

Unraveling Public Good Games*

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Abstract

This paper provides experimental evidence on how players predict end game effects in a linear public good game. Our regression analysis yields a measure of the relative importance of priors and signals on subjects' beliefs and let us conclude that, first, the weight of the signal is relatively unimportant, while priors have a large weight and, second, priors are the same for all periods. Hence, subjects do not expect end game effects and they do very little updating of beliefs.

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1 Motivation

Previous experimental research on public goods games has shown that in one-shot games contributions are relatively high (40%-60% of endowment) while in finitely repeated public good games contributions fall over time (see Davis and Holt, 1993; Isaac, McCue and Plott, 1985; Kim and Walker, 1984; Ledyard, 1995). Deviations from the free-riding zero contribution outcome and the decline over time have been rationalized through social preferences, *learning effects*, *strategic considerations* or *conditional cooperation*.¹

Cooperation may survive in an infinitely repeated game, but even in a finite game if there is a small probability that some subjects are not fully rational, rational subjects may react by contributing in the early periods and stop contributing toward the end of the game (see Kreps et al., 1982).

Players may not want to trigger a break in cooperation when the others are

¹See Andreoni (1988, 1995), Houser and Kurzban (2002), Chaudhuri and Paichayontvijit (2006), Ma, Sherstyuk, Dowling and Hill (2002), Keser and Winden (2000), Brandts and Schram (2001), Offerman, Sonnemans and Schram (1996) and Janssen and Ahn (2006), among others.

contributing but of course, this argument is no longer valid as the end of the game approaches and, in particular, lowering the contribution in the last period cannot trigger any retaliation. This argument can be extended. If players were aware that lowering contributions in the last period will not bring any consequence and they thought others were aware too, they might also realize that lowering contributions in the previous to last period could not trigger any retaliation either. This unraveling would make the finite game equivalent to a one-shot game but it requires common knowledge of rationality. The question is then whether subjects do indeed solve the game by backwards induction. There is some evidence that subjects find it difficult to apply this type of reasoning. Palacios and Volij, 2008, find that agents used to the backwards induction arguments (chess players) use it when playing the centipede game, while subjects more unfamiliar with it (students) did not use it to the same extent. Binmore, 1997 argues that rationality not always implies the use of backward induction. Using backwards induction seems to require some learning. In the usual laboratory experiment repetitions of the PGG will not provide that learning since subjects face the end of the game only once. Johnson, Camerer, Sen and Rymon, 2002, has shown that subjects taught to use backward induction made equilibrium offers in

an alternative offer bargaining game when playing with robots; however, when they played with untrained subjects they behaved differently, although closer to the equilibrium offers than before training. They conclude that both social preferences and a limited use of backward induction plays a role in the discrepancy between the experimental outcome and the equilibrium prediction.

Problems with backwards induction are not the only cognitive difficulties faced by players. Understanding the incentives in the one-shot game may also be an issue. Most papers have focused on this last type of limited cognition and how learning through repetitions of the one-shot PGG mitigate its effects (see Anderson et al, 1998; Andreoni, 1988, 1995; Brandts and Schram, 2001; Goeree et al, 2002; Houser and Kurzban, 2002; Palfrey and Prisbey, 1996, 1997). However, little attention has been paid to another source of cognitive limitation in PGG: the fact that subjects are not used to apply backward induction arguments in finite games nor do they believe that other subjects will use this type of reasoning. To analyze this problem, we focus on the end-game effect² in PGG and subjects' beliefs on this effect.³ Our work confirms

²Several papers have dealt with the question of *end-game effects*. Gonzalez et al. (2005) find that replacing a definite endpoint with an interval -commonly or privately known- does not change the timing of defection nor the average contribution levels.

³Several papers explore beliefs –and elicitation mechanisms– in PGG (see for instance

the difficulties related to backwards induction arguments in finite PGG.

Our main result indicates that a majority of subjects do not predict any end game effect at all, even when beliefs are elicited after playing the game. We model ex-post beliefs as a linear combination of prior beliefs and the signals observed during the game. In this set up, we find that the signal has a low weight in the determination of ex-post beliefs and, even though subjects experienced an end-game effect, this effect is absent from their ex-post beliefs.

The rest of the paper is organized as follows. In Section 2 we describe the experimental design and procedures. Section 3 presents our main results on average behavior and beliefs. In Section 4 we analyze individual behavior. Section 5 concludes.

