



Building social capital: Conditional cash transfers and cooperation



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ABSTRACT

Many conditional cash transfer (CCT) programs have important social components and, therefore, can have an effect on social capital. In 2007, we conducted a field experiment with 1451 subjects in Cartagena, Colombia. We interpret the behavior in the game as a measure of what in the literature has been called social capital. We played the game in two similar and adjacent neighborhoods. The 'treatment' neighborhood, *Pozón*, had been targeted for over 2 years by a CCT program, *Familias en Acción*; the 'control' neighborhood, *Ciénaga*, had not. In 2008, with the program being implemented in both neighborhoods, we played the same public goods game, and were therefore able to implement a difference in differences strategy to estimate the impact of the CCT on our measure of social capital.

In 2007, the level of cooperation we observed in the treatment neighborhood was considerably higher than that in the control one. Although similar in many dimensions, the two groups turned out to be significantly different in some observable variables; the positive result was robust to controls for these differences. In 2008, we found that the level of cooperation was statistically identical across the two neighborhoods, and similar to the levels observed in 2007 in the treatment one. We conclude that the CCT program did improve cooperation.

In analyzing the effect of the CCT on cooperation we also look at other (individual and group) determinants of individual behavior in the game, and we compare our measure based on behavior in the game to more traditional measures of social capital used in the literature that we collected in a context-specific survey.

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1. Introduction

Social policies may also improve economic outcomes through changes in the structures of social relationships and their forms of organization. Social capital refers to the set of resources that inheres in those relationships and the structure of such relationships (Uphoff, 1999). It can be understood in terms of social norms and networks (Putnam, 1993; Coleman, 1990) and it manifests itself in patterns of pro-social behavior (trust, reciprocity, and cooperation (Christoforou and Davis, 2014).

Conditional cash transfers (CCTs) have become one of the most popular interventions in developing countries. There is a strong line of research on the effects of CCTs that show that CCTs are successful in their goals as social assistance, raising

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service use and improvements in target outcomes (Fiszbein et al., 2009; Adato and Hoddinott, 2010). Although CCTs' designs were meant to have a 'social capital' approach¹ and anecdotal and qualitative evidence has been found (Cohen and Franco, 2006), rigorous studies on whether CCTs affect social capital has been a prevalent interest just recently and mostly on voting behavior.² Although the main goals of the CCTs are not related directly to improve participation and collective action, the fact that most of CCTs have a component of new interactions among the beneficiaries within the communities would lead to build or strength social ties (Putnam, 1995) and enforce pro-social norms.

In Colombia, there is a large CCT program, called Familias en Acción (FA) that has, like many other similar programs, especially in Latin America, a strong social component. Beneficiaries meet periodically and elect a representative that coordinates a number of activities in which they are involved. It is reasonable to argue that these activities could increase cooperation, trust and, more generally, interactions among beneficiaries that could improve the social fabric of poor communities and, potentially, improve collective action and the ability to overcome social dilemmas. The main goal of this paper is to test this hypothesis exploiting quasi-experimental variation induced by the roll out of FA to urban areas.

In this study, we estimate the impact of the introduction of a conditional cash transfer program on social capital. The first challenge we had to face was to devise a measure of social capital in the context we were studying. On one hand, integrating quantitative with qualitative measures as an improved approach to measure social capital in program evaluation is not new (Adato, 2008; Casey et al., 2012) but evidence is not conclusive.³ Adato (2000) uses qualitative data (e.g. focus groups and interviews) and finds positive link between social capital and the CCT Progreso in Mexico whereas Camacho (2014) uses survey measures only and finds no effects of the local CCT on social engagement in Peru.⁴

On the other hand, despite the complexity of conceptualization of social capital, what lies at its heart is the ability to overcome free-rider incentives in real-world situations (Coleman, 1988). These situations, that especially poor communities face, share the same game-theoretic representation. Then, as a community level attribute (Coleman, 1990), in order to build social capital, individual behavior might be subject to "free-riding" (Samuelson, 1954; Olson, 1965; Grossman and Hart, 1980; Ostrom, 1998). Evidence on social programs evaluation supports this claim (Adato et al., 2005; Fearon et al., 2009; Avdeenko and Gilligan, 2014).

For this reason, we propose a measure of social capital based on the behavior in a public goods game. The use of 'lab in the field' experiments as a method to study social preferences within a community and to measure social capital is not new.⁵ In particular, public good games have been used in a variety of different situations, both urban and rural, and are one of the experimental methods that have been often used in the literature to examine behavioral motivations such cooperation in groups. Our design has been used extensively in Colombia (Cárdenas and Jaramillo, 2007; Cárdenas et al., 2013; Attanasio and Pellerano 2012) and in other countries (Cárdenas, 2003; Cárdenas et al., 2009, 2013; Barr et al., 2014). Our original contribution is to implement the game to measure the impact of a specific intervention in a quasi-experimental setting.

The Public Goods game we use to measure social capital captures the willingness to cooperate among the members of a group of 25 people by choosing simultaneously whether to allocate a token in the private account with a private benefit or to allocate the token in the group account, where the benefits of all members increases and the well-being of the entire group is improved. The sum of tokens allocated to the public good is then multiplied by a factor to model the additional value generated from the public nature of the good and then shared equally among the group members. The situation just described constitutes a typical social dilemma. There is no incentive to invest in the group account due to a higher individual payoff by investing in the private account. The dominant strategy is not to contribute at all, undermining the social outcome. However, if all in the group invest their token in the private account (assuming players maximize their own monetary payoff and rationality is common knowledge), the group will be worse-off than if all the members invest in the group account, which constitutes the social optimum. The experimental literature has extensively documented that, in similar contexts, the Nash equilibrium is not observed, either in the lab or in the field. Groups of individuals seem to be able to internalize at least in part the externality built in the game.⁶ For this reason, this set up seems particularly attractive in devising a measure of 'social capital'.

¹ For example, Juntos (Together) in Peru, Puente (Bridge) in Chile, FA (Families in Action) in Colombia, Progreso (Progress) in Mexico, Bolsa Familia (Family pot) refer all to features of social capital: networks, 'bridging' social capital, family social capital, 'linking' social capital, cooperation (ECLAC, 2005; Arriagada and Mathivet, 2007). ECLAC (2005) present this approach in detail.

² Some recent evidence on the positive political impacts and voting behavior of CCT programs in Colombia (Nupia, 2012; Zárate et al., 2013), Brazil (Zucco, 2011), Romania (Pop-Eleches and Pop-Eleches, 2012), Uruguay (Manacorda et al., 2011), Philippines (Labonne, 2013) and Mexico (De la O, 2013).

³ There is a strong line of research on the impact of "community-driven development" (CDD) programs whose main goal is to actually build social capital and seem to have no effect on the desired outcomes. Although, many of these studies suggest the existence of bias in self-reported levels of pro-sociality and social cohesion from surveys (Avdeenko and Gilligan, 2014; Mansuri and Rao 2012; Wong, 2012; Fearon et al., 2009; Casey et al., 2012), very few conduct labs in the field (Avdeenko and Gilligan, 2014; Fearon et al., 2009; Nguyen and Rieger, 2014). Fearon et al. (2009) employed a framed public goods game and found a positive impact of a CDD initiative on individuals' ability to overcome collective action problems in Liberia. Nguyen and Rieger (2014) found a positive impact on a CDD and contribution in a public goods game in Morocco.

⁴ There is similar evidence from Cash Transfer (CT) programs (MacAuslan and Riemenschneider, 2011, in Malawi and Zimbabwe and Cameron and Shah, 2014, in India). However, Pellerano et al. (2014) find a positive link of a CT and engagement in reciprocal sharing activities in the community in Lesotho.

⁵ Carpenter (2002), Carpenter et al. (2004), Gächter et al. (2004), Karlan (2005), Cárdenas et al. (2009), Fearon et al. (2009), Voors et al. (2012), Gilligan et al. (2013).

⁶ In general, contributions to the public good are in a range of 40–60% of the group optimum (Ledyard, 1995; Croson, 1996; Camerer and Fehr, 2004; Attanasio et al., 2009). At the same time, there seems to be considerable amounts of heterogeneity in the ability different groups have to solve this kind of problems.

The possibility of cooperation within a group is determined by multiple factors such as repetition, communication, punishments or rewards and inequality in the payments. In our game, the incentives to invest in the group account are given by the specific features of the design, but also by the individual motivations concerning the group wellbeing. Individual attributes such as altruism, trust, social distance from the other members (Cárdenas, 2003), fairness (Rabin, 1993), reciprocity (Andreoni, 1988, 1995; Bowles and Gintis, 2004), a sense of affiliation as a member of a common group, or sympathy toward others in the group (Attanasio et al., 2009) determine social cohesion in a group and strengthen the ability of its members to cooperate and overcome collective action problems. In addition, community attributes that would be affected by the CCT, such as social norms and institutions, informal enforcement mechanisms, concerns for social reputation, social reciprocity (Bowles and Gintis, 2004) and group identification enforce the group interests over the individual, leading to attain a higher level of contribution and overcome the dilemma. We expect that our design will allow us to examine how cooperation has been affected by the presence of a CCT.

