

Community-Driven Reconstruction in Colombia: An Experimental Study of Collective Action beyond Program Beneficiaries

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Abstract – Increased community cooperation is an important objective of Community-Driven Reconstruction (CDR) programs in post-conflict settings. While these programs typically work with a limited group of beneficiaries, little is known about the potential community impact beyond these beneficiaries. To investigate this, we empirically analyze how cooperative behavior develops in a lab-in-the-field experiment with mixed groups of CDR program beneficiaries and non-beneficiaries, organized in 42 municipalities in Colombia with active CDR programs. In the experiment, we use two rounds of a binary public goods game with a communication stage between both rounds. The experimental data are complemented with information on pre-existing social proximity among the participants and whether they have participated in a CDR program. We find that cooperation increases after communication, and that it correlates positively with the proportion of cooperators before communication. This peer effect is mainly driven by the cooperative behavior of CDR program beneficiaries while the influence of non-beneficiaries is limited.

Key words – Collective action, community reconstruction, communication, experiments, South America, Colombia

1. INTRODUCTION

Local collective action is necessary for economic growth (Justino, 2006), market participation (IFPRI, 2014), and local governance (Ostrom, Gardner, & Walker, 1994) in post conflict states (Del Castillo, 2008; Özerdem, 2012). Conflict impedes economic growth and compromises the effectiveness of broad social institutions (Blattman & Miguel, 2010). Some researchers also suggest that conflict damages cooperation and social cohesion (Cliffe, Guggenheim, & Kostner, 2003). A recent meta-analysis by Bauer, Blattman, Chytilová,

Henrich, Miguel and Mitts (2016), however, reports that those exposed to violence are predisposed to show more cooperation towards in-group members and similar levels of cooperation towards out-group members than those with limited exposure to violent conflict after the conflict is over. Many questions remain, however, about how norms of cooperation diffuse through societies affected by violent conflict. Can international aid programs facilitate the transmission of such norms? Do such programs use extant social networks to facilitate this transmission?

Researchers and practitioners have tried to identify the most effective instruments to rebuild the damaged social fabric after the end of a conflict (Fearon, Humphreys, & Weinstein, 2009). To this end, the World Bank and other donors have begun extensive use of Community-Driven Reconstruction (CDR) programs in post-conflict states (see Kyamusugulwa, 2013). CDR programs give local community councils authority over how to distribute development funds. The premise behind these programs is that locally accountable decision-makers can target local projects to meet time- and place-specific needs and that the process of aid administration and local project management can reinvigorate social norms of cooperation (Cliffe et al., 2003). But despite the promises of CDR aid, empirical evidence of the effectiveness of CDR programs to increase social cohesion *beyond the original beneficiaries* is weak (Kyamusugulwa, 2013; Vervisch, Titeca, Vlassenroot, & Braeckman, 2013). The relevance of this is obvious: if collective action is limited to the original group of beneficiaries then the welfare gains would be smaller for their communities.¹

In this article, we will shed light on the effectiveness of CDR programs to increase social cohesion beyond the original beneficiaries by examining how cooperation changes after communication. We do not focus on whether the intervention itself increases cooperation levels, which has already been studied before experimentally, with mixed results on effectiveness (Advenko & Gilligan, 2015; Fearon et al., 2009; Fearon, Humphreys, & Weinstein, 2015; Humphreys, de la Sierra, & van der Windt, 2013; King & Samii, 2014).

Instead, we examine spill-over effects towards the wider communities in which CDR programs operate. To address this research question, we investigate communication effects on cooperative behavior between beneficiaries and non-beneficiaries of CDR programs. In particular, we study how communication influences cooperative behavior, by transferring information about people's past and future cooperative behavior.

There is strong evidence that non-binding communication influences cooperation in lab experiments used to study collective action behavior (e.g. Dawes, McTavish, & Shaklee, 1977; Cardenas, Stranlund, & Willis, 2000; Isaac & Walker, 1988; Ostrom et al., 1994 and see also the meta-analyses of Sally (1995) and Balliet (2010)).² Among other things, communication may facilitate the transfer of information about the propensity of others to cooperate in the group and then change the beliefs about others' future cooperation (Bornstein, 1992; Lopez & Villamayor-Tomas, 2017). There is extensive evidence that such beliefs influence individual cooperation decisions: People contribute more to collective action if they expect others to do the same, but they stop contributing when they observe that others free ride on their efforts. Such "conditional cooperation" tends to be an important driver of collective action (Fischbacher, Gächter, & Fehr, 2001).

Most of the existing literature on communication and collective action identifies the effect of within-group communication as a single average treatment effect for groups that are exposed to communication against those that are not. However, the effects of communication may vary considerably within groups, depending on factors such as the strength of social norms and the emergence of group identity, among others (Bornstein, 1992; Shankar & Pavitt, 2002; Cardenas, 2003; Ghate, Ghate, & Ostrom, 2013; Lopez & Villamayor-Tomas, 2017).

In particular for this study, we focus on how the effect of communication interacts with the identity of the people one communicates with, along two dimensions: their participation in CDR programs; and their social proximity in terms of pre-existing social ties and whether

they are of the same gender. We include social proximity in the analysis as it allows us to identify important interactions with the influence of participation in CDR programs. With CDR programs aiming to increase social cohesion, this could provide policy-relevant insights. In addition, if social proximity correlates with participation in CDR programs, it may act as a confounding factor, which should be controlled for in the analysis.

To study how communication influences collective action and how this interacts with participation in a CDR program and social proximity, we use data from a lab-in-the-field experiment with a pool of beneficiaries and non-beneficiaries of CDR programs from villages and small towns of Colombia.³ More specifically, we use two rounds of a binary public goods game with a communication stage between both rounds. After finishing the first decision, participants do not get any public feedback regarding the contributions of others during that round nor do they get information about their earnings in that round. Subjects may use communication to share information about their decisions in the previous round, make commitments about what to do in the second round, or communicate about any other topic. The experiment was organized in 42 municipalities, in six different regions with active CDR programs. The experimental data are complemented with information on the pre-existing social ties between the participants in our study, their gender and whether they had participated in a CDR program. Using econometric techniques we study how communication effects depend on the beneficiary status of the decision-makers and how they interact with the beneficiary status and social proximity of the peers (as measured by pre-existing social ties or whether they are of the same gender).

We summarize our results as follows: We find that the likelihood of cooperation after communication increases with the proportion of participants that cooperated before communication, and that beneficiaries of CDR programs exert a stronger influence on the collective action behavior of other community members than those that were not beneficiaries. We also find that this difference in communication effects is stronger among

people who are socially more distant from each other, either because they do not know each other or they are of different gender.

2. RESEARCH DESIGN

(a) Background

The armed conflict in Colombia has been ongoing for more than five decades. This conflict involves government forces, rebel insurgencies and illegal paramilitaries (Vargas, 2012). The majority of the victims of this conflict are civilians, with more than 220,000 deaths and 25,000 missing people between 1958 and 2012 (Centro Nacional de Memoria Histórica, 2013). Additionally 7% of the population was displaced by 2005 (Ibáñez & Velez, 2008).

In 1995 the first CDR program, the *Peace and Development Program of Magdalena Medio* (PDPMM or Peace and Development), was launched aiming to construct peace through inclusive development.⁴ In 1997 and 2001 this program received funding from World Bank Learning and Innovations Loans and in 2002 the European Union joined this initiative with the program “Peace Laboratory.” Late in 2004 the EU and the World Bank funded the expansion of these programs to other regions of Colombia.⁵ There are currently 19 programs of this kind around the country. In all programs, projects are formulated and developed at the local level with the assistance of local organizations that have been working in the regions for many years. In total, approximately 1400 projects have been developed, around three major topics: human rights, culture and peace; governability, institutional strength and citizen participation; and sustainable economic development (Guarín et al., 2008).

In 2006, the Colombian government and the donor agencies funded an impact evaluation of the programs operating in six different regions. One of the goals of the evaluation was to

assess if collective action had increased in the areas, and for this, economic experiments were designed. Among the six regions, 127 municipalities in nine departments were included. In total, these regions have approximately 4 millions of inhabitants, 260,000 of whom are direct beneficiaries of at least one of the CDR projects (Guarín et al., 2008). The data used in this paper were collected as part of this evaluation.⁶

(b) Experimental game

To study collective action we used a binary public goods game with groups of more than 20 participants. Participants are given one token that they could invest into a private account or a group account. Putting the token in the private account would give them 2000 pesos while in the group account the token would give *each* of the participants 100 pesos.⁷ Each participant, whether or not they donate to the group account, receives income from the group account.

