



The effect of voluntary participation on cooperation



Daniele Nosenzo^{a,*}, Fabio Tufano^{b,**}

^a School of Economics, University of Nottingham, University Park, NG7 2RD, UK

^b School of Economics, University of Nottingham, UK

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ABSTRACT

We study the effects of voluntary participation on cooperation in collective action problems. Voluntary participation may foster cooperation through a mechanism of assortative selection of interaction partners based on false consensus bias, or through a mechanism whereby the decision to not participate can be used as a threat against free-riders. We examine the effectiveness of these mechanisms in a one-shot public goods experiment. Voluntary participation has a positive effect on provision only through the threat of non-participation. Assortative selection of interaction partners seems to play a minor role in our setting, whereas the threat of non-participation is a powerful force to discipline free-riding.

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1. Introduction

Collective action is at the heart of many activities of vital importance for human societies. However, many groups fall prey to free-riding incentives and struggle to foster and sustain cooperation (Olson, 1965; Dietz et al., 2003). Thus, the success of human cooperation relies on the effectiveness of mechanisms and institutions designed to restrain free-riding and promote cooperation (e.g., Rand and Nowak, 2013). A substantial amount of research has been devoted to the study of such mechanisms and institutions. Several authors have shown that cooperation can be sustained by (in)direct reciprocity and reputational spill-overs, if the prospects of future interactions are non-negligible (e.g., Roth and Murnighan, 1978; Dal Bó, 2005). Altruistic punishment and rewards have also been shown to effectively promote cooperation (e.g., Fehr and Gächter, 2000; Sefton et al., 2007). In this paper, we study an alternative mechanism to reciprocity, reputation, punishment and rewards: voluntary participation to collective action.

Voluntary participation is a nearly ubiquitous characteristic of real-world social interactions: in many naturally occurring environments individuals can freely decide whether or not to partner with others to engage in cooperative endeavors. Voluntary associations, collectives, community groups, and collaborative institutions are typical real-world examples of organizations facing collective action problems where agents have the freedom to join in, or opt out from, participating in the common endeavors. And indeed voluntary participation is sometimes invoked to explain the relative success of such groups

* Corresponding author.

** Corresponding author.

E-mail addresses: daniele.nosenzo@nottingham.ac.uk (D. Nosenzo), fabio.tufano@nottingham.ac.uk (F. Tufano).

in solving collective actions dilemmas (e.g., Lin, 1990, and the symposium articles featured in the *Journal of Comparative Economics* – vol. 17, issue 2– for a discussion of the role of voluntary participation in Chinese agricultural collectives in 1959–1961). However, while there is a consensus that voluntary participation may foster cooperation, little is known about the mechanisms underlying this effect. The aim of this study is to contribute filling this gap in the literature by investigating the potential mechanisms that may make voluntary participation conducive to cooperation.

We focus on two specific mechanisms that have received substantial attention especially in the theoretical literature on voluntary participation. On the one hand, some authors have argued that the positive effects of voluntary participation may operate through a mechanism of assortative matching: the fact that participation in groups is not forced but voluntary may trigger a process of self-selection into groups that favors the inclusion of cooperators and the exclusion of free-riders. The literature has proposed different ways in which this process of assortative matching may arise. For instance, some authors have suggested that cooperators may have observable characteristics (“green beards”) that distinguish them from free-riders (e.g., Frank, 1987, 1988; Amann and Yang, 1998). If individuals are free to decide whether or not to enter into partnerships with others, they may use these observable characteristics to avoid partnerships with free-riders and favor partnerships with cooperators. Other models have instead proposed that assortative matching may be the result of a “false consensus bias”, whereby individuals tend to project their own cooperative attitudes onto others (Ross et al., 1977).¹ If individuals suffer from a false consensus bias, cooperators will be more optimistic about the prospects of cooperation, and may thus be more likely than free-riders to join others in cooperative endeavors (e.g., Orbell and Dawes, 1991; Macy and Skvoretz, 1998).

On the other hand, another strand of literature emphasizes a different mechanism: when participation is voluntary individuals are free to walk away from either potential or existing partnerships, and this can be used as a sanctioning device to discipline free-riders. Again, the existing literature suggests different possible mechanisms whereby this may happen. In some models, the possibility to quit partnerships is beneficial to cooperation simply because it allows cooperators who are willing to walk away from their partners, to avoid repeated interactions with free-riders and to reap instead the benefits of repeated interactions with other like-minded cooperators (e.g., Aktipis, 2004). Other authors emphasize instead the fact that the dissolution of partnerships may impose costs on all parties involved (e.g., Fujiwara-Greve and Okuno-Fujiwara, 2009; Izquierdo et al., 2010; Schumacher, 2013; Izquierdo et al., 2014). This gives group members a means to resist exploitation by free-riders, as the threat of costly exits may discipline opportunistic behavior and prevent free-riding.

Despite the many theoretical arguments proposing that voluntary participation may foster cooperation, only a few empirical studies (reviewed below) have so far examined the effects of voluntary participation on cooperation. Moreover, none of these studies have compared the relative effectiveness of the mechanisms outlined above in promoting cooperation. In this paper, we present a one-shot, two-person public goods game experiment designed to disentangle the effects on cooperation of voluntary participation operating either through a mechanism of assortative matching based on false consensus bias or through a mechanism whereby the decision not to participate can be used as a threat against free-riding. To disentangle these effects, we contrast three treatments: a Baseline treatment, with forced participation in the public goods game; an Unconditional voluntary participation treatment (‘Unconditional treatment,’ henceforth) where at the beginning of the game players choose between an outside option payoff and participation in the public goods game; and a Conditional voluntary participation treatment (‘Conditional treatment,’ henceforth) where, after interacting in the public goods game and pre-committing to a contribution, players can opt out of the game and secure an outside option payoff.

The key design difference between the Unconditional and Conditional treatments hinges on the information available to players when deciding whether to participate in the game: in the Conditional treatment players can condition their participation decision on the pre-committed contributions (which will of course be implemented only if players do not opt out of the game); thus, voluntary participation can be used as a sanctioning device since players can (tacitly) threaten to walk away from potential partnerships with free-riders. In contrast, in the Unconditional treatment the participation decision is made unconditionally (i.e., without receiving any information about contribution decisions) and cannot thus be used as a sanctioning device. However, voluntary participation may still be beneficial if cooperative types self-select into the game more often than free-riders. In order to study the role of false consensus bias in triggering this assortative matching effect, in all treatments the public goods game was preceded by a sequential two-player prisoner's dilemma game which we use to measure subjects' cooperative types and optimism about cooperation.

