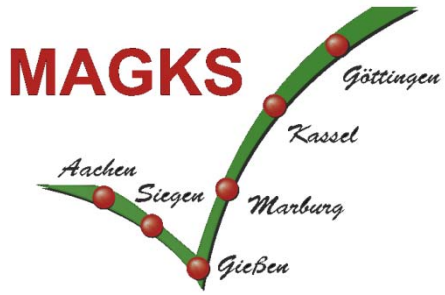

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Exchanging Land for Solidarity: Solidarity Transfers among Voluntarily Resettled and Non-Resettled Land-Reform Beneficiaries

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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 solidarity in established and newly resettled communities. Our experimental participants were part of an agricultural land-distribution project for which they signed up voluntarily. Half of our sample voluntarily resettled one and a half years before this study. Playing a version of the "solidarity game," we identify the effect of voluntary resettlement on willingness to help anonymous fellow villagers. We find that resettled farmers transfer substantially less money to their fellow villagers than farmers who have not resettled. Our experimental results indicate greater vulnerability on the part of resettled households in the initial years after resettlement.

Keywords: Cambodia, lab-in-the-field experiment, land distribution, social capital, solidarity, voluntary resettlement

JEL Codes: Q15, C93, D03

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Land reforms in developing countries are believed to have the potential to eradicate food insecurity, to alleviate rural poverty, and to reduce households' vulnerability to shocks via increased income, more savings, better access to the credit market, and increased returns to family labor. But a number of studies show that the outcomes of land distributions do not always meet expectations (Ravallion and Sen 1994; McCulloch and Baulch 2000; Valente 2009). In particular, if resettlement is involved, the social effects of leaving a well-functioning, cohesive community that supports its members in times of need are unclear and may counteract the economic benefits for individual farmers.

Our article looks at the social effects of a land-distribution program in Cambodia. We focus on village solidarity shortly after voluntary resettlement, information which is of particular relevance since the risk of making ill informed decisions related to allocation of labor and capital (i.e. how much and when to plant, harvest, sell, invest) is highest immediately after farmers obtain agricultural land, when they are still inexperienced and they do not have a financial buffer to cope with failure. Given that the land-distribution project under study was directed at the most vulnerable part of the population, the individuals' capacity to cope with shocks was extremely low. The prevalence not only of everyday problems such as lack of food but also of highly damaging idiosyncratic shocks such as the illness or death of a family member is high in rural Cambodia (World Bank 2006) and formal security systems are nonexistent. Informal networks are often the only fallback option for accessing credit and mutual help (Dinh, Dufhues, and Buchenrieder 2012; Okten and Osili 2004; Attanasio et al. 2012).

By moving, an individual can lose access to informal community support, as geographic proximity is one of the main determinants of social networks (Fafchamps and Lund 2003; Fafchamps and Gubert 2007). Also, political and social institutions need to be reestablished at the

new destination in order for social norms to emerge that enforce solidarity, cooperation, trust, and altruism and sanction free-riding. Thus, coping with risks may become more difficult after resettlement. The few available studies on the social consequences of voluntary resettlement concentrate on the redistributive land reform in Zimbabwe and suggest that negative effects on informal networks may be observed even 20 years after resettlement (Dekker 2004; Barr 2003; Barr, Dekker, and Fafchamps 2010).[1] Dekker (2004) finds evidence that non-resettled households in Zimbabwe rely on their network and solidarity in the village, while voluntarily resettled households are more likely to rely on individual risk-coping strategies.[2] 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.[3]

However, she also finds evidence for an increased effort to "community build." The number of formal group arrangements (for example, clubs and associations) in the resettled communities is more than twice as high as in traditional communities. Evidence from studies on civil war or natural disasters also suggest that voluntary resettlement could create "in-group feelings" among the resettled beneficiaries in the sense that a common shock stimulates social cohesion (Bellows and Miguel 2009; Cassar, Healy, and von Kessler 2011; Voors et al. 2012; Bauer et al. 2014). Thus, we do not have a clear a priori hypothesis on the direction of the solidarity effect.

Our participants were all recruited from a land-distribution project in rural Cambodia. We have compared the solidarity among those project farmers who voluntarily resettled with the solidarity among beneficiaries who stayed in their established villages. Similar to Barr (2003), we

have measured pro-social motivations by implementing a "lab-in-the-field" experiment. We decided to use a modified version of the solidarity experiment (Selten and Ockenfels 1998) that captures transfers motivated by pro-social concerns after a risk-induced income shock. Solidarity is an independent concept in sociology and is based on strong feelings of togetherness and social cohesion (Durkheim 1997, reprinted from 1893 and 1964). Solidarity is related to and encompasses altruism (the degree to which we care about others), inequity aversion (Fehr and Schmidt 1999), fairness (Rabin 1993), guilt aversion (Charness and Dufwenberg 2006), self-image (Tirole and Bénabou 2006), reciprocity (Gouldner 1960; Berg, Dickhaut, and McCabe 1995)[4] and other motivational models. We believe solidarity is suitable for capturing village norms and in-group feelings in a rural set-up where people are frequently exposed to shocks.

Our lab-in-the-field experiment consisted of two stages in which participants interacted only with randomly chosen land-reform beneficiaries from their own village. This means that the farmers in the control group (non-resettled players) played the game with other project members from their old community whereas the farmers in the treatment group (resettled players) played with project members of their new community. In the first stage of the experiment all participants played a risk game. In the second stage the winners of the risk game made a one-shot decision about whether or not to transfer payments to anonymous losers in their group of three. This set-up made it possible for the participants to reduce disparities by equalizing game outcomes through the transfer of money. Moreover, it allowed us to understand whether solidarity payments are influenced by the risk choice of the person in need (Trhal and Radermacher 2009).

We found a sizeable reduction in people's willingness to help others in the resettled community. Resettled players transferred on average between 47 percent and 75 percent less money than non-resettled players. This effect remained large and significant after we controlled

for sociodemographic factors such as income, personal networks, and different risk choices and when we controlled for differences in transfer expectations. People who have been resettled will therefore need not only intensive external support but also adequate microinsurance and better access to credit. Support measures for both voluntary and forced resettlement initiated by the government, aid agencies, or investors need to address these obvious risks more seriously.