As we have groups of more than 20 participants the social optimum is that everyone invests in the group account, as this would give more than 2000 pesos of group benefits per participant. However, for those who maximize their own income, it is better to free ride on the collective action of the others by putting the token in the private account. Own-income maximizing subjects have a strictly dominant strategy to put their token in the private account. In contrast, when a subject donates to the group account they engage in collective action.

Each subject made their contribution decision in private. They were given two cards, one card labeled ‘my token’ and one card without any label. A moderator went by each one of the participants with a bag and asked them to put one of the cards in the bag. Participants who wanted to invest in the group account put the ‘my token’ card in the bag and kept the other card. Participants who wanted to keep their token kept the card that says ‘my token’ and

deposited the other card in the bag. After all participants made their decision the experimenters counted the number of tokens invested in the group account, but they did not announce the total until the end of the experiment.

The game was played twice, with a communication stage of ten minutes in between. During the communication stage participants were free to move around the room and talk to each other. This communication allowed them to share information (for example about individual decisions in the first round or about ways to maximize group earnings) and make non-binding commitments about what to do in the second round. Once the ten minutes were over, the participants returned to their seats and participated in a second round of the public goods game, conducted in the same fashion.

Our design differs from existing experimental studies that give public information about the proportion of cooperators immediately after the decision is tallied; in our design, subjects engage in unstructured communication without public feedback on group behavior in the first round. Therefore, participants only could obtain information about others' previous cooperation decisions through communicating with others. While such decentralized communication may be more common in many types of social dilemmas, it also allows us to assess how participants respond to information provided by different types of peers (e.g. in terms of their participation in a CDR program or their social proximity).

(c) Behavioral expectations

In this section, we elaborate on potential mechanisms behind the causal effect of communication on cooperation and identify specific hypotheses. In the binary public goods game, an own-income maximizing person would always invest in the private account. The private investment generates 2000 pesos to that person instead of only 100 pesos when the token is invested in the group account. On the other hand, investment in the group account

raises 100 pesos for each member of the group; therefore, in groups strictly larger than 20, the group will be more efficient if all players invest in the public account. Despite these potential efficiency gains, investing the token in the private account is a strictly dominant best response to any strategy of the other players for own-income maximizing players.

However, existing evidence from public goods games shows that people do contribute to public goods (Ledyard, 1995). This contradicts the assumption of standard preferences. Instead of maximizing their individual income, many people are conditional cooperators; i.e. they contribute to collective action if they expect others to do the same, but they stop contributing when they think or observe that others free ride on their efforts (Fischbacher et al., 2001).

A particularly important factor that may influence conditional cooperation in public goods games is interpersonal communication. Although non-binding communication does not alter the payoff structure of the game, the communication process may transfer information about the propensity of others to cooperate in the group and then change the beliefs about others' future cooperation.⁸ By interacting with others during the communication stage individual participants may get a sense of what (some of) the participants decided in the previous round, and adapt their cooperation decision to this information.

Two conditions need to be fulfilled to promote communication effects. First, the behavior of an individual ('ego') will be influenced by the behavior of another individual ('alter') if there is a form of communication between both individuals during the communication stage. Second, such communication needs to convey information about the future action of alter. This can be information about the decision in the previous round and/or a commitment about their decision in the next round. This information is then processed to form a belief about the likelihood that alter will cooperate in round 2.

Whether there is any communication from alter to ego, and what information is conveyed, may depend on the beneficiary status of alter. Beneficiaries may be more

communicative than non-beneficiaries. Having participated in CDR programs, beneficiaries may be more used to communication and discussion in community-based collective action settings. Also, if CDR programs imbue cooperation norms, then beneficiaries may more likely communicate their commitment to cooperation, because they understand the importance of conditional cooperation. As a result, we hypothesize that beneficiaries will exert a stronger influence on cooperative behavior than non-beneficiaries.

At the same, communication effects may depend on the *social proximity* among the actors involved (in terms of pre-existing social ties or similarity on gender, for example). Direct communication is more likely among people who are socially proximate, and people may be more inclined to reveal their pro-sociality towards them. The importance of including this additional layer of analysis is two-fold. First, if social proximity correlates with participation in CDR programs (e.g. people who are acquainted are more likely to participate in the same organization), it may act as a confounding factor, which should be controlled for in the analysis. Second, it allows us to identify important interactions between social proximity and the influence of participation in CDR programs. For example, if individuals who are more socially distant from others engage less in *one-to-one* communication, it might be that as a result they would be more influenced by the information shared in *public* communication (i.e. talking to a group) in which beneficiaries are more inclined to take a leading role.

(d) *The experiments*

We organized 50 experimental sessions in 42 municipalities with a total of 1808 participants. For each session, we invited both beneficiaries and non-beneficiaries of CDR programs, with the goal of recruiting 20 beneficiaries and 20 non-beneficiaries. In one of the six regions the CDR program was in the initial phase at the time of the study and

beneficiaries had not received any benefits yet. For this reason, we excluded this region from all analyses in the paper. Table A2 in Appendix 2 gives details about the composition of the experimental sessions that we include in the analysis.

To select the beneficiaries and non-beneficiaries, we selected local organizations that received funding from the CDR programs (for beneficiaries) and did not receive such funding (for non-beneficiaries). The list of funded organizations was provided to us by the CDR programs. A list of non-funded local organizations was provided by the municipal authorities. From the latter list, in each municipality we chose 3 or 4 organizations that worked in different neighborhoods than the organizations funded by the CDR programs (to avoid contamination). The president/director of each of selected funded and non-funded organizations provided us with a list of their members, from which the research team randomly selected the participants for our study. Participants selected from organizations funded by the CDR programs were classified as ‘beneficiaries’, while participants from organizations without such funding were classified as ‘non-beneficiaries’.

Sessions were organized in local schools, community centers or any other space that was available to us and was of easy access for everybody participating in the experiments. We facilitated the transportation for people coming from rural areas. Each session had the same structure. It started by welcoming participants and verifying they were in the list of participants for that session. During the introduction of the experiments, participants were told that some of them were beneficiaries of CDR projects and others were not, but they were never told who was a beneficiary and who was not. Participants were asked to seat in a semi-circle away from each other and they were asked to remain silent.

The experimenter read the instructions aloud, using visual depictions on posters to help explain the different decision-making steps. Additionally, for all sessions the experimenter provided the same examples to maintain consistency among experimental sessions. During the communication stage all participants were free to walk around. As a consequence, they

could freely decide with whom to talk. This led to several subgroups of participants talking to each other, which varied in size. Nobody was forced to talk to others, and some individuals decided not to talk with any other participants. Some participants actively contributed during the communication stage, while others just listened.

After the experiments, participants were asked questions in private about their social ties with others in the session. In particular, they were told the names of each of the other subjects in the session and for each of them they were asked whether they knew them and whether they considered them to be acquaintances. Then, they were asked further questions to collect basic socio-economic characteristics. Thereafter, they were paid the money they had earned during the experiments. Average earnings from the public goods game were 3,054 pesos in the public goods game before communication and 3,232 pesos after communication.⁹ Earnings were paid in cash at the end of the experiment. In addition, lunch and snack were provided.¹⁰

3. ANALYSES

We start this section with descriptive statistics of important socio-economic characteristics of the participants, after which we analyze the average effect of communication on collective action. We then turn to communication effects and examine how these interact with the beneficiary status of the participants. Thereafter, we investigate whether social proximity via gender or social ties influences the strength of the identified communication effects.

(a) Descriptive statistics

Table 1 presents descriptive statistics of important socio-economic characteristics of the participants by beneficiary status. We do not find any statistically significant differences on age, wealth, gender, secondary education and years of residence in the area. We do find

important differences on urban residence and whether the subjects were displaced, with beneficiaries having a lower likelihood of living in an urban area and a higher likelihood of being displaced. Both differences are the result of CDR programs targeting displaced people (with displacement having affected rural areas more than urban areas).

