WPS7382

Policy Research Working Paper 7382

Self-Help Groups, Savings and Social Capital

Evidence from a Field Experiment in Cambodia

Radu Ban
Michael J. Gilligan
Matthias Rieger

WORLD BANK GROUP
Development Research Group
Impact Evaluation Team
July 2015

Policy Research Working Paper 7382

Abstract

This paper studies how self-help groups—village-based organizations designed to encourage savings, household production and social cohesion among the poor—can promote economic and social capital. The paper uses survey data and a wide array of social capital measures to assess the impact of a pilot program that was randomly rolled out in rural villages in Cambodia. The study finds that the program encouraged savings and associations via self-help groups. However it did not improve social capital measured by household and network surveys and lab activities that gauge trust, trustworthiness and the willingness to contribute to public goods. The findings contradict recent work that has found significant positive impacts of such groups on social capital. This paper evaluates community-wide impacts while most previous studies focus on program participants. In addition, the empirical strategy is based on a broader array of social capital measures, including behavioral indicators, suggesting that finding impacts of such programs on social capital is sensitive to the measurement strategy.

This paper is a product of the Impact Evaluation Team, Development Research Group. It is part of a larger effort by the World Bank to provide open access to its research and make a contribution to development policy discussions around the world. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The corresponding author may be contacted at mg5@nyu.edu.

Self-Help Groups, Savings and Social Capital: Evidence from a Field Experiment in Cambodia

Radu Ban,[1] Michael J. Gilligan[2] and Matthias Rieger[3]

Keywords: social capital, poverty, savings, self-help groups, field experiments, Cambodia

JEL Classification Codes: O1, I38, J16, Q1,D7, C93

[1] Bill and Melinda Gates Foundation
[2] Corresponding Author: Department of Politics, New York University, 19 West 4th Street 2nd Floor, New York, NY 10012. Phone: (212) 998-8519. Email: mg5@nyu.edu.
[3] International Institute of Social Studies of Erasmus University, Rotterdam

1. Introduction[4]

Can policy-makers promote both economic empowerment and social capital among the poor in developing countries? To answer this question, we study the impact of a World Bank program that established self-help groups in Cambodian villages on households' economic welfare and social capital. Self-help groups (SHGs) are "village-based organizations that focus on building the savings and credit as well as social empowerment of their (mostly female) members" (Desai and Joshi 2013) and provide scope for mutual, economic assistance (Fafchamps and La Ferrara 2012). SHGs differ from traditional microfinance groups in that they rely on no external financing. Members of the SHG pool their own savings and loan it to members of the group according to specified rules. Self-help groups have been found to benefit consumption and assets accumulation (Deininger and Liu 2013b) and improve food security, consumption smoothing and saving (Beaman et. al. 2014). They may be used as a commitment device, or a peer-pressure instrument to increase precautionary savings (Kast et al. (2012).

The evidence that SHGs promote broader social improvements, specifically the accumulation of social capital, has been somewhat mixed. Deininger and Liu (2013a) report increases in social capital, Desai and Joshi (2013) describe greater civic engagement among SHG members and Casini and Vandewalle 2011) argue SHGs fostered collective action of socially disadvantaged women. However in their study of SHGs in Mali, Beaman et al (2014) find no effect of the program on social capital. This lack of consensus is important because it stands in stark contrast to the celebrated findings of the increases in social capital enjoyed by microfinance groups in India (Feigenbaum et. al 2013).

[4] **Acknowledgements:**
This research collaboration was initiated by Development Impact Evaluation's (DIME's) IE in Fragile States program and received funding from the Knowledge for Change Program (KCP). In addition to Radu Ban (co-author; formerly with DIME), the team would like to thank Marcus Holmlund for his role in client engagement, data collection, and securing funding for the study. We are grateful to Younell Hay, Top Neth and Mok Tonh of the LEAP team and to Mudita Chamroeun, World Bank LEAP task team leader, for supporting this impact evaluation. We would also like to thank Ramji Dhakal, Ly Vouchlong, Pong Pheakdey Boramy, Chea Bunnary, Hang Chansophea, Soklang Kheang, Va Wisal, and SBK Research and Development for collecting the data used in this study.

We add to this discussion with evidence from a randomized study of LEAP (Livelihood Enhancements and Associations among the Poor), a World Bank sponsored pilot of a self-help-group program in Siem Reap, Cambodia. Our main contribution is a more extensive set of social capital measures than have been used in this literature so far. Putnam (2000, p. 19) defines social capital as "... social networks and the norms of reciprocity and trustworthiness that arise from them.''[5] Thus network and norms are the fundamental components of social capital and we designed our research to measure these components as directly as possible. We measured networks by carefully recording our subjects' relationships with each other in a set of groups that extensive focus group research revealed to be the most important in the region. We used lab-in-the-field techniques to measure subjects' norms by observing their behavior in a carefully incentivized, controlled laboratory setting rather than simply from their self-reported responses to a survey. We also conducted a standard survey in which we asked them, *inter alia*, about their social activities.

We designed our evaluation to test three propositions: whether the program increased savings and access to credit, whether it produced income and livelihood enhancements and finally whether the program increased civic engagement and social capital among the poor. In line with previous evidence, we find some economic impacts. Yet these impacts have not translated into the accrual of social capital. We find that LEAP had a large and positive effect on savings in communities. Indeed, our estimates indicate that, in the program communities, poor members (on whom the program was targeted) outsaved non-poor members. We also found that incomes from sales of meat and fish rose in the program communities, which is consistent with LEAP's creation of a relatively large number of poultry and pork producers groups.[6] Finally, while there is strong evidence that the program fostered participation in savings groups there is no consistent evidence that that participation in these savings groups generated the hoped-for spillovers in participation in

[5] Rothstein (2005) defines it similarly. Also see the review in Portes (1998).

[6] There are some perplexing patterns in the data, discussed in greater detail below, that raise questions about the validity of this finding, however.

other social networks, in greater civic engagement by the poor or in more pro-social norms of behavior.

There are several possible explanations for the discrepancy between the null results (ours and those of Beaman et. al.) and the positive ones. First, there is a fundamental difference in the comparisons in the various studies. Our treatment and the treatment of Beaman et. al, is at the community level: we compare the behavior of residents from treated communities to residents of control communities, so our treated and control groups contain both members and SHGs. The treatment in Feigenbaum et al. (2013) is the frequency of micro-credit group meetings, and they only study group members, not non-members. In their observational studies Deininger and Liu (2013a) and Casini and Vandewalle (2011) compare SHG members to non-members. Desai and Joshi (2013) did administer their treatment at the community level, so we suspect the difference between their results and ours stems from our different measures. They quiz respondents about their knowledge of authorities in their village and whether they have lodged complaints with those authorities. These are plausible measures of civic engagement, but they do not measure norms or social networks as we do. Feigenbaum et al. (2013) employ network measures that are a subset of the type we use. The other studies measure social capital via self-reported actions in retrospective surveys. Thus our use of behavioral laboratory activities represents a contribution to this literature and a departure from the earlier studies.

The paper is organized as follows: we first give some background information on the evaluated program. Thereafter, we outline our empirical strategy to measure economic outcomes and social capital, the sampling procedure, as well as the empirical model. We then present impacts of the program on economic outcomes such as savings and economic livelihoods, as well as on social capital as captured by pro-social behavior in lab activities, social networks, and self-reported survey measures. The last section concludes.

2. Program Description

Siem Reap, Cambodia is best known as the location of Cambodia's majestic Angkor Wat temple. Over two million tourists travel to Siem Reap each year to see the temple, and as a result the area around the temple including the town of Siem Reap has experienced explosive economic growth. The economic impact of this growth has not extended beyond a few miles from Angkor Wat however. Jobs in the tourism sector require literacy and some ability to speak English and as such are unavailable to the poorest members of Siem Reap province. In a 2008 study, 14 percent of Siem Reap province residents were classified as very poor (ID Poor 1) and another 15 percent were classified as poor (ID Poor 2) despite the increase in tourism to Angkor Wat temple.[7] Siem Reap province's rural poverty is readily apparent by casual observation a few miles outside of the Angkor Wat tourist region.

To address this persistent poverty in the rural areas of Siem Reap, the Cambodian government and the World Bank launched LEAP as a pilot project. LEAP was designed to meet three broad pro-poor objectives. These were captured by the three program components: 1) to build and strengthen SHGs among the poor to serve as intermediaries with the state and with lending institutions, 2) to provide the poor with better access to finance and 3) to forge better links between poor producers and important markets and value chains. The program hoped that through these activities the villages would accumulate social capital which would in turn strengthen villagers' trust, trustworthiness and capacity for collective action in pursuing these goals.

Overall LEAP inputs included coordination activities, training programs, monitoring as well as cash in the form of "seed" grants (see LEAP, 2012). Under the *first component*, SHGs were formed and trained (e.g. management, book keeping and meeting facilitation).

[7] The Ministry of Planning in Cambodia, with the help of various foreign donors has devised a set of criteria for the identification of poor households for targeting programs. This program called Identification of Poor, or IDPoor for short, surveys each household to determine if they belong in one of two categories (IDPoor 1 and IDPoor 2) or neither. IDPoor 1 are the very poor, those unable to provide enough food for the household. IDPoor 2 are less poor, corresponding to those households that are between the food poverty line and the poverty line. If the household is in neither category we consider them non-poor. About 15 percent of the Cambodian population fall into each of the categories IDPoor 1 and IDPoor 2 (see Ministry of Planning, 2013a,b).