Our results show that voluntary participation can have a strong, positive effect on cooperation. However, this positive effect is only observed in the Conditional treatment, whereas in the Unconditional treatment voluntary participation does not have an effect on cooperation. These findings point to the crucial relevance of the sanctioning mechanism in fostering cooperation in collective action situations. The effectiveness of this mechanism lies in the threat-value of voluntary participation: in our experiment subjects who decide not to participate do so mainly to retaliate against free-riders. Subjects seem to anticipate this effect, and this generally increases contributions. In contrast, the assortative matching mechanism does not have much bite in our experiment. This is not because our subjects are not affected by a false consensus bias: in fact, we observe a significant false consensus effect in our data. Rather, the assortative matching mechanism is unsuccessful because

¹ Several experimental studies have found evidence that individuals suffer from a false consensus bias across a variety of settings (e.g., Offerman et al., 1996; Selten and Ockenfels, 1998; Charness and Grosskopf, 2001; Heijden et al., 2007; Gächter et al., 2012; Blanco et al., 2014). For a discussion of whether the consensus effect is “truly” false, see Engelmann and Strobel (2000) and Engelmann and Strobel (2012).

in our experiment decisions to enter the public goods game do not seem to depend on subjects' cooperation types or their optimism about cooperation.

Our paper contributes to the study of mechanisms and institutions that can help groups overcome collective action dilemmas. We show that the institution of voluntary participation can substantially increase cooperation, and may thus be either an effective substitute or complement for other mechanisms based – for instance – on reciprocity, reputation, direct punishment, and rewards. As such, our paper directly contributes to the small experimental literature on the effects of voluntary participation in social dilemmas.² Ehrhart and Keser (1999) were among the first to study the effects of voluntary participation on public good provision. In their experiment, subjects are initially assigned to public good groups, but have then the opportunity to migrate to other groups, or create new ones. They observe a positive effect of voluntary participation on cooperation, in that contributions levels are above the theoretical prediction and closer to the socially efficient level. Hauk (2003) studies voluntary participation in repeated n -person prisoner's dilemma games where players can choose between playing the game and securing an outside option payoff, and finds a positive effect on cooperation. My and Chalvignac (2010) compare a standard repeated public goods game with a game with two stages: in the first stage subjects decide whether or not to participate in the game; in the second stage subjects who decided to participate choose a contribution level, whereas subjects who opted out receive an outside option payoff. They find a weak but positive effect of the opt-out option on contributions.³

A key difference between the studies mentioned above and our experiment is that previous studies typically entail repeated interactions between subjects, where the decisions to participate in a future round of interaction are made after having observed the outcome of previous interactions. This makes it difficult, if not impossible, to identify the distinctive roles of mechanisms based on assortative matching or sanction threats in promoting cooperation, as the two types of mechanisms are confounded in those experimental designs. In contrast, our experiment is inspired by the theoretical arguments discussed above that emphasize the different effects that these mechanisms may have on cooperation. Thus, our experiment is based on one-shot games where we can cleanly identify which mechanism drives the effects of voluntary participation on cooperation.

In this sense, our paper is most closely related to the experiments by Orbell and Dawes (1993), who study non-repeated prisoner's dilemma games with an unconditional participation option (subjects can decide whether to cooperate, defect, or not play the game), and Wilson and Wu (2017), who study infinitely repeated prisoner's dilemma games where players have an exit option to unilaterally and irrevocably terminate the relationship and secure an outside option payoff. However, Orbell and Dawes (1993) only focus on an assortative matching effect and Wilson and Wu (2017) only focus on sanctioning effects, whereas our experiment provides a unified framework where we can compare the relative effectiveness of either mechanism on cooperation. In line with our results, Wilson and Wu (2017) find that the presence of an exit option can substantially increase cooperation. Orbell and Dawes (1993) find that unconditional participation can also have a positive effect on cooperation and efficiency, which is in contrast to our finding. While the numerous differences between experimental designs make it difficult to precisely identify the source of this variation in experimental results, these discrepancies suggest that the specific details of the decision-making environment may play an important role in determining the effectiveness of participation. This may be an interesting issue for further research.

The paper progresses as follows. Section 2 introduces the experimental design and procedures. Section 3 presents the results and discusses the main findings. Section 4 concludes.

2. Experiment design and procedures

2.1. Experiment design

Our experiment is based on a one-shot version of the following two-person public goods game (PGG). At the beginning of the game, subjects are randomly matched into two-person groups. In each group, subjects receive an endowment of 20 tokens each, and simultaneously decide whether to allocate these tokens to either a private or a group account. Each token a subject allocates to the private account earns 3 points to that subject, whereas each token allocated to the group account earns 2 points to each of the two subjects in the group (in the experiment point earnings were converted to cash at a rate of 0.15 GBP per point). Thus, the game contains a tension between private and collective interests: group payoffs are maximized when subjects allocate their whole endowment to the group account, resulting in a payoff of 80 points per subject. However, the optimal decision of a self-interested individual is to allocate all tokens to the private account, resulting in an equilibrium payoff of 60 points per subject.

² Another, less closely related, literature entails the study of endogenous group formation in social dilemmas (e.g., Riedl and Ule, 2002; Cinyabuguma et al., 2005; Page et al., 2005; Ahn et al., 2008, 2009; Maier-Rigaud et al., 2010; Charness and Yang, 2014). The main focus of this literature, however, is on the effects of mechanisms, such as voting, partner-selection and ostracism that allow group members to regulate the participation of other individuals in their group. In contrast, the focus of our paper is on the decisions of the individual to self-select into or out of cooperative endeavors.

³ Voluntary participation has also been studied in the context of team production games (e.g., Keser and Montmarquette, 2011). Also related are the experiments by Cason et al. (2002) and Cason et al. (2004), who study a two-stage, non-linear public goods game where, in the first stage, subjects can commit to free-ride by announcing their non-participation in the game, and in the second stage subjects who have not committed to free-ride choose a contribution level.

We study different versions of this game across three between-subject treatments, where we vary whether and how subjects can voluntarily participate in the PGG. In a Baseline treatment there is no option of voluntary participation and subjects are required to participate in the PGG. In contrast, in our Unconditional and Conditional treatments subjects choose whether or not to play the PGG. The two treatments differ in whether subjects can express their voluntary participation before or after having obtained information about their opponent's contribution.

The Unconditional treatment is based on a two-stage game where, in the first stage, subjects simultaneously decide whether to participate in the PGG or take an outside option payoff of 61 points, that is just above the Nash equilibrium payoff of the underlying PGG (see Section 2.2 for further discussion of the theoretical implications of this design choice). In the second stage, subjects learn their opponent's participation decision. If both subjects have chosen to participate in the game, subjects play the one-shot PGG and are paid accordingly. If at least one subject in the group has chosen not to participate in the PGG, then both subjects receive the outside option payoff.⁴

The Conditional treatment is also based on a two-stage game, but the option of voluntary participation is available in the second stage. In the first stage, subjects make a simultaneous contribution decision in the PGG. In the second stage, subjects learn their opponent's contribution and simultaneously decide whether to confirm or withdraw their participation. If both subjects confirm their participation, subjects are paid according to the contribution decisions of stage one. If at least one subject withdraws participation, then both subjects receive the outside option payoff of 61 points.