Insert Table 1 here

Table 1 also reports network densities of non-beneficiaries and beneficiaries. To determine whether someone has a social tie with one of the other participants in the session we symmetrize the social network data, and use the so-called OR ties. In other words, a tie between i and j is assumed to exist if i or j reported having a social tie with the other.¹¹ For each participant ('ego') we calculate a network density, defined as the proportion of possible ties with others ('alters') from the session with whom they have a social tie. For example, *Acquaintance – all* indicates the acquaintance network density among all those in the session (i.e., the proportion of all people in the session with whom ego has an acquaintance). *Acquaintance – non-beneficiary* indicates the proportion of all non-beneficiaries with whom ego has an acquaintance, and *Acquaintance – beneficiary* indicates the proportion of all beneficiaries with whom ego has an acquaintance.

We do not find important differences in average densities of the acquaintance networks between beneficiaries and non-beneficiaries. Non-beneficiaries and beneficiaries have on average acquaintance relations with 26.55% and 26.82% of the people in their experimental session, respectively.

However, disaggregating network size by beneficiary status (see the different rows in Table 1) we find substantial sorting. Non-beneficiaries have more acquaintance ties with other non-beneficiaries than do beneficiaries. They have acquaintance ties with 33.57% of the non-beneficiaries in their session, while beneficiaries have such ties with only 19.75% of the

non-beneficiaries, respectively. Similarly, beneficiaries have more acquaintance ties with other beneficiaries than non-beneficiaries have. They have acquaintance ties with 35.12% of the beneficiaries in their session, while non-beneficiaries have such ties with only 19.52% of the beneficiaries, respectively. Based on a t-test these differences are statistically significant.

(b) The effect of communication

To assess the overall effect of communication on collective action we compare the proportion of co-operators between the first and second rounds. In Table 2, we observe that in the first round (that is before having had any communication) the percentage of co-operators is 63.34%, whereas after communication this increases to 74.37%. Applying a McNemar's χ^2 test we find that the proportion of cooperators is significantly higher after communication ($\chi^2 = 66.34$, two-sided $p = 0.000$; $N = 1623$).

Insert Table 2 here

While the net effect is positive, we also identify a considerable proportion of participants who move from cooperation to defection after communication. This suggests more complex adaptation behavior. In total, 29.76% of the participants change their decision in round 2: 20.39% move to cooperation after having defected and 9.37% change from cooperation to defection.

Insert Figure 1 here

At the session level we observe a similar shift towards higher proportions of cooperators after communication, as can be seen in Figure 1, which plots the proportion of cooperators

per session for the first and second round.¹² In most sessions the proportion of cooperators increases after communication (dots above the 45 degree line). Of particular importance is the large variation in the proportion of cooperators in round 1 across the sessions, which allows us to study its effect on individual cooperation decisions.

Insert Table 3 here

Table 3 presents the proportion of cooperators in both rounds by beneficiary status of the decision-maker. We observe that in both subsamples cooperation increases in round 2. Using a paired sample test of proportions we find that the increase is statistically significant for beneficiaries (two-sided $p = 0.000$) and non-beneficiaries (two-sided $p = 0.000$). Comparing cooperative behavior between beneficiaries and non-beneficiaries we cannot reject the null hypothesis of equal proportions of cooperators before communication (two-sided $p = 0.482$) and after communication (two-sided $p = 0.466$).¹³

(c) Econometric specification

It is likely that the observed increase in cooperation after communication is the result of a peer effect, but to identify such an effect we need to correlate individual decisions to the proportion of cooperators before communication. In particular, through communication people learn about others' behavior from round 1, change their beliefs about how cooperative others will be in round 2, and then are more likely to cooperate in round 2 the higher the proportion of cooperators in round 1. As specified in the hypotheses, we expect such an effect to interact with the beneficiary status of the participants and their social proximity. To test these hypotheses we estimate the following probit regression:

$$Pr(y_{i,t=2} = 1) = \Phi \left(\beta_0 + \beta_1 \sum_{j \neq i}^{N-1} y_{j,t=1} (N-1)^{-1} + \beta_2 X_i + \varepsilon_{i,t=2} \right)$$

with $y_{i,t=2}$ equal to 1 if individual i contributes to the public good in the second round of the binary public goods game, zero otherwise, Φ being the cumulative normal distribution, and $\varepsilon_{i,t=2}$ the error term. As main explanatory variable we use $\sum_{j \neq i}^{N-1} y_{j,t=1} (N-1)^{-1}$, which measures the proportion of the subjects in a session (excluding subject i) who cooperated in round 1 (i.e. before the communication stage). The larger this proportion the more likely it is that during the communication stage subject i receives information about others' cooperation in round 1 as well as others' commitment to cooperate in round 2, which through a conditional cooperator effect may then stimulate subject i to cooperate as well.

Note that we use the lagged proportion of cooperators instead of the contemporaneous proportion of cooperators. This avoids the common 'reflection problem' that complicates the identification of social interactions (Manski, 1993). For this to work, the two following conditions need to be fulfilled. First, the appropriate lag length needs to be used (Manski, 2000: 129), which is not an issue in our study as we constrain social interaction to one specific period. Second, we need to be sure that we are not in the steady-state, i.e. that a sufficient number of people change their decision over time (Clark and Loheac, 2007). This condition is fulfilled as around 30% of the participants change their decision between rounds 1 and 2 (see previous discussion of Table 4).

X_i is a vector of important individual characteristics, such as gender, age, education, wealth, whether their residence is in an urban area, the number of years that the participant has lived at the current residence, whether the participant was displaced, and whether the participant is a beneficiary of a CDR program. We also control for the region where the participant lives, distinguishing five different areas.¹⁴

To test the previously discussed hypotheses we study in a further analysis whether the influence of the cooperative behavior of peers depends on the beneficiary status of these peers. To do so, we replace the proportion of subjects who cooperated in round 1 with the proportion of subjects who cooperated in round 1 and are beneficiaries, and the proportion of subjects who cooperated and who are not beneficiaries, respectively. More specifically we replace the term $\sum_{j \neq i}^{N-1} y_{j,t=1} (N-1)^{-1}$ with the terms $\sum_{j \neq i}^{n_1} y_{j,b=1,t=1} (N-1)^{-1}$ and $\sum_{j \neq i}^{n_0} y_{j,b=0,t=1} (N-1)^{-1}$, with $b = 1$ if a subject is a beneficiary and n_1 and n_0 being the number of beneficiaries and non-beneficiaries in an experimental session, excluding subject i .

In a similar way, we test whether communication effects depend on the social proximity between the peers and the decision-maker. More specifically, we use the terms $\sum_{j \neq i}^{s_1} y_{j,s=1,t=1} (N-1)^{-1}$ and $\sum_{j \neq i}^{s_0} y_{j,s=0,t=1} (N-1)^{-1}$, with $s = 1$ if subjects i and j are ‘socially proximate’ and s_1 and s_0 being the number of subjects within the experimental session with whom subject i is socially proximate or not, respectively. We will use two different dimensions of social proximity. In a first analysis we will consider people of the same gender to be socially proximate, and in a further analysis we will use reported social ties among participants to determine whether or not they are socially proximate. Finally, we will also look at the combination of social proximity and beneficiary status to test the interaction between both. To do this, we will use proportions that take account of both beneficiary status and social proximity. Descriptive statistics of the different proportions used in the regressions can be found in Table A5 in the appendix.

An important advantage of our experimental approach is that social interaction is limited to the participants’ group which is the same for all participants in a session. As a result, the potential peer group is the same for all participants in a session. This avoids a common problem with studies on peer effects, which often have to make ad-hoc decisions about the identification of the reference group. Rather than making a decision about which peers are

most important, we can infer differences in peer influences from the experimental data, by testing how peer effects depend on the beneficiary status of the peers and the social proximity between decision-maker and peers, in terms of pre-existing social ties and whether they have the same gender.¹⁵

(d) Peer effects and beneficiary status

Table 4 reports the marginal effect of the proportion of cooperators in round 1 in probability terms, using the regression equation specified above (for the results of the probit regression used to calculate these results, see Table A6 in the appendix). The results of the pooled sample (Model 1) demonstrate that the proportion of cooperators before communication has a statistically significant positive effect on the likelihood of cooperation after communication. A one percentage point increase in the proportion of cooperators increases the likelihood of cooperation by a 0.6 percentage point. To allow a comparison along beneficiary status of the decision-maker, we also run the same regression for beneficiaries and non-beneficiaries separately. Comparing the results of the different subsamples we find that the peer effects exert an equally strong influence on beneficiaries and non-beneficiaries (Models 3 and 5).¹⁶

Beneficiaries and non-beneficiaries may also differ as to the strength of the influence they exert on others. To test this, we disentangle the behavior of beneficiaries and non-beneficiaries in round 1. As before, we also test whether peers exert a different effect on beneficiaries and non-beneficiaries, by running the same regression on different subsamples. The marginal probabilities of these regressions are presented in Models 2, 4 and 6.

