Essays in Behavioral and Experimental Economics

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Essays in Behavioral and Experimental Economics

A dissertation presented

by

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Essays in Behavioral and Experimental Economics

ABSTRACT

This dissertation consists of three essays which make use of laboratory experiments in order to investigate how procedural or contextual factors impact human behavior.

The first essay concerns cooperative behavior in groups formed with quota-based selection rules. Participants are randomly assigned to either an orange or a purple group. In the quota treatment, orange participants are chosen as members of a selected group by performance in a simple unrelated math task whereas purple participants are chosen based solely on the quota. The results show significantly less cooperation (measured as behavior in a public good game) in the quota treatment compared to a control treatment where all participants are treated symmetrically. Furthermore, this tendency is observed in both the orange and the purple participants, and regardless of the color of the matched player.

The second essay describes the results of a Prisoner's Dilemma experiment were we document that behavior is more cooperative when the game is called the Community Game than when it is called the Stock Market Game. However, the difference vanishes when only one of the subjects is in control of her action. The social framing effect also vanishes when the game is played se-

quentially. These findings are inconsistent with the hypothesis that the Community label triggers a desire to cooperate, but consistent with the hypothesis that social frames are coordination devices. More generally, our evidence indicates that social frames enter beliefs rather than preferences.

The third essay concerns testosterone change following monetary wins and losses and how these predict future financial risk-taking. We influence testosterone in male participants by having them win or lose money in a chance-based competition. Our design employs two treatments where we vary the amount of money at stake so that we can abstract from income effects. Controlling for the difference in score, we find that men who win have a higher increase in testosterone than men who lose. We also find that men who experience a greater testosterone increase are more willing to make risky gambles and this association remains when controlling for whether the man won or lost the competition.

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1. Quotas and Cooperation

1.1 Introduction

Selection by quotas is a commonly used policy tool for getting closer to a desired representation between genders, races or ethnicities. In political settings, the use of quotas is widespread (Krook, 2009) and its use has now also reached other spheres, such as the corporate world (see e.g. Ahern and Dittmar, 2012). Even though quotas are often effective in reaching a specific numerical representation, there can be unintended side effects (Pande and Ford, 2011). The focus of this chapter is on one such effect, namely cooperation problems in groups created by quotas.

In 2003, the Norwegian government unexpectedly announced that the country's public limited- and state-owned companies would be required to have at least 40 percent representation of each gender on their boards by 2008 to avoid forced liquidation. The policy has been found to have affected the management practices of boards and adversely influenced short-run profits (Matsa and Miller, 2011; Ahern and Dittmar, 2012). It has been hypothesized that one change in management practices was related to cooperation problems, which some boards experienced as a consequence of the quota (Stenseng, 2010; Dagens Naeringsliv, 2010; see also Clarke, 2010). [1]

Inefficient and uncooperative behavior following quotas or other procedures with preferential selection can also be found in other contexts, for example in education. In the United States,

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[1] Morten Huse and his coauthors have written extensively about the experience of women on corporate boards in Norway. In particular the discussion about how board members who enter the board by different procedures, for example women or employee representatives, may experience lower esteem than other board members is relevant for this chapter. See e.g. Huse and Solberg (2006), Huse et al. (2009), and Nielsen and Huse (2010). See also Terjesen et al. (2009).

quotas for minority students have been used at all levels of schooling in attempts to increase diversity of the student body. Sometimes the consequences have been dramatic, as was the case during the Boston riots in the 1970s following the institution of quotas for, and busing of, minority children to previously primarily white schools (Lukas, 1986). At both the college and the graduate school level quotas have been documented to lead to uncooperative behavior, for example in the form of reluctance to share information, both within the student population and between students and teachers (Dreyfuss and Lawrence, 1979; Crosby and VanDeVeer, 2000).

There are also examples of how affirmative action in the workplace can lead to cooperation problems. One area where this has been found is among firefighters. In the United States, affirmative action and quotas have been used since the late 1960s to increase representation of female and minority firefighters. This has been claimed to lead to a decrease in cohesion and cooperation between colleagues, both in training and on the job (Dreyfuss and Lawrence, 1979; Chetkovich, 1997).

The above examples suggest that quotas can give rise to cooperation deficits. But whereas a significant amount of research has been dedicated to understand the impact of quotas on group quality, there has been little systematic research regarding effects on cooperative behavior. The aim of this chapter is to use a laboratory experiment to investigate whether preferential selection in the form of an unrepresentative quota has a negative impact on group cooperation and, if so, to examine the underlying mechanisms.

The experimental method has some limitations. Key factors from the "real world" settings discussed above, such as interaction over longer time periods, will not be captured in the laboratory setting. However, laboratory experiments also have advantages: the controlled environment in the lab allows us to abstract away from pre-formed groups, such as gender or race, and focus on the process of how a group is formed. The laboratory environment also enables the researcher to use randomization to identify causation.

In the experiment described in this chapter groups are created by randomly allocating the 16 participants in each experimental session a color: either orange or purple. Participants are then asked to do a math task. In the unrepresentative quota treatment, four orange participants are chosen as members of a selected group by performance in the math task whereas four purple participants are chosen based on a quota that requires a certain representation of the two colors in the selected group. In the control treatment the orange and purple participants are treated symmetrically and all members of the selected group are chosen based on performance on the math task.

This design captures two aspects of how unrepresentative quotas are often perceived: first, that one group is treated preferentially over the other in the selection process and second, that less weight is given to performance when selecting candidates from this group.[2]

In order to study cooperative behavior, I next let the eight participants in the selected group play a two-person public good game. This is designed so that all members take part in seven public

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[2] These are not all potential aspects of unrepresentative quotas (others include slot-taking and fixed representation), but I limit the focus to these two aspects in this experiment. Note also that these two features, (1) preferential selection and (2) different role given to performance can describe also other situations than selection by quotas where favoritism plays a role.

good games: one against each other member of the selected group. Furthermore, the participants know the color of the person they are playing in each public good game. I find that cooperation is significantly lower when the selected group is created by the unrepresentative quota. In that case, participants contribute about 30 percent of their endowment to the public good, compared to a contribution rate of over 50 percent in the control treatment. I call this decline in cooperation the *quota effect*.

In addition to establishing this result, this design makes it possible to draw some inferences about the mechanism underlying the quota effect. Various theories offering explanations for the quota effect have different predictions about whether the quota effect should be relation-specific (i.e. only be present in some relations in the group, for example when those who are not advantaged play with those who are advantaged) or general (i.e. apply similarly to all relations). I find that the quota effect is general, a finding which contradicts some theories, for example about the quota effect having its roots in a desire by those who are not advantaged by the quota to punish those who are advantaged.

The experimental design also addresses a key policy aspect of quotas, namely the question of justification. Following on work which has shown that the acceptance of affirmative action policies can be affected by whether, and how, the policy is justified (see e.g. Murrell et al., 1994 and Heilman et al., 1996), I introduce two separate justifications for the unrepresentative quota, the first emphasizing efficiency gains and the second emphasizing fairness. However, even though participants report that they find our fairness argument especially convincing, they do not become more cooperative when a justification is provided.

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As discussed above, the unrepresentative quota in this experimental design captures two aspects of how affirmative action policies can be perceived. First, that it is a selection process that gives preferential treatment to one group over another. Second, that performance plays less of a role in selection for candidates from the group that is treated preferentially. In order to understand the respective role of these two aspects in causing the decrease in cooperation in the quota treatment, I also conduct two treatments that are identical to the two main treatments described above with the only difference that selection by performance is replaced with random selection. Hence, in this version of the quota treatment, preference is still given to one group, but the role of performance is the same for both groups. I find that this version of the unrepresentative quota has no negative impact on cooperation relative to the control treatment of symmetric treatment.

The research presented in this chapter is related both to the work on procedural fairness and procedural justice. The latter originally aimed at understanding the feelings of entitlement which can arise when individuals earn their roles in an experiment. (Hoffman and Spitzer, 1985). A well-known result in this strand of literature is due to Hoffman et al. (1994, 1996), who show that when the role as first mover in a dictator- or ultimatum game is earned, for example by answering a quiz, offers to receivers are significantly lower than when the role is allocated randomly. In the literature on procedural justice, it has instead been emphasized that a procedure which is perceived as just may provide social motivation and thereby influences cooperative behavior (Lind and Tyler, 1988; Tyler and Lind, 1992; Tyler and De Cremer, 2006).

This chapter is also related to the research of Balafoutas and Sutter (2012). Their starting point is the work of Niederle et al. (2013), who show that an affirmative action procedure which guaran-

tees women equal representation among the winners of a competition makes them as likely to enter a competitive setting as equally qualified men (in the absence of the affirmative action policy they find that women under-select into the competition, see also Gneezy et al., 2003; and Niederle and Vesterlund, 2007).

Balafoutas and Sutter (2012) conduct an experimental investigation of the effects of such an affirmative action procedure on things other than willingness to compete. They follow the design of Niederle et al. (2013) and add a post-competition teamwork task and a coordination game. They find no effects from the quota on performance in the teamwork or the coordination task. Importantly however, the team task and the coordination game that they use do not incorporate conflicting motives between the group and the individual, i.e. their games are not social dilemmas. The research described here, on the other hand, uses a public good game in order to specifically study cooperation.

The rest of the chapter is organized as follows. Section 1.2 describes the experimental design, provides a conceptual framework and generates testable hypotheses. In Section 1.3, I outline and discuss the results from the experiment. Section 1.4 discusses different theories that make predictions about the effects of an unrepresentative quota and relates those theories to the experimental results. Section 1.5 concludes.

1.2 Experimental Design

In the experiment 16 people participated in each session. After being seated in the laboratory, participants were told that the study would have several parts in which they could earn money. It

was also made clear that all the money earned would be paid to them in private at the end of the experiment and that the exchange rate between points in the experiment and dollars was such that 10 points corresponded to 3 dollars.[3]

After these instructions were explained, the 16 people in each session were randomly allocated a color: either orange or purple.[4] These colors were chosen since they are relatively neutral for American participants (unlike for example red vs. green). The participants were told their color allocation on the computer screen and were also given a silicon bracelet with their color to wear for the duration of the experiment. Thereafter they spent two minutes filling out a paper form with five associations to their color. This was done in order to give the participants some time to internalize their randomly allocated color.

After randomization of colors, participants were told that they would be given a number of math tasks where each task would consist of adding up five two-digit numbers and that they would be paid 1 point (30 cents) for each correct answer. They had five minutes available to do as many tasks as possible with the maximum available tasks being 15. It was made clear that the number of tasks that they answered correctly would not be revealed until the end of the experiment.[5]

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[3] All experimental instructions can be found in Appendix A.1.

[4] The fact that colors were allocated randomly ensured that the characteristics of the two groups were similar, e.g. with regards to gender, age and ethnicity. See Appendix A.2 for details.

[5] This task has been used previously by among others Niederle and Vesterlund (2007, 2010) and performance has generally been found not to differ between different groups such as between gender or ethnicities. I confirm that there are no differences in performance between people of different ethnicities, but I find both gender and age differences. See Appendix A.3.

After having solved math tasks for five minutes, part three of the experiment followed. Participants were told that out of the 16 people in the session, eight would be chosen as members of a selected group called the "high-stake group". The other eight participants would remain in the study as members of the "regular-stake group". The instructions made clear that the members of the two groups would do the same thing in the rest of the experiment, but that the high-stake group members would have the chance to earn more money. This design and the naming of the two groups was chosen in order to make it desirable to participants to be selected to the high-stake group without imposing a specific context on the situation.

It was also explained to the participants that the high-stake group would have four orange and four purple members. By varying the color composition of the underlying group of 16 participants (a composition which was made clear to all participants) this either meant that the two colors were treated symmetrically (the underlying group then consisted of eight purple and eight oranges players) or an unrepresentative quota favoring the purples (in these sessions the underlying group contained four purple and twelve orange players).

The aim of part four of the experiment was to measure the degree of cooperativeness within the high-stake group. I chose to do this with one of the classic social dilemmas, a two person public good game. In this game, participants were assigned to pairs and given a personal endowment of 20 points (USD 6) in the high-stake group and 10 points (USD 3) in the regular-stake group. I selected a two-person public good game instead of a n-person public good game (with $n > 2$) so that it could be made clear to the participants which color the person had, with whom they were

playing each game. This was done in order to be able to assess whether participants treated people of different colors differently.

The public good game was conducted in a standard manner (see Ledyard, 1995 for a survey of public good game experiments), and both participants in the pair had to choose how much of their endowment to keep and how much to contribute to a joint project. Points contributed by the two participants were summed together, multiplied by 1.5 and then distributed equally between them. Everyone played the game with each of the other seven members of their group (either the high-stake- or the regular-stake group) and the contribution decisions were made simultaneously in each of the seven games. Participants were told that they would be paid according to the outcome of one randomly chosen game out of the seven that they played. The only thing the participants were told about the person they played with in a specific game was that person's color.

After the participants had made their choices in all games, the experiment proceeded to a questionnaire in which the participants were asked to report their gender, age and ethnicity. In addition, the participants were asked if they found the process by which the members of the high-stake group was chosen fair.

1.2.1 Selection by performance: Treatments 1 and 2

As described above, the high-stake group was put together such that it would consist of four purple and four orange players. In the main treatments the selection was done so that the four players of each color who performed best in the math task were admitted into the high-stake group. In the unrepresentative quota treatment (treatment 1), there were twelve orange and four purple

players in the underlying group of 16. This implies that when the four best performers of each color were selected, performance did not matter for the purple players, since all of them were automatically selected. This selection process was hence characterized by a quota that preferentially treated the purple players, and made performance in the math task relevant only for the selection of the orange players.

I compare the participants' behavior in the unrepresentative quota treatment (treatment 1) to the participants' behavior in the control treatment (treatment 2). In the latter treatment there was equal representation of purple and orange players in the original group of 16 – i.e. there were eight orange and eight purple players. This meant that the two colors were treated symmetrically and the eight players who performed best in the math task in part 2 were in expectation selected into the high-stake group.[6]

With the data from treatments 1 and 2 we can compare behavior in the public good game in a high-stake group consisting of four players of each color, which differs only in how the group members were selected: either by a process characterized by an unrepresentative quota for the purple, or by a process where all eight high-stake group members were selected by performance and symmetrically treated, regardless of color. This comparison allows us to investigate whether cooperative behavior differs depending on the selection process. In addition, by examining if the

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[6] Note that selection may still not be entirely by performance. If, for example, there are five purple and three orange players among the eight best performers, only the four best of the purples will be selected and one orange player who is not among the eight best will be admitted into the high-stake group. As a future extension of this work, it would be interesting to compare this situation with one where the eight best players are chosen, completely disregarding their colors.

players' behavior depends on their own color and/or on the color of the person they are playing a particular game with, we can draw some inferences about the mechanism at work.

1.2.1.1 Justification of the Unrepresentative Quota: Treatments 1b and 1c

In the unrepresentative quota treatment (treatment 1) described above, participants were not provided any explanation for the preferential treatment that the purple players were given. When quotas are used it is most often for one of two reasons. A first argument that is commonly used for quotas is that they are there to enhance efficiency. For example, advocates of corporate-board quotas for women claim that they will lead to more valuable perspectives being represented on the board, which in turn will improve the board's decision making and the company's performance (see e.g. Daily and Dalton, 2003; and Huse and Solberg, 2006). Second, it is sometimes argued that a quota corrects unfairness. Examples of this can be found in the policy of affirmative action in the United States that gives preference to members of minority groups (for example African Americans) with reference to this process being a compensation for previous unjust treatment of members of this community (see e.g. Heilman et al., 1996 and Murrell et al. 1994).

To be able to address this practical policy aspect of quotas, I designed two additional treatments that were versions of the unrepresentative quota treatment, in which justifications along the above lines were given.

Treatment 1b was identical to treatment 1 with the exception that an efficiency argument for the quota was given: When the difference between the high-stake and the regular-stake group was introduced to the participants, the instructions explained that the payoff rule was such that the

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members of the high-stake group would only have a higher endowment than the members of the regular-stake group if the high-stake group had an equal proportion of purple and orange players. When the process by which the high-stake group members were selected was explained, the instructions pointed out that when all the four purple players were selected the arrangement fulfilled the requirement of equal proportions for the high-stake group, hence giving all high-stake group members the chance to earn more money than what would otherwise have been the case.

Treatment 1c was designed to capture the fairness argument, which was done by introducing a harder math task in which the numbers that were to be added up had three digits instead of two. The instructions then pointed out that there were two math tasks, one easy and one hard, and that the four purple players would do the hard math task. They would still be compensated with 1 point (30 cents) for every correct answer and the had the same amount of time available. When it was explained how the selection into the high-stake group would work, it was stated that all the four purple players were put in the high-stake group as a compensation for the fact that they did a harder math task.

Treatments 1, 1b and 1c are identical in the sense that in all three cases, the four orange players, out of a total of 12, with the best performance in the math task were admitted into the high-stake group whereas the four participants who were randomly selected to be purple were automatically admitted into the high-stake group. The difference between the three treatments is that whereas there was no rationale for the preferential treatment of purples in treatment 1, treatments 1b and 1c provided, respectively, an efficiency- and a fairness justification. These two versions of treatment 1 allow us to better understand whether the effect of the unrepresentative quota on

purples on cooperation in the public good game is different when a rationalization for the quota is given and, in turn, if it matters *how* the quota is rationalized.

1.2.2 Random Selection: Treatments 3 and 4

The unrepresentative quota, as it is designed in this experiment, captures two different aspects of how quotas are often perceived. First, that the selection process gives preferences to one group over another and second, that performance plays less, or no, role when candidates from the preferentially treated group are selected.

In order to understand the role played in generating the quota effect by these two factors, I designed treatments 3 and 4, which were identical to treatments 1 and 2 respectively, with the only difference being that the selection into the high-stake group was done randomly instead of by performance. Treatment 3 was the unrepresentative quota treatment with random selection, where the four purple players were automatically chosen for the high stake group whereas four of the twelve orange players were randomly selected. Treatment 4 was the control treatment with random selection, where four purple and four orange players were chosen out of the totally eight orange and eight purple participants.

By comparing the behavior in the public good game in the high-stake group between treatments 3 and 4, we can learn whether any differences in cooperation that are found between treatments 1 and 2 also appear in a similar environment where selection is not done on the basis of performance.

Since treatments 3 and 4 use random selection by design and no reference is made to performance in the math task, we can also use these treatments to investigate whether there is an inherent relationship between how well a person performs on the math task and how she behaves in the public good game. This is important in order to rule out the possibility that differences in behavior between treatments 1 (and 1b and 1c) and 2 are associated with math ability and its effect on public good game behavior.

1.2.3 Implementation

The experiment was conducted at the Harvard Decision Science Laboratory and a total of 22 sessions were run. There were five sessions each of treatments 1 and 2, three sessions each of treatments 1b and 1c, and three sessions each of treatments 3 and 4.[7] The sessions were conducted over the course of four days in late October and early November 2011 and five days in April 2012.[8] All sessions had 16 participants who were recruited through the SONA system at the Harvard Decision Science Laboratory. Out of the total of 352 participants, who were only allowed to participate once, 55.4 percent were women. The median age was 23 years and the participants earned on average 23 USD (including a USD 10 show-up fee) for participating in an experimental session that lasted about 45 minutes.

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[7] Two additional sessions which were started had to be interrupted and canceled due to computer and network malfunctioning. Participants in these two sessions were paid for their partial participation and were not allowed to participate again.

[8] In October/November 2011, treatments 1 and 2 were conducted. In April, treatments 1 and 2 were replicated and the other treatments were added. Apart from this, treatments were randomly allocated between sessions. The data on treatments 1 and 2 show no seasonal differences depending on whether data collection was done in October/November or in April. Pooled data from these two sets of sessions is therefore used in the analysis. Further details are given in Appendix A.4.

The participants arrived at the lab a few minutes before the scheduled start and signed consent forms. When 16 people had arrived they were taken to the lab by the experimenter.[9] They were all seated in one room, in 16 separate cubicles. The cubicles prevented them from seeing what any other participant was doing and which color he or she had been allocated. The experiment was programmed in z-tree (Fischbacher, 2007) and instructions were given both verbally, to ensure common knowledge, and on the participants' computer screens. Key pieces of information were also given on paper so that the participants could review these parts of the instructions at any time. Participants made all their decisions on the laboratory computers.

Instructions were given before each part of the experiment. Before the math task in part 2 and before the public good game in part 4, participants answered quizzes in order to ensure that everyone had understood the instructions correctly. The participants had to answer all questions correctly for the computer program to continue to the next screen. Those who experienced difficulties in answering any of the questions could request help by pressing a help button and they then received additional explanation in private by the experimenter. Thereafter they answered the questions in the quiz again.[10]

1.2.4 Conceptual Framework and Hypotheses Generation

I use the inequity-aversion model of Fehr and Schmidt (1999) to model the two-person public good game.

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[9] Over-recruitment was planned in order to reduce the probability that sessions had to be cancelled due to too few participants showing up. The 16 individuals who showed up first were allowed to participate in the session. Any additional participants above the first 16 received only the show-up fee of USD 10 and could not participate.

[10] Note was taken of the (very few) participants who experienced substantial difficulties in understanding the instructions. Excluding these observations does not make a difference to the results of the analysis.

Consider two individuals, i and j, who are matched in a public good game. Each individual has an endowment y_i and decides how much of the endowment to keep and how much to contribute to a joint project (contributions by individual i are denoted c_i). The total contributions made by both individuals are multiplied by R, which is the return of the project, and the resulting amount is shared equally between i and j. The monetary payoff for individual i is hence:

$$\pi_i(c_i, c_j) = y_i - c_i + \frac{R}{2}(c_i + c_j).$$

If $1 \leq R \leq 2$ this is a social dilemma, i.e. setting $c_i = 0$ maximizes profit for individual i but setting $c_i = y_i$ maximizes total social payoff for the two participants. In this experimental design $R = 1.5$.

Contributions in one-shot public good games are usually well above zero (see survey of public goods studies in Ledyard, 1995). This is a puzzle if players only care about their own monetary payoffs, but not if at least some players care also about the equality of payoffs. Following Fehr and Schmidt I assume that the utility of individual i is

$$U_i = \pi_i - \alpha_i \max(\pi_i - \pi_j, 0) - \beta_i \max(\pi_j - \pi_i, 0).$$

This utility function captures two things; first that a deviation from equal payoffs is aversive and second that this disutility may differ depending on whether the inequality comes from individual i having a lower or a higher payoff than individual j (Fehr and Schmidt assume that $\alpha_i \leq \beta_i$, i.e. that people suffer more from inequality that is to their disadvantage).

An individual with this utility function that is deciding how much to contribute in the public good game will need to form beliefs about how much the other person will contribute. Now there

is no longer a unique equilibrium with $c_i = 0$, but all contributions levels such that $0 \le c_i \le y_i$ can be equilibria when supported by appropriate beliefs about c_j. The extent to which participants contribute to the joint project in a public good game is viewed as a measure of cooperation, since all contributions such that $c_i > 0$ are signaling other-regarding preferences (either in the form of a higher α_i or β_i or as more positive beliefs about the contributions of the other player's contribution, c_j).

