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Anna Conte
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Friedrich Schiller University Jena
Carl-Zeiss-Str. 3
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www.uni-jena.de

Max Planck Institute of Economics
Kahlaische Str. 10
D-07745 Jena
www.econ.mpg.de

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Use of data on planned contributions and stated beliefs in the measurement of social preferences

Anna Conte[a,b,*], M. Vittoria Levati[a,c]

[a] *Max Planck Institute of Economics, Kahlaische Str. 10, 07745 Jena, Germany*
[b] *WBS, University of Westminster, EQM Department, 35 Marylebone Road, NW1 5LS London, UK*
[c] *University of Verona, Department of Economics, Via dell'Artigliere 19, 37129 Verona, Italy*

Abstract

In a series of one-shot linear public goods game, we ask subjects to report their contributions, their contribution plans for the next period, and their first-order beliefs about their present and future partner. We estimate subjects' preferences from plan data by a finite mixture approach and compare the results with those obtained from contribution data. Controlling for beliefs, which incorporate the information about the others' decisions, we are able to show that plans convey accurate information about subjects' preferences and, consequently, are good predictors of their future behavior.

JEL classification: C35; C51; C72; H41

Keywords: Public goods experiments; Social preferences; Mixture models

*Corresponding author. Tel.: +49 3641 686621; fax: +49 3641 686667. Email: aconte@econ.mpg.de; A.Conte@westminster.ac.uk

1. Introduction

A vast amount of experimental evidence has shown that individuals contribute voluntarily to public goods, even if self-interest implies that free riding should be their dominant strategy. Several researchers explain this finding in terms of social preferences, such as altruism (Levine, 1998), efficiency concerns (Charness and Rabin, 2002), inequity aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), and conditional cooperation (Fischbacher et al., 2001).

A handful of experimental studies have investigated the empirical validity of the various models of social preferences, mostly focusing on distribution games (e.g., Andreoni and Miller, 2002; Charness and Rabin, 2002; Cox et al., 2007). In the context of public goods games, the identification of social preferences seems to require a careful exploration not only of the decision maker's behavior, but also of his beliefs about others' behavior. In a piece of early work, Offerman et al. (1996) use a step-level public goods game with binary contributions to provide insights into the relationship between expectations and behavior. In linear public goods games, Croson (2007) detects a significant and positive relationship between an individual's own contribution and his beliefs about the contributions of the other members of his group. More recently, Fischbacher and Gächter (2010) and Ambrus and Pathak (2011) independently explain the decline of contributions in repeated public goods settings by combining the role played by beliefs in influencing contributions with the presence of different types of players.

We depart from the aforementioned literature in that we identify people's cooperative preferences by looking at the relationship between planned contributions and stated beliefs about the others' actions in the following period. We consider a sequence of fifteen one-shot two-person linear public goods games. In each game, subjects make two contribution decisions and specify two distributions of first-order beliefs: (a) they choose their contribution amount from a given set of 11 elements, and state their subjective probabilities that the participant they are currently matched with has opted for any element of the same set, and (b) they choose from an identical discrete set the amount that they plan to contribute in the following period, and state their subjective probabilities that the participant they will be matched with then will opt for any element of the set.

Our intuition is that, if other-regarding preferences are idiosyncratic and beliefs play a role in contribution determination, then a person should reveal his preferences not only in the way his contribution relates to his current beliefs, but also in the way his planned contribution relates to his beliefs about the other's contributions one period ahead. A major contribution of this paper is to verify this intuition and thus to test whether plans convey accurate information about preferences and behavior. It is surprising that, apart from a few studies on individual decision making (e.g., Bone et al., 2003; Hey, 2005; Bone et al., 2009), economists tend to ignore data on plans. The typical claim is that people cannot plan ahead, with the empirical divergence between plans and eventual behavior being interpreted as evidence of how poor predictors individuals are of their future acts. However, as Manski (1990, p. 934) points out, such a conclusion is unwarranted: planned and eventual behavior may differ as a consequence of events occurring between the time plans are elicited and the time actions take place. Since we consider public goods games in which each individual receives feedback about other's contribution after each period, what may cause one's own final behavior to differ from one's own earlier plan is a revision to first-order beliefs (which in turn captures the effect of the new information acquired in the interim period between elicitation of plans and final choices). To our knowledge, this paper is the first to condition on beliefs in order to assess whether the preference types estimated from plans are equal to those estimated from final contributions.

We believe this issue to be important because intentions to contribute (mostly gathered through 'consequential' survey questions)[1] are frequently used by businesses and governments to determine which product to offer and which policy to adopt. Additionally, purchase intentions are routinely used in marketing research to predict whether or not consumers will purchase products (see Morwitz, 1997; and references therein). As noticed by J. Hey (2005, p. 122), "central to effectively all economic theories of rational dynamic decision-making is the concept of a plan".

A public goods game lends itself to planning more naturally than other games because people are often asked to state how much they plan to contribute to, e.g., renewable energy sources, health, parks or infrastructure before they specify their actual contributions. For instance, a government that wants to equip all houses with solar panels may ask people how much they plan to contribute to such energy production technology prior to starting the project; only if the reported planned contributions are substantial, the project is started and people are asked to actually contribute. Our experiment allows us to elicit, in a controlled laboratory environment, planned and final contributions to public goods, and to investigate whether and to what extent people's preferences when planning differ from their preferences when finally contributing.

[1]Survey questions are consequential if the "survey's results are seen as potentially influencing an agency's actions and the agent cares about the outcomes of those actions" (Carson and Groves, 2007, p. 183).

The remainder of the paper is organized as follows. After introducing the basic games, Section 2 details our experimental treatments and procedures. Section 3 describes the data and reports preliminary statistical tests. In Section 4 we define the mixture model, and in Section 5 we present and discuss the estimates of the model. Section 6 summarizes our central findings and concludes.

2. The experiment

2.1. The public goods games

The basic decision situation is a standard linear public goods game. Let $N = \{1, \ldots, 30\}$ stand for a population of 30 individuals who interact in pairs for $t = 1, \ldots, 15$ periods according to a perfect-stranger matching design so that nobody meets the same person more than once.[2] At the beginning of every period, each individual $i \in N$ is endowed with 100 ECU (Experimental Currency Units) which he can either keep for himself or contribute to a public good. We discretize the choice set of each individual i to eleven alternatives: $\mathcal{A} \in \{(0, 100), (10, 90), (20, 80), \ldots, (80, 20), (90, 10), (100, 0)\}$, where the first and second amounts denote the number of ECU that i contributes to the public good and keeps for himself, respectively. More synthetically, we can denote each alternative by a ($a = 0, \ldots, 10$), so that each element of $\mathcal{A}$ can be expressed as ($a \times 10$, $100 - a \times 10$). For example, opting for $a = 0$ means contributing nothing and keeping everything for oneself. Let $c_{i,t}$ be i's contribution in period t. Likewise, let $c_{j,t}$ define player i's partner's (player j's) contribution in t.[3] In the standard voluntary contribution mechanism, participants make choices only for the present period and the monetary payoff of player i (for all $i \in N$) in each period $t = 1, \ldots, 15$ is given by:

$$\pi_{i,t} = 100 - c_{i,t} + 0.8\,(c_{i,t} + c_{j,t}), \tag{1}$$

where the public good is equal to the sum of the contributions of i and j.

The game we consider deviates from this usual practice in that we require subjects not only to choose for the present period, but also to plan what they intend to do in the following period. We do not want to force the participants to commit themselves to

[2] We chose this protocol to minimize strategic effects from repeated play and to allow for revisions to beliefs only at the population level.