We use the Conditional treatment to study the effect of the threat-value of voluntary participation on cooperation. As we discussed in the previous section, the theoretical literature suggests that the freedom to reject a partnership has a positive effect of cooperation because it gives cooperators a means to discipline free-riders: given that a rejection imposes costs on both those who reject the partnership and their potential partners, cooperators can use it as threat to restrain opportunistic behavior and encourage cooperation. In our experiment we capture the essence of this mechanism by specifying a relatively low outside option payoff, which make non-participation potentially costly. That is, if subjects decide to reject a partnership, this may lead to lower payoffs to both interacting parties, and thus players can use this as a threat to deter free-riding.

The Unconditional treatment allows us instead to study the effect of false consensus bias on cooperation. As discussed above, false consensus bias makes cooperators relatively more optimistic than free-riders about the prospects of interacting with a cooperator. If players are free to decide whether to participate in cooperative endeavors, false consensus bias may lead to a process of assortative matching whereby cooperators are more likely to self-select into the PGG than free-riders, with positive effects on cooperation.

To study the false consensus mechanism in more detail, one needs to (i) classify whether subjects are “cooperators” or “free-riders”, and (ii) measure their relative optimism about the prevalence of cooperators in their session. To do this, in all treatments the PGG was preceded by a one-shot version of the sequential prisoner's dilemma game (PD). At the beginning of the game, the first-mover chooses whether to cooperate or defect. If the first-mover defects the game ends and players receive a payoff of 50 points each. If the first-mover cooperates then the second-mover chooses between cooperation (where players receive 70 points each) and defection (where first- and second-movers receive respectively 35 and 85 points).⁵ In the experiment, subjects were randomly matched in pairs and made decisions in both roles. Subjects did not learn which role they were actually assigned to until the end of the experiment. Thus, both choices were elicited in an incentive-compatible way. Moreover, subjects were asked to submit a prediction about the number of other participants in the session who would cooperate in the role of second-mover. This prediction was incentivized and subjects received 10 points if their guess was correct.⁶

The PD game allows us to perform a more detailed test of the effect of false consensus bias on cooperation. First, based on their choices as second-mover, we can classify each subject as either a “cooperator” (if they cooperated) or “free-rider” (if they defected). Moreover, by eliciting subjects' beliefs about others' behavior in the role of second-mover, we can measure their estimate of the likelihood of meeting a cooperator, i.e. their “optimism” about cooperation.

2.2. Theoretical considerations

In the version of the PGG played in the Baseline treatment it is a dominant strategy for a rational and self-interested player to allocate all tokens to their private account. Thus, both players contribute zero tokens to the public good in the unique Nash equilibrium of the game, resulting in an equilibrium payoff of 60 points per subject.

Assuming common knowledge of rationality and self-interest, the option of voluntary participation does not lead to higher predicted contributions in the Unconditional treatment. To see this, consider the second stage of the Unconditional game. In the subgame where subjects make a contribution decision in the PGG, the optimal decision of a rational and self-interested

⁴ Note that in this sense the public good is not truly non-excludable in our setting, because a player's choice not to take part in the game prevents both players from enjoying the benefits of public good provision. We made this choice for simplicity, to avoid the complications of having to specify the structure of payoffs in case only one player commits to provide the public good.

⁵ Note that the structure of the game is similar to the binary trust game used by [Charness and Dufwenberg \(2006\)](#).

⁶ Since subjects were paid both for their choices in the PD game and their beliefs, a concern is that subjects may report distorted beliefs to hedge against their play in the game. [Blanco et al. \(2010\)](#), however, find no evidence of hedging in a sequential prisoner's dilemma game.

Table 1
Reduced Unconditional game.

PLAYER 1	PLAYER 2	
	Participate	Do not participate
Participate	60, 60	61, 61
Do not participate	61, 61	61, 61

player is to contribute zero tokens, yielding an equilibrium payoff of 60 points per player. By replacing this subgame with its subgame perfect equilibrium value, we obtain the following reduced game (Table 1).

The reduced game has multiple pure-strategy equilibria and in any equilibrium at least one subject refuses to take part in the PGG. Indeed, in the reduced game players have a weakly dominant strategy not to participate in the PGG and take the outside option payoff of 61 points, instead. Thus, if we refine the equilibrium set by requiring that players do not use weakly dominated strategies, only the equilibrium with mutual non-participation survives. Thus, the public good is not provided in equilibrium and each subject earns a payoff of 61 points.

In contrast, in the Conditional treatment voluntary participation can lead to small, positive contributions in equilibrium. To see this, consider the subgames following the contribution stage. In any subgame, mutual non-participation is always an equilibrium, leading to a payoff of 61 for each player. However, other pure-strategy equilibria may also arise, some of which involve mutual participation, depending on the contributions made in the contribution stage. Specifically, there are three cases. First, in some subgames mutual participation would lead to payoffs that are strictly higher than 61 for both players. In these subgames, participating in the PGG is a weakly dominant strategy (within the subgame) for both players. Using weak dominance as a refinement, we select the equilibrium with mutual participation in these subgames. Second, in other subgames, mutual participation would lead to payoffs that are either equal to 61 for both players, or strictly lower than 61 for at least one player. In these subgames, any equilibrium involves a payoff of 61 for each player. Finally, in a third class of subgames, mutual participation would lead to a payoff of 61 for one player, and a payoff higher than 61 for the other player. In these subgames, participating in the PGG is a weakly dominant strategy (within the subgame) for the player who receives a payoff higher than 61 from participation, while the other player is indifferent between participation and non-participation. Thus there are three pure-strategy equilibria, one involving mutual participation and two involving non-participation by at least one player. In these subgames we select the equilibrium involving mutual participation on the grounds that this equilibrium is strictly preferred by one player while the other player is indifferent between equilibria.⁷

By replacing each subgame with its subgame perfect equilibrium value, we obtain a reduced game with five pure-strategy equilibria. Three equilibria involve symmetric contributions to the public good of respectively zero, one and two tokens. In the remaining two equilibria one player contributes zero tokens and the other player contributes one token. Note that in the two asymmetric equilibria and in the symmetric equilibrium with zero contributions at least one player earns a PGG payoff lower than 61. Thus, in the corresponding subgame perfect equilibria of the extended game players withdraw participation from the PGG and the public good is not provided. In the remaining two symmetric equilibria, both players earn at least 61 points from the PGG and thus they confirm their participation in the PGG in the corresponding subgame perfect equilibria of the extended game.⁸

No other pure-strategy equilibria exists. To see this, note first that there cannot be equilibria with contribution profiles where players contribute more than 2 tokens. Following such contribution profiles two possible situations arise. First, if players' contributions are too different from each other, then participation leads to a payoff of 60 or lower for one player, thus triggering non-participation in the corresponding subgame and a payoff of 61 to both players. But then, at the contribution stage, players can deviate to contribution levels that lead to participation and thus increase payoffs. Second, if players' contributions are not too different from each other, then participation leads to a payoff of at least 61 for both players and players confirm their participation in the PGG. But then the usual incentives to free-ride exist and at the contribution stage (at least) the player who contributes more has an incentive to deviate to a lower contribution (as long as it does not trigger non-participation). Finally, note that there cannot be equilibria where one player contributes 2 tokens and the other contributes 1 or 0 tokens: in this case players withdraw participation in the corresponding subgame, leading to a payoff of 61. But then, at the contribution stage, the player that contributes 1 or 0 tokens has an incentive to increase her contribution to 2 tokens, thus ensuring a payoff of 62.

