in CHOICE with that of stakeholders who have been randomly assigned to the same project in RANDOM and who would have chosen this project also had they had the choice. Comparing subjects with the same actual and preferred project across treatments allows us to identify the behavioural effect of interest without additional assumptions.

Our experimental design not only allows us to identify the behavioural effect of interest free of mechanical effects, but also to assess whether mechanical effects matter. They occur if giving differs by stakeholders' project because CHOICE leads to a different distribution of projects than RANDOM. We find that such effects are important in our data and that they bias the average treatment effect upwards, which, therefore, underestimates the absolute value of the negative effect of CHOICE on giving. We also provide evidence for two potential causes of mechanical effects. Firstly, we show that stakeholders who are assigned to an unwanted project give less to their partner than those for whom assigned and preferred project coincide. This suggests that utility losses due to assignment to unwanted projects in RANDOM

[2] A similar idea has been used in other contexts, for example by Karlan and Zinman (2009), and by Dal Bó, Foster and Putterman (2010).

matters for giving. Secondly, payoff differences between the safe and the risky project in connection with inequality aversion seems to induce differential giving behaviour across projects within RANDOM.

Another advantage of our design that distinguishes us from existing studies is that it allows disentangling changes in giving behaviour due to attributions of responsibility for neediness from other explanations. 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 transfers. One the one hand, our design allows us to compare transfers to partners in the safe project across treatments free of mechanical effects to test for no behavioural effect. On the other hand, we can follow Cettolin and Tausch (2015) and Strobl and Wunsch (2018) and use a within-subject comparison of transfers to a partner choosing the risky project with the transfers to a partner choosing the safe option in CHOICE. However, in contrast to these studies we can account for the fact that the payoff difference between donor's and partner's project is not the same for partners in the safe and in the risky project without having to impose assumptions. We test whether payoff differences matter for giving by conducting the same within-subject comparison in the RANDOM treatment and use the estimate in RANDOM to correct the estimate in CHOICE for the effect of payoff differences to isolate the effect of responsibility for neediness.

With our design we find that free choice of risk exposure matters for giving but that attributions of responsibility play a negligible role in this context. We also find that the effects on giving depend on donors' risk preferences. Under free choice, risk takers give less compared to random exposure to risk when their risky choices succeed independent of the choices of transfer beneficiaries. In return, however, they also expect less support when their risky choices fail. Thus, their solidarity norm under free choice of risk seems to differ from the norm applied under random risk. In contrast, safety choosers expect and provide a certain level of support independent of the situation that has put them on either the giving or receiving end. It seems that the strong norm of mutual support in developing countries works against strong punishment of risky choices and makes risk choosers aware of the burden their investment behavior may impose on others, which induces them to take responsibility for their actions. In this respect, our findings differs substantially both from the evidence for Western countries (Trhal and Radermacher 2009, Cettolin and Tausch 2015, Akbas et al. 2016), and the evidence on crowding out of informal insurance by the availability of formal insurance for developing countries (Landmann et al. 2012, Lin et al. 2014, Lenel and Steiner 2017, Morsink, 2017). Our results suggest that not only the social norm regarding solidarity in a society matters, but also the situation in which individuals make choices that involve risk.

The remainder of the paper is organized as follows. The next section describes the experiment we conducted in detail. In Section 3 we derive the hypotheses for the empirical analysis and show how we can test them. Here we explain in detail how our experimental design helps to overcome the drawbacks of previous studies. Section 4 describes the empirical implementation. In Section 5 we present and discuss results. The last section concludes. An appendix contains supplementary information.

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 up to 25 computer-supported workplaces. It maintains a subject pool with currently around 12,000 registered individuals, many of them recruited from the Nairobi informal settlement Kibera. The living situation in this slum community 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).

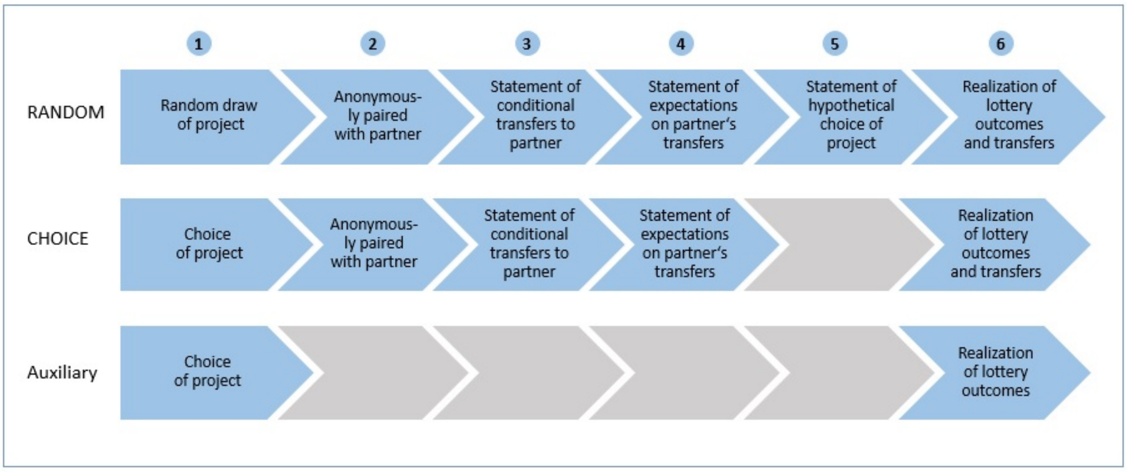
In Kenya, in general, there is a strong spirit of *harambee* (the Swahili term for 'pulling together') which encloses ideas of mutual support, self-help and cooperative effort. Harambee takes various forms, such as local fundraising activities to help persons in need or the joint implementation of community 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 (Ngau 1987, Mathauer et al. 2008, Jakiela and Ozier 2016). 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 of stakeholders 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 500 KSh and a risky alternative yielding either 1000 or 0 KSh with equal probability. Depending on the treatment, subjects could either choose (treatment CHOICE) or were randomly assigned (treatment RANDOM) to one of these two options (step 1). After having chosen one project or being informed about the randomly received option, each subject was randomly and anonymously paired with another person in the room, who followed the same experimental procedure and was hence in the same treatment condition than the subject herself (step 2).[3] Using the strategy method, all subjects were then asked how much money they wanted to transfer to their matched partner in case of winning the 'high' payoff of their option, which is 1000 KSh for subjects holding the risky option and 500 KSh for individuals with the safe income. Hence, before revealing their own realized payoff as well as their partner's project and earnings, participants stated their gift for the two possible payoffs of their partner (i.e. 500 or 0 KSh) (step 3). Subsequent to the transfer statements subjects were asked which monetary amount they expected to receive from their partner for the two cases where the subject herself earned the 'low' payoff and the partner the 'high' payoff (i.e. 500 KSh or 1000 KSh) (step 4).[4] In the RANDOM treatment, the next step consisted of eliciting subjects' preferred project. Here, we asked subjects which of the two projects they would have chosen given they had the possibility to choose (step 5).[5] Subjects did not know that this question would come up at the beginning of the game, which ensures that the transfers stated in step 3 of the game are unaffected by this question.[6] At the end of the session, lottery outcomes of all participants were determined and transfers between the partners effected according to the actually realizing states (step 6). The stakes of the game represented considerable amounts for the mainly very poor participants who reported an average daily income of 160 KSh ($\sim$ 1.60 USD).

Figure 1: Sequence of steps in the risk solidarity game



In order to address concerns about the way we elicit preferences regarding the safe and risky projects, we ran an auxiliary treatment (Auxiliary) with a third subject pool.[7] The sole task in this incentivized game was to choose between the safe and risky project, corresponding hence to the project choice task

[3]The subjects were informed about this step at the beginning of the game.

[4]These two expectation questions were not incentivized.

[5]The exact wording was as follows: "At the beginning of the game, the computer has randomly chosen a project for you. Given you had the possibility to choose yourself, which project would you have chosen?".

[6]An alternative approach, that has been used in other contexts (e.g. by Karlan and Zinman 2009, and Dal Bó, Foster and Putterman 2010), is to let subjects select their preferred option and then a randomization device determines whether choices are actually implemented. However, Long et al. (2010) and Marcus et al. (2012) show that denying subjects their preferred choice can affect behaviour (in our case stated transfers in RANDOM) and, thus, confound the effects we are interested in.

[7]In these sessions, the same two games (investment game and risk preference game) as in CHOICE and RANDOM were played before the auxiliary experiment.

in the CHOICE treatment (step 1). The participants played this game, however, in full autarky, i.e. they were not paired with another individual and transfers were not possible. The payoffs of this game corresponded to the safe amount or the realizing lottery outcomes, respectively. With the auxiliary experiment we address two concerns. Firstly, we can assess whether real monetary consequences matter for stated preferences by comparing the choices made in this experiment to the stated preference in the RANDOM treatment. Secondly, we address the issue that subjects' choices might be driven by the transfers they may have to make. If we find no differences in choices and stated preference between the main and the auxiliary experiment, then such strategic considerations do not matter.

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 equal 500 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. Trhal and Radermacher 2009, Bolle and Costard 2013, Cettolin and Tausch 2015). 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. Moreover, it allows us to distinguish subjects with distinct risk preferences (risk averse or not) without having to make assumptions about the underlying utility function.

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). It 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 risky choices on solidarity.

2.2.2 Procedures

For recruitment, subjects were randomly chosen from the Kibera subject pool registered at Busara and then invited by SMS. A precondition for being selected was an education level of at least primary school (8 years) to ensure some familiarity with numerical values as is necessary for our study. Using a between-subject design, the recruited persons were randomly assigned to one of the two treatments. The core experiment was run within 13 sessions in December 2017. Six sessions were conducted of the RANDOM treatment and seven of the CHOICE treatment. In total, 238 subjects participated in our study, thereof 120 in RANDOM and 118 in CHOICE. 33% of our subjects are male and 47% are married. On average, the participants are 31 years old and have a schooling level of 11 years. Table 1 gives an overview on selected basic characteristics of the participants by treatment and project. In addition, we ran 5 sessions of the auxiliary treatment in January 2018 where, in total, 111 subjects participated.

Table 1: Basic characteristics of participants by treatment and project

	All			Random			Choice		
	Random	Choice	Difference	Safe	Risky	Difference	Safe	Risky	Difference
	(1)	(2)	(2)-(1)	(3)	(4)	(4)-(3)	(5)	(6)	(6)-(5)
Age	3.5	31.4	.90	3.1	3.8	.70	31.2	32.1	.90
Male	.33	.35	.02	.30	.35	.05	.32	.48	.16
Schooling	11.5	11.2	-.30	11.2	11.9	.70	11.3	11.2	-.10
Married	.45	.48	.03	.43	.47	.03	.46	.57	.10
Occupational status									
Employed	.13	.14	.01	.15	.10	-.05	.13	.17	.05
Self-employed	.19	.27	.08	.15	.23	.08	.25	.35	.10
Unemployed	.50	.45	-.05	.50	.50	.00	.46	.39	-.07
Other	.18	.14	-.04	.20	.17	-.03	.16	.09	-.07
Social preferences									
Inequality aversion 1 (disadv.)	.18	.20	.03	.23	.12	-.12*	.19	.26	.07
Inequality aversion 2 (adv.)	.24	.32	.08	.30	.18	-.12	.31	.39	.09
Fairness	.32	.34	.02	.32	.32	.00	.35	.30	-.04
Trust	.13	.19	.07	.15	.10	-.05	.21	.13	-.08
Risk preference	3.42	3.59	.18	3.47	3.37	-.10	2.99	6.09	3.10***
Observations	*120*	*118*		*60*	*60*		*95*	*23*	

Note: */**/*** indicates significance on the 10/5/1% level.

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 C for an English version of the instructions, exemplarily for CHOICE).[8] 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

[8]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).

performed the actual experimental task. The experiment involved, firstly, a risk preference game which aimed at measuring subjects risk attitudes (see Appendix B for details) 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 which compensated mainly for the travel costs to the center. Moreover, subjects earned a minimum of 250 KSh in the experiment in order to guarantee an appropriate compensation for the time spent. However, participants were not informed about this minimum compensation before the end of the game. In total, average earnings amounted to 447 KSh per person. They were transferred cashless to the respondents' MPesa accounts.[9]

2.3 Outcomes of interest

The core game of our study measures stakeholders' giving behaviour under the CHOICE and RANDOM condition. 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 (500→0) and from lucky risky project holders to partners with safe or zero earnings (1000→500 and 500→0). The reason for this restriction is that we are interested in solidarity which is necessary for mutual aid arrangements to work, implying redistribution of income from better-off to worse-off subjects. We measure giving behaviour using six different variables. Table 2 provides summary statistics on these variables in the two treatments.

