Public goods and future audiences: acting as role models?*

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Abstract

Individuals' decisions to behave prosocially (or the contrary) can often be observed by other individuals, with no direct connection to them, but who may nevertheless be influenced by them (e.g. through social media). Does knowing that they may be viewed as role models by other, notably younger, people affect the way individuals behave? Does it make them more likely to behave prosocially? We study how participants' behavior in an experimental public good game is affected when they know that information about their choices and outcomes, together with different sets of information about their identity, will be transmitted the following year to a set of new, unknown, younger participants - with no payoff linkages between the two sets of players. When subjects know their photo, choices and outcomes will be transmitted, they contribute significantly *less*. We consider different possible explanations, and argue that the most convincing is based on image concerns, but in a surprising way: subjects in the photo treatment care about not being perceived as "suckers" by future players.

JEL classification: C91, C92, H41.

Keywords: Role models, image concerns, identity, audience, public goods.

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1 Introduction

There are many situations in which individuals' decisions to behave prosocially and cooperatively (or the contrary), can be observed in the future by other individuals, with no direct connection to them, but who may nevertheless be influenced by them. The widespread use of social media contributes to this visibility: a recent survey of 2000 US internet users aged 6-17 found that 34% of respondents chose "YouTube stars" when asked to select "the five people I look up to the most".[1] While only a small minority of individuals will become, sometimes to their surprise, social media "stars", many can achieve significant visibility, and potentially influence others.

Does knowing that they may be viewed as role models by other, notably younger, people affect the way individuals behave? Does it make them more likely, on average, to behave prosocially? This is a challenging question to answer with existing available data, as it is difficult to distinguish any role model effect from other influences on behavior. In this paper, we exploit the control afforded by a laboratory experiment. We study how participants' behavior in an experimental public good game is affected when they know that information about their choices and outcomes, together with some information about their identity, will be transmitted the following year to a set of new, unknown, younger participants - with no payoff linkages between the two sets of players.

The basic game in our experiment is a ten-period repeated voluntary contribution game (VCG) with partner matching. We explore two questions: first, does knowing that they will be acting as potential role models for future younger players have a significant impact on individuals' choices? Second, how does behavior vary with the degree of future visibility of identity?

We run two sets of experimental sessions. Subjects are recruited exclusively among graduate students for the first set of sessions, and exclusively among first-year undergraduates in the following academic year at the same university for the second set. Thus when information is transmitted, it is always from graduate student participants now to new undergraduates next year in the same university. We will refer to them as "first generation" and "second generation", respectively, for expositional convenience. We chose this feature of the design to make it as easy as possible for participants in the first set of sessions, the graduate students, to think of themselves as potential role models for participants in the second set of sessions, i.e., first-year undergraduates arriving the following year. The instructions for our treat-

[1] See https://www.mintel.com/press-centre/social-and-lifestyle/us-kids-say-social-media-stars-are-more-influential-than-athletes-actors-and-the-president.

ment sessions make the notion of acting as role models very salient, telling graduate participants that we will let first-year undergraduates playing the same VCG the following year "benefit from your 'knowledge' and 'experience'". The reference to knowledge and experience deliberately allows for different possible interpretations in our setting: being knowledgeable and experienced can be viewed as consistent with playing the no-contribution Nash equilibrium strategy in every period, but equally with building strategic reputation through positive contributions in early periods, leading potentially to Pareto-superior outcomes.[2]

The first set of sessions, with graduate students, is the focus of the present paper, since we are interested in the behavior of potential role models under different rules about information transmission[3]. It consists of three types of session. In the control sessions, subjects, randomly and anonymously assigned to groups of four, play a standard voluntary contribution game over ten periods. There is no information transmission. In the treatment sessions, there is transmission: subjects know that the history of play in their session (each player's contribution in each period), will be transmitted to first-year undergraduate participants in a future session. In addition, each subject is told that he/she will be individually matched to a subject in the future session, who will receive more personal information: his/her personal history in the game will be highlighted, and transmitted together with some personal characteristics (age, gender, nationality and academic status, i.e., Master or Ph.D student).

We have two treatments: the "information" treatment, exactly as just described, and the "photo" treatment, where transmission additionally includes a photo of the subject (graduate student). The photo is the only difference between the two treatments. This basic design enables us to study the two questions mentioned above. In particular:

(i) comparing behavior in the treatment groups with behavior in the control group allows us to investigate whether there is a significant effect associated with subjects' awareness of acting as potential role models, which is held constant in the two treatments while absent from the control condition[4];

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[2]Thus Andreoni and Croson [2008] remark: "with plenty of experience in a number of finitely repeated games, subjects will learn the benefits of reputation building".

[3]We provide some summary information about the second set of sessions in section 2, but analysis of data from these sessions is not the focus of the present paper.

[4]Specifically, our strategy aims at obtaining an upper bound on any such effect, by explicitly telling subjects that they will be acting as role models, and that their personal history of play, as well as characteristics, will be highlighted in the information transmitted. To the extent that this represents "experimenter demand", it does so by design (as in, for example, De Quidt et al. [2018]).

(ii) comparing behavior in the photo treatment with the information treatment makes it possible to explore the role of visibility of identity by future undergraduates.[5]

Our original conjecture was that increasing visibility would lead to higher contributions. This conjecture was motivated by the existing empirical literature on visibility and audience effects, which has generated considerable evidence of a positive effect: subjects tend to behave more prosocially and generously when their behavior can be observed by others than when it is private and anonymous[6].

Our experiment yielded two main findings. First, the average contribution rate in the information treatment was the same as in the control group: our experimental instructions encouraging graduate students to think of themselves as potential role models for future first-year undergraduates, and emphasizing their knowledge and experience, do not appear to have elicited higher (or lower) average contributions. The second and surprising result, contradicting our original conjecture, was that *subjects in our photo treatment*, far from contributing more, *contributed substantially less* than in the two other conditions. Our analysis shows that the photo treatment affected contribution decisions through two channels: *lower initial contributions* (in the first period), and *different dynamic behavior* over subsequent periods. Moreover, we allowed for a surprise restart game in our design, to test for learning effects[7]. We found that contributions in the photo treatment continued to be significantly lower during the restart game.

Our key finding then is that individuals appear to behave *less* prosocially and achieve *lower* levels of cooperation when they are aware that their behavior and identity will be visible to future, unknown and younger individuals facing similar circumstances. Why? We carefully examine the evidence for and against several possible explanations. We investigate whether subjects in the photo treatment were more likely to play Nash equilibrium zero-contribution strategies, or something very close to them: this conjecture is not supported by the data. We also find no evidence of a negative effect

[5]Visibility of identity by the experimenters is held constant for all three experimental conditions.

[6]See, for example, Alpizar et al. [2008], Andreoni and Bernheim [2009], Andreoni and Petrie [2004], Ariely et al. [2009], Bohnet and Frey [1999], Dana et al. [2006], Fox and Guyer [1978], Harbaugh [1998], List et al. [2004], Hoffman et al. [1996], Rege and Telle [2004], Soetevent [2005] . An exception is Dufwenberg and Muren [2006]: in their dictator game, dictators are less generous when their identity and decisions are observed by an audience of fellow students. However, several potential confounds are present in the study, partly because the design is intended to investigate gender effects and as a consequence the gender of the recipient is made salient to participants.

[7]Following Andreoni [1988].

of the degree of exposure to the teaching of economics on contribution rates in the photo treatment.

Given this, we consider three possible explanations for our findings. The first is that subjects in the photo treatment expect other participants to be less cooperative and adjust their own contributions accordingly. The second is that subjects in the photo treatment pay more attention to the game, and are less likely to make mistakes. The third is that subjects are reluctant to risk being perceived as "suckers" by future first-year undergraduates. The combined evidence from the original game (including elicited initial beliefs) plus the restart game, appears more consistent with this last explanation. In the concluding section, we discuss this result and its implications.

The remainder of the paper is organized as follows. We complete this section by discussing the relationship between our work and the existing literature. We then describe our experimental design in section 2. We present our first results in section 3, focusing on the first round (the VCG played over ten periods): here we show how average contributions evolved over time, and examine treatment effects for initial contributions and for total contributions over the game. Understanding the dynamics of contribution decisions over the ten periods requires a dynamic econometric model: we discuss our modeling approach in section 4, and present the results in section 5. We go on to study the surprise restart game in section 6. Finally, we discuss possible explanations for our results, and their implications, in section 7; we then offer some concluding remarks.

1.1 Literature Review

Role models play a part in theoretical analyses of cultural transmission: in Bisin and Verdier [2000], children who are not successfully "socialized" by their parents will acquire the traits of a role model instead (through imitation and learning). To the best of our knowledge, we are the first to investigate role models experimentally. Our work is also clearly related to the theoretical literature on image concerns, in which individuals care about the inferences that others will make based on their observable behavior: for example, inferences about how prosocial and disinterested they are (Bénabou and Tirole [2006]), or the extent to which they care about fairness (Andreoni and Bernheim [2009]).

Our result on contributions in the photo treatment is related to previous experiments that have varied the visibility of subjects' identity and contribution decisions in public good games. Andreoni and Petrie [2004] use photos, as we do, but for a different purpose: subjects in their photo treatment are identified to other members of their group (their co-players). The other treatment variable is information: subjects in their information treatment learn the contributions of each co-player. When identification and information are used together (information-and-photos treatment), contributions are substantially higher than in the control group. Rege and Telle [2004] also vary the visibility of subjects' choices and identity: in their "approval" treatment, each participant has to stand up in front of the others and write his contribution on a blackboard. This treatment increases contributions significantly. Our photo treatment differs from both these designs in a very important respect: we make identity visible not to current participants, who are co-players in the game, but to future, unknown and younger participants, with no payoff linkages.