This experiment tests whether there is less cooperation in a group that is put together by an unrepresentative quota (treatment 1) than in the control treatment where all participants are treated symmetrically and selected by performance (treatment 2). That implies that I am testing the null hypothesis of no difference in public good contribution against the alternative hypothesis that there is a difference in public good contribution between these two treatments. To understand the role of justification for the quota effect, I look at contributions to the public good in the two treatments where a justification is given, treatments 1b (efficiency justification) and 1c (fairness justification), and test the null hypothesis that they are the same as in treatment 1, where no justification was given.

Section 1.4 discusses different theories that could support a difference in cooperative behavior between the two treatments. As explained there, an important step to distinguish between the different mechanisms is to look at behavior broken down by the color-match of the two players, i.e. by whether the participant herself, and the person to whom she is matched, belong to the group which is disadvantaged or advantaged by the unrepresentative quota system. I hence test

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the null hypothesis that the difference between public good contribution in treatments 1 and 2 is the same for all color-matches against the alternative hypothesis that they are not all the same.

As discussed above, the unrepresentative quota in treatment 1 (and 1b and 1c) captures two aspects of how quotas are often perceived: preferential treatment and an asymmetric role for performance in the selection of candidates from the different groups. In order to understand the role of these features in generating the quota effect I consider contributions to the public good in treatments 3 and 4. In both of these treatments, selection is made randomly into the high-stake group. However, whereas there is an unrepresentative quota for purples in treatment 3, participants of both colors have the same chance of being selected in treatment 4. I test the null hypothesis of no difference in average contribution to the public good in these treatments.

1.3 Results

The experimental design, where each participant is making decisions in seven public good games, implies that there are multiple behavioral data points for each participant. It is hence important to adjust standard errors for the fact that observations for a single individual are not independent. In the analysis below, this is done by clustering standard errors on individual.[11] As an additional measure, in order to be as conservative as possible in testing the hypotheses, the tests utilize standard errors that are also clustered at the level of the experimental session in which the data were generated.[12]

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[11] Another way of handling the data is to calculate the average for each participant, and hence only use one data point per participant. The results reported here are not sensitive to the choice of either method.

[12] See Fréchette (2012) for a discussion about potential session effects in laboratory experiments. The two-dimensional clustering (i.e. clustering on both individual and experimental session) that is used here is an extension of the standard cluster-robust variance estimator for one-dimensional clustering, see Cameron et al. (2011). The results

1.3.1 Selection by performance: Treatments 1 and 2

We begin by looking at the results for treatments 1 and 2. Treatment 1 is the unrepresentative quota treatment, in which the four purple players were automatically selected for the high-stake group whereas the orange players were selected based on their performance in the math task. In the high-stake group in this treatment, average contribution in the public good game was 32.7 percent of the endowment. Treatment 2 is the control treatment in which four players of each color were selected based on performance in the math task. Under this condition, contributions in the public good game were on average 54.7 percent. These data are outlined in Figure 1.1.

Figure 1.1: Contribution in PG game, High-stake group, Treatments 1 and 2.
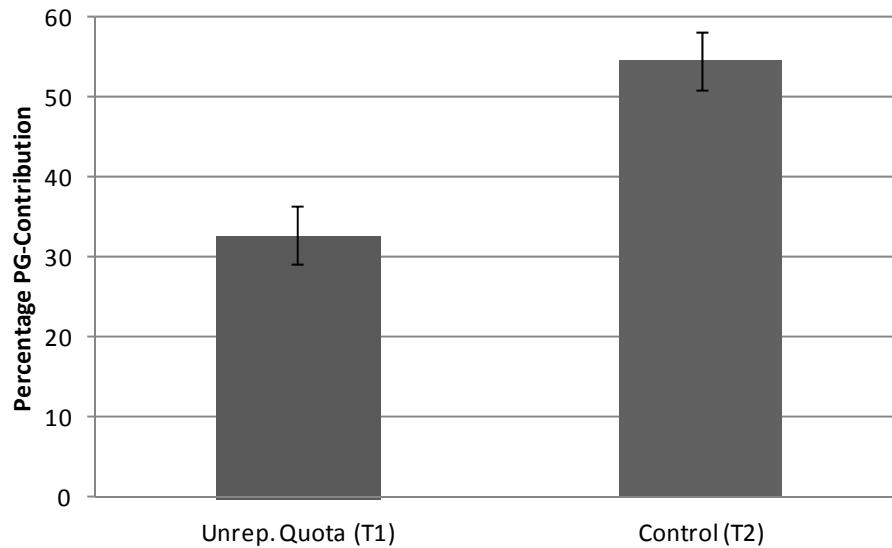
Error bars mark 95% confidence interval for a t-test with se clustered on participant and session. Number of observations: 560, number of participants: 80, number of sessions: 10.

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reported here are not sensitive to decisions regarding clustering of standard errors, see Appendix A.5. This appendix also concerns potential sensitivity to distributional assumptions and how the results reported here are robust to the use of non-parametric tests, such as the Mann-Whitney test.

The difference in public good contribution between the two treatments is 22.0 percentages points, which in turn is highly statistically significant ($p < 0.01$[13]). We therefore have the following result:

Result 1: There is significantly less cooperation in the public good game when the high-stake group is put together by the unrepresentative quota than in the control treatment.

We continue by breaking down the results by whether the player is orange (and hence disadvantaged by the unrepresentative quota in treatment 1) or purple (and hence advantaged by the quota in treatment 1). Figure 1.2 shows these data.

Figure 1.2: Contribution in PG game, High-stake group, Treatments 1 and 2, by Color.
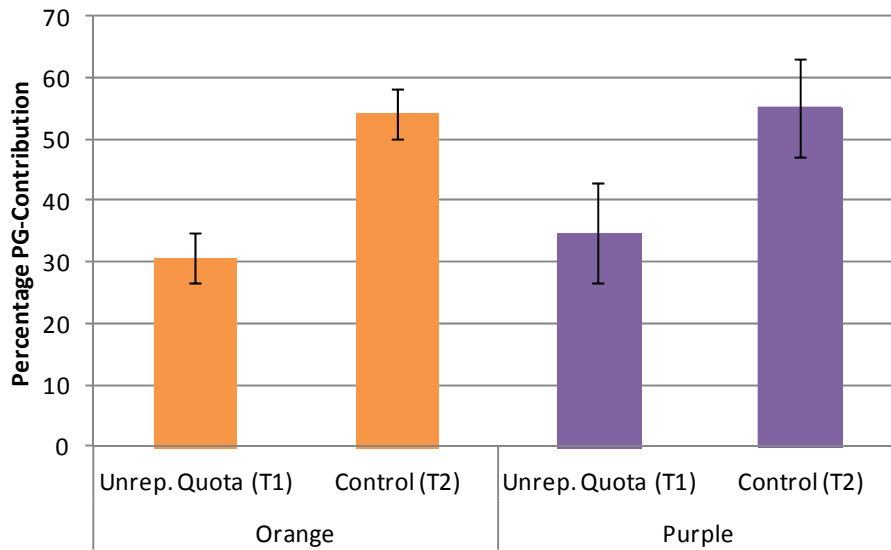
Error bars mark 95% confidence interval for a t-test with se clustered on participant and session. Number of observations: 560, number of participants: 80, number of sessions: 10.

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[13] This p-value and all other p-values reported below, unless otherwise noted, come from two-sided t-tests with standard errors clustered as described above. See Appendix A.6 for further details.

We see that for the oranges, who were disadvantaged by the quota, we have an average contribution of 30.7 percent in treatment 1, compared to 54.2 percent in treatment 2, i.e. a quota effect of 23.5 percentage points ($p < 0.01$). For the purples, the contribution levels are 34.8 percent and 55.1 percent in treatment 1 and 2 respectively, and the quota effect is hence 20.3 percentage points ($p < 0.05$). A difference-in-difference analysis reveals that the 3.2 percentage point difference in the quota effect between oranges and purples is not statistically significant. We therefore have the following result:

Result 2: In the unrepresentative quota treatment, both those disadvantaged and those advantaged by the quota cooperate less compared to the control treatment.

We continue the analysis of the results from treatments 1 and 2 by looking at whether there is a difference between how players act when they play the public good game with others who have the same color compared to those who have the other color. This can be seen in Figure 1.3.

Figure 1.3: Contribution in PG game, High-stake group, Treatments 1 and 2, by Color of

Matched Player.

Error bars mark 95% confidence interval for a t-test with se clustered on participant and session. Number of obser-vations: 560, number of participants: 80, number of sessions: 10.
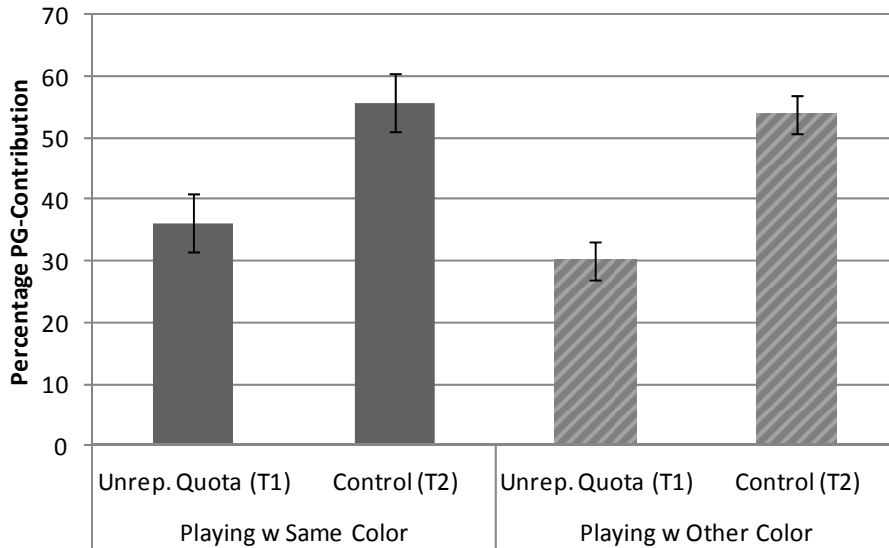
From the data shown in Figure 1.3, two conclusions can be drawn. We first note that there is less cooperation in the public good game in the unrepresentative quota treatment than in the control treatment regardless of whether participants play with someone who has the same color as themselves or with someone of the other color. The quota effect is 19.5 percentage points when participants play with others who have the same color ($p < 0.01$) and 23.8 percentage points when the game is against someone of the other color ($p < 0.01$). The difference in difference is not statistically significant. A second conclusion is that the participants display an ingroup favoritism similar to what has been found in previous research even if it is not what is driving the quota effect.[14] Considering both treatments, we find that participants contribute significantly less (on

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[14] Ingroup favoritism refers to the fact that people easily divide themselves and others into social categories and treat members of the own group (the ingroup) more favorable than people in other groups (the outgroup(s)). The condi-

average 3.9 percentage points, $p < 0.05$) when the person they are playing a game with does not have their color. However, even though significant, this effect is small compared to the quota effect of 22.0 percent (a t-test where the null hypothesis is that the size of the two effects are the same size is rejected with $p < 0.01$). Our third and fourth results are hence:[15]

Result 3: The quota effect is present and does not statistically differ in size depending on whether the matched players have the same color or not.

Result 4: Considering both the unrepresentative quota- and the control treatment, there is ingroup favoritism in the sense that participants contribute more when playing with people of the same color.

As discussed above, a key step in understanding which mechanism drives the quota effect is to examine in greater depth whether there are differences in how the people of the two different colors play with each other. This is graphed in Figure 1.4.

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tions for this effect to arise are weak, and the effect is hence often refered to as the "minimal group paradigm" (Tajfel, 1970). See also Tajfel and Billig, 1971. Ingroup favoritism is discussed further in Section 4.1 below.

[15] It can be hypothesized that the ingroup favoritism in this setting could be suppressed by the fact that participants saw all seven games simultaneously on the screen and therefore felt that they ought to treat participants of different colors similarly. One way to, at least indirectly, investigate this is to check for order effects, i.e. if participants whose first game involved someone of their own color behaved differently against all players (e.g. anchored at a higher cooperation level) than those whose first game was against a player of the other color. The analysis in Table A.4 in Appendix A.5 shows that no such order effects can be found in the data.

Figure 1.4: Contribution in PG game, High-stake group, Treatments 1 and 2, by Color of Matched Player.
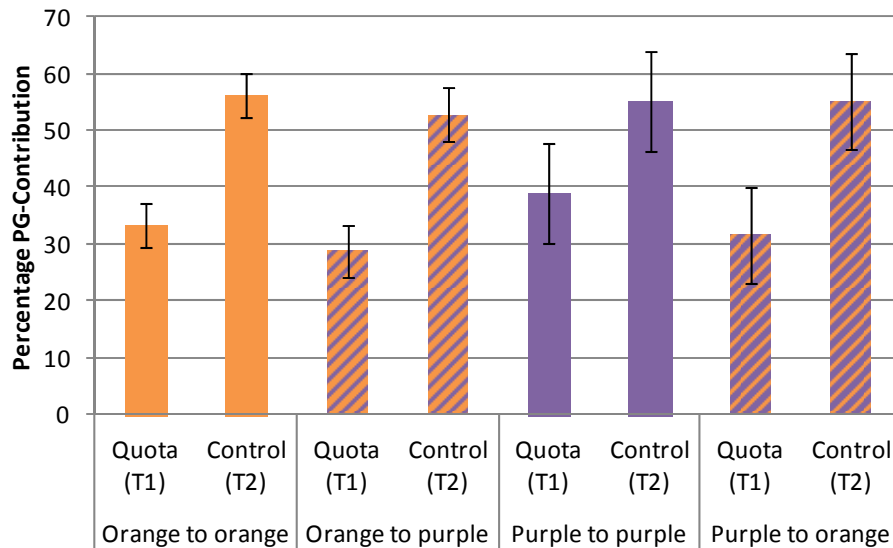
Error bars mark 95% confidence interval for a t-test with se clustered on participant and session. Number of observations: 560, number of participants: 80, number of sessions: 10.

Figure 1.4 shows that there is less cooperation in the unrepresentative quota treatment than in the control treatment, regardless of the match between the player's own color and the color of the person she plays with. The point estimate of the quota effect is largest when orange players play with purple players (24.1 percentage points) and smallest when purple players play with other purples (16.3 percentage points). However, the quota effect is statistically significant for all combinations of colors (orange to orange: $p < 0.01$, orange to purple: $p < 0.01$, purple to purple: $p < 0.1$, purple to orange: $p < 0.01$) and the differences in point estimates of the quota effect are not statistically different from one another. Our fifth result is therefore:

Result 5: The quota effect is present for all combinations of matched players' colors and not statistically different in size among the different cases.

As described in Section 1.2, I also asked the participants about whether they perceived the procedure through which the high-stake group was selected as fair. We will now look at the data from these questions.[16] Figure 1.5 outlines these data for treatments 1 and 2.

Figure 1.5: Fairness Perception, Treatments 1 and 2

Data are from high-stake- and regular-stake group and show percentage of participants who regarded the selection process into the High-stake Group fair. Error bars mark 95% confidence interval for a t-test. Number of observations = number of participants: 160, number of sessions: 10.

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[16] For the fairness data we only have one observation per participant (which is that participant's assessment of the fairness of the procedure) and hence the above discussion about clustering on the level of the individual does not apply here. Reported p-values from test on fairness data come from two-sided t-tests.

Figure 1.5 shows that 60.7 percent of participants found the process fair in the unrepresentative quota treatment, compared to 80.3 percent of participants in the control treatment. The difference of 19.6 is statistically significant ($p < 0.05$). We can therefore conclude that the unrepresentative quota was viewed as a more unfair process by the participants than the process in which everyone was selected based on performance.

1.3.1.1. Justification of the Unrepresentative Quota: Treatments 1b and 1c

As discussed in Section 1.2 some previous research suggests that the acceptance for affirmative action policies is higher when the policy is justified. I test whether justifications of the use of an unrepresentative quota have an effect on group cooperation with treatments 1b and 1c.

In treatment 1b, an efficiency argument was given as a rationalization for the use of the quota. The participants were told that the payoff rule for part 4 was such that the high-stake group would only have higher stakes than the regular-stake group if the high-stake group consisted of an equal number of orange and purple players. When the unrepresentative quota was introduced, it was done with reference to this payoff rule, pointing out that the unrepresentative quota guarantees equal representation of both colors and hence higher endowments for all members of the high-stake group. The result of this treatment in relation to treatments 1 and 2 is shown in Figure 1.6.

Figure 1.6: Contribution in PG game, High-stake group, Treatments 1, 1b and 2.
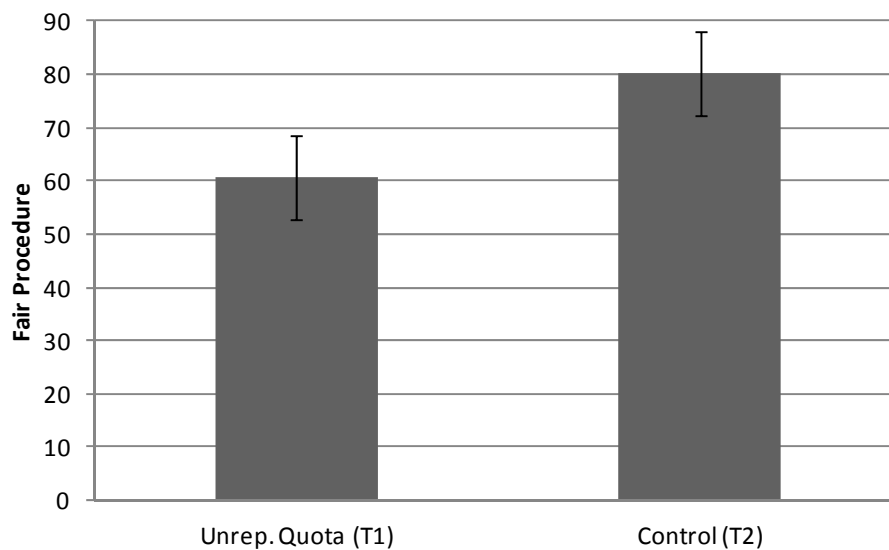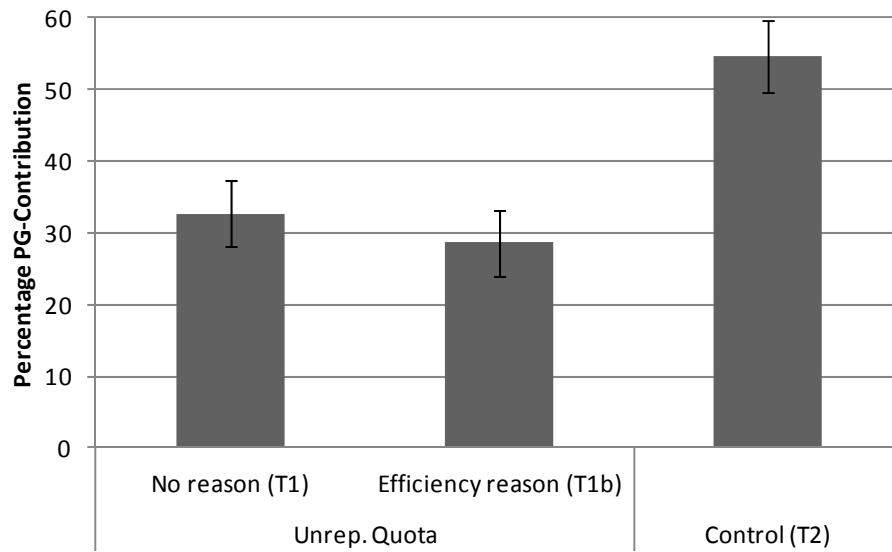

Error bars mark 95% confidence interval for a t-test with se clustered on participant and session. Number of observations: 728, number of participants: 104, number of sessions: 13.

Figure 1.6 reveals that the contribution level in treatment 1b was 28.6 percent. This is not statistically different from the case with no justification and the difference of 26.0 percentage points between treatments 1b and the control treatment, treatment 2, is highly statistically significant ($p < 0.01$).

In treatment 1c, the unrepresentative quota was rationalized as a compensation for previous unfair treatment. The four purple participants had to do a harder math task (summing up three-digit numbers instead of two-digit numbers) without getting compensated in terms of higher payoffs per correct answer. The fact that they were all automatically selected for the high-stake group was framed as making up for this unfairness. Figure 1.7 below outlines the extent to which participants contributed in the public good game in treatments 1 and 1c, compared to treatment 2.

Figure 1.7: Contribution in PG game, High-stake group, Treatments 1, 1c and 2.
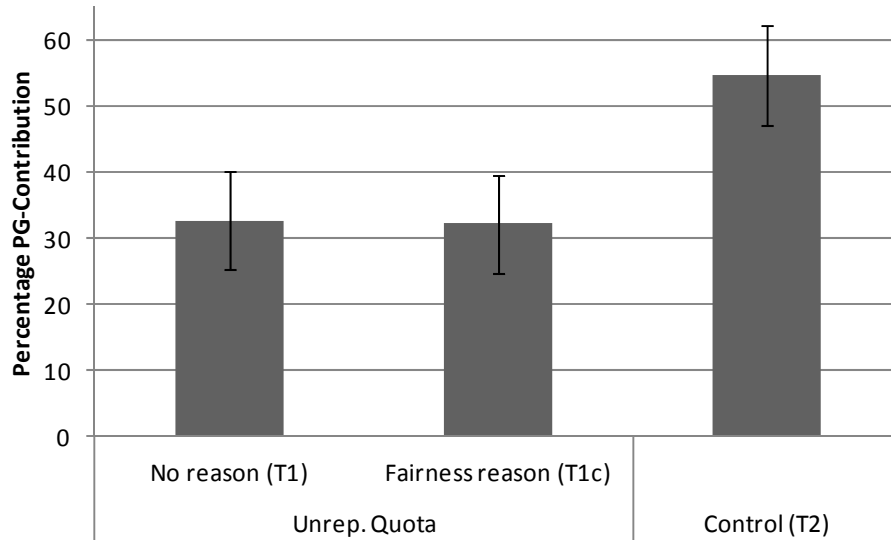
Error bars mark 95% confidence interval for a t-test with se clustered on participant and session. Number of observations: 728, number of participants: 104, number of sessions: 13.

As is evident from Figure 1.7, the fact that the quota was given with this justification did not have a positive impact on the cooperation level, compared to the situation in which the quota was left unjustified, as there is no statistical difference in cooperative behavior between treatments 1 and 1c. The difference between cooperation in treatment 1c and treatment 2 of 22.5 percentage points is however statistically significant ($p < 0.01$). The analysis of treatments 1b and 1c, in addition to the analysis of treatments 1 and 2, hence gives us the following result:

Result 6: There is less cooperation when the group is put together by an unrepresentative quota, compared to the situation where everyone is selected by performance. This difference remains even when the unrepresentative quota is justified with either an efficiency- or a fairness argument.

It is important to note that these results do not imply that there are no possible justifications that could increase cooperation in the unrepresentative quota treatment to the level that we see in the control treatment. However, they do suggest that the negative effect of the unrepresentative quota on cooperation is quite robust.[17]

In Figure 1.8 we again look at the participants' perception of whether or not the selection process was perceived as fair and consider the two treatments in which a justification for the use of the unrepresentative quota was provided.

