ESSAYS ON EXPERIMENTAL ECONOMICS IN THE FIELD: LESSONS FOR PUBLIC POLICY

Thesis presented
by

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I, **Sandra Viviana Polania Reyes** confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis and in the Statement of conjoint work.

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Abstract

Traditional economic theory has found in experimental economics a source of both support and challenge. Similarly, policy makers have found the need to understand the behavioral underpinnings of their target populations. To achieve efficiency policies need to exploit difficult concepts such as social capital, social preferences and crowding effects.

Despite its importance for development and poverty alleviation, social capital remains elusive as its parts, social norms and beliefs about others, willingness to cooperate and individual connections, move together. We disentangle social capital using an artefactual field experiment, to our knowledge the first of its kind to use a minimum effort coordination game to measure coordination. Together with network information, cooperation from a Public Goods game and traditional survey measures of social capital, we report a positive relation between a CCT program and the ability to coordinate in the most efficient outcome.

Social preferences help explain deviations from Nash equilibrium in game outcomes. An enduring challenge is to identify types in the presence of heterogeneity. Using data from a common pool resource (CPR) game in the field with 1,095 individuals (students and CPR users) we estimate a structural model including preferences for altruism, reciprocity and equity. Our type identification uses a latent class logit model based on exogenous determinants. A competing explanation is the existence of a cognitive factor (e.g. Quantal Response Equilibrium). We do not find much evidence for cognitive heterogeneity, and instead a great deal of behavioral heterogeneity. Types seem robust out-of-sample.

Workfare programs might crowd out private labor effort. We analyze the impact of a Colombian workfare program called Job in Action to shed light on: (1) labor crowding-out, (2) gains in household labor income and (3) persistence in gains after the program has finished. We see no evidence of effort crowding out. We find large positive transfer benefits and a positive effect on individuals' outcomes even six months after the program ended.

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I. Disentangling Social Capital: Lab-in-the-Field Evidence on Coordination, Networks and Cooperation

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Abstract

Although social capital has been considered of the utmost importance for development and poverty alleviation by governments and NGOs, it remains a complex and elusive concept. Different dimensions of social capital form part of the puzzle: cooperation is an individual other-regarding preference; social norms stem from beliefs about others' behavior; and individual connections arising in networks allow us to build such beliefs.

To disentangle social capital we conducted an artefactual field experiment at the inset of a Conditional Cash Transfer program in an urban context within a developing country. The first challenge is to disentangle the cooperation element from the coordination one. To our knowledge this is the first time this is achieved; we do so by conducting a minimum effort coordination game with Pareto ranked equilibria. Willingness to cooperate is teased out using a public goods game. By controlling for the density of network information we capture the role of connections, which is the third element of the mixture. We contrast our findings with traditional survey measures of social capital we also gather such as voting behavior, trust and membership in associations.

Our identification strategy allows us to assess whether exposure to the program could help individuals to overcome strategic uncertainty and achieve the most efficient equilibrium in the coordination game. We provide experimental evidence on the positive role of a monetary incentive on the existence of a social norm that allows individuals to overcome a coordination failure. We rule out confounding factors as individual socio-economic characteristics, social capital accumulation, willingness to cooperate and connectivity.

The effort choice is given by how connected the individual is and other members' socio-economic characteristics. We estimate a structural choice model of the individual decision to coordinate, which highlights the role of beliefs about others' behavior: high effort is only sustained under high beliefs. The regressions suggest that the CCT program helps overcome the coordination failure through different channels, and the structural model points to the beliefs channel.

JEL Codes: C92 (Experiments Laboratory, Group Behavior), D70 (Analysis of Collective Decision-Making); D78 (Policy making and implementation); H41 (Public goods); Z13 (Social norms and social capital)

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1. Disentangling Social Capital

Social policies may improve economic outcomes through changes in the structures of social relationships. Social capital refers to the set of resources inherent to those relationships and their structure within a community (Uphoff, 1999). It can be understood in terms of social norms and networks (Putnam et al., 1993, Coleman, 1990) and manifests itself through patterns of pro-social behavior or pro-social preferences (trust, reciprocity, and cooperation) (Christoforou et al., 2014).

Three elements lie at the core of social capital (Coleman, 1987, 1988): coordination, cooperation and networks [1]. Coordination and the social network determine the effectiveness of the social norms, as coordination reflects the ability to exploit Pareto-improving opportunities in the presence of uncertainty and the features of the social network provide the environment in which that ability is likely to emerge. The third element at the heart of social capital is the ability to overcome free-riding incentives in real-world situations (Coleman, 1988). These situations, especially salient among poor communities, share the same game-theoretic representation of a public goods game: in the process of building social capital, cooperative outcomes are subject to "free-riding"[2] incentives (Coleman, 1990).

The ability to solve cooperation and coordination problems by a community is key in many developing countries where weak institutions and a weak rule of law are prevalent. In particular, coordination on efficient outcomes is key to solve collective action problems and market failures thus bring economic development, build efficient institutions and avoid conflict (Rousseau, 1755, Coleman, 1987, Matsuyama, 1996, Hoff, 2001, Hoff and Stiglitz, 2001, Bowles, 2004, McAdams, 2009) as well as promote entrepreneurship (Adler and Kwon, 2002).

Despite its importance for development and growth, and after a boom of the literature of social capital in development economics (see Woolcock, 1998, Woolcock and Narayan, 2000, Fukuyama, 2001), social capital is still considered an elusive concept (Adler and Kwon, 2002, Brunie, 2009)[3]. There is still debate on the validity of

[1] In their seminal work, Putnam et al. (1993:167) define social capital as those "features of social organization, such as trust, norms, and networks that can improve the efficiency of society by facilitating coordinated actions" (emphasis added). Coleman (1988) was clear enough to define social capital by its function: "It is not a single entity but a variety of different entities, with two elements in common: they all consist of some aspect of social structures, and they facilitate certain actions of actors within the structure" (p. S98).

[2] See Samuelson (1954), Olson (1965), Grossman and Hart (1980), Ostrom (1998). Evidence on social program evaluations supports this claim (Adato et al., 2005, Fearon et al., 2009, Avdeenko and Gilligan, 2014).

[3] For example, in contrast to the structural (or functional) approach of social capital by Coleman (1988) and Putnam (1990), Fukuyama (2001) defines social capital as "an instantiated informal norm that promotes cooperation between two or more individuals". This paper also aims to contribute to settle this disagreement.

the survey measures and other qualitative measures to capture different dimensions of social capital (see Portes and Landolt, 2000, Putnam, 2001a, 2001b, Narayan and Cassidy, 2001, Kawachi et al. 2008). In contrast to survey measures, choice experiments are incentivized, involve real behavior and eliminate sources of heterogeneity that may confound estimation of preferences from life choices.

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[4]. Public good and trust games have been used in a variety of different situations, both urban and rural[5]. This is not the case for coordination games[6]. The main contribution of this study is the use of a new experimental measure of social capital based on the behavior in a coordination game with social networks. We also use a Public Goods (PG) game and traditional survey measures of social capital. This is the first study focused on behavior in these two games to disentangle beliefs from social preferences.

Any given form of social capital that helps to coordinate and achieve more efficient outcomes improves wellbeing and is thus highly valuable to the community. Conditional Cash Transfers (CCT) programs have become one of the most popular interventions in developing countries. There is a strong line of research showing that CCTs are successful in their goals (i.e. nutrition, education and health). It turns out most CCTs have a component of new interactions among the beneficiaries within the communities that should lead to building or strengthening social ties (Putnam, 1995) and enforcing pro-social norms. Though this makes CCT a natural policy intervention to study social capital accumulation, the literature remains sparse in that sense[7]. Yet the design of CCT programs originally included a strong 'social capital' component, making such analysis all the more relevant.

We conduct an artefactual field experiment[8] and combine experimental and non-experimental data to study the role of networks, leadership, other-regarding preferences and communication in the ability to coordinate in the context of a CCT program in a developing country. We examine the relation between the exposure to the program, behavior in the games, individual network information and traditional measures of social capital.

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[4] Carpenter (2002), Carpenter et al. (2004), Gaechter et al. (2004), Karlan (2005), Cárdenas et al. (2009), Fearon et al. (2009), Voors et al. (2012), Gilligan et al. (2014).
[5] Our Public Good game design has been used extensively in Colombia (Cardenas and Jaramillo, 2007, Cardenas et al. 2013, Attanasio and Pellerano 2012b, Attanasio et al. 2009, 2015) and in other countries (Cardenas et al. 2013, Barr et al., 2014).
[6] Using coordination games as instruments to identify social norms is very recent (Krupta and Weber, 2013, Erkut et al. 2014).
[7] For rigorous studies on whether CCTs affect social capital see Attanasio et al. (2015).
[8] According to Harrison and List (2004) taxonomy of field experiments.

We observe that the length of exposure of the beneficiary to the program matters. First, we find a large and significant relationship between an individual's exposure to the program and their ability to coordinate: longer exposure is positively correlated with choosing the Pareto-dominant equilibrium in our coordination game. The association remains robust to controlling for confounding effects such as wealth, other-regarding preferences and the group's network. The degree of the participant in the network is positively related to the ability to coordinate. We find also a weak and positive relation between willingness to cooperate and the ability to coordinate.

Second, use a Quantal Response Equilibrium (QRE) approach and estimate a structural choice model in order to relate beliefs with exposure to the CCT program and the ability to overcome the coordination failure.

Our paper contributes in several ways to a recent stream of research combining survey and experimental measures of social capital. First, to our knowledge this is the first study that implements an *n-person* coordination game with more than *two* ranked equilibria in the field in order to measure the presence of a social norm as the beliefs. Our game design aims to be a better adaptation to the field of the coordination problem. Agents must coordinate on a common action with the group's success depending on the least favorable action of a team member. The minimum effort game is an adaptation of the stag hunt game[9] (Holt, 2007) with multiple choices and subjects. This is a more realistic design if we want to examine how groups of more than two people coordinate and use a social norm to reach the most efficient outcome.

Brooks, Hoff and Pandey (2014) is the only other study so far that uses a coordination game in the field to study cooperation and coordination. They conducted a stag hunt game to examine the role of culture on the efficiency of coordination among men from different castes in India[10]. Our study differs in several ways with theirs. First, although their design is a useful abstraction of the ability to coordinate in the textbook, most of interactions in the field are n-participants and usually within a broader set of options. We use an eight-player-three choices coordination game with three ranked equilibria. Second, Brooks et al. (2014) is indeed the first study on

[9] The stag hunt game is a two-player, two-choice coordination game with a payoff-dominant equilibrium and a risk-dominant one.

[10] For a very similar experimental design in India, see also Chakravarty et al. (2015). Boschini et al. (2014) examined gender-based focal points or conventions by using a battle of the sexes game (i.e. a coordination game with multiple equilibria but the equally efficient and different allocations) and use a random sample of Swedish citizens. Bosworth (2013) also uses a Stag Hunt game in the lab as a measure of social capital, and find that traditional survey measures of trust is related to behavior in the experiment with 20 students. This positive relation is consistent with the findings of Anderson et al. (2004) and Thoni et al. (2012) on a Public Goods game and using students and a random sample from the Danish population, in the lab and online, respectively.

culture and coordination efficiency in the field and ours is the first one looking at our design as a potential measure of effectiveness of social norms in the field. Third, we do exploit the role of social networks on coordination. Fourth, though culturally diverse their sample is small and composed of only men. Our sample is larger, heterogeneous in a wide range of socioeconomic variables and focused on the poorest of the poor.

Second, we relate trust to cooperative behavior as other-regarding preferences whereas the literature has to a large extent focused on behavior in experimental trust games (For an extensive review see Thoni et al. (2012): 636). Third, by combining survey measures, network information and experimental data, we not only look to shed light on the behavioral validity of such measures but also to clarify in the debate multifaceted nature of social capital (Dasgupta and Serageldin, 1999, Brunie, 2009) and how different dimensions of social capital can be considered in empirical analysis.

Fourth, despite the extensive experimental literature on coordination in the lab, evidence on coordination in the field is almost nonexistent and there is no evidence on coordination within a policy intervention or with policy implications. Most of the literature focuses on the structure of the coordination game and how it is possible to achieve efficiency (i.e. learning, social networks, monetary incentives). This paper is the first that uses a coordination game to measure social norms within a CCT framework and our results are confirm the importance of the social component of these interventions at the community level[11].

Fifth, this is the first study that uses a QRE approach in order to estimate a structural choice model of the individual decision to coordinate with field data and a minimum effort game with more than two equilibria and two players.

Finally, this study also contributes to the small but growing literature that conduct behavioral experiments with real-world leaders in a natural field setting (Attanasio et al. 2015, Kosfeld and Rustagi, 2015, Jack and Recalde, 2015, Polania-Reyes, 2015).

The rest of the paper is organized as follows. Sections 2 and 3 explain how the behavioral experiments measuring coordination and cooperation are useful to disentangle social capital. Section 4 examines the role of networks on collective action and offers a brief introduction to the institutional setting of the CCT. Section 5 describes the experimental setting and data on group performance and group and

[11] This study is among the pioneers on social norms measurement literature. Mackie and Moneti (2014) examine about 200 publications on social norms and development; only 14% discuss norms-measurement methods, most of them on qualitative data and none of them on expectations.

individual characteristics. Section 6 quantifies the relation between the CCT and the ability to coordinate while accounting for confounding factors such as wealth, other-regarding preferences, network information and socio-economic environment. Section 7 presents our results from the structural model estimation. Finally, Section 8 offers concluding remarks.

2. Measuring a Social Norm with a Coordination game

Collective action may facilitate coordination in a strategic environment with multiple Pareto-ranked equilibria (see for example, Bryant, 1983, Hirshleifer, 1983).[12] In order to examine equilibrium selection in the presence of collective action (Harsanyi and Selten, 1988), we consider a very well-known game in the literature. The minimum effort coordination game introduces a conflict between payoff dominance and risk dominance[13]. An individual's payoff depends on her own effort as well as on the minimum effort of the group. The higher the minimal effort, the higher every member's payoff is. In contrast to social dilemma games (e.g., PG games), any common effort level chosen by all group members is an equilibrium, so it is in no one's interest to deviate upward or downward from the common effort. Hence choosing the most efficient (i.e., payoff-dominant) equilibrium is a problem of coordination rather than one of cooperation.

One important coordination device is found in social norms, which arise as an equilibrium selection criterion (Schelling 1960, Kandori, Mailath, and Rob, 1993, Horwitz, 1990)[14]. Social norms are now proposed by theory of law as efficient alternatives to solve collective action problems as they internalize negative externalities and provide signaling mechanisms (Ellickson, 1991, Posner, 2002, McAdams, 1997). A social norm is a pattern of behavior such that individuals prefer to conform to it on the condition that they believe that most people in their reference network i) conform to it (i.e. *empirical expectations*)[15] and ii) *think* they **ought**

[12] Harsanyi and Selten (1988) present payoff dominance as based on collective rather than on individual rationality. However, Most theoretical work on equilibrium selection in coordination games concerns 2x2 games (Harsanyi and Selten, 1988, Carlsson and van Damme, 1993, Kandori et al., 1993 and Young, 1993, Anderson et al., 2001).

[13] Notion introduced by Harsanyi and Selten (1988). This game is also called the weakest-link game. Many economic and organizational contexts feature situations where the worst component of a product or process determines its overall quality (See Camerer and Knez (1994), Foss (2001))

[14] This game-theoretical approach of social norms introduces them as customary rules of behavior that coordinate our interactions with others. Once a particular way of doing things becomes established as a rule, it continues in force because we prefer to conform to the rule given the expectation that others are going to conform (Schelling, 1960; Lewis,1969, Young, 2008)

[15] The definition of the coordinative role of institutions and practices of society is similar to Gauthier (1986)'s: "An institution or practice is coordinative if each person prefers to conform

conform to the norm (i.e. *normative expectations)* (Bicchieri, 2006, 2014)[16]. Given that decisions are private and individual in the ME game, the game is able to capture empirical expectations when all players coincide in any equilibrium and normative expectations when all players coincide in the equilibrium that is best for the group. The combined force of normative and empirical expectations makes norm compliance a superior choice and makes defection, in case the others are expected to cooperate, a bad choice indeed, be it because punishment may follow, or just because one recognizes the legitimacy of other's expectations (Sugden 2000).

Related literature in other social sciences undervalues the potential of coordination games to capture social norms (Bicchieri and Muldoon, 2014). However, our particular ME game and our quasi experimental approach in measuring the effect of the exposure to the CCT allow us to indicate how the Pareto-dominant and risk dominant equilibria are attained and how expectations –built via the CCT, become self-fulfilling.

Empirical expectations are key for social norms to evolve and they are mostly based on observations of what individuals in the reference group have done in the past (Bicchieri, 2014). In addition, in repeated encounters, people have an opportunity to learn from each other's behavior, and to secure a pattern of reciprocity that minimizes the likelihood of misperception (Bicchieri and Moldoon, 2014)[17]. On the other hand, communication is key in making efficient coordination a focal point (Blumme and Ortmann, 2007, Choi and Jihong, 2014)[18]. The CCT program may have allowed these observations occur in the community and time of exposure to the program provides the time frame that beneficiaries need to be able to see the benefits of overcoming coordination failures. This is consistent with the hypothesis that social norms emerge in small, groups in which people have ongoing interactions with each other (Hardin, 1982, Axelrod, 1986, Bicchieri, 1993). The CCT program may have also provided a normative framework that changed as well normative expectations.

In order to exist a social norm needs people who collectively believe it exist and also trust that all people believe that everyone should obey that norm. Hence, a social norm that helps a group to overcome a coordination failure exists when individual beliefs coincide, which in our game will be made evident by observing the

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to it provided (most) others do, but prefers not to conform to it provided (most) others do not" (in Jeske and Fumerton (2011: 81))

[16] These are conceptually equivalent to descriptive norm and injuctive norm in psychology (Cialdini and Trost, 1998, Fishbein and Ajzen, 2010).

[17] This is defined as Common Knowledge by the literature of team reasoning (Sugden, 2003)

[18] Although there is experimental evidence that shows otherwise (Clark et al. 2001, Burton et al. 2005).

highest level of effort. If individuals choose the payoff-dominant strategy in a one-shot minimum effort game in a group of more than two players, we would be able to interpret their decision as a belief that the others will choose that strategy, and hence that the group holds the same belief[19].

This particular coordination game has been used extensively in the lab[20]. In fact, experimental evidence supports the prediction that a risk-dominant equilibrium will be favored over the Pareto-dominant equilibrium – i.e. a coordination failure (Van Huyck et al. 1990, Camerer, 2003: 403, Anderson, Goree and Holt, 2001) unless there is a social norm or institution that re-directs behavior (Bowles, 2004)[21]. In the presence of strategic uncertainty, the risk associated with not knowing how your opponent will play the game, risk dominance may yield an individually rational outcome that is not efficient but safe. However, payoff dominance will guarantee a collectively rational and efficient outcome. Then, we would expect that a group rich in social capital is more likely to coordinate on the efficient outcome. Since communication and interactions make coordination possible, in the field we are able to examine the role of the individuals in the group, their interaction history, the members of his/her group and their social skills when coordinating and hence, solving collective action problems.

i. Our ME game

As an experimental design, consider an adaptation of Van Huyck et al. (1990) and the following values of parameters: $n = 8$ (eight players), $E = 3$ (three choices). Then the payoffs can be described by the matrix presented in Table 2.1 below. Players simultaneously determine their level of effort $e_i \in \{1,2,3\}$. The payoff

[19] This is different from focal points or conventions (Schelling, 1960, Lewis, 1969, Young, 1993, Sugden, 1995), a descriptive norm in which only empirical expectations are relevant – people don't expect others to respond if they stray from the convention. Conventions could be measured with a coordination game with multiple equilibria but equally efficient and different allocations e.g. battle of the sexes game. Lewis (1969:89, 95) treats the Pareto dominant equilibrium as the social contract that is not a convention.

[20] See Ochs (1995), Cooper (1999) and Devetag and Ortmann (2007) for nice surveys on payoff-asymmetric coordination games in the lab.

[21] Among the determinants of achieving the payoff-dominant equilibrium there is evidence in the lab on group size and cost of effort (Van Huyck et al. 1990, 1991), number of interactions (Berninghaus and Ehrhart, 1998, Knez and Camerer, 2000, Parkhurst et al. 2004), randomness in matching (Keser et al. 1998, Schmidt et al. 2003, Goree and Holt, 2005), information about other player's actions (Berninghaus and Ehrhart, 2001, Weber, 2006), leadership (Brandts and Cooper, 2007, Brandts et al. 2007, Gillet et al., 2011, Cartwright et al. 2013), advice (Brandts and MacLeod, 1995, Kuang et al. 2007), monetary incentives (Brandts and Cooper, 2006, 2007, Goeree and Holt, 2005), action set (Van Huyck et al., 2007), non-monetary incentives (Van Huyck et al., 1997, Bornstein et al. 2002, Blume and Ortmann, 2007, Rhodes and Wilson, 2008, Dugar, 2010, Cason et al. 2012), subject-pool characteristics (Dufwenberg and Gneezy, 2005, Engelman and Norman, 2010, Chen et al. 2014, Stoddard and Leibbrandt, 2014).

of player i is determined by the minimum level of effort in the group minus the cost of effort he incurs

(1) $$\pi_i^{ME} = \pi(e_i, e_{-i}) = 3\left(1 + e_{[1]} - \frac{2}{3}e_i\right)$$

where $\mathrm{e}_{[1]} = \min\{\mathrm{e}_1, \ldots, \mathrm{e}_\mathrm{n}\}$ and $\mathrm{e}_{-\mathrm{i}}$ is the $(n-1)x1$ -7x1, vector containing the other players' effort levels.

Any common level of efforts $e_1 = \ldots = e_n$ is a Nash equilibrium, and such equilibria are Pareto-ranked. In the presence of strategic uncertainty, risk dominance may yield an individually rational outcome that is not efficient but safe. However, payoff dominance will guarantee a collectively rational and efficient outcome. Given the strategic uncertainty, individual i maximizes the expected utility of his payoff in the game.

There are three Nash equilibria in this game and only the belief about other's choosing a certain level of effort will motivate the individual to exert that level of effort. This game shows the tension between payoff dominance and a secure and inefficient equilibrium. The only problem faced by the players is to coordinate in either one of the three Nash equilibria. Since the most efficient equilibrium – Pareto superior (i.e. to exert the highest effort level), it is reasonable to assume that players would prefer that outcome. However, choosing the highest level is still risky since if for some reason the other player defects, others are left with the lowest possible payoff. Hence, if there is uncertainty of the other player's action it might be better to defect.

We confirm this negative externality occurs in this particular coordination game when we compare the average individual payoffs of the players who chose the lowest level of effort ($4) with the average payoffs of those who chose the highest ($2.4) and medium level of effort ($2.8), both differences being significant at 1%. Comparing the payoffs of choosing the medium level with and the high level, those who chose the medium level of effort had higher payoffs than those who chose the highest level of effort (difference of 0.34, significant at 10%)[22].

ii. Capturing beliefs: a pseudo-Quantal Response Equilibrium approach

One of our main points is that the tradeoff between risk dominance and payoff dominance is directly linked to beliefs materializing as a social norm. In order to

[22] Choosing the lowest level of effort implied lower payoffs for those who did conform to the 'efficiency' norm. By contrast, the average payoffs of the groups who did conform to 'efficiency' social norm (i.e. all members in the group chose the highest level) was significantly larger ($6, by construction) than the individual payoffs of the groups in which the minimum effort was medium ($3.4) or low ($1.9), so conforming to the social norm had benefits to everyone.

estimate the probabilities of choosing the most efficient effort level we estimate a Quantal Response Equilibrium (QRE) model (McKelvey and Palfrey, 1995, 1998)[23].

Under the symmetry assumption commonly imposed on the QRE, each player uses a mixed strategy p, which itself induces a distribution p_{min} over the minimum effort of all opponents. The expected payoff from choosing $e_i \in \{1,2,3\}$ is thus given by

(2) $$E[\pi(e_i, p_{min})] = 3\sum_{k=1}^{3} p_{min}(k)\left(1 + \min(k, e_i) - \frac{2}{3}e_i\right).$$

The QRE condition relates the probability of playing a given strategy to the relative advantage of the expected payoff. Using the conventional logit specification, the logit equilibrium is characterized by the following

(3) $$p(e_i = j) = \frac{e^{\lambda E[\pi(j, p_{min})]}}{\sum_{k=1}^{3} e^{\lambda E[\pi(k, p_{min})]}}$$

where $\lambda \in [0, \infty)$ captures the degree of payoff-maximizing behavior (a higher value of λ meaning more payoff responsiveness – less noise, and with $\lambda = 0$ the density function becomes uniform over its support and behavior becomes random. random behavior).

Anderson et al. (2001) show that the QRE of a 2x2 coordination game is unique, even with a continuum of Nash equilibria. They also prove that the limit point of the QRE as $\lambda \to \infty$ is the risk-dominant equilibrium, given that the cost is greater than $1/8$. Having said this, we will abstract from the uniqueness of the global equilibrium, which is the global minimum (of all $p \in [0,1]^3$) of the mean squared error function

(4) $$\left\| p(j) - \frac{e^{\lambda E[\pi(j, p_{min})]}}{\sum_{k=1}^{3} e^{\lambda E[\pi(k, p_{min})]}} \right\|_2.$$

Instead we will consider the sensitivity of pseudo-equilibria to initial conditions (p^0). Pseudo-equilibria are defined as local minima of the mean squared error function. Different initial conditions lead to different minimization regions, and hence different pseudo-equilibria. Initial conditions matter for a highly non-monotonical objective function. In our problem, they also matter to the extent they describe beliefs and hence the social norm[24].

[23] The QRE model allows agents to make mistakes and assumes that agents take into account the possibility that others are making mistakes when drawing inferences from their actions. Since the ME game has three equally possible equilibria, we use this approach due to the different predictions it offers.

[24] This approach is consistent with the assumption by Mailath (1998) and de Paula (2013) on the equilibrium selection mechanism for the econometric analysis of incomplete-information games with possibly many equilibria. “If an equilibrium is established as a mode of behavior

Given that the QRE path converges to the risk-dominant equilibrium p=(1,0,0), our interest is to understand under what initial conditions (beliefs, i.e. values of p^0) a high value of p_3 , the probability of choosing the highest level of effort, in pseudo-equilibrium is sustained. To understand how sensitive the problem is to initial conditions we begin by comparing the effect of different initial conditions on p_3. Figure 1 shows that a high value of p_3 requires a very high initial condition p_3^0. This is in line with our thesis that reaching the Pareto dominant equilibrium is a matter of social norms, which are captured by initial beliefs about other players' actions. Only if others are perceived to be very likely to play the Pareto dominant equilibrium will the equilibrium be sustained.

3. Network Information and the Environment: a CCT Program

There are many advantages of social networks in community life, from exchange of goods and services to the transmission of information, values and norms (see Jackson, 2008). Networks are also important on effort individual decisions (see Jackson (2010), List and Rasul (2011) and the references therein for studies that use field experiments in combination with social network data). For example, friends may conform to a social norm and status may be a determinant of individual behavior (Bernheim, 1994), individuals may be averse to inequality within the network (Fehr and Schmidt, 1999; Charness and Rabin, 2002).

Network structure becomes an important factor to take into consideration when overcoming collective action problems and constitute a key component of social capital's definition and measurement. The structure of the network, the position of individuals in and their degree it determine, to a great extent, if collective action is successful or not (Gould, 1993, Jackson and Watts, 2002, and Jackson et al. 2012)[25].

There is a wealth of theoretical work supported by extensive evidence in that lab on the coordination problem of collective action on costly links and how information in the structure of the network affects individual's decision to coordinate. However, there is no evidence from the field on how the network attributes of each individual (e.g. number of people known, family ties) explains individual effort decision in situations problems where individuals do not have a single action that

by past play, custom, or culture, this equilibrium becomes a focal point for those involved. When observed games are drawn from a population that is culturally or geographically close, sharing similar norms and conventions, one would expect this assumption to be adequate." (de Paula, 2013:120)

[25] For evidence of the structure of the social network and coordination games in the lab see Goyal and Vega-Redondo (2005), Casar (2007), Jackson (2008), Choi and Lee (2014) and Charness et al. (2014).

constitutes a dominant strategy. To our knowledge this is the first study that looks at the relationship between individuals' features and their decision to coordinate. This would shed light in understanding the determinants of coordination in the field.

In our experiment, 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 for every known person whether the player considered that person to be trustworthy. We also asked the player to choose who would be considered as a leader in the community within the session. For every session, we are able to construct a relationship matrix that describes the shape of existing networks among players[26].

With the self-reported data on network connectivity within the session, we built a connectivity index for every player, given by whom the individual is acquainted with, the type of relationship and whether the individual considers the other person trustworthy: 3 points for each friend and relative, 2 points for each trustworthy acquaintances and 1 point for each untrustworthy acquaintances.[27] We ordered the individual's score and allocated the participants into three different groups: group A, with the first eight participants with the highest score, the most connected individuals; group C, with the last eight participants with the lowest score, the least connected and group B, the remaining players[28].

i. The Conditional Cash Transfer program: Familias en Acción

Familias en Acción is a CCT program including a social component, articulated around periodic meetings of beneficiaries, called Care follow-up Meetings (EC) [Encuentros de Cuidado][29]. Although participation to these meetings is not compulsory 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 discuss a topic or 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 Mother Leader [*Madre Líder*] (ML) who is in charge of communication with the local office and is also

[26] The fact that the network structure is not randomized but endogenous to the experiment is also a novelty in the literature of coordination and networks.
[27] The performance of the index was robust to different specifications.
[28] The size of group B varied according to the size of the session. The average size of group B was 8.7 (s.d. 0.73).
[29] For additional information on the program see the Appendix.

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.

As the theory of social capital suggests, the interaction requirement might create an environment where social networks operate 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 group" by perceiving a strong identification with the program (Tajfel, 1982) and their power to act 'together' (Warren, 1998). This not only facilitates group decision-making but also increases the willingness to intervene for the common good, which eventually could lead to overcoming collective action problems by enforcing pro-social norms (Coleman, 1990).

In addition, our CCT may affect the beliefs about others' behavior. 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 could create and enforce social norms so beneficiaries' perception of trust or cooperation is also modified.

4. Measuring Willingness to Cooperate: a Public Goods game

The Public Goods game is a Voluntary Contribution Mechanism (VCM). Other studies employ a dichotomous VCM game comparable to the one we use here: Attanasio et al. (2012) in 70 rural municipalities in Colombia, Cardenas et al. (2013) in 6 Latin American cities, Barr et al. (2012) in Uganda, Barr et al. (2014) in Albania and Alzua et al. (2014) in Mali[30]. The game 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 wellbeing of

[30] For more details on the experimental design see Section 2 in the appendix and on the role of willingness to cooperate on the measurement of social capital see Attanasio et al. (2009, 2015).

the entire group is improved[31]. 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 socially optimal outcome. However, if all in the group invest their token in the private account, the group will be worse-off than if all the members invested in the group account, which is the social optimum. The situation constitutes a typical social dilemma.

The possibility of cooperation within a group is determined by multiple factors such as repetition, communication, punishments or rewards and inequality in the payments[32]. 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. Other-regarding preferences 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 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.

In the first round, each player has to decide where to invest her token. The second round is a repetition of the first, except that the players are allowed to discuss for ten minutes before making simultaneously their private, anonymous decision[33]. We use behavior in the second round as a measure of how effective the opportunity to communicate could be in increasing willingness to cooperate and solving a social dilemma in the community.

5. Data

We analyze individual behavior by means of our coordination and cooperation games, making extensive use of the survey data we collected at the end of the experiment. In what follows we describe the recruitment process, experimental procedures and descriptive statistics of our sample.

[31] The dichotomous VCM makes the game easily understood by subjects and also time effective.
[32] See Attanasio et al. (2015) for a recent review.
[33] Communication is completely unstructured and during the discussion, the players can talk about whatever they want but they cannot leave the room. No one, except the experimenter, knows the other players' contributions in the first round.

i. Sampling, recruitment and allocation into sessions

We recruited the participants to the game with the help of the local office of FA (Enlace Municipal) in two neighborhoods -*Pozón* and *Ciénaga*- in the city of Cartagena, Colombia[34]. The program was already operating in both neighborhoods and we were able to contact beneficiaries directly. Invitations were sent to 500 randomly selected participants from the FA beneficiaries list in each neighborhood. 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 four days). We assumed a response rate of 70% and expected to run 14 sessions with 350 attendees in each neighborhood. The actual attendance rates for the new participants were 105.1% and 98.9% in Ciénaga and Pozón respectively. Our sample consisted of 714 participants, 710 of which had not participated in any game before.[35] This led to a total of 29 sessions with people who had never played before any game (14 in Pozón and 15 in Ciénaga), the average size being 24.7 participants.

Conducting lab in the field experiments in large cities presents many challenges in terms of costs, time, recruitment and attendance (Ñopo et al., 2008, Candelo and Polanía-Reyes, 2008). Since the sessions were scheduled on a short notice (less than a week) we gave the beneficiaries as much freedom to choose the session that suited them best as we could. This could have led to relatives or neighbors choosing the same sessions, if they both happened to be invited. In fact, some invited beneficiaries arrived to the session in groups[36]. The fact that individuals are not randomly allocated into sessions allowed us to explore the role of social networks on the effect of exposure to the program and social capital: We were able to obtain enough variation in terms of the density and quality of the network across sessions (See Table 5.5).

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,

[34] For more details on the CCT program a why we chose Cartagena, see Section 1 in the appendix.

[35] In 2007, we conducted only the VCM with 676 participants. In 2008, in addition to the sessions with new participants we also invited individuals who had participated in the public goods game in 2007. In 2008 we conducted a total of 53 sessions, 26 in *Pozón* and 27 in *Ciénaga*. 24 of these sessions had only former participants. The VCM protocol followed in the two years was identical. These old participants were meant to attend to sessions with only old participants. However, 4 people managed to stay in new sessions.

[36] 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 four days we conducted the experiments with participants in *Pozón* and the following four days with participants in Ciénaga.

although we put lot of effort in avoiding these contacts. Session level controls for this potential issue include the average outcome in the two sessions immediately before and an indicator variable for those sessions which were the first ones of the day.

ii. Experimental procedures

Participants were invited to come to the local public school in their neighborhood. After collecting their identification documents and checking their names on the recruitment lists, subjects in each session were given a random identification number and seated in semi-circle in a classroom where the instructions of the games 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 while they had a snack.

