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

Abstract

Do self-help groups (SHGs), village-based associations designed to encourage savings, household production and social cohesion among the poor, meet their goals? We examine economic outcomes and the pro-social behavior of 540 households in a randomized control trial (RCT) of a SHG program (randomized at the commune level) in rural Siem Reap, Cambodia using survey data and a rich set of economic and social capital indicators. We measured social capital—defined as social norms and the social networks that support them—with household and network surveys and lab activities that gauge altruism, trust, trustworthiness and the willingness to contribute to public goods. We find that the program successfully increased participation in SHGs and strengthened SHG-related networks. As intended the program significantly increased the number of households with non-zero savings as well as savings levels and it led to a noticeable shift in household production towards livestock. We cannot document increases in household incomes, assets or expenditure. There were also no sizeable and wider effects on social capital and networks other than those related to SHGs directly, although we cannot statistically rule out small positive effects in the case of some social capital indicators. In addition to these empirical findings the study provides an example of innovative program evaluation techniques that employed a field experiment, lab-in-the-field behavioral measures, network measures as well as traditional survey measures.

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

JEL Classification Codes: O1, I3, Q1, D7, C9

1. Introduction

We evaluate the impact of an eighteen-month-long program to promote economic and social empowerment among the poor in rural Siem Reap, Cambodia. The program, called LEAP (Livelihood Enhancement and Associations among the Poor), pursued these goals by establishing *self-help groups* (SHGs): *"village-based organizations that focus on building the savings and credit as well as social empowerment of their (mostly female) members."* (see Desai and Joshi 2013, p.3; citing Chen et al 2007). The program randomized at the commune level created 100 such groups serving 1291 members in rural areas of Siem Reap province. Our randomized evaluation examines economic outcomes and the pro-social behavior of 540 households.

We designed our research with three questions in mind: did the program increase savings; did it enhance livelihoods and did it increase civic engagement and social capital among the poor? We found robust and positive evidence for the first question: Savings (both their likelihood and levels) and related participation in SHGs increased substantially. The robust and positive savings and SHG creation effects from this short eighteen-month program with a sample size of only 540 are noteworthy, which indicates that the program was quite successful at its primary goal.[1]

We could not find consistent evidence of success on the program's other two goals though. Households in treated communities moved into meat production and out of other income-generating activities including plant cultivation which is more difficult in the program area's relatively dry climate compared to the rest of Cambodia. The extent of any overall improvements to livelihoods remains uncertain. Overall production incomes and assets did not increase significantly over the year and a half of the program possibly due to brevity of the program.

[1] As we describe in section 3 below, LEAP planned to work with each self-help group for a total of three years. However, the intervention was not fully implemented due to some general and sudden funding stops following disagreements over land evictions (unrelated to LEAP) between the World Bank and the Cambodian government. The situation only improved in 2016: *World Bank Will Resume Funding to Cambodia,* The Cambodia Daily, May 21, 2016. Available at: https://www.cambodiadaily.com/editors-choice/world-bank-will-resume-funding-to-cambodia-112866/ [Accessed July 4 2019]

The same is largely true with broader social change. While the program fostered greater networking via SHGs, the program's networking effects did not generally extend to other sorts of networks. Average effects were in the small to medium range for economic and social networks but they were statistically insignificant, pointing to low-powered test. Subjects in treated communities exhibited only slightly more pro-social behavior in laboratory activities or community service than did subjects in control communities, and these small positive effects are insignificant statistically. We did find promising evidence of greater group participation in treated communities. This average result was driven entirely by an increase in membership in rice seed banks. These seed groups were not created by LEAP but they are prevalent in our study area and seem to complement SHGs even in the absence of LEAP, suggesting that there were some downstream social changes due to the program.

As discussed in the literature review, one of our innovations over existing RCTs of savings and self-help groups is a more extensive set of social capital measures in addition to looking at a host of economic outcomes. Here we apply the widely-cited definition of social capital by Putnam (2000, p. 19): "*... social networks and the norms of reciprocity and trustworthiness that arise from them.*" We strived to capture these key components of social capital directly. We measured the networks across several domains after extensive focus group research in the region. To measure the norms, we devised lab-in-the-field techniques to record subjects' norms, observing their behavior in structured and incentivized choice activities. We also implemented a standard household survey covering a range of self-reported economic and social activities. In this way, we followed Chuang and Schechter's (2015, p. 151) advice: *"in a developing-country context, researchers should explore designing simpler experiments and including survey questions in addition to experiments when measuring preferences."*

Our study is set in an interesting social context. Previous qualitative and lab-in-the-field research has indicated that social capital in Cambodia is weak. Kerbo (2011) and Colletta and Cullen (2000) describe levels of trust that are particularly low even 30 years after the genocidal war. In a sentiment expressed by many of Kerbo's interviewees, a Cambodian

NGO worker described the Cambodia people thusly: *"They have lost much of their trust in fellow citizens that existed before the civil war and Khmer Rouge days."* (p.173-4). Kerbo describes Cambodia as a country that is *"missing civil society"* (p.183). Weingart and Kirk (2012) found levels of trust and trustworthiness to be relatively low levels compared to other countries. These findings point to an important impediment to social and economic development in the study area and the need for improvement sought by LEAP.

The social-capital-creating mechanism we have in mind is the one modeled by Avdeenko and Gilligan (2015). In that model, following Putnam's definition quoted above, people apply two different sets of norms, one for members of their social network and one for members outside their social network. The former set of norms is more trusting and altruistic and in general pro-social than the latter set of norms because it is supported and enforced by a set of rules and relationships within the social network. A program like LEAP, then, would enhance social capital by expanding social networks so that in-network more prosocial norms are applied to a larger group of people. While we find strong treatment effects on participation in SHGs and SHG-related networks, these do not correlate with pro-social behavior, and effects on wider networks may have been too limited or conducive to induce more substantial changes in pro-social behavior as suggested by this theory.

Another possible mechanism is inequity aversion (Fehr and Schmidt 1999).[2] If a development intervention successfully improves an individual's economic position, altruism towards needier members in the community could increase. This mechanism running from economic to social outcomes would be consistent with social preferences featuring aversion towards inequity experienced by others as proposed by Fehr and Schmidt (1999). Indeed, we find that total annual household production correlates positively with altruism towards needy households in the communities as measured by a dictator game.[3] Likewise, savings levels correlate positively with willingness to contribute

[2] We thank an anonymous referee for proposing to examine this mechanism ex-post.

[3] We ran these correlational regressions in the control group to abstract from treatment-related changes in social and economic outcomes.

to the community in a public good game. However, while the program appeared to cause large increases in savings, household production was not significantly increased by LEAP and may have been too small to cause larger increases in pro-social behavior.

This paper is organized as follows: Section 2 systematically reviews the previous literature and underlines the value-added of this study. Section 3 describes the program. Section 4 details the empirical strategy. Section 5 presents the results. Section 6 concludes.

2. Previous Research and Contribution of Present Study

Microfinance programs can be divided into three broad categories: microcredit, savings, and self-help programs. In microcredit programs, outside lenders (commercial banks, government agencies or non-governmental organizations) make small loans to groups. Microcredit programs bring new outside capital into communities and are sometimes called microloan programs for this reason, but they place no necessary emphasis on savings or asset accumulation by their members. Savings groups, as the name implies, do place an emphasis on savings and asset accumulation. Members of savings groups make regular contributions to a pool and apply for loans from that pool. The group awards loans according to a fixed decision rule. No outside capital is necessarily injected into these programs, although in many cases a small amount of seed money or matching funds may be provided by the program organizer. Savings groups programs go by a variety of names including village savings and loan associations (VSLAs), accumulating savings and credit associations (ASCAs) and savings and internal lending committees (SILCs).

While both microcredit groups and savings groups attempt to foster economic empowerment, neither, by our reading, necessarily attempt to create social capital, political and social empowerment or civic engagement. If anything, rather than using these groups to create social capital these programs appear to be designed to piggyback on existing social capital, using social pressure to induce higher rates of loan repayment and savings respectively (Attanasio et al 2015, Kast et al 2012, Ghatak and Guinnane, 1999). SHGs, the object of our study, are different in this regard: they explicitly attempt to foster social capital, social empowerment and political participation. Put another way SHG programs

are savings groups plus social capital training and encouragement (Desai and Joshi, 2013, Carter 2013).

To limit our review to an acceptable length we concentrate on the three outcomes that are the focus of this study: savings, livelihood enhancements and the formation of social capital. Furthermore, we include only studies that have an explicit strategy to causally identify program impacts. For readers interested in a general review of microfinance programs we recommend the helpful reviews by Brody et al (2017), Entz et al (2016), Graafland and Rijnevald (2016), Gash and Odell (2013), van Rooyen et al (2012), Duvendack et al (2011) and Fernandez (2006).

The studies that meet our criteria are summarized in Table 1. Each study occupies a row of the table. This first column lists the citation of the study. The study's identification strategy is specified in the second column: PSM stands for propensity score matching, DD stands for difference in difference and RCT stands for randomized control trial. "Pipeline" is a method applied to observational data in which new members are compared to older members based on the claim that those two groups are statistically interchangeable. The second column lists the locus of the treatment, whether it was administered at the village level (as in our case), the group level or the individual member. The next five columns indicate the results of the study (if any) on the outcomes in which we are interested: savings, livelihoods, and three social capital measures.

There is a strong consensus in the literature that SHGs improve savings. Five of the six SHG studies in Table 1 that offer findings on savings (including this one) registered significant increases in savings as a result of the programs they evaluated.[4] Deininger and Liu (2013a, p. 156) do not report on savings accumulation but in their study program beneficiaries reported a significantly greater "ability to save individually," meaning they

[4] We do not include Greaney et al (2016) in Table 1 because they did not test the effects of savings groups but of the method of creating those groups. They examined whether a program that paid private agents to set up savings groups was as effective as a traditional program where outside NGOs help set up these groups. They found that the private-agent scheme produced similar amounts of saving as the NGO model at lower cost. However private agents tended to produce greater savings for business rather than households.

had their husband's permission to save individually. Khanna et al (2015) do not report results on cash savings but show that the program they studied increased non-financial assets, mainly household durables and livestock. Savings groups also generally exhibit a positive impact on savings. All of the SG studies in Table 1 report significant increases in savings.[5] Finally, microcredit programs show little impact on savings, a not-unexpected result given that the purpose of these programs is to make loans, not encourage savings. Indeed, only four of the microcredit studies in Table 1 even report on savings and only one of these showed an increase in savings.

There is a similar consensus in the literature on the effects of SHG and SG programs on livelihoods. Each study defined livelihood improvement somewhat differently. Four of the six SHG studies that report findings on livelihoods found that the programs they evaluated improved them. Khanna et al (2015) found that the program they studied shifted livelihood portfolios toward higher skilled and more secure jobs. Desai and Joshi (2013) find that the program they study caused a significant increase in employment outside of agriculture, which was particularly beneficial in the drought-stricken period of their study. Swain and Varghese (2009) focus on asset creation but they did find significant increase in group members' total incomes which we take as a sign of livelihood improvement. Datta (2014) found no improvement in livelihoods, measured as shifts toward a particular livelihood and away from others. He also found no increase in the number of income earners in treated households. He does report a robust increase in animal husbandry of one-half a percent, but still concludes overall that the program did not improve livelihoods. Our results our surprisingly similar to Datta's. We found significant shifts toward animal husbandry but only a small (of 0.1 standard deviations) and statistically insignificant average effect on overall production outcomes.

Three of the four SG studies that covered livelihoods found positive impacts. The program Ksoll et al (2016) studied caused an increase in business incomes. Karlan et al (2017) found that the program that they studied over three countries improved an index composed of

[5] Deininger and Liu (2013b), which is not listed in the Table 1 because it does not report on any of the outcomes in Table 1 also showed a significant increase in assets, mainly livestock.

three business outcomes (number of business operated, months of operation in the preceding year and number of employees). Beaman et al (2014) did see increases in business income but no significant increases in small business profits or ownership. The program they studied also caused no increase in agricultural output. Annan et al (2013) did not report on livelihood outcomes but did report a significant reduction in households in poverty as a result of the SG program they study. Most studies of microcredit programs found no impact on livelihoods. The one exception was Crepon et al (2015) who found strong evidence of improved livelihoods: greater investment in and profit from self-employment activities and less reliance of casual labor. Karlan and Zinman (2011) found that the size of treated subjects' business enterprises actually shrunk compared to control subjects, however they did not assess other livelihood measures like income, skill accretion or job security.

As mentioned above SHGs explicitly strive to improve their members' social empowerment through civic engagement while SGs and microcredit groups tend to focus more narrowly on economic empowerment. This extra feature of SHGs is reflected in the literature: all but one of the studies of SHG programs evaluated the effect of their program on civic engagement. Only three SG and two microcredit studies did so and of those five only one found a positive impact.

There is strong agreement in the literature that SHGs cause greater civic engagement, generally in the form of greater attendance at community meetings. In addition, Desai and Joshi (2013) determined that members of treated communities possessed greater knowledge about their local government. In keeping with their focus on economic rather than social outcomes studies of savings groups address civic participation less commonly. Karlan et al (2017) and Beaman et al (2014) did ask respondents about attendance at community meetings, raising an issue with community leaders and other forms of civic participation but found no effect of the programs they studied. Concern with civic engagement is even less common in studies of microcredit groups. Pitt et al (2006) were particularly interested in women's empowerment so they did ask whether members of the microcredit groups they studied were more likely to attend community meetings and found

that they did. Banerjee et al (2015) were not interested in civic engagement *per se* but they were interested in consumption so they did examine household expenses on community festivals. They found that members of the microcredit group they studied spent significantly less on community festivals, suggesting a possible pathway between microcredit groups and a *reduction* in civic participation.

Social capital, measured by pro-social community norms and social networks to support them, have been rarely studied in this literature even in studies of SHGs. Besides this study only Deininger and Liu (2013a) and Kim et al (2009) address the effects of the programs they study on social norms and networks. Deininger and Liu (2013a) found that residents of treated communities self-reported significantly greater trust in community members and public officials than villagers in control communities. Respondents in Kim et al (2009) recount having larger social networks and subjectively assess greater community support and solidarity. Unlike our study both of these studies did not use behavioral measures of norms but relied on self-reports, which raises concerns about social desirability bias in self-reports in the treated communities. Beaman et al (2014) is the only SG study that addressed networks and norms. They asked if the respondent could borrow from or would be willing to lend to another woman in the community or would go to the market with a woman in the sample. It is unclear whether responses to these questions are measures of networks or norms. Regardless, the results effects of the program were very small and statistically insignificant.

Generally, studies of microcredit programs do not address social capital. The two papers by Feigenbaum and his coauthors are notable exceptions. They show that members' social networks are enhanced by participation in the Grameen style program they study. Both papers found that women in groups that met more frequently had more social contact than women who met less frequently. Feigenbaum et al (2014) also used a public goods game to measure pro-social norms and found that women who met more frequently exhibited greater pro-social norms. This latter article is the only other study (besides ours) that used behavioral measures in the study of microfinance. While they did not explicitly try to measure social capital Karlan and Zinman (2011) did ask about friends and family

networks in their study of a microcredit program in the Philippines. Respondents in treated communities reported significantly greater confidence in relying on friends and family for large amounts of financial assistance in an emergency, suggesting stronger friends and family networks in treated communities. Karlan and Zinman (2011) also asked about trust but found no significant impact of the program they studied. Finally, Angelucci et al (2015) found an increase in trust in people but no effect on trust in institutions. Bannerjee et al (2018) focused on networks and found that participation in microcredit group reduced the number of network links in their large sample of Indian villages. One possible reason for the disparity between the results of Bannerjee et al and those of Feigenbaum at al and Karlan and Zinman is that Bannerjee et al measured the quantity of links while the other three papers focused on those link's quality.