Figure 1.8: The Impact of Quota Justification on Fairness Perception
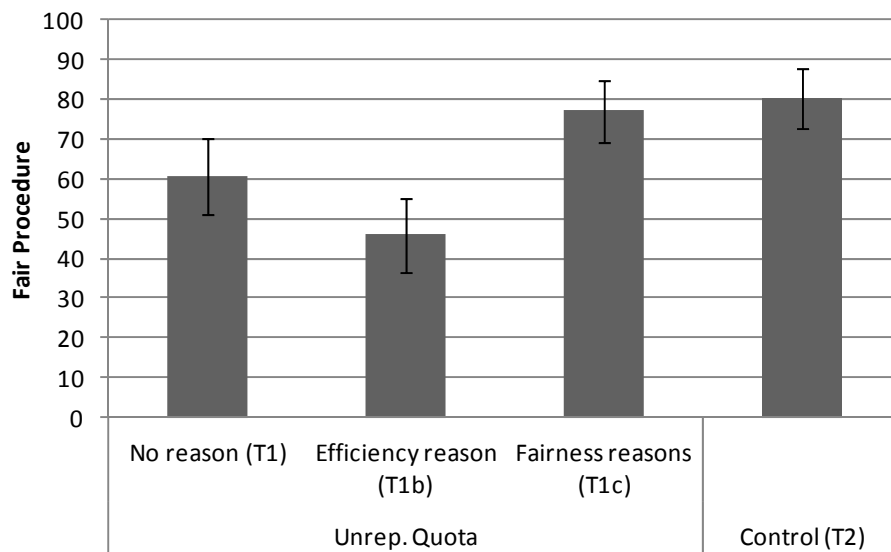
Data are from high-stake- and regular-stake group and show percentage of participants who regarded the selection process into the High-stake Group fair. Error bars mark 95% confidence interval for a t-test between adjacent categories. Number of observations = number of participants: 256, number of sessions: 16.

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[17] The reason that Section 3.1.1 does not describe the data from treatments 1b and 1c broken down by color of the player and/or the matched partner is that such an analysis does not add any new insights compared to what was discussed in the first part of section 3.1. For the interested reader the material is available from the author.

In Figure 1.8 we see that the two justifications had different effects on participants. The efficiency justification given for the use of a quota in treatment 1b did not change participants' perception of equity in the process as there was no statistically significant difference between the fairness perception in treatment 1 and 1b. However, appealing to the fairness justification for the use of an unrepresentative quota in treatment 1c, the selection was perceived to be more fair ($p < 0.1$) than in treatment 1, where no reason for the quota was given.

These data add an interesting dimension to the results described above. Even though the argument used in treatment 1c worked in the sense that participants did find the process more fair, it did not have an impact on participants' behavior in the public good game, compared to the situation in which no justification was given for the unrepresentative quota.

1.3.2 Random Selection: Treatments 3 and 4

In order to distinguish between the roles played by the two aspects of quotas (preferential selection and an asymmetric role of performance) for generating the quota effect, I also implemented two treatments that were identical to treatments 1 and 2 with the difference that selection by performance in the math task was removed. Instead participants were randomly selected into the high-stake group. In treatment 3, which is the unrepresentative quota treatment with random selection, the four purple participants were automatically selected into the high-stake group. The four orange participants, however, were randomly selected from a total of twelve orange participants. In treatment 4, which is the control treatment with random selection, there were eight players of each color in each session and four of each color were randomly selected for the high-stake group. Figure 1.9 outlines the data from treatments 3 and 4.

Figure 1.9: Contribution in PG game, High-stake group, Treatments 3 and 4.
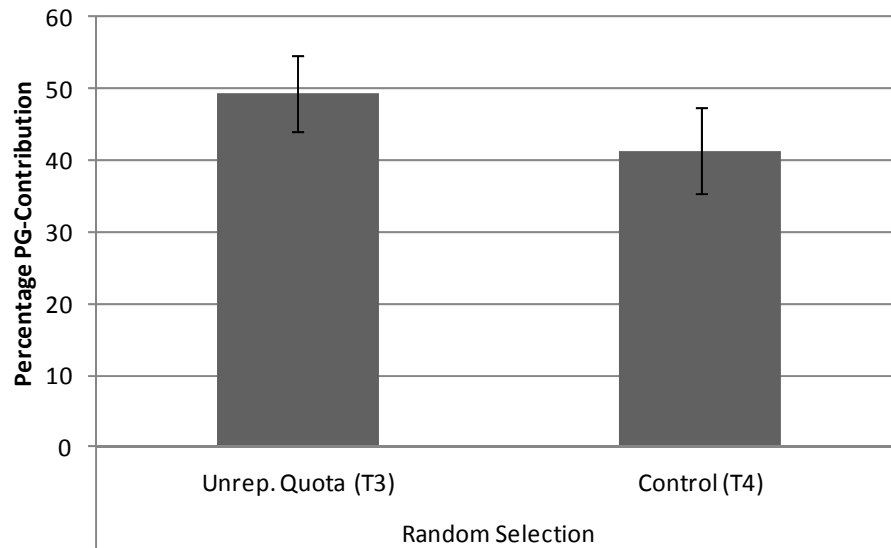
Error bars mark 95% confidence interval for a t-test with se clustered on participant and session. Number of observations: 336, number of participants: 48, number of sessions: 6.

From Figure 1.9 we learn that in treatment 3, contribution to the public good was 49.3 percent of the maximum whereas it was 41.3 percent in treatment 4. This difference is not statistically significant. Furthermore, a difference-in-difference analysis reveals that the quota effect is not the same when selection is made randomly as when it is made by performance ($p < 0.01$). This gives us the following result:

Result 7: When selection by performance is removed and participants are instead selected randomly into the high-stake group, there is no negative effect of the unrepresentative quota on cooperation in the public good game.

This result highlights the importance of the selection mechanism for the quota effect to arise. When those who were disadvantaged by the quota were selected by performance in the math task there was a negative effect on cooperation from the unrepresentative quota, but this effect went away when the selection was instead made randomly. From this we learn that a preferential selection of one color above the other is not enough to trigger lower cooperation but that the difference in selection criteria, and the role of performance, is decisive.

In Figure 1.10 we again ask whether participants find the process fair or unfair and consider these data for treatments 3 and 4.

Figure 1.10: Fairness Perception, Treatments 3 and 4
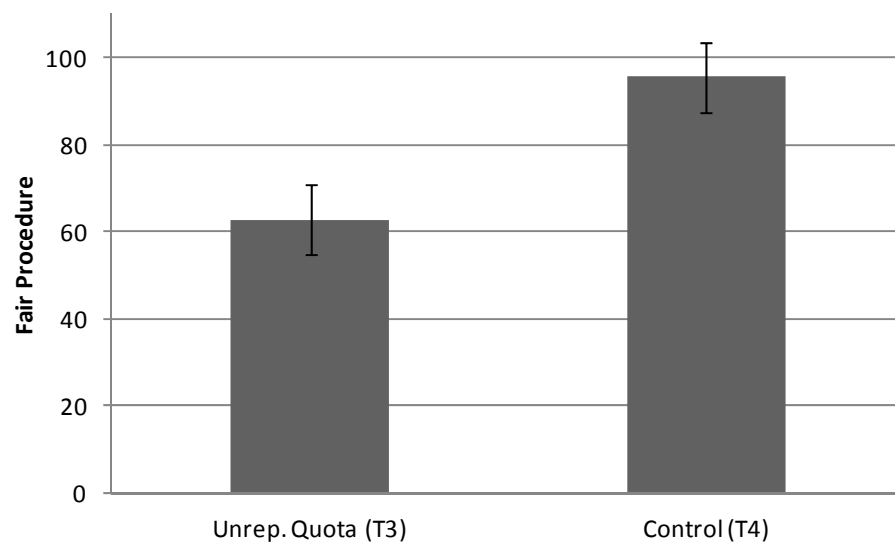
Data are from high-stake- and regular-stake group and show percentage of participants who regarded the selection process into the High-stake Group fair. Error bars mark 95% confidence interval for a t-test. Number of observations = number of participants: 96, number of sessions: 6.

Figure 1.10 reveals that whereas 62.8 percent found the selection process fair in treatment 3, the corresponding figure for treatment 4 was 95.6 percent. This difference of 32.8 percentage points is highly statistically significant ($p < 0.01$). This is interesting as we also just noted that there was no quota effect on cooperative behavior with random selection. This further supports our conclusion that the effect that the unrepresentative quota has on cooperation is not primarily about stated fairness perceptions. Even though participants found the unrepresentative quota in treatment 3 as unfair as in treatment 1, there was no quota effect when selection was made randomly instead of based on performance. Also, even though the participants report that they view the unrepresentative quota as more fair when the justification in treatment 1c is given, this does not change the level of cooperation compared to when no justification is given.

1.3.3 Math Task and Public Good Game – is there an Intrinsic Relation?

Since selection in treatments 3 and 4 is made randomly and no references to performance in the math task is made, data from these treatments make it possible to also investigate whether there is an intrinsic relationship between how a person performs in the math task and how she behaves in the public good game. Figure 1.11 shows a scatter plot of the number of correct answers in the math task against the percentage contribution in the public good game.

Figure 1.11: Math Task Performance and Public Good Contribution, Treatments 3 and 4.
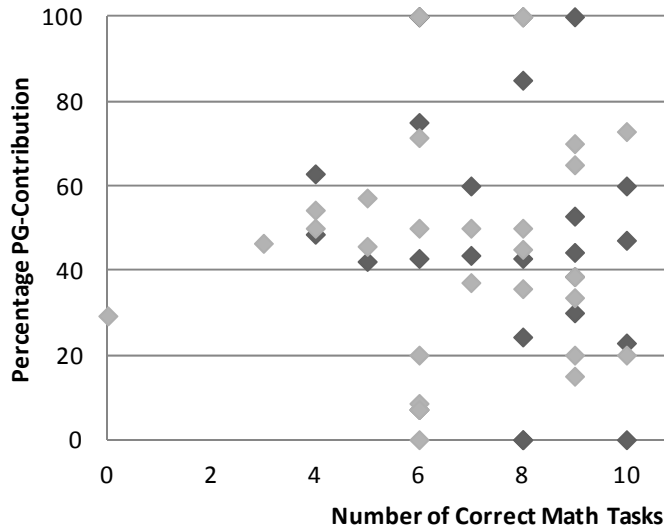
Data are from high-stake- and regular-stake group. N=96. Dark gray dots denote treatment 3 and light gray dots denote treatment 4.

Figure 1.11 suggests that there is no relation between the two. This is also confirmed in regression analysis (OLS) where the percentage contribution is regressed on the number of correct answers in the math task as the coefficient on number of correct answers in the math task is insignificant.[18]

This result tells us that the reason that we see less cooperation in treatment 1 than in treatment 2 is not a consequence of an inherent relationship between scores in the math task and behavior in the public good game.

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[18] This is true also when controls are added for whether the participant was in treatment 3 or 4, the color which the participant was randomly given and whether or not she was randomly selected into the high-stake group. See Table 1.1 for regression results with controls.

Table 1.1: Math Task Performance and Public Good Contribution, Treatments 3 and 4

	(1)	(2)
Correct	-0.007	-0.009
	(0.09)	(0.10)
High Stake Group		-0.035
		(0.07)
Orange		-0.05
		(0.07)
Quota		0.05
		(0.07)
Constant	0.528***	0.986*
	(0.09)	(0.55)
N	96	96

OLS. Levels of significance: * p < 0.1, ** p < 0.05, *** p < 0.01. Dependent variable: Percentage public good contribution. High-stake group is a dummy equal to 1 if the participant was randomly chosen as a member of the high-stake group. Orange is a dummy equal to 1 if participant is orange. Quota is a dummy equal to 1 for treatment 3.

1.4 Why a Quota Effect?

I have defined the quota effect as the negative impact that an unrepresentative quota has on co-operative behavior in a social dilemma, compared to a situation in which everyone in the group is treated symmetrically and selected by performance. In this section I discuss theories and previous research that provide explanations as to why we might see this effect. I start by looking at theories predicting a relation-specific quota effect and thereafter I consider research pointing in the direction of a general quota effect. Throughout, the theories are related to the results from the experiment.

1.4.1 A Relation-Specific Quota Effect

Several theories which can explain the existence of a quota effect also make the prediction that this should only appear in certain relations within the group, for example when those who were not advantaged by the quota interact with those who were advantaged. Here three such theories are considered.

First, there is ingroup favoritism (Tajfel, 1970; Tajfel et al., 1971), which captures people's tendency to put themselves and others into categories, and the fact that this categorization gives rise to favorable treatment of the people in the same social group as oneself compared to those in other groups (see also MacDermott, 2009; and Chen and Li, 2009). Ingroup favoritism has been found also with random groups, for example put together by a coin flip (Locksley et al., 1980). In a setting with quotas and affirmative action, the ingroup bias may be at play, since a sufficiently strong decrease in cooperation with the outgroup as a consequence of the quota could lead to a decline in overall cooperation (see also Goette et al., 2006 and Bernard et al., 2006).

However, the experimental findings in this chapter are *not* predicted by ingroup favoritism. Specifically, if it were a stronger feeling of ingroup- versus outgroup-identity that was the primary difference between the control and the quota treatments, we would expect to see the quota effect more strongly when participants play with those of the other color. As we do not observe this, it can be concluded that even though there is ingroup favoritism in the sense that participants in both treatments cooperate slightly more with those of the same color, this is not what is driving the quota effect.

A second set of theories is related to entitlement and punishment. Entitlement is a key concept in the literature on procedural fairness, and it has been shown that an experimental participant who earns an endowment in an experiment is less inclined to share those resources with others than if the endowment was randomly obtained (Hoffman et al., 1996; Cherry, 2001; Oxoby and Spraggon, 2008). Similarly, we also know that people are willing to punish those who behave in a non-approved way or who get undeserved benefits (see e.g. Fehr and Gächter, 2000; and Fehr and Fischbacher, 2004). In this case, it is not implausible that those who were not advantaged by the quota could feel an entitlement to more resources or a willingness to punish those who were advantaged by the preferential treatment. This could then give rise to the quota effect, i.e. less cooperation in the unrepresentative quota treatment compared to the control treatment.

The results that we observe in the experiment are, however, inconsistent with these mechanisms. If punishment were the main underlying factor behind the quota effect, we should not see an effect of the quota when those who were not advantaged by the quota, i.e. the orange participants, play with others of the same color. A feeling of entitlement induced by the quota treatment should on the other hand not make a difference in the case when the advantaged, i.e. purple, participants play with each other. As we observe a quota effect of similar size regardless of the participant's own color and that of the matched player, it can be concluded that the effect is not driven primarily by punishment or entitlement.

Third, it may be that (perceived) competence matters for the quota effect. Previous research has shown that affirmative action has a negative impact on how the competence of the selected person is viewed both by himself and by others (see e.g. Garcia et al., 1981; Heilman et al., 1987,

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1992). There are two ways that the performance in our experiment could matter for the quota effect. On the one hand, there may be an inherent relation between how many math problems an individual can solve and how she behaves in a social dilemma. Second, there is the question of how participants view one another's competence and what beliefs they hold about whether there is a relationship between math task solving ability and cooperative behavior.[19]

Returning to the results of the experiment, Section 3.3 above concluded that the quota effect is not driven by an inherent relationship between performance in the math task and behavior in the public good game. It can also be concluded that beliefs about competence are not the main driver of the quota effect, as this should not give rise to a symmetric quota effect, regardless of the color-match of the players, which is what we observe.

1.4.2 A General Quota Effect

There are also theories and previous research indicating that the quota effect should be general, i.e. affecting all relations within the group. We'll look at two strands of such research.

First, some previous work on affirmative action has focused on how preferential selection impacts people's general sense of interest and motivation and it has been found that when some employees or team members are selected using affirmative action, motivation, interest and social interactions can be negatively affected. Most importantly, this is true for both those who are advantaged and for those who are disadvantaged by the preferential selection (Chacko, 1982; Heilman et al., 1987, 1996. See also McFarlin and Sweeney, 1991).

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[19] See Rustichini et al., 2011, Benjamin et al., forthcoming and Mollerstrom and Seim, 2012 for examples of papers discussing the link between cognitive ability and cooperative (and other-regarding) behavior.

Second, in the procedural justice literature, a just process is regarded as a source of social motivation, i.e. it is believed to increase cooperative behavior (Tyler and Lind, 1992; Tyler and De Cremer, 2006). This has been found in several contexts including law compliance (Tyler, 1990) and organizational citizenship behavior (Tepper and Taylor, 2003). Moreover, this effect has been shown both in the case where the outcome following the procedure was one that was favorable to the agents, and in the case where the outcome was unfavorable (see Tyler, 2003; Tyler and Huo, 2002; Zapata-Phelan, 2009; Holmvall and Bobocel, 2008).

The finding that there is a quota effect for both those who were disadvantaged and those who were advantaged by the quota, and that the effect is of similar size regardless of which of these two groups the matched player belongs to, is in line with the above findings of a general impact of procedures on behavior.[20]

1.5 Conclusions

Affirmative action and quotas are widely used policies that are often successful in achieving desired numerical representations. However, there are also many anecdotal examples of unwanted negative side effects in the form of uncooperative and inefficient behavior in the groups that are formed with quotas. I conduct a laboratory experiment in order to investigate whether it is indeed

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[20] Neither the procedural justice research nor the psychology literature about affirmative action has much to say regarding the exact mechanism that underlies the decrease in motivation, interest and cooperativeness that follows in the wake of unjust procedures. These results could, however, be related to the growing literature on how moods and emotions impact economic decision-making. It has for example been shown that people who feel anger are less other-regarding and cooperative, see e.g. van Kleef et al., 2004; Lerner and Tiedens, 2006; and Small and Lerner, 2008. One way to further deepen our understanding about why the quota effect arises in our experiment would be to utilize measures of emotions and moods. By using Likert-type scales, physiological measures or other tools, it would be possible to understand if particular emotions, such as anger or annoyance, lie behind the quota effect. See Coan et al. (2007) for an overview of the literature on emotion elicitation and assessment.

the case that groups put together by an unrepresentative quota cooperate less than groups where everyone is treated symmetrically and selected by performance.

I create groups by randomly allocating colors to participants. In the unrepresentative quota treatment, I create a selected group by automatically admitting all participants of one color whereas participants of the other color are chosen in competition with each other, based on their performance in a previous task. Cooperation is measured as the level of contribution in a two-person public good game and I compare the level of cooperation in the unrepresentative quota treatment to a control treatment where participants of the two colors are treated symmetrically and everyone is chosen by performance.

The results show significantly less cooperation in the unrepresentative quota treatment and I furthermore find that this effect arises both for the players who were advantaged and those who were not advantaged by the quota. The unrepresentative quota also has the same negative effect on cooperation regardless of the color of the other player. These results contradict the predictions of some prominent theories, for example that the disadvantaged will punish the advantaged.

Furthermore I find that the level of cooperation remains low in the group that is put together by the unrepresentative quota even when a justification is given for the preferential selection. However, the negative effect of the quota on cooperation goes away when the selection criteria for the selected group is changed from performance-based to random, i.e. by removing selection by performance entirely, the quota effect can be turned off. This implies that the quota effect is tied

to the fact that the participants who are admitted into the selected group under the unrepresentative quota are chosen by different criteria.

There are several potential policy implications of these findings. For example, the fact that the unrepresentative quota affects all relationships in the group – not only those between the disadvantaged and the advantaged – can potentially make negative effects of affirmative action harder to detect. In organizations where affirmative action or quotas are used one often looks for differences in how people treat one another as an indication of negative effects of the policy. If no such differences are found, the conclusion may mistakenly be that all is well, even though the behavior in *all* relations in the group may be negatively affected by the policy.

Another policy lesson is that there is a difference between paying lip service to a policy and behaving in accordance with its intentions. When I introduce a fairness reason for the quota, participants move toward finding the unrepresentative quota to be more fair; in fact they find the selection process as fair as in the control treatment. However, they do not change their cooperative behavior but continue to contribute as little in the public good game as when no justification is given. This indicates that even though people may state that they find a policy to be justified or fair, its negative impact on behavior may persist.

Naturally, many laboratory findings may not translate into other settings. In group interactions outside the laboratory, for example on corporate boards, in schools and in the workplace, there are additional factors which are not captured in the laboratory. An example is the fact that relations outside the laboratory are generally of a longer duration. Also, in practice affirmative action

policies are applied to groups of people who already have associations and prejudices attached to them. This may play a role in how a particular quota is perceived. However, the fact that this experiment shows an effect from preferential selection also in the abstract, short-lived and "stripped down" environment of the laboratory is an indication that this effect can arise also under minimal conditions and is thus potentially strong.

As the use of affirmative action policies and quotas spreads in many parts of the world, it is important to understand their effects on how groups function. This chapter is a contribution to that research, but many questions remain unanswered. It would, for example, be interesting to conduct a version of our experiment in which measures of emotions and moods are utilized in order to investigate more specifically the effects that the unrepresentative quota have on participants.

Small alterations to the design used in this chapter would also make it possible to answer questions about whether an asymmetric role of performance in itself causes less cooperation, or if it is that asymmetry together with preferential treatment that gives rise to the quota effect. Finally, in order to better assess the generality and external validity of these findings, it would be interesting to examine other group compositions than the ones used here; would the results change if the underlying group consisted of, for example, five purples and eleven oranges instead of four and twelve respectively? In order to better understand the way affirmative action and quotas impact group cooperation, I plan to address these, and other, topics in future work.

2. Social Framing Effects: Preferences or Beliefs?[21]

2.1 Introduction

In a seminal experiment, Deutsch (1958) showed that behavior in a Prisoners' dilemma depends on whether experimental subjects are induced to feel cooperative or individualistic before making their choice. While Deutsch's instructions were heavily loaded,[22] it has long been clear that subtler contextual manipulations may also affect behavior. Eiser and Bhavnani (1974) find that behavior in a Prisoners' dilemma is more cooperative when the situation is framed as an international negotiation than when it is framed as a business transaction. Likewise, subjects cooperate more in a "social exchange study" than in a "business transaction study" (Batson and Moran, 1999), and substantially more in a "community game" than in a "Wall Street game" (Kay and Ross, 2003; Liberman, Samuels, and Ross, 2004), even when the subjects' instructions are otherwise neutral.[23]

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[21] This is joint work with Tore Ellingsen, Magnus Johannesson and Sara Munkhammar. The paper appeared in *Games and Economic Behavior*, 76: 117-130.

[22] According to Deutsch (1960), in the cooperative condition, the beginning of the instruction was: "Before you start playing the game, let me emphasize that in playing the game you should consider yourself to be partners. You're interested in your partner's welfare as well as in your own." In the individualistic condition, the beginning of the instruction instead was: "Before you start playing the game, let me emphasize that in playing the game your *only* motivation should be to win as much money as you can for yourself. You have no interest whatsoever in whether the other person wins or loses or in how much he wins or loses."