Insert Table 4 here

We observe that the coefficients of the proportion of participants that cooperated and are beneficiaries are positive and statistically significant in all models. It is slightly higher in the pool of non-beneficiaries (Model 4) than in the pool of beneficiaries (Model 6). As to the effect of non-beneficiaries, only in the beneficiary subsample is the coefficient of the proportion of participants that cooperated and are non-beneficiaries (marginally) statistically significant. Based on a χ^2 test we reject the null hypothesis that the coefficients of both proportions (as reported in Table A5) are the same for the pooled sample (two-sided $p = 0.010$) and for the sample of non-beneficiaries (two-sided $p = 0.030$), but not for the sample of beneficiaries (two-sided $p = 0.205$). We summarize the main observations in a first result.

Result 1. The likelihood of cooperation after communication increases with the proportion of participants who cooperated before communication. This effect is driven by the cooperation behavior of program beneficiaries.

(e) Peer effects and social proximity in gender

As explained before, it is important to study whether peer effects interact with social proximity among the peers. To do so, we include gender differences in the analysis by distinguishing between cooperators of the same gender and different gender. Table 5 presents the marginal probabilities of these regressions (for the results of the probit regression used to calculate these results, see Table A7 in the appendix).

Insert Table 5 here

In the pooled sample (Model 1) we find that the marginal effects of *cooperators Rdl (same gender)* and *cooperators Rdl (different gender)* are both statistically significant. Based

on a χ^2 test, however, we reject the null hypothesis that both coefficients are the same (two-sided $p = 0.038$). People are more influenced by participants of the same gender than by participants of different gender.

Looking at the marginal effects in the subsamples (Models 3 and 5), we observe that cooperators of the same gender exert a stronger influence on non-beneficiaries than cooperators of different gender, while these effects tend to be similar for beneficiaries. A χ^2 test confirms that the marginal probabilities are statistically different from each other in the non-beneficiary sample (two-sided $p = 0.002$), while it does not allow us to reject the null hypothesis of similar effects within the sample of beneficiaries (two sided $p = 0.632$).

To test whether the gender differences in peer effects interact with the beneficiary status of the peers we disentangle the behavior of beneficiaries and non-beneficiaries in round 1. To do so, we include the proportion of participants that cooperated in round 1 and are 1) of same gender and beneficiaries; 2) different gender and beneficiaries; 3) same gender and non-beneficiaries; 4) different gender and non-beneficiaries. Models 2, 4 and 6 present the marginal probabilities for the pooled sample and the subsamples of non-beneficiaries and beneficiaries, respectively.

In Model 2 we find that the difference in peer effects of beneficiaries and non-beneficiaries only exists with peers of different gender but not with peers of same gender. Based on a χ^2 test, the coefficients of *Cooperators Rd1 (same gender – beneficiaries)* and *Cooperators Rd1 (same gender – non-beneficiaries)* are not statistically different from each other (two-sided $p = 0.415$), while the difference between the coefficients of *Cooperators Rd1 (different gender – beneficiaries)* and *Cooperators Rd1 (different gender – non-beneficiaries)* is statistically significant (two-sided $p = 0.003$). Similarly, we observe that the gender difference in peer effects is only present with peers who are non-beneficiaries, but not with peers who are beneficiaries. This is confirmed by a χ^2 test. While the coefficients of

Cooperators Rdl (same gender – beneficiaries) and *Cooperators Rdl (different gender – beneficiaries)* are not statistically different from each other (two-sided $p = 0.488$), the coefficients of *Cooperators Rdl (same gender – non-beneficiaries)* and *Cooperators Rdl (different gender – non-beneficiaries)* are statistically different (two-sided $p = 0.004$).

A comparison across the subsamples tells us whether the results are similar to the results of the pooled sample and whether the results of the pooled sample might be driven by one of the subsamples. We find the same effects in the samples of beneficiaries and non-beneficiaries as in the pooled sample. We summarize these observations in a second result.

Result 2. Peers of the same gender exert a stronger influence on cooperative behavior than peers of different gender. This difference is present among peers who are non-beneficiaries, but absent among peers who are beneficiaries. The difference in peer effects between beneficiaries and non-beneficiaries is only present among peers of different gender.

(f) Peer effects and social proximity via social ties

Thus far, we included social proximity in the analysis by looking at whether participants have the same gender. Another dimension along which social proximity could be measured is pre-existing social ties among the participants. To investigate whether and how social ties interact with peer effects we disentangle the influence of the cooperative behavior of participants one is acquainted with from the influence of the cooperative behavior of participants one is not acquainted with. As peer effects may be different for beneficiaries and non-beneficiaries, we again run the same regression for different subsamples. Table 6 reports the marginal probabilities, using the regression results reported in Table A8 in the Appendix.

In the pooled model (Model 1) we observe that the coefficients of *Cooperators Rdl (acquaint)* and *Cooperators Rdl (non-acquaint)* are positive and statistically significant. The first coefficient is larger than the second and based on a χ^2 test their difference is

(marginally) significantly different from zero (two-sided $p = 0.077$). Looking at the estimates in the different subsamples (Models 3 and 5) we find that both coefficients are again positive and statistically significant. The difference between both coefficients, however, is not statistically significant in both subsamples (two-sided $p > 0.244$).

To investigate potential interactions between beneficiary status and social ties, we distinguish between the beneficiary status and whether peers are acquainted. To do so, we include the proportion of participants that cooperated in round 1 and are 1) acquaintances and beneficiaries; 2) non-acquaintances and beneficiaries; 3) acquaintances and non-beneficiaries; 4) non-acquaintances and non-beneficiaries. Models 2, 4 and 6 in Table 6 present the marginal probabilities of these proportions for the pooled sample and the subsamples, respectively.

Insert Table 6 here

With the pooled sample (Model 2) we find that all marginal probabilities are statistically different from zero, except for the proportion of cooperators who are non-beneficiaries and non-acquainted. We observe that the marginal probabilities do not strongly differ between peers who are beneficiaries and acquaintances, and peers who are beneficiaries and non-acquaintances. Similarly, we do not observe strong differences in peer effects between non-beneficiaries who are acquaintances and non-beneficiaries who are not acquaintances. This is confirmed by a χ^2 test. Comparing the first two coefficients, we cannot reject the null hypothesis of zero difference (two-sided $p = 0.640$), and neither can we do that when comparing the last two coefficients (two-sided $p = 0.172$).

The difference between the second and fourth coefficients, however, is statistically different from zero (two-sided $p = 0.036$), but not the difference between the first and the third coefficients (two-sided $p = 0.157$). This indicates that differences in peer effects

between beneficiaries and non-beneficiaries are mainly present with non-acquainted peers, but less so with acquainted peers.

Looking at the regression results of the subsamples, we identify the following effects. In the sample of non-beneficiaries (Model 4), the peer influence of non-beneficiaries is stronger among acquaintances than among non-acquaintances (two-sided $p = 0.033$). The peer effect of non-acquaintances who are beneficiaries is stronger than the effect of non-acquaintances who are non-beneficiaries (two-sided $p = 0.020$). In the sample of beneficiaries (Model 6), we find that the peer effect of beneficiaries is stronger among acquaintances than among non-acquaintances (two-sided $p = 0.020$). Also the peer effect of acquaintances who are beneficiaries is stronger than the effect of acquaintances who are non-beneficiaries (two-sided $p = 0.005$). We summarize these new observations in a third result.

Result 3. Among non-beneficiaries, the stronger peer effect of beneficiaries (compared to non-beneficiaries) is present among peers who are not acquainted. Among beneficiaries, the stronger peer effects of beneficiaries are present among peers who are acquainted.

