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### Keeping up appearances: Experiments on cooperation in social dilemmas

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## Keeping up Appearances

Experiments on cooperation in social dilemmas

Eva van den Broek

We cooperate. To be precise: we help out others at a cost to ourselves, daily and at an astonishing scale; among family, strangers and enemies, whether we are foraging, negotiating or waging war. Why are we willing to reduce our own chances of survival and reproduction to augment those of others?

This dissertation addresses such social dilemmas using laboratory experiments. It explores how we incorporate reputational concerns, whether imaginary competition with another group makes us more self-sacrificing, and why we behave more cooperatively if we can select our own partners. These institutions make us self-conscious of our appearance in comparison to others and boost cooperation in social dilemmas.

Eva van den Broek (1979) holds a Master in artificial intelligence from Utrecht University. Her research interests evolved from simulating evolutionary processes and experiments on human social behavior to applied behavioral economics. Since 2011 she has been working as a researcher at LEI Wageningen UR. She also writes about behavioral science for the popular press.

Keeping up Appearances

Eva van den Broek



## KEEPING UP APPEARANCES

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# Introduction

We cooperate. To be precise: people reduce their own chances of survival and reproduction to augment those of others, daily and at an astonishing scale; among family, strangers and enemies, whether we are foraging, negotiating or waging war. Why are we willing to help out others at a cost to ourselves? One reason is that collectives can accomplish more than individuals. That applies to cells, ants, monkeys, persons and firms. But our taste for altruism goes beyond rational calculations. We behave altruistically even when our kindness cannot be observed, let alone returned by anyone else.

This poses a puzzle to economists and biologists alike. Both disciplines have dwelled for centuries on (respectively) welfare or fitness sacrificing behavior. Let me discuss two excerpts that illustrate the stance of the foremost thinkers from each side of the sciences.

*“In the first place, as the reasoning powers and foresight of the members became improved, each man would soon learn that if he aided his fellow-men, he would commonly receive aid in return.”*

In this quote, Charles Darwin (1871) clearly formulates a rationale for human altruism. He approached human social behavior as any other animal’s social behavior: as a means to survive and proliferate. This strategic account of our behavior foregoes, however, the pleasure that we seem to derive from doing good. Such an ‘irrational’ warm glow is mentioned by the 18<sup>th</sup> century economist Adam Smith (1759):

*“How selfish soever man may be supposed, there are evidently some principles in his nature, which interest him in the fortune of others, and render their happiness necessary to him, though he derives nothing from it, except the pleasure of seeing it.”*

Darwin’s statement can be viewed as the strategic, fitness-enhancing account of altruism, whereas Smith’s statement addresses the proximate psychological mechanism by which evolution ‘implemented’ altruistic behavior in humans: we are more inclined to behave

altruistically if the delay until the reward is short. Precisely this psychological mechanism makes us vulnerable for exploitation.

In this thesis I address the issue of human cooperation. I study how the institutional environment steers behavior in situations in which an individual's interest is not in line with the general interest of the population at large. I use laboratory experiments to assess the effect of information about (others') previous behavior on cooperative decisions. In Chapter 2, I investigate the effects of reputations in games played by two players who meet repeatedly, or, alternatively, have some information about previous behavior of their partner towards others. In Chapter 3 and 4 I study games in which groups of people interact and obtain knowledge about each other or about other groups.

The main instrument for analyzing situations in which decision makers face strategic dilemmas is game theory. This was systematically developed by Von Neumann and Morgenstern (1944) as a set of mathematical tools to model multi-player situations. It does so by defining a set of players, an action space for all players and their preferences over outcomes. A prominent solution concept for such a game, the Nash equilibrium, is defined as a set of actions for which no player has an incentive to deviate, given that he knows that others play the equilibrium action, too. Game theory is grounded in a rational actor model, which is ubiquitous in economics for two reasons. First, any evolved life form has been adapted to an environment in such a way that it exploits regularities for self-preservation, so we may assume that whatever an individual does, it is consistent with optimizing some kind of preference function (Maynard Smith, 1958); second, although the assumptions about the behavior of individuals may be far-fetched, they produce testable predictions at the aggregate level. This is considered to be a good thing, and many such predictions (for example market equilibrium in a competitive environment) find empirical support. Nevertheless, these predictions hinge on strong assumptions, such as complete information, stable preferences and unlimited computing power of the individuals. Each of these assumptions has been challenged and refined (Simon, 1976; Selten, 1990; Tversky and Kahneman, 1974; Gigerenzer and Selten, 2001).

These extensions to the rational actor model have emerged partly from the surge of experimental economics around 1980 as a new field of science. The first market experiments in the 1960s were introduced as a way to test the external validity of economic models (Davis and Holt, 1993). The incongruities between game theoretical predictions and empirical findings ('anomalies') slowly motivated researchers, journals and institutions towards incorporating psychological evidence into economic theories. Over the past twenty years, some of the self-regarding agents have been pimped up with a more descriptive preference function that incorporates the well-being of others (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), fairness considerations (Andreoni and Miller, 2002), intentions (Rabin, 1993), or beliefs (Dufwenberg and Kirchsteiger 2004).

## ***1.1 Methodology***

Controlled laboratory experiments allow us to study the decisions of participants who find themselves on the brink of our disposition to do good and to be exploited by others. The situations we create for them are abstract translations of everyday situations: the decision to trust a stranger, whether to help someone with a good reputation or whether to join a group. In contrast to the outside world, the laboratory offers us an environment in which we can control the information each subject has. Stripping down the decision problem to its incentive structure allows us to focus on the strategic considerations of participants in the experimental setting. Experimental economists foster a treasury of such set-ups or games. By introducing isolated features of real life we can show their influence on behavior. Below I provide a short overview of the games that are central to this thesis, their theoretical predictions and the stylized facts of the empirical data.

### *Prisoner's dilemma*

Perhaps the most famous game is the Prisoners' dilemma. Consider a situation in which two suspects of a crime face the decision to either cooperate (remain silent with respect to their involvement) or defect (witness against the other). Both are better off when neither of them confesses, since then they receive a short sentence of one year; but if only one of the two confesses she will be set free and the other punished harshly with ten years. If both confess, however, they both face a long sentence: eight years (see table 1.1; note that payoffs –the numbers in the table– are negative because it is assumed that people do not like to be in prison). This situation is characterized by uncertainty about the other's choice, strategic reasoning and a tension between own and other's wellbeing. Note that irrespective of what one prisoner does, her partner in crime is better off defecting by confessing the crime.

**Table 1.1 Prisoner's Dilemma**

$\text{♀} \backslash \text{♂}$	Don't confess	Confess
Don't confess	-1, -1	-10, 0
Confess	0, -10	-8, -8

*Notes.* Payoff table for the two players for each of their actions. The first number in each cell is the payoff for player ♀, the second number is the payoff for player ♂.

This game does not feature in the thesis, but is presented here because it shows the descriptive limits of game theory: in laboratory situations and in real life, people behave much more cooperatively than predicted, even if the game is played only once- see for instance the final part of the UK television show “Golden Balls” called “Split or Steal”.<sup>1</sup>

#### *Helping game*

The helping game is the first half of an alternating variation of the Prisoners' dilemma described above. A player in the role of donor faces the decision to allocate a benefit (e.g., €2,50) to a fellow player – the recipient - at a certain cost (e.g., €1,50), which is lower than the benefit for the recipient. If the donor decides not to help the recipient, their payoffs do not change (see table 1.2 for the payoff matrix).

---

<sup>1</sup> There is a Dutch equivalent of this show known as “Deelt ‘ie het of deelt ‘ie het niet” that has been analyzed by Belot et al. (2010).

**Table 1.2 Helping game**

Donor \ recipient	
Defect	0,0
Help	€ -1,50, € 2,50

*Notes.* Payoff for the donor and recipient when they defect or help.

When two players of this game face each other repeatedly up to infinity, defection is not the best strategy (Trivers, 1971). If the chance of encountering a partner again is sufficiently high, direct reciprocal strategies such as Tit-for-Tat thrive (Axelrod and Hamilton, 1981). If the game is repeated among a group of players, a player may face the donor of some other player and have to rely on reputation. In that case, indirect reciprocity, or “I help you and somebody else helps me” is a feasible strategy (Alexander 1987), as can be observed in real life and in the laboratory (Seinen and Schram, 2006). A simple representation of previous interactions has been formalized in the seminal paper by Nowak and Sigmund (1998), who showed that *image scoring* can sustain cooperation. In Chapter 2, we use this game to study situations in which people can choose between information on direct and indirect reciprocity.

### *Public goods game*

A public goods game captures a social dilemma in which the individual interest of group members conflicts with the collective interest. It is a generalized multi-person version of the Prisoners’ dilemma. A typical public goods experiment consists of a number of rounds. Each round the participants receive a certain amount of money, for instance 1 euro. Independently and simultaneously they decide how much of this to contribute to a public good, and how much to keep in their personal account. The total of contributions to the public good is multiplied by a given factor and distributed evenly among all players<sup>2</sup>. Hunting, air pollution, and overfishing are all group activities that boil down to a public goods game and share the problem of how to keep up cooperation instead of unraveling into free riding singularities. Laboratory studies of such games show a surprising amount of collaboration in early rounds,

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<sup>2</sup> Note that if the number of players exceeds the multiplication factor, it is most profitable for each individual player if she contributes nothing.

but as the dilemma is repeated, members' contributions to the public good decline. Several explanations for this stylized fact exist. Some account for it by learning effects or by interaction between different types (Andreoni 1988; Kurzban and Houser, 2005); others extend the game by providing players with a longer time horizon or allowing them to communicate (Gächter et al., 2008).

### *Punishment*

One of the enforcement mechanisms we employ in real life is punishment. People punish free riders and norm violators, even at a cost to themselves (Ostrom et al., 1992). Recent laboratory evidence shows that the decrease in cooperation in a public goods game can be prevented if the game is extended with a stage during which players are allowed to punish other players at a cost to themselves. This is because such punishment is mostly directed towards non-cooperators who in response change their behavior in subsequent rounds of the game (Fehr and Gächter, 2002). The exertion of punishment is widespread throughout different cultures (Henrich et al., 2006) and has been observed also when players meet only once (Fehr and Fischbacher, 2004), and the effects extend even to non-monetary punishment (Masclot et al., 2003). Therefore costly punishment has been put forward as an explanation for the persistence of cooperation in human societies (Nowak, 2006).

Together, experimental evidence from these games and recent extensions of individual preference functions have succeeded in explaining a larger part of the richness of empirical observations of cooperation and altruism. Still, there remains a gap between evolutionary and economic theories on cooperation and our behavior in firms, in eBay transactions and on the street. Although in economic experiments participants always face real monetary rewards, incorporating reputation in a realistic way in laboratory settings is a stretch (Schram, 2005; Hagen and Hammerstein, 2006). By studying the effect of information about previous behavior, this thesis aims to narrow the gap between life outside and inside the laboratory in situations in which an individual's interest is not in line with the general interest.

## *1.2 Chapter Guide*

We will consider two aspects of the influence of information on cooperation. In Chapter 2, we look at reputation in games played by two players who meet repeatedly, or, alternatively, have some information about previous behavior of their partner. In Chapters 3 and 4 we study games in which groups of people interact. In each of these three Chapters specific game-theoretical predictions are tested by an experiment on human subjects. A general conclusion based on all chapters is provided in Chapter 5. What follows is an overview of the research questions to be answered in the three chapters describing experiments.

In Chapter 2 we study repeated interactions amongst pairs drawn from a group of individuals. People have access to information about their own previous experience with the partner, but also about others' experience with her. In such situations, cooperation can be sustained through direct and indirect reciprocity. This is a novel environment, because previous studies of indirect reciprocity focus on groups in which direct experience is excluded. This study examines experimentally which kind of information people prefer to use, when both types of information are available, yet costly. Simulations show that the use of reputations (i.e., information about experiences of third parties) can have a selective advantage in a population of agents only using information on direct interactions. In our experiments, we find that people request both kinds of information evenly, but assign different weights to them when deciding whether or not to reciprocate. A decrease in the reliability of the indirect information does not affect the cooperation level, but increases the demand for direct information.

Next, we turn to social dilemmas in groups (Chapter 3). Individuals are not simply born in social groups but can choose with whom they interact. We consider the relevance of this for voluntary public good provision in groups, noting that in many environments people choose with whom they prefer to cooperate, and may exclude some others altogether. Previous studies on endogenous group formation in the laboratory showed that voting and rank-based mechanisms can sustain cooperation. We present a laboratory environment with fixed group size, where we allow both for ranking and exclusion of partners based on information from their previous choices. We find that endogenous group formation with exclusion creates high

cooperation and efficiency levels. In combination with punishment, exclusion yields even higher contributions.

In Chapter 4 we investigate the influence of another group on the behavior of a group member in a social dilemma. Group competition has been put forward as explanation for sustained cooperation. Yet punishment costs are detrimental to a group's relative success in such competition with other groups. We investigate the dynamics of altruistic punishment in combination with intergroup competition by conducting a series of experimental public goods games, in which we systematically vary the possibility for social comparison between groups, group competition, and punishment. Our results indicate that the mere presence of another group is sufficient to induce high cooperation and punishment levels. Group competition attenuates punishing behavior. Despite the lower level of exerted punishment, contribution levels remain equally high, however.

Finally, Chapter 5 concludes and discusses the findings from earlier chapters before suggesting open questions and directions for future work. Overall, this thesis aims to extend our knowledge about reputation by zooming in on the sources of reputation and by broadening the perspective to partner choice and intergroup observation. In the conclusion we expand on the insights gleaned from these experiments.