Our article provides new evidence on the social costs of voluntary resettlement. It differs from Barr's (2003) article in several ways. Firstly, it concentrates on short-term effects and we have used an experimental design involving the provision of unconditional mutual aid after income shocks. Secondly and more importantly, the studies on Zimbabwe lack data from the period before resettlement and thus cannot discuss whether their effect is driven by the underlying characteristics of the resettled farmers instead of resettlement. It is possible that those who favored a certain political party or those willing to use violence were able to resettle. In our article we have circumvented this issue by enriching our experimental results with survey data from before and after resettlement. Furthermore, we present substantial evidence that rules out strong selection effects between the two groups of farmers and thus show that a causal interpretation of the resettlement effect is likely. First, almost all poor people who lived in the project area applied for the program, and all applied for both residential and agricultural land. Thus, our treatment and control groups were both willing to relocate and therefore share similar unobservable characteristics such as motivation to migrate and personality. Second, the samples were quite homogeneous in terms of observable socioeconomic factors due to the enforcement of eligibility criteria for the entire project. We have confirmed this with e*x ante* data showing that the groups did not differ in terms of a range of observable socioeconomic conditions (including wealth) and their social integration in their village of origin. All participants shared the same

ethnicity, were Buddhist, spoke the same language, and lived in the same political region. Hence, variation in behavior between groups is unlikely to be rooted in cultural, religious, or societal differences, which are frequently stressed as important contextual factors shaping behavior. Finally, we have also performed several econometric robustness tests.

The article complements the existing literature on the impact of resettlement. Most studies of the social consequences concentrate on involuntary displacement – for example, because of "development projects," natural catastrophes, or environmental protection (Berg 1999; Eguavoen and Tesfai 2012; Colchester 2004; Zhang et al. 2013; Schmidt–Soltau 2003; Rogers and Wang 2006; Abutte 2000; Goodall 2006; Lam and Paul 2013). However, voluntary resettlement combined with land reform has become increasingly common (see e.g. Dekker and Kinsey (2011) and Barr (2004) for Zimbabwe; Cousins and Scoones (2010) for South Africa, Namibia, and Zimbabwe; or Karanth (2007), Tefera (2009) and Margolius, Beavers, and Paiz (2002) for conservation areas in India, Ethiopia, and Guatemala) and further research is needed. Our work introduces the notion of risk sharing and solidarity as an additional dimension in this context.

Resettlement Context

The experiment was carried out in the context of a pilot project that allocated one to three hectares of agricultural land to land-poor and landless people and supports them in starting to farm. In Cambodia more than 50 percent of the rural population is land-poor, with less than half a hectare of land, and approximately 20 percent is landless (Cambodia, MoP and UNDP 2007).[5] Applicants could apply for residential and agricultural land parcels, only agricultural land parcels or only residential land parcels. All those who received residential land migrated permanently to

a newly founded village. All the agricultural plots are around this new village. Non-resettled project farmers have to commute to their agricultural plots.

Resettled and non-resettled project beneficiaries had to have lived in the project communes for several years and had to be landless or land-poor (i.e. owning less than half a hectare of agricultural land). Only between 1 and 2 percent of poor households eligible for the project did not apply. All applicants applied for both agricultural and residential land. Hence all of them were willing to relocate. As there was more demand for both agricultural and residential land than could be supplied (1,139 applicants), 525 applicants were selected according to the same neediness criteria. The project used best-practice methods for selecting beneficiaries and provided ongoing support to both resettled and non-resettled farmers. Residential land was granted to all eligible households that did not have any residential land but were living on the land of someone else.

This selection criterion did not reflect wealth differences between the groups. We found no differences in housing conditions (size and material of the house), income, land holdings, assets, and other socioeconomic characteristics between households accepted for resettlement and those refused in our *ex ante* data on the period before land distribution (table 1).

Focus group discussions revealed that the proximity to the agricultural plots and the expected future infrastructural improvements in the newly founded village, relative to the villages of origin, motivated households to apply for residential land (good road conditions, construction of a health station, a primary school, and a village pagoda).[6] Apparently people did not anticipate the decrease in solidarity, as this would at least partly have reduced some individuals' willingness to relocate.

Conditional on their acceptance for the project, farmers were allocated specific agricultural and residential land plots by lottery. Land was distributed to 525 households by the end of 2008 as a pilot project. The non-resettled group obtained agricultural land only (44 percent), while the resettled group received agricultural and residential land (52 percent). We excluded the 4 percent (N=20) of households that received only residential land as conclusions about this group are not reliable. The resettled group settled in a new village near the established villages. At the time of writing, around 10,000 hectares had been allocated to approximately 5,000 households.

Ex Ante Differences between Project Participants

When resettled farmers are nonrandomly selected from the general population, it may be difficult to obtain an appropriate comparison group of non-resettled farmers. Fortunately, our two groups had many similarities: they were all willing to relocate; they came from the same villages; they had obtained agricultural land of a similar size and thus had a similar potential income; and they had a similar *ex ante* status of poverty, which made it very likely that they were net-receivers of solidarity transfers and were similarly motivated to farm. Most importantly, the beneficiaries in both groups had lived in the project communes before the start of the project and were therefore able to establish strong social relations (mean: 32 years, minimum: 6 years).

To see whether resettled and non-resettled households differed in terms of social integration before resettlement we used data from a random survey of 84 project households in 2008, before the allocation of land by the project, and retrospective data from 2010 that provide information on 106 project households before resettlement (table 1). As a proxy for social capital

we used membership in formal groups, participation in prominent social events (number of wedding celebrations and frequency of visits to the pagoda), and the availability of informal credit, which may signal trust and a reputation for being trustworthy. Tests for differences in means between the resettled and non-resettled groups are insignificant for all social variables and none of these variables is significant in a probit regression for the resettlement dummy (table A.1 in the supplemental appendix online). There was also no significant difference in terms of income and savings, housing conditions (material and size of the house), nutrient provision of the household members, household size, education, material status and age of the household head, or relevant household assets in 2008. We confirm this finding with matching estimations for our experimental solidarity measure as well as a comparison of a difference-in-difference estimation with naïve ex-post estimation (tables A.2 and A.3, tables C.4 and C.5 and the corresponding discussion in the supplemental appendix online).

All the non-resettled villages lie within 29 km of the resettled village. These are small communities with between 128 and 669 households where neighbors typically live within earshot of each other. The same holds true for the resettled village. Here, residential plots are placed in rows (three to five) along both sides of a tarred road. The agricultural plots are at the eastern side of the village, adjacent to the residential plots. Credit is offered by private money lenders and commercial banks, but it is hard for the poor to obtain. Formal insurance is not available, and basic infrastructure (small markets, health station, primary and secondary schools) lies within a radius of 10 km. Based on key informant interviews in the resettled village and the non-resettled villages, we did not find relevant differences at the village level in the availability of credit, the types of shocks, fluctuation inside the villages, income composition, market integration, living

conditions relative to the rest of the country, collective action at the village level, the presence of minorities including religious minorities, or the availability of insurance.