The first challenge we had to face is related to how we estimate the effect of the CCT on willingness to cooperate. To evaluate the impact of an intervention one would like to rely on a Randomized Control Trial. As the outcome we are interested on is the 'social capital' in a community, one would ideally randomize the intervention across a number of communities. Unfortunately, such a design was not possible. We therefore rely on evidence on individual behavior and comparing the choices of individuals living in a community with the program with that of individuals living in a community without the program. Of course, any observed difference between the 'treatment' and 'control' individuals could be driven by systematic differences at the community level in the level of social capital that are not related to the program. To circumvent this problem, therefore, we rely on a relatively standard diff in diff approach and estimate differences between the two communities we consider that are unrelated to the program by observing behavior in a period in which both communities are either both exposed or both not exposed to the program.⁷ For such a strategy to be valid it is necessary that systematic differences between the two communities are constant over the period considered and determine behavior in an additive fashion. In the literature that uses diff in diff this assumption is often referred to as a common trend assumption. In what follows, we discuss its plausibility.

2. *Familias en Acción* and its expansion

Familias en Acción is a CCT that was inspired by the Mexican CCT PROGRESA and whose goal was to reduce extreme poverty in the medium term by providing resources to improve the nutritional status of poor households and in particular their children as well as school enrolment. To get access to the program's grants, beneficiary households have to comply with a number of requirements. FA has three components: a nutritional and health component aimed at households with children less than five, an education grant for children in primary school and an education grant for children in secondary school. The health and nutrition grant, roughly equal to US\$25 independent of family size, is conditioned on attending regularly growth and development check-ups for children, a vaccination program and some 'classes' on hygiene, diet and contraception. The educational grants, aimed at households with children aged seven to seventeen, are conditional on enrolment and regular attendance in school. Each child in primary (secondary) school entitles the household to about US\$8 (US\$16) per month. Households receive a total transfer which may oscillates between 10% and 21% of the minimum wage and between 25% and 50% of the average level income of the poor (DNP, 2010; MESEP, 2012).

The program also includes an important social component, articulated around periodic meetings of beneficiaries, called *Encuentros de Cuidado* (EC) [Care follow-up meetings]. Although participation to these meetings is neither compulsory nor part of the conditions to receive the transfer, most beneficiaries (95.94% in our sample) participate in the EC where, in addition to discussing hygiene, nutrition or other health-specific issues, they have the possibility to talk about different topics or even simply chat. Beneficiaries are invited to attend the meetings, which are introduced as key for human capital investment. Conversations with program's officials and with beneficiary mothers indicate that these social aspects are indeed an important feature of the program: beneficiary mothers start new activities, get to know each other better and improve their ability to act as a group.

Additionally, the beneficiaries elect a representative, called *Madre Líder* (ML) who is in charge of communication with the local office and is also in charge of organizing the social activities and educational meetings (such as the EC) to which the beneficiaries are supposed to participate. As a consequence, the ML's often assume a prominent and visible role in the community.⁸

As the theory of social capital suggests, the fact that our CCT would require people to interact with others may create an environment for them to experience the potential of social networks to improve their wellbeing. Our CCT would affect social capital once the program is able to affect social interactions and their environment (Coleman, 1988). FA may create networks or strengthen the current ones and improve the structure of social relationships among beneficiaries (Putnam, 1995), promote leadership (Latham and Saari, 1979; Bass, 1991) and give mothers the opportunity to start working as a "social

⁷ In our case we have one year (2007) when one community was exposed to the program and the other not and one year (2008) in which they were both exposed to the program. We therefore use 2008 to identify the systematic differences between the two communities and subtract that from the difference observed in 2007 to obtain the effect of the program.

⁸ By 2007, there were 800 ML with group size ranging between 50 and 80 beneficiaries.

Table 1
Number of players and sessions.

			Individuals			Sessions ^c		
			Pozón	Ciénaga	Total	Pozón	Ciénaga	Total
Baseline	2007		342	334	676	14	14	28
Follow-up	2008	Panel	237	290	527	7	11	18 ^b
		Independent cross section	404	371	775	14	15	29 ^a

^a Sessions with New recruits, i.e. with 95% or more people who were playing the game for the first time (i.e. 714 subjects).

^b Sessions with Old recruits, i.e. with 95% or more people who were playing the game for the second time (i.e. 443 subjects).

^c Sessions that were neither New nor Old (at 95% composition threshold) are not in the Table. There were 5 of them in Pozón and 1 in Ciénaga, corresponding to 145 subjects.

group” by perceiving a strong identification with the program (Tajfel, 1982) and their power to act ‘together’ (Warren, 1998). This facilitates group decision-making and increases the willingness to intervene for the common good, which eventually could lead to overcoming social dilemmas by enforcing pro-social norms (Coleman, 1990).

Empirically, we are able to distinguish between attitudinal (i.e. perception of cooperation or trust in the community) and behavioral social capital (i.e. voting behavior, participation in meetings and membership in associations as well as willingness to cooperate, social networks and social norms) (Carpenter, 2002). Our CCT may increase attitudinal social capital in some ways. First, by becoming a beneficiary a new group identity emerges which would change the perception of the community traits. Beneficiaries share the same paperwork load, health check-ups, payment logistics and the same interests. Second, the EC and beneficiaries’ assemblies are a place of encounter with people that face the same needs and interests. This continued interaction among beneficiaries would create and enforce social norms so beneficiaries’ perception of trust is also modified.

On the other hand, our CCT may increase behavioral social capital, first, by transforming voting behavior as confidence/perceptions on the current government or a specific political party change. Second, when social relations among members of a community are strengthened, incentives to participate in organizations or meetings in order to solve problems within the community may be stronger as well.

The program has become the flagship of the Colombian government’s social policy as it targets the poorest 20% of Colombian households.⁹ It started in 2002 in 627 small rural areas and in 2007 was expanded to all urban areas in order to include 1.5 million beneficiary households.¹⁰ In 2005, the FA authorities decided to pilot the program in Pozón, a neighborhood in the city of Cartagena. Between 2005 and the first half of 2007, the program operated in Pozón but had not been implemented in other neighborhoods, despite there being other two neighborhoods (Nelson Mandela and Ciénaga de la Virgen) identified by the FA authorities as eligible to participate in the pilot.¹¹

In late August 2007, just a month after our first game, a new wave of massive enrolment to the program started in every municipality in the country, regardless of its population. The program was also rolled out in all the poorest neighborhoods of Cartagena, including Ciénaga.

3. Data

In this paper, we utilize two types of data. First, we analyze the behavior of individuals in the field experiment that we describe below. Second, we use extensively the survey we collected at the end of the experiment. In what follows we describe the recruitment of subjects for the game in the two neighborhoods, the protocol of the games and the data we collected.

The data for the entire set of experimental and survey data contains information on a sample of 676 and 1302 participants in the baseline and the follow up, respectively. In 2007 we conducted 28 sessions, 14 in each neighborhood. In 2008 we conducted 47 clean sessions, 21 in Pozón and 26 in Ciénaga,¹² which constitutes 775 new participants and 527 individuals that had played in 2007.¹³ These figures are reported in Table 1. Overall we obtained a sample with 1,451 of individuals that did not participate in any game before.

⁹ In Colombia, most welfare programs are targeted using the so-called SISBEN score, a poverty indicator that is updated periodically. On the basis of this score, households are assigned to one of six categories. FA targets the level 1 of SISBEN and displaced people.

¹⁰ For evidence of success of FA on the target outcomes see Attanasio et al. (2005), Attanasio et al. (2010, 2012a, 2012b), Baez and Camacho (2011); for evidence about other outcomes such as crime and voting behavior see Camacho and Mejía (2013) and Zárate et al. (2013). Attanasio et al. (2012b) shows that these positive effects are still noticeable amongst beneficiaries 10 years after their first enrolment in the program.

¹¹ Ciénaga and Pozón were considered very similar to the local authorities and are adjacent. The third neighborhood (Nelson Mandela) is geographically distant and has received a much larger influx of displaced households from the countryside. For more information see the appendix A in the appendix.

¹² In 2008 we conducted 53 sessions, 26 in Pozón and 27 in Ciénaga. However, the complexity of the logistics led to 6 sessions in which more than 5% or less than 95% participants were new or former participants (called mixed sessions). We exclude them from the analysis.