Two studies, not listed in Table 1 because they do not directly address our outcomes of interest, hint at the development of social capital while not providing direct evidence of it. Using a survey of local public officials and SHG members in India, Casini and Vandewalle (2017) found that SHG members' community action on issues important to them spurred greater action on those issues by local public officials. Fafchamps and La Ferrara (2012) provide evidence that SHGs serve as mutual assistance groups, helping to insure members against negative household shocks.

To summarize, both SGs and SHGs improve savings and (although the evidence is a bit less strong) livelihoods. Microcredit groups have no appreciable effects on these outcomes. SHGs promote civic engagement while SGs do not, which is neither surprising nor a criticism of SGs because their *raison d'être* is economic not social empowerment. While the evidence that SHGs promote civic engagement is strong the evidence that SHGs promote social capital (defined as social networks and the pro-social norms they encourage) is sparser, not because it does not exist, but because scholars have not looked for it. Indeed, in the few cases where research has looked for impacts on networks and norms (including this paper) it has found them. Finally, while there is some evidence that microcredit groups promote some social capital that evidence is scant.

The take-ways from Table 1 are that SHGs and SGs are much more successful at bringing about positive social change than microcredit programs are. SGs and SHGs improve savings and livelihoods while there is no tangible evidence that microcredit programs do. SHGs have the added benefit over SGs that they promote civic engagement and social capital. Although the evidence on SHG's impacts on social capital is very promising, it is not as extensive as the evidence on civic engagement. More study on the effects of SHGs on networks and norms would be worthwhile.

3. Background and Program Description

Siem Reap hosts Cambodia's majestic Angkor Wat temple. Areas close to the temple have experienced a tourism boom with millions of tourists every year, and as a result the area around the temple including the town of Siem Reap has seen explosive economic growth. However, this tourism boom has not reached parts of the province some miles away from Angkor Wat and the city. Large parts of the local population do not have the education levels or English language skills needed to benefit from this boom directly. In a 2008 study, 14% of Siem Reap province residents were considered very poor (ID Poor 1) and another 15% were considered poor (ID Poor 2) despite the substantial tourism flows to Angkor Wat temple.[6]

To combat persistent poverty in the rural areas of Siem Reap, the Cambodian government and the World Bank launched LEAP, initially as a pilot project. LEAP had three official, pro-poor objectives: *1) building and strengthening SHGs among the poor to facilitate collective action with and serve as intermediaries to state and lending institutions, 2) providing the poor with better access to finance and 3) forging better links between poor producers and important markets and value chains.* The program hoped that through these

[6] The Ministry of Planning in Cambodia runs a program for the identification of the poor, IDPoor for short. The categorization of households by poverty status aids with program targeting. Using surveys, authorities put households in one of two poor groups (IDPoor 1 and IDPoor 2) or neither. IDPoor 1 are the very poor, struggling to have enough food. IDPoor 2 are less poor, living between the food poverty line and the poverty line. If the household is in neither group 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).

activities the villages would accumulate social capital which would in turn strengthen villagers' trust, trustworthiness and capacity for collective action in pursuing these goals.

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, bookkeeping, and meeting facilitation). Individual SHG members were instructed on how to increase savings and make and obtain loans. They were trained in gender mainstreaming and agricultural techniques. They also received information on civic participation, the identity and responsibilities of their government officials and how to approach them with their concerns. The SHGs were closely monitored to ensure regular and well attended meetings, steady saving and lending, adherence to internal group rules and proper bookkeeping. All SHGs were officially registered with the commune council. Each SHG also underwent an extensive performance rating and received overall performance scores. Groups met weekly for training and contributed to the savings pool monthly. 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 timing of the LEAP pilot (and our involvement) was as follows (see timeline in Figure 1). Members of the Cambodian LEAP team met with the authors in May 2010 at a conference in Dubai as part of the World Bank's Development Impact Evaluation (DIME) initiative where we began the first stage of designing the randomized evaluation for a project covering the entire province. The funding source (the World Bank) facilitated this exchange. For transparency note that the first author was working at the funding source at the time. As part of the randomization for the province-wide impact evaluation, we randomly selected the pilot communes to receive the program in June and LEAP launched the smaller pilot program in July of 2010, too soon after we were brought on to gather baseline data. The pilot phase ran until July 2012 and was supposed to be followed with and inform the full implementation of the program (accompanied by our full impact evaluation building on household and behavioral baseline data collection), however for

reasons beyond LEAP's control and responsibility, World Bank funding ceased and the pilot and subsequent full programs entered a period of budgetary uncertainty (LEAP, 2012; see footnote 3). From July through November 2012 pilot SHGs continued to meet without outside support. LEAP received a small grant of almost 10,000 USD in November 2012 to support existing SHGs until January 2013. We began field work for this pilot evaluation in April 2013. The larger province-wide impact evaluation of course never materialized.

The LEAP pilot led to the following officially reported outputs (see LEAP, 2012): To ameliorate the social institutions of the poor, LEAP created 100 self-help groups with 1,291 household members, 90 percent of whom were female. To improve savings and access to credit all 100 SHGs opened bank accounts at major commercial banks. Program staff reported that these 100 SHGs had accumulated total savings of about 78,000 USD at the time of our study in late April and early May of 2013.[7] As of May 2012, over 5,800 loans had been made from SHG funds, 85 percent for investments and 15% for consumption and the program had made over 33,000 USD in seed grants to the SHGs. On average each SHG received USD 336 corresponding to 26 USD per participating household. To boost the poor's access to markets and value chains these 100 SHGs reportedly created 52 producers' groups, 38 in chicken raising (73% of the total), seven in pig raising, four in basket weaving, two in vegetable raising and one in rice selling. Our findings reported in section 5 raise questions about how active these groups actually were, at least at the time of follow up.

4. Empirical Strategy and Data

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

[7] This statement by the program is somewhat at odds with our own findings. Our estimates, reported in section 5 below, indicate that each member had accumulated additional savings of about 7.5 USD. Total program savings of 78,000 USD reported by the program implies an average savings of 60 USD for each of the 1291 members. Subtracting the seed-grant average of 26 USD per member still leaves 34 USD per member, considerably in excess of our finding. It is possible that the 78,000 figure is the peak amount accumulated by the program and the lower amount we found was due to the lack of support during the World Bank funding hiatus.

villagers to markets produced 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 important to them. Any evidence that we could find that LEAP produced social capital, especially among the poor, would be taken as an extremely important impact of the program by the program's designers.

4.1 Measurement

We collected data to measure the savings, livelihoods and broader social impacts of the program in three ways. First, we conducted an extensive randomly sampled household survey in treated and control communes to measure the respondents' savings behavior, improvements in livelihoods, consumption behavior and incomes. This survey provided our measures of assets, savings, expenditures and livelihood activities. We also asked questions about civic participation and group membership that we use as social capital measures in combination with the measures described below to complete the picture of the social context of the villages. After completion of the survey, the household head (or their partner) was invited to participate in an experimental session and the collection of network data.

Networks form a key part of social capital, so we recorded socio-economic links between our laboratory subjects, essentially taking a snapshot or random sample of the overall community network.[8] More specifically, we collected data on the matrices of relationships across several socio-economic domains. We picked the most relevant domains for the impact evaluation following extensive focus group discussions (such as self-help group links, labor exchange, regular buying and selling). Our enumerator recorded the network links during a group discussion, which allowed crosschecking of links between participants. Sometimes individual participants would forget to mention a link, and others in the group would help to fill such gaps. Conversely, in some rare cases there was disagreement about a specific link between two subjects, which after some further

[8] These network data were collected after the social capital experiments (discussed next), so there are no concerns related to priming effects running from networks to prosocial behavior.

discussion and continued disagreement we would not record. We employ the total number of links *within* a given group as our main network measure. The maximum number of possible links is 14 (The number of laboratory session attendees minus one). One caveat that also applies to our group membership measure from the household survey: conscious of our subjects' time and recall limitations, we did not record the duration of the link or how intense it was, which would have allowed us to investigate impacts along the intensive or extensive margins.

We also conducted lab-in-the field activities to evaluate impacts of LEAP on social capital, since we were worried that purely self-reported measures may be systematically biased (Avdeenko and Gilligan, 2015; Mansuri and Rao, 2013): treated (unlike control) units were exposed to LEAP program staff, which may directly or indirectly prime community members to give socially desired answers to questions such as 'Do you contribute to public goods?' or 'How trustworthy are your neighbors?' We opted for activities in a controlled setting to tease out potential *behavioral* impacts of the program. It is important to note that our subjects were incentivized and made choices anonymously. Relatedly, subjects did know that they were playing with somebody else from the group (such as in the trust game), but we did not reveal exact identities in order to preserve anonymity.

We thought that the advantages of this experimental approach (coupled with the use of more traditional survey-based measures) outweighed potential disadvantages such as those described by Levitt and List (2007). They argue that people may be more cooperative in the lab than they are in real life because lab monitors are authority figures, subjects are being monitored (Hawthorne effects), people use different heuristics in real life than they do in the lab and stakes in the lab are typically lower than they are in real life. Levitt and List are clearly leveling their criticism at the use of behavioral activities as absolute measures of social preferences. These concerns are mitigated when behavioral activities are used, as they are here, to compare social preferences of subjects in treated and control communities in a RCT. For our purposes subjects' behavior in the lab does not need to match precisely their behavior in real life; it only needs to be positively correlated with it and any mismatches need to be uncorrelated with treatment. Since the treatment was

randomized, worries about such correlation with the treatment are eased. To address the issue of small stakes we ensured that the payouts in our lab sessions were substantial. At the end of the experimental session, the average subject won about 16,500 riels (over four dollars), which corresponds roughly to one daily wage. Acting pro-socially in the lab actually costs the subjects something and for that reason we argue that it captures something of their true beliefs and preferences. Furthermore, one of Levitt and List's arguments about lab measurement is that context matters, people bring norms from the real world with them into the lab. But this was precisely our motivation: we used lab activities to gauge if norms in the treated villages are more pro-social than those in the control villages. Our lab-in-the field strategy therefor echoes Hoffman, McCabe and Smith (1998, p. 350): "*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* [...]." (see Avdeenko and Gilligan, 2015) Another common and more practical worry for lab-in-the field studies is a non-random selection of participants or lack of representativeness (Cardenas and Carpenter, 2008). As we show below, there was no systematic non-response to our random invitation to participate in the activities. Finally, we do not rely solely on behavioral measures but combine them with standard survey measures as recommended by Chuang and Schechter (2015).

We implemented five well-established lab-in-the-field activities.[9] A similar strategy to measure social capital was used by Avdeenko and Gilligan (2015), Henrich et al (2010), Schechter (2007), Karlan (2005), Henrich et al (2004), and such activities have been widely used in the Global South (for an earlier review see Cardenas and Carpenter, 2008). Our three main activities were meant to capture subjects' pro-social behavior, closely following the procedures in Avdeenko and Gilligan (2015): altruism as expressed by the willingness to share with the needy, trust and trustworthiness and willingness to contribute to public goods. The remaining activities measured attitudes toward risk and intertemporal discounting.

[9] Theses activities and the network survey were done by a different team than the household survey.

The first activity captured altruism or generosity towards others in the community (benefiting others in need at a personal cost without receiving anything in return). In this simple activity, subjects received 3,000 riels and we instructed them to choose how much of it (if anything) to transfer to a poor family in their community. We did not reveal the identity of the family for privacy reason and more importantly to measure undirected or pure altruism. Subjects made their choices seated at a table in a private choice area. We placed a sheet of paper with a dividing red line on the table. We then put six 500-riel notes in front of the subject and asked her to push the donation (if any) across the red line. We emphasized that the remainder of the money was paid out at the end of all activities.

Our second activity was a classic game to measure trust and trustworthiness (Berg et al 1995). Unlike the previous activity, this investment game featured strategic interactions among our subjects. Each subject was randomly assigned to be an investor (or sender) or a trustee (or receiver) by drawing a number at the game station. We did not use these terms when explaining the activity in order to avoid priming effects, and simply referred to "Player 1" and "Player 2." We then anonymously matched each investor with a trustee. These pairs then interacted in two rounds: In round one, each player received a starting endowment of 3,000 riels in notes of 500. Notes were again placed on a sheet of paper with a red line running though the center. The investor was asked in private (at the game table) how much she would like to send to the trustee (by pushing notes across the red line). We told the subjects that we would triple the amount they sent and give that amount to the trustee. The trustee would subsequently decide how much (if anything) of that total pool to return to the investor in the second round. If the investor sent say 1,000 riels, the trustee would then have 6,000 riels (their 3,000 endowment plus the 3,000 from the investor's decision) to decide how to allocate in the second round. We made this process clear by tripling the amount sent on the sheet of paper in front of subjects. In this first round, player 2, the trustee, did not make any decisions and we simply informed her about the starting endowments. After all players had visited the game table once, we proceeded to the next round, again player by player. This time, investors did not make any decision. Instead,

trustees were shown how much they received (placing bills on the sheet of paper) and asked how much they would like to return (if anything) by pushing bills back across the red line.

Our third activity was played at the group level in the form of a dichotomous public good game akin to Barrett (2005). We gave two folded cards to each participant - one blank and one marked with an X inside the card. We then collected the cards in two rounds. For each X card handed in the *first* round, every subject in the group session received 500 riels. The other card was returned in the second round. If a subject handed in the blank card in the first round and kept the X card for the second round, she would receive 2,000 riels in addition to 500 riels times the total number of X cards turned in by the group in the first round. In other words, subjects could contribute to the group or defect while still benefiting from contributions by other participants (*viz*. free riding).

We also conducted activities to measure risk and time preferences to complement our data on social preferences. Risk preferences may be correlated with trust and public good game behavior (Schechter, 2007) and therefore confound our outcomes of interests. Importantly, gambling is quite common in the study area, and we wanted to identify preferences for it, so in our fourth activity of the day, we elicited risk attitudes of subjects. Subjects picked from five lotteries featuring two outcomes each, decided with a coin flip. We kept the expected value across lotteries fixed at 2,000 riels, only increasing variance. More specifically, the first lottery choice was risk-free. Subjects would receive 2,000 riels independently of the coin flip. In the fifth lottery, subjects could win 4,000 riels or zero, implying a variance in the expected payoff of 16,000 riels. The pay-off table is available in the online appendix. We measured each subject's willingness to gamble for a higher payoff on a simple five-point scale. Other standard elicitation methods are more complicated, so we adopted this simpler one in order not to burden participants, especially since this was not an outcome variable. A risk averse person would strictly prefer the no-risk lottery and increasingly risk acceptant people would prefer increasing levels of risk. We cannot distinguish risk neutral people, but we simply wanted to control for gambling behavior, which as we discuss below was not a confounder in the end anyway.

