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## Playing with the Social Network: Social Cohesion in Resettled and Non-Resettled Communities in Cambodia

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**PLAYING WITH THE SOCIAL NETWORK: SOCIAL COHESION IN RESETTLED  
AND NON-RESETTLED COMMUNITIES IN CAMBODIA**

Simone Gobien<sup>\*a</sup> Björn Vollan<sup>b</sup>

ABSTRACT

Mutual aid among villagers in developing countries is often the only means of insuring against economic shocks. We use “lab-in-the-field experiments” in Cambodian villages to study social cohesion in established and newly resettled communities. Both communities are part of a land reform project. The project participants all signed up voluntarily, and their socio-demographic attributes and pre-existing network ties are similar. We use a version of the “solidarity game” to identify the effect of voluntary resettlement on willingness to help fellow villagers after an income shock. We find a sizeable reduction in willingness to help others. Resettled players transfer on average between 49% and 75% less money than non-resettled players. The effect remains large and significant after controlling for personal network and when controlling for differences in transfer expectations. The costs of voluntary resettlement, not only monetary but also social, seem significantly higher than is commonly assumed by development planners.

*Keywords:* Voluntary resettlement, Social cohesion, Risk-sharing networks, “Lab-in-the-field experiment”, Cambodia

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## 1. INTRODUCTION

Land reforms in developing countries have tremendous economic and social effects on the people. Most studies focus on monetary issues even if resettlement of people dramatically changes their social environment. Those studies which do examine the social aspects of resettlement deal mostly with interventions where resettlement is involuntary. Lam and Paul (2013) identify a vicious cycle of poverty for resettled household consisting of poor harvest coupled with damaged informal risk-coping networks in Nepal, Berg (1999) concentrates on the loss of traditional religious systems in Nigeria, or Abutte (2000) describes the difficult process of readjustment to the new resettlement context in Ethiopia after the erosion of long-established social structures. The few available studies of social consequences of voluntary resettlement, concentrate mainly on redistributive land reform in Zimbabwe, suggesting that negative effects may arise even 20 years after voluntary resettlement (Barr, 2003; Barr, Dekker, & Fafchamps, 2010; Dekker, 2004). However, these studies lack data before resettlement and thus cannot rule out that their effect is driven by selection instead of resettlement. It is possible that in Zimbabwe especially those favoring a certain political party or those willing to use violence were resettled. In this study we further analyze and quantify the social and economic loss caused by voluntary resettlement within a land reform program controlling for endogenous project participation. Similar to the previous studies we measure “social cohesion” by implementing a “lab-in-the-field” experiment. Our participants are recruited from a land distribution project in rural Cambodia. We compare voluntarily resettled farmers with beneficiaries who stay in their established villages (non-resettled farmers).<sup>1</sup> We complement our results with survey evidence on income and social capital before and after resettlement.

The negative consequences of leaving one’s birthplace may be underestimated both by the people who are resettled and by the project staff. Geographic proximity is one of the main determinants of social networks (Fafchamps & Gubert, 2007; Fafchamps & Lund, 2003). Dekker (2004) finds evidence that while non-resettled households in Zimbabwe rely on their network and solidarity in the village, voluntarily resettled households are more likely to rely on individual risk-coping strategies.<sup>2</sup> Even when well intended and well planned, resettlement often requires people to leave a well-functioning, cohesive community that has served them in many ways. Among the many direct benefits of a functioning community or neighborhood are risk-coping strategies such as easier access to credit and mutual aid (Attanasio, Barr, Cardenas, Genicot, &

Meghir, 2012; Dinh, Dufhues, & Buchenrieder, 2012; Okten & Osili, 2004), and there are indirect benefits such as good institutions (Easterly, Ritzen, & Woolcock, 2006), low levels of violence (Sampson, Raudenbush, & Earls, 1997), provision of local public goods through collective action (Narayan & Pritchett, 1999), more accountable politicians (Tsai, 2007), and greater capacity to enforce norms (Miguel & Gugerty, 2005).<sup>3</sup>

While some other authors rely on self-reported information on trust and solidarity to measure social cohesion (Sampson et al., 1997), we believe that incentive based measures give a more reliable picture. The seminal study by Barr (2003) explores the implications of resettlement on trust in Zimbabwe using a standard trust experiment. Her findings show that resettled players trust each other significantly less than non-resettled players even 20 years after resettlement, and that the players' responsiveness to expected trustworthiness is lower in resettled communities. Barr (2003) argues that this lower level of trust is mainly the result of missing altruism.<sup>4</sup> We broaden the view by implementing a version of a solidarity experiment (Selten & Ockenfels, 1998) which captures transfers motivated by a set of pro-social concerns including altruism and inequity aversion. The experimental game consists of two stages in which participants interact only with randomly chosen players from the same village. In the first stage all participants play a risk game. Then winners of the risk game make a one-shot decision on whether to transfer payments to anonymous losers in their group or not. This experimental set-up makes it possible to reduce disparities by equalizing game outcomes through the transfer of money. Thus, the set-up of the solidarity experiment resembles risk-sharing situations where pro-social preferences are relevant, which may be more pronounced in non-resettled villages where people have a stronger feeling of belongingness and commitment. The solidarity experiment further includes elements of trust, since transfers depend on expectations about the solidarity of others (Selten & Ockenfels, 1998), but it does not have the strategic investment considerations included in the trust game which provoke selfish motives. Interactions are between anonymous villagers, there are no future interactions, and monetary transfers are not revealed. Thus, our experiment eliminates the possibility of reciprocal risk-sharing and captures a village norm of social cohesion expressed in the willingness to transfer payments to anonymous villagers.<sup>5</sup>

In our study, farmers in the control group (non-resettled players) received only agricultural land and still live in their village of origin, whereas farmers in the treatment group (resettled players) received agricultural *and* residential land. The resettled players moved to a

newly founded village about one year prior to our behavioral experiment, whereas non-resettled farmers stay in their village of origin and have to commute to their new plots. The new village is composed only of project farmers who come from different villages in the region. The agricultural land is of a similar size for both groups.

Our study provides new evidence on the social cost of voluntary resettlement. It differs from Barr (2003) in several ways. Firstly, we measure rather short-term effects of resettlement. This is relevant since agricultural risk is highest immediately after obtaining agricultural land, when farmers are still inexperienced (Lam and Paul, 2013). Secondly, we use an experimental design that mimics real-world risk-sharing situations and measures willingness to transfer resources that is motivated by pro-social preferences as a proxy for social cohesion on the village level. Thirdly, we measure and control for risk aversion which might influence the settlement decision. Fourthly, and most importantly, our treatment and control groups are closely homogeneous samples due to the enforcement of eligibility criteria. We also exploit ex ante data showing that the groups did not differ in social embeddedness in their village of origin. Lastly, we enrich our experimental results with survey data on income before and after resettlement to provide evidence of the welfare effects of the land distribution program.

We find a sizeable reduction in the willingness to help others. Resettled players transfer on average between 49% and 75% less money than non-resettled players. This effect remains large and significant after controlling for personal network and when controlling for differences in transfer expectations. At the same time, there is a greater need for support in the new village. Resettled farmers in the new village made 36% less income, (but since they received subsidies their overall income was 20% lower). Since both groups obtained land of a similar size in the same area, the income differences are not due to weather effects or different soil productivity. The costs of voluntary resettlement, not only monetary but especially social, seem significantly higher than is commonly assumed by development planners. People who have been resettled will therefore need not only longer and more intensive external support but inevitably also adequate micro-insurance and better access to credit.

## 2. BACKGROUND INFORMATION

Land scarcity, environmental degradation and unequal distribution of productive land prevent the economic development of the many people living in rural areas who rely on

agriculture as their main source of income. In Cambodia (our study region) more than 50% of the rural population are land-poor, with less than half a hectare of land, and about 20% are landless (MoP & UNDP, 2007).<sup>6</sup> These land-poor and landless rural people constitute the poorest and most vulnerable part of the population.

(a) Resettlement context: The LASED project

The experiment was carried out in the context of the Land Allocation for Social and Economic Development (LASED) project. This pilot project of the Royal Government of Cambodia, supported by the German Agency for International Co-operation (GIZ) and the World Bank, allocates one to three hectares of agricultural land to land-poor and landless people and supports them in starting to farm on the land.<sup>7</sup> The project is most advanced in Kratie Province, where we carried out our research. Applicants could apply for residential or agricultural land parcels. It was also possible to apply for both types of land. All those who received residential land migrated permanently to a newly founded village. All the agricultural plots are around this new village. Non-resettled farmers have to commute to their agricultural plots. The project beneficiaries are the neediest people in the communities: to qualify they had to be landless or land-poor (i.e. having less than half a hectare of agricultural land). As there was more demand for both agricultural and residential land than could be supplied, applicants were selected according to the degree of neediness.<sup>8</sup>

Agricultural and residential land plots were allocated by lottery to the selected applicants. In Kratie Province, land had been distributed to 525 households by the end of 2008 as a pilot project. Land recipients obtained either only agricultural land (44%), agricultural and residential land (52%) or only residential land (four %). We excluded households who received only residential land from our sample as conclusions about this group of 20 households are not reliable. At the time of writing, around 10,000 hectares had been allocated to approximately 5,000 households.

Before the allocation of land by the project, there was no significant difference in average annual household income between the two groups in our study. We refer to these two groups as the “non-resettled” group: those who were already resident in the established villages and were given agricultural land by the project, and the “resettled” group: those who were given both

residential and agricultural land by the project and were resettled in the new village near the established villages. (See Table 1.)