[3] To simplify notation, we always refer to player i's partner as j, although this is a different person in each period.

the plan, while at the same time we want them to honestly report their intentions. To this aim, we let the public good in periods 2 to 15 be based on either the sum of i's and j's final contributions or the sum of the contribution plans they formulated beforehand, with both possibilities being equally likely.[4] Let $p^t_{i,t-1}$ and $p^t_{j,t-1}$ denote the amounts that, respectively, player i and player j (player i's partner) plan at time $t-1$ to contribute in t, with $t = 2, \ldots, 15$.[5] The payoff function in the game with plans elicitation is given by (1) in the first period. Afterwards (i.e., in $t = 2, \ldots, 15$), it can be either (1) with 50% probability or

$$\pi_{i,t} = 100 - p^t_{i,t-1} + 0.8 \left(p^t_{i,t-1} + p^t_{j,t-1} \right), \tag{2}$$

with 50% probability. In what follows, for each player $i \in N$, we shall call $c_{i,t}$ the "final contribution" of i and $p^t_{i,t-1}$ the "planned contribution" of i.

2.2. *Treatments, decisions, and scoring rule for beliefs*

Using a between-subjects design, we study three treatments. In the control (C) treatment, subjects play the standard public goods game with payoff function (1). In every period $t = 1, \ldots, 15$, each participant i chooses one of the eleven alternatives in $\mathcal{A}$, thereby making a contribution choice $c_{i,t}$, and reports a first-order belief vector $\mathbf{b}^t_{i,t}$, i.e., a probability distribution over the eleven possible choices of his current partner j.

In the other two treatments, Plan-Info (P_I) and Plan-NoInfo (P_{NI}), subjects play the public goods game with plans elicitation described above where, in all periods but the first, the payoff function can be either (1) or (2), each with probability 1/2. In every period $t = 1, \ldots, 14$, besides choosing $c_{i,t}$ and stating $\mathbf{b}^t_{i,t}$, each participant i decides on the alternative that he plans to select in the next period, thereby providing a planned contribution $p^{t+1}_{i,t}$, and specifies his beliefs $\mathbf{b}^{t+1}_{i,t}$ about the alternative that his next-period partner will choose.[6]

In all three treatments, at the end of each period, participants receive feedback about the final contribution decision of their current-period partner, namely $c_{j,t}$.[7] Participants in

[4] A similar procedure for incentivizing subjects to state a carefully considered, truthful plan has been applied by Barkan and Busemeyer (1999).

[5] When convenient, we will equivalently use the notation $p^{t+1}_{\cdot,t}$, $t = 1, \ldots, 14$, to indicate contribution plans made in t for $t+1$.

[6] The instructions make clear that subjects have to predict the decisions of two different persons: the current-period partner ($\mathbf{b}^t_{i,t}$) and the next-period partner ($\mathbf{b}^{t+1}_{i,t}$).

[7] To simplify presentation, players' contributions in treatment C will be sometimes referred to as "final"

P_I, but not in P_{NI}, are also informed about the planned-in-the-previous period contribution decision of the person they are currently matched with, i.e., they also learn about $p^t_{j,t-1}$ with $t = 2, \ldots, 15$. In all treatments, no information about the realized public good and the period monetary payoff is provided until the end of the experiment. It is therefore impossible for participants in P_{NI} to infer their partner's plans during the game.

The control treatment is used to ensure that eliciting plans does not influence subjects' behavior. The two treatments with plans elicitation serve the main purposes of this paper. They enable us to estimate subjects' types based on their elicited plans, and to compare these estimates with those obtained using final contributions so as to test the hypothesis that there is no difference between them. This "consistency" hypothesis may, however, not be confirmed in treatment P_{NI} since non-disclosed plans could lead subjects to reveal their "genuine" preferences. It has been shown, for example, that some altruists may dislike being played for a sucker and consequently avoid to contribute in the belief that their fellow players will withhold contributions (see, e.g., Schnake, 1991). However, sucker aversion would diminish (or even vanish) if the other is not informed of one's own contributions.

We ask for beliefs because we want to assess the relationship between them and contributions, which we presume to differ across types. Previous research in experimental economics has shown that the mere act of eliciting beliefs can affect behavior in finitely repeated public goods games (see, e.g., Croson, 2000; Gächter and Renner, 2010), although the evidence regarding the undesirable effects of beliefs elicitation procedures is far from being conclusive (e.g., Wilcox and Feltovich, 2000), and it does not concern stranger matching protocols. As participants state their first-order beliefs in all our treatments and, in P_I and P_{NI}, for both the present and next periods, the unintended effects of beliefs on behavior (if any) would occur in all our treatments and apply to all our variables of interest.

Beliefs are elicited by endowing participants with 100 tokens and asking them to allocate these tokens on the 11 alternatives available to their partner. Participants are asked to assign tokens to each alternative in a way that reflects the probability they attach to the event that their partner chooses that alternative. We can think of each token as representing one percentage point.

We give subjects proper incentives for accurate predictions by using a quadrating scoring rule.[8] The rule is defined as follows. Assume that $\hat{c}_{j,t}$ is the alternative actually chosen by subject j (i's partner) in period t. Let i's beliefs in period $t - \tau$ be $\mathbf{b}^t_{i,t-\tau}$ with τ

even though no distinction between final and planned contributions is made in C.

[8]See Selten (1998) for an axiomatic characterization of the rule, and Offerman et al. (2009) for an experiment investigating its behavioral properties.

equals 0 in treatment C and either 0 or 1 in treatments P_I and P_{NI}. Let us indicate the generic element of the belief vector by $b^t_{i,t-\tau}(a)$, which denotes the probability (in percentage points) that in period $t-\tau$ subject i assigns to the event that his partner in period t chooses alternative a. In other words, $\mathbf{b}^t_{i,t-\tau} \equiv \left(b^t_{i,t-\tau}(0), b^t_{i,t-\tau}(1), \ldots, b^t_{i,t-\tau}(10)\right)$ with $\sum_{a=0}^{10} b^t_{i,t-\tau}(a) = 100$. Subject i's payoff for accuracy of predictions is:

$$v_{i,t} = 100 - 0.005 \times \sum_{a=0}^{10} \left(b^t_{i,t-\tau}(a) - 100 \times \mathbb{1}\left(a = \hat{c}_{j,t}\right)\right)^2, \tag{3}$$

where $\mathbb{1}(\cdot)$ is an indicator function taking on the value 1 if the statement in brackets is true and 0 otherwise.[9] Note that since beliefs are elicited in percentage points, they have to be divided by 100 to get probabilities.

In the instructions, we use a verbal description of the rule and give numerical examples. Problems of the quadrating scoring rule are that incentives are flat at the maximum and that it may be difficult to understand. To avoid this latter problem, our instructions emphasize that the more accurate the beliefs, the higher the payment.

2.3. Procedures

The experiment was programmed in z-Tree (Fischbacher, 2007) and conducted in the experimental laboratory of the Max-Planck Institute of Economics in Jena (Germany). Participants were undergraduate students from the University of Jena, who had never participated in public goods and prisoner dilemma experiments before. They were recruited using the ORSEE (Greiner, 2004) software. Upon entering the laboratory, participants were randomly assigned to visually isolated computer terminals. The instructions (reproduced in the supplement) were distributed and then read aloud to establish public knowledge. Before starting the experiment, subjects had to answer control questions which tested their comprehension of payoff functions (1) and (2). The experiment did not start until participants had answered all the questions correctly. We can therefore safely assume that participants understood the game.

Overall, we ran sixteen sessions: six for treatment C, and five for each of the two plan treatments (P_I and P_{NI}). In each session we had 30 participants so that, in total, our

[9] A similar rule has been used by, e.g., Offerman et al. (1996), Costa-Gomes and Weizsäcker (2008), and Rey-Biel (2009), although there exists no consensus among experimentalists about the optimal incentive mechanism for eliciting beliefs. Huck and Weizsäcker (2002) compare beliefs elicited via a quadratic scoring rule with beliefs elicited via a Becker-DeGroot-Marshak pricing rule, and find that the quadratic scoring rule yields more accurate beliefs.

analysis relies on 180 individuals observed in treatment C and 150 individuals observed in each of the other two treatments.