[23] From now on, we use the term social frame in the narrow sense of "the name of the game" – the labeling of the situation. Other studies that investigate the impact of game labels or strategy labels in social dilemmas include, inter alia, Andreoni (1995), Brandts and Schwieren (2009), Brewer and Kramer (1986), Cookson (2000), Cubitt, Drouvelis, and Gächter (2011), Dufwenberg, Gächter, and Hennig-Schmidt (2011), McDaniel and Sistrunk (1991), McCusker and Carnevale (1995), Meier (2006), Pillutla and Chen (1999), Rege and Telle (2004), Sell and Son (1997), van Dijk and Wilke (2000), and Zhong, Loewenstein, and Murnighan (2007). Labels have also been shown to affect cooperative behavior in other games; see for example Larrick and Blount (1997) and Barr and Serra (2009). Whereas we are only concerned with the effect of labeling on cooperation in social situations, Tversky and Kahneman (1981) (who coined the "framing effect" concept) showed that wording can have a significant impact on individual choice as well; see Levin, Schneider, and Gaeth (1998) for a survey of individual choice effects of wording.

Such context sensitivity has been interpreted as bad news for utility theory in general (Weber, Kopelman, and Messick, 2004) and for social preference theories in particular (Levitt and List, 2007). To the extent that people can be seen as maximizing utility at all, it appears that the utility function must include situational elements that conventional theory leaves out. At a more practical level, the results have been used to criticize economists' emphasis on material incentives. By triggering a selfish social frame, material incentives could potentially reduce, for example, employee effort (Frey and Osterloh, 2005; Pfeffer, 2007), legal compliance (Tyran and Feld, 2006; Bohnet and Cooter, 2001), and other prosocial behaviors (Koneberg, Yaish, and Stocké, 2010). Through this channel, the very language and assumptions of economics could be eroding cooperation (Ferraro, Pfeffer, and Sutton, 2005).

However, the lessons from the experimental findings are less obvious than they may first appear. The social framing results have several possible explanations, and additional evidence is needed to discriminate between them. As Camerer (2003, p. 75) puts it: "There is little doubt that describing games differently can affect behavior; the key step is figuring out what *general* principles (or theory of framing) can be abstracted from labeling effects." Our purpose here is to provide new evidence that helps to elucidate these general principles.

It is possible to distinguish at least three broad classes of social framing theories. The first class posits that frames affect internalized social norms or, alternatively, social preferences:[24] This would mean that the Community label triggers a stronger desire or compulsion to cooperate

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[24] Social norms are defined on the set of social situations (i.e., game forms; see below), whereas social preferences are typically defined on some set of ultimate outcomes.

(Montgomery, 1998; Weber, Kopelman, and Messick, 2004). We call this the *variable sociality hypothesis*.

Bacharach (2006) provides a formal treatment of the variable sociality hypothesis for a particular case of internalized social norms, namely team reasoning.[25] An alternative formalization, involving a smaller departure from conventional game theory, is to assume that certain decision frames affect social preferences, such as altruism. In this case, a Prisoners' dilemma in material payoffs may be transformed into, for example, a common interest game in utilities. More precisely, the *game form* (which summarizes the objective features of strategies and payoffs) is a Prisoners' dilemma, but the *game* (which involves von Neumann-Morgenstern utilities) is not.[26]

Another hypothesis is that people respond to social frames because the frame affects how others interpret their behavior, which in turn determines their social esteem. Even a person who has not internalized norms, and who holds strictly selfish preferences, may want to appear to be prosocial. This is the *social image hypothesis*.[27]

A third class of theories of social framing effects is that the frame affects the expectations that people have about each other's behavior, and these expectations in turn affect the own behavior.

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[25] Roughly, when a person engages in team reasoning, she asks which strategy profile is best for the group and picks her component of that profile; see Sugden (1993, 2003) and Bacharach (1999, 2006). The theory comes in several flavors; Bacharach considers unconditional norm compliance, whereas Sugden (2003) emphasizes that compliance may be conditional on the belief that others comply as well.

[26] The term "game form" originates from Gibbard (1973, p. 587), and is used synonymously with "mechanism" in the mechanism design literature. It is a description of the link between strategies and outcomes. Utility functions, in turn, link outcomes to real numbers (utilities).

[27] There is a sizeable literature documenting that people are motivated by social esteem considerations; see for example Brennan and Pettit (2004), Ellingsen and Johannesson (2007), Andreoni and Bernheim (2009) and the references therein.

For this theory to apply in a Prisoners' dilemma, it is necessary that people care not only about their own material payoffs, but also about others' actions (Sen, 1967), intentions (Rabin, 1993) or material payoffs (Becker, 1974; Fehr and Schmidt, 1999), in which case a Prisoners' dilemma in material payoffs may be transformed into a "Stag hunt" in utilities. Since a Stag hunt game has two pure strategy Nash equilibria, the frame can be used as an equilibrium selection device, as noted by Rabin (1998) and Fehr and Schmidt (2006). At the outset, we lump together all these models that are based on multiple equilibria and refer to them as the *coordination hypothesis.*

In order to evaluate the relative importance of the three hypotheses, we first state them formally. We then report three separate one-shot Prisoners' dilemma experiments, in which we systematically vary features of the game. The first experiment replicates the main finding of previous studies, namely the presence of framing effects in a simultaneous Prisoners' dilemma. It also shows that this finding is difficult to explain with some versions of the variable sociality hypothesis. The second experiment evaluates the social image hypothesis, which finds no support in the data. The third experiment considers framing effects in sequential Prisoners' dilemmas. It finds none.

In our view, the most striking finding is that there is indeed a significant social framing effect when subjects make their decisions simultaneously, as in our first experiment, but not when decisions are made sequentially, as in our third experiment.[28] This finding is consistent with the coordination hypothesis, but inconsistent with the variable sociality hypothesis. Briefly, the argument runs as follows: Because a sequential Stag hunt game has a unique subgame perfect

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[28] It would have been even better to have data from one experiment in which framing effects under simultaneous and sequential play is compared directly. We return to this and other caveats in the two final sections.

equilibrium, there is no room for a coordinating role of labels. On the other hand, team reasoning or altruism should affect behavior regardless of whether moves are simultaneous or sequential. Our findings thus suggest that framing effects in social dilemmas are well explained within the modern version of the rational choice paradigm, without any appeal to context-dependent preferences. (As we shall see, one caveat is that the beliefs themselves could be involving some notion that opponents have – or expect their opponents to have etc – context dependent preferences.)

The research described in this chapter is most closely related to Liberman, Samuels, and Ross (2004), whose results were circulated already in the early 1990's. They report on three studies. The first study compares behavior in a seven-round Prisoners' dilemma under a "Wall Street Game" frame to the corresponding behavior under a "Community Game" frame, using a selected group of 48 male college students.[29] The second study instead uses 40 Israeli pilot trainees, and the labels Bursa Game and Kommuna Game, but is otherwise similar. In both studies, cooperation rates are significantly higher under the Community/Kommuna Game frame. In the second study, both the pilot trainees themselves and their (flight) instructors are asked to make predictions about others' first-round behavior. On average, the participants are more optimistic regarding others' cooperation in the Kommuna Game than in the Bursa Game, but no such difference is observed among instructors. Moreover, participants expecting first-round cooperation are relatively likely to cooperate in the Kommuna Game, but not in the Bursa Game.[30] Finally, in the third study, college students who had not participated in Study 1 were asked to predict first-

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[29] One purpose of the study was to compare the influence of the social context with that of presumed personality characteristics. The subjects had been chosen by peers based on their likely propensity to cooperate.

[30] If the game had been one-shot, the latter difference would clearly have suggested that preferences depend on frames. However, here it might instead reflect different expectations about behavior in later rounds.

round choices. Like the flight instructors in Study 2, these subjects failed to predict the large difference in cooperation rates between the two frames, suggesting that beliefs depend on whether one is a participant in the situation or not.

Besides involving a much larger number of subjects, and hence having more statistical power, our experiments provide qualitatively new insights. First, by considering a one-shot Prisoners' dilemma, we narrow down the set of explanations:[31] We rule out the possible objection that even selfish materialists could find it in their interest to cooperate in the first round of a finitely re-peated game, either because of uncertainty about the opponent's type (Kreps et al, 1982) or be-cause the payoff loss from one round's cooperation is small enough to neglect (Radner, 1986). Second, and more importantly, we use variation in available strategies and information to dis-criminate between different explanations for social framing effects. Liberman, Samuels, and Ross (2004) cannot rule out the possibility that the frame's primary effect is on the preferences, and that beliefs only change as a result of the preference change. Our evidence suggests that the social frame only affects behavior through the beliefs, and not through preferences. (Of course, even if they are not affected by the frame, preferences are still important in our analysis, since they determine whether beliefs matter for behavior to begin with.)

Other closely related experimental studies include Cookson (2000) and Rege and Telle (2004), who study social framing effects in public goods contribution games. Both these papers find that a more community oriented frame creates more cooperation, although in the latter study the ef-

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[31] Cubitt, Drouvelis, and Gächter (2011) provide further reasons for studying framing effects in one-shot games.

fect is only marginally statistically significant, probably due to a small number of subjects per treatment.[32]

We are not the first to utilize a sequential Prisoners' dilemma to disentangle preferences and beliefs. In a study of in-group favoritism Yamagishi and Kiyonari (2000) find that there is more in-group favoritism in Prisoners' dilemmas with simultaneous play than in games with sequential play.[33] Although Yamagishi and Kiyonari do not explicitly invoke the game theoretic argument, it is clear that their idea is similar to ours: They interpret the sharp reduction of in-group favoritism in the sequential setting as an indication that the in-group favoritism in the simultaneous setting is driven primarily by expectations, not preferences. However, Yamagishi and Kiyonari only study the behavior of first-movers, whereas our strongest evidence comes from the absence of a framing effect among second movers in the sequential game.

Our findings also relate quite closely to Bohnet and Cooter (2001), who experimentally compare the behavioral impact of small penalties across different game forms. They find little effect of penalties in a many-player Prisoners' dilemma, but large effects in a coordination game. A natural interpretation is that the small penalties for defection from socially optimal actions moved the beliefs in a favorable direction, and that such movement only matters in a coordination game. However, as we point out, one cannot from looking at material payoffs alone infer what the real

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[32] Note however that two recent studies, Brandts and Schwieren (2009) and Dufwenberg, Gächter and Hennig-Schmidt (2010), report similar experiments that fail to establish the expected framing effect. In the latter, it turns out that there is a natural explanation – the particular subjects' negative view of their own community – but in the former there is no obvious reason for the lack of a framing effect.

[33] In-group favoritism refers to the phenomenon that people behave more favorably toward members of the own group than toward non-members. We refer to Chen and Li (2009) for an extensive review and experimental evaluation of in-group favoritism.

game is; for two conditional cooperators, the Prisoners dilemma game form is a coordination game. Thus, the above natural interpretation requires an independent argument for why material payoffs and utilities are likely to coincide in this case.

The chapter is organized as follows. Section 2.2 briefly discusses the different theories. Section 2.3 describes our first study, which establishes both that social framing effects exist and that they can be removed by suitable manipulations of the environment. More precisely, the study shows that the framing effect vanishes when the opponent is unaware of what game is being played and the opponent's action is controlled by a suitably programmed computer. The second study, reported in Section 2.4, shows that the framing effect remains absent under otherwise similar circumstances even if the opponent is informed, a finding which goes against the notion that people cooperate in the Community Game in order to impress their opponent with their altruism. The third and final study, reported in Section 2.5, shows that there is no framing effect in the sequential Prisoners' dilemma. Section 2.6 discusses some caveats and concludes.

2.2 Theory

Consider two players facing the actions and material payoffs (the game form) depicted in Figure 2.1.

Figure 2.1: Material Payoffs – the Game Form

	C	D
C	c, c	s, w
D	w, s	d, d

Let $w > c > d > s$. Moreover, let $s + w < 2c$. Thus, the sum of the material payoffs is largest if both players choose action C. However, if the players are selfish materialists, they will both be playing D, since D maximizes the own material payoff regardless of what the opponent does. That is, the game form in Figure 2.1 is a Prisoners' dilemma. The action labels are chosen to indicate Cooperation and Defection, respectively, and the letters for the payoff parameters c and d are chosen accordingly. In line with the classical treatments of the Prisoners' dilemma, the letters s and w indicate the "sucker" payoff and the "winner" payoff respectively.

From now on, we say that a player is selfish if she cares only about the own material payoff. All other player types are considered to be unselfish to some degree.

We introduce the following pieces of notation. Let $S_i \in \{C, D\}$ be a pure action for player i and let $m_i(S_i, S_j) \in \mathbb{R}$ be player i's material payoff given the strategy profile (S_i, S_j). Players may care about the opponent's payoff too, that is, they may have social preferences, or they may have a view about what is the right course of action in a certain situation, that is, they may care about social norms.

Players may also care about non-material outcomes, such as the opponent's belief about their social preferences. To capture such preferences, let **T** be a finite set of possible player types, with τ as a typical element; let τ_{ij} denote player i's expectation about player j's type; and let τ_{ji}^e de-

note player *i*'s expectation about τ_{ji}.[34] Finally, let **F** denote the set of frames, with typical element *F*. In general, player *i*'s utility can thus be written as a function $U_i(m_i, m_j, \tau, \tau_{ij}, \tau_{ij}^e, F)$.

Below, we consider some specific examples of how the frame may enter players' preferences and beliefs respectively. In most of the examples, we abstract from preference heterogeneity in the interest of simplicity. With heterogeneous preferences, we would have to employ different solution concepts, using the theory of games with incomplete information. In the fifth model considered below, we admit heterogeneity and also briefly indicate how insights from the complete information models are robust to (some) preference heterogeneity.

2.2.1 Frame-Dependent Preferences

The frame could be directly affecting the degree of unselfishness. Suppose players are altruists, but that their altruism depends on the frame *F*. Specifically, let each player *i* assign utility $\alpha(F)m_j$ to the opponent's material payoff m_j. That is, utility can be written

$$U_i = m_i + \alpha(F)m_j. \tag{1}$$

Suppose these preferences are common knowledge. The game corresponding to the game form in Figure 2.1 is then given by the bi-matrix depicted in Figure 2.2.

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[34] For early formal models of social esteem concerns, see Bernheim (1994), Ireland (1994) and Glazer and Konrad (1996). For recent extensions and applications, see Bénabou and Tirole (2006) and Ellingsen and Johannesson (2008). Experimental evidence suggests that people care about what others think about their actions even if the interaction itself is anonymous; see Dana, Cain, and Dawes (2006), Broberg, Ellingsen and Johannesson (2007), and Lazear, Malmendier, and Weber (2012). Beliefs may affect preferences also in a model without "types" – see Dufwenberg, Gächter, and Hennig Schmidt (2010) for references and an application to framing effects.

Figure 2.2: The Game Played by Frame-Sensitive Altruists

	C	D
C	$c + \alpha(F)c, c + \alpha(F)c$	$s + \alpha(F)w, w + \alpha(F)s$
D	$w + \alpha(F)s, s + \alpha(F)w$	$d + \alpha(F)d, d + \alpha(F)d$

If players are sufficiently altruistic, the game is not a Prisoners' dilemma. For example, if $\alpha > max\{(w - c)/(c - s), (d - s)/(w - d)\}$, each player's dominant choice is to play the cooperative action C, whereas action D remains dominant if $\alpha < min\{(w - c)/(c - s), (d - s/w - d$. (For intermediate values of α, we have that the game is Stag hunt if $(d - s)/(w - d) > (w - c)/(c - s)$, and Chicken if $(d - s)/(w - d) < (w - c)/(c - s)$.

Team reasoning, at least in the sense of Bacharach (2006), can be seen as an internalized social norm that requires players to unconditionally pick the strategy profile that is consistent with joint payoff maximization, here (C, C).[35] For the purposes of our study, team reasoning thus yields the same behavior as an altruism parameter $\alpha > max\{(w - c)/(c - s), (d - s)/(w - d)\}$.[36] Indeed, perhaps the most natural interpretation of the players' altruistic concern $\alpha(F)$ is that they have internalized an efficiency norm, not that they care strongly about the opponent's life-time consumption.[37] Observe that beliefs about the opponent are irrelevant whenever a player has a dominant action. Thus, even if players are heterogeneous, differing with respect to their propen-

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[35] Sugden (2003) proposes a theory of team reasoning in which players only want to stick to the norm if they expect opponents to do so too. This model of expectation formation entails coordination on efficient equilibria in common interest games.

[36] As noted by Sugden, 2008, there is always such correspondence between team reasoning and linear social preferences when the game is decomposable; however, for non-decomposable games the correspondence breaks down.

[37] See, Andreoni and Bernheim (2009), Krupka and Weber (2009), and especially López--Pérez (2008) for different models of norm compliance.

sity to comply with norms, the belief about the opponent's propensity does not enter into the decision problem.

If people obtain more utility from obeying the efficiency norm when the game form is called a "Community Game" than when it is called a "Stock Market Game," the propensity to play C will tend to be higher in the former case. For an economist, this is perhaps the most straightforward formalization of the variable sociality hypothesis.

The social esteem hypothesis instead says that people are concerned about what others may think about them. For example, a player may get positive utility from believing that the opponent believes that she is altruistic (or obeys the efficiency norm). Formally, player i's belief about player j's belief will then enter player i's utility function. For example, suppose that each player is either selfish $(\tau = 0)$ or altruistic $(\tau = \alpha(F))$, and that players' desire for social esteem is independent of their actual altruism, but possibly dependent on the frame. Then player i's utility function can be written

$$U_i = m_i + \alpha(F)m_j + v\big(\alpha_{ji}^e(S_i, F), F\big). \tag{2}$$

For simplicity, assume that $v(0, F) = 0 < v(\alpha, F)$ for all $\alpha > 0$ and any frame F – that is, selfishness is never a source of esteem. Note here that the opponent's ex post belief, and hence one's esteem, may depend on the own action. In general, the frame could affect both the opponent's interpretation (and hence be an argument of α_{ji}^e) and the utility of the opponent's belief – it's more valuable to be considered unselfish in a community setting (and hence appear as an independent argument of v).

With these preferences, the Prisoners' dilemma turns into a (two-sided) signaling problem, in which players may cooperate not only because they are altruistic, but also in order to *convey the impression* that they are altruistic. If the value of looking altruistic, $v(\alpha_{ji}^e, F)$, is greater under one frame than another, then this hypothesis works in essentially the same way as the variable sociality hypothesis.[38] However, since only the social esteem considerations are affected by external observability it is still possible to distinguish between the two hypotheses.

2.2.2 Frame-Dependent Beliefs

Let us next consider models in which frames do not enter preferences, but may be entering beliefs instead. If the game has multiple equilibria, the frame may then affect equilibrium selection. There are a variety of social preferences that transform a Prisoners' dilemma game form into a game with multiple equilibria.

Suppose first that players desire to behave altruistically if and only if their material payoff is no smaller than that of the opponent. That is, their utility function takes the form

$$U_i = m_i + \alpha I m_j, \qquad (3)$$

where I is an indicator variable taking the value 1 if $m_i \geq m_j$ and 0 otherwise.[39] Then, if preferences are common knowledge, the game is as depicted in Figure 2.3.

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[38] More formally, there will be an open set of parameters such that, in the unique perfect Bayesian equilibrium that satisfies the Intuitive Criterion, the altruists play C under one frame, but D under the other. (Egoists play D under either frame.)

[39] The key to our analysis is that altruism is greater when players are ahead, not that it vanishes completely when they are behind. However, this formulation is particularly simple. See Charness and Rabin, 2002, for a detailed discussion of the relevance of such social welfare preferences.

	C	D
C	$c + \alpha c, c + \alpha c$	$s, w + \alpha s$
D	$w + \alpha s, s$	$d + \alpha d, d + \alpha d$

If $\alpha > (w - c)/(c - s)$, the game has two pure strategy equilibria, namely (C, C) and (D, D), where the former equilibrium Pareto-dominates the latter. That is, the game is not Prisoners' dilemma, but Stag hunt.[40]

To the best of our knowledge, this kind of argument was first made by Sen (1967). However, Sen invoked the concept of "conditional cooperation," which is a general description of preferences over alternative actions in the specific situation of a social dilemma rather than a specific preference ordering over a general set of outcomes. As is well known, there are many other general preference orderings over outcomes that may also give rise to "conditional cooperation" in social dilemmas.

For example, as noted by Fehr and Schmidt (2006), an analogous argument holds if both players dislike "taking advantage of" their opponent. (Many people dislike even more to be taken advantage of, but for the current argument it is only the aversion to advantageous inequality that matters.) Specifically, suppose that players' utility can be written

$$U_i = m_i - \beta max\{0, m_i - m_j\}. \tag{4}$$

Then, if the utilities are common knowledge, the corresponding game is depicted in Figure 2.4.

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[40] We here employ a broad definition of Stag hunt. A narrower definition would admit only games in which (D,D) is risk-dominant. In our example, this condition is fulfilled if s is sufficiently small.

	C	D
C	c, c	$s, w - \beta(w - s)$
D	$w - \beta(w - s), s$	d, d

If $\beta > (w - c)/(w - s)$, the game is Stag hunt, with pure strategy equilibria (C, C) and (D, D).

There are several other versions of the above argument, including the conditional fairness model of López-Pérez (2008) as well as the intention-based fairness model of Rabin (1993). Taking inspiration from the theory and findings of Charness and Rabin (2002), here is one model that we like particularly well, and which will also turn out to rationalize our data. Suppose there are two types of players, egoists and conditional altruists with the utility function

$$U_i = m_i + \alpha I \alpha_{ij} m_j, \tag{5}$$

where I is an indicator variable taking the value 1 if $m_i \geq m_j$ and 0 otherwise. That is, altruism is only triggered when the opponent is behind and is not believed to be an egoist. Then, if α_{ij} and α are sufficiently large, the resulting incomplete information game has two pure strategy Bayesian Nash equilibria – one in which conditional altruists play C and one in which they play D. Egoists, of course, always play D.

When the game has multiple equilibria, as in models (2)-(5), it is a short step to see that the frame can be used as a coordination device, indicating a focal point (Schelling, 1960). Moreover, in order for the frame to have an effect on the agents' behavior, it is not necessary to assume that preferences change. Instead, since the social frame only affects beliefs, this approach is fully

compatible with the view that models of preferences ought to be parsimonious and portable across games.[41]

The notion that frames affect coordination is more than a theoretical possibility. There is substantial evidence that people use action labels for coordination purposes; see Mehta, Starmer, and Sugden (1994) and Crawford, Gneezy, and Rottenstreich (2008). A formal model of how this may happen has been developed by Bacharach (1993) and refined through the notion of level-k reasoning by Bacharach and Stahl (2000); see also Bacharach and Bernasconi (1997) and Bardsley et al (2010). [42]

Such a level-k model can also be used to rationalize the impact of the game label, as opposed to action labels, on behavior. Roughly, if a level-1 player i thinks that player j (if not selfish), is attracted towards joint payoff maximizing actions under the Community label but towards private payoff maximizing actions under the Stock Market label, then player i, if conditionally cooperative for either of the reasons specified above, will also cooperate under the Community label but not under the Stock Market label.

2.3 The First Study: Presence and Absence of Framing Effects

The first experiment was conducted at Södertörn University College and Stockholm School of Economics, both in Stockholm, Sweden, on three different occasions. The first sessions were run

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[41] For a discussion of the trade-off between fit and parsimony in the modeling of people's preferences, see Sobel (2005).

[42] Cachon and Camerer (1996) and Rydval and Ortman (2005) demonstrate that loss aversion furnishes another, and possibly related, coordination principle.

at Södertörn in April 2006. Subsequent sessions were run at Södertörn in November 2006 and at the Stockholm School of Economics in September 2007. On each occasion the subjects were randomly allocated between four treatments.

