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Public goods and future audiences *

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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, first-year students at the same university. 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 social image concerns. In this view, subjects in the photo treatment care about being perceived as smart and successful by future younger students.

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 the visibility of individuals' current behavior by future audiences, sometimes in conjunction with visibility of the individuals' personal identity, and sometimes under condition of anonymity. In this paper, we investigate how *awareness of visibility by a future audience* affects behavior, and how this depends on whether personal identity, as well as actions, will be observable.

We focus on two questions. First, does knowing that people's *actions* might influence other, including younger, people in the future, affect the way individuals behave? We refer to this as a role model effect, or influence effect. Second, when *personal identity* is also observable by the future audience, how do social image concerns interact with any role model effect? These are challenging questions to answer with existing available data, because of many potential confounds. In this paper, we exploit the control afforded by a laboratory experiment.

We compare participants' behavior in an experimental public good game across three conditions: a control condition with no future audience, and two treatments with a future audience. In the first treatment ("information"), subjects know that information about their contribution choices and their earnings during the game will be visible to the future audience, but their personal identity will not. The second treatment ("photo") differs from the first in only one respect: subjects know that their personal identity, captured by a photo, will also be visible to the future audience. This basic design enables us to study the two questions we are interested in. In particular: (i) comparing the information treatment and the control group allows us to investigate whether behavior is affected by a desire to influence the future audience; (ii) comparing the photo treatment and the information treatment allows us to study how social image concerns interact with any desire to influence the future audience.

The public good game that subjects play in our experiment is a ten-period voluntary contribution game with partner matching. This game has been studied extensively in the experimental literature, making it easy to compare our results to previous findings. Our two treatments with visibility by a future audience are, to our knowledge, novel. We recruit participants for all three experimental conditions exclusively among graduate students. In the two treatment groups, subjects are told that their contribution decisions and earnings throughout the game will be transmitted to future players of the same public good game, who will be recruited among first-year undergraduates arriving at the same university the following academic year.[1] These future first-year undergraduates are the future audience.

We chose this feature of the design to make it easier for participants in our treatment conditions, all graduate students, to think of themselves as potential role models for the future, younger and less experienced students in the same university (in the spirit of mentoring schemes, present in a variety of forms in many universities). The instructions made this clear, telling subjects that we would let the future first-year undergraduate participants benefit from their

[1] We provide some summary information about the experimental sessions with undergraduates in section 2, but analysis of data from these sessions is not the focus of the present paper.

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]

Thus while the instructions 'nudge' subjects to act as role models, they do not do so in a way that clearly encourages either high or low contributions. The nudge is exactly the same in both treatments, as is the transmission of information about contributions and earnings. The only difference is the additional transmission of the photo in the photo treatment. By making subjects aware that their personal identity will be visible to the future audience, the photo treatment introduces the possibility of social image concerns: participants may care about how they will be perceived by the first-year undergraduates arriving the following year.

Our experiment yielded two main sets of findings. First, there was no significant difference in contributions, overall, between the information treatment and the control group. Moreover, participants in the information treatment were not more likely to play the zero-contribution Nash equilibrium strategy, nor did they contribute significantly more in the early periods. Thus our nudge seems to have had no significant effect on behavior. The similar average contribution rate between the information treatment and the control group suggests that the desire to influence future, younger students in the same university was not a significant motivation for our participants.

Second, and in clear contrast, we find that *subjects in our photo treatment contributed significantly less* than in the other two conditions. Average contributions for the game were 15% lower. Thus participants behaved *less* prosocially and achieved *lower* levels of cooperation when they were aware that both their behavior and their personal identity would be visible to future, unknown and younger students arriving the following year. Our analysis shows that the photo treatment affected contribution decisions through two channels: lower contributions in the first period, and different dynamic behavior, leading to lower contributions, over subsequent periods.

Clearly, participants cared about how they would be perceived by future students, but why would these image concerns *reduce* contributions? Two potential explanations can be readily checked against the data, and ruled out. Lower contributions could be due to more pessimistic initial expectations of how much other players will contribute. Using data on beliefs we elicited before starting the game, we show that the photo treatment reduces contributions *controlling* for initial expected contributions by other players, while treatment has no significant effect on beliefs.

We can also rule out that subjects in the photo treatment felt they would be perceived more favorably by future first-year undergraduates if they played the Nash equilibrium strategy and consequently contributed zero more frequently. We show that lower average contributions in the photo treatment are not associated with a higher proportion of zero (or very low) contributions. Instead, we find a substantially lower proportion of large contributions in the photo treatment.

[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".

We then investigate whether treatments affected the way participants responded to their co-players' behavior during the game. To do this, we model the dynamics of contribution behavior during the game. This raises some interesting econometric issues; we address them by implementing an approach proposed by Wooldridge [2005]. We find that participants in the control group and the information treatment would raise their contributions significantly after observing that they had fallen short of the average contribution by co-players in the previous period. This was not the case for participants in the photo treatment, which may be part of the reason for the lower proportion of large contributions in that treatment. We discuss potential explanations for our results and argue that the most convincing is based on social image concerns, suggesting some motivation to appear successful in terms of earnings, in the eyes of the future audience.