The fifth and final activity captured time preferences or the level of patience. Subjects had to decide between receiving some money today (2,000 riels) or a series of larger amounts after a week. In six choices, we gradually increased the future amount (2,500, 3,000…5,000 riels), recording when subjects switched from an immediate payout to a delayed one (the table is shown in the online appendix). After a subject had completed the activity, she had to roll a dice to determine her payout. We use a six-point scale of patience, ranging between 1 (the subject always preferred an immediate payout) to six (the subject always preferred a delayed payout).[10]

Many of our subjects were illiterate. Following the advice of Cardenas and Carpenter (2008), we employed simple and visual instructions. Likewise, we were also forced to record behavior in our activities with the guidance of a facilitator at the game station (except in the public good game that was played in a group). Such close supervision is common (Karlan 2005, Henrich et al 2004), but can raise concerns relating to for instance social desirability bias or Hawthorn effects. However, all activities were implemented in the exact same way across communities. For instance, the roles and responsibilities of our survey team members were fixed, so any such effects should be balanced across treatment and control, and therefore should not bias point estimates.

Before any game play began, we explained to the subjects that they would receive their total payouts at the end of the experimental session.[11] We did not give them a running total of their winnings over the course of the session. We explained all activities orally both to the group and to individuals at the game station following a detailed experimental script in Khmer. We provide the English script in the online appendix.

4.2 Randomization and survey sample

[10] The subjects were told that the money would be left with their chief to be picked up in a week. We were aware that subjects may have had different levels of trust in their chief. As with the gambling variable this measure was not a main focus of our analysis, but included only as a possible confound or operating mechanism for social preferences. As we show below, patience was uncorrelated with the treatment and with interpersonal trust, so this should not be a concern.

[11] In a robustness check reported in section 5.5, we find no evidence that initial winnings (in the lottery activity) correlate with subsequent pro-social behavior.

This section describes the randomization, sampling strategy, and empirical model. The LEAP team asked us to implement a rigorous randomized control trial in Siem Reap province in order to inform an eventual roll-out of the project in other provinces of Cambodia. To mitigate inter-village spillovers, we randomized at the commune level—the lowest administrative level above the village. The pilot was budgeted to run in all villages of six communes out of a total of 50 communes in the province.

Our evaluation is based on the randomized introduction of the pilot scheme using follow-up data only. We could not collect proper baseline data before the roll-out of the pilot, because we started collaborating with the LEAP team only shortly before the launch of the project. We had planned a larger RCT with baseline data (using a more extensive household survey and also behavioral games), but the larger project did not continue as planned due to reasons beyond the scope of our involvement (see footnote 2 for details). Therefore, we were only able to evaluate the pilot scheme with follow-up data. We examine balancing success in terms of a large array of plausibly pre-determined commune, village, household and individual variables (as explained in more detail below). Since we did not have a pre-analysis plan in place nor pre-register, which was uncommon in the field at the time[12], this is an exploratory type of analysis. Finally, there were no power calculations performed for this pilot study.

The randomization and sampling strategy are summarized in Figure 2. To evaluate a causal effect of the project, we randomly selected 6 communes to receive the LEAP pilot. All 18 villages in the 6 treated pilot communes were treated and also surveyed. In addition, we randomly sampled 18 villages from 18 randomly selected control communes. This is an intent-to-treat design: the program was not offered in control communes and within treated communes the program was offered but participation was voluntary. In each of the 36 villages (18 treated, 18 control), we aimed to survey 15 households. Since this is an intent-to-treat design, our sample from the treated villages contains both SHG members and residents who elected not to participate in the program.

[12] For instance, the AEA's trial registry was only launched around 2013. See here: https://blogs.worldbank.org/impactevaluations/trying-out-new-trial-registries [Accessed January 20 2020]

We used earlier census and sampling lists with household names for each village to reach a total of 540 households. LEAP targeted poor households, those that were officially classified as IDPoor-1 and IDPoor-2. However, the success of targeting is ultimately an empirical question. 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). We had to sample 85 substitute households (42 control, 43 treated) since not all initially chosen households could be surveyed. Substitute households are balanced by treatment status as can be seen in column 1 of Table 2, where we regress missingness on a LEAP dummy, as well as an interaction term between LEAP and being a non-poor household (see section 4.5 for details on the empirical models including average effects and p-value calculation). The effect associated with the variable *LEAP* is the main point estimate of interest, showing impacts concentrated in the targeted group (IDPoor 1 and IDPoor 2). The additional effect among the Non-poor is captured by the term *LEAP x Non-poor*. Column 1 shows that the treatment effect on the likelihood of being a poor substitute household amounts to an insignificant 3%-points. The additional effect of being non-poor is negative and offsets the positive LEAP effect.

The household survey team gave each household an invitation to send a primary adult (mostly females) to a laboratory session on a later day in that village. After the household survey had passed through the village, the second team organized these laboratory sessions in the village. 526 households participated in these sessions. We did not sample substitute households for the experimental sessions to stay consistent with the household survey sample. In addition, we were not always able to match the household with the experimental session data (11 such cases in control, 14 in treated areas). The likelihood of missing households from the experimental session is thus very similar in treated and control villages (the difference is a mere 2.3%-points comparing IDPoor treated and untreated individuals), and as such the few missing households should not bias our main findings (see column 2, Table 2). In four matched cases balancing covariates are missing. The final analysis sample for behavioral outcomes consists of 511 households. Some further observations are missing for specific games (14 in the discount rate game, 1 in the public good game).

Our survey coverage was sufficient to reach enough treated households, in part because we oversampled ID Poor households. According to the 2008 poverty census there were 9785 households in LEAP communes. The LEAP program reports having 1291 members, for a maximum coverage rate (assuming one SHG member per household) of about 13%. In our survey, 28% of control communities' households were SHG members and 54% of treated community households were SHG members. Since the treatment was randomized the most plausible explanation for the 26-point difference is the LEAP program. The increase in SHG membership is larger in our survey sample than in LEAP program documentation due to our intentional oversampling of ID poor households.

The program clearly failed to target poor people exclusively. 47% of non-poor people were members of SHGs in treated areas, compared to 26% in control areas, for a difference of 21 points. That non-poor benefitted from the program is also apparent in the regression estimates in section 5: although point estimates generally indicate that non-poor benefitted less than the poor these estimates are rarely statistically significant, suggesting that poor and non-poor participated in and benefitted from the program indistinguishably in our relatively small sample.

4.3 Balance, descriptive statistics and representativeness

To demonstrate the validity of our identification strategy, we present randomization checks in Table 3. To that end, we use our survey data and complement that with the country's 2008 village census of the area (part of the national census and provided to us by the National Institute of Statistics of Cambodia, see also Kingdom of Cambodia, 2008). In the case of household and individual comparisons, p-values in Table 3 are wild bootstrapped clustering at the commune level due the small number of 24 clusters. Specifically, we use STATA's *boottest* with 10,000 replications provided by Roodman et al (2019). Due to multiple hypothesis testing concerns, we also report associated q-values controlling for the False Discovery Rate (FDR) by family of indicators as suggested by Anderson (2008).

Overall, treatment status is statistically insignificant at conventional levels for a large array of commune, village and household level variables (see Table 3). Among the aggregate statistics one is worth mentioning in more detail. The number of people and households in a village is well-balanced, which is re-assuring given that we sampled a fixed number (15) of households per village. Control villages have just 9 households more (5.7% difference) on average (treated 178.94 households vs. control 169.22 households). The difference is highly insignificant (p-value=0.74, q-value=0.89).

What are the basic characteristics and living conditions of households in our sample? Thirty-one percent of household heads are female and 52% of heads are literate. 69% of heads have always lived in their current village. Average household size is 4.8. Further, 39% of households can document that they own the land on which their house is built and 74% cultivate inherited land. Across-the-board differences in these household characteristics between treatment and control areas are statistically insignificant.

The lower part of Table 3 shows characteristics of our experimental participants. Most of them are female (as prioritized[13] by the intervention and survey teams), married and have less than three years of education. Subjects in the laboratory sessions average 3.23 family links with the other 14 participants in their session. There is some experimental imbalance in that participants in treated villages have double the amount of family links (Δ2.13, p-value=0.08, q-value=0.23). This imbalance may influence behavior in the lab-in-the field experiments, so we will discuss in the results section what happens to our unconditional findings once we control for the number of family links. Other variables are reasonably well-balanced across treatment and control. In other words, there is no apparent self-selection of participants as a function of treatment.

How representative is our sample? We can compare our sample of villages to the potential target villages identified by the government in Siem Reap Province. Table A1 shows that our sampled (and pilot intervention) villages are very similar in terms of population

[13] Men were however not excluded.

characteristics and education levels as well as female empowerment (proportion of female household heads and literacy) to the larger set of villages. The same table also gives available national indicators from the 2008 Cambodian census. Of note is that our intervention area is lagging behind national averages when it comes to literacy rates (Kingdom of Cambodia, 2008). We find that average household size and the proportion of female headed households is similar. Overall, these patterns suggest that our small survey and experimental sample is well in line with the characteristics of the target population.

4.4 Social context: clues from control communities

In what follows we describe the social context of our study area and population to help situate our subsequent findings. We present averages in the control group as a pseudo-baseline in the absence of proper baseline data. The primary occupations of household heads are exhibited in Table 4. Rice farming is clearly the predominant main occupation (both in treated and control households), followed by small business ownership, construction work, on-farm wage labor, and fishing. Table 5 provides descriptive statistics of the outcome variables from the household survey used in the impact evaluation. Like in the subsequent analysis, we have grouped them into group memberships, savings, borrowing, production, assets, expenditure, and community action variables. If variable definitions are not self-explanatory, further definitions can be found in Table A2.

28% of control households have at least one member in a SHG while 21% and 20% of households feature at least one member in a rice seed and a women's group, respectively. Thus, the types of groups created and encouraged by LEAP pre-existed in control communes. Other organizations established self-help groups in Siem Reap prior to LEAP's involvement in the area. Conversely, membership in the remaining groups is relatively low: Few control households are members of producer, funeral/death and irrigation associations.

Consider next the savings and borrowing indicators (some primary target outcomes of the LEAP project): it is worth underlining that merely 22% of households in control areas have non-zero savings. The average amount of savings is therefore low (151,360 riel or 37 USD)

and highly skewed. Given the prevalence and importance of non-zero saving in Siem Reap province, we examine the program impact on the likelihood of non-zero savings (which was large) in our subsequent analysis. Additionally, we report various transformations of the relatively "misbehaved" savings variables (winsorizing and inverse hyperbolic sine transformation). One-fourth of control households report being able to borrow from SHGs, 63% have applied for a loan over the last year and 63% obtained a loan in 2012. 70% of respondents have at least once applied for loans in the past.

Moving on to the economic situation of control households: Annual income from livestock sales per capita over the last one year was 164,700 riel (about 40 USD); assets per capita amount to 820,520 riel (about 200 USD); miscellaneous expenditures per capita over the past 12 months were 485,960 riel (about 199 USD), while bought-food consumption per capita over the last seven days prior to enumeration was 16,782 riel (about 4 USD). Finally, community action is an important dimension of any grass-roots intervention. The average control household attended 6.37 community meetings over the last year. Most households report concrete community action: 36% and 60% of control households report having helped repair/build a school and road, respectively and 24% of households claim to have cleaned up public spaces over the same time horizon.

Behavior in the games and network measures are reported in Table 6. Again, it is useful to consider the means in the control group in the absence of baseline data. In the altruism game, control households sent 24% of their endowment to a poor anonymous family in the village (733 out of 3000 riel). This amount is quite close to previous studies: in a meta-analysis Engel (2011) found that the average give rate was about 28%. 65% of subjects contributed in the dichotomous public good game. We could not find a meta-analysis of dichotomous public goods games. In a meta-analysis of linear public goods games subjects contributed about 38% of their endowment (Zellmer 2003). Barrett (2005) on whom this game is based reports that in the many cases and contexts in which he conducted this game, one- to two-third of the participants contribute. Our results are near the very high end of that range.

The trust game tells a different story, however, control group investors in the trust game sent on average 20% (598 out of 3000 riel). In comparison, a meta study by Johnson and Mislin (2011) of more than 200 trust games conducted around the globe indicates that subjects typically sent about 50%. Thus, our subject pool displays lower levels of trust, so, in principle, there is room for LEAP to improve trust. Consistent with the low levels of trust, control subjects returned only about 15% of the total amount available compared to the common one-third in the rest of the world as reported by Johnson and Mislin (2011). 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. Our trust game findings are similar to those from five Cambodia villages reported by Weingart and Kirk (2012).

Finally, consider the number of network links to other members in the experimental session. Most relevant, the links to other self-help and savings group members averaged well below one in control communities (out of a possible maximum number of 14 links). Participants did know about 2-3 other participants from exchange of hands or economic trades, respectively. Links via funeral group networks are rare and skewed in control units (for that reason we work with a binarized network variable capturing at least one or more link). Just 2% have at least one such link. The same is true for producer groups links, which we also binarize. Here the mean in the control group is zero links. Among further social networks, links via social groups and volunteering (>2 links each) stand out.

4.5 Empirical model

We will present simple regression-based differences-in-means between treated and control villages, differentiating by the poverty status of households. Recall, LEAP targeted households that were officially classified as poor beforehand (IDPoor 1 and IDPoor 2). Our data features four groups of households: poor ones living in a treated area, non-poor ones living in a treated area, poor ones living in a control area, non-poor ones living in a control area. In each village we sampled 10 poor and 5 non-poor households. Our model is

specified to capture all this: a point estimate associated with the LEAP indicator is the effect on the poor compared to the poor in control areas, our main comparison of interest. An interaction term denoted *LEAP x non-poor household* indicates the additional treatment effect (negative or positive) on the non-poor. And a *Non-poor household* indicator by itself shows differences between poor and non-poor households in control areas.

Throughout the analysis, all standard errors are clustered at the commune or treatment level (N=24). Due to the small number of clusters, we only provide wild bootstrapped p-values with 10,000 replications in all tables (see Cameron et al 2008, MacKinnon and Webb 2017a; we use the *boottest* command in STATA by Roodman et al 2019). One caveat to acknowledge is that we have fewer treated than control communes (clusters). And treated clusters feature more observations, as the same number of households are included in treatment and control overall. This may lead to wild bootstrapped p-values that are too low and leading us in turn to "overreject" (see MacKinnon and Webb, 2018, 2017b).
Further, we report q-values controlling for the False Discovery Rate (FDR) for each table's row of estimates as proposed by Anderson (2008). Finally, each table also shows average, standardized effects following Kling et al (2004) and Clingingsmith et al (2009). To that end, effects across indicators are estimated jointly in a seemingly unrelated regression framework allowing for cross-error correlation. Effects are then standardized (in the case of *LEAP* by the standard deviation of outcomes among the IDPoor in the control villages; in the case of *LEAP x Non-poor household* by the standard deviation of outcomes among the Non-Poor in the control villages) and the average effect is calculated. Each standardized effect comes with a wild bootstrapped p-value. Looking at the average effect reduces the danger of singling out significant results that arise by chance and provides an overall effect for each group of indicators. One way to gauge the distributional magnitude of the average effect is to apply a Cohen(1988)'s D interpretation where (absolute) effects equate to 0.2=Small, 0.5=Medium and 0.8=Large. Do note that these cut-offs are arbitrary and economic magnitudes may differ across fields and types of interventions; still it is a useful benchmark

5. Results

In what follows, we report effects by groups of indicators. The main effect of interest is the one associated with the *LEAP* treatment dummy, which shows the program's effect on poor households. We streamline our discussion in the following way. First, we always discuss the respective average effect (presented in the last column of each table). We use Cohen's D cut-offs to evaluate if average effect sizes are sizable or not. Throughout we clearly mention if we are under-powered for a group of indicators or not based on that measure. Second, we discuss the individual effects along with p- and q-values. For individual effects we relate to the poor household's means in the control group to judge economic magnitudes and mention possible power issues. Third, we comment on interaction effects of LEAP with the Non-poor household indicator throughout.

