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

Simone Gobien^{*a}, Björn Vollan^b

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 solidarity in established and newly resettled communities. Both communities are part of a land distribution project for which participants signed up voluntarily. Playing a version of the “solidarity game”, we identify the effect of voluntary resettlement on willingness to help fellow villagers. We find that resettled players transfer on average between 45% and 75% less money than non-resettled players. The social costs of voluntary resettlement seem significantly higher than is commonly assumed.

JEL classification: C93, O15, O22, R23

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

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1. INTRODUCTION

Land reforms in developing countries are believed to have the potential to eradicate food insecurity, to alleviate rural poverty and to reduce vulnerability to shocks due to higher income, larger savings, better access to the credit market, and increased returns to family labor. But households have to redirect time and effort to agriculture rather than to less risky activities thereby reducing income diversification as a common mean of informal insurance. Moreover, evidence on benefits of land reform is mixed. Valente (2009) shows for example higher food insecurity for land reform beneficiaries in South Africa, McCulloch and Baulch (2000) calculate only minor returns of land distribution to rural households in Pakistan concerning income smoothing and poverty reduction, and Ravallion and Sen (1994) claim that redistributive land reform in Bangladesh falls short to fulfill expectations for poverty reduction even if optimal circumstances are assumed.

Moreover, if resettlement is involved it is often neglected that the potential economic benefits for an individual farmer may be dampened by counteracting social effects of leaving a well-functioning, cohesive community. 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 and Lund 2003; Fafchamps and Gubert 2007). Due to the weakening of the ties to one's social network individuals lose access to mutual aid, informal credit and informal insurance (Dinh, Dufhues, and Buchenrieder 2012; Okten and Osili 2004; Attanasio et al. 2012). Most importantly, political institutions and social networks need to be re-established at the new destination in order for social norms to emerge that enforce solidarity, cooperation, trust and altruism and sanction free-riding and spite. Thus, coping with risks might become more difficult after resettlement as both reciprocal risk-sharing arrangements as well as solidarity towards others might be drastically lower. 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 (Dekker 2004; Barr 2003; Barr, Dekker, and Fafchamps 2010).¹ 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.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, 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. Similar to Barr (2003) we measure "solidarity" by implementing a "lab-in-the-field" experiment. Our participants are recruited from a land distribution project in rural Cambodia. We compare solidarity among voluntarily resettled farmers with solidarity among beneficiaries who stayed in their established villages (non-resettled farmers).

Barr (2003) argues that the lower level of trust in resettled communities is mainly the result of missing altruism. A trust game, however, might not be an adequate measure for altruism as it also measures risk and trust. The dictator game might be an easier way of measuring altruism, yet it is a very artificial measure (Bardsley 2008). Thus, we decided to use a modified version of the solidarity experiment (Selten and Ockenfels 1998) which captures transfers motivated by pro-social concerns like altruism and inequity aversion and in addition provides a measure for risk aversion. Selten and Ockenfels (1998, 518) define solidarity as the "willingness to help people in need who are similar to oneself but victims of outside influences such as unforeseen illness, natural catastrophes, etc." Hence, our experimental game mimics insurance against shocks based on unconditional help within

the village which are extremely important for resettled households but might be lost with resettlement inducing high social costs. The experimental game consists of two stages in which participants interact only with randomly chosen land reform beneficiaries from their 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 of three or not. This experimental set-up makes it possible to reduce disparities by equalizing game outcomes through the transfer of money. Moreover, it allows us to understand whether solidarity payments are influenced by the risk choice of the person in need (compare for example Trhal and Radermacher (2009) for the influence of self-inflicted neediness in the solidarity game). 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 solidarity expressed in the willingness to transfer payments to anonymous villagers.⁴

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 similar size for both groups. We hypothesize that transfers in the solidarity experiment are higher in the non-resettled villages.

In line with our hypothesis we find a sizeable reduction in the willingness to help others. Resettled players transfer on average between 47% 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 only 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. Most likely the lower income is due to lacking support of fellow villagers in planting, harvesting and selling their rice as well as in coping with shocks. 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. Compensation transfers for both voluntary and forced resettlement, made by the government, aid agencies or investors (e-g- "land grabbing"), need to consider these risks.

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 insurance against shocks based on unconditional help and measures willingness to transfer resources which is motivated by pro-social preferences as a proxy for solidarity on the village level. This is supported by our post-game questionnaire, as 96 % of all players see the similarity of the experiments with real life situations related to agricultural investment decisions incorporating different risk of failure and mutual support. Thirdly, 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. Lastly, and most importantly, we present evidence in interpreting our resettlement results as causal. It could be that resettled people are inherently different than non-resettled people in a way that affects both the settlement decision and the willingness to transfer. We address this concern in several steps: Our treatment and control groups were both willing to relocate

and thus share similar unobservable characteristics such as motivation to migrate and personality. They are closely homogeneous samples in terms of observable socio-economic factors due to the enforcement of eligibility criteria for the entire LASED project (i.e. also non-resettled participants fulfill the criteria to be resettled). Both groups have lived in their village of origin for at least four years and were therefore able to establish strong social ties. We confirm this with ex ante data showing that the groups did not differ in a range of observable socio-economic conditions and social embeddedness in their village of origin. We also perform several econometric robustness tests. Most importantly, following Altonji, Elder, and Taber (2005) and Bellows and Miguel (2009), we calculate that the selection on unobservables would need to be 15.62 times stronger than selection on observed variables in order to compensate the entire resettlement effect on solidarity transfers.

The paper relates to several strands in the literature. Firstly, our results complement the existing literature on the impact of resettlement. As the voluntary nature of resettlement is often questionable (Morris-Jung and Roth 2010; Schmidt-Soltau and Brockington 2007) most studies on social consequences concentrate on involuntary displacement e.g. because of “development projects”, natural catastrophe 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). But voluntary resettlement often combined with a land reform becomes increasingly common (see for example 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 highly needed. Our work introduces the notion of solidarity as an additional dimension in this context.

