May The Forcing Be With You: Experimental Evidence on Mandatory Contributions to Public Goods*

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Abstract

Evidence in the applied literature indicates that policies intended to stimulate positive externalities via coercion can backfire. For example, Davis (2008) finds that when in 1989, the government of Mexico City tried to control air pollution by banning most drivers from driving their vehicle one weekday per week, many drivers bought another, used, high emissions car, which ended up worsening pollution. In order to test for such effects, we run a repeated public goods experiment where subjects are randomly forced to contribute. All group members are informed about forcing after it happens. We find that when random forcing is present, intended contributions are significantly larger in absolute terms. Moreover, contributions decrease significantly after being forced to contribute, and tend to increase after another group member is forced to contribute. Hence, our results indicate that forcing mechanisms have indirect effects that must be taken into account when assessing the overall impact of policies aimed at stimulating positive externalities.

Keywords: unintended consequences, public good game, laboratory experiment, reciprocity.
JEL classification: C92, D04, H41.

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1 Introduction

In a standard public good game, positive externalities, or complementarities, are known to motivate agents to contribute, even though they may be able to free ride and still reap the benefits of the good (see Ledyard, 1995; Chaudhuri, 2011 for comprehensive surveys of the literature). In this paper, we study the effects of a policy that forces agents to contribute to the public good. We manipulate the policy assignment over a repeated public good game.

While the policymaker's goal is ideally to increase contributions to the public good, we are interested in the potential *unintended consequences* of this policy. Put simply, will the policy backfire, crowding out individual motivations and ultimately decreasing contributions? In general, the concept of unintended consequences denotes policy outcomes that differ from the goals of the policymaker. This definition builds on that of Merton (1936), who applied the term 'unanticipated consequences' to sociological matters. In fact, the applications of unintended consequences span the social sciences.[1]

In our experiment, subjects play a standard public good game for 30 rounds, but, in the treated groups, each round, have an exogenous probability of being forced to contribute their entire account. Our experimental design brings to mind the literature on noisy repeated games (Fudenberg and Maskin, 1990; Bereby-Meyer and Roth, 2006; Rand et al., 2015). For instance, Ambrus and Greiner (2012) run a public good game where the choices of participants are occasionally reversed, and Arechar et al. (2017) run a similar study on a repeated prisoners dilemma. Our setup crucially differs in that i) other group members are informed about the manipulation taking place, and ii) the manipulation only goes in one direction: that of increasing the contribution; so its structure is purposely different from that of a random *noise*, and meant to be perceived as such.

[1]In Economics, these can be due to a number of different rational and behavioral explanations, which include moral hazard, time inconsistency, and externalities. For example, a well-intentioned policy with negative spillovers may be implemented (Elster, 2017). Such policies may or may not achieve their desired effect, and may also produce unintended outcomes (Davis (2008); Antecol et al. (2018). Examples of recent policies with unintended consequences may be found at `http://sharongharrison.blogspot.com/`.

Forced contributions in public good games have mostly been studied in the context of threshold public good games (Dawes et al., 1986; Cartwright and Stepanova, 2017). Our study differs in two ways. First, we are interested in forced contributions which are not a consequence of other participants' choices. In a threshold public good game with forced contributions, the decision to contribute is akin to a (costly) vote: hence, the provision of the public good, and the forced contributions that may result from it, can be seen as the result of a collective decision process. In our experiment, by contrast, whether a contribution is forced does not depend in any way on *intended* contributions. Instead, forced contributions are a purely exogenous shock. Second, since our forced contributions are randomly assigned, we can estimate their causal effects. This also allows us to discriminate between effects on own and other group members' subsequent contributions. Brekke et al. (2011) study public good games where, in some groups, part of the payoff is subtracted and given to charities. Such contributions are fixed and might resemble our policy intervention, but they do not influence the payoff of group members. Most importantly, the forced contributions implemented by Brekke et al. (2011) are not exogenously imposed, as participants self-select into such groups. A family of experiments in which individuals are randomly forced to give more than intended is that studying the effect of tax auditing (Kogler et al., 2016; Mittone et al., 2017); however these experiments involve both a specific framing (tax declarations) and a peculiar game structure, where evasion is endogenous and payments do not affect other subjects; similarly, the literature studying climate change in the lab (Ghidoni et al., 2017) shares with our design the presence of random shocks within a PGG, but with different effects on payoffs.

Conceptually, our work is closely related to the issue of reciprocity and conditional cooperation. In the literature on voluntary provision of public goods, reciprocity has long been a central subject of study (Sugden, 1984). Several studies (including Keser and Van Winden, 2000; Fischbacher et al., 2001; Chaudhuri, 2011 among others) have observed conditional cooperation in public good games, providing evidence that many individuals are more willing to contribute conditional on their peers' doing so. One standard explanation for this phenomenon is that of *fairness*: if subjects value negatively large differences in payoff between group members, they have an incentive to contribute more than the Nash equilibrium of zero (Geanakoplos et al., 1989; Rabin, 1993). Other behavioral explanations, however, have been put forward: Bigoni et al. (2018) study the issue of *betrayal aversion* (Cubitt et al., 2017; Bohnet et al., 2008), relating contributions in a public good to those in a trust game.[2] This allows them to study how individual differences in betrayal aversion explain differences in contributions. However, their experiment does not consider the general question of whether betrayal aversion explains much of the observed conditional cooperation in the general population. Discriminating between betrayal aversion and fairness motives in contributions to public good games requires a distinction between *intended* contributions and *effective* contributions.[3] In our experiment, when subjects contribute more than they intended to, other group members are made aware of this. Hence, we can observe the extent to which conditional contributors react to actual contributions, as opposed to intentions to contribute.

Lastly, the policy we study is aimed at leveraging externalities. Hence, our work is related to a class of models in macroeconomics (for example Blanchard and Kiyotaki, 1987; Harrison, 2001; among many). We are interested in the causal effects of the forced contributions on own and other group members' subsequent contributions, similar to the macroeconomic literature. Hence we are concerned about welfare consequences of the policy: we explicitly look for aggregate effects in Section 2.1.

2 Theory

We analyze a standard public good game where the payoff, π, of player i at time t is given by

$$\pi_i^t = w_i - \bar{x}_i^t + cG^t \qquad \text{with } \bar{G}^t = \sum_j \bar{x}_j^t$$

where ω_i is the player's endowment, $\bar{x}_i$ is her contribution, and c is the size of the externality. In the experiment described later, we will adopt $\omega = 10$, and $c = 0.5$. At each round, a subject can be randomly selected for a forced contribution: when this happens, $\bar{x}_i^t = \omega$. We denote with $x_i^t \leq \bar{x}_i^t$ the *intended* contribution of player i at time t. We assume that individuals choose x_i^t based on a combination of the following traits:

[2]Betrayal aversion is elicited by comparing the contributions in a trust game to those in an analogous game where the other player's choice is replaced by a random draw.

[3]Gächter et al. (2017) show that the tendency to reciprocate varies significantly between different framings of the same game; but even in their design, contributions are, to other participants, indistinguishable from intentions to contribute.

1. preferences for larger payoffs π_i^t,
2. preferences for fairness[4](Rabin, 1993; Keser and Van Winden, 2000),
3. the desire to reciprocate others' contributions to own payoff, which we will refer to as *decision reciprocity*.