## Chapter 2: Direct and indirect reciprocity

### 2.1 Introduction<sup>3</sup>

Humans are allegedly the ‘champions of reciprocity’ (Nowak and Sigmund, 2005). Not only do we often return favors, we even ‘help’ (i.e., direct kind but costly acts towards) people who have been kind to others (Alexander, 1987). These two reciprocal strategies, which have been called direct and indirect reciprocity, respectively, ensure the survival of cooperative individuals by channeling help towards them. Direct reciprocity has been shown to be a stable strategy in small groups of individuals interacting repeatedly (Trivers 1971; Binmore, 1992). In sufficiently large groups, however, repeated interaction may be rare and the probability that two members meet again can be very low. Therefore studies on indirect reciprocity focus on groups in which the possibility of direct experience with a partner’s previous choices is negligible (Rosenthal, 1979). Presumably, however, intermediate sized groups were most common in our prehistory (Kelly, 1995). In this case, an individual (A) considering whether or not to help another individual (B) may resort to two kinds of information about B’s previous actions. In principle there is information available on how often B has helped A in previous interactions (information for Direct Reciprocity, *IDR*) and information on how often the recipient has helped third parties (C) (information for Indirect Reciprocity, *IIR*). In practice, information about behavior towards third parties is likely to be incomplete or noisy, however, either because of intentional spreading of false information (Hess and Hagen, 2006) or because accuracy simply may decrease through miscommunication. People may therefore treat the two sources of information differently<sup>4</sup>.

One way to quantify an individual’s willingness to cooperate is by means of a numerical proxy for reputation, i.e. an image score, as explained in Chapter 1. Cooperative strategies discriminatively responding to such a score can arise and are conserved in an evolutionary

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<sup>3</sup> Parts of this chapter have been published as “Molleman L., Van den Broek E., Egas M. (2013) Personal experience and reputation interact in human decisions to help reciprocally”.

<sup>4</sup>One study shows, counter-intuitively, that gossip influences people’s behaviour more than factual information (Sommerfeld et al., 2007).

setting (Nowak and Sigmund, 1998). The approach prompted many follow-ups, theoretically as well as experimentally (for a summary see Nowak and Sigmund, 2005). The most rigorous tests of image scoring show that people indeed base their decision to help on their partner's score (Seinen and Schram, 2006; see also Wedekind and Milinski, 2000). However, agents optimizing their own score pose a challenge to scoring models (Leimar and Hammerstein (2001). This proved to be more than just a theoretical problem, as shown by Engelmann and Fischbacher (2009). In their experimental study, agents had a publicly observable score only in the first or last half of the experiment, which precludes strategic reputation formation in the other half. Non-reciprocal, strategic reputation builders (those that based their choices mainly on their own reputation) earned on average more money than reciprocal players who cooperated based on the other's score. Still, 32 % of cooperative acts was purely reciprocal and uncontaminated by strategic considerations for the own score. In sum, information about previous' behavior of partners increases the likelihood that help is given, but the extent of the impact of this information depends on the strategic incentives for giving it and on the costs of giving (Bolton et al., 2005).

To date only a limited number of studies has investigated the possibility that behavior in an indirect setting might play a role in a subsequent direct interaction, or vice versa, as proposed by Panchanathan and Boyd (2004). In an alternating public goods game (PG) combined with an indirect reciprocity game, contributions in the PG remain high with repetition if they are subsequently disclosed to partners in the indirect reciprocity game (Milinski et al., 2002). In a similar setting, disclosure of PG contributions amplifies generosity in a prisoners' dilemma scenario (Wedekind and Braithwaite, 2002). These findings show that reputation is an effective instrument to enhance cooperative behavior and even transfers from one environment to another.

It is difficult to conclude from the evidence above whether or not people distinguish between direct and indirect information about others' past choices. To the best of our knowledge there are only two studies that try to tease apart the effects of IDR and IIR on helping, but the results are inconclusive. For example, Dufwenberg et al. (2001) present a trust game experiment where a receiver can reward either the sender, or, as a treatment variable, some other donor.

The latter treatment induces lower investments and, surprisingly, higher repayments. Both effects are statistically insignificant, however. This finding shows that, if anything, indirect reciprocity induces higher return rates. This counterintuitive finding is corroborated by a second study, a one-shot sequential gift exchange game in which direct (if A helps B, B helps A), indirect (if A helps B, C helps A) and generalized (if A helps B, B helps C) reciprocity are compared. Return rates are significantly higher in the generalized reciprocity treatment than in the indirect or even the direct reciprocity treatment (Stanca, 2009). This shows that if people are confronted with a situation in which they can only direct help towards a third party, they do so. As to the comparison between IDR and IIR, in the abovementioned studies people have been observed to use direct and indirect reciprocity interchangeably, directing more help towards helpful individuals in general.

In this chapter, we address a related but different question. We consider a situation where B has to decide whether or not to help A. A has interacted with B in the past, so B has her own experience, but A has also (far more often) interacted with various C's. We are interested in how B's distinguish between their own direct information about A and the indirect information they may have about A's behavior towards third parties.

Theoretical work on the evolutionary success of either form of reciprocity yields various conditions depending on the cost-benefit ratio of helping. A necessary condition that has been derived for direct reciprocity to evolve is that the probability of meeting the partner again in the future must be larger than the cost/benefit ratio (Axelrod and Hamilton, 1981). This is simply a special case of the condition for the evolution of indirect reciprocity, which reads that the probability of knowing the other's reputation must be larger than the cost/benefit ratio (Nowak and Sigmund, 1998). In situations with intermediately large groups, indirectly reciprocal strategies (that base a choice on third-party information) and directly reciprocal strategies (that base decisions on own experiences) can co-exist. Simulations have shown that as the number of encounters between two specific agents increases, for instance because of a smaller group size (in other words, as the probability of meeting a partner again is high relative to the probability that the partner knows your image score), strategies based on direct reciprocity are employed by a larger part of the population (Roberts, 2008). This result

suggests that selection favors strategies using direct information (since they have on average a higher fitness) as the relative reliability of indirect information decreases. The above simulation study disregards strategies that employ both types of information, however.

This prompts three behavioral questions:

- (i) Given that the two types of information are simultaneously available, which information do people rely on?
- (ii) Do people react differently to the two types of information?
- (iii) Do people display preferences for one kind of information that translate into differences in earnings?

The present study seeks to answer these questions in environments with reliable and unreliable third party information. Given the experimental literature mentioned above and Roberts' (2008) simulation results about a gradual shift in relative strength of different strategies in relatively large groups, we conjecture that evolution will favor strategies that take both types of information into account. This hypothesis is first tested by means of an evolutionary simulation that shows that strategies combining direct and indirect reciprocity can invade Robert's disparate strategies. They do not always displace them completely, as they may co-exist with one-dimensional strategies. We find that no particular combination of strategies is favored by selection. Then, we study the use of both types of information in a laboratory experiment. Our experimental results show that subjects request both kinds of information evenly, but assign more weight to direct information when deciding whether or not to reciprocate. A decrease in the reliability of indirect information does not affect cooperation levels, but increases the demand for direct information.

This chapter is organized as follows. In section 2 we describe simulation results intended to set a benchmark for observed behavior. Section 3 contains the setup for the experiment, of which the results are presented in section 4. Section 5 provides the conclusions.

## 2.2 *The helping game: simulations*

Standard equilibrium theory predicts that in any finitely repeated helping game, backward induction will cause cooperation to unravel. Limited foresight may permit some cooperation in early rounds. The folk theorem on infinitely repeated games applies to both direct and indirect reciprocity and therefore does not give prediction as to which kind may be prevalent.

To get more insight in the basins of attraction of distinct strategies that combine direct and indirect reciprocity, we ran an evolutionary simulation. We investigated the long-term consequences of strategies that employ both IDR and IIR. As a benchmark, we replicated the findings of Nowak and Sigmund (1998), and subsequently extend their model by allowing for strategies that take IDR and combinations of IIR and IDR into account<sup>5</sup>. In the simulations, the history of cooperation by an agent is provided in two formats: the (IIR) image score, quantifying the agent's actions towards others in a range between -5 to 5, and the (IDR) 'memory' of the agent's own interactions with a partner, also ranging from -5 to 5. Each agent carries a heritable strategy  $s$  denoting the relative weight he assigns to the IDR and IIR scores of a partner. Threshold values  $t$  that define whom to help ( $t \in \{-1, 0, 1\}$ ) are assigned randomly to agents every new generation. We chose to investigate the evolutionary pressure on  $s$  separately and reduce the values to arbitrary stable states for  $t$  to prevent drift.<sup>6</sup> The three threshold values we chose correspond to the long stable phases observed by Nowak and Sigmund in their 1998 paper and cover qualitatively all possible strategies.

An agent carrying strategy  $s$  and threshold  $t$  will offer help if  $t \leq s * IDR_{[partner]} + (1 - s) * IIR_{[partner]}$ . In words: if an agent's threshold is lower than the weighted sum of his direct and indirect reciprocity information about his partner, he will offer help. Fitness (and subsequently a relatively high share in offspring in the next generation) can be obtained by receiving help from others. For comparability this is set at the same levels as in the Nowak and Sigmund paper: a helping agent incurs a cost of 0.1, the partner receives a benefit of 1. Offspring inherits the strategy of the parent, with a mutation probability equal to 0.025. A decision to

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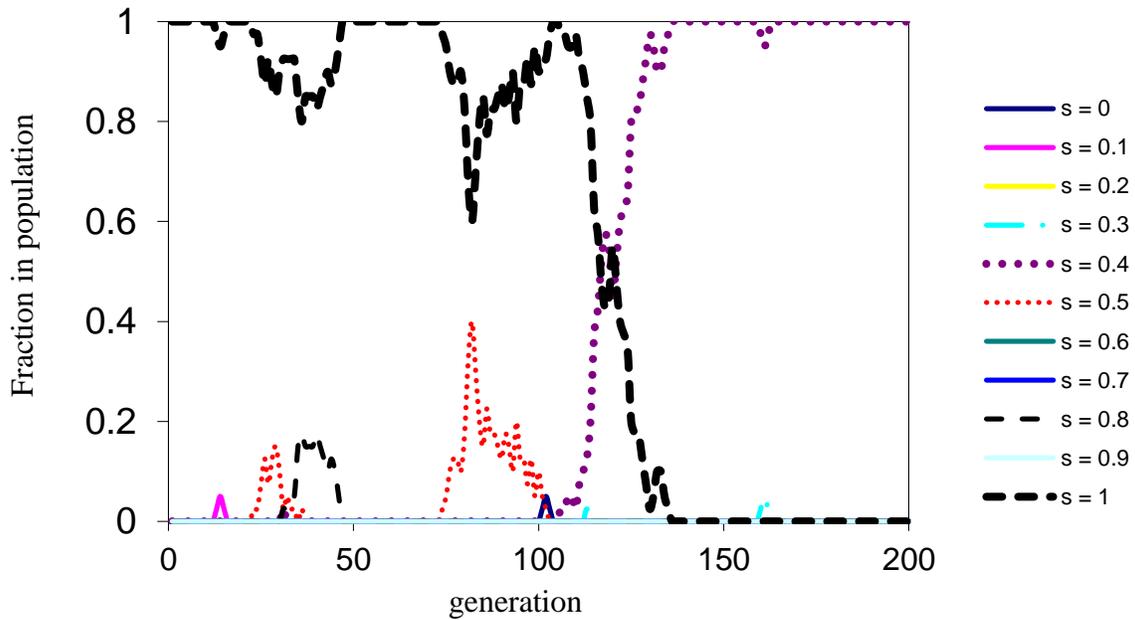
<sup>5</sup> The simulations were run in StarLogo, a Java-based program, version 2.21.

<sup>6</sup> Had we implemented selection pressure on the parameter  $t$ , too, the model would have converged to arbitrary combinations of  $s$  and  $t$ . Alternatively, linking  $s$  to  $t$  in any way would impose a constraint.

help leads to an increase in IIR and IDR of 1 point and a decision to pass leads to a decrease of 1. We further assume that information on indirect reciprocity is less perfect than information on direct reciprocity. To implement this, we introduce noise in the IIR measure by introducing a probability of 1/6 that a decision to help decreases the image score, or a decision to pass increases the score. The number of encounters per generation is set at 18000, and the population size at 40, which is the largest number we were able to run on our computers. On average, each individual is paired as donor with any specific other agent 5.77 times. We ran the simulations for 200 generations, which proved to be long enough for convergence.

We first consider a society in which no image scores are formed based on behavior towards third parties but only image scores based on direct information. We did so by initializing all agents such that they only use IDR ( $s = 1$ ). When doing so, we observed that mutants using a combination of direct and indirect reciprocity information were able to invade the population, although not in every run (see Figure 2.1 for a typical example of an invasion). 7 out of 70 runs converged to a strategy that was a mixture of direct and indirect reciprocity. Although this only makes up ten percent of the total number of runs, the result shows that with our parameters it is in principle a feasible strategy to not only use one's own memory, but also the other agent's image score.