(b) Some evidence on ex ante differences of project members

The advantage of this set-up for our experiment is that our two groups have many similarities: they come from adjacent villages, have obtained agricultural land of a similar size and thus similar potential income, have a similar ex ante status of poverty, and are similarly motivated to farm as they all signed up voluntarily for a development project to obtain agricultural land.<sup>9</sup> A possible difference between our samples could be their risk preferences. Applying for residential land and ultimately moving to the LASED village is a voluntary migration decision. The individual migration decision is based on a personal evaluation of the characteristics of the area of origin, the area of destination, and intervening obstacles such as travel costs, physical barriers, or immigration laws (Lee, 1966). One major migration determinant is personal attitude towards risk (Barsky, Juster, Kimball, & Shapiro, 1997; Heitmueller, 2005; Jaeger et al., 2007; Massey et al., 1993). Uncertainty about the area of destination, especially uncertainty about potential profits, hinders the migration of risk-averse individuals.<sup>10</sup> We therefore measure risk-taking in our experiment and control for differences in all subsequent multivariate analyses of willingness to transfer money.

Most importantly, social capital at the place of origin may influence the decision to resettle. Haug (2008) discusses the possible effects of social ties on the decision to migrate and finds that these ties can both reduce the likelihood of migration (“affinity hypothesis”) and increase it (“conflict hypothesis” and “encouraging hypothesis”). We do not have completely reliable information on the social capital of our study group before resettlement. The available data originate from a random survey conducted with 84 project households in 2008 before the allocation of land by the project and retrospective data from 2010 which provide information on the situation of 106 project households before resettlement (Table 1). In both samples around 55% of the households received both residential and agricultural land and 45% received only agricultural land. We use membership in formal groups, participation in prominent social events (number of wedding celebrations and frequency of visiting the pagoda), and availability of informal credit, which is based on trust and a reputation for being trustworthy, as proxy variables for social capital. High values point in the direction of the “affinity hypothesis” and low values

in the direction of the “conflict hypothesis”. Tests for differences in means between the resettled and non-resettled groups remain insignificant for all social variables. There is also no significant difference in terms of income and savings, size of the house, nutrient provision of the household members, household size, education, material status and age of the household head, as well as different relevant household assets in 2008.<sup>11</sup> There only seems to be more female headed households in the group of non-resettled households.

**[INSERT TABLE 1 HERE]**

In our data we find no differences between our two groups in social ties or social cohesion. Of course, unobserved factors correlated with both resettlement and willingness to transfer money, such as a lack of prospects in the original village, variations in self-confidence, and different ways of dealing with unknown situations and people, could influence our results. Most likely there are many reasons why people migrate and these reasons may differ for each individual. Hence, we do not expect a consistent bias that influences our results. A further robustness test is to estimate a difference-in-difference regression that mimics random assignment and to compare the obtained coefficient to the resettlement coefficient of simple ex post estimation. A significant different coefficient highlights potential ex ante differences. Although we cannot do this for our experimental measure of willingness-to-transfer, we can test for potential bias in related variables of social ties and income. Tables A.1 and A.2 in the appendix show that the coefficients of a difference-in-difference estimation and a “naïve” ex post estimation for 2010 do not differ for a range of relevant variables.<sup>12</sup>

### 3. METHODS

Those who had received only agricultural land played the game with other project members from their old community, and those who had received both agricultural and residential land played it with members of their new community. In both cases the participant pool was restricted to project members.



(a) The risk and solidarity experiments

We conducted a set of three independent experiments. The first is a simple risk game where participants can choose between three different options. The second is the same risk game followed by a solidarity game in the second stage, consisting of transferring money in anonymous groups of three. The third game replaces the random winning mechanism in the risk game with a task involving skill and is again followed by the solidarity game. (See Table 2.)

The participants were aware of whether they had won or lost in one of the three risk games, but we did not reveal transfer decisions. We informed participants that after all the games had been played we would randomly select one of the three games and pay out the earnings for that game. Earnings were paid out privately after a questionnaire had been completed. On average, a player earned 4,020 riel (KHR), which is about one USD and equals the salary for half a day's wage labor. We also offered a free meal instead of a show-up fee.<sup>13</sup>

Our risk game is adapted from Binswanger (1980; 1981).<sup>14</sup> The risk lottery follows an ordered lottery selection design (see Table 2). We reduced the risk choices to three lotteries instead of eight. This was necessary to reduce complexity once the risk game was combined with the strategy method in the solidarity game. In the event of losing, the payoff is zero to activate pro-social motives in the following stage. The outcome of the gamble is decided by the participant rolling a die. Option A provides a small but secure payoff (0.50 USD). Options B and C offer a higher expected payoff than option A, but also incorporate the risk of getting zero payoff. Option B has a winning probability of  $2/3$  and appeals to players who will accept a moderate risk, whereas option C with a winning probability of  $1/3$  is most attractive for risk-loving players willing to venture a higher risk.

We were interested in measuring social cohesion at the village level independent of reputation and reciprocal network ties. Therefore we implemented an anonymous one-shot solidarity experiment in the second stage of game two. Decisions to transfer money were taken after the risk choice only by winners of the game. We believe that this increases the validity of the transfers, since players already knew that transfers were going to be made in the event of there being losers in their three person group. Players were asked to make transfer decisions for different possible combinations of

- a) the number of players with zero payoff in the player's group (one or two) and
- b) the risk choice of these players (B or C).

This leads to a total number of six decisions per player (two transfer decisions with one loser in the group, and four transfer decisions with two losers in the group). To avoid strategic giving, players were not told about other players' transfer decisions.

**[INSERT TABLE 2 HERE]**

In the third game, we replaced the random winning mechanism of the risk game with a skilled task to enhance external validity. Following Gneezy, Leonard and List (2009), we set the task of throwing a ball into a bucket.<sup>15</sup> After we had pre-tested the task, we set winning probabilities and the resulting payoffs equal to those of the risk game (option A: at least zero out of 10, option B: at least four out of 10, option C: at least seven out of 10). Hence, overall changes in risk behavior and transfer payments can be attributed to the change from a random lottery to a test of skill. Again, the winners of the skilled task subsequently played the solidarity game.

#### (b) Experimental procedure and participants

Experimental sessions were carried out in April and May 2010 in four randomly chosen non-resettled project villages and in the newly founded village. In total, we conducted 16 sessions (two sessions in each of the four non-resettled village and eight sessions in the resettled village) with 225 participants (127 resettled players and 98 non-resettled players). Participants in the experiment were randomly chosen from a complete list of project participants (around 35% of all project households). Household members who were at least 18 years old were eligible for the experiment. Only one person per household could take part in each session and a maximum of two players per household were allowed to participate in total. A few days in advance, the village chief informed the people that they could participate in an activity in which they could earn money.<sup>16</sup>

Sessions were conducted by the same research team in each case. Instructions were read out loud to all players in the common room of the village community centre. All decisions took place in private. We illustrated the risk decision during the instruction by showing posters and reading out examples for gambling choices. Every player practiced throwing the die three times. Each time a different gambling choice was assumed and the players verified that they understood the outcomes of the game. When they were making their decisions, posters of the different gambling choices were available to the players. In games two and game three we explained money transfer decisions in the same way: firstly, in the common room with examples and posters for different numbers and types of losers and secondly in private with test questions about the solidarity game.

As Table 3 shows, all participants played the risk game in game one and game two (N= 225). The transfer decisions in the second stage of game two were only recorded for those players who won the risk game in the first stage of game two (N= 126, 76 resettled and 50 non-resettled players). Each player made six transfer decisions, leading to 756 observations in game two. For game three, we randomly determined half of the sessions for each group that played the game involving a skilled task (N= 116). Finally 64 subjects won the skills game and made transfer decisions (34 resettled and 30 non-resettled players).

**[INSERT TABLE 3 HERE]**

Although we chose participants randomly from a homogeneous group, there was a small difference between the two groups in terms of age, which we control for in our regression (Table 4). There are also more households who have some savings in the non-resettled group and household who have more than 50 USD credit in the resettled group, which might be a consequence of resettlement, since resettled farmers have higher investment needs. Furthermore, as expected, the non-resettled players reported on average significantly more friends and family members than the resettled players in the experimental sessions.<sup>17</sup> However, this difference is not very large (the average percentage of friends in the session is 10% for resettled players and 20% for non-resettled players). Also, 30% of players in both samples reported having no friends

taking part in the session. In our analysis we control for the network a person had within the experimental session.

**[INSERT TABLE 4 HERE]**

### (c) Hypotheses

Selten and Ockenfels (1998) find that what they call “giving behavior” in a solidarity game depends on one’s expectations about the giving behavior of others. As our groups are anonymous, expectations about transfers at the village level are relevant. Coming into a new community leads to uncertainties about other people’s behavior. Moreover, as solidarity can be unconditional and based on feelings of togetherness and cohesion, resettlement may have an effect on transfer sending beyond rational expectations. We expect a negative effect of resettlement on solidarity as a result of i) lower expectations that others would have helped, ii) lower desire to support fellow villagers stemming from lower social cohesion, and iii) fewer family members and friends taking part in the session.

In the third game, players could actively influence the outcome of the game, which induced a stronger feeling of being entitled to the money. As Cherry, Frykblom and Shogren (2002) and Hoffman, McCabe, Shachat and Smith (1994) show for an ultimatum game, subjects transfer substantially lower amounts if they earn their winnings or earn the right to be the first mover. This effect is in part attributed to a difference in performance or “status” (Cox, Friedman, & Gjerstad, 2007), “mental accounting” (Cherry & Shogren, 2008), or a reduction of the supply effect in experimental economics (Carpenter, Liati, & Vickery, 2010). Furthermore, losers in the skilled task are responsible for their failure because they misjudged their skills. According to Trhal and Radermacher (2009), self-inflicted neediness reduces solidarity payments. Therefore, when it comes to the skilled game we expect a reduction of transfers in both resettled and non-resettled groups and maybe even an increase in the difference between resettled and non-resettled players.