Finally, we exploit another feature of our experimental design, inspired by Andreoni [1988]. After the end of the game, we introduced a surprise, giving participants the option to play the same ten-period game, with the same partners, for a second and last time. All participants chose to play. The original rationale for such surprise restart games in repeated public goods experiments was to check whether higher initial contributions were due to mistakes. Our results for the restart game suggest that the behavior observed in the first game was not due to mistakes. While there is evidence of learning effects, reducing average contributions relative to the first game in all three experimental conditions, we find that in this surprise restart game, once again, contributions are significantly lower in the photo treatment; learning does not imply convergence.

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 results in section 3: we show how average contributions evolved over time, examine treatment effects and investigate possible explanations for the observed behavior. Section 4 concludes.

1.1 Literature Review

Our work builds on two main strands of theoretical literature. First, the literature on role models: for example, in the context of cultural transmission (e.g. Bisin and Verdier [2000, 2001]), or leadership by example (e.g. Hermalin [1998]). The information treatment in our experiment studies role models from the perspective of individuals who set an example. Second, the literature on social image concerns, in which individuals care about the inferences that others will make about them, based on their observable behavior: for example, inferences about how prosocial and disinterested they are (e.g. Bénabou and Tirole [2006]), or the extent to which they care about fairness (e.g. Andreoni and Bernheim [2009]). The photo treatment in our experiment allows us to examine different forms of social image concerns.

The experimental literature related to these strands of theoretical literature is vast. A large body of work has studied the impact of visibility on different kinds of prosocial behavior in a variety of settings, mostly finding 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.[3]

Within this literature, the closest papers to ours are perhaps those that have varied the visibility of subjects' identity and contribution decisions in public good games. Andreoni and Petrie [2004] use information and photos, as we do, but both features for a different purpose than ours: subjects in their information treatment learn the contributions of each co-player; subjects in their photo treatment are identified to other members of their group (their co-players). When information and identification 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/her 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.

Reinstein and Riener [2012] assign participants to make decisions in Phase 1 or Phase 2 of their experiment, with the possibility of an individual's decision or decision together with personal identity from Phase 1 being visible to a participant in Phase 2. The decision they study is the decision to donate to a charity; they do not distinguish between graduate and undergraduate students and there is no nudge to encourage participants to act as role models.

More broadly, our paper is also related to other work on the impact of identification and audience effects. 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/her 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.[4] 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.

[3]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]. Two exceptions are Dufwenberg and Muren [2006], where on-stage payments in front of an audience of co-students reduces generosity in the dictator game, relative to private payments, and Goette and Tripodi [2023], where blood donations are higher when solicited with a simple ask than when rewarded through social recognition on Facebook.

[4]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].

Also somewhat related to the notion of role models is the literature on leader-follower public good games. These are sequential voluntary contribution games in which one subject in the group makes the first contribution decision (the leader), while the other subjects (the followers) make their decisions after observing the leader's choice.[5] 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/her 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.[6] 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 our experiment were graduate (Master or PhD) students in Economics and related disciplines at Bocconi University in Milan and at the University of Toulouse.[7] 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]).

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

[5] 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].

[6] 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].

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

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 subsequent undergraduate sessions (the future audience) 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 sessions studied in the present paper. Note that, although our initial intention was to match every graduate participant to some undergraduate participant in the same university, both logistic and financial constraints led us to eventually implement this matching only partially (only a subset of graduate participants were actually matched to undergraduate 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 describe nor analyze the undergraduates' behavior in this paper.[8] In what follows, we will restrict our attention to the graduate students' 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 condition,[9] with 32 subjects (2 sessions) and 16 subjects (1 session) participating in the experiment in Milan and in Toulouse, respectively. Individuals enrolling to the same experimental session were randomly allocated to one of the three experimental conditions. Thus, treatments were randomized at session level, with only one treatment implemented in each experimental session, as is standard in voluntary contribution games.

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

[8] Unlike graduate subjects, undergraduate 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.

[9] We chose the sample size in line with the existing literature (see e.g. Fehr and Gächter [2000]).

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 game 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 5 in Appendix C).[10] 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).

Repeated game: surprise restart game 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 game, with partner matching, and within the same group as in the first game. This was a surprise restart game: participants did not know until the first game had ended that a second game would follow. Therefore, participants were offered the choice of ending the experiment and being paid only for the first game, or continuing the experiment and be paid also for the second. Once the surprise was introduced, as in the seminal study by Andreoni [1988], we were explicit in pointing out that after the second game 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,[11] but their identity would never be

[10]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 (himself/herself).

[11]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 anonymity relative to 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. In our setting, the absence of anonymity relative to the experimenter was necessary in the transmission treatments, because the experimenter needed to be able, a year later, to link the data on observed contributions to personal data. To avoid confounding effects, we implemented this by asking all subjects, including those in the control group, to fill in a paper form which specified their names, age, gender, nationality, year and field of study, as well as the number of the computer assigned to them. This allowed subsequent linking to computerized data on contributions, identified by the computer number.