¹³ In 2008 we were faced with the issue of whether to play with the same individuals or to recruit a fresh sample of players. Both strategies had advantages and disadvantages. Playing with ‘former’ players would allow us to construct a longitudinal data set and potentially control for individual fixed effects. However, behavior in 2008 would be conditioned to having experienced the game within a specific group in the previous year and it would require a larger sample and more funding to take this effect into account.

3.1. Experimental set up

In our design, a Voluntary Contribution mechanism (VCM), each individual in a session of 25 participants is given one token. The player has a simple discrete choice to make: she can either invest the token in what we label a 'group' account or in a 'private' account. Tokens placed in private accounts are eventually redeemed at \$5,¹⁴ while those placed in the group account are redeemed at \$10. Players who placed their tokens in the private account will be entitled to \$5 and an equal share of what is in the group account. The amount in the group account is divided among all players, regardless of who contributed to it: given that there are 25 players, each would receive \$0.40 for each token invested in the group account by any other member in the group. Individuals' private decisions are made simultaneously and contributions are private and anonymous, that is, players are not allowed to communicate. Those who put their tokens in the group account are only entitled to a share of what is in the group account. Note that a player contributing to the group account will get more than the \$5, which is the minimum she gets if she contributes to the private account, if at least 13 players contribute to the group account.¹⁵

The marginal propensity to cooperate also called marginal per capita return (MPCR) in this game is one of the lowest in the literature. The goal was to mimic a measure of what is called bridging social capital, the ability to overcome social dilemmas in a very large group. Instead, we argue it matches more closely the reality we are trying to depict. Given the level of deprivation in the neighborhoods we study, the intensity of the social dilemma is arguably much higher than in most other lab, or even lab-in-the-field, studies, and a low MPCR is better fitted than a higher one.

More importantly, the low MPCR makes our results more forceful. Because the power of our analysis would have been maximized if we had had a MPCR of 50%, a low value provides evidence that if the coefficient is subject to any bias it will be downward bias. This is particularly important in light of our main goal. The abovementioned studies do not measure social capital but rather correlations between game outcomes and decisions in other contexts. Because we claim to provide a social capital measure,¹⁶ where social dilemmas are key –and hence a low MPCR appropriate–, the low MPCR provides an additional source of validity to the measure.

The dichotomous VCM makes the game easily understood by subjects and also time effective. This is perhaps one of the reasons why only two other studies employ a dichotomous VCM game comparable we employed in Colombia: Cárdenas et al. (2013) in 6 Latin American cities, Barr et al. (2014) in Albania.

In July 2007 and July 2008 we conducted the Public Goods game described above in Pozón and Ciénaga. The VCM game we described above is played in two rounds. The subjects first decided whether to contribute to the group account individually and privately, without having the possibility of communicating with other players. After the first round (but before its results are revealed), they are given the possibility of talking about whatever they like. Then subjects had to decide again what to do with their token, individually and privately, before the results of the first round are announced. The role of communication and beliefs on individual willingness to cooperate is far beyond our research question. In this paper we only analyze willingness to cooperate in the first round.

The protocol followed in the 2 years was identical. Participants were invited to come to the local public school. After collecting their identification documents and checking their names on the recruitment lists, subjects in each session were given an identification number randomly and seated in semi-circle in a classroom where the instructions of the game were read and explained. After the participants played the second round of the game described above, we collected a network questionnaire on the existing relationships among them and a relatively short survey that gathers information on a wide range of socio-economic variables.

All recruited people were given US\$1.2 as part of their show-up fee in order to induce credibility and to subsidize the transportation cost from their homes or workplace to the school we assigned for the experiments stage. Once the session ended participants were paid their earnings based on the decisions in the experiments. On average each participant earned US\$9.6 (COL\$16867).¹⁷

3.2. Sampling, recruitment and allocation into sessions

We recruited the participants to the game with the help of the local office of FA (Enlace Municipal). For the first stage of our study, in 2007 the recruitment in the treatment and control neighborhoods was slightly different. In Pozón, where the program was already operating, we could contact directly beneficiaries. The ML representatives sent an open invitation to all beneficiary mothers. In Ciénaga, instead, beneficiary mothers had not yet been registered into the program. However, since the program was already planning the expansion in, the office also had list of 'future' beneficiaries in that neighborhood.

¹⁴ Units in thousands; thus \$5 means COL\$5000 (US\$2.85, according to the official exchange rate at that date TRM: US\$1=COL\$1753.01 (monthly mean average for July 2008, <http://www.oanda.com>)).

¹⁵ At the end of the session, only the percentage of players contributing to the group account is revealed and players are paid after they take a short socio-economic survey. Participants played the game for 2 rounds. After playing the first round, individuals are allowed to talk and communicate. Ten minutes later, there is a second round. For more details, see Appendix.

¹⁶ In fact we claim to have a measure of the effect of the program on a measure, as opposed to a mere correlation, which arguably makes the point more relevant.

¹⁷ FX rate US\$1=COL\$1,951.84 (monthly mean average for July 2007, <http://www.banrep.gov.co>).

These lists were used in recruiting the participants in Ciénaga. In addition, we also used an open communication strategy: a person with a loud speaker announced the invitation twice a day, 1 week before fieldwork started.¹⁸

In both localities, mothers were allocated into session as a first-come first served basis and there was no control over who attended which session, only whether the sessions were treatment or control and the participants dwelled in their respective neighborhood. The FA list and the open communication campaign in Ciénaga proved to be effective in identifying future FA beneficiaries, as 92.51% of participants in these sessions were subsequently selected by FA as beneficiary households in 2007.¹⁹

Unlike in 2007, in 2008 when we conducted the game for the second time, we could use the same recruitment strategy in both neighborhoods, as beneficiary lists were available in both neighborhoods. Besides contacting old participants, invitations were also sent to 500 randomly selected new participants from the FA beneficiaries list in each neighborhood.²⁰ The FA office sent the invitations through the ML to those specific households in order to attend to any of the sessions held (a span of 3 days).

Conducting lab in the field experiments in large cities provide many challenges to the experimental design in terms of costs, time, recruitment and attendance rates (Ñopo et al., 2008; Candelo and Polania-Reyes, 2008). For example, implementing sessions with 25 randomly allocated individuals was impractical and infeasible. The two neighborhoods are a 2 hour-drive apart; in order to minimize 'cross-talk' and its effects – participants talking about the experiment to future players who will participate in subsequent sessions – sessions were implemented in a four-day frame with four sessions each day in each neighborhood. For example, during the first 4 days we conducted the experiments with new participants in Pozón and the following 4 days with new participants in Ciénaga. Since the time frame was too short, we gave the beneficiaries the freedom to choose the session that best suits them. The requirements were to be in the list of randomly chosen beneficiaries. It could have been the case that people who might be neighbors arrived together to the sessions. In fact, some invited beneficiaries arrived to the session in groups.²¹

There is the possibility of contamination among subjects of different sessions: participants to a session could talk to participants to the next session on the way out, although we put lot of effort in avoiding these contacts. This type of contamination would be the same or very similar in both treatment and control. In order to control for this possibility we included in our analysis the average outcomes in previous session and the number of sessions that had occurred before.

3.3. The post-game survey

In the survey we collected at the end of the session, there is a module on individual socio-economic characteristics (e.g. age and level of education), a module on household characteristics (e.g. income, assets, household size), a module on social capital measures (e.g. membership in associations, trust), a module about FA (e.g. date of enrolment), a module about the game (e.g. whether the participant understood the game).

3.4. Measuring social capital in a FA locality

The data we collected both during and after the games allowed us to construct several social capital measures. In what follows, we consider three different sets of measures: those derived from the behavior we observe in the games, those we can derive from the socio-economic questionnaire and those we derive from the network data we also collect.

First, from behavior in the game, we consider the propensity to contribute to the public good as a measure of attitude to cooperate in a group. An index of social capital could then be the proportion of individuals who contribute to the public good. This measure captures the ability of a group to internalize an externality and the willingness individual players have to act as a group.

Second, although the use of direct survey questions on trust has been strongly questioned as a poor indicator of social capital in comparison with experimental outcomes, in order to compare our data with the literature, we included in the postgame survey traditional measures of social capital regarding trust (such as those derived from the WVS), membership in organizations and participation in political and social processes within the community. We also considered perception of cooperative attitudes. These variables have been widely used as a proxy for social capital in the non-experimental literature.

Finally, in our survey, each player was asked about her relation with all the other players, where the options given were: (a) relative, (b) friend, (c) acquaintance or (d) unknown. In addition, we also asked questions about who would the participant consider as a leader in the community. For every session, we are able to construct a relationship matrix that describes the

¹⁸ This is a method commonly used to send messages in the slums, given the lack of telephones.

¹⁹ In particular, 82.34% of participants in these sessions were subsequently selected by FA as beneficiaries in late 2007 and 10.18% of participants belonged to a household with a FA beneficiary.