#### 4. RESULTS

##### (a) Risk

Our analysis of the risk choices in game one confirms our expectation that resettled players would choose on average riskier gambles than non-resettled players.<sup>18</sup> We find a significant difference in means between the risk choices of the players in game one at the five % significance level (resettled players: 2.35, non-resettled players: 2.19).<sup>19</sup> Figure 1 shows that in the group of non-resettled players the mode lies with 58% of all players at option B, whereas in the group of resettled players the mode lies with 48% at option C. Anyhow, these differences disappear in game two and game three when risk choices are adapted to the experience in game one and the possibility of transfer sending.<sup>20</sup>

**[INSERT FIGURE 1 HERE]**

##### (b) Transfer decisions

In game two, and game three with the skilled task, winners of the risk game could transfer money to losers of the risk game in anonymous three-player groups. Table 5 shows that the mean transfers of resettled players are significantly lower. The resettled players transfer on average 38% less money than non-resettled players. Transfer sending decreases with the skill driven winning mechanism.<sup>21</sup> However, the decrease is larger in the resettled village (22%) than in the non-resettled villages (11%). Thus, individualistic motives of “earning” and “skill” are more important in the resettled village, while transfers are more unconditional in the non-resettled villages. These findings were confirmed through qualitative interviews after the experiment. Resettled players reported that norms of sharing are not present in the new community; as a resettled participant remarked, “Giving nothing is just the way people behave in this village” (April 4, 2010, session one).

**[INSERT TABLE 5 HERE]**

When we analyze transfers with respect to how much money a potential sender has at hand (whether the player chose option A or won option B or C) and how high a risk the potential receiver(s) took (lost option B or option C), we observe the following patterns (see Table C.1 in the appendix). Firstly, transfer senders sent lower transfers per person to two losers in their group than to one loser (except the few C-senders who transferred similar amounts no matter whether one or two other players lost). Secondly, even though absolute transfers increased with the available budget, A-senders were willing to give, with an average of 14.19%, the highest proportion of their earning (283.76 KHR), followed by B-senders (9.52%, 628.26 KHR) and C-senders (6.94%, 1,250 KHR).<sup>22</sup> Higher relative contributions of less wealthy people are also found in public good games (Buckley & Croson, 2006; Hofmeyr, Burns, & Visser, 2007).<sup>23</sup> Thirdly, there is no evidence that senders discriminate over the risk choice of the loser.

Figure 2 shows the cumulated density function of potential transfers to one B-loser for resettled and non-resettled players. The curve for the resettled players lies entirely above that for the non-resettled players. Hence, for the whole distribution of transfers, resettled players were more likely to receive lower transfers. In the non-resettled group the probability of getting no transfers is less than 10%, whereas for the resettled players it is close to 20%. Taking a transfer of 1,000 KHR as an example, only 14% of the resettled players received a higher transfer. The proportion of players receiving a transfer of more than 1,000 KHR increases to 41% in the group of non-resettled players.

**[INSERT FIGURE 2 HERE]**

#### (c) Transfer differences contingent on risk choice and expectations

Since transfer decisions depend on own and others' risk choices, simple descriptive analysis can be misleading. We estimate solidarity conditional on a specific risk choice, to control for potentially higher transfers made by risk-loving individuals, by including dummy variables for the type of sender and the type of receiver of the transfer.<sup>24</sup> We also control for the payoffs in game one since they might generate some unwanted learning effects. We estimate

Tobit regressions as our latent variable (willingness to support) is expressed by the left censored variable transfer payments with 24% of all observations censored at zero. Table 6 contains the results of Tobit regressions on the six transfer choices that every winner of a risk game made for all possible types of losers in that person's group. Individual socio-demographic controls and session size are included in all regressions.

We focus on the transfer difference between resettled and non-resettled players. We start by analyzing only the transfer decisions in game two (regression (1), N= 126, observations= 756). Here, the resettlement dummy is negative and significant at the five % level. In a second step, we estimate a random effects Tobit regression which also includes the transfer decisions in game three with the skilled task (regression (2), N= 156, observations= 1,140). The resettlement dummy increases in magnitude and remains negatively significant at the one % level.

To separate the effects of social cohesion from reciprocal motives, we include transfer expectations in regression (3) (N= 112, observations= 810).<sup>25</sup> These have a significant positive influence on transfers, confirming the results of Selten and Ockenfels (1998). The more interesting finding, however, is that resettlement remains negatively significant. That is, lower transfers are driven not only by lower expectations about the support of others, but also by a preference for not helping people in the resettled village.

In regression (4) (N= 156, observations= 1,140) we exclude the controls for the network of family and friends in the session. The negative coefficient of the resettlement dummy increases, as it now also accounts for the loss of social relations in the new village (compare regressions (2) and (4)). The increase in the coefficient is merely -44.8 KHR. Thus, we believe that the anonymity of our experiment cancelled out the effect of familiarity in the session. As a robustness check, we estimate the average treatment effect on the treated using the relative number of family members and friends with regard to session size as matching variables to estimate the propensity score (Table C.4 in the appendix). With all different matching methods we still find a significant negative coefficient of the resettlement dummy ranging from -163 to -391 KHR. These results show that unconditional giving is driven not so much by the presence of a personal social network as by social cohesion at the village level. Furthermore, the relatively small influence of number of family members and friends in the session suggests that anonymity,

independence of games and no communication successfully removed personalized trust motivations from the experiment.

Lastly, we estimate transfers without controlling for the risk choices of senders and receivers, which gives us the total effect of voluntary resettlement taking into account that there is more risk taking among resettled participants (regression (5),  $N=156$ , observations= 1,140). Since the resettled players adjusted their risk choice after game one we find hardly any differences between regressions (2) and (5).

**[INSERT TABLE 6 HERE]**

Applying regression analysis, taking the risk choice and variation in control variables into account, the resettlement dummy is significant in all the specifications with a magnitude from -384.9 KHR to -597.5 KHR. Thus, resettled players transfer between 49% and 75% lower amounts than non-resettled players in game two (792.3 KHR). The difference between the two groups is larger than that found by a simple descriptive analysis (38%). Regressions (2) to (5) show a significant negative coefficient for the skilled task, which confirms our hypothesis that effort and accountability for the game outcome reduces transfers.<sup>26</sup> The magnitude of this coefficient with -100.9 KHR in regression (2) is more than five times smaller than the resettlement effect.<sup>27</sup> It was interesting to note that households that have some savings transfer significantly lower amounts in all regressions. This is in line with findings that individuals with financial resources face heavy demands from relatives and friends to share their fortune and therefore use saving schemes to hide their wealth. In Africa, for example, women especially are willing to entrust their money to “susu men” in order to withdraw it from their network (Besley, 1995, page 2150) or to put it into formal saving accounts with effectively negative interest rates (Dupas & Robinson, 2009). Since non-resettled households are significantly more likely to have savings, these findings reduce the magnitude of our resettlement effect.

Considering the non-random nature of the resettlement choice, the work of McKenzie, Stillman and Gibson (2010) provides a conservative measure of the resettlement effect.



Comparing income improvements after migration, McKenzie et al. find a 25–35% bias in OLS regressions with non-experimental data in comparison to experimental migration data. But even then, the resettlement effect identified in regression (2), with -359.3 KHR and 45% of the average transfer payment of the non-resettled players in game two (792.3 KHR), is still substantial.

(d) Ex post survey data on the importance of network support

When we consider the prevalence of various types of shock – such as bad weather conditions, livestock disease, severe illness of a household member, or fire or theft destroying a household's property – the importance of risk-sharing for our sample becomes evident. About two-thirds of the players reported having experienced at least one severe shock during the last two years, and more than 28% reported several shocks. Furthermore, 97% of these players had experienced difficulties in coping with these shocks. Taking the monetary transfers in the games as an indicator of general willingness to support fellow villagers, coping with these shocks in the resettled community is clearly more difficult.

The importance of social cohesion becomes even more pronounced when we look at the poverty status before and after resettlement of project participants. Before resettlement in 2008, about 85% of the project households earned less than 1.25 USD per day. In 2010, the proportion increased in the group of resettled participants to 88%, whereas it decreased in the group of non-resettled participants to 79%. Similarly, there were no income differences in 2008 between the households applying for resettlement and those who did not apply (see Table 1). After resettlement in 2010, the yearly household income of resettled beneficiaries was on average about 20% lower than that of non-resettled participants (resettled participants: 1,130.61 USD, non-resettled participants: 1,429.09 USD, p-value: 0.09). Nevertheless, in our specific case, project transfers could compensate for the greater vulnerability of resettled players. On average 33.5% of the yearly income of resettled participants came from project transfers, while in the group of non-resettled participants project transfers account only for 18% of the average yearly income. Considering the yearly income per household without transfers, participants in the resettled village had a 36% lower income than non-resettled participants (resettled participants: 751.19 USD, non-resettled participants: 1,175.55 USD, p-value: 0.02). Here, 98% of the resettled participants would have fallen below the poverty line and 86% of the non-resettled beneficiaries.

Furthermore, resettled participants' income was lower in 2010 than it had been in 2008, whereas for non-resettled participants it was higher. The resettled participants' income was probably lower because of time lost building a new home and new community facilities, but more importantly because of the lack of social capital. Intuitively, a person's family and friends, community norms, institutions and associations constitute an important asset people can call for in a crisis but also in the normal production process (i.e. knowledge transfer, mutual help). As stated by Narayan and Pritchett (1999) "a village's social capital has an effect on the incomes of the households in that village, an effect that is empirically large, definitely social, and plausibly causal". One year after the land distribution, in both groups agricultural income accounted for only a minor share of their income. Nonetheless, non-resettled participants were earning significantly more income with agricultural production in 2010 (resettled participants: 230.89 USD, non-resettled participants: 164.89 USD, p-value: 0.08).

These findings illustrate the heavy dependence of resettled participants on transfers mainly coming from the project. It is therefore not surprising that perceived 'future security' in 2010 was weaker in the group of resettled participants (p-value: 0.07). We anticipated that especially after the end of the project in 2013, when no more transfer could be expected, social cohesion and solidarity inside the new village would become essential for the farmers if they are to succeed.