2 Experimental design

The experiment was carried out in a single session at Universidad de Granada on May 31th, 2007. Participants were first year undergraduate students in Economics. The total number of participants was 48 divided in 12 groups.

Gächter and Renner 2006, Dufwenberg et al. 2006, Iturbe-Ormaetxe et al. 2007, Kovarik 2008).

Students were told that they would perform several tasks.

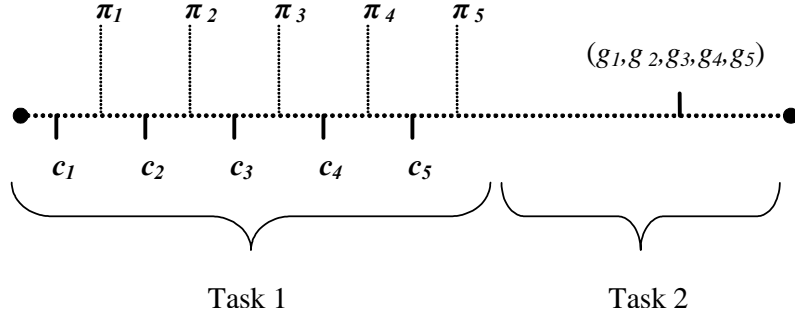
For the first task, subjects played a linear public good game (PGG) in each group for five periods. Subjects were informed that they would be playing with the same partners for the five periods. Each period subjects were given an endowment of 100 coins of 2 euro cents each. They were asked to make the decision on how much to allocate to a private account and how much to allocate to a public account. Contributions were expressed in number of coins, thus, they were integer numbers between 0 and 100, $c_{it} \in [0, 100]$. Participants were informed that any money allocated to the private account they could keep for themselves, and this independently of other subjects's actions, while all the money allocated to the public account (the sum of the money allocated by the four members of the group) would be multiplied by 1.5 and then it would be divided equally among the four members. Each participant earned the sum of payoffs obtained in the five periods.

After each period each subject received privately feedback in terms of his own payoff, π_{it} . Before the new period started they were given a new endowment of 100 coins of 2 euro cents. Figure 1 summarizes the timing of the experiment.

After making decisions on contributions to the public account for 5 peri-

ods, and getting feedback of their payoffs, subjects started Task 2.

Figure 1: TIMING



In Task 2, they were asked about their beliefs on the average contribution to the public accounts (in number of coins) of the 48 participants and for each of the five periods (g_{it}). We used an incentive scheme according to their errors, $e_{it} = g_{it} - \tilde{c}_t$ (being \tilde{c}_t the observed average and $t = 1, \dots, 5$). More precisely,

- If $5 < |e_{it}| \leq 10$ participant i received 1 euro;
- If $0 < |e_{it}| \leq 5$ participant i received 2 euros;
- If $|e_{it}| > 10$ participant i did not receive anything;
- Finally, if $e_{it} = 0$ participant i would receive 20 euros.

Participants were told that only one of the periods chosen at random would determine their payoff for Task 2.

Belief elicitation was placed after Task 1 to avoid any possible effects on contributions.⁴ The fact that they had observed their own payoffs could only increase belief accuracy. Since our main interest was to determine whether subjects could predict any end game effects and how they would react, we chose a design with a low number of periods. Subjects had enough time to think what they would do; after each decision a few minutes were left, then the feedback about payoffs was received and then the following period would start. The complete experiment lasted about an hour and subjects earned, on average, 13.47.