Overall this analysis shows that, according to standard predictions, voluntary participation has only a small positive effect on public good provision. Moreover, this positive effect obtains only when voluntary participation can be conditioned on the opponent's contribution decision.

These predictions may change if we relax the assumption that all individuals are self-interested and assume that at least some individuals have other-regarding preferences. In this case, positive contributions may emerge in equilibrium already

⁷ This has only minor implications for the set of subgame-perfect equilibria, as we discuss in the next footnote.

⁸ If in the subgames where mutual participation leads to a payoff of 61 for one player and a payoff higher than 61 for the other player we select the equilibrium with non-participation, the set of subgame-perfect equilibria changes slightly as there is now a sixth equilibrium involving symmetric contributions of three tokens to the public good.

in the Baseline treatment. To illustrate this point, assume that players have [Fehr and Schmidt \(1999\)](#) preferences so that player i 's utility is given by:

$$U_i = \pi_i - \alpha_i [\max \{ \pi_j - \pi_i, 0 \}] - \beta_i [\max \{ \pi_i - \pi_j, 0 \}]$$

for $i \neq j$, and where π_i is the player's material payoff from the game, the parameter α_i measures her aversion to disadvantageous payoff inequality, and the parameter β_i measures her aversion to advantageous payoff inequality. Fehr and Schmidt assume that $\beta_i \leq \alpha_i$ and $0 \leq \beta_i < 1$. As we show in the online appendix, if players have a sufficiently strong distaste for advantageous inequality ($\beta_i > 1/3$), then any symmetric contribution profile is an equilibrium already in the Baseline treatment.

In the presence of other-regarding players, positive contributions and provision of the public good can also occur when there is an unconditional participation option. To see this, consider first the case where players know each other's preferences. In the online appendix we show that if two sufficiently inequity averse players ($\beta_i > 1/3$) are matched together in the Unconditional treatment, then the only equilibrium in undominated strategies involves participation and symmetric positive contributions. However, if either player is not sufficiently inequity averse ($\beta_i < 1/3$), then the equilibrium involves non-participation and the public good is not provided. Thus, when players know each other's preferences, the prediction is that (sufficiently) other-regarding players participate and provide the public good, while players who are not other-regarding do not participate.

A similar mechanism of assortative matching may also take place if players do not know each other's preferences, but suffer from a false consensus bias. If players do not know the preferences of their opponent, they will base their decision of whether to participate in the PGG on their expectations about the proportions of other-regarding and self-interested players in the population. If players suffer from a false consensus bias, then other-regarding players are more optimistic than self-interested players about the chances of meeting another other-regarding player. Therefore, they may be more likely to sort themselves into the PGG than self-interested players, triggering a process of assortative matching that also leads to public good provision.⁹

Finally, the presence of other-regarding players may also strengthen the positive effects of conditional participation on contributions. In the online appendix we show that if players have a sufficiently strong distaste for disadvantageous inequity ($\alpha_i > 5.67$), then any symmetric positive contribution profile can be sustained in equilibrium, including the profile with full contributions.¹⁰ This is because players with other-regarding preferences may be willing to withdraw participation from the PGG even if this implies a sacrifice in their own material payoffs. Thus, threats that are not credible for self-interested players may become credible if players have other-regarding preferences, and this may further discipline free-riders.¹¹

2.3. Experiment procedures

The experiment was programmed in z-Tree ([Fischbacher, 2007](#)) and conducted at the University of Nottingham using students from a wide range of disciplines recruited through the online recruitment system ORSEE ([Greiner, 2015](#)). We conducted fourteen sessions: four sessions with a total of 68 subjects in each of Baseline and Conditional, and six sessions with a total of 92 subjects in Unconditional. We over recruited in the Unconditional treatment to account for potential attrition of subjects out of the PGG and we kept recruiting subjects until we obtained contribution decisions from 68 subjects in this treatment as well.

At the start of a session, subjects were randomly allocated to computer terminals and were given preliminary experimental instructions, which were read aloud (instructions are available in the online appendix). Subjects were informed that the experiment consisted of two parts, but they did not receive instructions for part two (the PGG) until everyone had completed part one (the PD game). Subjects were then given part-one instructions, which were again read aloud. These instructions were followed by a series of control questions aimed at testing subjects' understanding of the PD game. Part one began once all subjects had answered all questions correctly. Subjects were then randomly matched in pairs and played the one-shot PD game described above.

When everyone had completed part one, and without receiving feedback on the outcomes of the PD game, subjects were given part-two instructions, which were read aloud. Again, the instructions were followed by control questions to probe subjects' understanding of the game. Part two began when everyone had answered all questions correctly. Subjects were then randomly matched into new pairs and played one of the three versions of the PGG described above.

At the end of part two, subjects were informed of their earnings from the two parts of the experiment. For part one, in each pair one of the subjects was randomly assigned the role of first-mover and the other subject the role of second-mover. One of the two parts was then randomly selected for payment, and subjects were paid accordingly in private and in cash

⁹ See [Orbell and Dawes \(1991\)](#) for a model of selective cooperation when players suffer from false consensus bias.

¹⁰ This level of disadvantageous inequity aversion is high. [Blanco et al. \(2011\)](#), for instance, find that only 13% of subjects in their experiment have $\alpha_i \geq 4.5$. However, even for smaller levels of α_i , one can sustain positive contribution profiles in equilibrium where both players contribute more than 2 tokens (the highest contribution profile that can be sustained with self-interested players). See the online appendix for details.

¹¹ This is akin to rejection of low offers in ultimatum games (which the Conditional treatment indeed resembles). There, the average rejection rate is about 16 percent ([Oosterbeek et al., 2004](#)), with low offers being rejected more readily (about half of the time, see [Camerer, 2003](#)) than higher offers.