Having collected the individual network data and assigned every participant to a group according to the score we generated (see Section III above), we proceeded with the coordination game. The instructions of the coordination game were read and explained. After making sure the participants understood the game, subjects were seated in three circles, back facing, in a different classroom. they proceeded with their decision, simultaneously and without communication. The results were announced to each group in private. Afterwards we announced the results of the PG game, the subjects took a survey that gathered information on a wide range of socio-economic features.

A session lasted on average two hours. Once the session ended participants were paid their earnings[37] based on the decisions in the experiments. On average each participant earned US$10.04 (COL$17595), which just over the value of the daily minimum wage[38].

iii. Characteristics of short and long exposure beneficiaries of the CCT

In Table 5.1 we report the means by the main characteristics of the sample of a set of individual and household level characteristics (i.e. socio-economic characteristics, perception of wealth and CCT related variables). We report results separately for participants corresponding to the two levels of program exposure as of 2008: short exposure means less than a year in the program, whereas long exposure means over one year in the program)[39]. Participants come from very poor families, with low

[37] Those who chose not to participate were paid on their way out. All recruited people were given a show-up fee of US$1.1, to induce credibility and subsidize their transportation from and to their home or workplace.

[38] The daily minimum wage was COL$15383 for 2008. Source: www.banrep.gov.co

[39] The samples coincide almost exactly with the limits of *Pozón* (long exposure) and *Ciénaga* (short exposure) apart from 41 observations from *Pozón* that were subject to short exposure. This is due to new households in *Pozón* who became beneficiaries in the 2007 urban expansion (see more information in section i.in the appendix).

levels of income and education.

Both long and short exposure groups are very similar in important dimensions such as education level, asset tenure and income, although some small (though relevant) differences emerge. Participants with long exposure were more likely to have a partner, own the house where they lived, to own durables and to receive governmental aid by other social programs. In addition, the perception that the household income is above others' income in the neighborhood is higher for those with long exposure to the program. For instance owning a house and a mobile phone and monthly income may be affected by the fact that participants with long exposure had received the benefits from FA and other programs for a longer time. While there might be a causal effect of the program, we also found counterintuitive results such as lower likelihood of access to electricity and a landline. Participants who have been enrolled in the program for less than a year are significantly more likely to be head of household, have been living in the neighborhood for more years and have more years of education. In this case there might be a reverse causation: latter expansions of the CCT program might be explicitly targeting sectors that were previously not in. Whilst some of the effect of FA may be through its impact on socio economic outcomes, the relatively small size of differences and the presence of counterintuitive associations do not give strong support to conclude that all of its impact is through that channel.

In Table 5.2, we report the measures collected from the Minimum Effort game. We present the results separately by length of exposure (short or long) to the program. In all relevant variables that indicate the ability to coordinate on the efficient outcome, players with long exposure show significantly higher measures with +28% participants choosing the highest level of effort and +25% groups actually achieving the Pareto-efficient equilibrium. The percentage of individuals choosing the safe option was 26% higher among those with short exposure. While +26% short exposure participants chose the lowest level of effort and +35% short exposure groups achieved the risk-dominant equilibrium. This is consistent with the hypothesis that the longer the exposure to the program the better a community will coordinate.

In Table 5.3 we report behavior in the Public Goods game and experimental characteristics at the session level. We also report differences across levels of exposure in these cooperation measures. First, though the unique Nash equilibrium of the game is for individuals to invest their token in the private account, many individuals 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. However, the level of cooperation in the first round among the short exposure sample is significantly higher than in the long exposure one. In the second round, there is no significant difference in the cooperation variables. Finally, we observe that in the short exposure sample, the percentage of participants who had a perfect understanding of the Public Goods game was significantly higher. We would expect that cooperation should be higher in the Long exposure group than in the short exposure one. Attanasio et al. (2015) examine these intriguing effects by using a difference in difference regression analysis with data from 2007 and 2008, which controls for possible unobservable variables. They find that there was indeed a positive effect of the program in cooperation in the first round.

In Table 5.4, we report the descriptive statistics on several measures of social capital collected from the post-game survey and also report some characteristics related with the social component of the CCT. These measures are divided into four groups: variables based on individual participation to civic associations and neighborhood activities; variables reflecting voting behavior[40], variables derived from answers to questions about trust and perception of cooperativeness[41] and variables on attendance to the CCT meetings and the presence of the ML in the session.

In the first group, 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 organization, the percentage of participants who attend the meetings, perceive themselves as a leader or decision maker in that association, support the association with money or voluntary work and the number of hours per month spent in that association. Compared to what was observed in other studies (see Latorre López, 2004 and Polania-Reyes, 2005) the participants reported higher levels of participation in organized groups. Participants with long exposure are +15% involved in associations than participants with short exposure.

In the second group we report the percentage of individuals who voted in local and presidential elections, showing a 15% higher turnout among those with long exposure, which is consistent with some studies on voting behavior and CCT (see Zárate et al., 2013 and Nupia, 2012; for evidence on other countries see Attanasio et al., 2015).

[40] 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.

[41] 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.

In the third group, we report statistics derived from a question on trust[42]. Individuals may also have different perceptions on others' social preferences. They may perceive either a reciprocal ("most people help if others help"), selfish ("most people care only about themselves"), or cooperative ("most people help others unconditionally") behavior in their neighbors' actions. The levels of trust and perception about social preferences are no different across levels of exposure.

Finally, we report that the percentage of players that attended at least one meeting of care (EC) and the number of meetings the participant has attended to, are significantly higher in the long exposure group. Only 5 percent of the participants are self-reported as ML and at least a ML was present in 79% of the sessions.

In Table 5.5 we present the average number of friends, acquaintances and connections (the sum of relatives, friends and acquaintances) each participant reports in the session and within coordination group (i.e. A, participants with the highest connectivity score in the session and C, participants with the lowest score)[43]. We also report features of the in-session network such as 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 another player). In addition, we present a measure of leadership given by the percentage of players identified as an informal leader in each session (i.e. a person different to the ML), at least by one different player in the session. The fact that there are no statistical differences in terms of connectivity between levels of exposure indicates the recruitment process was successful

6. Relation between the CCT and behavior in the coordination game[44]

First, we look at the differences between the frequencies of choosing the risk-dominant and the Pareto-dominant outcomes in terms of exposure to the program.

[42] This question was adapted from the WVS source. We added an alternative "few people can be trusted".

[43] Table 5.5 also provides validity to the effectiveness of the score in allocating the most connected individuals into group A and the least connected in group C. We also find that the percentage of players identified as leaders is significantly lower in group C (15.9) than in group A (23.3, with a p-value of 0.00). Interestingly, the rate of reported leaders is significantly higher than the proportion of ML (participants who declared to have been elected FA beneficiary representatives) (5.2% and 5.1% respectively). We find that 46.2% among those identified as leaders in the session are MLs.

[44] In section 3 of the appendix we report the relation between traditional social capital measures collected in the survey introduced in Table 5.4 and exposure to the CCT. As controls we included willingness to cooperate, the ability to coordinate as well as network information and individual socio-economic characteristics. Among the socio-economic characteristics we include especially those characteristics we found to show differences between the short exposure and long exposure samples.

Figure 2 presents this comparison and also compares the differences among connectivity groups A, B and C. We observe that there is no difference across groups. However, there is a significant difference in exposure, regardless of which group the individuals were allocated to.

Second, we look at the relation between the effort decision and survey social capital measures. In Table 6.3 we report the relation between dichotomous and continuous social capital measures and the decision of effort in the coordination game, where -1 is the lowest level of effort and 1 is the highest level of effort. In the upper panel, we report the marginal effect of the effort decision in a probit regression model for each dummy social capital variable. In the lower panel, we report the effect of the effort decision in a linear regression model for each continuous social capital variable. We find no relation between the level of effort in the coordination game and social capital measures[45].

Third, we want to test the hypothesis that exposure is relevant for the ability to coordinate in the most efficient outcome. Our empirical specification has as its unit of observation individual i of group g in session s. We estimate the following partial proportional odds specification, where Y_{igs}
is the individual effort decision and D_i is a dummy for being enrolled in the program longer than a year,

(5) $$Y_{igs} = \alpha + \beta X_i + \gamma D_i + \delta N_{igs} + \lambda G_{gs} + \theta S_s + v_s + \varepsilon_{igs}$$

Where X_i are individual observable characteristics, N_{igs}, G_{gs} and S_s include individual network information and group and session level characteristics, respectively[46]. ε_{igs} are i.i.d. Gaussian distributed error terms with mean zero and variance $\sigma_\varepsilon^2 = 1$, independent from v_s, which are iid, $N(0, \sigma_v^2)$. Standard errors are clustered by session.

We treat the decision of the level of effort as an ordinal outcome as we the three possible equilibria are ordered from the least (i.e. level of effort 1) to the most (i.e. level of effort 3) efficient equilibrium. In order to estimate the ordinal model, we

[45] For the relation between the decisions in the PG game and social capital measures see Table A.4 a and b in the appendix. We find no relation on cooperation without communication whereas cooperative behavior after communication is related to helping others in the community, membership and participation in bonding and bridging associations. Table A.4c reports the results for network characteristics. We do find a positive relation between the network density (measured as friendships) and the traditional survey measure of trust and being considered a leader by other members of the network.

[46] N_{igs}includes number of friends, relatives and acquaintances in the group, S_s includes session size, a dummy if that was the first session of the day, and a dummy for one of the experimenters who conducted the session, G_{gs} includes a dummy if there is a man in the group, the average equilibrium in the group from the previous two sessions and the presence of a ML in the group.

applied Brant's test of parallel regression/ proportional odds assumption (see Long and Freese, 2006) and confirmed the assumption of parallel regressions is not met (we have a significant overall chi-square value)[47]. We also performed a likelihood-ratio test that confirms we cannot use an ordered logit model. However, it is not necessary either to implement a generalized ordered logistic model (Fu's, 1998) which sets all variables from the parallel-lines constraint since the assumption is violated only by one or a few of our independent variables, in particular the exposure to the program. We then fit a partial proportional odds model, where the parallel lines constraint is relaxed only for those variables where it is not justified[48].

Table 6.1 presents our results with eight different specifications. In the first 4 specifications we relate the effort decision to other experimental variables with the network information we used for the algorithm that allocated individuals into groups and the traditional measures of social that seemed to have an impact on the effort decision. Table 6.2, presents four specifications in which we control for participants' basic socio-economic characteristics, experimental variables at the session, group and individual level, and the factors we considered in Table 6.1.

The first panel in Table 6.1 shows the marginal effects of a partial proportional odds model for the decision to contribute to the lowest level of effort, the least risky decision and Pareto inefficient outcome. The negative coefficient for exposure means that the likelihood of coordinating on the least efficient equilibrium decreases when enrolment into the program is longer than a year. The second panel in Table 6.1 shows the corresponding results for the decision to contribute to the highest level of effort, the riskiest decision and the Pareto dominant strategy. The positive coefficient for exposure confirms the finding from the first panel.

There are confounding factors that affect the relation between the ability to coordinate as a measure of one dimension of social capital and the exposure to the program. In this section we will explore them.

i. Other-regarding preferences

The decision to exert the highest level of effort may be mediated by other-regarding preferences such as trust, altruism or reciprocity. For example, when people try to adopt a new norm, normative expectations may not be enough and individuals must also trust others to commit to the change. In fact, a player is aware that if she

[47] The proportional odds assumption states that our model with 3 categories is equivalent to 2 binary regressions with the critical assumption that the slope coefficients are identical across each regression.

[48] We used a Wald tests on each variable to see whether the variable meets the parallel-lines assumption. If the Wald test is statistically insignificant for one or more variables, the variable with the least significant value on the Wald test is constrained to have equal effects across equations.

chooses the highest level of effort, any other player may obtain a higher payoff by deviating from that strategy. We use behavior in the PG game as a proxy of such preferences. Specification IIa and IIb in Table 6.1 show that other-regarding preferences -measured as behavior in a cooperation game- is positively (negatively) related -although not significantly- with a high (low) individual level of effort.

We also explore how the decision to cooperate in rounds 1 and 2 is related to social capital measures. In Table A4a and A4b in the appendix, we report this relation for continuous and dummy survey measures, respectively. There is no relation between willingness to cooperate in the first round and traditional survey measures of social capital, with the exception of helping anyone with money, food or clothes, which constitutes altruistic behavior[49]. The decision to cooperate in both rounds is positive and significantly related to the presence of at least one ML in the session. In contrast, we find that willingness to cooperate in the second round, which would measure not only an individual's preference to cooperate but also the effect of cheap talk, is positively related with declaring that the player (usually or always) helps others outside the household by offering the seat in the bus, carrying packages or groceries and taking care of the house or the children. Finally, willingness to cooperate is negatively related to attendance to the FA meetings. In Table A4b we report the positive relation between the decision to cooperate in round 1 and in round 2 and continuous social capital measures (i.e. number of EC meetings, percentage of ML in the session and connectivity measures). Thus, we show in specifications IIIa-IIIb in Table 6.1 the role of network information in the effort decision with and without cooperation in order to avoid collinearity.

ii. Networks

A common limitation of most models of collective action is that they neglect that people can choose with whom they interact, which is known that is not random. Generally, people prefer to interact with people who are similar to them, and collective action is no exception. Empirical work has demonstrated that individuals who participate in collective action have more links to other participants than individuals who do not participate (Opp, 1989).

[49] To our knowledge the only evidence of positive correlation between behavior in the field and behavior in the lab (social capital related) in a PGG is Rustagui et al. (2010) on forest management activities and time spent on monitoring forest, De Oliveira et al. (2011) with amounts donated to neighborhood charities, Thoni et al. (2012) with survey measures on trust and fairness, and Barr et al. (2014) with elections on school authorities' accountability. With no correlation, Voors et al. (2011) with illegal commercial mining, logging, and hunting; illegal hunting of endangered species; support to forest conservation, Voors et al. (2012) with contribution to community project fund for the village and Cardenas et al. (2013) with participation in any social organization; attendance to their meetings; participation in their decision planning; hours in a month spent in them.

In the lower panel of Table 6.3 we report that the effort decision is significantly related to all the network measures which confirms we have to consider network information as a main counfounding factor. Table 6.1. specifications IIIa-IIIb we report that regardless of the density of the network (i.e. number of friends, relatives and trustworthy acquaintances in the session are identified by the player), players who were enrolled into the program more than a year before chose the Pareto efficient level of effort (difference is significant at 1%). While holding all other independent variables constant at their means, those players with an exposure of more than a year and having friends were 35% and 5% more likely to choose the highest effort level, respectively. In addition, those players with an exposure of more than a year and having friends were 25% and 3% less likely to choose the lowest effort level, respectively. This evidence on features of the network is consistent with the literature.

Once we control for the network features, the decision to cooperate is also positive (negative) and significant for those who choose the high (low) level of effort, respectively.

iii. Other dimensions of social capital – Traditional survey measures

In the upper panel of Table 6.3, we observe that the effort decision is only related to voting in presidential elections (a negative and significant relation) and declaring that the player usually has helped someone from another household with the groceries or carrying packages[50]. We include these relevant measures in specifications IVa-IVc in Table 6.1. Helping someone (which could be attributed to preferences for altruism) has the expected sign but is not significant: those who help others are more likely to exert more effort. However, voting behavior is positively related to the likelihood of choosing the lowest level of effort. This result is consistent with the literature of social capital which states that trust in formal institutions and the rule of law is related with a higher voter turnout in elections, whereas voter turnout rates in communities with a weak rule of law and relevance of informal institutions or social norms are lower.

iv. Leadership

Social status is relevant in the creation and transmission of social norms (Richerson and Boyd, 2004). In a coordination setting, a leader may have a strong influence on

[50] In Table A4c in the appendix we show the relation between the main network information (i.e. degree of friends, relatives and acquaintances the player identifies in the session) and dichotomous social capital measures. All of these are dummy survey measures and we report the marginal effect of the effort decision in a probit regression model for each social capital variable. Having friends and acquaintances reduces the probability to state that is not possible to trust in people in the community, which is a decrease in the perception of untrusting people in the community. These two network measures are also positively related with helping someone different from the household by taking care of the house or the children and with others declaring that the player is an informal leader in the session.

the equilibrium selection (Bala and Goyal, 1998, Eckel and Wilson, 2000, 2007). From Table 6.3, we find that only social capital measures related to the CCT program such as number of EC meetings and the percentage of ML in the session is significantly related to the level of effort. When including these measures in the analysis in specification VII (See Table 6.4b) they don't affect the effort decision. For example, contrary to behavior from previous coordination games in the lab (See Gillet et al. 2011, Foss, 2001, Brandts et al. 2014) we don't find a relation between being a ML, or the presence of a ML in the group, and the effort decision.

v. Basic demographics and wealth

We also consider the economic approach to social capital (Glaeser et al. 2002, Polania-Reyes, 2005) and examine the role of socio-economic characteristics at household and individual level in the individual effort decision. Table 6.2 reports the effect of the exposure to the program on the effort decision, when controlling for these individual characteristics. Overall, the effect is consistent with our previous specifications.

Specifications V-VII in Table 6.4 report the marginal effects of different socio-economic dimensions. First it reports individual demographic characteristics such as being a woman, age, level of education, number of years living in the neighborhood, whether the player is displaced, is the head of the household, has a partner or is beneficiary of another program different from FA. Then it reports housing conditions such as the number of people per room, if the housing is owned, if the housing does have electricity, water pipe access and sewage. Finally it reports wealth measured as assets[51], household income and household perception of wealth with respect to other households in the neighborhood.

The only characteristics with significant marginal effects at both levels of effort are having a landline and the individual perception of wealth. Having a landline will increase the probability to choose the lowest level of effort by 7% and decrease the probability of choosing the highest level of effort by 11%. This result would imply that having no land line would provide an incentive to strengthen their communication with others by more interactions or other means or the habit of effort with sometimes no reward by the player. In addition, an increasing perception of how rich is the household compared to others in the community will decrease the likelihood of the ability to coordinate.

vi. Experimental session variables

Specifications VI in Table 6.2 and Table 6.4 include session variables such as whether there is a man in the session, whether the player understood perfectly the

[51] The assets are landline, cellphone, sound-player and DVD player.

coordination game, a dummy of one of the experimenters and the size of the session. In addition, given the possibility of contamination among subjects of different sessions since participants in a session could talk to participants of the next session on their way in. Despite, our effort in avoiding that king of contamination effects in the field, we control for this possibility with the average level of effort in previous two sessions and a dummy whether that session was the first one on that day.

7. The effect of the program and the effect of beliefs

The QRE approach in section two enables us to evaluate the degree to which the theory explains behavior in the field[52]. In order to understand the effect of the program, we compute the pseudo-QRE separately for the subsample with low exposure to the program from that with high exposure to it. We calibrated λ with the objective of minimizing the mean squared error (MSE) between the distribution of efforts observed within the sample and the one predicted from the pseudo-QRE using λ[53]. We compare two pseudo-equilibria: the one implied by a high initial belief ($p_3^0 = 95\%$) and the one implied by a low initial belief ($p_3^0 = 33.33\%$). In Figure 3 below we plot the model outcome for each of the two groups.

In the panel b of Figure 3, we observe that the predicted equilibrium for individuals with high exposure is similar to the observed data. For those beneficiaries with low exposure, the actual data is very similar to the theoretical prediction.

As seen in the previous section, the program effect on coordination is notable, which is captured by the difference in observed distributions across the two figures. Again, low initial conditions cannot generate a prediction that accurately matches the real distribution: high initial conditions are needed to do so.

8. Concluding remarks

There is an increasing interest in measuring economic preferences using both choice experiments and surveys in order to identify relationships and causal effects of economic features related to prosocial behavior. However, the experimental literature on social capital has focused entirely on the dimension of cooperation and trust, omitting coordination. The main contribution of this study is the use of a new

[52] This approach is also used by Chen and Chen (2011) with a model of identity. They show that a salient group identity increases coordination on the Pareto superior outcome in the lab for a 2x2 ME game.

[53] In Table A.6 in the appendix we present the calibrated λ for the two possible scenarios (low beliefs and high beliefs as initial conditions) and whether the long exposure and short exposure samples. From Figure 1, we find that having high beliefs on others choosing the highest level of effort is not enough if there isn't a $\lambda > 0.6$ in order to obtain convergence to the Pareto-dominant equilibrium. Table A.6 tells us that only the long exposure group had such λ.

experimental measure of social capital based on the behavior in a coordination game with social networks. We also use other two well-studied measures in the literature (i.e. cooperation in a Public Goods game and traditional survey measures of social capital).

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[54]. Our study is in line with the literature in this sense.

Many CCT programs have important social components and, therefore, can be related to social capital measures. Our hypothesis is that coordination is an important aspect of social capital, which in our case is strongly affected by a CCT program. This study calls attention to the coordination dimension of social capital. We find a positive and significant relation between the individual effort decision and the exposure to a CCT program which has a social capital component. This relationship is consistent when controlling for all possible confounding factors. We also find that the degree of friends in the network is key to the ability to coordinate on the Pareto-efficient equilibrium[55].

Unfortunately our study is only a quasi-experiment, establishing the relation (but not the causation) between exposure to the program and ability to coordinate on the most efficient equilibrium. We do find the relation to be robust to controlling for potential confounding factors.

We use a QRE approach to support the validity of our minimum effort game as an instrument to measure a social norm in our particular sample. The most important question to ask about norms is what system of beliefs supports and defines norms. Once we understand these beliefs, we can tell whether the behaviors that we observe are norm-driven or not, measure the consistency between beliefs and behavior under different conditions, and make predictions about future behaviors (Bicchieri, 2014). We find that the theoretical prediction for long exposure beneficiaries is similar to the observed data. An interesting addition could be to use the QRE approach and estimate a structural model using a group-contingent social preference model similar to Basu (2006), McLeish and Oxoby (2007), Chen and Li

[54] On one hand Gächter et al. (2004) and Capra et al. (2008) find a positive correlation between stated and revealed preferences in the public goods game and trust game. On the other hand Cardenas et al. (2013) find no evidence of correlation between participation in a charity and contribution in a public goods game.

[55] Table A.3 in the appendix complements our analysis. We find a positive relation between exposure to the program and participation in neighborhood and program meetings as well as voting in presidential elections and participating in bonding and bridging associations (as defined by Woolcook, 1998).

(2009) and Chen and Chen (2011), where an agent maximizes a weighted sum of her own and others' payoffs, with weighting dependent on a group category of the other players. Those beneficiaries who have been exposed for a longer time are able to identify themselves as a group and behave 'altruistically' towards one another.

9. References

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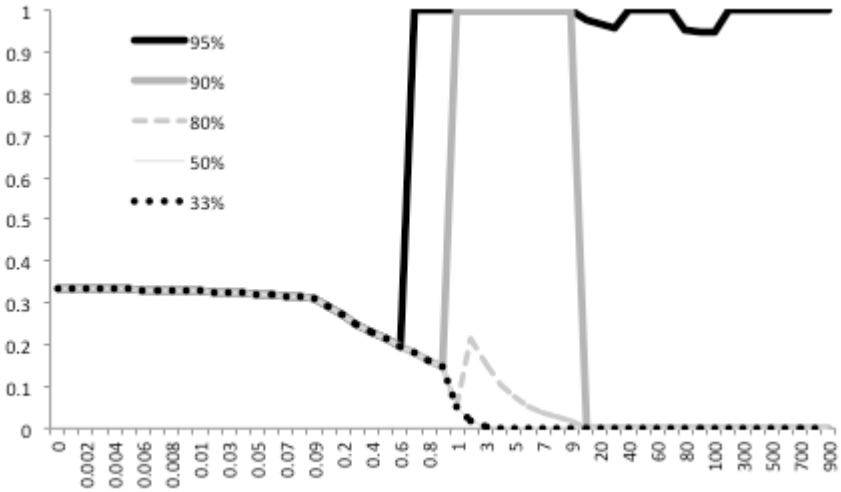
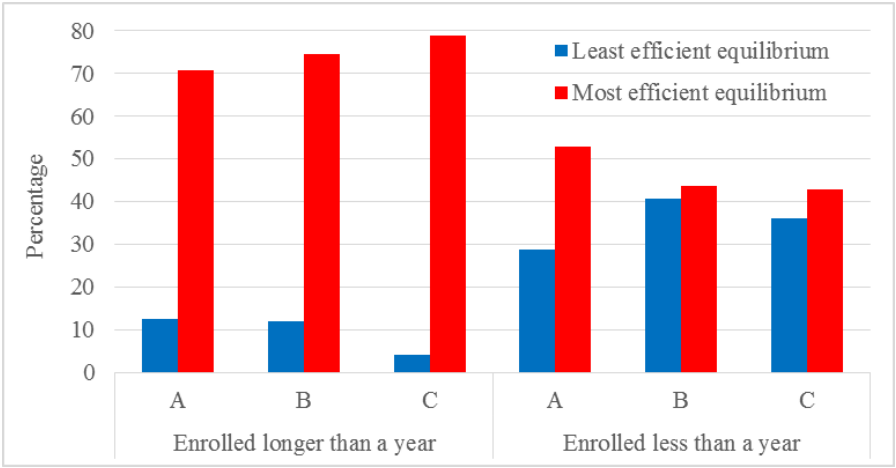
10. Tables and Figures

Figure 1. Pseudo-equilibrium p_3 as a function of λ



Note: Different series correspond to different values for p_3^0[56]

Figure 2. Exposure to the program and individual effort decision by players' degree allocation group.

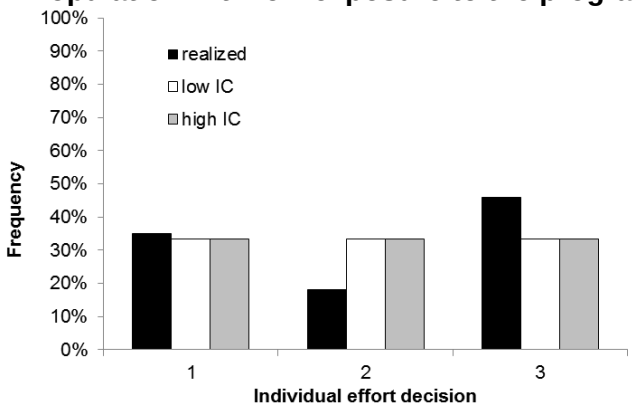


Note: A player's degree is the number of edges or relationships the player declares to have within the session. Every player has a weighed measure of her degree of friends, degree of relatives and degree of trustworthy acquaintances. Those with the highest measure are allocated to group A, those with the lowest measure to group C and the remaining ones to group B. We observe that regardless of the group, players with a higher exposure to the program coordinate into the Pareto optimal equilibrium whereas players with a lower exposure coordinate more in the risk-dominant equilibrium.

[56] p_1^0 and p_2^0 are calculated from p_3^0 as $p_2^0 = p_1^0 = (1 - p_3^0)/2$.

Figure 3. Predicted and realized effort distributions

a. Population with low exposure to the program



b. Population with high exposure to the program

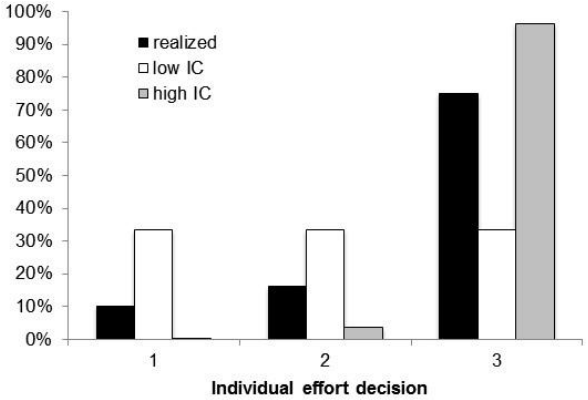


Table 2. 1. Coordination game. Payoffs table*.

		Minimum Effort Level chosen in the group		
		3	2	1
My decision (effort level)	3	$6	$3	$0
	2		$5	$2
	1			$4

*Values are in thousands of Colombia pesos.

Table 5. 1. Demographic characteristics of the participants by time of exposure to the CCT

	Variable	All	Long Exposure	Short Exposure	Difference
General characteristics	Percentage of female participants	98	99	98	1
	Average age (years)	36.2	36.8	35.8	1.1
	Years living in the neighborhood	18.5	14.9	21.2	-6.3***
	Percentage displaced	13	17	10	7**
	Percentage household head	33	24	40	-16***
	Percentage Single	11	10	12	-1
	Percentage married of civil partnership	72	78	68	10**
Educational level (percentage)	None (level 0)	3	2	3	-0
	Primary incomplete (level 1)	21	22	20	2
	Primary complete (level 2)	14	16	13	3
	Secondary incomplete (level 3)	33	35	31	4
	Secondary complete (level 4)	20	16	23	-6*
	More than secondary complete (level 5)	9	8	10	-2
Income variables	Percentage unemployed	4	3	5	-2
	Percentage with access to credit	71	72	70	2
	Percentage with access to formal credit	22	24	22	2
	Per. with food insecurity level (high)	9	7	10	-2
	Per capita monthly income (US$)	32.0	33.3	31.0	2.3
Dwelling characteristics	Household size	5.67	5.59	5.72	-0.13
	Number of people per room	2.98	3.21	2.81	0.40***
	Percentage dwelling with dirt floor	28	32	25	6*
	Percentage owning own house	59	69	52	17***
Public services (percentage)	Water by pipe	88	92	84	8**
	Sewer system	46	68	30	38***
	Does not have electricity	3	6	2	4**
	Land phone	18	8	24	-16***
Assets (percentage)	Mobile phone	72	78	68	9**
	DVD player	33	37	31	6
	Sound player	31	37	27	10***
Perception that HH income	...is above the lowest possible	70	72	69	2
	…is above the highest possible	43	47	40	7***
	…is above the average	56	59	54	5**
	is in which percentile	33	35	31	4**
CCT measures	Lives in *Pozon*	48	100	10	90***
	Years since enrollment	2.06	3.42	1.05	2.37***
	Years since first payment	1.87	3.20	0.88	2.32***
The participant has received (different from FA)	Any other governmental aid	28	39	19	20***
	Gov. aid for her house	14	27	4	22***
	Gov. aid for productive tasks	3	4	1	3**
	Gov. aid for childcare	8	7	10	-3
	Gov. aid for health and nutrition	17	32	6	26***
	Gov. aid for education	15	28	5	22***
Observations		714	346	368	714

Robust standard errors, clustered by session. * Significant at 10%; ** significant at 5%; *** significant at 1%. 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)

Table 5. 2. Behavior in the Coordination game

Variable	All	Long Exposure	Short Exposure	Difference
Average effort decision [b]	2.34	2.65	2.11	0.54***
	(0.11)	(0.12)	(0.13)	(0.17)
Percent of players that chose 1	24	10	35	-26***
	(0.5)	(0.4)	(0.7)	(0.8)
Percent of players that chose 3	59	75	46	28***
	(0.6)	(0.8)	(0.6)	(1.0)
Average Minimum effort in the group [b]	1.54	1.88	1.28	0.61***
	(0.13)	(0.21)	(0.10)	(0.21)
Percent of groups with a ME of 1	64	43	79	-35***
	(0.7)	(1.0)	(0.7)	(1.2)
Percent of groups with a ME of 3	17	31	6	25**
	(0.6)	(1.2)	(0.3)	(1.1)
Size of the group B[a]	8.70	8.79	8.62	0.17
	(0.12)	(0.13)	(0.19)	(0.22)
1 if the player understood that the best outcome	0.66	0.70	0.63	0.08**
is everyone to choose level of effort 3	(0.02)	(0.03)	(0.02)	(0.03)
Number of groups	87	42	45	87

Robust Standard errors, clustered at the session level, in parenthesis. * Significant at 10%; **significant at 5%; *** significant at 1%. [a] In all sessions groups A and C had 8 players each. [b] The average of 1,2,3 units of effort.

Table 5. 3. Behavior in the Public Goods game

Level	Variable	All	Long Exposure	Short Exposure	Difference
Round 1	Average percentage of contributors	29	22	34	-12*
		(0.4)	(0.4)	(0.5)	(0.7)
	Percentage of sessions with no contribution	11	15	7	7
		(0.6)	(1.0)	(0.6)	(1.1)
	Median percentage of contributors	10	0.0	17	-17*
		(0.5)	(0.0)	(0.9)	(0.9)
	Maximum percentage of contributors	89	85	93	-7
		(0.6)	(1.0)	(0.6)	(1.1)
Round 2	Average percentage of contributors	27	26	29	-3
		(0.4)	(0.7)	(0.5)	(0.8)
	Percentage of sessions with no contribution	14	23	7	16
		(0.7)	(1.2)	(0.6)	(1.3)
	Median percentage of contributors	17	23	13	10
		(0.7)	(1.2)	(0.8)	(1.4)
	Maximum percentage of contributors	86	77	93	-16
		(0.7)	(1.2)	(0.6)	(1.3)
Session Level	Session size	24.65	24.74	24.58	0.16
		(0.14)	(0.16)	(0.21)	(0.26)
	1 if the player understood that the best outcome is everyone investing in the group account	0.20	0.13	0.25	-0.12***
		(0.02)	(0.03)	(0.02)	(0.04)
	1 if the player declares she understood everything	0.67	0.67	0.68	-0.01
		(0.02)	(0.04)	(0.03)	(0.05)
Number of sessions		29	14	15	29

Robust Standard errors that are clustered at the session level in parenthesis. The standard errors for the median and maximum statistics are calculated at session level. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5. 4. Traditional social capital and CCT social capital measures

	Percentage (unless stated otherwise)		All	Long Exposure	Short Exposure	Difference
Civic Participation	Participation in neighborhood decisions		41	43	39	4
	Participation in the neighborhood meetings		48	51	45	6*
	Membership in at least one organization		27	35	20	15***
	Bonding Social capital		6	7	5	2
	Bridging Social capital		18	24	13	11**
	Linking Social capital		11	16	8	8**
	If is member of any civic association	Attendance to the meetings	26	34	20	14**
		Decision maker	24	30	18	12**
		Leader	10	12	8	4
		Supports with money or work	23	30	18	13***
		No. Hours	13.7	14.9	12.1	2.8
Voting behavior	Voted in local elections (2007)		72	73	72	1
	Voted in presidential elections (2006)		78	88	70	17*`**
Trust and cooperation perception	Trust	Most people	7	8	5	3
		Few people	62	60	63	-3
		None	31	32	31	0
	Perception within the community	Cooperation	32	31	33	-2
		Reciprocity	14	16	13	3
		Self-regarding	54	53	54	-1
Always or usually, the player has helped…	anyone with money, food or clothes		9	11	8	4
	by offering her seat in the bus		24	25	24	0
	someone with domestic work		12	15	10	5*
	someone by carrying a case, package, groceries		8	10	6	4**
	someone by taking care of the house or children		23	23	22	1
CCT measures	% ML in the session		5	6	5	0
	Players attended at least one meeting of care -EC		80	94	70	24***
	Number of meetings of care EC		2.3	4.0	1.1	2.9***
	% At least one ML in the session		79	79	79	0
Observations			714	346	368	714

Robust standard errors of the difference, clustered by session. * Significant at 10%; ** significant at 5%; *** significant at 1%. Bonding social capital is defined as membership in charity and /or religious associations. Bridging social capital is defined as membership in Volunteer work, ethnic, cultural, sports, and environmental associations. Linking social capital is defined as membership in Community action, education (parents' network), security associations, unions or political parties.