Table 2: Summary statistics of transfers to worse-off partners by treatment

		(1)	(2)	(3)	(4)	(5)	(6)
		KSh	Share	Share	Share	Share	Share
		T_i	τ_i	$\tau_i = 0$	$0 < \tau_i < .5$	$\tau_i = .5$	$\tau_i > .5$
	N	Mean	Mean	Fraction	Fraction	Fraction	Fraction
RANDOM	*180*	202.11	.313	.367	.306	.172	.155
CHOICE	*141*	157.24	.282	.383	.362	.121	.134
Difference		-44.87**	-.031	.016	.056	-.052	-.021

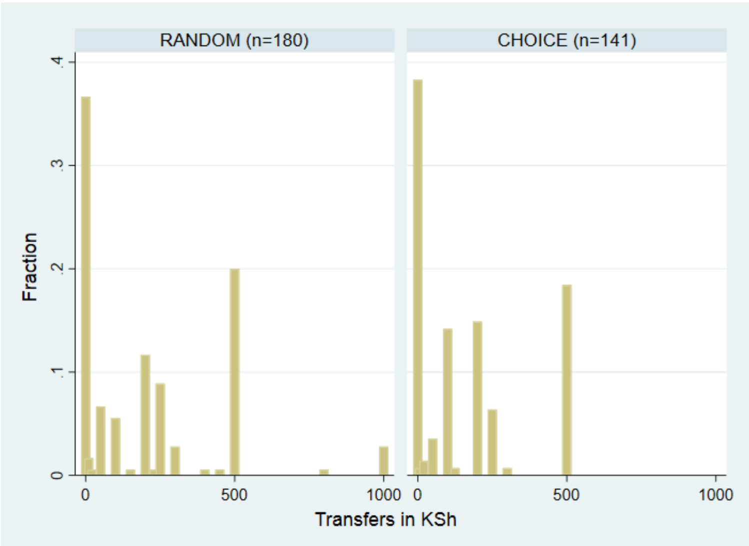
Note: Share of payoff difference given to worse-off partner: $\tau_i = T_i/(x_i - x_j)$, where T_i is the transfer to the partner, and x_i and x_j the payoffs of the donor and the partner, respectively. $\tau_i = .5$ corresponds to equal split.

The stated transfers are expressed, firstly, as shilling amounts (Table 2 column 1) and, secondly, as shares of the difference between donors' and partners' payoffs in order to relate the absolute transfer to the extent of income inequality (column 2). Figure 2 and 3 display the distributions of these two outcome

[9]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).

variables. Additionally, we distinguish four different types of giving using binary variables. The four dummies indicate whether the donor acts selfishly and gives nothing to the partner (column 3), whether she eliminates all income inequalities between herself and her partner by transferring 50% of the payoff difference (column 5), whether she reveals an intermediate behaviour with allocating any positive amount below the equal-split transfer (column 4), and the residual category with transfer shares of more than 50% (column 6).

Figure 2: Distribution of transfers to worse-off partners (in KSh) by treatment



We observe that, on average, donors transfer 202 KSh under RANDOM and 157 KSh under CHOICE. With their gifts, they offset around 31.3% and 28.2% of the payoff differences, respectively.[10] Figure 2 and Table 2 illustrate that a substantial part of the subjects decided to give nothing to their partner. On average, the cases of zero transfers account for nearly 40% in both treatments. 17.2% (12.1%) of the transfers in RANDOM (CHOICE) follow an equal sharing rule.[11] Overall, we find a slightly lower level of solidarity under CHOICE than under RANDOM in this simple average view. In Section 5 we will explore the transfer behaviour in more detail, taking donor's and recipients' risk preferences into account.

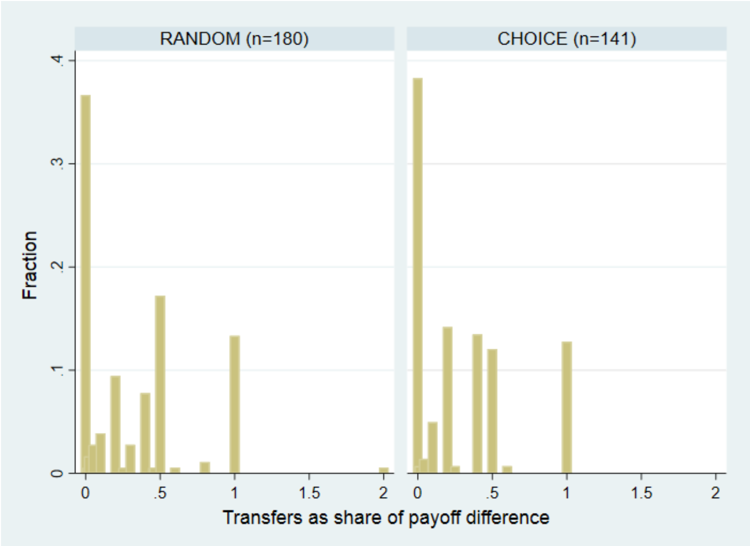
2.4 Supplementary data collected within the experiment

Since subjects' risk attitudes are an important determinant of risk taking, we elicitated an experimental measure of risk preferences which is comparable across all 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

[10] The partner's average final share of the pair's aggregated income, $(x_j + T_i)/(x_i + x_j)$, is 33.9% under RANDOM (31.5% under CHOICE). This finding is well in line with the results of dictator game experiments conducted in rural Kenya, whose sharing task might be comparable to that in our RANDOM treatment. For example, Jakiela (2015) finds a mean offer of 26.5%, Ensminger (2000) of 31% and Henrich et al. (2006) of 33% to 40%. See Cardenas and Carpenter (2008) for a summary of dictator game results in other, both developed and developing countries.

[11] Jakiela (2015) found a very similar percentage of even splits (16.1%) in her benchmark dictator game in Western Kenya.

Figure 3: Distribution of transfers to worse-off partners (as share of payoff difference) by treatment



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). The details of this game are described in Appendix B.

In the post-experimental survey we collected all other individual and household characteristics that are important drivers of risk taking and solidarity. We use this information to assess whether subjects with the same stated preferences for the risky or the safe project, respectively, do not differ in these important characteristics across treatments because they differ in the way these preferences are measured (hypothetical question in RANDOM versus incentivized decision in CHOICE and in the auxiliary experiment). Besides basic demographics this includes information on health, occupation, income, asset ownership, financial risk exposure as well as social preferences. Table A1 in Appendix A provides an overview of the retrieved variables.[12]

[12] Besides risk preferences, 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. We have collected 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 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): fairness, trust, helpfulness and two measures of inequality aversion. The first three variables are revealed by the following General Social Survey (GSS) questions: 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";

3 Hypotheses and how we test them

3.1 A simple model of optimal transfer decisions

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. This has two implications. Firstly, all payoff combinations for which transfer decisions have to be made are known. Secondly, transfer decisions are independent of expected transfers from the assigned partner because subjects only receive transfers if the are worse of than their partner in which case they do not have to make a transfer themselves. Consequently, the income of subject i is determined by the payoff from the project, x_i, and the transfer T_i made to partner j. 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 the subject i's utility from the net payoff after deducting one's own transfer to partner j if j is worse off than i, with $u'(\cdot) > 0$ and $u''(\cdot) < 0$. The solidarity norm, η_i, specifies the transfer amount to partner j 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, Strobl and Wunsch 2018), 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 following we allow the solidarity norm and, hence, optimal transfers to depend on the following factors:

- treatment C_i, where $C_i = 0$ denotes RANDOM and $C_i = 1$ denotes CHOICE
- own payoff $x_i(R_i)$ which depends on actual risk exposure where $R_i = 0$ denotes the safe project with $x_i(0) = 500$ and $R_i = 1$ denotes the risky project with $x_i(1) = 1000$
- the partner's payoff $x_j(r_j)$ where $r_j = 0$ denotes the safe project with $x_j(0) = 500$ and $r_j = 1$ denotes the risky project with $x_i(1) = 0$
- preferred risk exposure $R_i^* \in \{0, 1\}$.

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). 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.

Using the potential outcome framework typically applied in the statistical evaluation literature, we denote by $T_r^C(R)$ potential transfers 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 $C \in \{0,1\}$. Hence, potential transfers in treatment C correspond to $Y_r^C(R^C)$.

In general, we are interested both in average transfers, $E[T_r^C(R)]$, and in average transfers conditional on preferred project, $E[T_r^C(R)|R^* = R']$, in order to account for heterogeneity in transfers with respect to risk preferences. In the CHOICE treatment, actual projects correspond to preferred projects. Hence, we have $R = R^1 = R^*$ by construction. In the RANDOM treatment, however, some subjects may end up with a project they would not have chosen for themselves. Consequently, we have $R = R^0 \neq R^*$ for some subjects in RANDOM. Table 3 lists all combinations of donor's actual and preferred project as well as partner's project by treatment we observe and shows the notation we use for the corresponding average transfers. In the following, we discuss the effects we are interested in and derive hypotheses regarding these effects from theoretical considerations. We also discuss how we test these hypotheses based on the groups summarized in Table 3.

Table 3: Observed groups and notation

Project of donor		Project of	Donor's	Partner's	Average
Actual	Preferred	partner	earnings	earnings	transfer
R	R^*	r	$x(R)$	$x(r)$	$E[T_r^C(R)\|R^* = R']$
		RANDOM ($C = 0$)			
SAFE	SAFE	RISKY	500	0	$E[T_1^0(0)\|R^* = 0]$
SAFE	RISKY	RISKY	500	0	$E[T_1^0(0)\|R^* = 1]$
RISKY	RISKY	SAFE	1000	500	$E[T_0^0(1)\|R^* = 1]$
RISKY	RISKY	RISKY	1000	0	$E[T_1^0(1)\|R^* = 1]$
RISKY	SAFE	SAFE	1000	500	$E[T_0^0(1)\|R^* = 0]$
RISKY	SAFE	RISKY	1000	0	$E[T_1^0(1)\|R^* = 0]$
		CHOICE ($C = 1$)			
SAFE	SAFE	RISKY	500	0	$E[T_1^1(0)\|R^* = 0]$
RISKY	RISKY	SAFE	1000	500	$E[T_0^1(1)\|R^* = 1]$
RISKY	RISKY	RISKY	1000	0	$E[T_1^1(1)\|R^* = 1]$

3.2 Effects of interest and theoretical predictions

The first hypothesis corresponds to the main prediction tested by previous studies that responsibility for neediness affects solidarity negatively. The arguments brought forward by these 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), Lenel and Steiner (2017) and Strobl and Wunsch (2018) 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:

Hypothesis 1 (free choice matters): Subjects who self-select into the risky project on average receive lower transfers than subjects who end up in the risky project for exogenous reasons.

We can test hypothesis 1 in two ways. The first one is to compare transfers to partners in the risky project across treatments as in Trhal and Radermacher (2009), Cettolin and Tausch (2015) and Akbas et al. (2016). A simple comparison of mean transfers to test hypothesis 1 would correspond to estimating $E[T_1^1(R^1)-T_1^0(R^0)]$, which is what these papers do. However, as we also point out in Strobl and Wunsch (2018), this ignores the fact that the endogenous risk treatment CHOICE changes two things at the same time compared to the exogenous risk treatment RANDOM. On the one hand, it changes the process by which transfer recipients end up in the risky project, which is the behavioural effect of interest that corresponds to the direct effect of CHOICE on transfers (T^1 versus T^0). At the same time, however, it changes which donors end up in the risky and the safe project, i.e. it changes the distribution of donors' projects from R^0 to R^1. Ideally, the experimental design would ensure that the probability that a donor with given characteristics ends up in a given project is the same in both treatments, which corresponds to keeping the distribution of donors' projects constant across treatments. This is not possible, though, when we change the process from random assignment to free choice. Under CHOICE, subjects with certain characteristics self-select into the risky project. Therefore, they differ systematically from the subjects who are randomly assigned to the risky project in RANDOM. We can formalize the idea of fixing the distribution of donors' projects to isolate the behavioural effect of changing the process from the mechanical effect of changing the distribution of projects using ideas from causal mediation analysis (Robins and Greenland 1992, Pearl 2001, Robins 2003, Imai et al. 2011). By adding and subtracting $E[T_1^0(R^1)]$ we can decompose the average treatment effect as follows:

$$E[T_1^1(R^1)-T_1^0(R^0)] = \underbrace{E[T_1^1(R^1)-T_1^0(R^1)]}_{\text{behavioural effect } \beta_1} + \underbrace{E[T_1^0(R^1)-T_1^0(R^0)]}_{\text{mechanical effect } \mu_0}. \quad (2)$$

The behavioural effect β_1 corresponds to the average effect of CHOICE on transfers when fixing the distribution of donors' projects to that under CHOICE (R^1).[13] Since projects in CHOICE correspond to preferred projects, i.e. $R^1 = R^*$, we have

[13] Alternatively, one can fix risk exposure to that under RANDOM using the decomposition $E[T_1^1(R^1)-T_1^0(R^0)] = E[T_1^1(R^1)-T_1^1(R^0)] + E[T_1^1(R^0)-T_1^0(R^0)] = \mu_1 + \beta_0$. We will show, however, that we are able to identify β_1 but not β_0. Moreover, we show below that conditioning on preferred projects in crucial for isolating the behavioural effect.

$$\beta_1 = E[T_1^1(R^*) - T_1^0(R^*)] = \sum_{R\in\{0,1\}} E[T_1^1(R) - T_1^0(R)|R^* = R]Pr(R^* = R) \quad (3)$$

$$\mu_0 = E[T_1^0(R^*) - T_1^0(R^0)] \quad (4)$$

which we can construct directly from the data since we observe preferred projects R^* for all subjects in the data (see Section 4 for a more detailed discussion of this).