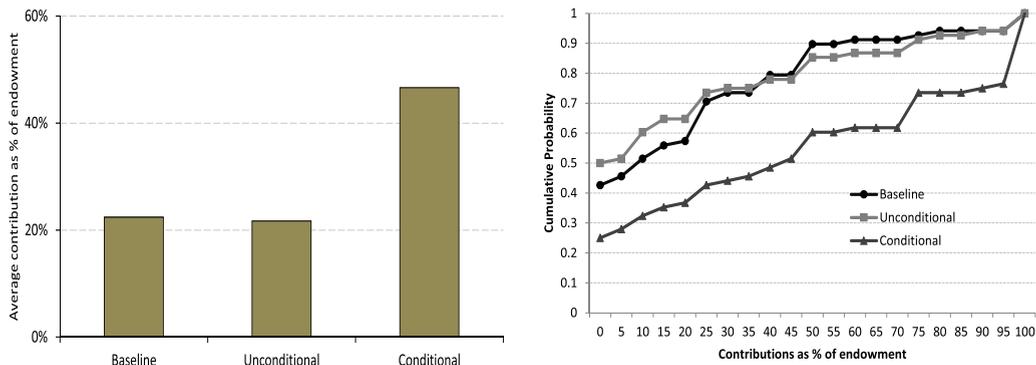


Fig. 1. Contributions to public good across treatments.

while they were completing a short post-experimental questionnaire, where we elicited standard socio-demographic and attitudinal information. The questionnaire included a self-assessment of subjects' risk and trust attitudes. Risk attitudes were elicited using the German Socio-Economic Panel (SOEP) general risk question discussed in [Dohmen et al. \(2011\)](#), and trust attitudes were elicited using the World Values Survey (WVS) Trust question.¹² Sessions lasted approximately 60 min and earnings averaged GBP 9.13.

3. Results

3.1. Contribution behavior across treatments

[Fig. 1](#) summarizes the main result of the experiment. The left panel of the figure shows the average contributions to the public good as percentage of endowment in our three treatments. In the right panel of the figure we plot, for each treatment, the cumulative distribution function (CDF) of contributions as percentage of the subjects' initial endowment.

On average in Baseline subjects contribute about 22% of their endowment to the public good. The right panel of [Fig. 1](#) shows that a substantial fraction of subjects (43%) act as complete free-riders and make no contributions to the public good. Only 10% of subjects contribute at least half of their endowment, and only 6% contribute the entire endowment. The average contribution rate in Baseline is similar to those reported in the one-shot public goods game experiments by [Cubitt et al. \(2011\)](#), [Dufwenberg et al. \(2011\)](#) and [Fischbacher et al. \(2012\)](#), who also find average contributions in the 20%–30% range. Other one-shot experiments report average contributions in the 35%–45% range ([Gächter et al., 2004](#); [Gächter and Herrmann, 2009](#)), while [Walker and Halloran \(2004\)](#) find an average contribution rate of 53%. Overall, these results show that the prospects for cooperation among strangers can be dismal in the absence of mechanisms that can discipline free-riding. The question is whether voluntary participation can be used as such a mechanism to improve cooperation.

[Fig. 1](#) suggests that the answer is positive, although the effectiveness of voluntary participation crucially depends on whether the participation decision can be conditioned on the opponent's contributions. In particular, the Conditional treatment produces a more than two-fold increase in contributions relative to Baseline: subjects contribute on average 47% of their endowment. Note that this statistic is computed using subjects' first stage contribution decisions, regardless of whether these were confirmed or withdrawn in the second stage and thus exclude any potential effects of selection (e.g., if the high-contributing groups may be more likely to agree to play the PGG). In the analysis of [Section 3.4](#) we will explore selection effects in more detail.

Inspection of the right panel of [Fig. 1](#) reveals that conditional participation has a dramatic impact on the distribution of contributions: the percentage of free-riders drops to 25%; 40% of subjects contribute at least half of their endowment; and 24% contribute their full endowment to the public good. We use Mann-Whitney and Kolmogorov-Smirnov tests to formally compare contributions between the Conditional and Baseline treatments. In both cases the treatment differences are highly significant (MW: $z = 3.475$, $p = 0.001$, two-tailed; KS: $D = 0.309$, $p = 0.003$, two-tailed; $n = 68$ per treatment).

In contrast, voluntary participation has only a negligible impact on cooperation when the participation decision is unconditional. Average contributions in the Unconditional treatment are 22% of endowment, exactly as in Baseline. The right panel of [Fig. 1](#) reveals that unconditional participation has two contrasting effects on contribution behavior. On the one hand, it somewhat increases the share of high contributions relative to Baseline (15% of subjects in Unconditional contribute at

¹² The SOEP question reads: "Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?", and subjects answered on a Likert scale ranging from 0 ("risk averse") to 10 ("fully prepared to take risk"). The average response to the SOEP risk question was 5.64 (s.d. 2.12). The WVS Trust question reads: "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?", to which subjects replied either by saying that they believe that "most people can be trusted" or that one needs "to be very careful in dealing with people". Responses to the WVS Trust question reveal that about 41% of our subjects believe that "most people can be trusted".

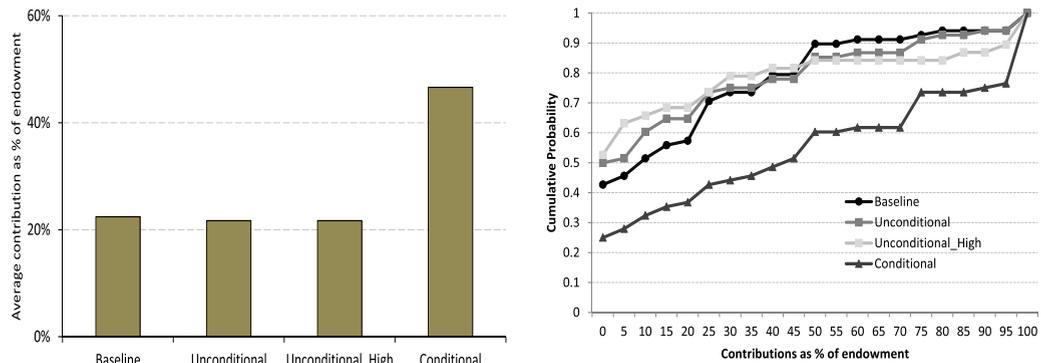


Fig. 2. Contributions to public good in Unconditional.High.

least half of their endowment). On the other hand, it seems to encourage free-riding: in Unconditional half of subjects contribute zero tokens to the public good. However, these effects are small. In fact, we cannot detect statistically significant differences between Unconditional and Baseline using Mann-Whitney and Kolmogorov-Smirnov tests (MW: $z = 0.641$, $p = 0.522$, two-tailed; KS: $D = 0.088$, $p = 0.954$, two-tailed; $n = 68$ per treatment). Contributions are instead significantly different between Unconditional and Conditional, both using Mann-Whitney and Kolmogorov-Smirnov tests (MW: $z = 3.800$, $p < 0.001$, two-tailed; KS: $D = 0.309$, $p = 0.003$, two-tailed; $n = 68$ per treatment).

3.2. A robustness treatment: Unconditional.High

Overall, our results suggest that voluntary participation can promote cooperation and have a positive effect on public good provision. However, this positive effect is observed only when participation can be conditioned on the opponent's contribution decision. Unconditional participation does not seem to have bite in our setting as cooperation levels when voluntary participation is available are very similar to the case of forced participation.