Table 5. 5. Network characteristics across sessions

	Variable	All	Long Exposure	Short Exposure	Difference
Session level (714 obs.)	Average degree of relatives [a]	0.13	0.14	0.13	0.01
	Average degree of friends	1.46	1.46	1.46	-0.00
	Average degree of acquaintances	0.44	0.50	0.40	0.10
	Average degree of trustworthy players	1.50	1.48	1.52	-0.05
	Friendship density [b]	0.11	0.11	0.11	-0.00
	Acquaintanceship density	0.03	0.04	0.03	0.01
	Percentage of players identified as leader	0.18	0.20	0.16	0.04
	Algorithm	5.38	5.45	5.32	0.13
Group Level A (232 obs.)	Average degree of relatives	0.22	0.29	0.17	0.12
	Average degree of friends	1.99	1.93	2.04	-0.11
	Average degree of acquaintances	0.41	0.42	0.40	0.02
	Average degree of trustworthy players	2.08	2.05	2.10	-0.05
	Percentage of players identified as leader	23.3	22.0	24.0	-2.0
Group Level C (232 obs.)	Average degree of relatives	0.00	0.00	0.00	0.00
	Average degree of friends	0.00	0.00	0.00	0.00
	Average degree of acquaintances	0.01	0.01	0.01	0.00
	Average degree of trustworthy players	0.00	0.00	0.00	0.00
	Percentage of players identified as leader	15.9	18.0	15.0	-3.0

[a] Average degree for a network graph is the average number of edges that nodes in the network have. [b] Network density is the average degree divided by (N-1), where N is the number of nodes in the network. Robust standard errors of the difference clustered by session. For more details on the Network analysis see Advani and Bansi (2014). * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6. 1. Marginal effects of a partial proportional odds model for the lowest and highest individual level of effort (N=714)

	I	II	IIIa	IIIb	IVa	IVb	IVc	IVd
Independent Variable	**Low**	**Low**	**Low**	**Low**	**Low**	**Low**	**Low**	**Low**
Beneficiary longer than a year (enrolment)	-0.23***	-0.24***	-0.23***	-0.24***	-0.24***	-0.25***	-0.24***	-0.25***
	(0.09)	(0.09)	(0.08)	(0.09)	(0.09)	(0.09)	(0.08)	(0.09)
Cooperation decision round 1		-0.08*		-0.08*		-0.08*		-0.08*
		(0.05)		(0.05)		(0.05)		(0.05)
Cooperation decision round 2		0.03		0.04		0.03		0.04
		(0.05)		(0.05)		(0.05)		(0.05)
Degree of Player (friends)			-0.03*	-0.03*			-0.03*	-0.03*
			(0.02)	(0.02)			(0.02)	(0.02)
Degree of Player (relatives)			0.04	0.04			0.03	0.04
			(0.04)	(0.04)			(0.04)	(0.04)
Degree of Player (acquaintances)			-0.01	-0.01			-0.01	-0.02
			(0.02)	(0.03)			(0.02)	(0.03)
Voted in presidential elections (2006)					0.06**	0.06**	0.06**	0.06**
					(0.03)	(0.03)	(0.03)	(0.03)
Always or usually help anyone with money, food or clothes					-0.07	-0.07	-0.07	-0.06
					(0.06)	(0.06)	(0.06)	(0.06)
Independent Variable	**High**	**High**	**High**	**High**	**High**	**High**	**High**	**High**
Beneficiary longer than a year (enrolment)	0.32***	0.33***	0.32***	0.34***	0.33***	0.34***	0.33***	0.35***
	(0.11)	(0.12)	(0.11)	(0.12)	(0.11)	(0.12)	(0.11)	(0.12)
Cooperation decision round 1		0.11*		0.12*		0.11*		0.12*
		(0.07)		(0.07)		(0.07)		(0.07)
Cooperation decision round 2		-0.05		-0.05		-0.04		-0.05
		(0.07)		(0.07)		(0.07)		(0.07)
Degree of Player (friends)			0.04*	0.05**			0.04*	0.05**
			(0.02)	(0.02)			(0.02)	(0.02)
Degree of Player (relatives)			-0.06	-0.06			-0.05	-0.05
			(0.05)	(0.05)			(0.05)	(0.05)
Degree of Player (acquaintances)			0.02	0.02			0.02	0.02
			(0.03)	(0.04)			(0.03)	(0.04)
Voted in presidential elections (2006)					-0.08**	-0.08**	-0.08*	-0.08**
					(0.04)	(0.04)	(0.04)	(0.04)
Always or usually help anyone with money, food or clothes					0.10	0.10	0.10	0.09
					(0.08)	(0.08)	(0.08)	(0.08)

Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6. 2. Marginal effects of a partial proportional odds model for the lowest and highest individual level of effort

	V	VI	VII
Independent Variable	**Low**	**Low**	**Low**
Beneficiary longer than a year (enrolment)	-0.19***	-0.29***	-0.30***
	(0.08)	(0.08)	(0.07)
Cooperation decision round 1			-0.06
			(0.04)
Cooperation decision round 2			0.02
			(0.05)
Degree of Player (friends)			-0.03**
			(0.02)
Degree of Player (relatives)			0.03
			(0.04)
Degree of Player (acquaintances)			0.00
			(0.03)
Independent Variable	**High**	**High**	**High**
Beneficiary longer than a year (enrolment)	0.27***	0.43***	0.45***
	(0.11)	(0.1)	(0.1)
Cooperation decision round 1			0.09
			(0.06)
Cooperation decision round 2			-0.03
			(0.07)
Degree of Player (friends)			0.05**
			(0.03)
Degree of Player (relatives)			-0.05
			(0.05)
Degree of Player (acquaintances)			0.00
			(0.04)
Basic characteristics	Yes	Yes	Yes
Experimental variables	No	Yes	Yes
CCT measures of Social capital	No	No	Yes
Network Information	No	No	Yes
Behavior in the Cooperation game	No	No	Yes
Observations	712	712	712

Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6. 3. Relation between the effort decision and survey social capital measures

Independent variable:		**Effort decision (-1,0,1)**			
Dependent variable: Dummy survey measures		1_E	2_E	4_E	5_E
Participation in neighborhood decisions		0.03	0.02	0.03	0.02
Participation in the neighborhood meetings		0.02	0.01	0.00	0.00
Membership in at least one organization		0.02	-0.01	-0.01	-0.01
Bonding Social capital		0.01	-0.01	-0.01	-0.01
Bridging Social capital		0.02	0.00	0.00	-0.00
Linking Social capital		0.00	-0.00	-0.00	-0.00
Membership in a charity		0.00	-0.00	-0.00	-0.00
Membership in a religious association		0.00	-0.01	-0.01	-0.01
Membership in a community action association		-0.00	-0.01	-0.00	-0.00
Voted in local elections (2007)		-0.00	-0.01	-0.00	-0.01
Voted in presidential elections (2006)		0.00	-0.03*	-0.04**	-0.04**
Trust: do you consider that you can trust in?	Most people	0.01	0.01	0.01	0.00
	Few people	0.02	0.02	0.03	0.02
	None	-0.03	-0.03	-0.04	-0.03
Perception within the community	Cooperation	-0.02	-0.02	-0.02	-0.02
	Reciprocity	0.02*	0.02	0.02	0.01
	Self-regarding	-0.00	-0.00	-0.00	0.00
Always or usually, has she helped	anyone with money, food or clothes	0.02*	0.02	0.02	0.02
	by offering her seat in the bus	0.01	0.01	0.01	0.01
	someone with domestic work	0.02	0.01	0.01	0.01
	someone by carrying stuff	0.03**	0.03	0.03**	0.03**
	someone by childcare	0.04	0.04	0.04	0.03
CCT program measures	She is a ML	-0.00	-0.00	0.00	0.00
	She has attended to EC meetings	0.02	-0.02	-0.02	-0.02
	If anyone consider the player a leader	0.02	0.01	0.01	0.01
	There is at least a ML	0.01	0.02	0.01	0.02
Dependent variable: Continuous survey measures		1_E	2_E	4_E	5_E
CCT program measures	Number of meetings she has attended	1.37***	0.02	-0.10	2.10***
	% of ML in the session	0.02**	0.01	0.00	0.06***
Connectivity in the group A. B, C or session	Score	2.87***	1.33*	0.82*	5.64***
	Number of connections	1.07***	0.46*	0.26*	2.09***
	Degree of friends	0.78***	0.37*	0.24*	1.56***
	Degree of relatives	0.08***	0.04	0.02	0.14***
	Degree of acquaintances	0.21***	0.05	-0.00	0.39***
	% who considered the player as leader	0.00**	0.00	-0.00	0.01***
	% informal leaders in the group/session	0.09***	0.03	0.01	0.19***
Controls	*Beneficiary for longer than a year*	No	Yes	Yes	Yes
	Basic Characteristics	No	No	Yes	Yes
	Network information	No	No	No	Yes
	Effort decision	Yes	Yes	Yes	Yes
	Cooperation decision in round 1	No	No	No	No
	Cooperation decision in round 2	No	No	No	No

For dummy survey measures we report the marginal effects of probit regression. For the continuous survey measures we report the linear regression coefficients. Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%;** significant at 5%; *** significant at 1%.

Table 6. 4a Control variables in Table 6.2 Marginal effects of a partial proportional odds model for the lowest and highest level of effort – Basic Characteristics

	V	VI	VII	V	VI	VII
Independent Variable	**Low**	**Low**	**Low**	**High**	**High**	**High**
1 if the player is a woman	-0.15	-0.07	-0.04	0.22	0.1	0.06
	(0.15)	(0.1)	(0.11)	(0.21)	(0.15)	(0.16)
Age	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Level of education (0 to 5)	0.01	0.02*	0.02	-0.02	-0.02*	-0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Number of years living in the neighborhood	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
1 if the player is displaced (self-declared)	0.01	0.02	0.01	-0.02	-0.02	-0.02
	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)	(0.08)
1 if the player is the head of household	0.00	0.02	0.01	0.00	-0.03	-0.02
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)
1 if the player has a partner	-0.03	-0.02	-0.02	0.05	0.02	0.03
	(0.04)	(0.04)	(0.04)	(0.05)	(0.06)	(0.06)
Number of people per room	-0.01	-0.01	0.00	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
1 if the player has her own housing	-0.02	-0.02	-0.01	0.03	0.03	0.02
	(0.03)	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)
1 if the player's home has no electricity	-0.01	0.02	0.03	0.01	-0.02	-0.04
	(0.07)	(0.07)	(0.06)	(0.1)	(0.11)	(0.09)
1 if the player has a landline	0.06**	0.07**	0.07**	-0.09**	-0.11**	-0.11**
	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.05)
1 if the player has a cellphone	-0.01	0.01	0.00	0.01	-0.02	-0.01
	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.05)
1 if the player's home has water pipe access	-0.07**	-0.05	-0.06	0.11**	0.08	0.09
	(0.04)	(0.03)	(0.04)	(0.05)	(0.05)	(0.06)
1 if the player's home has sewage	0.02	-0.01	-0.01	-0.03	0.01	0.02
	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.05)
1 if She has received (different from FA) any other government aid	-0.02	-0.04	-0.03	0.03	0.06	0.04
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
1 if Perceives that HH income is above the highest possible	-0.33***	-0.25***	-0.06	0.1	0.01	0.09
	(0.1)	(0.07)	(0.07)	(0.12)	(0.1)	(0.1)
1 if Perceives that HH income is above the average	-0.04	-0.01	-0.01	0.06	0.02	0.02
	(0.06)	(0.05)	(0.05)	(0.08)	(0.08)	(0.08)
1 if the HH has a sound player	0.00	-0.01	0.00	0.00	0.01	0.00
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
HH income per capita	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
1 if the HH has a DVD player	0.01	0.01	0.01	-0.01	-0.01	-0.01
	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)	(0.04)
Basic characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Experimental variables	No	Yes	Yes	No	Yes	Yes
CCT measures of social capital	No	No	Yes	No	No	Yes
Network Information	No	No	Yes	No	No	Yes
Behavior in Cooperation game	No	No	Yes	No	No	Yes
Observations	712	712	712	712	712	712

Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%;** significant at 5%; *** significant at 1%.

Table 6. 5b Control variables in Table 6.2 Marginal effects of a partial proportional odds model for the lowest and highest level of effort –Experimental variables and CCT measures of Social Capital

	VI	VII	VI	VII
Independent Variable	**Low**	**Low**	**High**	**High**
1 if there is at least one man in the group	0.09	0.12	-0.14	-0.18
	(0.09)	(0.09)	(0.13)	(0.14)
1 if the player understood the activity perfectly	-0.02	-0.02	0.02	0.02
	(0.02)	(0.03)	(0.04)	(0.04)
1 if Experimenter n°2 (female) in 2008	-0.18**	-0.17**	0.26**	0.26**
	(0.09)	(0.09)	(0.13)	(0.13)
Number of players in session	0.09**	0.08**	-0.13***	-0.13***
	(0.04)	(0.04)	(0.05)	(0.05)
1 if First session in the day	-0.03	-0.02	0.05	0.03
	(0.08)	(0.08)	(0.13)	(0.12)
Average level of effort in the last two sessions [a]	-0.1	-0.11	0.14	0.16
	(0.11)	(0.11)	(0.16)	(0.16)
1 if player is chosen as leader by anyone in the group		0.03		-0.05
		(0.05)		(0.08)
1 if player is a ML (self-declared)		-0.02		0.02
		(0.06)		(0.09)
1 if there is at least 1 ML in the group		-0.02		0.03
		(0.04)		(0.07)
Basic characteristics	Yes	Yes	Yes	Yes
Experimental variables	Yes	Yes	Yes	Yes
CCT measures of social capital	No	Yes	No	Yes
Network Information	No	Yes	No	Yes
Behavior in Cooperation game	No	Yes	No	Yes
Observations	712	712	712	712

Robust Standard errors that are clustered at the session level in parenthesis. [a] Average deviation from the neighborhood mean of the average effort in the previous 2 sessions * Significant at 10%;** significant at 5%; *** significant at 1%.

11. Acknowledgememts

We thank Liliam Puello and Beatriz Jimenez from the local office of Familias en Acción in Cartagena, and Hernando Sánchez from the national office of Familias en Acción. Without the logistic support of the program officials at all levels this exercise would not have been possible. We also thank Syngjoo Choi, Orazio Attanasio, David Echeverry, Patricia Padilla and Vivian Rodríguez. The experiments reported here were sponsored by the European Commission, the Institute for Fiscal Studies and EDePo.

12. Appendices

i. The Conditional Cash Transfer program: Familias en Acción and Cartagena

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 and MESEP, 2012).

The program has become the flagship of the Colombian government's social policy as it targets the poorest 20% of Colombian households[57]. 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[58]. This CCT is targeted to women, like every other CCT in Latin America.

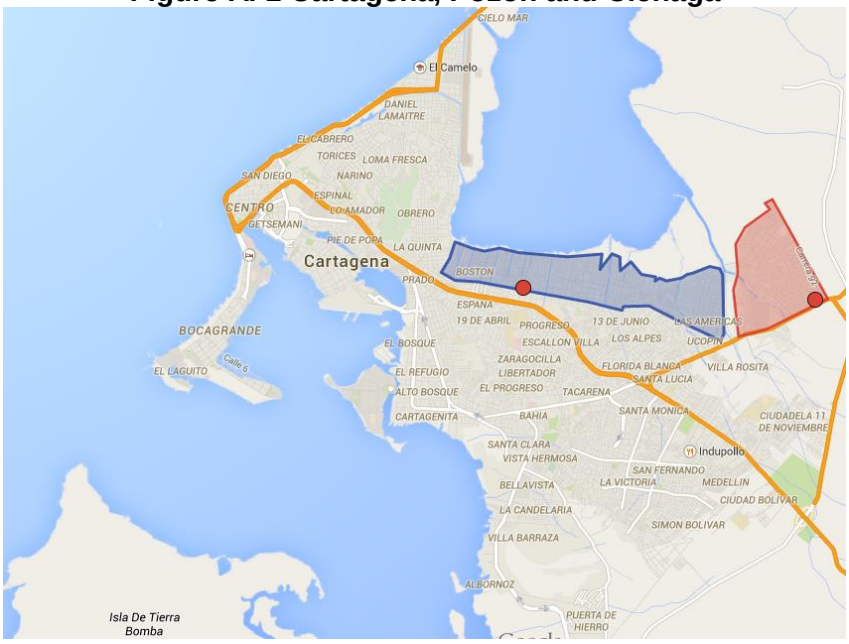
Cartagena is the fifth largest city in Colombia, with 993 thousand inhabitants in 2008. It is the third poorest city in the country, with 40.2% poor and 6.9% in extreme poverty in 2008 (MESEP, 2012). *Ciénaga* and *Pozón* belong to the poorest locality (i.e. the lowest level of income, the lowest education coverage, the highest infant mortality rates and the worst living conditions in Cartagena (CCV, 2011). In 2009, Pozón is recognized as the densest neighborhood with an area of 273 Ha and 45 thousand inhabitants while Ciénaga has 463 Ha and 102 thousand inhabitants (see Figure A1). By 2006, Pozón and Ciénaga are considered by the local authorities as very similar, with a percentage of households with lower income (56%) and the same average time in school (6 years)[59].

In January 2005, the FA authorities decided to pilot the program in Pozón with 5 thousand Sisben 1 households. A new enrolment wave took place in March 2006 for 2.5 thousand *displaced* households (i.e. households that were forced to leave their home because of the civil conflict). After that, displaced households have been allowed to enroll in the program at any time. 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 Vírgen)* identified by

[57] 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.
[58] For evidence of success of FA on the target outcomes and other outcomes such as crime and voting behavior see a survey in Attanasio et al. (2015).
[59] Source: CCV. URL: http://www.cartagenacomovamos.org/ucg.swf. Calculations made with data provided by the Mayor office in Cartagena in the study by Pérez et al. (2007).

the FA authorities as eligible to participate in the pilot. In late August 2007, 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*. In total 35.5 thousand households were enrolled in Cartagena, including new households from Pozón. In our data set 58 of 404 individuals who attended the follow-up (14.4% of Pozón sample) were enrolled in the program in 2007. The following enrollment waves after 2007 (for non-displaced households) took place in 2009 (32.000 households), in 2012 (22,000 households) and 2013 (7,000 households). Regarding the payment procedure of FA, the first payment in Pozón was in March 2005 followed by a bi-monthly payment. In 2007, the first payment in Ciénaga was in October 2007.

Figure A. 1 Cartagena, Pozón and Ciénaga



Source: http://midas.cartagena.gov.co/ and Map data © 2014 Google. Red dots are where the sessions were held. The red area is Pozón and the blue one Ciénaga.

At the EC, ML and beneficiaries follow up on their current health and education status. They discuss any aspect related to the community, and by doing so they reach a common ground to make decisions and take actions aimed at improving their life conditions. It is also a space for enjoyment among peers. From January 2005, ECs were held quarterly. However, a ML was allowed to organize EC with her beneficiaries whenever she considered. The number of ECs was determined by how proactivity the ML was. There were differences in the EC in Pozón between the period 2005 to 2007 and from 2007 to 2008. As the ML were trained, they felt empowered within their community, displacing other community leaders.

In addition to the EC, the beneficiaries take part in the general assembly. The general assembly is a public meeting where beneficiaries discuss and decide about problems affecting beneficiaries in Cartagena. The ML are elected in the general assembly. There are four annual assemblies, taking place on a date set by the local office. Although the national office does not make attendance to the assemblies a mandatory requirement, from 2005 to 2010 the local office made it so.

The percentage of the neighborhood population receiving the program was 79% in Pozón in 2006 and 22.4% in all of Cartagena in 2008.

ii. The Voluntary Contribution Mechanism[60] (See Attanasio et al. 2015)

In our game, each player receives an endowment of one token to be invested either in a private or a group account. The decision is made privately and simultaneously. The earnings are calculated in the following way: if the player chooses to invest in the private account, the token is converted into $5 and will be given entirely to her. In addition each player receives, regardless of how she has invested her own her token, $0.40 for each token invested in the group account by any other member in the group of 25 players. Therefore, her total earnings at the end of this round are ($5) + ($0.40 x Sum of Tokens invested by the group). If the player chooses to invest her token in the group account, she will receive 0.4 for each token invested in the group account by her and in the rest of the group. In this case her total earnings at the end of the round will be ($0) + ($0.40 x Sum of Tokens invested by the group). Each player makes her private decision by selecting a card which says if she is going to invest her money in the group account or to keep it for herself (i.e. private account). The experimenter then collects the "decisions cards," totals them up, multiplies by $0.40 the amount and credits the relevant amounts to each player. The relevant amounts, however, are only revealed and paid at the end of the session and after a second round of the same game[61].

iii. Relation between the CCT and traditional social capital measures

In this section we report the relation between traditional social capital measures collected in the survey and exposure to the CCT, experimental measures and individual network

[60] The experimental design of the VCM described here was developed by Juan Camilo Cárdenas, Maria Claudia Lopez, Natalia Candelo and this author.

[61] The marginal per capita return (MPCR) of this game is one of the lowest in the literature. The goal was to mimic a measure of what would be 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. In addition, 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. Since we claim to provide a social capital measure, where social dilemmas are key -and hence a low MPCR appropriate-, the low MPCR provides an additional source of validity to the measure.

information. We use different sets of controls. Among the socio-economic characteristics we include especially those characteristics we found to show differences between the short exposure and long exposure samples.

In Table A3a. we show the effect of exposure to the program (1 if the player has been in the program for more than a year in dichotomous social capital measures). All of them are dummy survey measures and we report the marginal effect of exposure in a probit regression model for each social capital variable.

All social capital measures are positively related to being enrolled in the program for more than a year. In particular, we find a positive and significant relation between exposure to the program and membership in organizations, participation in bonding and bridging associations, voting in the presidential elections and attendance to the FA meetings, as well as network information such as the connectivity score assigned to each player, the percentage of participants who considered the player as a leader and the percentage of these informal leaders in the session. Among associations, there is a positive relation with religious participation and exposure to the program. This analysis is supported by Table A1 in this appendix.

Finally, we also report the analysis for the number of connections, friends, relatives and acquaintances the player identifies in the session. We report the marginal effect of exposure of a linear regression model for each social capital variable.

In Table A3b, we show the effect of experimental measures in continuous Social capital measures. We report the marginal effect of exposure of a linear regression model for each social capital continuous variable. All experimental measures are positively related with network information such as the connectivity score assigned to each player, the percentage of participants who considered the player as a leader and the percentage of these informal leaders in the session. Finally, we also report the analysis for the number of connections, friends, relatives and acquaintances the player identifies in the session.

iv. Relation between the CCT and behavior in the cooperation game

In Table 6.3 and 6.4 we report the results for the regression analysis of willingness to cooperate in round 1 and round 2. In both cooperation decisions, the presence of at list one ML in the session increases the likelihood to cooperate by 17%. This result is robust to different specifications and highlights the importance of leaders in collective action (Jones and Olken, 2005, Kosfeld and Rustagi, 2015).

We also observe that being a ML decreases the individual willingness to cooperate in the first round by 12%. This result is consistent with previous studies on spitefulness (Fehr, Hoff, and Kshetramade, 2008, Kosfeld and Rustagi, 2015) and the role of status on cooperation (Brooks, Hoff and Pandey, 2014). Spiteful preferences -the

desire to reduce another's material payoff for the mere purpose of increasing one's relative payoff- (Fehr et al. 2008) and in social psychology, by sacrificing total surplus and equality for the sake of a larger payoff difference between "self" and "other." (Van Lange, 2009). Fehr et al. (2008) suggest that the willingness to reduce another's material payoff is stronger among individuals belonging to high caste status in India.

v. Complimentary Tables

Table A. 1 Spearman correlation coefficients experimental with survey measures

Survey Social Capital measures		Level of effort	Cooperation Round 1	Cooperation Round 2
Participation in neighborhood decisions		0.05	-0.02	0.04
Participation in the neighborhood meetings		0.03	-0.04	0.03
Membership in at least one organization		0.04	0.01	0.07*
Bonding Social capital		0.02	0.00	0.06*
Bridging Social capital		0.06	0.00	0.07*
Linking Social capital		0.01	-0.02	0.00
Membership in a charity		0.00	0.02	0.09**
Membership in a volunteer work association		0.05	-0.02	0.03
Membership in a religious association		0.01	-0.01	0.02
Membership in a community action association		-0.01	-0.01	0.02
Membership in a cultural or sports association		-0.06*	-0.06	-0.03
Membership in a education association		0.06	0.03	0.09**
Membership in an environmental association		0.08**	-0.04	0.04
Membership in a security association		0.03	-0.02	-0.02
Membership in a union, labor association		0.04	-0.03	-0.03
Membership in a political party		0.04	-0.03	-0.03
Voted in local elections (2007)		0.00	0.00	0.03
Voted in presidential elections (2006)		0.01	-0.01	0.02
Trust: do you consider that you can trust in?	Most people	0.04	-0.03	-0.03
	Few people	0.04	0.02	-0.06
	None	-0.06	0.00	0.07**
Perception within the community	Cooperation	-0.02	-0.02	-0.02
	Reciprocity	0.05	0.00	0.00
	Self-regarding	-0.01	0.02	0.02
Always or usually, has she helped anyone	with money	0.07*	0.04	0.02
	by offering her seat in the bus	0.01	-0.03	0.13***
	someone with domestic work	0.04	0.01	0.09**
	by carrying a case, package, groceries	0.10***	0.02	0.09**
	by taking care of the house or children	0.07**	0.03	0.06

Bonding social capital is defined as membership in Charity and /or religious associations. Bridging social capital is defined as membership in Volunteer work, ethnic, cultural, sports, and environmental associations. Linking social capital is defined as membership in Community action, education (parents' network), security associations, union, political parties. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table A. 2 Spearman correlation coefficients experimental measures with network information

Network measures		Level of effort	Willingness to cooperate Round 1	Willingness to cooperate Round 2
Score		0.13***	0.03	0.11***
Connectivity in the group A. B, C	Number of connections	0.18***	0.05	0.07*
	Number of friends	0.14***	0.06	0.04
	Number of relatives	0.10***	0.09**	0.02
	Number of acquaintances	0.15***	0.07*	0.14***
	If anyone consider the player a leader	0.00	-0.05	-0.04
	Number of people considered trustworthy	0.08**	0.03	0.03
Connectivity in the session (for the PGG)	Number of connections	0.12***	0.02	0.10***
	Number of friends	0.11***	0.03	0.11***
	Number of relatives	0.06*	0.02	0.01
	Number of acquaintances	-0.01	-0.03	0.01
	If anyone consider the player a leader	0.03	-0.02	-0.04
	Number of people considered trustworthy	0.12***	0.05	0.08**

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.3 a Effect of exposure to the program (1 if the player has been in the program for more than a year) on social capital measures. Dummy Survey measures (marginal effects of probit regression reported)

Dependent variable		I	III	IV	VI	VII	IX	X	XII
Participation in neighborhood decisions		0.04	0.03	0.03	0.02	0.04	0.03	0.04	0.03
Participation in the neighborhood meetings		0.06*	0.03	0.05*	0.03	0.05	0.03	0.06*	0.03
Membership in at least one organization		0.15***	0.12**	0.15***	0.13**	0.16***	0.13**	0.15***	0.12**
Bonding Social capital		0.08**	0.07**	0.09***	0.08**	0.08**	0.07**	0.08***	0.07**
Bridging Social capital		0.11***	0.09**	0.11**	0.09**	0.11***	0.09**	0.11***	0.08**
Linking Social capital		0.02	0.01	0.02	0.01	0.02	0.01	0.02	0.01
Membership in a charity		0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Membership in a religious association		0.06**	0.05*	0.07***	0.06**	0.06**	0.05*	0.06**	0.05*
Membership in a community action association		0.02	0.01	0.02	0.01	0.01	0.01	0.01	0.01
Voted in local elections (2007)		0.01	0.01	0.02	0.01	0.01	0.01	0.01	0.01
Voted in presidential elections (2006)		0.17***	0.18***	0.19***	0.20***	0.17***	0.17***	0.17***	0.18***
Trust: do you consider that you can trust in?	Most people	0.03	-0.00	0.02	-0.01	0.03	-0.01	0.03	-0.00
	Few people	-0.03	-0.02	-0.04	-0.03	-0.03	-0.01	-0.03	-0.01
	None	0.00	0.02	0.02	0.03	0.00	0.02	0.00	0.02
Perception within the community	Cooperation	-0.02	-0.03	-0.01	-0.02	-0.03	-0.04	-0.02	-0.03
	Reciprocity	0.03	0.04	0.02	0.04	0.03	0.04	0.03	0.04
	Self-regarding	-0.01	-0.01	-0.01	-0.01	-0.01	-0.00	-0.01	-0.01
Always or usually, has she helped	anyone with money, food or clothes	0.04	0.02	0.03	0.01	0.04	0.02	0.04	0.02
	by offering her seat in the bus	0.00	0.03	0.00	0.03	0.00	0.03	0.01	0.03
	someone with domestic work	0.05*	0.06	0.05*	0.05	0.06**	0.06	0.05**	0.06
	someone by carrying stuff	0.04**	0.02	0.03	0.01	0.04***	0.02	0.04***	0.02
	someone with childcare	0.01	-0.00	-0.01	-0.02	0.01	0.00	0.01	-0.00
CCT program measures	She is a ML	0.00	-0.00	0.00	-0.00	0.00	-0.01	0.00	-0.00
	She has attended to EC meetings	0.24***	0.23***	0.25***	0.24***	0.24***	0.22***	0.24***	0.23***
	If anyone consider the player a leader	0.04	0.03	0.03	0.03	0.04	0.03	0.04	0.03
	There is at least a ML in the session	0.00	0.02	-0.01	0.01	0.02	0.03	0.01	0.02
Controls	*Basic Characteristics*	No	Yes	No	Yes	No	Yes	No	Yes
	Network information	No	Yes	No	Yes	No	Yes	No	Yes
	Effort decision	No	No	Yes	Yes	No	No	No	No
	Cooperation decision in round 1	No	No	No	No	Yes	Yes	No	No
	Cooperation decision in round 2	No	No	No	No	No	No	Yes	Yes

Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%;** significant at 5%; *** significant at 1%

Table A.3 b. Effect of exposure to the program (1 if the player has been in the program for more than a year) on social capital measures. Continuous survey measures (Linear regression coefficients reported)

Dependent variable		I	III	IV	VI	VII	IX	X	XI	XII
CCT program measures	Number of meetings she has attended	4.02***	2.85***	4.01***	2.90***	3.83***	2.87***	3.84***	2.85***	2.85***
	% of ML in the session	0.05***	-0.00	0.05***	-0.00	0.04***	-0.00	0.04***	-0.00	-0.00
Connectivity in the group A. B, C or session	Score	5.45***		4.58***		4.52***		4.32***	0.86	
	Number of connections	2.10***		1.80***		1.76***		1.69***	0.37	
	Number of friends	1.46***		1.22***		1.20***		1.14***	0.21	
	Number of relatives	0.14***		0.12***		0.12***		0.12***	0.02	
	Number of acquaintances	0.50***		0.47***		0.44***		0.43***	0.14*	
	% people who considered the player as leader	0.01***	0.00	0.01***	0.00	0.01***	0.00	0.01***	0.00	0.00
	% informal leaders in the group/session	0.20***	0.03	0.18***	0.03	0.17***	0.03	0.17***	0.03	0.03
Controls	*Exposure*	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Basic Characteristics	No	Yes	No	Yes	No	Yes	No	Yes	Yes
	Network information	No	Yes	No	Yes	No	Yes	No	No	Yes
	Effort decision	No	No	Yes	Yes	No	No	No	No	No
	Cooperation decision round1	No	No	No	No	Yes	Yes	No	No	No
	Cooperation decision round2	No	No	No	No	No	No	Yes	Yes	Yes

Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%;** significant at 5%; *** significant at 1%.

Table A.4. a. Relation between willingness to cooperate and Social capital measures. Continuous survey measures (Linear regression coefficients reported)

Independent variable:		**Cooperation decision in round 1**			**Cooperation decision in round 2**		
Dependent variable		1_C1	2_C1	4_C1	1_C2	2_C2	4_C2
CCT program measures	Number of meetings she has attended	2.10***	0.85***	0.16	2.22***	0.68***	0.04
	% of ML in the session	0.06***	0.04***	0.01*	0.06***	0.04***	0.01
	Score	5.64***	4.17***	0.56	6.16***	4.43***	1.05
Connectivity in the group A. B, C or session	Number of connections	2.09***	1.52***	0.16	2.28***	1.61***	0.33
	Degree of friends	1.56***	1.16***	0.16	1.71***	1.26***	0.32
	Degree of relatives	0.14***	0.11***	0.03	0.13***	0.09***	0.01
	Degree of acquaintances	0.39***	0.25***	-0.03	0.44***	0.26***	0.01
	% people who considered the player as leader	0.01***	0.01***	-0.00	0.01***	0.01*	-0.00
	% informal leaders in the group/session	0.19***	0.14***	0.02*	0.17***	0.10***	-0.01
Controls	*Beneficiary longer than a year (enrolment)*	No	Yes	Yes	No	Yes	Yes
	Basic Characteristics	No	No	Yes	No	No	Yes
	Network information	No	No	No	No	No	No
	Effort decision	No	No	No	No	No	No
	Cooperation decision round1	Yes	Yes	Yes	No	No	No
	Cooperation decision round2	No	No	No	Yes	Yes	Yes

Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%;** significant at 5%; *** significant at 1%.