More broadly, our paper is related to other work on the impact of identification and audience effects (see, e.g., footnote 6). Charness et al. [2007] explore the impact of a partisan audience on play in two experimental games, the Battle of the Sexes and the Prisoner's Dilemma. In their Face-to-Face experiments, participants are randomly assigned to the Row group or the Column group, who sit in separate rooms. Each subject plays with a member of the other group. To investigate audience effects, there is a treatment in which each subject plays once as a "Host", in front of other members of his own group, and once as a "Guest", in front of members of the other group. The presence of a partisan audience leads "Hosts" to behave more aggressively in both games. Our design investigates instead the impact of a non-partisan, future audience.

Since we have two "generations" (graduates and undergraduates), there is a link with the experimental literature on intergenerational games. In this literature each group of subjects represents a generation, and is replaced by another group (generation) when they finish playing[8]. A key feature of these games is the transmission of advice from each generation to the succeeding one, and the presence of monetary linkages between them, implying that each generation has a monetary stake in the behavior and outcome of the generation that will receive the advice. We differ from this literature in both respects (no advice and no monetary linkages), as well as in other ways, since we focus on quite different research questions.

Also somewhat related to the notion of role models is the literature on leader-follower public good games. These are sequential voluntary contributions games in which one subject in the group makes the first contribution decision (the leader), while the other subjects (the followers) make their

[8]The intergenerational approach was pioneered by Schotter and Sopher [2003, 2007], and developed in the context of public good games by Chaudhuri et al. [2006].

decisions after observing the leader's choice[9]. However, these games differ significantly from ours in several important respects, including the fact that the other players in the group are the audience, and the leader's payoff will depend directly on their choices as well as his own.

Finally, our transmission treatments can be thought of as making subjects' group identity - as graduate students and potential role models - particularly salient. Our results therefore contribute to the experimental literature on group identity[10]. Eckel and Grossman [2005] investigate the impact of varying induced group identity on behavior in repeated public good games. They find that simply assigning subjects to identifiable teams does not affect cooperation, but increasing team identification by having team members first cooperate on an unrelated task does increase cooperation. In a similar vein, we find that simply making group identity salient through our instructions to subjects (information treatment) does not have a significant impact on behavior. Combining this with visibility and identification by future players, on the other hand, does change behavior significantly.

2 Experimental design

2.1 Procedures

Participants in the first generation were graduate (Master or PhD) students in Economics and related disciplines at Bocconi University in Milan and at the University of Toulouse.[11] We chose such a specific population to ensure they would be credible as potential role models for first-year undergraduates playing the same experimental public good game the following year. None of the graduate student participants were being supervised by the experimenters, or attending any of their courses. They voluntarily showed up at experimental sessions after replying to E-mail or poster invitations. Experimental sessions in Milan were conducted in a computerized classroom of Bocconi University and subjects were seated at spaced intervals. Sessions in Toulouse were conducted at the Laboratory of Experimental Economics of the Toulouse School of Economics. The experiment was programmed and implemented using the z-Tree software (Fischbacher [2007]).

[9]See, for example, Arbak and Villeval [2013], Gächter et al. [2012], Güth et al. [2007], Levati et al. [2007], Potters et al. [2007], Rivas and Sutter [2011].

[10]See, among others, Charness and Sutter [2012], Chen and Li [2009], Chen and Chen [2011], Klor and Shayo [2010], Kranton and Sanders [2017], Kranton et al. [2018].

[11]In Toulouse, 67% of subjects were enrolled in an economics graduate programme and in Milan 45%. Other fields of study were mostly business administration, management, finance, and statistics.

We had 9 sessions (6 in Milan and 3 in Toulouse) with 16 subjects per session, hence 144 subjects in total. Each person could only participate in one of these sessions. Thus, we ended up having 2/3 (96/144) of the subjects participating in Milan and the remaining 1/3 (48/144) in Toulouse. Furthermore, 59% (85/144) were Master students and the remaining 41% (59/144) PhD students, with a good balance between the two locations. Average earnings were €37.50, including a €5 show-up fee; the average duration of a session was 65 minutes, including instructions and payment.

Participants in the second generation were first-year undergraduate students in Economics and related disciplines. We had 12 sessions with 16 subjects per session hence 192 subjects in total. Each person could only participate in one of these sessions. All the sessions were conducted in Milan (Bocconi University) a year after the corresponding first-generation sessions. Note that, although our initial intention was to match every first-generation participant to some second-generation participant in the same university, both logistic and financial constraints led us to eventually implement this matching only partially (only a subset of first-generation participants were actually matched to second-generation subjects). Given the constraints, we focused on one location, Milan. Average earnings were €19.25 including a €5 show-up fee. The average duration of a session was 40 minutes including instructions and payment. Since our focus is on potential role models' behavior, we will not further describe nor analyze the second generation behavior in the paper.[12] In what follows, we will restrict our attention to the first generation behavior.

2.2 Design

Treatments The experiment consisted of a voluntary contribution game (same for all conditions) and of three conditions (between-subject design), depending on whether subjects were told that behavior in the experiment and some of their personal characteristics would be transmitted, and whether transmitted identity features included a subject's photo. We had the same number of subjects (48) in each of the three conditions, with 32 subjects (2 sessions) and 16 subjects (1 session) participating in the experiment in Milan and in Toulouse, respectively.

[12] Unlike first-generation subjects, second-generation subjects were not told that any of their information would be transmitted to any subsequent generation. In other words, they were not in a position to act as role models.

Initial questionnaire A questionnaire about personal characteristics was submitted before the instructions. Each subject was asked his/her gender, age, nationality, year and field of study.

2.2.1 Voluntary contribution game

Stage game The stage game was the standard voluntary contribution game (VCG) of Andreoni [1988] and follow-up papers: Each subject, randomly and anonymously assigned to a group of n subjects, was given an initial endowment of 100 euro-cents (€1), and asked to allocate them between a public and a private account. The set of possible contributions to the public account included all integer numbers between 0 and 100. The marginal per capita return of a contribution to the public account was set to k/n with $k < n$. The subject's payoff was the sum of the per capita return of the group contribution to the public account and the amount of euro-cents left in the private account (initial endowment minus individual contribution). In our parametrization of the game, each group was made of $n = 4$ subjects, and the marginal per capita return was set at $k = 2$. Therefore, each euro-cent in the group account was doubled and then shared equally among the four subjects in the group. In other words, each individual in a group received half of the amount of the group contribution to the public account.

Repeated game: first round The stage game was initially repeated for 10 periods, under a partner matching design. Hence, once randomly formed, group composition remained the same during all the 10 periods. At the end of each period, subjects in a group were informed about the four ranked individual contributions and payoffs (for an example of how the information appeared on the subject's screen, see Figure 6 in the Appendix).[13] Before the first period of the repeated game, each subject was asked to guess the average contribution of his/her three co-players in period 1 (in integer numbers between 0 and 100 euro-cents).

[13]Notice that, due to ranked individual contributions, such feedback did not in general enable participants to obtain information about individual behavior across periods of another subject in the group. Indeed, if e.g. subject 1 contributed to the public account 100 euro-cents in period 1 and 0 euro-cents in period 2, with each of the other three subjects in the group contributing 10 in both periods, feedback was (100,10,10,10) after period 1 and (10,10,10,0) after period 2 on each computer screen. This did not allow any subject other than subject 1 to know that the highest contribution in period 1 and the lowest contribution in period 2 were made by the same subject (herself/himself).

Repeated game: second round As in Andreoni [1988] and follow-up papers, at the end of the 10 periods of VCG, participants were exposed to a surprise: they were offered the opportunity to play again the same 10-period repeated VCG under the same rules and parametrization of the first round, with partner matching, and within the same group as in the first round. This was a surprise restart game: participants did not know until the first round had ended that a second round would follow. Therefore, participants were offered the choice of ending the experiment and being paid only for the first round, or continuing the experiment and be paid also for the second round. Once the surprise was introduced, as in the seminal study by Andreoni [1988], we were explicit in pointing out that after the second round the experiment would be over, in order to make subjects aware that there would not be other surprises. All participants chose to continue the experiment with the restart game.

2.2.2 Treatment Manipulations

Section 2.2.1 described the control treatment. We had two treatment manipulations, "information" and "photo". Note that in all three conditions, subjects knew that their identity, choices and outcomes would be observed by the experimenters,[14] but their identity would never be revealed to their co-players, and could not be inferred by them.

The two treatment conditions differed from the control condition in the following way. At the end of the instructions, each subject was told that both his answers to the initial questionnaire (age, gender, nationality and academic level) and the history of his choices and outcomes in the VCG (contributions, earnings, and ranking within the group) would be transmitted to the first-year undergraduate student who, a year later, would be sitting in the same place,[15] playing the same VCG with other first-year undergraduates, under the same group matching.[16] In particular, in both conditions, subjects were

[14] Our study is comparative (behavior in the photo treatment is analyzed in contrast to behavior in the information treatment, which in turn is analyzed in contrast to behavior in the control treatment). Hence, the absence of anonimity toward the experimenter, which is quite common in repeated VCG like ours and held constant for all three conditions, should not affect our main comparative results.

[15] The time delay (one year) and the specification of "first-year" for undergraduates participating in the subsequent session were meant to make graduate students aware that subjects to whom their information would be transmitted would belong to a cohort of undergraduate students not yet enrolled at the university at the moment when the experiment was run.

[16] The subject was told that the same three computers that would be randomly assigned to his computer so as to form a group during his session would be assigned to the computer of the undergraduate student sitting in his place the following year.

told that we would transmit the information contained in the history table they would see at the bottom of their screen at the end of each period. Figure 6 in the Appendix shows an example: this specific history table would have been seen at the end of period 10, with partial versions being seen at the end of each previous period.

The photo and information conditions differed from each other in only one respect: in the photo treatment, each subject was also told that, in addition, his photo would be transmitted. The photo was taken during the initial questionnaire, with the subject seated in front of the computer screen, and the randomly assigned computer number easily visible. To ensure that participation was entirely voluntary in both treatments, we always gave subjects the option to leave the experiment at the end of all instructions; i.e. after learning what would be transmitted and how. In this case, the subject would be paid the show-up fee, and he would be replaced by another graduate student in the experiment (one who, offered the same option to leave, chose to stay).[17] In the information treatment, only 1 out of 48 subjects opted to leave and was replaced. In the photo treatment, 2 out of 48 participants opted to leave (in two separate experimental sessions), and were similarly replaced. Then, all actual participants filled in a release document for their photo. This document stated that their photo would be displayed on the same computer screen during all the experimental session attended by the randomly chosen undergraduate student a year later, and then destroyed at the end of that experimental session and no longer used. Then, the experiment started.