Participants in treatment C were paid according to their contributions in one randomly chosen period t_1, at a rate of €0.15 per ECU, and according to the accuracy of their belief statements in another randomly chosen period $t_2 \neq t_1$, using incentive rule (3). In treatments P_I and P_{NI} two further random draws determined (i) whether the public good in t_1 was based on either the sum of the final contributions or the sum of the contribution plans formulated beforehand, and (ii) which of the two reported belief vectors ($\mathbf{b}_{i,t_2}^{t_2}$ or $\mathbf{b}_{i,t_2-1}^{t_2}$) counted for payment in t_2.[10] Sessions lasted, on average, two hours with most of the time being used up for reading the instructions and answering the control questionnaire. Average earnings per subject were €26.82 (inclusive of a €2.50 show-up fee), ranging from €14.50 in treatments C and P_{NI} to €43.30 in treatment P_{NI}.

3. Data description and preliminary tests

We will approach the description of our data with the intention to assess whether the elicitation of plans induces distortions in individual propensity to contribute.

Figure 1 displays the time path of the average final contributions (solid lines) for each of the three treatments as well as the average planned contributions (dashed lines) for the two experimental treatments. The figure shows three things. First, consistent with previous experimental results, all five time series of average contributions begin high and decrease over time. Second, the average final contributions in C are quite similar to those in the two plan treatments, especially in period 1. Finally, planned contributions lie, on average, always slightly above the respective final contributions. The gap seems to reduce in P_I toward the end of the game. Following Manski (1990), we argue that the gap between final and planned contributions may be explained by differences in the belief formation process. Disregarding for the time being the relationship between choices and beliefs (which will be investigated in detail in the next two sections by a structural approach), we concentrate on descriptive statistics of choices and beliefs separately. Our main aim here will be to establish whether or not asking subjects about plans can change their final contributions and beliefs.

Regardless of the treatment, at the beginning of the game participants have no information about the other participants. Hence, their period-1 final contribution, $c_{i,1}$, cannot

[10] See the instructions in the supplement for a description of the random procedures that were used.

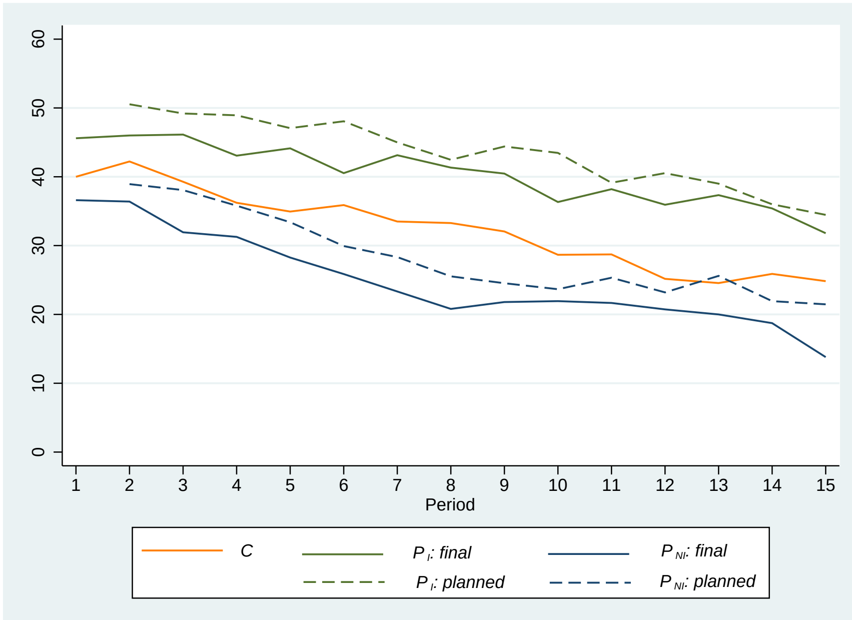


Figure 1: Time path of average (final and planned) contributions.

be affected by the observation of others' behavior. If the mere elicitation of plans has an effect on subjects' final contributions, we would expect this to emerge from a comparison of the distribution of period-1 final contributions in the control and the two experimental treatments. Such distributions are reported in Figure 2. The three diagrams share some similarities: they all have some mass at the extreme contributions (0 and 100) and the remainder somewhat at the center. In effect, both the Wilcoxon rank-sum (WRS) and Kolmogorov-Smirnov (KS) tests reveal that there are no significant differences in $c_{i,1}$ between C and P_I (WRS: p-value = 0.17; KS: p-value = 0.21) as well as between C and P_{NI} (WRS: p-value = 0.15; KS: p-value = 0.17). To ensure that the lack of significant difference in final contributions between the control and the two plan treatments is not confined to the first period, we performed WRS tests using session averages (aggregated over all 30 players and 15 periods) as independent observation units[11] and obtained similar results (p-value = 0.20 for both C vs. P_I and C vs. P_{NI}).

Turning to first-order beliefs, based on $\mathbf{b}^t_{i,t-1}$ and $\mathbf{b}^t_{i,t}$ we can compute the amount that in period $t-1$ subject i expects his partner j to contribute in t (one-period-ahead expected contribution, or $\mathrm{E}_{i,t-1}[c_{j,t}]$) and the amount that in period t i expects j to contribute in t (final expected contribution, or $\mathrm{E}_{i,t}[c_{j,t}]$). These amounts are calculated by averaging

[11]Because of our re-matching protocol, the numbers of statistically independent observations are 6 in C, 5 in P_I, and 5 in P_{NI}.

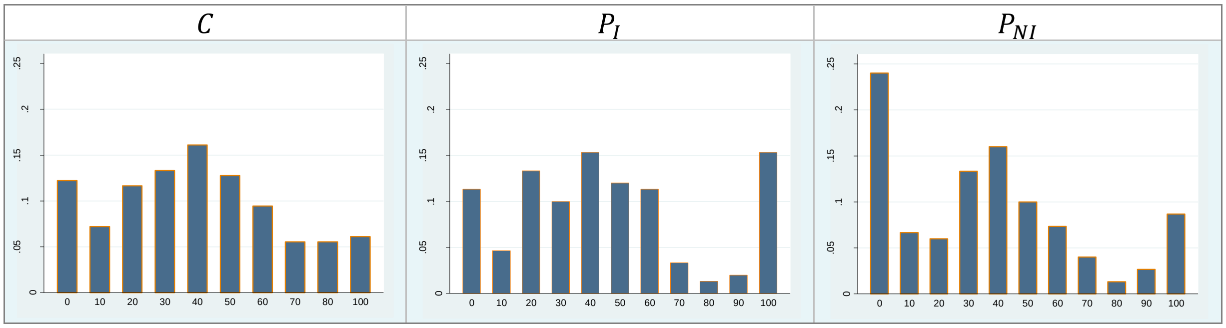


Figure 2: Bar-diagrams of period-1 final contributions (the bar height indicates the proportion of times the corresponding contribution has been chosen).

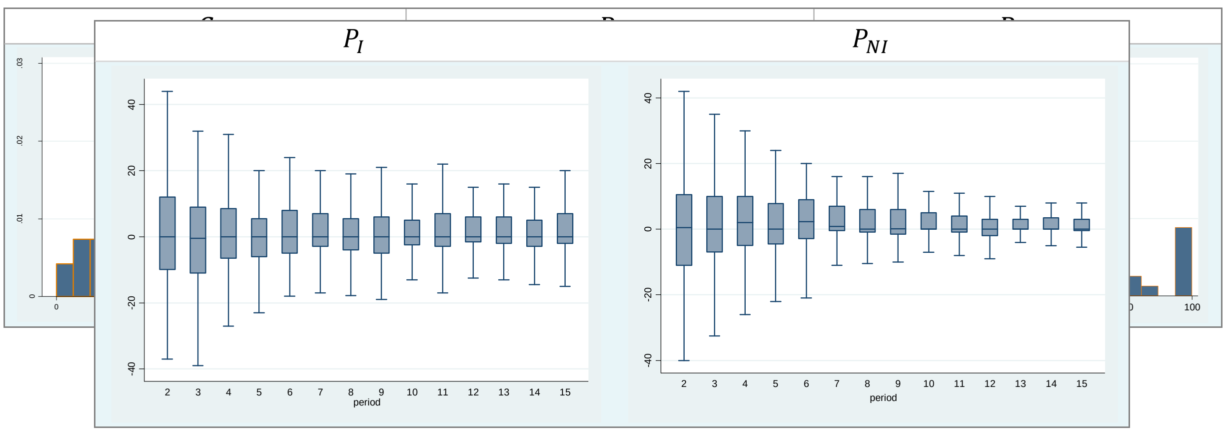


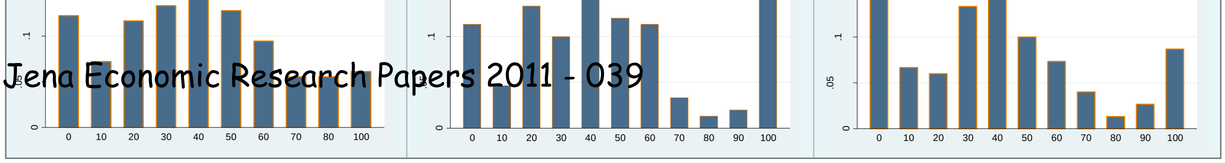
Figure 3: Difference between final expected contributions and one-period-ahead expected contributions.