[INSERT TABLE 1 HERE]

Methods

Subjects who received only agricultural land played the game with other project members from their old community, and those who received both agricultural and residential land played with project members of their new community.

The Solidarity Experiment

The experiment consisted of a risk stage followed by a solidarity stage. Subjects were randomly allocated into groups of three. When making their risk decision, participants knew about the second stage. However, they did not know who they were paired with and they could not communicate. Our risk lottery followed an ordered lottery selection design adapted from Binswanger (1980; 1981) and Barr and Genicot (2008) (table 2) that included three lotteries. In the event of loss, the payoff was zero, which is thought to activate pro-social motivations in the following stage. The outcome of the risk stage was decided by the participant's dice roll. Option A provided a small but secure payoff (2,000 Riel/ KHR). Options B and C offered a higher expected payoff than option A, but also incorporated the risk of getting zero payoff. Option B appealed to players willing to accept a moderate risk, whereas option C was most attractive to risk-loving players.

Exchanging Land for Solidarity

We were interested in measuring intrinsic solidarity. Therefore, we implemented an anonymous one-shot solidarity experiment in the second stage. Due to the combination of the risk stage with the solidarity stage, a player could have expected a nonzero payoff in the event of losing the game (depending on the player's expectation of transfers from fellow villagers). Hence, transfers can be interpreted as an informal insurance mechanism. Decisions to transfer money were taken after the risk choice by winners of the game only for different possible combinations of the number of players with zero payoff in the player's group (one or two) and the risk choice of these players (B or C). This yielded 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' actual transfer and risk decisions.

[INSERT TABLE 2 HERE]

In addition we randomly selected half of our sessions to include a second independent game afterwards.[7] Here, we replaced the random-winning mechanism of the risk-stage game with a skilled task to test whether solidarity was lower when winners felt that they had "earned" their money. Following Gneezy, Leonard, and List (2009), we set the task of throwing a ball into a bucket. After we had pretested the task, we set winning probabilities and 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 could be attributed to the change from a random lottery to a test of skill. Again, the winners of the skilled task subsequently made the solidarity decisions. A discussion, including the hypothesis and the

interpretation of results for the skilled task, is presented in the supplemental appendix online (appendix B).

Those who participated in the two games were aware of whether they had won or lost in the previous game, but we did not reveal how many players had won the previous game nor did we reveal the transfer decisions. We informed those participants that new solidarity groups were formed and that after both games had been played we would randomly select one game 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 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.

Experiment Procedure and Participants

The experiment 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). The resettled project participants in our sample originate from nine surrounding villages which are between 12 and 29 km away (average distance 24 km).

Instructions were always read out loud by the same person to all players in the common room of the village community center. All decisions took place in private. Random recruiting procedure and experimental protocol, including practice rounds and posters used for visualization are included in the supplemental appendix online (appendix F).[8]

Table 3 shows that all participants played the risk game (N=225). The transfer decisions in the second stage were only recorded for those players who won the risk game in the first stage

(N=126, 76 resettled and 50 non-resettled players).[9] Each player made six transfer decisions, leading to 756 observations. In half of the sessions we played game two involving a skilled task (N=116). Sixty-four participants 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, savings and credit (probably due to higher investment need of resettled farmers) which we have controlled for in our regression (table 4). Furthermore, as expected, the non-resettled players reported having on average significantly more friends and family members in the session than the resettled players.[10] However, this difference was not very large (the average percentage of friends in the session was 10 percent for resettled players and 20 percent for non-resettled players). Also, 30 percent of players in both samples reported having no friends taking part in the session. In our analysis we controlled for the network a person had within the experimental session.

[INSERT TABLE 4 HERE]

Hypotheses

Selten and Ockenfels (1998) find that "giving behavior" in a solidarity game depends on one's expectations about the giving behavior of others. As our groups were anonymous, expectations about transfers at the session level were relevant. However, as solidarity can be unconditional and based on feelings of togetherness and cohesion, resettlement may have an effect on transfer sending beyond rational expectations. A negative effect of resettlement on solidarity would thus be the result of (i) lower expectations that others would help, (ii) a reduced preference for helping others in need, and (iii) fewer family members and friends taking part in the session.

On the other hand, a number of researchers have recently begun to empirically study parochial altruism – that is, kindness towards members of one's own group and aggressive spite towards members of an "out group" (e.g. Abbink et al. 2012; Choi and Bowles 2007). Bellows and Miguel (2009) have shown that experiences of war can increase collective action, and experimental studies have reported that exposure to civil war increases in-group altruism (Voors et al. 2012). It also increased people's egalitarian motivations towards their in-group in the aftermath of the conflicts in Georgia and Sierra Leone (Bauer et al. 2014). Thus, one could imagine that the households' experience of a common shock in the form of resettlement could breed an in-group sensibility ("we are the pioneers…") and thus increase solidarity.[11] Therefore, the effect of resettlement on village level solidarity is a priori unclear.

Risk taking in stage one of the experiment was likely influenced by the possibility of informal insurance in stage two, as the risk of losing could be partly shared within the solidarity group. Expecting positive solidarity transfers can thus be said to foster risk taking. If resettlement has a negative effect on solidarity and players' expectations are correct, resettled players should be less likely to choose higher-risk options than non-resettled players. On the other hand, a

stronger "empathy effect," as discussed by Alger and Weibull (2010), might offset this effect in the group of non-resettled players. People in long-standing communities might feel a stronger desire to support their co-villagers if need be and therefore chose a rather safe lottery. In addition, people might want to avoid being a burden to anyone and thus play the safe lottery more often – to avoid the shame of losing. This effect is likely to have been greater in the group of non-resettled players, who were more integrated in their community. Therefore, the effect of resettlement on risk taking is a priori unclear.

Results

We start our empirical part with a descriptive analysis and carry on with multivariate regressions. Many more regressions were run than can be included in the article. The interested reader can find them in a supplementary appendix online.

Descriptive Analysis

Transfers in the second stage were contingent upon a player winning the random mechanism in game one or the skilled task in game two, and therefore upon the risk choice, luck, or ability of the players in the first stage. Figure 1 shows the choices of resettled and non-resettled participants for the first stage. We could not identify significant differences in the choices between the groups in either of the games and our setting did not allow us to disentangle different possible risk-taking motives.

[INSERT FIGURE 1 HERE]

After a player made his or her risk choice, but before he or she rolled the dice (or threw the ball) and possibly took the solidarity decision, we asked the player to state how much transfer he or she would expect from a player who had won the different risk options. Hence, expectations are only available for players who were at risk of losing the risk game (risk option B or C) and were contingent upon the player's own risk choice.[12] In line with our interpretations, we have found that higher transfer expectations go along with greater risk taking (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 differed between resettled and non-resettled players at the 1 percent significance level (resettled players: 595.63 KHR, non-resettled players: 900.63 KHR, p-value: 0.00).