Secondly, our results fill an important gap in the literature on conflict resolution as land reform programs often intend to reverse historical inequalities and give poor people new

opportunities for their lives as for example in Southern Africa or Latin America. In line with psychological research that emphasizes the role of vulnerability, distrust, injustice and helplessness as significant belief domains that trigger or constrain conflict between groups (Eidelson and Eidelson 2003), Albertus and Kaplan (2013) and Mason (1998, 1986) have found a reduction in civil unrest due to land reform programs. Thirdly, our study relates to the literature on solidarity giving, confirming the importance of the social and economic setting to the emergence of solidarity (compare Ockenfels and Weimann (1999) and Brosig-Koch et al. (2011) for the consequences of economic and social differences within Germany, and more generally Henrich et al. (2001) and Leibbrandt, Gneezy, and List (2013) for the endogenous formation of social preferences).

The rest of the paper is organized as follows. Section 2(a) offers a brief introduction to the institutional setting and the selection of farmers for the resettlement project. Section 2(b) describes the socio-economic data before resettlement stemming from two earlier household surveys. Section 3 describes the field experiment we used to measure a person's propensity to express solidarity, our hypotheses for why solidarity should decrease with resettlement and socio-demographic variables of our subject pool. Section 4 identifies and quantifies the resettlement effect, followed by robustness tests and data on the importance of network transfers for project participants in real life. Section 5 summarizes and offers concluding remarks.

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 and

UNDP 2007).⁵ 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.⁶ The project is most advanced in Kratie Province, where we carried out our research. 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 farmers have to commute to their agricultural plots. The project beneficiaries (both resettled and non-resettled) had to be living in the project communes. They are the neediest people in the communities: to qualify they had to be landless or land-poor (i.e. owning less than half a hectare of agricultural land). According to estimations from the project staff, only between 1-2% of poor households, which would have been eligible for the project, did not apply. All applicants applied for both types of land agricultural and residential. Hence all of them were willing to relocate. As there was more demand for both agricultural and residential land than could be supplied, applicants were selected according to the degree of neediness.⁷ Residential land was granted to those households who did not have any residential land before the land allocation. However, we do not find any differences in housing conditions (size and material of the house) between households accepted for resettlement and those refused in our ex-ante data before land distribution (see Table 1). Moreover, both groups had similar income, land holdings, assets and

other socio-economic characteristics before land allocation. Therefore, our data does not suffer from bias caused by motivation to relocate and differences in poverty status.

Conditional on acceptance for the project, specific agricultural and residential land plots were allocated by lottery. 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. 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.). At the time of writing, around 10,000 hectares had been allocated to approximately 5,000 households.

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

With non-random selection of resettled farmers from the general population it is always hard to obtain an appropriate comparison group of non-resettled farmer. The advantage of this set-up for our experiment is that our two groups have many similarities: they were all willing to relocate, come from the same 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.⁸ Most importantly, the vast majority of beneficiaries in both groups had lived in the project communes for at least four years and could therefore establish strong social relations, Moreover, we use data originating 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) to see whether resettled and non-

resettled households differ in terms of in social integration before resettlement. In both samples around 55% of the households received both residential and agricultural land and 45% received only agricultural land. We do not have completely reliable information on the social capital but 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. 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, 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, as well as different relevant household assets in 2008.⁹

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 ^b
	N	Mean	Std dev	N	Mean	Std dev	Significance level
Variables for social integration							
Member of self-help group ⁺	63	0.12	0.33	43	0.11	0.32	n.s. ^a
Number of wedding celebrations	43	6.12	5.23	41	6.15	5.42	n.s.
Times of visiting the pagoda	43	7.53	9.61	41	7.68	7.43	n.s.
Informal credit	43	98.41	25.40	41	100.42	26.96	n.s.
Total credit	43	169.04	226.59	41	192.80	242.11	n.s.
Housing conditions							
Size of the house ^c	43	1.46	0.59	41	1.68	0.72	n.s.
Main material of the roof ^d	43	1.51	0.70	41	1.41	0.67	n.s.
Main material of the exterior walls ^e	43	1.32	0.47	41	1.27	0.50	n.s.
General condition of the house ^f	43	1.84	0.57	41	1.90	0.62	n.s.
Socio-demographic variables							
Income per month (USD)	43	123.30	157.23	41	111.77	106.87	n.s.

Land before the project start (hectare)	43	0.28	0.64	41	0.27	0.57	n.s.
Savings ⁺⁺	43	0.60	0.49	41	0.59	0.50	n.s.
Nutrient provision ⁺⁺⁺	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.
Age of household head	43	41.37	9.43	41	42.17	10.85	n.s.
Household head is married ⁺⁺	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: ^a n.s. not significant

^b 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

^c 20 square meters or less (1) / 21–50 square meters (2) / 51 square meters or more (3)

^d Thatch, palm leaves, plastic sheet, tarpaulin or other soft materials (1) / Corrugated iron (2) / Tiles, fibrous cement, or concrete (3)

^e Saplings, bamboo, thatch, palm leaves, or other soft materials (1) / Wood, sawn boards, plywood, corrugated iron (2) / Cement, bricks, concrete (3)

^f In dilapidated condition (1) / in average condition, livable (2) / in good condition and safe (3)

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

+++ Months enough to eat during the last year

In our data we do not find differences between our two groups for a set of socio-economic characteristics. It might still be the case that the project identified differences which are correlated with both resettlement and willingness to transfer money. As a robustness check we use the extent of attenuation of our estimation results to calculate the bias caused by omitted variables which would be necessary to explain our results (compare Altonji, Elder, and Taber 2005; Bellows and Miguel 2009).

A further robustness test is to estimate a difference-in-difference (d-i-d) regression that, given parallel time trend assumption, provides an unbiased resettlement effect for certain outcome variables related to solidarity transfers, and to compare the obtained d-i-d 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.¹⁰ Thus, we do not expect a large bias when using simple ex-post measure of solidarity in our experiment. Lastly, we also provide different matching estimations for our experimental solidarity measure that also suggest that there is no strong selection bias in resettlement.

