

ISSN: 2038-7296
POLIS Working Papers
[Online]

Dipartimento di Politiche Pubbliche e Scelte Collettive – POLIS
Department of Public Policy and Public Choice – POLIS

POLIS Working Papers n. 188

August 2011

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Periodico mensile on-line "POLIS Working Papers" - Iscrizione n.591 del 12/05/2006 - Tribunale di Alessandria

An experimental inquiry into the nature of relational goods

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Abstract

Our experiment aims at studying the impact of two types of relational goods on the voluntary contributions to the production of a public good, i.e. acquaintance among the contributors and having performed a common work before the experiment. We implement two treatments with 128 participants from two different groups. In the first treatment the subjects are left talking in a room before the experiment (cheap talk treatment); they are not suggested any particular topic to talk about, nor are they requested to perform any activity in particular. The second treatment involves the performance of a common work (namely, the computation of some indices of economic performance of three companies, based on their balance sheets). The two groups of subjects are composed either by people with or without previous acquaintance. An equal number of subjects from each of these groups is then allocated to either treatment. After that the subjects played a standard 10-rounds public goods game in groups of 4. The groups were gender-homogeneous. This allows us also to inquire for the possible presence of a gender effect in our experiment. Our results show that: 1) both common work and previous acquaintance increase the average contribution to the public good, 2) there is a relevant gender effect with women contributing more or less than men, depending on the treatment. Therefore, we conclude that relational goods are important to enhance cooperation, that acquaintance and working together are rather complements than substitutes, and that different relational goods produce different effects on cooperation. Also, we find further evidence for women's behaviour to be more context-specific than men's.

JEL codes: C91; D03; H41

Keywords: relational goods; public goods experiments; gender effect

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1. Introduction

Among the relevant discoveries of the so-called economics of happiness is that *relational goods* (RGs) play a powerful role in determining the subjective well-being (SWB) of human beings¹. It is not surprising; after all, a very short time elapsed since we were social animals to any effect, and it is highly plausible that we have not had time enough to adapt to the very recent individualistic society where most of us are living. The more we enjoy RGs, the happier we are (or our SWB is greater); hence RGs are normal goods. Probably they are not only final goods, but also *intermediate* goods, i.e. they are also part of the endowment of our *human capital*. A person well endowed with RGs is happier than a person who is less endowed; but s/he also has more human relations, possibly a more productive attitude towards others, a better experience in dealing with people, and so on. Hence we may expect that RGs affect not only our well-being, but also our activities. But what exactly is a relational good? A Cro-Magnon fellow would probably be very surprised with some current definitions, that point towards "being together" irrespectively of what is done while together². S/he dwelt in an environment where (simplifying) *everything* was made together. To disentangle *being* together from *doing* together would probably look quite convoluted and useless to her/him. It is reasonable to suppose that the feeling of "togetherness" we miss today, and which makes relational goods so precious, is strongly correlated with the *cooperation* that characterized (and characterizes) gathering-hunting societies, i.e, with the collective accomplishment of some basic duty.

This hypothesis is highly coherent with a well-known criticism to the notion of *homo oeconomicus* (see, for instance, Strober, 1994; Ferber, 1995; Nelson, 1995), according to which people are used to socialise into groups (family, friends, colleagues, etc.) and are used both to rely on and to feel responsible for the (relevant) others. According to this interpretation of the *homo oeconomicus*, people's actions can be expected to reflect their group identity and their differences in socialisation.

What above provides support for the partition of relational goods into *relational consumption goods* and *relational capital goods* suggested by Gui and Stanca (2010, p. 109), and is also the basis for the hypothesis that we will try to test in this paper. The hypothesis is that there is a hierarchy in relational goods. To spend a night together playing cards or singing songs is less "relational" than spending a day working together to accomplish a relevant duty or to reach a common goal. Our concern will not be with the consumer good facet of different relational goods (how they affect our SWB), but with their facet as human capital goods; more precisely, with their effects on the propensity to cooperate. The hypothesis is that the endowment of relational goods affects that propensity positively. People linked by "weak" relational goods will cooperate less than people linked by "strong" ones³. In other terms, while we may expect that all kinds of RGs enhance cooperation, we may also expect that the effect may be greater or smaller according to their kind. The normative implications could be of some interest: if it is confirmed that working together is

¹ See mainly Becchetti et al. (2008, 2009b), Prinz and Bürger (2009), Bartolini and Bilancini (2010), Bruni and Stanca (2008), Bartolini et al. (2007), and Pugno (2009). According to the first quotation, the economic analysis of relational goods started with Gui (1987).

² See for instance Gui and Stanca (2010, p. 109): "This framework has been developed with primary reference to encounters taking place within productive organisations or at their interfaces with counterparts in transactions. However, it lends itself to also analysing interpersonal relationships within families, informal social groups, or associations. Here the 'conventional output' to be taken into consideration may be, for instance: a home meal, the completion of a mountain hike, or the performance of an amateur music band. But the process of generation of relational outputs is basically the same". Analogously, Gui and Sugden (2005, p. 3) claim that "relational goods are the affective components of interpersonal relations [that] are usually perceived as having value through their sincerity or genuineness"; and Becchetti et al. (2009b, p. 2) that "relational goods [... are] the expressive/affective side of social interactions, for instance time spent with friends and family, in meetings of associations or communities etc.".

³ See next section for a brief survey of the effects of cheap talk on cooperation.

more effective than spending free time together, the enhancement of cooperative behaviours through the development of relational goods would require a promotion of cooperative work; if the opposite is true, the promotion of free-time institutions. Obviously the mix of the two could, in principle, be fine-tuned according to the relative weights of the two kinds of RGs (and possibly of other ones). We may attach a label to the possible extremes. The first one (only common work produces cooperation through a RG) can be defined the "Marxian" notion of RG: RGs are the manifestation of the common (or "class") consciousness of cooperating workers. The second one (being together produces cooperation), the "Catholic" one - all people are brothers (or sisters), and feel (or should feel) so irrespective of what they do or think, because they belong to the same church: actually, *Church* comes from the Latin *Ecclesia*, which in turn comes from the Greek word *Εκκλησία*, whose meaning is "general assembly". For our paper these labels are purely illustrative, but they help us to remember that the assessment of the role of RGs is likely to be crucial both for the Christian and for the socialist social philosophies. We will not pursue further this subject, but we think it is sensible to call it into light to emphasize the richness of contents that characterizes the study of RGs.