²⁰ Old participants were sent an invitation and were allocated in a session with a specific day and time in order to reproduce the same sessions we hold in 2007.

²¹ The fact that individuals are not randomly allocated into sessions allowed us to explore the role of social networks in the effect of the program on cooperation. We were able to obtain enough variation in terms of the density and quality of the network across the neighborhoods and compared the results with those of pre-existing networks in Ciénaga 2007. We also performed a sensitivity analysis and included the network data in the regression analysis, which did not affect the results.

shape of existing networks among players. This information allows us to calculate a set of variables on session connectivity and eventually control for the strength of relationships in each group. We construct measures of how well connected the participants of each session were. The simplest measure of the “social integration” of each player within the session is the number of acquaintances, relatives and friends a participant had in her session. We also use an index of connectivity defined as the total number of connections (i.e. as an acquaintance, as a friend or as a relative) in the session and a dummy if she was considered a leader (see Appendix A).

Despite social networks being a key dimension of social capital to improve socioeconomic outcomes (Coleman, 1990; Putnam, 1995), we do not focus on our networks measures since individuals were not randomly allocated in the sessions. Instead, we focus on our behavioral measure of social capital and we do describe the other measures for illustrative purposes.

3.5. Descriptive statistics

Attanasio et al. (2009) reports descriptive statistics and the tests of significance for the difference between the two neighborhoods in 2007. These statistics are also reported in the first panel of Table 2 and give a good representation of the main characteristics of this population. Participants come from very poor families, with low levels of income and education. Although the two neighborhoods are very similar in many dimensions, and they are next to each other, important differences emerge quite clearly. Participants from Pozón were significantly less likely to be head of household (indicating a larger presence of partners) and have better access to public services. Moreover they are more likely to be educated. Most importantly, participants from Pozón seem considerably less poor than those in Ciénaga. They are more likely to own the house where they live, to be connected to piped water, to own several durables and other assets and less likely to have a dirt floor in the house (although there is no significant difference in the level of income or food security). They are also less likely to be unemployed. Some of the differences observed in 2007 could have been induced by the program and, given the nature of the data; it is hard to disentangle the effect that FA may have had on many of the socio economic outcomes in both neighborhoods. For instance asset tenure and monthly income may be clearly affected by the fact that participants from Pozón had already been received the benefits from FA.

In the second panel of Table 2 we report the differences in observable background variables between Ciénaga and Pozón in 2008. The striking feature of this second panel is that, at least in terms of observable characteristics, the differences observed in 2007 are reproduced in 2008. This is important to address the concerns about differences in selection and recruitment between the two years that we discuss below.

In Table 3, we report the average level of cooperation in our sessions in 2007 and 2008, without distinguishing neighborhoods. The data from 2008 are divided between the subjects that had already played in 2007 and those that played the game for the first time. Several considerations are in order. First, although – as mentioned – the unique Nash equilibrium of that game assuming people maximize their own short-run earnings is that individuals invest their token in the private account, many individuals do deviate from the Nash equilibrium and contribute to the public good. Despite having a very low MPC and conducting the game in an urban context, the overall level of cooperation we observe in our sample is similar to that observed in similar labs in the field. In the first round, only 14.8% and 12% of the sessions did not contribute at all. Second, there is a marked difference between the behavior of ‘old’ and ‘new’ players.

Virtually none of the ‘old’ players contributed to the public good and they almost uniformly played the Nash equilibrium. This result is likely to be driven by learning and by the fact that in the previous year contribution rates had been relatively low so that, even in Pozón, those who contributed to the public good received considerably less than the other players. This finding is also consistent with lab evidence on repeated VCM games in which, most of the times, in repeated rounds players tend to converge toward the Nash equilibrium.

In our analysis below, we present the results of the independent cross section only²². In Table 4, we report the descriptive statistics on several measures of ‘social capital’ that can be obtained from the post-game survey. These measures are divided into three groups: variables based on individual participation to civic association and neighborhood activities; variables reflecting voting behavior²³ and variables derived from answers to questions about trust and perception of cooperativeness.²⁴ The six columns of the table refer to the measures in Pozón, Ciénaga and the difference for the baseline and the follow-up.

²² In order to avoid confounding the impact of the program with learning effects. Subjects learn from other’s behavior, and update their beliefs about what other will do the next time they have the chance to play. This effect is particularly strong when the subjects know each other before entering the lab (a key issue of experiments in the field). In 2007, it is very likely that subjects met and discuss the results of the activity once they left the session. Participants in 2007 that participated again in 2008 have a different characteristic from those who face the game for the first time: they may contribute as the experimental evidence affirms that contributions decrease over time but increase in the presence of communication, they may also contribute if they contributed before.

²³ The last local elections in Cartagena (Governor, mayor, members of the *Asambleas Departamentales*, Municipal council and *Juntas Administradoras Locales*) were held three months after the inscription in the program (Oct. 2007) and eight months before the beginning of the program in Pozón (April 2003). Presidential elections were held sixteen months after the beginning of the program in Pozón, in May 2006.

²⁴ There is a risk that the attitudinal measures of social capital were influenced by the experimental games (Carpenter, 2002) as the players answered the questionnaire after they knew the outcome of the games and they possibly might have expressed their emotions in their answers on perception of trust and cooperation.

Table 2
Demographic characteristics of the participants – 2007 and 2008.

	Variable	2007			2008		
		Pozón	Ciénaga	Difference	Pozón	Ciénaga	Difference
General characteristics	Percentage of female participants	100 (0.0)	97.9 (1.1)	2.1** (1.0)	98.8 (0.7)	98.1 (0.7)	0.7 (0.9)
	Average age (years)	38.0 (0.6)	33.6 (0.7)	4.5*** (0.9)	36.2 (0.6)	36.2 (0.4)	0.0 (0.7)
	Number of years living in the neighborhood	14.5 (0.3)	22.4 (0.5)	−7.9*** (0.6)	14.6 (0.5)	22.3 (0.6)	−7.7*** (0.7)
	Percentage displaced	19.0 (2.2)	8.4 (1.4)	10.6*** (2.6)	16.5 (2.4)	10.3 (1.4)	6.1** (2.7)
	Percentage household head	21.3 (2.3)	30.5 (3.4)	−9.2** (4)	24.3 (1.7)	41.3 (3.1)	−17.0*** (3.4)
	Percentage single	5.6 (1.4)	11.7 (1.6)	−6.1*** (2.1)	10.7 (1.7)	12.0 (2.7)	−1.3 (3.1)
	Percentage married of civil partnership	76.9 (1.7)	62.9 (3.8)	14.0*** (4.1)	77.2 (2.5)	67.4 (3.2)	9.8** (4)
	None (level 0)	2.0 (10)	4.2 (1.4)	−2.1 (1.7)	2.3 (0.7)	2.7 (0.7)	−0.4 (0.9)
	Primary incomplete (level 1)	18.1 (2.6)	18.6 (2.4)	−0.4 (3.5)	21.1 (2.9)	20.9 (2.4)	0.2 (3.7)
	Primary complete (level 2)	13.5 (2.1)	13.8 (1.5)	−0.3 (2.5)	15.0 (1.9)	13.6 (2.2)	1.4 (2.9)
Educational level (percentage)	Secondary incomplete (level 3)	37.4 (2.5)	30.2 (3.3)	7.2* (4.1)	35.3 (2.6)	30.7 (2.2)	4.6 (3.4)
	Secondary complete (level 4)	24.0 (2.9)	25.4 (2.6)	−1.5 (3.8)	18.2 (2.1)	21.7 (2.5)	−3.5 (3.2)
	More than secondary complete (level 5)	5.0 (1.2)	7.8 (1.2)	−2.8* (1.7)	8.1 (2.6)	10.3 (1.5)	−2.2 (2.9)
	Household size	5.6 (0.1)	5.8 (0.2)	−0.2 (0.2)	5.6 (0.1)	5.8 (0.1)	−0.2 (0.1)
	Number of people per room	3.2 (0.1)	2.9 (0.1)	0.3** (0.1)	3.6 (0.1)	3.3 (0.1)	0.2* (0.1)
Dwelling characteristics	Percentage with dirt floor	24.3 (2.1)	41.0 (3.0)	−16.7*** (3.6)	30.6 (2.2)	25.8 (3.1)	4.8 (3.7)
	Percentage owning own house	82.7 (2.0)	58.7 (3.0)	24.1*** (3.5)	66.5 (2.2)	51.9 (2.7)	14.6*** (3.4)
	Water by pipe	94.7 (1.0)	76.9 (3.8)	17.8*** (3.9)	91.9 (1.6)	83.4 (2.9)	8.5*** (3.3)
Public services (percentage)	Sewer system	64.3 (2.8)	12.6 (2.9)	51.8*** (4.0)	67.3 (3.2)	25.5 (3.7)	41.8*** (4.8)
	Does not have electricity	1.8 (0.7)	5.4 (1.5)	−3.6** (1.6)	4.9 (1.4)	1.9 (0.7)	3.0* (1.6)
	Land phone	9.6 (1.6)	26.6 (2.1)	−17.0*** (2.6)	8.1 (1.7)	26.4 (2.5)	−18.3*** (2.9)
Assets (percentage)	Mobile phone	86.3 (2.1)	67.1 (2.3)	19.2*** (3.0)	76.3 (2.2)	68.7 (3.5)	7.6* (4.1)
	Bicycle	21.9 (3.3)	14.4 (1.8)	7.6** (3.7)	19.4 (2.5)	15.2 (2.4)	4.1 (3.3)
	Color TV	80.7 (1.5)	74.6 (3.0)	6.2* (3.3)	84.4 (1.5)	84.5 (2)	−0.1 (2.5)
	Washing machine	23.7 (3.1)	13.8 (1.5)	9.9** (3.4)	27.5 (2.1)	27.7 (2.9)	−0.3 (3.5)
	Sound player	39.8 (2.8)	25.1 (2.8)	14.6*** (3.9)	35.8 (2.2)	26.4 (2.6)	9.5*** (3.3)
Income variables	Percentage unemployed	2.9 (0.9)	10.8 (2.3)	−7.9*** (2.5)	3.8 (1)	4.9 (1.1)	−1.1 (1.5)
	Percentage with access to credit	67.8 (2.4)	66.8 (2.5)	1.1 (3.4)	72.3 (1.8)	69.3 (2)	3.0 (2.7)
	Percentage with access to formal credit	21.1 (1.7)	22.2 (1.9)	−1.1 (2.5)	23.7 (2.1)	21.2 (2.8)	2.5 (3.4)
	Percentage with food insecurity level (high)	9.6 (1.4)	9.9 (1.9)	−0.2 (2.3)	7.8 (1.6)	9.2 (1.6)	−1.4 (2.2)
	Per capita monthly income (US\$)	25.3 (0.9)	22.3 (0.6)	3.0*** (1.1)	30.3 (1.6)	27.1 (1.0)	3.2* (1.9)
Observations	342	334	676	346	368	714	