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 solidarity experiments

Our experiment consists of a risk stage followed by a solidarity stage. Each participant was randomly allocated to two other players that formed a group. When making their risk decision participants knew about the second stage. However, they neither knew with whom they were paired nor could they communicate. Our risk lottery follows an ordered lottery selection design adapted from Binswanger (1980; 1981) (see Table 2).¹¹ 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 risk game 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 solidarity at the village level independent of reputation and reciprocal network ties. Therefore we implemented an anonymous one-shot solidarity experiment in the second stage. 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. However, since winning option B or C is determined by pure chance the sample of winners does not differ from the losers. 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.

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)

In addition we randomly choose half of our sessions in which we played a second independent game.¹² Here, we replaced the random winning mechanism of the risk game with a skilled task to test whether solidarity is lower when winners feel that they “earn” their money. Following Gneezy, Leonard and List (2009), we set the task of throwing a ball into a bucket.¹³ 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 made the solidarity decisions.

Those who participated in two games were aware of whether they had won or lost in the previous games, but we did not reveal transfer decisions. We informed those participants 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 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.¹⁴

(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.

Instructions were read out loud by the same person 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. To reduce the complexity of the game, every player practiced in addition the risk game by playing a practice game which was independent of the actual game.¹⁵ 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: 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. Here no practice game took place.

As Table 3 shows, 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). Each player made six transfer decisions, leading to 756 observations. For game two with the skilled task, 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).

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

	1st game		2nd 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)

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.¹⁶ 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.

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

	Resettled, N= 127		Non-resettled, N= 98		Difference in means ^b
	Mean	Std dev	Mean	Std dev	Significance level
Income per month (USD)	124.40	101.89	113.52	85.71	n.s.
Savings ⁺	0.27	0.44	0.40	0.49	5%

Nutrient provision ⁺⁺	2.65	0.48	2.63	0.48	n.s.
Household size	5.46	1.88	5.74	1.92	n.s.
Gender of experimental participant (1= female, 0= male)	0.58	0.49	0.58	0.49	n.s. ^a
Experimental participant is household head ⁺	0.48	0.50	0.50	0.50	n.s.
Age	37.08	10.66	41.14	12.31	1%
Married ⁺	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 ⁺⁺⁺	10.54	12.00	19.71	22.10	1%
Relative number of family members ⁺⁺⁺	2.24	5.59	7.47	11.52	1%

Notes: ^a n.s. not significant

^b 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

(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 solidarity, and iii) fewer family members and friends taking part in the session.

In the second 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, and Gjerstad 2007), “mental accounting” (Cherry and Shogren 2008), or a reduction of the supply effect in experimental economics (Carpenter, Liati, and Vickery 2010). Furthermore, losers in the skilled task are fully 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) Descriptive analysis

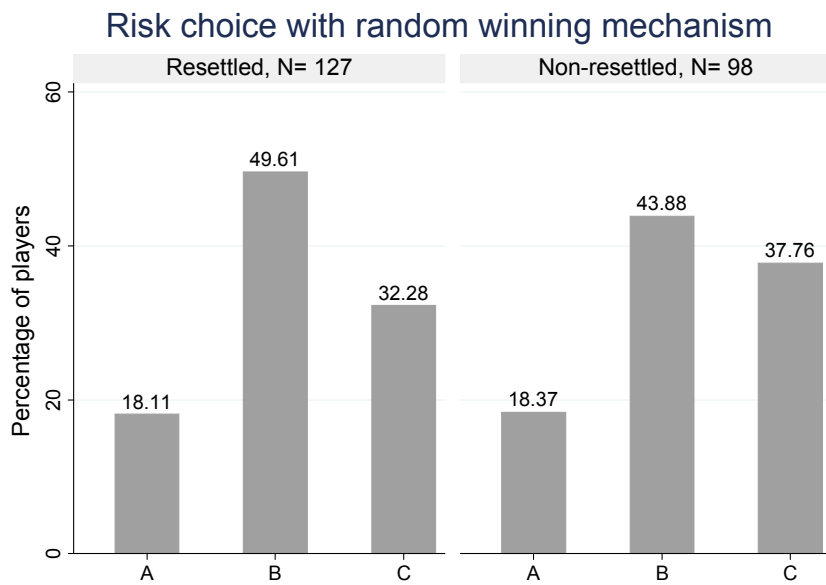
Transfers in the second stage are contingent on winning the random mechanism in game one and the skilled task in game two and therefore on the choice of the players in the first stage. Figure 1 shows choices of resettled and non-resettled participants for the first stage.¹⁷ For both games we do not find a significant difference in choices between the resettlement groups.¹⁸

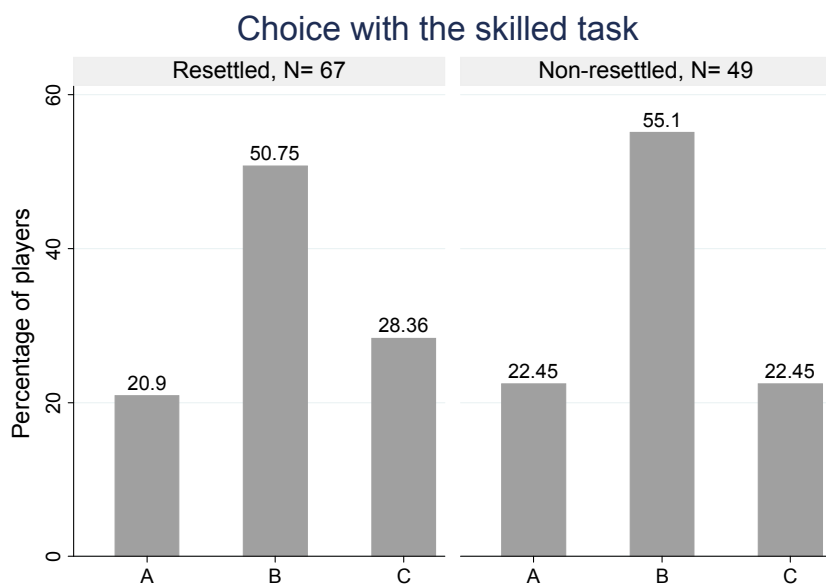
Due to the combination of the risk game with the solidarity game a player might expect a non-zero payoff in the event of losing the game (depending on the player’s expectation of transfers from fellow villagers). Hence the risk of losing can be partly shared within the solidarity group and transfers 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.