Individual SHG members were instructed on how to increase savings and make and obtain loans. They also received information on gender mainstreaming and training on agricultural techniques. The SHGs were closely monitored to ensure regular and well attended meetings, steady saving and lending, adherence to internal group rules, as well as proper bookkeeping. All SHGs were officially registered with the commune council. Each SHG also underwent an extensive performance rating and received overall performance scores. As part of the *second component*, all SHGs opened formal bank accounts and received seed grants to kick-start activities. The *third component* involved the establishment of producer groups, the provision of livelihoods training (e.g. home-gardening, chicken-raising), as well as the promotion of market linkage of producer groups.

The LEAP pilot, which began in July 2010, led to the following outputs (see LEAP, 2012): To improve the social institutions of the poor, LEAP created 100 self-help groups containing 1,291 household members, 99 percent of whom were classified as poor by the Cambodian government and 90 percent of whom were women. To encourage savings and access to credit all 100 SHGs had bank accounts at major commercial banks. These 100 SHGs had amassed total savings of about $78,000 USD at the time of our study in late April and early May of 2013. As of May 2012, over 5,800 loans had been made from SHG funds, 85 percent for investments and 15 percent for consumption and the program had made over $33,000 in seed grants to the SHGs. On average each SHG received USD 336 corresponding to USD 24 per participating household. To improve the poor's access to markets and value chains these 100 SHGs formed 52 producers groups, 38 in chicken raising (73 percent of the total), seven in pig raising, four in basket weaving, two in vegetable raising and one in rice selling.

At the outset LEAP planned to work with each self-help group for a total of three years. In reality however, the intervention could only be implemented for one year. After this one year, the self-help groups were completely left to themselves. The data collection was then done another six months later. It is therefore quite interesting to determine if this intense, but relatively short intervention led to sustainable economic and social impacts.

3. Empirical Strategy

Our evaluation was designed to test three propositions: First, we were tasked with determining whether the program increased savings and access to credit. Second, we were asked to evaluate whether greater access to credit and LEAP's programs to better link poor villagers to markets produced income and livelihood enhancements. Finally, we were asked to ascertain whether the program increased civic engagement and social capital among the poor. The program's interest in social capital was motivated by an interest in encouraging the poor to take collective social action to address issues of importance to them. Any evidence that we could find that LEAP produced social capital, especially among the poor, would be taken as an extremely important side effect of the program.

3.1 Measurement

We measure savings, incomes and sources of incomes with a standard survey. Measuring social capital poses particular challenges, especially when evaluating a program like LEAP. Program staff stressed the importance of participation and civic engagement in the program communities, but did not operate at all in the control communities. Often researchers measure social capital with responses to survey questions like "Do you think people are generally trustworthy?" "Would you be willing to contribute to public good X?" and so on). The obvious problem with this measurement approach is that program staff teach villagers the "right" answer to these questions as part of the program and, of course, they do not operate in the control communities at all. Thus respondents in treated communities may, consciously or sub-consciously, give the "right" answer to these questions, when in fact there was no change in their community or their behavior. Control-community members, by contrast, may not even know what the "right" answer is, since they have not received this training from the program (Mansuri and Rao, 2013).

Observational measures of social capital are on the surface a better measurement strategy, but they possess their own different set of problems. First, they are confounded. Observational measures of pro-social action like voter turnout, participation in public meetings and participation in public works projects are potentially manipulable by elites, including through corruption and coercion. In extreme cases high levels of these activities

may actually indicate a paucity of social capital—civil societies' inability to resist elite corruption and coercion.[8] The second problem with observational measurement of social capita is that it easily stumbles into tautology. This problem is readily apparent in the following passage from Putnam (1993, p. 36, quoted in Portes 1998, p. 20):

> *Some regions of Italy ... have many active community organizations ... These "civic communities" value solidarity, civic participation and integrity. And here democracy works. At the other end are "uncivic" regions like Calabria and Sicily … The very concept of citizenship is shunned here.*

Portes (1998, p. 20) aptly summarizes the passage: "In other words if your town is 'civic' it does civic things; if it is 'uncivic' it does not."

Measures of behavior in activities that are under the researcher's control are therefore more appealing. We used behavioral games that permit us to measure these attributes through subjects' choices in a controlled laboratory setting. Since our measurements were taken in the laboratory, where subjects interacted with each other anonymously and according to specific incentives of our design, local governing institutions or informal social punishments play much less of a role in subjects' decisions, allowing us to better isolate the effects of the program on potential changes in subjects' pro-social norms. Moving data collection to the laboratory may raise questions of external validity, but we argue that the trade-off is worth it, particularly in light of the established results using other measurement techniques mentioned above.

We implemented adaptations of well-established laboratory activities. Activities like these were used to measure social capital by Avdeenko and Gilligan (2015), Karlan (2005) and Heinrich et al (2004) and in the studies in developing countries as reviewed in Cardenas and Carpenter (2008). We conducted three activities to measure subjects' pro-social norms: (1) willingness to share with the needy, (2) trust and trustworthiness and (3) willingness to

[8] Voter turnout and participation in public works in the Democratic People's Republic of Korea are an extreme example but they illustrate the general problem. On the effect of authoritarian regimes on social capital see Putnam's (1993) discussion of the Norman kingdoms and Sztompka's (1999) discussion of the trust in the context of Eastern Europe.

contribute to a collective good, and two possible confounders (4) attitudes toward risk and (5) discount rates.

The activities are described in greater detail in the appendix. We measured subjects' willingness to share with the needy (i.e. altruism) with a simple alteration of the standard dictator game. Subjects were given 3,000 riels in 500 riel notes and asked to decide how much, if anything, of that amount to donate to an anonymous local needy family. We used the standard trust game (Berg et al. 1995) to measure trust and trustworthiness. Investors and trustees[9] were each given an endowment of 3,000 riels in 500 riel notes. We tripled the amount sent by the investor to the trustee. We used a dichotomous public goods game similar to the one described in Barrett (2005). Our measures of the two possible confounders, risk and time preferences, are described in the appendix.

Total payouts from all five games were aggregated and made in one lump sum at the end of the session. The average payout was approximately 16,500 riels (a little over 4 USD), which is about one day's wage in the rural areas where we worked. Instructions to all the activities were given verbally according to a specific script in the local language (Khmer). Illiteracy rates are very high in rural Siem Reap, and our subjects were very unfamiliar with the use of paper and pens, so we had subjects complete the game tasks for four of the five games under the supervision of a facilitator/record keeper. This is a common practice when conducting games in the field in developing countries with illiterate populations (Karlan 2005, Henrich et al. 2004). Such observation was not required for the public goods game. While we were concerned about Hawthorne effects, having the subjects play under supervision was the only way we could ensure that they understood the decisions they were making. Further the games were conducted in precisely the same way in the treated and control communities so, in order to affect our results, any Hawthorne effects would have had to be worse in the control communities despite the fact that only treated communities received coaching from the program on the importance of pro-sociality.

[9] In lab we used the neutral terms "Player 1" and "Player 2."

Laboratory activities are arguably a particularly effective way to uncover norms—a definitional component of social capital. As Hoffman, McCabe and Smith (1998, p. 350) compellingly assert "A one-shot game in a laboratory is part of a life-long sequence, not an isolated experience that calls for behavior that deviates sharply from one's reciprocity norm. Thus we should expect subjects to rely upon reciprocity norms in experimental settings [...]."

Social networks are the other definitional component of social capital. Therefore, we also gathered network data from all of our laboratory subjects. We completed a matrix of relationships among our subjects for each of several different categories of social relationships. We developed this list of social relationships through extensive focus group discussions to ensure that we were asking subjects about the most important relationships in the villages. We instructed our enumerator to crosscheck each relationship with the other person in the reported relationship to make sure that both people agreed they were in such a relationship. For example, every person was asked whether he or she participated in a voluntary community activity such as repairing a school or a road. If a subject reported that she engaged in such an activity with another person in the group we checked with that other subject to insure that the information was accurate. We summarize the density of a person's network in each category with a dichotomous variable equal to one if the person reported two or more such links with another person in the group and zero otherwise. In other words, we estimate the likelihood of establishing new links due to the LEAP intervention. In an unreported robustness check, we also used the total number of links. The results and conclusions are virtually identical and available on request.

Finally we gathered data through a standard household survey. This survey provided our measures of income, savings, expenditures and so on. We also asked a few questions about civic participation and group membership that we use as measures of social capital. We use them in combination with the measures described above to complete the picture of the social context of the villages.

3.2 Randomization and survey sample

Before moving on to the results section, we briefly outline the randomization, sampling strategy, and empirical model. The LEAP team asked us to implement a rigorous randomized-control-trial in order to inform an eventual roll-out of this project in the rest of the province, as well as other poor provinces of Cambodia. The pilot was budgeted to run in all villages of seven communes out of a total of 50 communes. Hence the randomization of the LEAP was done at the "commune" level – the lowest administrative level above the village level. Unfortunately, we could not collect baseline data, that is, data before the roll-out of the pilot, since we started collaborating with the LEAP team only shortly before the launch of the project. However using baseline data as covariates in the regression models would only influence standard errors (and increase efficiency) and not coefficient estimates given that the program is orthogonal to the error term.