An analysis of the transfers sent by winners to losers in game one shows that the mean transfers of resettled players were significantly lower (table 5).[13] The resettled players transferred on average 38 percent less money than non-resettled players. Transfers decreased with the skill-driven winning mechanism but remained significantly lower in the group of resettled players. These findings were confirmed through qualitative interviews after the experiment. Resettled players reported that sharing norms are not present in the new community; as one resettled participant remarked, "Giving nothing is just the way people behave in this village" (May 4, 2010, session one).

[INSERT TABLE 5 HERE]

When we analyze the transfers conditioned on the earning in the first stage of game one, we observe the following patterns (table C.1 in the supplemental appendix online). Firstly, transfers per person were lower when there were two losers in a group than when there was one but the total sum of transfers was greater in the case of two losers. Secondly, even though absolute transfers increased with the available budget, A-senders were willing to give on average 14.19 percent of their earnings (283.76 KHR), followed by B-senders (9.52 percent, 628.26 KHR) and C-senders (6.94 percent, 1,250 KHR).[14] Higher relative contributions by less wealthy people are also found in public-good games (Hofmeyr, Burns, and Visser 2007; Buckley and Croson 2006).

Thirdly, and contrary to Trhal and Radermacher (2009), who played with German university students, we identified no evidence that wealthy individuals helped less when neediness was self-inflicted by taking higher risk. This was the case for both resettled and non-resettled communities. Given the importance of "fate" in Asian countries, this is not too surprising. We also did not find any evidence of homophily or in-group bias in the form of larger transfers to people with the same risk choice. If high-risk investments are insured to the same extent as low-risk investments, there does not seem to be an innovation barrier caused by a lack of insurance.

Figure 2 shows the cumulative density function of potential transfers to one B-loser (most common situation) for resettled and non-resettled players. The curve for the resettled players lies entirely above that for the non-resettled players. Hence, over the entire distribution of transfers, resettled players were more likely to receive lower-value transfers. Taking a transfer of 1,000 KHR as an example, the probability that a resettled player received a higher transfer was 14 percent. The probability for non-resettled players was 41 percent.

[INSERT FIGURE 2 HERE]

Transfer Differences Contingent on Risk Choice and Expectations

Since individual transfer decisions can depend on one's own risk choices and those of others, simple descriptive analysis may be misleading. We estimate solidarity transfers 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 also the type of receiver).[15] We estimate Tobit regressions as our latent variable (willingness to support) has a nonnegligible number of observations at the corner solution (24 percent of all observations are zero). Table 6 contains the results of the 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 sociodemographic controls and session size are included in all regressions.

In our analysis, we focus on the transfer difference between resettled and non-resettled players. We start by analyzing only the transfer decisions in game one, with the random-winning mechanism in stage 1 (regression (1), N=126, observations=756). Here, the resettlement dummy is negative and significant at the 5 percent level. In a second step, we estimate a random effects Tobit regression that also includes the transfer decisions in game two, with the skilled task in stage 1 (regression (2), N=156, observations=1,140). The resettlement dummy increases in magnitude and remains negative and significant at the 1 percent level.

Exchanging Land for Solidarity

The solidarity experiment also includes elements of trust, since transfers depend on expectations about the solidarity of others (Selten and Ockenfels 1998). To further separate the effects of solidarity from beliefs about others' solidarity, we include transfer expectations in regression (3) (N=112, observations=810). These have a significant positive influence on transfers. The more interesting finding, however, is that resettlement remains negatively significant. That is, lower transfers are neither solely driven by a change in financial or other constraints nor by lower expectations about the support of others but also by a preference for not helping people within the resettled village. This is interesting as standard economic literature usually highlights exogenous preferences.

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 -40.9 KHR. Thus, we believe that the anonymity of our experiment cancelled out the effect of familiarity in the session.[16]

Table C.6 in the supplemental appendix online shows a random effects Tobit regression restricted to the resettled players that addresses two possible individual-level motives for solidarity. Firstly, the lower level of solidarity might be driven by players originating from nearby villages who can easily rely on historical networks; players from distant villages might have a greater incentive to engage in community building in the newly formed villages. Secondly, it might be the case that solidarity transfers increase when the odds of being matched with a former co-villager increase. Neither the distance to the village of origin nor our measure for session homogeneity in terms of village of origin are significant.[17] Taking these results together shows that unconditional giving in our experiment is driven not so much by the presence

of a personal social network, the availability of historical networks, or the session composition, but rather by intrinsic motivations. Furthermore, the relatively small influence of the number of family members and friends in the session suggests that the anonymity, the independence of the games, and the lack of communication successfully removed much of personalized trust and reciprocity motivations from the experiment.[18]

Lastly, we estimate transfers without controlling for the risk choices of senders and receivers, which gives us the total effect of voluntary resettlement (regression (5), N=156, observations=1,140). Since there are no significant differences in risk choices between resettled and non-resettled players, we find hardly any differences between regressions (2) and (5).

[INSERT TABLE 6 HERE]

When we apply regression analysis, taking risk choice and variation in control variables into account, the resettlement dummy is significant in all the specifications with a magnitude of -371.6 KHR to -590.6 KHR. Thus, resettled players transferred amounts that were between 47 percent and 75 percent lower than the transfers of non-resettled players in game one (792.3 KHR). The difference between the two groups is estimated more precisely in this way and thus greater than that identified using a simple descriptive analysis (38 percent). Regressions (2) to (5) show a significant negative coefficient for the skilled task. This shows that effort and accountability for the game outcome reduce transfers compared to a situation purely based on luck. However, the coefficient for the skilled task (-100.9 KHR in regression (2)), is more than five times smaller than the resettlement effect. In accordance with our descriptive results, we do

not find in-group bias or significant discrimination with respect to risk taking on the part of the loser for any of the three sender groups.[19]