In total 448 subjects participated as decision-makers in the experiment. All were freshmen enrolled in a basic microeconomics course. In addition, 220 student subjects participated as recipients in the asymmetric information treatment (described below).

Two of the treatments are intended to investigate whether we can replicate previous findings of social framing effects within an experiment that satisfies current requirements in behavioral economics. Specifically, the sample size is large, real money is at stake and each subject is exposed only to one decision frame. Moreover the social framing is quite light; the name of the game differs across treatments, but otherwise the description of the situation is neutral.

The other two treatments are designed to test whether it is possible to reduce or eliminate any framing effects, by manipulating several features of the situation. This is described in detail below.

2.3.1 Design

In treatments 1 and 2, henceforth called the symmetric information treatments, subjects are seated in four different rooms. Each subject is, anonymously and randomly, paired with a subject in another room, and both subjects receive identical oral and written instructions. Indeed, with the exception of the name of the game, all subjects in treatments 1 and 2 receive identically

worded instructions. In one pair of rooms, the situation is called the Stock Market Game (treatment 1); in the other pair of rooms, it is called the Community Game (treatment 2).

The paired subjects simultaneously choose between two options, denoted A and B respectively. If both subjects choose option A, each earns 50 SEK[43] (Swedish Kronor; $1≈SEK 7.50 at the time of the experiment). If both subjects choose option B, each earns SEK 20. If one subject chooses option A and the other subject chooses option B, the former earns SEK 5 and the latter earns SEK 80. The associated game form is depicted in Figure 2.5.

Figure 2.5: The Game Form in the Experiment

	A	B
A	50, 50	5, 80
B	80, 5	20, 20

Since each subject earns more by choosing B than by choosing A, and the pair of actions (B, B) yields lower payoffs for both subjects than the pair (A, A), and the total payoff from (A, B) or (B, A) is lower than from (A, A) the situation is a true Prisoners' dilemma. Indeed, the game form is a special case of that in Figure 2.1, but with the letters A and B replacing C and D. In all experiments, we used letters A and B in order to minimize the risk that any of our subjects would associate the labels with particular meanings, such as cooperation and defection. However, for ease of reference, we now revert to using letters C and D in the current text.

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[43] In April 2006, USD 1= SEK 7.6. At the time of the following experiments the krona's exchange rate is slightly better, with the krona hitting its highest value against the dollar (USD 1= SEK 6.7) in September 2008 and September 2009.

In treatments 3 and 4, henceforth called the asymmetric information treatments, only one person in each pair was in control of the own decision. These decision-making subjects were given oral and written instructions that differed from the ones given in treatments 1 and 2 only with respect to the matched subject's choice. The matched subject was explained to be an uninformed receiver, whose action is chosen by a computer. The computer would make the opponent's action choices with the same frequencies as actual play in the corresponding active opponent treatment. Only information regarding the procedure was given to the decision-maker, and not the actual frequency. The instructions for treatments 3 and 4 were identical except for the name of the game, which was the Stock Market Game in treatment 3 and the Community Game in treatment 4. The receivers were given written information that they were taking part in an economic experiment, but received no information about why they received a specific payoff.

After the experiment, the participants received information about their matched subject's action and were paid accordingly. Appendices B.1 and B.2 contain translations of the complete experimental instructions.

2.3.2 Predictions

Let models be indexed by the equation number of the corresponding utility function. All the models reviewed above allow the outcome that there is more cooperation in treatment 2 (standard Community Game) than in treatment 1 (standard Stock Market Game). However, they differ substantially regarding their predictions regarding treatments 3 and 4.

If we take the game label to be the frame, and under the assumption that beliefs are really the same in *T3* (*T4*) as in *T1* (*T2*), then Model (1) predicts that the behavior is the same in *T3* as in *T1*, and the same in *T4* as in *T2*. Thus, the framing effect ought to be the same when comparing *T3* and *T4* as when comparing *T1* and *T2*.

Model (3) and Model (4), predict that behavior in *T3* (*T4*) is identical to that in *T1* (*T2*) under the sole condition that beliefs are the same in *T3* as in *T1* and in *T4* as in *T2*. According to these two models it only matters what the opponent ends up doing, not what she knows or wants. Thus, subjects should disregard whether their opponent is active or passive, informed or uninformed.

Model (2) allows behavior to depend on the opponent's information. If anything, there should be more cooperation in *T1* than in *T3*, as social esteem is only at stake in the former case.

Model (5) allows an active subject's behavior to depend on the opponent's freedom of choice.[44] To see how, let μ be the common prior that the opponent is a conditional altruist. Suppose that conditional altruists attempt to coordinate on cooperation in T2 but not T1. As an egoist always defects (plays *D*), the requirement for a conditional altruist to be playing *C* in any equilibrium is

$$\mu(50 + 50\alpha) + (1 - \mu)5 \geq \mu(80 + 5\alpha) + (1 - \mu)20,$$

which simplifies to

$$\mu(3\alpha - 1) \geq 1.$$

Since μ must lie between 0 and 1, this in turn boils down to the requirements that $\alpha \geq 2/3$, and $\alpha \geq (1 + \mu)/3\mu$.

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[44] McCabe, Rigdon and Smith (2003) compare behavior in a standard trust game with that in an "involuntary" trust game, in which the trustor has no choice but to trust. The trustee is more likely to reward voluntary trust than involuntary trust. Since the prisoners' dilemma is essentially a simultaneous move version of the trust game, their finding is quite relevant here.

But suppose now instead that one of the players is unable to choose freely – instead the cooperation rate is simply fixed at μ for both egoists and conditional altruists, as will be the case in *T4* under our assumption about play in *T2*. Then, the active player chooses to play C if and only if

$$\mu(50 + 50\alpha\mu) + (1 - \mu)5 \geq \mu(80 + 5\alpha\mu) + (1 - \mu)(20 + 20\alpha\mu),$$

which simplifies to

$$\mu\big((13\mu - 4)\alpha - 3\big) \geq 3.$$

Again, we can use the fact that μ lies between 0 and 1 to deduce the necessary condition $\alpha \geq 2/3$, just as before. However, the additional requirement on the two parameters is stronger than before. (To see this, rewrite the condition as $\alpha\mu(13\mu - 4) \geq 3(1 + \mu)$. This inequality is never satisfied if $\mu \leq 4/13$. If instead $\mu \geq 4/13$, the condition can be written $\alpha \geq 3(1 + \mu)/(13\mu - 4)\mu$, which is implied by the previous condition $\alpha \geq (1 + \mu)/3\mu$ for all $\mu < 1$.) For example, when $\alpha = 1$, the critical value of μ jumps from 1/2 in the active opponent case to more than 4/5 in the passive opponent case. Likewise, if $\alpha = 3$, the critical value of μ jumps from 1/8 in the active opponent case to more than 1/2 in the passive opponent case. Thus, Model (5) suggests that a cooperative equilibrium which exists when both players are active could well be vanishing when one player is passive. Intuitively, this happens both because the active player's utility from the (C, C) outcome is smaller under *T4* than *T2* – as some of the cooperation benefits in *T4* go to an egoistic opponent – and because the active player's utility from (D, D) is larger under *T4* than *T2* – as some defecting opponents are conditional altruists (any defecting active opponent is an egoist).

2.3.3 Findings

The findings are displayed in Figure 2.6.[45] They reveal a social framing effect in the symmetric information condition. The fraction of subjects making the cooperative choice increases from 26.4 percent with the Stock Market Game frame to 44.9 percent with the Community Game frame. This difference is statistically significant (one-sided z-test; $z = 2.972$, $p = 0.0015$),[46] rejecting the null hypothesis of zero framing effect.

Figure 2.6: Fraction of Cooperative Actions in Treatments 1-4.
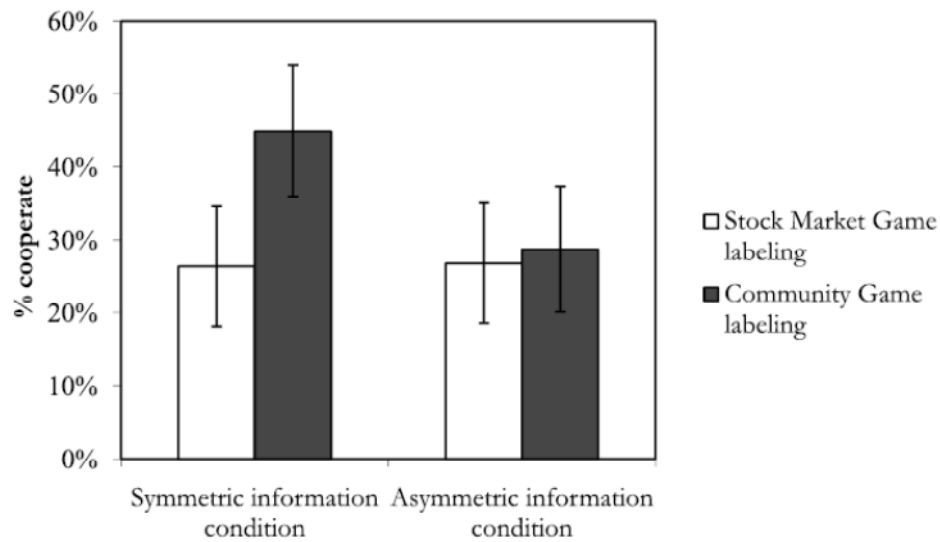
The difference between the first two bars is the social framing effect in the standard symmetric information condition. The difference between the second two bars is the social framing effect in the asymmetric information condition. Error bars indicate 95% confidence intervals.

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[45] Throughout, the confidence intervals are normal approximation intervals; given that our samples are large and that the cooperation probability is not too close to 0 or 1, the normal approximation to the binomial distribution is known to be good. A possible exception is the last pair of bars of Figure 2.10.

[46] As our alternative hypothesis is one-sided (more cooperation under the Community label), we use one-sided tests for all comparisons of cooperation levels.

Under asymmetric information, on the other hand, the framing effect is insignificant with 26.8 percent making the cooperative choice with the Stock Market game frame and 28.7 percent with the Community Game frame ($z = 0.316$, $p = 0.376$). The difference-in-difference between the two information conditions is also statistically significant ($z = 1.910$, $p = 0.028$), rejecting the null hypothesis that the framing effect is the same under asymmetric information as under symmetric information, and favoring the alternative hypothesis that the framing effect is larger under symmetric information.[47]

The finding of a social framing effect in treatments 1 and 2 shows that prior findings are robust to such features as monetary incentives and lightly loaded instructions. It is somewhat less clear how we should interpret the absence of a social framing effect in treatments 3 and 4. Essentially subjects in both treatments 3 and 4 have the same cooperation rates as in treatment 1.

Of the five models presented above, only Model (2) and Model (5) are directly consistent with the evidence.

As mentioned above, Model (1), the variable sociality hypothesis, could be rescued by invoking the argument that the frame is more than just the label. However, Models (3) and (4) can only be rescued by assuming that subjects, erroneously, hold other beliefs in the active condition than in the passive condition. Liberman, Samuels and Ross (2004) elicited beliefs both by participants

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[47] Whereas the framing effect was stable between our sample at Södertörn and the one at SSE, the cooperation ratios (levels) were not the same. At Södertörn, with a total sample size of 230, the cooperation ratio was 0.327 in treatment 1 and 0.530 in treatment 2 ($z=2.245$, $p=0.012$), and 0.328 in treatment 3 and 0.370 in treatment 4 ($z=0.530$, $p=0.298$). At SSE we had a total sample of 218 and the cooperation ratios were 0.207 in treatment 1 and 0.346 in treatment 2 ($z=1.625$, $p=0.052$), and 0.204 in treatment 3 and 0.204 in treatment 4 ($z=0$, $p=0.5$).

and non-participants in their social framing experiment, finding that participants' beliefs respond more strongly to the frame than beliefs of non-participants. Similar differences could in principle arise between the active and passive opponent conditions. Arguably, belief elicitation would have enabled us to see whether beliefs vary across conditions. However, belief data have problems of their own. Belief elicitation before subjects choose their action may affect behavior (Croson, 1999), belief elicitation after the action choice may be biased by the chosen action (Dawes, McTavish, and Shaklee, 1977), and in any case the elicited beliefs may be quite different from the subjective beliefs that would rationalize the observed choice (Costa-Gomes and Weizsäcker, 2008). Thus, we think that a better way to control for differences in beliefs would be to induce beliefs directly. For example, one might re-run our four treatments with the modification that all subjects in treatments 1 and 3 (2 and 4) are informed about empirical frequencies in our treatment 1 (2).[48]

The subsequent experiments reported below instead pursue different directions, attempting to discriminate between the various hypotheses without eliciting or inducing beliefs.

2.4 The Second Study: Social Esteem?

Our own initial hypothesis was that social framing effects are caused largely by people's desire to look good in the eyes of others, as in Model (2). The findings of the first study are consistent with this hypothesis. The second study is designed to provide a sharper test.

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[48] We are grateful to Ernst Fehr for this suggestion.

We now modify treatments 3 and 4 of the first study in one crucial respect, namely, by letting the passive player observe the game and the choice of the active player. Call the two new treatments 1' (Stock Market Game) and 2' (Community Game) respectively. If it is the difference in information that created the discrepancy between treatments 2 and 4, then there should now be a similar gap between treatments 1' and 2' as between treatments 1 and 2. To be precise, there should be a similar gap if subjects' expected utility of being esteemed by the opponent is independent of whether subjects learn about the opponent's type. On the other hand, if subjects anticipate that they will experience stronger feelings of pride or shame if they learn what the opponent chooses, as is admitted by Model (2), framing might matter more in treatments 1 and 2 than in 1' and 2'.

In total, 137 subjects participated as decision-makers in the experiment, which was conducted in September 2008. All were freshmen enrolled in a basic microeconomics course at Stockholm School of Economics (SSE). In addition, 137 student subjects participated as passive players. Notice that the subjects have very similar characteristics to the SSE subjects in Study 1. In both cases virtually the entire cohort participates, as participation was the default option for participants in the course. The only difference is that subjects in Study 2 belong to a later cohort. Both experiments were conducted on freshmen very early in the term, and since the program has extremely competitive entry requirements, the pool of students always comprises the top echelon of Swedish students. Since we did not use students from Södertörn (Sn) this time around, we also checked for differences in effects between the two populations in the first study. The relative magnitude of the framing effects in Study 1 is as large at SSE as at Sn, as discussed in footnote 47, but the baseline level of cooperation is lower. Therefore, any difference in framing effects is unlikely to be caused by subject pool effects. Figure 2.7 displays the findings.

Figure 2.7. Fraction of Cooperative Actions in the Second Experiment, Treatments 1' and 2'.
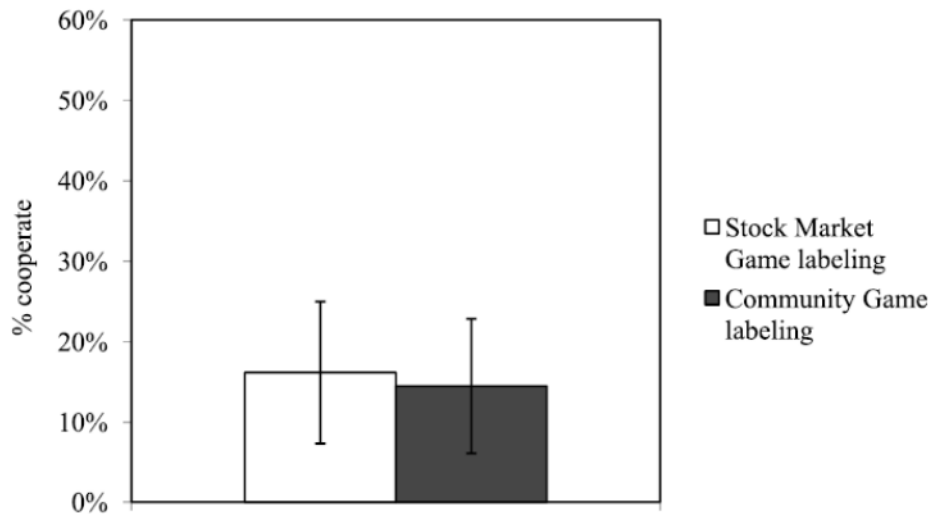
The difference between the two bars is the social framing effect in the symmetric information passive opponent condition. Error bars indicate 95% confidence intervals.

The difference in behavior across treatments is insignificant ($z = -0.271$, $p = 0.393$), and the point estimate has the wrong sign. Therefore, subject to the caveat that feelings of pride and shame could be stronger in the active opponent condition, we find no support for the hypothesis that the social framing effect in Study 1 was caused by social esteem considerations. That is, the evidence is unsupportive of Model (2).

2.5 The Third Study: Preferences or Coordination?

Only after conducting the first two studies did it occur to us that there is another straightforward way to distinguish between the variable sociality hypothesis and the coordination hypothesis, namely by letting the moves be sequential instead of simultaneous.[49] If a Stag hunt game is played sequentially, the second mover can always assure herself of playing a best reply, and

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[49] For a detailed study of behavior in sequential Prisoners' dilemmas, see Clark and Sefton (2001).

hence the efficient equilibrium is the unique subgame perfect outcome. Thus, if any version of the coordination hypothesis is correct, there should be no social framing effect.[50] On the other hand, if the variable sociality hypothesis is correct, there ought to be a social framing effect even in the sequential game. In particular, the second mover should be more willing to respond to C by playing C in the Community Game than in the Stock Market Game.

To investigate this issue, we conducted a third study with two treatments that we call 1'' (Stock Market Game) and 2'' (Community Game). These are similar to treatments 1 and 2, except moves are sequential instead of simultaneous. Moreover, in order to maximize statistical power we ask the second mover to report a contingent strategy; one choice in case the first mover plays C and one choice in case the first mover plays D. That is, we adopt the strategy method (Selten, 1967). While we recognize that the strategy method by itself could have a dampening effect on subjects' willingness to reciprocate (Casari and Cason, 2009), there is no immediate reason to expect that the strategy method should also affect the impact of social framing.

In total, 272 subjects participated as decision-makers in the experiment, which was conducted in September 2009. As in Study 2, all were freshmen enrolled in a basic microeconomics course at SSE. Although they come from a later cohort, we thus expect them to have similar characteristics to the SSE students of studies 1 and 2.

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[50] Observe that our argument pertains specifically to sequential stag hunt games, and not to sequential games in general. We do not know to what extent the argument generalizes. For example, we have not been able to ascertain whether the presence of a framing effect in the multi-round relative of the trust game (form) considered by Burnham, McCabe, and Smith (2000) is consistent with a coordination argument. Moreover, and perhaps more importantly, we think that equilibrium arguments, while always problematic in one-shot situations, are even less appropriate in complicated settings such as theirs.

Figure 2.8 displays the proportions of first-movers that choose to play C (i.e., to cooperate) under each of the two social frames in the third study. As expected, the level of cooperation is higher than in the case of simultaneous moves.[51] However, there is no significant social framing effect ($z = 0.316$, $p = 0.376$). Provided that the expectation about player 2's behavior is at least as optimistic under the Community Game frame as under the Stock Market Game frame, this evidence contradicts the variable sociality hypothesis.

Figure 2.8: Fraction of First-Mover Cooperation in the Third Experiment Treatments 1'' and 2''
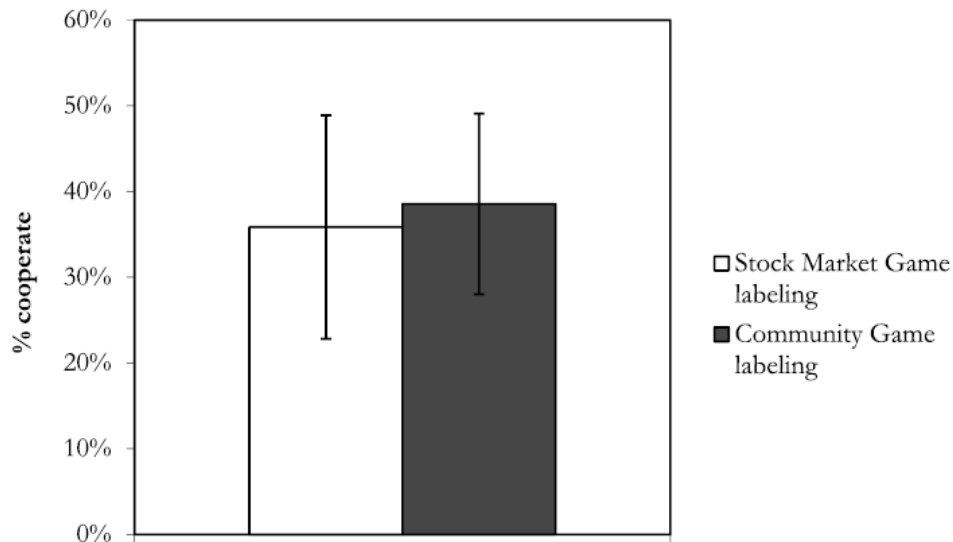
The difference between the two bars is the social framing effect for player 1 in the sequential moves condition. Error bars indicate 95% confidence intervals.

Since player 2 can condition the action on player 1's move, there is no role for beliefs when we interpret player 2's behavior. Player 2's action thus provides an even stronger test of the variable sociality hypothesis. Figure 2.9 displays the results. The first pair of bars denotes, for the Stock

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[51] Recall that the high average cooperation rates in T1 and T2 is driven by the Södertörn subjects. See also footnote 47.

Market Game and the Community Game respectively, the fraction of subjects in the role of player 2 that cooperate if player 1 cooperates. The second pair of bars gives the corresponding cooperation rates for the case in which player 1 defects.

Figure 2.9: Second-mover cooperation in the Third Experiment Treatments 1'' and 2''
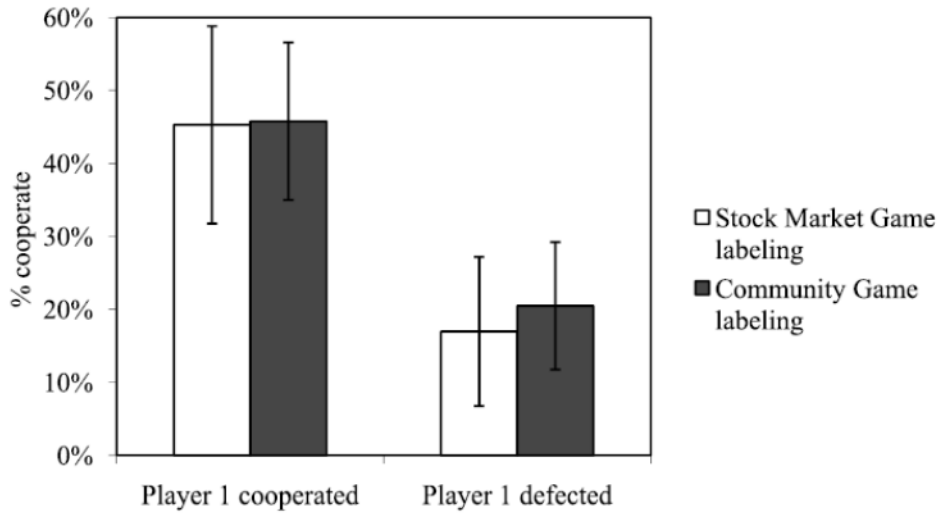
The difference between the first (second) two bars is the social framing effect for player 2, when player 1 cooperated (defected), in the sequential moves condition. Error bars indicate 95% confidence intervals.