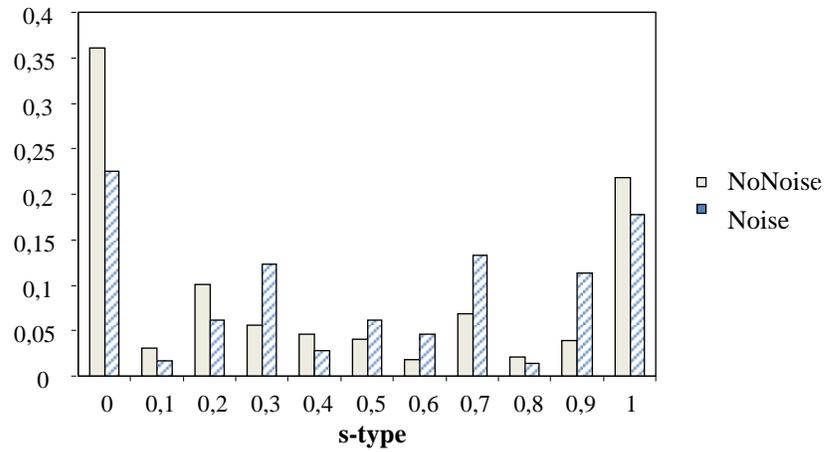
**Figure 2.1. Invasion of strategies using reputation**



*Notes.* Lines show the fraction of agents in the population with specific strategies in a typical invasion run over generations. Strategies are defined by the relative weight ( $s$ ) they assign to direct information. The strategy using only IDR ( $s=1$ , dashed line) loses its dominance and is invaded by a strategy combining IDR and IIR ( $s=0.4$ , double line). See the main text for a definition of parameters.

As a further test of the stability of strategies that combine information for direct and indirect reciprocity, we initialized the population with randomly picked strategies and ran that simulation 50 times with and 50 times without noise. Cooperation levels (i.e., choices to help) were around 66% (70%) in the sessions with(out) noise. The average  $s$  (the weight an agent attaches to IDR relative to IIR) of all strategies is 0.40 in the simulations without noise and 0.50 in the noise condition (ANOVA:  $F=3.854$ ,  $d.f.=1$ ;  $p=0.05$ ). Hence, noisy IIR information increases the weight attached to DR, but does not eliminate agents using IIR information (see Figure 2.2). Finally, we observed that no single weighted combination of IDR and IIR dominates the runs. Though strategies using both IDR and IIR information survive in this environment, no optimal combination appears to evolve.

**Figure 2.2 Influence of noise on strategies**



*Notes.* Fraction of agents with a specific strategies averaged over 50 runs in generation 50-200 with (blue bars) or without noise (red bars). Strategies range from using IDR only ( $s=1$ ) via a weighted average of IDR and IIR, to strategies using only image scores ( $s=0$ ).

All in all, our simulations show that the use of image scores can have a selective advantage in a population that can also condition their strategies on personal experience with a partner. Even when indirect information is noisy, strategies that use a combination of both types of information survive evolutionary pressures. Moreover, such strategies are able to invade populations that start off with only strategies solely based on direct experience.

### *2.3 Experimental design and procedures*

A computerized experiment with human subjects was run at the CREED laboratory of the University of Amsterdam, in June and July of 2007. The 120 participants were students from various departments, including economics (43%) and psychology (15%), with an average age of 23. Two treatments were conducted, each with 5 independent groups of 12 subjects, making about 50 decisions each. For every session subjects are randomly assigned to cubicles in the laboratory. No communication among participants is allowed. Written instructions for the experiment (in Dutch; see appendix A for an English translation) are provided. A quiz is used to ensure that the subjects have understood the instructions. When all subjects have finished reading the instructions and have answered the quiz correctly, the experiment starts. Subjects know that after 100 rounds, a next round will start with a probability of 90% (this is done to minimize end game effects). Every session lasts for approximately 90 minutes. Subjects receive, in addition to a show up fee of 7 euros, an initial endowment of 3000 points (300 points = €1). To avoid income effects as much as possible, no information is given about the subjects' current earnings during the experiment, although subjects can calculate these with pen and paper. On average subjects earned €34.50, including the show up fee<sup>7</sup>. At the end of the experiment they are asked to fill out a questionnaire about their personal background and the way they made their decisions.

The setting of the experiment is a helping game. In each round every subject is randomly paired to another; one being assigned the role of donor, the other of recipient. The donor must choose to either pass a specified amount to the recipient, at a cost to himself; or to pass, resulting in no change in payoff for either. Parameters are chosen such that helping costs 150 points to the donor and yields 250 points to the recipient. In the experiment, the choices to help or pass are referred to as yellow and blue choices, respectively. Before the donors decide to help or pass, they are offered the possibility to request information about previous actions by their recipients, when the latter was in the role of donor.

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<sup>7</sup> This was the equivalent of \$48.

Our experiment closely follows the design in Seinen and Schram (2006), which is a direct translation of the Image Scoring model of Nowak and Sigmund to a laboratory setting. As noted by Bolton et al. (2005), with the group size of fourteen<sup>8</sup> used in the Seinen and Schram experiments, two subjects will ‘meet’ quite often, though they do so without recognizing each other. Therefore, the Seinen and Schram results on indirect reciprocity may implicitly be blurred by direct reciprocal motivations. In our experiment this re-matching with prior partners was made explicit when presenting information about the current partner’s past behavior by distinguishing between his choices when previously facing the donor concerned on the one hand and when facing other parties on the other. Since this experiment was run in the same laboratory as the Seinen and Schram experiments, we will compare our results to their findings instead of running a separate control treatment with only indirect reciprocity<sup>9</sup>.

We implement the provision of these two kinds of information as follows. Two boxes are displayed on the donor’s computer monitor. Ticking the first gives information about the donor’s own experience with this recipient in previous encounters between the two, when the roles were reversed. There is no other way for them to retrieve this information, since subjects cannot be identified. The second box gives the donors insight in the recipient’s reputation, by showing choices of this recipient when paired as a donor with others. If requested, the direct and indirect information summarizes the previous six decisions of the recipient in the role of donor<sup>10</sup>. The donor’s own public score may be computed, but is not presented. When requested, only the *number* of the current recipient’s blue and yellow choices in the last six decisions is given, not the order. This limited information reflects limited memory and gives subjects an opportunity to clean their record. Note that it takes longer to gather direct than indirect information about the helping behavior of another subject. Therefore, for a long part of the experiment (on average 66 rounds), the indirect information is based on more observations than the direct information. Avoiding this problem, which reflects a trade-off that we also face in real life, would have required either a very small group size or an even larger number of rounds.

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<sup>8</sup> We used a group size of twelve.

<sup>9</sup> But see Molleman et al. (2013) for the comparison with another control treatment.

<sup>10</sup> In early rounds, the total number of decisions may be smaller than six, or even zero.

We impose a cost of five points for every information request, to ensure that subjects deliberately click on the information they are interested in<sup>11</sup>. Note that these costs are very low compared to the costs of helping (150) or the benefits of receiving help (250). We did not impose an order in the information requests, since we are interested in which type of information people prefer when both types are available. After the decision has been made, the donors and recipients are informed about their earnings in the round.

To study the extent to which subjects are sensitive to the reliability of images we implemented two treatments similar to the simulations in section 2. In the *baseline* treatment the information on both direct and indirect information is completely accurate. The alternative, *noise*, treatment deviates only in the reliability of the indirect information; one out of every six pieces of information is switched from positive to negative or vice versa. This intends to reflect distortions of information that may occur, for example, as a consequence of gossip. This implementation is equivalent to a noise level of 17 % and does not affect the standard equilibrium predictions as noted in section 2. Note that our implementation is slightly biased against extreme scores, since an observed 5:1 score is more likely to stem from a true 6:0 than from a true 4:2, but since these extreme values (0 times uninterrupted helping or passing) hardly occurred, this effect is unlikely to have affected our results, should any of the participants have noticed.

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<sup>11</sup> An alternative would be to let the cost depend on the amount of information they receive, but that would make it more difficult to infer a person's preference for each type of information.

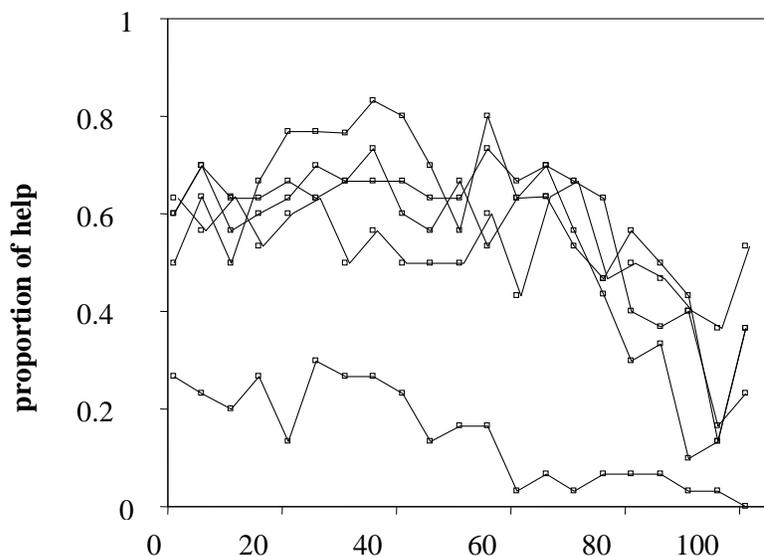
## 2.4 Experimental results

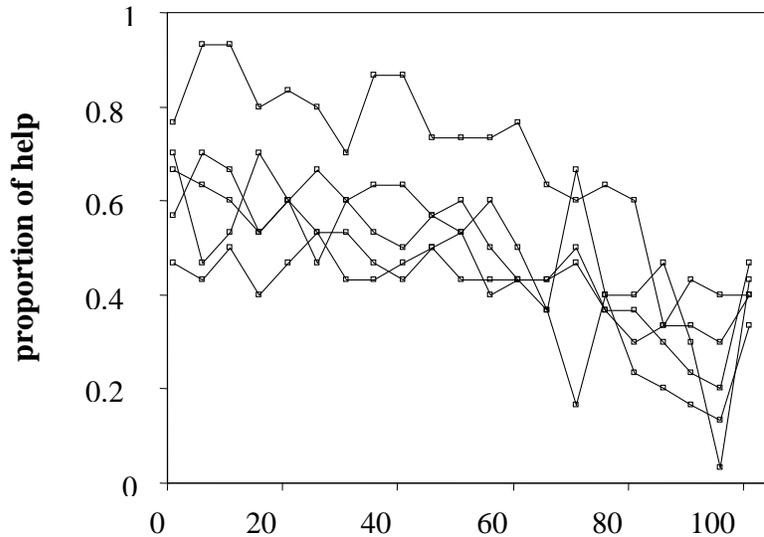
This section is divided into four subsections. After presenting overall helping levels in 4.1, the next subsection gives an overview of the information requests (4.2). The aggregate influence of information on helping behavior is discussed in 4.3 and followed by a detailed overview of individual types (4.4).

### 2.4.1 Helping levels

Help was given in 49 % of all interactions. Reciprocity is a strong phenomenon in our setting: the correlation between the donors' cooperative choices and the number of times they received help previously by others is very high (Spearman  $r = 0.88$ ,  $p < 0.001$ ). Note that this correlation does not express the direction of help: it may be due to direct (A helps B, then B helps A), indirect (A helps B, then C helps A), or generalized (A helps B, B helps C) reciprocity. The average helping level in the first 75 rounds of our baseline treatment (57%) is comparable to the level found in a similar treatment by Seinen and Schram (70%), where information about the six last choices was provided (costlessly) in every round. Furthermore, we observe that the average cooperation level declines, notably so after round 75 (see Figure 2.3).

**Figure 2.3. Helping levels**





Notes. Average proportion of helping per group per 5 rounds in the baseline (upper panel) and the noise (lower panel) treatment.

In the first 75 rounds the fraction of helpful choices in groups was on average 0.56 (*s.e.* 0.05); we do not find a significant difference between the treatments without (0.57) and with (0.53) noise (MW (10 groups)  $z=0.629$ ,  $p=0.53$ ). Within treatments, however, large differences are observed across groups. In particular, one outlier group in the baseline treatment shows a distinct pattern of only 19 % cooperative choices (see the upper panel in figure 2.3). Closer inspection of the individual data reveals five of 12 subjects in that specific group who never helped.

These results on helping choices are summarized by:

**Result 1:** *Helping levels are high and decline over rounds. In aggregate, there is no difference in helping between the baseline treatment and the noise treatment.*

### 2.4.2 Information requests

In on average 49% of all rounds subjects used the possibility to request costly information about others' previous choices. Table 2.1 provides an overview of the information requests. Donors requested (direct) information on decisions that concerned behavior of the recipient towards themselves (32% of the interactions) and indirect information on decisions that concerned others (26%). Without noise the frequency of requests for these two kinds of information was almost equal (29%, 28%). In the noise treatment, the direct information was requested more often (36%: 24%), but not significantly so (Wilcoxon  $T = 2$ ,  $p = 0.14$ ,  $r=0.44$ ,  $N = 10$ ). The difference in IDR requests between treatments is not significant either (MW (5 groups)  $z=0.73$ ,  $p=0.46$ ), nor is the difference in IIR requests (MW (5 groups)  $z=0.73$ ,  $p=0.46$ ).