all the possible contributions, weighted for the corresponding beliefs.[12] The distributions of the difference between final and one-period-ahead expected contributions are plotted as a time series of box plots in Figure 3. The figure shows that, as time progresses, the box plots collapse to zero in P_{NI}, indicating a tendency for final beliefs to catch up with one-period-ahead beliefs. Such a tendency is less pronounced in P_I.

In order to provide a formal test of the hypothesis that beliefs are not affected by plans elicitation, we proceed similarly to the analysis of contribution choices and consider first the distributions of period-1 final expected contributions. Figure 4 lends first visual support to the fact that the distributions of period-1 final expected contributions do not differ across

[12] In particular, the one-period-ahead and final expected contributions are computed, respectively, as:

$$\mathrm{E}_{i,t-1}[c_{j,t}] = \frac{\sum_{a=0}^{10} (a \times 10) \times b_{i,t-1}^{t}(a)}{100} \qquad \text{and} \qquad \mathrm{E}_{i,t}[c_{j,t}] = \frac{\sum_{a=0}^{10} (a \times 10) \times b_{i,t}^{t}(a)}{100}.$$



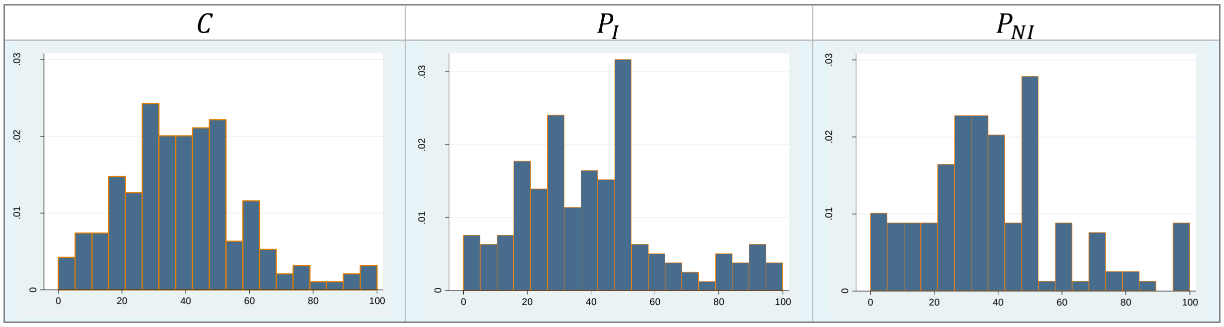


Figure 4: Histograms of period-1 final expected contributions.

treatments, which is confirmed by non-parametric hypothesis testing (for the comparison between C and P_I, the WRS test reveals a p-value of 0.87 and the KS test reveals a p-value of 0.66; for the comparison between C and P_{NI}, the corresponding p-values are 0.56 and 0.54). This result is not altered if we consider session averages as independent observations (p-values equal 0.20 and 0.10 for C vs. P_I and C vs. P_{NI}, respectively; WRS tests).

The analysis presented here suggests that eliciting plans does not affect behavior. Yet, there appears to be a divergence between planned and final contributions. In the next sections, we will examine whether a joint evaluation of choices and stated beliefs can account for such divergence.

4. Distinguishing contribution motives: the mixture model

The following empirical analysis focuses on the interaction of choices and beliefs in revealing individual cooperative decisions. It is based on the assumption that there are different types of individuals in the population and that each type is defined by a rule that describes his decision process. Each behavioral rule has a peculiar content in terms of preferences and beliefs. We consider selfish agents and non-selfish agents, concentrating our attention, in particular, on unconditional cooperators (which we call altruists) and conditional cooperators. Our mixture assumption states that each player is of one of these three types and that he cannot switch type between periods.

In order to distinguish between types, we have to define the rule that each type uses to decide on final and planned contributions. Let us assume that i's final contribution, $c_{i,t}$, and i's planned contribution, $p_{i,t}^{t+1}$, at time t are empirical realizations of the random variable $Y_{i,t}$. Note that we assume that, although players cannot change type across periods, they can behave according to one type's rule when choosing their final contributions and according to another type's rule when making their contribution plans. In other words, the random variable that generates final contributions and planned contributions is not necessarily unique.

Let us now proceed by defining the behavioral rule of each type. The first type we consider is the *selfish* type (SEL). This individual is only interested in maximizing his own monetary payoff. Since the marginal per capita return in payoff functions (1) and (2) is less than unity, the dominant strategy for such a type is to contribute and plan to contribute nothing. Hence, the behavior of a selfish player is described by the following equation:

$$Y_{i,t} = 0 \quad \forall t.$$

We define an *altruist* (ALT) as an unconditional cooperator who always contributes a fixed positive amount. An altruistic agent is expected to behave according to the following rule:

$$Y_{i,t} = m_i \quad \forall t, \qquad m_i > 0.$$

Following Bardsley and Moffatt (2007), we take m_i to equal alternatively the median of i's 15 final contributions and the median of i's 14 planned contributions.

A *conditional cooperator* (CC) dislikes contributing different amounts than others (Fischbacher et al., 2001; Fischbacher and Gächter, 2010). His behavior is described by:

$$Y_{i,t} = Y_{j,t} \quad \forall t.$$

Since player i is unaware of $Y_{j,t}$ when deciding on his own contributions, i's conditional choices can only be based on his first-order beliefs about j's final (or planned) contributions. We assume that conditional cooperator i contributes an amount equal to his partner's most likely contribution, namely an amount equal to the mode of the distribution of first-order beliefs. If this distribution is multimodal, we assume that conditional cooperator i chooses an alternative corresponding to any of the modes. More formally, $Y_{i,t} \in \text{mode}\left(\mathbf{b}_{i,t}^{t+\tau}\right)$ $\forall t$, with $\tau \in \{0, 1\}$.[13] This process of conditioning on beliefs together with repeated

[13]To characterize the behavior of conditional cooperators, we could have used either a utility function *à la* Fehr and Smith (1999) or a different rule for $Y_{j,t}$ like, e.g., the final (one-period-ahead) expected contribution. We opted for our simple rule for three reasons: (a) finding the functional form that fits the data best is not one of the objectives of this paper; (b) we wanted for the conditional cooperator type a behavioral rule as straightforward as the one used for the other two types; (c) finally, but most importantly,

observations per subject allow us to distinguish conditional cooperators from altruistic and selfish types.[14]

We do not expect a player to faithfully comply with what is dictated by the behavioral rule corresponding to his type. As argued by, e.g., Andreoni (1995), Palfrey and Prisbrey (1996, 1997), Anderson et al. (1998), and Houser and Kurzban (2002), subjects may be confused and make mistakes. We allow for the possibility of sub-optimal behavior by introducing a tremble, $w_{i,t} \in [0,1]$.[15] This represents the probability that player i – whatever the reason – chooses completely at random between the alternatives. We also assume that each player is characterized by an individual-specific probability of trembling, and that the tremble probability $w_{i,t}$ is distributed $Beta\left(\beta_t, \gamma_t\right)$ over the population. The Beta distribution is the most natural candidate to represent the distribution of probabilities that are framed within the interval $[0,1]$. It is a pretty flexible distribution whose shape is determined by two parameters that we allow to depend on time, namely $\beta_t > 0$ and $\gamma_t > 0$. We assume the following simple functional forms: $\beta_t = \exp\left(b_0 + b_1\left(t-1\right)\right)$ and $\gamma_t = \exp\left(g_0 + g_1\left(t-1\right)\right)$.[16] With this hypothesis we want to capture the possibility that players *learn* during the game such that they are more firm in their decisions and tremble less toward the end. Hence, we expect to see the Beta distribution more and more concentrated toward zero as the game goes by.