While there is more cooperation in the Community Game than in the Stock Market game, the difference is minor and not statistically significant (conditional on player 1 cooperating, $z = 0.057$, $p = 0.477$; conditional on player 1 defecting, $z = 0.511$, $p = 0.305$).

Figure 2.10: Second-Mover Strategies in the Third Experiment Treatments 1'' and 2''
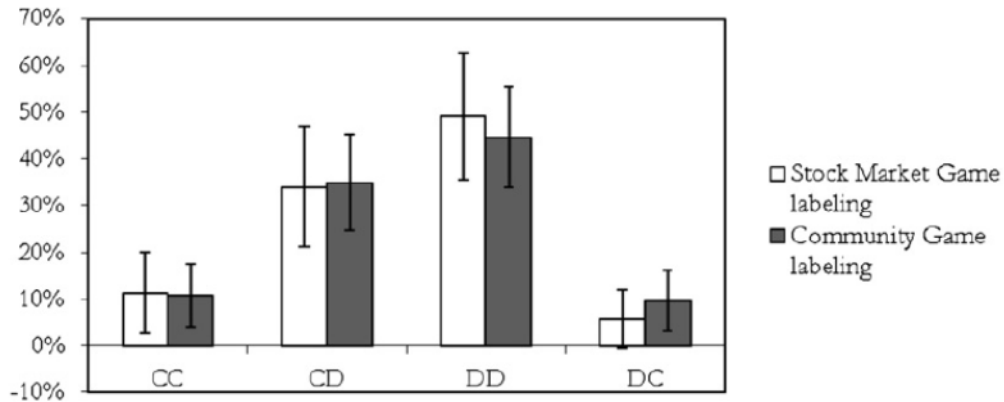
Each staple indicates the fraction of second movers that chose the strategy in the Stock Market frame (first staple in each pair) and the Community frame (second staple) respectively. Error bars indicate 95% confidence intervals.

The lack of significant differences is further emphasized in Figure 2.10, where we break down the observations further and consider all the four strategies that player 2 may use; *CC* denotes unconditional cooperation (i.e., always playing *C*), *CD* denotes conditional cooperation (play of *C* in response to *C* and *D* in response to *D*), *DD* denotes unconditional defection, and *DC* denotes defection in response to cooperation and cooperation in response to defection.[52] As the figure shows, the differences across the two treatments are minor. The null hypothesis that the two distributions are identical cannot be rejected (Pearson chi-square $= 0.789$, $p = 0.852$). Among players who cooperate after cooperation, there is no difference at all. Among players who defect after cooperation, cooperation after defection is somewhat more frequent in the Community treatment, but the effect is statistically insignificant ($p = 0.192$).

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[52] The strategy DC may seem unintuitive, but actually makes good sense for an unconditional altruist. If the opponent cooperates, own cooperation means giving up 30 in order to give 45. If the opponent defects, own cooperation means giving up 15 in order to give 60.

In our view, the last experiment contradicts the hypothesis that framing effects come exclusively through the preference channel, at least as we have modeled it.

However, we acknowledge the possibility that the insignificant aggregate effect at the second stage is due to interaction between several types of behavior, of which "variable sociality" is one.[53] For example, suppose that a fraction of the subjects are conditionally altruistic, with their degree of altruism depending on their beliefs about the opponent's altruism. Suppose that these subjects believe that the variable altruism may be applying to their opponent. Then, in the role of Player 2, they are more prone to reward cooperation under the Stock Market frame (when only the most altruistic opponent types will cooperate) than under the Community frame. Thus, the aggregate insensitivity of Player 2 behavior could be generated by a mix of such conditionally cooperative types and altruistic types responding to the frame. However, the problem with this argument is what it implies for Player 1 equilibrium behavior: In equilibrium, Player 1 behavior must differ across frames, with more cooperation under the Community frame, otherwise conditional altruists will not cooperate more under the Stock Market frame. The lack of a significant difference in Player 1 behavior thus speaks against this interpretation of Player 2 behavior.

We also acknowledge the possibility that the subject pool in 2009 was significantly different from the pool in 2006. While the measurable SSE student characteristics are not likely to have changed (the program is exceptionally popular and therefore attracts the top new students every year), it is quite possible that important events such as the financial crisis may have affected the students' sensitivity to social frames. If students have become less prone to behave selfishly un-

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[53] We are grateful to Jean Tirole and Dirk Engelmann for helpful discussions concerning the ensuing argument.

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der the stock market frame, for example due to the public criticism of greedy bankers, that might have eliminated frame-sensitive sociality in 2009 even if it was there in 2006.

2.6 Conclusion

Our first experiment demonstrates that situational labels significantly affect behavior in social dilemma situations even under the kind of experimental conditions conventionally imposed by behavioral economists. In this respect, we confirm previous findings that people cooperate more when the name of the game emphasizes the community rather than the individual's interest.

However, the presence of social framing effects does not prove that preferences depend directly on situational labels. Taken together, our three experiments – involving more than a thousand subjects altogether – instead suggest that social frames primarily serve as coordination devices. These findings are good news for economists, whose analysis often rest on the assumption that preferences are stable and reasonably simple, a methodological principle most stoutly defended by Becker and Stigler (1977). Conversely, the results suggest that some of the critics of economic theory, as exemplified by Ferraro, Pfeffer and Sutton (2005) and references therein, might shift their focus from economists' modeling of preferences to their modeling of beliefs.

Among the five explicit models that we consider, a version of the social preferences proposed by Charness and Rabin (2002) is the only model not to be severely questioned by our experimental data. That is, this formulation seems to be the best description of the game that, to our subjects, corresponds to the Prisoners' dilemma game form. Given such a frame-free description of the game, we conjecture that existing models of belief-based framing effects, notably the level-k

model of Bacharach and Stahl (2000), can be adapted to articulate more precisely how framing affect coordination. However, such a complete non-equilibrium analysis is beyond the scope of this chapter.

It is clearly desirable to investigate the robustness of our findings. For example, one might vary the social frame in other simple game forms, and study other subject pools and contexts. In on-going work, we are therefore measuring social framing effects in the Dictator game (form). If present, such effects would resuscitate the variable sociality hypothesis, since our preferred model leaves no room for coordination in a game with only a single active player.[54] However, in support of the present analysis, the data indicate no social framing effects in the Dictator game.

Although our study is large by current standards, our findings call for replication. Since inter-experiment comparisons are always less credible than intra-experiment comparisons, we would especially like to see a single study that directly compares the impact of social frames in simultaneous and sequential Prisoners' dilemmas.

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[54] Signaling models of Dictator game behavior, such as Andreoni and Bernheim (2009), do have multiple Perfect Bayesian Nash equilibria.

3. Testosterone Change following Monetary Wins and Losses Predicts Future Financial Risk-taking[55]

3.1 Introduction

National stock markets often suffer negative returns on days following a country's elimination from major sporting events (Edmans et al., 2007). This is a phenomenon not predicted by classical economic theory, which assumes investors to be rational, calculating and unaffected. Events such as this suggest that real decision-makers are subject to feelings, sentiments and sometimes error. Indeed, research over the last thirty years has documented numerous ways in which humans routinely deviate from rational behavior (Kahneman and Tversky, 1984; Loewenstein, 2000). While documenting these departures has been an important step in understanding economic and financial decision-making, explaining why these behavioral phenomena exist is now the current challenge. One candidate proximate mechanism mediating financial decision-making is the hormone testosterone (T). While trait level differences in T explain some of the observed individual differences in financial risk-taking between men (Apicella et al., 2008; Stanton et al., 2011) and may account for some of the observed sex differences in risk-taking (Sapienza et al., 2009), there is much reason to suspect that transient changes in T may also influence financial risk-taking and if so, these changes may help to explain documented behavioral phenomena such as the sensitivity of economic markets to sporting outcomes (Edmans et al., 2007).

While accumulating evidence over the past two decades has demonstrated widespread effects of circulating T on the social behavior of males in many species, including its effects on aggression

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(Archer, 2006), mate-seeking (Roney et al., 2003), dominance (Kemper, 1990; Mazur and Booth, 1998) and more recently, financial risk-taking (Apicella et al., 2008; Stanton et al., 2011) and financial profitability in humans (Coates and Herbert, 2008), it has also, conversely, demonstrated that men's T levels respond to their social milieu and status. For instance, T rises during brief interactions with attractive women (Ronay et al., 2003; Ronay and von Hippel, 2010) and with the anticipation of a social challenge or contest (Wingfield et al., 1990; Mazur and Booth, 1998). Also, winners of competitive challenges often experience a relative increase in T compared to losers; a differential response that primarily occurs in men (Bateup et al., 2002), and applies to physical competitions, such as wrestling (Elias, 1981) and tennis Booth et al., 1989), non-physical competitions, such as chess (Mazur et al., 1992), chance-based competitions, such as coin-tosses (McCaul et al., 1992) and political elections (Stanton et al., 2009; Apicella and Cesarini, 2011).

Plasticity in T has been interpreted as a means by which male organisms are able to adjust their behavior adaptively to a changing social environment (Oliveira, 2004). In humans, an increase in T following success in a cognitive spatial task against an opponent predicts men's willingness to participate in another competition against the same opponent (Mehta and Josephs, 2006). Furthermore, T changes following a competitive line tracing task also predict future aggressive behavior in men (Carré et al., 2009). And finally, increases in T following exposure to attractive women predicts male skateboarders' willingness to engage in challenging and physically risky stunts (Ronay and von Hippel, 2009). In short, transient increases in T are thought to enhance risky and competitive behaviors in men, precisely when those behaviors are likely to be profitable in the evolutionary sense.

While this behavioral flexibility to varying levels of T likely evolved because it, on average, benefited males, it is also possible that it could lead to irrational decision-making. This may be especially true in evolutionarily novel domains, such as financial decision-making or when changes in T resulting from one event influence the behavior of an organism in a separate and independent event. Indeed, if changes in T influence financial decision-making in men, this may help to explain a number of documented behavioral economic phenomena such as a) the house money effect (an increase in risk-seeking following a prior gain, Thaler and Johnson, 1990), b) increases in financial risk-taking by men following physical contact with a woman (Levav and Argo, 2010) and c) national stock market declines after a country's sports team experiences a defeat (Edmans et al., 2007).

While T changes at the level of the group may help explain group-level phenomena, we are specifically interested in whether individual differences in T reactivity are associated with financial risk-taking since there is much evidence that the magnitude of the T response in men does vary (Cohen et al., 1996; Pound et al., 2009). In this study, we investigate whether changes in T, following monetary wins and losses, influence subsequent financial risk-taking. Male participants played 15 rounds of a largely chance-based game (rock, paper, scissors) against one another for money. The pairs were assigned to one of two monetary treatments. Depending on the treatment, the pair of participants had either USD 5 or USD 15 placed in front of them. Each participant had the same amount of money in front of them as their opponent and in both conditions the participants could either win USD 5 from their opponent or lose USD 5 to their opponent. We specifically designed the study such that the winners who ended up with USD 10 could be directly compared to the losers who ended up with same amount, given the distinct possibility that earn-

ings could affect their risk-aversion. Thus we compare the winners from the USD 5 condition to the losers from USD 15 condition since both groups would have the same resulting income of USD 10 ($n = 49$). The participant who won the most rounds of the game was declared the winner. About twenty minutes following the competition, we measured participants risk-aversion as their willingness to accept a lottery over a certain amount in a series of incentivized choices. This is a common method used to elicit risk-aversion, and it has been showed to correlate with risk-taking behavior outside the lab (Dohmen et al., 2011). To assess change in T, a first saliva sample was collected from participants immediately upon arrival to the study site. A second saliva sample was collected approximately 30 minutes following the competition.

3.2 Methods

3.2.1 Subjects

A total of 114 men participated in the experiment which was conducted at the Harvard Decision Science Laboratory during five days in late November and early December 2011. Of those, 16 men were excluded from the analysis because they reported taking psychotropic or steroid medications. Exclusion of these participants is standard procedure due to the possibility that these medications could affect their T levels and/or behavior. This exclusion criterion was determined prior to running any of the analyses. In total, 98 men participated in the study and 49 ended up with USD10 after having won or lost the competition. Each participant was only allowed to participate once. The average participant was 33 years old. A total of 19 experimental sessions with six participants each were conducted. Sessions were conducted throughout the day (while T does display significant diurnal variation we were interested in T change rather than absolute levels and were hence not confined to conduct sessions at a specific time of day for all participants).

Sessions lasted approximately 45 minutes and participants earned on average USD 25.6, including a show-up fee of USD 10.

3.2.2 Procedures

After having signed the consent forms, participants were seated in the lab. Before any other instructions were given, a first sample of saliva was collected from each participant in a 2 ml cryovial. The second saliva sample was collected about 30 minutes after the competition. The cryovials were frozen immediately after collection and the samples were sent to Salimetrics for analysis.

A filler task containing a non-incentivized price estimation task was conducted immediately following the first sampling of saliva. Thereafter participants were informed about the rules and procedures for the Rock, Paper and Scissors competition. Pairs of two participants were randomly formed and each pair was taken to an adjacent room by one of the three female experimenters. In this room the two contestants were seated at opposite sides of the table and each given three laminated cards with the symbols of Rock, Paper, and Scissors respectively. Participants were again informed of the rules (rock beats scissors, scissors beats paper and paper beats rock) and were told that they would play 15 rounds against each other by choosing one card each round. The person who won the majority of rounds would win the competition and get USD 5 from the loser. Before the start of the competition both participants had a certain amount of dollars at the table in front of them (either USD 5 or USD 15 depending on the treatment). As noted above, we specifically designed the study such that the 49 winners who ended up with USD 10

could be compared to the 49 losers who ended up with same amount, given the distinct possibility that earnings could affect their risk-aversion.

During the competition, the experimenter kept score on a small whiteboard. After the 15 rounds were conducted, the experimenter announced the winner and took five dollar bills from the loser and gave to the winner. If there was a tie at this point more rounds were played until a winner could be declared. The competition lasted for 5-10 minutes and thereafter participants were taken back to the computer lab where they were given five minutes to write about their experiences in the competition. The reason for this writing exercise was to provide participants the opportunity to reflect on the competition and the outcome. The participants were then told that the remainder of the study would consist of incentivized choices. Here risk attitudes were elicited with participants making ten computerized choices between a certain amount and a lottery. The latter was a coin toss with a 50 percent chance of winning USD 10. The certain amount varied from USD 1 to USD 10 and the number of times that a participant chose the certain payoff was used as the measure of risk aversion. This results in a risk preference score between 0 and 10. An average of 5.5 indicates risk neutrality. At the end of the experiment participants were paid according to the outcome of one of their ten decisions. The decision that was realized was selected at random.

After having been paid their earnings from the experiment in private, participants filled out a questionnaire that among other things contained questions about age, sexuality, relationship status and potential use of psychotropic or steroid medications.

3.3 Results

Table 3.1 provides the summary statistics for the whole sample of 49 men and separately by winners and losers.

Table 3.1: Summary Statistics.

	All Men	Winners	Losers
Baseline T	M=106.14 s.d.=41.84 N=49	M=102.51 s.d.=30.50 N=25	M=109.92 s.d.=51.5 N=24
Change in T	M=27.82 s.d.=41.52 N=49	M=33.00 s.d.=29.42 N=25	M=22.43 s.d.=52.33 N=24
Year of Birth	M=1976 s.d.=14.90 N=45	M=1976 s.d.14.63 N=25	M=1977 s.d.=15.49 N=24
Heterosexual (0=no;1=yes)	M=0.84 s.d.=0.37 N=43	M=0.86 s.d.=0.36 N=21	M=0.82 s.d.=0.39 N=22
Risk Aversion Score	M=6.29 s.d.=1.84 N=49	M=6.00 s.d.=1.5 N=25	M=6.58 s.d=2.12 N=24

As indicated, baseline T does not differ between winners and losers (t-test $p = 0.5407$). There was an overall rise in T in 73 percent of the participants from before the competition to after the competition. This result is not surprising since increases in T, in response to a challenge, have been documented (Wingfield et al., 1990; Wobber et al., 2010), though this is the first time it has been shown for a financial challenge. The change in T is also associated with the outcome of the competition though as expected from previous studies (Pound et al., 2009; Levav and Argo, 2010) there is substantial inter-individual variation in the change within both the winners and losers. On average, winning men's T levels increased 12 percent more than losing men (see Table 3.1) and for men who won by a tighter margin the increase was even greater. That is, the closer the final scores were between the participants or the more fierce the competition, the greater the increase. In a regression analysis where we control for the difference in score between the player

and his opponent, winning leads to a significantly greater increase in T compared to losing ($coeff = 44.92$, $p = 0.020$).

Changes in T significantly predict risk aversion in our sample of men (see Table 3.2). That is, men who experience an increase in T are less risk-averse ($coeff = -0.015$, $p = 0.009$). To isolate the importance of changes in T on risk-aversion, we control for the binary winner variable when examining the relationship between T change and risk aversion. We still find that the greater the T increase, the lower the risk aversion ($coeff = -0.014$, $p = 0.020$). That is, men who experienced a greater increase in T were less risk-averse (Figure 3.1). In fact, whether participants won or lost in this model does not predict risk-aversion ($coeff = -0.44$, $p = 0.404$).

Table 3.2: Summary Statistics.

DV: Risk aversion

	(1)	(2)	(3)	(4)
$\triangle T$	-0.015***	-0.014**	-0.016**	-0.021**
	(0.005)	(0.006)	(0.007)	(0.008)
Winner		-0.44	0.29	0.75
		(0.52)	(0.87)	(0.83)
Scorediff			-0.12	-0.15
			(0.11)	(0.11)
Controls	No	No	No	Yes
N	49	49	49	43
R2	0.11	0.12	0.14	0.41

*Ordinary least squares regressions of risk aversion on testosterone change, whether the participant won or lost, score difference between participant and opponent and controls (age, sexual orientation and time of day). Robust standard errors in parentheses. Levels of significance: * p < 0.1, ** p <0.05, *** p < 0.01.*

This provides evidence that it is changes in T that influence risk-taking in men and not the act of winning or losing itself or other biological mechanisms that are orthogonal to T.

The result is also robust when we control for both the winner variable and the difference in score between the participant and his opponent (*coeff* = -0.016, *p* = 0.022), and when we include additional control variables such as year of birth, time of the day for data collection, and sexual orientation (variables that might correlate with the change in T or risk-aversion) to the previous regression (*coeff* = -0.021, *p* = 0.011).

Figure 3.1: Testosterone Change by Risk Aversion.
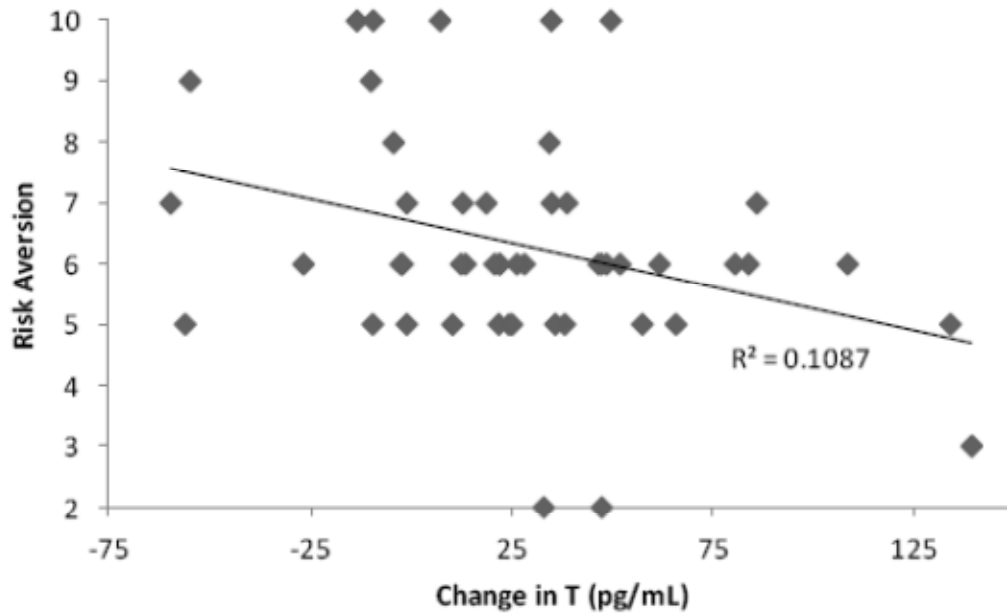
N = 49.

On average, participants made choices exhibiting more risk aversion than the risk neutral, profit maximizing choices. However, participants in the quintile with the largest T increase experienced a reduction in risk aversion leading to decisions that were in fact, risk neutral, and hence profit-maximizing. On the other hand, participants in the quintile with the largest T decrease had a risk aversion score almost 40 percent higher than the risk neutral score (see Figure 3.2). Taken

together, our results provide strong evidence that T reactivity to competitive outcomes influences risk-aversion in men in ways that influence their financial payoffs.

Figure 3.2: Quintile of Testosterone Change by Risk Aversion.
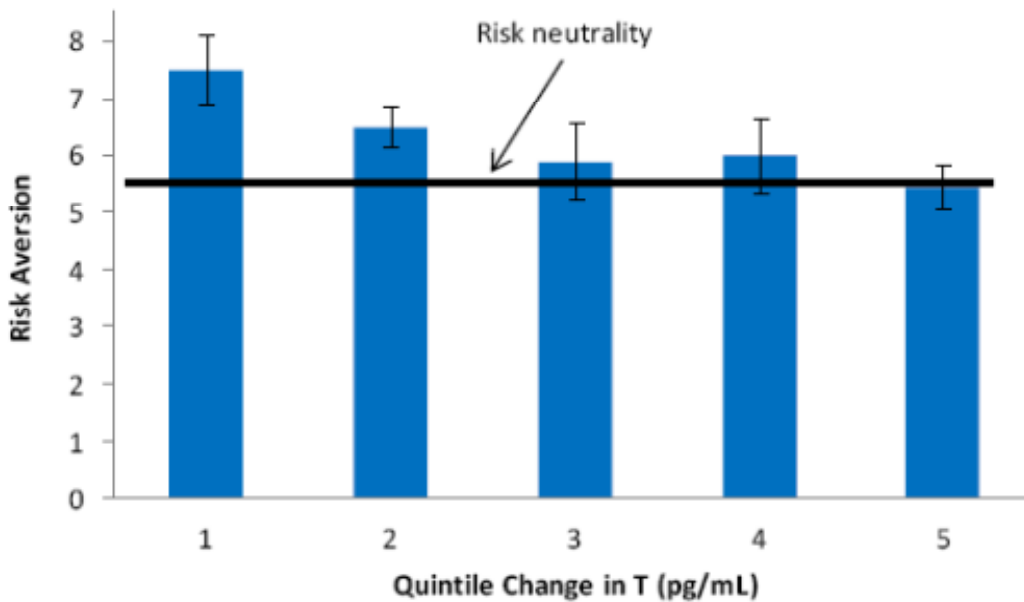
Line represents risk neutrality.