5.1 Self-help group (SHG) membership

Before discussing the socio-economic impacts of LEAP, it is vital to examine whether LEAP caused the desired increase in SHG membership. Other organizations besides LEAP were fostering self-help groups in Siem Reap, so we need to establish that LEAP caused an increase in self-help group membership over and above what other programs would have accomplished. Table 7's last column reports a large (Cohen's D>0.8) and significant average effect of 1.112 (p-value=0.002) across two SHG indicators (self-help group membership and number of links to the other experimental participants via a self-help group, max. 14 links). The average interaction effect is close to a medium Cohen's D and negative, indicating reduced impacts on the non-poor (who were not targeted by the program), but we lack power to precisely estimate the differential effect (p-value=0.323). Moving to the individual effects, column 1 suggests a 28.8%-point increase in the probability that a poor household residing in a treatment area features at least one member in a SHG. This represents a doubling of SHG membership over the 28.7% in the control group (poor households in control villages). The effect is highly significant as judged by the wild bootstrapped p-value (0.003) and associated q-value (0.006). The interaction term with the non-poor indicator is negative and moderate in size (-7.4%-points), albeit imprecisely estimated (p-value/q-value=0.592), qualitatively suggesting that non-poor households have been less able to increase SHG membership as would be expected from a program that targeted the poor. For illustration, LEAP's effect on the non-poor is thus

0.288-0.074=0.214 (p-value=0.090, unreported in table). Column 2 indicates that experimental participants residing in treated areas report on average 0.849 more links to co-members in SHGs in the network survey, which constitutes a massive seven-fold increase relative to the 0.127 average links reported in the control group. The interaction term is again negative, as would be expected from a program that targeted the poor, but it is highly insignificant (-0.294, p-value=0.402, q-value=0.592).

In sum, since LEAP has successfully expanded SHG membership and networks in treated areas it is not unreasonable to expect the broader socio-economic impacts that we explore in the following sections.

5.2 Savings and borrowing

The effects of the program on savings and borrowing ability – an important outcome for the project - are reported in Table 8. The average effect of LEAP on the poor in the final column points to a medium-sized Cohen's D across all six savings outcomes (0.544, p-value=0.097). The average effect among the non-poor is small and almost half the size, the interaction term is -0.234, but this heterogeneity is just about insignificant (p-value=0.129). The individual regressions paint a similar picture: Column 1 reports the effect of the program on the probability of non-zero savings, an important indicator in the LEAP program area where many households have no savings at all. LEAP increased the likelihood of having some savings by 28.2%-points with both p- and q-values well below the 5% threshold of significance. In comparison, a mere 19.9% of households in control areas have savings, so this constitutes a more than doubling of households with some savings. As expected and given the program's pro-poor intentions, the impact on non-poor households is smaller: non-poor households in treated areas are 10.5%-points more likely to have non-zero savings than those residing in control areas (0.282-0.177), but this difference is only suggestive since it is imprecisely estimated (p-value=0.17, q-value=0.427). The impact on actual savings is also positive and significant in column 2: 1.2 higher than the control mean and amounting to 30,037 riel or 7.5 USD at the time (p-value/q-value=0.082). Column 3 reports similar patterns after "winsorizing" savings to account for outliers (0.1;0.9 percentiles), suggesting a 1.8 increase over the control mean

(p-value=0.006, q-value=0.011). In both cases (columns 2 and 3), the interaction effect between the Non-poor indicator and LEAP is negative and sizeable relative to the main treatment effect, however highly insignificant. Column 4 shows impacts on per capita savings of 8,167 Cambodian riels (p-value=0.004, q-value=0.011), a meaningful increase relative to the control-village mean (5,347 riels). Column 5 employs a "log-like" inverse hyperbolic sine transformation suitable in the presence of zeroes and outliers. Point estimates can be interpreted like a log-specification. Similar patterns emerge – sizeable effects on the poor's savings and smaller effects on the non-poor's savings (though the interaction term is not precisely estimated). To put above patterns in a broader economic perspective it is useful to consider column 6 where we scale total savings by total annual household production. LEAP increased this proportion by 4.1 points over the 2.2 points observed in the control group (p-value=0.019, q-value=0.023).

Finally, column 7 looks at borrowing ability. Poor respondents in treated areas are 23.2%-points more likely to report that they have the ability to borrow from an SHG (p-value=0.009, q-value=0.013). That said, there were no other appreciable effects on actual borrowing of the program and so we choose not to report them for brevity's sake. There was a small and insignificant 5.2%-point (p-value=0.124) increase in the probability of reporting being able to borrow from a bank and no significant difference in respondents reporting actually taking advantage of their self-reported increase in borrowing ability (all unreported). In sum, we infer that the program enjoyed clear success when it comes to increasing savings but some muted success in targeting the poor.

5.3 Livelihood and household expenditure outcomes

Tables 9 through 11 report the estimated impact of LEAP on household production, assets and expenditures. The last summative column of Table 9 points to a positive, but small and insignificant average effect on production-related outcomes (0.102, p-value=0.182). The average interaction effect is close to zero and also insignificant (0.020, p-value=0.850). These results imply no large net program impacts on production.

When considering individual indicators, livestock production in the form of meat and fish, as well as associated income increased substantially and statistically significantly among the poor in treated villages (columns 1 and 2). Both p- and q-values are well below the 5% threshold of significance. Magnitudes are economically important. Production increased by 93%, which relative to the control group, amounts to 173,836 riels per capita (about 43 USD). There is a substantial and corresponding increase in sales income as well. These increases are consistent with our informal interviews with LEAP participants and program personnel who stated that participants frequently used LEAP funds to buy and raise chickens. They are are also consistent with the claims of the program that 45 of the 52 producers' groups created by LEAP SHGs involved raising livestock (38 chicken-raising and 7 pig-raising groups). However as we report in Tables 12 and 14 below we could find no significant participation in producer groups in our sample, raising questions about how active these producer groups actually were.

These increases plausibly induced by LEAP were offset by a suggestively significant (significant p-values, but insignificant q-values) decreases in the annual income from crop sales per capita (column 3), and imprecisely estimated decreases in other annual revenue-generating activities per capita (see Appendix Table A2 for items included in these categories). Column 5 looks at the aggregate effect of LEAP on total production and similar to the last column displaying the average effect this exercise points to no production-enhancing effect of LEAP. Finally note that by and large the interaction effects (LEAP x Non-poor household) do not allow us to document any significant nor sizeable heterogeneity.

Table 10 presents estimates of LEAP's impacts on asset stocks. They indicate that LEAP villages did not hold statistically larger stocks of assets than control villages. The average effect is tiny and insignificant (0.026, p-value=0.773); there are no power issues. Individual point estimates are also small and insignificant. While Table 9 showed that LEAP villages experienced greater livestock production, this greater production did not lead to significantly greater holdings of livestock. A few remarks are in order: First this could be because LEAP livestock growers raised chickens and pigs which reach maturity and are

sold for slaughter in eight to twelve weeks and six months, respectively, an interpretation that is consistent with the statistically significant increase in income from livestock. The null effect on livestock holdings could be due to the difference between a stock and a flow. Second, the effect on the livestock acquired (i.e. livestock flows) is expressed in individual animals (e.g. chicks or piglets for raising). The histogram in Figure A1 suggests differences between treated and control communities in this count variable. Treated communities had slightly fewer households with no livestock acquisitions and generally slightly more households in treated communities acquired livestock at each level with one major exception: control communities had two households that were extreme outliers, acquiring more than thirty animals. When those two outliers are excluded the point estimate on the treatment effect doubles to 0.32 but it is still statistically insignificant at conventional levels. In total these results point to slightly greater acquisition of livestock in treated communities but those few households that did switch into livestock production earned more production income for doing so. Overall, our data may not be detailed enough to pinpoint these relative dynamics and apparent heterogeneity.

Increases in savings should require reduced expenditures in the short run. The second column of Table 11 indicates suggestively that households in LEAP communities reduced miscellaneous expenditures, but the impact on expenditures was in general not statistically significant and the standardized mean effect in the last column is substantively very small, statistically insignificant (-0.017, p-value=0.880. While some reductions in expenditures must have been necessary for SHG members to increase savings, LEAP does not appear to have required large impacts on expenditures overall. Finally, note that the average effect associated with the interaction term of treatment with the non-poor indicator is negative, insignificant and small (-0.107, p-value=0.505).

5.4 Social capital outcomes

We now turn to the effects of the program on social capital as measured by economic networks (Table 12), social networks (Table 13) and group memberships other than SHGs (Table 14). One thing to note here is that we did not record when links were formed or groups were joined, so the following results provide a temporal snapshot only. Consider

Table 12 where we present the impacts on six types of economic relationships: funeral group,[14] "exchanging hands,"[15] buying or selling, as well as employee, co-worker and producer group ones. The program had no statistically significant average effect on these economic networks (We did not include producer group links in this average effect calculation due to zero links and thus a zero standard deviation in the control.). The effect of 0.360 standard deviations falls in to the small-medium range according to Cohen's D with a p-value of 0.16. So, we may be lacking power here. The average interaction effect of LEAP and poverty status is very small and insignificant (-0.047, p-value=0.501). These patterns warrant a closer look at the individual estimate sets. Impact estimates are positive and large relative to the mean in the poor control group when it comes to the funeral group and exchange of hands-related network links. For instance, the impact on the likelihood of reporting at least one funeral group link is 17.7%-points compared to an average prevalence of 2.3% in control areas (column 1). However, this relatively large difference may be misleading given the low number of such links in the first place and their concentration in few villages: 17 (14) of the 18 control (treated) villages have *no* funeral group links at all. Column 2 shows that LEAP close to doubles the number of network links due to the exchange of hands but we suffer from power problems (p-value=0.153, q-value=0.765). The effect on buying and selling links is positive, modest relative to the control mean (12%) and highly insignificant (p-value=0.772, q-value=0.924). The next two columns indicate negative but very small and highly insignificant effects (-4.2%;-7.9%) on work-related network links. The effect on producer networks in column 6 is positive as expected (4.8%-points) but insignificant. This is disappointing given the purported creation of producer groups as indicated by the official LEAP report. In our data the producer network was very sparse and not well distributed. In control communes nobody reported producer group

[14] These are risk-sharing groups in which members agree to cover the cost for funerals of other members. This is a relatively rare link in our sample so we use a binarized version of that takes on one for having at least one link to another experimental session participant and zero otherwise.

[15] This 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.

links.[16] In LEAP communes 0.348 links were reported on average (with a median of zero). That said only 4.8% of the sample or 13 subjects report at least one link. All of these non-zero links are due in just two villages. These findings raise questions about how real and certainly how active the LEAP-created producer groups were. The overall conclusion from Table 12 is that LEAP had no broad nor robust effects on economic networks other than those directly linked to SHGs (recall column 2, Table 7). However, we cannot statistically rule out small to medium average effects and meaningful effects on some sub-indicators (in column 1 and 2).

Table 13 reports effects on the number of links to others via social activities and associations: Worship, Babysitting, Advice-seeking, Social Groups, Volunteering, Borrowing (non-monetary). We document a positive but insignificant average impact that is just above the "small" cutoff based on Cohen's D (0.256 standard deviations, p-value=0.205). The average interaction effect associated with the non-poor status is merely suggestive of a reduced positive effect among the non-poor (-0.091, p-value=0.205). Across the board, the individual effects of LEAP are positive and sizable and unfortunately imprecisely estimated. We acknowledge power issues.

Table 14 displays LEAP's effects on group membership (other than SHG and savings groups). These data, from the household survey, are self-reported and thus different to the aforementioned network measures. The average effect points to some modest overall increases in such memberships induced by LEAP (0.210, p-value=0.066). The effect on the poor and non-poor is qualitatively similar as indicated by a small interaction term (0.028, p-value=0.854). Interesting patterns emerge when dissecting this average effect. Self-reported membership in producers' groups, shown in column 1, was statistically no different in program and control communities, which is disappointing given the program's putative creation of 52 producer groups. The small absolute coefficient on the treatment dummy (0.034) does represent a doubling of producer group membership compared to the

[16] As the mean in the control group was zero, we did not include the producer network effect in the average effect calculation in the final column of Table 2; one cannot divide the effect by a standard deviation of zero.

low control group mean (0.039), but it is not statistically significant (p-value=0.238, q-value=0.494) and one would expect a larger effect given the program's claim that almost half the SHGs created livestock groups.

LEAP more than doubled membership in rice seed groups (p-value=0.001, q-value=0.006), which are common across villages. Rice seed groups were *not* created by the LEAP program so LEAP appears to have caused follow-on increases in membership in new or existing ones. To examine the broader relationship between rice seed and SHG groups, we regressed rice seed membership on a SHG dummy controlling for poverty status and village fixed effects (absorbing the LEAP treatment). SHG membership is correlated with a 71%-point increase in rice seed membership (p-value=0.000) even controlling for LEAP treatment, pointing to complementarity of these groups. This suggests that Cambodian villagers that save via SHGs also are more likely to "save" in rice-seed banks. Thus, LEAP suggestively correlates with increased membership in rice-seed groups because it increases the numbers of SHGs. We did not expect this result and so, unfortunately, we cannot comment on it further with any confidence. Perhaps in rural Cambodia, where savings institutions are very weak, these are the only two common savings institutions available to villagers. If so, it could be that those who have a taste are prone to save, make use of both of them.

Further, the effect on funeral group membership was negative, which is at odds with the positive but insignificant effect we found on the likelihood knowing somebody from a funeral group. The correlation between funeral group membership and the number of links to funeral group members is negative and small both in treated (-0.072) and control (-0.048) villages. Funeral group membership is quite rare and skewed (11.9% in control and 5.6% in treated villages), which may explain these results. Column 4 and 5 point to no significant and sizeable absolute effects on youth and irrigation groups, which are not very present in the control group.

Column 6 suggests a 63% increase in women's group participation. However, the effect is insignificant (p-value=0.421, q-value=0.494) and the test is underpowered. SHGs were

newly created and while men were not excluded by the program, the local context and nature of the program led to mainly female members. This is reflected in who participated in our behavioral activities as well. The overwhelming majority was female. In control villages the correlation between women's groups and SHG is -0.015 and in treated villages the correlation is 0.04. In other words, there is a weak correlational link.