## 5. CONCLUSIONS

We conducted a lab-in-the-field experiment comparing voluntarily resettled and non-resettled participants of a land reform project. Farmers in our control group (non-resettled players) received only agricultural land and were still living in their villages of origin. Our treatment group (resettled players) also received residential as well as agricultural land and moved to a newly founded village about one year prior to our behavioral experiment. We conducted a solidarity experiment measuring willingness to transfer money to anonymous community members and then compared transfer differences between the resettled village and the non-resettled villages.

We found that resettled players in the experimental game transferred on average between 49% and 77% lower amounts than non-resettled players. Close to 20% of the losers in the resettled group received no transfers at all, whereas less than 10% of the non-resettled group

received no transfers. One might argue that non-resettled farmers are richer (given the survey data) and therefore more likely to transfer money. However, this income effect was not significant for our experimental participants. On the contrary, we suggest that our analysis estimates a lower bound of the “social effect of resettlement”. This is because we would expect even less giving if (i) resettlement was forced instead of voluntary, (ii) no project support was offered, (iii) we used a non-anonymous experiment to measure reciprocal ties, (iv) savings were equally distributed, or (v) village composition in the non-resettled villages was taken into account, instead of including only the poorest individuals, since richer community members often constitute the main source of financial and technological assistance and share their agricultural equipment with poorer neighbors (Lin, 2001). A survey carried out before resettlement indicates that there were no observable differences regarding social integration predating resettlement. Consequently, the transfer difference is probably caused by voluntary resettlement. We further find that the resettlement effect remains large and significant when we match participants with respect to their network size and when we include expectations. Both results support our view that transfer difference is the result of lower social cohesion in resettled communities and that this difference is not mainly driven by the specific network people have and goes beyond consideration of reciprocity. We do not find that people “punish” high risk taking (or inability); instead, the norm of solidarity applies similarly to everyone and is on average 10% of available income.

Our findings on solidarity transfers in the experiment also relate to the lower real world income of resettled project participants. Solidarity transfers are related to ‘social capital’ which is thought to be an important ingredient for coping with shocks and production. Considering the low income level of project participants, especially in the resettled community, network support plays a vital role. Two-thirds of all players in our experiment reported experiencing substantial shocks such as bad harvests or illness since receiving the land from the project. Hence, besides support from their network of family and friends, willingness to support each other inside the village is a major source of help at the moment. At the time of our study, reciprocal ties of friendship in the resettled village were not yet established and social cohesion was very low. With the loss of social cohesion, our study identifies an important effect of voluntary resettlement that has not been fully explored up to now. We deliver a more complete picture of the costs and benefits of land reforms involving voluntary resettlement and underline the

importance of counteracting negative social consequences even of voluntary resettlement projects. Our findings are relevant for resettlement policies based on the “economics of compensation”, which often neglect these and other social costs. Furthermore, our study can offer help in understanding the difficulties of migrants who leave their village to find a job in the metropolitan areas or the difficulties faced by refugees who are forced to resettle in a new environment.

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<sup>1</sup> We use the term “social cohesion”, broadly following Emile Durkheim, to mean a “social order [which] results from interdependence, shared loyalties and solidarities” (Jenson, 1998).

<sup>2</sup> Somewhat related to the topic of resettlement is the experimental literature on “social distance”, which captures people’s increased willingness to give when they have clues about nationality, occupation, race, religion (Charness & Gneezy, 2008), or friendship and kinship (Vollan, 2011).

<sup>3</sup> In his theoretical model of displacement and reconstruction, Cernea (1997, 2000) discusses the risks involved in resettlement, using the term “social disarticulation”. He observes that planners of resettlement schemes often neglect these risks. Schmidt-Soltau (2003) and Rogers and Wang (2006), among others, use this model as a basis for their empirical analysis of the social impacts of involuntary resettlement.

<sup>4</sup> Barr and Genicot (2008) construct a game in which participants form risk-sharing groups to insure against income shocks. This study does not explicitly test an effect of resettlement. The authors do not find a significant difference between resettled and non-resettled players’ willingness to share risks, but they do find that resettled villagers form significantly larger risk-sharing groups.

<sup>5</sup> While reciprocal, incentive-based risk-sharing motives also play a role, altruism seems to explain the largest part of transfers in previous lab-in-the-field experiments (Leider, Möbius, Rosenblat, & Do, 2009; Ligon & Schechter, 2012).

<sup>6</sup> Furthermore, the risk of losing land mainly through forced eviction because of large infrastructure development projects is substantial. Amnesty International (2008) estimates that at least 150,000 Cambodians (one % of the rural population) are living at risk of forced eviction.

<sup>7</sup> The average land parcel in Cambodia is 0.69 hectares and small-scale farming is common, with 68% owning less than 0.5 hectares (MoP & UNDP, 2007). Since the yearly average rice yield between 2000 and 2008 was 2.26 ton/hectare (Yu & Fan, 2011), the distributed land parcels provide a good opportunity for the project participants.

<sup>8</sup> Out of 1,139 applicants 525 households were selected as land recipients.

<sup>9</sup> There is thus no influence of social distance due to variation in nationality, education, occupation, race, or religion between the two groups.

<sup>10</sup> Risk-averse individuals may also be more likely to migrate when the area of origin is highly insecure (Jaeger et al., 2007). Data from our post-game questionnaire suggest, however, that before resettlement resettled players

experienced fewer economic shocks than non-resettled players. Thus, it is unlikely that the resettled people left because their area of origin was risky.

<sup>11</sup> Additionally, we estimate a probit regression that includes those proxies available for 2008 and socio-demographic information about the households. None of the social variables is found to be significant. We also do not find any difference at the village level between the non-resettled villages and the newly founded village with regard to availability of credit, types of shocks, fluctuation inside the villages, income composition, market integration, living conditions in the village relative to the rest of the country, collective action on the village level, presence of minorities including religious differences, or availability of insurance.

<sup>12</sup> The same holds true if we restrict the sample to those households with panel data.

<sup>13</sup> The experimental protocol and posters used for visualization are available from the authors on request.

<sup>14</sup> This game was also used by Barr and Genicot (2008) in Zimbabwe.

<sup>15</sup> Gneezy et al. (2009) do not find any gender differences. In our task men performed slightly better than women (mean value men: 4.38, mean value women: 3.92) but the difference is only significant at the 10% level. We also do not find a correlation between performance in the task and age.

<sup>16</sup> Originally, we planned for 15 players per session. In reality the number of players per session varied and we include session size in our estimations. Sessions were smaller in the non-resettled villages because of poor infrastructure connecting the community center to the outskirts of the village, problematic information flow from the village chief to the chosen players, and higher costs for the players to reach the meeting point. Recruitment was in general easier in the LASED village, since the whole village consists of project participants who worked closely together with the project staff.

<sup>17</sup> The non-resettled players also reported a slightly higher number of players they disliked in their session. As there were only three non-resettled and two resettled players who disliked other players, we do not discuss the possible consequences of this.

<sup>18</sup> We use the Wilcoxon-Mann-Whitney test, t-test, or test of proportions to compare resettled and non-resettled players and the Wilcoxon signed rank sum test to compare the behavior of players over the three games.

<sup>19</sup> We assume an ordinal scale: option A=1, option B= 2, and option C= 3.

<sup>20</sup> Compare appendix B for a detailed discussion.

<sup>21</sup> As there is no significant difference between the mean risk choices of resettled and non-resettled players in game two and in the skilled task, we only report mean solidarity transfers independent of the risk choice. But also comparing those players who made the same risk choice in game two and in the skilled task shows a significant reduction in transfer sending (N= 21, game two: 638.89, skilled task: 607.14, p-value: 0.02). Regression analysis controls for the type of sender. Graphs of the transfer difference between resettled and non-resettled players in game two and in the skilled task are shown in Figures C.1, C.2 and C.3 in the appendix. In all risk groups in game two, considerably more resettled players sent no transfer than non-resettled players.

<sup>22</sup> Figure C.1 in the appendix shows a Gaussian probability curve for the relative transfers from the three risk groups. Even though A-senders have the highest probability of sending no transfer, the above described order of relative transfers becomes evident for transfers bigger than 0.3% of the payoff.

<sup>23</sup> Winning in game one and game two does not have a significant influence on solidarity transfers in our multivariate analysis.

<sup>24</sup> In total 17 dummies are considered. The coefficients of the dummies and other control variables are presented in Tables C. 2 and C. 3 in the appendix.

<sup>25</sup> Data on expectations are only available for players who were at risk of losing the risk game (risk option B or C)

<sup>26</sup> We test also for heterogeneity of treatment effects for resettlement by stepwise including interaction terms between resettlement status and all socio-demographic variables included in our regression. We also test an interaction term between resettlement and the ability task. All interaction terms turn out to be insignificant.

<sup>27</sup> As robustness check we follow Cameron and Trivedi (2009). The resettlement effect remains significant when by exclude the upper five % of transfer sending, when we use the natural logarithm and estimate with Tobit or OLS specification. The effect also holds when we separate censored data from non-censored data using to specifications. Firstly, we estimate a two-part model which models the decision to send transfers as a logit estimation and secondly the level of transfers conditional on the transfer being non-zero as an OLS estimation. Secondly, we estimate the same decisions with a Heckman selection model. In both cases the resettlement dummy is negatively significant for the decision to send transfers and negatively significant for the level of transfers.