Recall that the indicator function $\mathbb{1}\left(\cdot\right)$ takes the value 1 if the statement into brackets holds and 0 otherwise. Let T_s, $s \in \{final, planned\}$, indicate the last period, that is $T_{final} = 15$ and $T_{planned} = 14$, and let $f\left(w;\beta,\gamma\right)$ represent the Beta density function. Given our assumptions, we can now define the individual likelihood contribution for each

our data analysis suggests that about 50% of the contributions, both final and planned, comply with such a rule.

[14] Nevertheless, identification fails to achieve in the following cases: when one of the modes of i's distribution of beliefs always corresponds to $a = 0$ and i always chooses to contribute 0 (in this case, a conditional cooperator is indistinguishable from a selfish subject); when one of the modes of i's distribution of beliefs always corresponds to the median of i's contributions and i always chooses to contribute exactly that amount (in that case, a conditional cooperator is indistinguishable from an altruist); when subjects change preferences over time.

[15] See Moffatt and Peters (2001) and Loomes (2005).

[16] To our knowledge, this is the first time a tremble is estimated under such assumptions. We want the tremble probability to be individual-specific because this allows us to capture different kinds of behavior. There can be subjects who stick with their rule in all periods, subjects whose decisions are noisy at the beginning but not toward the end, and vice versa. Finally, there can be subjects whose decisions are extremely noisy throughout the entire game.

subject type. For a *selfish* player, the likelihood contribution is:

$$\begin{aligned} l_i^{SEL} &= Prob\,(Y_{i,1} = 0,\ \ldots,\ Y_{i,T_s} = 0 | i = SEL) \\ &= \int_0^1 \prod_{t=1}^{T_s} \left\{ (1 - w_{i,t}) \times \mathbb{1}\,(Y_{i,t} = 0) + \frac{w_{i,t}}{11} \right\} f\,(w; \beta, \gamma)\, dw. \end{aligned} \tag{4}$$

For an *altruistic* player, the individual contribution to the likelihood is:

$$\begin{aligned} l_i^{ALT} &= Prob\,(Y_{i,1} = m_i,\ \ldots,\ Y_{i,T_s} = m_i | i = ALT) \\ &= \int_0^1 \prod_{t=1}^{T_s} \left\{ (1 - w_{i,t}) \times \mathbb{1}\,(Y_{i,t} = m_i) + \frac{w_{i,t}}{11} \right\} f\,(w; \beta, \gamma)\, dw. \end{aligned} \tag{5}$$

Finally, the likelihood contribution for a *conditional cooperator* is:

$$\begin{aligned} l_i^{CC} &= Prob\left(Y_{i,1} = mode\left(\mathbf{b}_{i,1}^{t+\tau}\right),\ \ldots,\ Y_{i,T_s} = mode\left(\mathbf{b}_{i,T_s}^{t+\tau}\right) | i = CC\right) \\ &= \int_0^1 \prod_{t=1}^{T_s} \left\{ (1 - w_{i,t}) \times \mathbb{1}\left(Y_{i,t} = mode\left(\mathbf{b}_{i,t}^{t+\tau}\right)\right) + \frac{w_{i,t}}{11} \right\} f\,(w; \beta, \gamma)\, dw. \end{aligned} \tag{6}$$

The use of a mixture approach is suggested by the observation that different individuals may behave differently in a public goods game. We then proceed by assuming that a proportion π_{SEL} of the population from which the experimental sample is drawn behaves selfishly; a proportion π_{ALT} shows altruistic attitudes; and finally a proportion $\pi_{CC} = 1 - \pi_{SEL} - \pi_{ALT}$ behaves conditionally cooperative. Accordingly, the likelihood contribution of player i is:

$$L_i = \pi_{SEL} \times l_i^{SEL} + \pi_{ALT} \times l_i^{ALT} + \pi_{CC} \times l_i^{CC}.$$

The full sample log-likelihood for the set I of individuals is given by:

$$LogL\,(\beta, \gamma, \pi_{SEL}, \pi_{ALT}, \pi_{CC}) = \sum_{i \in I} logL_i. \tag{7}$$

The model is estimated using data (choices and beliefs) from each treatment separately, discriminating between final contribution data and planned contribution data. Our samples I consist of 150 subjects for each of the two treatments P_{NI} and P_I, where each subject's final (planned) contribution is observed 15(14) times. To estimate the model, we use the method of Maximum Simulated Likelihood. Integration over w (equations (4), (5) and (6)) is performed by simulation using two sequences of 100 Halton draws per subject.[17]

A post-estimation step makes use of Bayes' rule in order to calculate the posterior probability of each individual of being of a certain type. For subject i, the posterior probability of being of type $k \in \{SEL, ALT, CC\}$ is computed as follows:

$$\begin{aligned} Pr\left[i = \text{type } k \mid \text{obs}_i\right] &= \frac{Pr\left[i = \text{type } k\right] \times Pr\left[\text{obs}_i \mid i = \text{type } k\right]}{Pr\left[\text{obs}_i\right]} \\ &= \frac{\lambda_k \times Pr\left[\text{obs}_i \mid i = \text{type } k\right]}{Pr\left[\text{obs}_i\right]} = \frac{\pi_k \times l_i^k}{L_i}, \end{aligned} \tag{8}$$

where obs_i represents the observations collected from i (either final contribution data or plan data and including both choices and stated beliefs), and l_i^k is the component of the likelihood function resulting from type k's behavior, alternatively defined by (4), (5), and (6). In practice, π_k, l_i^k and L_i are replaced by their counterparts evaluated at the parameter estimates obtained by maximizing Eq. (7). Subjects are assigned to types according to the highest posterior probability.

We also use this rule to compute the posterior probability of the prediction of final contributions from plan data estimates. For this purpose, we proceed similarly to Conte and Hey (2011): we use plan data to estimate the mixture model; we calculate the likelihood of the prediction from final contribution data using the parameter estimates obtained from plan data; we finally compute, by Eq. (8), the posterior probability for each subject of being of each type from the likelihood of the prediction and assign subjects to types according to the highest posterior probability of the prediction.

5. Results

Here we present and discuss the estimates of the mixture model defined in the previous section and their implications so as to address our main research questions, namely whether plans convey accurate information about people's preferences and final behavior, and whether this depends on the amount of information provided. In Section 5.1 we present mixture model estimates that account for as population level results. In Section 5.2, we test the consistency hypothesis at an individual level.

[17] Details can be found in Train (2003).

Table 1: Maximum likelihood estimates of the mixture model's parameters (the log-likelihoods are maximized using two sequences of 100 Halton draws).