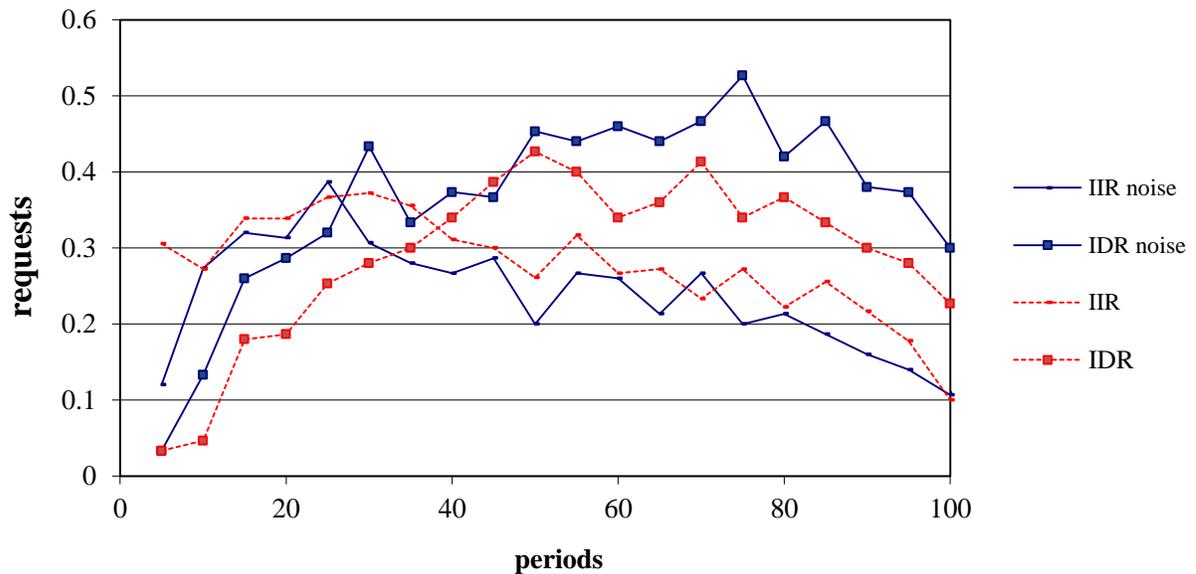
**Table 2.1. Information requests**

	IIR and/or IDR	IIR	IDR	IIR and IDR
Baseline	46%	28%	29%	11%
Noise	52%	24%	36%	8%

*Notes.* Frequency of information requests (any, information for indirect (IIR) or direct (IDR) reciprocity or both) for the baseline and the noise treatments.

The development of information requests across rounds is shown in figure 2.4. It appears that the total number of IDR requests increased slightly across rounds until round 75 (Spearman  $r = 0.04$ ,  $p<0.05$ ). IDR becomes more popular at the expense of IIR ( $r = 0.14$ ,  $p < 0.001$ ); from round 40 onwards, IDR was requested more often than IIR in both treatments (Figure 2.4). This may indicate that the subjects understood the incremental nature of this information, and specifically the fact that gathering direct information takes more rounds than indirect information. The difference between IDR and IIR requests is significant from round 45 on in the noise treatment and marginally so in the baseline treatment (noise: MW (5 groups)  $z=1.98$ ;  $p=0.04$ ; baseline: MW (5 groups)  $z=1.77$ ,  $p=0.07$ ).

**Figure 2.4. Information requests over rounds**



*Notes.* Fraction of subjects per round that requested information on direct (IDR) and indirect reciprocity (IIR) for the baseline treatment (in red) and the noise treatment (in blue).

We again summarize these findings.

**Result 2:** *The number of requests for direct reciprocity information increases over the first 75 rounds and is higher in the noise treatment. The number of requests for indirect reciprocity information decreases after the first 25 rounds and is lower in the noise treatment.*

### 2.4.3 Use of information

The information provided is a score either summarizing the recipient’s choices in previous encounters with the donor (information for direct reciprocity) or others (image score). We classify a score as positive if the observed helping choices outnumber or are equal to the observed choices to pass<sup>12</sup>. Subjects reacted strongly to positive and negative information; see Table 2.2 for the percentage of decisions to help subjects with a positive or negative score.

<sup>12</sup> We classify ‘no information available’ as positive.

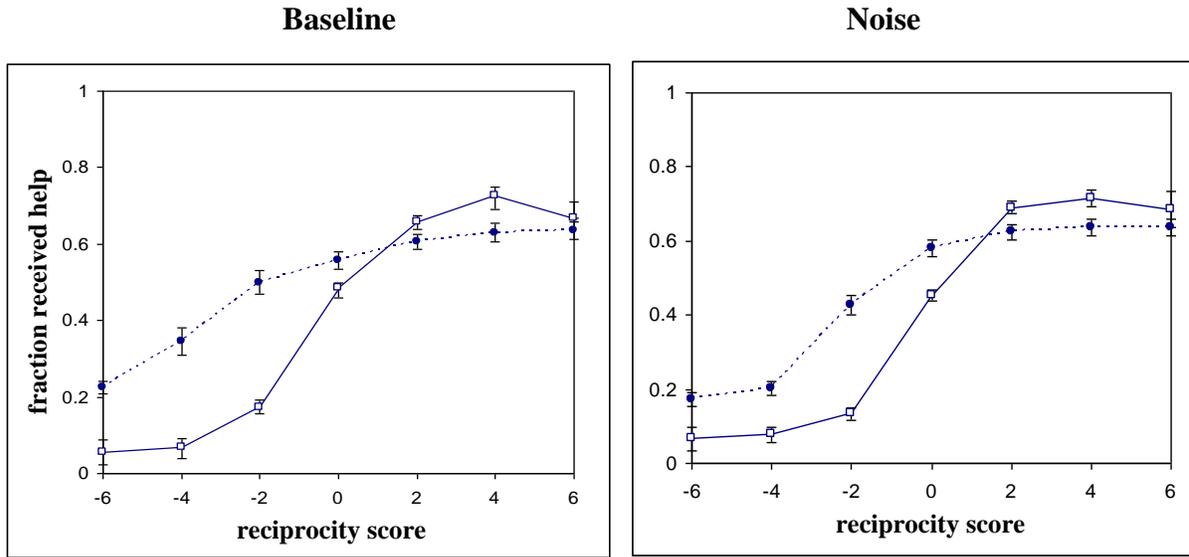
**Table 2.2. Helping behavior following information**

	Baseline		Noise	
	Positive score	Negative score	Positive score	Negative score
<b>IDR exclusively</b>	85 %	10 %	81 %	4 %
<b>IIR exclusively</b>	85 %	18 %	70 %	32 %
<b>IDR and IIR</b>	86%	10 %	92 %	0 %
<b>No info requested</b>	50 %		35 %	

*Notes.* Percentage of decisions to help after requesting direct (IDR) and/or indirect information (IIR) and learning the outcome in the baseline and the noise treatment. See Table 3.5 for the cases where both IIR and IDR had been requested, but yielded conflicting information (not included here).

In the treatment without noise, subjects were much less inclined to help after receiving negative information, both in the case of indirect (18% cooperation) and direct information (10% cooperation). Positive information led to help in 85% of the encounters, both for direct and indirect information. When no information had been requested help was given in 50% of the cases, perhaps indicating subjects' strategic concern for their own score. This is corroborated by the finding that subjects sometimes offered help even after receiving negative information about the recipient. Uninformed decisions were on average less cooperative in the noise treatment (35%), which could be explained by subjects' hoping to 'hide behind the noise'. As a general tendency noise dampens the effect of information by adjusting helping rates after positive or negative IIR to the average (positive: from 85 to 70%; negative: from 18 to 32%).

**Figure 2.5. Average receiving rates per score**



*Notes.* Lines show average receiving rate for direct (full line) and indirect reciprocity scores (dotted line) in the baseline and the noise treatment.

The expected help as a function of a subject's score is shown in Figure 2.5<sup>13</sup>. The graphs show a sharp increase between a direct score of -2 and 0, both in the baseline and the noise treatment<sup>14</sup>. In comparison, the graphs for indirect information are flatter. The marginal effect of a higher indirect score is more constant. Having a negative direct score thus reduces the receiving rate more than a negative indirect score does. A possible explanation is that subjects prefer to use direct information, if available, and use indirect information in case of doubt.

To correct for other factors that may contribute to the decision to help, such as the received help so far, we present a probit regression with random effects at the group level (to correct for interdependencies within groups). We estimated two models; both including observed scores as independent variables. One also includes the events of the previous round and the other includes average variables up to round  $t$ . Table 2.3 shows the results.

<sup>13</sup> In 10 % of the cases subjects requested both types of information. Since we do not know on which information they conditioned their decision, we include all observed and unobserved scores in the graphs instead of counting some decisions twice. Removing the decisions that were based on unobserved scores or on both types of information does not change the graph qualitatively.

<sup>14</sup> To obtain more reliable data (i.e., sufficient observations with certain scores) we aggregate by rounding up uneven scores to the next even integer.

**Table 2.3. The decision to help**

<b>Dep. var: Help of <math>j</math> by <math>i</math> in <math>t</math></b>	<b>I: lagged help</b>		<b>II: average help</b>	
	<b>Baseline</b>	<b>Noise</b>	<b>Baseline</b>	<b>Noise</b>
<b>Observed IDR score in <math>t-1</math></b>	0.45***	0.57***	0.42***	0.53***
<b>Observed IIR score in <math>t-1</math></b>	0.21***	0.24***	0.21***	0.24***
<b>Round</b>	0.41	1.15***	0.64	1.31**
<b>Round<sup>2</sup></b>	-1.31***	-2.18	-1.90	-2.74
<b>Received help <math>t-1</math></b>	0.34***	0.40***		
<b>Gave help <math>t-1</math></b>	1.04***	0.75***		
<b>Average help <math>i</math> received</b>			-0.03	0.42**
<b>Average help <math>i</math> gave</b>			2.85***	2.31***
<b>Constant</b>	-0.56***	-0.61***	-1.49***	-1.44***
<b># observations (groups)</b>	2974 (5)	2963 (5)	2974 (5)	2963 (5)
<b>log likelihood</b>	-1343.56	-1256.75	-1152.18	-1098.82

*Notes.* The table presents the results of a random effects probit regression used to explain help of  $j$  by  $i$  in  $t$ . Formally, it gives the estimated coefficient vector  $\beta$  in  $\Pr(\text{Help})_{jit} = \Phi(\sum_i X'_{ijt}\beta + \mu_m)$  where  $\Pr(\text{Help})_{jit}$  is the probability that  $i$  helps  $j$  in round  $t$ ;  $\Phi$  denotes the cumulative normal distribution and  $X_{ijt}$  is a vector of variables relating to  $i$  and  $j$  in  $t$  as described in the first column of the table.  $\mu_m$  is a (white noise) matching-group-specific error that corrects for the dependencies within matching groups. Variables included are the observed IDR score, the observed IIR score, round, round squared, help received in the previous round as a receiver, help given in the previous round as a donor, IDR IIR cumulative average help received up to  $t$ , cumulative average help given up to  $t$ , and a constant. The first line per variable denotes the coefficient (\*, \*\*, \*\*\* indicating significance at the 0.10, 0.05, and 0.01 level, respectively).

As Table 2.3 shows, both the observed direct and indirect score are highly significantly positive in both models. Other things equal, an increase in the observed IDR score increases the propensity to give help more than an increase in the observed IIR does in both treatments ( $z=7.42$ ,  $p<0.001$ ;  $z=9.81$ ,  $p<0.001$ ; marginal effects: observed IDR = 0.17, observed IIR = 0.09 (baseline): observed IDR = 0.21, observed IIR= 0.09 (noise)), corroborating Result 2

about IDR being preferred; The noise treatment further increases the difference between the impact of IDR and IIR.

The distinction between models I and II lies in how the donor's history is added to the model. These models allow us to test for the effect of generalized reciprocity (if A helped B, B helps C). In model I, history is restricted to one round. We include dummies for having received or given help at the previous respective opportunity<sup>15</sup>. The coefficients for both are significantly positive. The first suggests that in both treatments subjects are motivated by some form of non-strategic helping (i.e., generalized reciprocity). The coefficient for the dummy for having helped in the previous round picks up individual differences in tendencies to help.

The second model confirms that the effects of the observed scores are robust against a control for histories, now represented by the average across all previous rounds. The most remarkable finding is the large and significantly positive influence of average helping rates in both treatments. This strongly confirms the individual heterogeneity in the propensity to help, even after controlling for what donors observe and how much help they have previously received on average. A second noteworthy difference between the two models is that the average help now received has no significant influence on helping in the baseline treatment. This is an indication that generalized reciprocity has a limited memory in the sense that only recent good experiences are positively responded to. Although being helped in the previous round significantly increases the propensity to help, on average generalized reciprocity cannot explain the help given in the baseline treatment.

This brings us to the third main result.

***Result 3: The direct reciprocity score has more impact on the helping decision than the indirect reciprocity score. Noise on indirect reciprocity information diminishes the relative impact of this information on the helping rate.***

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<sup>15</sup> We tested for the effects of imbalances between the number of times an individual has been in the role of donor compared to receiver, but found only insignificant or negligibly small (0.001 smaller than other) effects.

#### 2.4.4 Individual strategies

We can define a donor's strategy as a mapping from the information seen (and possible the own reputation) to a choice whether or not to help. We therefore classify individuals based on their information requests. This yields four types: those who did not request any type of information more than twice; those who requested both types of information three times or more; and subjects who asked for either direct or for indirect reciprocity information more than twice, but not for the other type<sup>16</sup>. Below, we provide an overview of the behavior per type (see Table 2.4). We will discuss our observations for each type in turn.

**Table 2.4. Behavior per type**

Type	No Info		Both Info		DR		IR	
	Base	Noise	Base	Noise	Base	Noise	Base	Noise
<b>% subjects</b>	27	23	43	42	12	20	18	15
<b>Helping rate (%)</b>	26	19	62	65	70	47	46	48
<b>No. requests IDR</b>	1	0	52	59	51	57	1	1
<b>No. requests IR</b>	2	1	41	36	1	1	50	57
<b>Av. earnings</b>	6319	5480	5517	6041	6174	5818	6160	5540

*Notes.* Rows give, respectively, the player type, the percentage of subjects classified as a certain type, their average helping rates, average number of requests for information for direct and indirect reciprocity and earnings for the two treatments.

**No Info.** Subjects who never requested any information make up for a quarter of the participants (baseline: 27%; noise: 23%). Since their decisions are not influenced by information (except for their personal record of receiving help), we can only analyze the general 'giving' pattern in this group. On average they help in 26 (noise: 19) % of the interactions; 15 of the 30 subjects of this type never helped, 5 subjects behave as unconditional

<sup>16</sup> Since subjects requested both types of information in 10% out of 50 rounds on average, we chose to set a cut-off between types at two requests or more.

cooperators and helped in (almost) every interaction, and the others in this category alternated between ‘giving’ and ‘passing’ in some orderly manner<sup>17</sup>.