A direct implication of the decomposition in (2) is that the average treatment effect corresponds to the behavioural effect of interest β_1 if and only if the mechanical effect μ_0 is zero. The mechanical effect μ_0 is caused by differential risk exposure of the same donor in CHOICE than in RANDOM, i.e. by the fact that some donors are assigned to unwanted projects in RANDOM but not in CHOICE, $R_i^0 \neq R_i^1 = R_i^*$ for at least some subjects i. To see this more clearly, note that

$$\begin{aligned}\mu_0 = \; & [E[T_1^0(0)|R^1 = 0]Pr(R^1 = 0) + E[T_1^0(1)|R^1 = 1]Pr(R^1 = 1)] \\ & - E[T_1^0(0)|R^0 = 0]Pr(R^0 = 0) + E[T_1^0(1)|R^0 = 1]Pr(R^0 = 1).\end{aligned} \quad (5)$$

The first important lesson from equation (5) is that choosing $Pr(R^0 = 1) = Pr(R^1 = 1)$, i.e. assigning the same share to the risky project in RANDOM as would choose the risky project in CHOICE is not sufficient to ensure that the mechanical effect is zero. The reason is that $E[T_1^0(R)|R^1 = R] \neq E[T_1^0(R)|R^0 = R]$ because subjects self-select into projects in CHOICE but not in RANDOM. Consequently, we cannot ensure with the experimental design that the mechanical effect is zero. Therefore, testing hypothesis 1 requires testing $\beta_1 < 0$. The only case in which the mechanical effect will be zero is if $Y_{1,i}^0(0) = Y_{1,i}^0(1)$ for all subjects, i.e. if all donors choose to make the same transfers in the safe project as in the risky project in RANDOM because then different distributions of donors' projects do not matter for transfers.

There are at least two reasons why donors with the same characteristics could make different transfers if assigned to different projects. On the one hand, payoff levels differ by project. Donors in the safe project have 500 KSh to share with a partner in the risky project who earns nothing while donors in the risky project have 1000 KSh to share. Payoff differences matter for giving if subjects are inequality averse. In our data, between 19% and 28% of subjects are inequality averse depending on the measure we use (see Section 2.4). Hence, we would expect that, on average, donors give more when assigned to the risky project than when assigned to the safe project. The second reason for differences in transfers across projects within RANDOM is related to the argument of Lenel and Steiner (2017) that subjects who are assigned to an unwanted project in RANDOM suffer a utility loss compared to assignment to their preferred project. These subjects may compensate *themselves* for this loss by giving less to others. Our design allows us to test for such behaviour within RANDOM based on the following hypothesis:

Hypothesis 2 (utility loss matters): Average transfers are higher when assigned to the preferred project in RANDOM compared to assignment to the alternative project.

We test this hypothesis by comparing donors with the same project in RANDOM who differ in their preferred project. Specifically, we test whether donors assigned to project R who also prefer R, i.e. $R = R^*$, make higher average transfers than donors assigned to the same project R who prefer the other project, i.e. $R \neq R^*$: $E[T_r^0(R)|R^* = R] - E[T_r^0(R)|R^* = 1 - R]$.[14] We do this separately for all possible cases $r = 1, R \in \{0, 1\}$ and $r = 0, R = 1$.

If self-compensation for assignment to unwanted projects matters, then we expect higher transfers in the risky than in the safe project for donors who prefer the risky project and lower transfers for donors who prefer the safe project. In contrast, payoff differences imply higher transfers in the risky than in the safe project for both types of donors. Hence, both effects go in the same direction for donors who prefer the risky project implying a positive net effect. For donors who prefer the safe project they go in opposite directions such that the net effect is unclear. It may even be zero if both effects cancel. The same holds for the population where safety choosers make up a substantial part. We can test for non-zero net effects, i.e. for non-zero differences between transfers in the risky versus the safe project within RANDOM based on the following hypothesis:

Hypothesis 3 (mechanical effects matter): Average transfers to partners with the risky project differ by assigned project in RANDOM with the difference being positive for donors who prefer the risky project.

We test hypothesis 3 both for the population based on $E[T_1^0(1) - T_1^0(0)]$, and conditional on preferred project based on $E[T_1^0(1) - T_1^0(0)|R^* = R]$ for $R \in \{0, 1\}$. Any evidence for differences in giving behaviour across projects within RANDOM implies that the mechanical effect μ_0 is not zero. Hence, with hypothesis 3 we test whether mechanical effects matter. It is important to note that the issue of mechanical effects arises in *all* experimental designs where stakeholders decide about transfers such as in Trhal and Radermacher (2009) and Cettolin and Tausch (2015). Thus, the comparisons of average transfers across treatments presented in their papers are not informative about the behavioural effect of interest unless they can rule out that giving behaviour differs by donors' project within treatments which does not seem to be the case, though.[15]

The second way to test hypothesis 1 that rules out mechanical effects by construction is to compare average transfers to partners in the risky project across treatments for donors in their preferred project:

[14] Preferred projects possibly correlate with other characteristics that affect transfers which is not accounted for when calculating this difference. However, here we are not interested in the causal effect of having different risk preferences. We just want to test whether transfers differ for donors with different preferences.

[15] The design of Trhal and Radermacher (2009) allows identifying $\beta_0 = E[T_1^1(R^0) - T_1^0(R^0)]$ and $\mu_1 = E[T_1^1(R^1) - T_1^1(R^0)]$ which would require that transfers do not depend on donor's project within the endogenous risk treatment for the mechanical effect to be zero. However, they find that risk takers always give considerably more than safety choosers in this treatment (see Table 3 in their paper). Cettolin and Tausch (2015) have a design similar to ours implying that hypothesis 3 must hold for the mechanical effect to be zero. However, subjects facing the low-risk option in the exogenous risk treatment show different transfers than subjects facing the high-risk option (see Figures 1 and 4 in their paper).

$E[T_1^1(R) - T_1^0(R)|R^* = R]$ for $R \in \{0, 1\}$ (approach 2). Here, we directly fix projects to the preferred ones which is equivalent to fixing projects to those under CHOICE because in CHOICE everyone receives the preferred project, i.e. $R_i^1 = R_i^*$ for all i by construction. Calculating the conditional treatment effects is also necessary to obtain the behavioural effect β_1 as can be seen from equation (3). Additionally, the conditional effects allow us to test for effect heterogeneity with respect to risk preferences. For example, Trhal and Radermacher (2009) show that safety choosers punish risk takers more than other risk choosers do. Moreover, Cettolin et al. (2017) find that giving under uncertainty depends on risk preferences. D'Exelle and Verschoor (2015) provide evidence for voluntary risk takers having different fairness views. Cappelen et al. (2013) and Morsink (2017) provide arguments why a discrepancy between donor's and beneficiary's risk preferences can explain differences in giving behaviour. There are also papers that show that risk preferences interact with social preferences (e.g. Müller and Rau 2016), which would imply that risk takers have other solidarity norms than safety choosers.

Another advantage of our design is that we are able to distinguish changes in the solidarity norm due to attributions of responsibility for neediness from other explanations of differences in solidarity norms across treatments. If responsibility for neediness is the only driver behind differential transfers across treatments, then transfers to partners who self-select into the safe project should not differ by treatment. This leads to the following hypothesis:

Hypothesis 4 (no other explanation): Subjects who self-select into the safe project on average receive the same transfers as subjects who end up in the safe project for exogenous reasons.

We test this hypothesis by comparing the transfers subjects make to partners who self-select into the safe project in CHOICE with the transfers to partners who are randomly assigned to the safe project in RANDOM. Noting that only donors in the risky project make transfers to partners in the safe project,[16] hypothesis 4 corresponds to testing whether $E[T_0^1(1) - T_0^0(1)|R^* = 1] = 0$. We condition on preferring the risky project to account for self-selection into the risky project in CHOICE such that we only compare subjects with the same actual and preferred project. If the data reject hypothesis 4, then approaches 1 and 2, which compare average transfers across treatments, measure the total behavioural effect of CHOICE independent of the causal channels behind this effect.

Our design also allows us to measure the effect of responsibility directly based on a within-subject comparison of transfers to partners with different projects in CHOICE as in Cettolin and Tausch (2015) and Strobl and Wunsch (2018). Specifically, we can compare the transfers a subject in the risky project makes to partners who self-select into the risky project with transfers the same subject makes to partners who self-select into the safe project, $E[T_1^1(1) - T_0^1(1)|R^* = 1]$. However, taking the difference between the two will not isolate the effect of interest if payoff differences matter for giving. To see this, note that

[16]Donors in the safe project are equally well-off as partners in the safe project.

donors in the risky project make a decision on sharing 1000 KSh with a partner who either earns 500 KSh if in the safe project, or 0 KSh if in the risky project. Thus, the payoff difference is 500 KSh in the former case and 1000 KSh in the latter. Consequently, the within-subject comparison needs to correct for possible effects of different payoff differences. As noted above, payoff differences matter for giving if subjects are inequality averse, which is the case for a substantial share of our subjects. This leads to the following hypothesis:

Hypothesis 5 (payoff differences matter): Average transfers increase with the difference between own and partner's payoff.

To test this hypothesis, we can use the subjects assigned to the risky project in RANDOM and compare what they give to partners in the safe versus the risky project. Specifically, we can test whether the average difference in transfers is positive, $E[T_1^0(1) - [T_0^0(1)] > 0$, and whether it is positive conditional on preferred project, $E[T_1^0(1) - [T_0^0(1)|R^* = R] > 0$ for $R \in \{0, 1\}$. We need the latter to isolate the effect of responsibility for neediness in the within-subject comparison of transfers by risk takers in CHOICE. By estimating $E[T_1^1(1) - T_0^1(1)|R^* = 1] - E[T_1^0(1) - T_0^0(1)|R^* = 1]$ we correct the within-subject difference in transfers to partners with different projects in CHOICE by the same difference in RANDOM. Note that the within-subject and within-treatment comparisons presented by Cettolin and Tausch (2015) are also subject to this problem because payoff differences are present in their within-subject comparisons as well. Hence, what they claim to be the effect of responsibility for neediness might be biased by possible effects of payoff differences on giving.

To see why the corrected within-subject comparison of transfers to partners with different projects isolates the effect of responsibility for neediness, note that we can rewrite the estimand as follows:

$$
\begin{aligned}
&E[T_1^1(1) - T_0^1(1)|R^* = 1] - E[T_1^0(1) - T_0^0(1)|R^* = 1] \qquad (6)\\
&= E[T_1^1(1) - T_1^0(1)|R^* = 1] - E[T_0^1(1) - T_0^0(1)|R^* = 1]
\end{aligned}
$$

The first term on the right hand side of equation (6) is the effect of CHOICE on transfers made by subjects who prefer the risky project to partners with the *risky* project, which is what we estimate with approach 2. The second term on the right hand side of equation (6) is the effect of CHOICE on transfers made by subjects who prefer the risky project to partners with the *safe* project, which is what we estimate when we test hypothesis 4. Hence, the effect we estimate with equation (6) corrects for any other possible factors that cause differential giving behaviour of risk takers by treatment. Thus, with equation (6), we can test the following hypothesis:

Hypothesis 6 (responsibility matters): Subjects who self-select into the risky project in CHOICE on average receive lower transfers than subjects who self-select into the safe project.