Limitations and Discussion

In our regression analysis we have found that resettled players send significantly lower solidarity amounts than non-resettled players. Given the proximity to the villages of origin (average distance: 24 km), one can argue that the lower level of solidarity in the resettled community could be explained by a lower need to rely on within village networks. Then again Fafchamps and Lund (2003) show that over 80 percent of all informal lending comes from households in the same village and Fafchamps and Gubert (2007) find for the rural Philippines that even the distance within the same "sitio," which is a small unit of 15 to 30 households, significantly influences the likelihood of risk sharing. In a similar vein, Schechter and Yuskavage (2012) and De Weerdt (2004) find that a distance of less than 2 km within the village significantly reduces the likelihood of social network formation. Moreover, while a distance of 24 km (one way) can be covered within one day, we have to bear in mind that 71 percent of our resettled sample did not have any motorized means of transportation, of which a further 32 percent did not have any means of transportation. Walking 24 km to the village of origin takes approximately six hours (one way). The main arguments for why geographic proximity matters for social networks are that monitoring and enforcement and certain types of support, such as regular visits in the event of illness, are much easier to maintain. These activities can be hindered by even small distances. The main advantage of longer distances, insurance against locally covariate shocks, likely needs a longer distance to come into effect. Nevertheless, we cannot rule out that resettled farmers would still get support from people in their village of origin especially in the first years after

resettlement. Our experiments deliberately tried to abstain from measuring motives such as personalized giving (i.e. to kin or friends) or reciprocity. These motives clearly are important drivers for solidarity giving but have to be addressed in a separate study.[20]

Our experiment measures solidarity at a very early stage – approximately one and a half years after resettlement – and from a single resettled village. We therefore do not claim to measure a lasting effect nor can we claim generality as the results may not translate even to other resettled villages that have or may exist in this region. In line with Barr (2003), we could expect societal responses (formation of associations, collective action, intra-household marriages) that, in the long run, counteract the initially lower level of social capital in the resettled villages. Yet the short-term effects are also important, as the risk of failure is greatest at the beginning.[21]

A weakness of our combined risk and solidarity game is that there exist multiple equilibria. Players may have tried to coordinate on different equilibrium such as: “low risk and low solidarity” or “high risk and high solidarity”. While this design complicated the identification of a clean solidarity effect we have shown that solidarity was significantly lower in the resettled village than in the non-resettled villages for players choosing the secure option A (resettled players: 209.09 KHR, non-resettled players: 482.35 KHR, p-value: 0.00) as well as for risk takers (resettled players: 688.89 KHR, non-resettled players: 1112.12 KHR, p-value: 0.02). Moreover, there are no significant differences in risk-taking between the non-resettled villages and the resettled village. Thus, differences in behavior stem from solidarity only. Furthermore, we did neither observe discrimination according to risk-taking in the resettled nor in the non-resettled sample and figures C.2 and C.3 in the supplemental appendix online show that independent of the risk choices of losers as well as the number of losers per group, resettled players were more likely to receive lower-value transfers than non-resettled players.

We believe that the combined effect of "risk-taking and solidarity" is very relevant for behavior of farmers in this setting. Farmers have to take their planting decisions choosing between traditional and unconventional crops. The former inhibit lower risk as investment costs and expected returns are more secure. The latter are characterized by higher uncertainty but might fill a gap in the market and thus lead to higher profits. Agricultural risk decisions determine therefore both the need for solidarity and the available budget to show solidarity to fellow villagers. Results from our post-game questionnaire supported that our experiment is close to reality. Ninety-six percent of all players acknowledged the similarity of the experiments with real-life agricultural investment decisions involving different risks and mutual support. Undoubtedly, the equilibrium on which the resettled players coordinated yielded to a high probability of going away empty-handed. In the resettled group, the probability of receiving no transfers was close to 20 percent, whereas for the non-resettled players it was less than 10 percent (see figure 2).

Lastly, given the nonrandom nature of the resettlement choice one might still argue that these substantial differences in solidarity are not due to resettlement but to some degree caused by underlying factors. We have presented various robustness tests and explanations why we believe that the bias should not be large in our study.[22] Also the work of McKenzie, Stillman, and Gibson (2010) provides some information on the magnitude of a potential bias. Comparing income improvements after migration, McKenzie, Stillman, and Gibson (2010) find a 25 to 35 percent bias in OLS regressions with nonexperimental data in comparison to experimental migration data. If we apply this bias to our much more homogenous case, the resettlement effect identified in regression (2) decreases to -357.3 KHR and 45 percent of the average transfer payment of the non-resettled players in game one (792.3 KHR) which is still substantial. As a

further robustness check we follow Altonji, Elder, and Taber (2005) and Bellows and Miguel (2009), who use the attenuation caused by selection on observables as a guide to the degree of selection on unobservables. A comparison of regression (2), with a resettlement coefficient of -549.7 KHR (including full controls), with regression (6), which leads to a resettlement coefficient of -514.5 KHR (without any controls), shows that attenuation is, with 35.2 KHR, very small. Given these estimates, the selection on unobservables would need to be 15.62 times stronger than selection on observed variables in order to compensate for the entire resettlement effect. Given the rich set of control variables, this seems highly unlikely.[23]

Conclusions

Solidarity in the form of unconditional transfers and labor support is an important ingredient for the smooth functioning of communities, particularly for coping with shocks as well as for successful small-scale agricultural production (planting, weeding, etc.). Considering the low income of participants, network support is vital. In 2010, about 88 percent of the resettled and 79 percent of the non-resettled households are below 1.25 USD per day of income. Two-thirds of our players experienced shocks such as bad harvests or illness since receiving the land from the project. Hence, in addition to support from their network of family and friends, village mutual support is a major source coping with idiosyncratic shocks.

We investigated the impact of a voluntary resettlement program on the propensity to make solidarity transfers to fellow villagers during a lab-in-the-field experiment among voluntarily resettled and non-resettled land recipients. All the farmers participating in the experiment applied for residential and agricultural land and were hence willing to relocate, but the farmers in our

control group (non-resettled players) received only agricultural land and still lived in their villages of origin. Our treatment group (resettled players) received residential as well as agricultural land and moved to a newly founded village about one and a half years prior to the experiment.

We found that resettled players transferred between 47 and 75 percent less than the non-resettled players. Close to 20 percent of the losers in the resettled group received no transfers at all, whereas less than 10 percent of those in the non-resettled group received no transfers. When we consider that about two-thirds of players experienced at least one severe shock in the last two years, the importance of solidarity for our sample population becomes evident. At the time of our study, solidarity in the resettled village was very low. However, the demand for support is highest at such times. Our article thus identifies an important effect of voluntary resettlement – the loss of solidarity – that has not been fully explored to date. Moreover, it is unlikely that informal pro-social norms in the resettled village will catch up to the level of the old villages in the near future, as Barr (2003) and Dekker (2004) show that even 20 years after resettlement negative consequences persist. Nonetheless, formal arrangements might compensate in the longer run for the loss of informal networks.