After the player took her risk choice but before rolling the die (or throwing the ball), we ask her to state how much transfer she expects from a player winning the different risk options. Hence expectations are contingent on own risk choice and the possibility of losing. Therefore expectations are only available for players who were at risk of losing the risk game (risk option B or C). In line with our interpretations, 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) likely being caused by stronger solidarity in the established villages.¹⁹

Fig. 1: Choice of non-resettled and resettled players with the random winning mechanism and the skilled task





Analyzing transfer sending of winners to losers in game one Table 5 shows that 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.²⁰ 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).

Table 5: Mean transfers in game 1 and game 2 with the skilled task

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

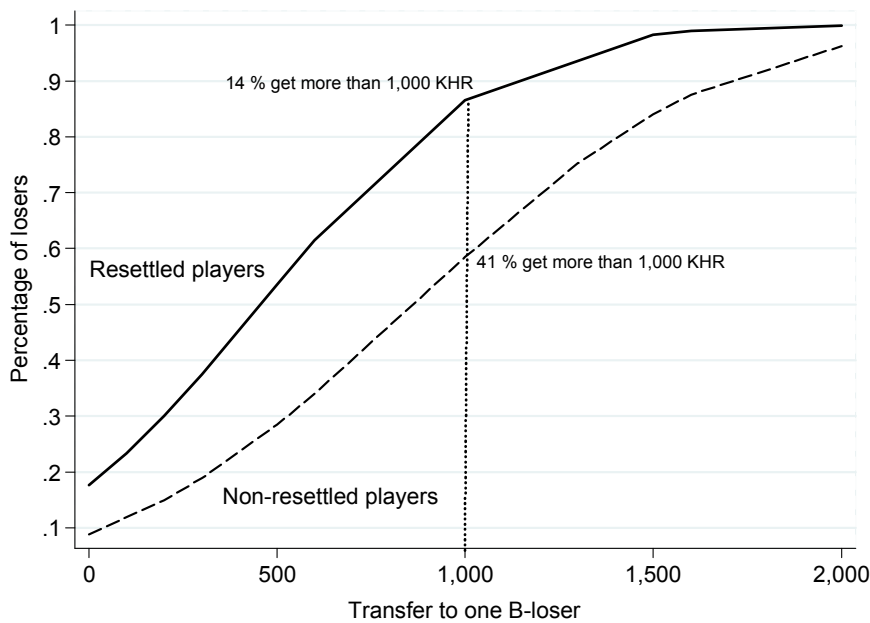
Note: ^a Wilcoxon-Mann-Whitney test for difference in means between resettled and non-resettled players

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 per person was lower 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) but the total sum of transfers is bigger in the case of two losers. 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).²¹ Higher relative contributions of less wealthy people are also found in public good games (Hofmeyr, Burns, and Visser 2007; Buckley and Croson 2006). Thirdly, there is no evidence that senders discriminate over the risk choice of the loser. This holds both in resettled and non-resettled communities. Contrary to Trhal and Radermacher (2009) who played with German university students, we find no evidence that wealthy individuals help less if they realize that neediness is self-inflicted. Given the importance of 'fate' in asian countries this seems not too surprising. High risk participants who are incautious are not “punished” with lower transfers. Average sending to C-losers has a tendency to be lower but this difference is small and insignificant. We also do not find any evidence of homophily or in-group bias with higher transfer sending towards people with the same risk choice. If high risk investments are insured the same way as low risk investments there does not seem to be an innovation bias caused by a lack of insurance.

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.

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



(b) 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.²² 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 one with the random winning mechanism (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 two 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.

The solidarity experiment further includes elements of trust, since transfers depend on expectations about the solidarity of others (Selten and Ockenfels, 1998). To separate the effects of solidarity from reciprocal motives, we include transfer expectations in regression (3) (N= 112, observations= 810). 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.²³

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. 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 solidarity 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 (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).

Table 6: Multivariate analysis explaining transfers (marginal effects)

	(1)	(2)	(3)	(4)	(5)	(6)
	Tobit regression ⁺	Random- effects Tobit regression ⁺⁺	Random- effects Tobit regression ⁺⁺	Random- effects Tobit regression ⁺⁺	Random- effects Tobit regression ⁺⁺	Random- effects Tobit regression ⁺⁺
VARIABLES	Transfers game 1 (risk choice)	Transfers game 1 and 2 (skilled task)	Transfers game 1 and 2 (skilled task) for B- and C- senders	Transfers game 1 and 2 (skilled task)	Transfers game 1 and 2 (skilled task)	Transfers game 1 and 2 (skilled task)
Resettlement	-371.6** (179.9)	-549.7*** (151.5)	-413.9** (197.6)	-590.6*** (140.6)	-556.8*** (160.3)	-514.5*** (152.2)
Skilled task		-100.9*** (28.93)	-186.2*** (40.53)	-100.1*** (28.92)	-107.9*** (30.00)	-106.2*** (30.03)
Transfer expectations			0.424*** (0.137)			
Controls for session	Yes	Yes	Yes	No	Yes	No

network						
Controls for sender and receiver type	Yes	Yes	Yes	Yes	No	No
Individual controls	Yes	Yes	Yes	Yes	Yes	No
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
+ 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.

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 -371.6 KHR to -590.6 KHR. Thus, resettled players transfer between 47% and 75% lower amounts than non-resettled players in game one (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.²⁴ The magnitude of this coefficient with -100.9 KHR in regression (2) is more than five times smaller than the resettlement effect.²⁵ Confirming our descriptive results we do not find in-group bias or significant discrimination with respect to risk taking of the loser for all three sender groups.²⁶

It is 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, 2150) or to put it into formal saving accounts with effectively negative interest rates (Dupas and Robinson 2013). Since non-resettled households are significantly more likely to have savings, these findings reduce the size of our resettlement effect.