	P_I		P_{NI}	
	final	planned	final	planned
π_{SEL}	0.216	0.164	0.316	0.294
	(0.040)	(0.036)	(0.048)	(0.051)
π_{ALT}	0.326	0.382	0.157	0.186
	(0.042)	(0.045)	(0.034)	(0.036)
π_{CC}	0.458	0.454	0.527	0.519
	(0.049)	(0.052)	(0.052)	(0.058)
$\beta_1 = exp(b_0)$	0.562	0.723	1.234	0.816
	(0.120)	(0.165)	(0.306)	(0.202)
$\gamma_1 = exp(g_0)$	0.459	0.535	1.416	0.807
	(0.102)	(0.133)	(0.394)	(0.208)
$\beta_{T_s} = exp\big(b_0 + b_1(T_s - 1)\big)$	0.636	0.331	0.155	0.142
	(0.139)	(0.068)	(0.035)	(0.032)
$\gamma_{T_s} = exp\big(g_0 + g_1(T_s - 1)\big)$	1.982	0.856	0.614	0.503
	(0.562)	(0.200)	(0.155)	(0.126)
I (number of subjects)	150	150	150	150
T_s (observations per subject)	15	14	15	14
LogL	-3207.34	-3193.81	-2945.63	-2842.28

5.1. The mixing proportions of types in the population

Table 1 displays the parameter estimates from the maximization of (7). The estimates of the mixing proportions (π_{SEL}, π_{ALT}, and π_{CC}) show that, under any treatment and sample used, the conditional cooperator is the most common type, representing about half of the population. The conditional cooperator type is followed by the selfish (altruistic) type under treatment P_{NI} (P_I); the estimated mixing proportion of selfish people ranges between 16% and 32%, and that of altruists between 16% and 38%.

Table 1 also shows estimates of the parameters in the distribution of the tremble probability, which – as explained in Section 4 – we assume to be distributed Beta. The table displays the estimated values of the two parameters characterizing the Beta distribution in the first and last periods of the game. Under each treatment, for both final and planned contributions, the effect of time is strongly significant.[18] Figure 5 shows the distributions of the tremble probabilities in the first and last periods of the game based on the estimates in Table 1. It is worth noting that, when the two parameters which characterize the Beta

[18] In unreported analysis, we estimate the four models in Table 1 without time effects (i.e., constraining b_1 and g_1 to equal zero). Likelihood-ratio tests strongly reject the null hypothesis of no time effects (in all cases the p-values of the tests are < 0.000). The regression results of these models are available from the authors upon request. We do not report the results here for two reasons: none of the conclusions concerning the main hypothesis under investigation changes when time effects are added to the mixture model; the models *with* time effects showed to be far superior on statistical grounds.

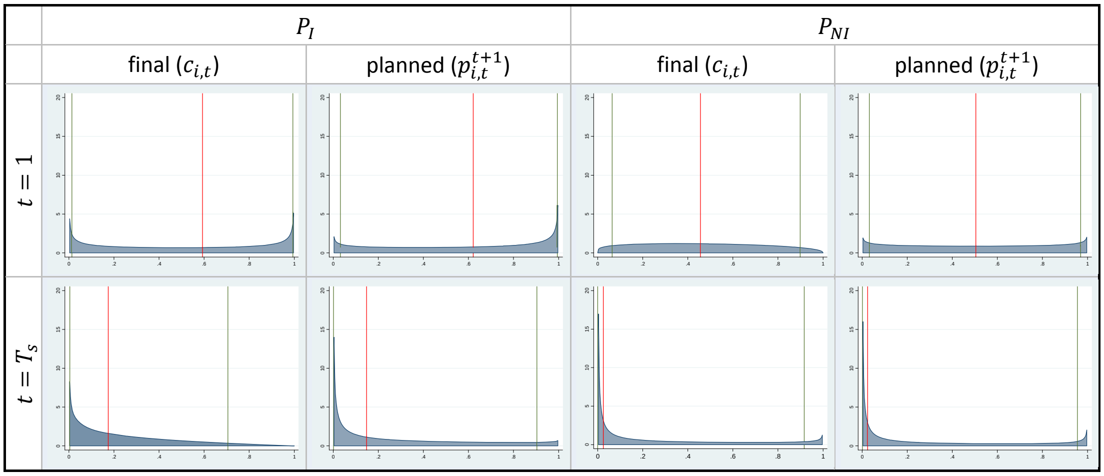


Figure 5: Distributions of tremble probability from the estimates reported in Table 1, separately for each sample and for the first and the last period. The red vertical line indicates the median of the distribution. The two green vertical lines on the left and on the right of the red line represent the 5$^{\text{th}}$ and the 95$^{\text{th}}$ percentile, respectively.

distribution equal one, the Beta distribution collapses to a Uniform distribution. We cannot reject the null hypothesis that the tremble probabilities are uniformly distributed in the first period of P_{NI} for both final and planned contributions. In the first period of P_I, instead, the distribution of the tremble, for both final and planned contributions, shows that more than half of the decisions are extremely noisy, being concentrated towards 1; the remainder, though, shows a rather small tremble probability.

Moving to the last period, the fact that the four bottom graphs in Figure 5 share a single pattern becomes immediately evident: all the distributions are concentrated towards zero. Such result suggests – in line with previous research – that players are rather confused about their type and the actions to take at the beginning, but they get used to the game period by period, thereby learning how to play.

A further conclusion we can get from model (7)'s estimates is that there is substantial heterogeneity across the population, confirmed by the statistically significant estimates of the mixing proportions of types and the parameters of the tremble that warrant a non-degenerate distribution. These results vindicate the reasons for choosing our approach.

The estimates reported in Table 1 allow us to verify whether there is any difference in subjects' behavior between planned and final contributions, having controlled for beliefs, which bear the information about the other's action acquired at the end of each period. The null hypothesis is that, conditional on beliefs, there is no difference between the mixing proportions estimated from planned contributions and those estimated from final contributions, against the alternative that the two distributions actually differ. The Wald tests for the null hypothesis result in a $\chi^2(2) = 1.39$ in the P_I case and in a $\chi^2(2) = 0.79$ in the P_{NI} case (the p-values of these tests equal, respectively, 0.500 and 0.675). This implies that we cannot reject the consistency hypothesis under both treatments, even if we might have reasonably expected a rejection under P_{NI}.

5.2. The consistency hypothesis at an individual level

The result discussed in the previous paragraph is quite strong, but it only holds at a population level. In actual facts, it does not exclude that individual players behave according to one type's rule when they plan and according to another type's rule in their final deed, in a sort of reshuffling-of-types process that keeps invariant the proportion of the population who are of each type. Therefore, to test the consistency hypothesis we must go a little further and verify whether it holds at an individual level as well by computing for each subjects posterior probabilities as described in Section 4. Once we have determined each subject's type , we can establish (a) whether plans are good predictors of final contributions, and (b) whether subjects are consistent, in the sense of not changing their type between plans and final contributions.

To address point (a), we compare subject's type from final contribution data to subject's type obtained by computing the posterior probability of the prediction of final contributions from plan data estimates. In both treatments, we achieve a 100% correspondence. This result is extremely strong and testifies that for evaluating subjects' behavior from contribution data we could have used indifferently parameters estimates either from final contribution data or from plan data, obtaining exactly the same results. We can conclude that plans are *indeed* good predictors of final contributions.

In order to address point (b), we compute for each subject posterior probabilities from both final contribution data and plan data. The posterior probabilities so obtained, are represented on 2-simplexes and shown in the graphs displayed in Figure 6. Each vertex of a simplex corresponds to one type. Each subject is indicated by a point in the simplex: the closer this point is to a vertex, the higher the subject's posterior probability of being of the type represented on that vertex. The size of the circles indicates the number of individuals concentrated in the same area of the simplex: the larger the circle, the higher the concentration of subjects in that area. We have rounded all posterior probabilities to the nearest 0.05, to create the graphs. The triangle inscribed in each simplex gives a measure of the strength of the model in assigning players to types. A player whose posterior

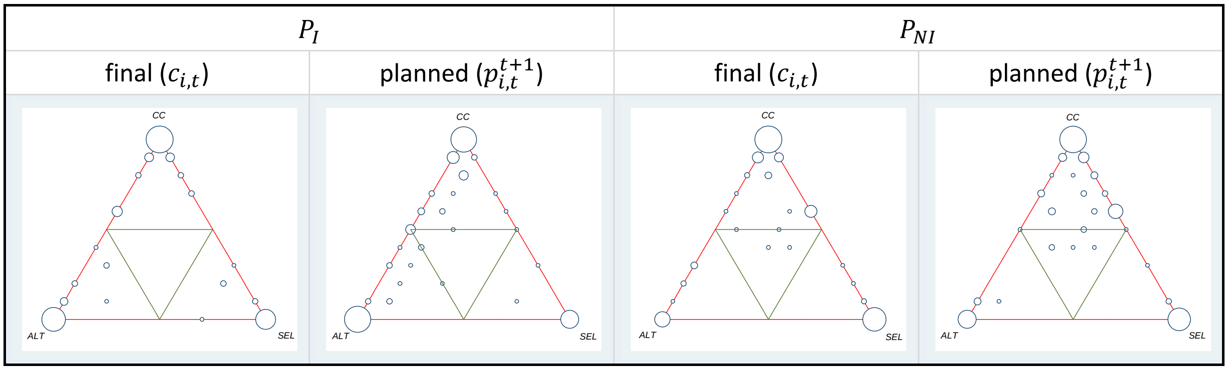


Figure 6: Posterior probabilities distribution of the three types from the estimates reported in Table 1.