Our results also inform an emergent literature showing that a greater need for cooperation due to e.g. experience of natural disaster or civil war increases prosocial behavior towards an in-group (Voors et al. 2012; Bauer et al. 2014). However, and in line with Prediger, Vollan and Herrmann (2014), who analyze the effect of long-term resource scarcity on pro- and anti-social behavior, we find no increase in in-group altruism after voluntary resettlement. Several features of our study are distinct from those that show in-group effects: First, subjects selected voluntarily into the resettlement challenge; second, there was no out-group enemy; third, people in our study

did not know each other beforehand; and fourth, the negative event only affected a subset of the population. A forced resettlement scheme, for example, provides a clear out-group enemy and thus, may lead to higher cooperation among in-group members.

Land reforms are high on the international agenda, but studies on their consequences mostly concentrate on economic variables or involuntary resettlement. Therefore, we believe that our article presents useful insights on the social and economic losses caused by voluntary resettlement within a land-reform program. Our finding that there is a lower level of solidarity in resettled communities complements the analysis of Barr (2003), who finds that resettled farmers display a lower level of trust in their fellow villagers. Our findings are relevant for resettlement policies based on the "economics of compensation," for example when infrastructure projects force resettlement and offer compensation that falls to reflect such social costs. Moreover, they show that insurance against shocks is urgently needed in developing countries, particularly in remote rural areas.

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Table 1: Household Characteristics before the Allocation of Land by the Project

Variables	Resettled			Non-resettled			Difference in Means
	N	Mean	Std Dev	N	Mean	Std Dev	P-Value[h]
Variables for Social							
Member of Self-help Group[a]	63	0.12	0.33	43	0.11	0.32	0.87
Number of Wedding	43	6.12	5.23	41	6.15	5.42	0.98
Number of Visits to Pagoda	43	7.53	9.61	41	7.68	7.43	0.94
Informal Credit	43	98.41	25.40	41	100.42	26.96	0.95
Total Credit	43	169.04	226.59	41	192.80	242.11	0.64
Housing Conditions							
Size of the House[b]	43	1.46	0.59	41	1.68	0.72	0.17
Main Material of Roof[c]	43	1.51	0.70	41	1.41	0.67	0.48
Main Material Exterior Walls[d]	43	1.32	0.47	41	1.27	0.50	0.46
General Condition of House[e]	43	1.84	0.57	41	1.90	0.62	0.64
Sociodemographic Variables							
Income per Month (USD)	43	123.30	157.23	41	111.77	106.87	0.70
Land (Hectare)	43	0.23	0.59	41	0.22	0.53	0.91
Savings[f]	43	0.60	0.49	41	0.59	0.50	0.86
Nutrient Provision[g]	43	5.40	0.53	41	4.80	0.55	0.44
Household Size	43	6.06	2,73	41	5.48	1.92	0.26
Age of Household Head	43	41.37	9.43	41	42.17	10.85	0.72
Household Head is Married[f]	43	0.81	0.06	41	0.71	0.07	0.25
Education Household Head	43	4.02	0.49	41	3.78	0.48	0.72
Number of Radios	43	0.30	0.51	41	0.27	0.45	0.75
Number of TVs	43	0.42	0.50	41	0.32	0.47	0.34
Number of Mobile Phones	43	0.16	0.37	41	0.20	0.40	0.70
Number of Bicycles	43	0.88	0.82	41	0.76	0.70	0.45
Number of Motorbikes	43	0.21	0.41	41	0.17	0.38	0.66

Note: Data from a random household survey of project members in September 2008, [a] Dummy variable: (1=yes, 0=no) taken from ex-post data from a random household survey in 2010, [b] 20 square meters or less (1) / 21–50 square meters (2) / 51 square meters or more (3), [c] Thatch, palm leaves, plastic sheet, tarpaulin, or other soft materials (1) / Corrugated iron (2) / Tiles, fibrous cement, or concrete (3), [d] Saplings, bamboo, thatch, palm leaves, or other soft materials (1) / Wood, sawn boards, plywood, corrugated iron (2) / Cement, bricks, concrete (3), [e] In dilapidated condition (1) / in average condition, livable (2) / in good condition and safe (3), [f] Dummy variable: (1=yes, 0=no), [g] Months with enough to eat during the last year , [h] Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means between resettled and non-resettled players

Table 2: Payoffs in the Risk Experiment

Player's Choice	Probability of High Payoff	Dice 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

	First game		Second game	
	Risk	**Solidarity**	**Skilled Task**	**Solidarity**
Resettled	127	76 (456)	67	34 (204)
Non-resettled	98	50 (300)	49	30 (180)
Total	**225**	**126 (756)**	**116**	**64 (384)**

Table 4: Individual Characteristics of the Experiment Participants

Variables	Resettled, N=127		Non-resettled, N=98		Difference in means[e]
	Mean	**Std dev**	**Mean**	**Std dev**	**P-value**
Income per Month (USD)	124.41	101.89	113.52	85.71	0.40
Savings[a]	0.27	0.44	0.40	0.49	0.04
Nutrient Provision[b]	2.65	0.48	2.63	0.48	0.75
Household Size	5.46	1.88	5.74	1.92	0.27
Gender[c]	0.58	0.49	0.58	0.49	0.98
Household Head[a]	0.48	0.50	0.50	0.50	0.77
Age	37.08	10.66	41.14	12.31	0.01
Married[a]	0.77	0.41	0.81	0.38	0.50
Years of Education	3.92	2.75	3.95	2.28	0.91
More than 50 USD Debt[a]	0.71	0.45	0.50	0.50	0.00
Years Living in the Village	1.16	0.51	33.45	13.92	0.00
Relative Number of Friends[d]	10.54	12.00	19.71	22.10	0.00
Relative Number of Family Members[d]	2.24	5.59	7.47	11.52	0.00
Shock During the Last Two Years[a]	0.69	0.47	0.63	0.48	0.41

Notes: Data from the from the Post-Game Questionnaire, [a] Dummy variable: 1=yes, 0=no, [b] Average number of meals with enough food for all household members during the last month, [c] Dummy variable: 1=female, 0=male, [d] In relation to the session size, [e] Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means between resettled and non-resettled players

Table 5: Mean Transfers in Risk and Skill Games

Games	Resettled Players			Non-resettled Players			
	Obs.	**Mean Transfers**	**Standard Deviation**	**Obs.**	**Mean Transfers**	**Standard Deviation**	**Significance Level[a]**
Game 1 (Risk)	456	490.79	711.84	300	792.33	689.49	1%
Game 2 (Skill)	204	381.37	337.54	180	703.61	640.05	1%