The tension between trait 1 and the two others is relatively standard in the public good games literature, with null contribution being a dominant strategy, but positive contributions being consistently observed in experimental settings. The distinction between 2 and 3 instead is the focus of the model we propose. *Decision reciprocity* is in some sense the symmetric effect to betrayal aversion (Bohnet et al., 2008): [5] whereas the latter involves *distaste* for a payoff reduction due to another subject's choice (rather than pure luck), the former involves *appreciation* for a payoff increase due to another subject's choice (rather than luck, which in our case operates via forced contributions).

Both the general concept of reciprocity and the specific phenomenon of *conditional cooperation*, today a relatively standard concept in the literature on public good games (Fischbacher et al., 2001), are consistent with both traits 2 and 3. Namely, a subject may wish to reciprocate another subject's contributions because this makes their final payoffs more similar among them (preference for fairness), but also because she wants to reward the other subject's decision to contribute (decision reciprocity). Asymmetric PGGs, where reciprocity does not necessarily correspond to increased fairness (an individual with a small endowment could increase overall fairness by *not* reciprocating contributions of peers with large endowments), provide evidence that fairness appears to be an important motivation (Van Dijk and Wilke, 1995), but precisely the asymmetric setup makes it difficult to draw conclusions on motivation reciprocity.

In this model, as in the experiment later presented, we do not employ strategy methods, and participants are placed in the context of uncertainty on peers' contributions that is typical of PGGs. However, to the extent

[4]In our framework, "fairness" can be operationalized as a disutility in the absolute difference between own and others' payoffs or, equivalently, as a generic positive and concave utility in other agents' payoff, i.e. a form of "sympathy for the victim". The latter definition does not necessarily result in equal payoffs being optimal, but it is sufficient to involve a tension between own and others' payoffs, which is what matters for our analysis.

[5]The idea of decision reciprocity also brings to mind the the warm glow effect (Andreoni, 1995), where the subject themself feels good giving to others.

that they have a propensity to conditionally cooperate, they will pick their contribution based on what they *expect* their peers to contribute. Note that in practice, in a repeated PGG such as the one we implement, such expectation is likely to depend on observed past behavior of one's peers. In what follows, we do not explicitly model such dependence as we are not interested in the level of contributions as such, but only in the difference in contributions due to our treatment, that is, the forcing.

We hence assume that contributions from round 2 onward are determined by the following general formula:

$$x_i^t = x^t\left(\mathbb{E}[G_{-i}^t], S_{-i}^{t-1}, S_i^{t-1}\right). \tag{1}$$

where $G_{-i}^t = G^t - x_{-i}^t$ denotes aggregate intended contributions by peers, $S_i^{t-1} = \bar{x}_i^{t-1} - x_i^{t-1}$ denotes the extent of forcing on i, and S_{-i}^{t-1} on i's peers.

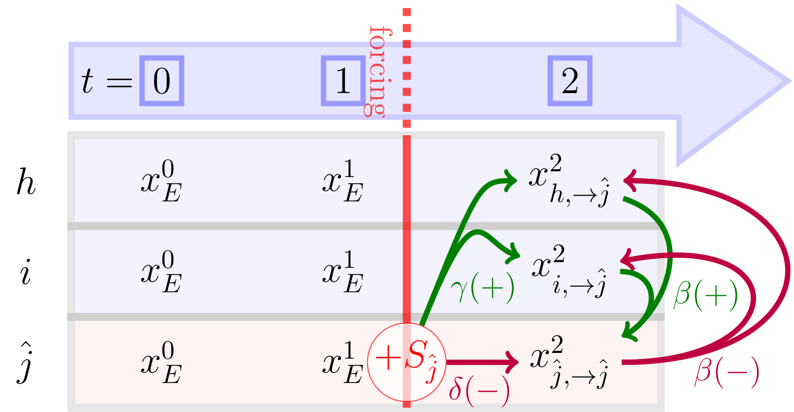
Notice that in our experiment, a maximum of one peer per group and round is forced, so $\min(S_{-i}^{t-1}, S_i^{t-1}) = 0$.

As already mentioned, both the dependence of x^t on its first and its second argument are related to conditional cooperation. However, the two arguments have markedly different roles with respect to our classification outlined above. Specifically, x^t could be increasing in $\mathbb{E}[G_{-i}^t]$ either because of fairness motives — with own contributions counterbalancing the transfer of wealth due to peers' contributions — or because of decision reciprocity — reciprocating the peers' *willingness* to contribute. Instead, x^t can be increasing in S_{-i}^{t-1} only because of the fairness motives. In other words, an individual who reciprocates decisions but does *not* have a taste for fairness should reciprocate intended, but *not* forced, contributions. Vice–versa, an individual that appreciates fairness but does not reciprocate decisions should react in the exact same way to the intended component and to the forced component of peers' contributions.

Finally, x^t also depends on S_i^{t-1} because it is likely (for instance according to prospect theory or, even just fairness motives) that being forced to contribute *decreases* subsequent willingness to contribute, that is, that x^t is *decreasing* in its third argument. Note that both the reaction to own and peers' forcing could include a (positive) component related to the formation of a *habit* to contribute — which our experiment is not designed to disentangle.

Our experiment allows us to compare two counteracting effects of this model. Whenever a peer $j \neq i$ is forced to contribute in period $t-1$, then

Figure 1: Effects of forcing



Note: example of forcing on group member $\hat{j}$ at period 1.

S_{-i}^{t-1}, the forcing shock affecting i's peers, increases. At the same time, S_j^{t-1} also increases, and hence in period t individual j will want to decrease x_j which decreases $\mathbb{E}[G_{-i}^t]$. In other words, a fairness lover will react to the past, reciprocating the peer's forced contribution, while a more decision reciprocating individual will react to the expected future decrease in (intended) contribution.

To clarify this tension we consider, and illustrate graphically in Figure 1, a linear formulation of x_i^t that naturally reconnects to our later analysis of experimental data:

$$x_i^t = \alpha_t + \beta\mathbb{E}[G_{-i}^t] + \gamma S_{-i}^{t-1} + \delta S_i^{t-1}. \tag{2}$$

We allow the constant term to change over rounds in accordance with the abundant evidence that contributions decay over time in repeated PGGs (Andreoni, 1988). Indeed, from Equation (2) we immediately get that, before any forcing event occurs, in equilibrium

$$x_E^t = \alpha_t + \beta\mathbb{E}[G_{-i}^t] = \alpha_t + \beta(N-1)x_E^t \tag{3}$$

$$\implies x_E^t = \frac{\alpha_t}{1-\beta(N-1)} \tag{4}$$

(notice that G^t_{-i} does not include forcing, and hence consists in assuming individual contributions of x^t_E each), so that x^t_E is uniquely determined by the parameters.

Equation (4) is undefined for $\beta = \frac{1}{N-1}$ and takes negative values for $\beta > \frac{1}{N-1}$. Indeed, for such parameters each agent wants to contribute more than the average of peers, which is impossible. As a special case, we obtain an indeterminate form if $\alpha_t \equiv 0$ and $\beta = \frac{1}{N-1}$, that is, if subjects are perfect conditional cooperators in expectations — Equation (3) becomes a tautology. In practice, the experimental literature suggests that β is typically distributed between 0 and $\frac{1}{N-1}$, including both extremes (Fischbacher et al., 2001): we can consider β to be on average lower than $\frac{1}{N-1}$.