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/her answers to the initial questionnaire (age, gender, nationality and academic level) and the history of his/her 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,[12] playing the same VCG with other first-year undergraduates, under the same group matching.[13] In particular, in both conditions, subjects were 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 5 in Appendix C 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/her 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).[14] 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

[12]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.

[13]The subject was told that the same three computers randomly assigned (by the experimental software) to his/her computer so as to form a group of four players in his/her VCG would be assigned to the computer of the undergraduate student sitting in his/her place the following year. Thus, a year later each undergraduate student in the same group would get the individual history of choices and outcomes of a graduate student compared to those of the other three graduate students that his/her three (undergraduate) co-players would respectively get.

[14]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. Having 2 additional potential participants at the beginning of the experiment allowed us to immediately replace subjects eventually leaving the experiment at the end of all instructions, with no delay – due to looking for new subjects and reading them the experimental instructions – in the implementation of the treatment sessions.

let future first-year undergraduates benefit from their knowledge and experience.[15] 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.

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 game, although subjects discovered that there could be a second game (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.

3 Results

We start by examining contribution patterns and differences between treatments in the first game, i.e. the public good voluntary contribution game played over ten periods, described in detail in section 2. We will briefly discuss the results for the second (surprise) restart game at the end of the section (details are available in Appendix A.2).

For the first game, 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.[16] 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 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 conditions using Dunn [1964]'s test with a Bonferroni adjustment for multiple comparisons reveal a highly significant difference in both cases ($p = 0.0088$ when comparing

[15] The precise wording is exactly the same in both treatments; see the experimental instructions in Appendix B.

[16] 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].

photo and control, $p = 0.0097$ when comparing photo and information).[17] There is no significant difference between the control group and the information treatment. We can summarize the results as follows:

R1 Contributions are significantly lower in the photo treatment (relative to the other two conditions);

R2 There is no significant difference between the control group and the information treatment.

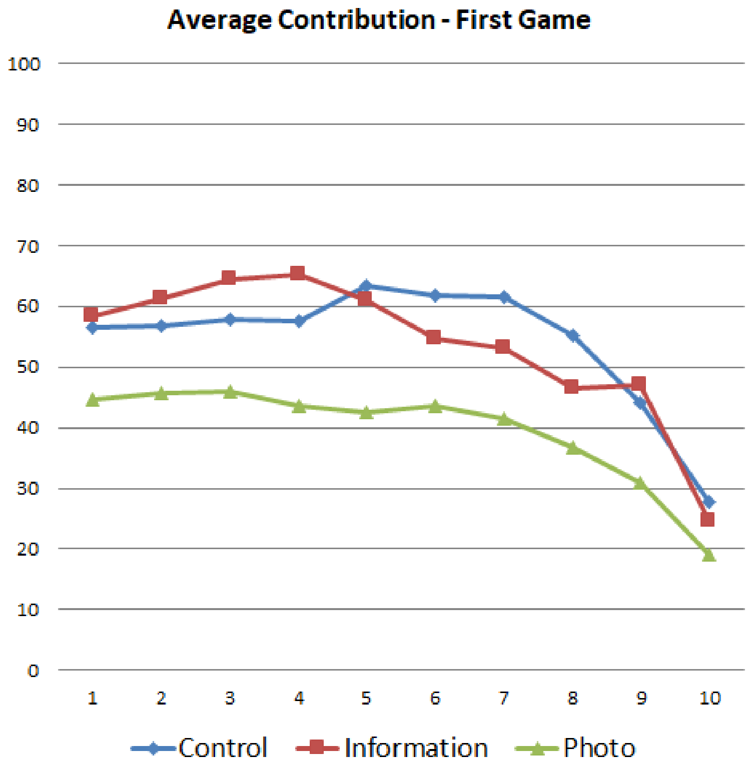

Figure 1: Average contribution across periods in first game

Discussion These results are interesting in several ways. When participants know they will not be identified personally by the future audience, our nudge, encouraging them to let future first-year undergraduates benefit from their knowledge and experience, appears to have no effect (result R2). In other words, knowing that their contribution choices and resulting earnings (but not their personal identity) will be visible to future undergraduates does not induce them to cooperate significantly more or significantly less. However, knowing that their contribution choices and earnings will be visible to future undergraduates together with their personal identity, does have a significant and quite substantial negative effect on their contributions (result R1). In many contexts, increasing visibility of personal identity tends to encourage more prosocial behavior, as discussed in section 1. Our result is therefore surprising, and deserves further

[17] In both cases $N = 96$ (i.e. 48 subjects in each condition); we follow Andreoni and Petrie [2004] in taking as the unit of observation the average contribution per subject over the 10 periods.

investigation. Our data allows us to examine several possible explanations. We begin by checking whether participants in the photo treatment expected their co-players to contribute less than in other treatments before the game started, and contributed less themselves in the first period for this reason. The evidence, reviewed below, does not support this hypothesis. We then investigate alternative explanations for result R1.