To evaluate a causal effect of the project, we randomly selected 7 communes to receive the LEAP pilot. All villages in the 7 treated pilot communes were treated and also surveyed. In addition, we randomly sampled 18 villages at random from 18 randomly selected control communes. In each of the 36 villages (18 treated, 18 control), we aimed to survey 15 households. We used census lists with household names for each village. In principle, the sample should have a total of 540 households. However we have slightly more (548 households) because we gave the survey team a list of substitute households in case some were no longer present in the village or absent. This happened in a few cases and some households acted as substitutes. Of these 548 some 85 are substitute households (42 control, 43 treated). In 4 cases, the survey team located "missing" households after substitutes had been enumerated, and so the team surveyed both substitute and original households. We checked that the likelihood of absent households was statistically unrelated to living in a LEAP village, which could influence our estimates. Fortunately, this is not the case. The final sample is thus composed of 548 households (272 control, 276 treated). LEAP targeted poor households, those that were officially classified as IDPoor-1 and IDPoor-2. To explore effects across the poor and non-poor, we randomly sampled five households from each of the three official, poverty groups (IDPoor 1, IDPoor 2, Non-Poor). Table A1 in the appendix shows that the samples of households in the treatment and control

units are balanced: we cannot detect differences in pre-determined characteristics of individuals and households.

At the end of each household's enumeration the household survey team gave each household head or primary couple of the household an invitation to a laboratory session on a later day in that village. After the household survey had passed through the village, the second survey team organized these laboratory sessions in the village. 524 of the 548 households participated in these sessions for an attendance rate of 95.6%. We did not sample substitute households for the experimental sessions to stay consistent with the household survey sample. The likelihood of absent households is the same in treated and control villages, and the few missing households are therefore unlikely to have systematic impacts on our main results.

3.3 Empirical specification

We will present simple regression-based differences-in-means between treated and control villages. Furthermore, since LEAP targeted households that were officially classified as poor beforehand (IDPoor 1 and IDPoor 2), we also split the sample by poor and non-poor households to see how effective LEAP was in targeting the poor, explore possible spill-overs across poverty groups, and gauge access to the program by the non-poor. In these models, all standard errors are clustered at the village level (36 villages) to account for arbitrary correlation of errors. In particular, this is to account for game-session-specific (i.e. village) error correlation and error correlation due to the fact that SHGs were created at the village level. Alternatively, we also clustered at the level of randomization, that is at the (administrative) commune level. Recall that all villages in a commune were treated. Standard errors are very similar when clustered at the commune level (24 communes) and available on request.

Finally, we group all indicators and provide average, standardized effects in the final column of each table following Kling et al. (2004) and Clingingsmith et al. (2009). This method reduces the danger of cherry-picking significant results that arise by chance. It provides us with an overall effect for each group of indicators.

4. Results

The effects of the program can be summarized as follows. The program generated significant effects on the behavior it most directly targeted: villagers' savings and their associations in SHGs. Both increased significantly in the treated communities. These effects were particularly profound among the poorest members of those villages as was the intention of the program. Indeed the program had no significant impact on the savings on the non-poor in treated communities. There is also some evidence of enhanced livelihoods: Respondents in treated communities reported significantly greater production of and income from meat and fish. There were no impacts on other sorts of production or income though.

There was no evidence for broader social impacts of the program however. Looking across six sets of indicators of social capital (behavior in the laboratory, a survey of economic networks, a survey of social networks, retrospective self-reported group membership and retrospective self-reported community voluntary activity) the program produced significant increases in only one of these areas—retrospectively self-reported group membership.

4.1 Savings outcomes

The effects of the program on savings outcomes and membership in savings groups are exhibited in Tables 1 and 2. Table 1 reports the effect of the program on log savings for the full sample and the sample split into the poor (IDPoor 1 and 2) and non-poor. The results show that the log of savings among the poor in treated communities was about 3.27 larger. This means that ID-poor respondents in treated communities had savings of about 30,320 riels more than those in control areas. This amounts to roughly 7.6 USD and corresponds to 1.2 times the savings of poor people in control communities. Put differently, this is 30% of the overall sample standard deviation in savings of the IDPoor. Furthermore, these savings were almost entirely concentrated in SHGs as shown in the second column of Table 1. The estimated increment in savings is virtually identical for both savings in general and savings in SHGs. The effect of the program on the log savings of the non-poor

was small (only a third of that of the poor) and insignificant. Indeed, in the treated communities the poor were actually outsaving the non-poor.

While the effects on savings among the poor were clear the program had no discernible effect on borrowing behavior of either poor or the non-poor, as shown in columns 4 and 5 of Table 1. These columns report estimates of a linear probability model to ease the interpretation of coefficients.[10] The probability that a respondent applied for or received a loan in 2012 was no different in the treated and control communities regardless of the respondent's IDPoor status. The final column presents the standardized mean effect across the specification in the first five columns of Table 1. The estimates indicate that the program caused an increase of 0.21 standard deviation increase in our measures of savings and borrowing for the full sample and an increase of one-third of a standard deviation for poor respondents. The mean standardized effect of the program on non-poor is substantively and statistically zero.

Table 2 presents estimates of the impact of the program on subjects' participation in savings groups. We asked subjects whether they were members of both SHGs and other sorts of savings groups such as rotating credit associations (called tontines in Cambodia), which are reportedly common in the region we worked.[11] Furthermore, several NGOs were sponsoring SHGs in the region so LEAP was not the only route through which a villager could enter a self-help group. As shown in Table 2, LEAP had a large effect on villagers' participation in SHGs and savings groups. The program caused an 18 percentage point increase in the number of villagers reporting two or more links in an SHG. This represents a quadrupling of the percentage of villagers who reported links in an SHG, a substantively large effect that is highly significant statistically. The program also appeared to have caused a 23 percentage point increase in the number of villagers reporting two or more links via another type of savings group. This latter savings group affect is likely spurious. We asked our networks enumerator to draw the distinction between participation in an

[10] Probit or logit estimates are qualitatively similar and available on request.
[11] See Liev (1997). The commonality of tontines was also mentioned by several Cambodians in informal interviews.

SHG, like those provided by LEAP and a stand-alone savings group such as a tontine. We observed during field implementation that both our enumerator and the respondents seemed to be confused by this distinction despite our repeated explanations and so we are not confident that the links in these groups were recorded properly. This may explain the large increase in non-SHG savings group participation in LEAP communities since subjects may have been conflating their participation in an SHG with participation in a traditional savings group. This misunderstanding in implementation is unfortunate because we had hoped to assess the extent to which LEAP caused a substitution out of more traditional saving arrangements like tontines, a question we can now not confidently answer. Regardless both effects are substantively large and highly significant statistically suggesting that LEAP had a large effect on SHG membership and in participation in savings group schemes either through LEAP SHGs or some other group. Interestingly LEAP villages even saw a significant increase in SHG membership among the non-poor even though the program was targeted to the poor and LEAP SHGs were made up overwhelmingly of poor members. As shown in column 3 the standardized mean effects of these two indicators are substantial and highly significant statistically. In the full sample the program caused an estimated increase of 0.74 standard deviations in financial network linkages. Among the poor the effects are very large—over one standard deviation increase. Non-poor saw an estimated 0.46 standard deviation in financial network linkages.

4.2 Livelihood and household expenditure outcomes

Tables 3 through 5 exhibit our results of the impact of LEAP on livelihood and household expenditure outcomes. The first and second columns of Table 3 show that meat and fish production increased substantially and statistically significantly as did sales of meat and fish. This increase in incomes from meat and fish sales is consistent with the observation above that 45 of the 52 producers groups created by LEAP SHGs involved raising livestock (38 chicken-raising and 7 pig-raising groups).

Several factors raise questions about the veracity of this estimated increase, however. First, the non-poor, who were not targeted by the program, enjoyed large (indeed the largest) increases in sales of meat and fish. Second, increases in meat and fish production were not

matched by program-attributable increases in livestock holdings or livestock accumulation: As shown in columns 2 and 3 of Table 4 average treatment effects in these categories were quite small and not at all statistically significant. Finally, the improvement meat and fish production was not corroborated by an increase in producer-group membership in LEAP communities. If the large increases in meat and fish production were due to poultry and pig raising producer groups, we would expect some increase in producer membership in LEAP communitie, but as shown in column 2 of Table 9 we observe none, suggesting that the 52 producers groups (of which 45 were livestock producers groups) were small or not very active. In sum, although the causal path between LEAP and increased meat sales is clear (LEAP created 45 meat producing groups) the large increase in sales by the non-poor, the absence of any evidence of livestock accumulation and the absence of a significant rise in membership in producers groups raises questions about the validity of the large increases in sales of meat and fish and whether those increases are due to the program.

The effects of LEAP on two other income categories are reported in columns 3 and 4 of Table 3. They show that LEAP did not cause any increases in crop production or off-farm incomes. To some extent this null finding is not surprising given the small number of producer groups created for these activities. The overall effect of LEAP on incomes, given by the standardized mean effect in column 5 of Table 3, shows that due to the large increases in meat and fish production LEAP did have a modest overall mean impact on incomes of about 13 percent of a standard deviation across all of the income categories reported.