Moving to the case in which one subject was forced at round $t-1$, and denoting as $x^t_{i,\to j}$ the contribution at round t by agent i after agent j was forced, we find that for $i \neq \hat{j}$

$$\begin{aligned} x^t_{i,\to\hat{j}} =& \alpha_t + \beta\mathbb{E}\left[G^t_{-i}| \to \hat{j}\right] + \gamma S^{t-1}_{-i} \\ =& \alpha_t + \beta(x^t_{\hat{j},\to\hat{j}} + (N-2)x^t_{i,\to\hat{j}}) + \gamma S^{t-1}_{\hat{j}} \end{aligned} \tag{5}$$

where the second line is equivalent to the first because a subject whose peer was forced expects to interact with that peer plus $N-2$ other (non–forced) peers. Similarly,

$$\begin{aligned} x^t_{\hat{j},\to\hat{j}} =& \alpha_t + \beta\mathbb{E}\left[G^t_{-\hat{j}}| \to \hat{j}\right] + \delta S^{t-1}_{\hat{j}} \\ =& \alpha_t + \beta(N-1)x^t_{i,\to\hat{j}} + \delta S^{t-1}_{\hat{j}}. \end{aligned} \tag{6}$$

We define the effect of a peer being forced as $\Delta_p = x^t_{i,\to\hat{j}} - x^t_E$ and of being oneself forced $\Delta_o = x^t_{\hat{j},\to\hat{j}} - x^t_E$, and compute these terms by combining Equation (5) and (6), respectively, with Equation (3):

$$\begin{aligned} \Delta_p =& \alpha_t + \beta((N-2)x^t_{i,\to\hat{j}} + x^t_{\hat{j},\to\hat{j}}) + \gamma S^{t-1}_{\hat{j}} - \alpha_t + \beta(N-1)x^t_E \\ =& \beta((N-2)\Delta_p + \Delta_o) + \gamma S^{t-1}_{\hat{j}} \end{aligned} \tag{7}$$

$$\begin{aligned} \Delta_o =& \alpha_t + \beta(N-1)x^t_{i,\to\hat{j}} + \delta S^{t-1}_{\hat{j}} - \alpha_t + \beta(N-1)x^t_E \\ =& \beta(N-1)\Delta_p + \delta S^{t-1}_{\hat{j}} \end{aligned} \tag{8}$$

and finally by replacing Δ_o inside Δ_p

$$\begin{aligned}\Delta_p =&\beta\left((N-2)\Delta_p+\beta(N-1)\Delta_p+\delta S_{\hat{j}}^{t-1}\right)+\gamma S_{\hat{j}}^{t-1}\\ =&\beta((N-2)+\beta(N-1))\Delta_p+(\beta\delta+\gamma)S_{\hat{j}}^{t-1}\\ =&\frac{\beta\delta+\gamma}{-(N-1)\beta^2-(N-2)\beta+1}S_{\hat{j}}^{t-1}\end{aligned}\tag{9}$$

and this last expression inside Δ_o

$$\begin{aligned}\Delta_o =&\beta(N-1)\frac{\beta\delta+\gamma}{-(N-1)\beta^2-(N-2)\beta+1}S_{\hat{j}}^{t-1}+\delta S_{\hat{j}}^{t-1}\\ =&\frac{\beta\gamma(N-1)+\delta(1-(N-2)\beta)}{-(N-1)\beta^2-(N-2)\beta+1}S_{\hat{j}}^{t-1}.\end{aligned}\tag{10}$$

The denominator in these two equations has one root in $\beta=\frac{1}{N-1}$, which we had already excluded based on Equation (4), and one in $\beta=-1$, which is also out of the admissible range $(0,\frac{1}{N-1})$: on such range, the polynomial is always strictly positive. As for the numerator of Δ_p, since γ is likely to be positive (reflecting reciprocity towards a peer's forced contribution) but δ is likely to be negative (compensating one's own previous forced contribution), the overall sign is the result of two contrasting forces: on one hand, when $\hat{j}$ is forced other peers may be willing to reciprocate the (unintended) extra contribution (via γ); on the other hand, they may anticipate a lower subsequent contribution (via δ) and react to it (via β). In principle, the sign of Δ_o is also undetermined, since $\beta<\frac{1}{N-1}$ implies $1-(N-2)\beta>0$: despite having being forced, a subject can react positively to the expected positive reaction of her peers. However, it can be easily observed from Equation (8) that if $\Delta_p<0$, then since $\delta<0$, necessarily $\Delta_o<0$; so the effect of forcing is more likely to be negative on the forced subject than on peers.

The analysis outlined above rests on the assumption that x_E^t are equilibrium values of contributions before any forcing event occurs. However, the algebra developed applies straightforwardly to x_E^t being equilibrium values *conditional* on a history of forcing events. The assumption that these equilibrium values are equal for all group members is not even required: for instance we could abandon it by reformulating Equation (3) as $x_{Ei}^t=\alpha_t+\beta\sum_{j\neq i}x_{Ej}^t$, Equation (5) as $x_{i,\rightarrow\hat{j}}^t=\alpha_t+\beta\sum_{j\neq i}x_{j,\rightarrow\hat{j}}^t+\gamma S_{\hat{j}}^{t-1}$ and Equation (6) as

$x^t_{\hat{j},\rightarrow\hat{\jmath}} = \alpha_t + \beta \sum_{j \neq \hat{j}} x^t_{j,\rightarrow\hat{j}} + \delta S^{t-1}_{\hat{j}}$: Equations from (7) to (10) would remain unchanged. The only relevant assumption is that the *effects* Δ_o and Δ_p are homogeneous across group members, regardless of the past history of forcing events; this is reasonable precisely because equilibrium contributions can internalize the effect of past forcing events.

2.1 Hypotheses

The first–order effect of forcing is to rise contributions to the public goods, but the above model suggests it can backfire, depending on the parameters values. We now outline hypotheses that express, at different levels of analysis, whether forcing contributions achieves the intended goal, or if instead there are unintended consequences to this policy.

(H1) If a subject is forced, this affects subsequent contributions of other group members: $x^t_{j,\rightarrow i} \neq x^t_E$.

With the linear specification provided by Equation (2), this hypothesis is equivalent to testing the sign of $\beta\delta + \gamma$.

(H2) If a given round is forced, this affects subsequent contributions of the subject: $x^t_{\hat{j}\rightarrow\hat{j}} \neq x^t_E$.

In the linear specification, this amounts to testing the sign of $\beta\gamma(N-1) + \delta(1-(N-2)\beta)$. Notice that our model predicts that if $x^t_{\hat{j}\rightarrow\hat{j}} > x^t_E$ (as in (H1)), then necessarily $x^t_{i,\rightarrow\hat{j}} > x^t_E$.

(H3) When random forcing is present, average intended contributions differ from when forcing is absent.

This more general hypothesis is not only a combination of hypotheses (H1) and (H2). The average effect *of a forcing event* is a weighted sum of its effect on the forced subject and on the peers, but the values x^t_E could also be affected by the *expectation* of forcing, and contribute to the overall effect of the forcing scheme.