Our approach is experimental. Basically, in our experiment we aim at studying the effects, on the propensity to cooperate, of two different kinds of RGs, that is, previous acquaintance and the performance of a collective task. Section 2 reviews the related experimental literature, in particular the experimental literature on relational goods and public goods; section 3 illustrates the structure of the experiment and its implementation; after a digression on the gender effect in section 4, section 5 presents the results, and section 6 concludes.

2. Related experimental work

Experimental literature on relational goods is in its infancy. In their very recent contribution Becchetti et al. (2011) use for the first time the concept of relational goods to explain cooperative behaviour in an experimental context. According to them, no other studies exist which introduce in an experiment the possibility of consuming RGs in order to study their impact on cooperation. Becchetti et al (2011) do so indirectly, by referring to the concept of RGs to interpret the results of two previous experiments that study the effect of the reduction of social distance on deviation from purely selfish behaviour in an Investment Game (Becchetti et al., 2007) and in a Traveller's Dilemma (Becchetti et al., 2009a). In both experiments reduction of social distance - through removal of anonymity - is the consequence of a *voluntary* choice made by subjects to meet the counterpart after play. This generates "the possibility to consume relational goods through a personal encounter that agents will share after having interacted in the laboratory"⁴, therefore creating an experimental design which allows to study the effect of RGs on cooperation. The main results of the two experiments are then interpreted as the willingness to influence positively the disposition of the other players to the meeting, "which is a crucial factor to create and consume relational goods during the encounter". The most significant result considered is that the willingness to consume RGs seems to increase the chance of detecting cooperative behaviour "even though it entails a monetary risk or a sure material sacrifice".

If we accept that the endowment of relational goods decreases if competition increases, some experiments on the effects of competition could be assumed to consider the effect of lacking of relational goods on that propensity. Brandts et al. (2009) study the effects of competitive rivalry on the disposition towards others in a social dilemma game without complete contracts. They find that rivalry impacts negatively on the behavioural disposition towards those interacted with, and decreases 'subsequent willingness to help', potentially leading to the 'obstruction of future cooperation' (p. 20). Besides, they find that: rivalry neither increases efficiency nor does it favour

⁴ No indication of the page because quotations are from the manuscript (here and below).

the short side of the exchange relation in terms of gains in earnings; it has a negative effect on the subjective well-being of those on the long side, while it increases the well-being of those on the short side, therefore generating inequalities. A negative effect of competition on the propensity to cooperate has also been found by Canegallo et al. (2008) and by Carpenter (2005).

On the other hand, since the early experimental contributions (for instance, Smith, 1979, 1980; Marwell and Ames, 1981), experimental literature on the public goods allocation problem has grown extensively in many different directions. A very consistent number of experimental works studied (both in single and multiple-round experiments) the effects on contribution rates of different treatment factors, like the levels of the Marginal Per Capita Return (MPCR); the size of the players' group; the distinction between internal and external return; the existence of repetition with a fixed or a variable group; the opportunity to punish or reward the other players; the possibility for participants or experimenters to observe individual contributions; the introduction of pre-play communication (for references see surveys by Ledyard, 1995; Holt 2006; and works by Isaac et al., 1984; Isaac and Walker, 1988a, 1988b; Isaac et al., 1994; Goeree et al., 2002; Croson, 1996; Andreoni, 1988; Andreoni et al., 2003, among many others). Other experiments have studied variations in the payoff structure (by introduction of a non-linear payoff functions, see Laury and Holt, 2000 for a survey), or by requiring that total contributions exceed a specified threshold before any benefit can occur (see Bagnoli and McKee, 1991; Croson and Marks, 2000, Canegallo et al., 2008); the effects of other factors like culture, age and gender; the effect of altruism, of relative earnings or attitude toward others (see Laury and Holt, 2000 for a survey).

Our implementation of the PGs problem follows the standard structure for research in this area, a game with multiple stages with a fixed group and constant MPCR, as introduced by Isaac et al. (1984). Since we introduce a form of pre-play communication (cheap talk), out of all the factors which may have an impact on contribution levels, the experimental study of the effects of non-binding pre-play communication on free-riding behaviour is of particular relevance for this work.

The issue of communication has been addressed by many experimental works which have studied the impact of non-binding pre-play communication – cheap talk – on cooperative behaviour and efficiency, in both non-threshold and threshold environments (see Ledyard, 1995; Davis and Holt, 1993 for surveys). In non-threshold games – despite the theoretical prediction of no effect – many studies have found a significant increase in group payoffs in social dilemma experiments (Dawes et al., 1977) and in the level of contributions and efficiency in PGs (Isaac et al., 1985); Isaac and Walker (1988b) notice how their results document the “impact of face-to-face communication in the reduction of free riding behavior” (p. 602), even when communication is costly (Isaac and Walker, 1991). Differently from the benchmark case, in presence of communication, repetition seems to increase cooperation (see Leyard, 1995 for a survey). Evidence is more mixed in threshold environments (Palfrey and Rosenthal, 1991 among others).

However, discussing in more detail Isaac and Walker's (1988b) results, Davis and Holt (1993) also note that nonbinding communication works to increase contributions only if environments are sufficiently simple, with a small number of participants, and that results are not particularly robust, thus ruling out communication as a “particular general remedy” to the PGs problem (p. 337).