Table 4 presents estimates of the impact of LEAP on asset accumulation. We already discussed the absence of any increases in livestock holding or greater livestock accumulation in LEAP villages. Other categories also saw no program-induced change. For example the average treatment effect of the program on total assets in log, shown in the first column of Table 4, is tiny and not remotely significant statistically. The average effect of the program across all of the categories, shown in column 4, is similarly diminutive at eight percent of a standard deviation, and not at all significant statistically. The coefficients throughout this table are very close to zero, especially when compared to

the average assets holding represented by the constant term. We stress, therefore, that the estimated lack of an effect on asset accumulation cannot be attributed to low-powered tests, but to very small differences between treated and control communities.

Table 5 shows that the program had no significant impacts on household expenditures, at least in the hypothesized direction. Indeed, the second column of Table 5 indicates that households in LEAP communities suffered a reduction in miscellaneous expenditures. We think it is best to chalk that result up to random error. The main point of Table 5 is illustrated by the standardized mean effect presented in column 6: LEAP produced no increase in expenditures in the villages where it operated.

4.3 Social capital outcomes

We turn now to another downstream effect that programmers were interested in achieving: greater social capital among and civic engagement by the poor participants in LEAP. We bring three pieces of evidence to bear on this question: the villagers' actions in the laboratory behavioral activities (our measure of pro-social norms), the networks survey described above and self-reports of pro-social action in villagers' responses to a retrospective survey.

The results clearly indicate that the program had no effect on subjects' pro-social behavior in the laboratory nor did it have any overall effect on network linkages among villagers beyond the creation of the SHGs themselves. There was some mixed evidence from the retrospective survey that the program caused greater participation in some community groups (other than the obvious case of the SHG itself) especially in rice seed banks and no evidence from the retrospective survey that members of LEAP communities engaged in more community voluntary action.

We first present the effects of the program on behavioral measures of pro-social norms from the lab. We then turn to measures of the effect of LEAP on density of social linkages using our crosschecked measures of social networks. We close with a discussion of the results from the self-reported behavior in the retrospective survey.

4.3.1 Behavioral measures from the lab

The effects of the program on behavior in the lab are presented in Table 6. The first two columns of Table 6 report results for possible confounding factors: risk and time preferences. We were concerned that people may behave differently in the lab not due to norms of trust, trustworthiness or a sense of public duty but instead due to different risk or time preferences.[12] The lab activities used to develop these measures are described in greater detail in the appendix. The risk activity produced a five-point scale ranging from one (least risk acceptant) to five (most risk acceptant). The discount rate activity produced a six-point scale ranging from one (most patient) to six (most impatient). Since these specifications merely perform confound checks we will not dwell on a discussion of them here. The OLS estimates on these measures indicate that there is no reason for concern about possible confounding. There is no difference between the risk or time preferences of the subjects in treated and control communities. Estimates using our five-point scale of risk preferences shown in column 1. The second column of Table 6 exhibits the linear estimate of the effect of LEAP on our six-point scale of patience (time preferences).[13]

Column 3 presents the estimated effect of the program on altruism, measured by the amount donated by the subject to the needy family. Again, the results indicate that there is no difference between the treated and control communities. In the full sample subjects from the control community donated about 742 riels, a little less than one-fourth of the endowment. Subjects from treated communities contributed slightly more (about 50 riels or six percent more), but this increment is not statistically significant. The amount donated does exhibit some income effects. Non-poor subjects gave over 100 riels more to the needy family than did poor subjects. In neither subsample did members of the treatment community give significantly more than subjects from control communities.

[12] Schechter (2007) provides an example where risk-acceptant subjects played as if they were more trusting.

[13] We obtained similar results with discrete choice models. We also created binary variables splitting risk taking and patience at the sample mean, and no differences due to treatment emerge. All results are available on request.

Column 4 presents linear probability estimates of the effect of LEAP on contributions in the dichotomous public good activity. Around 64 to 65 percent of subjects contributed to the public good. There was little variation in the behavior of the poor and non-poor subjects in this case. More importantly there was no significant difference between subjects from treated and control communities, neither in the full sample nor in sub-categories of poverty. Roughly five to six percent more subjects contributed in the LEAP villages than did subjects in the control village, but this increment was not statistically significant across samples.

Estimates from the trust activity are presented in columns 5 and 6. Column 5 shows the effect of the program on trust—the amount sent by the "investor" in the activity. Column 6 shows the impact of the program on trustworthiness—the amount returned by the trustee as a percentage of the total amount available to the trustee. The total amount available to the trustee is his or her initial endowment of 3,000 riels plus triple the amount the "investor" gave him or her. The number of "investors" and "trustees" is unequal because on a few occasions an odd number of subjects arrived for the games due to attrition. Rather than turning away sure-to-be disappointed subjects, we randomly matched two trustees to one investor in those sessions. In such a case the trustees received the payoff consistent with his or her actions and the relevant investors received the payoff decided by the first trustee with whom they were randomly paired.

Focusing first on column 5, there is no significant difference in trust between subjects in treated and control villages in the full sample or in either of the subsamples (poor and non-poor). Subjects did send more in the treated villages than in the control villages, but the difference was never statistically significant. There may be a problem with low-powered tests in this particular case, since poor subjects in treated villages sent about fifteen percent more than did poor subject in control villages. This behavior was highly variable however; the standard error was considerably larger than the coefficient so the estimated effect was not at all close to statistical significance. Still the sample size of only 165 raises concerns about low power in this case.

Before turning to the results on trustworthiness in column 6, it is worth mentioning two interesting features of the results in column 5. First, there are stark differences in the behavior of the poor and non-poor in the amount sent in the first round of the trust game. Non-poor subjects sent roughly 809 riels or a little over one-quarter of their endowment. Poor subjects sent only 524 riels or a little over one-sixth of their endowment. Thus non-poor subjects sent over 50 percent more, on average, than did poor subjects. It is not clear, if this difference between poor and non-poor is due to greater trust by the non-poor subjects or a greater sense of altruism. The amount sent in the first round of the trust game may be confounded by altruism—the "investor" may be motivated at least in part by a desire to send a gift to his or her trustee. The fact that non-poor sent more in the altruism game (as shown in column 3) suggests that greater altruism may have played a role in this difference. Indeed, our altruism measure is highly correlated in a regression model with trust and trustworthiness.[14] Of course including or excluding altruism in these models leaves the effect size and standard errors associated with the LEAP program unchanged.

A second interesting feature of the trust models in column 5 is that the amount sent by the investor is quite low compared to standard results in the trust-game literature. Johnson and Mislin (2011) provide a meta-analysis of over 200 trust-game studies that were conducted over dozens of countries. They found that on average subjects in the "investor" role sent about half of their endowment. The subjects in this study sent only about 20 percent of the endowment on average. Even the non-poor subjects sent only a little over one-fourth of their endowment. Thus at least according to these results the subjects in our study appear to be considerably less trusting than subjects in previous trust-game studies conducted around the world. In principle, then, there is room for LEAP to improve levels of trust.

Turning to the effects of the program on trustworthiness in column 6, again the program did not produce significant increases in trustworthy behavior in the full sample or either of the two subsamples. In this case there is little concern over low-powered tests, because the

[14] Altruism strongly correlates with trustworthiness – both unconditionally and conditional on the amount received in the first place.

estimated treatment effect is very small.[15] Indeed for the IDPoor sample (the portion of the population that was targeted by the program) the estimated effect is zero to two decimal places. In the full sample the estimate indicates that those in the treated communities returned about one percentage point more, a small increment given the average amount returned, and an estimate that is only half the size of its standard error. There are no significant differences between poor and non-poor according to this measure of trustworthiness. Both groups returned about 14 to 15 percent of their total pot to the investor.

Consistent with the remarkably low level of trust exhibited by our subjects compared to previous studies (mentioned above), the results in column 6 indicate low levels of trustworthiness compared to findings in previous trust-game studies. Subjects returned only about 15 percent of the total amount available to them. In their meta-analysis Johnson and Mislin (2011) found that subjects returned about one-third of the total amount available to them on average. Thus subjects in this experiment returned only about half of what has been observed on average in the past. This lack of trustworthiness can help explain the similarly low levels sent in the first round of the trust game. It appears that the communities in which we worked possessed relatively weak norms of trustworthiness, which in turn support relatively low levels of trust at least as measured in our games. If these norms, uncovered in the lab, mirror the norms practiced in the community at large, they point to an important impediment to economic development in Siem Reap.

We also detect no overall effect of LEAP on the behavioral measures of social capital when we account for their co-variance in the final column of Table 6. Here we present the inverse-covariance weighted average treatment effect of the program on all of the laboratory measures (Kling et al. 2004; Clingingsmith et al. 2009). Not surprisingly, given that none of the measures showed any significant effect of the program, the estimate of the average effect is very small. The largest effect, which was among the poor subjects, is only about eight percent of a standard error and not at all statistically significant.

[15] This result is independent of conditioning on trust (i.e. the amount sent)—unsurprising given that the program had no discernible effect on trust.

Our laboratory-based measure of the social capital impact of LEAP could not be clearer—there was none at the community level. True, there were some issues with low-powered tests in one or two of the measures, especially the trust measure among poor subjects, but the average effect across all four measures of pro-social preferences points to an unmistakable conclusion: the program had no effect on these measures. Before closing we should mention that we estimated the impact of the program just on SHG members using the treatment as an instrument. Of course, these specifications require the stark assumption that the treatment only works through SHG membership. These results are available in the appendix in Table A2. In any case, the instrumental-variables estimated effects of the program on SHG members were considerably larger than on the community as a whole. Estimates of altruism, propensity to contribute to public goods and trust were about one-third larger and estimates of trustworthiness were about one-fifth larger for SHG members than for non-members. Unfortunately these estimates were highly imprecise so these effects were not statistically significant.