In summary, our experiment consists of three distinct conditions as described above: control, information, and photo. Observability by the experimenters is held constant for all three conditions. There is no transmission in the control condition, and hence no possibility for the graduate student participants to act as role models for future undergraduates. In both treatment conditions, there is transmission, and graduate participants are told that we will let future first-year undergraduates benefit from their knowledge and experience.[18] The only difference between the two treatments is the information that will be transmitted to future undergraduates: in one condition a photo is included, in the other it is not.

[17]In each treatment session, we recruited 18 potential participants, although only the first 16 were allowed to participate in the experiment. The last two recruited participants listened to the experimental instructions, and were paid the show-up fee when none of the other 16 participants opted to leave the experimental session.

[18]The precise wording is exactly the same in both treatments; see the Appendix.

2.2.3 Payment

At the end of the experiment subjects were paid in cash the sum of their payoffs in each period of the repeated VCG. They were also paid (€ 5) if their guess of the co-players' average contribution in the first period was correct (i.e., if the difference between the guess and the actual average contribution was less than or equal to 10 euro-cents).

All of the above held for both the first and the second round of the 10-period VCG, although subjects discovered that there was a second round (and that they would be paid for it) only at the end of the first one.

In treatments "information" and "photo", there was no extra payment for the transmission of information and history to the subsequent undergraduate session. Subjects were told that their earnings and the undergraduate students' earnings would be independent.

2.2.4 Instructions

The instructions used in the experiment are available in the Appendix.

3 Contribution patterns and differences between treatments

We start by examining contribution patterns and differences between treatments. In this section, we focus on the main game, i.e. the public good voluntary contribution game played over ten periods, described in detail in section 2. We will discuss the results for the subsequent (surprise) restart game later (in section 6).

Figure 1 shows average (mean) contributions in each period for the control group, and for each of the two treatments with transmission.

Average contributions in the control group start at 56%; they peak at 63%, falling to their lowest level, 28%, in the last period. This is broadly consistent with patterns observed previously in the experimental literature on public good games.[19] For the information treatment, average contributions start marginally higher (58%); they peak at 65%, falling to their lowest level of 25% in the last period. Finally, average contributions in the photo treatment start at a much lower level (45%), and remain substantially lower

[19]See Ledyard [1995]. Contribution rates are somewhat lower in Andreoni [1988], Andreoni and Petrie [2004]; a little higher in Chaudhuri et al. [2006]; quite similar in Croson [1996].

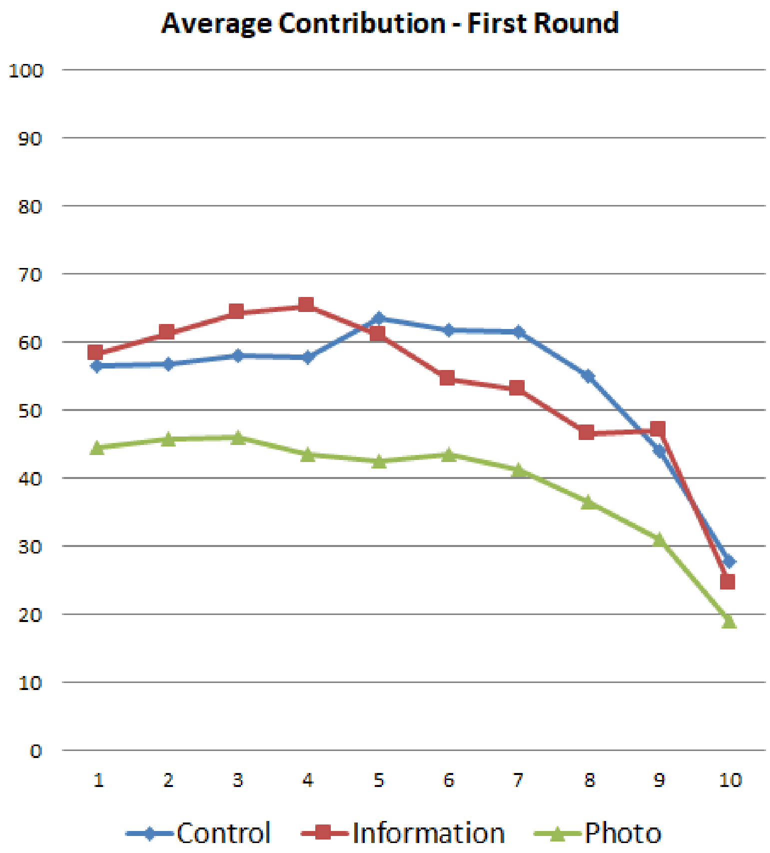

Figure 1: Average contribution across periods in first round

until the end, reaching the lowest level (19%) in the last period. As a consequence, average contributions for the game (pooling observations over the ten periods) are equal to 54% for the control group and the information treatment, but only 39% for the photo treatment.

The difference between the photo treatment and the other conditions is statistically, as well as economically, significant. The Kruskal-Wallis rank test rejects the equality of populations hypothesis at the 1% level ($p = 0.0067$). Pairwise comparisons between the photo treatment and each of the other groups using Dunn [1964]'s (1964) test with a Bonferroni adjustment for multiple comparisons reveal a highly significant difference in both cases ($p = 0.0088$ when comparing photo and control, $p = 0.0097$ when comparing photo and information)[20]. There is no significant difference between the control group and the information treatment. If we pool all observations of choices at every period and estimate a standard regression with individual contribution as the dependent variable, or a Tobit regression (to take into account the fact that the data is censored at 0 and 100), we find again that *contributions are significantly lower in the photo treatment*, controlling for participants' age, gender, education level (Master or PhD), location (Milan or Toulouse) and nationality (local or foreign), as well as a time trend (period) to allow for a decline in contributions as the end of the game approaches. The results are reported in Table 1. They show that participants' characteristics cannot explain the lower contributions in the photo treatment. In what follows, we investigate further the differences between treatments and possible explanations for our results.

Are subjects in the photo treatment more inclined to play the Nash equilibrium strategy?

To investigate this conjecture, Figure 2 presents histograms showing the distribution of contributions for the three experimental conditions, pooling observations for all ten periods of the game.

We see immediately from Figure 2 that the significantly lower average contributions in the photo treatment are not explained by subjects' greater propensity to behave in line with game-theoretic equilibrium predictions, leading to a much higher proportion of very low contributions in this treatment. Indeed, if we focus on zero contributions (the equilibrium prediction), the proportions are as follows: control group 21%; information treatment 19%; photo treatment 16%. On the other hand, there is a striking difference between the photo treatment and the other two groups when it comes to large contributions: these account for a much smaller proportion of contri-

[20] In both cases $N = 96$; we follow Andreoni and Petrie [2004] in taking as the unit of observation the average contribution per subject over the 10 periods.

	Regression	**Tobit regression**
Period	-2.5739***	-4.6890***
	(0.3300)	(0.5684)
Location	3.0103	7.0686*
	(2.3924)	(4.1153)
Gender	-6.3949***	-10.8698***
	(2.0289)	(3.4535)
Education	-0.5854	-2.7659
	(2.0984)	(3.5761)
Age	-0.7078**	-0.8307
	(0.3055)	(0.5199)
Nationality	-0.0095	0.2240
	(2.1877)	(3.7504)
Information	-1.7516	-3.4702
	(2.4970)	(4.2932)
Photo	-16.2494***	-23.5220***
	(2.4922)	(4.2620)
Constant	89.6799***	109.8855***
	(8.4383)	(14.3668)
Log likelihood		-5135.6661
R^2	0.0847	
χ^2		123.0048***
F	16.5510***	
N	1440	1440
N uncensored		828
N lower uncensored		269
N upper uncensored		343

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1: Regression results for contributions in first round (observations are pooled over all ten periods)

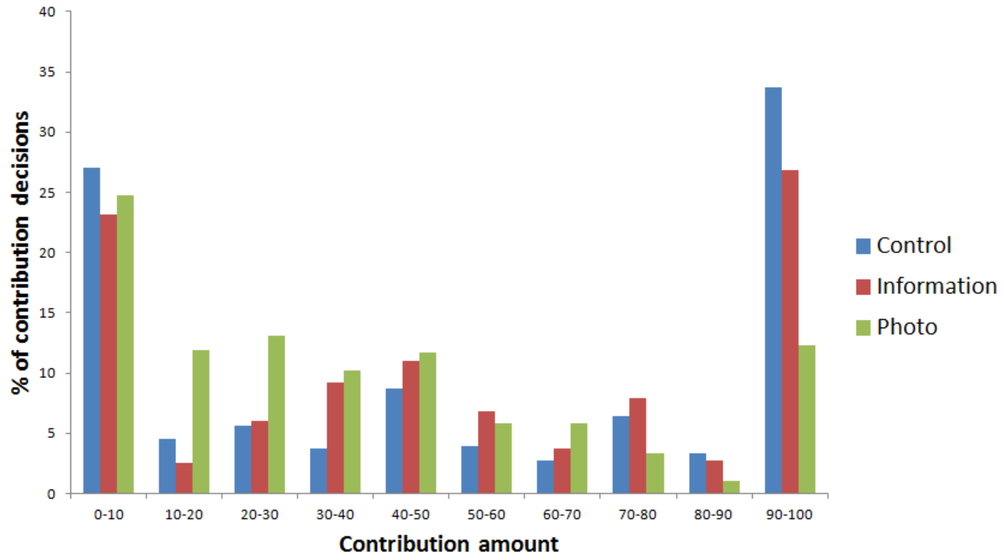

Figure 2: Distribution of contributions across conditions in first round (observations are pooled over all ten periods)

bution decisions in the photo treatment. The difference is mostly reflected in a higher proportion of moderate contributions (over 10% but not exceeding 40%) in the photo treatment.