Taking all of the above arguments together, an experimental design that aims to answer the question whether responsibility for neediness affects the solidarity of stakeholders needs to have the following features. Firstly, it needs to measure and condition on donor's preferred option because this rules out mechanical effects which are caused, for example, by payoff differences across projects or self-compensation for utility losses due to assignment to unwanted options under exogenous risk. Secondly, it needs to create a group that is unaffected by attributions of responsibility such that it allows testing for alternative explanations for possible treatment effects. Thirdly, it needs to create a group for which it is possible to isolate the effect of responsibility for neediness. In contrast to previous studies, our experimental design satisfies all of these criteria. Additionally, it allows testing whether any of the concerns discussed above matter. The following Table 4 summarizes the six hypotheses we have derived and how we test them.

Table 4: Hypotheses

Hypothesis	Statistical equivalent
1 (free choice matters)	$E[T_1^1(R^*) - T_1^0(R^*)] \sum_{R\in\{0,1\}} E[T_1^1(R) - T_1^0(R)\|R^* = R]Pr(R^* = R) < 0$
	$E[T_1^1(R) - T_1^0(R)\|R^* = R] < 0$ for $R \in \{0, 1\}$
2 (utility loss matters)	$E[T_r^0(R)\|R^* = R] - E[T_r^0(R)\|R^* = 1 - R] > 0$ for $r = 1, R \in \{0, 1\}$ and $r = 0, R = 1$
3 (mechanical effects matter)	$E[T_1^0(1) - T_1^0(0)] = 0$
	$E[T_1^0(1) - T_1^0(0)\|R^* = 0] = 0$
	$E[T_1^0(1) - T_1^0(0)\|R^* = 1] > 0$
4 (no other explanation)	$E[T_0^1(1) - T_0^0(1)\|R^* = 1] = 0$
5 (payoff differences matter)	$E[T_1^0(1) - [T_0^0(1)] > 0$
	$E[T_1^0(1) - [T_0^0(1)\|R^* = R] > 0$ for $R \in \{0, 1\}$
6 (responsibility matters)	$E[T_1^1(1) - T_0^1(1)\|R^* = 1] - E[T_1^0(1) - T_0^0(1)\|R^* = 1] < 0$

It should be noted that some but not all of the above issues are specific to designs that are interested in giving behaviour of stakeholders, i.e. of subjects who are exposed to the safe or risky options themselves. Designs that use transfers of uninvolved spectators, such as the one of Akbas et al. (2016), rule out mechanical effects that occur because involved donors respond to the situation they are exposed to in a given treatment. However, payoff differences that occur because different beneficiaries end up in different situations can matter here as well. Moreover, these designs also need to rule out alternative explanations for treatment effects. Finally, average treatment effects may hide important heterogeneity in responses by donors with different risk and social preferences as shown, for example, by Cettolin et al. (2017).

4 Implementation

4.1 Empirical counterparts

In the following we show that all hypotheses can be tested by either comparing mean observed transfers across randomized samples, or across randomized samples conditional on actual and preferred project. As can be seen from Table 4, we need to estimate average transfers of different forms to obtain the empirical counterparts of the effects of interest.[17] Firstly, we need to estimate average transfers conditional on

[17]To estimate the behavioural effect β_1, we additionally need the share preferring the risky project to obtain $Pr(R^* = 1) = E[R^*]$ which is observed in the data.

project within RANDOM, $E[T_r^0(R)]$. Since subjects have been randomized into projects in RANDOM, these can be obtained from mean transfers in the respective group:

$$E[T_r^0(R)] = E[T_r|C = 0, R = R] \quad r, R \in \{0, 1\} \tag{7}$$

where T_r are observed transfers to a partner with project r. Secondly, we need average transfers conditional on actual and preferred project in RANDOM for all combinations of actual and preferred project, $E[T_r^0(R)|R^* = R']$ for $r, R, R' \in \{0, 1\}$. Here, we need to use the subgroup of subjects where assigned and preferred project coincide:

$$E[T_r^0(R)|R^* = R'] = E[T_r|C = 0, R = R, R^* = R'] \quad r, R, R' \in \{0, 1\}. \tag{8}$$

Finally, we need to estimate average transfers conditional preferred project for CHOICE, $E[T_r^1(R)|R^* = R]$. Since actual projects correspond to preferred projects in CHOICE, $R = R^*$, these can be calculated from those observed in project R in CHOICE:

$$E[T_r^1(R)|R^* = R] = E[T_r|C = 1, R = R^*] \quad r, R \in \{0, 1\}. \tag{9}$$

It is important to note that conditioning on preferred projects is the only way we can identify the behavioural treatment effects without imposing additional assumptions. Isolating the behavioural effect requires conditioning on projects. Yet, we can only identify average transfers conditional on project in CHOICE for the group that is actually observed in this project which corresponds to the group that prefers this project. Hence, we can only identify treatment effects conditional on projects for the subgroup of subjects where actual and preferred project coincide. For this, it is crucial that we correctly measure preferred projects R^* for all subjects. We discuss this in detail in Section 4.3.

4.2 Did randomization work?

To obtain unbiased estimates we need to make sure that randomization into treatments and into projects within RANDOM created comparable groups. In Table A1 in Appendix A, we report the means of all variables in our data by treatment and by project within treatment. Randomization of treatments worked very well. The majority of means is very similar. For only 2 out of 50 variables we find differences that are significant on the 10% level. The randomization of projects within RANDOM also succeeded in creating comparable groups with only 3 out of 50 differences in means being significant on the 10% level. For CHOICE, Table A1 shows the selectivity of project choice. Subjects who choose the risky project have a much stronger preference for risk as expected, higher income, fewer other earners in the household, and they are more likely to be the household head, where the latter is explained by a higher share of males.

4.3 Do we correctly measure preferred projects?

Given that randomization worked very well, the most crucial part of our experiment is whether we correctly measure preferred projects. A major concern could be that subjects in RANDOM only answer a hypothetical question without any monetary consequences whereas the subjects in CHOICE have to face the consequences of their choice. There are several ways to assess whether there are any systematic differences in the preferences stated by the subjects in RANDOM compared to those in CHOICE. As a first check, we compare the share of subjects preferring the risky project in CHOICE and RANDOM. In CHOICE, 19.5% of subjects choose the risky project whereas in RANDOM 24.2% prefer it. The difference is not statistically significant with a p-value of .38 (see Table 5).

Table 5: Distribution of projects by treatment

	RANDOM				CHOICE		Auxiliary		Differences	
	Actual		Preferred							
	(1)		(2)		(3)		(4)		(3) - (2)	(4) - (2)
	N	%	*N*	%	*N*	%	*N*	%		
Safe project	*60*	5.0	*91*	75.8	*95*	8.5	*86*	77.5	4.7	1.6
Risky project	*60*	5.0	*29*	24.2	*23*	19.5	*25*	22.5	(.38)	(.77)
Observations	*120*		*120*		*118*		*111*			

Note: P-values in parentheses.

As a second check, we test whether subjects rationalize the project they have been assigned to in RANDOM when stating their preferred project ex post. To do so, we firstly, check for a statistically significant difference in the share being assigned to and preferring the risky project in RANDOM in line (1) of Table 6. The difference is 25.8 percentage points and highly statistically significant. Secondly, we test for a positive correlation between assigned and preferred project in RANDOM in line (2) of Table 6. The correlations are positive but small and not statistically significant with p-values around 30%. Hence, we cannot reject the hypothesis that assigned and preferred projects are unrelated in RANDOM.

Table 6: Relation between assigned and preferred projects in RANDOM

	Assigned	Preferred	Difference	P-value		
(1) RISKY	.500	.242	.258	.000		
(2) Correlation	Pearson	P-value	Spearman	P-value	Tetrachoric	P-value
	.097	.290	.097	.290	.167	.394

The third check compares the characteristics of the subjects preferring the same project across the two treatments. These should not differ systematically because subjects with the same characteristics should state the same preference in CHOICE as in RANDOM if preferences are measured correctly. In Table A1 in Appendix A we report the means of all variables by chosen project in CHOICE and by preferred project in RANDOM, and we test for statistically significant differences between the two. For subjects who prefer the safe project, only 3 out of 50 differences in means are significant on the 10% level and 2 of them correspond to the ones for which we find small sample imbalances between RANDOM and CHOICE in general. For subjects who prefer the risky project, there are only 2 statistically significant differences in means on the 5% level which is to be expected with 50 variables tested. In Table A2 in Appendix A we additionally report the same statistics conditional on assigned project in RANDOM. Specifically, we compare subjects with the safe project in CHOICE with the subgroup of subjects assigned to the safe project in RANDOM that also prefers the safe project and correspondingly for the risky project. These are the groups that will be used for the estimation. For both the safe and the risky project we only find one variable with a difference in means that is significant on the 10% level. Thus, we can conclude that subjects in CHOICE and RANDOM who state to prefer the same project do not differ systematically in the large number of observed drivers of project choice and willingness to give.

The fourth check makes use of the auxiliary experiment, where all subjects choose between the safe and the risky project as in CHOICE but without running the solidarity game. This addresses two possible concerns. Firstly, we can assess whether real monetary consequences matter for stated preferences. In the auxiliary experiment, project choice is incentivized as it determines subjects' payoff from the experiment. Secondly, we address the issue that subjects may choose projects strategically because they have to make a decision on transfers to worse-off partners after having chosen a project where the probability to face a partner who is worse off differs by project.[18] This would imply that project choice is determined by other factors than risk preferences. In Table A3 in Appendix A, we report the means of all variables for the subjects in RANDOM and in the auxiliary experiment, as well as in the subgroups that state to prefer the safe and the risky project, respectively. The subjects we sampled for the auxiliary experiment are somewhat better educated than the ones we sampled for the main experiment, which results in lower rates of unemployment and higher wealth, and which is correlated with ethnicity. Other than that, there are 3 more statistically significant means which do not show a systematic pattern, though. Apart from these small sample imbalances we do not find any systematic differences in stated preferences, though. The share of subjects preferring the risky project in the auxiliary experiment is 22%, which is 1.6 percentage points lower than in RANDOM with the difference being not statistically different from zero at a p-value of .77 (see Table 5). Differences in mean characteristics of safety choosers between the main and the auxiliary experiment only mirror the sample imbalances. The findings for risk takers are similar with only few statistically significant differences in means that mostly mirror the sample imbalances. There are only 2 out of 50 variables with significant difference that are not directly related to the sample imbalances. Thus, the auxiliary experiment does not provide any evidence for differences in stated preferences. This, together with the other evidence presented above, makes us very confident that we correctly measure preferred projects for all subjects and that project choices reflect risk preferences.

[18] Conditional on choosing the safe project, the probability to face a worse-off partner is $\pi(0) = Pr(R^* = 1) \cdot .5$. Conditional on choosing the risky project it is $\pi(1) = .5 \cdot [Pr(R^* = 1) \cdot .5 + (1 - Pr(R^* = 1)] = .5 \cdot [1 - \pi(0)]$. This will differ unless $\pi(0) = 1/3$. In our case, we have $\pi(0) = .11$.

5 Results

In Table 7 we report mean transfers for each relevant group including mean transfers by treatment, which can be used to calculate the average treatment effect. We show the means for all six transfer outcomes we have described in Section 2.3. In Table 8 we report the relevant differences between mean transfers that we need to test hypotheses 1-6 as summarized in Table 4.

Table 7: Mean transfers by group

	Project of donor i		Project of	Donor's	Partner's		Transfer to worse-off partner					
	Actual	Preferred	partner j	earnings	earnings		KSh	Share	Share τ_i equals or is in range			
	R_i	R_i^*	R_j	x_i	x_j	N	T_i	τ_i	0	(0, .5)	.5	(.5, 1]
RANDOM												
(R1)	All	All	RISKY		0	120	207	.277	.350	.342	.200	.108
(R2)	SAFE	All	RISKY	500	0	60	141	.281	.383	.350	.133	.133
(R2a)	SAFE	SAFE	RISKY	500	0	48	142	.285	.354	.375	.167	.104
(R2b)	SAFE	RISKY	RISKY	500	0	12	134	.268	.500	.250	.000	.250
(R3)	RISKY	All	RISKY	1000	0	60	273	.273	.317	.333	.267	.083
(R3')	RISKY	All	SAFE	1000	500	60	193	.385	.400	.233	.117	.250
(R3a)	RISKY	RISKY	SAFE	1000	500	17	259	.518	.294	.294	.059	.353
(R3b)	RISKY	RISKY	RISKY	1000	0	17	398	.398	.176	.353	.235	.235
(R3c)	RISKY	SAFE	SAFE	1000	500	43	167	.333	.442	.209	.140	.209
(R3d)	RISKY	SAFE	RISKY	1000	0	43	223	.223	.372	.326	.279	.023
CHOICE												
(C1)	All	All	RISKY		0	118	169	.298	.339	.390	.136	.136
(C2a)	SAFE	SAFE	RISKY	500	0	95	161	.322	.305	.442	.084	.168
(C3a)	RISKY	RISKY	SAFE	1000	500	23	99	.198	.609	.217	.043	.130
(C3b)	RISKY	RISKY	RISKY	1000	0	23	200	.200	.478	.174	.348	.000

Note: Share of payoff difference given to worse-off partner: $\tau_i = T_i/(x_i - x_j)$ with equal split when $\tau_i = .5$.