4. CONCLUSION

In this article, we used data from a lab-in-the-field experiment in Colombia to investigate the potential impact of CDR programs in terms of increased social cohesion beyond the original beneficiaries of these programs. For this, we analyzed peer effects in collective action and how it relates to participation in a CDR program. Our results indicate that CDR programs have an interesting potential to make cooperation spill-over from their participants ('beneficiaries') to the non-participating part of the communities in which they operate. More specifically, we found that non-beneficiaries are more strongly influenced by beneficiaries than by non-beneficiaries, and that this difference in peer effects is stronger among people who are socially more distant from each other, either because they are of different gender or

they do not know each other. These results suggest that CDR beneficiaries exert an important peer influence in settings characterized by low social cohesion, which is an important observation, given the objective of CDR programs to increase social cohesion.

The following notes are needed when interpreting this evidence in the light of the potential of CDR programs to make cooperative behavior spill-over to the part of the community that does not participate in CDR programs. First, while our results confirm that CDR programs strengthen peer effects in collective action, this does not necessarily mean that they have an overall positive effect on collective action in the communities where they operate. Whether collective action extends beyond the initial beneficiaries of CDR programs will mainly be driven by the initial cooperation levels of the beneficiaries. As people are more likely to be influenced by the behavior of beneficiaries than of non-beneficiaries the initial cooperation decisions beneficiaries make are crucial. The larger the proportion of them that decides to cooperate, the more likely that this will push the whole group towards collective action.

Second, as explained in the theoretical section, for peer effects to take place communication is needed and information about future cooperation needs to be conveyed during such communication. As we do not have any transcripts of the conversations it is difficult to tell which of both mechanisms is responsible for the identified peer influences. However, given the fact that we do not observe any difference in cooperative behavior between beneficiaries and non-beneficiaries *before* communication (they are both equally cooperative), it is more likely the structure of communication than the information conveyed that drives the identified peer effects.

While we do not know who talked to whom, the inclusion of social proximity (along acquaintance ties and gender) in the analysis allows us to obtain more detailed insight into the structure of communication. More specifically, we distinguish two forms of communication: i) bilateral communication, and ii) public communication. In addition, we assume that the

first type of communication is more strongly influenced by pre-existing social proximity (along acquaintance ties and gender), as people who know each other or have the same gender, more likely approach each other to directly communicate with each other during the communication stage. Public communication, on the other hand, might be more strongly influenced by beneficiary status to the extent that participation in CDR programs makes participants more used to public debate/communication. As we control for social distance in the analysis, the stronger peer effects exerted by CDR beneficiaries are most likely the result of their larger involvement in public communication. This is also consistent with the observation that the peer effect of beneficiaries on non-beneficiaries is stronger with lower social proximity. Lower social proximity decreases the likelihood of direct communication of non-beneficiaries with others, and as a result makes them more exposed to the influence of public communication.

Finally, in our analysis we did not distinguish the different types of projects funded. Any attempt to study CDR programs or projects must inherently be limited to examining the non-standardized pool of projects adopted across the communities and projects, which will vary across time and space because of the very nature of the policy process. We mimic previous literature by averaging across the pool of projects in each community. Pooling the diversity of projects, our results should be interpreted as an average for this pool of projects. Future research that takes account of the variety of types of projects would be very valuable. Also, it should be noted that our results are based on the pool of projects that were supported by the five first CDR programs in Colombia. At the moment, more than 17 programs are in function and with the continuation of the peace process more programs and projects will be created. To the extent that the pool of projects in our analysis is representative for new projects and programs that are developed in Colombia or in other countries, our results could be extrapolated across place and time.

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Table 1. *Descriptive statistics*

	Non-beneficiary		Beneficiary		p-value
	Mean/ Percent	N	Mean/ Percent	N	
Age	41.21	856	40.32	762	0.189
Wealth	-0.063	826	-0.202	728	0.213
Male	44.98%	856	47.77%	762	0.261
Secondary education ^a	41.36%	856	39.51%	767	0.448
Years at Residence	20.35	856	20.33	762	0.988
Urban	61.75%	855	48.95%	762	0.000***
Displaced	23.13%	856	31.63%	762	0.000***
Acquaintance – all	26.55%	856	26.82%	767	0.773
Acquaintance – non-beneficiary	33.57%	856	19.75%	767	0.000***
Acquaintance – beneficiary	19.52%	856	35.12%	767	0.000***

Notes. A t-test and a proportion test were used for continuous and dichotomous variables, respectively. ^a Equal to 1 if respondent started secondary education, but not necessarily completed it. Significance levels (two-sided): *=10%, **=5%, ***=1%. The wealth score is equal to the first factor of a principal component analysis using different types of asset holdings, such as the types of goods each household owns (e.g. television, radio, car), self-reported income, and the size of their home. Details on the wealth score can be found in Table A3 in the Appendix. Network sizes are always calculated as proportions relative to the total number of participants, of the particular type (either all, beneficiary, or non-beneficiary) in each session.

Table 2. *Cooperation/defection before and after communication*

		<i>After communication</i>		
		Defection	Cooperation	Total
<i>Before communication</i>	Defection	16.27%	20.39%	36.66%
	Cooperation	9.37%	53.97%	63.34%
	Total	25.63%	74.37%	100.00%

Note. N = 1623

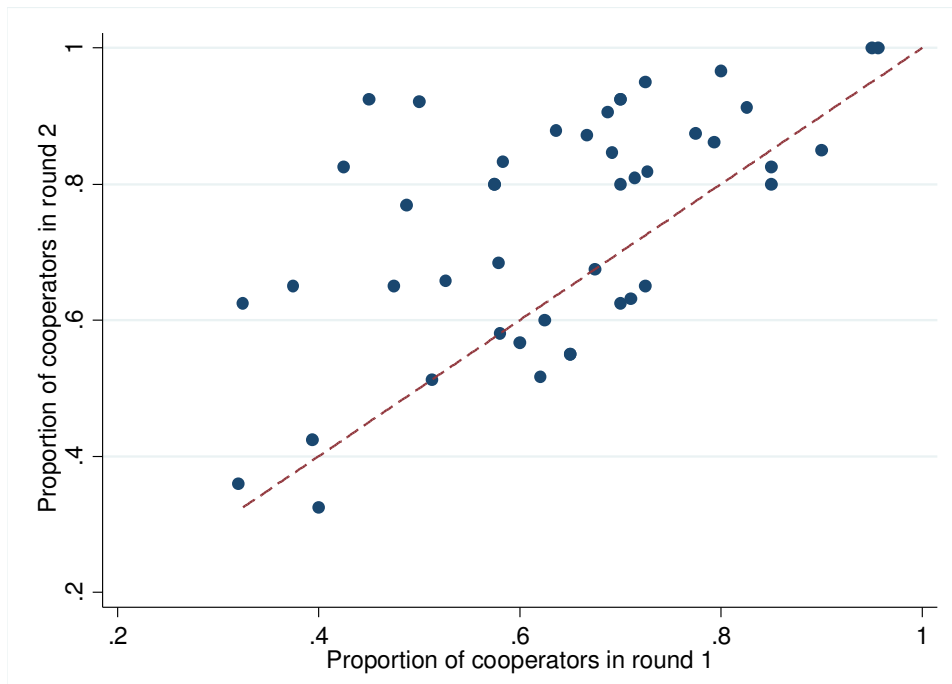


Figure 1. *Variation in proportion of cooperators*

Table 3. *Proportion of co-operators before and after communication*

	round 1	round 2	N
Non-beneficiary	64.14%	75.12%	856
Beneficiary	62.45%	73.53%	767

Table 4. *Peer effects by beneficiary status*

	Pooled (N = 1554)		Non-beneficiary (N = 826)		Beneficiary (N = 728)		
	(0) ^a	(1)	(2)	(3)	(4)	(5)	(6)
Cooperators Rd1 (all)	0.540*** (0.141)	0.619*** (0.147)	- -	0.619*** (0.239)	- -	0.621*** (0.172)	- -
Cooperators Rd1 (beneficiaries)	-	-	0.894*** (0.167)	-	0.989*** (0.256)	-	0.798*** (0.257)
Cooperators Rd1 (non-beneficiaries)	-	-	0.266 (0.231)	-	0.197 (0.366)	-	0.387* (0.210)

Notes. Probit regression. Marginal probabilities reported at sample means of covariates. Controls used for gender, age, education, wealth, urban area, years at residence, displaced and region. Standard errors (between brackets) clustered at the session level. Significance levels (two-sided): *=10%, **=5%, ***=1%. ^a Model without controls with N = 1623.