Table 2: Two-way matrices of frequency counts classified according to the two categorical variables: type based on posterior probabilities from planned contribution data (rows) and type based on posterior probabilities from final contribution data (columns).

P_I

		final ($c_{i,t}$)			
		SEL	***ALT***	***CC***	Tot.
planned ($p_{i,t}^{t+1}$)	***SEL***	21	0	1	22
	ALT	7	37	10	54
	CC	4	10	60	74
	Tot.	32	47	71	150

P_{NI}

		final ($c_{i,t}$)			
		SEL	***ALT***	***CC***	Tot.
planned ($p_{i,t}^{t+1}$)	***SEL***	28	0	6	34
	ALT	3	14	7	24
	CC	11	6	69	86
	Tot.	42	20	82	144

probabilities result in a point situated within the inner triangle cannot be classified to any type with reasonable confidence; players located outside the inner triangle are assigned to a type with success. All the models appear highly successful at segregating subjects: most subjects are close to the vertices and very few fall in the center of the triangle. This result provides a further confirmation of the validity of our modeling approach.

In order to establish whether subjects change type between the planning and the contributing phase we resort to a cross tabulation of their types. The results are shown in a 3×3 matrix format in Table 2. Each cell reports the frequency counts of subjects who are of the type indicated by the corresponding row label when planning and of the type indicated by the corresponding column label when finally contributing. Obviously, subjects are assigned to types according to the posterior probabilities of types based on Table 1's estimates. To create the matrices of frequency counts subjects are only assigned to a type if their posterior probability of being of that type is larger than 0.5. If none of the posterior probabilities is larger than 0.5, he is not classifiable to any type, i.e., he is represented by a point in the inner triangle of the 2-simplexes.[19] The main diagonal of the two matrices shows the frequency counts of subjects assigned to the same type with both samples. Ideally, for the consistency hypothesis to be confirmed, all the frequency counts should be arrayed on the main diagonal.

We can see that the vast majority of subjects are, in fact, on the main diagonals (79% in P_I and 77% in P_{NI}). Respectively, 32 and 33 subjects change type between plans and final contributions in treatment P_I and treatment P_{NI}.[20] None of the subjects who are most likely to be selfish in their plans seems to be altruistic in their final contributions under both treatments. Instead, under P_{NI}, six subjects with selfish preferences when planning become conditional cooperators when finally contributing. This suggests that there exists a minority of selfish forward-looking individuals (4.17%) who try to manipulate the beliefs of the conditional cooperators by contributing positive amounts if observed. There are as well ten participants (6.94%) who, in P_{NI}, shift from being altruistic in their plans to being either selfish or conditionally cooperative in their actual contributions, thereby exhibiting sucker aversion.

The few changes in non-selfish preferences under P_I are more difficult to justify. They may be attributable to the incapacity of these subjects to plan ahead or to the presence of preferences that are more sophisticated than those allowed here. We speculate, however, that we are dealing with inconsistencies, and that these may be caused by the larger amount of information that participants in P_I must process. Receiving feedback about both the partner's final contribution and his planned-in-the-previous period contribution may have rendered some individuals more uncertain about the others' behavior.

To corroborate this conjecture, in Appendix A, we look at the accuracy of beliefs in both experimental treatments. As a matter of fact, such an analysis clearly demonstrates that the belief updating process is much slower in P_I than in P_{NI}. We argue that this phenomenon can be due to a problem of signal extraction: receiving feedbacks about the others' final and planned contributions might have made the signal concerning their behavior more difficult to read.

[19] For this reason, we exclude six subjects from treatment P_{NI}.

[20] Similar inconsistencies, referred to as 'instabilities of preferences', are found and discussed in Wilcox (2007) and Conte and Hey (2011).

6. Conclusions

In this paper, we include subjects' plans and beliefs about the opponents' future actions into the modeling of behavior in public goods games. Whereas there exists a large body of literature that has attempted to classify subjects according to their cooperative dispositions based on their choices in various games, to our knowledge no study has measured people's preferences toward cooperation on the basis of their plans and beliefs about others' future contributions. We believe that this issue demands attention because often people are required to plan contributions that will help to implement some public goods. For example, across Africa there has been a number of programs in which local residents have been involved in the prevention of parasitic diseases using techniques that are effective only if households make substantial 'contingent' contributions (e.g., Echessah et al., 1997; Kamuanga et al., 2001). In similar cases, having a good measure of the effective correspondence and the possible discrepancy between contribution plans and final contributions is crucial.

We concentrate on three preference types (selfishness, altruism, and conditional cooperation) and define for each type a simple behavioral rule. Notwithstanding its simplicity, our modeling approach is able to accommodate the behavior of almost all experimental subjects (only 6 out of 300 participants are not classifiable to type with high posterior probability). In particular, our model's estimates indicate that about half of the players exhibit some inclination toward conditional cooperation.

We specifically design our experimental treatments to address two main questions: (1) Do planned contributions convey accurate information about people's preferences and final behavior? (2) To what extent do final contributions differ from planned contributions when people have no incentive to misreport their plans? In addition, we test whether eliciting plans induces distortions in subjects' contribution decisions, and find that this is not the case.

Concerning the "consistency" hypothesis, the evidence is clear-cut: plans are predictors of future behavior if, following Manski (1990), we control for beliefs, which capture the effect of the new information acquired in the interim period between elicitation of plans and final decisions. Such a result holds both at the population level and at the individual level. We detect in fact consistency of preferences for nearly 80% of our subjects. This finding stands against the existing empirical evidence which has frequently observed divergence between stated intentions and subsequent behavior. The reason for this divergence may lie in the fact that the former studies simply compare plans and final choices, discounting events that may occur between the time plans are elicited and the time behavior is determined. Barkan and Busemeyer (1999, p. 547), for instance, acknowledge that the inconsistencies they observe could be due to "the effect of actual experience on the reference point used for the evaluation of the decision problem", experience that the authors disregard.

Turning to the extent to which final contributions differ from planned contributions when people have no incentive to misreport their plans, our results show that (a) there is a small number of participants (4.2%) who try to manipulate the others' beliefs when their contributions are observed, and (b) there are as well participants (6.9%) who withdraw observable contributions for fear of being played for a sucker.

A further interesting result is that participants in the Plan-Info treatment, who are informed at the end of each period about both the final and the planned-in-the-previous period contribution decisions of the person they are currently matched with, are more confused about the distribution of types in the population compared to participants in the Plan-NoInfo treatment, who receive periodical feedback only on final contribution decisions. We find, indeed, that stated beliefs are less accurate in the Plan-Info treatment. This indicates, in line with previous work on cognitive limitations (Costa-Gomes and Crawford, 2006; Gabaix et al., 2006) and information overload (Edmunds and Morris, 2000), that too much information can actually backfire.

Overall, our data corroborate past findings, but we go further because we show that people's stated plans are good predictors of subsequent behavior if one controls for events not yet realized at the time in which plans were elicited. The ultimate lesson is that researchers can and should expect much from intentions data, if they treat them appropriately.