3 Experimental design

In our experiment, we bring this model into the lab. The sessions were run online, on Zoom, between June 22 and June 30, 2021 under the auspices

Table 1: Summary of experimental sessions

Session	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**Total**
Sample	15	15	12	12	9	12	12	9	18	15	15	12	**156**
Control	x				x				x			x	**54**
LOW		x		x		x				x			**54**
HIGH			x				x	x			x		**48**

Note: Excluded: three subjects from each of sessions 2 and 8, because of one group member leaving the session prematurely.

of the CELSS lab at Columbia University. Subjects were recruited using ORSEE.

Each session consisted of 30 rounds. At the beginning of each session, subjects were randomly and anonymously matched in groups of 3, which remained stable for the entire session.

At the beginning of each round, each subject was asked to decide how much to contribute – out of the initial endowment of \$10 – to their group's common pool. The contribution was restricted to be an integer amount between 0 and 10. Subjects were instructed that the total amount contributed was then multiplied by 1.5 and redistributed in equal shares to the group members (for a marginal per capita return of $1.5/3 = 0.5$). Before the experiment began, subjects answered control questions to verify that they understood the game. At the end of the experiment, each subject was paid a \$5 show up fee, and the earnings from a round drawn randomly by the computer.

We ran 4 sessions with each of three treatments, for a total of 12 sessions, on 156 subjects.[6] Our treatments were: a *control* with no forcing (C) and 2 *policy* treatments, one with a LOW (L) (p= 0.1) probability of forcing, and one with a HIGH (H) (p=0.2) probability of forcing, the exact meanings of which are described below. The numbers of subjects in each session are summarized in Table 1.

We begin by describing the control treatment. In these sessions, there was no forcing. Subjects simply played the game for 30 rounds, deciding how much to contribute each round. At the end of each round, each subject was told their groupmates' contributions,[7] how their own payoff was calcu-

[6]Observations from six more subjects were dropped because two of them left the experiment early, invalidating their entire groups' observations.

[7]It was necessary to provide individual, rather than average, past contributions because,

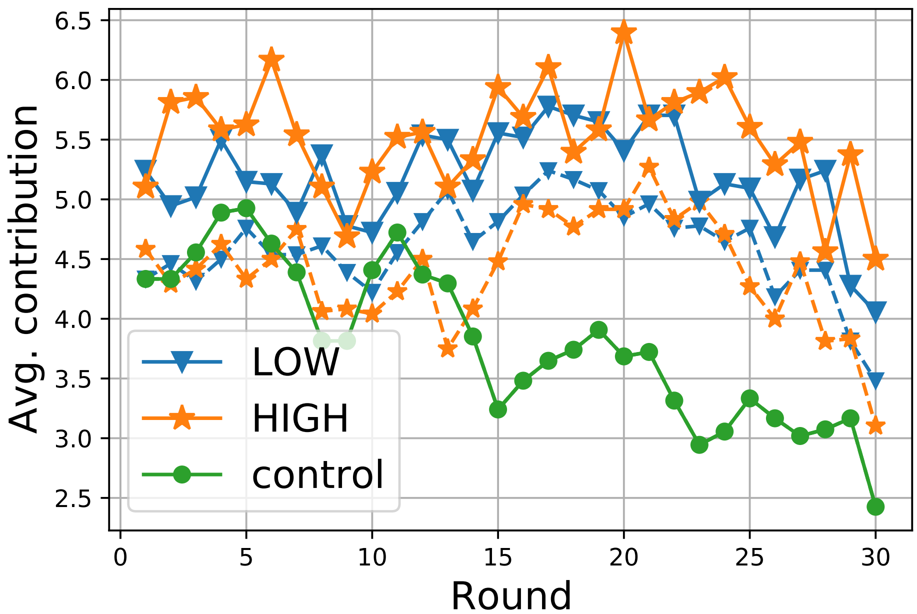
lated from that round, and the value of their payoff. See the screenshots in Appendix A. The *policy* treatments were the same as the control treatment, except that in every round, each subject was, with a probability of either $p = 0.1$ (LOW) or $p = 0.2$ (HIGH), forced to contribute their entire endowment, \$10.[8] Specifically, each group was selected with a probability of 0.3 and 0.6, respectively, and from each selected group a subject was randomly chosen for the forced contribution. Subjects were informed of this before starting the game, and when they were forced, the computer automatically contributed for them. In these treatments, the information provided at the end of every round included whether or not a subject's groupmate was forced to contribute. (If any subject was forced, neither of their groupmates was forced in that round.)

After the experiment, participants completed a short demographics questionnaire, that also asked "Please explain why you made the choices you made during the experiment." The average payoff was \$16.70, including the \$5 show up fee; sessions lasted between 43 and 71 minutes. The instructions for the LOW policy treatment are in Appendix B. (The instructions for the control treatment left out any mention of forcing, and for the HIGH policy treatment simply had the higher probability of forcing.)

when a peer is forced, the other two peers know it — hence, knowing the average of contributions is equivalent to knowing the individual contribution of the non–forced peer. Since the literature has shown that information on individual contributions can affect behavior differently from information on average contributions (Cason and Khan (1999), footnote 1), this would have represented a confounding effect to forcing.

[8] We purposely avoided framing the intervention in a negative way — e.g. by referring to it as a "punishment" — because our aim was for forced contributions to be closely comparable to intended contributions; in the same spirit, we did not attach to them any sunk costs that are typical of the experimental literature on punishment and tax audits.

Figure 2: Contributions over time



Note: Average contributions per round, disaggregated by treatment. Solid lines: actual contributions (including forced); dashed lines: intended contributions.

4 Results

Figure 2 displays average contributions over time, depending on treatment. Both LOW and HIGH sessions result in larger contributions than CONTROL: this holds true also for intended contributions (that is, disregarding forcing).

Contributions in LOW and HIGH sessions are relatively similar, and the increase in the forcing probability from 0.3 to 0.6 does not seem to result in a further increase in intended contributions (if anything, the opposite).

Looking at the evolution of contributions over repetitions, treated sessions do not display any different propensity to contribute in the very first rounds, but they seem more immune to the progressive decrease of contributions that, consistent with the literature on repeated PGGs (Fehr and Gächter, 2000), characterizes CONTROL sessions. In what follows, we confirm and further investigate these observations by employing OLS regressions.