Hypothesis 1 (free choice matters): We find that, on average, when we fix the distribution of projects, transfers do not differ by treatment in line (1a) of Table 8. However, this hides important heterogeneity in responses by donors with different risk preferences. Safety choosers do not reduce transfers to risk choosers, whereas risk choosers give significantly less to other risk choosers compared to random assignment to the risky project. Average transfers fall by about 200 KSh, the share with zero transfers increases by 3.2 percentage points and the share with more than equal split falls by 23.5 percentage points. This is in line with previous findings for Kenya (Strobl and Wunsch 2018) but in contrast to the finding from Western countries that safety choosers tend to punish more than risk choosers (Trhal and Radermacher 2009). We explore possible explanations for this finding at the end of this section.

Hypothesis 2 (utility loss matters): Our data support the hypothesis that self-compensation for utility losses due to assignment to unwanted projects matters. Lines (2b) and (2c) in Table 8 show that subjects assigned to the risky project who prefer this project on average give considerably more than those who prefer the safe project. The differences are statistically significant on the 10% level for partners in the risky project. For partners in the safe project, the results are qualitatively the same but the p-values are larger with 20-30%. For subjects assigned to the safe project we find a significantly smaller share of donors who equally split the payoff difference (-16.7 percentage points). These findings suggest that mechanical effects are likely to be an issue which we test directly with hypothesis 3.

Table 8: Results for transfers to worse-off partners

Hypothesis	Row in Table 7	KSh T_i	p	Share τ_i	p	Share τ_i equals or is in range: 0	p	$(0,.5)$	p	$.5$	p	$(.5,1]$	p
(1) Free choice matters													
(1a) $E[T_1^1(R^*) - T_1^0(R^*)]$		-29	.36	-.014	.77	.028	.70	.013	.86	-.040	.49	-.001	.98
(1b) $E[T_1^1(0) - T_1^0(0)\|R^* = 0]$	(C2a)-(R2a)	19	.50	.037	.50	-.049	.56	.067	.44	-.082	.18	.064	.28
(1c) $E[T_1^1(1) - T_1^0(1)\|R^* = 1]$	(C3b)-(R3b)	-198*	.06	-.198*	.06	.302**	.04	-.179	.22	.113	.45	-.235**	.03
(2) Utility loss matters													
(2a) $E[T_1^0(0)\|R^* = 1] - E[T_1^0(0)\|R^* = 0]$	(R2b)-(R2a)	-8	.90	-.016	.90	.146	.37	-.125	.39	-.167***	.00	.146	.28
(2b) $E[T_0^0(1)\|R^* = 0] - E[T_0^0(1)\|R^* = 1]$	(R3c)-(R3a)	-92	.21	-.185	.21	.148	.28	-.085	.51	.081	.31	-.144	.29
(2c) $E[T_1^0(1)\|R^* = 0] - E[T_1^0(1)\|R^* = 1]$	(R3d)-(R3b)	-175*	.07	-.175*	.07	.196$^+$	.11	-.027	.84	.044	.73	-.212*	.05
(3) Mechanical effects matter													
(3a) $E[T_1^0(1) - T_1^0(0)]$	(R3)-(R2)	132***	.00	-.008	.88	-.067	.45	-.017	.85	.133*	.07	-.050	.38
(3b) $E[T_1^0(1) - T_1^0(0)\|R^* = 0]$	(R3d)-(R2a)	81*	.06	-.061	.29	.018	.86	-.049	.63	.112	.21	-.081$^+$	.11
(3c) $E[T_1^0(1) - T_1^0(0)\|R^* = 1]$	(R3b)-(R2b)	264**	.02	.130	.41	-.324*	.08	.103	.57	.235**	.04	.015	.93
(4) No other explanation													
(4) $E[T_0^1(1) - T_0^0(1)\|R^* = 1]$	(C3a)-(R3a)	-160**	.04	-.320**	.04	.315**	.05	-.077	.60	-.015	.84	-.223$^+$	.12
(5) Payoff differences matter													
(5a) $E[T_1^0(1) - T_0^0(1)]$	(R3)-(R3')	80***	.01	-.112**	.02	-.083*	.10	.100*	.08	.150**	.01	-.167***	.01
(5b) $E[T_1^0(1) - T_0^0(1)\|R^* = 0]$	(R3d)-(R3c)	57$^+$	.12	-.110**	.05	-.070	.26	.116*	.06	.140**	.03	-.186***	.01
(5c) $E[T_1^0(1) - T_0^0(1)\|R^* = 1]$	(R3b)-(R3a)	139**	.02	-.119	.18	-.118	.17	.059	.67	.176	.19	-.118	.34
(6) Responsibility matters													
(6) $E[T_1^1(1) - T_0^1(1)\|R^* = 1]$ $-E[T_1^0(1) - T_0^0(1)\|R^* = 1]$	(C3b) - (C3a) - [(R3b)-(R3a)]	-38	.56	.122	.23	-.013	.93	-.102	.50	.128	.42	-.013	.92

Note: ***/**/*/$^+$ indicates significance on the 1/5/10/15% level. Share of payoff difference given to worse-off partner: $\tau_i = T_i/(x_i - x_j)$ with equal split when $\tau_i = .5$.

Hypothesis 3 (mechanical effects matter): We have to reject the hypothesis that there are no mechanical effects. Average transfers in RANDOM differ significantly by project both on average (line (3a)), and in subsamples defined by preferred project (lines (3b) and (3c)). Average transfers are significantly higher for donors in the risky project than for donors in the safe project. As expected, the differences are largest for subjects who prefer the risky project and smallest but still positive for subjects who prefer the safe project. This suggests that for the latter group, payoff differences matter more than self-compensation for utility losses, or that there are other factors that lead to different transfers across treatment. Since we can rule out that mechanical effects are zero, the average treatment effect that can be obtained from subtracting lines (C1) and (R1) in Table 7, is not informative about the behavioural effect of changing the process by which transfer recipients end up in the risky project. In our case, the mechanical effect is positive implying that the average treatment effect is biased upwards and, therefore, underestimates the absolute value of a negative behavioural effect. Hence, we must fix projects when we compare transfers across treatments in order to isolate the behavioural effect of interest.

Hypothesis 4 (no other explanation): In line (4) of Table 8 we present the effect of CHOICE on transfers to partners in the safe project. If attributions of responsibility for neediness drive reduced solidarity of risk takers towards other risk takers in CHOICE, then this effect should be zero. Instead, we find statistically significant effects that are of a similar magnitude as the ones for partners in the risky project in line (1c). This suggests that attributions of responsibility are not the main driver behind reduced solidarity. We will explore this further once we have discussed hypothesis 6, which directly test for reduced solidarity due to attributions of responsibility for neediness.

Hypothesis 5 (payoff differences matter): With the rejection of hypothesis 2 we have already provided some evidence that the difference between donor's and partner's payoff seems to matter for transfers. Lines (3a), (3b) and (3c) in Table 8 confirm that this is indeed the case. We find that average transfers in RANDOM are significantly higher for partners in the risky project with a payoff difference of 1000 KSh than for partners in the safe project with a payoff difference of 500 KSh. Similarly, the share of subjects who give nothing is smaller whereas the share with intermediate redistribution and equal split of the payoff difference increases. This also holds conditional on the preferred project. The fact that payoff differences matter implies that within-subject comparisons of transfers to partners with different projects need to correct for the effect of the involved payoff differences.

Hypothesis 6 (responsibility matters): In line (6) of Table 8 we present the estimated effect of responsibility that accounts for payoff differences. In line with our results for hypothesis 4, we find no statistically significant differences in transfers to risk choosers compared to safety choosers within CHOICE. Thus, there is no evidence for reduced solidarity due to attributions of responsibility for neediness.[19] Instead, our results suggest that the possibility of free choice of projects changes the solidarity norm of risk takers independent of their partners' behaviour. Table 9 provides a potential explanation for this. It reports mean expected transfers from better-off partners by treatment as well as the differences across treatments for the groups of donors for whom actual project corresponds to preferred project.

Table 9: Mean expected transfers by group

	Project of donor i		Project of	Donor's	Partner's		Expected tranfer from better-off partner					
	Actual	Preferred	partner j	earnings	earnings		KSh	Share	Share τ_i equals or is in range			
	R_i	R_i^*	R_j	x_i	x_j	N	T_j	τ_j	0	(0, .5)	.5	(.5, 1]
RANDOM												
(R2a)	SAFE	SAFE	RISKY	500	0	48	289	.577	.188	.208	.104	.500
(R3a)	RISKY	RISKY	SAFE	1000	500	17	224	.447	.235	.294	.176	.294
(R3b)	RISKY	RISKY	RISKY	1000	0	17	482	.482	.118	.235	.412	.235
CHOICE												
(C2a)	SAFE	SAFE	RISKY	500	0	95	275	.550	.242	.242	.063	.453
(C3a)	RISKY	RISKY	SAFE	1000	500	23	122	.243	.478	.261	.130	.130
(C3b)	RISKY	RISKY	RISKY	1000	0	23	237	.237	.435	.261	.217	.087
Difference												
(C2a)-(R2a)	SAFE	SAFE	RISKY	500	0		-13.5	-.027	.055	.034	-.041	-.047
(C3a)-(R3a)	RISKY	RISKY	SAFE	1000	500		-101.8*	-.204*	.243$^+$	-.033	-.046	-.164
(C3b)-(R3b)	RISKY	RISKY	RISKY	1000	0		-244.8**	-.245**	.317**	.026	-.194	-.148

Note: Share of payoff difference expected from-off partner: $\tau_i = T_j/(x_i - x_j)$ with equal split when $\tau_i = .5$.

We find that expected transfers are always larger than given transfers which is in line with other studies (Büchner et al. 2007, Trhal and Radermacher, 2009). Moreover, we find similar effects for expected transfers as for given transfers. There are no effects for subjects in their preferred safe project. Risk takers, however, expect significantly less from their partners independent of their partner's choice of

[19] This differs from what we find in Strobl and Wunsch (2018). There, we find no effects for partners in the safe project and that the comparisons across treatments and within subjects coincide. However, in this study a large part of our sample (58%) was from a different environment, the Viwandani slum, which is a very unstable informal settlement where residents typically only stay for a short period. These subjects show much lower willingness to give than the residents of the Kibera slum, which comprise our study population. This suggests, that their solidarity norm favours less sharing which may make them more likely to punish risk taking. However, also in this study there is some suggestive evidence that responsibility for neediness is not the main driver behind reduced solidarity for some subjects but the data did not allow us to explore this further.

project. This suggests, that risk choosers take responsibility for their own risky choice by expecting less from others. In return, though, they are less willing to share high payoffs with worse-off partners. These findings are in line with D'Exelle and Verschoor (2015) who study investment behavior and risk sharing in a lab-in-the-field experiment in Uganda. They find that individuals who make risky investments share less of both their profits, and their losses. On the one hand, they argue that investors may consider it to be unfair if other people share a substantial part of the loss of their risky investment if it failed. On the other hand, they argue that investors may at the same time consider it to be unfair if they have to share a substantial part of the profits if the investment was successful. This implies that investors are aware of the burden their investment behavior may impose on others in societies with a strong social norm towards sharing while at the same time feeling less obliged to share the profits they make out of these investments. Our results show in addition that individuals who prefer to avoid risky situations expect and provide a certain level of support independent of the situation that has put them on either the giving or receiving end in such societies.

6 Conclusion

In this paper, we show that studying the question whether responsibility for neediness matters for solidarity of stakeholders involves a number of challenges that can only be addressed with an appropriate experimental design. Such a design needs to measure and condition on donors' preferred option because this allows distinguishing the behavioural effect of interest from other, mechanical effects. Such effects are caused, for example, by payoff differences across projects or self-compensation for utility losses due to assignment to unwanted options under exogenous risk. Secondly, it needs to create a group that is unaffected by attributions of responsibility such that it allows testing for alternative explanations for reduced solidarity. Thirdly, it needs to create a group for which it is possible to isolate the effect of responsibility for neediness. None of the designs used by previous studies satisfies all of these criteria, which implies that the effects reported by existing studies are biased away from the behavioural effect of interest or that they are unable to distinguish attributions of responsibility for neediness from other causes of reduced solidarity. Our experimental design not only satisfies all of the above criteria, but also allows testing whether any of the discussed challenges matter.