Considering the non-random nature of the resettlement choice, the work of McKenzie, Stillman, and Gibson (2010) provides some information on the magnitude of the bias. Comparing income improvements after migration, McKenzie, Stillman, and Gibson (2010) 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 -357.3 KHR and 45% of the average transfer payment of the non-resettled players in game one (792.3 KHR), 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. Comparing regression (2) with a resettlement coefficient of -549.7 Riel (including full controls) with regression (6) leading to a resettlement coefficient of -514.5 Riel (without any controls), shows that attenuation is with 35.2 Riel 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 the entire resettlement effect. Given the rich set of control variables this seems highly unlikely.²⁷

(c) 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 solidarity 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 solidarity 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 which got residential land and those who did not get residential land (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 in clearing the field, planting, weeding, harvesting, selling, etc.). 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 is with around 25% of income excluding transfers for the resettled and 30% of income excluding transfers for the non-resettled project members, the second most important income source. But, 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, solidarity and solidarity inside the new village would become essential for the farmers if they are to succeed.

5. CONCLUSIONS

Solidarity is required for the well functioning of communities and even the society as a whole. But, because other-regarding preferences evolve endogenously depending on the context (compare for example Ockenfels and Weimann (1999) and Brosig-Koch et al. (2011) who identify significant differences in solidarity (and cooperation) between East and West German subject tracing back to opposing economic and social history) close monitoring of interventions is needed.

The aim of this paper is to investigate systematic the impact of resettlement on the propensity of individual's to express solidarity norms with fellow villagers. We carry out this investigation in the context of a unique resettlement project in Cambodia. We conducted a lab-in-the-field

experiment comparing voluntarily resettled and non-resettled participants of a land reform project. All farmers applied for residential and agricultural land and were hence willing to relocate but 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) 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 compare transfer between the resettled village and the non-resettled villages.

We found that resettled players in the experimental game transferred on average between 47% and 75% 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” carried out in less carefully designed resettlement programs. 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 an experiment to also 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 solidarity 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; instead, the norm of solidarity applies similarly to everyone and is on average 10% of available income. However, we find a slight decrease in solidarity when participants could earn their experimental money.

Our findings on solidarity transfers in the experiment also relate to the lower real world income of resettled project participants after resettlement. Solidarity transfers are related to ‘social capital’ which is thought to be an important ingredient for coping with shocks and production (planting, weeding, harvesting, and selling). 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 solidarity was very low. With the loss of solidarity, our study identifies an important effect of voluntary resettlement that has not been fully explored up to now. Most likely the lower income of resettled farmers (although they did not need to commute) is stemming from the lack of mutual aid in production.

Land reforms are high on the international agenda but studies on their consequences mostly concentrate on economic variables, revealing mixed results, or on involuntary resettlement. Therefore, we believe that our study presents useful insights on the social and economic losses caused by voluntary resettlement within a land reform program. Our results of lower solidarity in

resettled communities complement the analysis of Barr (2003) who finds that resettled players show lower trust. While trust is important for investment and reciprocal arrangements, solidarity is a better measure for altruism.

Our findings are relevant for resettlement policies based on the “economics of compensation”, which often neglect these and other social costs by offering too small compensation amounts. They have important implications for the design of resettlement policies underlining that the provision of community building measures and their monitoring is mandatory. Moreover, the availability of insurance against shocks in developing countries and in particular in remote rural areas is urgently needed. There are several directions for future fruitful research. Monitoring social changes and their interaction with economic changes over time, both in the medium and in the long run, would provide deeper guidance for policy makers. Thereby, solidarity and reciprocal relations within the resettled village, but also the development of networks with the village of origin should be in the focus.

NOTES

¹ Unsurprisingly, forced resettlement can lead to a poverty trap consisting of poor harvest 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 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.

⁴ 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 et al. 2009; Ligon and Schechter 2012).

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 (one % of the rural population) are living at risk of forced eviction.

⁶ 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 and UNDP 2007). Since the yearly average rice yield between 2000 and 2008 was 2.26 ton/ hectare (Yu and Fan 2011), the distributed land parcels provide a good opportunity for the project participants.

⁷ Out of 1,139 applicants 525 households were selected as land recipients.

⁸ There is thus no influence of social distance due to variation in nationality, education, occupation, race, or religion between the two groups.

⁹ 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.

¹⁰ The same holds true if we restrict the sample to those households with panel data.

¹¹ This game was also used by Barr and Genicot (2008) in Zimbabwe.

¹² Due to time constraints we could not play a second game in all sessions.

¹³ Gneezy, Leonard, and List (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.

¹⁴ The experimental protocol and posters used for visualization are included in appendix D and E.

¹⁵ Even though, the practice game is 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 of the outcome is identified.

¹⁶ 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.

¹⁷ We assume an ordinal scale: option A=1, option B= 2, and option C= 3.

¹⁸ 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.

¹⁹ Additional information on the risk choice with the skilled task is provided in appendix B.

²⁰ As there is no significant difference between the mean risk choices of resettled and non-resettled players in both games, we only report mean solidarity transfers independent of the risk choice. But also comparing those players who made the same risk choice in game one and in the skilled task shows a significant reduction in transfer sending (N= 21, game one: 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 one 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 one, considerably more resettled players sent no transfer than non-resettled players.

²¹ 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.

²² 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.

²³ The resettlement coefficient also remains significant and of relevant size if we run the estimation only on A-senders which are non-randomly determined as winners (-345.9 Riel on the 1% significance level) and if we run the estimation on B- and C- senders excluding expectations for which winning is randomly determined (-529.3 Riel on the 1% significance level).

²⁴ 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 skilled task. All interaction terms turn out to be insignificant.

²⁵ 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 two 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.

²⁶ Mean comparison tests for all three sender types over adequate receiver types are insignificant in all regressions.

²⁷ 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 bigger to explain away the resettlement effect.