Are contribution rates in the photo treatment higher for groups of non-specialists (in economics)?

We can complement the previous findings by checking whether the lower contributions in the photo treatment appear to be driven by the extent to which subjects have been exposed to Economics teaching. Specifically, is it the case that contributions are *higher* in groups where no participants are specialising in the study of economics, relative to those where at least some participants are indeed specialising in the study of economics? For expositional convenience, we will refer to students enrolled in an Economics Master or PhD programme as "specialists", while those enrolled in a Master or PhD programme in another subject, or a combination of subjects (e.g. Master in Law, Economics and Management, or Master in Finance and Information Technology) will be described as "non-specialists".

We can identify 12 subjects in the photo treatment who are non-specialists and played the game in groups consisting exclusively of non-specialists; i.e groups where the effect of economics teaching might be expected to be weaker. Their average contribution rate over the ten periods of the game was 35%. For the remaining 36 subjects in the photo treatment, the average contribution rate was 41%. Obviously the sample size is too small to say anything general about differences between specialist and non-specialist groups. We can note, however, that in our sample the contribution rate is in fact *lower* for the non-specialist groups. Thus we find no evidence that the low contributions in the photo treatment can be explained by the degree of exposure to economics teaching.

In order to better understand the reasons for our results so far, we need to investigate decisions in the first period, when subjects have not yet observed the behavior of other members in their group, and then study dynamic behavior over subsequent periods, when subjects can observe and respond to their co-players' choices. We begin by focusing on initial decisions.

Contributions in the first period

In the first period, average contributions are equal to 56% in the control group, 58% in the information treatment, and 45% in the photo treatment. Table 2 presents Tobit regression results for the initial contribution.

The first column in Table 2 shows that *initial contributions are significantly lower in the photo treatment*, controlling for age, gender, nationality, education level and location.

Why do photo treatment participants contribute significantly less in the first period? One possibility could be that the photo treatment affects subjects' initial beliefs, making them more pessimistic about their co-players' contributions and hence reducing their own contributions. We can investigate this possibility thanks to a feature of our experimental design described in detail in section 2: we elicited our subjects' beliefs about their co-players' average contribution just before the start of the game. The second column in Table 2 differs from the first by including this variable, denoted by BELIEFS. As expected, more optimistic beliefs about the average contribution by other players in the group increase own contributions. However, there is still a significant negative effect of the photo treatment on initial contributions, which is not explained by initial beliefs. Indeed, when we then checked whether initial beliefs depend on treatment, we did not find any significant effect of treatment on beliefs.

To shed more light on why initial contributions are lower in the photo treatment, we can repeat the earlier analysis of heterogeneity that we applied to pooled contributions for the entire game. Once again, the aim is to check whether the lower average contribution is due to a higher proportion of very low contributions. Figure 3 presents histograms showing the distribution of initial contributions for the three experimental conditions.

Clearly, the pattern of heterogeneity is similar to the one observed earlier: the lower average initial contributions in the photo treatment are not explained by a higher proportion of very low contributions. Indeed, for zero

	Init	**Init_bel**
Location	-6.2230	-9.9954*
	(9.5171)	(5.3069)
Gender	-7.8502	-4.0318
	(8.0584)	(4.4568)
Education	21.8135**	10.6982**
	(8.4283)	(4.7352)
Age	-2.0713*	-1.0495
	(1.2333)	(0.6960)
Nationality	10.0639	11.8943**
	(8.7188)	(4.8677)
Information	-6.8511	-10.2293*
	(10.0090)	(5.7351)
Photo	-21.7250**	-15.3608***
	(9.9782)	(5.6490)
BELIEFS		1.3566***
		(0.0949)
Constant	114.8566***	21.3330
	(33.2470)	(19.5481)
Log likelihood	-569.7983	-491.3946
χ^2	12.4331*	169.2406***
N	144	144
N uncensored	101	140
N lower uncensored	8	0
N upper uncensored	35	4

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Regression results for initial contributions in first round

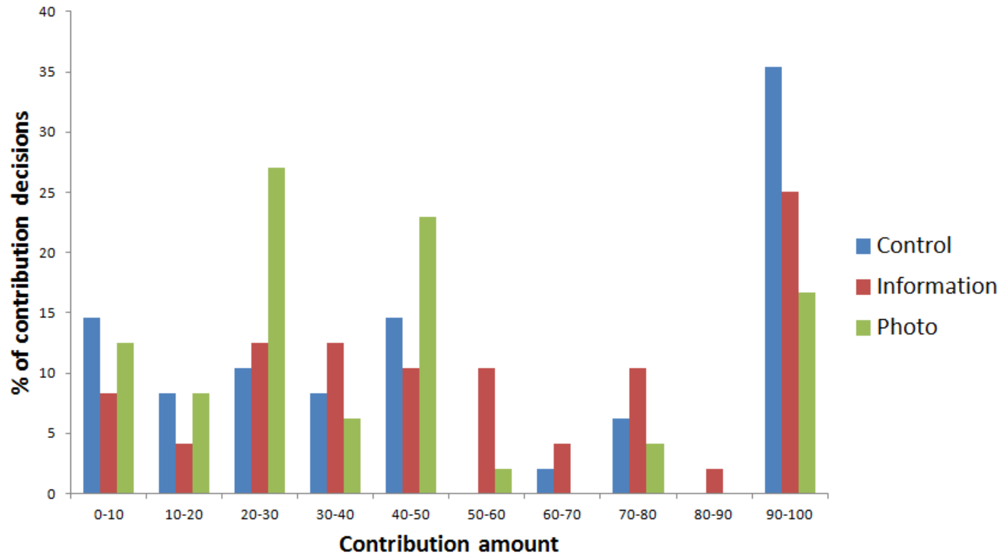

Figure 3: Distribution of initial contributions across conditions in first round

contributions, the proportions are 8.33% (control), 6.25% (information), and 2.08% (photo); while for contributions not exceeding 20% they are 22.92% (control), 12.5% (information), and 20.83% (photo).

We have established one channel through which treatment affects average contributions: the photo treatment has a significant negative effect on initial contributions. This does not appear to be explained by more pessimistic initial beliefs, nor by a greater propensity to behave in line with game-theoretic equilibrium predictions. In the next section, we turn to subjects' dynamic behavior. Does treatment affect the way participants respond to the choices made by other members of their group? To explore this question we need to study the dynamics of contribution decisions. We go on to do this in the following sections, starting with a discussion of the methodological issues involved.

4 Dynamics: methodology and econometric issues

The aim of the econometric analysis developed below is to identify the determinants of participant contributions in the panel data setup generated by our experiment, since we have repeated observations on a cross section of participants as they progress through the trials.

The basic relations to be investigated can be described as

$$contribution_{it} = f\left(contribution_{it-1}, avcontribution_{it-1}, controls_{it}\right)$$

where $contribution_{it}$ is the contribution of individual i at time t, $avcontribution_{it}$ is the average contribution of the rest of individual i's group at t and $controls_{it}$ a vector of other determinants (location, age, gender, etc.).

As an empirical approximation to this relation we can write the following linear stochastic equation:

$$contribution^{*}_{it} = \lambda contribution_{it-1} + \gamma DIFF_{it-1} + \beta' z_{it} + \delta time_t + u_{it}$$
$$\text{with } u_{it} = \alpha_i + \varepsilon_{it}$$

where $contribution^{*}_{it}$ is a latent variable reflecting the desired contribution of individual i at time t, $contribution_{it-1}$ the actual contribution of individual i at time $t-1$, the term $DIFF_{it-1} = contribution_{it-1} - avcontribution_{it-1}$ is the difference between own lagged contribution and the average contribution of the other three members of individual i's group at time $t-1$, z_{it} a set of individual specific conditioning variables (that may or may not be time varying) and $time_t$ some form of time trend (we experiment with various formulations to capture the possibility that contributions decline as t approaches the end of the experiment). The error u_{it} is a random disturbance term reflecting any omitted variables or other sources of randomness. We assume u_{it} comprises two components, an individual specific random effect α_i and an idiosyncratic error term ε_{it} that is assumed independent of the z's and α's. Notice that it is the lagged outcome that is included, not the lagged latent variable. This is appropriate in our setup where the truncation occurs due to corner solutions and not as a result of top and bottom coding the data.

The coefficient λ captures a degree of persistence in an individual's contribution whilst the coefficient γ captures the effect of an individual's contribution differing from the average contribution of the remainder of his/her group in the previous period. We shall also allow for an asymmetry in this effect by including an extra term of $DIFF_{it-1}$ interacted with a dummy capturing when $DIFF_{it-1}$ is positive (we call this variable $POSDIFF_{it-1}$). This allows the impact of $DIFF_{it-1}$ to be different depending on whether it is positive (so in period $t-1$ individual i contributed *more* than the average of his/her co-players) or negative (in period $t-1$ individual i contributed *less* than the other group members' average).

The observational rule is then:

$$\begin{array}{lll} contribution_{it} & = 0 & \text{if } contribution^*_{it} < 0 \\ contribution_{it} & = 100 & \text{if } contribution^*_{it} > 100 \\ contribution_{it} & = contribution^*_{it} & \text{otherwise} \end{array}$$

A number of econometric issues arise with this specification, particularly when we include the individual effect, i.e., $u_{it} = \alpha_i + \varepsilon_{it}$. Firstly there is an essential non-linearity in that the observed outcome $contribution_{it}$ is truncated at 0 and 100 (and in our experiment there is a significant degree of censoring at these boundaries) so that a maximum likelihood tobit estimator is appropriate.