Table 5. *Peer effects by gender and beneficiary status*

	Pooled (N = 1554)		Non-beneficiary (N = 826)		Beneficiary (N = 728)	
	(1)	(2)	(3)	(4)	(5)	(6)
Cooperators Rd1 (same gender)	0.776*** (0.153)	-	(0.836)*** (0.243)	-	0.720*** (0.160)	-
Cooperators Rd1 (different gender)	0.566*** (0.157)	-	0.445* (0.255)	-	0.665*** (0.192)	-
Cooperators Rd1 (same gender – beneficiaries)	-	0.827*** (0.212)	-	0.937*** (0.301)	-	0.782*** (0.287)
Cooperators Rd1 (different gender – beneficiaries)	-	1.012*** (0.214)	-	0.902*** (0.308)	-	1.101*** (0.300)
Cooperators Rd1 (same gender – non-beneficiaries)	-	0.577** (0.240)	-	0.611* (0.366)	-	0.510** (0.219)
Cooperators Rd1 (different gender – non-beneficiaries)	-	0.103 (0.236)	-	-0.029 (0.385)	-	0.215 (0.240)

Notes. Probit regression. Marginal probabilities reported at sample means of covariates. Controls used for gender, age, education, wealth, urban area, years at residence, displaced and region. Standard errors (between brackets) clustered at the session level. Significance levels (two-sided): *=10%, **=5%, ***=1%.

Table 6. *Peer effects by social ties and beneficiary status*

	Pooled (N = 1554)		Non-beneficiary (N = 826)		Beneficiary (N = 728)	
	(1)	(2)	(3)	(4)	(5)	(6)
Cooperators Rd1 (acquaint)	0.857*** (0.189)	-	0.824*** (0.286)	-	0.860*** (0.269)	-
Cooperators Rd1 (non-acquaint)	0.637*** (0.154)	-	0.652*** (0.243)	-	0.638*** (0.162)	-
Cooperators Rd1 (acquaint – beneficiaries)	-	0.995*** (0.228)	-	0.649*** (0.350)	-	1.385*** (0.301)
Cooperators Rd1 (non-acquaint – beneficiaries)	-	0.899*** (0.192)	-	1.031*** (0.255)	-	0.666** (0.270)
Cooperators Rd1 (acquaint – non-beneficiaries)	-	0.580** (0.265)	-	0.715* (0.397)	-	0.097 (0.381)
Cooperators Rd1 (non-acquaint – non-beneficiaries)	-	0.337 (0.224)	-	0.162 (0.365)	-	0.406** (0.196)

Notes. Probit regression. Marginal probabilities reported at sample means of covariates. Controls used for gender, age, education, wealth, urban area, years at residence, displaced and region. Standard errors (between brackets) clustered at the session level. Significance levels (two-sided): *=10%, **=5%, ***=1%.

APPENDIX 1: INSTRUCTIONS AND PROCEDURES

During this exercise you will be part of an exercise where the XX people in this room will make decisions in an individual, simultaneous and quiet manner.

Starting each round you will have a token available to invest (deposit) into two possible alternatives or accounts: A Private Account (individual) or a Group Account.

According to your choices and the choice of others you may obtain an amount of Money.

Your profits will be calculated as follows:

You earn \$2000 when you invest (deposit) your token in the Private Account

For each token you and/or the others invest (deposit) in the Group Account, you and each one of the participants will get \$100.

Each one of you will decide by yourself, without telling your neighbor or talking, where you will invest your token: in the Private Account or in the Group Account. Now we will explain how decisions are made. To make the decisions each one of you will be given cards like this. (*Show the two cards on both sides. Explain that each card has the participant's number and the round number.*)

One of the cards has on one side "MY TOKEN" written and the other side is empty. Remember, you must decide if you want to invest your token in the Group Account or in the Private Account. If you want to invest your token in the Private Account you must keep the card marked "MY TOKEN", but if you want to invest your token in the Group Account you must give that card to the Group Account. A moderator will go by each one of you with a bag while you are seated, then you must choose if you want to keep the token or put it in the bag, the Group Account. If you want to keep your token, that means, to invest in the Private Account, you keep the card that says "MY TOKEN" and deposit in the bag the empty card, the one with nothing written on it. On the contrary, if you want to invest in the Group Account, put in the bag the card that says "MY TOKEN" and keep the empty card, with

nothing on it. Once each one of you puts in your preferred card, the moderators will count how many tokens were invested in the group account, that is to say, they will count how many “MY TOKEN” cards are inside the bag.

Remember, everyone in the group will benefit equally from the tokens invested in the Group Account, but only you will benefit from the token invested in the Private Account. That is to say, if you put the card that says “MY TOKEN” in the bag, all participants will benefit from your token, but if you keep that card and put the empty one in the bag, nobody but you will benefit from it, because you choose to keep your token, the card that says “MY TOKEN”.

We will do a few examples to see how this exercise works. Please pay really close attention. *(Show which card must be put in the bag for each case.)*

Let's assume that there are 40 people in this group. Let's suppose that you decided to keep your token. When the moderator comes by you put in the bag the empty card and keep the one marked with “MY TOKEN”. Once all the participants have made their choice, the moderator will count how many “MY TOKEN” cards are inside the bag. The moderator then announces to everyone that there were 20 “MY TOKEN” cards. That is to say that 20 people decided to put their token in the Group Account and the other 20 decided to keep the token. The profits of the group account are 2000 ($20 * 100$) pesos for each person. Since you had decided to keep your token you earned on this round \$2000 from your token plus the \$2000 pesos from the Group Account, that to say you earned \$4000 pesos.

Now let's think of this same example but instead of calculating the earnings of someone who kept the token, let's think of someone who gave his/her token to the Group Account. The person who decided to give the token to the Group Account earns the \$2000 pesos profits from the Group Account and has no earnings from the Private Account.

Now we will do another example. Let's suppose that you decided to invest your token in the Group Account, which means you put in the bag the card marked with “MY TOKEN”.

After all participants have made their choices, the moderator will count how many “MY TOKEN” cards are there in the bag. The moderator announces that there were 30 cards marked with “MY TOKEN” in the bag. This means that 10 people decided to keep their token. The profits from the Group Account are 3000 pesos ($30 * 100$) for each person. Since you had chosen to put your token in the Group Account, you earn \$3.000 pesos in this round.

Now let’s think of this same example, but instead of calculating the earnings of the person that gave his/her token to the Group Account, let’s do it for one that kept his/her token. That person earned \$3000 pesos from the Group Account plus \$2000 from the Private Account, so this person earned \$5000 pesos.

Are there any questions? Shall we begin with the exercise? All decisions are private and are not to be consulted with anyone. Do not talk please.

Let us begin with the first round of the exercise. The moderators are giving you the cards. Remember you will keep one and put the other one in the bag. **Please do not lose the card you did not put in the Group Account.**

Now we will begin the second round. In the second round, each one of you will have the possibility of communicating with one or more participants of the group before deciding how to invest your token this round. This communication is absolutely voluntary. You have **10** minutes to communicate. Once the allowed time is up, all communications should be suspended and we will proceed to making the individual decision of investing your token either in the Private Account or in the Group Account. These choices will continue to be private and confidential. Please keep your second round card safe, since we will use them to make the final calculation.