More recently many authors have studied the impact of different forms of communication on cooperation and efficiency in different contexts (see among others Belianin and Novarese, 2005; Cason and Kahn, 1999; Bochet et al., 2006; Wilson and Sell, 1997; Güth et al., 2007) with mixed results, which, however, establish cheap talk as an important factor to enhance cooperation. Hence in our experiment we use a cheap talk environment as a benchmark, to rule out the possibility that an apparent effect of relational goods be due to the cheap talk that unavoidably accompanies it.

To our knowledge, only three papers may be compared more directly to ours. Brown-Kruse and Hummels (1993) and Cadsby and Maynes (1998) implement a design based on a threshold public goods game (TPGG), in which a sort of common work is present. A half of the subjects involved are requested to fill in a questionnaire before the experiment. The questionnaire “asks participants to share information about themselves. The goal [...] was to engender a sense of membership in a

group”⁵. The other half of the participants are randomly selected and play the TPGG without any previous treatment. Solow and Kirkwood (2002) test the effect of group identity in a basic (i.e. without threshold) public goods game (PGG). They select one half of the subjects “from a [...] pool of undergraduates [...] who had volunteered to take part in experiments generally” (p. 406) and the other half from the University of Iowa Hawkeye Marching Band. The former constitute the group of “strangers”, the latter that of people having a group identity. In addition, half of the strangers filled in a questionnaire as in Brown-Kruse and Hummels (1993) and in Cadsby and Maynes (1998), while the other half just played the PGG.

Our design differs from those used in the works quoted above in two fundamental aspects. The first is that our cheap talk is not finalised to the accomplishment of any duty, and does not concern the players’ strategies as in the public goods literature on cheap talk, but it is simply left open to the subjects. The second difference is in how we build the group identity: we do not rely on the mutual exchange of personal information nor on something in between a hobby and a job (playing in the Band), but rather on a task which is very closely related to the type of studies followed by the subjects and mirrors a possible job.

3. The experimental design and implementation

The experiment was conducted by the Laboratorio di Economia Sperimentale e Simulativa of the University of Piemonte Orientale at the Faculty of Economics of the University of Torino. A total of 128 undergraduate students took part in the experiment in 32 sessions, 8 for each of the 4 treatments. Out of them, 64 were females and 64 were males equally distributed across the sessions and the treatments: the groups were made of subjects of the same gender, to avoid possible chivalry (see Eckel and Grossman, 2001) or jealousy effects.

The game. In each session 4 subjects of the same gender were asked to play 10 repetitions of the following standard version of a public goods game (PGG). In each repetition each subject was endowed with 60 experimental monetary units (EMU) of the value of €0.01 each, and s/he had to decide whether to keep them or to allocate all or part of them to a common fund. The total amount of the EMU allocated to the fund were doubled and divided in equal parts to all participants, irrespective of how much each one had contributed to the fund. Every EMU which was not allocated to the common fund remained wholly to the participant as earnings, while for each EMU that the participant allocated to the fund - and for each EMU allocated to the fund by another participant- s/he kept as earnings 0.5 EMU. The total earnings in a session were given by the sum of what was earned in each of the 10 repetitions. No participation fee was paid.

In this standard version of PGG the unique sub-game perfect Nash equilibrium predicts always to contribute nothing to the common fund (perfect free-riding), while the Pareto-efficient solution predicts allocation to the fund of all the initial endowment (perfect cooperation).

The treatments. The experimental set-up consisted of four different treatments which combined the following different features: 1) the subjects knew each other when registering to the session, in which case it was required in the invitation to register together as a group for the same session; 2) the subjects did not know each other, in which case they registered to the experiment individually; 3) before playing the PGG the subjects have been asked either to perform a common work or 4) to engage in a cheap talk. In the common work treatment the subjects were given the balance sheets of three different companies and had to perform a budget analysis task, by calculating the Return on Sales (ROS), Return on Investment (ROI) and Return on Equity (ROE) ratios, and the cash flow margins for each company. They were asked to write down a short report and ranking of the companies on the basis of their calculations. The task lasted about 45-50 minutes. In the cheap talk treatment the subjects were gathered in a separate room and left together to chat. Differently from

⁵ Cadsby and Maynes (1998, p. 608).

other experiments, where cheap talk always implies pre-play communication on the game strategies, here no hint was given to the subjects about the topic of their conversation. In summary, we have the following four different treatments: 1) Previous acquaintance and common work; 2) previous acquaintance and cheap talk; 3) no previous acquaintance and common work; 4) no previous acquaintance and cheap talk, each treatment played by 4 groups of 4 males and 4 groups of 4 female subjects. As explained in the previous section, the cheap talk treatment is the benchmark for the assessment of the role of the two relational goods.

At the beginning of each session the subjects gathered in front of the computer room. In the common work treatment they were asked to sit together around a single computer terminal in the centre of the room which was available to them for consulting any material they might have needed to complete the task. In the cheap talk treatment they were taken to a different small room (with no computers) and asked to wait while the computer room was set and were conducted back to the computer room after 20 minutes.

Once the common work or the cheap talk was over, the subjects were sat at the four computer terminals at the extreme corners of the room, so that no communication was possible. At the beginning of the experiment the instructions appeared sequentially on each participant's computer screen and were read aloud by the experimenter at the same pace; the instructions would proceed to the following page only when all the four participants had clicked on the 'Continue' button on the screen.

The PGG game described above was illustrated to the subjects. It was made clear to them that their total earnings would be the sum of the earnings in each repetition and that they would be paid at the end of the experiment under the usual conditions of anonymity. It was highlighted that all the initial sum would be kept as earnings in case no EMU was allocated to the common fund and that the total earnings in case all participants allocated all the sum to the fund would be 120 EMU. After the description a trial round was run, making clear that the choice in this round would not contribute to the final earnings. After this a written summary of the instructions was distributed to the subjects and the real experiment started.