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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 ⁺ Wedding celebrations	Ex-post Wedding celebrations	D-i-d ⁺ Pagoda visits	Ex-post Pagoda visits	D-i-d ⁺ Informal credit	Ex-post Informal credit	D-i-d ⁺ Income per year	Ex-post Income per year	D-i-d ⁺ Income per year without transfers	Ex-post Income per year without transfers
Interaction resettlement and ex-post dummy	-2.706 (2.703)		-1.427 (2.765)		-8.999 (41.27)		-253.0 (446.7)		-370.0 (444.6)	
Resettlement dummy	1.830 (2.003)	-0.876 (1.353)	-0.148 (2.049)	-1.575 (1.968)	-2.007 (30.58)	-11.01 (22.05)	138.3 (331.0)	-114.6 (282.7)	143.3 (329.5)	-226.7 (281.3)
Ex-post dummy	-0.588 (2.003)		0.178 (2.049)		-47.73 (30.58)		87.80 (331.0)		-126.9 (329.5)	
Constant	6.146*** (1.433)	5.558*** (1.043)	7.683*** (1.466)	7.860*** (1.517)	100.4*** (21.88)	52.70*** (17.00)	1,341*** (236.8)	1,429*** (217.9)	1,302*** (235.7)	1,176*** (216.9)
Observations	190	106	190	106	190	106	190	106	190	106
R-squared	0.018	0.004	0.005	0.006	0.036	0.002	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

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

	Interaction resettlement and ex-post dummy of d-i-d estimation	Resettlement dummy of ex- post estimation	Significance level of test for equality
Wedding celebrations	-2.706	-0.876	n.s. ^a
Pagoda visits	-1.427	-1.575	n.s.
Informal credit	-8.999	-11.01	n.s.
Income per year	-253.0	-114.6	n.s.
Income per year without transfers	-370.0	-226.7	n.s.

Notes: ^a n.s. not significant

APPENDIX B: RISK CHOICE IN GAME TWO WITH THE SKILLED TASK

In game two, the average risk choice in the skilled task is significantly lower than the average risk choice in game one (game one: 2.19, game two: 2.04, p-value: 0.05, see also Fig. 1). This reduction is driven by the less confident non-resettled players who decreased their risk significantly (non-resettled: game one: 2.24, game two: 2.00, p-value: 0.02; resettled: game one: 2.14, game two: 2.07, p-value: 0.54). There is no significant difference in risk choice with the skilled task between resettled and non-resettled players (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 hint at overconfidence especially among the resettled players.

APPENDIX C: ADDITIONAL ANALYSES ON TRANSFER SENDING

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

	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 sender
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 receiver	688.09	661.90	579.76	564.68	592.06	576.19	-
Significance level ^b	n.s.		n.s.		10%		-

Notes: ^b 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 risk choices in game 1