Robust standard errors, clustered by session.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%. For year 2008 we use Independent Cross section data.

Table 3

Behavior in the VCM – first round.

	Baseline 2007	Follow-up 2008			
		Panel ^a	Difference ^b	Independent cross section	Difference ^d
Average percentage of contributors	20.0 (3.4)	7.3 (1.8)	–8.9** (3.9)	29.3 (3.7)	9.3* (4.9)
Percentage of sessions with no contribution	14.8 (6.9)	28 (10.9)	9.1 (13.7)	10.5 (5.8)	–4.3 (9.0)
Observations	676	439	878	714	1390

^a Old participants in sessions with Old recruits, i.e. with 95% or more people were playing the game for the second time^b Difference is calculated with respect to the same individuals' decisions in 2007.^c Sessions with New recruits, i.e. with 95% or more people were playing the game for the first time.^d Difference is calculated with respect to all decisions in baseline 2007.

Robust standard errors in parenthesis, clustered by session.

*Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

Table 4

Traditional social capital measures – independent cross section.

	Percentage	2007	2008	Difference	Standard errors	
Civic participation	Participation in neighborhood decisions	54.3	40.9	−13.4***	3.8	
	Participation in the neighborhood meetings	61.8	47.9	−13.9***	3.0	
	Membership in at least one organization	38.6	26.8	−11.9***	3.7	
	If is member of any civic association	Attendance to the meetings	38.5	26.3	−12.1***	3.7
		Decision maker	33.6	23.5	−10.1***	3.5
		Leader	18.5	9.8	−8.7***	2.1
Voting behavior		Supports with money or voluntary work	35.7	23	−12.7***	3.4
	Voted in local elections (2003 and 2007)	52.4	72.4	20.0***	3.0	
	Voted in presidential elections (2006)	67	77.7	10.7**	4.4	
	Trust	Most people	6.7	6.6	−0.1	1.6
Trust and cooperation perception		Few people	62	62	0.0	2.9
		None	31.3	31.4	0.1	2.8
	Perception within the community	Cooperation	26.5	32.3	5.8***	2.2
		Reciprocity	24.2	14	−10.2***	2.3
	Selfishness	49.4	53.7	4.3	2.8	
Observations		676	714	1390		

Robust standard errors of the difference, clustered by session. *Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

In the first panel, we report the percentage of participants actively involved in neighborhood decisions or meetings on topics related to the community, and for active members in (at least one) civic organizations, the percentage of participants that attends to the meetings, are perceive her or himself as a leader, or as a decision maker and supports the association with money or voluntary work. In the second panel, we report the percentage of individuals who voted in local and presidential elections. In the third panel, we report statistics derived from a question on trust.²⁵ In addition, individuals may also have different perceptions on others' social preferences. They may perceive either a reciprocal (a dummy if most people help if others help), selfish (a dummy if most people care only about themselves), or cooperative (a dummy if most people help others unconditionally) behavior in their neighbors.

Compared to what was observed in other studies (see [Latorre López, 2004](#); [Polania-Reyes, 2005](#)) the participants reported higher levels of participation in organized groups. However, our measures of civic participation are significantly lower in the follow up. Voting behavior in 2008 is significantly higher than in 2007 for both local and presidential elections, which is consistent with some studies on voting behavior and CCT (see [Zárate et al., 2013](#); [Nupia, 2012](#); see footnote 3 for evidence on other countries). In the first two panels, the time trend in social capital measures is the same in both years. In the local context, voting participation is different in 2007 although we are able to argue that this measure is dependent on the time of the survey: in 2007, three months passed after the local elections and it was more than a year we ask the same question in 2008. Direct measures of trust are not significantly different across time and yield particularly a low estimate of trust. However, the sign of the difference is consistent with what we would expect: the program might have reduced the percentage of people who think no one can be trusted and the percentage of people who think that most of the people in the community are selfish.

²⁵ This question was adapted from the WVS source. We added an alternative “few people can be trusted”.

Table 5

Networks characteristics across sessions – independent cross section.

	2007	2008	Difference	Standard errors
Average number of friends	1.0	1.5	0.4*	0.2
Average number of acquaintances	7.7	0.4	−7.3***	0.6
Average number of connections	8.9	2.0	−6.9***	0.7
Friendship density ^a	0.1	0.1	0.1***	0.0
Acquaintanceship density	0.4	0.0	−0.3***	0.0
Connections density	0.4	0.2	−0.3***	0.0
Percentage of players identified as leader	33.4	18.1	−15.4***	3.3
Observations	676	714	1390	

^a Network density is calculated as the number of connections/(number of connected people)*(number connected people – 1). For example, Friends density is calculated as the number of friends/(number of people with friends)*(number of people with friends – 1). Robust standard errors of the difference clustered by session.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

In Table 5 we present the average number of friends, acquaintances and connections (the sum of relatives, friends and acquaintances) each participant reports in the session where she is playing. We also report a feature of the network in the session, the friendship, acquaintanceship and connectivity densities, measured as the ratio of the total number of identified specific links in the session and the total possible number of specific links among connected people, i.e. those individuals that are identified as an acquaintance at least once by other player. In addition, we present a measure of leadership as the percentage of players identified as leader in each session, at least by one different player in the session.

Participants in Pozón in 2007 attended sessions with higher levels of connectivity but these differences are gone in the follow up. We are not able to explain rigorously these differences as we may find that the program balanced the difference between networks in the sessions in the follow up. We may also say that the recruitment process in 2007 facilitated participants to attend in groups of people with pre-existing links. For example, in Ciénaga in 2007, participants had fewer friends than in Pozón 2007 and 2008 but in 2008 participants in Ciénaga attended the sessions with more friends.

We also find that the percentages of players identified as leaders in the session is significantly lower in the follow up, suggesting that a certain fragmentation affects the patterns of leadership consolidation in the social context of the study. Interestingly, the rate of reported leaders in baseline and follow up is significantly higher than the proportion of ML (participants who declared to have been elected FA beneficiary representatives) in the cross-section sample (9.0% and 5.1%, respectively).²⁶

Surveys record stated preferences while experiment outcomes provide revealed preferences. The experimental literature on economic experiments and surveys does not find a consistent answer on whether these two are positively correlated.²⁷ On one hand Gächter et al. (2004) find a positive correlation between stated and revealed preferences. On the other hand Capra et al. (2008) and Cárdenas et al. (2013) find no evidence of correlation between participation in a charity and contribution in a public goods game.²⁸

4. Identification issues and empirical specification

To establish the relationship between the expansion of the CCT program and social capital we compare the behavior in the game of individuals who had been exposed to the program to those who had not. In July 2007, the program had been operating in the Pozón for over a year, while it had not been operating in Ciénaga. It should be stressed that the expansion to Ciénaga had already been decided so that the FA office could provide us with lists of potential beneficiaries in Ciénaga. In July 2008, we conducted the same game in both neighborhoods, when the program was operating in both. In addition we gathered information on individual and household level variables, as well as on the links (relatives and friends) among the participants to the sessions. By the time we conducted the follow up experiments, Ciénaga had been already targeted for over 12 months.