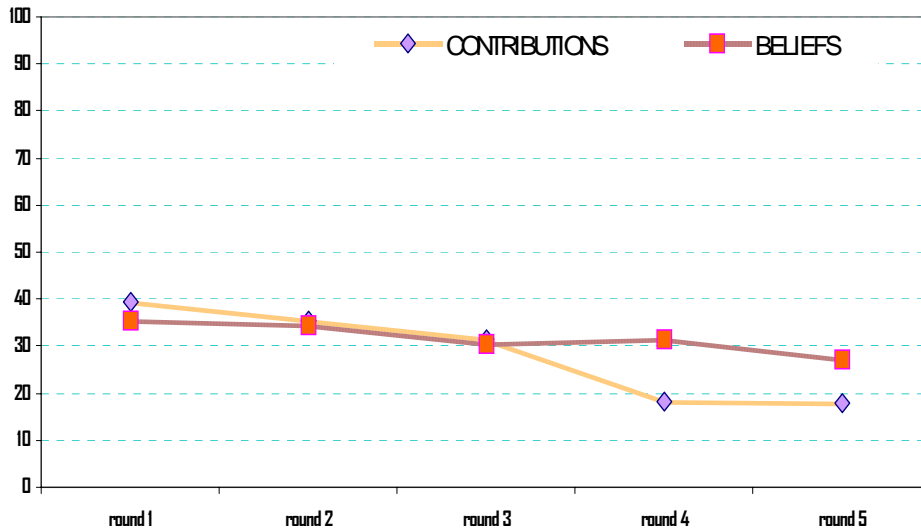
3 Average Behavior

First we compare actions and elicited beliefs. We check whether subjects, who had played the PGG for five periods and had received feedback about their own payoff after each one of them, could accurately predict the mean contribution of the population and to what extent they could predict any end-game effects. Since beliefs were elicited in Task 2, they will be called posterior beliefs.

⁴The evidence on whether belief elicitation may affect contribution is mixed. See for example Gächter and Renner (2006).

Figure 2 shows both the average posterior beliefs (over the whole population) and average actions in each period in the 4-player public good game.

Figure 2: CONTRIBUTIONS AND BELIEFS



As is usual in this literature we see that there is a decline in the average contribution over time. In sharp contrast, the declining trend for guesses is almost inexistent. To explore differences between actions and beliefs in period t we define the discrepancy between them as d_t , $d_t = c_t - g_t$, ($t = 1, 2, \dots, 5$). Table 1 shows the relevant statistics for d_t .

Table 1: BELIEFS ACCURACY

	<i>mean</i>	<i>median</i>	<i>st. dev.</i>
d₁	4.02	-1.50	33.83
d₂	0.97	-2.50	32.05
d₃	0.81	-5.00	36.57
d₄	-13.04	-12.50	23.36
d₅	-9.37	-16.00	28.20

For the first three periods the difference between actions and beliefs is relatively small. However, the difference increases in periods 4 and 5.⁵ Hence subjects' beliefs matched actions fairly well for the first three rounds but failed to do so in T and $T - 1$. In period 4, when the end game effect is first observed, the difference between the two is statistically significant.

We check whether there is a significant change from one round to the next. Table 2 explores the evolution of actions and beliefs. We use the Wilcoxon test to check differences between c_t and c_{t-1} (g_t and g_{t-1}).

There is a significant decline in contributions between periods 3 and 4

⁵We check whether c_t and g_t are drawn from the same population using paired non-parametric test. The Wilcoxon test compares c_t and g_t for each round. $Z_1=-0.50$ (p -value = 0.61); $Z_2=-0.11$ (0.91); $Z_3 =-0.28$ (0.77); $Z_4=-3.83$ (0.00) and $Z_5=-2.96$ (0.00). Sign tests yield identical results.

(see also Figure 2) but this trend does not continue to period five. We do not see a similar declining pattern for beliefs.

Table 2: Evolution of c_t and g_t .

	Z	$p - value$		Z	$p - value$
$\mathbf{c}_1, \mathbf{c}_2$	-0.02	0.98	$\mathbf{g}_1, \mathbf{g}_2$	-0.25	0.80
$\mathbf{c}_2, \mathbf{c}_3$	-0.19	0.84	$\mathbf{g}_2, \mathbf{g}_3$	-1.30	0.19
$\mathbf{c}_3, \mathbf{c}_4$	-2.34*	0.01	$\mathbf{g}_3, \mathbf{g}_4$	-0.15	0.88
$\mathbf{c}_4, \mathbf{c}_5$	-0.52	0.60	$\mathbf{g}_4, \mathbf{g}_5$	-1.69	0.09

Observe that whereas subjects change their behavior in period 4, this change was not incorporated into posterior belief and subjects overstated the value of the participants' contribution at the end of the game. We may conclude that, concerning average behavior:

Result 1.- There is an end-game effect at period $T - 1$. Players do not predict end-game effects.

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Result 1 refers to average behavior. However, different types of players may follow different patterns.⁶ We address this issue in the next section.

⁶Previous work on PGG has shown evidence of subjects' heterogeneity. For instance,

4 Individual results

Figure 2 showed the extent of the end-game phenomenon in aggregate beliefs and contributions. To explore the question more deeply, we analyze individual behavior and beliefs.