Table 2: Cross-treatment results: actual contributions

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	4.911***	1.973***	4.943***	1.947***	0.738**	5.498***
	(0.986)	(0.736)	(0.974)	(0.737)	(0.367)	(0.977)
Treatment	1.570***	0.669*	1.392***	0.218	0.907***	0.673
	(0.358)	(0.402)	(0.424)	(0.438)	(0.140)	(0.414)
Treatment × Peer contr.		0.005		0.040		
		(0.048)		(0.050)		
Treatment × Round						0.058***
						(0.020)
Peer contr.		0.296***		0.296***		
		(0.044)		(0.043)		
H			0.389	1.076**		
			(0.462)	(0.456)		
Peer contr. × H				−0.081**		
				(0.041)		
Peer int. contr.					0.151***	
					(0.015)	
Shock size					0.039*	
					(0.021)	
Own contribution					0.512***	
					(0.029)	
Round	−0.028***	−0.017**	−0.028***	−0.017**	−0.013***	−0.065***
	(0.010)	(0.008)	(0.010)	(0.008)	(0.005)	(0.016)
Observations	4,680	4,524	4,680	4,524	4,524	4,680
R^2	0.045	0.266	0.047	0.269	0.443	0.049

Note: Dependent variable: actual (including forced) contributions; full sample. All featured variables except *Treatment*, *Round* and *H* are lagged. All estimates are run controlling for age, a dummy variable taking value 1 for females and a dummy variable indicating reported ethnicity different from "white". Clustered standard errors at the subject level in parentheses. Two-sided p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

We begin by comparing overall contributions between treatment and CONTROL sessions. This is done in Table 2, where the dependent variable, actual contributions, includes forced contributions. The independent variables shown are all lagged values except *Round* and the dummy variables *Treatment* equal to 1 for both treatments with forcing, and H equal to 1 only for the HIGH treatment. *Shock size* is the average size or amount, of the lagged forcing, that is the difference between actual and intended contributions. The coefficient on *Treatment* is always positive. In particular, it is significant in column (1), indicating that absolute contributions are larger with forcing and allowing us to conclude the following.

Result 1 *The presence of forced contributions raises overall contributions.*

In particular, Hypothesis 1 is confirmed with forced contributions *increasing* intended contributions.

It might seem unsurprising that forcing increases contributions — it has an obvious mechanical effect, given that in general, average contributions are much lower than 10 (see Figure 2). However, the treatment coefficient significantly decreases from 1.570 to 0.669 ($p = 0.000$), becoming only marginally significant, when we control for lagged contributions of peers, and interact this variable with the treatment dummy (column (2)). The reduction in the size of the coefficient for the treatment variable, together with the variable *Peer contr.* being positive and highly significant, show that in treatment sessions, subjects contribute more, at least in part, as a reaction to higher contributions by peers. Moreover, the interaction coefficient, which is close to zero, hints at a reaction in intended contributions. Indeed, if participants were choosing contributions comparable to those in the control sessions, we would observe a *negative* coefficient here — signaling the tendency to not reciprocate the forced component of a peer's past contribution. In other words, reciprocity means a positive relation between the contribution of a subject and the subsequent contributions of her peers: forcing is a random shock which, all else equal, should weaken this relation. However, overall, the degree to which participants reciprocate seems comparable between control and treatment sessions, since the coefficient on this interaction variable is small and not significant whenever it is included.

When disaggregating along the dimension of forcing probability (columns (3) and (4)), the treatment effect is lower in LOW than in HIGH sessions: this again is not surprising because in HIGH sessions more forcing events happen, so that participants are forced to give more (regardless of peers' past contributions).

In column (5), we look in more detail at the reaction to previous contributions. We split overall contributions in the previous round into own (*Own contr.*) and peers' intended contributions (*Peer int. contr.*) on one side, and the size of the forcing (if any) *Shock size* on the other. We hence find a strong significant autocorrelation between subjects' choices; and a comparatively small reaction (but still strongly significant) to peers' past intended contributions. [9] On the other hand, the size of the forcing has a small and non-significant effect (*Shock size*). We will analyze this later in even more detail, as this variable conflates forcing that affects a participant and forcing that affects her peers.

Finally, column (6) confirms the observation made in Figure 2 that the decay of contributions is attenuated under the forcing scheme — in fact, the sum of coefficients for "round" and "round $\times$ *Treatment*" is close to zero and non–significant ($p = 0.318$). That is, in treated groups, subjects actually do not contribute less over time.

We next look at effects on *intended* contributions in Table 3, which closely mirrors Table 2 except for the different dependent variable. We still find a positive and significant treatment effect in column (1), this time providing clean evidence that forcing increases participants' overall willingness to contribute. Indeed, we find a positive effect of the random assignment to a treatment session, allowing us to conclude what follows, and confirm hypothesis (H3).

Result 2 *The presence of forced contributions raises intended contributions.*

Column (2) reveals that this effect disappears when we condition on peers' previous contributions, and this variable interacted with the treatment: the treatment variable actually now has a negative coefficient. Although it is only marginally significant, it represents suggestive evidence that forcing can backfire, in the sense of decreasing *conditional* willingness to contribute. Results are consistent, although at most marginally significant, when we disaggregate between the LOW and the HIGH treatment (columns (3) and (4)).

[9] It is worth mentioning that subjects do not always observe *Peer int. contr.* (intended contribution of forced subjects remain hidden); but since forcing is random, what subjects observe does *on average* correspond to the intended contributions of a given group at a given round.

Table 3: Cross-treatment results: intended contributions

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	5.174***	2.282***	5.165***	2.219***	0.546	5.773***
	(1.055)	(0.765)	(1.062)	(0.791)	(0.344)	(1.045)
Treatment	0.768**	−0.681*	0.822*	−0.753*	0.085	−0.148
	(0.378)	(0.406)	(0.449)	(0.452)	(0.122)	(0.422)
Treatment × Peer contr.		0.057		0.080		
		(0.049)		(0.052)		
Treatment × Round						0.059***
						(0.021)
Peer contr.		0.294***		0.295***		
		(0.043)		(0.043)		
H			−0.118	0.182		
			(0.530)	(0.502)		
Peer contr. × H				−0.051		
				(0.047)		
Peer int. contr.					0.175***	
					(0.016)	
Shock size					0.020	
					(0.013)	
Own contribution					0.555***	
					(0.030)	
Round	−0.027**	−0.016**	−0.027**	−0.016**	−0.010***	−0.065***
	(0.011)	(0.008)	(0.011)	(0.008)	(0.004)	(0.016)
Observations	4,680	4,524	4,680	4,524	4,524	4,680
R^2	0.020	0.334	0.020	0.337	0.581	0.024

Note: Dependent variable: intended contributions; full sample, except where indicated. All featured variables except *Treatment*, *Round* and *H* are lagged. All estimates are run controlling for age, a dummy variable taking value 1 for females and a dummy variable indicating reported ethnicity different from "white". Clustered standard errors at the subject level in parentheses. Two-sided p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Column (5) again disaggregates past contributions into own and peers' intended contributions and the size of the forcing, featuring results analogous to those in Table 2.

Results from column (6) are also in line with those featured in Table 3: specifically, the sum of coefficients for "round" and "round $\times$ treatment" is close to zero and non–significant ($p = 0.318$), while the interaction coefficient itself is positive and strongly significant, allowing us to state the following.

Result 3 *The presence of forced contributions neutralizes the decay of contributions over repetitions.*

In order to get deeper insights on the mechanisms at play, and to test the two disaggregated hypotheses (H1) and (H2), we next focus on separately investigating the effect of forcing on a given subjects, or on her peers: this is done in Table 4. We again see the positive autocorrelation in contributions, in column (1), and in every column. In addition, we see a significant tendency to reciprocate in all the coefficients on *Peer int. contr.*. In columns (1) and (2), which feature a dummy for forced contributions and the size of the forcing effect, respectively, [10] we see that when a subject is forced to contribute, subsequent contributions decrease. However, effects of a peer being forced (columns (3) and (4)) are positive, but only significant for the size variable, not for the dummy variable.[11] [12] Results are consistent in coefficient sign and size, although only significant for *Shock size peer*, when we check for own and peer effects simultaneously (columns (5) and (6)).