While rock, paper, scissors is a largely chance-based game and it is unlikely from our pool of subjects that there were sophisticated players, it should still be noted that winning or losing may not be exactly random and that factors associated with the T response may be correlated with the skill of the game. While not explicitly asked, many of the participants reported that they either did not have a strategy or that they perceived the game to be chance-based. To test whether our winners were more likely to win compared to chance expectations we ran 1000 simulations of our exact rock, paper, scissors game but where the outcomes were based on chance. We found no evidence that the number of rounds of rock, paper, scissors that was won by the winners was greater than what would be expected by chance. The winners in our random simulation won by

the same margin ($M = 2.62$, $sd = 1.84$) as the real winners in our game ($M = 2.80$, $sd = 2.25$), suggesting that winners and losers were randomly assigned (*95 percent CI*: 2.38-2.90).

3.4 Discussion

There is ample heterogeneity in economic preferences both within and between individuals - an observation that has motivated a large body of research to try to explain why these differences exist. Despite considerable efforts, individual differences in economic decision-making remain largely unexplained (Camerer, 2003). One possible reason is that traditional attempts to understand economic behavior have been made under the assumption that preferences are stable within individuals (Stigler and Becker, 1977). While relatively stable individual differences do exist, it is clear that preferences are flexible with the most extreme occurrence being preference reversal (Tversky and Thaler, 1990). The failure to find robust correlates to variation in economics preferences suggests that other factors, largely independent of socio-demographics, such as biological, psychological and situational sources, as well as their interactions, account for some of the variation between and within individuals. Here we demonstrate that individual differences in T reactivity in response to a situational event is associated with financial risk-taking.

The extent to which biological factors, such as T, mediate differences in economic decision-making, is a current source of investigation by many researchers (Burnham, 2007; Apicella et al., 2008; Cesarini et al., 2009; Eisenegger et al., 2009; Bos et al., 2010; Stanton et al., 2011). While some previous research suggests that baseline differences in T may account for differences in risk preferences between men (Apicella et al., 2008; Stanton et al., 2011), and that male traders' profits increase on days when their T is highest (Coates and Herbert, 2008) our findings are the

first to directly link transient changes in T to financial risk in men. Specifically, we demonstrate that changes in T, following monetary wins and losses, predict men's risk-aversion. By controlling for income effects in our design and whether or not men won or lost in our analyses, we can be confident that it is changes in T that mediate men's risk preferences.

While our sample size is small, it is comparable, if not larger, than samples found in many hormonal studies examining changes in T within individuals (Elias, 1981; Roney et al., 2003; Mehta and Josephs, 2006; Coates and Herbert, 2008; Bos et al., 2010; Apicella and Cesarini, 2011). Our findings are also consistent with the non-human animal literature where males across a range of species engage in risky behaviors in response to increases in T, particularly during the breeding season (Wingfield et al., 1990). Short-term increases in T may be designed to increase risk in males precisely when risky behaviors could lead to a good outcome and possibly greater reproductive success. As an example, mating calls by males are risky because they open males up to predation, but they also attract more females. As such, increased risk is not linked to greater reproductive success in females as it is in males. Females also have less androgen-responsive neurons, rendering them less responsive to the behavioral influences of T (Wood and Newman, 1999). Indeed, the only other study to directly examine the influence of increased T (administered exogenously), on risk preferences was conducted on women and did not find an effect Zethraeus et al., 2009).

The significance of our results not only depend on how closely tied men's risk preferences are to changes in their T levels, but also to how readily changeable T is within men. If T were stable in men then our findings would bear little significance. But T levels are not static – in fact they vary

in predictable ways. Changes in T have been linked to diet (Hämäläinen et al., 1984), exercise (Weiss et al., 1983), age (Kaufman and Vermeulen, 2005), mate seeking (Roney et al., 2003; Roney et al., 2007), competitive challenges (Wingfield et al., 1990; Mazur and Booth, 1998), victories and defeats (Elias, 1981; Booth et al., 1989; Mazur et al., 1992; McCaul et al., 1992; Stanton et al., 2009; Apicella and Cesarini, 2011) as well as major life events such as fatherhood and marriage (Gray et al., 2002; Burnham et al., 2003; Gettler et al., 2011). Likewise, many of these events have also been linked to changes in risk-taking in ways that are consistent with what we would expect if the phenomena were mediated by changes in T.

To the extent to which we can predict events that influence T levels in individuals and in groups, we would be in a better position to predict the behavior of not only individual investors but ultimately, market economies. Many instances of collective phenomena, such as crowd effects or herd behavior have been documented in financial markets. It is possible that changes in T at the group level may be a proximate cause of some of these observations. Likewise, T may impact economic behavior at various stages in the business cycle. If the fact that people experience more frequent financial gains in an upturn, and more frequent financial losses in a downturn, and this in turn influences T and thereby risk aversion, we could observe T propagating the business cycle. We look forward to future research investigating these possibilities further.

A: Appendices to Chapter 1

Appendix A.1: Experimental Instructions

[Screen 1:]
Hi and welcome! In this study you can earn some money. The amount will depend on your decisions and the decisions of the other participants. The study has one introductory part and several parts where you can earn money. At the end of the study, your earnings (10 points correspond to $3) will be added to the show-up fee of $10 and you will be paid in private, in cash before you leave. We will go through the instructions now. If you have any questions after you have read and heard the instructions, please press the help button or raise your hand. Otherwise, no communication is permitted during the study. You are also not allowed to use mobile phones or other electronic devices.

[Shown in treatment 1, 1b, 1c and 3] There are 16 people, including you, participating in this study at the same time as you. You are all in this room. The 16 of you are divided into two colors: orange and purple. There are 12 orange people and 4 purple people. On the next screen you will learn which color you have.

[Shown in treatment 2 and 4] There are 16 people, including you, participating in this study at the same time as you. You are all in this room. The 16 of you are divided into two colors: orange and purple. There are 8 orange people and 8 purple people. On the next screen you will learn which color you have.

[Screen 2:]
Your color is: [ORANGE/PURPLE]. There is a total of [12/4/8/8] [ORANGE/PURPLE] people, including you. The other color is [PURPLE/ORANGE]. There are [4/12/8/8] [purple/orange] people. Please do not press the OK button until we have given you a wristband with your color.

[Screen 3:]
Use the paper and pen that are available on your desk. Write down a list of 5 associations to your color, which is [ORANGE/PURPLE]. You have a total of 2 minutes available for this task. When you have finished, press the OK button. *[Papers collected after 2 minutes and a paper with key info about next part given out.]*

[Screen 4:]
[Shown in treatment 1, 1b, 2, 3 and 4] We now move on to the first part of the study where you can earn money. In this part of the study you are asked to correctly solve as many math problems as possible. You have five minutes available. In each problem, you are asked to sum up five two-digit numbers. An example could be 32+97+13+62+20. In this case the correct answer is 224. For each correct answer you will receive 1 point. At the end of the study you will learn how many of your answers were correct and how many points you earned. This will then be converted to dollars and paid out in cash. Please make sure to stop solving and press the OK button when we tell you that the time limit is up. Failing to do so can negatively affect your payoffs. If

you have finished all the tasks before the time limit is up, press ok and then wait until the study continues.

[Shown in treatment 1c] We now move on to the first part of the study where you can earn money. In this part of the study, the first part, you are asked to correctly solve as many math problems as possible. You have five minutes available. In each problem, you are asked to sum up five numbers. The math task can be either easy or hard. If it is easy, the numbers have two digits. An example could then be 32+97+13+62+20. In this case the correct answer is 224. If it is hard, the numbers have three digits. An example could then be 223+761+130+409+901. In this case the correct answer is 2424. Regardless of whether the math task is easy or hard, you receive 1 point for each correct answer. The orange players do the easy math task and the purple players do the hard math task. At the end of the study you will learn how many of your answers were correct and how many points you earned. This will then be converted to dollars and paid out in cash. Please make sure to STOP solving and press the OK button when we tell you that the time limit is up. Failing to do so can negatively affect your payoffs. If you have finished all the tasks before the time limit is up, press ok and then wait until the study continues.

[Screen 5:]
We will now check that everyone has understood the instructions for part 1 correctly! Please answer the questions on this screen. If you need help, please press the help button or raise your hand. When you have finished answering, please press "I understand". If any of your answers are incorrect, the program will tell you so and you get to answer that question again. *[Quiz is given.]*

[Screen 6:]
The math solving task will start in a few moments.

[Screen 7:]
Add up the five numbers in each row and write the sum in the box labeled "total". Please make sure to STOP solving and press the OK button when we tell you that the time limit is up. *[Math tasks on screen.]*

[Screen 8:]
It has now been determined how many of your answers were correct. At the end of the study, you will learn how many correct answers you gave and the money you earned will be given to you in cash. We now move on to part 2 of the study where you can earn more money. Please do not press OK until you have received the paper with the key information about part 2. *[Paper with key info about part 2 handed out].*

[Screen 9:]
We now move on to part 2 of the study. In this part two different groups will be formed. 8 out of the 16 people in this room will be members of a HIGH-STAKE GROUP. The other 8 will remain in the study as members of the REGULAR-STAKE GROUP. The people who are selected for the high-stake group will have the chance to earn more money than those who are in the regular-stake group.

[Shown in treatment 1b:] The payoff rule in part 2 is as follows: If the high-stake group consists of an equal number of purple and orange participants, the payoffs in the high-stake group will be

twice as large as in the regular-stake group. If the high-stake group does not consist of an equal number of purple and orange participants, the payoffs in the high-stake group will be the same as in the regular-stake group.

[Shown in treatment 1, 1b, and 1c:] The 8 high-stake group members will consist of 4 orange and 4 purple players. Since there are only 4 purple participants, the 4 purple high-stake members are simply these 4. The 4 orange high-stake members are the 4 orange participants, out of the totally 12 orange participants, who solved most math problems correctly in part 1.

[Shown in treatment 3:] The 8 high-stake group members will consist of 4 orange and 4 purple players. Since there are only 4 purple participants, the 4 purple high-stake members are simply these 4. The 4 orange high-stake members are 4 randomly chosen orange participants, out of the totally 12 orange participants.

[Shown in treatment 1b:] The reason that all the purple participants are selected as members of the high-stake group is that this group then has an equal number of orange and purple participants. Thereby the payoffs for all high-stake group members are doubled.

[Shown in treatment 1c:] The reason that all the purple participants are selected as members of the high-stake group is that they thereby are compensated for the fact that their math task was harder in Part 1.

[Shown in treatment 2:] The 8 high-stake group members will consist of 4 orange and 4 purple players. The 4 purple high-stake members are the 4 purple participants, out of the totally 8 purple participants, who solved most math problems correctly in part 1. The 4 orange high-stake members are the 4 orange participants, out of the totally 8 orange participants, who solved most math problems correctly in part 1.

[Shown in treatment 4:] The 8 high-stake group members will consist of 4 orange and 4 purple players. The 4 purple high-stake members are 4 randomly chosen purple participants, out of the totally 8 purple participants. The 4 orange high-stake members are 4 randomly chosen orange participants, out of the totally 8 orange participants.

You will shortly be informed about whether you have been selected as a member of the high-stake group or whether you remain in the study as a member of the regular-stake group. After the high-stake group has been formed, everyone will play a game. The game will be identical for everyone, but the people in the high-stake group will have the chance to earn more money. We will go through the instructions for the game in part 2 now. Please press the OK button.

[Screen 10:]
THE GAME: This game is played in pairs so you will play with one person at a time. Every person will play the game seven times with seven different people. At the end of the study, you will get paid according to the outcome of one randomly chosen game out of the seven. What you earn in that game will be converted to dollars and paid out in cash together with your other earnings. In this game both people in the pair start with an endowment of a certain number of points. Your task is to choose how much of your endowment to keep and how much to contribute to a project. The sum of the points that you, and the person you are playing with, contribute to the project will

be multiplied by 1.5. The resulting number of points will then be divided equally between the two of you. Your earnings will hence be whatever you keep plus your share of the payoff from the project.

There are two differences between the high-stake group and the regular-stake group. 1. Everyone will only play with members of their own group. I.e. members of the regular-stake group will play with each other and the members of the high-stake group will play with each other. 2. The endowment is 10 points in the regular-stake group and 20 points in the high-stake group. I.e. members of the high-stake group have the chance to earn more money. Let's now look at two examples.

EXAMPLE 1: A game in the regular-stake group. Imagine that you are a player in the regular-stake group and hence you play with another member of the regular-stake group. You both have an endowment of 10 points. You contribute 4 points to the project and the person you are playing with contributes 6 points. The sum of the contributions is then 4+6=10 points. The final payoff from the project will be 10*1.5=15 points. This will be divided between you and the person you are paired with so that you both get 15/2=7.5 points from the project. Since you kept 6 points out of your original 10, you will end up with 6+7.5=13.5 points from this game. The other person kept 4 points and will get 4+7.5=11.5 points.

EXAMPLE 2: A game in the high-stake group. Imagine that you are a player in the high-stake group and hence you play with another member of the high-stake group. You both have an endowment of 20 points. You contribute 12 points to the project and the person you are playing with contributes 8 points. The sum of the contributions is then 12+8=20 points. The final payoff from the project will be 20*1.5=30 points. This will be divided between you and the person you are paired with so that you both get 30/2=15 points from the project. Since you kept 8 points out of your original 20, you will end up with 8+15=23 points from this game. The other person kept 12 points and will get 12+15=27 points. Please click OK.

[Screen 11:]
We will now check that everyone has understood the instructions for part 2 correctly! Please answer the questions on the screen. If you need help, please press the help button or raise your hand. When you have finished answering, please press "I understand". After everyone has finished answering the questions below, we will announce if you have been selected for the high-stake group or not. *[Quiz is given.]*

[Screen 12:]
[Shown to those selected for the high-stake group:] You have been selected as a member of the high-stake group. You will now play the game with each of the other seven members of the high-stake group.

[Shown to those not selected for the high-stake group:] You have not been selected as a member of the high-stake group. Hence you remain in the study as a member of the regular-stake group. You will now play the game with each of the other seven members of the regular-stake group.

[Screen 13:]

[Shown to those in high-stake group:] You are a member of the high-stake group and you will play the game with each of the other 7 members of the high-stake group. All 7 games are carried out at the same time. The game is played anonymously and the only thing you know about the person you play a certain game with is that person's color. In each game you have 20 points. You have to decide how many points to keep and how many to contribute to the project. When you have made your choice in all games, please press OK.

[Shown to those in regular-stake group:] You are a member of the regular-stake group and you will play the game with each of the other 7 members of the regular-stake group. All 7 games are carried out at the same time. The game is played anonymously and the only thing you know about the person you play a certain game with is that person's color. In each game you have 10 points. You have to decide how many points to keep and how many to contribute to the project. When you have made your choice in all games, please press OK.

[Screen 14:]

All 7 games have now been conducted and one game has been chosen randomly for payment. You will learn the outcome at the end of the study, just before your earnings are paid out in cash. We now move on to part 3, which is the last part of the study. Please press OK to proceed to part 3.

[Screen 15 to end:]

While we prepare your payments, please answer a few questions.
[Questions about fairness perception, gender, age, ethnicity, etc given]

Appendix A.2: Orange and Purples

Figure A.1: Percentage Female, Orange and Purple
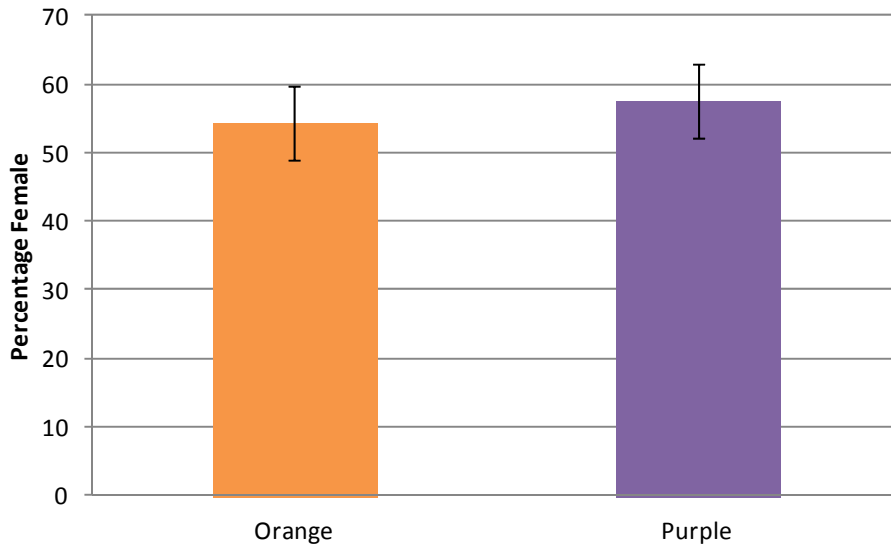
Error bars mark 95% confidence interval for t-test. No difference between colors (p>0.1 in two-sample t-test with equal variances). Number of participants: 352.

Figure A.2: Mean Age, Orange and Purple
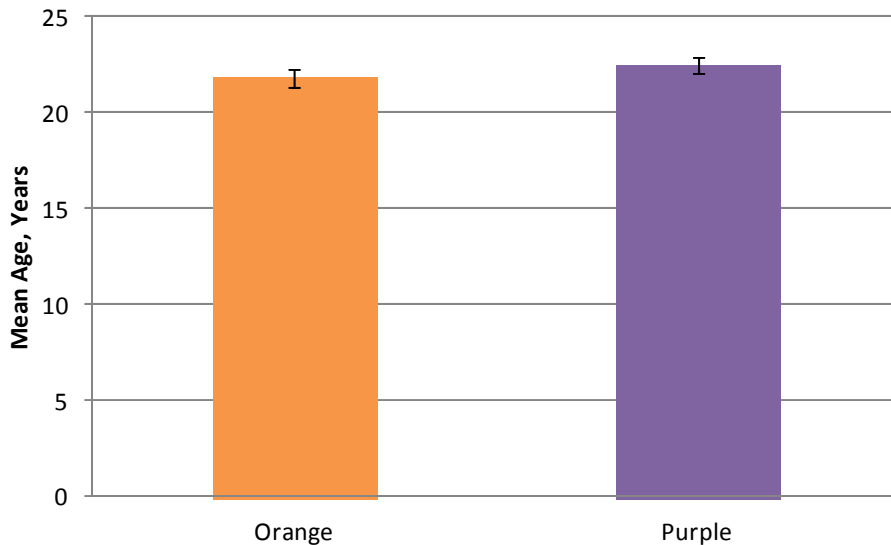
Error bars mark 95% confidence interval for t-test. No difference between colors (p>0.1 in two-sample t-test with equal variances). Number of participants: 350.

Figure A.3: Percentage Non-White, Orange and Purple
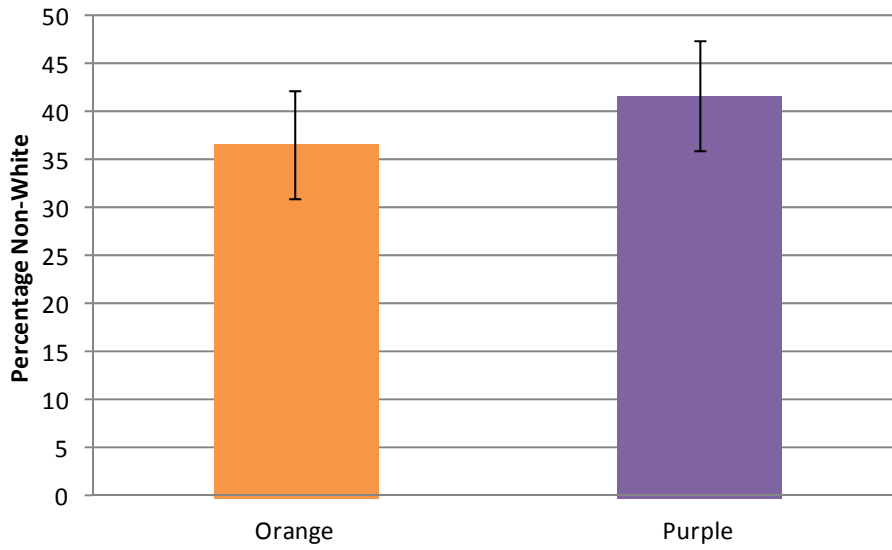
Error bars mark 95% confidence interval for t-test. No difference between colors (p>0.1 in two-sample t-test with equal variances). Number of participants: 313.

Appendix A.3: Math Task Performance

Figure A.4: Distribution of Math Task Performance
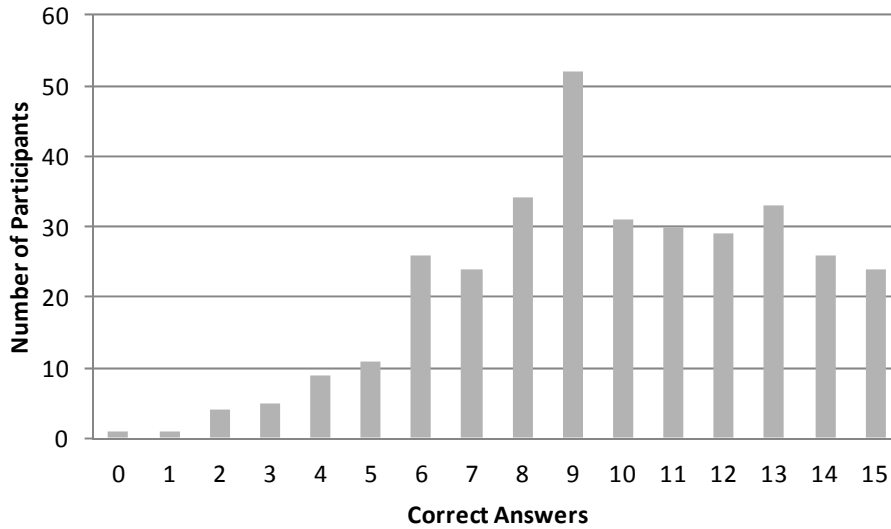
Number of participants with specific number of correct answers in the math task. Excludes the participants in treatment 1c who added up three-digit numbers. Number of participants: 340.