A potential reason why unconditional participation performs poorly in our setting is that the value we chose for the outside option payoff is too low to trigger a mechanism of assortative matching based on beliefs of cooperation. That is, when the value of the outside option payoff is low, even a pessimistic free-rider, who attaches a low probability to the event of meeting a cooperator, may find it profitable to self-select into cooperative groups. In fact, *prima facie* our data seem to support this conjecture. We observe very little selection in the Unconditional treatment as 86% of our subjects chose to take part in the PGG. Moreover, as noted in the previous sub-section, unconditional participation seems to increase the fraction of both high contributors and free-riders.

To address this issue, we conducted an additional treatment with unconditional participation, *Unconditional.High*. This treatment is identical to the original Unconditional treatment except that the outside option payoff was set at 70 points, halfway between the Nash equilibrium payoff level and the joint-payoff maximizing level. We recruited an additional 154 subjects for the Unconditional.High treatment across eight sessions, using the same subject pool, recruitment and procedures as in the other treatments. We observe substantial selection in Unconditional.High: only 50% of the subjects decided to take part in the PGG, whereas the remaining subjects chose to secure the outside option payoff. Overall, in fifty-eight of the seventy-seven groups at least one subject did not take part in the PGG. Hence, in Unconditional.High we observe contribution decisions from 38 subjects in total.

Fig. 2 shows the average contribution level and the CDF of contributions in Unconditional.High. For ease of comparison, the figure also reproduces the average contribution levels and CDFs of the other three treatments, which were already shown in Fig. 1.

The substantially higher value of the outside option payoff does not improve the effectiveness of unconditional participation. As in Baseline and Unconditional, also in Unconditional.High subjects contribute on average about 22% of the endowment. Moreover, the right panel of Fig. 2 shows that the distribution of contributions in Unconditional.High overlap substantially with those of Baseline and Unconditional. The figure also reinforces the impression that unconditional participation has two contrasting effects on contributions as it seems to encourage both more free-riding and more cooperation relative to Baseline. The fraction of full free-riders in Unconditional.High is 53%. At the same time, 16% of subjects contribute at least half of their endowment to the public good, with 11% contributing the whole endowment. Overall, using Mann-Whitney and Kolmogorov-Smirnov tests, we do not find any significant differences between Unconditional.High and Baseline (MW: $z = 0.876$, $p = 0.381$, two-tailed; KS: $D = 0.176$, $p = 0.439$, two-tailed; $n = 68$ in Baseline, $n = 38$ in Unconditional.High), or between Unconditional.High and Unconditional (MW: $z = 0.283$, $p = 0.777$, two-tailed; KS: $D = 0.117$, $p = 0.893$, two-tailed; $n = 68$ in Unconditional, $n = 38$ in Unconditional.High). Contributions are instead significantly different between Unconditional.High and Conditional (MW: $z = 3.259$, $p = 0.001$, two-tailed; KS: $D = 0.352$, $p = 0.005$, two-tailed; $n = 68$ in Conditional, $n = 38$ in Unconditional.High).

Table 2
Logit regressions of participation decisions – Unconditional treatments.

	I	II	III
Cooperator in PD game	0.003 (0.066)	– –	–0.024 (0.069)
Optimism in PD game	–	0.167 (0.117)	0.179 (0.121)
1 if male	0.037 (0.064)	0.029 (0.065)	0.027 (0.065)
1 if studies Economics	0.039 (0.080)	0.060 (0.080)	0.059 (0.080)
SOEP risk loving	0.066*** (0.015)	0.066*** (0.015)	0.066*** (0.015)
WVS high trust	0.047 (0.064)	0.036 (0.065)	0.036 (0.065)
N	246	246	246

Note: Marginal effects of logit regressions, standard errors in parentheses. In all models the dependent variable assumes value 1 if a subject chose to participate in the PGG and 0 otherwise. Significance levels: *** 1%.

Overall, these data point to the limits of a mechanism of assortative matching based on different beliefs of cooperation between cooperators and free-riders. In our original Unconditional treatment we observe a negligible effect of voluntary participation on cooperation, but we also observe limited evidence of sorting into the game. In contrast, we observe substantial selection in the Unconditional.High treatment. Yet, we again find no effect of voluntary participation on cooperation. In the next sub-section we look further into the reasons for the failure of unconditional participation as well as the reasons for the success of the conditional mechanism.

3.3. The determinants of conditional and unconditional participation

As argued above, a possible instance in which unconditional participation may affect cooperation through an assortative matching mechanism hinges on individuals suffering from a false consensus bias whereby cooperators are more optimistic than free-riders about the likelihood of meeting another cooperator. If this bias is sufficiently strong, and if subjects act on these biased beliefs when they decide whether to participate in the PGG, cooperators will be more likely than free-riders to self-select into the PGG. Within this framework, the limited effects of unconditional participation on cooperation may be explained by two different reasons. First, subjects in our experiment may not suffer from a false consensus bias. Alternatively, they could display a false consensus bias, but they may fail to act on their biased beliefs when deciding to enter the PGG.

Our data from the PD game allows us to explore these alternative explanations. We use choices in the role of second-mover in the PD game to classify subjects as “cooperators” (if they cooperated) or “free-riders” (if they defected). Across our four treatments, we classify 42% of our subjects as cooperators and 58% as free-riders.¹³ We use subjects’ beliefs about others’ behavior in the role of second-mover in the PD game to measure their optimism about the likelihood of interacting with a cooperator. On average, cooperators believe that 48% of the other subjects are also cooperators. In contrast, free-riders believe that only 31% of others are cooperators. This difference in beliefs is significant at the 1% level using a Mann-Whitney test ($z = 5.893$, $p < 0.001$, two-tailed; cooperators $n = 160$, free-riders $n = 222$).¹⁴ Thus, in line with the existing experimental evidence, our subjects display a false consensus bias.

We next use regression analysis to examine whether this bias in subjects’ optimism about the prospects of meeting a cooperator affects their decision to participate in the PGG. Table 2 reports marginal effects from logit regressions of participation decisions of subjects in the Unconditional and Unconditional.High treatments. In all models the dependent variable assumes value 1 if a subject chooses to participate in the PGG, and 0 otherwise. In model I we use as regressor a dummy variable assuming value 1 if a subject is classified as a cooperator in the PD game, and 0 otherwise. In model II we use instead our measurement of subjects’ optimism, defined as a subject’s estimate of the fraction of other participants in the session that act as cooperators in the PD game (on a scale from 0 to 1). Note that the implicit assumption here is that a subject’s beliefs about cooperation in the PD game are correlated with their beliefs about behavior in the PGG, which we did not elicit. In model III we include both the cooperator dummy and the optimism variable. In all models we also include a gender dummy (1 if subject is male), a dummy assuming value 1 if the subject studies Economics and 0 otherwise, and

¹³ The classification of types in the PD game correlates well with contribution behavior in the PGG. A regression of subjects’ contributions in the PGG on PD type and treatment dummies shows that cooperators contribute about 14% more than free-riders and the difference is significant at the 1% level.