With our design we find that free choice of risk exposure matters for giving but that attributions of responsibility play a negligible role in this context. We also find that effects on giving depend on donors' risk preferences. Under free choice, risk takers give less compared to random exposure to risk when their risky choices succeed independent of the choices of transfer beneficiaries. In return, however, they also expect less support when their risky choices fail. Thus, their solidarity norm under free choice of risk seems to differ from the norm applied under random risk. In contrast, safety choosers expect and provide a certain level of support independent of the situation that has put them on either the giving or receiving end. It seems that the strong norm of mutual support in developing countries works against strong punishment of risky choices and makes risk choosers aware of the burden their investment behavior may impose on others, which induces them to take responsibility for their actions. In this respect, our findings differs substantially both from the evidence for Western countries (Trhal and Radermacher 2009, Cettolin and Tausch 2015, Akbas et al. 2016), and the evidence on crowding out of informal insurance by the availability of formal insurance for developing countries (Landmann et al. 2012, Lin et al. 2014, Lenel and Steiner 2017, Morsink 2017). Our results suggest that not only the social norm regarding solidarity in a society matters, but also the situation in which individuals make choices that involve risk.

Our findings have important implications for policies that aim to encourage entrepreneurship or investments into new but risky business opportunities to reduce poverty and foster economic growth in developing countries. They suggest that crowding out of informal support by subjects who prefer not to engage in these activities may actually not be an issue. Moreover, negative effects on overall solidarity in a society will depend on the share of individuals who make use of these risky opportunities. The larger this share is, the more likely it is that crowding out of informal support by other risk takers has notable effects on overall support for these activities which would counteract the intention of these policies.

Our study also has some broader methodological implications. It shows that randomization to treatments does not always ensure that the average treatment effect corresponds to the behavioural effect of interest. If the nature of the treatment is such that it changes not only the dimension of interest but also others, then the average treatment effect is biased away from the behavioural effect of interest. We show that in this case, ideas from mediation analysis can be used to isolate the behavioural effect. Moreover, in order to be able to identify this effect without having to impose strong assumptions an appropriate experimental design is crucial. The required adjustments to the standard design depend on the research question and the treatment. Our case provides an example that may work for other questions as well.

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A Appendix: Full set of descriptive statistics

Table A1: Means of variables by treatment and project

	All			Random (assigned)			Random (preferred)			Choice				
	Random	Choice	Difference	Safe	Risky	Difference	Safe	Risky	Difference	Safe	Risky	Difference	Difference	Difference
	(1)	(2)	(2)-(1)	(3)	(4)	(4)-(3)	(5)	(6)	(6)-(5)	(7)	(8)	(8)-(7)	(7)-(5)	(8)-(6)
A. Individual characteristics														
Age	3.5	31.4	.9	3.1	3.8	.7	3.8	29.4	-1.4	31.2	32.1	.9	.4	2.6
Male	.33	.35	.02	.30	.35	.05	.32	.34	.03	.32	.48	.16	.00	.13
Schooling	11.5	11.2	-.3	11.2	11.9	.7	11.8	1.8	-1.0*	11.3	11.2	-.1	-.5	.4
8 years	.28	.27	.00	.35	.20	-.15*	.23	.41	.18*	.24	.39	.15	.01	-.02
8 <years < 12	.11	.11	.00	.12	.10	-.02	.11	.10	-.01	.13	.04	-.08	.02	-.06
12 <=years <15	.46	.52	.06	.40	.52	.12	.48	.38	-.10	.54	.43	-.10	.05	.06
years>=15	.16	.10	-.06	.13	.18	.05	.18	.10	-.07	.09	.13	.04	-.08	.03
Married	.45	.48	.03	.43	.47	.03	.43	.52	.09	.46	.57	.10	.03	.05
Household (HH) head	.64	.66	.02	.67	.62	-.05	.65	.62	-.03	.63	.78	.15	-.02	.16
Monthly income	4844	4897	53	4571	5117	546	4590	5641	1052	4569	6252	1684	-21	611
Religion (1=christian)	.84	.85	.01	.83	.85	.02	.87	.76	-.11	.86	.78	-.08	.00	.02
Occupational status														
Employed	.13	.14	.01	.15	.10	-.05	.09	.24	.15*	.13	.17	.05	.04	-.07
Self-employed	.19	.27	.08	.15	.23	.08	.20	.17	-.03	.25	.35	.10	.05	.18
Unemployed	.50	.45	-.05	.50	.50	.00	.52	.45	-.07	.46	.39	-.07	-.05	-.06
Other	.18	.14	-.04	.20	.17	-.03	.20	.14	-.06	.16	.09	-.07	-.04	-.05
Ethnicity:														
Kamba	.07	.05	-.02	.07	.07	.00	.08	.03	-.04	.05	.04	-.01	-.02	.01
Kikuyu	.07	.05	-.02	.07	.07	.00	.05	.10	.05	.04	.09	.04	-.01	-.02
Kisii	.13	.10	-.02	.13	.12	-.02	.12	.14	.02	.09	.13	.04	-.03	-.01
Luhya	.35	.36	.01	.33	.37	.03	.40	.21	-.19**	.37	.30	-.06	-.03	.10
Luo	.27	.31	.05	.28	.25	-.03	.25	.31	.06	.31	.35	.04	.05	.04
Nubian	.11	.10	-.01	.12	.10	-.02	.09	.17	.08	.11	.09	-.02	.02	-.09
Other	.02	.03	.01	.00	.03	.03	.01	.03	.02	.03	.00	-.03*	.02	-.03
Health-related characteristics														
Health problem	.33	.33	.01	.27	.38	.12	.33	.31	-.02	.32	.39	.08	-.01	.08
Chronical health problem	.13	.20	.08*	.15	.10	-.05	.09	.24	.15*	.18	.30	.13	.09*	.06
Visited health care provider	.43	.43	.00	.38	.48	.10	.45	.38	-.07	.43	.43	.00	-.02	.06
Health expenditures[a]	2497	1016	-1481	4242	752	-3490	3104	590	-2514	991	1115	124	-2113	525
Health expend.= 0	.57	.59	.03	.57	.57	.00	.56	.59	.03	.61	.52	-.09	.05	-.06
Enrolled in health insurance (HI)	.42	.44	.02	.48	.35	-.13	.36	.59	.22**	.44	.43	-.01	.08	-.15
Enrolled in other insurance	.08	.09	.01	.10	.07	-.03	.05	.17	.12	.09	.09	-.01	.04	-.09
Social preferences														
Inequality aversion 1 (disadv.)[b]	.18	.20	.03	.23	.12	-.12*	.15	.24	.09	.19	.26	.07	.04	.02
Inequality aversion 2 (adv.)[b]	.24	.32	.08	.30	.18	-.12	.23	.28	.05	.31	.39	.09	.07	.12
Fairness	.32	.34	.02	.32	.32	.00	.33	.28	-.05	.35	.30	-.04	.02	.03

	All			Random (assigned)			Random (preferred)			Choice				
	Random	Choice	Difference	Safe	Risky	Difference	Safe	Risky	Difference	Safe	Risky	Difference	Difference	Difference
	(1)	(2)	(2)-(1)	(3)	(4)	(4)-(3)	(5)	(6)	(6)-(5)	(7)	(8)	(8)-(7)	(7)-(5)	(8)-(6)
Trust	.13	.19	.07	.15	.10	-.05	.14	.07	-.07	.21	.13	-.08	.07	.06
Helpfulness	.30	.31	.01	.30	.30	.00	.35	.14	-.21***	.32	.26	-.05	-.04	.12
GSS Index[c]	.74	.84	.10	.77	.72	-.05	.82	.48	-.34**	.87	.70	-.18	.05	.21
B. Household characteristics														
No. of adults	2.71	2.60	-.11	2.90	2.52	-.38	2.59	3.07	.48	2.63	2.48	-.15	.04	-.59
No. of children	2.49	2.44	-.05	2.52	2.47	-.05	2.68	1.90	-.78	2.07	3.96	1.88	-.61	2.06
Monthly per capita (p.c.) income	2072	2958	886*	1929	2215	286	1904	2600	696	2954	2974	20	1050*	374
No. of other earners	.63	.73	.10	.68	.57	-.12	.57	.79	.22	.82	.35	-.47***	.25*	-.45**
No. of dependent HH members	3.48	2.96	-.52	3.43	3.52	.08	3.65	2.93	-.72	2.88	3.26	.38	-.76	.33
HH is in wealth index quartile[d]														
Poorest quartile	.13	.17	.04	.12	.15	.03	.14	.10	-.04	.18	.13	-.05	.04	.03
Poorer quartile	.36	.44	.08	.30	.42	.12	.38	.28	-.11	.44	.43	-.01	.06	.16
Richer quartile	.27	.22	-.05	.30	.23	-.07	.26	.28	.01	.21	.26	.05	-.05	-.01
Richest quartile	.24	.17	-.07	.28	.20	-.08	.21	.34	.14	.17	.17	.01	-.04	-.17
Health-related characteristics														
Health expenditures (p.c.)	1634	692	-941	2621	647	-1975	1596	1753	157	678	751	73	-918	-1001
Expected future health shock[e]	4.11	4.65	.54	4.27	3.95	-.32	4.18	3.90	-.28	4.43	5.57	1.13	.26	1.67
Foregone health care	.43	.51	.08	.35	.50	.15*	.44	.38	-.06	.52	.48	-.04	.08	.10
Prop. of HH members enrolled in HI	.25	.25	.00	.28	.21	-.08	.21	.38	.17*	.25	.24	-.02	.05	-.14
C. Experimental outcomes														
Risk preference[f]	3.42	3.59	.18	3.47	3.37	-.10	3.07	4.52	1.45***	2.99	6.09	3.10***	-.08	1.57**
Understanding of instructions[g]	1.22	1.23	.01	1.21	1.24	.03	1.21	1.26	.05	1.22	1.28	.07*	.01	.03
Observations	120	118		60	60		91	29		95	23			

Note: Statistically significant mean differences are marked as follows: *$p < .10$, **$p < .05$, ***$p < .01$.; [a]in the past 3 months; [b]Inequality aversion 1 (disadvantageous): Dummy which takes the value 1 if respondent thinks that others should not own much more than herself; Inequality aversion 2 (advantageous): dto. ...not own much less... (see Section 2.4); [c]No. of GSS questions positively answered (see Section 2.4); [d]The wealth index bases on the ownership of 11 household items (house, land, poultry, goats, sheep, cows/bullocks, refrigerator, radio, bicycle, motorcycle, car) and is constructed by using weights generated by principal component analysis; [e]Expected likelihood of unaffordable HH health expenditures within next year; [f]No. of lottery the subject has chosen out of 8 different lotteries with an increasing degree of riskiness, with 1(=safe income) to 8 (=riskiest lottery) (see Section 2.4); [g]Average number of trials needed to answer the comprehension test questions correctly.

B Appendix: The risk preference game

In the game, each subject was asked to choose one out of eight different lotteries (see Table A4, columns 2 to 4). The first alternative offers a certain amount of 320 Kenyan Shillings. The subsequent lotteries yield either a high (HEADS) or a low (TAILS) payoff with probability .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).

Table A4: Risk preference game: payoffs, expected values, risk and levels of risk aversion

Lottery number	Lottery	High payoff HEADS (p=.5)	Low payoff TAILS (p=.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	33.2
2	L	400	280	340	60	1.32 to 2.46	14.3
3	M	480	240	360	120	.81 to 1.32	9.2
4	N	560	200	380	180	.57 to .81	12.6
5	O	640	160	400	240	.44 to .57	2.5
6	P	720	120	420	300	.34 to .44	8.0
7	Q	800	80	440	360	0 to .34	11.3
8	R	880	0	440	440	-infinity to 0	8.8

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.

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). Strobl and Wunsch (2018) provide more details as well as a plausbility test of this measure.

C 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. 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.