In Table 3 we summarize actions and elicited beliefs of all subjects in terms of the period in which they lowered contributions and the period in which they believed the end game phenomenon would occur.

A decrease in contributions (in rows) is defined here as lowering the contribution to a value (*i*) lower than $\frac{2}{3}$ of the previous value and (*ii*) lower than $\frac{2}{3}$ of the average of the own contribution in previous periods, provided the decrease is maintained up to the last period.⁷

Note that 25% (12 out of 48) of subjects decrease their contribution in period 4, but also a high percentage of subjects (23%) did not decrease their contribution as the end of the game approached.

Fischbacher *et al.* (2001) and Chaudhuri and Paichayontvijit (2006) have found that some players are conditional cooperators and others are free-riders.

⁷The actual decrease in the average contribution in period 4 was from (39.3; 35, 4; and 31.4) to (18.4; 17.9) which fulfills this criterion. Small changes in the threshold do not change results (choosing 0.6 or 0.7 leaves results almost unchanged).

Table 3: ACTIONS (∇) & PREDICTED END GAME EFFECTS (EG)

$\nabla \frac{2}{3}$	<i>EG at T-3</i>	<i>EG at T-2</i>	<i>EG at T-1</i>	<i>EG at T</i>	No <i>EG</i>	total
∇ at $T - 3$	1	3	0	3	5	12
∇ at $T - 2$	0	2	0	1	4	7
∇ at $T - 1$	1	2	1	3	5	12
∇ at T	1	0	0	1	4	6
No ∇	2	0	0	2	7	11
total	5	7	1	10	25	48

Concerning beliefs (in columns): *i*) 25 out of 48 subjects (52%) did not predict any end game effect; *ii*) 10 subjects (21%) believed that the end game effect would occur at the last period and *iii*) the remaining subjects (13) predicted the decline at $T = 4$ or before.⁸

This means that 73% of the players either predicted the decrease in contributions later than the period in which the decrease took place (period 4) or they did not predict it at all. This is remarkable since at the time of the prediction they had already seen the outcome of the five periods of the contribution game in their own group of four subjects (although the prediction

⁸Here we cannot distinguish between those subjects who expected ex ante a decrease in contributions and those who learned about the end game effect during the game.

referred to the average of all participants). Subjects had the opportunity to update their beliefs with the observed behavior in their group, in case they have not predicted ex ante the end-game effect.

Result 2.- Half of the subjects did not incorporate the observed end game effects into their posterior beliefs.

In the next section we try to rationalize this result by looking at how posterior beliefs are formed.

5 Posterior Beliefs

The result of the previous section was that more than half of the subject pool did not incorporate the observed end-game effect in their own beliefs. Beliefs were elicited after playing the PGG so that they must be a combination of ex-ante beliefs (priors) and the signals observed throughout the game. Subjects do not observe other players contributions, but they do observe the part of the payoff that comes from their group contributions to the public account.

We define this value as the signal⁹ observed by individual i in group z at each period t : $s_{it} = \frac{1.5 \sum_{i \in z} c_{it}}{4}$.

⁹An alternative signal could be the subjects' payoffs (private + public account). We also used this variable as the signal (see footnote 11).

We model ex-post beliefs as a linear combination of prior beliefs and the signal observed in the game for each individual i :

$$\mathbf{g}_{it} = (1 - \alpha)p_{it} + \alpha\mathbf{s}_{it},$$

where prior beliefs of individual i , p_{it} , might vary across periods.

Since we observe \mathbf{s}_{it} and \mathbf{g}_{it} we can get an estimation of α . The assumption is that the weight given to the signal and that given to the priors are the same for all individuals. We estimate the following panel regression with fixed effects:

$$\mathbf{g}_{it} = \underbrace{\left[\gamma_o + \gamma_i + \sum_{t=2}^5 \beta_t d_t \right]}_{(1 - \alpha)p_{it}} + \alpha\mathbf{s}_{it} + e_{it} \quad [1]$$

where γ_o is the constant, γ_i are individual fixed effects (reflecting subjects' heterogeneity), d_t are period dummies allowing priors to be different across periods, the estimated parameters $\hat{\beta}_t$ will be an indication of how individuals predict the end game effect (if they do, parameters $\hat{\beta}_4$ and $\hat{\beta}_5$ will be negative and significant), and e_{it} is the error term.