Table A.4. b Relation between willingness to cooperate and social capital measures (marginal effects of probit regression reported)

Independent variable:		**Cooperation decision in round 1**				**Cooperation decision in round 2**			
Dependent variable: Dummy survey measures		1_C1	2_C1	4_C1	5_C1	1_C2	2_C2	4_C2	5_C2
Participation in neighborhood decisions		-0.02	-0.01	-0.01	-0.02	0.04	0.05	0.06	0.05
Participation in the neighborhood meetings		-0.04	-0.03	-0.03	-0.03	0.03	0.03	0.05	0.05
Membership in at least one organization		0.01	0.03	0.03	0.03	0.07	0.07*	0.07**	0.07*
Bonding Social capital		-0.00	0.01	-0.01	-0.00	0.05	0.05*	0.04*	0.04*
Bridging Social capital		0.00	0.02	0.02	0.02	0.06	0.06*	0.06**	0.06**
Linking Social capital		-0.01	-0.01	-0.01	-0.01	0.00	0.00	-0.00	-0.00
Membership in a charity		0.01	0.01	0.00	0.00	0.03**	0.03**	0.02*	0.02**
Membership in a religious association		-0.01	0.00	-0.01	-0.01	0.02	0.02	0.02	0.02
Membership in a community action association		-0.00	-0.00	-0.00	-0.00	0.01	0.01	0.00	0.00
Voted in local elections (2007)		-0.00	-0.00	-0.02	-0.02	0.03	0.03	0.01	0.01
Voted in presidential elections (2006)		-0.01	0.02	-0.02	-0.02	0.02	0.03	0.02	0.02
Trust: do you consider that you can trust in?	Most people	-0.01	-0.01	-0.02	-0.02	-0.02	-0.02	-0.01	-0.01
	Few people	0.02	0.01	0.02	0.02	-0.06	-0.06	-0.06	-0.07
	None	-0.00	-0.00	-0.01	-0.00	0.08*	0.08*	0.07*	0.08*
Perception within the community	Cooperation	-0.02	-0.03	-0.05	-0.05	-0.02	-0.02	-0.03	-0.03
	Reciprocity	-0.00	0.00	0.02	0.02	0.00	0.00	0.01	0.01
	Self-regarding	0.02	0.02	0.03	0.03	0.02	0.02	0.02	0.02
Always or usually, has she helped	anyone with money, food or clothes	0.03	0.03	0.04*	0.04*	0.02	0.02	0.02	0.02
	by offering her seat in the bus	-0.03	-0.03	-0.03	-0.03	0.12***	0.12***	0.12***	0.12***
	someone with domestic work	0.01	0.02	0.02	0.01	0.06**	0.06**	0.05*	0.05*
	someone by carrying stuff	0.01	0.02	0.02	0.02	0.05**	0.05**	0.05**	0.04**
	someone by childcare	0.03	0.03	0.04	0.03	0.05	0.05	0.06	0.05
CCT program measures	She is a ML	-0.01	-0.01	-0.01	-0.01	-0.02	-0.02	-0.02	-0.02
	She has attended to EC meetings	-0.10**	-0.06*	-0.04	-0.04	-0.09**	-0.09***	-0.07**	-0.07***
	If anyone consider the player a leader	-0.02	-0.01	-0.01	-0.01	-0.04	-0.04	-0.04	-0.05*
	There is at least a ML	0.14**	0.15**	0.15**	0.15**	0.17**	0.17**	0.17***	0.18***
Controls	*Beneficiary for longer than a year*	No	Yes	Yes	Yes	No	Yes	Yes	Yes
	Basic Characteristics	No	No	Yes	Yes	No	No	Yes	Yes
	Network information	No	No	No	Yes	No	No	No	Yes
	Effort decision	No	No	No	No	No	No	No	No
	Cooperation decision round1	Yes	Yes	Yes	Yes	No	No	No	No
	Cooperation decision round2	No	No	No	No	Yes	Yes	Yes	Yes

Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%;** significant at 5%; *** significant at 1%

Table A.4. c. Relation between network variables and social capital measures (marginal effects of probit regression reported)

Independent variable		5_E	5_E	5_E	5_C1	5_C1	5_C1	5_C2	5_C2	5_C2
Dependent variable: **Dummy Survey measures**		Friends	Relatives	Acquaint.	Friends	Relatives	Acquaint.	Friends	Relatives	Acquaint.
Participation in neighborhood decisions		0.02*	0.08	0.02	0.02*	0.08	0.02	0.02*	0.08	0.02
Participation in the neighborhood meetings		-0.00	-0.01	-0.01	-0.00	-0.01	-0.01	-0.00	-0.01	-0.01
Membership in at least one organization		0.00	-0.00	0.02	0.00	-0.00	0.02	0.00	-0.00	0.02
Bonding Social capital		0.00	-0.01	0.02	0.00	-0.01	0.02	0.00	-0.01	0.02
Bridging Social capital		0.01	0.03	0.01	0.01	0.03	0.01	0.01	0.03	0.01
Linking Social capital		-0.00	-0.01	0.00	-0.00	-0.01	0.00	-0.00	-0.01	0.00
Membership in a charity		0.00	0.00	-0.00	0.00	0.00	-0.00	0.00	0.00	-0.00
Membership in a religious association		0.00	-0.03	0.02*	0.00	-0.03	0.02*	0.00	-0.03	0.02*
Membership in a community action association		-0.01**	-0.00	-0.00	-0.01*	-0.00	-0.00	-0.01*	-0.00	-0.00
Voted in local elections (2007)		0.01	0.05	0.03*	0.01	0.05	0.03*	0.01	0.05	0.03*
Voted in presidential elections (2006)		0.01	0.10**	0.02	0.00	0.10**	0.02	0.00	0.10**	0.02
Trust: do you consider that you can trust in?	Most people	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	Few people	0.04***	0.01	0.03*	0.04***	0.01	0.03*	0.04***	0.01	0.04*
	None	-0.04***	-0.01	-0.04*	-0.04***	-0.01	-0.04*	-0.04***	-0.01	-0.04*
Perception within the community	Cooperation	0.00	-0.08*	-0.00	0.00	-0.08*	-0.00	0.00	-0.08*	-0.00
	Reciprocity	0.00	0.05	0.01	0.01	0.05	0.01	0.00	0.05	0.01
	Self-regarding	-0.01	0.02	-0.01	-0.01	0.02	-0.01	-0.01	0.02	-0.01
Always or usually, has she helped	anyone with money, food or clothes	0.00	0.00	-0.00	0.00	0.00	-0.00	0.00	0.01	-0.00
	by offering her seat in the bus	0.01	-0.01	0.03	0.01	-0.01	0.03	0.01	-0.01	0.03
	someone with domestic work	0.01*	-0.00	-0.00	0.01*	-0.00	-0.00	0.01*	-0.00	-0.01
	someone by carrying stuff	0.01	0.00	0.00	0.01	0.01	0.00	0.01	0.01	0.00
	someone by childcare	0.03***	0.00	0.03*	0.03***	0.00	0.03*	0.03***	0.00	0.03*
CCT program measures	She is a ML	0.00	0.02	-0.00	0.00	0.02	-0.00	0.00	0.02	-0.00
	She has attended to EC meetings	0.01	0.02	0.02	0.01	0.03	0.02	0.01	0.02	0.02
	If anyone consider the player a leader	0.02***	0.02	0.02*	0.02***	0.02	0.02*	0.02***	0.02	0.02**
	There is at least a ML in the session	-0.01	0.01	0.00	-0.01	0.01	0.00	-0.01	0.02	0.00
Controls	*Beneficiary longer than a year*	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Basic Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Network information	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Effort decision	Yes	Yes	Yes	No	No	No	No	No	No
	Cooperation decision round1	No	No	No	Yes	Yes	Yes	No	No	No
	Cooperation decision round2	No	No	No	No	No	No	Yes	Yes	Yes

Robust Standard errors that are clustered at the session level. * Significant at 10%;** significant at 5%; *** significant at 1%.

Table A.5. a. Marginal effects of probit regression model. Cooperation in round 1

VARIABLES	I	II	III	IV	V	VI	VII
Beneficiary longer than a year (enrolment)	-0.12*	-0.15**	-0.14***	-0.12**	-0.06	-0.04	-0.07*
	(0.07)	(0.07)	(0.05)	(0.06)	(0.05)	(0.04)	(0.04)
Cooperation decision round 2			0.29***				0.20***
			(0.08)				(0.08)
Degree of Player (friends)		0.00	-0.00			0.01	-0.00
		(0.01)	(0.01)			(0.01)	(0.01)
Degree of Player (relatives)		0.01	0.02			0.02	0.02
		(0.05)	(0.05)			(0.05)	(0.05)
Degree of Player (acquaintances)		-0.01	-0.01			-0.02	-0.02
		(0.02)	(0.02)			(0.02)	(0.02)
Effort decision		0.06	0.06**				0.03
		(0.04)	(0.03)				(0.03)
Beneficiary is a ML						-0.07	-0.05
						(0.05)	(0.06)
There is at least 1 ML in the session						0.18***	0.16***
						(0.04)	(0.04)
1 if player is chosen as leader by anyone in the group						-0.03	-0.02
						(0.05)	(0.05)
Percentage of informal leaders in the session						0.14	0.28
						(0.23)	(0.21)
Basic characteristics	No	No	No	Yes	Yes	Yes	Yes
Experimental variables	No	No	No	No	Yes	Yes	Yes
Network Information	No	No	No	No	No	Yes	Yes
Observations	714	714	714	712	712	712	712

Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.5. b. Marginal effects of probit regression model. Cooperation in round 2

VARIABLES	I	II	III	IV	V	VI	VII	VIII	IX
Beneficiary longer than a year (enrolment)	-0.03	0.00	0.01	0.02	0.01	0.02	0.04	0.09*	0.10**
	(0.08)	(0.08)	(0.08)	(0.07)	(0.07)	(0.06)	(0.06)	(0.05)	(0.05)
Cooperation decision round 1		0.28***	0.28***	0.28***		0.21***	0.21***	0.19***	0.19***
		(0.07)	(0.07)	(0.07)		(0.07)	(0.07)	(0.06)	(0.06)
Degree of Player (friends)			0.02*	0.02*				0.02*	0.02*
			(0.01)	(0.01)				(0.01)	(0.01)
Degree of Player (relatives)			-0.01	-0.01				-0.02	-0.02
			(0.04)	(0.04)				(0.05)	(0.04)
Degree of Player (acquaintances)			0.01	0.01				-0.01	-0.01
			(0.01)	(0.01)				(0.01)	(0.01)
Effort decision				-0.02			-0.02		-0.02
				(0.04)			(0.04)		(0.03)
Beneficiary is a ML								-0.12***	-0.12***
								(0.04)	(0.04)
There is at least 1 ML in the session								0.17***	0.17***
								(0.0`6)	(0.06)
1 if player is chosen as leader by anyone in the group								-0.03	-0.03
								(0.03)	(0.03)
Percentage of informal leaders in the session								-0.68*	-0.68*
								(0.36)	(0.36)
Basic characteristics	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Experimental variables	No	No	No	No	No	Yes	Yes	Yes	Yes
Network Information	No	No	No	No	No	No	No	Yes	Yes
Observations	714	714	714	714	712	712	712	712	712

Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%;** significant at 5%; *** significant at 1%.

Table A.5. c. Control variables in Table A6a Marginal effects of a probit regression model.

Independent Variable: Cooperation in round 1	IV	V	VI	VII
1 if the player is a woman	0.06	0.13*	0.14**	0.17***
	(0.10)	(0.07)	(0.06)	(0.04)
Age	0.01***	0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
Level of education (0 to 5)	-0.02	-0.03**	-0.03*	-0.02
	(0.02)	(0.02)	(0.02)	(0.01)
Number of years living in the neighborhood	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
1 if the player is displaced (self-declared)	0.02	0.01	0.02	0.00
	(0.05)	(0.06)	(0.06)	(0.05)
1 if the player is the head of household	-0.02	-0.02	-0.03	-0.03
	(0.06)	(0.06)	(0.06)	(0.06)
1 if If the player has a partner	-0.07	-0.06	-0.06	-0.03
	(0.06)	(0.05)	(0.05)	(0.04)
Number of people per room	0.02	0.02	0.02	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
1 if the player has her own housing	-0.06	-0.05	-0.06*	-0.05
	(0.04)	(0.03)	(0.03)	(0.03)
1 if the player's home has no electricity	0.18*	0.17	0.20*	0.18
	(0.10)	(0.11)	(0.11)	(0.11)
1 if the player has a landline	-0.04	-0.05	-0.06	-0.05
	(0.05)	(0.05)	(0.05)	(0.05)
1 if the player has a cellphone	0.03	0.01	-0.00	-0.01
	(0.04)	(0.03)	(0.03)	(0.03)
1 if the player's home has water pipe access	0.00	-0.01	-0.01	-0.03
	(0.06)	(0.05)	(0.05)	(0.05)
1 if the player's home has sewage	-0.03	0.00	0.00	-0.00
	(0.04)	(0.03)	(0.03)	(0.04)
1 if She has received (different from FA) any other government aid	-0.04	-0.03	-0.03	-0.02
	(0.03)	(0.04)	(0.04)	(0.03)
Wealth perception (0-the poorest, 1-the richest)	0.10	0.05	0.05	0.06
	(0.07)	(0.07)	(0.07)	(0.08)
1 if Perceives that HH income is above the average	-0.00	-0.04	-0.03	-0.03
	(0.07)	(0.07)	(0.07)	(0.08)
1 if the HH has a sound player	0.00	0.01	-0.00	0.00
	(0.04)	(0.04)	(0.04)	(0.04)
HH income	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
1 if the HH has a DVD player	0.03	0.00	0.02	0.01
	(0.04)	(0.04)	(0.05)	(0.04)
1 if there is at least one man in the group		0.13**	0.15***	0.13***
		(0.06)	(0.05)	(0.05)
1 if Experimenter n°2 (female) in 2008		0.07	0.11	0.10
		(0.07)	(0.07)	(0.06)
1 if the player understood the activity perfectly		-0.05	-0.06	-0.06
		(0.04)	(0.04)	(0.04)
Number of players in session		-0.06**	-0.07***	-0.06***
		(0.02)	(0.02)	(0.02)
1 if First session in the day		0.23***	0.18***	0.13**
		(0.07)	(0.07)	(0.06)
Average level of cooperation in the last two sessions [a]		0.30**	0.27*	0.20
		(0.14)	(0.14)	(0.13)
Participant socioeconomic characteristics	Yes	Yes	Yes	Yes
Experimental variables	No	Yes	Yes	Yes
Network Information	No	No	Yes	Yes
Observations	712	712	712	712

Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%;** significant at 5%; *** significant at 1%.

Table A.5. d. Control variables in Table A6b Marginal effects of a probit regression model.

Indep.. Variable: Cooperation in Round 2	V	VI	VII	VIII	IX
1 if the player is a woman	-0.24	-0.19	-0.19	-0.19	-0.19
	(0.18)	(0.18)	(0.18)	(0.19)	(0.20)
Age	0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Level of education (0 to 5)	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Number of years living in the neighborhood	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
1 if the player is displaced (self-declared)	0.08	0.05	0.05	0.07	0.07
	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)
1 if the player is the head of household	0.00	0.02	0.02	0.01	0.01
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
1 if If the player has a partner	-0.15***	-0.12**	-0.12**	-0.13***	-0.13***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Number of people per room	0.00	-0.00	-0.00	-0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
1 if the player has her own housing	-0.04	-0.02	-0.02	-0.03	-0.02
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
1 if the player's home has no electricity	0.12	0.09	0.09	0.09	0.09
	(0.10)	(0.09)	(0.09)	(0.09)	(0.09)
1 if the player has a landline	0.03	0.04	0.04	0.04	0.03
	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)
1 if the player has a cellphone	0.08**	0.07**	0.07**	0.06*	0.06*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
1 if the player's home has water pipe access	0.06	0.06	0.06	0.06	0.07
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
1 if the player's home has sewage	-0.03	-0.00	-0.00	0.00	0.00
	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
1 if She has received (different from FA) any other government aid	-0.07**	-0.06**	-0.06**	-0.07**	-0.07**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Wealth perception (0-the poorest, 1-the richest)	-0.02	-0.05	-0.04	-0.03	-0.03
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
1 if Perceives that HH income is above the average	0.03	0.02	0.02	0.01	0.01
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
1 if the HH has a soundplayer	0.02	0.00	0.00	-0.02	-0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
HH income	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
1 if the HH has a DVD player	0.04	0.01	0.01	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
1 if there is at least one man in the group		0.12	0.12	0.14*	0.13*
		(0.09)	(0.09)	(0.07)	(0.07)
1 if Experimenter n°2 (female) in 2008		-0.03	-0.02	0.06	0.07
		(0.07)	(0.07)	(0.05)	(0.06)
1 if the player understood the activity perfectly		0.01	0.01	0.02	0.02
		(0.05)	(0.06)	(0.05)	(0.05)
Number of players in session		0.02	0.02	0.00	-0.00
		(0.03)	(0.03)	(0.04)	(0.04)
1 if First session in the day		0.26***	0.26***	0.24***	0.24***
		(0.07)	(0.07)	(0.07)	(0.07)
Average level of cooperation in the last two sessions [a]		0.20	0.22*	0.14	0.17
		(0.13)	(0.13)	(0.13)	(0.13)
Participant socioeconomic characteristics	Yes	Yes	Yes	Yes	Yes
Experimental variables	No	Yes	Yes	Yes	Yes
Network Information	No	No	No	Yes	Yes
Observations	712	712	712	712	712

Robust Standard errors that are clustered at the session level in parenthesis. * Significant at 10%;** significant at 5%; *** significant at 1%.

Table A.6. Calibrated of the sensitivity parameter $\hat{\lambda}$

$\hat{\lambda}$		Initial Condition	
		Low beliefs	High beliefs
Individual Characteristic	Low exposure	0	0
	High exposure	0	40

vi. Experimental Instructions

The sentences in italic are not read in public; they are instructions for a supervisor and coordinators.

The supervisor introduces the team, the session and reads the consent form in order to obtain oral consent.

Exercise 1 *(The Public Goods Game) (Attanasio et al. 2009, 2015)*

You are going to take part in the first exercise that consists of two decision rounds. Now, we will describe in detail the process that will be repeated in all two decision rounds.

In each round each participant in this room will make a decision in an individual, simultaneous, and silent way. In the beginning of each round, you will be endowed with one token which you will be asked to invest in one of two accounts: a Private (and individual) Account or a Group Account.

Your earnings will partly depend on your decisions and partly on the decisions of the other participants in this room. Specifically, your earnings in each round will depend on the number of tokens in your Private Account and on the total number of tokens in the Group Account in the following way:

• You earn $5,000 if you invest (put) your token in the Private Account.
• For each token that you and other participants invest (put) in the Group Account, every participant will obtain $400.

Note again that each decision is individual and that you should make your own decision without consulting other participants and without announcing where you have put the token: in the Private or in the Group Account.

Now we are going to explain you how to make a decision. Each participant will receive two cards like these.

Show two cards on both sides. Explain that each card has a participation number and a round number. (See Figure A2)

One of the cards has the word “MY TOKEN” written on one side, called henceforth the MY TOKEN card, and the other card has a blank side, called henceforth the BLANK card. When everyone is ready to make a decision, one coordinator will go around the room with a bag to collect one card from each of you.

Show a bag. Explain that this bag indicates the Group Account.

If you want to invest your token in the Private Account, you just need to keep the MY TOKEN card in your pocket and put the BLANK card in the bag. Alternatively, if you

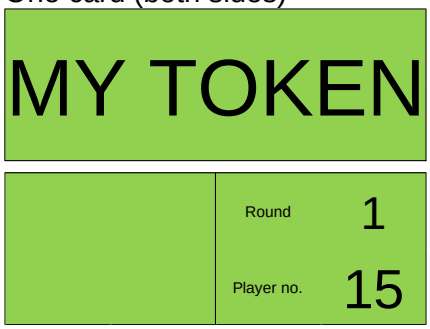
want to invest your token in the Group Account, then you need to put the MY TOKEN card in the bag and keep the BLANK card in your pocket.

Once every participant has put one card in the bag, coordinators will count how many tokens have been placed in the Group Account, that is, how many MY TOKEN cards have been put in the bag. The number of tokens that have been placed in the Group Account in this round will not be revealed until the end of all two exercises. This information will be publicly announced at the end of all the exercises when we compute your total earnings.

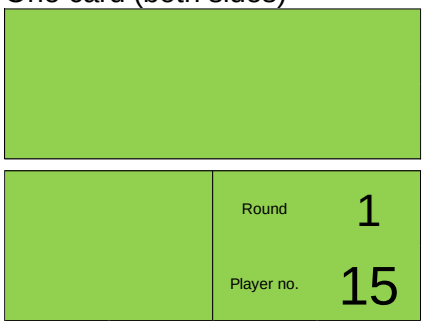
Figure A. 2 Public Goods game Decision cards.

a. Decision cards for the placer no. 15 in the first round: [MY TOKEN], [Round], [Player number]

One card (both sides)

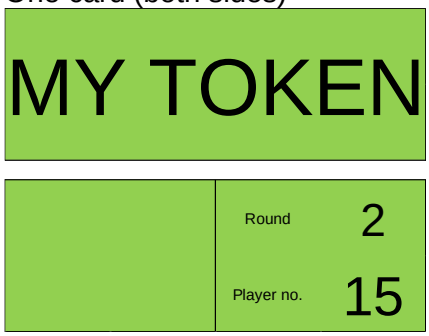


One card (both sides)

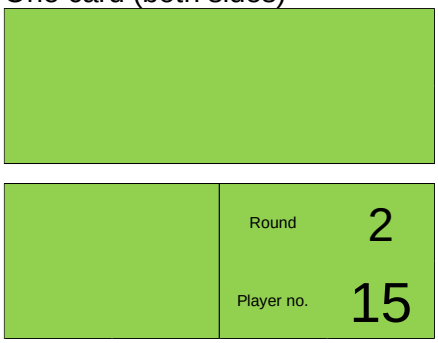


b. Decision cards for the placer no. 15 in the second round: [MY TOKEN], [Round 2], [Player number 15]

One card (both sides)



One card (both sides)



Note that you will retain one card after you have made a decision, regardless of where you have put the token. Please keep one remaining card by the end of today's activities. We will use the card you retain when we compute your earnings. Please do not show any other participants the card you have retained. This entire procedure of decision making is intended to make sure that other participants will not know what decision you have made.

You will explain this exercise further with several examples below. Please remember not to use extreme examples such as the case in which all the participants put their tokens in the Group Account and the case in which all the participants put their tokens in the Private Account. If a participant asks a question by referring to one of these examples, reply to that question by giving afterwards the other example showing what happens in the other situation, and write down the occurrence in the session log.
Please do not skip any examples but go through all examples as in the instructions. Please make sure that all understand the exercise by using the examples below. It is not necessary to follow script by script in each example. However, it is necessary to use the same values in each example that are shown in the instructions.

Now let's take several examples to see how the exercise works. Please pay careful attention and feel free to ask any question if you do not understand examples. Throughout the examples, we will assume that there are 25 participants.

Show which card one should put in each example. After each example a coordinator should show how one should fold the card so that a coordinator cannot read the card.

• Suppose that you decided to invest your token in the Private Account. In other words, you have put the BLANK card in the bag and have kept the MY TOKEN card in your possession. After all the participants made their decisions, a coordinator will count how many MY TOKEN cards are in the bag. Suppose that there were 13 MY TOKEN cards in the bag. That is, 13 out of 25 people decided to put their tokens in the Group Account and the other 12 people (including you) decided to keep their tokens in their Private Accounts. Then, each participant will earn $5,200 (= 13*$400) pesos from the Group Account. Since you have decided to keep your token in your Private Account, you will earn $5,000 pesos from the Private Account. Therefore, your earnings in this round are the sum of earnings from the Group Account and the Private Account, which is $5,200 + $5,000 = $10,200 pesos.

• Now consider the same above example. Instead of calculating the earnings of participants who have kept their tokens in Private Accounts, let's consider a participant who invested his/her token to the Group Account. This participant will not receive any earnings from his/her Private Account since his/her token was not invested in his/her Private Account. Therefore, his/her earnings in this round are simply earnings from the Group Account: $5,200 pesos.

• Now let's take another example. Suppose that you decided to invest your token in the Group Account. That is, you have put the MY TOKEN card in the bag. After all the participants made their decisions, a coordinator will count how many MY TOKEN card were put in the bag. Suppose that there were 20 MY TOKEN cards in the bag. That is, 20 out 25 people (including you) invested their tokens in the Group Account, while the other 5 people kept their tokens in their Private Accounts. Each participant will earn $8,000 (=20*$400) pesos from the Group Account. Since your token was not invested in your Private Account, you earnings are determined by the earnings from the Group Account, which is $8,000 pesos.

• Let's consider the same example but with a participant who kept the token in his/her Private Account. This participant earns $8,000 pesos from the Group Account. In addition, he/she earns $5,000 pesos from the Private Account since he/she invested the token in the Private Account. Therefore, the earnings for this participant are the sum of $8,000 and $5,000, which is $13,000 pesos.

• Let's have one more example. Suppose that you decided to invest your token in your Private Account. That is, you have put the Blank card in the bag. After all the participants made their decisions, a coordinator will count how many MY TOKEN card were put in the bag. Suppose that there were 5 MY TOKEN cards in the bag. It means that 20 out of 25 people (including you) kept their tokens in their own Private Accounts and the other 5 participants invested their tokens to the Group Account. Then, each participant earns $2,000 (=5*$400) pesos from the Group Account. In addition, you earn $5,000 pesos from your Private Account since you kept your token in the Private Account. Therefore, your earnings in this round are in total $7,000.

• Now consider the same example but with a participant who invested the token to the Group Account, that is, who put the MY TOKEN card in the bag. This participant will earn $2,000 pesos from the Group Account like all other participants. However, this

participant will earn nothing from his/her Private Account since his/her token was not invested in his/her Private Account. Therefore, earnings for this participant in this round are $2,000 pesos.

In summary, if you invest your token in the Group Account by putting your MY TOKEN card in the bag, every participant in this room will earn from your investment by $400 pesos. Alternatively, if you invest your token in your Private Account by putting the Blank card in the bag, you will be the only one that earns $5,000 from this decision.

Is there any question?

Coordinators should make sure that all participants have understood the exercise itself and its procedure.

Shall we start the first exercise? Note again that your decisions are private: no other participant will know where you invest your own token. Please do not consult any other participants about what decisions you should make.

Let's start the first round of the exercise. Coordinators will hand out two cards (a MY TOKEN card and a BLANK card) for each participant. Remember that you keep one and put the other in the bag. **Please do not throw away the card you decided to keep**. **You need to keep this card by the end of today's activities. We will compute your earnings at the end, using cards you have kept.**

At this moment, coordinators distribute exercise cards to participants according to PARTICIPANT NUMBERs, that is, the order they were seated in the U. Please check if a number that is on two cards corresponds to an identification number of each participant. Check also that each coordinator delivers cards corresponding to a correct round.

Once all the participants have finished playing the first round, two coordinators count the numbers of blank cards and "MY TOKEN" cards in the bag. These coordinators should fill the MONITORS CALCULATION SHEET. Nothing is announced to participants at this point. Before initiating round 2 one of the coordinators should start taking care of the FINAL PAYMENTS SHEET (F6) outside the room and fill the payment receipts with full name and ID.

Important. When collecting cards, the coordinator should not have any physical contact with participants' cards. Each participant should put his/her card directly in the bag. Nevertheless, coordinators should verify if any participant has placed two cards in the bag or if there is a participant that has not decided yet. Coordinators inside the room cannot leave the group alone thus they cannot exit to count the results.

Please keep your retained card in your pocket by the end of today's activities. We will use this card when we compute your total earnings.

Now let's start the second round. Before the second round of this exercise, you will have an opportunity to communicate for 10 minutes with one or more participants in this room about this exercise. This communication is totally voluntary. After the 10-minute permitted time is over, all communications will be suspended. And we will proceed to the second round of this exercise. Coordinators will hand out two cards (a MY TOKEN card and a BLANK card) for each participant. Just as in the first round, you will just need to decide which card (either MY TOKEN card or BLANK card) you want to put in the bag. Again all decisions in this round will be private and be kept strictly confidential.

Past the 10 minutes, once all the participants have finished playing the second round, two coordinators should count how many blank cards and how many "MY TOKEN" cards there are in the bag. These two coordinators should fill the MONITORS CALCULATION SHEET and finish processing the PAYMENT SHEET.

Please keep this second round card safe, since we'll use these cards to calculate your earnings at the end of today's activities.

Today's first exercise is finished. Thank you all for the cooperation.

Before participating in the second exercise, we will ask each of you to fill out a short form. While a coordinator works with you to fill out a form, you are offered snacks that we have prepared for you. It will take approximately 20 minutes. After that, we will start the second exercise.

At this moment coordinators start to help each participant fill out the network-connectivity questionnaire. When all the participants finished the survey, one coordinator will process the information of network connectivity to form 3 different sub groups. Another coordinator is filling up the PAYMENTS SHEET. The rest of coordinators start asking the postgame survey to participants.

Exercise II *(The Coordination game)*

Now you will participate in the second exercise. This exercise is independent of the first exercise which you already participated in. Your earnings in this exercise are not related to the decisions you made or earnings you obtained in the first exercise.

In this exercise, each of you will be assigned to one of three groups. Allocation into groups is determined by the coordinators. The allocation into groups will be announced after we explain the exercise. Each group will move to a separate classroom in order to participate in this exercise. After each group finishes the second exercise in a different classroom, we will meet all together again in this room and we will then proceed to calculate your earnings in the first exercise and in the second exercise.

Is there any question?

Shall we start?

This exercise consists of a single round in which you will make one decision. In this exercise, each participant in a group will make one decision, individually, simultaneously and in silence. Each participant will choose an individual level of effort to a Group Project. Any participant can neither see nor discuss what other participants in the group choose. There are three possible units of effort, {1, 2, 3}, where "1" may be interpreted as a low level of effort to the Group Project, "2" as a medium level of effort to the Group Project and "3" as a high level of effort to the Group Project. When you are ready to choose, you just need to mark with a cross X the number you wish to choose in the YELLOW DECISION SHEET as this one (see figure A3). In this card, there is the player number and the three possible options of levels of effort. You will choose your low, medium or high level of effort marking the cell with a X.

Exercise 2			
L1	L9		
Player no.	My Decision (level of effort)		
	1	2	3

Figure A. 3 Decision card, Minimum Effort game

Your earnings in this activity are determined as follows:
You will be in a group of 8 or 9 people.
At the beginning of the activity, each of you will have $3,000.
Your earnings will depend on your decision and the lowest level of effort among all group members.
Your earnings, given by these $3000 may decrease depending on the level of effort you choose and increase depending on the minimum level of effort in the group.

You decide the level of effort {1, 2 or 3} units of effort. You mark it on your yellow decision sheet. Once everyone in the group has made its decision, a coordinator will collect all yellow decision sheets.

We will know what the minimum level of effort is among all players in the group, this could be {1, 2, or 3} and multiply that minimum effort times $3000 and each of you win that amount.

If the minimum effort in the group is 1, i.e. the lowest level of effort among all the people in the group is 1, i.e., at least 1 person chose the low level of effort, the earnings for everyone in the group are $ 3,000 * 1 = $ 3000.

If the minimum effort in the group is 2, i.e. the lowest level of effort among all the people in the group is 2, i.e., no one chose 1 and at least one person chose the medium level of effort, the earnings for everyone in the group are $ 3,000 * 2 = $ 6000.

If the minimum effort in the group is 3, i.e. the lowest level of effort among all the people in the group is 3, i.e., no one chose either 1 or 2 and everyone chose 3, the high level of effort, the earnings for everyone in the group are $ 3,000 * 3 = $ 9000.

Then you must subtract from those earnings, according to your level of effort, $2,000 for each unit of effort you decided to add to the group project.

Per unit effort you must subtract $ 2,000: If you choose 1 unit of effort, the cost of this unit is (1*2000 = $2000) and you must subtract from your earnings $2000. If you choose 2 units of effort, the cost of these two units is (2*2000 = $4000) and you must subtract from your earnings $4,000. If you choose 3 units of effort, the cost for these three units is (3*2000 = $6000) and you must subtract from your earnings $6,000.

Which can be summarized in the following table:

The coordinator will show the formula and table on a poster (See figure A5).

In summary, the calculation of your earnings can be seen as follows:

My Earnings = $ 3,000 + $ 3,000 X the minimum effort in the group (the lowest level of effort among all group members) - $ 2,000 X each unit effort

In brief, your earnings decrease the higher your level of effort and increase the higher the minimum effort in the group.

To help participants understand their earnings, the coordinator will use the examples in that order.

How should we read this table? Each row, called my decision of level of effort indicates the earnings you could obtain for different levels of the minimum effort in the group. For example, if you choose 3, you can either win $6,000, $3,000, or $0. Each column indicates the earnings you could obtain for different minimum levels of effort in the group, i.e., the lowest effort among all effort levels chosen by the group. For example, if the minimum effort level chosen in the group is 2, then you win or $ 3,000 or $ 5,000.

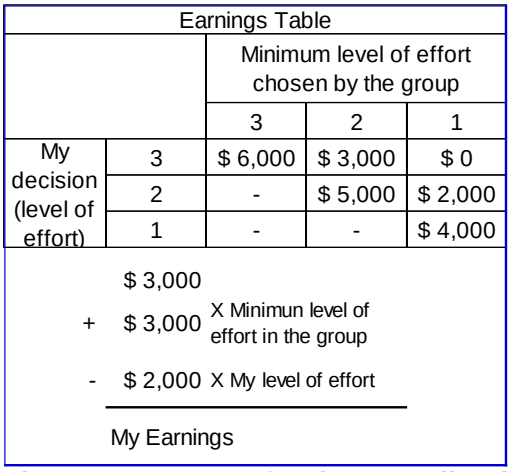


Figure A. 4 Poster for the Coordination Game

Let's do some examples to understand how earnings are determined. Please pay close attention and feel free to ask if anything is not clear in the examples.