4.3.2 Network effects

We now turn to the effects of the program on social capital as measured by our network survey. We are interested in whether the program caused greater network density. Did the increased participation in SHGs lead to a greater participation in other sorts of social relationship as well? Our measure of connectedness of members of the community is dichotomous, equal to one if a subject reports a given relationship with two or more other subjects from the experimental session.[16] We think this measure best captures the type of social transformation that LEAP was attempting to achieve. Our measure characterizes a society as having more social capital when a large number of villagers are connected by social links even if just by a few links in contrast to measures in which a society would be characterized as well endowed with social capital when a small number of villagers

[16] Our network measure is based on the connections between household members that participated in the experimental session. However this group was chosen randomly from the village, and is thus a reasonable proxy of the complete village network.

possessed a large number of links. We do not think the latter interpretation of network structure is in keeping with LEAP's broad-based pro-poor agenda.[17]

We first present the results for economic networks in Table 7. We questioned the subjects about several types of economic relationship. The first is "exchanging hands," which is a direct translation of the Khmer expression for voluntary labor exchange in which one farmer works on another farmer's land in exchange for an explicit agreement that the second farmer will work on the first farmer's land at a later date. This is as much a favor exchange relationship as an economic one. Although given the importance of agriculture to the region, we include it here as a fundamental economic relationship. We also include other basic economic relationships: Do the subjects buy and sell from each other? Does another subject employ the subject? Do the subjects work together at the same farm or plant?

In most cases the program had no significant effect on economic networks. The one exception is the exchange-hands relationship. There is fairly strong evidence, significant at the five percent level, that more subjects from treated communities were related in this way than were subjects from control communities. About 72 percent of respondents from treated communities possess these links with more than one other subject from their village, whereas this was true of only about 48 percent of respondents from control villages. This was the only economic relationship where the program had an impact. The causal effect of the program on the other economic networks was nil. The point estimate of the effect of the program on buying and selling is actually negative, although not significant. The estimates of the effect of the program on the other two types of relationships (employee and co-workers) are substantively very close to zero and not statistically significant. Thus despite the significant effect of the program on exchange-hands relationships, the standardized average effect across all four of these types of relationships, shown in column 5, is small and insignificant.

[17] Results are qualitatively similar when we use the total amount of links rather than the binary indicator. Also count data and discrete choice models do not lead to different insights. All results are available on request.

Table 8 presents the estimates of the impact of the program on what we call *social relationships*. The first type of relationship we examine is joint membership in the same funeral group. Funeral group members provide social, emotional and financial support for each other after the death of a loved-one. There is some evidence, significant at the five-percent level, that more members of our treated community were linked in such groups. The groups appear to have been quite rare in the control communities, with only about two percent of our subjects reporting two or more links in such groups. By contrast some 20 percent of the subjects in the treated communities had two or more links in a funeral group with other subjects. The effect is not significant for the non-poor members, however, given that the coefficient is of the same magnitude, that lack of significance is likely due to the smaller sample size in that category. Among the poor the percentage of subjects that had two or more links was higher by 18 points (larger by a factor of ten compared to the treated communities).

The second column of Table 8 presents the effect of the program on connectedness through religious organizations. In this part of Siem Reap, this would have consisted of attending services at the same Buddhist temple. Although the point estimate is rather large—suggesting that about twice as many subjects in control communities had two or more links through religious activities than did subjects in control communities—the effects is highly variable and the associated standard error is large. As such there was no significant difference between the treated and control communities, despite the relatively large point estimate. Thus we may have concerns about low-powered tests in this particular case.

The third column of Table 8 presents the estimated effect of the program on socialization among subjects (sharing a meal or tea, playing cards and so on). In general the communities were quite well networked in this regard. Over sixty percent of our subjects reported engaging in these activities with two or more other subjects. There is quite clearly no effect of the program, however, and in this case we cannot attribute the null results to low-powered tests. The difference in means between treated and untreated communities is very

close to zero, especially when compared to the average level in control communities of over 60 percent.

We also asked subjects if they participated with other subjects in volunteer projects to improve a school, road or other public works. According to the estimates in column 4 about 40 percent of our subject engaged in these activities with two or more other subjects. The point estimate for non-poor was a bit higher than for poor subjects (45 percent compared to 37 percent). The estimated effect of the program on the density of this type of network was very close to zero, both in the full sample in in each of the sub-samples.

Columns 5 through 7 present estimates of the effect of the program on networks of favor-exchange relationships. We asked about three such relationships: babysitting services, offering advice and borrowing household items. In all cases we specified that these activities were undertaken without payment in return. These types of favors are quite common among our subject pool. About 40 percent of subjects received babysitting services from two other subjects, about 50 percent sought advice from at least two other subjects and about 60 percent borrowed household items from at least two other subjects. In none of these cases did the program lead to an increase in the percentage of subjects who were linked in this way. In all cases the increase in the percentage of people who had two or more of these links was very close to zero or negative.

The standardized average effect across all seven of these measures is presented in column 8. Since the only measure that produced a significant effect was membership in funeral societies it is not surprising that there is no significant average effect of the program across all seven of these networks measures. Feigenbaum et. al. (2012, 2013) found that social links in microcredit groups spilled over into other sorts of social relationships especially friendships among microcredit group members. We found that the effect of LEAP on joint membership in SHGs is very strong, but that it did not translate into a community-wide increase in other social links. There is no inherent contradiction between these two sets of results since Feigenbaum et. al. (2012, 2013) examined only microcredit group members and we studied the entire community.

4.3.3 Self-reported activity in retrospective surveys

Finally, we turn to the effects of the program in increasing social capital using self-reported retrospective survey measures. Table 9 reports linear probability model estimates of respondents' answers to yes-or-no questions about whether they belonged to the specified groups. As expected the program had a large impact on SHG membership. SHG membership was about 30 percentage points higher for poor respondents and 21 points higher for non-poor respondents in LEAP communities. Both results are statistically significant.

Membership in producers groups, shown in column 2, was no different in program and control communities, which is perplexing given the large increase in incomes from meat and fish sales reported in the incomes survey and the program's reported creation of 52 producer groups. These figures raise questions as to whether the increases in meat and fish sales were due to LEAP-sponsored producer groups or some other factor. Membership in rice seed bank groups and irrigation groups, two groups we did not ask about in our networks survey, appear to be substantially greater in LEAP communities than in control communities, although the former only showed significant increases for poor respondents and the latter only for non-poor respondents. Interestingly funeral group membership is no different between LEAP and control communities even though this was the one and only type of social group in our networks survey that exhibited significantly positive results. There was no difference between program and control communities in respondent's participation in women's groups—a bit of a disappointment given the focus of the SHG model on women. There were significantly more youth group members in program communities, although these extra members clearly came from the non-poor who were not targeted by the LEAP program. The average effect of the program on group membership shown in column 8 is a moderate 36 percent of a standard deviation and highly significant. This significant overall effect is due almost entirely to the greater participation in SHGs and rice seed banks in LEAP communities.

We present our final piece of evidence on the effects of LEAP on social capital and pro-social action in Table 10. The first column of Table 10 present linear estimates of the effect of LEAP on the number of village meetings respondents reported attending. According to these estimates villagers in program communities attended more village meetings than did members of control communities, although most of this increase appears to have been among the non-poor, who were not targeted by the program. Non-poor members of LEAP communities report attending over two more meetings than their counterparts in non-LEAP communities. The increment is smaller among the poor. Poor members of program communities attended about one-and-a-half more meetings than did poor members of control communities. Both results, those of the poor and the non-poor, are statistically significant at the five percent level or better.

The remaining columns of Table 10 present linear probability estimates of respondents' answers to yes-or-no questions about whether they participated in the voluntary activities listed. The estimated differences between the program and the control communities were very close to zero (and in some cases negative) and statistically insignificant.

In summary, other than the increase in the number of village meetings attended, there was no difference in the voluntary community action of LEAP-community and control-community members as reported by respondents in a retrospective survey. The standardized average effect across these four measures shown in column 5 of Table 10 captures this lack of difference between treated and control communities. The average effect is small and insignificant suggesting that there was no significant difference between program and control communities in the voluntary actions of their members.

5. Conclusion

LEAP delivered more than feared and less than hoped. The program clearly had a positive effect on the poor's savings behavior, dramatically increased associations among the poor via SHGs and may have produced substantial improvements in the poor's meat production and income derived from it (although the evidence on this latter impact was somewhat contradictory). These positive effects have materialized after a one-year intensive pilot

program rather than a three-year intervention as was originally planned. Hopes for broader social transformation, raised by Feigenbaum et al. (2013), however were unfortunately not corroborated in this particular study. There are several potential reasons for these different results. Clearly, country and program context differ. Their focus on microfinance group members in contrast to our search for community-wide effects among both members and non-members of SHGs probably played a role as well. In addition, we adopted a different measurement strategy that may also contribute to these different results. We chose to use lab-in-the-field activities to measure social capital and pro-social attitudes in addition to network surveys and other self-reported measures.