Note: [a] 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)	(6)
Variables	**Transfers Game 1 (Risk Choice)[a]**	**Transfers Games 1 and 2 (Skilled Task)[b]**	**Transfers Games 1 and 2 for B- and C-senders[b]**	**Transfers Games 1 and 2[b]**	**Transfers Games 1 and 2[b]**	**Transfers Games 1 and 2[b]**
Resettlement	-371.6**	-549.7***	-413.9**	-590.6***	-556.8***	-514.5***
	(179.9)	(151.5)	(197.6)	(140.6)	(160.3)	(152.2)
Skilled Task		-100.9***	-186.2***	-100.1***	-107.9***	-106.2***
		(28.93)	(40.53)	(28.92)	(30.00)	(30.03)
Transfer Expectations			0.424*** (0.137)			
Controls for Session Network	Yes	Yes	Yes	No	Yes	No
Controls for Sender and Receiver Type	Yes	Yes	Yes	Yes	No	No
Individual	Yes	Yes	Yes	Yes	Yes	No

Exchanging Land for Solidarity						
Controls						
Observations	756	1,140	810	1,140	1,140	1,140
Number of Individuals	126	156	112	156	156	156

Notes: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1, [a] Tobit regression with standard errors clustered on the individual level, [b] Random-effects Tobit regression with random effects implemented on the individual level, The individual covariates used in the regressions can be seen in table C.2 in the supplemental appendix online and the dummies for different sender and receiver combinations in table C.3.

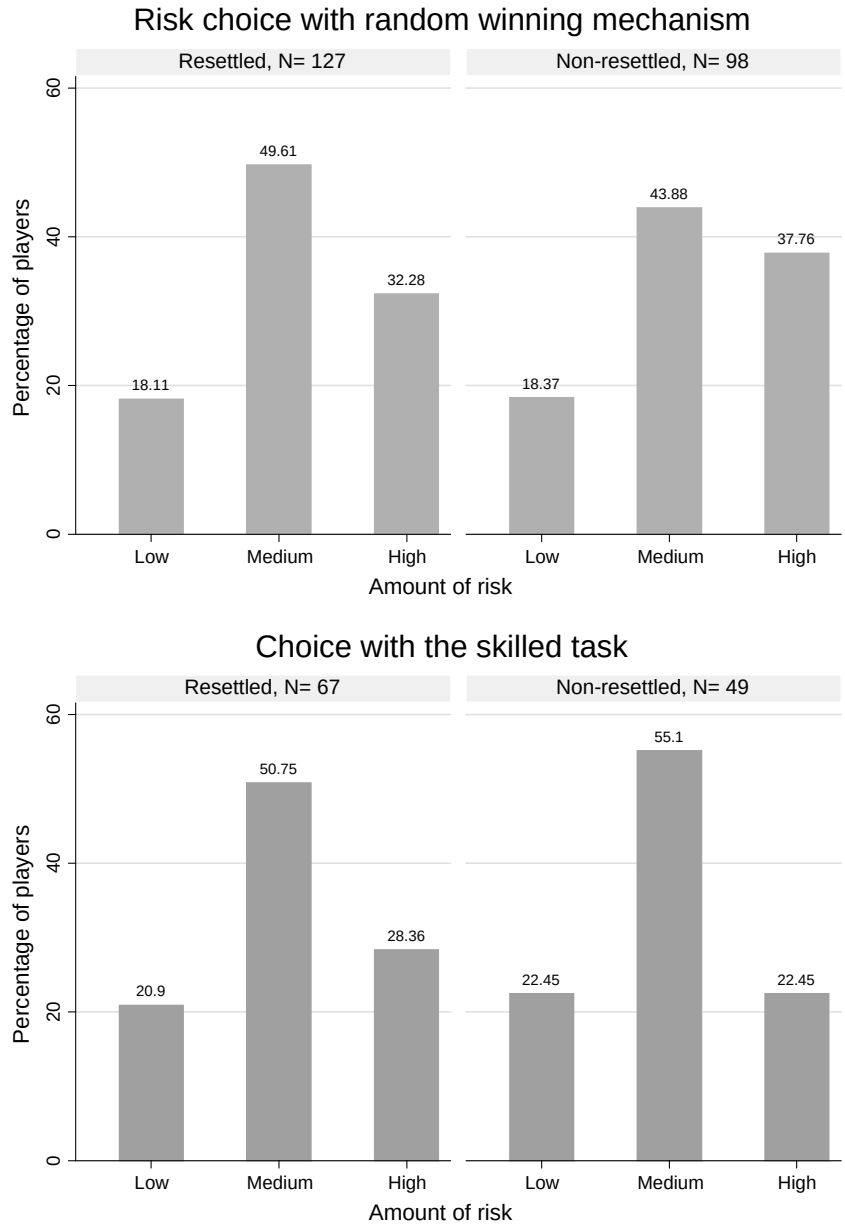

Figure 1: Choices of non-resettled and resettled players in the risk and the skill game

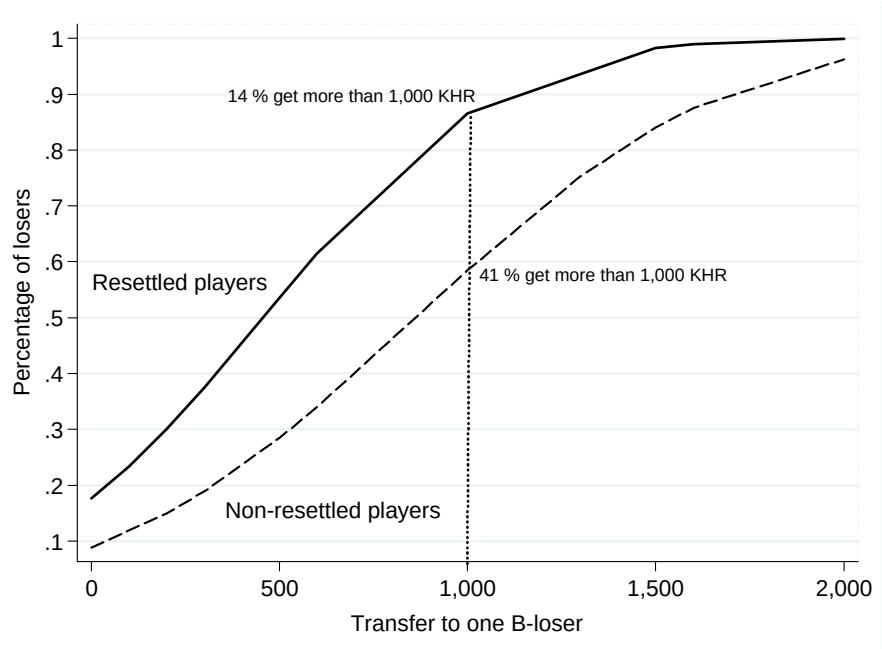

Figure 2: Transfer payments to one B-loser in game 1

[1] Unsurprisingly, forced resettlement can lead to a poverty trap consisting of poor harvests and damaged informal risk-coping networks (Lam and Paul 2013).