We were not only interested in testing for LEAP's effects on networks and group formation, but also on self-reported pro-social actions. Table 15 tells us that LEAP had an insignificant and less than small effect on a host of community action indicators (see last column, average effect=0.143, p-value=0.337). The effect is (suggestively) reduced among the non-poor (average effect=-0.076, p-value=0.355). It is worth noting that households in program areas attended 1.511 more meetings, a 24% increase over the average number of attended meetings of 6.221 by poor control households. The point estimate is significant at the 10% level when taking its p-value, but the positive finding does not pass adjustment for multiple hypothesis testing (q-value=0.352); low power may be an issue. The remaining columns (2-4) cannot document significant effects of LEAP on helping to build or rebuild a school or road nor cleaning up a public space in the last 6 months, so we conclude that LEAP had no wider and economically important effect on community actions.

Table 16 reports the impacts of the program on laboratory measures of social norms. The final column reports the average effect across measures: 0.12 standard deviations and insignificant (p-value=0.242). While positive as hoped for by LEAP, the average effect is very modest (well below Cohen's D of 0.2 for small effect size). The average effect is suggestively smaller among the poor but the interaction is highly insignificant (by -0.048, p-value=0.719).

Turning to the results on individual indicators, column 1 presents effects on altruism, measured by the amount donated by the subject to the needy family. LEAP increased donations among the treated poor by 99.9 riels. In comparison, poor subjects from control communities donated about 685 riels, a little less than one-fourth of the endowment. The treatment effect amounts to a 15% increase. While the effect is individually significant at

the 10% level (p-value=0.089), it does not survive adjusting for multiple hypothesis testing (q-value=0.356). Interestingly, the interaction effect associated with the non-poor is the opposite sign and the same magnitude but imprecisely estimated. Column 2 presents linear probability estimates of the effect of LEAP on contributions in the dichotomous public good activity. Around 64% of subjects contributed to the public good in poor control households. There is no significant difference between subjects from treated and control communities, either in the full sample or in sub-categories of poverty. Contributions in the LEAP villages were 4.3%-points (or 7%) higher than in control villages, but this modest increment was not statistically significant. The interaction term with the poverty indicator is tiny and highly significant (-0.005, p-value=0.949).

Column 3 shows the effect of the program on trust—the amount sent by the "investor" in the activity. Column 4 shows the related impact of the program on trustworthiness—the amount returned by the trustee as a percentage of the total amount available to the trustee. Recall, the total amount available to the trustee is their initial endowment of 3,000 riels plus triple the amount the investor gave them. The number of investors and trustees is unequal because, when there was an odd number of subjects in the lab session, we randomly matched two trustees to one investor. In those cases, the trustees received the payoff consistent with their actions and the relevant investors received the payoff decided by the first trustee with whom they were randomly paired. Focusing first on column 3, poor subjects did send more in the treated villages than those residing in control villages, but the difference is not statistically significant (116.6 riels, p-value=0.297, q-value=0.594). There may be a problem with a low-powered test since this equates to a meaningful 24% mean increase. Similar to behavior in the dictator game, we find that the treated non-poor's behavior suggestively offsets the positive treatment effect (-135.112, p-value=0.393). Turning to the effects of the program on trustworthiness in column 4, the program did not produce significant increases in trustworthy behavior. In this case there is little concern over a low-powered test, because the estimated treatment effect is tiny (-0.1%-points relative to a control mean of 15.4%, p-value/q-value=0.96). Likewise, the interaction effect is small and insignificant (2.4%-points, p-value=0.497, q-value=0.663).

5.5 Social capital outcomes - Robustness and mechanisms

We now present a series of robustness and/or mechanisms checks relating to the behavioral patterns:

First, LEAP did not impact risk and time preferences (see Table A3 in the online appendix). The average effect (0.070, p-value=0.409) and both individual effects are very small relative to the control mean (4.6%, 2.9%) in magnitude and highly insignificant, so we can exclude the influence of these deeper mechanisms/cofounders.

Second, while we pass most randomization balancing tests, the number of family links is imbalanced (see Table 3). This may have influenced laboratory behavior. If treated participants were more familiar with other session members, this may have spuriously increased pro-social behavior. Table A4 controls for the total number of family links in the game session. On the one hand note that the number of family links is positively correlated with pro-social behavior, however the relationship tends to be statistically weak. We can only report a significant and positive correlation between trustworthiness and family links (see column 4). On the other hand, we find that controlling for family links tends to lower the positive coefficients associated with the LEAP treatment indicator, which is reflected in a lower average effect (falling from 0.120 to 0.075, compare last columns of Tables 17 and A4). This is suggestive of a spurious correlation of LEAP and pro-social behavior operating via family links. Notably, when it comes to altruism, the coefficient falls by 11%, but is slightly more precisely estimated. Overall, the effect of LEAP on altruism remains relatively modest relative to the mean (13%). In sum, controlling for family links has some but no substantive implication for the interpretation of our results.

Third, there may be a concern that winnings earlier in the experimental sessions are correlated with subsequent behavior. In Table A5 we regress pro-social behavior in the control group (to abstract from treatment effects) on prior winnings in the lottery activity. There is no evidence of a significant link.

Fourth, LEAP had little effect on economic and social networks (other than SHGs), which may explain the small and insignificant positive effects on pro-social behavior. That said, LEAP has changed select economic variables such as savings, which may subsequently boost pro-social behavior. One theoretical explanation may relate to social preferences that feature inequity aversion (both with respect to own and other's inequitable economic outcomes, see Fehr and Schmidt, 1999). In particular, the willingness to help a poor and unknown family in the community may be influenced by one's own economic situation. This economic mechanism could explain the positive albeit small and imprecise effects we saw in Table 16.[17] To examine this further, we regressed pro-social behavior (in the dictator, trust and public good games) on total annual production and savings. In that regression, we also include the number of network links to SHG members in line with social-capital-creating mechanism modeled by Avdeenko and Gilligan (2015). We perform this exploratory and correlational analysis in the sample of control households to avoid treatment-related endogeneity concerns. Table A6 documents a positive and significant correlation between altruism and household production, as well as a positive and significant correlation between savings and the likelihood of public good contribution. Relatedly, savings were induced via group activities, which could link to the public good game behavior or the willingness to contribute to a group/community. Individual economic outcomes in turn may influence altruism. While the boost to savings was significant, the very small and highly insignificant increase in total household production due to LEAP may have been too small to bring about larger and noticeable pro-social behavioral change. The same table also points to no significant and if at all negative correlation between the number of SHG links and pro-social behavior. One caveat here is that of course the nature of such SHG links may differ between control and treated areas. In sum, these "control group" patterns are suggestively in line with a plausible theory of change running from improved economic conditions to pro-social behavior.

[17] We are grateful to an anonymous referee for suggesting this exploratory investigation ex-post.

6. Conclusion

We reported the results of a randomized field trial of the Livelihood Enhancement and Associations among the Poor (LEAP) program, which was designed to improve livelihoods, encourage savings and civic participation and create social capital among the poorest residents of Siem Reap province by organizing self-help groups. The program enjoyed partial successes in its goals, in particular, substantially increasing the number of households that had some savings. The program also dramatically increased the poor's membership in self-help groups and led to shifts in the poor's livelihoods towards meat production and income derived from it. In net, livelihoods were not enhanced significantly however. Average effects on civic engagement and social capital were positive but small and insignificant. We acknowledge low-powered tests here.

These promising results emerged from a small sample of only 540 households after only eighteen months of a pilot program rather than the three-year intervention that was originally planned. With respect to the small sample our aim in this paper was to transparently acknowledge under-powered estimates whenever pertinent. In the same vein, we also acknowledge the commonly ignored fact that significant results in a small sample may be overestimates (Button et al 2013). Our design also suffered from some flaws (few and imbalanced treated units), that may lead to p-values that are too small (MacKinnon and Webb, 2017a, 2017b, 2018). Yet given the still limited evidence stemming from RCTs in this area, and our comprehensive measurement strategy, we are confident that our study adds new insights to the literature.

This study also points to avenue for future research. First the program we study was short and its implementation was interrupted due to the moratorium on World Bank funding to Cambodia. Given the promising findings in this study even under those adverse conditions, it would be worthwhile to evaluate the program as it was meant to be implemented. The large impact we find on the number of people who had some savings and the significant but perhaps smaller impacts on absolute amounts of savings could indicate that savings may have been higher while the program was running at full force (before the World Bank funding hiatus). Given more time, the increased association among the poor in SHGs may

produce even stronger community-wide improvements. A second avenue of study concerns the intensity of the social links that the program created. We show that SHG membership and related networks links have increased. Unfortunately, due to our respondents' time and recall constraints we could not collect data on how active or intense this participation in the groups was during and after the end of the program. Doing so may be an interesting topic for future research.

In summary, while further study is always prudent before drawing firm conclusions, the results of this evaluation relating to SHG group creation and savings are sufficiently encouraging to recommend that LEAP-like programs be further instituted and investigated.

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6. Figures

Figure 1: Timeline

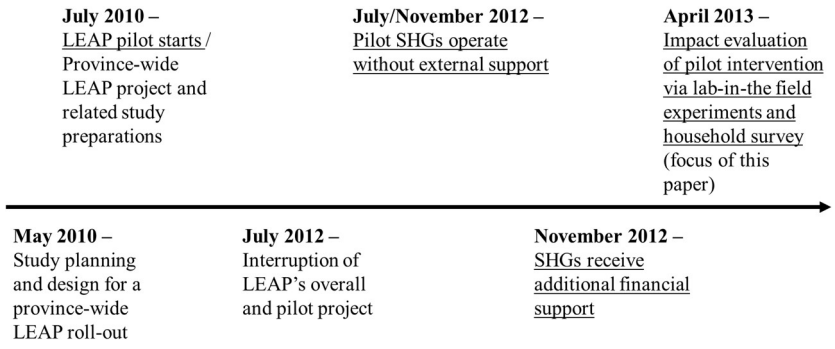
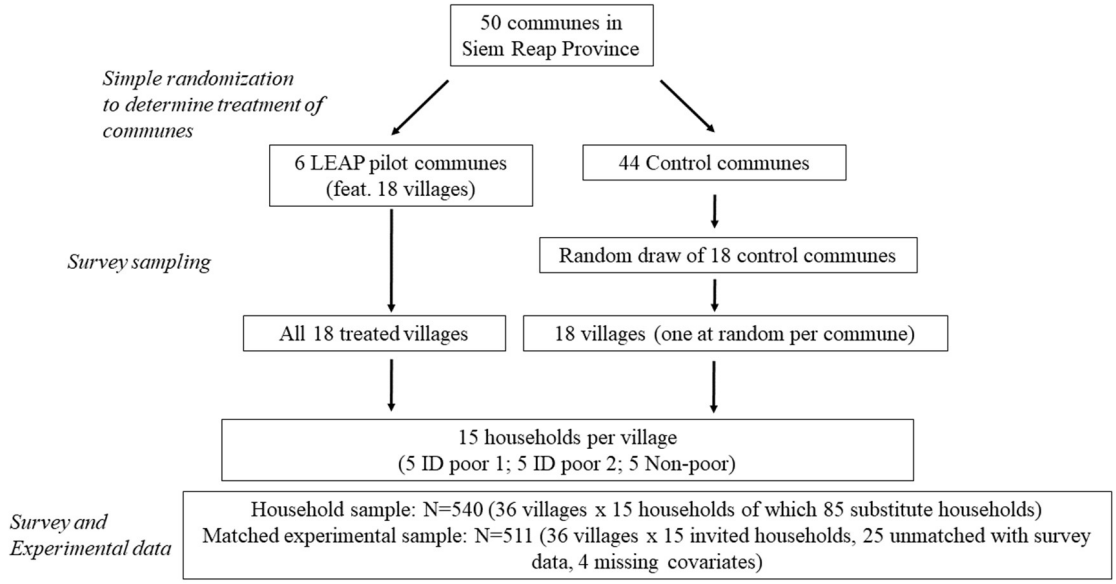


Figure 2: Randomization and Sampling Strategy



7. Tables

Table 1: Summary of the Literature

	Identification Strategy	Comparison Level	Savings	Livelihoods	Civic Engagement	Social Capital	
						Norms	Networks
Self-help group programs							
Khana et.al. 2015	PSM	Village	[i]	+	+		
Datta 2014	PSM	Village	+	0	+		
Desai & Joshi 2013	RCT	Village	0	+	+		
Deininger & Liu 2013a	Pipeline DD PSM	Village			+	+	
Swain and Vargese 2009	Pipeline	Group	+	+	+		
Kim et al 2009[ii]	RCT	Village	+	+		+	+
Pronyk et al 2006[ii]	RCT	Village	+		+		
This study	RCT	Village	+	[iii]	[iv]	0	[v]
Savings group programs							
Karlan et.al. 2017	RCT	Village	+	+	0		
Ksoll et.al. 2016	RCT	Village	+	+			
Beaman et.al. 2014	RCT	Village	+	0	0	0	0
Annan et.al. 2013	RCT[vi]	Group	+	[vii]		0	
Kast et. al 2012	RCT	Group	+				
Micro-credit group programs							
Banerjee et. al. 2018	RCT	Village					-
Angelucci 2015	RCT	Clusters[viii]		0		0	
Attanasio 2015	RCT	Village		0			
Augsburg 2015	RCT	Village	0	0			
Banerjea et al 2015	RCT	Neighborhood		0	-[ix]		
Crepon et al 2015	RCT	Village	+	+			
Tarozzi et al 2015	RCT	village	0	0			
Feigenbaum et.al. 2014	RCT[x]	Group					+
Feigenbaum et.al. 2012	RCT[x]	Group				+	+
Karlan and Zinman 2011	RCT	Individual	0	-		0	+
Pitt et al 2006	IV	Individual			+		

[i] These studies report statistically significant accumulation of non-financial assets but do not report impacts on savings.
[ii] Kim et al and Pronyk et al are distinct but very similar studies of the same program in Limpopo, South Africa. It was a micro-credit program that included self-help training.
[iii] Large and significant livelihood switching toward meat and fish production but only a small and insignificant average-effect increase in overall production
[iv] Borderline significant average-effect increase in group participation drive entirely by large and significant increase in rice seed group membership
[v] Large and significant increase in SHG networks; no significant increase in other social or economic networks.
[vi] A randomly-chosen half of the groups began the program immediately and the other half a year later.
[vii] They did not examine livelihoods but determined that the program did cause a significant reduction of households below the poverty line.
[viii] Clusters were neighborhoods in urban settings and villages or groups of villages in rural settings.
[ix] The program caused a reduction in household spending on community festivals, indicating some reduction in civic engagement. The study did not address other civic engagement.
[x] Some randomly selected groups met weekly and the remainder met monthly
[xi] Their sole measure of livelihoods was size of subjects' business enterprises, which they found shrunk.
[xii] They did not compute average effects. Treated individuals exhibited significantly more self-reported trust in their neighbors but were no more trusting according to three other measures. We suspect average effects were zero.
[xiii] They only asked about friends and family networks. Respondents in treated communities were more confident in the strength of those network ties: they reported significantly more confidence in relying on friends and family for large amounts of financial assistance in an emergency

Table 2: Missingness

	(1) Substitute survey household	(2) Missing household from experimental session (but in survey)	(3) Avg. effect
Mean poor control households	0.160	0.039	
LEAP	0.030	0.023	0.099
Wild boot. p-value	*0.471*	*0.471*	*0.321*
Q-value	*0.471*	*0.471*	
LEAP x Non-poor household	-0.077	-0.035	-0.192
Wild boot. p-value	*0.352*	*0.478*	*0.306*
Q-value	*0.478*	*0.478*	
Non-poor household	-0.014	0.006	
Wild boot. p-value	*0.823*	*0.765*	
Q-value	*0.823*	*0.823*	
N	540	540	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table 3: Balancing Tests