3.1 Are lower contributions in the photo treatment due to more pessimistic initial beliefs?

We are able to investigate this possibility thanks to a feature of our experimental design described in detail in section 2: before starting the game, we elicited our subjects' expectations about their three co-players' average contribution in the first period. At this point, subjects knew the rules of the treatment they were in; we can therefore check whether they had more pessimistic beliefs in the photo treatment. We construct the variable Pessimism = 100 – (expected average contribution by co-players). This variable captures the extent to which subjects were pessimistic about how much their co-players would contribute; it takes values between 0 (if they expected all co-players to contribute their entire endowment, equal to 100 euro-cents) and 100 (if they expected all co-players to contribute zero). In the first column of Table 1, we report the results of a Tobit regression with Pessimism as the dependent variable. We see that there is no significant effect of treatment on Pessimism.

The second and third columns in Table 1 present results for Tobit regressions with the individual average contribution over ten periods as the dependent variable. The results in the second column show that Pessimism, as expected, has a negative effect on contributions: when subjects expect their co-players to contribute less, they also contribute less. Controlling for the effect of Pessimism, contributions are significantly lower in the photo treatment: this confirms that our result R1 is not explained by pessimistic initial beliefs. It could still be the case that the negative effect of the photo treatment on contributions is driven by the contribution choices of participants in that treatment who had more pessimistic initial beliefs. To investigate this, we interact Pessimism with the treatment dummies to create variables Interinfo and Interphoto. The results in column 3 of Table 1 show that these interaction terms are insignificant: the significant negative effect of the photo treatment is not driven by pessimistic beliefs.

3.2 Are lower contributions in the photo treatment due to more frequent play of Nash equilibrium strategy?

Since participants in the photo treatment do not appear to choose lower contributions because of more pessimistic initial beliefs about their co-players' behavior, what motivates their contribution decisions? The only difference between the photo treatment and the information treatment is that subjects' personal identity will be visible to future first-year undergraduates in the case of the photo treatment.[18] Our subjects seem to care about how they will be perceived personally by future first-year undergraduates. Clearly, they are not trying to be perceived as particularly prosocial (cooperative, generous), since they contribute less in the photo treatment. Are they

[18]Recall that the nudge is identical in both treatments, and transmission of information about contributions and earnings to the future audience is also identical: the only difference is the transmission of the subject's photo.

trying to be perceived as playing the Nash equilibrium strategy? This could be the case if they think that some of the future first-year undergraduates will be familiar with basic notions of Nash equilibrium (e.g. first-year undergraduates in Economics) and will judge them favorably if they play according to those notions.

In fact, it is straightforward to verify that our subjects do not play the Nash equilibrium (zero contribution) strategy more frequently in the photo treatment (16%), relative to either the control group (21%) or the information treatment (19%).

	Pessimism	**Average Contribution**	**Average Contribution**
Location	-2.5669	2.3175	2.2036
	(5.5266)	(4.6053)	(4.5905)
Gender	2.2695	-5.3686	-5.1037
	(4.6847)	(3.9077)	(3.9400)
Education	-8.2524*	-4.0885	-4.6413
	(4.8481)	(4.0761)	(4.0840)
Age	0.6220	-0.4467	-0.4674
	(0.7065)	(0.5895)	(0.5926)
Nationality	1.3344	0.6029	0.5200
	(5.0516)	(4.2096)	(4.1925)
Info	-2.2652	-2.8585	-2.6824
	(5.7735)	(4.8110)	(8.8505)
Photo	5.1466	-14.5766***	-25.3172**
	(5.7535)	(4.8148)	(10.4406)
Pessimism		-0.4162***	-0.4633***
		(0.0714)	(0.1097)
Interinfo			-0.0096
			(0.1653)
Interphoto			0.2062
			(0.1819)
Constant	34.1282*	89.9110***	92.9694***
	(19.0411)	(16.5605)	(16.1534)
Log likelihood	-661.3138	-638.5045	-637.7126
χ^2	6.77	46.18***	47.77***
N	144	144	144
N uncensored	140	141	141
N lower uncensored	4	0	0
N upper uncensored	0	3	3

Standard errors in parentheses

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

Table 1: Tobit regressions for beliefs and average contributions in first game

Figure 2 presents histograms showing the distribution of contributions for the three experimental conditions, pooling observations for all ten periods of the game. The most striking difference between the photo treatment and the other two conditions is not associated with playing the Nash equilibrium strategy or something very close to it; instead, it is the much lower proportion of large contributions (70 or more), and correspondingly higher proportion of