In each repetition the subjects faced a screen with an empty box where they had to enter the amount of EMU they wanted to allocate to the common fund. It was reminded to them that they had 60 EMU to allocate, and 2 minutes to take their decision, write the number and click the 'Save' button; after that time they would earn nothing on that repetition and would not be considered for the common fund division. After each subject had taken the decision or the time allocated had passed, a new screen for a new repetition appeared. In every repetition each subject could see summarized in a table the total amount of the common fund (but not the amount allocated by each subject), her/his earnings for that repetition, the amount of EMU kept and the division of the common fund and the total profit up to that repetition in EMU and euros. At the end of the 10 repetitions the total profit (in EMU and euros) appeared on the screen.

Once the experiment was over, the subjects were asked to leave the room and come back individually to be paid and fill in their receipts.

4. A digression on the role of gender effects

Among the number of results obtained by the experimental literature, one appears to be common to (almost) all the designs and settings: gender matters for choices. Croson and Gneezy (2009) sum up this individuating three macro-results: 1) women are more risk averse than men, 2) women and men are not differently socially oriented, but the social preferences of the former are more malleable than those of the latter; 3) women are more averse to competition than men. A public goods game, as the one that we use in this work, involves a certain level of risk and is based on pro-social attitudes. However difference (1) narrows as education and labour income increase (Croson and Gneezy, 2009); while our subjects are undergraduate students, and have no significant labour

income, they have a high level of education, which should therefore attenuate the effect of risk. As for effect (2), Nowell and Tinkler (1994), Rooney et al. (2005) and the survey by Croson and Gneezy (2009) find that women are generally more generous than men, and that this holds also in public goods games. This general finding is consistent with Gilligan (1982), who claims that women think of social problems in terms of care and responsibility in relationships, whilst men emphasise the role of rights and rules. Indeed, on this psychological basis it is straightforward to expect that, in a public goods game, women contribute more than men. However, while males' behaviour does not react significantly to different designs and contexts, that of females does (Croson and Gneezy, 2009). Gilligan (1982) and Croson and Gneezy (2009) suggest also that, while women should contribute more, they might respond quicker than males to the context and thus converge faster than males to the equilibrium (namely free riding); since cooperation and free riding are mutually exclusive, in reality it is not possible to cast any hypothesis on women's behaviour *ex ante*. In particular women's generosity is more evident in mixed-gender than in single-gender (female only) groups. This last result, however, does not appear to be robust across different games and contexts. Brown-Kruse and Hummels (1993) find the opposite effect: in their experiment (based on a PG game) females contributed significantly less than males; Cadsby and Maynes (1998) find no gender effect in a replication of Brown-Kruse and Hummels' design as well as in a threshold public goods experiment, and Solow and Kirkwood (2002) obtain the same results in a standard PG game. Dufwenberg and Muren (2006) find that people tend to be more cooperative with persons of their own than of the opposite sex, and this outcome is stronger among women than among men. Croson and Gneezy (2009, p. 16) suggest also that "women are more sensitive [than men] to social cues in determining appropriate behavior" and conclude that women's behaviour is much more context specific than men's. In a public goods experiment, we should thus expect that the choices of the female participants will be more path-dependent than males', i.e. women will retaliate more than men against behaviours that signal poor cooperation, while they will respond to cooperation more cooperatively than men. Nevertheless the literature also suggests that the presence of greed would result detrimental for cooperation more among men than among women (Simpson, 2003; Kuwabara, 2005), i.e. the level of cooperation of women in a scarcely cooperative environment is generally higher than that of men in an analogous situation.

A major element of context specificity is the gender composition of the group of players; in particular females tend to be very sensitive to this aspect and to condition their behaviour on it (Cadsby et al., 2010; Charness and Rustichini, 2011). There is evidence that both men and women condition their behaviour (and thus their choices) on the gender of their opponent(s), but the effect is stronger among women than among men. Andreoni and Vesterlund (2001) find that men and women are alternatively more altruistic than each other, depending on the price of giving: as it grows, females become more altruistic than males. However this is in contrast with Cox and Deck (2006), who find that women are more generous than men when "the costs of generous behaviour are low" [...] and "there is an absence of reciprocal motivation" (p. 597). Although the quoted works are not based on public goods games, they suggest 1) the presence of a gender effect when giving and reciprocity are involved and 2) the uncertainty of the direction of the gender effect. Eventually Charness and Rustichini (2011) find women to cooperate more when they are observed by in-group members than when out-group members observe them. This suggests that female players tend to cooperate less with strangers than with people they are already acquainted with.

As the extant literature on the gender effect is not conclusive, it is not possible to cast hypotheses *a priori*; however it is likely that also in our experiment gender will play some role, and therefore, in order to be able to control this better, we decided to form homogeneous gender groups of participants. Of course, this is not a neutral choice, as the composition of the group matters, but at least it ensures that all the groups have the same variance (0) as regards to their gender composition, and therefore our results cannot be biased by differences in the sex composition of the groups⁶.

⁶ On this see, for example, Iredale et al. (2008).

Incidentally, let us note that one might argue that our sample suffers another gender-related selection bias, as a typical BA female student of economics could be more aggressive than a male one, because of the presence of sexist stereotypes. An answer to this concern comes from Gerdes and Gränsmark (2010), who, studying the world of chess players at professional level (i.e. a male-dominated environment), find that this type of selection does not produce outcomes significantly different from those of other environments considered in the literature.

5. Results

We have considered mainly three variables, i.e. the individual contribution to the common fund, and the two possible extreme behaviours, perfect cooperation and perfect free riding. In each repetition, we define a player a “perfect co-operator” when he contributes the entire endowment of 60 experimental monetary units (EMU) to the mutual fund, whereas a participant who keeps all (i.e. contributes 0 to the fund) is defined a “perfect free rider”. Basically this entails that these two behaviours are represented by a couple of dummies, which take value 1 if the participant has displayed the relative behaviour and 0 otherwise.

The impact of the treatments and of the player’s gender on the absolute contribution is analysed by Mann-Whitney tests, while for the two extreme behaviours probit regressions are run.