				Mean		Diff.	
				Control	LEAP	P-value	Q-value
	N	Mean	SD				
Commune characteristics							
Nr. of households (in 100s)	24	13.23	6.17	12.21	16.31	0.16	0.33
Fraction of poor households (ID 1 & 2)	24	0.30	0.09	0.31	0.28	0.49	0.49
Village characteristics							
Total population (in 100s)	36	8.33	4.03	8.54	8.45	0.76	0.89
Nr. of households	36	174.08	84.63	178.94	169.22	0.74	0.89
Fraction of households female headed	36	0.27	0.16	0.27	0.29	0.81	0.89
Male literacy rate (>15 years)	36	0.74	0.16	0.73	0.77	0.66	0.89
Female literacy rate (>15 years)	36	0.58	0.20	0.58	0.65	0.89	0.89
Dependency ratio	36	0.66	0.12	0.68	0.66	0.44	0.89
Household characteristics							
Female headed household (binary)	540	0.31		0.30	0.31	0.83	0.89
Age of household head	540	46.38	13.71	46.26	46.50	0.89	0.89
Literate household head (binary)	540	0.52		0.51	0.53	0.75	0.89
Highest completed school primary 6+ (binary)	540	0.13		0.14	0.12	0.49	0.89
Head has always lived in the village	540	0.69		0.68	0.71	0.60	0.89
Head is married (binary)	540	0.70		0.73	0.67	0.21	0.89
Household size	540	4.80	1.92	4.86	4.74	0.48	0.89
Average age of household members	540	28.88	12.61	28.73	29.03	0.84	0.89
Owns land on which house is built (with documents, binary)	540	0.39		0.33	0.44	0.13	0.89
Cultivates inherited land (binary)	540	0.74		0.72	0.76	0.64	0.89
Experimental participant characteristics (matched with household survey)							
Male (binary)	511	0.16		0.16	0.16	0.98	0.98
Age	511	42.26	14.30	42.33	42.18	0.93	0.98
Education (in years)	511	2.65	2.98	2.75	2.56	0.68	0.98
Married (binary)	511	0.72		0.73	0.71	0.76	0.98
Additional balancing tests							
Nr. of links to the other experimental participants (max. 14)							
Family	511	3.23	3.64	2.17	4.30	0.08	0.23
Neighbor	511	1.66	1.90	1.52	1.80	0.67	0.76
Krum (administrative unit, below Village)	511	1.27	1.91	1.19	1.35	0.76	0.76

Note: Standard p-values for comparisons of commune characteristics. All other comparisons, wildbootstrap clustered p-values at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by groups of indicators (commune, village, household survey, experiment, additional) using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table 4: Primary Occupations

Primary occupation of household head	Control	Treatment	Total
Rice farming	87	82	169
Small business	38	25	63
Construction work	29	28	57
On-farm wage labor	24	18	42
Fishing	13	19	32
Off-farm wage labor	14	16	30
Salary work	7	19	26
Dependence	11	10	21
Livestock raising	12	7	19
Not working person	10	7	17
Handicraft	4	12	16
Vegetable farming	5	10	15
Chamkar	5	4	9
Other categories	11	13	24
Total	270	270	540

Table 5: Survey Outcomes

			Mean		Diff.	
	Mean	SD	Control	LEAP	P-value	Q-value
Group memberships						
Household is member in the following community groups (all binary):						
Self-help-group (SHG)	0.41		0.28	0.54	0.00	0.00
Producer association	0.04		0.03	0.06	0.22	0.26
Rice seed association	0.34		0.21	0.46	0.00	0.00
Death association	0.09		0.12	0.06	0.23	0.26
Youth association	0.05		0.03	0.06	0.12	0.22
Irrigation association	0.04		0.02	0.05	0.09	0.21
Women association	0.26		0.20	0.32	0.40	0.40
Current savings (with MFIs, SHGs, lenders, friends/relatives, other)						
Household currently has non zero savings (binary)	0.33		0.22	0.44	0.00	0.00
Total current household savings (in 1000s riel)	141.96	1200.14	151.36	132.56	0.86	0.86
…. in log (inverse hyperbolic sine transformed)	1.49	2.29	1.01	1.98	0.00	0.00
Borrowing (any household member, all binary)						
Can the household borrow now from:						
Self-help-group (SHG)	0.35		0.25	0.46	0.00	0.02
Friend	0.71		0.70	0.72	0.76	0.76
Bank	0.13		0.10	0.16	0.09	0.27
Applied for a loan at financial institutions/informal/SHG, over the last year	0.62		0.63	0.60	0.62	0.74
Received a loan from financial institutions/informal/SHG, last year (2012)	0.61		0.63	0.59	0.37	0.63
Ever applied for loan at formal financial institutions/informal/SHG	0.68		0.70	0.67	0.42	0.63
Household production (see appendix for precise definitions)						
Annual livestock production per capita, last one year (in 1000s riel)	316.65	660.87	241.09	392.21	0.13	0.14
Annual income from livestock sales per capita, last one year (in 1000s riel)	230.60	594.93	164.70	296.49	0.14	0.14
Annual income from crop sales per capita, 2012 harvest season (in 1000s riel)	115.44	290.34	146.15	84.74	0.13	0.14
Other annual revenue-generating activities per capita, 2012 (in 1000s riel)	1376.35	2587.13	1614.06	1138.65	0.08	0.14
Household assets (see appendix for precise definitions)						
Assets per capita (in 1000s riel)	590.11	2029.63	820.52	359.71	0.00	0.01
Livestock holdings per capita, current (in 1000s riel)	442.10	2007.68	356.67	527.52	0.57	0.72
Livestock acquired (total number), over last one year	1.00	3.04	0.94	1.06	0.72	0.72
Household expenditures (see appendix for precise definitions)						

Non-food expenditures per capita, past 30 days (in 1000s riel)	25.15	31.96	25.61	24.69	0.79	0.79
Misc. expenditures per capita, past 12 months (in 1000s riel)	461.18	665.46	485.96	436.40	0.47	0.71
Bought food consumption per capita, past 7 days (in 100s riel)	157.23	197.50	167.82	146.64	0.44	0.71
Community action						
Number of community meetings attended over the last year	7.28	6.38	6.37	8.18	0.02	0.07
Household member helped, past 6 months (all binary):						
Build or rebuild school	0.36		0.36	0.35	0.96	0.98
Build or repair road	0.60		0.60	0.60	0.98	0.98
Clean up public space in the community	0.26		0.24	0.28	0.62	0.98

Note: N=540. P-values are wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by group (demarcated by horizontal borders) using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].
1 USD ~ 4,000 Cambodian Riel.

Table 6: Experimental Session Outcomes

				Mean		Diff.	
	N	Mean	SD	Control	LEAP	P-value	Q-value
Risk taking [1=low - 5=high]	511	2.24	1.04	2.18	2.30	0.42	0.83
Impatience [1=very patient - 6=very impatient]	497	3.41	2.38	3.38	3.44	0.86	0.86
Altruism (amount sent in dictator game, 0-3000 riel)	511	766.14	569.32	732.56	800.40	0.16	0.65
Public good contribution (yes=1, no=0)	510	0.67		0.65	0.69	0.44	0.71
Trust (amount sent in the trust game, 0-3000 riel)	243	633.74	650.50	597.56	670.83	0.57	0.71
Trustworthiness (fraction returned in the trust game)	266	0.15	0.14	0.15	0.16	0.71	0.71
Nr. of links to the other experimental participants (max. 14):							
SHG/Savings Networks							
Self-help-group (nr. of links)	511	0.48	1.52	0.11	0.85	0.09	0.09
Savings group (nr. of links)	511	0.50	1.66	0.07	0.95	0.05	0.09
Economic Networks							
Funeral group (1=having at least one link, 0 otherwise)	511	0.11		0.02	0.19	0.31	0.92
Exchange hands (for free, nr. of links)	511	3.53	4.49	2.48	4.60	0.15	0.90
Regular buying and selling link (undirected, nr. of links)	511	3.23	3.99	3.06	3.40	0.78	0.93
Employee-relationship (undirected, nr. of links)	511	1.61	2.79	1.64	1.57	0.93	0.93
Coworker (nr. of links)	511	2.53	3.73	2.64	2.42	0.83	0.93
Producer group (1=having at least one link, 0 otherwise)	511	0.02		0.00	0.04	0.46	0.92
Social Networks							
Worhsip group (nr. of links)	511	2.14	4.23	1.46	2.83	0.34	0.85
Babysitting (nr. of links)	511	0.39	0.87	0.39	0.40	0.94	0.94
Seek advice (nr. of links)	511	0.92	1.51	0.75	1.10	0.38	0.85
Social Group (nr. of links)	511	3.18	4.08	2.70	3.67	0.54	0.85
Volunteering (nr. of links)	511	2.17	3.71	2.05	2.30	0.81	0.94
Borrow (non monetary, nr. of links)	511	1.67	2.77	1.36	1.98	0.56	0.85

Note: P-values are wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by group (demarcated by horizontal borders) using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].
1 USD ~ 4,000 Cambodian Riel.

Table 7: SHG Outcomes

	(1)	(2)	(3)
	At least one household member is in a self-help group (binary)	Nr. of links to the other experimental participants in a self-help group (max. 14)	Avg. effect
Mean poor control households	0.287	0.127	
LEAP	0.288	0.849	1.112
Wild boot. p-value	*0.003*	*0.057*	*0.002*
Q-value	*0.006*	*0.057*	
LEAP x Non-poor household	-0.074	-0.294	-0.487
Wild boot. p-value	*0.592*	*0.402*	*0.323*
Q-value	*0.592*	*0.592*	
Non-poor household	-0.029	-0.057	
Wild boot. p-value	*0.69*	*0.583*	
Q-value	*0.69*	*0.69*	
N	540	511	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table 8: Savings

	(1) Household currently has non zero savings (binary)	(2) Total current household savings (in 1000s riel)	(3) Total current household savings (in 1000s riel) - **winsorized (0.1;0.9)**	(4) Total current household savings **per capita** (in 1000s riel)	(5) Inverse hyperbolic sine transf. Total current household savings (in 1000s riel)	(6) Savings (relative to annual total production)	(7) Can house-hold borrow from a SHG?	(8) Avg. effect
Mean poor control households	0.199	24.901	14.901	5.347	0.842	0.022	0.265	
LEAP	0.282	30.037	26.768	8.167	1.246	0.041	0.232	0.544
Wild boot. p-value	*0.004*	*0.082*	*0.006*	*0.004*	*0.005*	*0.019*	*0.009*	*0.097*
Q-value	*0.011*	*0.082*	*0.011*	*0.011*	*0.011*	*0.023*	*0.013*	
LEAP x Non-poor household	-0.177	-153.323	-16.713	-87.799	-0.833	-0.161	-0.083	-0.234
Wild boot. p-value	*0.17*	*0.623*	*0.221*	*0.337*	*0.168*	*0.244*	*0.400*	*0.129*
Q-value	*0.427*	*0.623*	*0.427*	*0.467*	*0.427*	*0.427*	*0.467*	
Non-poor household	0.048	383.628	10.594	125.785	0.501	0.201	-0.04	
Wild boot. p-value	*0.489*	*0.033*	*0.119*	*0.043*	*0.158*	*0.022*	*0.492*	
Q-value	*0.492*	*0.101*	*0.209*	*0.101*	*0.222*	*0.101*	*0.492*	
N	540	540	540	540	540	540	540	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table 9: Production

All dep. variables inverse hyperbolic sine transf.	(1) Annual livestock production per capita, last one year (in 1000s riel)	(2) Annual income from livestock sales per capita, last one year (in 1000s riel)	(3) Annual income from crop sales per capita, 2012 harvest season (in 1000s riel)	(4) Other annual revenue-generating activities per capita, 2012 (in 1000s riel)	(5) Annual total production (in 1000s riel)	(6) Avg. effect
Mean poor control households (levels)	187.121	105.624	113.25	1387.08	1793.074	
LEAP	0.657	1.120	-0.577	-0.097	0.028	0.102
Wild boot. p-value	*0.006*	*0.002*	*0.097*	*0.698*	*0.860*	*0.185*
Q-value	*0.015*	*0.010*	0.162	*0.860*	*0.860*	
LEAP x Non-poor household	0.331	0.111	0.513	-0.511	-0.039	0.020
Wild boot. p-value	*0.382*	*0.828*	*0.530*	*0.291*	*0.861*	*0.850*
Q-value	*0.861*	*0.861*	*0.861*	*0.861*	*0.861*	
Non-poor household	-0.019	0.315	0.797	0.304	0.300	
Wild boot. p-value	*0.941*	*0.392*	*0.024*	*0.215*	*0.091*	
Q-value	*0.941*	*0.490*	*0.120*	*0.359*	*0.228*	
N	540	540	540	540	540	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table 10: Assets

	(1) Assets per capita, in 1000s riel -inverse hyperbolic sine transf.	(2) Livestock holdings per capita, current, in 1000s riel - inverse hyperbolic sine transf.	(3) Livestock acquired (total number), over last one year	(4) Avg. effect
Mean poor control households (levels)	276.949	241.315	0.950	
LEAP	0.025	0.030	0.161	0.026
Wild boot. p-value	*0.925*	*0.955*	*0.675*	*0.773*
Q-value	*0.955*	*0.955*	*0.955*	
LEAP x Non-poor household	-0.234	0.509	-0.105	0.015
Wild boot. p-value	*0.362*	*0.464*	*0.866*	*0.909*
Q-value	*0.696*	*0.696*	*0.866*	
Non-poor household	1.448	0.896	-0.040	
Wild boot. p-value	*0.000*	*0.020*	*0.946*	
Q-value	*0.001*	*0.030*	*0.946*	
N	540	540	540	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table 11: Expenditures

All dep. variables **inverse hyperbolic sine transf.**	(1) Non-food expenditures per capita, past 30 days (in 1000s riel)	(2) Misc. expenditures per capita, past 12 months (in 1000s riel)	(3) Bought food consumption per capita, past 7 days (in 100s riel)	(4) Avg. effect
Mean poor control households (levels)	25.700	350.094	144.400	
LEAP	0.105	-0.172	0.008	-0.017
Wild boot. p-value	*0.262*	*0.161*	*0.956*	*0.880*
Q-value	*0.393*	0.393	*0.956*	
LEAP x Non-poor household	-0.118	-0.013	-0.118	-0.107
Wild boot. p-value	*0.327*	*0.948*	*0.526*	*0.505*
Q-value	*0.789*	0.948	*0.789*	
Non-poor household	0.174	0.629	0.183	
Wild boot. p-value	*0.023*	*0.000*	*0.086*	
Q-value	*0.035*	*0.001*	*0.086*	
N	540	540	540	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table 12: Economic Networks