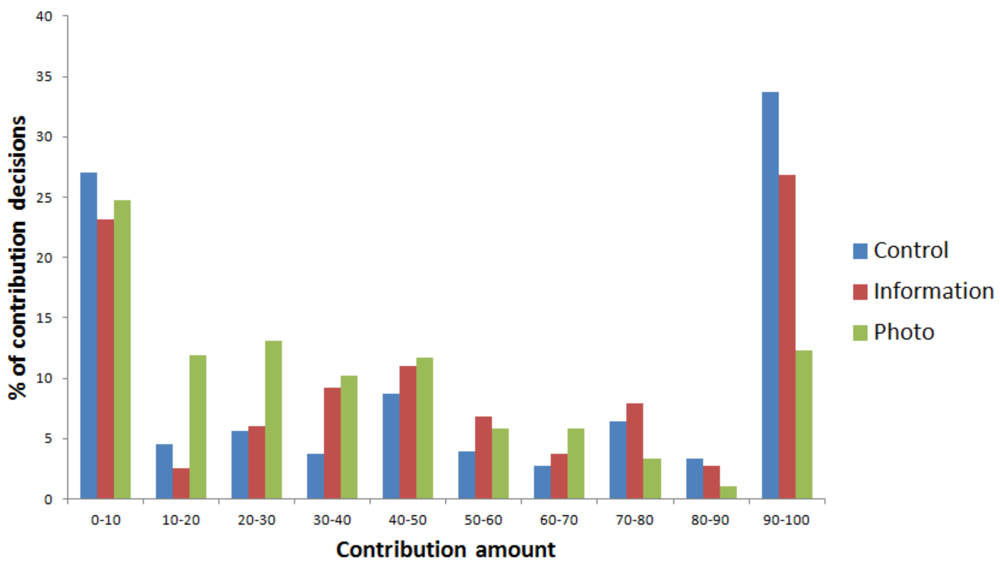

Figure 2: Distribution of contributions across conditions in first game (observations are pooled over all ten periods)

moderate contributions (between 10 and 50).

3.3 Do subjects in the photo treatment respond differently to co-players' behavior?

We have ruled out more pessimistic initial beliefs and greater willingness to play the Nash equilibrium strategy as explanations for result R1. Did the image concerns at play in the photo treatment influence the way subjects responded to co-players' behavior during the game? Different responses could arise, for example, if subjects are reluctant to stand out as higher-than-average contributors (hence lower-than-average earners) in their group, when they know that future undergraduates will be able to identify them personally in this way.[19]

We can investigate how subjects responded to their co-players' behavior by estimating a dynamic model, where each player's contribution in the current period is allowed to depend on whether the player's contribution in the previous period exceeded the average contribution by the three co-players in the group. The correct specification and estimation of such a dynamic model raises some interesting econometric issues, discussed in Appendix A.1. Table 2 presents results for a dynamic Tobit model, following the approach of Wooldridge [2005] which corrects these issues, as detailed in Appendix A.1.[20]

The dependent variable is $contribution_{it}$, the contribution of individual i at time t. LCONTRIBUTE is the individual's contribution in the previous period. PERIOD is a time trend, capturing the possibility that contributions decline as the last period of the game approaches. The main variables of interest are DIFF and POSDIFF. The first of these, DIFF, denotes the difference between own contribution and average contribution of co-players in the previous

[19] This could be due to not wanting to appear as "losers" in terms of earnings, or not wanting to be perceived as "virtue signaling" (showing off their generosity to impress the future audience).

[20] The dynamic model is estimated for each of the three experimental conditions. We also tested against pooled and treatment dummy models which were rejected, results are available if requested.

period. POSDIFF is obtained by interacting DIFF with a dummy capturing when DIFF is positive.

The first two columns of Table 2 show the results for the control group and the information treatment, respectively. We see that the estimated coefficients for DIFF and POSDIFF are both significant, and similar in magnitude.[21] 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. The third column shows results for the photo treatment. We find that only the estimated coefficient for POSDIFF is significant: in the photo treatment, participants would *not* increase their contributions significantly in response to higher contributions by co-players. They would only reduce them after observing that the average contribution of the other members of the group was lower.

	Control	**Information**	**Photo**
LCONTRIBUTE	1.6809***	1.3450***	0.9906***
	(0.2143)	(0.1620)	(0.1076)
DIFF	-0.5973**	-0.5431***	-0.2308
	(0.2435)	(0.1828)	(0.1437)
POSDIFF	-0.4882*	-0.4844**	-0.5408***
	(0.2860)	(0.2219)	(0.1769)
Period	-4.5912***	-4.1451***	-2.8795***
	(1.2131)	(0.9079)	(0.6011)
Nationality	-5.2070	0.8296	-0.7518
	(9.7280)	(6.0080)	(4.3311)
Gender	-3.1582	-10.2720	-4.1521
	(9.4073)	(6.7936)	(3.3765)
Education	-15.9641*	-17.8239***	7.0442
	(9.7026)	(6.2664)	(5.6195)
Age	1.1377	0.4909	0.0849
	(1.0398)	(1.3461)	(0.9943)
Location	11.9643	3.5628	-3.1364
	(10.8800)	(7.7306)	(4.8504)
Constant	-45.7118	-1.8241	7.6657
	(37.6588)	(33.7028)	(23.9012)
Log likelihood	-1208.6195	-1327.4116	-1327.0579
Wald χ^2	133.29***	249.04***	325.89***
N	432	432	432
N uncensored	191	229	307
N lower uncensored	98	88	75
N upper uncensored	143	115	50
Nb groups	48	48	48

Standard errors in parentheses

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

Table 2: Dynamic Wooldridge Tobit regression results for contributions across each treatment in first game

[21] 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.