The analysis of the results highlights that both previous acquaintance and common work increase the individual contribution⁷. Table 1 reports the average contributions for the whole sample: the participants who already knew each other before the experiment contributed on average 45% more than those who had no previous acquaintance (39.29 instead of 27.08); having worked together raises the contribution from 31.53 to 34.84 EMU, a difference of more than 10 percentage points. However, since the treatments cross each other, these two groups have a non-empty intersection, and therefore these results might be misleading. Columns 2 to 5 of Table 1 analyse the same effects in sub-samples which are homogeneous with respect to one of the treatments. So, column 2 (3) reports the effect of common work among those with (without) previous acquaintance. The figures show that the common work increases significantly the average contribution in presence of previous acquaintance, while produces the opposite effect in absence of it. This result, however, is due to strong gender effects that we will discuss further below: common work enhances cooperation among males, while decreases it among females. Column 4 (5) reports the average contributions of the players subject to the “common work” (“cheap talk”) treatment: the effect of the previous acquaintance is strong in both the columns, indicating that this variable has a positive and very significant impact on the choice of how much to contribute. In particular we can notice that people who already knew each other and worked together are those with the highest contributions. Hence, the results in Table 1 suggest also that the effects of the common work and of the previous acquaintance reinforce each other, originating a higher level of cooperation.

Table 2 introduces a variable that proved to be very relevant: gender. We have already discussed the importance played by gender in the human decision-making process, and in particular we have highlighted that men and women generally show gender-related differences in preferences (Croson and Gneezy, 2009). Indeed we find a strong and significant gender effect when the sample is divided according to the fact that the common work has been performed or not. In the former case men contributed 16% more than women, while, surprisingly, this result is reversed in the latter group (here women put in the common fund 14.5% more than men). The two effects are almost of the same magnitude, but they work in two opposite directions. As discussed before, this is consistent with Gilligan (1982) and Croson and Gneezy (2009); indeed, in our experiment in the first round the females without previous acquaintance in the common work treatment contributed significantly (at 90% level) less than the males from the same group: 25.37 and 36.94 EMU

⁷ In the following discussion all the results are significant at least 90%, if not specified otherwise. Details are in the tables.

respectively. Perhaps as a consequence of the low level of cooperation exhibited in the first round, the female propensity to converge to the equilibrium has then prevailed on the propensity to contribute.

Given the presence of a gender effect, all the following analyses compare the different treatments within groups of the same gender. Table 3 highlights that the previous acquaintance fosters cooperation: the effect is very strong and significant for all the sub-groups but one (namely the females under the common work treatment). Table 4 reports the average contributions with and without the common work; here the effect of performing some task together is always very significant, and positive, but in the case of women who did not know each other before the experiment. However, also in this case the treatment has a significant effect, but its sign is opposite than expected (i.e. it is strongly negative). The results contained in Tables 3 and 4 underline four main results: 1) acquaintance has a positive and strong effect on the contribution to the public good; 2) working together fosters cooperation among males and females with previous acquaintance, but disrupts it among females who are stranger to each other; 3) the influence of the acquaintance is generally stronger than that of the common work for both genders and 4) the effect of acquaintance is stronger among females than among males under the common work treatment. In sum: on the one hand both common work and previous acquaintance foster cooperation, on the other their value (and therefore their effect) is gender-related, and working together has a positive effect on women's contribution only when it is associated with previous acquaintance. Moreover, in this case the combined effect of the two relational goods is stronger for females than for males.

Public games allow for cooperation and for free riding. Free riding in our experiment is analysed in table 5. The dependent variable is a dummy which takes value 1 any time a player contributes 0 to the mutual fund. The table thus presents probit estimates. Previous acquaintance and common work have the expected sign (negative) and are strongly significant for both the whole sample and the male sub-sample. Among women only the acquaintance has a (hardly) significant influence, suggesting that the results for the whole sample are driven by the males. Indeed this is not surprising, as we have already highlighted that women without previous acquaintance react in an opposite way than males under the common work treatment. The magnitude of the marginal effects confirms the results of the Mann-Whitney test: while both the common work and the acquaintance have a positive influence on cooperation, the latter is stronger than the former. The variable "period", which captures the time trend for each session, suggests, consistently with the extant literature, that the free riding increases from the first to the tenth repetition.

The last table presents the results for the behaviour opposite to the previous one: here the dependent variable is a dummy capturing perfect cooperation, i.e. the contribution of the full endowment. It takes value 1 if the player puts 60 EMU in the mutual fund and 0 otherwise. As expected both the previous acquaintance and the common work are positively related to this extreme cooperative behaviour both in the whole sample and in the male group, which leads the result in the whole sample, as among females the significance of the coefficients drops under the usual levels of confidence (for the same reason as before). Here it is interesting to notice that the influence of working together before playing the game is (slightly) stronger than that of the previous acquaintance, a feature somehow at odds with the datum on the average cooperation. Previous acquaintance appears to be a strong preventer of free riding, while full cooperation is enhanced especially by team working among men independently of previous acquaintance and among women only in presence of this last factor.

Each group involves four participants; the variable "value of the fund" is the sum of the contributions to the mutual fund of the players other than the one whose behaviour is analysed, lagged one period. As expected, this variable displays a negative impact on the probability of free riding and a positive one on the probability of fully cooperating. It is assumed as a measure of how

a player reacts to the others' contributions⁸. It is interesting to notice that the marginal effect is larger for women than for men and that the effect is stronger on full cooperation than on free riding. This suggests that the individuals of both sexes are more prone to respond to cooperation with cooperation than to punish non-cooperative behaviours through free riding. In both cases, however, the marginal effects indicate that the influence of this variable is small in magnitude.

Finally, also cheap talk may be considered a sort of relational good; the positive effect of common work on the propensity to cooperate that we found implies that the common work is more effective than cheap talk to promote cooperation.