Table 4 shows the regression results.

Table 4: REGRESSION RESULTS. BELIEFS g_{it}

<i>beliefs</i> (g_{it})	(1)	(2)
<i>signal</i> (s_{it})	0.11* (0.04)	0.14* (0.03)
<i>constant</i>	28.80* (3.11)	25.97* (1.68)
d_2	-0.21 (2.38)	
d_3	-3.38 (2.42)	
d_4	-0.41 (2.74)	
d_5	-4.49 (2.76)	
	$R^2 = 0.09$ $n = 240$	$R^2 = 0.07$ $n = 240$
	$F = 4.15$ $p - val. = 0.00$	$F = 14.98$ $p - val. = 0.00$

(*) significant at 1%; (std. errors).

The period dummies are not significant. The implication is that when we separate the effect of the priors and that of the signal, priors are not time dependent, i.e., on average subjects did not predict ex ante a decline in contributions over time. Eliminating the time dummies from the regression yields the coefficients shown on the right of Table 4, regression (2).

The estimated weight of the signal, $\hat{\alpha}$, is 14% but the weight of the priors, $1 - \hat{\alpha}$, is six times larger.

In regression (2) we may obtain a measure of each individual prior beliefs weighted by $(1 - \alpha)$ through the predicted constant and fixed effects: $\hat{\gamma}_o + \hat{\gamma}_i$ (see equation [1]). We calculate the main statistics for the (predicted) prior beliefs: the mean (std. dev.) is 25.97 (14.47) and the max (min) is 69.3 (-0.8).¹⁰ Therefore, we observe a large heterogeneity in priors across subjects.

Summing up our results in this section,

Result 3

- Priors are constant across periods, that is, subjects did not predict ex ante any end game effects. There is a large heterogeneity in priors.
- In the formation of posterior beliefs, the weight given to the signal ($\hat{\alpha}$) is relatively low: 10% – 15%. Priors are given a much larger weight.

To check the robustness of this result, we consider two alternative signals that the subject could use to update his priors: the own contributions or the payoff he received in each period. However, these signals turn out to be not significant.¹¹ In sum, to form their posterior beliefs subjects do not use

¹⁰The unweighted values are 30.20 and 80.58, respectively.

¹¹Using the individual payoffs as signals yields a coefficient 0.02 (p – value 0.61); for

their own contribution or the payoff as signals but the average contribution of their group.

The low weight given to the signal is consistent with the fact that although individuals experienced an end game effect, they did not guess it after the game. Other papers have found evidence in the same direction: subjects update their beliefs but very little (see Kovarik, 2008). Given the low weight given to signals we should not expect large learning effects from repetitions of a finite PGG.¹²

6 Discussion

In the PGG repetition of the one-shot game has been shown to decrease contributions. Repetition introduces learning effects, strategic considerations and the possibility of punishment for the unfair behavior of others¹³ that could be related to the decrease in contributions.

We contribute to this literature on experimental public good games with

individual contributions the coefficient is 0.04 (p – value 0.25). Adding subjects’ contributions or payoffs in regressions (1) and (2) does not change results substantively in terms of the estimated coefficient of s_{it} .

¹²This is also consistent with the low speed of learning observed in the centipede game (see Palacios and Volij, 2008).

¹³See Andreoni (1995) and Houser and Kurzban (2002).

the idea that the subjects' different abilities to unravel the game (or their beliefs on the ability of others to do so) may be an important factor behind the experimental results.

We performed this analysis asking subjects their beliefs on average contributions for each period. The belief elicitation was placed after the PGG to avoid any interference with contributions..

Our regression analysis let us measure the relative importance of priors and signals on subjects' belief formation. Our main results are that priors are constant for all periods and they are given a large weight compared to the signals observed throughout the game.

Our analysis suggest that before playing the game subjects do not expect backward induction, not even in the last few periods, and their updating with the observed signals is slow. Therefore, the posteriors beliefs do not incorporate the end-game effect.

Previous papers have studied the reasons behind contributions: kindness, altruism or warm-glow vs. errors or confusion (see Croson, forthcoming; Andreoni 1995; Houser and Kurzban, 2002, among others). Our paper focuses on a different sort of confusion: people do not expect others to apply backward induction. However, this confusion is not inconsistent with individuals

endowed with *other-regarding* preferences and, more precisely, with subjects who consider that other players could have social preferences.

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