[2] 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 and Gneezy 2008), or friendship and kinship (Vollan 2011).

[3] 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 the 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.

[4] Although we followed standard lab procedures to eliminate reciprocal motives (anonymity, no communication, strategy method for transfer, random payout, etc.), reciprocity may not be fully eliminated. First, repeated transfers can be enforced anonymously by contagion. Second, with a small number of participants, one might be able to extract information *ex post*. We thank a referee for pointing this out.

[5] 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 (1 percent of the rural population) are living at risk of forced eviction.

[6] As our sample consists only of poor households who are net recipients of financial aid it is rather implausible that those who lived on the land of their family did decide to resettle in order to escape existing sharing obligations. The literature on "forced solidarity" or "dark side of social capital" (e.g. di Falco and Bulte 2011) shows that kinship networks may deter investment and

effort. Escaping these network obligations might be easier by resettling to a new village. While this literature emerged in the African context we are not aware of such obligations in Asian countries. If at all this effect should be present for richer people who are not in our sample. Also, almost all households applied for resettlement and non-resettled households are significantly more likely to have savings – and thus "hide" money from their network.

[7] Due to time constraints we could not play a second game in all sessions.

[8] We illustrated the risk decision during the instruction by showing posters and reading out examples of the gambling choices. Every player practiced throwing the dice three times. Each time a different gambling choice was assumed and the players verified that they understood the outcomes of the game. To reduce the complexity of the game, every player also practiced the risk game by playing a practice game independently of the actual game. Even though the practice game was independent from the actual game, we controlled for the outcome of the practice game in another specification. All results remained robust and no significant influence on the outcome was identified. When they were making their decisions, posters of the different gambling choices were available to the players. We explained money transfer decisions in the same way: first in the common room with examples and posters for different numbers and types of losers, and second in private with test questions about the solidarity game. Here no practice game took place.

[9] Using the same dice, B-players in the group of non-resettled participants have been unlucky in the risk game. While the probability of winning risk option B was 68 percent only 56 percent of the non-resettled players have won this option; compared to 71 percent of the resettled players. However, a test of proportions shows that these results are still within the statistical boundaries (p-value for resettled players: 0.45; p-value for non-resettled players: 0.12). Furthermore, if more non-resettled B-players had won the experiment our results should not change much. The

resettlement coefficient for a regression restricted to B-players in game 1 is with -370.3 (column 2, table C.9 in the supplemental appendix online) nearly equal to the resettlement coefficient for the whole sample (371.6, column 1, table 6).

[10] 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.

[11] We thank an anonymous referee for pointing out this alternative hypothesis.

[12] Nevertheless, it is unclear if higher transfer expectations lead to higher risk taking or if more positive individuals chose higher risk. We thank an anonymous reviewer for pointing that out.

[13] Graphs of the transfer difference between resettled and non-resettled players in game one and in the skilled task are shown in figures C.1, C.2 and C.3 in the supplemental appendix online. In all the risk groups in game one, considerably more resettled players than non-resettled players sent no transfers.

[14] Figure C.1 in the supplemental appendix online 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 order of relative transfers described above becomes evident for transfers greater than 0.3 percent of the payoff.

[15] 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 supplemental appendix online. All results remain robust if we control for the size of the village of origin of the resettled players. As lottery choices are endogenous we show in table C.9 in the supplemental appendix online separate regressions for A- and for B-senders for game 1 (there are only 18 C-senders which makes regression analysis unreliable). For A-players, who are nonrandomly determined as winners, the

resettlement coefficient is -242.48 and significant. This resembles a transfer reduction of 58 percent compared to the transfers of non-resettled A-players in game 1 (416.67 KHR). For B-players, it is -370.32 with a p-value of 0.055. Here the transfer decreases by about 43 percent compared to non-resettled B-players (855.56 KHR). Both coefficients are not statistically different from the total resettlement coefficient for game 1 (table 6, regression 1, -371.6, 47 percent of the transfer payments of the non-resettled players in game 1) which is confirmed by a t-test (p-value for -242.48: 0.47; p-value for -370.33: 0.99). Furthermore, the very small difference between a regression controlling for the risk choices of senders and receivers (table 6, regression 2) and a regression without these control variables (table 6, regression 5) shows that any bias caused by the endogeneity of risk choices is small.

[16] 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 supplemental appendix online). With all the matching methods we still find a significant negative coefficient of the resettlement dummy.

[17] We thank the anonymous referees for these suggestions. The resettlement coefficient remains significant and negative when we exclude all resettled players originating form the two/three nearest villages, which is about 14/17 percent of the sample of resettled players, and when we exclude all resettled players originating from the furthest /two furthest villages, which is about 13/48 percent of the sample of resettled players (table C.7 in the supplemental appendix online).

[18] As we played eight sessions in the resettled village, we had to pay special attention to communication issues because participants in later sessions might have been aware of the behavior of participants in earlier sessions and adjusted behavior accordingly. To understand whether this kind of contamination drove our results, we plotted the average transfer per session

against the timing of the session and indicated whether the session took place in the morning or in the afternoon (figure C.4 in the supplemental appendix online). We found no meaningful pattern. In a second step, we estimated a random effects Tobit regression for game 1 and game 2, including an ordinal variable measuring the timing of the session (running from 1 to 4 for both groups) and a dummy variable that took a value of 1 if the session took place in the afternoon (table C.8 in the supplemental appendix online). Both variables turned out to be insignificant in a regression including the whole sample (regression (1), N=156, observations=1,140) and in a regression restricted to the resettled players (regression (2), N=87, observations=660).

[19] Mean comparison tests for all three sender types over adequate receiver types are insignificant in all regressions.

[20] We thank an anonymous referee for pointing at the fact that commuting distance of the non-resettled players could influence solidarity. If commuting time is long the non-resettled farmers might need to rely more on village solidarity e.g. to watch their children. To test the influence of commuting distance we estimated a random effects Tobit regression for game 1 and game 2 for the non-resettled players including the distance between the resettled village, where the agricultural plots are situated and the non-resettled villages as a control variable (table C.10 in the supplemental appendix online). Commuting distance turns out insignificant.

[21] Further information on the economic situation of the project beneficiaries can be found in the supplemental appendix online (appendix D).

[22] Further robustness checks can be found in appendix C in the supplemental appendix online.

[23] Including the controls for sender and receiver types, the resettlement coefficient is, with -508.51, only slightly smaller than without any controls. Here attenuation caused by unobservables would have to be 13.35 times greater to explain away the resettlement effect.