The expansion to Ciénaga allows us to implement a difference in differences (DiD) strategy to control for the possibility that the two neighborhoods differ in some unobserved dimension, provided that such ‘unobservable’ differences do not change over time. Although this is an untestable assumption we show below that most structural individual and household characteristics were stable within each neighborhood during the evaluation time.

²⁶ We find that 84.2% (for baseline) and 46.2% (for follow up) among those identified as leaders in the session are MLs.

²⁷ This could be explained with what Carpenter (2002) refers to as the idealized persona bias, i.e. a desire to appear as the person one would like to be rather than what they are, and/or the surveyor effect, i.e. the desire to answer in accordance to what one thinks the surveyor would like to hear or see.

²⁸ Our study is not exceptional, we present a comparison between behavior in the game and the traditional measures of social capital with traditional measures exhibit a weak correlation with such behavior. In fact, the correlations we find are not stable over time, which reinforces the concern of using these measures in the analysis of social capital.

The first identification issue raised by our study is the lack of randomization in the allocation of the program across neighborhoods. Based on conversation with program Administrators, it was felt that the initial allocation of FA was not driven by specific pre-existing differences between neighborhoods.

Although the availability of information on a wide range of pre-intervention variables allows us to control for the possibility that the two neighborhoods differed in the level of social capital for reasons different from the operation of the program, it is possible that there are unobserved differences that we could not control for. This motivated us to perform a follow up study a year after the first stage of the study. As discussed in the introduction, in 2008 both neighborhoods were exposed to the program so that, under some assumptions, it is possible to use the data from our second experiment to estimate these unobserved differences and correct the estimates derived from considering the difference between treatment and control in 2007.

The approach we take is essentially a DiD one. The only difference relative to a standard situation where at baseline both treatment and control are not exposed to the program while at follow up only the treatment are, is that in our case only one neighborhood was exposed at baseline, while both were exposed to the treatment at follow up. Unlike in the standard case where unobserved differences between the two groups are estimated using baseline data, we use the follow up data to do the same thing.

For our approach to be valid we need a number of assumptions. First, we need to assume that, conditional on observed variables, unobserved differences between the two neighborhoods are constant over time and enter additively. This is what is usually referred to as the common trend hypothesis. Unfortunately, it is not possible to test this assumption because of the lack of retrospective information at the individual or neighborhood level. However, we discuss below why we think this is a reasonable assumption to make on the basis of the available descriptive statistics.

Second, we need to assume that, after a few months of exposure, the impact of the intervention on social capital does not change with exposure. That is, if the program affects social capital, it does so by moving, after a few months, the social capital to a new level. This assumption is necessary to allow us to identify the unobserved differences in social capital between the two neighborhoods using the 2008 data when the program had operated for over 2 years in Pozón and only for just over 11 months in Ciénaga. Such a hypothesis is plausible as intensive interaction among beneficiaries starts early in the process. We assume that the interactions between beneficiaries in the first year of exposure to the program are sufficient to bring the level of social capital at a new equilibrium level that does not change after this first initial period.

To improve efficiency of our analysis, we can also control for a number of variables that are unaffected by the program. The common trend assumption we make to use the DiD approach, has to hold conditional on these variables.

In order to model the probability to contribute to the public good, we run a simple probit regression, where a single value index Y_{ist} is modeled as follows:

$$Y_{ist} = \alpha + \beta X_{ist} + \gamma D_{i07} + \delta T_{i07} + \lambda (1 - D_{i07} * T_{i07}) + \theta S_{st} + v_{st} + \varepsilon_{ist} \quad (1)$$

where Y_{ist} is the individual decision to contribute to the group account in session s at time t ; D_{i07} is a dummy for Ciénaga (which is the *control* group in 2007); T_{i07} is a dummy for the year 2007, when Ciénaga was not exposed to the program; X_{ist} are individual observable characteristics, S_{st} are session level characteristics and ε_{ist} are i.i.d. Gaussian distributed error term with mean zero and variance $\sigma_\varepsilon^2 = 1$, independently of v_{st} , which are i.i.d., $N(0, \sigma_v^2)$. The parameter λ in the expression above captures the effect of the program on the single value index Y_{ist} and determines the impact of the program on the probability to contribute to the public good. The time dummy captures common trend effects by exploiting the difference in contribution rates between 2007 and 2008 in Pozón (assuming that any FA impact had reached a stable level already in 2007); the neighborhood dummy captures fixed effects by exploiting the difference in contribution rates between Pozón and Ciénaga in 2008 (assuming that any FA impact had reached a stable level in both neighborhoods then).

Another potentially important issue is related to our sampling method. As we discussed above, due to logistical and data availability constraints, the recruitment approach was different across in 2007 and 2008 and, in 2007 was different between treatment and control neighborhoods. In both neighborhoods and both data collection stages the sample of participants in session includes individuals that self-selected for participation into the experiment by responding to an invitation. Hence the study is not based on a pure random sample and results might be non-representative of the whole beneficiary population. However the design was established to ensure that the self-selection bias affected the treatment and control group in a similar way at each stage of the study. Table 2 above constitutes an important and reassuring piece of evidence in this respect. As we mentioned, whilst the two neighborhoods appear to be different in a number of observable dimensions, these dimensions are virtually identical over time. If the selection process had been substantially different in Ciénaga in 2007 and 2008 and had been the same in Pozón, then this would have resulted in the observed differences between the two samples to change over time.

We conclude this discussion with a consideration about statistical inferences. Observations within a small neighborhood could be correlated because neighbors share common characteristics besides the assignment into treatment and control. However, the DiD approach does take into account the presence of a systematic difference between the two subsamples. As it turns out, for many outcomes, the intra-class correlation coefficient within neighborhoods is relatively low at 0.010.²⁹ On

²⁹ Individuals within neighborhoods are no more similar than individuals in different neighborhoods (in part for our sample size), and we effectively assigned 1390 individuals to treatment or control. We can reject that we have only two independent observations.

Table 6

Program impact on cooperation – first round.

Independent Variables	I	II	III	IV
Dependent variable: 1 if player contributed to the group account in the first round				
Dummy <i>Ciénaga</i>	0.105 [*] (0.060)	0.084 (0.057)	0.048 (0.062)	0.064 (0.052)
Dummy 2007	0.089 [*] (0.050)	0.090 [*] (0.049)	0.064 [*] (0.037)	0.096 ^{***} (0.033)
Program impact	0.306 ^{***} (0.044)	0.293 ^{***} (0.046)	0.241 ^{***} (0.043)	0.233 ^{***} (0.034)
Basic controls – participant characteristics	No	Yes	Yes	Yes
Session characteristics	No	No	Yes	Yes
Session composition (info within session)	No	No	No	Yes
Observations	1390	1384	1384	1384

Marginal effects reported. Robust standard errors, clustered by session, in brackets after probit estimation.

^{*} Significant at 10%.^{**} Significant at 5%.^{***} Significant at 1%.

the other hand, the behavior of participants at a given session is affected by many common factors, ranging from the identity of the game master, to the time of day to the general atmosphere in the session. Therefore we account for associations among observations within sessions (clusters) and cluster our standard errors at the session level.

5. Do CCTs build social capital?

We identify the effects of FA on our experimental measure of social capital by comparing first the contribution rates to the group account in our VCM in the two neighborhoods in 2007 and 2008. Then, we analyze the impact of the program on different and more traditional measures of social capital (Chattopadhyay and Duflo, 2004; Jones and Olken, 2005).

5.1. Identifying the effect of FA on the willingness to contribute to a public good

In Table 6, we pool together the 2007 and 2008 data to identify the effect of FA on the probability to contribute to the group account, after controlling for a variety of observable variables (the complete set of results is presented in Table 8), using the probit model in Eq. (1) in all four columns. In Column I we do not add any controls. In column II we control only for individual characteristics such as the age and education level of the participant. In Column III we control for session characteristics. An issue that worried us considerably on the field was the possibility that individuals who played in early sessions would 'contaminate' other individuals that were about to enter subsequent session by talking to them and commenting on the game. While we tried to avoid these contacts as much as possible and instructed the subjects not to talk to subsequent players, some contacts were unavoidable. For this reason, we control for the sequence order in which a particular session is played in a day and in the full sequence of sessions and also for results in previous sessions.³⁰ We do find these session effects to be significant. However, their introduction does not change the size of the marginal impact, partly because these effects were present in both treatment and control sessions.