• Suppose you choose an effort level of 1. Since you have chosen the lowest level of effort possible, the minimum effort in your group is 1, regardless other levels of effort that the other participants have chosen. Then the group project benefit is $3,000 for each member ($3,000*1). Furthermore, the cost of your own effort level that is subtracted from your earnings is $2,000 ($2,000*1). Therefore, your earnings will be $3,000 + $3,000 - $2,000 = $4,000, which is where the row of your effort level 1 intersects with the minimal effort column equal to 1.

• Suppose you choose an effort level 3, and the minimum effort in your group is 1, i.e. among all levels of effort in your group, the lowest one is 1. This means that at least one participant in your group chose an effort level of 1. Since the minimum level of effort in your group is 1, the group project benefit is $3,000 (=$3000*1) for each member. And as your own effort level is 3, the cost of your effort that is subtracted from your earnings is $ 2000 * 3 = $ 6,000. Therefore, your earnings will be $ 3,000 + $ 3,000 - $ 6,000 = $ 0, which is where the row of your effort level 3 intersects with the minimal effort column equal to 1.

• Suppose you choose an effort level of 3, and the minimum effort level in your group is 3. This means that all participants (including yourself) in your group, chose an effort level of 3. Then the group project benefit is $ 9000 (=$3.000 * 3) for each member. And as your own effort level is 3, the cost of your effort that is subtracted from your earnings is $ 2000 * 3 = $ 6,000. Therefore, your earnings will be $ 3,000 +

$ 9,000 - $ 6,000 = $ 6,000, which is where the row of your effort level 3 intersects with the minimal effort column equal to 3.

• Suppose you choose an effort level 2 and the minimum effort level in your group is 2. This means that everyone in your group chose or 2 (like you) or 3. Since the minimum effort in your group is 2, the group project benefit is $ 6.000 (= $ 3.000 * 2) for each member. And as your own effort level is 2, the cost of your effort that is subtracted from your earnings is $2000*2 = $4,000. Therefore, your earnings will be $ 3,000 + $ 6,000 - $ 4,000 = $ 5,000, which is where the row of your effort level 2 intersects with the minimal effort column equal to 2.

• Suppose you chose an effort level 2 and the minimum effort level in your group is 1, i.e. among all levels of effort in your group, the lowest one is 1. This means that at least one participant in your group chose an effort level of 1. Since the minimum effort in your group is 1, the group project benefit is $ 3,000 (=$3000*1) for each member. And as your own effort level is 2, the cost of your effort that is subtracted from your earnings is $ 2000 * 2 = $ 4,000. Therefore, your earnings will be $ 3,000 + $ 3,000 - $ 4,000 = $ 2,000, which is where the row of your effort level 2 intersects with the minimal effort column equal to 1.

• Suppose you choose an effort level 3 and the minimum level of effort of the group is 2. This means that everyone in your group chose or 2 (like you) or 3. Since the minimum effort in your group is 2, the group project benefit is $ 6.000 (=$3,000*2) for each member. And as your own effort level is 3, the cost of your effort that is subtracted from your earnings is $ 2000 * 3 = $ 6,000. Therefore, your earnings will be $ 3,000 + $ 6,000 - $ 6,000 = $ 3,000, which is where the row of your effort level 3 intersects with the minimal effort column equal to 2.

Note that the more units of effort you choose is more costly for you but that the higher is the minimum effort, you and others in the group earn more.

Are there any questions?

After each group has completed the activity, it will be announced the minimum effort chosen in the group. Then we meet again in this room to finish today's activities. We will announce the number of cards MY TOKEN invested in the group account for the first and second round of the first activity. A coordinator will call you to answer a questionnaire. When you have completed the questionnaire, you will go with another coordinator to calculate the total earnings of the two activities and will receive your total earnings.

Are there any questions?

Are there any on this activity? Please do not talk to anyone about the exercise.

Now we will form three groups and announce which group each participant belongs to. From this moment onwards we ask you to remain silent.

Participants are allocated into groups according to the network score. The main coordinator will announce which group each participant is allocated to. There is a room assigned to each group with its assigned coordinator. Please ask participants to remain silent when they move to another room and during the experiment. The main coordinator keeps the group C.

Please remain silent when moving from one room to another and during activity.

Each coordinator in his/her group: Let's start the only round of this activity. A coordinator provides the YELLOW CARDS to each participant. Please make sure the player number matches with your player number. Please make your choice by marking an X on the level of effort you want to choose.

At this time, the coordinators give each participant the YELLOW DECISION SHEET according to their player number. Check whether the player number on the sheet is the same as the player number.
Once participants have made their decision, the coordinators will collect the YELLOW CARDS in an envelope and find out the minimum effort in the group. These coordinators must fill out the MONITORS CALCULATION SHEET.
Important. When the yellow decision CARDS are collected the coordinator should check whether every participant made a decision.

Today's second exercise is finished. Now we will move back to a classroom where we participated in the first exercise.
Each coordinator announces the results ONLY for his//her group and then, groups gather in the main room.
The lowest effort level chosen was, which means that if you decided one unit of effort, your earnings are ... if you decided 2 units of effort your earnings are ... and if you decided 3 units of effort, your earnings are ...

The main coordinator announces the results of the first activity. Participants are called to answer the survey and then receive their earnings.

We're going to calculate your earnings and we'll call you one by one. For the first exercise, we are going to announce the number of tokens that were invested in the group account in the two rounds.

Then the coordinator will announce the number of tokens that have been invested in the group account in the two rounds. Afterwards, two coordinators will go behind two desks to calculate each participant's earnings for each round and the final earnings of this exercise.

The number of tokens in the group account in the first round was … in the second round was… This means, in terms of earnings, that in the first round, the group account has earned… ($400x the number of tokens = total amount); in the second round…

Now each one of you should wait until one of the coordinators calls your name to calculate your earnings and hand you the payment of today's activities. In the meantime, one coordinator will be calling you to ask you to answer a short questionnaire.

We strongly recommend you not to discuss today's activity with someone in next groups because activities for next groups may be different and thus participants in next groups might get confused by receiving incorrect information.

vii. The post-game survey and Traditional measures of social capital

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, a module about FA (e.g. date of enrolment), a module about the game (e.g. whether the participant understood the game). The following are the different questions we used to build our traditional measures of social capital.

Table A.7. Survey Social Capital measures

Question		Variable	
Civic Participation			
Do you think that in this neighborhood there are opportunities to participate and give an opinion on the decisions that affect most people? If Yes, have you participated in these discussions?		Participation in neighborhood decisions	
How often there have been meetings in the neighborhood to work out problems in the community? For example, to discuss problems about the streets, the school or cleaning the park. Have you or anyone from your Household participated in these initiatives?		Participation in the neighborhood meetings	
Do you actively participate in any of the following groups, associations or organizations?		Membership in at least one organization	
If Yes, do you attend to the meetings? Yes/No		If is member of any civic association	Attendance
If Yes, do you participate in the decision making processes? Yes/No			Decision maker
If Yes, Are you one of the leaders in the organization?			Leader
If Yes, Do you support the organization with money or volunteer work?			Supports with money or work
If yes, how many hours do you spend in this organization monthly?			No. Hours
Humanitarian or charitable organization		Bonding social capital	
Church or religious organization			
Volunteer work with the community organization		Bridging social capital	
Environmental organization			
Ethnic organization			
Sport or recreational or cultural organization			
Education organization (i.e. parents association)		Linking social capital	
Community action [Accion comunal] association			
Security organization			
Labor Union, cooperative or professional organization			
Political party			
Voting behavior			
Did you vote in the last local elections in 2007 (Mayor, council, State, Congress)?		Voted in local elections (2007)	
Did you vote in the last presidential elections in 2006?		Voted in presidential elections (2006	
Trust and cooperation perception			
Generally speaking, would you say that most people in your community can be trusted, few people can be trusted or that you need to be very careful in dealing with people?		Trust: do you consider that you can trust in?	Most people, Few people, None
Would you say that most people in your community try to be helpful, they help only if others help or that they are just looking out for themselves?		Perception within the community	Cooperation, Reciprocity, Self-regarding
Altruism			
In the last 12 months, how often do you help anyone (different from family) with any of the following (Always, usually, sometimes, rarely, never)…	food, money, clothes	Always or usually, the player has helped anyone…	with money, food or clothes
	by offering your seat in the bus		by offering her seat in the bus
	Domestic work		with domestic work
	Carrying a case, box, bag, package, groceries		by carrying a case, box, bag
	Child care or taking care of the animals, the plants or the house when the person is not present		by taking care of the house or children

II. Identification of Other-regarding Preferences: Evidence from a Common Pool Resource game in Colombia[*†]

March 27th, 2015

Abstract

Social preferences have been important to the explanation of deviations from Nash equilibrium in game outcomes. An enduring challenge in any model of other-regarding preferences is to identify heterogeneity within the population. Using data from a common pool resource (CPR) game in the field with $1,095$ individuals (21% students and 79% villagers, users of a CPR) we estimate a structural model including preferences for altruism, reciprocity and equity. We identify behavioral types using a latent class logit model. Exogenous determinants of type are examined such as socio-economic characteristics, perceptions on the CPR, perceived interest in cooperation among the community, whether the participant does volunteer work and whether the CPR is the household main economic activity of the household.

A competing explanation of deviations from Nash equilibrium is the existence of a cognitive factor: the construction of a best reply might make rational expectations about other players' mistakes (e.g. quantal response equilibrium). Whilst a cognitive aspect would help the model better fit the data, we do not find much evidence for cognitive heterogeneity, and instead a great deal of behavioral heterogeneity. Choice prediction based on types is robust out of sample.

JEL classification: Q2, C51, C23, C93, D64, H39, H41.

Keywords: Common-pool resources, social preferences, laboratory and field experiments, explicit incentives, inequity aversion, latent heterogeneity, finite mixture models.

[*]This chapter is joint work with David Echeverry (University of California Berkeley). My contribution to this chapter consists of the literature review, the theoretical framework for the structural estimation and the data and regression analysis. All co-authors worked equally in the text.

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1 Introduction

Common pool resources (CPR) are held by a collective of individuals for each of them to extract from in order to derive an individual payoff. At their heart lies the danger that profit-maximizing individual behavior leads to depletion of the common pool resource, and hence to a loss of utility for all the agents. The social dilemma that arises from the wedge between the Nash equilibrium (NE) and the social optimum is a key concern for the governance of the commons. The corresponding (positive) question of how agents respond to social dilemmas in real life is key to make inference about the motivants of individual behavior.

Consistent deviations from NE have been documented in the empirical literature (Rassenti et al., 2000). The existence of pro-social behavior has been widely documented, suggesting that social preferences are important influences on economic behavior Fehr et al. (1997); Bewley (1999); Fehr and Schmidt (1999); Fehr and Gächter (2000); Sobel (2005). An individual behaves pro-socially in order to help others -including himself- to achieve a common good. Social preferences are those concerns for the well-being of others and desires to uphold ethical norms. They reduce social inefficiency in the absence of complete contracts (Arrow, 1971; Becker, 1976; Akerlof, 1984) and thus are key to solve social dilemmas (Ostrom, 1990), in which the uncoordinated actions of individuals result in an outcome that is Pareto inefficient.

Charness and Rabin (2002) find empirical evidence about preferences for altruism (social welfare in their setting), for reciprocity and to a lesser extent against inequality. Similarly, Bolton and Ockenfels (2000) argue for other-regarding preferences in the sense of concern for total welfare. Fehr and Schmidt (1999) argue inequity aversion is important. Since then, structural models of pro-social behavior Falk and Heckman (2009); Manski (2011); Andreozzi et al. (2013) have been used to assess the prevalence of a given type of social preferences.

Social preferences have been studied in the context of public goods games (Isaac and Walker, 1988). Common Pool Resource games have been implemented by assuming homogeneous preferences either with only students (Fischbacher et al., 2001; Kurzban and Houser, 2001; Carpenter et al., 2009; Falk et al., 2002) based on Walker et al. (1990) or only real users (see evidence in forest management by Rustagi et al. (2010) or Margreiter et al. (2005); Vélez et al. (2009)). In our dataset, users deviate more from the NE than students (Cárdenas (2004), 2011) which is in line with other empirical findings (Carpenter and Seki, 2010; Molina, 2010; Cárdenas et al., 2013). Several of these papers explain their findings with the existence of social preferences but do not explore the role of heterogeneity.

Yet heterogeneity of social preferences is both a prevalent and a relevant phenomenon (Charness and Rabin, 2002; Leider et al., 2009; Manski and Neri, 2013; Kurzban and Houser, 2005; Goeree et al., 2002; Burlando and Guala, 2005). Using a random coefficient model, Polania-Reyes (2014) identifies types using the same classification as we do. Specifications similar to ours have been used before. Similarly, Vélez et al. (2009) use a random effects specification to assess the prevalence of different social preferences, finding evidence of preferences for conformity. Rodriguez-Sickert et al. (2008) use a model

similar in spirit to derive preferences for selfishness, altruism and cooperation. They focus on the effect of incentives rather than on type identification, as we do here. Compared to these studies, our contribution lies on type identification, which remains largely unexplored among the empirical literature, and less so within a structural model.

This paper uses a structural approach to examine which types of social preferences individuals exhibit in a common pool resource environment, in which the CPR is collectively owned or shared (e.g. natural resources, land, software) and foregoing the overexploitation of the jointly used resource leads to a Pareto superior outcome. Understanding heterogeneity of individual preferences in this environment is the first step to the design of Pareto efficient incentives: we estimate simultaneously the distribution of types proposed by economic theory and the parameters of each type in our sample. We then examine determinants of social preferences as suggested by the empirical literature (Almås et al., 2010).

One recent development towards a structural model with heterogeneous preferences has been the simultaneous estimation of preference parameters and type composition in the sample by means of finite mixture models (Cappelen et al., 2007, 2010). Finite mixure models have been applied to estimate structural models such as Cappelen et al. (2011, 2013). But to our knowledge this is the first model to identify behavioral types with their preference parameters under heterogeneous social preferences. To do so we use a latent class model as described in Pacifico (2012), namely the expectation maximization (EM) algorithm.

A growing literature exploiting latent class models (Train, 2009) to identify social preferences (Breffle et al., 2011; Morey et al., 2006) has already proven fruitful in Ecological Economics. Varela et al. (2014) study the heterogeneity of social preferences for fire prevention in Europe; they argue for the existence of four types: typical, yea-saying, burnt-worried and against. Farizo et al. (2014) estimate a multilevel latent class model to capture five classes: typical, environmentalist, budgetary, futurist, against. Our estimation method closely follows those used in these papers within a CPR context. However, our use of the structural model is grounded in the theory of social preferences literature reported before gives external validity to our labeling of altruists, selfish, reciprocators and inequity averse.

Our sample is composed of both students and real users of the CPR and has different CPR environments (water, firewood and fish) under a rich set of (economic and non economic) incentive schemes. Second, we improve the type classification method currently used in the literature of social preferences (i.e. random coefficients model) and explain these types of social preferences within an economic model. Finally, we propose an alternative method based on a structural estimation of both the type preferences composition and their respective theoretical parameters.

An alternative to individuals deviating from the NE out of concern for others' outcomes or behavior is the incorporation of (foreseeable) errors into the best reply function. This is the principle of QRE - quantal response equilibrium (McKelvey and Palfrey, 1995). Close to the concept of QRE, another possible explanation of the consistent deviation from Nash behavior comes from the dynamic aspect of learning. Using the same data we use for the present study, Cárdenas et al. (2013) argue that students' behavior likely follows a

payoff sampling equilibrium (PSE). It is a satisfactory explanation for several features of the distribution of outcomes.

Though QRE has been suggested as a competing explanation as opposed to social preferences, they can actually work as complements (Arifovic and Ledyard, 2012). In particular, it has been shown that QRE cannot account for social preferences (Ioannou et al., 2012; Hoppe and Schmitz, 2013). Largely due to the fact that it only uses one parameter, it cannot explain the large cross-sectional variation in the data. We compare the predictions from our model to a baseline QRE in order to highlight the tradeoff between parsimony and goodness of fit across the two specifications.

Burlando and Guala (2005) discuss the learning process in repeated games and conclude that the 'decay of overcontribution' over time, which depends critically on the group composition. Group composition is indeed an important factor, which lends credency to the QRE approach. Empirical findings suggest a negative relationship between group heterogeneity and public goods provision (e.g. Alesina et al. 1999; Miguel and Gugerty 2005; Vigdor 2004, Lucas, Oliveira and Banuri 2012; Fischbacher and Gächter 2010; Gächter and Thöni 2005). In that sense, Arifovic and Ledyard (2012) combine other-regarding preferences and learning. Subjects have a utility function determined by their own payoff, the average group payoff (altruism, or welfare) and the level of disparity between their own payoff and the group average (envy). We use a similar specification[62].

One main difference between our model and theirs is that in their model, agents only have other-regarding preferences (ORP) over outcomes and not over intentions, which implies reciprocity arises as an equilibrium behavior and not as a type.[63] Because our empirical estimates allow to identify individual types, and given the novelty of their approach, one of our contributions is to assess the empirical soundness of introducing a 'cooperator' type.

Compared to Arifovic and Ledyard (2012) beliefs in our model are simplistic: they only take into account immediately preceding experience, whereas their model incorporates evolutionary learning. In such setting, the variables are a finite set of remembered strategies for each agent and a corresponding probability distribution; learning happens by experimentation, replication and learning. Now, just as preferences can be heterogeneous so can approaches to learning be. Because our focus is on heterogeneity and we cannot jointly identify heterogeneous social preferences and (indepedent) heterogeneous cognitive types, we shut down the learning channel allows to focus on the preferences one.

The paper is organized as follows. In section 2 we introduce the CPR framework for this study. In section 3 we introduce the different models of social preferences we will examine. Section 4 estimates the latent class model. Section 5 concludes the paper by summarizing the main results and suggesting points of future research.

[62] Our specification follows Fehr and Schmidt (1999) rather than Arifovic and Ledyard (2012), who nevertheless establish the equivalence between the two formulations.

[63] The model in Vélez et al. (2009) highlights the difference between playing reciprocally and being a reciprocator type. In their model, reciprocal behavior arises from preferences for conforming to what others are expected to do.

2 Common Pool Resource framework

i Description of the CPR game

The setting is well explained in Cárdenas (2004),(2011). Here we briefly describe the game.[64]

Each individual i is endowed with $e = 8$ units of effort (e.g. hours of extraction or investment in equipment) which he can use to extract $x_i \in \{1, \ldots, 8\}$ units from the CPR. Given the group size $n = 5$ and the other players' extraction decisions $x_{-i} \in \{4, ..., 32\}$, the individual payoff is given by

$$\pi_i = \pi(x_i, x_{-i}) = ax_i - \frac{1}{2}bx_i^2 + \varphi(40 - (x_i + x_{-i})) \tag{1}$$

In our setting $(a, b, \varphi) = (60, 5, 20)$. The payoff features direct benefits from extraction $60x_i - \frac{5}{2}x_i^2$ and the indirect costs from depletion $\varphi(40 - (x_i + x_{-i}))$ following from i's as well as others' extraction level.

Participants play a finitely repeated ($T = 10$)[65] partner matching game. At the beginning of period t individuals decide simultaneously (x_{it}, x_{-it}). At the end of period t, the experimenter announces aggregate extraction $(x_{it} + x_{-it})$ and players are informed about other players' aggregate behavior. That is i does not know individual extraction by $-i$. She only knows the *average* extraction by $-i$: $\overline{x}_{-it} = \frac{\sum_{j \neq i}^{n-1} x_{jt}}{n-1}$. The lack of detail about individual extractions favors the simplification of learning aspects in order to focus on the identification of preferences.

The composition of the group remains the same during the following T rounds $t = 11, \ldots, 20$. At the beginning of round 11 the experimenter announces (and implements) an incentive. The incentive could be monetary (fine or subsidy) or non-monetary (e.g. affecting reputation or other considerations rather than payoffs).

The efficient outcome or social optimum (SO) maximizes the aggregate payoff of the group

$$(x_1^{SO}, \ldots, x_5^{SO}) = \underset{(x_1,\ldots,x_5)\in\{1,\ldots,8\}^5}{\arg\max} \sum_{i=1}^{5} \pi_i.$$

Our socially optimal decision of extraction $x_i^{SO} = 1$ of extracting the minimum level possible. There is a conflict between the Nash equilibrium and the socially efficient strategies (see table 8). The Unique Nash Equilibrium (NE) for a self-regarding individual is given by

$$x_i^{NE} = \underset{x_i}{\arg\max}\, \pi_i \; \forall \, i$$

[64] Apart from the game outcomes, survey data was gathered covering sociodemographic (idiosyncratic) factors, though only for villagers. For this reason we can only estimate our social preferences model for villagers.

[65] Individuals did not know how many rounds they would play. There were 2 example rounds and 1 practice round and the game started once the experimenter assured the participants understood the procedure.

which gives $x_i^{NE} = e = 8$.[66]

The subgame perfect Nash equilibrium of the repeated game is the same for all the rounds and is equal to the Nash equilibrium, i.e. the self-regarding outcome. Individuals might behave pro-socially in the presence of reputation effects (Kreps et al., 1982; Bohnet and Huck, 2004; Mailath and Samuelson, 2006) (see figure A.1 in the appendix). However, our setting precludes such a reputation channel.

ii Game outcome in the field

The final sample is composed of 230 students and 705 villagers.[67] The first result to motivate this study is that individuals who consistently play the NE strategy are a small proportion of the sample. Figure 5.1 presents how consistently (between 1 and 10 times) individuals extracted 8 units.

Overall, 35% of the players never played the NE strategy, a quarter of the players chose the NE strategy only once among the ten rounds and only 2% of 1000 individuals played the NE consistently. Figure 5.2 shows the path of average extraction.

Students extract consistently more than villagers (though the difference does not appear to be significant). Also students seem more prone to the last-round effect: between rounds 9 and 10 the proportion of the sample extracting 8 units goes from 18% to 28% among students while in the villager sample it remains at 18%.[68] This raises the question of whether students and villagers have differing levels of rationality. Following Cárdenas et al. (2013) we estimate a QRE model and compare the outcome for both samples.

iii Static quantal response equilibrium

We estimate a logit QRE specification following Cárdenas et al. (2013) and extend it to the sample of villagers. Suppose players make errors in choosing from $\{1, \ldots, e\}$ but the distribution of choices $P(x = k)$, $k \in \{1, \ldots, e\}$ is common knowledge. If $\pi(x_i, x_{-i})$ is the payoff for x_i given others' pure strategy x_{-i}, let $\pi(x_i, P)$ be the expected payoff of x_i given others' are mixing strategies according to $P(.)$. Then the logistic QRE[69] associated to the parameter $\lambda \in [0, \infty)$[70] is a stable outcome of a belief and choice formation process given

[66] Both the social optimum and the Nash equilibrium are corner solutions, which constitutes a potential drawback for identification (especially under incentives). Cárdenas et al. (2013) point out that QRE outperforms payoff sampling equilibria under corner solutions.

[67] Though the full sample contains 865 villagers, some of them ended up participating more than once. The second observation for those who did have been removed. See Polania-Reyes (2014).

[68] If looking at the last-round effect in terms of the cooperative strategy (extract 1 unit) we see a slight reduction for students (8% to 7%) and an increase for villagers (12% to 14%), which speaks to the stylized finding that CPR users have the habit of "not finishing everything on the table".

[69] Logit is the most common specification for a QRE. Assuming a symmetric equilibrium, errors ϵ_{ik} of individual i adopting strategy k are independent and identically distributed according to a type I extreme value distribution.

[70] λ indicates the degree of rationality: when $\lambda \to \infty$ (the error rate tends to zero) subjects are rational and when $\lambda = 0$ subjects are acting randomly according to a uniform probability function.

by

$$P(x_i = k) = \frac{\exp(\lambda\pi(k, P))}{\sum_{j=1}^{8} \exp(\lambda\pi(j, P))} \forall k \in \{1, \ldots, 8\}.$$

λ is chosen to match the QRE distribution, which derived from the payoff function alone, to the empirical distribution. Like Cárdenas et al. (2013) we choose λ in order to minimize mean squared error (MSE). Figure 5.4 shows the outcome:

The value of λ minimizing MSE is very close across the samples: 0.03 for students and slightly lower for villagers at 0.02. Though this suggests a somewhat higher level of rationality among the student sample, the order of magnitude is the same. We take this as indicative evidence that using a constant λ across the population and across types is an adequate assumption.

Figure 5.5 compares the predicted and realized distributions. As Cárdenas et al. (2013) point out, a slightly better fit is achieved within the student sample ($MSE_s = 0.053\%$) than that of villagers ($MSE_v = 0.065\%$). In particular, the higher (respectively lower) incidence of payoff-maximizing (resp. socially efficient) behavior among students seem better matched by the QRE. However, and in spite of the overall constant trend over time (see figure 5.2) a lot of cross-sectional variation remains that cannot be explained using symmetric strategies. We now turn to a model of other-regarding preferences.

3 A structural model of social preferences

We introduce a structural model to estimate preferences for altruism, selfishness, reciprocity and inequity aversion.

Individual i with preferences type q has a utility function U_i^q where $\mathbf{\Theta} = \{\rho, \mu, \beta\}$. We will consider the most popular types of individuals in the behavioral economics literature: i) self-regarding, ii) altruist, iii) reciprocator and iv) inequity averse. All these other-regarding preferences are defined over payoffs: they incorporate concerns over outcomes (as captured by the payoffs). Reciprocators instead exhibit preferences over behaviors (as captured by others' extraction levels).

As discussed in section 1, a general specification of preferences takes into account own payoff, others' payoff and others' behavior. Consequently, at each point in time each individual chooses a level of extraction in order to solve[71]

$$\max_{x_{it}} U^i(\pi_{it}, E_{t-1}[\overline{\pi}_{-it}], E_{t-1}[\overline{x}_{-it}]; \Theta) \tag{2}$$

where $E_{t-1}^i[\overline{\pi}_{-it}]$ denotes individual expectations about others' strategy, $\overline{\pi}_{-i} = \frac{\sum_{j \neq i} \pi_j}{n-1}$, given their information at hand (and similarly for $\overline{x}_{-it}$). Our previously discussed simpliying

[71] For simplicity, we will be assuming linear individual utility functions, which makes our formulation in terms of expected payoffs equivalent to one in terms of expected utilities. However, neutrality is an important matter measuring social preferences. The analysis becomes more complicated with other functional forms of the utility function.

assumption about beliefs reads as

$$E^i_{t-1}[\overline{\pi}_{-it}] = \overline{\pi}_{-i,t-1} \text{ and } E^i_{t-1}[\overline{x}_{-it}] = \overline{x}_{-i,t-1}.$$

i Baseline: self-regarding preferences

Individuals that exhibit self-regarding preferences care only about their own monetary cost and benefits and are usually called in the literature as free-riders, selfish or defectors. A *self-regarding* individual i has a utility function given by $U^S_i = \pi_i$. Note that the (self-regarding) best reply is the maximum extraction level $x^S_i = 8$.[72]

ii Altruistic preferences

We adapt our CPR framework to the models proposed by Levine (1998) and Casari and Plott (2003). Individuals that exhibit these preferences are those who care about others' utility - i.e. altruists in Andreoni and Miller (2002); Carpenter et al. (2009), unconditional cooperators in Fischbacher et al. (2001) or pure cooperators in Rabin (1993).

An *altruist* i has a utility given by

$$U^A_i = \pi_i + \rho_i \overline{\pi}_{-i} \tag{3}$$

The specification above is rescaled from a general regression model which would put weights on both variables.

$$U^A_i = \eta_A \pi_i + \rho \overline{\pi}_{-i} \tag{4}$$

A challenge to interpretation arises if $\eta < 0$ (and similarly in subsequent models). In the case of altruism this deserves special discussion: in the context of a social dilemma, a purely altruistic solution (i.e. to give a large weight to others' payoff) is equivalent to foregoing own payoff. If altruism is seen as a particular (extreme?) form of concern for efficiency, we argue that the sign of η is helpful in making a distinction between altruism and concern for efficiency (Charness and Rabin, 2002), the presence of a negative coefficient calling for the former label rather than the latter.

[72] By construction the Nash equilibrium of the game is the stable strategic outcome from a game between self-regarding players.

iii Reciprocity

Our reciprocators are individuals that cooperate only if others cooperate and present similar behavior to conformism (Rabin, 1993; Bowles, 2004; Levine, 1998). A *reciprocator* i has a utility given by

$$U_i^R = \pi_i + \mu(x^{*i} - \overline{x}_{-i})\overline{\pi}_{-i} \; \forall \; i \tag{5}$$

where x^{*i} is a norm based on which i rates extractions from others, deriving more utility if others' extraction is below the norm and less otherwise. A positive value of μ would indicate a desire to uphold the social norm.

Polania-Reyes (2014) estimate the structural parameters ρ and μ by means of a random coefficients model, which assumes idiosyncratic coefficients for each individual. Selfish behavior is identified as the opposite of selfless behavior as given by the value of ρ (ρ_i in her specification).

iv Fairness and inequity aversion

This model is based on Fehr and Schmidt (1999); Bolton and Ockenfels (2000). An *inequity averse* individual i has a utility given by

$$U_i^I = \pi_i + \alpha \max(\overline{\pi}_{-i} - \pi_i, 0) + \beta \max(\pi_i - \overline{\pi}_{-i}, 0) \; \forall \; i \tag{6}$$

The second term in equation 6 measures the utility loss from disadvantageous inequality, and the third term measures the loss from advantageous inequality. It is assumed that the utility gain from i's payoff is higher than her utility loss for advantageous inequality and her utility loss from disadvantageous inequality is larger than the utility loss if player i is better off than other players, $0 \leq -\beta < 1$. In addition, i is loss averse in social comparisons: i suffers more from inequality that is to his disadvantage (Loewenstein et al., 1989): $\alpha_i \geqslant \beta_i$.

Disadvantageous inequality can only be identified under interior solutions (Fehr and Schmidt, 1999; Vélez et al., 2009). Because our CPR setting yields boundary solutions for both the Nash equilibrium and social optimum, our regression specification only incorporates advantageous inequality:

$$U_i^I = \eta_I \pi_i + \beta \max(\pi_i - \overline{\pi}_{-i}, 0) \; \forall \; i \tag{7}$$

The sign on β will identify preferences for inequity, if positive, and for equity otherwise.

v Beliefs

The formulation of beliefs is as important as that of preferences. In fact one of the basic insights behind QRE is that if agents make errors, they expect others to make the same

mistakes. The formulation of beliefs raises an identification challenge. Expectations are closely linked to learning. Arifovic and Ledyard (2012) provide a model that incorporates both social preferences (altruism, selfishness and inequity aversion) and learning (through an Individual Evolutionary Model, IEM). An IEM is characterized by experimentation, replication and learning (each of these adding one free parameter to the model).

We assume agents only take into account other players' immediately preceding action. This simplification, which allows us to focus on the classification of behavioral types, is warranted by the fact that agents only learn previous round average extraction. A more detailed model of belief formation such as Arifovic and Ledyard (2012) might add precision to the model, in return for more free parameters to be estimated, but it wouldn't help the identification procedure itself because of its reliance on symmetric gognitive profiles across players.

vi Summary: a mixture model without type identification

We suppose the population comprises 4 homogeneous (unobservable) types. On each round $t \in \{1, \ldots, T\}$, individual i makes her extraction decision x_{it} in order to maximize their utility, given the other 4 player's previous behavior in the group, $\overline{x}_{-it-1}$. We then define the structure of the error term as we introduce errors in decisions for each type and use a random utility specification in this choice environment. The expected utility takes the linear form for an individual type q, being self-regarding, inequity averse, reciprocator or altruist, $q \in \{S, I, R, A\}$. At time t, agent i chooses an action $j \in \{1, \ldots, J\}$ to derive utility

$$\tilde{U}^q(x_{ijt}; \theta_q, \overline{x}_{-it-1}) = U^q(x_{ijt}; \theta_q, \overline{x}_{-it-1}) + \varepsilon^q_{ijt} \quad \forall\, j \in \{1, \ldots, J\} \tag{8}$$

The choice probability, conditional on type q, is then determined by the logit function

$$\tilde{f}_q(x_{ijt}; \theta_q, \lambda_q, \overline{x}_{-it-1}) = \frac{\exp[\lambda_q U^q(x_{ijt}; \theta_q, \overline{x}_{-it-1})]}{\sum_{m=1}^{J} \exp(\lambda_q U^q(x_{imt}; \theta_q, \overline{x}_{-it-1}))} \tag{9}$$

This logit function is reminiscent of the QRE specification of section iii. As we argued back then, we will drop λ_q, $q \in \{S, I, R, A\}$ from the problem assuming a constant parameter applies throughout.

The individual contribution to the total likelihood function is the sum of the component densities $f_q(x_i; \theta_q, \overline{x}_{-i})$ weighted by the probabilities p_q that individual i belongs to type q such that $q \in Q = \{S, I, R, A\}$:

$$f(x_i; \Theta) = \sum_{q \in Q} p_q \prod_{t=1}^{T} \prod_{j=1}^{J} \left(f_q(x_i; \theta_q, \overline{x}_{-i})\right)^{d_{ijt}} \tag{10}$$

where d_{ijt} is a dummy for whether action j was indeed chosen at time t. This leads to the likelihood function

$$\ln L(\Psi; x) = \sum_{i=1}^{N} \ln f(x_i; \Psi) = \sum_{i=1}^{N} \ln \sum_{q \in Q} p_q f_q(x_i; \theta_q, \overline{x}_{-i}) \tag{11}$$

Assuming $U^q(x_{ijt}; \theta_q, \overline{x}_{-it-1}) = U(x_{ijt}; \theta_q, \overline{x}_{-it-1})$ where $\theta_q = \theta \sim F(.)$ allows us to estimate $\mathbf{p} = \{p_S, p_I, p_A\}$, $\Theta = \{\theta_q\} = \{\rho, \beta, \mu\}$ by direct maximization of

$$\ln L(\Psi; x) = \sum_{i=1}^{N} \ln f(x_i; \Psi) = \sum_{i=1}^{N} \ln \sum_{q \in Q} p_q \int_{-\infty}^{\infty} (f(x_i; \theta_q, \overline{x}_{-i}))\, \mathrm{d}F(\theta) \tag{12}$$

Among the structural preference models that take into account agent heterogeneity, this continuous mixture model is the most commonly used ((Cappelen et al., 2007, 2010, 2011, 2013), (Cappelen et al., 2013), all of which assume a lognormal distribution for the parameters). In addition to the need for a predefined functional form for the continuous mixture, the finite mixture model does not allow the estimation of separate parameters for the different preference functions, i.e. $U^q(x_{ijt}; \theta_q, \overline{x}_{-it-1}) \neq U'^q(x_{ijt}; \theta_q, \overline{x}_{-it-1})$. Because this is precisely what we intend to do, we refine the formulation of p_q following a latent class model.