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## TABLES

**Table 1: Household characteristics before the allocation of land by the project (data from a random household survey of project members in September 2008)**

	Resettled			Non-resettled			Difference in means <sup>b</sup>
	N	Mean	Std dev	N	Mean	Std dev	Significance level
<b>Variables for social integration</b>							
Member of self-help group <sup>+</sup>	63	0.12	0.33	43	0.11	0.32	n.s. <sup>a</sup>
Number of wedding celebrations	43	7.97	15.14	41	6.14	5.42	n.s.
Times of visiting the pagoda	43	7.53	9.61	41	7.68	7.43	n.s.
Credit from money lender	43	42.47	103.61	41	64.40	164.76	n.s.
Credit from friends and family	43	55.43	142.46	41	35.25	73.99	n.s.
Credit from employer	43	0.52	2.75	41	0.77	4.95	n.s.
Total credit	43	169.04	226.59	41	192.80	242.11	n.s.
<b>Socio-demographic variables</b>							
Income per month (USD)	43	123.30	157.23	41	111.77	106.87	n.s.
Savings <sup>++</sup>	43	0.60	0.49	41	0.59	0.50	n.s.
Size of the house <sup>+++</sup>	43	1.46	0.59	41	1.68	0.72	n.s.
Nutrient provision <sup>++++</sup>	43	5.40	0.53	41	4.80	0.55	n.s.
Household size	43	6.06	2,73	41	5.48	1.92	n.s.
Gender of household head (1= female, 0= male)	43	0.21	0.41	41	0.41	0.50	5%
Age of household head	43	41.37	9.43	41	42.17	10.85	n.s.
Household head is married <sup>++</sup>	43	0.81	0.06	41	0.71	0.07	n.s.
Years of education of household head	43	4.02	0.49	41	3.78	0.48	n.s.
Number of radios	43	0.30	0.51	41	0.27	0.45	n.s.
Number of TVs	43	0.42	0.50	41	0.32	0.47	n.s.
Number of mobile phones	43	0.26	0.66	41	0.22	0.47	n.s.
Number of bicycles	43	0.88	0.82	41	0.76	0.70	n.s.
Number of motorbikes	43	0.21	0.41	41	0.17	0.38	n.s.

Notes: <sup>a</sup> n.s. not significant<sup>b</sup> Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means between resettled and non-resettled players

+ Dummy variable: (1= yes, 0= no) taken from ex-post data from a random household survey in 2010

++ Dummy variable: (1= yes, 0= no)

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+++ 20 square meters or less (1) / 21–50 square meters (2) / 51 square meters or more (3)  
 ++++ Months enough to eat during the last year

**Table 2: Payoffs in the risk experiment**

Player's choice	Probability of high payoff	Die numbers assigned to high payoff	High payoff in KHR (USD)	Low payoff in KHR (USD)	Expected payoff in KHR (USD)
Option A	1	1, 2, 3, 4, 5, 6	2,000 (0.5)	2,000 (0.5)	2,000 (0.5)
Option B	2/3	3, 4, 5, 6	6,600 (1.65)	0	4,400 (1.10)
Option C	1/3	5, 6	18,000 (4.50)	0	6,000 (1.50)

**Table 3: Number of participants (number of observations) in each game**

	1st game	2nd game		3rd game	
	Risk	Risk	Solidarity	Skilled task	Solidarity
Resettled	127	127	76 (456)	67	34 (204)
Non-resettled	98	98	50 (300)	49	30 (180)
<b>Total</b>	<b>225</b>	<b>225</b>	<b>126 (756)</b>	<b>116</b>	<b>64 (384)</b>

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**Table 4: Individual characteristics of the experimental participants from the post-game questionnaire**

	Resettled, N= 127		Non-resettled, N= 98		Difference in means <sup>b</sup>
	Mean	Std dev	Mean	Std dev	Significance level
Income per month (USD)	124.40	101.89	113.52	85.71	n.s.
Savings <sup>+</sup>	0.27	0.44	0.40	0.49	5%
Nutrient provision <sup>++</sup>	2.65	0.48	2.63	0.48	n.s.
Household size	5.46	1.88	5.74	1.92	n.s.
Gender of household head (1= female, 0= male)	0.12	0.32	0.11	0.32	n.s.
Gender of experimental participant (1= female, 0= male)	0.58	0.49	0.58	0.49	n.s. <sup>a</sup>
Experimental participant is household head <sup>+</sup>	0.48	0.50	0.50	0.50	n.s.
Age	37.08	10.66	41.14	12.31	1%
Married <sup>+</sup>	0.77	0.41	0.81	0.38	n.s.
Years of education	3.92	2.75	3.95	2.28	n.s.
More than 50 USD debt	0.71	0.45	0.50	0.50	1%
Years living in the village	1.15	0.51	33.45	13.92	1%
Relative number of friends <sup>+++</sup>	10.54	12.00	19.71	22.10	1%
Relative number of family members <sup>+++</sup>	2.24	5.59	7.47	11.52	1%

Notes: <sup>a</sup> n.s. not significant

<sup>b</sup> Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means between resettled and non-resettled players

+ Dummy variable: (1= yes, 0= no)

++ Average number of meals with enough food for all household members during the last month

+++ In relation to the session size

**Table 5: Mean transfers in game 2 and game 3 with the skilled task**

	Resettled players			Non-resettled players			Significance level <sup>a</sup>
	Obs.	Mean transfers	Standard deviation	Obs.	Mean transfers	Standard deviation	
Game 2 (risk)	456	490.79	711.84	300	792.33	689.49	1%
Game 3 (task)	204	381.37	337.54	180	703.61	640.05	1%

Note: <sup>a</sup> Wilcoxon-Mann-Whitney test for difference in means between resettled and non-resettled players

**Table 6: Multivariate analysis explaining transfers (marginal effects)**

	(1)	(2)	(3)	(4)	(5)
	Tobit regression <sup>+</sup>	Random-effects Tobit regression <sup>++</sup>	Random-effects Tobit regression <sup>++</sup>	Random-effects Tobit regression <sup>++</sup>	Random-effects Tobit regression <sup>++</sup>
VARIABLES	Transfers game 2 (risk choice)	Transfers game 2 and 3 (skilled task)	Transfers game 2 and 3 (skilled task)	Transfers game 2 and 3 (skilled task)	Transfers game 2 and 3 (skilled task)
Resettlement	-384.9** (175.7)	-552.7*** (150.9)	-421.7** (197.3)	-597.5*** (140.4)	-559.6*** (159.9)
Skilled task		-100.9*** (28.93)	-186.1*** (40.53)	-100.0*** (28.93)	-107.9*** (30.00)
Transfer expectations			0.424*** (0.137)		
Controls for session network	Yes	Yes	Yes	No	Yes
Controls for sender and receiver type	Yes	Yes	Yes	Yes	No
Individual controls	Yes	Yes	Yes	Yes	Yes
Observations	756	1,140	810	1,140	1,140
Number of individuals	126	156	112	156	156

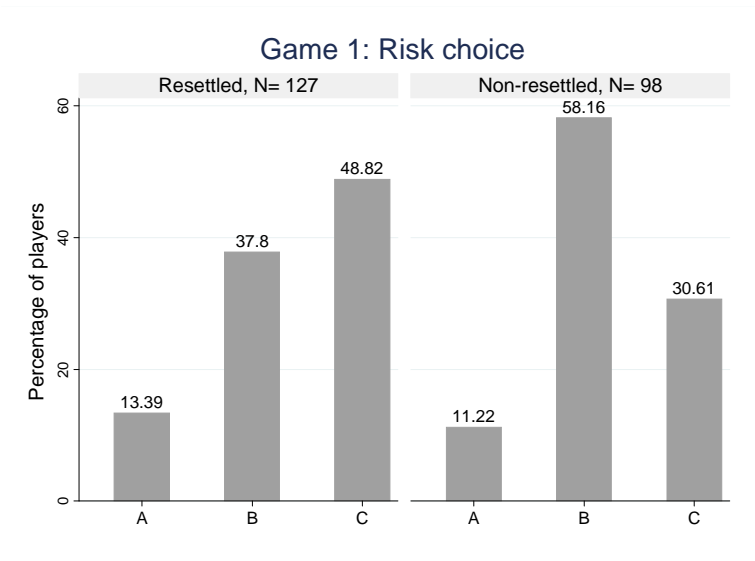
Notes: Standard errors in parentheses; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

+ Standard errors are clustered on the individual level

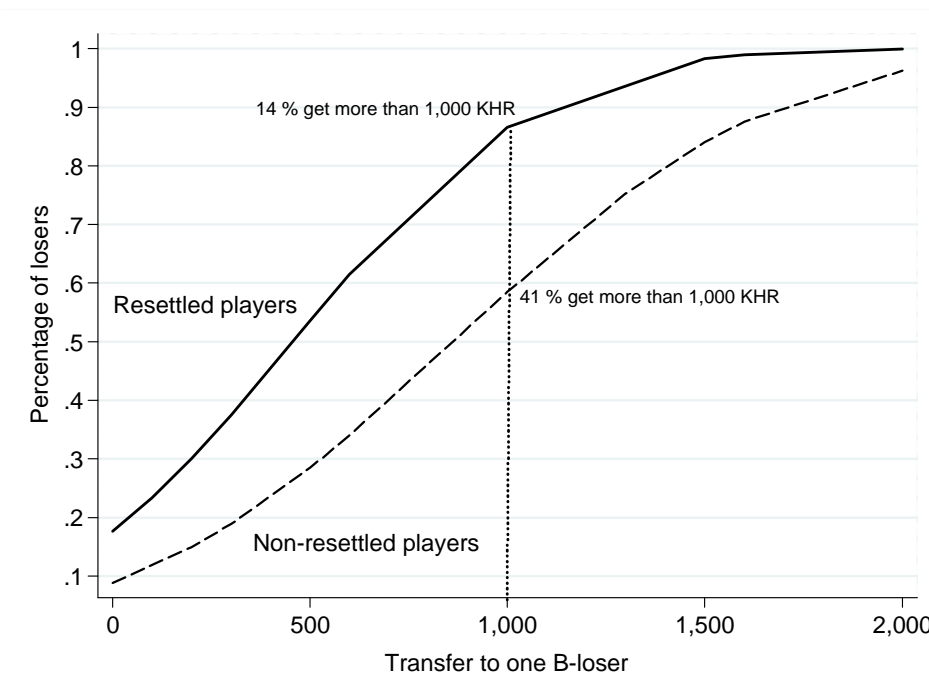
++ Random effects are implemented on the individual level

The individual covariates used in the regressions can be seen in Table C.2 and the dummies for different sender and receiver combinations in Table C.3 in the appendix. It seems that players who have some savings and those who live in bigger households tend to give less. In addition, players with higher education and those who enjoy regular meals tend to give more.