APPENDIX 2: ADDITIONAL ANALYSES

Table A1. *Information about CDR program regions included the study*

Program	Departments	Municipalities	Date when it started
Programa de Desarrollo y Paz del Magdalena Medio (PDMM)	Antioquia, Bolívar, Cesar y Santander	29	1995
Programa de Desarrollo para la Paz del Oriente Antioqueño (PRODEPAZ)	Oriente Antioqueño, Nordeste y Magdalena Medio Antioqueño	28	1999
Programa Nueva Sociedad de la Región Nororiental de Colombia (CONSORNOC)	Norte de Santander	40	1999
Corporación de Desarrollo para la Paz del Piedemonte Llanero (CORDEPAZ)	Orinoquia	29	1999
Programa de Paz y Desarrollo y Laboratorios de Paz en la región del Macizo Colombiano	Nariño y Cauca	25	1999
Fundación Red Desarrollo y Paz de los Montes de María (FRDPMMa)	Sucre y Bolívar	15	2003

Source. Based on information from García & Sarmiento (2002)

Table A2. *Number and proportion of beneficiaries and non-beneficiaries by session*

	mean	std.dev.	min	max
Number of participants	36.07	5.71	21	40
Number of beneficiaries	17.04	4.06	7	24
Number of non-beneficiaries	19.02	4.90	5	28
Proportion of beneficiaries	0.48	0.10	0.24	0.76

N = 45

Table A3. *Wealth score*

<i>Wealth Score</i>	Wealth Quintile				
	Poorest	Second	Third	Fourth	Richest
	-2.389	-1.415	-0.535	0.655	3.684
Own House?	0.677	0.627	0.667	0.675	0.690
Rooms in House	2.058	2.394	2.748	3.234	4.152
Bedrooms	1.562	1.843	2.045	2.270	2.780
Telephone	0.018	0.079	0.113	0.370	0.667
Mobile Phone	0.354	0.680	0.801	0.848	0.945
Refrigerator	0.034	0.218	0.486	0.740	0.955
Audio System	0.105	0.234	0.381	0.512	0.751
Laundry Machine	0.000	0.003	0.005	0.055	0.454
Oven	0.000	0.003	0.008	0.031	0.228
Sewing Machine	0.010	0.042	0.092	0.160	0.325
Black and White TV	0.239	0.260	0.121	0.071	0.037
Color TV	0.121	0.388	0.646	0.874	0.966
DVD or VHS	0.000	0.008	0.042	0.123	0.396
Computer	0.000	0.000	0.005	0.008	0.239
Bike	0.110	0.249	0.375	0.475	0.638
Motorbike	0.003	0.016	0.037	0.094	0.304
Car	0.000	0.003	0.005	0.029	0.144
Moto-Taxi	0.000	0.000	0.000	0.010	0.021
Annual Earnings from Home (10,000 pesos)	14.005	19.780	28.722	36.616	79.003
Last Week Food Expenditure (10,000 pesos)	3.554	4.775	5.453	6.905	9.949
Last Month Frequency of Food Shortage	1.819	2.226	2.404	2.648	2.990
Last Year Frequency of Savings	3.971	3.911	3.845	3.819	3.423
Male	0.554	0.472	0.488	0.428	0.428
Age	45.730	41.885	40.675	39.790	36.556
Education	2.100	2.879	3.202	3.861	5.514
Urban	0.315	0.436	0.522	0.656	0.816
Displaced	0.373	0.367	0.299	0.197	0.123
Household Size	4.827	5.283	5.391	5.344	5.052
No. School Age Children	1.433	1.559	1.564	1.559	1.270

Notes. The PCA Wealth Score is an index of the assets held by the individual recorded in the post-experiment survey. Data on a variety of assets was collected for each subject. We developed a wealth asset index by performing principal components analysis. The index is formed by using the first principle component scores. This table reports means for each component used in constructing the index by quintile. For an exposition of PCA applied to asset holdings to form wealth indexes, see Montgomery, Gagnolati, Burke, & Paredes (2000).

Table A4. *Proportion of co-operators before and after communication
(by location and displacement)*

	round 1	round 2	N
<i>a) By location</i>			
Rural – non-beneficiary	65.75%	72.78%	327
Rural – beneficiary	62.73%	72.75%	389
Urban – non-beneficiary	63.07%	76.52%	528
Urban – beneficiary	62.74%	74.53%	373
<i>b) By displacement</i>			
Non-displaced – non-beneficiary	64.74%	75.99%	658
Non-displaced – beneficiary	65.26%	73.51%	521
Displaced – non-beneficiary	62.12%	72.22%	198
Displaced – beneficiary	57.26%	73.86%	241

Table A5. *Descriptive statistics of proportions of cooperators in round 1*

	non beneficiaries (N = 856)		beneficiaries (N = 767)	
	mean	st.dev.	mean	st.dev.
Cooperators Rd1 (all)	0.62	0.15	0.64	0.16
Cooperators Rd1 (beneficiaries)	0.29	0.12	0.30	0.11
Cooperators Rd1 (non-beneficiaries)	0.34	0.10	0.34	0.10
Cooperators Rd1 (same gender)	0.36	0.15	0.36	0.15
Cooperators Rd1 (different gender)	0.28	0.13	0.29	0.14
Cooperators Rd1 (same gender – beneficiaries)	0.15	0.08	0.18	0.09
Cooperators Rd1 (different gender – beneficiaries)	0.14	0.08	0.14	0.08
Cooperators Rd1 (same gender – non-beneficiaries)	0.21	0.10	0.18	0.10
Cooperators Rd1 (different gender – non-beneficiaries)	0.15	0.08	0.16	0.10
Cooperators Rd1 (acquaint)	0.13	0.11	0.14	0.13
Cooperators Rd1 (non-acquaint)	0.43	0.16	0.45	0.18
Cooperators Rd1 (acquaint – beneficiaries)	0.05	0.07	0.08	0.08
Cooperators Rd1 (non-acquaint – beneficiaries)	0.22	0.11	0.18	0.10
Cooperators Rd1 (acquaint – non-beneficiaries)	0.08	0.07	0.06	0.07
Cooperators Rd1 (non-acquaint – non-beneficiaries)	0.20	0.10	0.27	0.12

Table A6. *Peer effects by beneficiary status*

	Pooled (N = 1554)		Non-beneficiary (N = 826)		Beneficiary (N = 728)		
	(0) ^a	(1)	(2)	(3)	(4)	(5)	(6)
Cooperators Rd1 (all)	1.702*** (0.432)	1.974*** (0.453)	- -	2.006*** (0.746)	- -	1.968*** (0.574)	- -
Cooperators Rd1 (beneficiaries)	- -	- -	2.871*** (0.522)	- -	3.245*** (0.811)	- -	2.536*** (0.851)
Cooperators Rd1 (non-beneficiaries)	- -	- -	0.853 (0.733)	- -	0.645 (1.186)	- -	1.228* (0.681)
Constant	-0.401 (0.287)	-0.643 (0.421)	-0.532 (0.445)	-0.821 (0.624)	-0.770 (0.641)	-0.405 (0.514)	-0.318 (0.511)
χ^2	15.500	39.764	68.745	24.967	44.455	39.959	38.031
N	1623	1554	1554	826	826	728	728

Notes. Probit regression. Controls used for region, gender, age, education, urban area, years at residence, displaced and wealth. Standard errors (between brackets) clustered at the session level. Significance levels (two-sided): *=10%, **=5%, ***=1%. ^a Model without controls.

Table A7. *Peer effects by gender and beneficiary status*

	Pooled (N = 1554)		Non-beneficiary (N = 826)		Beneficiary (N = 728)	
	(1)	(2)	(3)	(4)	(5)	(6)
Cooperators Rd1 (same gender)	2.493*** (0.470)	-	2.739*** (0.755)	-	2.292*** (0.538)	-
Cooperators Rd1 (different gender)	1.817*** (0.488)	-	1.458* (0.814)	-	2.117*** (0.637)	-
Cooperators Rd1 (same gender – beneficiaries)	-	2.679*** (0.678)	-	3.106*** (0.997)	-	2.511*** (0.956)
Cooperators Rd1 (different gender – beneficiaries)	-	3.281*** (0.672)	-	2.991*** (0.978)	-	3.535*** (0.996)
Cooperators Rd1 (same gender – non-beneficiaries)	-	1.869** (0.759)	-	2.026* (1.168)	-	1.637** (0.713)
Cooperators Rd1 (different gender – non-beneficiaries)	-	0.334 (0.763)	-	-0.0967 (1.278)	-	0.690 (0.780)
Constant	-0.815* (0.432)	-0.699 (0.451)	-0.950 (0.656)	-0.866 (0.667)	-0.607 (0.512)	-0.494 (0.512)
χ^2	44.306	77.047	36.623	65.812	48.079	47.774
N	1554	1554	826	826	728	728

Notes. Probit regression. Controls used for region, gender, age, education, urban area, years at residence, displaced and wealth. Standard errors (between brackets) clustered at the session level. Significance levels (two-sided): *=10%, **=5%, ***=1%.