**Both Info.** The second class of subjects consists of those who regularly (more than twice) requested information about both types of information. This is the most common type (baseline: 43%; noise: 42%). Subjects in this category helped in 63% of all encounters. They requested IDR in 56 %, IIR in 39% of the rounds, and both in 9%. In 20.1% of these cases (165 observations) the two sources of information conflicted, i.e., the sign of the image score in the indirect information did not correspond to the sign of the score in the direct information. Table 2.5 shows choices for this group of donors. Although this is a relatively small sample of all observations, it reveals a similar preference for direct reciprocity as the information requests pattern: in 63 % of these conflicting cases people make a decision in line with the direct information they received. Noise changes this pattern slightly (and counter-intuitively) towards more indirect information, but not significantly so.

**Table 2.5. Conflicting information**

	Baseline	Noise	Overall
Subjects behaved in line with IDR	69 %	60 %	63 %
Subjects behaved in line with IIR	31 %	40 %	37 %

*Notes.* Percentage of decisions that were in line with either IDR or IIR when the two scores were conflicting.

**IDR.** The third category consists of subjects who mostly requested direct reciprocity information (on average in 56 % of the rounds), but hardly ever requested indirect information. There is no significant treatment effect on the proportion of these types (MW,  $N=20$ ,  $z=0.86$ ,  $p=0.39$ ); these IDR types offered help more often in the baseline treatment than in noise (70% vs. 47%) but not significantly so at the group level (MW,  $N=8$ ,  $z=1.64$ ,  $p=0.10$ ).

<sup>17</sup> One of them exercised a distinct repeated pattern consisting of one time ‘passing’, two times ‘helping’ - an example of the ‘score optimisers’ who cares only about their own score ( Engelmann and Fischbacher, 2009). These subjects earned 1.67 times the average, confirming that such strategic types can exploit image scorers (Leimar and Hammerstein 2001).

**IIR.** The subjects in the final category mainly asked for indirect reciprocity information. They helped in 47 % of the cases. This is less than the IDR types. Since the average of the two scores follow the same pattern, this suggests that IIR subjects require a higher score before they give help. We do not observe a treatment effect for either the proportion of subjects that fall into this category or the helping rates.

Finally, earnings do not differ significantly across types. This can be compared to the results from the evolutionary simulation, where we do not observe any single strategy dominating others.

Our results are summarized as follows.

***Result 4:*** *There is heterogeneity in behavior with respect to preferences for and reactions to reputation and direct experience. The types do not differ with respect to earnings.*

## *2.5 Discussion*

This is to our knowledge the first experimental study in which people are given the choice between direct and indirect information about their partner's history of cooperative behavior. In this chapter we have shown that humans use both direct and indirect information when deciding about a reciprocal gift, even when the indirect information is less reliable. Summarizing, we find that 1. helping levels do not decrease in a noisy environment; 2. people substitute more reliable direct information for noisy indirect information; 3. direct information has more impact on the decision to help on average, but 4. people consistently differ in their use of the two types of information. Each of these results is put into perspective below, followed by a reflection on the link between the results from simulations and experiments.

Previous studies provide all participants with information about aggregated previous behavior (Seinen and Schram, 2006; Engelmann and Fischbacher, 2009). In comparison with the findings of Seinen and Schram (2006), information in our setup came at a cost, yet helping rates appeared to be only slightly lower. This shows that people are willing to incur a cost for either type of information. It has been argued that indirect reciprocity is likely to dilute in large groups because of the noisy nature of information (Engelmann and Fischbacher, 2009). We find, in contrast, that people show sensitivity to noise not by lowering their propensity to help but by switching to their own experience. This demonstrates that people can cope with relatively unreliable gossip and respond by switching to more reliable sources of information.

As discussed in our overview of the literature, some studies have shown that behavior in an indirect setting affects choices in a subsequent direct setting and vice versa, suggesting that the two forms of reciprocity are exchangeable (Wedekind and Braithwaite 2002; Milinski et al., 2002). These findings reverberate with our observation that people substitute direct information for noisy indirect information. Our results provide direct evidence that people integrate indirect information with their own experience.

To date only a few studies have investigated whether people react differently to direct and indirect information about others' previous actions<sup>18</sup>. Studies that attempt to tease apart the effects of direct and indirect information on helping report inconclusive results (Dufwenberg et al. 2001; Bolton et al., 2005). We observe that direct reciprocity is more often decisive for the decision to help, but we find no difference in average donation rates between users of direct and indirect reciprocity information. Neither do we find that helping rates immediately after receiving positive direct information differ significantly from helping rates after receiving positive indirect information; nor do helping rates differ after negative direct versus negative indirect information. Only when we compare direct information to noisy indirect information do we find a difference in helping: after positive (negative) noisy information we observe less (more) helping than after direct information. This dampening effect of noisy indirect information on helping rates can be compared to the observation in Sommerfeld et al. (2007), who find that third party information (which was not necessarily accurate) leads to a less pronounced reaction to positive or negative behavior than the direct observation of that behavior.

Like in real life situations, most participants in our experiment use indirect information alongside direct information; those who do not combine them show a clear preference for either of the two. The heterogeneity in strategies we observe does not lead to large differences in earnings. The finding that people differ consistently in how they use and weigh the two kinds of reciprocal information suggests that studies that do not distinguish between the two risk overlooking structural behavioral patterns (cf. the literature on personalities in behavioral ecology, Wolf et al. (2008)).

The use of direct reciprocity has been established in various species (Dugatkin, 2002) and some species appear to use mental bookkeeping in order to direct help towards individuals that have been helpful to them (Krams et al. 2008, Wilkinson 1984). Indirect reciprocity, on the other hand, has only occasionally been documented in other species (Jansen and Van Baalen, 2006), but is widespread among humans (Nowak and Sigmund 2005). Models taking direct

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<sup>18</sup> Van den Broek and Hopfensitz (2010, working paper) disentangle the effects of emotions evoked by direct experience versus third party reputation information in a repeated trust game.

and indirect information into account have thus far been restricted to strategies using either of the two (Roberts, 2008). Our simulations suggest that strategies incorporating both types of information are key to the invasion of a strategy using indirect reciprocity information. Although simulations are sensitive to parameter settings and the specific properties of the implementation, these predictions were echoed by the experimental findings. Broadly speaking, they suggest that the use of indirect information in addition to direct information can evolve and be maintained, even when indirect information is less reliable than direct information and all other agents use only direct information. Indeed, we observe distinct self-consistent strategies in the human population that do not yield differences in earnings. These behavioral results suggest that there is not much evolutionary pressure on individual variation in preferences for direct and indirect reciprocity information.<sup>19</sup> Together, these results may contribute to the long-standing issue of how the use of reputations has gained a foothold in social interactions. The combination of simulation and experimental results indicate that people use costly information on noisy indirect interactions in addition to information on direct interactions, and that it is adaptive to do so.

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<sup>19</sup> An alternative explanation would be that disruptive selection causes different personalities to arise that coexist in a polymorphism (Wolf et al., 2011).



## Chapter 3: Public goods and private aversions

### 3.1 Introduction<sup>20</sup>

In the previous chapters I studied situations in which people were confronted with someone to whom they could offer help. In everyday life, such confrontations are more than simply random events. Instead, people choose with whom they interact; moreover, acting cooperatively is not limited to one person but may extend to a group. In this chapter I shift the focus from dyadic helping to cooperation in groups and consider the relevance of partner choice. I do so in a setting of voluntary public good provision.

A public goods game, as described in Chapter 1, captures a social dilemma in which the individual interest of the group members conflicts with the collective interest. A general finding of laboratory studies of such games is that members' contributions to the public good decline as the dilemma is repeated. Many studies have been devoted to formal<sup>21</sup> and informal enforcement mechanisms that may sustain long-term efficiency. Two informal mechanisms stand out as particularly effective in solving the free-rider problem: costly punishment of fellow group members and the possibility to form groups with preferred partners (Chaudhuri, 2011).

In the world outside the laboratory, the options for punishment and endogenous group formation may often occur simultaneously. People choose with whom they prefer to interact and punish or exclude others. The interplay between partner choice on the one hand, and punishment on the other hand, is not straightforward. Groups may use exclusion as a substitute for or an extreme form of punishment (Ahn et al., 2008). Alternatively, the two informal mechanisms may enforce each other's effects: a group member who punishes fiercely may be valued either as a reliable partner (Nelissen, 2008) or, conversely, excluded out of fear for retaliation.

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<sup>20</sup> This chapter is based on Van den Broek, Kocher and Schram (working paper, 2010).

<sup>21</sup> Formal enforcement mechanisms include for instance exclusion from club goods on the basis of formal contracts and agreements (Buchanan, 1965).

To date, endogenous grouping and punishment have been studied in isolation, however. An important part of the literature on endogenous group formation addresses the exclusion of group members. Granting players the power to exclude fellow players may enhance cooperation in two ways. Firstly, the threat of irreversible expulsion may be enough to discipline free-riders. In Cinyabuguma et al. (2005) participants could exclude fellow group members through a majority voting system. Low contributors avoided expulsion by increasing their contributions after receiving a high number of exclusion votes. Secondly, endogenous group formation through exclusion, but also through voluntary matching, may raise average contributions by assortment. For example, the formation of groups consisting of a majority of conditional contributors (who constitute around 50% of the population, Fischbacher et al., 2001) increases contributions (Gunnthorsdottir et al., 2007).

Our experiment adds to an emerging literature on endogenous grouping that started with Ehrhart and Keser (1999). Here, costly migration sustained high cooperation levels but created iterated chasing of cooperators by free riders. Other mechanisms of endogenous group formation also sustain high cooperation levels, like majority voting about entry and exit of group members with evolving group size (Charness and Yang 2010). Group reduction or ostracism, the inverse of group formation, is another effective institution (Maier-Rigaud et al. 2005). Subjects use exclusion to punish ‘unfair’ behavior (non-strategic reason) and expect changes in behavior in response to exclusions (strategic reason; Masclet et al., 2003).

To our knowledge, the only study that combines endogenous group formation with punishment is Page et al. (2005)<sup>22</sup>. In their setup subjects can rank each other’s candidacy as a co-member and groups are formed that optimize agreement on candidacy. Ranks are based on contribution levels only, and subjects cannot exclude each other with certainty. Although ranking is costly, 80% of the subjects always use the ranking opportunity, showing that ranking alone is a potentially strong mechanism. There are two drawbacks to this combination of ranking with punishment, however. Outside of the laboratory, we not only prioritize between people but also exclude them; and secondly, a most important aspect of the interplay between punishment and partner preferences is the reputation effect of punishing. In Page et

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<sup>22</sup> In Ones and Putterman (2007) low contributors are exogenously matched with punishers, which increased contributions.

al. (2005) it remains unclear how punishers are viewed; i.e., would they be excluded or preferred.<sup>23</sup>

This chapter deals with the empirical questions that arise from the interplay between endogenous group formation and punishment. Our main research questions are

- (i) To what extent do the two mechanisms, punishment and endogenous group formation (including exclusion), increase contributions to a public good?
- (ii) How efficient are these mechanisms?
- (iii) Are they complements or substitutes?

We present a laboratory environment with fixed group size, where we allow both for ranking and exclusion of partners based on information on their previous contribution and punishment choices. We find that endogenous group formation with exclusion creates high cooperation and efficiency levels. In combination with punishment, exclusion yields even higher contributions.

The remainder of this chapter is organized as follows. The following section presents a brief overview of the theory applicable to the environment we study in our experiments. A description of the experimental procedures and design in Section 3 is followed by the results in Section 4. Section 5 concludes.

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<sup>23</sup> In Rockenbach and Milinski (2011), a single observer may learn about contributions and punishment behavior in the first 15 periods and exclude a partner for the second remaining periods. Here, punishment did not have an effect on eligibility as a co-player.

### 3.2 Game description and theoretical predictions

The game consists of three stages. At stage 1, groups are either exogenously formed, or endogenously determined by individuals' preferences with respect to whom they would like to be matched with. At stage 2, individuals participate in a public goods game (see Chapter 1) where each member of a group decides whether or not to contribute to a public good. At stage 3, members of the group may decide to enforce costly punishment on any other member(s).

Starting with stage 2, consider the standard linear public goods game as introduced in Chapter 1. Let  $I = \{1, 2, \dots, m\}$  denote a group of  $m$  subjects who interact in  $T$  periods. In each period  $t \in \{1, 2, \dots, T\}$  individual  $i \in I$  receives an endowment  $\omega$  which can be allocated either to a private good or a public good. The voluntary contribution of individual  $i$  to the public good in period  $t$ ,  $g_{i,t}$ , is a binary choice, *i.e.* it must satisfy  $g_{i,t} \in \{0, \omega\}$ . The marginal per capita return from investing in the public good is denoted by  $a$ , and satisfies  $0 < a < 1 < ma$ , meaning that the self-interested choice and the socially optimal one are in conflict.

Consequently,  $i$ 's payoff is

$$\pi_{i,t} = \omega - g_{i,t} + a \sum_{k=1}^m g_{k,t} \quad (1)$$

At beginning of stage 3, each group member is informed about the individual contributions by the other group members and decides whether to punish other individuals in his group. Punishment is costly for the punisher as well as for the punished member. We implement punishment as a *binary* decision.<sup>24</sup> The effectiveness of punishment is captured by the variable  $c$ . Taking into account the monetary consequences of the second stage in each period yields the following payoff function for  $i$ .

$$\pi_{i,t} = \omega - g_{i,t} + a \sum_{k=1}^m g_{k,t} - c \sum_{k \neq i} p_{ik,t} - \sum_{h \neq i} p_{hi,t} \quad (2)$$

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<sup>24</sup>The reason to choose a simple binary punishment technology is that it creates relatively simple summary statistics of any particular individual's choices. These statistics may be used by others when deciding on with whom they wish to be matched in the endogenous group formation of stage 1.

where  $p_{kh,t} = 1$  if member  $h$  has punished member  $k$  in period  $t$  and zero otherwise and the costs of punishing have been set to 1.