These negative findings on the broader social capital impacts of the program should be no reason for discouragement. The program's improvements to the poor's savings behavior and association through SHGs are important achievements in themselves. If we include the possible increase in incomes though meat production as a result of the SHGs producers groups, the case for these programs is even stronger. Finally, we must mention that we conducted this study less than three years after the pilot program's launch. Given more time, the increased association among the poor in SHGs may produce the hoped-for community-wide gain in social capital.

5. References

Avdeenko, Alexandra and Michael J. Gilligan. 2015 International Interventions to Build Social Capital: Lab-in-the-Field Evidence from Sudan. *American Political Science Review*. 109(3).

Barrett, S. 2005. *Environment and statecraft: the strategy of environmental treaty-making*. New York: Oxford University Press.

Beaman, L, Karlan, D and Thuysbaert, B. 2014. Saving for a (Not So) Rainy Day: A Randomized Evaluation of Savings Groups in Mali. *NBER Working Paper*, 20600.

Berg, J, Dickhaut, J and McCab, K. 1995. Trust, Reciprocity, and Social History. *Games and Economic Behavior*, 10(1), 122-142.

Cardenas, JC and Carpenter, J. 2008. Behavioral Development Economics: Lessons from Field Labs in the Developing World. *Journal of Development Studies*, 44:337-64.

Casini, P and Vandewalle, L. 2011. Public Good Provision in Indian Rural Areas: The Returns to Collective Action by Self -Help Groups. *University of Namuar Department of Economic Working Paper*, WP1119.

Clingingsmith, D, Khwaja, AI and Kremer, M. 2009. Estimating the Impact of the Hajj: Religion and Tolerance in Islam's Global Gathering. *Quarterly Journal of Economics*, 124(3): 1133-1170.

Deininger, K and Liu, Y, 2013a. Economic and Social Impacts of an Innovative Self-Help Group Model in India. *World Development*, 43: 149-163.

Deininger, K and Liu, Y. 2013b. Evaluating Program Impacts on Mature Self-help Groups in India. *World Bank Economic Review*, 27(2): 272-296.

Desai, Raj and Joshi, S. 2013. Collective Action and Community Development Evidence from Self-Help Groups in Rural India. *World Bank Policy Research Working Paper*, 6547.

Fafchamps, M and La Ferrara, E. 2012. Self-Help Groups and Mutual Assistance: Evidence from Kenyan Slums. *Economic Development and Cultural Change*, 60(4): 707-34.

Feigenbaum, B, Field, E, Pande, R, Rigol, N and Sarkar, S. 2013. Do Group Dynamics Influence Social Capital and Female Empowerment? Experimental Evidence from Microfinance. Available at http://economics.mit.edu/files/9208.

Feigenbaum, B, Field, E and Pande, R. 2012. The Economic Returns to Social Interaction. *Review of Economic Studies*, 80(4): 1459-1483.

Henrich, J, Boyd, R, Bowles, S, Camerer, C, Fehr, E and Gintis, H. 2004. *Foundations of Human Sociality: Economic Experiments and Ethnographic Evidence from Fifteen Small-Scale Societies*. New York: Oxford University Press.

Hoffman, E, McCabe K, and Smith, V. 1998. Behavioral Foundations of Reciprocity: Experimental Economics and Evolutionary Psychology. *Economic Inquiry*, 36: 335–52.

Johnson, ND. and Mislin, AA. 2011. Trust Games: A Meta-Analysis. *Journal of Economic Psychology*, 32: 865-89.

Karlan, D. 2005. Using Experimental Economics to Measure Social Capital and Predict Financial Decisions. *American Economic Review*, 95(5): 1688-1699.

Kast, F, Meier, S and Pomeranz, D. 2012. Under-Savers Anonymous: Evidence on Self-Help Groups and Peer Pressure as a Savings Commitment Device. *NBER Working Paper*, 18417.

Kling, JR, Liebman, JB, Katz, LF and Sanbonmatsu, L. 2004. Moving to Opportunity and Tranquility: Neighborhood Effects on Adult Economic Self-Sufficiency and Health from a Randomized Housing Voucher Experiment. *KSG Working Paper*, RWP04-035.

LEAP. 2012. Pilot Phase – Brief Update of Report. Livelihood Enhancement & Association of the Poor in Siem Reap (LEAP), PPA No. Q7100-KH.

Lev, MH. 1997. Tontine an Alternative Financial Instrument in Cambodian Communities. Paper presented to the 12th NZASIA Conference. Massey University. Available at http://www.cambodia.org.nz/Tontine.htm.

Mansuri, G. and Rao, V. 2013. Localizing Development: Does Participation Work? Policy Research Report, Washington, DC: The World Bank.

Mansuri, G., and Rao, V. 2004. Community-Based and -Driven Development. *World Bank Research Observer*, 19: 1-40.

Ministry of Planning. 2013a. Identification of Poor Households (IDPoor). Available at http://www.mop.gov.kh/Projects/IDPoor/tabid/154/Default.aspx

Ministry of Planning. 2013b. Cambodia's Identification of Poor Households Program (IDPoor). Available at http://www.medicam-cambodia.org/activities/info/4ms/source/2009/7/handouts/Presentation3.pdf

Portes, A. 1998. Social Capital: Its Origins and Applications in Modern Sociology. *Annual Review of Sociology*, 24: 1-24.

Putnam, R. 1993. The Prosperous Community: Social Capital and Public Life. *American Prospect*, 13: 35-42.

Putnam, R. 2000. *Bowling Alone: The Collapse and Revival of American Community*. Simon and Schuster.

Schechter, L. 2007. Traditional Trust Measurement and the Risk Compound: An Experiment in Rural Paraguay. *Journal of Economic Behavior and Organization*, 62(2): 272-292.

6. Tables

Table 1: Effects of LEAP on savings and borrowing

	(1) Savings in log	(2) SHG savings in log	(3) Ever applied for loan	(4) Applied for loan in 2012	(5) Got loan in 2012	(6) Average effect
Full sample						
LEAP	2.55**	2.67**	-0.02	-0.02	-0.04	0.21**
	(0.77)	(0.75)	(0.04)	(0.04)	(0.04)	(0.09)
Constant	2.45**	2.18**	0.70**	0.63**	0.63**	
	(0.45)	-0.43	(0.02)	(0.03)	(0.03)	
N	548	548	548	548	548	
ID Poor 1 and 2						
LEAP	3.27**	3.29**	0.03	0.01	-0.02	0.33**
	(0.89)	-0.88	(0.05)	(0.05)	(0.05)	(0.12)
Constant	2.16**	2.02**	0.69**	0.61**	0.63**	
	(0.50)	-0.52	(0.03)	(0.03)	(0.03)	
N	367	367	367	367	367	
Non-poor						
LEAP	1.08	1.42	-0.13*	-0.08	-0.09	-0.01
	(1.09)	(1.00)	(0.08)	(0.08)	(0.08)	(0.13)
constant	3.03**	2.51**	0.72**	0.66**	0.64**	
	(0.73)	(0.66)	(0.05)	(0.06)	(0.05)	
N	181	181	181	181	181	

Note: Columns 1-5 report OLS estimates with village-clustered standard errors. Column 6 reports the standardized average effect, as described in Kling et. al. (2004) and Clingingsmith et. al. (2009), across the measures in columns 1-5.
* = significant at the five percent level, **=significant at the one-percent level

Table 2: Effects of LEAP on finance networks

	(1) SHG	(2) Savings group	(3) Average effect
Full sample			
LEAP	0.17**	0.16**	0.74**
	(0.06)	(0.06)	(0.21)
Constant	0.05**	0.05*	
	(0.02)	(0.03)	
N	526	526	
ID Poor 1 and 2			
LEAP	0.18**	0.23**	1.07**
	(0.07)	(0.07)	(0.28)
Constant	0.06**	0.03	
	(0.03)	(0.03)	
N	351	351	
Non-poor			
LEAP	0.15**	0.03	0.46**
	(0.06)	(0.07)	(0.21)
Constant	0.04	0.11**	
	(0.03)	(0.05)	
N	175	175	

Note: Columns 1-2 report OLS estimates with village-clustered standard errors. Column 3 reports the standardized average effect, as described in Kling et. al. (2004) and Clingingsmith et. al. (2009), across the measures in columns 1-2
* = significant at the five percent level, **=significant at the one-percent level

Table 3: Effects of LEAP on production and income

	(1) Meat and fish Production in 2012 in log	(2) Meat and fish sales in 2012 in log	(3) Income crop production in 2012 in log	(4) Off-farm income in 2012 in log	(5) Average effect
Full sample					
LEAP	1.47**	2.90**	-0.87	-0.44	0.13**
	(0.56)	-0.69	(0.93)	(0.41)	(0.06)
constant	10.68**	6.74**	7.14**	14.19**	
	(0.51)	(0.53)	(0.68)	(0.27)	
N	548	548	548	548	
ID Poor 1 and 2					
LEAP	1.26**	2.81**	-1.39	-0.07	0.13*
	(0.55)	(0.71)	(1.00)	(0.39)	(0.07)
constant	10.79**	6.59**	6.73**	14.09**	
	(0.46)	(0.53)	(0.76)	(0.26)	
N	367	367	367	367	
Non-poor					
LEAP	1.91**	3.07**	0.14	-1.20	0.13*
	(0.79)	(1.15)	(1.26)	(0.76)	(0.07)
constant	10.46**	7.06**	7.98**	14.40**	
	(0.72)	(0.92)	(0.84)	(0.47)	
N	181	181	181	181	