Result 4 *Forcing results in an immediate decrease in contributions on behalf of the forced subject, which is larger the larger the extent of the forcing.*

This confirms hypothesis (H2); in particular, subsequent contributions of a subject are *negatively* affected by a forcing event. Results concerning hypothesis (H1) are less robust, but the available evidence suggests that a forcing event on a peer will actually *increase* subsequent contributions.

[10] Notice that while *Forced self* is a random unexpected shock, *Shock size self* is not: the extent of forcing is larger if the intended contribution was lower. Thus, it is important to include own lagged contribution among controls. Analogously, controlling for *Peer int. contr.* compensates for the non–randomness of *Shock size peer*.

[11] Notice that since groups have three members, there are twice as many observations with *Shock size peer*=1 than there are with *Shock size self*=1.

[12] Results do not change if we interact the forcing dummy with the forcing frequency (HIGH or LOW). *Forced peer* in column (1) is only marginally significant if we do not control for past contributions. These results are available upon request.

Result 5 *Forcing of a peer results in an immediate increase in contributions which is only marginally significant, but significantly increases with the extent of the forcing.*

Table 4: Within-treatment results

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.630	0.685	0.594	0.511	0.628	0.544
	(0.472)	(0.474)	(0.461)	(0.461)	(0.463)	(0.463)
Forced self	0.061				−0.203	
	(0.198)				(0.124)	
Forced peer	−0.020*					
	(0.012)					
Shock size self			0.197*		0.154	
			(0.103)		(0.110)	
Shock size peer		−0.036**				−0.020
		(0.016)				(0.017)
Own contribution				0.056***		0.052***
				(0.014)		(0.015)
Peer int. contr.	−0.006	−0.009**	−0.009**	−0.009**	−0.009**	−0.009**
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Round	0.555***	0.550***	0.555***	0.555***	0.555***	0.552***
	(0.039)	(0.040)	(0.039)	(0.039)	(0.039)	(0.039)
oth_tot_int_1	0.182***	0.182***	0.182***	0.191***	0.182***	0.190***
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Observations	2,958	2,958	2,958	2,958	2,958	2,958
R^2	0.615	0.614	0.614	0.616	0.615	0.616

Note: Dependent variable: intended contributions; sample restricted to sessions with forcing (both LOW and HIGH). All featured variables except *Treatment* and *Round* are lagged. All estimates are run controlling for age, a dummy variable taking value 1 for females and a dummy variable indicating reported ethnicity different from "white". Clustered standard errors at the subject level in parentheses. Two-sided p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4.1 Dynamic analysis

Results 2, 4, and 5 might seem contradictory, as forcing is found to significantly increase overall intended contributions, despite individual forcing events significantly *decreasing* forced subjects' subsequent contributions, while having an effect on peers' subsequent contributions that is positive but lower in absolute terms, and only marginally significant.

These observations, however, can be easily reconciled in two ways. First, by trivially observing that in groups of size three, a subject's peer is forced twice as often as the subject is.[13]

Second, we next consider the evolution of contributions in rounds that *do not* immediately follow a forcing event. In order to do so, we create a new variable "distance", defined for all observations that follow at least one forcing event at the group level, and recording the distance, in rounds, from the last event; for instance, "distance=1" means that a forcing event took place in the previous round. Looking at values of "distance" (from 0 to 8) for which at least two groups are observed, we find that between two forcing events, and controlling for usual covariates (round, age, female, ethnicity), contributions increase by 0.116 per round on average. When interacting "distance" with another variable "self" that takes value 1 if the last forcing event affected oneself and 0 if it affected a peer, we find that indeed contributions increase on average by 0.124 per round after a peer is forced. These results however are not significant ($p = 0.129$, $p = 0.162$) when we include clustered standard errors.[14] No similar trend is found for the forced subject's contributions.

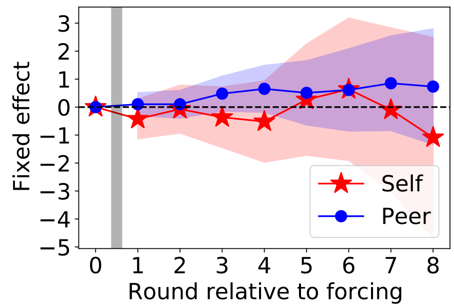
Figure 3 visually displays the same analysis where the "distance" variable was replaced with fixed effects for each value: while individual fixed effects are not significant, the overall increasing trend for "Peer" (referring to the non–interacted "distance" variable) can be observed.

Overall, while this evidence must be considered suggestive, as coefficients are not significant to conventional levels with clustered standard errors, it helps to reconcile empirically the apparent inconsistency of results 2, 4 and 5, by showing that the immediate negative effect for the forced subject is more than compensated not just by the immediate positive effect for peers, but also by the following dynamics of contributions. Beyond statistical significance, one word of warning is required concerning the interpretation: what appears to be a delayed, indirect effect of forcing, might also be driven by the *absence* of new forcing events in the following rounds. For instance, subjects may be

[13]The fact that, despite this, the self effect is significant but not the peer is likely due due to the smaller magnitude of the latter.

[14]$p = 0.006$, $p = 0.017$ if we omit clustered standard errors. These results are otherwise robust to the selection of any maximum distance between 5 (observed 60 times) and 8 (observed 18 times).

Figure 3: Contributions over time after a forcing event



Note: For ease of interpretation, both series are shifted so they take value 0 for distance=0. In other words, the coefficient of the "self" variable (-0.031, $p = 0.881$) — which is purely random, given the random assignment of forcing — is disregarded.

driven to contribute by the fact that a peer was forced to contribute *and they were not*, afterwards.

Another approach to analyzing the dynamic effects of forcing is to look at how its effectiveness changes over time, that is, to enrich specifications in columns (1) and (3) of Table 4 with the interaction of the forcing dummy and the round. If we do so, we obtain in both cases (with forcing on self and on peers) a negative but small and non–significant coefficient, again not providing any conclusive result.

5 Conclusions

Our experiment shows that policies that enforce higher contributions to public goods can foster not just overall contributions, but also *intended* contributions, when not enacted at every step of the contribution decision. Forced contributions appear to reinforce the trust– and reputation–building effect of intended contributions, as they neutralize the contribution decay over time typically observed in public good games.

In line with predictions of our model, the effect of forcing is more negative on the forced subject than on peers. Specifically, subjects react to being forced to contribute by *decreasing* subsequent contributions; instead, the larger the forcing of a subject, the higher the subsequent contributions of peers. The results also shed new light on the phenomenon of conditional cooperation. Subjects react to *actual* contributions in the same direction but to a much lower extent than if they were intended, which implies that decision reciprocity, not just a preference for fairness, seems to be a key driver for conditional cooperation.

Increasing the frequency of forcing events does not result in a proportional effect on intended contributions: in fact, if any, doubling the probability of forcing reduces this effect. This suggests that experimenting with different probability of forcing could reveal the optimal policy from the point of view of social welfare.