FIGURES



**Fig. 1: Risk choice of non-resettled and resettled players in game 1**



**Fig. 2: Transfer payments to one B-loser in game 2**



## APPENDIX A: INFORMATION BEFORE RESETTLEMENT

Table A.1: Difference-in-difference and ex-post (2010 after resettlement) estimations for indicators of social integration

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	D-i-d <sup>+</sup>	Ex-post	D-i-d <sup>+</sup>	Ex-post	D-i-d <sup>+</sup>	Ex-post	D-i-d <sup>+</sup>	Ex-post	D-i-d <sup>+</sup>	Ex-post
	Wedding celebrations	Wedding celebrations	Pagoda visits	Pagoda visits	Credit from friends and family	Credit from friends and family	Income per year	Income per year	Income per year without transfers	Income per year without transfers
Interaction resettlement and ex-post dummy	-2.706		-1.427		-14.60		-253.0		-370.0	
	(2.703)		(2.765)		(29.20)		(446.7)		(444.6)	
Resettlement dummy	1.830	-0.876	-0.148	-1.575	20.18	5.584	138.3	-114.6	143.3	-226.7
	(2.003)	(1.353)	(2.049)	(1.968)	(21.64)	(16.87)	(331.0)	(282.7)	(329.5)	(281.3)
Ex-post dummy	-0.588		0.178		-12.10		87.80		-126.9	
	(2.003)		(2.049)		(21.64)		(331.0)		(329.5)	
Constant	6.146***	5.558***	7.683***	7.860***	35.25**	23.15*	1,341***	1,429***	1,302***	1,176***
	(1.433)	(1.043)	(1.466)	(1.517)	(15.48)	(13.01)	(236.8)	(217.9)	(235.7)	(216.9)
Observations	190	106	190	106	190	106	190	106	190	106
R-squared	0.018	0.004	0.005	0.006	0.014	0.001	0.002	0.002	0.016	0.006

Notes: Standard errors in parentheses;\*\*\* p<0.01, \*\* p<0.05, \* p<0.1  
+ D-i-d= difference-in-difference estimation

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**Table A.2: Test for equality of the coefficients of the difference-in-difference and the ex-post estimation**

	<b>Interaction resettlement and ex-post dummy of d-i-d estimation</b>	<b>Resettlement dummy of ex- post estimation</b>	<b>Significance level of test for equality</b>
Wedding celebrations	-2.706	-0.876	n.s. <sup>a</sup>
Pagoda visits	-1.427	-1.575	n.s.
Credit from friends and family	-14.60	5.584	n.s.
Income per year	-253.0	-114.6	n.s.
Income per year without transfers	-370.0	-226.7	n.s.

Notes: <sup>a</sup> n.s. not significant

## APPENDIX B: ADAPTATION OF RISK IN GAME TWO AND GAME THREE

In the solidarity game (game two) a player might expect a non-zero payoff in the event of losing the game (depending on the player's expectation of transfers from fellow villagers). Hence the risk of losing can be partly shared within the solidarity group and transfers can be interpreted as an informal insurance mechanism. People might want to avoid being a burden to anyone and thus play the safe lottery more often. This is, however, an unrealistic interpretation since the choices were anonymous, and thus humility, shame or other motives cannot be involved. With informal insurance, players might rather choose a higher risk option as they do not have to bear the cost of losing alone. Choosing a higher risk is also more efficient for the group of three, provided that redistribution among them takes place.

A significant increase in higher risk choices in game two is seen only for those players who won game one, while those players who lost game one significantly decrease their higher risk choices (see Table B.1 and Fig. B.1 in the appendix). Therefore, players' reasoning processes about what level of risk to take is influenced between game one and game two by the introduction of transfer possibilities, but also (and maybe more) by previous experience in game one. Given our limited set of choices, with one safe and two risky options, players might be indifferent to the two risky choices in the first game, but their consequent success or failure is likely to influence their choices in the second game. The non-resettled players show a stronger increase in cases where they win in the first game, and a weaker decrease in cases where they lose, than the resettled players. This different adjustment magnitude can be explained by differences in transfer expectations. For players who were at risk of losing the game (option B or C), we find that higher transfer expectations go along with taking higher risks (mean expectation of players who chose option B: 643.91 KHR, mean expectation of players who chose option C: 838.81 KHR, p-value 0.02). Mean expectations differ at the one % significance level between resettled and non-resettled players (resettled players: 584.28 KHR, non-resettled players: 905.55 KHR, p-value: 0.00). Non-resettled players expected higher transfers because of stronger social cohesion and therefore may have been willing to react more strongly to the introduction of informal insurance in game two.

In game three, the average risk choice in the skilled task is significantly lower than the average risk choice in game two (game two: 2.19, game three: 2.04, p-value: 0.05, see also Fig.

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B.2 in the appendix). This reduction is driven by the less confident non-resettled players who decreased their risk significantly (non-resettled: game two: 2.24, game three: 2.00, p-value: 0.02; resettled: game two: 2.14, game three: 2.07, p-value: 0.54). There is no significant difference in risk choice in the skilled task between resettled and non-resettled players in this game (resettled: 2.07, non-resettled: 2.00, p-value: 0.56), but actual skills are significantly higher in the non-resettled group (mean times a player got the ball into the bucket: resettled: 3.79, non-resettled: 4.51, p-value: 0.02). This means that 10% of the resettled players underestimated their skill and 48% overestimated it, whereas 16% of the non-resettled players underestimated their skill and only 37% overestimated it. These findings, in addition to higher risk preferences, hint at overconfidence especially among the resettled players.

**Table B.1: Difference in means of gamble choice in game 1 and game 2 split according to gamble outcomes**

	Resettled players		Non-resettled players	
	Winning	Loosing	Winning	Loosing
Observations	68	59	55	43
Game 1	2.00	2.76	1.92	2.53
Game 2	2.14	2.13	2.29	2.07
Significance level <sup>b</sup>	1%	1%	1%	1%

Notes: <sup>b</sup> Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means between game 1 and game 2

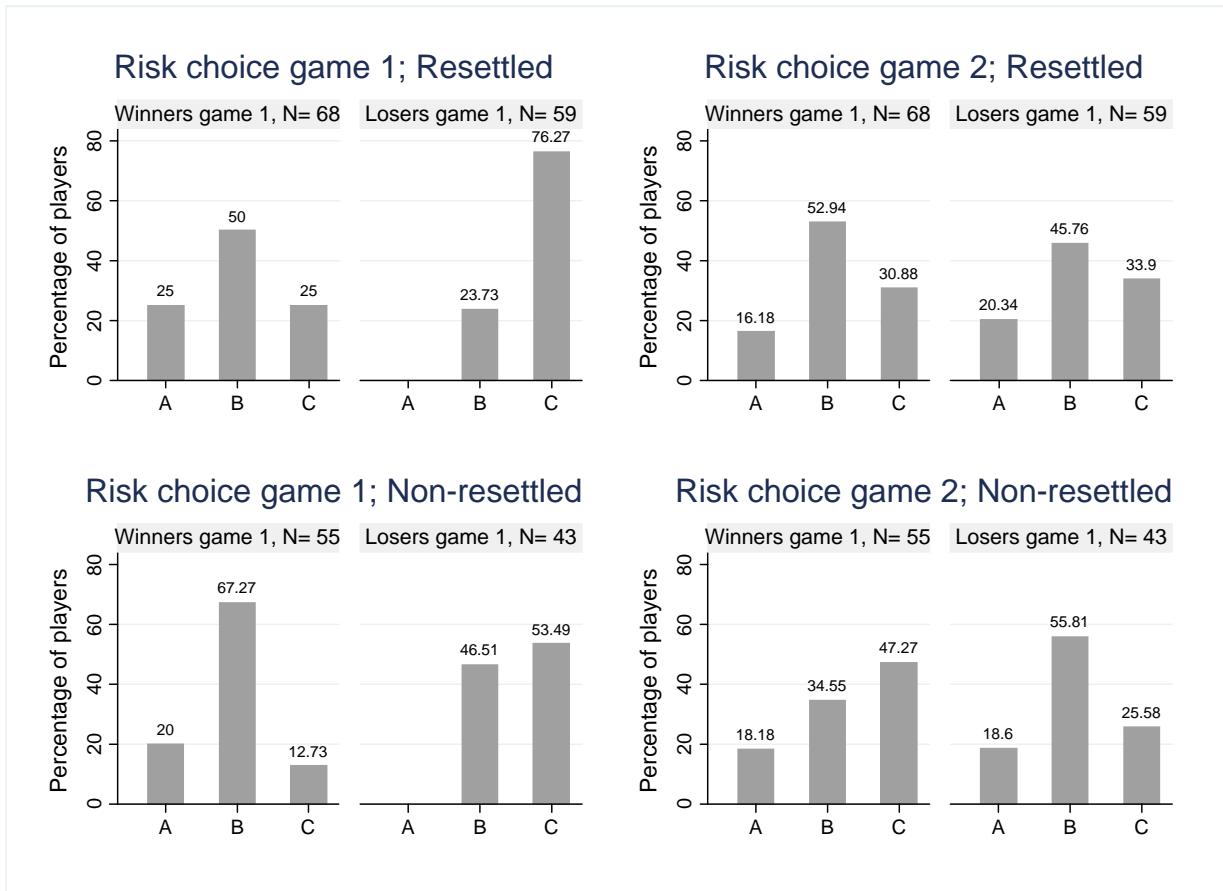
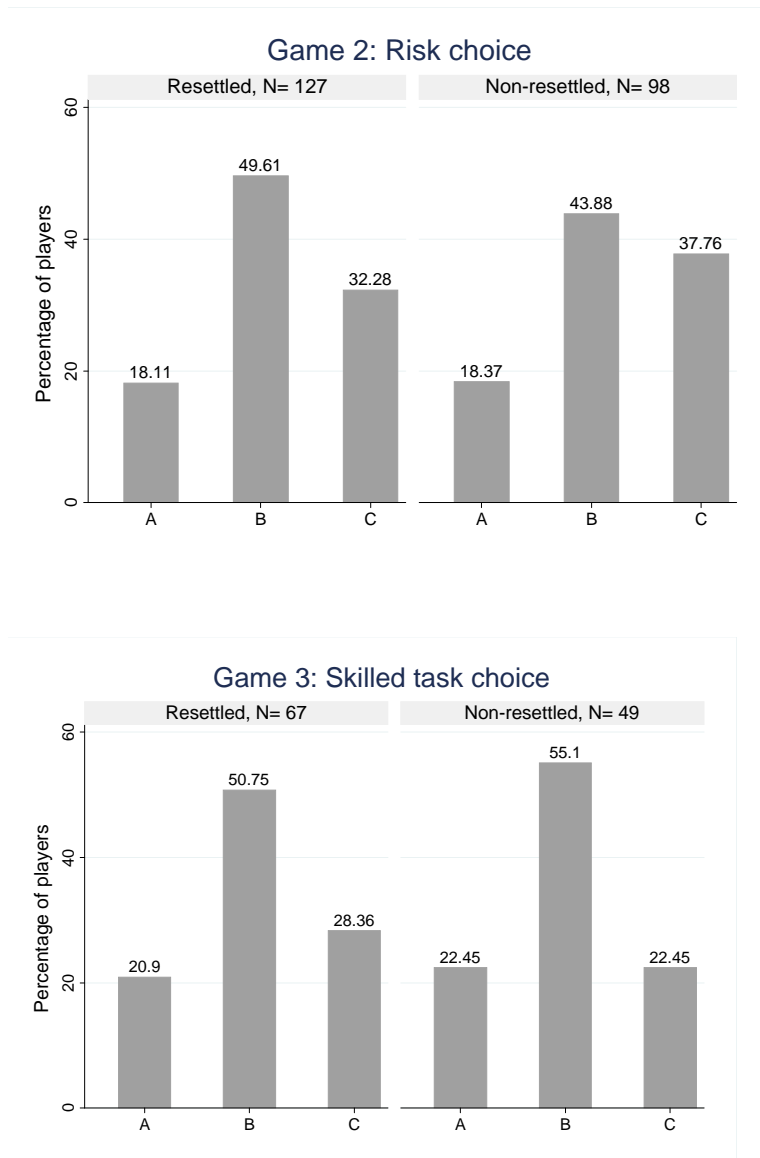