Secondly relative to a standard random effects panel with exogenous regressors the presence of the lagged dependent variable $contribution_{it-1}$ (both on its own and as a component of $DIFF$) is problematical. The lagged dependent variable interacts with the individual effects to generate biases in the estimated parameters (see Nickell [1981], Hsiao [1986]). This bias can be severe particularly if the time dimension of the panel is small. In a linear framework there are well known methods to eliminate this bias by first differencing the data to eliminate the individual effects and then using an IV or GMM approach to deal with the induced endogeneity in the resulting transformed model (Anderson and Hsiao [1982], Arellano and Bond [1991]). In a non-linear model this approach will not work. In general for non-linear panel models there need not be a transformation that can eliminate the individual effects and produce a viable set of moment conditions for estimation. This problem has attracted a great deal of attention and some progress has been made in the context of certain specific non-linear models in deriving exact inferential procedures. An alternative that we follow here is due to Wooldridge [2005]. Here the idea is to specify auxiliary (conditional) distributional assumptions for the individual heterogeneity. The disadvantage of this approach is that misspecification arises if this assumption is violated. The advantage is that this can give rise to a relatively straightforward maximum likelihood estimation.

To implement Wooldridge's suggestion we here specify the individual effects as:

$$\alpha_i = \alpha_0 + \alpha_1 contribution_{i1} + \alpha_2^{'} \bar{z}_i + \zeta_i$$

where $\bar{z}_i$ contains the time averages of the exogenous variables $(avcontribution_{it-1}, z_{it})$ in the sample and ζ_i is a normally distributed error term independent of these variables and the ε_{it}. Estimation then proceeds by substituting the fixed effects in the regression with these additional variables, and estimating the resulting model by maximum likelihood tobit procedure.

5 The dynamics of contributions: results

This section presents the main results from our econometric analysis of the dynamics of contributions over the ten periods of the first round. The second round (surprise restart game) will be analyzed in the following section.

5.1 Pooled data

To begin with, we pool observations for the three experimental conditions. Table 3 shows results for the symmetric and asymmetric model, where the dependent variable is the contribution by player i at time t. Recall that the symmetric model assumes responses of equal magnitude when a subject observes a difference, positive or negative, between his/her last contribution and the last average contribution by his/her co-players; while the asymmetric model allows for different magnitudes of response, and in particular for the possibility that individuals respond more (less) when they see that they have contributed above (below) the average. The first two columns in the table are based on our preferred specification, the dynamic tobit using Wooldridge's method, as discussed in detail in the previous section. We find a large and significant effect for own lagged contribution ($LCONTRIBUTE$) and for the difference between own lagged contribution and the average of the three co-players' lagged contributions ($DIFF$).[21] Thus individuals tend to make a similar contribution to the one they made in the previous period, but adjust this in the light of their co-players' behavior in that period: if the other players in the group contributed more on average, they revise their own contribution upwards, while if the others contributed less, they revise their own contribution downwards.

Comparing the symmetric model (column 1) and the asymmetric model (column 2), we see that the latter seems more appropriate for our data: the coefficient on the asymmetric effect (represented by the variable $POSDIFF$) is highly significant. Thus when they realize they have been contributing more than the average, individuals decrease their contribution *more* than they increase it after discovering that they contributed less than the average.

We also include, for comparison, the results from random effects tobit regressions (although subject to bias as discussed in the previous section), for the symmetric model in column 3 and for the asymmetric model in column 4. They are consistent with the main findings discussed above: once again,

[21] Note that the total effect of the lagged dependent variable is captured partly by the coefficient of $LCONTRIBUTE$ but also partly by the coefficients of the variables measuring the difference between own lagged contribution and average lagged contribution by the other members of the group.

	Dynamic (Wooldridge) Tobit		RE Tobit	
	Symmetric	**Asymmetric**	**Symmetric**	**Asymmetric**
LCONTRIBUTE	1.3162***	1.2741***	1.3462***	1.3218***
	(0.0874)	(0.0874)	(0.0682)	(0.0684)
DIFF	-0.6995***	-0.4163***	-0.6646***	-0.4414***
	(0.0796)	(0.1046)	(0.0672)	(0.0946)
POSDIFF		-0.5120***		-0.4126***
		(0.1265)		(0.1265)
Period	-3.6710***	-3.8291***	-3.6356***	-3.7504***
	(0.4939)	(0.4943)	(0.4929)	(0.4931)
Nationality	-2.2095	-2.9454	-1.1620	-1.6565
	(4.0662)	(3.9554)	(4.1492)	(4.1060)
Gender	-5.3873	-3.8582	-6.2832	-5.1519
	(3.7405)	(3.6529)	(3.8259)	(3.7997)
Education	-9.7306**	-9.5366**	-5.8839	-5.3744
	(3.9683)	(3.8543)	(3.9543)	(3.9125)
Age	0.7075	0.8661	0.3622	0.4506
	(0.5688)	(0.5538)	(0.5759)	(0.5701)
Information	1.1111	-1.7060	0.5990	-1.7767
	(4.6328)	(4.5588)	(4.7344)	(4.7412)
Photo	2.5248	-0.4309	0.1228	-2.7120
	(4.8515)	(4.7646)	(4.8196)	(4.8447)
Location	7.0911	6.7754	6.2924	6.0198
	(4.4577)	(4.3317)	(4.5511)	(4.5018)
Constant	-21.7443	-16.9087	-4.4035	1.9594
	(17.1913)	(16.7394)	(16.6284)	(16.5723)
Log likelihood	-4186.5575	-4178.3598	-4196.7995	-4191.4658
Wald χ^2	579.5878***	602.6511***	523.1235***	529.5387***
N	1296	1296	1296	1296
N uncensored	727	727	1296	727
N lower uncensored	261	261	261	261
N upper uncensored	308	308	308	308
Nb groups	144	144	144	144

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Tobit regression results for contributions in first round

the estimated coefficients for $LCONTRIBUTE$, $DIFF$ and $POSDIFF$ are economically important and statistically significant.

The estimates presented in Table 3 are obtained from the full sample, pooling data from all three experimental conditions. They demonstrate the importance of allowing for dynamics, and for asymmetric responses to co-players' behavior. As for treatment effects, the only way in which treatment is allowed to influence contributions is through inclusion of two treatment dummies. This would imply an intercept effect, essentially shifting contributions up or down. However, we might expect treatment to affect the coefficients: for example, treatment could influence the way participants respond to their co-players' behavior. In this case, the estimates in Table 3 would be subject to a specification error. To investigate this possibility and shed light on how treatment affects behavior, we go on to estimate our model separately for each experimental condition.

5.2 Treatment effects

We begin with our preferred specification, the dynamic tobit using Wooldridge's method. Table 4 presents the results from the symmetric and asymmetric model, for each experimental condition.

The estimates for each treatment do differ in several important respects. First, when we estimate the symmetric model we find that the *responsiveness to the average contribution by co-players*, measured by the coefficient of the variable $DIFF$, is very similar for the control and information treatment, but much *lower* for the photo treatment. Second, the asymmetric model reveals important differences between the photo treatment and the other two experimental conditions. In the control and information conditions, the estimated coefficients for $DIFF$ and $POSDIFF$ are both significant, and similar in magnitude.[22] Thus participants in these conditions would *increase* their contributions after observing that their co-players had contributed more, and *decrease them, proportionately more*, after observing that the other members of their group had contributed less, on average. For the photo treatment, however, only the estimated coefficient for $POSDIFF$ is significant: in this condition, participants do *not* appear to increase their contributions significantly in response to higher contributions by co-players; they only reduce them after observing that the average for the other members of the group was lower. This difference in dynamic behavior points to a second channel

[22] The coefficients for $DIFF$ are significant at the 5% (control) and 1% (information) levels; for $POSDIFF$ they are significant at the 10% (control) and 5% (information) levels.

	Control		**Information**		**Photo**	
	Symmetric	**Asymmetric**	**Symmetric**	**Asymmetric**	**Symmetric**	**Asymmetric**
LCONTRIBUTE	1.7677***	1.6809***	1.3715***	1.3450***	0.9941***	0.9906***
	(0.2128)	(0.2143)	(0.1616)	(0.1620)	(0.1084)	(0.1076)
DIFF	-0.8854***	-0.5973**	-0.7871***	-0.5431***	-0.5383***	-0.2308
	(0.1812)	(0.2435)	(0.1465)	(0.1828)	(0.1057)	(0.1437)
POSDIFF		-0.4882*		-0.4844**		-0.5408***
		(0.2860)		(0.2219)		(0.1769)
Period	-4.2916***	-4.5912***	-4.2222***	-4.1451***	-2.6950***	-2.8795***
	(1.2082)	(1.2131)	(0.9075)	(0.9079)	(0.6005)	(0.6011)
Nationality	-4.8885	-5.2070	1.8758	0.8296	0.0419	-0.7518
	(9.6593)	(9.7280)	(6.2565)	(6.0080)	(4.6067)	(4.3311)
Gender	-4.8990	-3.1582	-11.5506	-10.2720	-5.8347	-4.1521
	(9.2887)	(9.4073)	(7.0525)	(6.7936)	(3.5515)	(3.3765)
Education	-17.6463*	-15.9641*	-16.9128***	-17.8239***	6.7630	7.0442
	(9.5991)	(9.7026)	(6.5237)	(6.2664)	(5.9741)	(5.6195)
Age	0.9374	1.1377	0.4116	0.4909	0.2098	0.0849
	(1.0251)	(1.0398)	(1.4076)	(1.3461)	(1.0554)	(0.9943)
Location	12.1381	11.9643	3.8536	3.5628	-5.2232	-3.1364
	(10.7954)	(10.8800)	(8.0823)	(7.7306)	(5.1005)	(4.8504)
Constant	-49.6517	-45.7118	-6.0817	-1.8241	2.8733	7.6657
	(37.3527)	(37.6588)	(35.1604)	(33.7028)	(25.3087)	(23.9012)
Log likelihood	-1210.0717	-1208.6195	-1329.7895	-1327.4116	-1592.4711	-1587.8581
Wald χ^2	129.7343***	133.2879***	238.6824***	249.0444***	280.6883***	305.7947***
N	432	432	432	432	432	432
N uncensored	191	191	229	229	307	307
N lower uncensored	98	98	88	88	75	75
N upper uncensored	143	143	115	115	50	50
Nb groups	48	48	48	48	48	48

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Dynamic Wooldridge Tobit regression results for contributions across each treatment in first round

through which the photo treatment reduces overall contributions, in addition to the effect on initial contributions examined in section 3.