6. Conclusions

Our results suggest that: 1) relational goods do enhance cooperation; 2) both common work and previous acquaintance increase the average contribution to the public good; 3) acquaintance and working together are rather complements than substitutes; 4) there is a relevant gender effect, where the women contribute more or less than men, depending on the treatment, and thus providing further evidence that the women's behaviour is more context-specific than men's; and 5) different relational goods have different effects on the propensity to cooperate. It is of interest that result 4 is not at odds with some previous findings (see also section 2 above). Brown-Kruse and Hummels (1993) and Cadsby and Maynes (1998) find no difference between the questionnaire and strangers groups overall, although the females who filled-in the questionnaire start contributing significantly less to the public good than all the other groups, yet they end up with contributions that are statistically equal to the others'. Solow and Kirkwood (2002) show that the mean contributions are the same for both the strangers and the questionnaire subjects; furthermore the average contribution of the males from the Band is significantly larger than that of stranger and questionnaire males, while no significant effect is detectable between different groups of females (although those who filled in the questionnaire contributed less than the strangers and those from the Band).

Result 5 also deserves a further comment. We found that common work is more effective than cheap talk (with one exception), but obviously we cannot generalize this results to the real world. The case of non-acquainted females indicates that there may even be cases where the common work is detrimental. However, we found strongly different effects of different relational goods: common work, acquaintance and cheap talk. Our experiment supports the hypothesis that the specific nature of the relational goods is relevant. This holds also for not previously acquainted females: in this case the cheap talk is more effective than common work, but again the two relational goods induce a different behaviour. Needless to say, the last result was quite unexpected, and to our opinion deserves a deeper inquiry.

To sum up, we may resort to a biological metaphor to suggest quite safely that the "relational goods" are more a family (or a gender) than a species. It should be of great interest to study the specific patterns of the different species.

⁸ Andreoni and Petrie (2004) find that informing the participants about the others' average contribution does not affect the players' behaviour. Also Frey and Meier (2004) do not find evidence in favour of this type of conditional cooperation. However here we are considering the impact on extreme choices instead of the mean contribution.

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Table 1. Effects of previous acquaintance and common working on contributions (Mann-Whitney test)

	Whole sample	With previous acquaintance	Without previous acquaintance	With common working	Without common working
With previous acquaintance	39.29			44.62	33.95
Without previous acquaintance	27.08			25.05	29.10
Significance ¹	***			***	***
With common working	34.84	44.62	25.05		
Without common working	31.53	33.95	29.10		
Significance ¹	***	***	*		

1) * Significant at 90% level; *** significant at 99% level.

Table 2. Gender effect on contributions (Mann-Whitney test)

	Whole sample	With previous acquaintance	Without previous acquaintance	With common working	Without common working
Males	33.42	38.35	28.49	37.45	29.39
Females	32.94	40.22	25.66	32.22	33.66
Significance ¹	°	°	°	***	***

1) ° Non significant at conventional levels; * significant at 90% level; *** significant at 99% level.

Table 3. Effect of previous acquaintance on contributions, by gender (Mann-Whitney test)

	With common working (males)	Without common working (males)	With common working (females)	Without common working (females)
With previous acquaintance	42.14	34.57	47.11	33.34
Without previous acquaintance	32.76	24.22	17.34	33.99
Significance ¹	***	***	***	°

1) ° Non significant at conventional levels; * significant at 90% level; *** significant at 99% level.

Table 4. Effect of common working on contributions, by gender (Mann-Whitney test)

	With previous acquaintance (males)	Without previous acquaintance (males)	With previous acquaintance (females)	Without previous acquaintance (females)
With common working	42.14	32.76	47.11	17.34
Without common working	34.57	24.22	33.34	33.99
Significance ¹	***	***	***	***

1) *** Significant at 99% level.

Table 5. Effects on free riding (probit estimates; standard errors in brackets)

	Whole sample		Males		Females	
	Coefficient	Marginal effect	Coefficient	Marginal effect	Coefficient	Marginal effect
Male	0.057 (0.104)	0.009 (0.017)				
Common working	-0.257 (0.106)***	-0.043 (0.017)***	-0.378 (0.150)***	-0.059 (0.024)***	-0.208 (0.158)	-0.036 (0.027)
Previous acquaintance	-0.431 (0.112)***	-0.072 (0.019)***	(0.163)*** -0.009	(0.026)*** -0.001	(0.164)* -0.010	(0.028)* -0.002
Value of the fund (lagged one period)	-0.010 (0.001)***	-0.002 (1*10 ⁻⁴)***	(0.001)***	(2*10 ⁻⁴)***	(0.001)***	(2*10 ⁻⁴)***
Time to choose	0.004 (0.004)	6*10 ⁻⁴ (6*10 ⁻⁴)	0.003 (0.006)	5*10 ⁻⁴ (0.001)	0.003 (0.005)	5*10 ⁻⁴ (8*10 ⁻⁴)
Time to review the results	0.003 (0.005)	5*10 ⁻⁴ (9*10 ⁻⁴)	-0.006 (0.008)	-0.001 (0.001)	0.007 (0.007)	0.001 (0.001)
Period	0.035 (0.022)	0.006 (0.004)	0.093 (0.032)***	0.014 (0.005)***	-0.014 (0.031)	-0.002 (0.005)
Constant	-0.236 (0.501)		0.252 (0.753)		-0.342 (0.639)	
Pseudo R ²	0.253		0.270		0.215	

* significant at 90% level; ** significant at 95% level; *** significant at 99% level.