Appendix A: Estimating expected beliefs

Here, we describe the rationale behind the econometric approach we use to estimate (from belief data) the expected probability that each alternative in $\mathcal{A}$ is being chosen – referred to as "expected beliefs". In this sense, it can be helpful to picture a subject with an urn containing colored balls. Each color corresponds to an alternative. The composition of the urn reflects the subject's beliefs concerning his partner's actions. When asked to report his beliefs, we can imagine that this subject draws 100 balls from his urn with replacement and reports the number of times each color/alternative has been drawn. At the end of each period, the composition of the urn is updated as a consequence of the new acquired information about the other's action. We allow each subject to be characterized by his own urn. More technically, the composition of any of these urns can be interpreted as a point on a 10-simplex. Each urn is located in a different point of the simplex that reflects its composition, i.e., player's beliefs.

We analyze data on beliefs with this picture in mind, and, consequently, we estimate the distribution of the different points (one for each player) on the 10-simplex. The natural choice to model a distribution over a simplex or the composition of an urn from which we observe several draws per subject (100 in the specific) is the Dirichlet-multinomial distribution. Hausman et al. (1984) and Guimarães and Lindrooth (2007) provide a full description of the distribution and the model estimated here. This distribution is characterized by the 11-dimensional vector of parameters $\boldsymbol{\lambda} \equiv (\lambda_0, \ldots, \lambda_a, \ldots, \lambda_{10})$ and has the nice and useful property that, for each alternative, the expected probability of being selected (expected belief) is:

$$\mathrm{E}[b_a] = \frac{\lambda_a}{\sum_{\tilde{a}=0}^{10} \lambda_{\tilde{a}}}, \qquad \text{with} \quad a = 0, \ldots, 10.$$

We can think of $\mathrm{E}[b_a]$ as the composition of the average urn. We use this property to estimate, period by period, the vector of parameters $\boldsymbol{\lambda}$ and to calculate the expected beliefs.

Figure 7 shows – separately for the two treatments – bar graphs of the proportion of times each alternative in $\mathcal{A}$ has been chosen in periods 2 and 15, and superimposes on these bar graphs connected dots representing the expected probabilities that the others choose the corresponding alternative (as computed from belief data). In period 2, participants in both P_I and P_{NI} underestimate the probability that the others will choose extreme contributions (0 and 100). Inspection of the graphs for the last period demonstrates how the beliefs updating process is much slower in P_I than in P_{NI}. In fact, while beliefs in

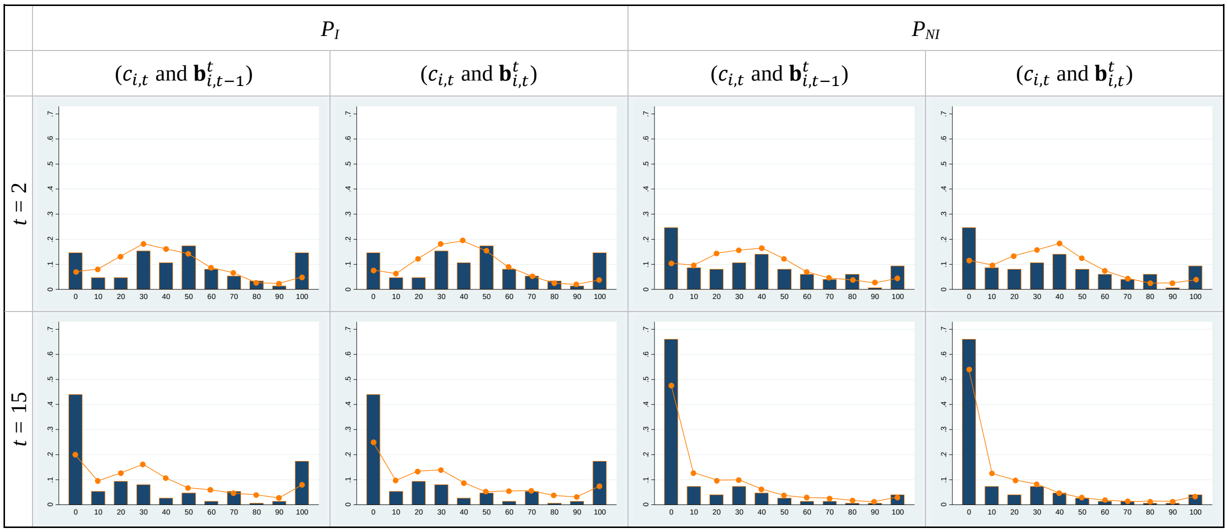


Figure 7: Contribution choices vs. expected beliefs. The bar height indicates the proportion of times the corresponding alternative has been chosen, calculated from final contribution data. The connected red dots represent the expected beliefs about that alternative.

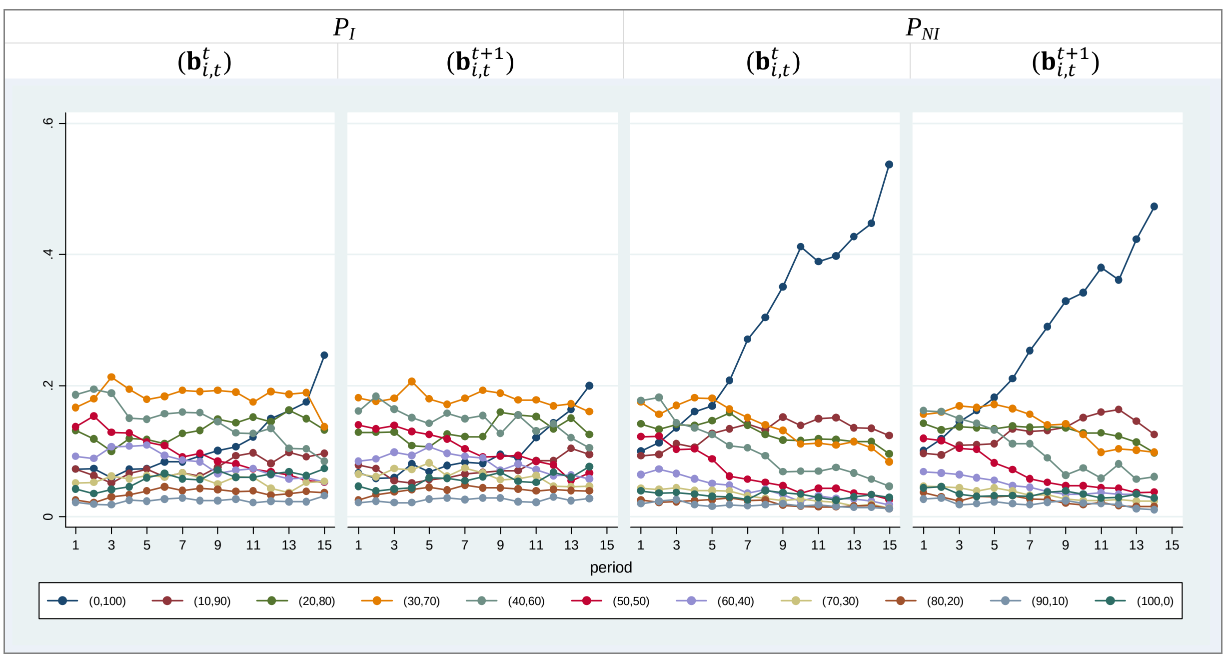


Figure 8: Time evolution of expected beliefs for each alternative. Different alternatives are represented by different colors. The legend with associations alternative-color is displayed at the bottom.

P_{NI} clearly incorporate the tendency of final contributions to move towards zero, beliefs in P_I are still unable to capture about 50% of the mass concentrated at the two extreme contributions.

The slower movements of beliefs in P_I can also be appreciated in Figure 8, displaying the time evolution of expected beliefs about current and future contributions separately for the two treatments. Each color represents an alternative and the associations color-alternative are listed in the legend located at the bottom. The two pairs of graphs look quite different. In P_{NI}, there is a clear tendency for $\mathrm{E}[b_0]$ to grow almost steadily over time and for the other expected beliefs to reduce. Conversely, in P_I, the real take-off of $\mathrm{E}[b_0]$ occurs around period 10 and the other expected beliefs patterns do not show any clear modification.

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