Fig. B.1: Gamble choice in game 1 and game 2 split according to gamble outcomes



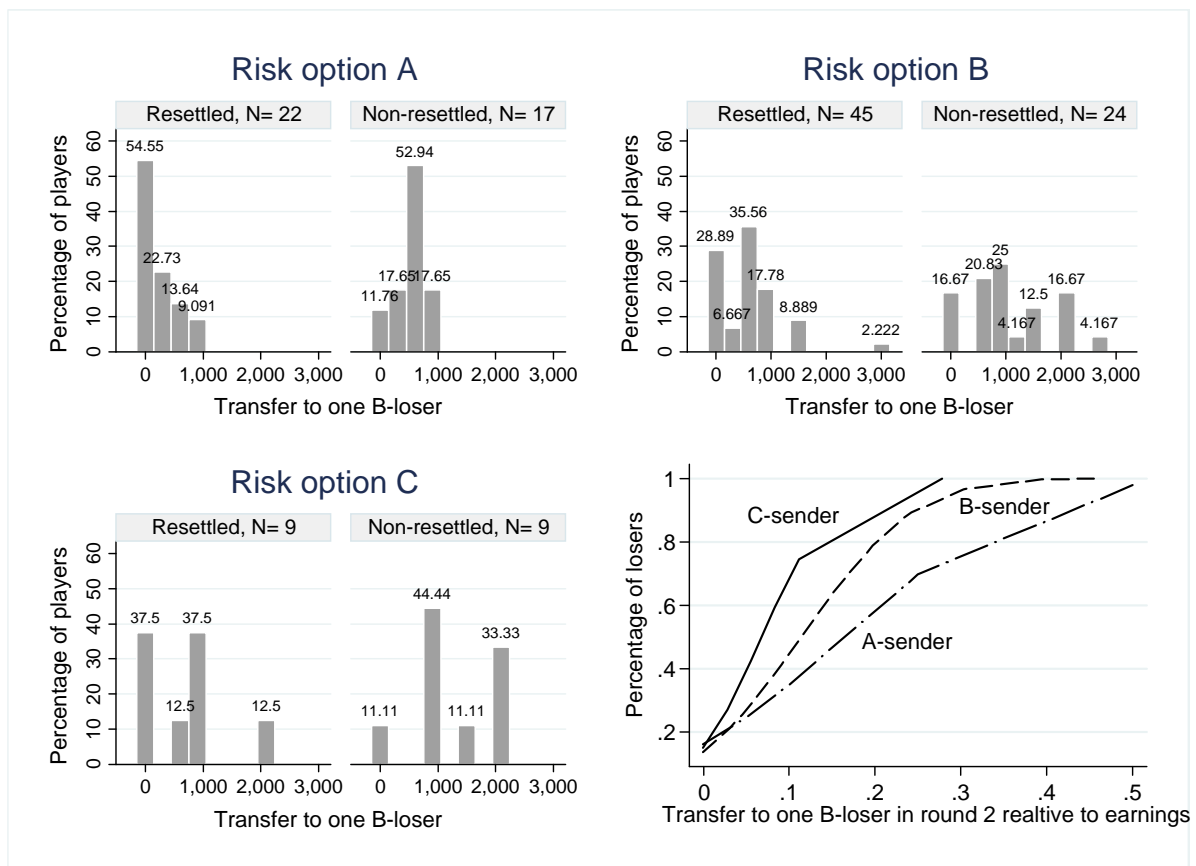
**Fig. B.2: Risk choice of non-resettled and resettled players in game 1 and game 3**

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 APPENDIX C: ADDITIONAL ANALYSES ON TRANSFER SENDING

**Table C.1: Mean transfer per person dependent on risk choices of winners and losers in the solidarity game**

	1 B- loser	1 C- loser	2 B- losers	2 C- losers	2 losers: 1 B- and 1 C-loser; transfer to the B- loser	2 losers: 1 B- and 1 C-loser; transfer to the C- loser	Average over receivers
A-sender	328.21	323.08	255.13	264.38	264.10	266.67	283.76
B-sender	752.17	692.75	581.16	569.57	605.80	568.12	628.26
C-sender	1,222.22	1,277.78	1,277.78	1,194.44	1,250.00	1,277.78	1,250.00
Average over senders	688.09	661.90	579.76	564.68	592.06	576.19	-
Significance level <sup>b</sup>	n.s.		n.s.		10%		-

Notes: <sup>b</sup> Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means between resettled and non-resettled players



**Fig. C.1: Transfer payments according to gamble choices in game 2**

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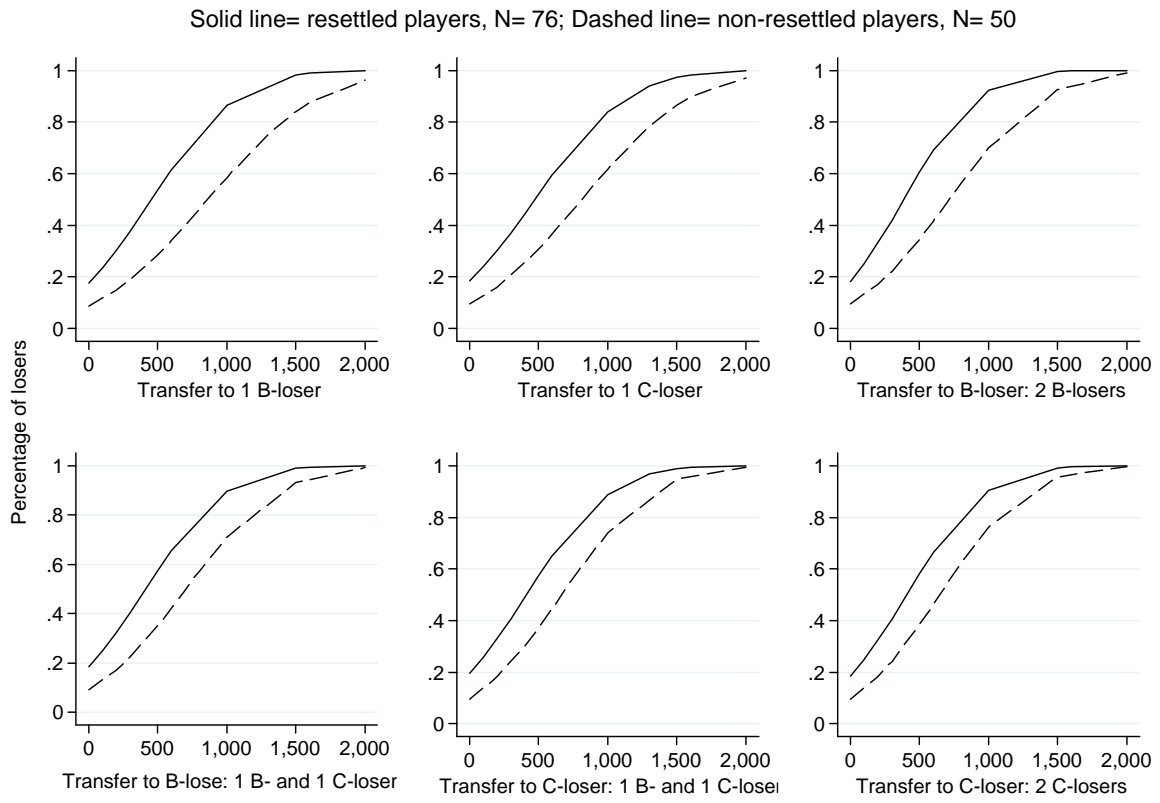
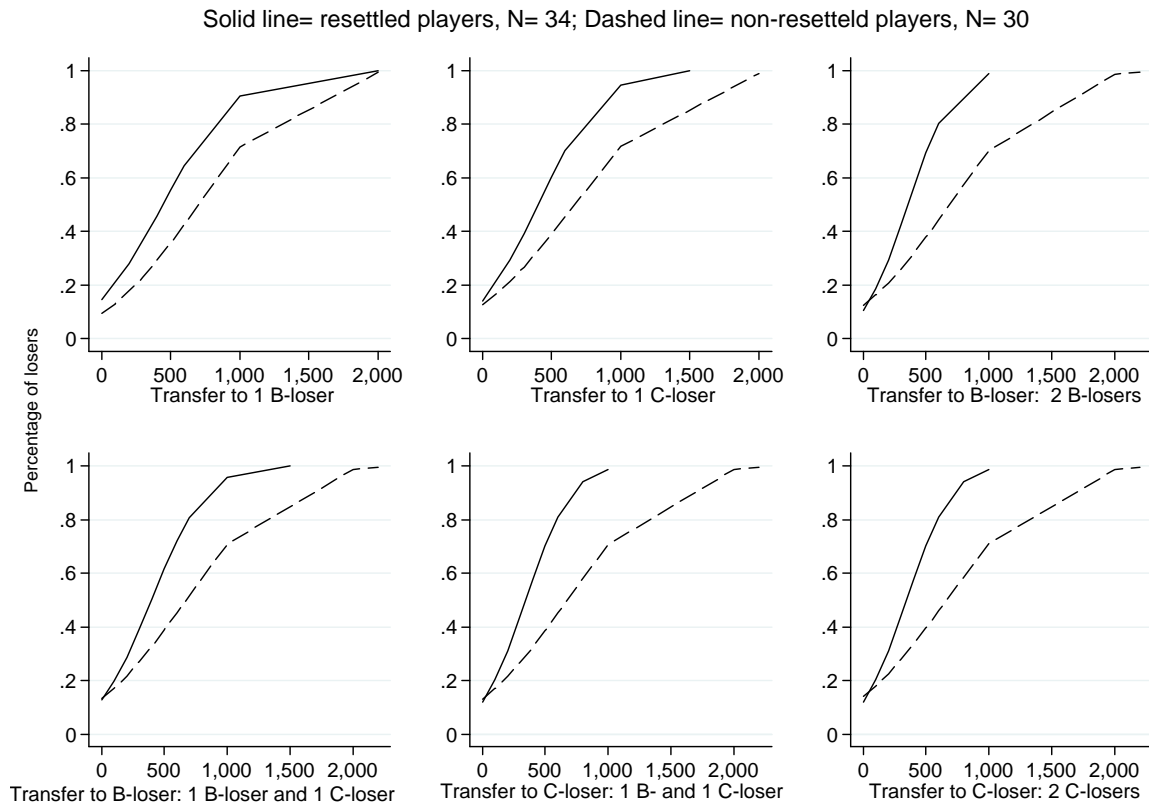