3.4 Discussion

We have found that participants in the photo treatment respond differently to their co-players' behavior, relative to participants in other conditions. In the presence of image concerns, participants are less willing to increase their contributions after observing a higher average contribution by co-players. This helps to shed light on the fact that large contributions are far less frequent in the photo treatment. It also suggests that participants were not simply trying to avoid being perceived by future undergraduates as virtue signaling: some increase in contributions when a player has contributed less than co-players in the previous period could be interpreted in many other ways, including reciprocity. That is, subjects could have increased contributions in such circumstances without it being interpreted as virtue signaling. Yet they chose not to.

We can summarize our key findings as follows: lower contributions in the photo treatment are not explained by more pessimistic initial beliefs; they are not explained by more frequent play of the Nash equilibrium strategy; they are not explained by a concern to avoid being perceived as virtue signaling. The results appear consistent, on the other hand, with some motivation to appear successful in terms of earnings, in the eyes of the future audience. Such a motivation could induce subjects who would otherwise have made large contributions to choose more moderate contributions. It could also make subjects more reluctant to increase their contributions.

If this interpretation is correct, it offers a way to reconcile our results with previous findings in the literature. First of all, comparing with Reinstein and Riener [2012], the fact that they study an individual's decision to donate to a charity removes any motivation to appear above average, or at least average, relative to co-players, in terms of earnings. In their setting, each subject in the visibility treatment knows that their individual decision will become visible to a subsequent participant; there is no group comparison. In our photo treatment, each subject knows that their contribution decisions will become visible to the future audience *together with* the contribution decisions of the three co-players. This introduces an element of group comparison into image concerns, and the possibility that subjects will care about how they are perceived by the future audience *relative to their co-players*. The presence of this motive in our setting could explain why making personal identity visible has a different effect.

Second, comparing with Andreoni and Petrie [2004], their setting makes contributions and identity visible to co-players, which may induce more cooperative behavior, as they suggest, because it "may serve to reduce social distance and encourage some level of social responsibility to the group". This effect is absent from our setting, since we make contributions and identity visible, not to co-players, but to a future audience. In this context, any motivation to appear successful in the eyes of the future audience can drive a wedge between the behavior that would be chosen in the absence of future visibility and actual behavior, reducing contributions.

3.5 The restart game

Andreoni [1988] introduced a surprise restart game into a public good experiment to examine whether higher initial contributions were due to mistakes. The idea was that mistakes would not be repeated: if contributions in the first game were initially too high by mistake, subsequently falling, initial contributions in the surprise restart game would be lower. Figure 3 shows average contributions across periods for each experimental condition, comparing the first game and the second (restart) game. We see that for all three conditions, average contributions in the first period of the second game are at similar levels to the first period of the first game, and substantially higher than in the last period of the first game.

This suggests that our results for the first game were not due to mistakes. We also see that in the restart game contributions are, again, lower in the photo treatment (see Appendix A.2 for a detailed analysis).