Table A2: Means of variables by treatment and preferred project

	CHOICE	RANDOM (preferred)		CHOICE	RANDOM (preferred)	
	Safe	Safe	Difference	Risky	Risky	Difference
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)
A. Individual characteristics						
Age	31.18	31.04	.14	32.09	31.59	.50
Male	.32	.29	.02	.48	.35	.13
Schooling (years)	11.26	11.40	-.13	11.17	11.06	.12
8 years	.24	.31	-.07	.39	.35	.04
8 <years < 12	.13	.13	.00	.04	.12	-.07
12 <=years <15	.54	.42	.12	.43	.41	.02
years>=15	.09	.15	-.05	.13	.12	.01
Married	.46	.40	.07	.57	.47	.09
Household (HH) head	.63	.69	-.06	.78	.65	.14
Monthly income	4569	4478	90	6252	6135	117
Religion (1=christian)	.86	.83	.03	.78	.71	.08
Occupational status						
Employed	.13	.10	.02	.17	.18	.00
Self-employed	.25	.17	.09	.35	.24	.11
Unemployed	.46	.54	-.08	.39	.53	-.14
Other	.16	.19	-.03	.09	.06	.03
Ethnicity						
Kamba	.05	.08	-.03	.04	.06	-.02
Kikuyu	.04	.08	-.04	.09	.18	-.09
Kisii	.09	.13	-.03	.13	.12	.01
Luhya	.37	.35	.01	.30	.18	.13
Luo	.31	.23	.08	.35	.18	.17
Nubian	.11	.13	-.02	.09	.24	-.15
Other	.03	.00	.03*	.00	.06	-.06
Health-related characteristics						
Health problem	.32	.27	.04	.39	.35	.04
Chronical health problem	.18	.13	.05	.30	.24	.07
Visited health care provider	.43	.38	.06	.43	.35	.08
Health expenditures(Hexp)						
No health expenditures	.61	.58	.03	.52	.65	-.13
0 < Hexp <= 500	.15	.06	.08*	.26	.12	.14
500 < Hexp <=2500	.16	.23	-.07	.09	.12	-.03
Hexp > 2500	.08	.13	-.04	.13	.12	.01
Enrolled in health insurance (HI)	.44	.42	.03	.43	.47	-.04
Enrolled in other insurance	.09	.08	.01	.09	.18	-.09
Social preferences						
Inequality aversion 1 (disadv.)	.19	.17	.02	.26	.06	.20*
Inequality aversion 2 (adv.)	.31	.29	.01	.39	.24	.16
Fairness	.35	.33	.01	.30	.29	.01
Trust	.21	.17	.04	.13	.06	.07
Helpfulness	.32	.33	-.02	.26	.12	.14
GSS Index	.87	.83	.04	.70	.47	.23
B. Household characteristics						
No. of adults	2.63	2.75	-.12	2.48	2.76	-.29
No. of children	2.07	2.69	-.61	3.96	1.94	2.02
Monthly per capita (p.c.) income	2954	1964	990	2974	3171	-197
No. of other earners	.82	.63	.20	.35	.71	-.36
No. of dependent HH members	2.88	3.54	-.66	3.26	2.88	.38
HH is in wealth index quartile						
Poorest quartile	.18	.13	.05	.13	.12	.01
Poorer quartile	.44	.33	.11	.43	.35	.08
Richer quartile	.21	.27	-.06	.26	.18	.08
Richest quartile	.17	.27	-.10	.17	.35	-.18
Health-related characteristics						
Health expenditures (p.c.)	678	2614	-1936	751	1119	-368
Expected future health shock	4.43	4.46	-.03	5.57	4.18	1.39
Foregone health care	.52	.40	.12	.48	.53	-.05
Prop. of HH members enrolled in HI	.25	.25	.00	.24	.36	-.12
C. Experimental outcomes						
Risk preference	.89	.85	.04	.43	.65	-.21
Understanding of instructions	1.22	1.19	.03	1.28	1.24	.04
Observations	95	48		23	17	

Note: Statistically significant mean differences are marked as follows: $*p < .10$, $**p < .05$, $***p < .01$.

Table A3: Means of variables by year and preferred project

	All			Safe preferred			Risky preferred		
	2017^{+}	2018	Difference	2017^{+}	2018	Difference	2017^{+}	2018	Difference
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	(5)	(6)	(5)-(6)
A. Individual characteristics									
Age	3.5	3.2	.3	3.8	3.0	.8	29.4	3.9	-1.5
Male	.33	.32	.00	.32	.34	-.02	.34	.24	.11
Schooling (years)	11.5	12.7	-1.2***	11.8	12.9	-1.1**	1.8	11.9	-1.1
8 years	.28	.17	.11*	.23	.16	.07	.41	.19	.22*
8 <years < 12	.11	.12	-.01	.11	.11	.00	.10	.19	-.09
12 <=years <15	.46	.43	.02	.48	.44	.05	.38	.43	-.05
years>=15	.16	.27	-.12**	.18	.29	-.12*	.10	.19	-.09
Married	.45	.52	-.07	.43	.49	-.07	.52	.62	-.10
Religion (1=christian)	.84	.82	.02	.87	.82	.04	.76	.81	-.05
Household (HH) head	.64	.56	.09	.65	.60	.05	.62	.38	.24*
Monthly income	4844	4959	-115	4590	5054	-464	5641	4576	1065
Occupational status									
Employed	.13	.14	-.02	.09	.13	-.04	.24	.19	.05
Self-employed	.19	.27	-.08	.20	.26	-.06	.17	.33	-.16
Unemployed	.50	.39	.11*	.52	.42	.09	.45	.24	.21
Other	.18	.20	-.01	.20	.19	.01	.14	.24	-.10
Ethnicity									
Kamba	.07	.08	-.01	.08	.07	.01	.03	.10	-.06
Kikuyu	.07	.07	.00	.05	.07	-.02	.10	.05	.06
Kisii	.13	.05	.08**	.12	.04	.09**	.14	.10	.04
Luhya	.35	.36	-.01	.40	.36	.03	.21	.33	-.13
Luo	.27	.32	-.05	.25	.32	-.06	.31	.33	-.02
Nubian	.11	.08	.03	.09	.07	.02	.17	.10	.08
Other	.02	.06	-.04	.01	.07	-.06**	.03	.00	.03
Health-related characteristics									
Health problem	.33	.42	-.10	.33	.41	-.08	.31	.48	-.17
Chronical health problem	.13	.15	-.03	.09	.15	-.07	.24	.14	.10
Visited health care provider	.43	.44	-.01	.45	.46	-.01	.38	.38	.00
Health expenditures	2497	559	1938	3104	640	2465	590	231	359
Health expend.= 0	.57	.66	-.09	.56	.64	-.07	.59	.76	-.18
Enrolled in health insurance (HI)	.42	.32	.10	.36	.31	.06	.59	.38	.21
Enrolled in other insurance	.08	.05	.04	.05	.04	.02	.17	.10	.08
Social preferences									
Inequality aversion 1 (disadv.)	.18	.31	-.14**	.15	.28	-.13**	.24	.43	-.19
Inequality aversion 2 (adv.)	.24	.30	-.06	.23	.31	-.08	.28	.29	-.01
Fairness	.32	.24	.08	.33	.24	.09	.28	.24	.04
Trust	.13	.07	.06	.14	.08	.06	.07	.00	.07
Helpfulness	.30	.32	-.02	.35	.27	.08	.14	.52	-.39***
GSS Index	.74	.62	.12	.82	.59	.24**	.48	.76	-.28
B. Household characteristics									
No. of adults	2.71	3.20	-.49	2.59	3.20	-.61	3.07	3.19	-.12
No. of children	2.49	2.20	.29	2.68	2.09	.59	1.90	2.62	-.72**
Monthly per capita (p.c.) income	2072	2101	-30	1904	2242	-339	2600	1532	1068
No. of other earners	.63	.69	-.06	.57	.71	-.13	.79	.62	.17
No. of dependent HH members	3.48	2.57	.91*	3.65	2.51	1.14*	2.93	2.81	.12
HH is in wealth index quartile									
Poorest quartile	.13	.12	.01	.14	.12	.03	.10	.14	-.04
Poorer quartile	.36	.15	.21***	.38	.14	.24***	.28	.19	.09
Richer quartile	.27	.35	-.08	.26	.40	-.14**	.28	.14	.13
Richest quartile	.24	.38	-.14**	.21	.34	-.13**	.34	.52	-.18
Health-related characteristics									
Health expenditures (p.c.)	1634	520	1114	1596	524	1072	1753	501	1252
Expected future health shock	4.11	4.07	.04	4.18	4.53	-.35	3.90	2.19	1.71*
Foregone health care	.43	.46	-.04	.44	.47	-.03	.38	.43	-.05
Prop. of HH members enrolled in HI	.25	1.56	-1.31	.21	.26	-.05	.38	6.81	-6.43
C. Experimental outcomes									
Risk preference	3.42	3.57	-.15	3.07	3.29	-.23	4.52	4.67	-.15
Understanding of instructions	1.22	1.13	.10***	1.21	1.12	.09***	1.26	1.15	.11**
Observations	120	106		91	85		29	21	

Note: Statistically significant mean differences are marked as follows: *$p < .10$, **$p < .05$, ***$p < .01$. $^{+}$RANDOM only.

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.

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Instructions for Game 2

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

<u>Project Income:</u>

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

[Screenshot 1]

Game 2

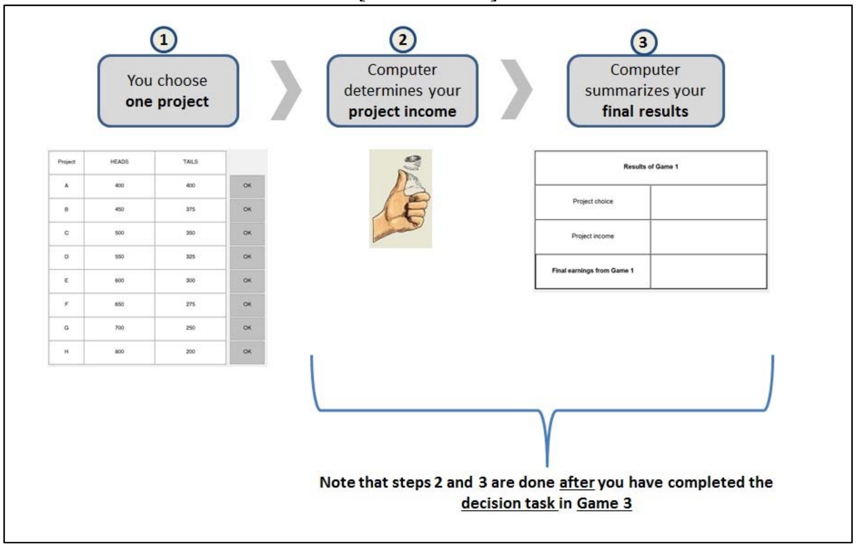
Project	HEADS	TAILS	
K	320	320	OK
L	400	280	OK
M	480	240	OK
N	560	200	OK
O	640	160	OK
P	720	120	OK
Q	800	80	OK
R	880	0	OK

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

<u>Summary:</u>

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

[Screenshot 2]



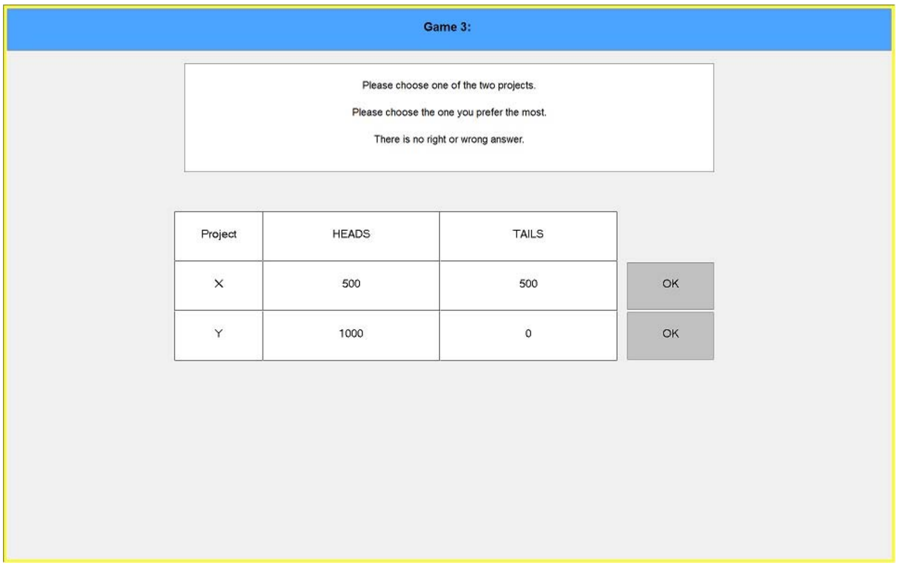
Instructions for Game 3

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

1) Project Choice

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

[Screenshot 3]



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

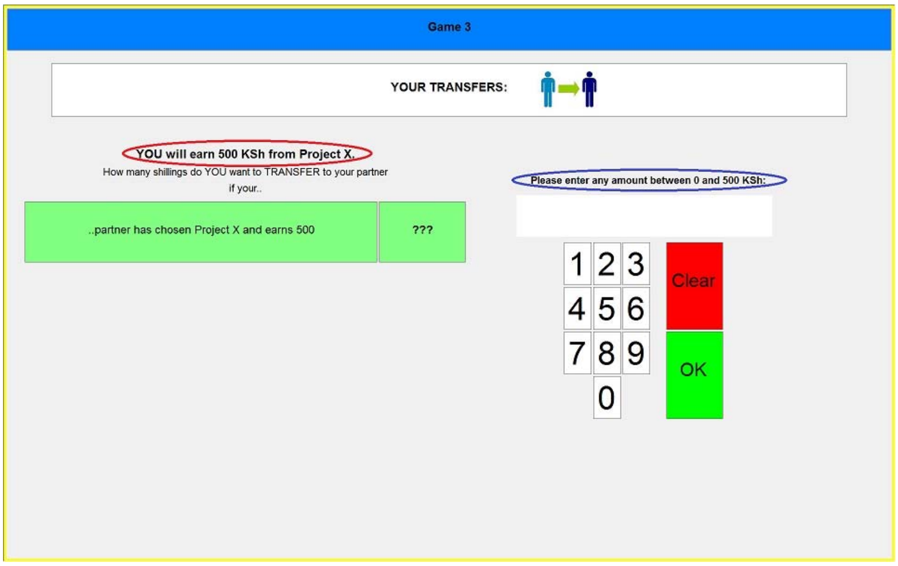
In this game, you can give some of your project income to your partner if you want to. Please note that you can give some of your income to your partner, but you do not have to. The amount that you decide to transfer to your partner will be deducted from your project income and added to your partner's 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 2 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 500 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 500 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 500 shillings to your partner if your partner has also chosen Project X and earns 500 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 500 shillings in this example.