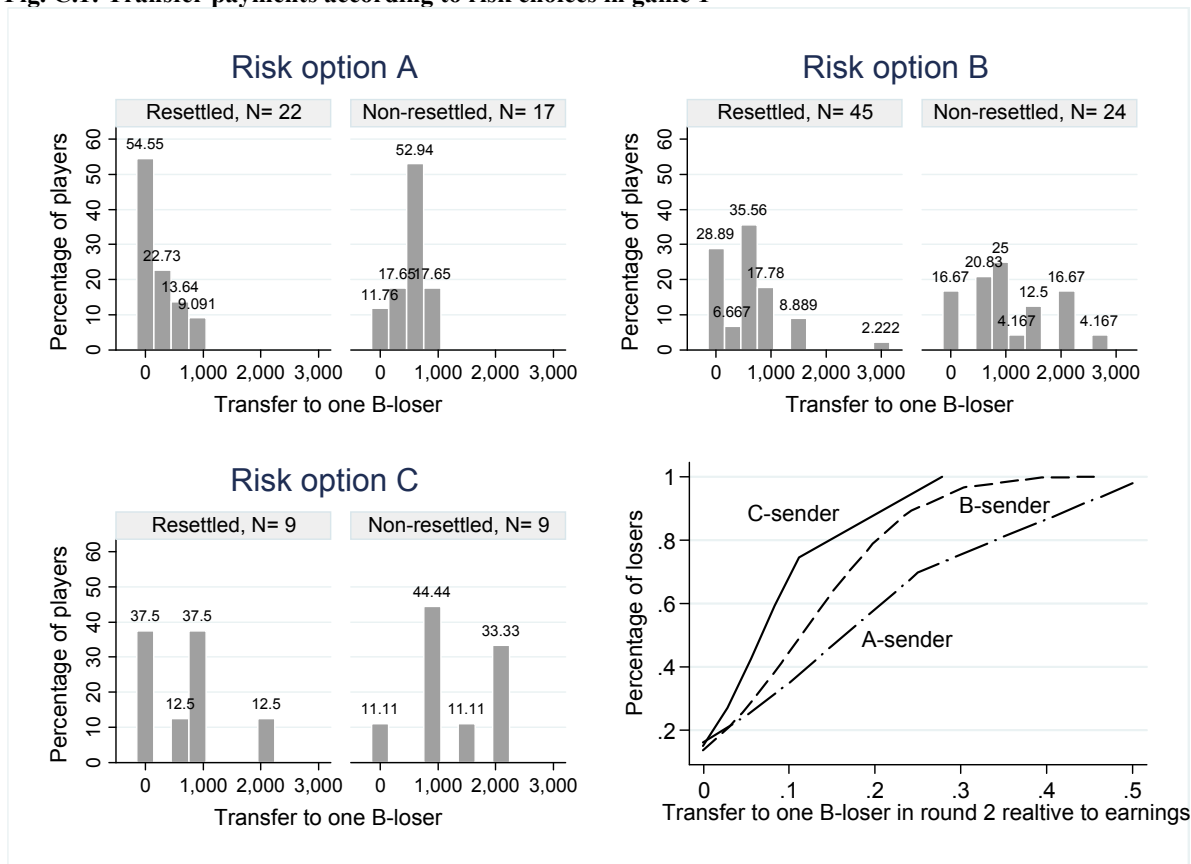


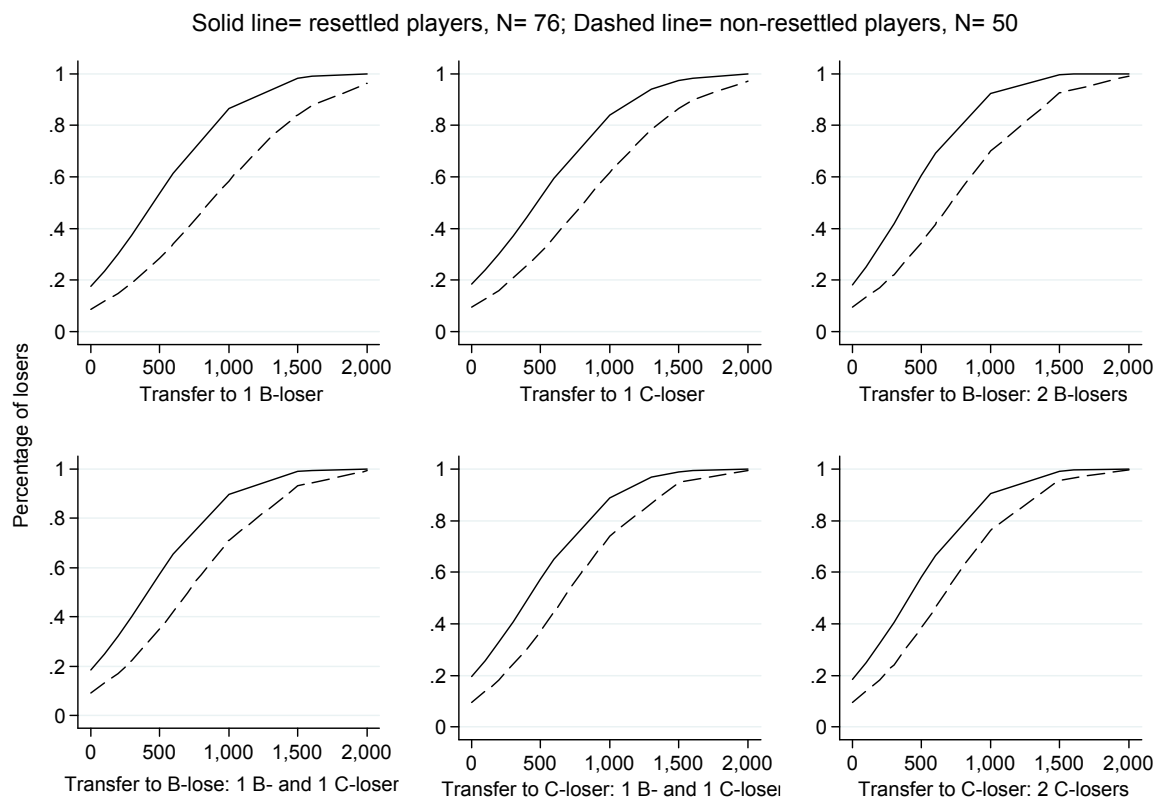
Fig. C.2: Gaussian probability curves of transfer payments in game 1 of resettled and non-resettled players