Links to other experimental participants	(1) Funeral group (1=having at least one link, 0 otherwise)	(2) Exchange hands (for free, nr. of links)	(3) Regular buying and selling link (undirected, nr. of links)	(4) Employee-relationship (undirected, nr. of links)	(5) Coworker (nr. of links)	(6) Producer group (1=having at least one link, 0 otherwise)	(7) Avg. effect (cols. 1-5)
Mean poor control households	0.023	2.364	3.058	1.480	2.671	0	
LEAP	0.177	2.199	0.372	-0.062	-0.210	0.048	0.36
Wild boot. p-value	*0.330*	*0.153*	*0.772*	*0.924*	*0.837*	*0.452*	*0.16*
Q-value	*0.904*	*0.904*	*0.924*	*0.924*	*0.924*	*0.904*	
LEAP x Non-poor household	-0.019	-0.270	-0.091	-0.039	-0.026	-0.014	-0.047
Wild boot. p-value	*0.697*	*0.499*	*0.874*	*0.915*	*0.935*	*0.434*	*0.501*
Q-value	*0.935*	*0.935*	*0.935*	*0.935*	*0.935*	*0.935*	
Non-poor household	0.000	0.365	0.013	0.485	-0.094	0.000	
Wild boot. p-value	*0.536*	*0.177*	*0.976*	*0.153*	*0.734*	*0.970*	
Q-value	*0.976*	*0.531*	*0.976*	*0.531*	*0.976*	*0.976*	
N	511	511	511	511	511	511	

Note: Col. 6 is excluded from the average effect calculation due to zero links (and thus zero standard deviation) in the control group. Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table 13: Social Networks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Worship group	Babysitting	Seek advice	Social Group	Volunteering	Borrow (non monetary)	Avg. effect
Nr of links to other experimental participants							
Mean poor control households	1.439	0.353	0.711	2.578	1.838	1.364	
LEAP	1.494	0.066	0.338	1.052	0.477	0.660	0.256
Wild boot. p-value	*0.318*	*0.712*	*0.344*	*0.529*	*0.641*	*0.575*	*0.205*
Q-value	*0.712*	*0.712*	*0.712*	*0.712*	*0.712*	*0.712*	
LEAP x Non-poor household	-0.340	-0.161	0.032	-0.243	-0.687	-0.104	-0.091
Wild boot. p-value	*0.466*	*0.456*	*0.865*	*0.626*	*0.025*	*0.718*	*0.233*
Q-value	*0.862*	*0.862*	*0.865*	*0.862*	*0.150*	*0.862*	
Non-poor household	0.055	0.106	0.113	0.363	0.644	-0.023	
Wild boot. p-value	*0.698*	*0.214*	*0.248*	*0.207*	*0.002*	*0.883*	
Q-value	*0.838*	*0.372*	*0.372*	*0.372*	*0.012*	*0.883*	
N	511	511	511	511	511	511	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table 14: Group Memberships

	(1) Producer	(2) Rice seed	(3) Funeral	(4) Youth	(5) Irrigation	(6) Women	(7) Avg. effect
Memberships of household in associations (binary)							
Mean poor control households	0.039	0.210	0.110	0.033	0.017	0.199	
LEAP	0.034	0.293	-0.055	0.017	0.017	0.125	0.210
Wild boot. p-value	*0.238*	*0.001*	*0.282*	*0.494*	*0.425*	*0.421*	*0.066*
Q-value	*0.494*	*0.006*	*0.494*	*0.494*	*0.494*	*0.494*	
LEAP x Non-poor household	-0.012	-0.110	-0.025	0.048	0.048	-0.008	0.028
Wild boot. p-value	*0.730*	*0.285*	*0.531*	*0.227*	*0.117*	*0.914*	*0.854*
Q-value	*0.876*	*0.570*	*0.797*	*0.570*	*0.570*	*0.914*	
Non-poor household	-0.027	-0.008	0.024	-0.011	0.006	-0.008	
Wild boot. p-value	*0.157*	*0.914*	*0.461*	*0.643*	*0.628*	*0.811*	
Q-value	*0.914*	*0.914*	*0.914*	*0.914*	*0.914*	*0.914*	
N	540	540	540	540	540	540	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table 15: Community Actions

	(1)	(2)	(3)	(4)	(5)
		Household member helped, past 6 months (all binary):			
	Number of community meetings attended over the last year	Build or rebuild school	Build or repair road	Clean up public space in the community	Avg. effect
Mean poor control households	6.221	0.343	0.580	0.215	
LEAP	1.511	0.037	0.018	0.058	0.143
Wild boot. p-value	*0.088*	*0.779*	*0.880*	*0.514*	*0.337*
Q-value	*0.352*	*0.880*	0.880	*0.880*	
LEAP x Non-poor household	0.859	-0.134	-0.042	-0.053	-0.076
Wild boot. p-value	*0.520*	*0.026*	*0.524*	*0.524*	*0.355*
Q-value	*0.524*	*0.104*	0.524	*0.524*	
Non-poor household	0.464	0.051	0.049	0.065	
Wild boot. p-value	*0.329*	*0.137*	*0.350*	*0.311*	
Q-value	*0.350*	*0.350*	0.350	*0.350*	
N	540	540	540	540	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table 16: Pro-Social Behavior

	(1) Altruism (amount sent in dictator game, 0-3000 riel)	(2) Public good contribution (yes=1, no=0)	(3) Trust (amount sent in the trust game, 0-3000 riel)	(4) Trustworthiness (fraction returned in the trust game)	(5) Avg. effect
Mean poor control households	684.971	0.642	493.827	0.154	
LEAP	99.877	0.043	116.562	-0.001	0.120
Wild boot. p-value	*0.089*	*0.505*	*0.297*	*0.96*	*0.242*
Q-value	*0.356*	*0.674*	*0.594*	*0.96*	
LEAP x Non-poor household	-99.744	-0.005	-135.112	0.024	-0.048
Wild boot. p-value	*0.479*	*0.949*	*0.393*	*0.497*	*0.719*
Q-value	*0.663*	0.949	*0.663*	*0.663*	
Non-poor household	144.441	0.025	303.792	-0.01	
Wild boot. p-value	*0.083*	*0.680*	*0.022*	*0.715*	
Q-value	*0.166*	0.715	*0.088*	*0.715*	
N	511	510	243	266	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

8. Online Appendix

Figure A1: Histogram of the number of livestock acquired in the last one year

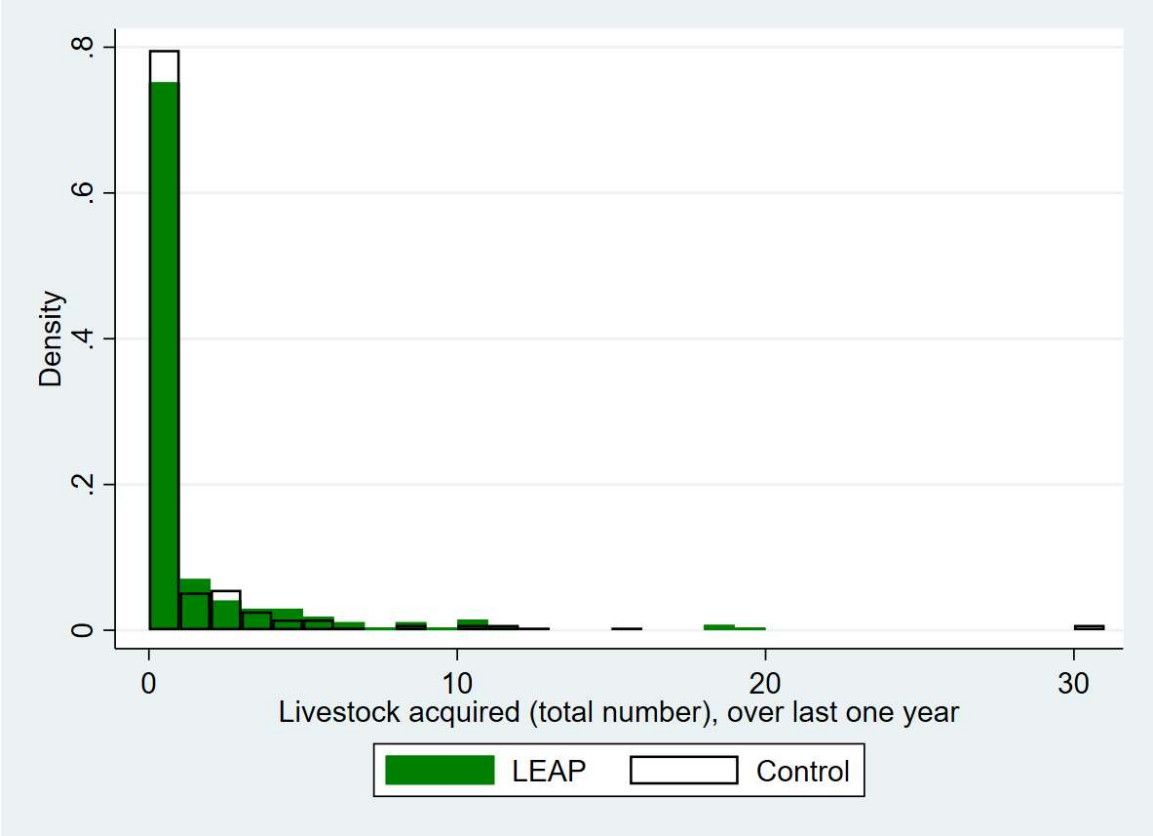


Table A1: Representativeness

Census Means	Survey Village	All 200 Potential Target Villages (Siem Reap Province)	National Census 2008
Village characteristics (average)			
Total population (in 100s)	8.492	8.328	
Total number of households	170.189	174.083	
Average household size	4.962	4.806	4.7 [rural 4.6]
Fraction of households female headed	0.251	0.273	0.256
Male literacy rate (>15 years)	0.712	0.739	0.851 [rural 0.825]
Female literacy rate (>15 years)	0.572	0.581	0.709 [rural 0.663]
Dependency ratio	0.694	0.664	0.612 [0.671]

Table A2: Definitions of production, asset and expenditure variables

Household production	**Items**
Annual livestock production per capita, last one year (in 1000s riel)	Fish, Processed fish eggs, Meat, Hide and Other livestock by-products
Annual income from livestock sales per capita, last one year (in 1000s riel)	Fish, Processed fish eggs, Meat, Hide and Other livestock by-products
Annual income from crop sales per capita, 2012 harvest season (in 1000s riel)	Rainy, wet, all year season: Wet season paddy, Dry season paddy, Soya bean, Maize, Groundnut, Sunflower, Coconuts, Palm, Sugarcane, Vegetables, Fruits, Flowers, Aqua culture, Others
Other annual revenue-generating activities per capita, 2012 (in 1000s riel)	Off-farm, Labor employment, Gifts, Remittances and Transfers [excluding household cropping, livestock raising, fishing and fish culture].
Household assets	
Assets per capita (in 1000s riel)	Radio/Transistor, TV, Telephone(Fixed), Cell Phone, Personal Computer, Bicycle, Motorcycle, Car/Van, Boat, Big Tractor, Hand Tractor, Sewing Machine, Motor Remorque, Other
Livestock holdings per capita, current (in 1000s riel)	Cow, Calves, Buffalo, Pigs, Chicken, Ducks, Others
Livestock acquired (total number), over last one year	Cow, Calves, Buffalo, Pigs, Chicken, Ducks, Others
Household expenditures	
Non-food expenditures per capita, past 30 days (in 1000s riel)	Purchased, received in-kind, home produced: Cooking fuel (wood, gas, charcoal), Kerosene oil (for lighting), Electricity (impute per month), Matches, candles, lighters, lanterns etc., Toilet soap, toothpaste, shampoo, other personal care items, Newspapers, books, & recreation and entertainment expenditures, Transport, Household cleaning articles (soap, bleach, washing powder), Others
Misc. expenditures per capita, pas 12 months (in 1000s riel)	Purchased, received in-kind: Clothing for men, Clothing for women, Clothing for children including school uniform if any, Total educational expenditure, Footwear (shoes, slippers, etc.), Healthcare expenditure, Remittances sent to othermhouseholds / individuals, Recreation, Toys, sports goods, etc., Taxes, other charges, Social expenses, for events organized by the household, Social expenses, for events attended by the household, Others
Bought food consumption per capita, past 7 Days (in 100s riel)	Rice, Sugar, Milk products, Oils, Meat, Fish or Prahoc, Eggs, Salt and spices, Vegetables and Fruit, Alcohol and other intoxicants, Cigarettes, Other

Table A3: Risk and Time Preferences

	(1) Risk taking [1=low - 5=high]	(2) Impatience [1=very patient - 6=very impatient]	(3) Avg. effect
Mean poor control households	2.180	3.424	
LEAP	0.100	0.101	0.070
Wild boot. p-value	*0.462*	*0.750*	*0.409*
Q-value	*0.750*	*0.750*	
LEAP x Non-poor household	0.053	-0.105	0.004
Wild boot. p-value	*0.789*	*0.844*	*0.982*
Q-value	*0.844*	*0.844*	
Non-poor household	0.009	-0.134	
Wild boot. p-value	*0.951*	*0.651*	
Q-value	*0.951*	*0.951*	
N	511	497	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table A4: Pro-Social Behavior and Family Links

	(1) Altruism (amount sent in dictator game, 0-3000 riel)	(2) Public good contribution (yes=1, no=0)	(3) Trust (amount sent in the trust game, 0-3000 riel)	(4) Trustworthiness (fraction returned in the trust game)	(5) Avg. effect
Mean poor control households	684.971	0.642	493.827	0.154	
LEAP	88.705	0.027	83.729	-0.009	0.075
Wild boot. p-value	*0.068*	*0.695*	*0.397*	*0.642*	*0.411*
Q-value	*0.272*	*0.695*	0.695	*0.695*	
LEAP x Non-poor household	-100.559	-0.006	-146.179	0.026	-0.051
Wild boot. p-value	*0.483*	*0.941*	*0.364*	*0.448*	*0.706*
Q-value	*0.644*	*0.941*	*0.644*	*0.644*	
Non-poor household	144.393	0.025	302.010	-0.010	
Wild boot. p-value	*0.084*	*0.684*	*0.023*	*0.722*	
Q-value	*0.168*	*0.722*	*0.092*	*0.722*	
Nr. of family links	5.369	0.008	14.639	0.004	
Wild boot. p-value	*0.616*	*0.340*	*0.104*	*0.015*	
Q-value	*0.616*	*0.454*	0.208	*0.060*	
N	511	510	243	266	

Note: Wildbootstrap clustered at the commune level (24), 10,000 replications. Q-values controlling for the False Discovery Rate (FDR) as suggested by Anderson(2008) were calculated by row using the STATA do-file by Michael L. Anderson, available at: http://are.berkeley.edu/~mlanderson/downloads/fdr_qvalues.do.zip. [Accessed May 25 2017].