¹⁴ This result holds also if we focus on the Unconditional and Unconditional.High treatments only. Here, cooperators and free-riders believe that, respectively, 47% and 31% of the others are cooperators, and the difference is significant at the 1% level using a Mann-Whitney test ($z = 4.321$, $p < 0.001$, two-tailed; cooperators $n = 94$, free-riders $n = 152$).

Table 3
Logit regressions of participation decisions – Conditional treatment.

	I	II	III
Subject <i>i</i> 's own contribution	–	–0.033** (0.016)	–
Subject <i>j</i> 's contribution	0.072*** (0.016)	0.070*** (0.020)	–
Difference between <i>i</i> 's and <i>j</i> 's contributions	–	–	–0.069*** (0.024)
Cooperator in PD game	0.020 (0.147)	0.091 (0.103)	0.104 (0.148)
1 if male	0.125 (0.164)	0.034 (0.120)	0.086 (0.154)
1 if studies Economics	0.074 (0.161)	0.098 (0.088)	0.242 (0.171)
SOEP risk loving	–0.067* (0.037)	–0.036 (0.034)	–0.064 (0.041)
WVS high trust	0.116 (0.161)	0.034 (0.111)	–0.052 (0.173)
N	68	68	68

measurements of subjects' self-assessment of their risk attitudes (the SOEP general risk question) and trust attitudes (the WVS Trust question).¹⁵

The regressions show that there is little evidence in our data of sorting based on cooperativeness or beliefs of cooperativeness. Subjects' cooperativeness, as captured by the cooperator dummy, has virtually no impact on the decision to participate in the PGG (model I). Subjects' degree of optimism has somewhat more success in explaining participation decisions, as shown in model II: an increase in optimism by 10 percentage points increase the likelihood to enter the PGG by about 2%, and the effect is insignificant ($p = 0.152$). In model III we include both cooperativeness and optimism as explanatory variables, but neither reaches statistical significance at the 10% level. In fact, the cooperator dummy enters with a *negative* sign in the regression, indicating that, controlling for their degree of optimism, cooperators are actually less likely to enter the PGG than free-riders.¹⁶ None of the other regressors are statistically significant in any of the models except the measurement of risk attitudes, which is positively related to the decision to participate.

Overall, this analysis shows that, while our subjects do display a false consensus bias, they seem not to act on their biased beliefs about others' cooperativeness when deciding to participate in the PGG. This explains the limited effectiveness of unconditional participation in our experiment.

We next turn to subjects' participation decisions in the Conditional treatment. First, we note that the decision to withdraw participation is made quite frequently in our experiment. In 76% of groups at least one subject decided to withdraw their participation from the PGG. This decision is predominantly taken when subjects contribute more than their opponent: when this happens, subjects choose to withdraw participation in 80% of cases. On the contrary, subjects confirm their participation in the PGG in 90% of cases where their contributions are the same or lower than that of their opponent. This suggests that withdrawal decisions are mainly used to retaliate against free-riding. In most of cases, withdrawals are also materially beneficial for the subject who makes that decision because the payoff of withdrawing participation exceeds that of participating in the PGG. However, we also have 7 cases where the payoff of non-participation is lower than that of participation, and in 2 of these (29%) the subject decided not to participate.

We analyze these patterns of withdrawal decisions more formally in Table 3, where we report marginal effects of a logit regression of participation decisions in the Conditional treatment. The dependent variable assumes value 1 if a subject chose to participate in the PGG, and 0 otherwise. We use three specifications. In model I, we include a variable measuring the amount contributed by the other subject in the group. In model II, we add a subject's own contribution as a regressor. Finally, in model III, we use a variable measuring the difference between subject *i*'s contribution and the contribution of the other subject *j*. All models also include the cooperator dummy and the control variables used in Table 2.

Note: Marginal effects of logit regressions, robust standard errors in parentheses, adjusted for intragroup correlation (PGG groups are used as independent clustering units). The dependent variable assumes value 1 if subject *i* chose to participate in the PGG and 0 otherwise. Significance levels: *** 1%; ** 5%; * 10%.

The regressions confirm that participation is strongly influenced by subjects' decisions in the contribution stage. Model I and II show that the likelihood of participating in the PGG is positively related to the other subject's contribution and negatively related to the subject's own contribution.¹⁷ Model III shows that this polarized impact of other's and own con-

¹⁵ Note, however, that the variables measuring risk and trust attitudes may not be entirely exogenous since they may to some extent reflect subjects' ex-post rationalizations of their behavior in the experiment (e.g., a subject who has self-selected into the PGG may be more likely to perceive him/herself as risk loving in the questionnaire).

¹⁶ We also ran an additional regression where we interacted the cooperator dummy with the optimism variable. The interaction term and the cooperator and optimism variables are all statistically insignificant in this regression.

¹⁷ This effect is robust to alternative specifications, e.g. if we drop the PD cooperator dummy (since it is correlated with own contributions in the PGG).

tributions on participation can be described as an effect of the differences in contributions: a one-token difference between contributions of the two players decreases the likelihood of participation by about 7%. The effect is statistically significant at the 1% level. Subjects' cooperativeness does not explain the decision to participate in the PGG. None of the other explanatory variables have a systematic effect on participation decisions.

3.4. Earnings across treatments

We conclude this section with an analysis of earnings across treatments. In Baseline subjects on average earn 64.49 points, somewhat more than the Nash equilibrium outcome of 60 points, but substantially less than the joint-payoff maximizing level of 80 points. Computing efficiency as attained earnings in excess of the zero-contribution Nash equilibrium earnings as a percentage of maximum possible gains, we find that in Baseline this is equal to $\frac{(64.49-60)}{(80-60)} = 22.5\%$. If we consider earnings conditional on both subjects in the group agreeing to participate in the game, we find that conditional participation has a substantial beneficial effect compared to Baseline. The average earnings of the subjects who agreed to play the PGG in the Conditional treatment are 76.38 points (an efficiency of 81.9%), and earnings in this treatment are significantly different from Baseline (Mann-Whitney test: $z = 4.032$, $p < 0.001$, two-tailed; $n = 34$ in Baseline, $n = 8$ in Conditional).¹⁸ Thus, conditional participation leads to a gain in efficiency of $81.9\% - 22.5\% = 59.4\%$ relative to Baseline. This efficiency gain reflects both a potential selection effect (the high-contributing groups may be more likely to agree to play the PGG) and the effect of the threat-value of voluntary participation. To disentangle the two, we also compute average earnings of all subjects based on their contributions in the PGG, regardless of their participation decisions, thus removing selection effects. We find that average earnings are 69.32 points (an efficiency of 46.6%), and earnings are significantly different from Baseline (Mann-Whitney test: $z = 3.479$, $p < 0.001$, two-tailed; $n = 34$ per treatment). This suggests that the threat-value of voluntary participation alone accounts for $46.6\% - 22.5\% = 24.1\%$ out of the 59.4% efficiency gain of Conditional relative to Baseline.