Table A.1: Correlates of Math Task Performance

	(1)	(2)	(3)	(4)
Female	-0.70**			-0.76**
	(0.34)			(0.34)
Age		-0.10**		-0.10**
		(0.04)		(0.04)
Non-white			0.32	0.37
			(0.34)	(0.34)
Three-digits	-2.55***	-2.42**	-2.48***	-2.42***
	(0.93)	(0.93)	(0.94)	(0.93)
Constant	10.15***	12.08***	9.26***	12.26***
	(0.26)	(0.98)	(0.23)	(1.02)
N	352	352	352	352

*OLS. Robust standard errors in parentheses. Levels of significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Dependent variable: Number of correct math tasks. Female: dummy equal to 1 if female. Age: age in years. Non-white: dummy equal to 1 if participant has ethnicity other than white. Three-digits: dummy equal to 1 if participant solved math task with three digits (true for N=12 in treatment 1b).*

Appendix A.4: Sessions Fall 2011 and Spring 2012

Figure A.5: Results of treatments 1 and 2: fall 2011, spring 2012 and pooled
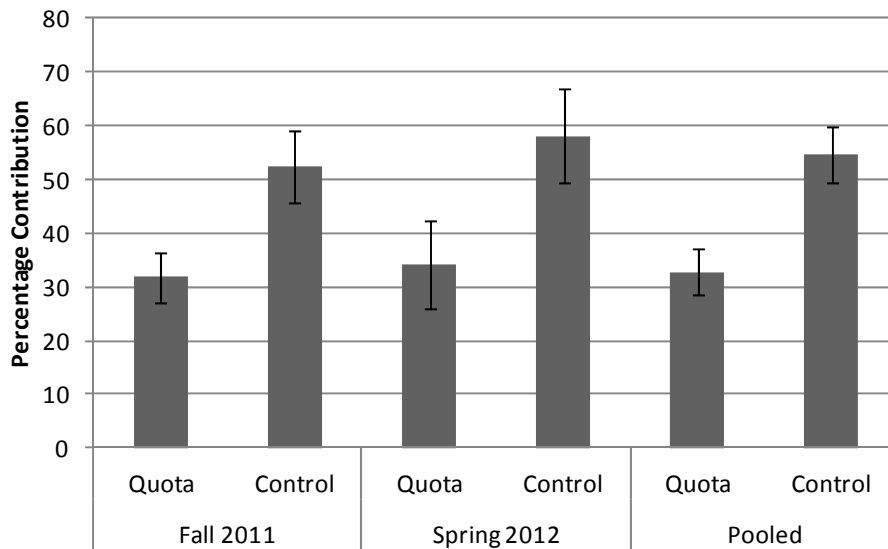
Error bars mark 95% confidence interval for Wald test with se clustered on participant and session. 2011: Number of observations: 336, number of participants: 48, number of sessions: 6. Spring 2012: Number of observations: 224, number of participants: 32, number of sessions: 4.

Table A.2: Regression results of treatments 1 and 2: fall 2011, spring 2012 and pooled

	(1)	(2)	(3)	(4)
Quota	-0.21***	-0.24***	-0.22***	-0.21***
	(0.04)	(0.07)	(0.04)	(0.04)
Spring				0.06
				(0.06)
Quota*Spring				-0.03
				(0.7)
Constant	0.52***	0.58***	0.55***	0.52***
	(0.02)	(0.06)	(0.03)	(0.02)
Multicluster	Yes	Yes	Yes	Yes
Obs (N)	336	224	560	560
Cluster (ind)	48	32	80	80
Cluster (session)	6	4	10	10

*Dependent variable: Percentage cooperation .OLS. Levels of significance: * p<0.1, ** p<0.05, *** p<0.01. Robust standard error in parentheses. Clustered as indicated in table. (1): Only data from fall 2011. (2): Only data from spring 2012. (3) and (4): Pooled data*

Appendix A.5: Robustness Checks

Order Effects

It can be hypothesized that the size of the ingroup favoritism is suppressed as a consequence of how the seven public good games are displayed. The fact that players make all decisions simultaneously may create an experimented demand effect for treating subjects of different colors equal. If this would be the case, we could expect subjects whose first game is against someone of the same color to have a higher overall cooperation level than subjects whose first game is against someone of the other color. This is under the assumption that subjects make a decision which is unaffected (or at least less affected) by the abovementioned demand effect in the first game and thereafter anchor decisions on their own choice in that game. The table below tests for such order effects and finds no evidence for their existence in the data.

Table A.3: Cooperation controlling for color of opponent in first game

	(1)	(2)	(3)	(4)
Same_color_first	0.01	0.04	0.02	0.01
	(0.09)	(0.06)	(0.05)	(0.09)
Quota				-0.24***
				(0.04)
Quota*Same_color_first				0.03
				(0.1)
Constant	0.54***	0.31***	0.42***	0.54***
	(0.04)	(0.01)	(0.04)	(0.04)
Multicluster	Yes	Yes	Yes	Yes
Obs (N)	280	280	560	560
Cluster (ind)	40	40	80	80
Cluster (session)	5	5	10	10

*Dependent variable: Percentage cooperation. OLS. Levels of significance: * p<0.1, ** p<0.05, *** p<0.01. Robust standard error in parentheses. Clustered as indicated in table. (1): Only data from control treatment (T2). (2): Only data from quota treatment (T1). (3) and (4): Data from both treatments.*

Standard Error Clustering and Distributional Assumptions

The data can be analyzed in different ways with regards to how standard errors are clustered and which distributional assumptions are made, without the results being affected. The table below shows five methods of analyzing the data, using the main result reported in Figure 1 as an illustrative example. Results reported in the paper are from method (3) but all conclusions drawn are robust to the use of either method (comparable tables with the results for all analysisin Section 3 is available from the author).

Table A.4: Cooperation controlling for color of opponent in first game

	(1)	(2)	(3)	(4)	(5)
Quota	-0.2194643***	-0.2194643***	-0.2194643***	-0.2194643***	-0.2194643***
	(0.0673486)	(0.0363049)	(0.0363049)	(0.0677183)	(0.0365042)
N	560	560	560	80	80
Cluster individual	Yes	No	Yes	No	No
Cluster session	No	Yes	Yes	No	Yes
Average obs per ind	No	No	No	Yes	Yes

Dependent variable: Percentage cooperation. OLS. Levels of significance: * p<0.1, ** p<0.05, *** p<0.01. Robust standard error in parentheses. Clustered as indicated in table. (1): Only data from control treatment (T2). (2): Only data from quota treatment (T1). (3) and (4): Data from both treatments.

Results are also robust to the use of non-parametric tests, such as the Mann-Whitney test. For the hypothesis tested in figure 1, the Mann-Whitney test yields z=-2.797 and prob>|z|= 0.005 if behavioral observations are averaged for each individual and z=-2.611 and prob>|z|= 0.009, if they are averaged for each session. Not only this result, but also all other results reported in the paper, are robust to using this non-parametric test instead of the t- test. Results of Mann-Whitney tests for the complete analysis in Section 3 are available from the author.

Appendix A.6: Regression Tables

Table A.5: Regressions Treatment 1 and 2, part 1

Dependent variable: Percentage contribution in PG-game

	All HS	Orange HS	Purple HS	To same HS	To diff HS	O to O HS	O to P HS	P to P HS	P to O HS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Quota	-0.2194643***	-0.2353571***	-0.2035714**	-0.1954167***	-0.2375***	-0.2283333***	-0.240625***	-0.1625*	-0.234375***
	(0.0363049)	(0.0394357)	(0.0799323)	(0.0475174)	(0.0313234)	(0.0375582)	(0.0466552)	(0.0869258)	(0.0828503)
Multi-cluster	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs (N)	560	280	280	240	320	120	160	120	160
Cluster (ind)	80	40	40	80	80	40	40	40	40
Cluster (sessions)	10	10	10	10	10	10	10	10	10

*OLS. Levels of significance: * p<0.1, ** p<0.05, *** p<0.01. Robust standard error in parentheses. Cluster as indicated in table.*

Table A.6: Regressions Treatment 1 and 2, part 1

Dependent variable: Percentage contribution in PG-game

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quota	-0.2194643***	-0.2353571***	-0.2194643***	-0.2375***	-0.2170455***	-0.211***	-0.235***	-0.2135***
	(0.0363049)	(0.0394711)	(0.0363375)	(0.0313586)	(0.0423961)	(0.0571418)	(0.0292869)	(0.0309376)
Purple		0.0089286						
		(0.0887349)						
Quota*Purple		0.0317857						
		(0.1030826)						
Samecolor			0.0386458**	0.0176042				
			(0.0161641)	(0.0144428)				
Quota*Samecolor				0.0420833				
				(0.0291402)				
Orange_to_orange					0.0191667			
					(0.0373125)			
Quota*Orange_to_orange					-0.0112879			
					(0.04818)			
Orange_to_purple						-0.02675		
						(0.0792236)		
Quota*Orange_to_purple						-0.029625		
						(0.0893928)		
Purple_to_purple							0.0064394	
							(0.0485828)	
Quota*Purple_to_purple							0.0725	
							(0.0757208)	
Purple_to_orange								0.005625
								(0.0701816)
Quota*Purple_to_orange								-0.020875
								(0.0804317)
Multi-cluster	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs (N)	560	560	560	560	560	560	560	560
Cluster (ID)	80	80	80	80	80	80	80	80
Cluster (session)	10	10	10	10	10	10	10	10

OLS. Levels of significance: * p<0.1, ** p<0.05, *** p<0.01. Robust standard error in parentheses. Cluster as indicated in table.

Table A.7: Regressions Treatment 1, 1b, 1c and 2

Dependent variable: Percentage contribution in PG-game

	T1 and T1b	T1b and T2	T1 and T1c	T1c and T2	T1, T1b, T1c, T2
	(3)	(4)	(1)	(2)	(5)
Quota		-0.2602976***		-0.2245833***	-0.2194643***
		(0.0500737)		(0.0763087)	(0.0355993)
Efftreat	-0.0408333				
	(0.0458854)				
Fairtreat			-0.005119		
			(0.0736282)		
Efftreat*Quota					-0.0408333
					(0.0443543)
Fairtreat*Quota					-0.005119
					(0.0711714)
Multi-cluster	Yes	Yes	Yes	Yes	Yes
Nr of obs	448	448	448	448	896
Nr of cluster (id)	64	64	64	64	128
Nr of cluster (session)	8	8	8	8	16

*OLS. Levels of significance: * p<0.1, ** p<0.05, *** p<0.01. Robust standard error in parentheses. Clustered as indicated in table.*

Table A.8: Regressions Treatment 1, 2, 3 and 4

	T3 and T4	T1, T2, T3, T4
	(1)	(3)
Quota	0.0797619	-0.2194643***
	(0.0929517)	(0.0355993)
Random		-0.1332143
		(0.0869024)
Random*Quota		0.2992262***
		(0.0946053)
Multi-cluster	Yes	Yes
Nr of obs	336	896
Nr of cluster (id)	48	128
Nr of cluster (session)	6	16

*OLS. Levels of significance: * p<0.1, ** p<0.05, *** p<0.01. Robust standard error in parentheses. Clustered as indicated in table.*

Table A.9: Regressions on Fairness Perceptions, all treatments

Dependent variable: Fairness perception

	T1 and T2	T1 and T1b	T1 and T1c	T3 and T4
	(1)	(3)	(2)	(4)
Quota	-0.1964729**			-0.3276486***
	(0.079424)			(0.0795196)
Fairtreat			0.164276*	
			(0.089525)	
Efftreat		-0.148224		
		(0.0959723)		
N	127	109	109	88

*OLS. Levels of significance: * p<0.1, ** p<0.05, *** p<0.01. Robust standard error in parentheses.*

B: Appendices to Chapter 2

Appendix B.1: Instructions in Treatment 1 and 2 in study 1

Stock Market Game [Community Game][56], instructions

Hi and welcome. You are going to take part in the **Stock Market Game [Community Game]**. For your participation you will get compensation. This compensation is dependent on the choices you make.

Please read the instructions carefully. If you have any questions, please raise your hand and the experimenter will come and help you. Do not ask questions without raising your hand first. It is also important that you do not speak to the other participants while the experiment is taking place.

In the Stock Market Game [Community Game] you are paired up with a person in another room. You will not get any information about who that person is, neither before nor after the experiment. The other person will not get information about your identity either.

All people in this room and all people in the other room get the same instructions and compensations for taking part in the experiment.

The Stock Market Game [Community Game] looks like this:

Stock Market Game [Community Game]

		You	
		A	B
		50	**80**
The other	A	50	5
		5	**20**
person	B	80	20

You and the other person choose simultaneously between A and B. Depending on your respective choices, you end up in one of the four squares in the matrix above. The bold numbers in the upper right corner represents, in Swedish kronor, what you get and the numbers in the lower left corner represents what the other person gets.

Examples:
- If both you and the other person choose A you both get 50 kronor.

---

[56] The name 'Stock Market Game' was used in treatment 1 and the name 'Community Game' was used in treatment 2.

- If both you and the other person choose B you both get 20 kronor.
- If you choose A and the other person chooses B you get 5 kronor and the other person gets 80 kronor.
- If you choose B and the other person chooses A you get 80 kronor and the other person gets 5 kronor.

Please note that you will not know anything about the decision of the other person when you make your decision.

Write your decision on the form marked "answering form". Then turn the form upside-down and put it in front of you.

When the Stock Market Game [Community Game] is finished, the experimenter will compile the results and prepare an envelope for each participant. These envelopes will then be distributed. The envelope will contain information about how the other person decided and what the result of the game was. The sum you are allotted will also be in the envelope.

Thank you for your participation!

Appendix B.2: Instructions in Treatment 3 and 4 in study 1

Stock Market Game [Community Game][57]**, instructions**

Hi and welcome. You are going to take part in the **Stock Market Game [Community Game]**. For your participation you will get compensation. This compensation is dependent on the choices you make.

Please read the instructions carefully. If you have any questions, please raise your hand and the experimenter will come and help you. Do not ask questions without raising your hand first. It is also important that you do not speak to the other participants while the experiment is taking place.

In the Stock Market Game [Community Game] you are paired up with a computer. To the computer a receiver is connected. The receiver is a person sitting in another room. You will not get any information about who the receiver is, neither before nor after the experiment. The receiver will not get information about your identity either.

The receiver makes no decision during the experiment but get the compensation that the computer is allotted. This person knows only that the money that she or he gets is the results of an experiment. The person does not know which game that has been played and thus not how you or the computer acted.

All people in this room get the same instructions and compensations for taking part in the experiment.

The Stock Market Game [Community Game] looks like this:

Stock Market Game [Community Game]

		You	
		A	B
The computer / the receiver	A	50 / **50**	5 / **80**
	B	80 / **5**	20 / **20**

You and the computer choose simultaneously between A and B. Depending on your respective choices, you end up in one of the four squares in the matrix above. The bold numbers in the upper right corner represents, in Swedish kronor, what you get and the numbers in the lower left corner represents what the receiver gets.

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[57] The name 'Stock Market Game' was used in treatment 3 and the name 'Community Game' was used in treatment 4.

Examples:
- If both you and the computer choose A you get 50 kronor and the receiver gets 50 kronor.
- If both you and the computer choose B you get 20 kronor and the receiver gets 20 kronor.
- If you choose A and the computer chooses B you get 5 kronor and the receiver gets 80 kronor.
- If you choose B and the computer chooses A you get 80 kronor and the receiver gets 5 kronor.

When the computer chooses between A and B it is done in the following way: we conduct this experiment also with people playing against each other. Depending on how the players act in that game we calculate with which probability the computer must choose A and B respectively to "imitate" the behavior of a human player.

Please note that you will not know anything about the decision of the computer when you make your decision.

Write your decision on the form marked "answering form". Then turn the form upside-down and put it in front of you.

When the Stock Market Game [Community Game] is finished, the experimenter will compile the results and prepare an envelope for each participant in this room. These envelopes will then be distributed. The envelope will contain information about how the other person decided and what the result of the game was. The sum you are allotted will also be in the envelope. The receivers in the other room will also get an envelope with the money that the computer was allotted, but no further information.

Thank you for your participation!

Appendix B.3: Instructions in Study 2

Stockholm Market Game [Community Game][58], Instructions

Hi and welcome. You are going to take part in the **stock market game [community game]**. For your participation you will get compensation. This compensation is dependent on the choices you make.

Please read the instructions carefully. If you have any questions, please raise your hand and the experimenter will come and help you. Do not ask questions without raising your hand first. *It is also important that you do not speak to the other participants while the experiment is taking place!*

In the stock market game [community game] you are paired up with a computer. To the computer a receiver is connected. The receiver is a person sitting in another room. You will not get any information about who the receiver is, neither before nor after the experiment. The receiver will not get information about your identity either.

The receiver makes no decision during the experiment but get the compensation that the computer is allotted. The receiver has received a copy of these instructions and has been told orally that he/she will receive the compensation that the computer is allotted.

The stock market game [community game] looks like this:

Stock market game [Community game]

		You	
		A	B
		50	**80**
The computer / the	A	50	5
receiver		**5**	**20**
	B	80	20

You and the computer choose simultaneously between A and B. Depending on your respective choices, you end up in one of the four squares in the matrix above. The bold numbers in the upper right corner represents, in Swedish kronor, what you get and the numbers in the lower left corner represents what the receiver gets.

Examples:
- If both you and the computer choose A you get 50 kronor and the receiver gets 50 kronor.

---

[58] The name 'stock market game' was used in treatment 1' and the name 'community game' was used in treatment 2'.

- If both you and the computer choose B you get 20 kronor and the receiver gets 20 kronor.
- If you choose A and the computer chooses B you get 5 kronor and the receiver gets 80 kronor.
- If you choose B and the computer chooses A you get 80 kronor and the receiver gets 5 kronor.

When the computer chooses between A and B it is done in the following way: the choice is done randomly with the same probability that the computer will choose A as B (i.e. like flipping a coin between option A and B).

Please note that you will not know anything about the decision of the computer when you make your decision.

Write your decision on the form marked "answering form". Then turn the form upside-down and put it in front of you.

When the stock market game [community game] is finished, the experimenter will compile the results and prepare an envelope for each participant. These envelopes will then be distributed. The envelope will contain information about how the computer decided and what the result of the game was. The sum you are allotted will also be in the envelope. The receivers in the other room will also get an envelope with the money that the computer was allotted, and information about your choice and the choice of the computer.

Thank you for your participation!

Appendix B.4: Instructions in Study 3

Stock Market Game [Community Game][59]**, Instructions**

Hi and welcome. You are going to take part in the **stock market game [community game]**. For your participation you will get compensation. This compensation is dependent on the choices you make.

Please read the instructions carefully. If you have any questions, please raise your hand and the experimenter will come and help you. Do not ask questions without raising your hand first. *It is also important that you do not speak to the other participants while the experiment is taking place!*

In the stock market game [community game] you are paired up with a person in another room. The persons in one of the rooms are called player 1 and the persons in the other room are called player 2. You and everyone else in your room are **player 1 (2)**. You will not get any information about who the person in the other room is and he/she will not get any information about your identity, neither before nor after the experiment. All the persons in both rooms have received these instructions.

The stock market game [community game] looks like this:

Stock market game [Community game]

Player 1

		A	B
		50	**80**
Player 2	A	50	5
		5	**20**
	B	80	20

Player 1 first chose between A and B without knowing which choice player 2 will make. Thereafter player 2 chose between A and B, given that she knows which choice player 1 has made. The bold numbers in the upper right corner represents, in Swedish kronor, what player 1 get and the numbers in italics in the lower left corner represents what player 2 gets.

Examples:
- If both player 1 and player 2 choose A you both get 50 kronor.
- If both player 1 and player 2 choose B you both get 20 kronor.
- If player 1 choose A and player 2 choose B, player 1 get 5 kronor and player 2 gets 80 kronor.

---

[59] The name 'stock market game' was used in treatment 1'' and the name 'community game' was used in treatment 2''.

- If player 1 choose B and player 2 choose A, player 1 get 80 kronor and player 2 gets 5 kronor.

Player 1 write down their choice on the form marked "answering form (player 1)" and player 2 write down their choice on the form marked "answering form (player 2)". Player 2 writes down their choice both for the case when player 1 chose A and for the case when player 1 chose B (the choice of player 2 for the choice actually made by player 1 will then be used to determine the payments). When you have made your choice, turn the form upside-down and put it in front of you.

The experimenters then collect the forms in both rooms and prepare an envelope for each participant that contains the payment of each participant. These envelopes will then be distributed and the experiment is then over.

Thank you for your participation!

C: Appendix to Chapter 3

Appendix C.1: Experimental Instructions

Stage 1
SALIVA COLLECTION INSTRUCTIONS
1. Rinse your mouth with the water that is provided on your desk.
2. Imagine eating your favorite food and allow saliva to pool in your mouth.
3. Tilt your head forward, drool down the straw (or directly into the tube) and collect saliva in the vial.
4. Repeat as often as necessary until sufficient sample is collected (the vial should be at least 3/4 full with clear fluid).
5. If your mouth is dry, gently chew on the end of the straw. This will stimulate saliva production.
6. Please make sure the cap on your vial is closed tightly.
7. Press the OK button after you have handed in your vial.
DON'T: Drink water during saliva collection.
DON'T: Provide saliva sample that is all foam.
OK

Stage 2
Thank you for participating in this study. Please read, and listen to, the following instructions carefully. If you have any questions, please raise your hand or press the help button. Aside from this, no communication is allowed during the study. This study is about decision making. Everyone will receive a show-up fee of $10 for participating in the study. You have the opportunity to make more money depending on the decisions you and others make. Everything that you earn will be paid out in cash either during the study or at the end of it, before you leave. The study consists of several different parts. Each part will come with an explanation of the task. The first part will only involve hypothetical questions. Please answer them truthfully. OK

Stage 3
[Unincentivized price estimation task]

Stage 4
COMPETITION
We now move on to part 2, which is a competition.

Stage 5
INSTRUCTIONS FOR COMPETITION
This is your main opportunity to win money in this study! In this part of the study you will compete in a "Rock, Paper, Scissors" game against another person. You will be matched with another person and go into a competition room with that person and a member of the staff. The "Rock, Paper, Scissors" game works like this. In each game you choose Rock, Paper or Scissors and your opponent does the same. In this game, Rock beats Scissors, Sicssors beats Paper and Pa-

per beats Rock. This is illustrated in the picture to the right. You will play a total of 15 rounds of "Rock, Paper, Scissors" against your opponent. The winner of the game is the person who has won the most of these 15 rounds. When you walk into the competition room, you and your opponent will both be given $5[15]. The person who wins the "Rock, Paper, Scissors" game keeps their own $5[15] and wins an extra $5 from the opponent. The person who loses the "Rock, Paper, Scissors" game will lose [their] $5 to their opponent. Hence, from the competition, the winner will take home $10[20] and the loser will not take home anything [$10]. Please wait in your seat until a member of the staff comes and tells you that it is time for your competition!
[Competition takes place in separate rooms]

Stage 6
Welcome back from the competition! Did you win or lose? WIN LOSE

Stage 7
Use the paper and pen that are available on your desk. Write down your experiences during the "Rock, Paper, Scissors"-competition. Please include expectations, strategies contemplated and used, the pace and mood of the competition and the outcome. Make sure to describe your feelings throughout. You have a total of 5 minutes available for this task. When you have finished, press the OK button below.

Stage 8
We now move over to the next part. OK

Stage 9
In this part you will make 10 decisions between pairs of choices. In each case, you will choose between an amount of money that you could earn for certain, and a coin toss that gives you a 50% chance of earning $10, and a 50% chance of earning nothing. When you have made your 10 decisions, one of the questions will be randomly chosen to determine your earnings in this part. If you have chosen the certain amount of money in this question, you will get this amount. If you have chosen the coin toss, the outcome of a computerized coin toss will determine whether you are paid $10 or $0. Click next to the alternative that you prefer for each decision:

Stage 10
SALIVA COLLECTION INSTRUCTIONS
1. Rinse your mouth with the water that is provided on your desk.
2. Imagine eating your favorite food and allow saliva to pool in your mouth.
3. Tilt your head forward, drool down the straw (or directly into the tube) and collect saliva in the vial.
4. Repeat as often as necessary until sufficient sample is collected (the vial should be at least 3/4 full with clear fluid).
5. If your mouth is dry, gently chew on the end of the straw. This will stimulate saliva production.
6. Please make sure the cap on your vial is closed tightly.
7. Press the OK button after you have handed in your vial.
DON'T: Drink water during saliva collection.
DON'T: Provide saliva sample that is all foam.
OK

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