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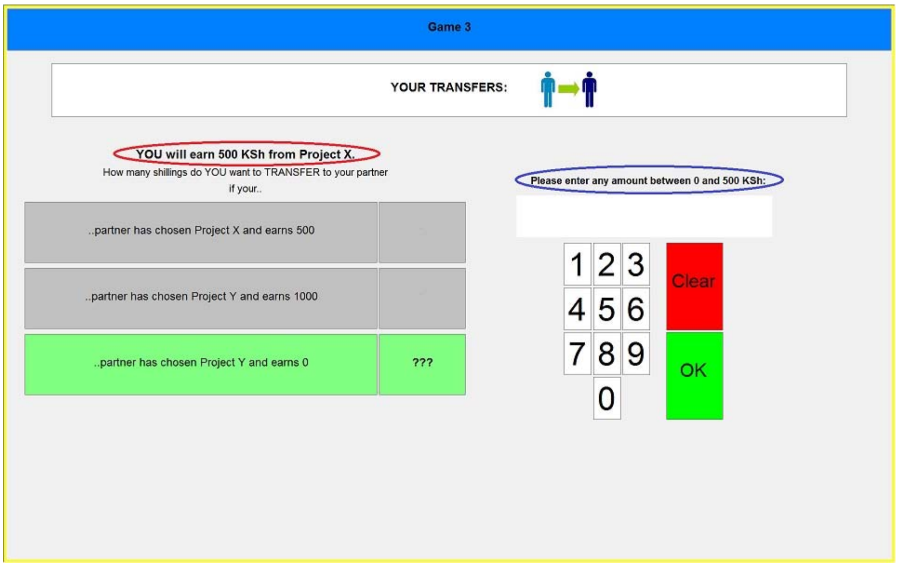
[Screenshot 5]



Similarly, the second and third questions ask what amount you would like to transfer to your partner if you earn 500 shillings and your partner has chosen Project Y and earns 1000 or 0 shillings. For each question, you can enter any amount between 0 and your full project income, that is 500 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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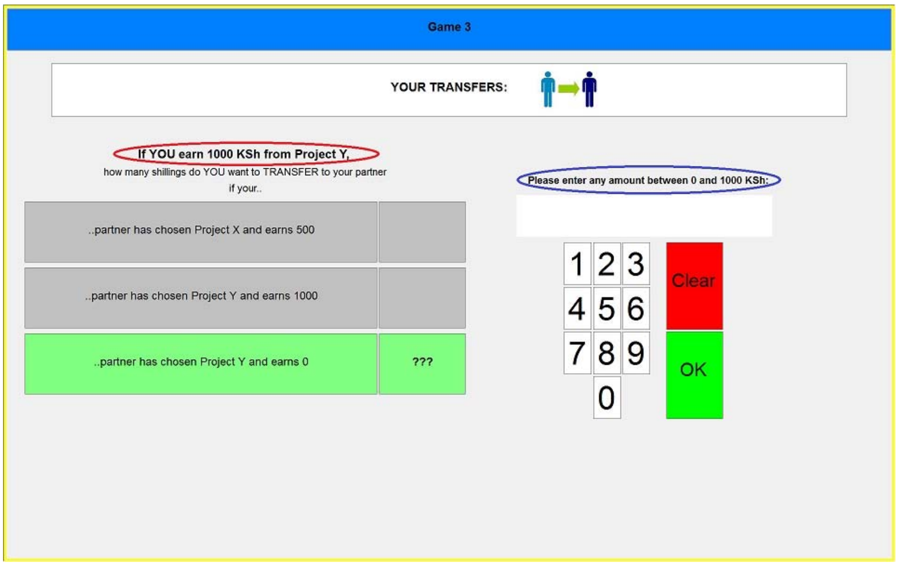
Example 2 – You choose Project Y

[Screenshot 6]



If you have chosen Project Y, you will earn 1000 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 1000 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 1000 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 500, 1000 and 0 shillings. Again, you can enter any amount between 0 and your full project income, that is 1000 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.

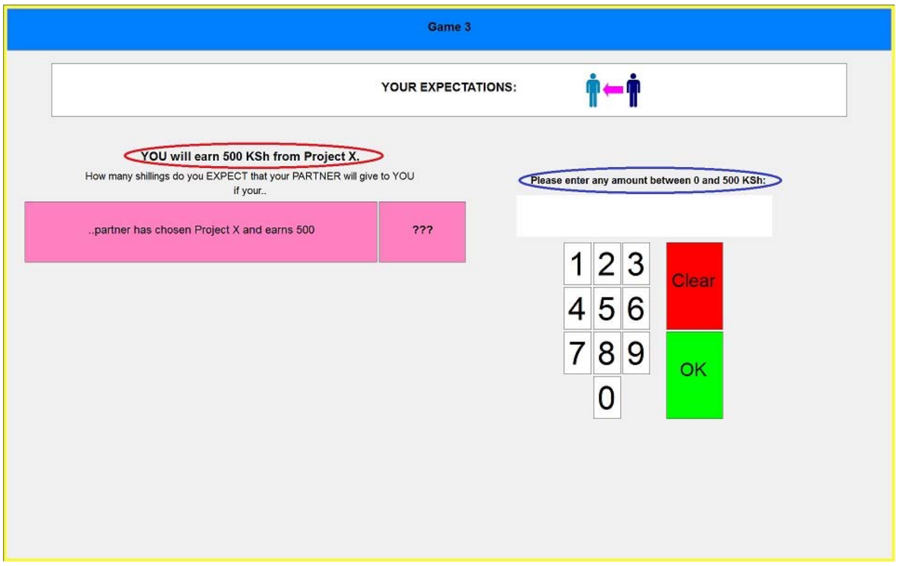
Please note that you and your partner make the transfer decisions simultaneously. Please also note that you will not know how much your partner has decided to give to you until the end of Game 3. Also, your partner will not know your transfer decisions until the end of Game 3.

4) Expectation about the transfer you receive

After you have entered your three transfer decisions, we will ask you to estimate how much money your partner will transfer to you.

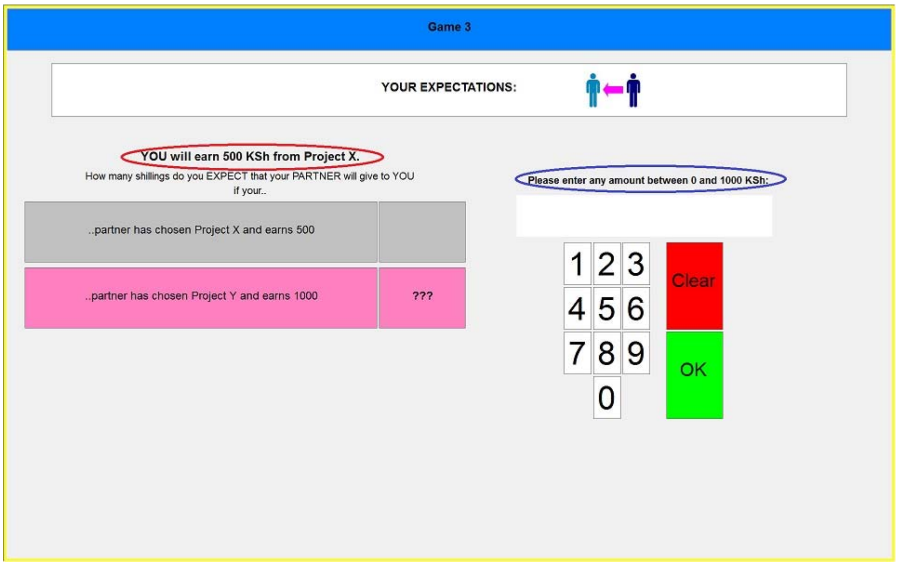
Example 1 – You have chosen Project X

[Screenshot 7]



This screen appears if you have chosen Project X. With Project X, you will earn 500 shillings, regardless of whether the coin lands on heads or on tails. The first question (in pink) asks how much money you expect to receive from your partner in the case that your partner has also chosen Project X and also earns 500 shillings. You can enter any amount between 0 and the full income of your partner, that is 500 shillings in this case.

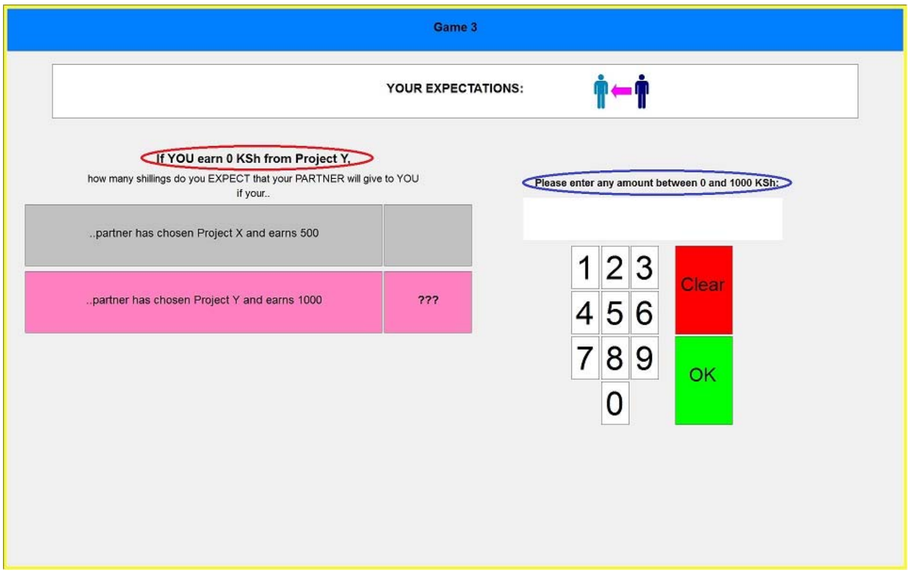
[Screenshot 8]



Similarly, the second question asks how much money you expect to receive from your partner in the case that you earn 500 shillings and your partner has chosen Project Y and earns 1000 shillings. You can enter any amount between 0 and the full income of <u>your partner</u>, that is 1000 shillings in this case. Please note that your partner CANNOT transfer money to you if he/she has chosen Project Y and earns 0 shillings, so we do not ask you about your expectations in this case.

Example 2 – You have chosen Project Y

[Screenshot 9]



Similarly, if you have chosen Project Y, you will earn either 1000 shillings or 0 shillings, depending on the result of your coin flip. We will, however, only ask you to enter how much money you expect to receive from your partner if YOU earn 0 shillings and YOUR PARTNER earns 500 shillings or 1000 shillings.

Please note that your partner will never be informed about your expectations. Also, you will never be informed about the expectations of your partner.

5) Coin flip

After you have entered the amounts that you expect to receive in transfers, 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.

<u>Summary:</u>

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

[Screenshot 10]