Table 6. Effects on perfect cooperation (probit estimates; standard errors in brackets)

	Whole sample		Males		Females	
	Coefficient	Marginal effect	Coefficient	Marginal effect	Coefficient	Marginal effect
Male	-0.394 (0.091)***	-0.125 (0.029)***				
Common working	0.163 (0.094)*	0.052 (0.030)*	0.388 (0.131)***	0.113 (0.038)***	-0.029 (0.149)	-0.010 (0.051)
Previous acquaintance	0.186 (0.093)**	0.059 (0.030)**	0.343 (0.132)***	0.101 (0.038)***	0.015 (0.139)	0.005 (0.047)
Value of the fund (lagged one period)	0.014 (0.001)***	0.004 (2*10 ⁻⁴)***	0.012 (0.001)***	0.003 (3*10 ⁻⁴)***	0.015 (0.001)***	0.005 (4*10 ⁻⁴)***
Time to choose	-0.006 (0.003)*	-0.002 (0.001)*	-0.020 (0.007)***	-0.006 (0.002)***	0.001 (0.004)	3*10 ⁻⁴ (0.001)
Time to review the results	0.006 (0.005)	0.002 (0.002)	-0.010 (0.008)	-0.003 (0.002)	0.014 (0.007)**	0.005 (0.002)**
Period	-0.030 (0.020)	-0.010 (0.006)	-0.021 (0.028)	-0.006 (0.008)	-0.038 (0.030)	-0.013 (0.010)
Constant	-2.781 (0.436)***		-1.520 (0.675)**		-3.542 (0.613)***	
Pseudo R ²	0.330		0.261		0.413	

* significant at 90% level; ** significant at 95% level; *** significant at 99% level.

APPENDIX

ISTRUZIONI

Buongiorno. Vi ringraziamo per avere accettato di partecipare all'esperimento. Non ci saranno particolari difficoltà, né domande trabocchetto. Dovrete seguire attentamente le istruzioni che compariranno via via sullo schermo. Le risposte che fornirete saranno assolutamente anonime. Non sarà in nessun modo possibile a chi elabora i dati risalire all'identità di chi ha fornito le singole risposte

In questo esperimento avrete a disposizione un certo numero di Unità Monetarie Sperimentali (UMS), ciascuna del valore di 0.01 euro, che potrete versare in un fondo comune.

Potrete scegliere liberamente quante UMS versare, da un minimo di zero a un massimo di tutte, secondo modalità che vi saranno spiegate in seguito.

Vi verrà chiesto di effettuare questa scelta per 10 volte. Per ciascuna di esse avrete a disposizione 60 UMS.

Non è consentito trasferire UMS da una volta all'altra.

Il vostro guadagno complessivo sarà dato dalla somma di quanto avrete guadagnato ciascuna volta. Questo guadagno sarà calcolato e vi sarà pagato alla fine dell'esperimento, seguendo le modalità di riservatezza descritte in precedenza.

Il totale versato nel fondo comune verrà moltiplicato per 2 e distribuito in parti eguali fra tutti voi indipendentemente da quanto ciascuno abbia versato.

Per ogni UMS che NON versate al fondo comune guadagnerete 1 UMS, mentre per ogni UMS che versate nel fondo comune guadagnerete 0,5 UMS (dato che l'UMS viene moltiplicata per 2 e poi divisa per 4). Guadagnerete anche 0,5 UMS per ogni UMS versata nel fondo comune da qualcun altro.

E' importante che vi sia chiaro che:

- ogni volta in cui non versate nulla nel fondo comune, vi terrete la somma che vi è stata assegnata, cioè 60 UMS;
- se tutti versano l'intera somma nel fondo comune, ciascuno guadagnerà $2 \cdot 60$ UMS.

Adesso avrete la possibilità di familiarizzarvi con la videata di scelta che verrà utilizzata durante l'esperimento.

La vostra scelta in questo caso non verrà salvata né contribuirà al vostro pagamento finale.

Con la prossima videata comincerà l'esperimento.

Vi ricordiamo che dovrete effettuare la scelta 10 volte.

Comparirà una videata per ogni ripetizione, e vi sarà indicato ogni volta l'esito delle scelte precedenti: vedrete l'ammontare complessivo del fondo comune e il vostro guadagno, ma non quanto ha versato ciascuno degli altri partecipanti.

Alla fine di ogni ripetizione, il valore del fondo comune verrà moltiplicato per 2 e interamente distribuito in parti eguali. All'inizio di ogni ripetizione il valore del fondo sarà zero.

Vi ricordiamo che all'inizio di ogni ripetizione vi verranno assegnate 60 UMS (ciascuna del valore di 0.01 euro), che devono essere interamente utilizzate in quella ripetizione.

Il vostro guadagno finale sarà la somma di quanto avrete guadagnato in ciascuna ripetizione.

Se avete dei dubbi, potete consultare in ogni momento il foglio di istruzioni che vi è stato consegnato

SCREEN SHOTS

Tempo rimanente [secondi]: 92

RIPETIZIONE NUMERO 1

Avete a disposizione 60 UMS. Dovete scrivere nella casella sottostante quante ne versate al fondo comune e cliccare sul pulsante "SALVA" per registrare la vostra scelta.

Avete 2 minuti per effettuare la scelta.
Se entro 2 minuti non avrete scritto un numero e cliccato su "SALVA", il programma proseguirà automaticamente. In tal caso il vostro pagamento per questa ripetizione sarà di 0 euro, e non verrete considerati per la divisione del fondo comune.

Vi ricordiamo che potete versare da un minimo di 0 a un massimo di 60 UMS.

Quante UMS volete versare al fondo comune?

SALVA

In questa ripetizione il fondo comune ha incassato complessivamente 25 UMS.

Il vostro guadagno in questa ripetizione è di 85.00 UMS:

-> 35 UMS che avete tenuto

->più

-> 50 UMS dalla divisione del fondo comune (ottenuti moltiplicando il fondo comune per 2 e dividendo per il numero di partecipanti che hanno effettuato una scelta).

Il vostro guadagno complessivo è di 85.00 UMS, cioè 0.85 euro.

CONTINUA

Tempo rimanente [secondi]: 113

Ripetizione	Totale UMS nel fondo comune	UMS che avete contribuito al fondo comune
1	25	25

RIPETIZIONE NUMERO 2

Avete a disposizione 60 UMS. Dovete scrivere nella casella sottostante quante ne versate al fondo comune e cliccare sul pulsante "SALVA" per registrare la vostra scelta.

Avete 2 minuti per effettuare la scelta.

Se entro 2 minuti non avrete scritto un numero e cliccato su "SALVA", il programma proseguirà automaticamente. In tal caso il vostro pagamento per questa ripetizione sarà di 0 euro, e non verrete considerati per la divisione del fondo comune.