Note: Columns 1-4 report OLS estimates with village-clustered standard errors. Column 5 reports the standardized average effect, as described in Kling et. al. (2004) and Clingingsmith et. al. (2009), across the measures in columns 1-4
* = significant at the five percent level, **=significant at the one-percent level

Table 4: Effects of LEAP on assets and livestock holdings and accumulation

	(1) Assets in log	(2) Livestock value in log	(3) Livestock acquired 2012 in log	(4) Average effect
Full sample				
LEAP	0.07	0.44	0.08	0.08
	(0.33)	(0.92)	(0.08)	(0.08)
constant	12.92**	10.33**	0.33**	
	(0.24)	(0.63)	(0.04)	
N	548	548	548	
ID Poor 1 and 2				
LEAP	0.10	0.24	0.08	0.07
	(0.38)	(0.99)	(0.10)	(0.08)
constant	12.39**	9.91**	0.34**	
	(0.24)	(0.66)	(0.04)	
N	367	367	367	
Non-poor				
LEAP	-0.01	0.82	0.09	0.08
	(0.47)	(1.12)	(0.13)	(0.11)
constant	14.01**	11.19**	0.32**	
	(0.41)	(0.85)	(0.08)	
N	181	181	181	

Note: Columns 1-3 report OLS estimates with village-clustered standard errors. Column 4 reports the standardized average effect, as described in Kling et. al. (2004) and Clingingsmith et. al. (2009), across the measures in columns 1-3
* = significant at the five percent level, **=significant at the one-percent level

Table 5: Effect of LEAP on household expenditures

(1)	(2)	(3)	(4)	(5)	(6)
Non-food expenditures last month in log	Miscellaneous Expenditures past 12 months in log	Bought food expenditures last 7 days in log	Bought rice expenditures last 7 days in log	Meat and Fish consumption self-produced in 2012 in log	Average effect
0.04	-0.24**	-0.12	0.53	0.87	-0.00
(0.10)	(0.11)	(0.14)	(0.91)	(0.54)	(0.07)
11.27**	14.14**	10.84**	3.91**	9.46**	
(0.08)	(0.05)	(0.10)	(0.70)	(0.49)	
548	548	548	548	548	
0.09	-0.25*	-0.10	1.05	0.70	0.02
(0.11)	(0.14)	(0.17)	(0.88)	(0.51)	(0.08)
11.18**	13.91**	10.77**	4.27**	9.68**	
(0.09)	(0.08)	(0.10)	(0.71)	(0.44)	
367	367	367	367	367	
-0.08	-0.25*	-0.17	-0.50	1.23	-0.06
(0.12)	(0.13)	(0.20)	(1.18)	(0.83)	(0.09)
11.45**	14.63**	10.99**	3.15**	9.01**	
(0.08)	(0.09)	(0.16)	(0.83)	(0.68)	
181	181	181	181	181	

Note: Columns 1-5 report OLS estimates with village-clustered standard errors. Column 6 reports the standardized average effect, as described in Kling et. al. (2004) and Clingingsmith et. al. (2009), across the measures in columns 1-5
* = significant at the five percent level, **=significant at the one-percent level

Table 6: Effects of LEAP on pro-social laboratory behavior

	(1) Risk taking	(2) Impatience	(3) Altruism	(4) Public good	(5) Trust	(6) Trustworthiness	(7) Average Effect
Full sample							
LEAP	0.10	0.01	49.62	0.05	38.73	0.01	0.07
	(0.11)	(0.34)	(57.44)	(0.05)	(84.68)	(0.02)	(0.06)
Constant	2.19**	3.41**	742.37**	0.64**	620.00**	0.15**	
	(0.07)	(0.24)	(35.13)	(0.04)	(39.47)	(0.01)	
N	524	510	524	523	251	271	524
IDPoor 1&2							
LEAP	0.08	0.07	72.86	0.05	79.56	0.00	0.08
	(0.13)	(0.36)	(65.57)	(0.06)	(107.55)	(0.02)	(0.07)
Constant	2.18**	3.41**	706.21**	0.64**	524.10**	0.15**	
	(0.09)	(0.26)	(43.41)	(0.04)	(62.37)	(0.02)	
N	349	341	349	349	165	182	349
Non-poor	0.13	-0.11	-0.98	0.06	-48.16	0.02	0.05
LEAP	(0.17)	(0.49)	(96.27)	(0.07)	(141.78)	(0.03)	(0.09)
	2.21**	3.41**	817.65**	0.65**	809.52**	0.14**	
Constant	(0.12)	(0.34)	(64.11)	(0.06)	(91.54)	(0.02)	
	175	169	175	174	86	89	175

Note: Columns 1-6 report OLS estimates with village-clustered standard errors. Column 7 reports the standardized average effect, as described in Kling et. al. (2004) and Clingingsmith et. al. (2009), across the measures in columns 3-6
* = significant at the five percent level, **=significant at the one-percent level

Table 7: Effects of LEAP on economic networks

	(1) Exchange hands	(2) Buy and sell	(3) Employee	(4) Coworker	(5) Average effect
Full sample					
LEAP	0.24*	-0.11	0.02	0.05	0.09
	(0.12)	(0.12)	(0.13)	(0.13)	(0.19)
Constant	0.48**	0.75**	0.41**	0.48**	
	(0.09)	(0.08)	(0.10)	(0.09)	
N	526	526	526	526	
ID Poor 1 and 2					
LEAP	0.23*	-0.09	0.03	0.07	0.12
	(0.12)	(0.12)	(0.13)	(0.12)	(0.19)
Constant	0.48**	0.72**	0.40**	0.49**	
	(0.09)	(0.09)	(0.10)	(0.09)	
N	351	351	351	351	
Non-poor					
LEAP	0.24*	-0.15	-0.00	0.01	0.03
	(0.13)	(0.12)	(0.14)	(0.14)	(0.21)
Constant	0.47**	0.79**	0.41**	0.46**	
	(0.09)	(0.08)	(0.10)	(0.10)	
N	175	175	175	175	

Note: Columns 1-4 report OLS estimates with village-clustered standard errors. Column 5 reports the standardized average effect, as described in Kling et. al. (2004) and Clingingsmith et. al. (2009), across the measures in columns 1-4
* = significant at the five percent level, **=significant at the one-percent level

Table 8: Effects of LEAP on social networks

(1) Funeral group	(2) Religious	(3) Babysit-ting	(4) Seek advice	(5) Social group	(6) Volun-teering	(7) Borrow (non-monetary)	(8) Average effect
0.17*	0.19	-0.05	0.00	0.04	0.03	-0.02	0.24
(0.09)	(0.14)	(0.09)	(0.12)	(0.11)	(0.13)	(0.10)	(0.17)
0.02	0.16*	0.39**	0.50**	0.61**	0.39**	0.65**	
(0.02)	(0.09)	(0.06)	(0.09)	(0.09)	(0.10)	(0.07)	
526	526	526	526	526	526	526	
0.18*	0.20	-0.05	0.02	0.03	0.07	0.04	0.28
(0.09)	(0.14)	(0.09)	(0.12)	(0.12)	(0.13)	(0.10)	(0.18)
0.02	0.16*	0.39**	0.48**	0.62**	0.37**	0.63**	
(0.02)	(0.09)	(0.07)	(0.08)	(0.09)	(0.09)	(0.07)	
351	351	351	351	351	351	351	
0.15	0.19	-0.04	-0.03	0.06	-0.05	-0.14	0.16
(0.09)	(0.14)	(0.10)	(0.13)	(0.12)	(0.14)	(0.13)	(0.17)
0.02	0.16*	0.39**	0.55**	0.60**	0.45**	0.69**	
(0.02)	(0.09)	(0.08)	(0.10)	(0.10)	(0.10)	(0.09)	
175	175	175	175	175	175	175	

Note: Columns 1-7 report OLS estimates with village-clustered standard errors. Column 8 reports the standardized average effect, as described in Kling et. al. (2004) and Clingingsmith et. al. (2009), across the measures in columns 1-7
* = significant at the five percent level, **=significant at the one-percent level

Table 9: Effects of LEAP on self-reported group memberships

	(1) SHG	(2) Producer	(3) Rice Seed	(4) Funeral	(5) Youth	(6) Irrigation	(7) Women	(8) Average effect
Full sample								
LEAP	0.27**	0.03	0.26**	0.12	0.04*	0.03**	0.12	0.36**
	(0.07)	(0.02)	(0.07)	(0.09)	(0.02)	(0.02)	(0.09)	(0.09)
Constant	0.28**	0.03**	0.21**	0.19**	0.03**	0.02**	0.19**	
	(0.04)	(0.01)	(0.04)	(0.06)	(0.01)	(0.01)	(0.06)	
N	548	548	548	548	548	548	548	
ID Poor 1 and 2								
LEAP	0.30**	0.03	0.30**	0.13	0.03	0.02	0.13	0.36**
	(0.09)	(0.02)	(0.08)	(0.09)	(0.03)	(0.02)	(0.09)	(0.11)
Constant	0.28**	0.04**	0.21**	0.20**	0.03**	0.02*	0.20**	
	(0.06)	(0.01)	(0.05)	(0.06)	(0.02)	(0.01)	(0.06)	
N	367	367	367	367	367	367	367	
Non-poor								
LEAP	0.21**	0.02	0.18*	0.11	0.06**	0.06**	0.11	0.36**
	(0.09)	(0.02)	(0.09)	(0.10)	(0.03)	(0.03)	(0.10)	(0.13)
Constant	0.26**	0.01	0.20**	0.19**	0.02	0.02	0.19**	
	(0.06)	(0.01)	(0.06)	(0.07)	(0.02)	(0.02)	(0.07)	
N	181	181	181	181	181	181	181	