Examining the potential unintended consequences of a forced contribution in a repeated public good game provides an understanding of the behavioral effects of a policy of this kind. Clearly, having purely random forced contributions is a design choice made for ease of interpretation rather than for realism; however, it mimicks a large number of real world instantances, from tax audits to eligibility thresholds, in which the obligation to contribute to a public good is heterogeneous in the population. Our results can help policymakers evaluate the net effects of policies concerning public goods.

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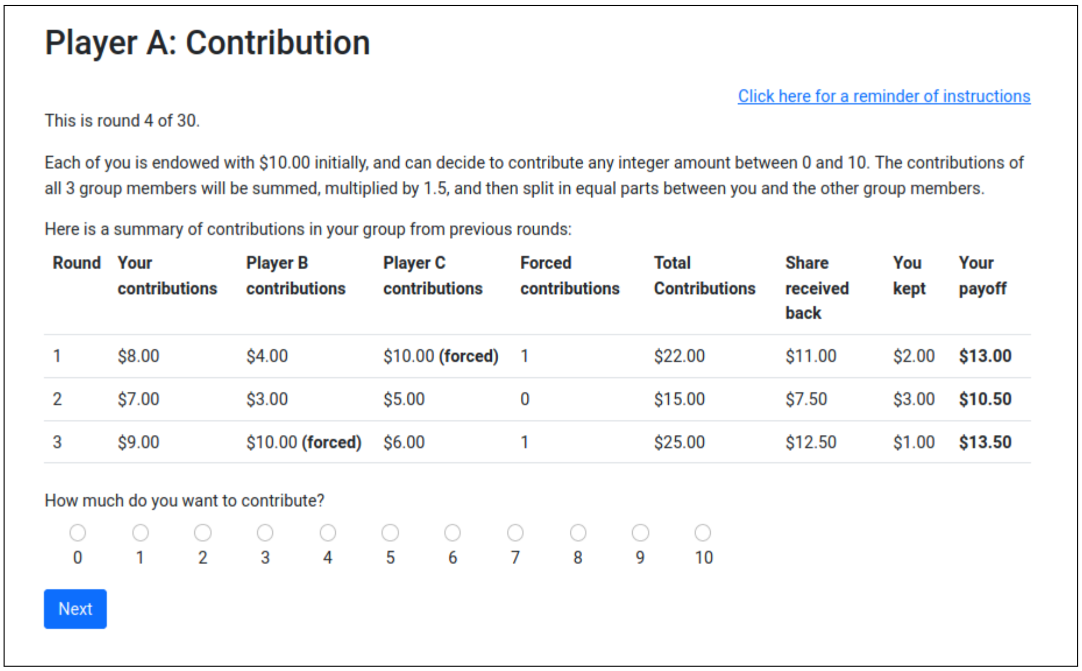
A Screenshots

The experimental software was developed in Otree (Chen et al., 2016). In the following, we provide screenshots for the *policy* treatment: the *control* treatment is simpler, in that some elements are not present.

Figure 4: Screenshot of the contribution phase



Player A: Contribution

Click here for a reminder of instructions

This is round 4 of 30.

Each of you is endowed with $10.00 initially, and can decide to contribute any integer amount between 0 and 10. The contributions of all 3 group members will be summed, multiplied by 1.5, and then split in equal parts between you and the other group members.

Here is a summary of contributions in your group from previous rounds:

Round	Your contributions	Player B contributions	Player C contributions	Forced contributions	Total Contributions	Share received back	You kept	Your payoff
1	$8.00	$4.00	$10.00 **(forced)**	1	$22.00	$11.00	$2.00	**$13.00**
2	$7.00	$3.00	$5.00	0	$15.00	$7.50	$3.00	**$10.50**
3	$9.00	$10.00 **(forced)**	$6.00	1	$25.00	$12.50	$1.00	**$13.50**

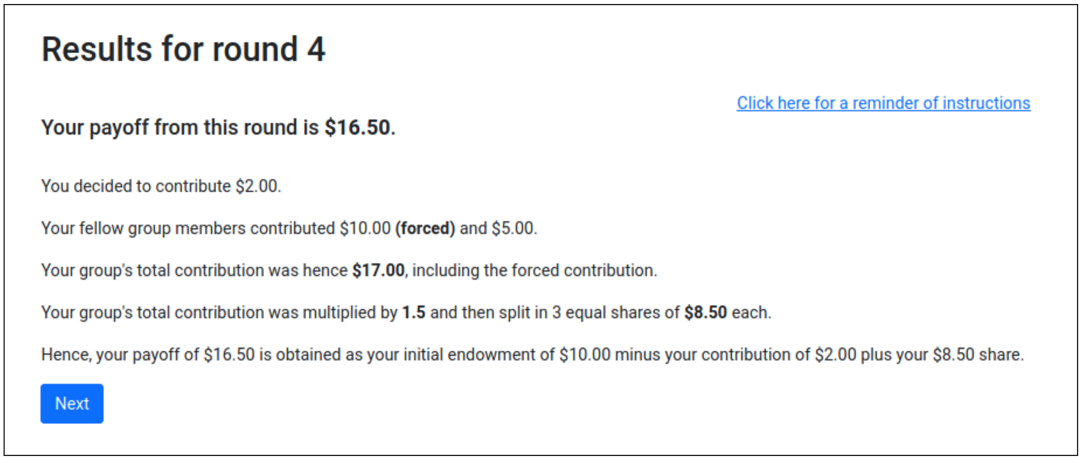
How much do you want to contribute?

0 1 2 3 4 5 6 7 8 9 10

Next

Figure 4 displays the step in which a participant is asked to determine their contribution; Figure 5 displays the step in which the participant sees the results of the round; Figure 6 displays the same step in a case in which the participant was forced to contribute.

Figure 5: Screenshot of the results phase



Results for round 4

Click here for a reminder of instructions

Your payoff from this round is $16.50.

You decided to contribute $2.00.

Your fellow group members contributed $10.00 **(forced)** and $5.00.

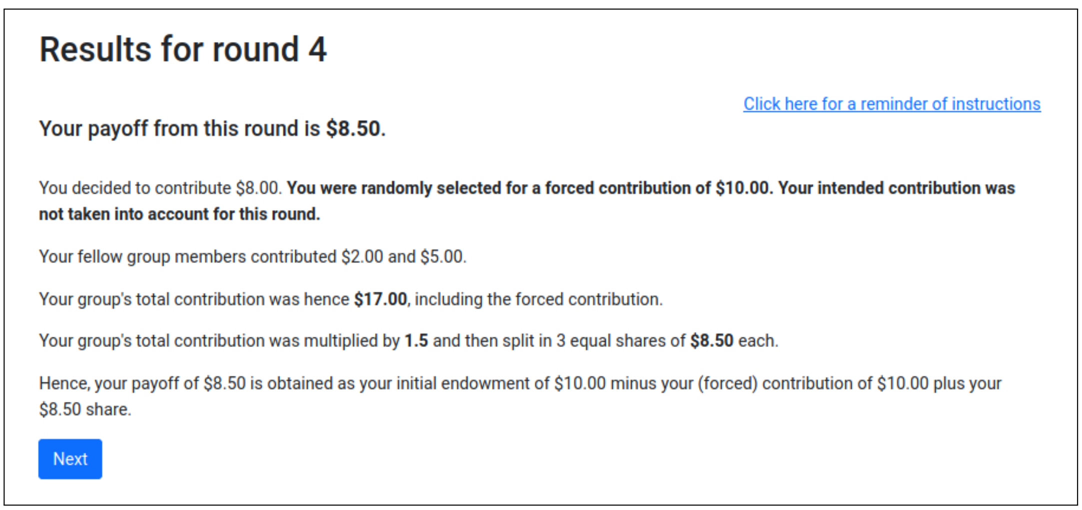
Your group's total contribution was hence **$17.00**, including the forced contribution.