The results summarised in Table 4 suggest that treatment effects in our experiment cannot be adequately captured by including treatment dummies in specifications with pooled data. We test this formally, and indeed find that the restricted model (pooled data estimations) is rejected relative to the unrestricted model (separate estimations for the three experimental conditions).

For comparison, we again obtain estimates for each experimental condition using the random effects tobit specification: these are presented in Table 5. Comparing this with the previous table shows important differences in the estimated $POSDIFF$ coefficients: these underscore the value of correcting for potential bias by using the Wooldridge method. Nevertheless, although subject to bias, the estimates in Table 5 confirm that *participants in the photo treatment are less responsive to their co-players' behavior overall,* and that *this lower responsiveness is associated with a substantially lower willingness to increase their contributions.*

The analysis in this section sheds some additional light on the reasons for lower average contributions in the photo treatment: when their personal identity is salient and observable by future players, subjects appear to react differently to the behavior of other players in their group. In particular, they are less responsive overall, and substantially more reluctant to raise their contributions after observing higher average contributions by their co-players.

5.3 Discussion

At this point it is worth reviewing our main results so far, and possible explanations. We have found that behavior in the photo treatment differs significantly from the other two conditions. In particular, the photo treatment is associated with:

(i) lower initial contributions;

(ii) lower responsiveness to co-players;

(iii) responsiveness more focused on reducing contributions, when higher than co-players' average.

How can we account for these differences? The results do not appear to stem from the desire to set an example for future undergraduates to follow: if this played an important role, we would also observe a significant difference between the control group and the information treatment, which is not the case. In this sense, we find no evidence of a role model effect.

	Control		**Information**		**Photo**	
	Symmetric	**Asymmetric**	**Symmetric**	**Asymmetric**	**Symmetric**	**Asymmetric**
LCONTRIBUTE	1.7791***	1.7182***	1.4410***	1.4215***	1.0360***	1.0661***
	(0.1846)	(0.1885)	(0.1191)	(0.1188)	(0.0760)	(0.0777)
DIFF	-0.8275***	-0.6021***	-0.7735***	-0.5942***	-0.4944***	-0.2698**
	(0.1569)	(0.2219)	(0.1213)	(0.1664)	(0.0900)	(0.1269)
POSDIFF		-0.4028		-0.3403		-0.4234**
		(0.2876)		(0.2197)		(0.1750)
Period	-4.2933***	-4.5391***	-4.0837***	-4.0270***	-2.6469***	-2.7526***
	(1.2168)	(1.2221)	(0.8882)	(0.8887)	(0.5990)	(0.6000)
Nationality	-3.5450	-3.7105	2.1704	1.4534	0.8184	0.3283
	(9.8375)	(10.0120)	(6.3716)	(6.2701)	(4.6148)	(4.3925)
Gender	-10.0428	-9.0521	-7.4792	-6.1583	-6.1759*	-4.9249
	(9.2464)	(9.4345)	(6.8989)	(6.8226)	(3.5914)	(3.4635)
Education	-11.0878	-9.4151	-15.4938**	-16.0354**	9.7348	10.0827*
	(9.2130)	(9.4429)	(6.5643)	(6.4508)	(6.0737)	(5.8410)
Age	0.3636	0.4585	1.4558	1.6330	-0.5231	-0.6768
	(1.0115)	(1.0313)	(1.3826)	(1.3606)	(1.0563)	(1.0148)
Location	11.8607	11.5801	4.5343	4.2491	-4.9879	-3.5198
	(10.9704)	(11.1710)	(7.7452)	(7.5996)	(5.0584)	(4.8732)
Constant	-21.1888	-12.8671	-28.5192	-28.3366	26.1036	32.3608
	(32.4333)	(33.3294)	(34.8527)	(34.1689)	(25.0386)	(24.3398)
Log likelihood	-1213.0597	-1212.0806	-1333.1315	-1331.9297	-1597.7386	-1594.8357
Wald χ^2	113.3674***	115.1344***	222.7732***	226.0913***	249.6721***	256.8093***
N	432	432	432	432	432	432
N uncensored	191	191	229	229	307	307
N lower uncensored	98	98	88	88	75	75
N upper uncensored	143	143	115	115	50	50
Nb groups	48	48	48	48	48	48

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Random effect Tobit regression results for contributions across each treatment in first round

Another possible explanation might be that subjects, when told we would let future undergraduates benefit from their 'knowledge' and 'experience', were influenced to act in line with received teaching, and in particular the Nash equilibrium prediction of zero contributions in the standard finitely repeated voluntary contribution game (ignoring the possibility of strategic reputation building). We saw in section 3 that subjects in the photo treatment were not more likely to play the zero-contribution strategy, or something close to it, than subjects in the other experimental conditions. Could it be that the experimental instructions' mention of knowledge and experience nevertheless primed subjects to associate "being smart/rational" with "playing selfishly", and hence induced them to contribute less?[23]

If this had been the case, we should have observed lower contributions in both the information and the photo treatment, whose instructions contained exactly the same wording concerning knowledge and experience, relative to the control group, whose instructions did not include that part. We found instead that the photo treatment had significantly lower contributions than both other conditions, while there was no significant difference between the control group and the information treatment.

Two other possible explanations suggest themselves. First, subjects in the photo treatment cared about how they would be perceived by future first-year undergraduates, and this led them to contribute less. We will return to this possible explanation in section 7. Second, participants in the photo treatment paid more attention to the game, and as a consequence made fewer contributions due to mistakes and confusion about how the game should be played. If this were the correct explanation, we would expect the difference between the photo treatment and the other two conditions to diminish in the restart game, as subjects learn from their experience of playing the first game. We examine the evidence from the restart game in the next section.

6 The restart game

In this section we present the results for the second round, which was a surprise restart game: participants did not know until the first round had ended that a second round would follow. The addition of the restart game, following Andreoni [1988], is intended to test for learning effects. As in Andreoni [1988] and Croson [1996], we find that average contributions increase

[23] The idea that the teaching of Economics modifies students' behavior towards greater selfishness has been investigated in a number of empirical papers (see, among others, Bauman and Rose [2011], Frank et al. [1993], Frey and Meier [2003], Marwell and Ames [1981]).

markedly between the last period of the first round and the first period of the second round, in all three experimental conditions: this can be seen by comparing Figure 4 with Figure 1.

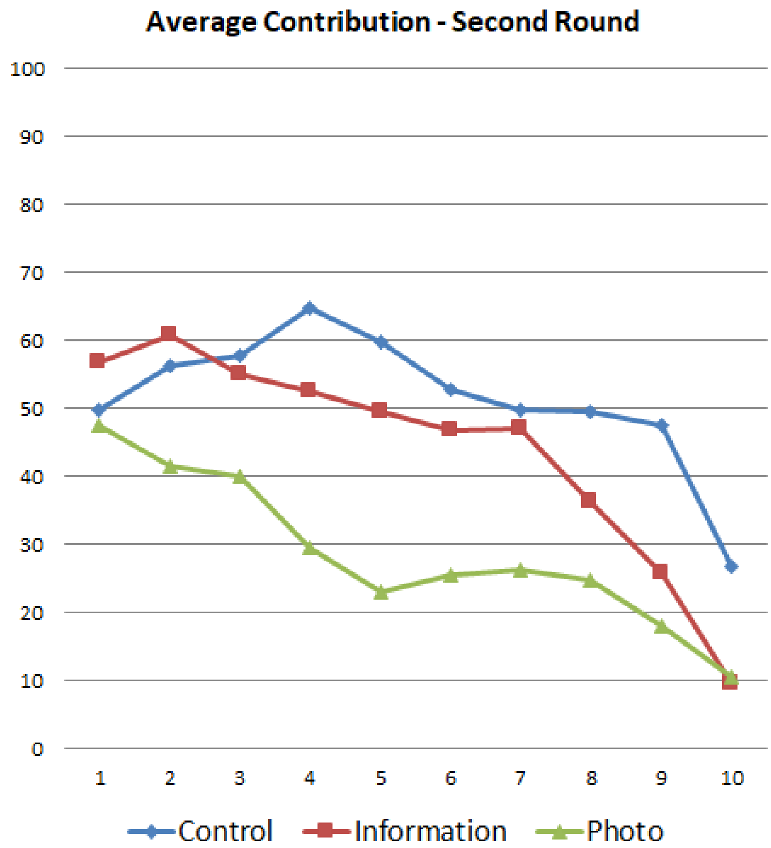

Figure 4: Average contribution across periods in second round

Nevertheless, *average contributions in the photo treatment continue to be lower than in the other two conditions, throughout the second round.* Looking at distributions of contributions over the 10 periods in Figure 5 shows that, even though the proportion of very low contributions is now higher for the photo treatment than for the other groups, the lower average in the photo treatment is due primarily to a much lower proportion of large contributions, in favor of more moderate ones, as in the first round. We also repeated the analysis applied earlier to the data for the first round: in particular, Table 6 reports the estimates for our preferred specification, the dynamic tobit using Wooldridge's method. The symmetric model gives qualitatively similar results to those for the first round: once again, participants' responsiveness to their co-players' average contributions is lower in the photo treatment. For the asymmetric model, we find that in all three experimental conditions, the coefficient for $DIFF$ is insignificant while the one for $POSDIFF$ is highly significant. Thus participants' (positive) responsiveness when their co-players contribute more is no longer significant, even in the control and the information conditions. However, participants in the photo treatment are more responsive (negatively) than in the other conditions when their co-players contribute less. Thus while there appear to be some learning effects, they do not eliminate the significant difference between the photo treatment and the other experimental conditions.