Note: Columns 1-7 report OLS estimates with village-clustered standard errors. Column 8 reports the standardized average effect, as described in Kling et. al. (2004) and Clingingsmith et. al. (2009), across the measures in columns 1-7
* = significant at the five percent level, **=significant at the one-percent level

Table 10: Effects of LEAP on self-reported community action

	(1) Nr. of village meetings	(2) Repair school	(3) Repair road	(4) Clean public space	(5) Average effect
Full sample					
LEAP	1.78**	-0.00	0.01	0.04	0.14
	(0.69)	(0.10)	(0.10)	(0.06)	(0.11)
constant	6.37**	0.36**	0.60**	0.24**	
	(0.43)	(0.07)	(0.07)	(0.04)	
N	548	548	548	548	
ID Poor 1 and 2					
LEAP	1.46*	0.04	0.02	0.06	0.18
	(0.78)	(0.10)	(0.10)	(0.06)	(0.12)
constant	6.22**	0.34**	0.58**	0.22**	
	(0.45)	(0.06)	(0.07)	(0.04)	
N	367	367	367	367	
Non-poor					
LEAP	2.40**	-0.09	-0.03	0.01	0.07
	(0.86)	(0.11)	(0.11)	(0.08)	(0.12)
constant	6.69**	0.39**	0.63**	0.28**	
	(0.55)	(0.08)	(0.07)	(0.06)	
N	181	181	181	181	

Note: Columns 1-4 report OLS estimates with village-clustered standard errors. Column 5 reports the standardized average effect, as described in Kling et. al. (2004) and Clingingsmith et. al. (2009), across the measures in columns 1-4
* = significant at the five percent level, **=significant at the one-percent level

7. Appendix

Game Descriptions

Altruism: We measured subjects' altruism by their willingness to share with the needy with a simple alteration of the standard dictator game. Subjects were given 3000 riels in 500 riel notes. They were asked to decide how much, if anything, of that amount to donate to a local needy family. The subjects were not told the name of the needy family to protect the family's privacy and avoid any differences between subjects in their affinity with the needy family. Each subject was called individually to the games area. The six half-pound coins were set side by side on sheet of paper with a line drawn across the middle. The subjects were instructed to push the amount they wished to donate to the needy family across the line on the paper and they were told that any remaining amount would be added to the lump sum that they received at the end of the session.

Trust and Trustworthiness: We used the standard trust game (Berg et. al. 1995) to measure trust and trustworthiness. The game was conducted in two rounds. In the first round all subjects were called, one by one, to the private game area. They drew a number from a bag. That number determined whether they were a *sender* or a *receiver* and senders and receivers were anonymously paired according to the number they drew. In actual game play in the field we used the neutral terms “Player 1” and “Player 2” for sender and receiver respectively. Senders did not know the identity of their receiver and vice versa. Both senders and receivers were given an initial endowment of 3000 riels in 500 riel notes. Receivers had no decision to make in the first round. Senders were asked how many riels (in 500 riel increments) they wanted to send to their receiver, knowing that we would triple that amount and that in the second round their receiver would decide how much to return to their sender. The 500-riel notes coins were placed side by side on a sheet of paper with a line through the middle. Senders indicated their choice by pushing the number of notes they wanted to send to the receiver over a line on the sheet of paper. We then tripled that amount and added the receiver's endowment of 3000 riels to show the sender exactly how much money the receiver would have in front of her when she made the decision about how much to return. Once all players had been called to the game area, round one ended and we began round two by calling each player back one by one. Senders had no decision to make in the second round but they were reminded of the decision that they made in round one. Receivers were shown their pot (triple what the sender had sent plus their initial endowment of 3000 riels) in 500-riel notes placed side by side on the game sheet. Receivers indicated the amount they wished to return to the sender by pushing that number of notes over the line on the sheet of paper.

Public Goods: We used a dichotomous public goods game similar to the one described in Barrett (2005). This game did not require supervision of the subjects to play. Each subject was given two folded cards. One of the cards had an X written inside the fold and the other card was blank inside the fold. Play proceeded in two rounds. In the first round subjects were asked to turn in one of their cards. For each X card that was turned in every person in the group received 500 riels regardless of whether they turned in their X card or not. In the second round we asked the subjects to turn in their remaining card. If a subject

turned in an X card in the second round that subject (and only that subject) was given an additional 2000 riels on top of the amount determined by the number of X cards turned in in the first round. If the subject turned in the blank card in the second round that subject was given no extra money, only the 500 riels per X card turned in in the first round.

We also measured two possible confounders to our measures of pro-social preferences:

Risk: It is possible that persons with greater risk tolerance may exhibit behavior that mimics trust but is actually a greater willingness to gamble on the cooperative behavior of the other player. To control for this potential confounder we measured our subjects' attitudes toward risk. Subjects were asked to choose one from among five lotteries each with two possible outcomes. The lotteries were decided by a coin flip performed by the subject. The expected value of all of the lotteries was 2000 riels but the lotteries contained increasing levels of risk. The first lottery contained no risk, with subjects receiving 2000 riels regardless of the result of the draw so the expected payoff had a variance of zero. In the riskiest lottery subjects would receive zero riels if they lost and 4000 pounds if they won, for a variance in the expected payoff of 16,000 riels. As a result, this game offered a five-point scale of willingness to gamble for a higher payoff. Risk-averse people should choose lottery 1 and increasingly risk acceptant people should choose increasingly higher numbered lotteries. The lotteries have the same expected value, so risk neutral people will be indifferent between the five lotteries. Risk neutrality requires a very specific parameterization of the subject's utility function so we considered it unlikely that there were any precisely risk-neutral people in our sample and as such were not concerned about this ambiguity for those specific types of risk preferences.

Discount Rate: We measured discount rates by offering the subjects a choice of receiving an amount on the day of the games or to opt for a larger amount to be disbursed in one week. We presented each subject with six different situations. The first situation gave the subject an option of receiving 2000 riels on the day of the games or 2500 riels in a week. In each subsequent situation (2 through 6) we raised the amount that the subject would receive in a week by 500 riels always keeping the amount received on the game day at 2000 riels. Subjects were asked to specify their preference in each of the six possible situations. Once the subject specified his or her preference in each situation the subject rolled a die to determine which payoff they would receive. In this way we constructed a seven-point scale of subjects' discount rates (or patience) ranging from zero (the subject chose to receive 2000 riels on the game day in all six cases) to six (the subject chose to receive the higher amount in a week in all six cases).

Table A1: LEAP effects on exogenous networks and socio-demographic characteristics (These results are included as a placebo test and balance check)

	(1) Family	(2) Neighbor	(3) Krum	(4) Sex
Full sample				
LEAP	0.12	0.02	-0.00	-0.00
	(0.09)	(0.09)	(0.12)	(0.03)
Constant	0.70**	0.66**	0.45**	0.16**
	(0.08)	(0.06)	(0.09)	(0.02)
N	526	526	526	523

	(5) Age	(6) Household size	(7) Education in yrs.	(8) Average effect
Full sample				
LEAP	-0.13	-0.04	-0.21	0.10
	(1.44)	(0.21)	(0.42)	(0.13)
constant	42.30**	4.97**	2.75**	
	(1.02)	(0.16)	(0.36)	
N	521	521	521	

Note: Columns 1-7 report OLS estimates with village-clustered standard errors. Column 8 reports the standardized average effect, as described in Kling et. al. (2004) and Clingingsmith et. al. (2009), across the measures in columns 1-7
* = significant at the five percent level, **=significant at the one-percent level

Table A2: Instrumental variables estimation of effects of SHG Membership on laboratory measures of pro-social norms

IV Models	(1) Risk taking	(2) Impatience	(3) Dictator	(4) Public good	(5) Trust	(6) Trustworthiness
Full sample						
SHG member	0.38	0.03	191.18	0.20	176.20	0.03
	(0.41)	(1.31)	(220.95)	(0.20)	(365.76)	(0.06)
constant	2.08**	3.41**	687.64**	0.59**	566.44**	0.14**
	(0.18)	(0.57)	(90.61)	(0.09)	(132.01)	(0.03)
N	524	510	524	523	251	271
IDPoor 1&2						
SHG member	0.28	0.23	253.31	0.17	321.17	0.01
	(0.45)	(1.23)	(235.26)	(0.23)	(405.34)	(0.07)
constant	2.09**	3.35**	631.80**	0.59**	423.49**	0.15**
	(0.21)	(0.57)	(104.82)	(0.10)	(167.40)	(0.03)
N	349	341	349	349	165	182
Non-poor						
SHG member	0.64	-0.54	-4.73	0.28	-285.26	0.10
	(0.87)	(2.44)	(457.09)	(0.34)	(856.68)	(0.13)
constant	2.04**	3.56**	818.93**	0.58**	891.03**	0.12**
	(0.34)	(0.93)	(172.19)	(0.14)	(310.80)	(0.05)
N	175	169	175	174	86	89

Note: IV estimates with village-clustered standard errors.
* = significant at the five percent level, **=significant at the one-percent level