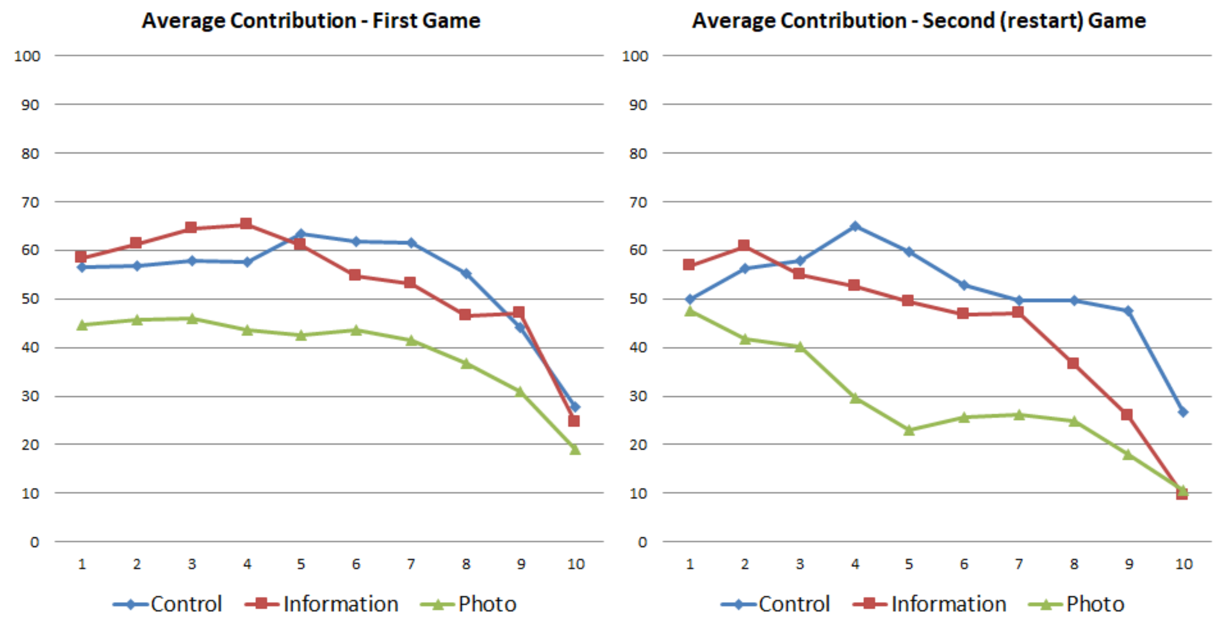

Figure 3: Average contribution across periods in both games

4 Conclusions

In this paper, we have compared behavior during a ten-period public good game in a standard control condition with behavior in two treatments. Both treatments made our participants' (graduate students') actions and outcomes visible to a future audience of new, younger students arriving the following year, but only one of the treatments made personal identity visible as well. Both treatments nudged participants to act as role models for the future, less experienced students.

Our results show that behavior in the treatment with anonymity did not differ significantly from the control condition, while contributions were significantly *lower* in the treatment with visibility of personal identity, and this substantial difference was also present in the surprise restart game.

Our results seem consistent with a form of social image concerns in which individuals wish to be perceived, at least to some extent, as successful in terms of earnings by the future audience. Further exploration of how visibility by a future audience can affect behavior, and how this depends on individual characteristics, the nature of the information that will be available to the future audience, as well as the nature of the future audience itself, seems a promising avenue for future research.

Finally, although we found no evidence of a role model effect in our experiment, we believe this merits further investigation in different settings, in future work.

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Appendix A Additional analyses

A.1 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 game.

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:

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

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.

A.2 The restart game

We applied to the data from the surprise restart game the same analysis we had developed for the first game. In particular:

(i) We repeated the econometric analysis of dynamic contribution behavior. Table 3 reports estimates for the dynamic tobit using Wooldridge's method. We find that, in all three experimental conditions, the coefficient for $DIFF$ is insignificant while the one for $POSDIFF$ is highly significant. Thus participants' willingness to contribute no longer increases significantly when they observe that the other players contributed more in the previous period, not even in the control and information conditions. Participants' willingness to contribute does respond negatively when they observe that other players contributed less in the previous period.

	Control	**Information**	**Photo**
LCONTRIBUTE	1.5105***	1.3449***	1.0534***
	(0.2079)	(0.1656)	(0.1144)
DIFF	-0.3545	-0.2945	-0.0403
	(0.2576)	(0.1824)	(0.1240)
POSDIFF	-0.6890**	-0.5690***	-0.7588***
	(0.3158)	(0.2197)	(0.1635)
Period	-7.2865***	-5.5149***	-1.8823***
	(1.4658)	(1.0326)	(0.6190)
Nationality	-0.3230	-7.4196	-1.1978
	(10.5586)	(5.5089)	(3.9356)
Gender	-14.1351	-0.8633	-6.8632**
	(9.8084)	(6.3225)	(3.2372)
Education	-2.0431	-15.8255***	-1.2267
	(10.8028)	(5.8108)	(5.0889)
Age	3.2878***	0.7883	1.1840
	(1.1750)	(1.1460)	(0.8879)
Location	-24.3224*	24.7269***	-3.3260
	(13.6635)	(8.8866)	(4.5728)
Constant	-111.1339***	-3.9677	-15.7163
	(41.8694)	(29.1002)	(21.3287)
Log likelihood	-1075.5848	-1245.7273	-1459.1592
Wald χ^2	148.79***	324.23***	330.27***
N	432	432	432
N uncensored	161	213	287
N lower uncensored	129	112	1213
N upper uncensored	142	107	22
Nb groups	48	48	48

Standard errors in parentheses

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

Table 3: Dynamic Wooldridge Tobit regression results for contributions across each treatment in second (restart) game

(ii) We investigated the distribution of contributions across treatments, to see whether the photo treatment is associated with more very low contributions and/or fewer large contributions. Figure 4 presents histograms for each experimental condition. We see evidence of both effects: the photo treatment has a higher proportion of very low contributions, and a much lower proportion of large contributions.

Taking all the results for the surprise restart game together, there appear to be some learning effects relative to the first game, but contributions in the photo treatment remain significantly lower than in the other conditions.