Under the assumptions discussed above, the coefficient λ in Eq. (1) can be interpreted as the impact of the program and its estimates are reported in the third row of the table. The impact is large and significant at the 1% level.

The positive coefficient on the 2007 dummy indicates (under the maintained assumptions) the presence of a negative common trend in our measure of social capital. Finally, the positive coefficient on the *Ciénaga* dummy indicates that unobserved differences in social capital (not related to the presence of the program) are higher in *Ciénaga* than in *Pozón*.

The coefficients in the first column are the results of mean participation rates that are higher in *Pozón* than in *Ciénaga* (when the result was operating in both neighborhoods) and they flip in 2008 when the program was operating in both. Therefore the bias in estimating the impact of the program using only the data from 2007, as in Attanasio et al. (2009) would be negative. Notice, however, that the coefficient on the *Ciénaga* dummy becomes statistically insignificant once we control for observable variables. Notice also that if one thinks that the impact of the program takes more than 12 months to develop and that in 2008 its impact was stronger in *Pozón* than *Ciénaga* because the program had operated longer in the former than in the latter, then the estimates we present in Table 6 would be biased downwards.

In the specifications in Column III and IV, the point estimates of the impact of the program are now lower, but they are still significantly different from zero at the 5% level. The main message of the table, therefore, is that FA had a strong impact on social capital; at least as measured by the behavior in the game we played in Cartagena. This result seems to be robust to the presence of systematic differences between the two neighborhoods we have compared, both in terms of observable

³⁰ Particularly, in order to control for contiguous sessions correlation, we include in specification (IV): a) a dummy for the first session each day; b) a variable capturing the deviation from the neighborhood mean of the average contribution to the public account in the previous 2 sessions, and c) the order (number) in which the session came about in the full sequence of sessions within the neighborhood.

Table 7

Program impact on traditional measures of social capital.

Probit marginal effect of the program impact ^a			I	II	
Civic Participation	Membership in at least one organization		−0.070 (0.071)	−0.116 [*] (0.069)	
	Participation in neighborhood decisions		0.236 ^{***} (0.049)	0.221 ^{***} (0.054)	
	Participation in the neighborhood meetings		0.158 ^{***} (0.046)	0.111 ^{**} (0.050)	
	Vote Local Elections (2003 and 2007)		0.159 ^{***} (0.055)	0.134 ^{**} (0.058)	
Voting behavior	Vote Presidential Elections (2006)		0.171 ^{***} (0.061)	0.095 (0.062)	
Trust and cooperation perception	Trust	Most people	0.005 (0.029)	0.003 (0.027)	
		Few people	0.127 ^{**} (0.056)	0.116 [*] (0.062)	
		None	−0.136 ^{***} (0.051)	−0.122 ^{**} (0.057)	
	Perception within the community	Cooperation	0.082 ^{**} (0.040)	0.099 ^{**} (0.043)	
		Reciprocity	0.013 (0.047)	0.009 (0.045)	
		Selfishness	−0.104 ^{**} (0.050)	−0.118 ^{**} (0.052)	
		Number of player's relatives		0.034 (0.023)	−0.185 ^{***} (0.049)
		Number of player's friends		1.255 ^{***} (0.139)	0.671 [*] (0.380)
Individual social integration	Number of player's acquaintances		1.097 ^{***} (0.347)	2.350 ^{**} (1.026)	
	Number of player's connections		2.386 ^{***} (0.352)	2.836 ^{***} (1.028)	
Player is identified as leader in the session			0.136 ^{***} (0.041)	0.122 ^{***} (0.044)	
Basic controls – Participant's characteristics		No	Yes		
Session characteristics		No	No		
Session composition (info within session)		No	No		
Observations		1390	1384		

Coefficients for the marginal effects in probit specifications I and II, the dependent variable being the one listed on the left hand side. Robust standard errors, clustered by session, in brackets after probit estimation.

^a Coefficients for the measures of individual social integration are OLS estimates.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

and unobservable variables. Given the magnitude and robustness of the estimate, it doesn't seem plausible that this result is driven exclusively by differential trends in unobservable across Ciénaga and Pozón.

5.2. Impact on traditional measures

We now analyze the impact of the program on the measures of social capital we summarized in [Tables 4 and 5](#). We implement Eq. (1) to model the probability that an individual engages in a 'pro-social' activity (e.g. participation in neighborhood decisions) with a probit regression³¹ in which we include the same individual control variables that we considered in the range of specifications for the decision to contribute in the game in [Table 6](#). [Table 7](#) reports the impact of the program (the coefficient λ) on each of these measures of social capital.

A few social capital measures are statistically significant across both specifications. The clearest effect is on 'individual social integration' variables as well as on 'voting behavior'.

The effect on 'civic participation' is still positive but mixed: it is still positive except for participation in at least one civic organization (where coefficients are negative but not significant, consistent with [Pellerano \(2008\)](#) who, considering the rural program, observed that participation into the EC seemed to substitute for participation in other organizations).

Finally there is a clear effect on perceptions about trust and cooperation. Whilst the program does not seem to create positive perceptions about other people's attitudes toward trust and cooperation, it does seem to reduce the negative ones. This is a noteworthy result that sheds light on the manner in which the CCT can affect beliefs.

³¹ For the measures on Individual social integration we use a linear regression model.

Overall, Table 7 depicts a picture that complements the one arising from Table 6 in terms of impact of the CCT program.³²

6. The determinants of social capital

The evidence we have presented so far seems to indicate that FA had a strong impact on different dimensions of social capital. In the process of getting this result, in Section 5, we have controlled for several variables, at the individual, household and session level, to ensure that the results we obtained could not be explained by differences in these variables between the two neighborhoods considered. The coefficients on these variables are interesting on their own, as they are informative about the main determinants of cooperative behavior. In this section, we discuss the coefficients we obtained on the various variables we considered as determinants of the behavior in the game.

6.1. Individual and session variables effects on social capital

In the first panel of Table 8, we report the coefficients on the individual and household level variables in the regressions that were used to compute the impact of the program as reported in Table 6. Age is an important determinant of cooperation: older players seem to be more likely to contribute to the group account. The few men who participated (mainly in 2007) in the game were less likely to cooperate. As for household level variables, we find that players with a large household size tend to cooperate more. Interestingly, we do not find any education effects and very limited effects of other household level variables, including various wealth indicators.

Several of the session level variables are significant determinants of cooperation. Whilst men are less likely to contribute, the presence of a man in the session seems to induce cooperation. Players in the first session of the day were much more likely to cooperate, while the total number of players in a session had a negative effect on cooperation. Finally, when we consider (in Column IV) another set of variables controlling for the *composition* of the sessions, we find that participants in a session with a high percentage of the highest level of education (i.e. secondary education) within the session and a session socially distant with respect to other sessions in the neighborhood (in terms of the level of education of its participants) will be more willing to contribute to the public good.³³

We also note that, although the three game administrators (who were rotated across the sessions in both neighborhoods) were instructed to read exactly the same protocol and their behavior in the sessions differed (necessarily) only when answering the questions posed by the participants, we find strong ‘administrator’ effects.

6.2. Leadership and social capital

An important feature of the FA program is the importance that the role of leaders acquires in the community activities and therefore their role in strengthening social ties with the communities and becoming channels of wellbeing improvement (Latham and Saari, 1979; Bass, 1991).³⁴ In this subsection, we investigate whether the presence of a ML in the session affected individual decisions. Table 9 presents the results we obtain if we consider, among the determinants of individual behavior, the presence of a ML among the participants. Cooperation is positively associated with the presence of at least one ML in the session. The marginal effect is large at around 13.2%, when we control for session characteristics and composition (specifications VII and VIII), and above 16% when we do not control for session these variables (see specifications V and VI). The Program impact has a lower coefficient but still positive and significant. We can propose that one of the channels through which cooperation succeeds in the community is their leader. The results we obtain on the other control variables are not different from those presented in Table 8 and are reported, for completeness, in Appendix C in Table C2.

In addition, being an ML may be a determinant of the attitudes to cooperate, not only to the beneficiary that was elected as a ML but also to those who may be considered leaders by the group despite of not having an official title. In Table C3 in the appendix we present the results when we include whether the individual is considered as a Leader in the group and the percentage of participants who consider this particular individual as one of their leaders in the group.

³² The question arises as to whether Table 7 says the effect of the CCT is indirect, via the variables captured by the survey, or direct. From Table A2 we observe there is no significant link between the survey variables and the behavioral one, which provides some external validity to the claim that the program has an impact on cooperation. Having discussed the plausibility for lack of correlation between stated and revealed preferences, this should not raise a concern.