The novel element in our experiment takes place at stage 1. Individuals observe information about contributions,  $g_{h,t}$ , and punishment decisions,  $p_{kh,t}$ , by all other  $h$  in the population in all previous periods  $\tau < t$ . Subsequently, they can use this information to rank other individuals in terms of how much they would like to be in a group with them in period  $t$ . In doing so, they may exclude specific others altogether. A matching procedure is then used to form groups in accordance with these preferences.<sup>25</sup>

Assuming that subjects care only about their own monetary payoffs and assuming common knowledge of rationality, a risk-neutral decision maker will abstain from costly punishment. Since the game is finitely repeated, backward induction yields the standard result that the contribution decision will not be affected by the possibility of punishment. Consequently, the game will yield zero contributions (*i.e.*  $g_{i,t} = 0$  for all  $t$ ) because free-riding is a dominant strategy due to  $a < 1$ . Therefore common knowledge of selfishness and full rationality implies that all individuals will be indifferent with respect to whether they end up in a group or not, and whom they will be grouped with. The theoretical prediction in case of selfish preferences is therefore that any group may be formed at stage 1; there are no contributions at stage 2 and no punishment at stage 3.

Empirically, such reasoning proves to be of very limited use, because the slightest belief of somebody contributing a positive amount would induce even a purely selfish player to prefer being in a group with this person. As long as the threat of punishment is used rationally (in line with social preferences), there are equilibria (e.g., in Fehr-Schmidt models, see Chapter 1) in which everyone wants to join a group and contributes positive amounts, regardless of whether players are selfish (Kosfeld, 2009). The underlying intuition of such equilibria is that once punishment is a credible threat, everyone in a group will contribute. If all contribute,

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<sup>25</sup> Cf. section 3.3 for details about the matching procedure used in our experiment.

everyone is welcome in all groups<sup>26</sup>. For cases where (some) individuals have other-regarding preferences, such results allow us to formulate the expected patterns per type. As for group formation, free riders prefer to team up with cooperators (whether punishment is possible or not); but cooperators would not reciprocate this preference. The latter holds even if cooperators have the option to punish free riders. This is because punishment costs are expected to be lower in groups without free riders. If punishment is possible, free riders try to avoid punishers; cooperators may be indifferent towards punishers. Finally, the predictions for contributions differ on whether punishment is possible. If it is, free riders may believe that the threat of punishment is so high that they actually contribute, if not, they will not.

It is beyond the scope of this chapter to develop a more detailed formal model of how preferences about group composition and exclusion are formed and how they develop based on observed choices. Ultimately, we aim for empirical evidence on the effects of endogenous group formation (with an exclusion possibility) and, particularly, on its interplay with punishment.

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<sup>26</sup> In a different vein, Brekke et al. (2007) show that the fear of exclusion from a high-contribution team may lead to high cooperation levels. Hirshleifer and Rasmusen (1989) show that equilibrium with cooperation exists in a finitely repeated public goods game with costless expulsion.

### 3.3 *Experimental design and procedures*

A total of 90 subjects participated in 6 sessions of the experiment in March and April 2008 at the CREED laboratory in Amsterdam. Subjects were students with a variety of majors, including economics (33%) and psychology (24%). Each session lasted approximately 90 minutes and subjects earned on average € 24.06 including a show-up fee of € 7.

Subjects were brought into the laboratory and told that they would participate in two experiments. The first consisted of a value orientation test (Offerman *et al.* 1996) and served to obtain an independent measure of each participant's social value orientation (see Appendix A for further details).<sup>27</sup> After completion of this first experiment, subjects were informed that the second experiment would comprise two independent parts, that Part I would last 10 periods and that they would receive instructions for Part II after Part I had been completed. After the computerized instructions (see Appendix A) they completed a quiz to check for understanding. The experiment was computerized.<sup>28</sup> No participant was informed about the identity of others. Earnings in the experiment were in 'francs'. Aggregate earnings were exchanged for euros individually and privately after the experiment, at an exchange rate of € 0.03 for each franc.

Our experimental treatments in the second experiment are based on the public goods games introduced in the previous section. Part I consists of a standard ten-period game ( $T = 10$ ) with or without punishment (depending on the treatment). In both cases, subjects are in groups of  $m = 3$ ; the composition changes after each period within a matching group of nine. In each period, each subject is endowed with  $\omega = 20$  francs (equivalent to € 0.60). They have a binary choice of either keeping the endowment or investing it in a public account (which we call a "group project" in the experiment). We use an MPCR of  $a = 0.5$ , which means that the sum of the group members' contributions in a period is multiplied by 1.5 and then equally divided amongst the three members. In the treatment with punishment, each subject is informed about the group members' decisions and can subsequently decide to allocate one "subtraction point"

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<sup>27</sup> Details about the results of the first experiment are available upon request. The second experiment is the main focus of this chapter.

<sup>28</sup> We thank CREED programmer Jos Theelen for writing the Delphi program. It is available upon request.

to either or both of the other two in the group. This is again a binary decision, where allocation of a point costs the individual giving it 1 franc and the member receiving it  $c = 3$  francs.

In Part II the game is exactly the same as in Part I (either with or without punishment). The difference is that groups are no longer exogenously formed. In stage 1 of Part II, subjects are matched according to their own preferences as expressed in their willingness to be in a group with specific other subjects. This is organized as follows. First, subjects are, like in Part I, allocated to matching groups of nine that remain constant throughout Part II. In each of the ten periods, at most three groups of three are formed. Group formation takes place in three steps in each period. In step 1, three players are randomly selected to be *proposers* (called “type A” in the experiment). The remaining six players are *responders* (“type B”). Note that most players will be proposer in some periods and responder in others. A group always consists of one proposer and two responders.

In step 2, proposers are asked to submit a preference profile over all responders. Before doing so, they are informed about each responder’s contributions to the group project and (if applicable) her allocation of punishment points in all previous periods.<sup>29</sup> A preference profile consists of a score for each of the six responders on an ordinal 6-point scale ranging from 1=“highly preferred” to 6=“least preferred”. Ties are allowed. It is also possible to exclude one or more responders from the proposer’s group by not giving them a score.<sup>30</sup> While the proposers are scoring the responders, the latter have to indicate for each of the proposers their willingness to be in a group with her. This is a binary decision and can be based on information we provide about the proposers’ contributions and punishment decisions in all previous periods.<sup>31</sup>

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<sup>29</sup> During the group formation stage, subjects can scroll through information about the contribution, number of punishment points assigned and the earnings of the other players in every previous period. See the instructions in Appendix A for an example. A “–” appears in the table if a subject had not participated in the public good game in a specific period. The same information remains visible in the subsequent contribution phase.

<sup>30</sup> For example, a proposer submitting scores 2,3,1,–,2,– for respondents 1...6, respectively, indicates that she refuses to be in a group with respondents 4 and 6, that she most prefers to be with 3 followed by a tie for 1 and 5 and that respondent 2 is the least preferred of the acceptable ones. Note that the ranking is ordinal, submitting 2,3,1,–,2,– indicates the same preference as 4,6,3,–,4,–.

<sup>31</sup> Numbers indicating specific proposers or respondents are scrambled in each period such that individuals cannot be identified across rounds.

In step 3 the preferences of proposers and responders are used to form groups in the following way: a proposer is randomly chosen and matched with her two most preferred responders.<sup>32</sup> If the preferred responders have agreed to join this proposer, a group is formed; otherwise, the next preferred responder that is willing to join that proposer will be chosen. If it is not possible to find two responders that are acceptable for the proposer and at the same time are willing to join her, this proposer is not allocated to a group in that period. This procedure is repeated for the other proposers with responders who have not yet been assigned. All players that have not been assigned to a group at the end of step 3 receive their endowment  $\omega$  but cannot play the public goods game in the period concerned or allocate punishment points and receives no information during that period about the other players.

For each of the six responders coupled to a proposer, the proposer can either indicate a score on a six-point scale or no score if she wishes to exclude them. A responder can either agree or decline to play with each of the three proposers, in the latter case effectively excluding someone from her group. Each type of player can effectively withdraw from playing by excluding all players of the other type. Aside from giving us a procedure to develop groups based on preferences, this structure provides detailed information allowing us to carefully analyze social exclusion. We can do so by comparing per player the choice to exclude someone when the player is a responder, i.e. when the only other option is to agree to join, with the choice to exclude someone when the player is a proposer, i.e. when the alternative includes the possibility to assign a relatively low score<sup>33</sup>.

In short, our design consists of a repeated public goods game and distinguishes between two treatments variables: (i) punishment opportunities versus no-punishment, and (ii) exogenous versus endogenous group formation. We varied (i) between subjects and (ii) within subjects. The reason for the latter choice is that it allows subjects to get acquainted with the game (in Part I) before they need to form preferences about group membership in Part II. Table 3.1

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<sup>32</sup> In case of a tie, one is chosen at random.

<sup>33</sup> In the treatment without punishment, excluding is weakly dominated by assigning a relatively low score to someone, because a small chance that one of the group members contributes a positive amount makes it more profitable to be in a group than to end up alone.

summarizes our treatments and gives the number of independent observations (*i.e.* matching groups) in each treatment.

**Table 3.1. Treatments**

	<b>Baseline treatment</b> <i>(4 groups)</i>	<b>Punishment treatment</b> <i>(6 groups)</i>
<b>Part I (10 periods)</b>	Random grouping Contribution	Random grouping Contribution Punishment
<b>Part II (10 periods)</b>	Endogenous grouping <ul style="list-style-type: none"> <li>• type allocation</li> <li>• preference elicitation</li> <li>• grouping</li> </ul> Contribution	Endogenous grouping <ul style="list-style-type: none"> <li>• type allocation</li> <li>• preference elicitation</li> <li>• grouping</li> </ul> Contribution Punishment

*Notes.* Timeline of treatments. Matching groups will be used as the unit of observation for our statistical tests.

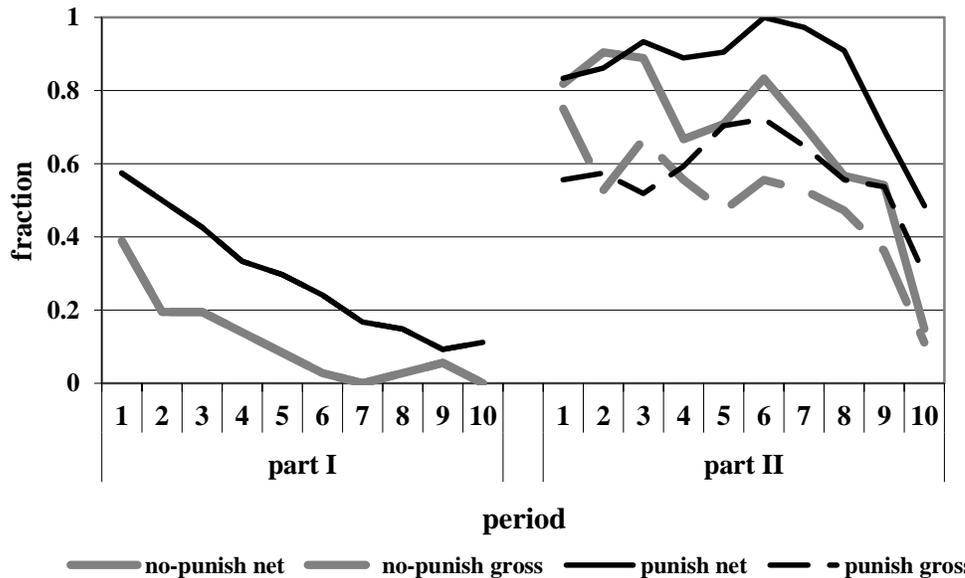
### 3.4 Experimental results

We start this section with a general overview of contributions and efficiency (section 4.1). The overview is followed by an analysis of punishment and its effect on contributions (section 4.2). In section 4.3, we analyze the choice to exclude and individual preferences with respect to the other group members. Finally, in section 4.4, we will compare between distinct mechanisms and analyze how our subjects choose what to use.

#### 3.4.1 Contributions

Figure 3.1 gives an overview of the fraction of subjects contributing their endowment to the public good. For part II, it distinguishes between gross fractions (the number of contributions divided by the total number of individuals) and net fractions (the number of contributions divided by the number of individuals that are allocated to groups). Treatments with and without punishment are shown separately.

**Figure 3.1. Contributions to the public good over time**



*Notes.* For each period, the graph shows the fraction of participants that contributed their endowment to the public good. Black (grey) lines show the fraction for treatments with(out) punishment. In part II solid lines show the ‘net’ contributions, i.e., as fraction of participants who have been allocated to groups and dashed lines show the ‘gross’ contributions, i.e., as a fraction of all participants.