Table A8. *Peer effects by social ties and beneficiary status*

	Pooled (N = 1554)		Non-beneficiary (N = 826)		Beneficiary (N = 728)	
	(1)	(2)	(3)	(4)	(5)	(6)
Cooperators Rd1 (acquaint)	2.754*** (0.588)	-	2.688*** (0.880)	-	2.744*** (0.906)	-
Cooperators Rd1 (non-acquaint)	2.046*** (0.483)	-	2.129*** (0.766)	-	2.036*** (0.553)	-
Cooperators Rd1 (acquaint – beneficiaries)	-	3.219*** (0.737)	-	2.149* (1.145)	-	4.463*** (0.984)
Cooperators Rd1 (non-acquaint – beneficiaries)	-	2.908*** (0.621)	-	3.414*** (0.816)	-	2.148** (0.902)
Cooperators Rd1 (acquaint – non-beneficiaries)	-	1.877** (0.844)	-	2.370* (1.256)	-	0.313 (1.232)
Cooperators Rd1 (non-acquaint – non-beneficiaries)	-	1.088 (0.715)	-	0.538 (1.201)	-	1.308** (0.648)
Constant	-0.821* (0.447)	-0.740 (0.463)	-1.014 (0.649)	-0.932 (0.660)	-0.592 (0.529)	-0.403 (0.512)
χ^2	51.919	64.867	26.742	45.952	46.278	64.728
N	1554	1554	826	826	728	728

Notes. Probit regression. Controls used for region, gender, age, education, urban area, years at residence, displaced and wealth. Standard errors (between brackets) clustered at the session level. Significance levels (two-sided): *=10%, **=5%, ***=1%.

Table A9. *Peer effects by beneficiary status*
(with control for predicted cooperation before communication)

	(1)	(2)	(3)	(4)	(5)	(6)
Cooperation Rd 1 (predicted)	-0.59	-0.722	0.118	-0.074	-1.499*	-1.564*
	-0.632	-0.63	-0.885	-0.917	-0.885	-0.911
Cooperators Rd1 (all)	1.873***		1.928***		1.823***	
	-0.443		-0.629		-0.597	
Cooperators Rd1 (beneficiaries)		2.859***		3.064***		2.642***
		-0.48		-0.707		-0.865
Cooperators Rd1 (non-beneficiaries)		0.684		0.567		0.849
		-0.735		-1.014		-0.904
Constant	-0.129	0.076	-0.573	-0.309	0.441	0.574
	-0.457	-0.447	-0.575	-0.586	-0.58	-0.597
chi2	17.931	36.088	11.276	21.011	9.985	10.516
N	1554	1554	826	826	728	728
<i>Marginal probabilities</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
Cooperators Rd1 (all)	0.591***		0.599***		0.583***	
	(0.145)		(0.201)		(0.184)	
Cooperators Rd1 (beneficiaries)		0.896***		0.941***		0.842***
		(0.157)		(0.220)		(0.273)
Cooperators Rd1 (non-beneficiaries)		0.214		0.174		0.270
		(0.233)		(0.314)		(0.285)
chi2	17.931	36.088	11.276	21.011	9.985	10.516
N	1554	1554	826	826	728	728

Notes. Probit regression. Controls used for region, gender, age, education, urban area, years at residence, displaced and wealth. Standard errors (between brackets) clustered at the session level. Significance levels (two-sided): *=10%, **=5%, ***=1%.

NOTES

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- ¹ This relates to a wider debate, in which it is argued that development programs may reproduce and even reinforce divisions within communities (e.g. D'Exelle, 2009; Bastiaensen, De Herdt, D'Exelle, 2005).
- ² The use of laboratory experiments to study communication effects allows full control on the variables of interest. In the field it is impossible to observe behavior before and after communication, in otherwise identical situations. To causally investigate the effect of communication, the controlled environment of the laboratory is very helpful, and has been increasingly used by development economists (for a literature review see Cardenas & Carpenter, 2008) as well as other social scientists.
- ³ For an example of a study that used a similar approach of combining a real intervention with a lab-in-the-field study in Colombia see Attanasio, Polania-Reyes and Pellerano (2015).
- ⁴ This program was created by the petroleum union USO, the petroleum company ECOPETROL, the dioceses of Barrancabermeja and a consortium of two NGOs in a region of Colombia devastated by the conflict and the poverty conditions.
- ⁵ Both the Peace and Development and the Peace Laboratory programs are very similar. However, a main difference is that the projects funded through the World Bank loans focus on displaced families whereas the European Union programs did not have that focus (Guarin, Navarro, & Pellerano, 2008).
- ⁶ We refer to Table A1 in the Appendix for more details about the six regions.
- ⁷ At the time of the experiment 1 dollar was equal to 1.833 pesos. In each experimental session we conducted a trust game, a dictator game vis-à-vis a public service, and a public good game. For the purpose of this paper we only report the results from the public good game. For more information on the other games see Coleman & Lopez (2012).
- ⁸ As communication is non-binding and does not change the dominant strategy economic theory predicts that such 'cheap talk' will not change individual behaviour (Farrell & Rabin, 1996: 113). This contrasts strongly with the extensive experimental evidence in support of a positive effect of non-binding communication on collective action (Sally, 1995).
- ⁹ In the full workshop, which also included the other experimental games and lasted about four hours, participants earned between 3,000 and 21,400 pesos with an average of 10,477 pesos (about US\$4.55). This amount was about one day's wage in these regions.
- ¹⁰ For more details we refer to the experimental script in Appendix 1.

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- ¹¹ In doing so, we follow the practice of earlier empirical studies that use social networks (e.g. Leider, Mobius, Rosenblat, & Do, 2009; Jackson, Rodriguez-Barraquer, & Tan, 2012).
- ¹² This contrasts with existing studies that demonstrated that when information on collective action is publicly fed back (e.g. by publicly revealing the average contribution or the number of people who contributed) conditional cooperation makes that collective action decreases over time (e.g. Fischbacher & Gächter, 2010). We think the positive trend in cooperation in our study is the result of how communication is organized in our setting, which allows participants to conceal free-riding behavior. At the same time, there is a large literature that showed that even when participants receive public information about the degree of cooperation, free face-to-face communication increases collective action (see the literature review in Isaac & Walker (1988)). This positive effect may work through an increased understanding of the implications of individual decisions for the group welfare and an increased credibility of expected decisions.
- ¹³ Given potential operational differences in CDR programs between rural and urban areas, and potential differences in their effectiveness between populations affected differently by conflict, we applied the same analysis on subsamples disaggregated by location (urban vs. rural) and whether the participants were displaced or not (as proxy for conflict exposure). Table A4 in the Appendix presents the results. We find a similar increase in cooperation in all subsamples, and no statistically significant differences in cooperation between the subsamples before and after communication.
- ¹⁴ The selection of the beneficiaries was organised by the local organisations that were funded by the CDR programs. As for the non-beneficiaries we relied on similar local organisations that did not receive such funding, the important assumption we make is that funded and non-funded programs follow similar recruitment procedures, so that beneficiaries and non-beneficiaries are comparable on as many characteristics as possible. With both beneficiaries and non-beneficiaries being selected from local organisations, we assume that the only difference between both is the funding they receive. Beneficiaries participate in organisations that receive funding from CDR programs, and are therefore expected to be more active than non-funded organisation, which would then have an influence on their members.
- ¹⁵ Another difference with studies on social peer effects is that most of them use the average behavior in the peer group as main explanatory variable, with each individual having an individual-specific peer group (e.g. Clark & Loheac (2007); McVicar & Polanski (2014)). While in these studies the idea is that people are only exposed to influence from their peer group, in our study all participants are exposed to the same group of

people. That is why we use the same denominator (total number of participants) for all participants in the same session.

¹⁶ To address potential endogeneity concerns related to the omission of confounding factors of participants' inclination to cooperate, we ran the following additional regressions. First, in a regression on cooperation before communication, we find no significant effect of beneficiary status (coef.= -0.025, s.e. = 0.068). Furthermore, adding a control for cooperation before communication in the regressions on cooperation after communication (for this we use the predicted cooperation in the first round, using a set of exogenous individual characteristics) we find that the results (reported in Table A9) are not qualitatively different from the original results.