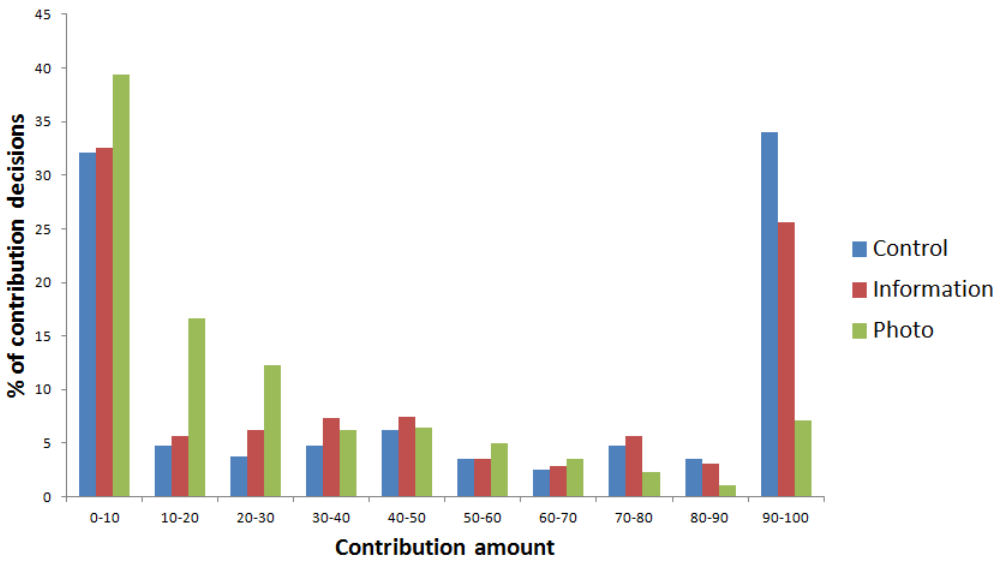

Figure 5: Distribution of contributions across conditions in second round (observations are pooled over all ten periods)

	Control		**Information**		**Photo**	
	Symmetric	**Asymmetric**	**Symmetric**	**Asymmetric**	**Symmetric**	**Asymmetric**
LCONTRIBUTE	1.5684***	1.5105***	1.3724***	1.3449***	0.9054***	1.0534***
	(0.2110)	(0.2079)	(0.1647)	(0.1656)	(0.1108)	(0.1144)
DIFF	-0.7386***	-0.3545	-0.5850***	-0.2945	-0.3696***	-0.0403
	(0.1947)	(0.2576)	(0.1469)	(0.1824)	(0.1060)	(0.1240)
POSDIFF		-0.6890**		-0.5690***		-0.7588***
		(0.3158)		(0.2197)		(0.1635)
Period	-6.6844***	-7.2865***	-5.7756***	-5.5149***	-2.0105***	-1.8823***
	(1.4495)	(1.4658)	(1.0295)	(1.0326)	(0.6314)	(0.6190)
Nationality	-0.4431	-0.3230	-7.3253	-7.4196	-2.5789	-1.1977
	(10.7623)	(10.5586)	(5.7511)	(5.5088)	(4.5188)	(3.9354)
Gender	-13.5839	-14.1351	-2.4777	-0.8633	-9.0116**	-6.8631**
	(9.9916)	(9.8084)	(6.5908)	(6.3225)	(3.6848)	(3.2370)
Education	-6.2255	-2.0431	-13.7726**	-15.8255***	-4.0777	-1.2267
	(10.8480)	(10.8028)	(6.0072)	(5.8108)	(5.8106)	(5.0886)
Age	3.1392***	3.2878***	0.3866	0.7883	1.2855	1.1840
	(1.1948)	(1.1750)	(1.1970)	(1.1460)	(1.0181)	(0.8879)
Location	-20.9049	-24.3224*	22.8881**	24.7269***	-4.0572	-3.3261
	(13.8309)	(13.6635)	(9.2472)	(8.8866)	(5.2413)	(4.5726)
Constant	-119.6523***	-111.1339***	4.2824	-3.9677	-17.1863	-15.7167
	(42.5899)	(41.8694)	(30.4246)	(29.1000)	(24.3727)	(21.3275)
Log likelihood	-1077.959	-1075.5848	-1249.0765	-1245.7273	-1469.627	-1459.1592
Wald χ^2	142.2036***	148.7893***	311.3750***	324.2366***	271.5699***	330.2730***
N	432	432	432	432	432	432
N uncensored	161	161	213	213	287	287
N lower uncensored	129	129	112	112	123	1213
N upper uncensored	142	142	107	107	22	22
Nb groups	48	48	48	48	48	48

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Dynamic Wooldridge Tobit regression results for contributions across each treatment in second round

7 Conclusions

We have investigated experimentally how behavior in a ten-period repeated public good game is affected when subjects know that their decisions and outcomes will be observed by a future audience made up of younger students enrolled at the same university. We did not find a significant effect when the information transmitted did not permit identification of individual participants. However, in the photo treatment, where subjects knew that they would be identified personally by future first-year undergraduate participants, contributions were significantly lower. The photo treatment reduced contributions in the first period of the game, and this effect was not explained by differences in initial beliefs. The photo treatment also affected subsequent dynamic behavior, notably by making subjects less willing to increase their contributions after observing a higher average level of contributions by their co-players, and more willing to reduce their contributions in response to a lower average level of contribution by co-players. The effect on dynamic behavior amplified the effect of treatment on initial choices.

The fact that this surprising result only applies to the photo treatment, while contributions in the information treatment do not differ significantly from the control group, suggests that we can rule out some possible explanations. First, the results are not driven simply by subjects' desire to set an example for future undergraduate participants to follow (i.e., the role model idea), as this should apply to the information treatment too. Similarly, the results are not explained by a possible effect of the experimental instructions' reference to knowledge and experience, since this reference was identical for the information and photo treatments. Third, visibility by the experimenters was held constant for all three experimental conditions and cannot account for the differences between them.

One possible explanation could be that the photo treatment induces subjects to pay more attention and think more carefully about their strategy, because their personal reputation is at stake. This would reduce large contributions due to mistakes and limited attention. While such an effect seems plausible and intuitive, it appears less convincing as an explanation for the persistent difference between the photo treatment and the other two conditions in the surprise restart game. The evidence from this game suggests that subjects are paying attention and thinking carefully about their strategy in all three conditions, as evidenced by the changes in dynamic behavior relative to the first game. In particular, subjects in the control group and the information treatment are now significantly less willing to raise their contributions after observing higher average contributions by their co-players. However, this does not close the gap between treatments, because subjects in the photo treatment are also learning, and reducing their contributions proportionately more after observing a lower average level of contributions by co-players (more relative to the first game, and also relative to the other two experimental conditions). Thus differences in attention and mistakes do not appear to fully explain the data.

Our results seem best explained by participants caring about how they will be perceived by first-year undergraduates arriving the following year, in line with the literature on image concerns. The novelty here is that this *reduces* contributions.

This could be because the graduate students in our photo treatment expected future first-year undergraduates to evaluate them based on the "*homo economicus*" paradigm, equating being smart/rational with zero contributions. However, if subjects in the photo treatment had wanted future first-year undergraduates to perceive them as adhering to the *homo economicus* paradigm, they should have chosen more frequently the Nash equilibrium strategy of contributing zero (or something very close to it). Instead, the difference in average contributions between the photo treatment and the other two conditions is mostly accounted for by a substantially lower proportion of very large contributions, in favor of moderate, but not very small, amounts, which do not have a clear signaling value in terms of *homo economicus*.

This suggests an alternative explanation: subjects in the photo treatment care about not being perceived as "suckers" by future participants. This has a dampening effect on their contributions. Moreover, it makes them less willing to increase their contributions when they have contributed less than the average of the rest of the group (which could reverse their ranking), and more willing to reduce their contribution when they have been contributing above the others' average.

The notion that people do not like the "highly aversive" feeling of being a sucker, and that their behavior will be motivated in a variety of ways to avoid it, has received considerable attention in the psychology literature [Vohs et al., 2007].[24] Our experimental results suggest something more: individuals' willingness to cooperate and engage in prosocial behavior may be inhibited by the fear of being perceived as suckers by others, even when they would not themselves feel like suckers privately.

This could potentially apply to a broad array of cooperative and prosocial behaviors, implying that the impact and value of visibility are highly context-dependent. Further investigation of these effects, in the field as well as the

[24] In this literature, to be a sucker requires that "one has given more than one has gotten or... one has gained less than one thinks one deserves" [Vohs et al., 2007, p.128]. See also Wilkinson-Ryan [2008], who argues that there is a social norm against being a sucker, which may be a factor in discouraging victims from reporting certain crimes (e.g. fraud).

lab, seems a promising avenue for future research.

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Appendix A Experimental Instructions

Welcome to the Laboratory of Experimental Economics of Bocconi University, Milan [Toulouse School of Economics].

This is an experiment in group and individual decision making.

All decisions you make in this experiment are anonymous. Please do not talk with one another during the experiment. If you have any questions, please raise your hand.

In this experiment you will participate in 10 periods of a voluntary contribution game.

At the beginning of period 1, everyone is randomly assigned to a group of 4 individuals.

You will continue to be part of the same group for all the 10 periods.

The 3 other members of your group will never know your identity nor will you know their identity. You only know that all participants at this experiment are graduate students at your university.

Rules of the game you will play in each period

At the beginning of each period each group member will get 100 'euro-cents' (€ 1) in his/her *private account.*

In every period each of you must decide how many of your cents you want to contribute to the group account. Euro-cents not contributed to the *group account* remain in your private account.

The number of cents in the group account equals the sum of cents contributed by you and the other 3 group members in that period.

Example: If you contribute 20 and the other members of the group contribute respectively 40, 0 and 80, the amount in the group account will be $20 + 40 + 0 + 80 = 140$.

Your earnings from each period will be the sum of the eurocents *you leave* in your private account and of *your share* of the group account.

Earnings from the group account depend on the total number of euro-cents (TC) in that account. Each euro-cent in the group account will be *doubled* and then *shared equally* among the 4 subjects in the group. In other words, each individual in a group will receive *half of the amount* of the group account ($TC/2$).

Let us clarify all of the above through some examples.