Four things stand out in figure 3.1. First, far more participants contribute in part II than in part I. This holds for the whole population but even more so if we only look at participants that have been allocated to groups. The introduction of endogenous group formation increases average (gross) contribution levels from 0.11 to 0.5 without punishment and from 0.29 to 0.57 with punishment.<sup>34</sup> The increase is statistically significant in the treatments with and without punishment ( $p < 0.01$ , resp.  $p = 0.02$ ; Wilcoxon-signed-rank tests<sup>35</sup>). Second, the possibility of punishment yields higher contribution levels in both parts I and II. In part I, participants contribute 11% of the time without and 29% with the opportunity to punish. This difference is statistically significant on the level of matching groups ( $p = 0.05$ ; Mann-Whitney-U test). In part II, punishment yields an increase in (net) contributions from 43% to 57%. The difference, however, is not significant ( $p = 0.16$ ; Mann-Whitney-U test). The opportunity to punish group members does not have significantly less of an effect if participants have a say in the formation of their groups ( $p = 0.17$ ,  $F(1,8) = 2.22$ ). Third, while contributions gradually diminish towards zero in part I, they remain at higher levels longer in part II.

Finally, some individuals are excluded from groups. This can be seen from the fact that the gross fraction is always lower than the net fraction (with one exception in the final round), meaning that the denominators in the net fractions (*i.e.*, the number of individuals in groups) must be lower than the corresponding denominators in the gross fractions (the number of individuals). We will closely analyze the pattern of exclusion below. At an aggregate level, the number of groups that is formed is an indication of the extent of exclusion. Without punishment, 74% of the possible groups are formed and with punishment the percentage is 67%. The difference is statistically not significant ( $p = 0.32$ ; Mann-Whitney-U-test).

**Results 1:** (a) *Endogenous grouping significantly increases contributions levels, both in the treatments with and without punishment.* (b) *Punishment increases contributions.* (c)

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<sup>34</sup> We compare gross contribution levels because they are the more straightforward indicators of cooperation within the whole population.

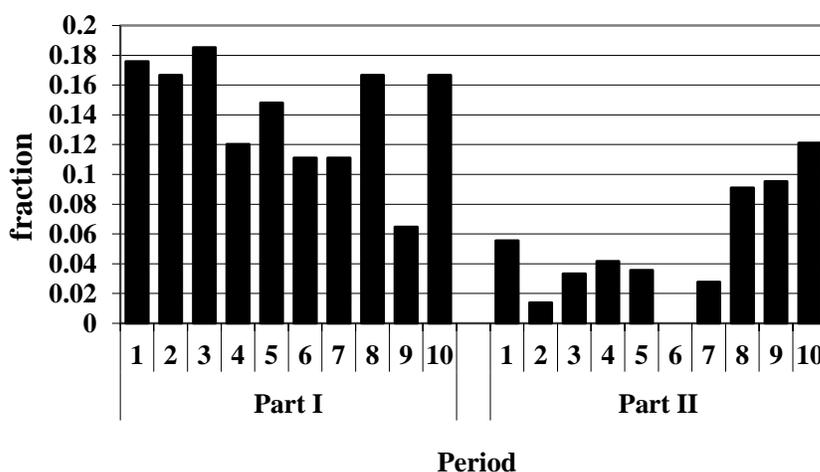
<sup>35</sup> Unless indicated otherwise, tests reported in this section use matching groups consisting of nine subjects as the unit of observation.

*Endogenous grouping leads to the exclusion of almost one out of every three participants. (d) High contributions through endogenous grouping break down in the final rounds of the game.*

### 3.4.2 Punishment behavior

Figure 3.2 gives an overview of the frequency with which participants punished other group members as a fraction of the total number of times they could have punished them. For part II this fraction is calculated for the groups that were endogenously formed. The figure shows that punishment is lower across all rounds in part II (5 % of all punishment opportunities are used) than in part I (14%). The difference is statistically significant ( $p = 0.05$ ; Wilcoxon-signed-rank test). Note, however, that the frequency of punishment increases in the final rounds of part II (but remains less than the average in part I).

**Figure 3.2. Punishment behavior over time**



*Notes.* For each period, bars show the fraction of times participants punished group members.

The fact that participants punish less in part II can be attributed to three possible causes. First, they may have less reason to punish because their group members are contributing more (*cf.* figure 3.1). Second, they may use punishment less because there are other mechanisms (group formation and exclusion) that they can use to enforce cooperation. Third, they may be excluded if they punish. To distinguish between these options, we ran random effects probit regressions where the decision to punish is explained by a number of variables, including the group member's contribution decision. We do so separately for parts I and II. If an individual

responds differently to identical situations in both parts, this is support for the second explanation for reduced punishment. Table 3.2 presents the results.

**Table 3.2. Determinants of the decision to punish**

	<b>Part I</b>	<b>Part II</b>
<b>Own contribution</b>	0.65***	-0.068
<b>Third party contribution</b>	-0.61***	-0.94***
<b>Negative deviation</b>	1.37***	2.13***
<b>Positive deviation</b>	-0.86***	1.89***
<b>Period</b>	-0.23**	0.07
<b>Period<sup>2</sup></b>	0.02***	-0.00
<b># observations (groups)</b>	1080 (4)	1080 (6)
<b>Wald chi<sup>2</sup> (6)</b>	201.41***	48.17***

*Notes.* The table presents the results of a random effects probit regression used to explain punishment of  $j$  by  $i$ . Formally, it gives the marginal effects at the means in  $P_{jit} = \Phi(\sum_i X'_{ijt}\beta + \mu_m)$  where  $P_{jit}$  is the probability that  $i$  punishes  $j$  in period  $t$ ;  $\Phi$  denotes the cumulative normal distribution and  $X_{ijt}$  is a vector of independent variables relating to  $i$  and  $j$  in  $t$  as described in the first column of the table.  $\mu_m$  is a (white noise) matching-group-specific error that corrects for the dependencies within matching groups. The independent variables are defined as follows. “Own Contribution”= dummy variable equal to 1 if  $i$  contributed in  $t$ ; “Third party contribution”= dummy variable equal to 1 if third member contributed in  $t$ ; “Negative deviation”=dummy variable equal to 1 if  $j$  did not contribute and there was at least one contribution; “Positive deviation”=dummy variable equal to 1 if  $j$  contributed and there was at most one other contribution; “period”= period number; “period<sup>2</sup>”=period number squared.\*=statistically significant at the 10% level; \*\*=statistically significant at the 5% level; \*\*\* = statistically significant at the 1% level.

The results show that individuals react strongly to a negative deviation of the other’s contribution, even when controlling for own contribution and third party contribution. In the first and the second part, an individual is significantly more likely to punish a non-contributor if the other two group members (including the punisher) did contribute. The own contribution has a significant positive effect on punishment behavior in Part I and no effect in Part II. This may reflect a shift from punishment to exclusion; alternatively, it may result from a selection

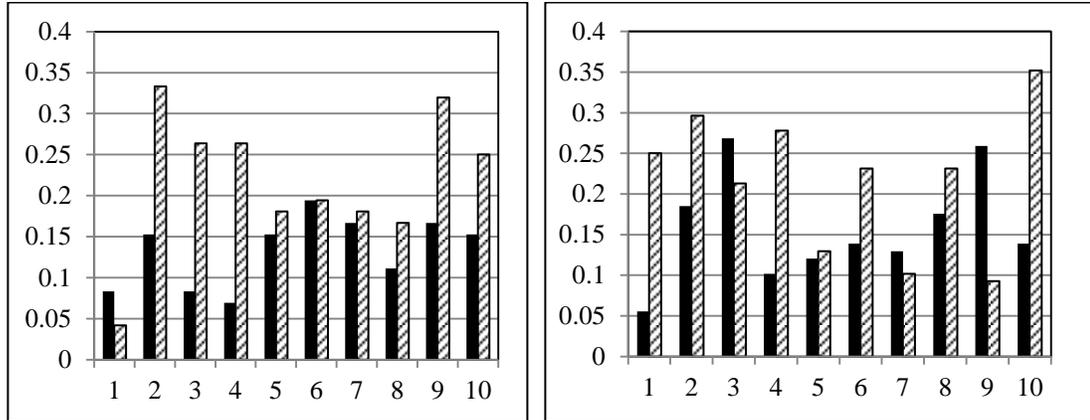
effect, since contributing participants are more likely to be included in a group. Third party contribution matters in both parts. The effect of a positive deviation of the punished person changes from a significantly negative to a significantly positive effect in Part II, probably because almost everyone contributes. In Part I, period and squared period have a significant effect; in Part II the effects disappear. Taken together, these results suggest that while multiple factors affect the decision to punish in part I, punishment in Part II is more focused towards negative deviators, as the increase in the negative deviation suggests. These differences show that the overall reduction in punishment is only partly explained by the higher contributions in the second part; participants resort to other methods than punishment. The fact that the dummy for negative deviation does not disappear in Part II suggests that fear of exclusion cannot explain the reduction in punishment completely.

***Result 2:*** *The introduction of endogenous grouping partly crowds out punishment and directs punishment more strongly towards negative deviators.*

### *3.4.3 Exclusion and ranking*

In the treatment without punishment, subjects excluded on average 18 % of the potential partners, compared to 19 % in the punishment treatment. Proposers excluded 15 % of the responders, while responders excluded 22 % of the proposers from interaction in the forthcoming period (MW,  $z=-1.82$ ,  $p=0.07$ ). The correlation between a subject's decisions to exclude as a proposer and as a responder is 0.27 ( $p < 0.01$ ; Spearman rank correlation between the exclusion percentage of an individual in the role of responder and in the role of proposer). Figure 3.3 gives the pattern of exclusion over time; it shows no trend across periods.

**Figure 3.3 Exclusion over time**



*Notes.* For each period, bars show the fraction of potential partners that proposers (solid bars; fraction excluded per 6 options) and responders (striped bars; fraction they excluded per 3 options) excluded in the treatments without (left) and with punishment (right).

As a consequence of exclusion, on average 33 % (26 %) of all subjects did not participate in a group in the treatment with(out) punishment, either because they were deliberately excluded, or because they withdrew by excluding all others, or because all others they were willing to match with were already allocated to other groups. The difference between the two treatments is statistically not significant ( $p = 0.32$ ; Mann-Whitney-U test) and does not reveal a clear pattern over time.

To further analyze the possible determinants of the decision to exclude others we ran random effects probit panel regressions. The binary decision by  $i$  whether to exclude  $j$  in period  $t$  ( $\text{exclude}_{ijt}$ ) is explained by variables related to  $i$ 's and  $j$ 's history of contributing and punishing, as well as a dummy distinguishing between proposers and responders. Random effects on the matching group level correct for dependencies within the matching groups. We ran separate regressions for the punishment treatment and the baseline treatment and

distinguish between models with lagged variables (Model I and II) and models with averages of previous choices (Model III and IV). Table 3.3 presents the results<sup>36</sup>.

**Table 3.3 Determinants of i's decision to exclude j**

<b>Model</b>	<b>I</b>	<b>II</b>	<b>III</b>	<b>IV</b>
<b>Treatment</b>	<b>Baseline</b>	<b>Punishment</b>	<b>Baseline</b>	<b>Punishment</b>
<b>Subject</b>	-0.00*	0.00	-0.00	0.00
<b>Responder (dummy)</b>	0.31***	0.19***	0.47***	0.19**
<b>Period</b>	0.21***	0.13**	0.01**	-0.05
<b>Period<sup>2</sup></b>	-0.02***	-0.01**	0.02**	0.01
<b>play i (t-1)</b>	0.28**	0.23***		
<b>play j (t-1)</b>	0.90***	0.91***		
<b>contribution i (t-1)</b>	Coll	0.13		
<b>contribution i (average)</b>			0.51***	0.34***
<b>punishment i (t-1)</b>		0.07		
<b>punishment i (average)</b>				0.14
<b>contribution j (t-1)</b>	-2.70***	-2.24***		
<b>contribution j (average)</b>			-0.20	-0.77***
<b>punishment j (t-1)</b>		0.39**		
<b>punishment j (average)</b>				1.31***
<b># observations (groups)</b>	1440 (4)	2160 (6)	1296 (4)	2160 (6)
<b>Wald Chi<sup>2</sup></b>	239.78***	393.05***	45.48***	148.48***

*Notes.* The table presents the results of four random effects probit regression used to explain exclusion of  $j$  by  $i$ . Formally, it gives the marginal effects at the means in  $P_{jit} = \Phi(\sum_i X'_{ijt}\beta + \mu_m)$  where  $P_{jit}$  is the probability that  $i$  excludes  $j$  in period  $t$ ;  $\Phi$  denotes the cumulative normal distribution and  $X_{ijt}$  is a vector of independent variables relating to  $i$  and  $j$  in  $t$  as described in the first column of the table.  $\mu_m$  is a (white noise) matching-group-specific error that corrects for the dependencies within matching groups. Coll means the variable is collinear. Only data

<sup>36</sup> We ran a logit with the same specifications with similar results, in which no collinearity showed up. Results are available on request.

are used where  $i$  had to make a decision about  $j$ . The lagged contribution of the excluder is collinear with having played in the previous period. Model I and II include only lagged variables; Model III and IV only averages over previous periods.

The results show again that responders exclude more than proposers, also in the baseline treatment where they could not punish free riders. Having played in the previous round increases the chances of excluding and being excluded. This could be due to two reasons. Either someone who did not play does not reveal any (potentially negative) information about previous behavior, and is therefore not excluded; or it could be due to a selection effect, since people who did not play may have been excluded in the previous round because of a history of non-contributing. This statistically increases the likelihood that someone who played in the previous period has a good track record.

Having contributed decreases the chance to be excluded, whereas punishing increases it. The models including averages confirm the picture. For free riders, it is intuitive that they exclude punishers; for contributors, the effect may be due to fear for antisocial punishment (Herrmann et al., 2008). Finally, the positive coefficients for the average contribution of the excluder show that on average subjects who contribute more often also exclude more often. This, too, is understandable: they have more to lose.

Although neither the own average punishment nor punishment in the previous period influences exclusion significantly, a difference between the treatments is the importance of the average contribution of the excluded. In the punishment treatment, this average contribution has a negative effect on exclusion, as expected; however, in the baseline treatment, this effect is not significant<sup>37</sup>. Overall, contributors exclude more often than free-riders, irrespective of whether they can punish or not. This result is in line with the predictions formulated in 3.2.