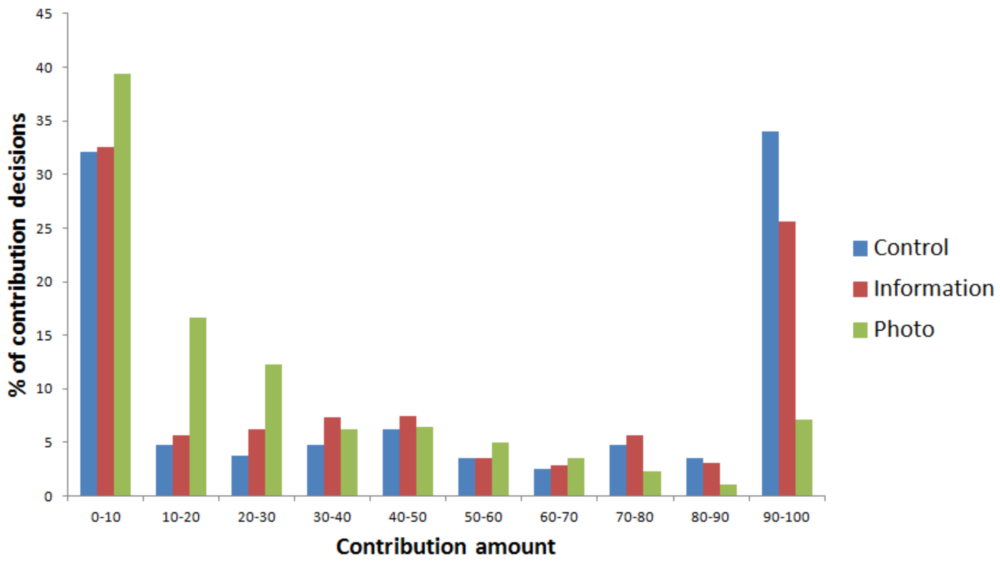


Figure 4: Distribution of contributions across conditions in second (restart) game (observations are pooled over all ten periods)

Appendix B 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

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;

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

- 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 privately 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 game 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

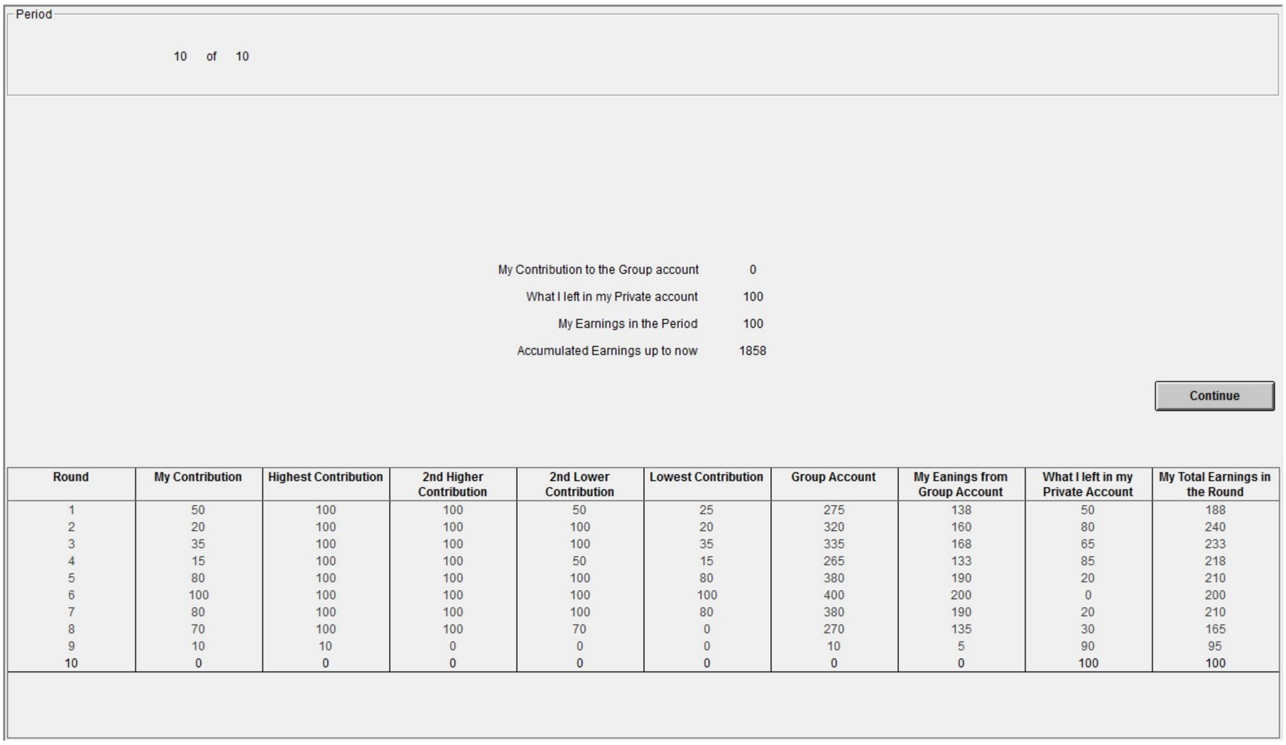
Undergraduates in the sessions that we will run at the beginning of the next academic year will observe, at this point of the experiment, the same "surprise": they will be given the possibility to play a second round with 10 periods of the game, under the same rules, type of information and with the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



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 5: Feedback after 10 periods