Table A5: Controlling for Winnings in the Risk Activity (Sample of Control Communes)

	(1) Altruism (amount sent in dictator game, 0-3000 riel)	(2) Public good contribution (yes=1, no=0)	(3) Trust (amount sent in the trust game, 0-3000 riel)	(4) Trustworthiness (fraction returned in the trust game)
Winnings in lottery (100s)	-1.079	0.002	-7.479	0.000
	(5.228)	(0.004)	(6.104)	(0.002)
Non-poor household	142.048*	0.029	291.194**	-0.010
	(77.825)	(0.064)	(122.296)	(0.027)
Constant	707.603***	0.600***	647.108***	0.154***
	(123.985)	(0.089)	(123.658)	(0.039)
N	258	257	123	133

Robust standard errors, * 0.10 ** 0.05 *** 0.01

Table A6: Mechanisms (Sample of Control Communes)

	(1) Altruism (amount sent in dictator game, 0-3000 riel)	(2) Public good contribution (yes=1, no=0)	(3) Trust (amount sent in the trust game, 0-3000 riel)	(4) Trustworthiness (fraction returned in the trust game)
Total Annual Production (in logs)	53.203**	0.006	-6.310	0.005
	(24.971)	(0.025)	(57.501)	(0.006)
Savings (in logs)	-6.504	0.027**	13.515	-0.001
	(15.259)	(0.013)	(40.912)	(0.004)
Nr. of SHG network links	-26.354	-0.065	-125.720	-0.005
	(73.863)	(0.062)	(98.228)	(0.025)
Non-poor household	130.159*	0.010	299.756**	-0.010
	(76.955)	(0.064)	(133.468)	(0.026)
Constant	321.943*	0.584***	543.076	0.123***
	(172.711)	(0.179)	(397.474)	(0.043)
N	258	257	123	133

Robust standard errors, * 0.10 ** 0.05 *** 0.01

Experimental Protocol and Instructions
(closely following Avdeenko and Gilligan, 2015)
Group Activities Invitation
Thank you very much for participating in this important project. The answers that you gave will help us understand how to better design programs to help the economy of developing countries and in Cambodia and around the world.

I would like to ask you if would be willing to participate in further research that we are conducting for this project. These new activities are not a survey like this one but a group discussion and series of group activities and games that you would play with other members of your community. There will be a chance to win some money during the activities. We expect people will win about 16,000 riels on average. You may win a little more or less depending on how well you play the games and your luck but you will certainly win something.

These activities will be held at [STATE DATE AND TIME OF MEETING] AT [STATE LOCATION OF MEETING]
Would you like to participate in these activities?
ENUMERATOR: Consent given: ☐ Yes ☐ No

Respondents Name_____
Respondents address _____
Respondents telephone number _____
IDNO (question 6 on first page of survey)_____

ENUMERATOR: Complete the portion below, remove it, give it to the respondent **and tell him/her to bring it to the game session**. KEEP THE TOP PORTION ATTACHED TO THE SURVEY FORM FOR OUR RECORDS.

Consent given: ☐ Yes ☐ No

Respondent's Name_____
Respondent's address _____
Respondent's telephone number _____
IDNO (question 6 on first page of survey) _____
DATE AND TIME OF SESSION _____
LOCATION OF SESSION _____

Cambodia LEAP
Instructions for Social and Economic Games

Supplies

- 12,000 KHR worth of 500 KHR notes for players to make their allocation decisions.
- Large sheet of paper with a line drawn across it, on which players' allocation decisions are made.
- Poster with lottery example.
- A Cambodian coin
- Poster with discount rate example.
- Dice
- Poster for dictator game example.
- Poster for trust game example.
- Poster for public goods game example
- Marked Public Goods cards
- Game data recording sheets.
- Post-it notes to cover previous play on discount rate recording sheet
- 15 Slips each with a different letter to identify the players easily
- 15 slips with numbers on them to draw from hat/bag for role assignments

Each subject is given an alphabetical identifying code when they arrive. Since there will be a maximum of 15 subjects in the field identifiers will be the letters A through O.

Introduction

Good morning/afternoon! Thank you all very much for coming today. We are researchers from the United States who are interested in understanding more about Cambodian society. The more we understand about Cambodian society the better we can design programs that will help the Cambodian people prosper economically and live in peace. Also the lessons that we learn here will be used to inform our design of programs that will help other countries around the world. So your participation today will not only help your neighbors and others in Cambodia but the knowledge that you help create today will improve programs for poor countries around the world. So in a very special way what we learn here today will be your and Cambodia's gift to the poor people of the world.

This is important work because of what it will teach us about how to design programs for poor countries. But it is also going to be fun because we are going to play some games today that we hope you will enjoy playing. You will also earn some money by playing them. These games may seem strange at times and you may wonder why we are playing them. The reason we are having you play the games is that we can learn about Cambodian society by watching you play the games. I will explain more about what we learn from the games after we are done playing all of them.

We hope you make a lot of money today and we hope that you have fun playing the games, but please remember this is serious endeavor we are undertaking. We hope you have fun but we are not doing it for fun. We are doing it to learn how to make better programs to help poor people. Since that is a serious endeavor we ask that you take it seriously and behave respectfully. By that we mean please do not talk out of turn. Please take what you are doing here today seriously. And most important of all do not talk to any of the other villagers about the choices that you make when you play the games. If you cannot follow these rules we will have to ask you to leave the session and you will not receive the money that you earned from playing the games. Does everyone understand these rules?

As I said we will be playing some games today. I will explain the rules to these games in a few minutes. For right now all you need to know is that you are supposed to play the games as <u>you</u> think best. We want you to play the games in the way that you think is best for <u>you</u>. There is no right or wrong way to play them. We want you make the decisions that are best for <u>you personally</u>.

We are going to play a total of five different games today. You will play the first three games one-by-one privately in this area over here [POINT to it] . We will play the fifth game all at once in this room.

For all five games it is very important for you to know that none of the other players will know how you played the games. For the first three games there will be a games facilitator in the game area to help you make your decision and to make sure you understand the game but for all four games none of the other players will ever find out your actions.

For that reason I repeat again that it is very important that you do not tell anyone else what you did when you played the game. We want to completely insure your anonymity so that you can feel free to play the game in the way that <u>you</u> think best. If you cannot comply with this rule of not talking to the other players about your decisions in the games then you will have to leave.

OK let's have some fun! Let's go on to a description of the rules of the game.

Activity 1 (Lotteries)

The first game is a coin-flip game. In this activity, we will give you 5 options. Each optionwill be a different kind of gamble. In each gamble, there are two possible prizes. We will ask you to choose which gamble you like the best. Then, we will flip a coin. If it is [picture], you will get the prize on the left of whichever gamble you picked. If it is [no picture], you will get the prize on the right.[NB: Head is picture.]

Option	Picture	No picture
1	2000	2000
2	1500	2500
3	1000	3000
4	500	3500
5	0	4000

Example (demonstrate with coin and showing lottery example): Suppose you pick the third gamble. Then, if it is picture [heads], you get 1000 KHR, if it is [tails], you get 3000 KHR. [Flip coin and say what you would get.]

Okay, does everyone understand?

[If yes…]Okay, you will come up one by one, and we will play the game.

Activity 2 (Discount Rates)

This game is a dice-game. In this activity, we will show you six different choices. For each choice, you must decide between two payment options: in the first option, you will be paid 2000 riel now. In the second option, you will wait and be paid a larger amount of riel in one week. You will pick which of the two payment options you prefer. In other words, you will choose whether you want 2000 KHR today which is listed on the left in this chart [INDICATE ON THE CHART] or the larger amount in one week, which is listed on the right [INDICATE ON THE CHART].

If you choose and win an amount to be disbursed in a week you will be able to pick up your money at [SPECIFY THE LOCATION] one week from today. If you choose and win an amount to be disbursed today it will be added to your winnings at the end of this session.

In each of the six choices pick the one you like best. In the previous game you picked only one of the five choices. In this game you must express a preference for *each* of the six possible choices.

Once you specified your preferences over all six of the choices you will roll a die with the numbers one through six on each side. The number that you roll will determine which of the payouts you receive.

Choice	Now	In one week
1	2000	2500
2	2000	3000
3	2000	3500
4	2000	4000
5	2000	4500
6	2000	5000

Example (demonstrate with a die): Suppose a person specifies the following preferences:

Choice	Now	In one week
1	2000	2500
2	2000	3000
3	2000	3500
4	2000	4000
5	2000	4500
6	2000	5000

(Circle the preferences on the chart you are using to illustrate the game). In this case the person prefers receiving 2000 now to receiving 2500, 3000 or even 3500 in one week. But this person also prefers to wait a week to receive 4000, 4500 or 5000 in one week rather than taking 2000 today.
Now we will roll the die to see what this person wins. [Roll the die and announce the result]
Okay, does everyone understand this game?

Activity 3 (Dictator Game---Do not tell people the name of the game!!!)
In this game, you will be given 3000 KHR in 500 KHR notes. Once we give it to you this will become your money to keep and do with as you like. We will give you an opportunity to donate some of the money to a needy family in your community. We are required to guarantee the anonymity of this family so we cannot tell you their name. The family that will receive your donations has been picked by us in consultation with local community leaders. Our team will give the money to the needy family after the game session is over.
When it is your turn, you will be given 3000 KHR in 500 KHR notes. You will be asked to decide how much of that 3000 KHR, if any, to give to this needy family and how much to keep for yourself.
You will indicate how much you wish to give to the family by pushing the 500 KHR notes that you wish to give over the line on a sheet of paper. Those bills that you keep on your side of the line are yours to keep. You will be awarded that amount of money along with your other winnings at the end of the game.
[DEMONSTRATE ON THE DRAWING]
Finally I want to emphasize to you that there is no right or wrong choice in this activity. You should choose to give the amount that you think is best for you, whether that amount is zero KHR, 500 KHR or any other amount all the way up to 3000 KHR. Whatever choice you make that you think is best for you is the right choice.
I also want to emphasize that no one in this room will know how much you send to the needy family. Your choices are completely anonymous. No one will ever know what you decided to do in this game.
Does everyone understand this activity?

Activity 4 (Trust Game -- Do not tell people the name of the game!!!)
This is a new game. It is completely different from the last two games. In this game, you will be either a "Player 1" or a "Player 2." Both Player 1 and Player 2 get 3000 KHR in 500 KHR notes to start. Then, player 1 decides how much of his or her 3000 KHR, if any, to send to the player 2 and how much to keep. Whatever player 1 sends to the player 2 is then *tripled* by us. So if 500 KHR are sent, we will make it 1500 KHR. If 1000 KHR is sent, we will make it 3000 KHR. If 1500 KHR is sent, we will make it 4500 KHR. If 2000 KHR is sent, we will make it 6000. If 2500 KHR is sent we will make it 7500 KHR. If 3000 KHR is sent we will triple it to 9000 KHR. Player 1 also has the choice of sending nothing in this round and keeping the 3000 for himself.
Once player 1 has made his or her choice, we will give this tripled amount to the Player 2. Player 2 will then decide how much of it to keep and how much to send back to player 1.

Example (demonstrate with bills): Suppose player 1 sends 1000 KHR and keeps 2000. We will make the 1000 KHR that were sent into 3000 KHR and give it to the receiving person. Player 2 now has this 3000 KHR plus the original 3000 KHR that we gave him or her at the start of the game, so in this particular example player 2 would have a total of 6000 KHR. Player 2 now decides how much of this 6000 KHR to send back and how much to keep. He or she can send all of the 6000 KHR back and keep nothing, send 5500 KHR back and keep 500, send 5000 KHR back and keep 1000, send 4500 KHR back and keep 1500, send 4000 back and keep 2000, send 3500 back and keep 2500, send 300 back and keep 3000, send 250 back and keep 3500, send 2000 and keep 4000 and so on including sending nothing back and keeping all 6000.

[OPTIONAL EXAMPLE TO BE USED ONLY IF PEOPLE LOOK CONFUSED] Here is another example. Suppose player 1 sends 3000 KHR and keeps none. We will triple this amount so that it is 9000 KHR and give it to player 2. These 9000 KHR plus player 2's original 3000 KHR that we gave him or her at the start of the game gives him or her a total of 12000 KHR. Player 2 can keep all of the 12000 KHR and send nothing back, or keep 11500 of the KHR and send 500 KHR back, or keep any other amount---11000, 10500, 10000, 9500, 9000, 8500, 8000, 7500, 7000, 6500, 6000, etc.---and send the rest back. Here are the steps:

The game proceeds in two rounds.

1. In the first round you will come up one by one and draw a number. We will tell you if your number means that you are player 2 or player 2. We will randomly match up each player 1 with a player 2. But no one will know who is their partner in this game.
    a. If you are player 2: Your first job will be to show us how much you want to send to your receiving person and how much to keep. Remember that we will triple whatever you send before player 2 gets it. You will show us your choice and then return to your seat. Do not tell anyone what you sent.

    b. If you are player 2: You do nothing this time. You just return to your seat.

Now we proceed to the second round.
Once again each person will be called up one by one.

a. If you are player 1 you have no further decisions to make. You can return to your seat

b. If you are player 2 we will tell you how much was sent to you. Your job will be to decide how much to send back and how much to keep. You will show us this decision by pushing the amount of money you want to return to the investor over the line on the sheet of paper between you and the game facilitator. *Do not tell anyone what you sent back.*

2. You will find out how much you are paid after all the games are finished.

I want to remind you that no one in this room will know whether you are a player 1 or a player 2 and no one will know the person in this room with whom you are paired. Similarly, you will not know the person with whom you are paired. Your choices, then, are completely anonymous. No one will ever know what you decided to do in this game. Does everyone understand how this game is played? Are there any questions?

Activity 5: Dichotomous Public Goods game [DO NOT TELL THEM THE NAME OF THE GAME]

We will play this activity all at once in a group here in the discussion area. In this activity each person will be handed two folded cards like these [show them a pair of folded cards].

As you can see on the front of each these folded cards is an identifying letter for each player—A through N. Each pair of cards will be given to the proper player according to the identifying letter. [SHOW THEM THE DRAWING OF THE PERSON WITH 2 CARDS.]

The inside of the two cards is different. The inside of one of the cards is completely *blank*. Nothing is written in it. [POINT TO THE BLANK CARD ON THE POSTER.] Inside the other card we have drawn an "X". [POINT TO THE "X" CARD ON THE POSTER.]

The activity is played in two rounds.

In the first round I will ask you will choose which of the two cards you wish to turn in. For every card with an X in it that is turned in the first round, we will give *every player* 500 riels. [INDICATE ON THE DRAWING THAT EACH X CARD TURNED IN GIVES 500 riels TO *EVERYONE*]. For every blank card that is turned in the first round, we will not increase the money. [INDICATE ON THE DRAWING THAT EACH BLANK CARD TURNED IN GIVES 0 extra riels.]

If you turn in the X card in the first round you will be left with one blank card. If you turn in a blank card in the first round you will be left with one X card. Once you turn in the card of your choice you will not get it back. We will keep it and you will be left with only one remaining card.

In the second round, I will ask you to turn in your only remaining card. If you turn in an X card, we will give *you and only you* an extra 2000 riels. This is in addition to the amount that you received from the cards turned in Round 1. [POINT TO POSTER.] If you turn in a blank card in the second round we will not give you any extra money, nor will we give anyone else any extra money. [POINT TO POSTER.]

In summary, if you want to increase the amount of money that *everyone* receives by 500 riels, turn in the X card in the first round. If instead you want to increase the amount that you *and only you* receive at the end of the activity by 2000 riels, keep the "X" card in the first round and instead turn in blank card in the first round. Remember that you get 500 riels for every X card that is turned in by another person in the fist round *even if you did not turn in your "X" card in the first round*. IF you do not turn in your X card in the first round you will get 500 for each X card turned in by the other people PLUS 2000 riels for turning in your X card in the second round.

Any questions? [IF NECESSARY GIVE AN EXAMPLE TO SHOW HOW THE GAME WORKS]

OK. Now we will collect the cards for Round 1.