4 Type identification using a latent class model

In order to identify individual types, we use a latent class estimated using the Expectation Maximization (EM) algorithm (Dempster et al., 1977; Train, 2008). More specifically we follow an implementation of (Train, 2008) by Pacifico (2012)[73] using the specification in section vi.[74]

The simultaneous estimation of types and parameters relies on an iteration of two steps: one where likelihood conditional on types is maximized (the M-step) and one where idiosyncratic type distribution is updated.

i The E-step

During the E-step, we take the conditional expectation of the complete-data log likelihood, $\ln L^c(\Psi)$ given the observed extraction profiles x, using the current fit for Ψ. Let $\Psi^{(0)}$ be the value specified initially for Ψ. Then on the first iteration of the EM algorithm, the E-step requires the computation of the conditional expectation of $\ln L^c(\Psi)$ given x, using $\Psi^{(0)}$ for Ψ:

$$G(\Psi, \Psi^{(0)}) = \mathbb{E}_{\Psi^{(0)}}[\ln L^c(\Psi) | X = x] \tag{13}$$

[73] We use the Stata module developed by Pacifico (2012) called lclogit

[74] The model specification is time-invariant, which implies that $\upsilon_{qt} = \upsilon_q$. Kasahara and Shimotsu (2009) study type identification in finite mixture models with panel data.

On the *(k + 1)*th iteration the E-step requires the calculation of $G(\Psi, \Psi^{(k)})$ where $\Psi^{(k)}$ is the value of Ψ after the *k*th EM iteration. Since $\ln L^c(\Psi)$ is linear in the unobservable υ_{iq}, it requires that $\mathbb{E}_{\Psi^{(k)}}(V_{iq}|X = x) = \tau_{iq}^{(k+1)}(x; \Psi^{(k)})$ [75], where V_{iq} is the random variable corresponding to υ_{iq} and[76]

$$\tau_{iq}^{(k+1)}(x; \Psi^{(k)}) = \frac{p_q^{(k)} f_q(x_i; \theta_q^{(k)}, \overline{x}_{-i})}{\sum_{q \in Q} p_q^{(k)} f_q(x_i; \theta_q^{(k)}, \overline{x}_{-i})} \tag{14}$$

are the *a posteriori* probabilities that the ith member of the sample with observed value x_i belongs to the *q*th component of the mixture, computed according to Bayes law given the actual fit to the data, $\Psi^{(k)}$. Then

$$G(\Psi, \Psi^{(k)}) = \sum_{i=1}^{N} \sum_{q \in Q} \tau_{iq}^{(k+1)}(x_i; \Psi^{(k)}, \overline{x}_{-i})[\ln p_q^{(k)} + \ln f_q(x_i; \theta_q^{(k)}, \overline{x}_{-i})] \tag{15}$$

ii The M-step

The M-step on the *(k + 1)*th iteration, the complete-data log likelihood function 15 is maximized with respect to $\Psi^{(k)}$ to provide the updated estimate $\Psi^{(k+1)}$.[77]

As the E-step involves replacing each υ_{iq} with its current expectation $\tau_{iq}^{(k+1)}(x; \Psi^{(k)})$ in the complete-data log likelihood, the updated estimate of p_q is giving by replacing each υ_{iq} in (23):

$$\widehat{p_q}^{(k+1)} = \sum_{i=1}^{N} \frac{\tau_{iq}^{(k+1)}(x_i; \Psi^{(k)}, \overline{x}_{-i})}{N} \tag{16}$$

Dempster et al. (1977) show that the sequence of likelihood values $\{L(\Psi^{(k+1)})\}$ is bounded and non-decreasing from one iteration to the next, so the EM algorithm converges monotonically to its maximum. The E- and M-steps are thus alternated repeatedly until the difference $L(\Psi^{(k+1)}) - L(\Psi^{(k)})$ changes by a -previously fixed- arbitrarily small amount.

Note that these posterior probabilities of individual group membership are not only used in the M-step, but they also provide a tool for assigning each individual in the sample to one of the Q types. Thus, finite mixture models may serve as statistically well grounded tools for endogenous individual classification (Bruhin et al., 2010).

iii Testing for the number of types

An important aspect of the contribution by (Arifovic and Ledyard, 2012) is that reciprocity arises not as a type but as an equilibrium behavior. This raises the empirical question of

[75] $\mathbb{E}_{\Psi^{(k)}}(V_{iq}|X = x) = Pr_{\Psi^{(k)}}[V_{iq} = 1|X = x]$ is the current conditional expectation V_{iq} of given the observed data $X = x$

[76] $f(x_i; \Psi^{(k)}, \overline{x}_{-i}) = \sum_{q \in Q} p_q^{(k)} f_q(x_i; \theta_q^{(k)}, \overline{x}_{-i})$

[77] For the FMM the updated estimates $p_q^{(k+1)}$ are calculated independently of the update estimate $\boldsymbol{\xi}^{(k+1)}$ of the parameter vector containing the unknown parameters in the component densities. See (Cappelen et al., 2007, 2010, 2011, 2013,?)

whether reciprocity can be thought of as an attribute. We provide some empirical information to this question by testing for the optimal number of types using a latent class model to fit the data.

Table 1 summarizes the performance of a different number of factors for the sample of villagers along the dimensions of information (as measured by the Consistent Akaike Information Criterion and Bayesian Information Criterion) and of likelihood (as measured by the likelihood ratio).

Table 1 provides evidence that the optimal model to describe the data is either one with 4 classes, the information criteria such as the CAIC or the BIC being less prone to overparametrization than the likelihood criterion. Table 2 presents similar results for the student sample.

The picture arising from the student sample (table 2) is not exactly the same as from that of villagers, as it seems to suggest the use of a fifth class. In the absence of theoretical support for the additional class, our observation is that the results found across the two populations are in broad agreement.

The results so far support the use of four types, which according to our theoretic model are the self-regarding, altruistic, inequity averse and reciprocators.

iv Latent class model results

iv.1 Utility parameters: coefficients and labels

Table 3 provides the results for the class share determinants model estimated with the villager sample.

Inequity aversion occupies a large share within the villager sample: most villagers are affected negatively by advantageous inequality in their payoffs. Pure selfish and pure altruists make up a smaller share of the sample, very close to the random coefficients model outcome in Polania-Reyes (2014) using a (10% altruists, 7% selfish).

As discussed before, the negative sign on the weight to own payoff is at first sight unsettling, but highlights the nature of the social dilemma. In making a distinction between altruism and concern for efficiency, the presence of a negative coefficient argues for altruism in the present case.

Only a small percentage in our sample are reciprocators (to the point of Arifovic and Ledyard (2012)). Here we are far from the results in Polania-Reyes (2014) where a high incidence of reciprocating behavior is found. We note that her random coefficient model cannot accommodate inequity aversion and has a high share of unidentified types (32%). This limits the interpretability and comparability of results across the two studies. The negative sign on the concern for the norm is counterintuitive and suggests a specification issue in our function, possibly in how the social norm itself is defined.

In order to compare estimates across populations, we constrain the coefficients on the student sample so that the weight on own payoff matches the one from the villager sample.[78] The results are recorded in table 4.

Again we observe a large number of inequity averse individuals (with a similar magnitude for the utility parameter), similar to the results from the villager sample. In stark contrast, when trying to match altruistic behavior we end up with a negative coefficient. Interpreted directly this coefficient points to spiteful behavior, whereby agents are affected negatively by both their outcomes and those of others. Polania-Reyes (2014) does not provide a point of comparison on the student sample.

Our latent class estimate allows to make choice prediction. In order to understand the relative performance of each model, we document the choice prediction outcome below. So far we haven't taken advantage of the data from rounds 11 to 20. We do so now by comparing the model performance in-sample (rounds 1 to 10) and out-of-sample (rounds 11 to 20).

A naive model (e.g. our static QRE) can only attain a 1/8 choice probability, which is improved within all classes except that of reciprocators, in line with the concern expressed previously about this category. The out-of-sample performance is comparable (sometimes slightly higher) than in-sample, something we take as an important sign of internal validity. In terms of relative performance, the altruistic-spiteful category performs better than the rest, and better still than the self-regarding category. This is a surprising finding, given that the Nash strategy is expected to be more stable (hence a priori more predictable) than others.

In order to understand the type classification above, we now examine the drivers for the probability of belonging to each type.

iv.2 Class share determinants

Table 3 reports an estimation of the class share model for villagers.

Those users whose income depends 100% on the CPR are more likely altruistic or inequity averse than those whose income doesn't. The belief that the community has no need of an external authority to rule them increases the likelihood of altruistic or inequity averse classification, and decreases that of the self-regarding one. The perception that the resource will remain still greatly decreases the likelihood of being altruistic as opposed to self-regarding. Voluntary participation, instead, shows a counterintuitive role, leading to a lower probability of being inequity averse and instead a higher probability of being self-regarding.

5 Conclusion

This is a study on type classification for social preferences from a CPR game. We bring a novel method to identify types in a unique sample including villagers and students. We

[78] See alternative specifications in appendix B

examine the most popular types of social preferences in the theory literature, testing for the optimal number of types. Our structural estimation relies on four types, which the data supports. The most salient feature is the prevalence of aversion to inequity across both samples. There is evidence both of pure altruistic behavior in the villagers' sample and of spiteful behavior among students. The lack of empirical evidence for reciprocal types sheds doubt on our specification, but also gives an indirect signal that reciprocity arises not as a type but as an equilibrium behavior across types.

A key feature of heterogeneity is the role of individual background. For example, the use of CPR in real life by the participants. Figure 5.3 shows the fraction of players that extract 8 units according to their dependence to the CPR. Those users whose income depends 100% on the CPR extract significantly less whereas those users whose income depends 0% on the CPR extract significantly more. Those who in real life depend more on the common pool resource have a lower probability of being allocated to the selfish type.

Using an RCM classification, (Polania-Reyes, 2014) finds that non-monetary incentives are more effective in groups where other-regarding preferences are prevalent and only the subsidy is effective in promoting behavior among self-regarding individuals. While we leave aside the treatment of incentives, we note that types are likely to be state dependent. Our finding that in-sample and out-of-sample model outcomes are comparable provide internal validity to our findings. This is particularly important in the latent class literature where, as previously discussed, labels are commonly found to be driven by data rather than theory.

We acknowledge the importance of beliefs in the decision making progress. Facing the possibility of heterogeneous preferences as well as that of heterogeneous learning, we shut down the latter to focus on the former. We assume an overly simplistic system of beliefs, namely that agents only take into account what others did in the previous round. While an IEM type model would take into account the likely higher complexity of the thought process (at the cost of parsimony), an identification challenge remains in terms of the two types of heterogeneity (cognitive and behavioral). The development of heterogeneous QRE under cognitive hierarchies proposed by Rogers et al. (2009) might be helpful in that sense. Our conjecture is that cognition and social preferences are correlated, suggesting the importance of identifying such correlations.

Testing and identification remain a challenge, both for a model of social preferences or for a pure model of bounded rationality such as QRE (McCubbins et al., 2013). On one hand, classical competive behavior might obtain in an economy subject to social preferences (Dufwenberg et al., 2011). On the other hand, there is evidence that social preferences are subject to framing effects (Dariel, 2013; Ackermann et al., 2014) or the institutional setting (Cassar et al. (2013)).

Group composition is indeed a key feature. While we restrict ourselves to variables at the individual level, Polania-Reyes (2014) performs a regression analysis with a probit model where the probability of being type q depends on socioeconomic characteristics at the individual and village level. She finds community level drivers are important, and in particular that types are somewhat correlated. If types are robust over time (as the evidence discussed here suggests) yet at the same time context- or group-dependent, an evolutionary

approach might be fitting not only for the learning but also the behavioral aspect of choice in CPR settings as well as similar collective action problems.

Figure 5.1: Number of times individuals behaved as self-regarding

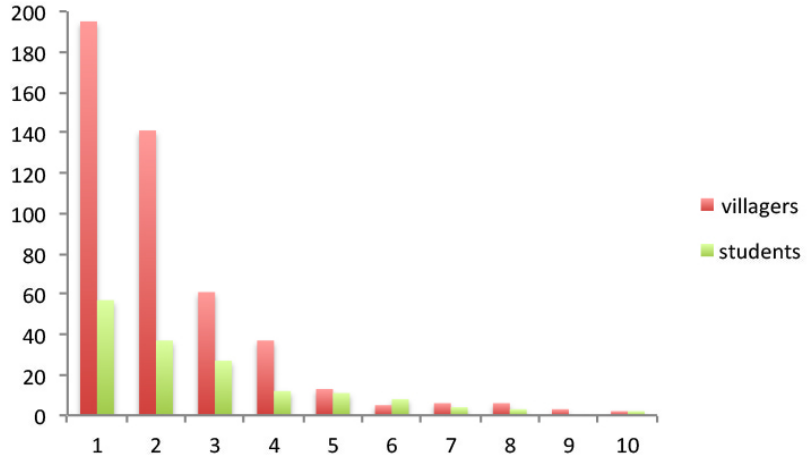


Figure 5.2: Average individual extraction over time

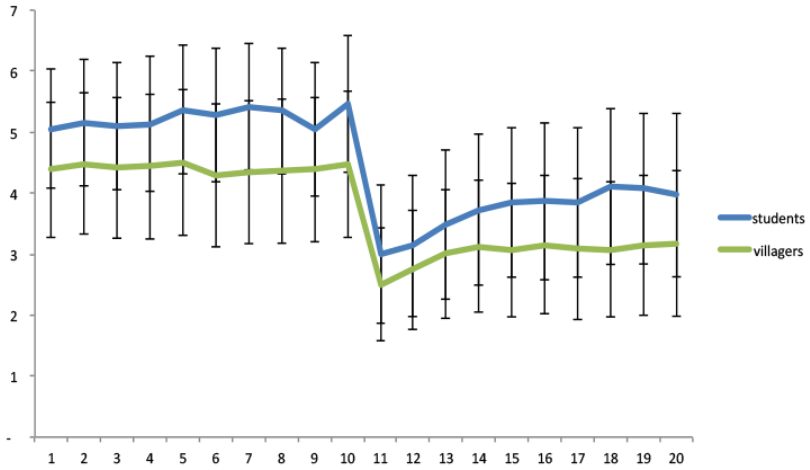


Figure 5.3: Heterogeneity of real level extraction of the CPR in the game all CPR users vs. students ($N = 1095$). The solid line shows the % time that the Self-regarding NE was chosen in the game by the Students sample. The round-dot line shows the case with individuals who use 0% of the real CPR. The square-dot line shows the average level of extraction in the game by individuals who use 50% of the real CPR. The long-dashed line the average level of extraction in the game by individuals who use 100% of the real CPR. The difference in means in the last round is significant at 10%.

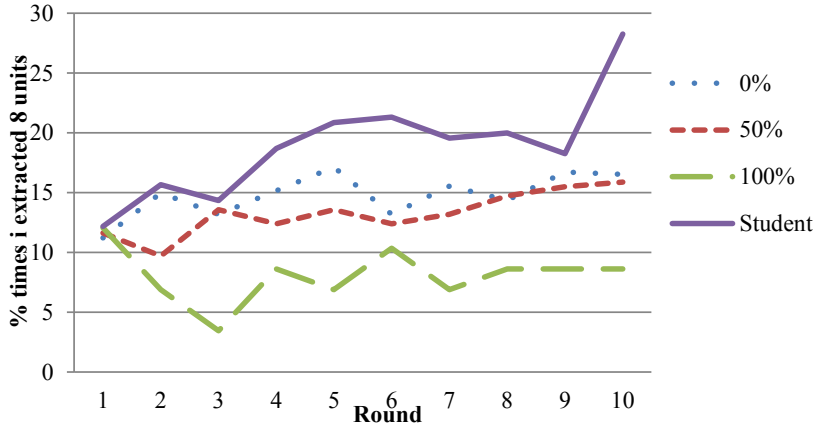


Figure 5.4: log(MSE) as a function of λ.

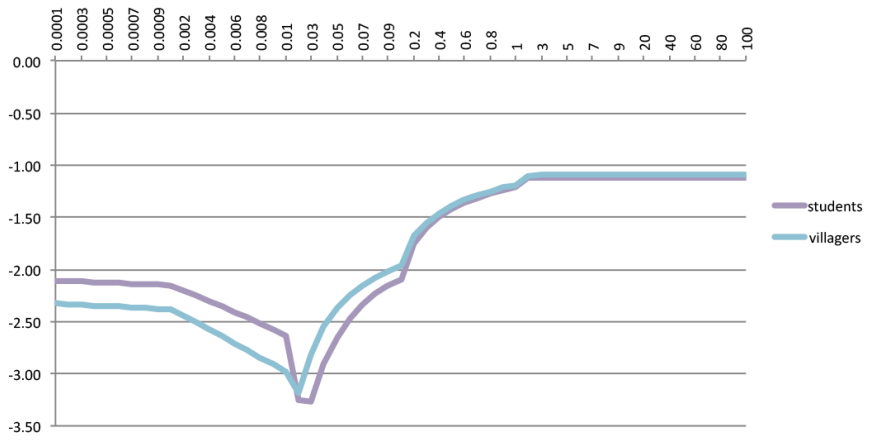
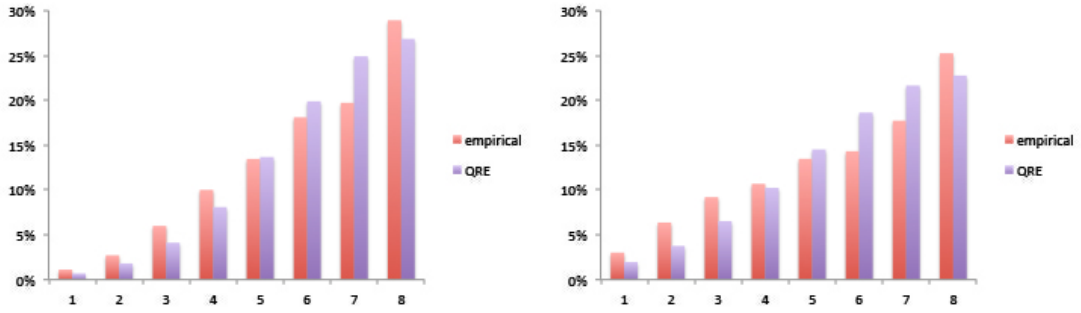


Figure 5.5: Empirical distribution of choice outcomes and QRE distribution



(a) Students: $\lambda = 0.03$ (b) Villagers: $\lambda = 0.02$

Table 1: Comparison of model performance by number of classes - villager sample

No. classes	LL	No. parameters	BIC	CAIC
2	-6992.937	21	14144.6	14123.6
3	-6887.851	38	14062.91	14024.91
4	-6821.428	55	**14058.56**	**14003.56**
5	-6783.39	72	14110.97	14038.97
6	**-6761.148**	89	14194.98	14105.98

Table 2: Comparison of model performance by number of classes - student sample

No. classes	LL	No. parameters	BIC	CAIC
2	-4045.653	9	8149.25	8140.25
3	-3999.159	14	8088.451	8074.451
4	-3981.109	19	8084.542	8065.542
5	-3956.442	24	**8067.399**	**8043.399**
6	**-3948.002**	29	8082.708	8053.708

Table 3: Class share determinants - villager sample

Variable	Self-regarding	Altruistic	Inequity averse	Reciprocator	Std. error[b]
π_i	0.019	-0.026	0.019	0.235	0.001
$\overline{\pi}_{-i}$	0[a]	0.003	0[a]	0[a]	0.000
$\max(\pi_i - \overline{\pi}_{-i}, 0)$	0[a]	0[a]	-0.023	0[a]	0.000
$\overline{\pi}_{-i}(x^{i*} - \overline{x}_{-i})$	0[a]	0[a]	0[a]	-0.015	0.000
Class Share	0.185	0.107	0.687	0.021	-

[a] Constrained to 0 in estimation

, [b] Standard errors are computed from the covariance matrix of regression coefficients over the full sample. Implied variances and covariances of choice model coefficients are averaged across individuals in the prediction sample.

Table 4: Class share determinants - student sample

Variable	Self-regarding	Spiteful	Inequity averse	Reciprocators	Std. error[d]
π_i	0.034	-0.026[b]	0.019[c]	0.045	0.002
$\overline{\pi}_{-i}$	0[a]	-0.047	0[a]	0[a]	0.001
$\max(\pi_i - \overline{\pi}_{-i}, 0)$	0[a]	0[a]	-0.025	0[a]	0.001
$\overline{\pi}_{-i}(x^{i*} - \overline{x}_{-i})$	0[a]	0[a]	0[a]	-0.038	0.000
Class Share	0.122	0.129	0.731	0.018	-

[a] Constrained to 0 in estimation

,[b] Constrained to -0.026 in estimation

,[c] Constrained to 0.019 in estimation

, [d] Standard errors are computed from the covariance matrix of regression coefficients over the full sample. Implied variances and covariances of choice model coefficients are averaged across individuals in the prediction sample.

Table 5: Class-conditional probability of choice

Class	Villagers		Students	
	In-sample	Out-of-sample	In-sample	Out-of-sample
Self-regarding	0.173	0.166	0.205	0.189
Altruistic / Spiteful	0.3136	0.437	0.336	0.341
Inequity averse	0.164	0.210	0.173	0.217
Reciprocator	0.136	0.064	-	-

Table 6: Drivers of class share - villager sample

Variable	Self-regarding	Altruistic	Inequity averse	Reciprocator
HH size	-0.479	-0.417	-0.478	0
Age	-0.021	-0.011	-0.019	0
Sex	-1.942	-1.95	-1.609	0
Education Level	-0.26	-0.05	-0.166	0
Land owner	0.869	0.814	1.134	0
Interest_com_perceived	-0.686	-0.414	-0.73	0
Belief_local_govern	1.762	1.925	1.943	0
Voluntary_part	0.22	0.104	-0.346	0
CPR_Still_perceived	-1.773	-2.485	-1.844	0
HH_Income_CPR	33.482	34.475	33.874	0

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Appendix

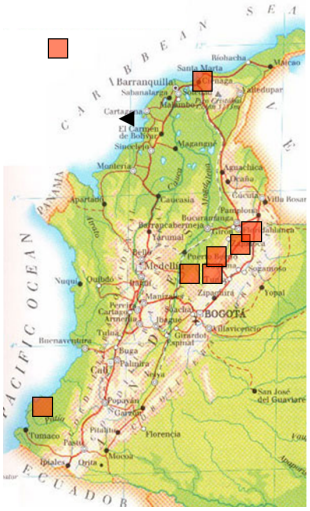
A Labs in the field Data

The experiments were conducted in 8 Colombian villages (see Figure 1) during 2001 and 2002 and a university in Bogotá. A total of 1095 participants attended the sessions, 230 undergraduate students and 865 real users of a CPR. Every village may depend on a different CPR (see Table **??**).

All data were collected using standard procedures in experimental economics in the laboratory: no deception, no field referents, fully salient choices. We collected information on individual characteristics of the villagers only.

Table 7: Labs in the field

Villages	*CPR*
Providencia	Coral reefs
	Coastal fisheries
	Crab gatherers
Gaira	Coastal fisheries
Sanquianga	Clamps
	Fisheries
	Shrimp
	Mangroves
Barichara	Andean Forests
Chaina	Firewood
Tabio	Andean Forests
	Water
La Vega	Water
Neusa	Damn reservoir
	Trout fishing



Note: the red squares are the villages.

Table 8: Table points of the CPR game.

	My Level of Extraction from the Resource								
Total Level of the extraction by others	Total Level of the extraction by others	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	Average Level of extraction by others
	4	758	790	818	840	858	870	878	880	1
	5	738	770	798	820	838	850	858	860	1
	6	718	750	778	800	818	830	838	840	2
	7	698	730	758	780	798	810	818	820	2
	8	678	710	738	760	778	790	798	800	2
	9	658	690	718	740	758	770	778	780	2
	10	638	670	698	720	738	750	758	760	3
	11	618	650	678	700	718	730	738	740	3
	12	598	630	658	680	698	710	718	720	3
	13	578	610	638	660	678	690	698	700	3
	14	558	590	618	640	658	670	678	680	4
	15	538	570	598	620	638	650	658	660	4
	16	518	550	578	600	618	630	638	640	4
	17	498	530	558	580	598	610	618	620	4
	18	478	510	538	560	578	590	598	600	5
	19	458	490	518	540	558	570	578	580	5
	20	438	470	498	520	538	550	558	560	5
	21	418	450	478	500	518	530	538	540	5
	22	398	430	458	480	498	510	518	520	6
	23	378	410	438	460	478	490	498	500	6
	24	358	390	418	440	458	470	478	480	6
	25	338	370	398	420	438	450	458	460	6
	26	318	350	378	400	418	430	438	440	7
	27	298	330	358	380	398	410	418	420	7
	28	278	310	338	360	378	390	398	400	7
	29	258	290	318	340	358	370	378	380	7
	30	238	270	298	320	338	350	358	360	8
	31	218	250	278	300	318	330	338	340	8
	32	198	230	258	280	298	310	318	320	8

Note: The Self-regarding Nash Equilibrium produces an individual payoff of 320MU whereas the social optimum leads to an individual payoff of 758 MU.

Table 9: Real Users' Socio-economic Characteristics

Variable		Mean	Median	Min.	Max	SD	%N
HH Size		5.59	5	1	51	3.1	87
Age average		34.0	32	7	85	13.9	88
Woman(==1)		46.9	0			49.9	88
Years of education (average)		6.0	5	0	18	3.7	81
Landowners %		75.0	1			43.3	87
Membership %		46.3	0			0.5	95
Meetings Attendance %		11.3	1	0	2080	89.9	77
Perception cooperation %		46.5	50	0	75	28.0	82
Perception interest in CPR	%	62.5	30			37.6	76
Community should control	%	59.7	50	-1	1	42.6	85
Fraction of players with	100%	22.0					
Extraction of the CPR as	50%	65.2					88
main economic activity	0%	12.8					

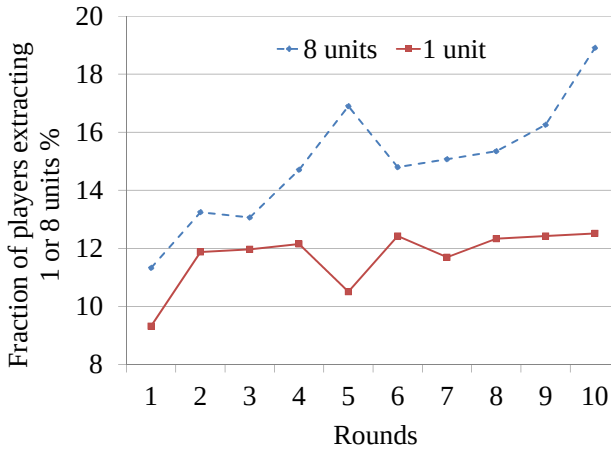


Figure A.1: Baseline: behavior over rounds for Pure Self-regarding and Pure cooperator

B Results of the latent class model under alternative specifications

Below we provide the estimates of class share for students without the restrictions coming from the villagers' model.

Now we present an alternative formulation of the model for students without any restrictions.

Now we present an alternative formulation of the model for villagers without any restrictions.

Table 10: Class share determinants (student sample)

Variable	Self-regarding	Spiteful	Inequity averse	Reciprocators	Std. error
π_i	-0.017	-0.049	0.026	0.043	0.002
$\overline{\pi}_{-i}$	0[a]	-0.064	0[a]	0[a]	0.000
$\max(\pi_i - \overline{\pi}_{-i}, 0)$	0[a]	0[a]	-0.029	0[a]	0.001
$\overline{\pi}_{-i}(x^{i*} - \overline{x}_{-i})$	0[a]	0[a]	0[a]	0.001	0.000
Class Share	0.240	0.107	0.740	0.130	-

[a] Constrained to 0 in estimation

Table 11: Class share determinants - student sample

Variable	Class 1	Class 2	Class 3	Class 4	Std. error
π_i	-0.017	0.058	-0.042	0.010	0.002
$\overline{\pi}_{-i}$	-0.039	0.014	-0.123	-0.053	0.002
$\max(\pi_i - \overline{\pi}_{-i}, 0)$	-0.044	-0.054	-0.056	-0.048	0.000
$\overline{\pi}_{-i}(x^{i*} - \overline{x}_{-i})$	-0.006	-0.018	-0.014	-0.015	0.000
Class Share	0.216	0.137	0.193	0.453	-

[a] Constrained to 0 in estimation

Table 12: Class share determinants - villager sample

Variable	Class 1	Class 2	Class 3	Class 4	Std. error
π_i	-0.005	-0.018	-0.203	0.030	0.001
$\overline{\pi}_{-i}$	-0.045	-0.028	-0.196	-0.104	0.001
$\max(\pi_i - \overline{\pi}_{-i}, 0)$	-0.037	-0.043	-0.071	-0.073	0.000
$\overline{\pi}_{-i}(x^{i*} - \overline{x}_{-i})$	-0.011	-0.012	-0.026	-0.019	0.000
Class Share	0.631	0.243	0.027	0.098	-

[a] Constrained to 0 in estimation

III. Work pays: different benefits of a workfare program in Colombia[79]

This version: **May 18, 2015**

Abstract

Workfare programs provide a low paid employment guarantee to individuals in selected public works. They are designed to self-select the poor and provide insurance against job losses by informal sector workers at the possible cost of crowding out private labor effort. We analyze the impact of a Colombian workfare program called Job in Action [*Empleo en Acción*] to shed light on the following issues: (1) whether the program crowds out labor effort by members of the household different from the participant in the particular context of a middle-income economy, (2) whether there are gains in household labor income, but also in consumption, which is important to assess the role of the program as an insurance mechanism and (3) whether there are some gains from participating in the program six months after the program has finished.
Our results show no evidence of the program crowding out private labor effort by other household members. In addition, we find that the program had large positive transfer benefits, as the program increased individual's labor income and labor supply in large as well as small municipalities. There is a positive significant impact in small municipalities on consumption which is doubled when focusing only on food consumption. Finally, we do find that the program had a significant positive effect on individuals' outcomes as well as on households' monthly labor income per capita in small rural municipalities six months after the program ended. We shed light on the potential channels explaining this novel result in the literature on public work schemes.

JEL codes: D04 (Microeconomic Policy: Formulation, Implementation, and Evaluation); H53 (Government Expenditures and Welfare Programs); I38 (Government Policy, Provision and Effects of Welfare Programs); J48 (Particular labor markets, Public Policy); J38 (Wages compensation and labor costs, Public Policy); J22 (Time Allocation and Labor Supply)

Keywords: Workfare, *Empleo en Accion*, Transfers, Stabilization, Impact on ex-participants, Colombia, antipoverty program, safety net, intra-household allocation

[79] This chapter is joint work with Arthur Alik-Lagrange (Toulouse School of Economics), Orazio Attanasio (UCL), Costas Meghir (Yale University), and Marcos Vera-Hernández (UCL). My contribution to this chapter is the literature review and the data and regression analysis to which Arthur has made subsequent improvements as coordinated by Marcos. All co-authors worked equally in the text.

1. Introduction

Workfare programs provide a low wage to individuals that work in selected public works. Such schemes are seen as a substitute to unemployment insurance. The low wage and the work requisite mean that poor individuals are more likely to participate in the program, which is desirable for the targeting of the program (Ravallion (1991), Besley and Coate (1992)). This self-selecting feature of workfare programs makes them a popular cost-effective intervention in developing countries where governments usually lack the capacity and information systems to identify poor and/or unemployed individuals, possibly because of the presence of a large informal sector (Zimmermann (2014b)).

As pointed out by Ravallion (1991), workfare programs potentially have two different benefits: a transfer benefit, and a stabilization benefit. The transfer benefit is simply measured by the net amount of resources that an individual receives from the program. The stabilization or risk reducing benefit emerges because participation in the program can contribute towards consumption smoothing when individuals get unemployed or are hit by another type of adverse shock such as adverse weather conditions or crop loss (see Zimmerman (2014a) for a model in a poor rural economy).[80]

The existing empirical literature has documented large transfer benefits in workfare programs of India and Argentina, measured by the gains in income from participating in the program (Datt and Ravallion (1994), Jalan and Ravallion (2003), Ravallion et al. (2005)). A growing empirical literature on India's massive public work scheme (MNREGA) identifies positive impacts on wages and households labor income (see e.g. Imbert and Papp (2015) or Azam (2012)).

Recent empirical studies on MNREGA, have also found positive impacts of the scheme on households' consumption in rural areas of specific states, with higher impact on food consumption (Deininger and Liu (2013) and Ravi and Engler (2015)). However, these results are for poor rural economies and it might be the case that a better access to formal insurance mechanisms in middle-income economies mitigates the positive impact identified in rural India.

[80] Unemployment insurance could provide the stabilization benefits that we refer to. However, workers of the informal sector cannot get access to unemployment insurance, partly because they do not contribute, and partly because the public sector cannot identify whether or not they are working.

The first contribution of this paper is to document both the transfer and stabilization benefits within the same workfare program, *Job in Action [Empleo en Acción] (EA)* implemented by the Colombian government between 2002 and 2004 in urban and rural municipalities.

In addition to transfer and stabilization benefits, however, workfare programs might have negative effects on efficiency. In particular, public works hiring might crowd out households private labor effort. In the absence of the program, households might offset an individual's unemployment shock by increasing the labor effort exerted by other individuals of the households.

In the context of a low-income economy, Datt and Ravallion (1994) find that for one village of the state of Maharashtra in India, a poor rural labor-surplus economy, the work of men on the farm increases when women participate in the workfare program that they analyze. This is consistent with household members taking up the activities displaced by the workfare program rather than the program crowding out labor effort, and can be related to high rates of involuntary unemployment.