In the treatments with unconditional participation, average earnings (conditional on participation) are instead 64.34, both in Unconditional and Unconditional.High (an efficiency of 21.7%). In both cases, earnings are not significantly different from Baseline (Mann-Whitney tests: Baseline vs. Unconditional, $z = 0.179$, $p = 0.858$, two-tailed, $n = 34$ per treatment; Baseline vs. Unconditional.High, $z = 0.252$, $p = 0.801$, two-tailed, $n = 34$ in Baseline, $n = 19$ in Unconditional.High). These results confirm the relative advantage of conditional versus unconditional participation in our setting.

Of course, voluntary (non-)participation can imply substantial costs if subjects choose to forgo positive levels of public good provision and prefer to secure the outside option payoff instead. To examine this, we also compute *realized* earnings in the Conditional and Unconditional treatments as equal to subjects' PGG earnings if both subjects in the group agreed to participate in the game, and to the outside option payoff otherwise.¹⁹ Average realized earnings in the Conditional treatment are 64.62 points (an efficiency of 23.1%), and we do not find significant differences relative to Baseline (Mann-Whitney test: $z = 0.556$, $p = 0.578$, two-tailed; $n = 34$ per treatment). Thus, the frequent decisions to withdraw participation reduce the positive effects of voluntary participation. Average realized earnings in Unconditional are equal to 63.47 points (an efficiency of 17.4%). Again, we do not detect significant differences in earnings relative to Baseline (Mann-Whitney test: $z = 0.908$, $p = 0.364$, two-tailed; $n = 34$ in Baseline, $n = 46$ in Unconditional).

This latter set of results should be taken with caution as they are inevitably sensitive to the details of our experimental implementation, such as the specific value of the outside option payoff, or the fact that we implemented a one-shot version of the PGG rather than a repeated version of the game.²⁰ In practice (as well as in the theoretical models discussed in the Introduction) the value of participation is partly determined endogenously, and may depend, among other things, on the expected time horizon of the interaction and individuals' discount factor, and all this may affect the efficiency consequences of the conditional mechanism. Nevertheless, our findings point to a potential limitation of this mechanism. Subjects frequently withdraw participation when this option is available even when doing so reduces joint payoffs (this actually occurs in 75% of cases). As a consequence, when one accounts for the costs of withdrawals, realized earnings are not significantly different between settings with forced and voluntary participation.

4. Conclusions

Our study has shed light on the effects of voluntary participation on cooperation. In a one-shot two-person public goods game, we have found that allowing players to voluntarily take part in the game can have a beneficial effect on contributions relative to a setting where participation is forced. However, our study also shows that voluntary participation is most effective when subjects are let free to reject (potential) partnerships they are not satisfied with. This allows cooperators to shield

¹⁸ Note that tests on earnings are conducted at the pair level rather than at the subject level because subjects' earnings within a pair are not independent from each other.

¹⁹ In doing this we only focus on our three original treatments and we do not include Unconditional.High in the analysis. The artificially high value of the outside option payoff in this treatment implies that not participating in the PGG is unlikely to lead to efficiency losses.

²⁰ There is evidence that retaliation mechanisms that are socially inefficient in the short-run may nonetheless lead to long-run benefits (e.g., Gächter et al., 2008).

themselves from exploitation by free-riders, as the threat of costly rejections is powerful in disciplining free-riding and fostering higher contributions.

By contrast, voluntary participation does not foster cooperation when it operates through a mechanism of assortative matching driven by false consensus bias: if cooperators are more optimistic than free-riders about the prospects of meeting other cooperators, then voluntary participation may facilitate a process of self-selection whereby cooperators participate into the public goods game and free-riders stay out. In the experiment we do find evidence of a false consensus bias, but we also find that subjects do not seem to act upon their biased beliefs when they decide whether to participate in the public goods game. Thus, the participation decisions of cooperators and free-riders are not statistically distinguishable from each other. As a consequence, this mechanism is not successful in fostering public good provision.

While the specific assortative matching mechanism based on false consensus bias that we have studied here does not seem to be successful in fostering cooperation, this is not the only mechanism through which voluntary participation may trigger assortative matching, and alternative mechanisms may be more successful in fostering cooperation. For instance, [Aimone et al. \(2013\)](#) study a modified public goods game where groups are formed based on subjects' willingness to sacrifice returns from private investments. They find that cooperators are more willing to sacrifice private investments than free-riders, which leads to assortative matching and increased public good provision.

Moreover, in natural environments individuals may rely on communication and other observable characteristics about their potential interaction partners to predict their cooperative inclination and, hence, decide whether or not to start a partnerships with them. Some authors have argued that this may also lead to assortative matching of cooperative types (e.g., [Frank, 1987; 1988; Amann and Yang, 1998](#)). However, the empirical evidence suggests that individuals are only to some extent able to predict whether others are likely to cooperate. [Belot et al. \(2012\)](#), for example, ask subjects in an experiment to watch clips of a prisoner's dilemma game played on a TV show, and then predict the extent to which TV show contestants are likely to cooperate. While subjects estimate a higher likelihood of cooperation for cooperators than free-riders, the difference is small (7 percentage points). In line with this, [van den Assem et al. \(2011\)](#) find that contestants in a related TV show do not predict well the cooperative behavior of their opponents.

Though the two mechanisms studied in our paper cannot be applied to non-excludible public goods such as clean air or national security, there are several naturally occurring settings in which goods are publicly provided only to members who voluntarily participate in such cooperative endeavors. Typical real-world examples are associations, clubs, workplaces, partnerships, and the like, in which individuals may have the freedom to join in, or opt out from, participating in the provision of (local) public goods.²¹

In those and similar instances, cooperation among individuals is crucial while it is often characterized by intrinsic fragility due to the conflict between individual incentives and social optimal actions. Despite its fragility, in naturally occurring environments, a large variety of mechanisms are at work to foster cooperative outcomes. Scholars have explored a significant number of mechanisms conducive to cooperation. Our study has focused on a fundamental mechanism, voluntary participation, in the attempt to uncover useful stylized patterns for the design of institutions aimed at overcoming under-provision of public goods. The development of such institutions is an intellectual journey that will require the understanding of "how diverse polycentric institutions help or hinder the (...) levels of cooperation of participants, and the achievement of more effective, equitable, and sustainable outcomes at multiple scales" ([Ostrom, 2010, p. 665](#)); such a journey seems far from reaching its end.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at <http://dx.doi.org/10.1016/j.jebo.2017.07.009>.

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²¹ However, note that in our two-player experiment withdrawal of participation by one player effectively means the automatic exclusion of the other. It may be interesting to extend our design in order to adapt the two mechanisms of participation to a setting with more than two players.

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