Fig. C.2: Gaussian probability curves of transfer payments in game 2 of resettled and non-resettled players



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**Fig. C.3: Gaussian probability curves of transfer payments with the skilled task of resettled and non-resettled players**

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**Table C.2: Individual control variables for the transfer regressions in table 6**

<b>VARIABLES</b>	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>
Gender of experimental participant (1= female, 0= male)	-97.82 (162.6)	-114.0 (138.0)	-155.3 (180.1)	-119.2 (136.8)	-104.6 (146.7)
Age	3.284 (6.377)	3.640 (6.095)	2.154 (7.919)	3.513 (6.120)	3.202 (6.426)
Years of education	65.67** (26.00)	47.99* (26.94)	44.66 (33.85)	47.73* (27.06)	58.84** (28.45)
Household size	-65.42* (39.45)	-92.79*** (35.07)	-59.84 (46.28)	-86.54** (34.88)	-94.31** (36.81)
Gender of household head (1= female, 0= male)	141.9 (195.5)	115.9 (229.2)	60.38 (293.7)	89.52 (228.8)	133.3 (243.6)
Married <sup>+</sup>	272.2 (186.9)	196.0 (182.1)	263.0 (224.3)	181.0 (182.5)	161.9 (192.0)
Income per month	0.101 (0.676)	0.282 (0.723)	-0.315 (0.998)	0.162 (0.719)	0.142 (0.765)
Nutrient provision <sup>++</sup>	407.3** (170.9)	359.2** (141.4)	273.1 (188.8)	334.4** (139.6)	383.9** (149.3)
Savings <sup>+</sup>	-395.3*** (146.2)	-389.5*** (138.8)	-488.7** (190.0)	-371.6*** (138.3)	-404.3*** (146.7)
More than 50 USD debt <sup>+</sup>	69.38 (133.7)	159.2 (136.9)	108.3 (187.4)	144.0 (137.0)	197.0 (144.9)
Shock during the last 3 years <sup>+++</sup>	-83.22 (126.0)	-27.89 (137.0)	99.45 (169.2)	-11.80 (136.8)	-39.12 (145.0)
Shocks of friends or family <sup>+++</sup>	234.3* (139.4)	134.1 (133.5)	174.1 (164.6)	135.5 (133.8)	115.4 (141.7)
Relative number of friends <sup>++++</sup>	1.275 (4.232)	4.282 (3.869)	0.906 (5.244)		5.109 (4.095)
Relative number of family members <sup>++++</sup>	0.0888 (7.324)	0.586 (7.315)	-5.988 (10.43)		0.111 (7.766)
Responsibility for own fate <sup>+++++</sup>	143.5 (111.6)	130.2 (122.3)	87.24 (161.4)	129.2 (122.7)	150.4 (129.7)
Always somebody in the village who helps <sup>+++++</sup>	-133.8 (110.0)	-103.7 (111.0)	-146.7 (140.6)	-96.64 (111.2)	-92.51 (118.1)

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Payoff game 1	-0.0253 (0.0182)	-0.0147 (0.0137)	-0.0140 (0.0170)	-0.0128 (0.0137)	-0.00844 (0.0143)
Session size	-5.662 (35.02)	17.38 (27.49)	6.187 (33.84)	22.93 (27.17)	13.94 (29.20)

Notes: Standard errors in parentheses, \*\*\* p<0.01, \*\* p<0.05, \* p<0.1  
 + Dummy variable: (1= yes, 0= no)  
 ++ Average number of meals with enough food for all household members during the last month  
 +++ "Shock" refers to illness, accident, fire, theft, natural disaster  
 ++++ In relation to the session size  
 +++++ 1= strongly agree - 4= strongly disagree

**Table C.3: Sender and receiver dummies for the transfer regressions in table 6**

VARIABLES	(1)	(2)	(3)	(4)	(5)
Sender A & receiver C	-5.958 (34.60)	-30.45 (56.95)	-70.18 (118.8)	-30.43 (56.94)	
Sender A & 2 receivers B B - receiver B	-85.33*** (29.71)	-91.96 (57.16)	-99.41 (119.0)	-91.91 (57.15)	
Sender A & 2 receivers B C - receiver B	-74.80** (29.85)	-88.52 (57.15)	-116.2 (119.1)	-88.47 (57.14)	
Sender A & 2 receivers - B C receiver C	-71.79** (34.44)	-88.52 (57.15)	-124.6 (119.2)	-88.47 (57.14)	
Sender A & 2 receivers C C - C receiver	-73.30** (31.02)	-89.67 (57.15)	-124.6 (119.2)	-89.62 (57.14)	
Sender B & receiver B	461.8*** (150.0)	326.4*** (68.12)	252.0** (125.0)	327.6*** (68.12)	
Sender B & receiver C	397.6*** (148.7)	269.9*** (68.17)	195.8 (125.0)	271.0*** (68.17)	
Sender B & 2 receivers B B - receiver B	276.1** (138.6)	144.8** (68.27)	58.54 (125.1)	145.9** (68.27)	
Sender B & 2 receivers B C - receiver B	303.0** (142.1)	176.7*** (68.25)	96.00 (125.1)	177.9*** (68.25)	
Sender B & 2 receivers B C - receiver C	261.8* (141.9)	130.5* (68.28)	42.80 (125.1)	131.6* (68.28)	

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Sender B & 2 receivers C C - receiver C	263.4*	125.7*	45.22	126.8*
	(140.9)	(68.29)	(125.1)	(68.29)
Sender C & receiver B	1,014***	264.7**	176.8	269.2**
	(374.9)	(129.4)	(174.7)	(129.4)
Sender C & receiver C	1,072***	313.7**	228.7	318.2**
	(387.7)	(129.3)	(174.6)	(129.2)
Sender C & 2 receivers B B - receiver B	1,072***	319.2**	234.4	323.6**
	(404.6)	(129.3)	(174.6)	(129.2)
Sender C & 2 receivers B C - receiver B	1,043**	292.0**	205.6	296.4**
	(411.5)	(129.3)	(174.6)	(129.3)
Sender C & 2 receivers B C - receiver C	1,072***	319.2**	234.4	323.6**
	(409.8)	(129.3)	(174.6)	(129.2)
Sender C & 2 receivers C C - receiver C	984.5**	237.4*	147.9	241.8*
	(411.4)	(129.5)	(174.8)	(129.4)

Notes: Standard errors in parentheses, \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table C.4: Transfer differences based on matching results according to the network size in the session**

	<b>Obs. resettled players</b>	<b>Ob. non-resettled players</b>	<b>Average treatment effect on the treated*</b>	<b>Std. err.</b>	<b>T- value</b>
Stratification method	456	294	-283.07	58.20	-4.86
Nearest neighbour (random draw)	456	180	-391.62	81.88	-4.78
Kernel matching (with bootstrapping ,repetitions 50)	456	300	-314.59	145.3 4	-2.16
Radius matching (0.01)	390	192	-163.20	72.33	-2.26

Notes: \* If the common support option is specified the average treatment effect on the treated is also significant for all matching methods.