The second contribution of this paper is to investigate whether *EA* crowded out labor supply of other adult household members in rural and urban areas of a middle-income economy. This aspect is crucial because crowding out may affect the extra earnings from the program and because it creates heterogeneous forgone earnings on which relies the targeting performance of public work schemes. Recent empirical studies find mixed evidence in this respect for MNREGA. Deininger and Liu (2013) and Zimmerman (2012, 2014a) do not find significant crowding out effects, while Imbert and Papp (2015)'s results suggest that India's massive workfare did crowd out private labor effort.[81] We are however not aware of any other empirical research that looks at how workfare programs affect intra-household allocation of labor in middle-income economies, such as Colombia. Crowding out effects might be different in low and in middle-income economies (Ravallion (1999)). Involuntary underemployment might be less pronounced than in rural India, leading to potentially higher crowding out effects where non-participating households members tend not to take over forgone participant's labor opportunities. Workfare may also crowd out private transfers and we test whether households stop receiving external transfers because of their participation in *EA*.

[81] Notice that the potential negative direct effect on labor force participation may results in an increase in wage rates on the casual labor market, hence in positive second order effects.

The third contribution of this paper is to test whether workfare programs, in the long run, improve the labor market opportunities of participants. According to Besley and Coate (1992) workfare programs do not only self-target the poor (i.e. the *screening argument*), but they can also lead the poor to make better *ex ante* choices increasing their future earnings abilities and lower their dependence to workfare (i.e. the *deterrent argument*). Moreover, the participation in work might prevent the depreciation of human capital or even some increases in it. Participants in a workfare program might improve their skills and expand their contacts, or change their labor search habits, which might enhance persistently their labor market opportunities and labor income, as well as other household's members ones, even after the program finishes.

However, little is known empirically about these potential lasting benefits. Ravallion, et al. (2005) considered this important issue by testing whether there are income gains for non-participants who had previously participated in *Trabajar*, a workfare program in Argentina. The authors cannot reject that there are no income gains after participation though they recognize that their test has low power because of their small sample size.[82] They also discuss the importance of the aggregate state of the labor market at end of participation date as a key factor explaining heterogeneous recovery speed from program retrenchment.

Ravallion, et al. (2005)'s results considered only urban households. Having data on rural and urban municipalities, we can check if rural participants did not benefit more in the long term. Rural households were asked to perform new tasks related to building far from their usual farming activities, which may have increased their productivity and help them switch to better paid sectors or to find a job if they were unemployed. The larger sample that we use in this paper allows to test for these long lasting effects with higher power, and to shed light on their heterogeneity with respect to participants' pre-intervention occupation and their economic environment.

The following section describes the particularities of *EA* and the data collected. In section 3 we present our identification strategy. The results are analyzed in Section 4, while Section 5 concludes.

2. The program and the data

[82] Testing for this is not the main purpose of their paper, but a requisite to interpret the income losses from leaving the project versus staying in the project as the net income gain from participation.

Starting in the mid-1990's, Colombia experienced a lost decade in terms of economic growth, as the real GDP per capita in 2004 was roughly the same as in 1995. In response to the severe recession of the late 1990s and early 2000s, the Colombian government implemented a variety of different welfare programs, including *EA*, a workfare program whose main objective was to serve as a safety net (DNP (2007)). The program consisted of subsidizing non-skilled labor of qualifying projects.[83] The nature of the projects ranged from building or repairing roads, sewage system, health infrastructure, education infrastructure, entertainment, sport or cultural venues. They must be proposed by local governments, NGOs or other community organizations which had to cover the non-labor costs of the projects.[84] The maximum duration of each project was 5 months.

Individuals eligible to participate had to be older than 18, could not be studying during the morning or afternoon, could not be currently employed on the formal job and had to belong to the first or second level of the Colombian Social Classification System (SISBEN)[85]. Eligible individuals could work part-time up to a maximum of 5 months in an EA project. On average, individuals worked only for 2.4 months in an EA project, probably because pay conditions were worse than in the market[86].

According to government statistics, 3724 projects were approved for funding, 63% of them in municipalities with less than 100,000 inhabitants. Projects were approved between the end of 2000 and March 2003, and started at different times in different municipalities. The last projects funded by the EA program finished in May 2004 (DNP (2007):12).

This paper uses a sample of 116 randomly selected projects to study the impact of *EA.* Three waves of a longitudinal household survey were collected for each project. The first wave was collected between December 2002 and December 2003. This survey was intended to be a baseline survey. The second wave of data was collected between March 2003 and January 2004, while the projects were still ongoing. The objective of this second wave is to measure the impact of the program while the participants have access to the program. The third wave was collected

[83] The program paid 2004 US$69 (COL$180,000 Colombian pesos in 2001) a month for each individual working part time (24 hours) per week.
[84] There were some exceptions for projects proposed by local governments.
[85] The Colombian Social Classification System, called SISBEN, is used as an eligibility tool for most social programs in Colombia. There are six possible categories. The first and second one correspond to the poorest in the population.
[86] Workfare programs generally pay worse than in the market to assure that individuals will take normal jobs when available. Individuals could only work part time so that they could look for normal jobs.

between June and September 2004. The projects had finished between 4 and 13 months before this third wave was collected. This third wave is the one that allows us to study the impact of the workfare program once it has finished. Something to note about the first wave of data collection (baseline) is that, for some projects, individuals were already working in the *EA* project when the data was collected, although no individuals had been paid. This was due to some projects starting earlier than originally planned with the objective of providing relief as soon as possible. We will explain below how our empirical strategy accommodates this issue.

Before a project started, individuals who were interested in participating needed to register their interest in a given project. The local authorities were asked to create two lists, one of randomized-in individuals, and another one of randomized-out. However, from conversations with program officials and field workers, we know that such randomizations did not always take place and some individuals were included in these lists through some *ad hoc* process different from a randomized mechanism. Although we have both lists of individuals, we do not know whether a given person was included in one of the two lists through a randomized mechanism or through a different process. Our identification strategy will also take this into account.

As it is to be expected, some individuals who were in the list of randomized-in, eventually decided not to participate in *EA* (i.e. non-compliance), in which case replacements were found among the list of randomized-out.[87] Our sample is a random sample drawn from the list of individuals who were in the randomized-in and randomized-out lists (so they had all expressed their willingness to work for an *EA* funded project), independently of their actual participation in the project. Finally, when we analyze individual level outcome variables, we exclude from the sample 401 individuals who were living in households who had members in both the list of randomized-in and randomized-out individuals, as one would expect strong intra-household interactions in the behavior of these individuals.[88]

3. Identification Strategy

To better explain our identification strategy, it is useful to introduce some notation. Let the variable $\boldsymbol{IP_i}$ be an indicator function that equals one if the individual $\boldsymbol{i}$ was part of

[87] For some projects there was a substantial lag between the moment when the list was drawn and the project started.

[88] We have run our entire analysis without dropping these individuals and obtained very similar effects, in magnitude and quality, which is a first sign of the absence of crowding out effects.

the randomized-in list, and 0 if he was part of the randomized-out one. Let the variable $\boldsymbol{P_i}$ be an indicator function that equals one if the individual $\boldsymbol{i}$ actually participated in the EA project, $\mathbf{0}$ otherwise. The joint distribution of $\boldsymbol{IP_i}$ and $\boldsymbol{P_i}$ is given in Table 1. The total number of people that was initially allocated to the program is 3185 (i.e. $\boldsymbol{IP_i = 1}$ in the first follow up) and the total number of people that participated in a EA project is 2753 (i.e. $\boldsymbol{P_i = 1}$ in the first follow up).

i. Identifying transfer benefits

We aim at identifying the intention-to-treat (ITT) effect, that is, the effect of being in the list of randomized-in individuals (IP=1).[89] Our identification strategy must consider the challenge that the process of allocating individuals to the randomized-in and out list was not entirely random. Tables A1 and A2 in the appendix compare the characteristics of randomized-in and out. Table A1 compares basic individual characteristics such as gender, age, education, health indicators, migrant status, training indicators and labor history. Table A2 compares household variables. The comparison confirms the reports from program officials and field workers that the allocation process was not entirely random. Though differences are generally not large, the differences are statistically significant. Hence, we cannot rule out that some unobserved characteristics might be correlated with both the outcome variables and the allocation to the randomized-in list. We will use difference-in-difference to control for the violations in the protocol of randomization. More precisely, we will estimate the following regression model:

$$\Delta y_{it} = \alpha IP_{ik} + \beta X_i + \theta_k + \varepsilon_{ikt}, \qquad E[\varepsilon_{ikt}|IP_{ik}, X_i, \theta_k] = 0 \qquad (1)$$

where $\Delta y_{it} = y_{it} - y_{i0}$ is the difference for individual *i*, registered in the list of project k, between the outcome y variable (labor income, hours worked and transfers) in period t and the reference period 0.[90] X_i is a vector of individual *i*'s time invariant household and individual characteristics at baseline including education, gender, age, socio-economic classification of the neighborhood, household's demographics and

[89] The average impact on the participants can be then easily obtained from this estimate dividing the ITT by the difference between the observed compliance rate and the share of randomized-out individuals used as replacements for non-compliers ($E[P|IP = 1] - E[P|IP = 0]$). This holds under monotonicity and independence assumption as shown e.g. in Duflo, Glennerster and Kremer (2008).

[90] The reference year will be 2001 for income and hours worked, and the baseline survey date for consumption and transfers, c.f. infra.

assets and whether the household faced some shock since 2000;[91] θ_k is a project fixed effect, and ε_{ikt} is an error term. The project fixed effect is used to reflect that the allocation to $IP = 1$ or $IP = 0$ was conditional on being registered in a list of a given project. The estimator of α, to which we will refer as DIF-in-DIF, will provide a consistent estimate of the ITT as long as there are no time varying unobserved variables that determine both the outcome variable and the allocation of individuals into $IP = 1$. Importantly we are identifying the effects by taking within project differences rather than treating all projects as homogeneous.

The existence of an Ashenfelter's pre-programme dip among applicants (Ashenfelter (1978)) is a potentially serious limitation of a difference in difference estimation as the one that we outline above. According to this, individuals that apply for a welfare program are the ones that experience a temporal decline in mean earnings in the period prior to program entry. If that is the case, the difference in difference estimator might overestimate the impact of the program (Heckman and Smith (1999)). We tackle this issue in two different ways. First, both those individuals with $IP = 1$ and $IP = 0$ are obtained from the pool of applicants, so both groups would have experienced, on average, the sae temporal dip in income when they applied for the program (except if those reallocated non-randomly are indeed those with larger dips).

This makes our approach close to Ravallion (2008). Second, as baseline measures of income and labor supply, y_{i0}, we will use retrospective measures of income and labor supply that refer to 2001[92], and which were collected in the baseline interview, rather than those relating to the first wave of data (Dec. 2002-Dec. 2003) was collected, but. Since, the application process took place in the year 2002, our measure of income and labor supply refers to a period before the potential temporal dip in income that is contemporaneous with the application decision. Beside these classical issues related to potential temporal pre-treatment dip, there are two other reasons to use 2001 measures of income and labor supply as y_{i0}. First, it allows us to ensure that y_{i0} is not affected by expectations of future participation. Second it tackles the problem that some individuals were already working in the EA project when the first wave of data was collected (Dec. 2002-Dec. 2003).

[91] If the regressions are at the household level, then we control for the same household's characteristics plus household head's education, gender, and age.

[92] We could alternatively use values reported for 2000. We have run robustness checks (not reported here) and we did not find significant discrepancies. Values for 2000 and 2001 hours worked are quite similar in mean and variance, income reported for 2000 show however higher standard deviation than 2001 values (as can be seen in Figure 1).

In the following, we refer to municipalities with more than 100,000 inhabitants in major metropolitan areas and big cities as “large” and to municipalities with less than 100,000 inhabitants outside major metropolitan areas as “small”.[93] As reported in Table 2, twice as many projects per habitants were initiated in small municipalities, relative to the large ones. In terms of expenditure, projects in small municipalities were 23% higher per project Expenses per project were 23% higher in small municipalities, with US$9 per capita versus US$4 per capita in large ones. Small municipalities are mostly rural areas, where poverty is more prevalent and inequality more pronounced. Moreover, applicants to EA differed between small and large municipalities with significantly more females and lower educated individuals in the smaller ones. Finally, applicants to *EA* in small municipalities were more likely to be farm workers and less likely to be unemployed (Table 3). As a result of these differences in both the composition of the population and in treatment intensity we have decided to present separate estimates for large and small municipalities.

The key assumption of our differences-in-differences identification strategy is that the counterfactual growth in the outcome variables for those with $IP = 1$ *within* each project, would have been the same as the growth we observe for those not allocated to a project. While this identifying assumption cannot be tested we present some corroborative evidence by use retrospective data on income and labor supply for 2001 and 2000 collected at baseline. Figure 1 illustrates the absence of such a pre-trend while Table 4 reports the results of a regression of the growth of these variables between 2001 and 2000 on the *IP* dummy variable, whose coefficient is small and insignificant, corroborating our assumption of no differential trends. Table 4 shows that the difference in growth in labor income and hours worked between those with $IP = 1$ and those with $IP = 0$ are small and not statistically different from zero. This goes in favor of our identification assumption and we will thus mostly focus on DIF-in-DIF regressions models.

Figure 1 provides a graphical representation of these results. Over the period 2000, 2001 and baseline date, we observe that monthly labor income and weekly hours worked tend to be indeed systematically lower for randomized out individuals, suggesting that the allocation process favored individuals more in need for the program (though 95% confidence intervals do overlap). However, we observe a clear parallel trend over this pre-program period for both outcome variables, which comforts further our common trend assumption. Notice also that no pre-program dip can be observed

[93] This corresponds to the administrative categories of “urgent” (large) and “not-urgent” (small) municipalities defined for the implementation of *EA*.

on these figures. We will also attempt to identify the ITT effects at the household level. In doing so we will define several household outcome variables, like total weekly hours worked per capita and total monthly labor income per capita.

ii. Identifying stabilization benefits

We estimate ITT effects on log consumption per capita, but the estimation of the effect of the program on household consumption is more complicated because retrospective information for 2000 and 2001 could not be collected. Moreover, we cannot rule out anticipation effects because some projects had already started to operate (and individuals to work on them) when the first data collection took place.

An alternative way of estimating the ITT effect on consumption level is however to estimate a Diff-in-Diff regression model where the dependent variable is log household consumption in the second wave (1st follow up) minus household consumption in the third wave (2nd follow up)[94,95]:

$$\log(C_{i1\text{st f.u.}}) - \log(C_{i2\text{nd f.u.}}) = \alpha IP_{ik} + \beta X_i + \theta_k + \varepsilon_{ik}, \qquad E[\varepsilon_{ikt}|IP_{ik}, X_i, \theta_k] = 0 \qquad (2)$$

We will report both the result from the previous identification strategy and estimates from this last model. When commenting on these last estimates, we will put them in perspective with long-lasting impact of the program on households' labor income. If there is a lasting positive impact of EA on income, then the consumption level after program retrenchment may be higher than before its implementation and the second approach may hence provide a lower bound of the ITT effect on household consumption while the projects were still going on. Finally, we also proceed to a robustness check in estimating (1) and (2) on the sub-sample of projects that had not started at the baseline survey.

4. Results

Results are reported according to four sets of hypothesis that our experimental data allows to test. The two first sets hypothesis are related to transfer and stabilization benefits. We then explore the potential crowding out effects of *EA* hiring. Finally we

[94] Our measure of consumption does not include rent because it was not asked in the second wave.
[95] We do not need to do this for labor supply or income because our measure of pre-program labor supply and income refer to 2001, when the projects had not started.

describe the ITT effect of *EA* after six months and shed light on potential channels explaining these long lasting impacts.

We assess whether *EA* provided extra income and hours of work to participants while the projects were on-going. In doing this, we do not take into account participation costs of the individual or any other benefits of EA, such as increases in productivity due to public works output[96].

The upper part of Tables 5 refers to the ITT effect of the program at individual level while the projects were still on-going (1st follow up). Means of outcome variables in the randomized-out sample are included for comparison purpose. We provide estimates with and without extra controls. The results show that the program had large positive *transfer* benefits, as the program increased individuals' income and labor supply while the program was on going. The increase in hours work and labor income is significantly positive and very similar in small and large towns: around 10 more hours per week for randomized in individuals (compared to 27.5 and 22 weekly hours worked on average in the control group for small and large municipalities respectively) and around 19 more US$ (compared to US$53 and US$47 per month in the control group for resp. small and large).

Dividing the ITT (19.1 US$ per month) by 73.5% the sample mean estimate of $E[P_i|IP_i = 1] - E[P_i|IP_i = 0]$[97] gives 26US$ per month, which is the LATE estimate for the program.

Our estimates are lower than what Jalan and Ravallion (2003) and Ravallion et al. (2005) identified for *Trabajar* (resp. US$ 157and US$140). In term of share of the monthly wage rate on EA (69 US$) we get 36%, which is also lower than the shares found in these two studies (about half of the program wage) and also lower than Galasso and Ravallion (2004) results on *Jefes* (about two third of the program wage for their preferred specification). These differences might be partly explained by the fact that 25% of participants were already off the program during the first follow up, which may lead to lower impact if some become unemployed after the program ended. A similar exercise for the impact on hours worked per week gives an estimated LATE of 13.5 hours per week, which is higher than the preferred estimate OF Galasso and

[96] In the case of MNREGA, Imbert and Papp (2015) and Azam (2012) do find such second orders positive impacts of the program, in particular on private labor market wage rates.
[97] Monotonicity holds in the sense that $E[P_i|IP_i = 0] \leq E[P_i|IP_i = 1]\ \forall i$, and independence is verified if $(\Delta Y_i^{P=0}, \Delta Y_i^{P=1}, P_i|IP_i = 0, P_i|IP_i = 1)$ is independent of IP_i. On the later identification assumption, one may argue that the program may lower the competition among involuntary unemployed casual workers, hence positively impact non-treated individuals, which would lead to an upward biased estimate of the LATE. This is however probably not the case since *EA* was framed in a way that participants could still look for a job while participating, hence keep competing with non-participants.

Ravallion (2004) found for *Jefes*, (9 hours for a work requirement of 20 hours for *Jefes* compared to 17 hours for a work requirement of 24 hours for *EA*).

We do not observe major changes in ITT effects when adding individuals and households controls to the regression model, which is consistent with our results on the common-trend assumption and provides more credibility to our results.

The workfare program increased income substantially and seems to have had some longer run effects: the workfare program may provide a channel for obtaining longer term employment, either because of the work experience gained or even because of contacts made during the program, since many of the companies undertaking the public works may offer further employment opportunities. In this case we will observe increases in consumption as a response to what may be perceived as a longer-term increase in income. Indeed, in Table 6 we present evidence that income and hours worked remained higher in the second follow up, when the public works programs had already finished: in small towns hours at the individual level have increased by 3.9 hours and income by US$12.[98] No effect is observed in large towns. In that case there were already ample work opportunities and the connections obtained through EA may not have been as important.

These increases in income may be reflected in increases in consumption for two main reasons. First, if households have had a negative shock and they do not have own assets or other mechanisms of insurance or consumption smoothing at their disposal, they will spend the EA income. This is the insurance or stabilization property of workfare. Second, to the extent that workfare leads to further permanent labor market opportunities (say because of newly acquired networks) the increase in income may represent a permanent change, which we know will increase consumption. Indeed the evidence is that in small towns, the increases in income were sustained beyond the program. On the other hand if workfare provides an easy earnings opportunity for otherwise inactive members of the household, it will act as a transitory increase in income and increase assets, rather than consumption. Looking at consumption can get at these issues.

We measure consumption at three points: baseline, first follow up and second follow up. However at baseline some of the projects had already started and indeed they were preannounced, which implies that consumption could have already increased. We thus measure the incremental effect of the program having started

[98] A first obvious reason explaining why small municipalities would show a significant long-lasting impact would be that in these municipalities EA participation happened systematically more recently than in large ones. We show in Table A4 that this was not the case and that small municipalities' participants actually stopped participating earlier in the past.

everywhere in follow up one. We also measure the extent to which consumption is sustained in follow up 2

In the upper part of Table 8 we report the impact of the program on log total consumption and food consumption between the first-follow up and the baseline survey. We find an increase of 5% in small municipalities on log consumption; the effect on food consumption is (+11%) implying (contrary to Engel's "law") a higher sensitivity of food in such poor communities. These positive impacts are in the range of those found for the impact of MNREGA on rural households' consumption. For the state of Andhra Pradesh, Deininger and Liu (2013) find an increase in consumption of 7%, going up to 13% and 11% when focusing on protein and energy intakes. Following a similar identification strategy, Ravi and Engler (2015) find a similar pattern (+9.6% on food expenditure, but no significant impact on total consumption). However, when we turn to the longer term effect, when the program has ended we find no change in consumption relative to baseline. If the baseline consumption level was not significantly increased relative to before the program was announced, the implication is that the program financed increased consumption only during its operation, with no longer term impacts. This is despite the fact we see some increases in income and hours of work in the second follow up. This means either that baseline consumption already reflected some increase or indeed that households view the program effects as transitory and save the extra income.

Indeed, when comparing these impacts on consumption with those identified on income, they are significantly smaller. One may thus wonder what participating households actually did with the share of extra income that is not used to smooth or increase consumption. In the second follow-up survey ex-participants were asked on how they used the extra income earned on EA used (Table 9): 85% of the ex-participants interviewed used EA income to buy food, clothes, and other consumption goods or invest it in education. Interestingly 44% of ex-participants report to have used EA income to repay debt. This is consistent with theoretical findings of Chau and Basu (2003) who describe the potential positive impact of public work program on debt-bondage in poor rural economies and is of course consistent with the idea that transitory income is saved rather than (fully) consumed. Of course someof it is consumed, reflecting the heterogeneous circumstances of the households.

i. Did EA hiring crowd out households' private labor effort and transfers received by participating households?

One issue with workfare programs is that they may crowd out other work effort, possibly because these jobs may have been designed "too generously". This is an

important question both in understanding the overall effects of the program and in designing better its targeting. Indeed Imbert and Papp (2015) do find MRNEGA public work hiring crowding out private work, in contrast to the results by Deininger and Liu (2013) and Zimmerman (2012). To address this issue we compare the effects of the program at the individual participant levl and at the household level: If individual level income and work effort outcomes are larger than the household level ones, then we cannot reject the absence of crowding out evidence[99].

The lower part of Tables 5 refers to the ITT effect of the program at household level. The effect of *EA* on household labor hours and earnings is never smaller than at the individual level. There is thus no evidence of *EA* hiring crowding out private labor effort by other household members.[100] The effect seems even larger, which suggests that non-participating household members do profit from the participation, possibly in the form of job opportunities found by the participant while she is on the program (extended networks, job offers related to the project, etc.). These results are consistent with self-reported crowding-out effects: in the second follow-up ex-participants where asked if some household members reduced their labor effort to which 99.5% of them answered negatively.

The program may also crowd out private transfers. Households may stop receiving external transfers because of the extra-income earned on *EA*. As shown in Table 7 we find no significant impact, neither in the first, or in the second follow-up survey.

ii. Investigating the lasting impact of EA

To our knowledge the strongly significant lasting impact in small municipalities is in the empirical literature on public work schemes the first evidence showing significant persistent effects on ex-participants' income and hours worked. It is thus important to understand what may explain this long lasting impact.

As mention in Ravallion et al. (2005) the state of local labor markets can be a catalyst of the impact of workfare on ex-participants (the authors find smaller losses from retrenchment in the provinces with lowest unemployment rates). We do not have access to unemployment rates by municipalities, but in our sample unemployment is more pronounced in large municipalities (Table 3 and Table A7). It may be the case that ex-participants in small municipalities evolved in a less tight labor market where

[99] Crowding out is here understood in its broad definition as a situation where the total household's labor supply ouside EA is lowered by the participation on EA.

[100] The average number of participants in households with at least one participant is 1.05.

their acquisition of new skills and contacts can help reallocating their labor supply toward better paid sectors or/and with higher demand for workers.

Six months after participation, ex-participants were asked to self-assess the impact of *EA* (Table A6). Male ex-participants were more prone to think that EA has eased their job search than female ones and the former found a job in a shorter period of time. This suggests that the positive significant long lasting ITT observed in small town may be driven by male participants. Rerunning our ITT estimation by gender in small municipalities, we do not find however striking sign of heterogeneity (Table A5), but estimates for female appear to be less precise, probably due to shrinking sample size.

Comparing further small and large municipalities in Table A6, we find that small municipalities' participants explain more often the ease by objective skills enhancement, like learning a new job or gaining work experience. This is consistent with a high share of the labor force in small municipalities hired on farming work before *EA*, while the work offered on the projects was mostly related to building activities. In large municipalities, ex-participants mention more subjective reasons, like getting in contact with someone helping them to find a job or gaining in self-confidence.[101]

Our data allows us to compare the labor force occupations transitions between pre and post *EA* labor force occupations, conditional on pre *EA* occupations for randomized-in and -out in small and large municipalities. As can be seen in Tables A8 and A9, it appears first that the share of previously working falling into unemployment tends to be higher in large municipalities.

In order to compare further the transition differentials between randomized in and out, we reported the sample mean estimator of $P[O_t|O_{t-1}, IP = 1]$- $P[O_t|O_{t-1}, IP = 0]$ where O_t is the labor occupation in second follow-up and O_{t-1} the one three months before the baseline survey. We observe a lower probability to stay out of the labor force for ex-participants (-4%pt and -5%pt for resp. small and large municipalities). But several differences emerge between small and large municipalities. In small municipalities randomized-in individuals have a higher probability to switch from farming to building and community work activities[102] (+7%pt), from unemployment /out

[101] We observe similar shares of participants reporting that it has been easier to find a job thanks to *EA*, which contrasts with reported objective success on the labor market. This over-optimistic view on the state of the labor market for ex-participants has been documented in the case of MNREGA in Dutta et al. (2013).

[102] The positive impact on the transition from farming to building is also observed in large municipalities, but farming workers represent a smaller share of the labor force in large ones (Table 3).

of labor force to building and farming (+10%pt and +3%pt). They also have a lower probability to stay in farming (-13%pt) and to stay unemployed (-22%pt).

As previously mentioned, our survey is characterized by a mass point in self-employment and unemployed/out of labor force. All other categories represent a rather small share of the population and it is thus difficult to test the significance of these results. However, the overall picture emerging from these descriptive statistics is the one of small rural areas contrasting with large urban ones. In the former, ex-participants may have learned new skills and get new contacts on EA projects, which helped them switch from badly to better paid activities and from unemployment to building and farming jobs. In the latter, participants' previous occupations were probably more similar to those demanded on EA projects, implying lower human capital accumulation, which could be anyway hardly materialize in extra-income because of tighter labor market constraints.

5. Conclusions

Workfare programs provide a low paid employment guarantee to individuals in selected public works. They are designed to self-select the poor and provide insurance against job losses by informal sector workers at the possible cost of crowding out private labor effort. We analyze the impact of a Colombian workfare program called Job in Action [Empleo en Acción] to shed light on the following issues.

First, we test whether the program crowds out labor effort by members of the household different from the participant in the particular context of a middle-income economy. Our results show no evidence of EA hiring crowding out private labor effort by other household members. In addition, we find no evidence of crowding out both monetary and in-kind transfers to the beneficiary household by the program.

Second, we test whether there are gains in household labor income, but also in consumption, which is important to assess the role of the program as an insurance mechanism. We find that the program had large positive *transfer* benefits, as the program increased individual's labor income and labor supply (i.e. hours of work) while the program was on going in large as well as small towns. Finally, we find that EA may have provided stabilization benefits in small municipalities with a positive significant impact in small municipalities on log consumption which is doubled when focusing only on food consumption, which is consistent with previous studies for rural India (Deininger and Liu (2013) and Ravi and Engler (2015)).

Third, we test whether there are some gains from participating in the program six months after the program has finished. We do find that EA had a significant positive effect on individuals' labor income and labor supply as well as on households' monthly labor income per capita in small municipalities. We provide descriptive statistics on labor occupation transitions pre and post intervention. For EA workers in small rural municipalities we exhibit evidence of sectors switch from farming to building and community activities, consistent with new skills accumulation, as well as higher probability to escape unemployment and to return to the labor force. These results support the idea that public work schemes may change participants' human capital accumulation or participants' labor market conditions when the work offered is far from their previous labor occupation, which can favor their future labor income after the program ended. This is to our knowledge a new results in the empirical literature on workfare program.

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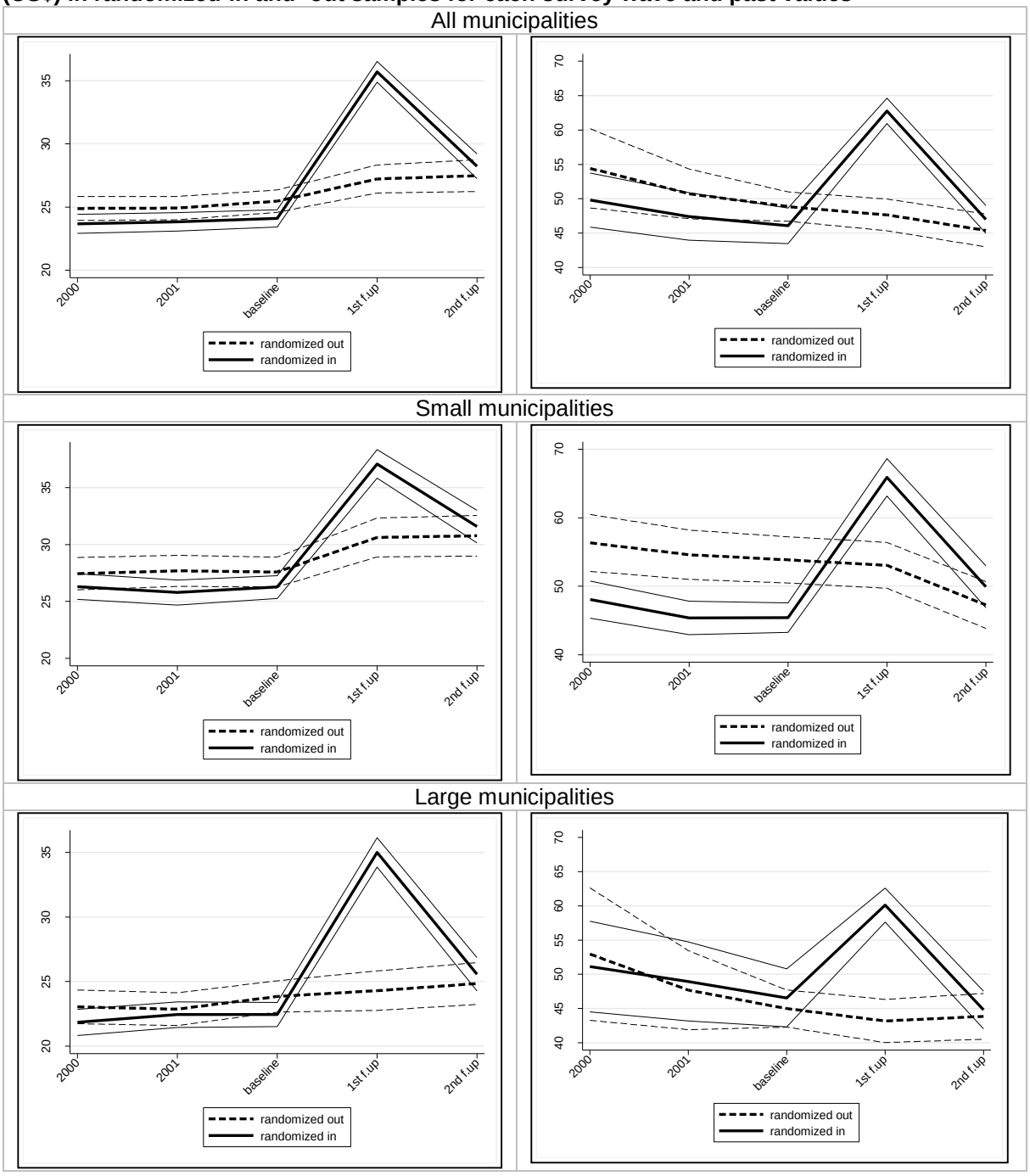
7. Tables and figures

Figure 1. Mean individual weekly hours per week and individual monthly labor income (US$) in randomized-in and -out samples for each survey wave and past values



Note: Thin lines are 95% C.I. bounds.

Table 1. Joint distribution of participants (P) and randomized in (IP).

FIRST FOLLOW UP

When *P*=currently participating in EA during the first follow-up

First follow up	$IP = 1$	$IP = 0$
$P = 1$	1944 (64%)	120 (5%)
$P = 0$	1115 (36%)	2064 (95%)

When *P*= participated or currently participating in EA

First follow up	$IP = 1$	$IP = 0$
$P = 1$	2591 (81%)	162 (8%)
$P = 0$	594 (19%)	1902 (92%)

SECOND FOLLOW UP

When *P*=participated or currently participating in EA

Second follow up	$IP = 1$	$IP = 0$
$P = 1$	2441 (86%)	293 (15%)
$P = 0$	405 (14%)	1610 (85%)

Table 2. Descriptive statistics on large and small municipalities.

	Large municipalities			***Small municipalities***			***Whole sample***		
	Mean	Med.	S.d.	Mean	Med.	S.d.	Mean	Med.	S.d.
Population in 2004 (1000)	628	262	1499	33	20	35	249	43	934
Number of projects	35	23	46	7	5	6	17	7	31
Number projects for 100,000 habitants	16	7	22	34	16	38	28	14	34
Expenses by project (2004 US$)	19334	19415	6559	23813	24676	6981	22191	22403	7113
Expenses by habitant (2004 US$)	4	1	6	9	4	11	7	3	10
Gini index (2005)	38	41	13	44	44	8	42	44	10
Poverty rate (2005)	11	9	10	52	52	22	37	36	27
Rural index (2004)	38	35	17	67	68	15	57	60	21
Applicants characteristics									
	Mean	Sd	N	Mean	Sd	N	Diff	Ttest, P(Ho:Diff=0)	
Age	35.4	12.84	3239	35.12	12.42	2532	0.28	0.405	
Female	0.45	0.5	3239	0.26	0.44	2532	0.19	0.000	
Edu1: Level of Education 1	0.09	0.28	3239	0.13	0.33	2530	-0.04	0.000	
Edu2: Level of Education 2	0.26	0.44	3239	0.31	0.46	2530	-0.05	0.000	
Edu3: Level of Education 3	0.24	0.42	3239	0.2	0.4	2530	0.03	0.002	
Edu4: Level of Education 4	0.27	0.44	3239	0.2	0.4	2530	0.07	0.000	
Edu5: Level of Education 5	0.13	0.34	3239	0.14	0.35	2532	-0.01	0.161	
Edu6: Level of Education 6	0.01	0.12	3239	0.02	0.12	2532	0.00	0.620	

Note: Gini index, rural index (rural population/population) and poverty rate (poverty head count index based on Multidimensional Poverty Index) are from the Municipal Panel Data CEDE (an initiative of the Center of Economic Development Studies (CEDE for its acronomy in Spanish) website.