Vi ricordiamo che potete versare da un minimo di 0 a un massimo di 60 UMS.

Quante UMS volete versare al fondo comune?

SALVA

In questa ripetizione il fondo comune ha incassato complessivamente 16 UMS.

Il vostro guadagno in questa ripetizione è di 76.00 UMS:

-> 44 UMS che avete tenuto

->più

-> 32 UMS dalla divisione del fondo comune (ottenuti moltiplicando il fondo comune per 2 e dividendo per il numero di partecipanti che hanno effettuato una scelta).

Il vostro guadagno complessivo è di 161.00 UMS, cioè 1.61 euro.

CONTINUA

L'esperimento è terminato.
Avete guadagnato complessivamente 161.00 UMS cioè 1.61 euro.

Prima di ricevere il vostro pagamento, vi chiediamo di compilare il breve questionario che segue.

CONTINUA

COMMON WORK

1. Sulla base del calcolo dei consueti indici di redditività (ROI, ROE e ROS), stabilire quale delle tre società in esame risulta più redditizia.
2. Confrontare anche le tre società sulla base dell'indice di auto-copertura del capitale fisso (rapporto tra capitale proprio e immobilizzazioni) e del quoziente di tesoreria (rapporto tra liquidità correnti e differite sulle passività correnti).

INSTRUCTIONS

Good morning. Thank you for taking part in this experiment. There would not be any difficulties or tricky questions. You are required to follow carefully the instructions which will appear sequentially on your screen. Your answers will be completely anonymous. It would not be possible for the experimenters to know the identity of participants from the answers.

In this experiment you will be given a certain number of Experimental Monetary Units (EMU), each of a value of 0.01 euros, which you will be able to allocate to a common fund.

You will be free to choose the amount of EMU to allocate, from a minimum of zero to a maximum of all, in a way which will be explained to you below.

You will be asked to make this choice 10 times (rounds). In each round you will have 60 EMU available.

You are not allowed to transfer EMU from one round to another.

Your total earnings will be given by the sum of what you have earned in each round. These earnings will be calculated and will be paid to you at the end of the experiment, following the anonymity mode described above.

The total amount allocated to the fund will be doubled and distributed in equal parts among all participants independently of how much each participant has allocated to the fund.

For each EMU you DO NOT allocate to the fund you will earn 1 EMU, while for each EMU you allocate to the fund you will earn 0.5 EMU (as an EMU gets multiplied by 2 and then divided by 4). You will also earn 0.5 EMU for each EMU allocated to the common fund by another participant.

It is important that the followings are clear to you:

- each time you don't allocate anything to the common fund, you will keep all the sum given to you, that is, 60 EMU.
- If all participants allocate the entire amount to the common fund, each participant will earn $2 \cdot 60$ EMU.

Now you will have the possibility to get used to the experiment screen in a practice round.

Your choice in this case will not be recorded and will not therefore contribute to your final payment.

With the following screen the experiment will start

We remind you that you will have to take your choice 10 times.

Each round a screen will appear, and each time you will see indicated the outcome of previous choices: you will see the total amount of the common fund and you earnings, but not the amount allocated individually by the other participants.

At the end of each round the value of the fund will be doubled and wholly distributed in equal parts.

At the beginning of each round the value of the fund will be zero.

We remind you that at the beginning of each round you will be given 60 EMU (each of the value of 0.01 euros, which must be entirely used in that round).

Your final earnings will be the sum of what you have earned in each round.

If you have any doubt, you can at any moment consult the instructions you have been given.

SCREEN SHOTS

Remaining Time (seconds)

ROUND NUMBER 1

You have 60 EMU. You must write in the box below how many of them you want to allocate to the common fund and click on "SAVE" to record your choice.

You have 2 minutes to make your choice.

If you have not written a number and clicked on "SAVE" within 2 minutes the program will proceed automatically. In this case your payment for this round will be 0 and you will not be considered for the division of the common fund.

We remind you that you can allocate from a minimum of 0 to a maximum of 60 EMU.

How many EMU do you want to allocate to the common fund?

SAVE

In this round the common fund has collected 25 EMU overall.

Your earnings in this round amounts to 85.00 EMU:

- 35 EMU you have kept

plus

- 50 EMU from the division of the common fund (obtained by multiplying the common fund by 2 and dividing by the number of participants who have made a choice).

Your total earnings amounts to 85.00 EMU, that is, 0.85 euros.

CONTINUE

Remaining Time (seconds)

Round	Total EMU in the common fund	EMU you have allocated to the common fund
1	25	25

ROUND NUMBER 2

You have 60 EMU. You must write in the box below how many of them you want to allocate to the common fund and click on “SAVE” to record your choice.

You have 2 minutes to make your choice.

If you have not written a number and clicked on “SAVE” within 2 minutes the program will proceed automatically. In this case your payment for this round will be 0 and you will not be considered for the division of the common fund.

We remind you that you can allocate from a minimum of 0 to a maximum of 60 EMU.

How many EMU do you want to allocate to the common fund?

SAVE

In this round the common fund has collected 16 EMU overall.

Your earnings in this round amounts to 76.00 EMU:

-44 EMU you have kept

plus

-32 EMU from the division of the common fund (obtained by multiplying the common fund by 2 and dividing by the number of participants who have made a choice).

Your total earnings amounts to 161.00 EMU, that is, 1.61 euros.

CONTINUE

The experiment is now over.
You have earned 16 EMU, that is, 1.61 euros overall.

Before receiving your payment we ask you to fill in the following short questionnaire.

CONTINUE

COMMON WORK

1. On the basis of the usual profitability ratios (ROI; ROE and ROS), establish which of the three companies under consideration is the most profitable.
2. Compare the three companies also on the basis of the ratio of self-coverage of capital assets (ratio between net assets and capital assets) and cash flow margin (ratio between current assets and current liabilities).

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**Political Theory Series

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