³³ Despite considering our network measures as measures of social capital instead of variables that explain cooperation, we explored the role of preexisting family and friend networks among the beneficiaries in the decision to contribute. Among all the specifications we attempt by adding different combinations of the network measures to specification (IV) in Table 6, only found a significant association between Friendship density of the session and the decision to contribute (i.e. marginal effect of 0.538, significant at 1%).

³⁴ Brune and Bossert (2009) find a positive effect of an intervention based on leadership development on a wide array of social capital measures from the World Bank's Social Capital Assessment Tool in Nicaragua. Macours and Vakis (2014) found that social interactions with nearby leaders positively affected human capital and productive investments in a CCT in Nicaragua.

Table 8

Controls for willingness to cooperate in first round (independent cross section).

Dependent variable: 1 if player contributed to the group account in the first round		II	III	IV
<i>Basic controls – participant characteristics</i>	1 if the player is a woman	0.118** (0.058)	0.156*** (0.030)	0.158*** (0.028)
	If the player has a partner	–0.002 (0.036)	0.006 (0.033)	0.008 (0.033)
	Level of education (0–5)	–0.001 (0.010)	–0.007 (0.009)	–0.005 (0.009)
	Household size	0.016*** (0.006)	0.012** (0.006)	0.013** (0.006)
	Ground floor (house)	–0.045* (0.025)	–0.031 (0.023)	–0.030 (0.023)
	Number of years living in the neighborhood	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
	1 if the player's home has water pipe access	0.043 (0.035)	0.038 (0.032)	0.044 (0.031)
	1 if the player's home has sewage	–0.039 (0.029)	–0.031 (0.028)	–0.024 (0.027)
	1 if the player's home has no electricity	0.128 (0.072)	0.086 (0.071)	0.096 (0.072)
	1 if the player is the head of household	0.019 (0.039)	0.022 (0.039)	0.024 (0.040)
	Age	0.005*** (0.001)	0.005*** (0.001)	0.005*** (0.001)
	1 if the player is unemployed	–0.017 (0.057)	0.007 (0.058)	0.008 (0.058)
	1 if the player has her own housing	–0.041 (0.027)	–0.049* (0.025)	–0.048* (0.025)
	1 if player belongs to the fifth quintile per capita income	0.047 (0.031)	0.036 (0.030)	0.041 (0.030)
	1 if the player has a phone	–0.001 (0.038)	–0.001 (0.038)	–0.005 (0.038)
	Number of rooms in the house	–0.018 (0.014)	–0.011 (0.014)	–0.010 (0.015)
	1 if the player is displaced (self-declared)	0.025 (0.032)	0.015 (0.032)	0.013 (0.032)
	1 if the player declares she understood everything	–0.048 (0.029)	–0.043 (0.030)	–0.044 (0.030)
	1 if the household receives support from another institution	–0.063** (0.029)	–0.054** (0.027)	–0.052* (0.027)
	Number of players in session		–0.030*** (0.009)	–0.037*** (0.008)
	1 if there is at least one man in the session		0.166*** (0.043)	0.201*** (0.044)
	Experimenter n°2 (female) in 2007		–0.049 (0.052)	–0.071* (0.043)
	Experimenter n°3 (male) in 2007		0.121** (0.047)	0.135*** (0.043)
<i>Session characteristics</i>	Experimenter n°2 (female) in 2008		0.099* (0.058)	0.096** (0.049)
	First session in the day		0.080** (0.040)	0.095*** (0.035)
	Behavior in the 1st round of the last two sessions ^a		–0.316* (0.162)	–0.226* (0.124)
	Number of sessions held before (t)		–0.020*** (0.003)	–0.019*** (0.003)
<i>Session composition</i>	Percentage of participants with less than secondary complete			0.370*** (0.109)
	Mean absolute deviation - level of education in the neighborhood			0.198* (0.103)
Observations		1384	1384	1384

Marginal Probit for II–IV. Marginal effects reported. Robust standard errors, clustered by session.

^a Calculated as the Mean of previous 2 Sessions' Deviation from the average contribution in the neighborhood.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

Table 9

Role of the leader in the first round.

Independent variables	V	VI	VII	VIII
Dependent variable: 1 if player contributed to the group account in the first round				
Dummy <i>Ciénaga</i>	0.103 [*] (0.056)	0.083 (0.051)	−0.003 (0.042)	0.028 (0.039)
Dummy 2007	0.056 (0.050)	0.059 (0.048)	0.060 [*] (0.036)	0.091 ^{***} (0.031)
Program impact	−0.202 ^{***} (0.070)	−0.188 ^{***} (0.070)	−0.107 [*] (0.065)	−0.121 ^{**} (0.056)
1 if there is at least a ML in the session	0.165 ^{***} (0.062)	0.166 ^{***} (0.058)	0.144 ^{***} (0.040)	0.132 ^{***} (0.041)
Basic controls – participant characteristics	No	Yes	Yes	Yes
Session characteristics	No	No	Yes	Yes
Session composition (information within session)	No	No	No	Yes
Observations	1.390	1384	1384	1384

Marginal probit. Marginal effects reported. Robust standard errors, clustered by session, in brackets after probit estimation. We do not use the dummy indicating whether participant is a ML: it is collinear with program impact and the coordinator 2 in 2007. We would have to exclude the data from *Ciénaga* in 2007 as the program was not operating there and, as a consequence, there were no ML in those sessions and the DID would not be feasible.

^{*} Significant at 10%.

^{**} Significant at 5%.

^{***} Significant at 1%.

7. Conclusions

Our main purpose is to examine, by combining survey and experimental methods, the effect of a social intervention on individual and group behavior and their ability to obtain better social and economic outcomes. There are two main difficulties in testing rigorously the hypothesis that CCTs increase social capital. The first lies in the difficulty in obtaining a quantitative measure of social capital. The second is the standard evaluation problem of observing the counterfactual to a given intervention.

We addressed the first difficulty implementing a public goods game. Our main measure of social capital is based on the behavior during this game, whose rewards, for the individuals in our sample, are salient. [Attanasio et al. \(2009\)](#) started our analysis by comparing the level of cooperation in two similar neighborhoods in 2007 when the CCT program we have been studying was operating only in one of them and show that the level of cooperation we observe in the ‘treatment’ community is considerably higher than in the ‘control’ community. In 2008, after the program had been expanded to the control community, we retake the measurements taken in 2007 and find no strong difference between cooperation levels in the two neighborhoods. By using a difference in difference approach, we control for unobserved differences in the social fabric of the two communities and find evidence to support the hypothesis that the introduction of the CCT we have been studying did indeed increase social capital.

The other focus of the paper is on how to measure social capital. While we implemented a VCM game and used observed individual behavior in the game as our main measure, we also collected information on self-reported behavior along a number of dimensions that have been used in the literature as measures of social capital. We show that while these measures roughly correlate with ours, they are also affected by measurement error, which makes the identification of the impacts of CCTs on social capital harder to detect. Despite this, with our DiD approach we found a positive effect of the CCT on participation in neighborhood decisions, participation in the neighborhood meetings and a negative effect of the CCT on the perception that “it is not possible to trust in anybody” and in the perception that “in the community people only care about themselves”. We only found a weak effect of the CCT on the number of connections subjects have within the session. We also explore the role of preexisting family and friend networks among the beneficiaries in an alternative effect of a CCT. Although it has been documented how pre-existent networks may determine the effect of a CCT program on the main outcome ([Angelucci and De Giorgi, 2009](#); [Angelucci et al., 2010](#); [Macours and Vakis, 2014](#)), it has not been the case with the effect of our particular CCT.

It must be noted that the external validity of our results might be limited by some of the sample selection issues we discussed above: our sample was made of self-selected individuals that responded to an invitation to participate to game sessions and the formation of groups was also left to individual self-selection into sessions depending on the timing of arrival at the game site. Despite these caveats, and whilst results are not representative to the whole population of beneficiaries of FA, they provide compelling evidence about the potential influence of CCTs on social capital formation and the mechanisms through which such effect is likely to realize.

The effect that CCT programs such as FA might have on social capital, probably because of the many social activities linked to the program, has long been debated. Some policy makers even hypothesized that the program might have a negative impact on social capital creating division between beneficiaries and (marginal) non-beneficiaries. Our evidence represents one of the first quantitative assessments of the impact of a CCT on social capital and provides strong support for the hypothesis that social capital is improved. The policy implications of these findings are obvious. This impact should be

added to the benefits that conditional cash transfers could deliver. Of course, one has to be careful and understand that the manner in which a CCT is delivered might be key to the social capital impacts we see.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at <http://dx.doi.org/10.1016/j.jebo.2015.04.004>.

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