Table 3. Descriptive statistics on labor force occupation in large and small municipalities 3 months before baseline.

Whole population	***N***	***share (%)***	***cumul (%)***	***Large municipalities***	***N***	***share (%)***	***cumul (%)***	***Small municipalities***	***N***	***share (%)***	***cumul (%)***
Out of labor force	1599	37.34	37.34	*Out of labor force*	976	40.1	40.1	*Indpt self-imp*	694	37.55	37.55
Indpt self-imp	1255	29.31	66.65	*Indpt self-imp*	561	23.05	63.15	*Out of labor force*	623	33.71	71.27
Unemployed	608	14.2	80.85	*Unemployed*	455	18.69	81.84	*Unemployed*	153	8.28	79.55
Building	152	3.55	84.4	*Community*	102	4.19	86.03	*Farming*	99	5.36	84.9
Community	137	3.2	87.6	*Building*	97	3.99	90.02	*Building*	55	2.98	87.88
Farming	127	2.97	90.57	*Domestic*	83	3.41	93.43	*Commerce*	41	2.22	90.1
Domestic	113	2.64	93.2	*Commerce*	49	2.01	95.44	*Business owner*	37	2	92.1
Commerce	90	2.1	95.31	*Manufacture*	29	1.19	96.63	*Community*	35	1.89	93.99
Business owner	49	1.14	96.45	*Farming*	28	1.15	97.78	*Domestic*	30	1.62	95.62
Manufacture	46	1.07	97.52	*Help for free*	14	0.58	98.36	*Help for free*	30	1.62	97.24
Help for free	44	1.03	98.55	*Business owner*	12	0.49	98.85	*Manufacture*	17	0.92	98.16
Transport Communication	21	0.49	99.04	*Transport Communication*	10	0.41	99.26	*Public work*	14	0.76	98.92
Public work	18	0.42	99.46	*Electr., Gaz, Water*	8	0.33	99.59	*Transport Communication*	11	0.6	99.51
Electr., Gaz, Water	9	0.21	99.67	*Public work*	4	0.16	99.75	*Not specific*	4	0.22	99.73
Teaching	7	0.16	99.84	*Teaching*	4	0.16	99.92	*Teaching*	3	0.16	99.89
Not specific	6	0.14	99.98	*Not specific*	2	0.08	100	*Mines*	1	0.05	99.95
Mines	1	0.02	100					*Electr., Gaz, Water*	1	0.05	100

Note: Recall during the second follow-up on the main occupation three months before baseline

Table 4. Common trend assumption. Coefficient of *IP* on a regression of differences between 2001 and 2000.

Dependent variable		Without additional controls			With additional controls		
		All	Small towns	Large Towns	All	Small towns	Large Towns
Weekly hours worked	Coeff.	-0.398	-0.765	-0.0856	-0.300	-0.791	0.206
	s.e	[0.480]	[0.653]	[0.693]	[0.498]	[0.672]	[0.735]
	N	5615	2453	3162	5439	2397	3042
Monthly labor income (US$)	Coeff.	-0.0340	-0.787	0.600	0.601	-0.773	2.229
	s.e	[3.275]	[1.898]	[5.826]	[3.179]	[1.808]	[5.903]
	N	5586	2428	3158	5409	2371	3038

Note: Control variables are education, gender, age, socio-economic classification of the neighborhood, households' characteristics (demographics, assets and facilities). Robust standard errors in brackets.

Table 5. Diff-in-Diff estimates of the ITT effect on individuals and households outcomes in the first follow up survey.

Dependent variable		Without additional controls			With additional controls		
		All	Small towns	Large Towns	All	Small towns	Large Towns
Individuals' outcomes							
Weekly hours worked		9.89***	9.55***	10.20***	9.68***	9.69***	9.39***
		(0.92)	(1.30)	(1.30)	(0.93)	(1.31)	(1.37)
	N	*4918*	*2238*	*2680*			
	Mean (IP=0)	*24.68*	*27.55*	22.33			
Monthly labour income (US$)		19.10***	19.21***	19.00***	19.47***	19.35***	19.71***
		(2.37)	(2.73)	(3.76)	(2.53)	(2.71)	(4.23)
	N	*4865*	*2216*	*2649*			
	Mean (IP=0)	*49.68*	*52.98*	*46.99*			
Households' outcomes							
Weekly hours worked		13.94***	12.85***	14.75***	14.05***	13.96***	13.79***
		(1.90)	(2.78)	(2.60)	(1.93)	(2.85)	(2.66)
	N	*3574*	*1483*	*2091*			
	Mean (IP=0)	*64.31*	*65.96*	*63.14*			
Monthly labour income (US$)		31.87***	23.25**	38.41***	31.01***	25.11***	37.86***
		(5.92)	(7.42)	(8.77)	(5.93)	(7.32)	(9.13)
	N	*3456*	*1449.00*	*2007*			
	Mean (IP=0)	*133.23*	*120.94*	*141.91*			

Note: *** p<0.001, ** p<0.05, * p<0.1. Robust Standard errors in parenthesis.

Table 6. Diff-in-Diff estimates of the ITT effect on individuals and households outcomes in the second follow up.

Dependent variable		Without additional controls			With additional controls		
		All	Small towns	Large Towns	All	Small towns	Large Towns
Individuals' outcomes							
Weekly hours worked		1.60	3.58*	-0.10	1.61	3.89**	-0.52
		(1.00)	(1.40)	(1.41)	(1.02)	(1.47)	(1.45)
	N	*4213*	*1860*	*2352*	*4213*	*1861*	*2352*
	Mean (IP=0)	*24.48*	*27.07*	*22.32*			
Monthly labour income (US$)		4.48	11.49***	-1.64	4.81	12.15***	-0.79
		(2.66)	(3.07)	(4.21)	(2.79)	(3.11)	(4.71)
	N	*4201*	*1846*	*2354*	*4201*	*1847*	*2354*
	Mean (IP=0)	*49.95*	*52.95*	*47.50*			
Households' outcomes							
Weekly hours worked		3.56	6.11	1.66	4.03	7.37*	0.94
		(2.20)	(3.32)	(2.93)	(2.25)	(3.41)	(3.04)
	N	*3058*	*1227*	*1831*	*3058*	*1227*	*1831*
	Mean (IP=0)	*63.08*	*63.45*	*62.82*			
Monthly labour income (US$)		11.23	17.85**	6.22	10.06	21.18**	3.53
		(6.18)	(6.25)	(9.77)	(6.20)	(6.42)	(10.14)
	N	*3046*	*1230*	*1816*	*3046*	*1230*	*1816*
	Mean (IP=0)	*133.9*	*120.09*	*143.74*			

Note: *** p<0.001, ** p<0.05, * p<0.1. Robust Standard errors in parenthesis.

Table 7. Diff-in-Diff estimates of the ITT effect on household's monetary and in-kind net transfers.

		Without additional controls			With additional controls		
		All	Small towns	Large towns	All	Small towns	Large towns
1st follow up	Coeff.	-0.98	-0.3	-1.58	-0.58	0.23	-1.02
	s.e	(1.08)	(1.62)	(1.44)	(1.09)	(1.67)	(1.47)
	N	*4668*	*2119*	*2549*			
2nd follow up	Coeff.	1.35	2.2	0.63	1.4	1.64	1.31
	s.e	(1.82)	(2.37)	(2.70)	(1.91)	(2.50)	(2.83)
	N	*3946*	*1716*	*2230*			

Note: *** p<0.001, ** p<0.05, * p<0.1. Robust Standard errors in parenthesis.

Table 8. Diff-in-Diff estimates of the ITT effect on household's consumption.

Dependent variable		Without additional controls			With additional controls		
		All	Small towns	Large Towns	All	Small towns	Large Towns
			1st follow up				
log consumption	*Coeff.*	0.01	0.05*	-0.03	0.01	0.05*	-0.02
	s.e	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
	N	*3853*	*1687*	*2166*			
log food consumption	*Coeff.*	0.02	0.10***	-0.05	0.02	0.10***	-0.05
	s.e	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
	N	*4580*	*2085*	*2495*			
			2nd follow up				
log consumption	*Coeff.*	-0.00	0.01	-0.01	0.01	0.02	0.01
	s.e	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
	N	*3063*	*1328*	*1735*			
log food consumption	*Coeff.*	-0.02	0.04	-0.07*	-0.01	0.03	-0.05
	s.e	(0.02)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)
	N	*3965*	*1744*	*2221*			

Note: *** p<0.001, ** p<0.05, * p<0.1. Robust Standard errors in parenthesis.

Table 9. Ex-participants self-reported use of income earned on EA (second F.U.)

Did you use EA income on…	*mean*	*N*
accommodation	4%	2580
repay debt	44%	2585
business creation	3%	2581
medical treatment	6%	2575
public services	41%	2579
other (food, clothes, education)	85%	2574

Table 10. Differences in labor occupation transitions probabilities between randomized in and out for the most frequently reported occupations (3 months before baseline to second F.U.)

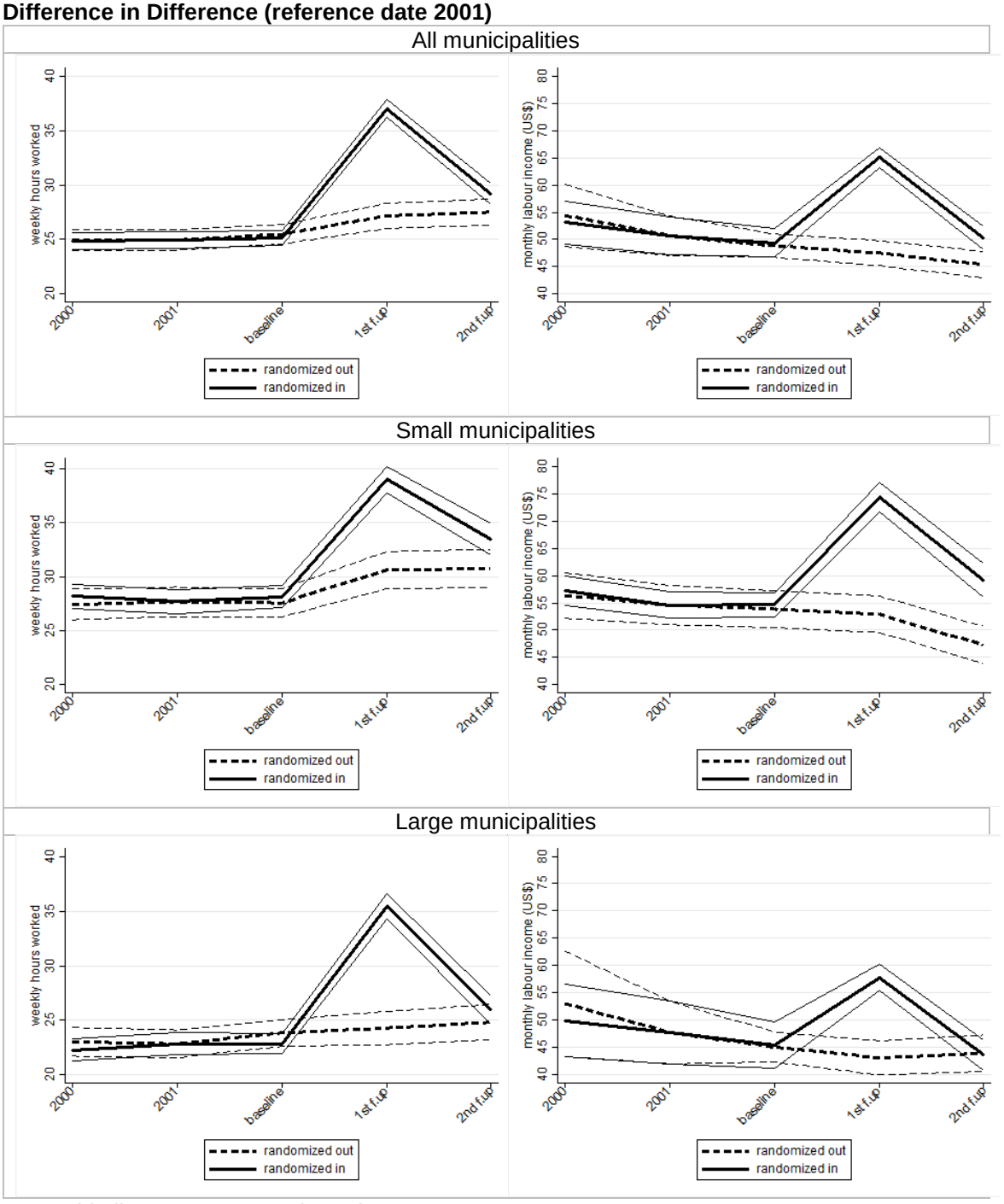
SECOND FOLLOW UP OCCUPATION	PRE BASELINE LABOUR OCCUPATION								
SMALL	Farming	Manufacture	Building	Commerce	Community	Domestic	Indpt self-imp	unemployed	out
Farming	**-0.13**	0.08	-0.05	0	-0.13	0.06	0.01	0.07	0
Manufacture	0	-0.1	0	0.08	0	-0.08	0	-0.02	-0.01
Building	**0.07**	0.08	-0.22	-0.11	0	0	0	**0.1**	**0.03**
Commerce	-0.01	0	0.03	0.04	0.11	0	0	-0.02	0
Community	**0.07**	0	0.01	-0.06	**0.19**	**0.11**	-0.01	0	0.02
Domestic	0.02	-0.2	0.03	0.09	0.05	-0.06	0	0	0.02
Indpt self-imp	-0.07	0.08	0.09	0.02	-0.12	-0.08	-0.01	-0.01	-0.03
unemployed	0.03	-0.12	0.09	-0.11	-0.08	0	0.01	**-0.22**	0.01
out	0.02	0.17	0.02	0.04	-0.01	0.06	-0.01	0.1	**-0.04**
LARGE	Farming	Manufacture	Building	Commerce	Community	Domestic	Indpt self-imp	unemployed	out
Farming	0.01	0	0	0	0	0	0.01	-0.01	0.01
Manufacture	0.05	-0.17	0	0	0.04	0	0	-0.01	0
Building	0.1	0	-0.22	0.02	0.02	0	0.02	0.02	0
Commerce	0.1	0	0	0.07	0.05	-0.04	0	-0.01	-0.03
Community	0	0	-0.02	0	-0.11	0.04	0.01	0	0.01
Domestic	-0.29	0	-0.02	-0.05	0	-0.13	0.01	0.01	0
Indpt self-imp	0.25	-0.01	0.12	0.17	-0.1	0.02	-0.03	0.02	0.07
unemployed	-0.19	0.12	0.07	-0.05	0	-0.04	0	**0.03**	0.01
out	-0.04	0.06	0.06	-0.16	0.09	0.15	-0.02	-0.05	**-0.05**

Reading note: We report the difference in the transition shares reported in Table A.8. For example, in small municipalities individuals previously in farming have a -13% points less chance to end up in farming if they are randomized in.

8. Appendix

i. Additional Figures

Figure A. 1. Mean individual weekly hours per week and individual monthly labor income (US$) in randomized-in and -out samples for each survey wave and past values in Difference in Difference (reference date 2001)



Note: Thin lines are 95% C.I. bounds.

ii. Additional tables

Table A. 1. Balance of individual characteristics between those that initially intended to participate and not (beneficiaries) – Difference

Variable		All	Large Towns	Small Towns
Sex (1=Female)		-0.0445**	-0.0850**	0.00268
		[0.0124]	[0.0180]	[0.0167]
Age		-0.366	-0.298	-0.446
		[0.361]	[0.509]	[0.510]
Illness (= 1 if …)	Any health problem in the last 2 weeks	-0.0452**	-0.0425**	-0.0484**
		[0.0100]	[0.0139]	[0.0144]
	Had to stay in bed in the last 2 weeks	-0.0288**	-0.0269**	-0.0310**
		[0.00747]	[0.0103]	[0.0109]
	Had to stay in hosp in the last 12 months	-0.00846	0.00263	-0.0214+
		[0.00743]	[0.00998]	[0.0111]
Migrant		0.0125	0.0072	0.0188
		[0.0127]	[0.0178]	[0.0181]
Education	No studies	0.00377	0.0132	-0.00722
		[0.00860]	[0.0106]	[0.0140]
	Primary incomplete	0.0191	0.0247	0.0124
		[0.0130]	[0.0172]	[0.0196]
	Primary complete	-0.019	-0.0405*	0.00612
		[0.0118]	[0.0166]	[0.0168]
	Secondary incomplete	0.00291	-0.000339	0.0067
		[0.0122]	[0.0175]	[0.0168]
	Secondary complete	-0.000844	0.00897	-0.0123
		[0.00988]	[0.0133]	[0.0147]
	More than secondary complete	-0.00598+	-0.0061	-0.00583
		[0.00353]	[0.00465]	[0.00538]
	Has done a training course	-0.0246*	-0.0139	-0.0370*
		[0.0105]	[0.0149]	[0.0148]
Work	Has done paid work in the last 20 years	0.00551	0.0233**	-0.0153*
		[0.00523]	[0.00706]	[0.00775]
	Has done paid work during at least a month in 2001	-0.00336	0.0192	-0.0300+
		[0.0124]	[0.0179]	[0.0170]
	Has done paid work during at least a month in 2000	-0.00773	0.0108	-0.0296+
		[0.0129]	[0.0184]	[0.0179]
	Number of months worked during 2001	-0.363*	0.0808	-0.889**
		[0.145]	[0.201]	[0.209]
	Number of months worked during 2000	-0.320*	0.063	-0.772**
		[0.149]	[0.205]	[0.215]
	Number of hours a week worked during 2001	-1.268+	-0.257	-2.463**
		[0.654]	[0.907]	[0.942]
	Number of hours a week worked during 2000	-0.855	-0.146	-1.689+
		[0.676]	[0.936]	[0.977]
	Monthly individual labor revenue in 2001 (in dec 2003 pesos),	-2.14	4.383	-9.891**
		[2.684]	[4.507]	[2.414]
	Monthly individual labor revenue in 2000 (in dec 2003 pesos),	-2.186	3.857	-9.351**
		[3.382]	[5.745]	[2.878]
	Observations	5724	3218	2505

Note: ** p<0.01, * p<0.05, + p<0.1. Robust Standard errors in brackets.

Table A. 2. Balance of household characteristics between those that initially intended to participate and not (beneficiaries) – Difference

			All		Large Towns		Small Towns	
			Difference	s.e.	Difference	s.e.	Difference	s.e.
Household composition	Number of people…	In the household	-0.086	[0.0741]	-0.058	[0.103]	-0.120	[0.106]
		Younger than 7 years old	0.002	[0.0309]	-0.001	[0.0424]	0.005	[0.0451]
		Between 7 and 18 years old	-0.038	[0.0391]	-0.047	[0.0533]	-0.028	[0.0576]
		Older than 18	-0.050	[0.0438]	-0.010	[0.0623]	-0.097	[0.0611]
Housing conditions	Housing is a house		-0.0248**	[0.00894]	-0.0508**	[0.0135]	0.005	[0.0112]
	1= if housing has	Tile flooring	-0.0195+	[0.0103]	-0.004	[0.0147]	-0.0379**	[0.0142]
		Wood flooring	0.003	[0.00438]	-0.006	[0.00607]	0.0129*	[0.00631]
		Conglomerate floor tiles	0.014	[0.0133]	0.026	[0.0184]	-0.001	[0.0192]
		Earthen flooring	0.003	[0.00977]	-0.017	[0.0130]	0.0258+	[0.0148]
		A ceiling	-0.002	[0.0105]	0.0139	[0.0156]	-0.0204	[0.0134]
		Sewage system	-0.006	[0.00902]	0.0187	[0.0123]	-0.0343**	[0.0132]
		A toilet connected to housing	0.007	[0.00960]	0.00663	[0.0120]	0.00726	[0.0153]
		No toilet	-0.005	[0.00786]	-0.00125	[0.00900]	-0.00843	[0.0134]
		A toilet exclusive of household	0.005	[0.0120]	-0.0101	[0.0164]	0.0234	[0.0177]
	1= if walls are made of	Brick	-0.0189+	[0.0112]	0.0105	[0.0147]	-0.0531**	[0.0172]
		Adobe	0.0335**	[0.00910]	0.0206*	[0.00923]	0.0487**	[0.0165]
		Wood	-0.0147+	[0.00750]	-0.0311*	[0.0126]	0.00450	[0.00690]
	1=if housing receives	Water service by pipe	-0.0175*	[0.00803]	0.00394	[0.0107]	-0.0425**	[0.0120]
		Rubbish disposal and collection service	-0.010	[0.00751]	0.0193**	[0.00748]	-0.0445**	[0.0136]
	Number of	Rooms	-0.0844*	[0.0354]	-0.0439	[0.0506]	-0.132**	[0.0491]
		Bedrooms	-0.0499+	[0.0267]	-0.0335	[0.0373]	-0.0690+	[0.0380]
	1= if kitchen is	Also used as bedroom	0.010	[0.00696]	0.0184+	[0.0109]	-0.000732	[0.00804]
		Shared with other households	-0.012	[0.00880]	-0.00501	[0.0134]	-0.0207+	[0.0109]
	1= if household uses different source of energy to electricity and gas		-0.0245*	[0.0121]	-0.00648	[0.0149]	-0.0455*	[0.0196]
	1= if household has landline		-0.017	[0.0122]	0.00446	[0.0174]	-0.0427*	[0.0169]
	House ownership status (1= if housing is	Owned	-0.0487**	[0.0136]	-0.0555**	[0.0191]	-0.0408*	[0.0194]
		Rented	0.0232*	[0.0117]	0.0334*	[0.0168]	0.0113	[0.0160]
		Neither rented nor owned	0.0255*	[0.0101]	0.0221	[0.0137]	0.0295*	[0.0148]
		Books	0.0219+	[0.0123]	0.0423**	[0.0162]	-0.002	[0.0188]
	Observations		569		3238		2531	

Note: ** p<0.01, * p<0.05, + p<0.1. Robust Standard errors in brackets.

Table A. 2. Balance of household characteristics between those that initially intended to participate and not (beneficiaries) – Difference (Cont.)

			All		Large Towns		Small Towns	
			Difference	s.e.	Difference	s.e.	Difference	s.e.
Assets and Properties	1= if household owns other properties		0.0145+	[0.00743]	0.0151*	[0.00755]	0.014	[0.0135]
	1= if household has	Fridge	-0.0493**	[0.0138]	-0.011	[0.0188]	-0.0947**	[0.0203]
		Sewing machine	0.005	[0.00912]	0.003	[0.0122]	0.007	[0.0137]
		Black & white tv	0.019	[0.0118]	0.014	[0.0166]	0.026	[0.0168]
		Music machine	-0.0234*	[0.0116]	-0.023	[0.0164]	-0.024	[0.0164]
		Bike	0.0432**	[0.0131]	0.0689**	[0.0171]	0.013	[0.0202]
		Motor vehicle	0.002	[0.00614]	-0.001	[0.00748]	0.004	[0.0100]
		Fan	0.004	[0.00982]	0.012	[0.0140]	-0.004	[0.0136]
		Juice machine	-0.004	[0.0141]	0.016	[0.0191]	-0.028	[0.0210]
		Color tv	-0.022	[0.0141]	-0.002	[0.0193]	-0.0462*	[0.0207]
		Books	0.0219+	[0.0123]	0.0423**	[0.0162]	-0.002	[0.0188]
Participation in other social programs	1 if any member of the household participates in ..	*Empleo en Acción - EA*	0.539**	[0.00961]	0.664**	[0.0124]	0.392**	[0.0140]
		Familias en Acción	-0.006	[0.00665]	-0.001	[0.00156]	-0.012	[0.0143]
		Jóvenes en Acción	-0.00584*	[0.00254]	-0.00927*	[0.00459]	-0.002	[0.00130]
		Hogares comunitarios	0.013	[0.00802]	0.0206*	[0.0102]	0.004	[0.0127]
		Other	-0.006	[0.00436]	-0.006	[0.00682]	-0.006	[0.00508]
Health, Education and shocks indicators	1 if household suffered a shock in 2000, 2001 or 2002 due to …	Violence or displacement	0.005	[0.00791]	0.008	[0.0118]	0.003	[0.0102]
		Fire, flooding or natural disaster	0.000	[0.00536]	0.012	[0.00767]	-0.0132+	[0.00739]
		Either business or crop loss	0.0339**	[0.00831]	0.014	[0.00955]	0.0566**	[0.0141]
		A member loss of job	0.0303*	[0.0122]	0.021	[0.0178]	0.0408*	[0.0163]
		A member severe illness	0.0269*	[0.0106]	0.0424**	[0.0142]	0.009	[0.0159]
		A member death	0.0153*	[0.00688]	0.0192*	[0.00975]	0.011	[0.00963]
	Observations		569		3238		2531	

Note: ** p<0.01, * p<0.05, + p<0.1. Robust Standard errors in brackets.

Table A. 3. Diff-in-Diff estimates of the ITT effect on household's consumption – Robustness check for projects not started at baseline survey.

Project had not started		Without additional controls			With additional controls		
Dependent variable		All	Small towns	Large Towns	All	Small towns	Large Towns
				1st follow up			
	Coeff.	0.03	0.04	0.02	0.04	0.06	0.01
Log consumption per capita	s.e	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)
	N	1537	931	606	1537	931	606
	Coeff.	0.07	0.11**	-0.03	0.07	0.14**	-0.05
Log food consumption per capita	s.e	(0.04)	(0.04)	(0.07)	(0.04)	(0.04)	(0.07)
	N	1812	1132	680	1812	1132	680
				2nd follow up			
	Coeff.	0.06	0.03	0.09	0.05	0.05	0.07
Log consumption per capita	s.e	(0.03)	(0.04)	(0.05)	(0.03)	(0.05)	(0.05)
	N	1306	718	588	1306	718	588
	Coeff.	0.03	0.04	0.01	0.02	0.05	-0.06
Log food consumption per capita	s.e	(0.04)	(0.05)	(0.07)	(0.04)	(0.05)	(0.08)
	N	1620	918	702	1620	918	702
				1st minus 2nd follow up			
	Coeff.	0.00	0.05	-0.08	0.04	0.06	0.02
Log consumption per capita	s.e	(0.03)	(0.04)	(0.05)	(0.05)	(0.07)	(0.08)
	N	1162	682	480	1507	909	598
	Coeff.	0.04	0.04	0.08	0.05	0.09	-0.00
Log food consumption per capita	s.e	(0.05)	(0.07)	(0.08)	(0.04)	(0.05)	(0.07)
	N	1507	909	598	1494	907	587

Note: *** p<0.001, ** p<0.05, * p<0.1. Robust Standard errors in parenthesis.

Table A. 4. Time elapsed since end of participation in *EA* at second follow up date

Days since end of participation in EA (2nd f.u.)	Mean	Median	S.d.
Large municipalities	319	281	152
Small municipalities	384	396	131
Total	343	357	148

Table A. 5. Long lasting ITT effect in small municipalities, gender heterogeneity.

Small municipalities only		Without additional controls			With additional controls		
Dependent variable		All	female	male	All	female	male
Individuals' outcomes							
Weekly hours worked	Coeff.	3.51*	3.71	3.41*	3.89**	3.56	4.07*
	s.e	(1.40)	(2.83)	(1.66)	(1.47)	(3.11)	(1.77)
	N	1861	500	1361	1861	500	1361
Monthly labor income [US$]	Coeff.	11.39***	9.58	11.00**	12.15***	10.29	12.58**
	s.e	(3.07)	(5.26)	(3.79)	(3.11)	(6.09)	(3.89)
	N	1847	496	1351	1847	496	1351

Note: *** p<0.001, ** p<0.05, * p<0.1. Robust Standard errors in parenthesis.

Table A. 6. Self-reported impact of EA on participants' job search constraints.

	Small municipalities		Large municipalities	
	male	female	male	female
Thanks to EA, has it been easier to find a job?	21%	14%	21%	12%
Why? main reason				
gained work experience	47%	22%	40%	26%
learned a new job	15%	17%	7%	10%
got in contact with someone who helps	31%	46%	38%	44%
gained in self-confidence	5%	15%	13%	18%
other	2%	0%	3%	1%
Why not? main reason				
have to little work experience	11%	12%	7%	15%
did not learn enough	11%	8%	9%	3%
have no contact with people who may help	24%	21%	40%	33%
I am not able	3%	4%	5%	4%
Other (mostly employment shortage, then age and illness)	52%	56%	39%	45%
Did you find a job?	87%	67%	74%	54%
How long did it take? mean ; median (months)	1.7 ; 1	3.3 ; 1	2.1 ; 1	2.9 ; 1

Note: Subsample = Ex-participants in second follow-up survey.

Table A. 7. Share of unemployed among labor active in small and large municipalities in second follow up (Community sample)

	N	Mean	Sd
Large municipalities	6807	14%	0.004
Small municipalities	6309	6%	0.003
Whole	13116	10%	0.003
t-test: P(Ho: diff = 0)	0.000		

Table A. 8 a. Labor force transitions between pre-baseline and 2nd follow-up for the most frequently reported occupations in Small municipalities

		PRE BASELINE LABOUR OCCUPATION								
	Small	Farming	Manufacture	Building	Commerce	Community	Domestic	Indpt self-imp	unemployed	out
SECOND FOLLOW UP OCCUPATION	***IP = 0***									
	Farming	0.78	0.00	0.13	0.00	0.13	0.00	0.02	**0.05**	0.04
	Manufacture	0.00	0.60	0.00	0.05	0.00	0.08	0.00	**0.02**	0.01
	Building	**0.00**	0.00	0.53	0.16	0.00	0.00	0.01	0.02	0.01
	Commerce	0.03	0.00	0.00	0.37	0.00	0.00	0.01	0.07	0.06
	Community	**0.00**	0.00	0.07	0.11	0.33	0.00	0.03	0.07	0.04
	Domestic	0.00	0.20	0.00	0.00	0.00	0.50	0.00	0.00	0.03
	Indpt self-imp	0.17	0.00	0.07	0.16	0.33	0.08	0.87	0.27	0.34
	unemployed	0.00	0.20	0.07	0.11	0.13	0.00	0.02	**0.48**	0.03
	out	0.03	0.00	0.13	0.05	0.07	0.33	0.04	0.02	0.44
	IP = 1									
	Farming	0.65	0.08	0.08	0.00	0.00	0.06	0.03	**0.11**	0.04
	Manufacture	0.00	0.50	0.00	0.14	0.00	0.00	0.00	0.00	0.01
	Building	**0.07**	0.08	0.32	0.05	0.00	0.00	0.01	**0.12**	0.04
	Commerce	0.02	0.00	0.03	0.41	0.11	0.00	0.01	0.05	0.05
	Community	**0.07**	0.00	0.08	0.05	0.53	0.11	0.02	0.07	0.07
	Domestic	0.02	0.00	0.03	0.09	0.05	0.44	0.01	0.00	0.05
	Indpt self-imp	0.10	0.08	0.16	0.18	0.21	0.00	0.86	0.27	0.31
	unemployed	0.03	0.08	0.16	0.00	0.05	0.00	0.03	**0.26**	0.04
	out	0.05	0.17	0.16	0.09	0.05	0.39	0.03	0.12	0.40

Reading Note: Each sub column sums up to 1. E.g. in small municipalities 78% of randomized out individual who were in farming before the baseline are still in farming in the second follow-up.

Table A. 8 b. Labor force transitions between pre-baseline and 2nd follow-up for the most frequently reported occupations in Large municipalities

		PRE BASELINE LABOUR OCCUPATION								
	LARGE	Farming	Manufacture	Building	Commerce	Community	Domestic	Indpt self-imp	unemployed	out
	IP = 0									
SECOND FOLLOW UP OCCUPATION	Farming	0.29	0.00	0.00	0.00	0.00	0.00	0.00	**0.02**	0.01
	Manufacture	0.00	0.57	0.03	0.00	0.00	0.00	0.01	0.07	0.02
	Building	**0.00**	0.00	0.58	0.03	0.00	0.00	0.01	**0.08**	0.04
	Commerce	**0.00**	0.00	0.03	0.28	0.04	0.07	0.01	0.05	0.06
	Community	0.00	0.00	0.03	0.00	0.54	0.00	0.00	0.08	0.04
	Domestic	0.29	0.00	0.03	0.10	0.00	0.59	0.00	0.01	0.07
	Indpt self-imp	0.00	0.14	0.10	0.28	0.19	0.07	0.80	0.16	0.20
	unemployed	0.29	0.14	0.13	0.10	0.13	0.07	0.04	**0.35**	0.05
	out	0.14	0.14	0.06	0.21	0.11	0.19	0.11	0.17	0.52
	IP = 1									
	Farming	0.30	0.00	0.00	0.00	0.00	0.00	0.02	**0.01**	0.01
	Manufacture	0.05	0.40	0.03	0.00	0.04	0.00	0.01	0.06	0.02
	Building	**0.10**	0.00	0.36	0.05	0.02	0.00	0.03	**0.10**	0.04
	Commerce	**0.10**	0.00	0.03	0.35	0.09	0.04	0.02	0.04	0.04
	Community	0.00	0.00	0.02	0.00	0.42	0.04	0.02	0.08	0.05
	Domestic	0.00	0.00	0.02	0.05	0.00	0.46	0.01	0.02	0.06
	Indpt self-imp	0.25	0.13	0.22	0.45	0.09	0.09	0.78	0.18	0.27
	unemployed	0.10	0.27	0.20	0.05	0.13	0.04	0.04	0.37	0.06
	out	0.10	0.20	0.13	0.05	0.20	0.34	0.09	0.13	0.47

Reading Note: See table A.8a