Your group's total contribution was multiplied by **1.5** and then split in 3 equal shares of **$8.50** each.

Hence, your payoff of $16.50 is obtained as your initial endowment of $10.00 minus your contribution of $2.00 plus your $8.50 share.

Next

Figure 6: Screenshot of the results phase, with forced contribution



Results for round 4

Click here for a reminder of instructions

Your payoff from this round is $8.50.

You decided to contribute $8.00. **You were randomly selected for a forced contribution of $10.00. Your intended contribution was not taken into account for this round.**

Your fellow group members contributed $2.00 and $5.00.

Your group's total contribution was hence **$17.00**, including the forced contribution.

Your group's total contribution was multiplied by **1.5** and then split in 3 equal shares of **$8.50** each.

Hence, your payoff of $8.50 is obtained as your initial endowment of $10.00 minus your (forced) contribution of $10.00 plus your $8.50 share.

Next

B Instructions

The experimental instructions are reported below, for the LOW version. The "Forced contribution" panel was absent in the *control* version. Parts in **bold** were appropriately adapted for the HIGH version. Parts in *italic* were part of the script but they were only read aloud, not included in the on–screen instructions.

General instructions

Welcome, and thanks for your participation in this experiment. If you have a question at any time, please "raise your hand" or chat the experimenter.

- Please silence your phone, and be sure that your environment is free of all of other distractions.
- At the beginning of the experiment, you will be randomly assigned to a group of 3 people.
- During the entire experiment, you will interact only with the other 2 members of your group.
- The composition of your group will never be revealed.
- All members in your group are subject to the same rules.

Structure of the task

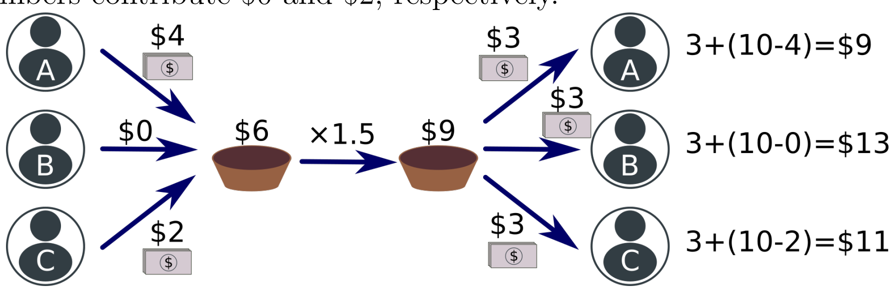
- There will be 30 rounds of the experiment.
- At the beginning of each round you will be given an account with \$10.
- You will then be asked to decide how much of your account you want to put into the pot. You must put in an integer dollar amount, with no cents. For example, you can put in \$0 or \$1 or \$2 or \$3 . . . up to \$10.

- The contributions of all 3 of your group members will be summed up, multiplied by 1.5, and split equally between the 3 group members.

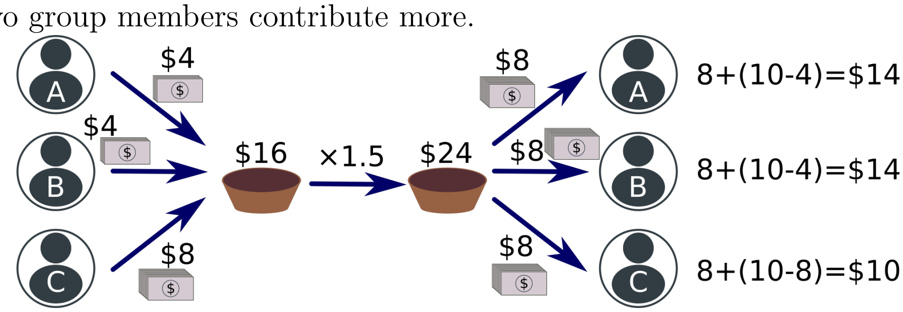
- Hence, your payoff from the round is your share of the pot plus the remaining amount that you didn't contribute out of your $10 account.

Below are a couple of examples.

In this first example, player A contributes $4, the other two group members contribute $0 and $2, respectively.



In this second example, player A still contributes $4, but the other two group members contribute more.



So as you can see, in general keeping money for one self is a guarantee to have that money at the end, but at the same time if participants contribute more, they get more as a group, because contributions are augmented.

Forced contributions

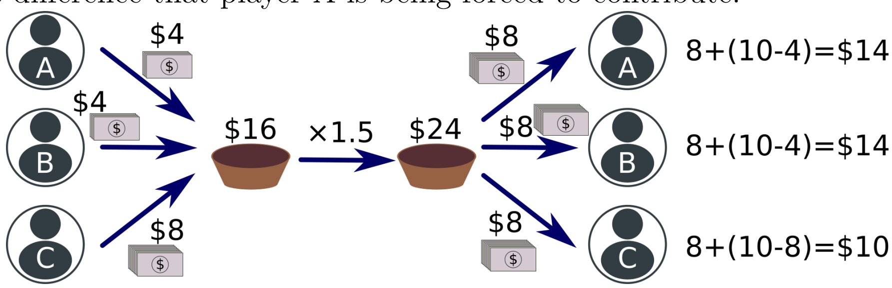
- Every round, after all of your group members have selected their contributions, with some probability one of the members of your group will be randomly selected to contribute their entire account.

The probability that *you* will be selected is **0.1** or **10%**. In other words, you can expect to be forced to contribute the full $10, your entire account, in **3** of the 30 rounds on average. The same applies to the two other members of your group.

- The computer will tell you if and when this happens to you, and when it does, it will contribute your entire account for you. Notice that in any given round, if you are forced, neither of your groupmates will be forced.

- If at a given round one of your groupmates was forced to contribute, you will be informed immediately after, and similarly they will be informed when you are forced.

Below is an example similar to the second previously shown, but with the difference that player A is being forced to contribute.



So if you prefer another way to state this is that your group is selected on average three rounds every ten, and when it is, one of the members will be randomly picked to select the entire contribution.

Notice that you might in principle never, or always, be forced: the numbers we give are only an average, and the fact that you were or not forced in the early rounds doesn't tell you anything about whether you will in the following ones.

Payoffs

- At the end of each round, you will be provided with a summary of the results of the round, including your payoff.

- At every step of the experiment, you will find in the top right of the page a link allowing you to access these instructions again in case you ant a reminder.
- Everyone gets a \$5 show-up fee. To calculate your earnings for the experiment, at the end of the session, one round will be randomly drawn by the computer. We will add your earnings from this round to the \$5 show-up fee.

One final advice before we start: if you happen to get an error screen in your browser, please just try to reload or reopen the same address, and you should be back precisely where you left.