Information at the end of each period

Example 1

Contributions to the group account

You	Other	Other	Other	*TC*
100	100	100	100	400

Earnings from the group account = *TC*/2

You	Other	Other	Other
200	200	200	200

Total earnings in the Round

You	Other	Other	Other
200	200	200	200

Example 2

Contributions to the group account

You	Other	Other	Other	*TC*
0	0	0	0	0

Earnings from the group account = *TC*/2

You	Other	Other	Other
0	0	0	0

Total earnings in the Round

You	Other	Other	Other
100	100	100	100

Example 3

Contributions to the group account

You	Other	Other	Other	*TC*
45	0	75	0	120

Earnings from the group account = *TC*/2

You	Other	Other	Other
60	60	60	60

Total earnings in the Round

You	Other	Other	Other
115	160	85	160

Example 4

Contributions to the group account

You	Other	Other	Other	*TC*
0	80	0	20	100

Earnings from the group account = *TC*/2

You	Other	Other	Other
50	50	50	50

Total earnings in the Round

You	Other	Other	Other
150	70	150	130

At the end of each period, each member of the group will see at the bottom of the screen:

- the contribution of the 4 members of the group, ranked from the highest to the lowest (notice that it is not possible to link a specific contribution to a specific individual);
- the total amount of the group account;
- his/her earnings from the group account ($TC/2$);
- the amount he/she decided to leave in his/her private account;
- his/her total earnings in that period;
- the sum of his/her total earnings in all the previous periods.

Guesses

In period 1, before taking your decision about the contribution, we ask you to guess the average contribution of your 3 co-players. This will be a number between 0 and 100 euro-cents.

- If the difference between your guess and your co-players's average contribution is less than or equal to 10 euro-cents, you will receive 500 euro-cents (€5) at the end of the experiment.

- If the difference between your guess and your co-player's average contribution is greater than 10 euro-cents, you will receive nothing.

Important: the correctness of your guess in period 1 does not depend on your contribution in period 1.

Example 1: Suppose that the other members of the group contribute respectively 12, 0 and 56. Therefore, their average contribution is (12 + 0 + 56) : 3 = 22.7, i.e. 23. You win 500 euro-cents if your guess is a number between 13 and 33 (13 and 33 included).

Example 2: Suppose that the other members of the group contribute respectively 69, 82 and 93. Therefore, their average contribution is (69 + 82 + 93) : 3 = 81.3, i.e. 81. you win 500 euro-cents if your guess is a number between 71 and 91 (71 and 91 included).

Total Earnings

Your earnings from the experiment will be the sum of the total earnings from all 10 periods plus €5 in case your guess of your co-players' average contribution in period 1 is right.

Remember that the sum of your earnings in the voluntary contribution game is recorded and displayed at the end of each period at the bottom of your screen. Your earnings for the correctness of the guess will be added at the end of the experiment, when we will pay you everything in cash.

Are there any questions?

[The following part only concerns treatments "information" and "photo"]

Important

At the beginning of the next academic year, we will run other sessions of this experiment, this time with first-year undergraduate students of this university as participants.

Undergraduate students are supposed to be younger than you and less experienced in playing such a game. Moreover, being graduate students, you are supposed to have studied and analyzed this game many more times than the undergraduates.

We will let each undergraduate student enrolling in one of the sessions we will run in the new year benefit from your 'knowledge' and 'experience'.

More precisely, in one year from now we will transmit your behavior in the game to the undergraduate student that (by chance) will be seated, during the session in which he/she will take part, in the same place where you are now, playing the same game you are playing, using the same computer you are using. In particular, he/she will play the game with other first-year undergraduates, under the same group matching as yours. This means that the same three computers that will be randomly matched to yours so as to form a group during this session will be matched to the computer of the undergraduate student to whom we will transmit your behavior in the game.

Each undergraduate student participating in the session, before starting the experiment, will know:

- all the **'history'** (contributions in each period and earnings in each period) of the graduate student sitting in the same place as himself/herself;

- the **'ranking'** of the graduate student sitting in the same place as himself/herself in terms of contributions and earnings, relative to the other 3 graduate students in his/her group.

In other words, we will print and transmit to the undergraduate student sitting in the same place as you the history table that you will see at the bottom of your screen at the end of each period.

Notice that:

- the undergraduate student sitting in the same place as you will know your **gender**, **age**, **academic level**, and **nationality**, but not your other personal data (name, surname, email address, phone, etc.);

- *[only in treatment "photo"]* he/she will also see, during all the session he/she will attend, your **picture**, in the same screen in which we will put your data (contributions and earnings in the experiment, ranking inside your group, gender, age, academic level, and nationality);

- there is no payment (neither for you, nor for the undergraduate student) for this transmission; in particular, your earnings and the undergraduate student's earnings will be independent.

Is everything clear? Are there any questions before we begin?

[The following part is distributed, in all treatments, only at the end of the first round of the experiment.]

Second Round of the Experiment

Surprise: the experiment has not ended! We offer you the opportunity to play a second round with 10 periods of the game, under the same rules as the first round of the game, and with the same type of information provided at the end of each period (total amount of the group account, your earnings from the group account, etc.).

More importantly, you would play the other 10 periods of the game with the same 3 co-players with whom you just played the first 10 periods (same group as in the first round).

Participation in this second round of the game is not compulsory: if you wish, you can give up the experiment and be paid only your earnings in the first round.

However, if you choose to continue the experiment, you will be paid also for the second round. In this case, as for the first round, your earnings will be the sum of the total earnings from all 10 periods plus €5 in case your guess of your co-players' average contribution in the new period 1 is right.

In view of the above, do you want to give up or continue the experiment?

[The following part only concerns treatments "information" and "photo"]

Important

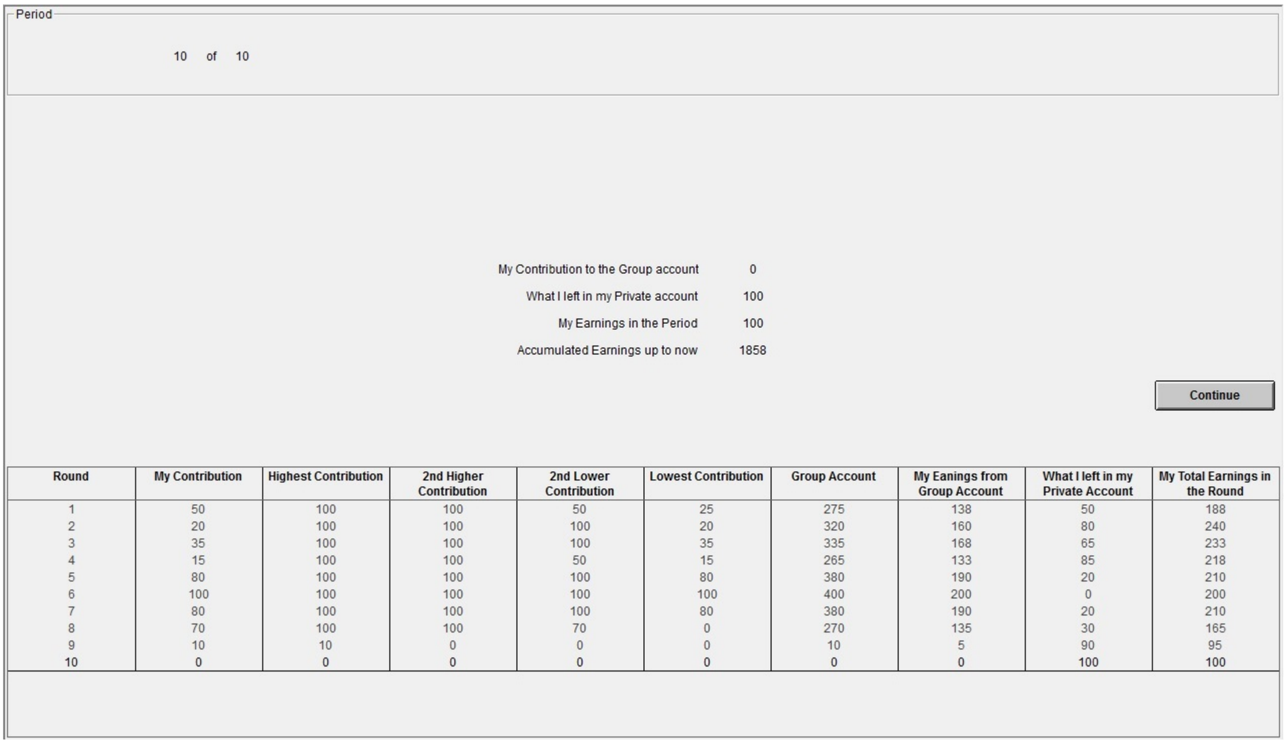
Undergraduates in the sessions that we will run at the beginning of the next academic year will observe, at this point of the experiment, the same "surprise": they will be given the possibility to play a second round with 10 periods of the game, under the same rules, type of information and with the same co-players as in the first round.

They will also be told that, before playing the second round, they will be shown your 'history' and 'ranking'. In other words, as we did for the first round, and only after its end, we will transmit to the undergraduate student sitting in the same place as you the history table that you will see at the bottom of your screen at the end of each period of the second round.

As for the first round, there is no payment (neither for you, nor for the undergraduate student) for this transmission; in particular, your earnings and the undergraduate student's earnings in the second round of the experiment will be independent.

Is everything clear? Are there any questions before we begin the second round?

Appendix B Screenshot



Period

10 of 10

My Contribution to the Group account	0
What I left in my Private account	100
My Earnings in the Period	100
Accumulated Earnings up to now	1858

Continue

Round	My Contribution	Highest Contribution	2nd Higher Contribution	2nd Lower Contribution	Lowest Contribution	Group Account	My Eanings from Group Account	What I left in my Private Account	My Total Earnings in the Round
1	50	100	100	50	25	275	138	50	188
2	20	100	100	100	20	320	160	80	240
3	35	100	100	100	35	335	168	65	233
4	15	100	100	50	15	265	133	85	218
5	80	100	100	100	80	380	190	20	210
6	100	100	100	100	100	400	200	0	200
7	80	100	100	100	80	380	190	20	210
8	70	100	100	70	0	270	135	30	165
9	10	10	0	0	0	10	5	90	95
10	0	0	0	0	0	0	0	100	100

Figure 6: Feedback after 10 periods