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

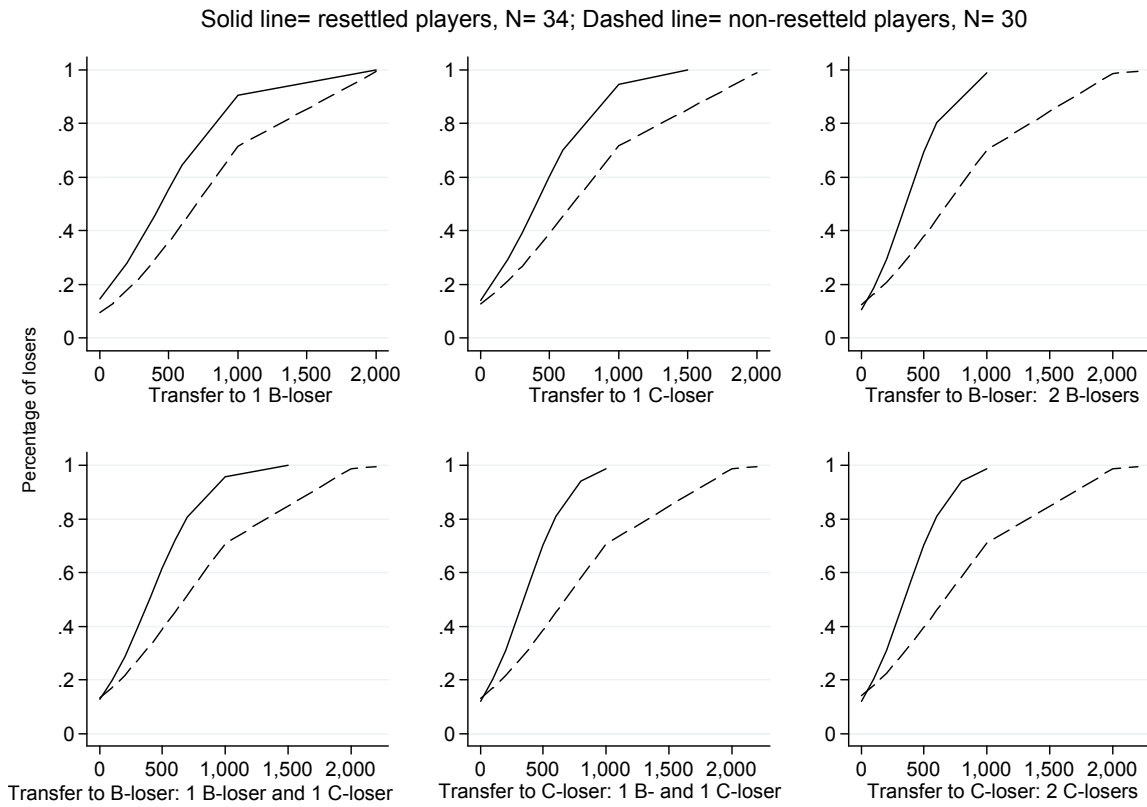


Table C.2: Individual control variables for the transfer regressions in table 6

VARIABLES	(1)	(2)	(3)	(4)	(5)
Gender of experimental participant (1= female, 0= male)	-74.77 (159.4)	-108.0 (134.9)	-151.0 (177.8)	-114.0 (133.3)	-92.66 (143.4)
Age	2.887 (6.514)	3.278 (6.085)	1.814 (7.949)	3.188 (6.101)	3.016 (6.393)
Years of education	65.74** (26.05)	45.70* (26.59)	46.03 (33.84)	45.97* (26.67)	56.00** (28.09)
Household size	-61.73 (37.89)	-91.25*** (35.06)	-62.24 (46.40)	-86.00** (34.79)	-93.14** (36.88)
Married ⁺	198.1 (163.7)	139.7 (163.4)	237.8 (202.5)	136.5 (163.8)	106.0 (172.2)
Income per month	-0.101 (0.650)	0.147 (0.713)	-0.329 (1.002)	0.0631 (0.710)	0.0270 (0.755)
Nutrient provision ⁺⁺	428.1** (173.6)	360.6** (141.7)	284.4 (189.4)	339.0** (139.5)	384.7** (149.6)
Savings ⁺	-378.3*** (142.9)	-373.4*** (137.1)	-467.1** (186.1)	-358.6*** (136.4)	-398.7*** (144.9)
More than 50 USD debt ⁺	39.64 (131.7)	146.4 (136.9)	87.57 (185.6)	134.4 (136.8)	189.0 (144.6)
Shock during the last 3 years ⁺⁺⁺	-83.75 (123.3)	-26.84 (137.3)	106.6 (167.5)	-13.08 (136.9)	-37.04 (145.2)
Shocks of friends or family ⁺⁺⁺	272.4* (148.4)	157.8 (132.1)	196.1 (163.1)	156.5 (132.3)	131.5 (140.1)
Relative number of friends ⁺⁺⁺⁺	0.761 (4.435)	3.653 (3.838)	0.0750 (5.164)		4.613 (4.057)
Relative number of family members ⁺⁺⁺⁺	1.834 (7.335)	0.735 (7.358)	-5.043 (10.46)		0.158 (7.774)
Responsibility for own fate ⁺⁺⁺⁺⁺	114.7 (118.6)	121.7 (122.0)	70.57 (159.8)	121.5 (122.1)	147.8 (129.2)
Always somebody in the village who helps ⁺⁺⁺⁺⁺	-123.3 (109.6)	-98.47 (111.2)	-147.4 (141.3)	-93.01 (111.3)	-89.97 (118.1)
Session size	-13.78 (36.13)	13.71 (27.33)	2.698 (33.64)	19.07 (26.88)	11.42 (29.06)

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	-5.927 (34.42)	-30.43 (56.92)	-70.17 (118.7)	-30.42 (56.92)	
Sender A & 2 receivers B B - receiver B	-84.87*** (29.54)	-91.93 (57.13)	-99.39 (118.9)	-91.89 (57.12)	
Sender A & 2 receivers B C - receiver B	-74.40** (29.69)	-88.49 (57.12)	-116.2 (119.1)	-88.45 (57.11)	
Sender A & 2 receivers - B C receiver C	-71.41** (34.25)	-88.49 (57.12)	-124.6 (119.1)	-88.45 (57.11)	
Sender A & 2 receivers C C - C receiver	-72.90** (30.85)	-89.63 (57.12)	-124.6 (119.1)	-89.60 (57.12)	
Sender B & receiver B	426.7*** (149.8)	323.5*** (68.02)	251.2** (125.0)	324.8*** (68.01)	
Sender B & receiver C	362.6** (149.6)	266.9*** (68.06)	195.1 (125.0)	268.2*** (68.05)	
Sender B & 2 receivers B B - receiver B	241.3* (139.3)	141.9** (68.17)	57.79 (125.1)	143.1** (68.16)	
Sender B & 2 receivers B C - receiver B	268.2* (142.6)	173.8** (68.14)	95.26 (125.1)	175.1** (68.13)	
Sender B & 2 receivers B C - receiver C	227.1 (142.6)	127.5* (68.18)	42.05 (125.1)	128.8* (68.17)	
Sender B & 2 receivers C C - receiver C	228.7 (141.8)	122.8* (68.19)	44.47 (125.1)	124.0* (68.18)	
Sender C & receiver B	863.7*** (324.4)	243.9* (127.7)	161.1 (173.5)	250.2** (127.5)	
Sender C & receiver C	921.9***	292.9**	212.9	299.1**	

	(338.5)	(127.6)	(173.3)	(127.4)
Sender C & 2 receivers B B - receiver B	921.9***	298.3**	218.6	304.6**
	(351.3)	(127.6)	(173.3)	(127.4)
Sender C & 2 receivers B C - receiver B	892.8**	271.2**	189.9	277.4**
	(356.7)	(127.6)	(173.4)	(127.5)
Sender C & 2 receivers B C - receiver C	921.9**	298.3**	218.6	304.6**
	(359.6)	(127.6)	(173.3)	(127.4)
Sender C & 2 receivers C C - receiver C	834.6**	216.6*	132.2	222.9*
	(358.3)	(127.7)	(173.5)	(127.6)

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 sessions

	Obs. resettled players	Ob. non-resettled players	Average treatment effect on the treated*	Std. err.	T- value
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	145.34	-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.

Table C.5: Transfer differences based on matching results according to income

	Obs. resettled players	Ob. non-resettled players	Average treatment effect on the treated*	Std. err.	T- value
Stratification method	456	300	-297.77	50.60	-5.89
Nearest neighbour (random draw)	456	282	-317.11	56.70	-5.59
Kernel matching (with bootstrapping ,repetitions 50)	456	300	-300.24	119.17	-2.52
Radius matching (0.01)	456	300	-348.95	55.86	-6.25

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

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Playing with the social network: Social cohesion in resettled and non-resettled communities in Cambodia

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 distribution 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 47% and 74% 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.

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