**Results 3(a) Low contributors and punishers are excluded more often. (b) Contributors exclude more than free-riders.**

Aside from exclusion, proposers can also reveal preferences about the responders they want to be grouped with by giving scores on a scale from 1 (high score) to 6 (low score). These scores

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<sup>37</sup> A possible explanation is that contribution in the previous period is used as a cut-off rule for exclusion and average contribution is used for ranking.

allow us to investigate the factors that make a responder a relatively desirable group member. To do so, we ran an ordered probit regression explaining the scores given to a certain responder in round  $t$  by previous choices made by the involved proposer and responder.<sup>38</sup> Table 3.4 presents the results, separately for the cases with and without punishment and again distinguishing between a model with lagged variables and one with averages over previous rounds.

**Table 3.4. Determinants of the proposer's score of responders**

<b>Model</b>	<b>V</b>	<b>VI</b>	<b>VII</b>	<b>VIII</b>
<b>Treatment</b>	<b>Baseline</b>	<b>Punishment</b>	<b>Baseline</b>	<b>Punishment</b>
<b>Subject</b>	0.000	0.00**	0.00	0.00***
<b>Period</b>	-0.30	0.00	-0.01	-0.14
<b>Period2</b>	0.02	0.00	0.00**	0.01**
<b>contribution i (t-1)</b>	-0.21	-0.04		
<b>contribution i (average)</b>			0.10	0.12
<b>punishment i (t-1)</b>		0.06		
<b>punishment i (average)</b>				0.26
<b>contribution j (t-1)</b>	-0.99***	-1.01***		
<b>contribution j (average)</b>			-0.29*	-0.17
<b>punishment j (t-1)</b>		0.51***		
<b>punishment j (average)</b>				1.78***
<b># observations (groups)</b>	805 (4)	1493 (6)	805 (4)	1493 (6)
<b>Log Lkh</b>	-513.51	-511.10	-539.21	-517.69

*Notes.* The table presents the results of four ordered probit regressions, used to explain the score of  $j$  (a responder) by  $i$  (a proposer). Ranking options ranged from high (1) to low (6) (exclusion (7) not included in this table). Formally, it gives the marginal effects at the mean of a vector of independent variables relating to  $i$  and  $j$

<sup>38</sup> In these regressions, we disregard responders that were excluded by the proposers (*i.e.*, not given a score). Alternatively, one could consider exclusion as the lowest score (*e.g.*, a score of 7). We prefer not to do so, because we consider exclusion (as analysed in table 3) to be a qualitatively different decision than scoring. The alternative regressions are available upon request.

in  $t$  as described in the first column of the table. A (white noise) matching-group-specific error corrects for the dependencies within matching groups. Only data are used where  $i$  had to make a decision about  $j$ . Model V and VI include only lagged variables; Model VII and VIII include averages over previous periods.

The results show that proposers prefer responders who contributed in the previous period. Consistent with their exclusion decisions, they dislike subjects who punished more in the previous period and prefer subjects who contributed more. In the models with average variables, responder's average punishment weighs more heavily than her average contribution. Remarkably, average contributions of the responder influence neither their rank nor whether they are excluded, whereas their contribution in the previous period does. The variables controlling for the proposer's own contribution and punishment have no significant effect on ranking.

When we compare the above result to the determinants of exclusion (result 3a and 3b), it appears that exclusion and low ranks are spurred by similar circumstances. Both free riders and punishers are excluded and receive lower ranks. A participant's own contribution increases exclusion rates but not low ranking. Participants with a steady inclination to contribute (who contributed in previous rounds, too) may exclude co-players more readily and therefore end up in table 3.

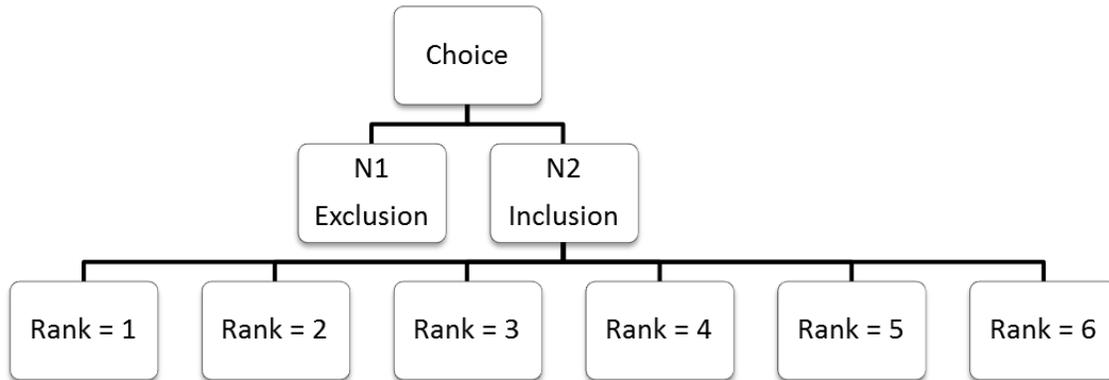
***Result 4: Lower preferences are given to non-contributors and to punishers.***

The decision to rank a group member may be a two-stage decision, where a person first decides whether or not to exclude someone, and subsequently how to rank her. In such a nested model there are two nests (exclusion<sup>39</sup> or inclusion,  $N_1$  and  $N_2$ ) and six specifications in the second ranking level (rank = 1, ..., 6) (see Figure 3.4).

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<sup>39</sup> Exclusion is a degenerate nest, since it has only one alternative; so the conditional probability  $P(r=0|N=1) = 1$ .

**Figure 3.4. Choice set**



*Notes.* In the nested model there are two nests (exclusion or inclusion,  $N_1$  and  $N_2$ ) and six specifications in the second ranking level (rank = 1, ..., 6).

The independence of irrelevant alternatives (IIA) characteristic of a multinomial logit model means that the probability to choose one alternative is independent of the nature of the alternatives; in this case, that the choice to exclude someone is made simultaneously with the choice to attribute a certain rank to someone.

The “inclusive value”  $I_{ij}$  (see Schram 1990) of person  $i$  ranking person  $j$  can be defined as

$$I_{ij} = \log \left\{ \sum_{k=1}^M \exp(X'_i \beta_k) \right\}$$

with  $X'$  being the parameter vector and  $\beta_k$  the coefficients per rank. The probability to give *any* rank at all to a person (that is, the chance to not exclude a person) is then

$$Pr_{ij} = 1/[1 + \exp(X'_i \beta_0 - I_i)]$$

If we relax the IIA and allow for a different coefficient for the inclusive values we obtain the more general framework

$$Pr_{it} = 1/[1 + \exp(X'_i \beta_0 - (1 - \sigma)I_i)]$$

If  $\sigma$  differs significantly from 0, the simultaneous decision model can be rejected in favour of the sequential model and the IIA does not hold for the decision to exclude versus rank a person. If  $\sigma$  differs significantly from 1, we can reject the sequential model.

To derive the sigma, we estimated the individual coefficients of the ranks (without exclusion) by means of a multinomial logit. In the baseline treatment the decision to rank was hypothesized to depend on the previous contribution of the other and the period (based on Table 3.4). In the punishment treatment the decision to exclude and the ranking decision were explained by the punishment of the other in the previous round. With these coefficients we determine the  $I_i$ ; with these inclusive values included as a parameter, we then estimate the chance of inclusion (or ranking) given all decisions (see Appendix B for the results). In the punishment treatment, we can reject the hypothesis that exclusion and ranking are a sequential decision (Wald  $X^2=6.95$ ,  $p<0.01$ ); but we cannot reject the hypothesis that exclusion and ranking are a simultaneous decisions (Wald  $X^2 = 2.71$ ;  $p=0.10$ ). In the baseline treatment we have to reject the sequential model (Wald  $X^2 = 8.94$ ,  $p<0.01$ ) and the simultaneous model (Wald  $X^2 =7.33$ ,  $p<0.01$ ).

**Result 5:** *The decision to include and rank someone is not a sequential decision in the punishment treatment.*

#### 3.4.4 Comparing enforcement mechanisms

In part II of the treatment with punishment, proposers can choose between various mechanisms to enforce cooperation by others: punishment, ranking, or exclusion. Responders can choose between punishment and exclusion. Note that exclusion may be seen as a specific kind of punishment, since it denies the excluded participant future benefits from cooperation. Like punishing, exclusion is costly for the executer, because it increases the probability that she will not be included in any group. In this section, we compare the two enforcement mechanisms exclusion and punishment in terms of their effect on individual earnings and group efficiency. Obviously, we can only do so for the treatment with punishment.

Having established that both mechanisms are used in reaction to low contributions, we first check whether subjects exhibit distinct preferences for either. Consider reactions to a potential partner who played, but did not contribute in the previous round. For such a non-contributor, 80 % of the subjects decided to exclude at least once, but never punish and 20 % punished at least once, but never excluded such a non-contributor. 15 % neither punished nor excluded the defector and only 15 % of the subjects employed each option at least once<sup>40</sup>.

**Result 6:** *Participants exhibit a preference for either punishing or excluding; many prefer exclusion to punishment.*

Ex ante it is unclear which of the two mechanisms yields higher earnings to the individual applying it. Obviously, the costs related to excluding (stemming from a higher chance of remaining groupless in future rounds) decrease as the number of remaining periods declines, but this may also be the case for the hidden costs of punishing. Subjects who excluded at least one fellow player in the first round<sup>41</sup> earned on average less than subjects who did not (239 vs 254 francs, MW  $z = 2.45$ ,  $p = 0.01$ ), which suggests that not playing may be costly. Punishing in the first round did not significantly decrease earnings in the long run (248 (first round punishers) vs 254 francs, MW  $z = 0.19$ ,  $p = 0.85$ ). Considering all rounds, subjects who excluded at least one fellow player who did not contribute in the previous round earned on average the same as subjects who excluded no free riders (252 francs). Subjects who punished at least one free rider earned on average the same as subjects who punished no free riders (248 vs 254 francs, MW,  $z = 0.02$ ,  $p = 0.98$ ).

**Result 7:** *Excluding someone in the first round has hidden costs, punishing does not.*

Next, we consider the efficiency of the choices made in the various treatments. Maximum efficiency is achieved in a period if all participants are in groups and every member contributes her endowment to the public good. Earnings are then 270 francs per matching

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<sup>40</sup>Note that the categories show overlap.

<sup>41</sup>Subjects who excluded another subject in the first round had no information to base their decision on.

group of nine, *i.e.*, 30 francs per person.<sup>42</sup> In the treatment without punishment, lowest possible welfare is obtained if no participant contributes, irrespective of whether they are in a group. Everyone then earns 20 francs (*i.e.*, 10 less than with maximum efficiency, and 200 francs over 10 periods). Since not ending up in a group is the default option, we take this as reference point for the efficiency calculations. For any given level of contributions, punishment decreases welfare and may lead to negative efficiency in a period.

We therefore measure efficiency in the 10 period game is as  $\left( \sum_{t=1}^{10} \pi_{i,t} - 200 \right) / (270 - 200)$ , with payoff defined after subtracting punishment points.

The observed efficiency in part I of our experiment is 0.16 without punishment and 0.26 with punishment. This difference is statistically insignificant (MW,  $p = 0.29$ ). In part II, efficiency was higher, to wit, 0.71 without and 0.77 with punishment. This difference is statistically insignificant (MW,  $p = 0.20$ ). We conclude that there is no evidence of an efficiency enhancing effect of punishment (in fact, evidence on exogenously formed groups shows that punishment in previous studies usually reduces efficiency in the short run<sup>43</sup>). The difference between Part I and Part II is marginally significant without punishment (Wilcoxon signed rank test,  $p = 0.07$ ) and significantly so with punishment ( $p = 0.03$ ). Hence, the endogenous formation of groups increases efficiency levels when combined with punishment, even when considering the welfare loss due to punishment per se. Finally, the efficiency observed in part I with punishment is significantly lower than in part II without (MW,  $p = 0.01$ ). This is an indication that exclusion and voluntary group formation are, together, a more efficient way to support cooperation in social dilemmas than punishment in isolation.

**Result 8.** *Endogenous group formation increases efficiency.*

Finally, we observed in Figure 3.2 that the option to punish is used much less often in Part II, when there is also an opportunity to exclude and express preferences for group formation. This large difference in exerted punishment (153 vs 32 punishment points in total, or 14% vs 3% of all opportunities to punish someone) is an indication that 80 % of these acts of punishment are

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<sup>42</sup> Each matching group consists of 3 groups. In each group the public good yields at most 90 francs to be split amongst the members.

<sup>43</sup> See also Gächter et al. (2008) for other papers on efficiency-reducing punishment.

replaced by exclusion in Part II (in 103 cases, or 6 % of all 1560 opportunities a participant was excluded). It seems that at least some of the subjects treat exclusion as a substitute for punishment; however, the efficiency results at the group level show that endogenous group formation and punishment are most effective in combination.

**Result 9.** *Endogenous group formation is used as a substitute for punishment, but works as a complement at the group efficiency level.*

### 3.5 *Discussion*

In this chapter we have presented experimental results on the interplay between endogenous grouping and punishment in a public goods game. Participants ranked and excluded their group members based on behavior in previous rounds, including previous contributions to the public good and punishment of other group members. Standard theory predicts that group formation is irrelevant, since no rational player contributes to the public good and thus everyone is indifferent about being in a group or not.

Our results show that (i) endogenous grouping increases contributions more than the punishment option does. The combination of the two mechanisms increases contributions even further. Subjects use the possibility to rank and exclude others and therefore punish less. Exclusion and low ranking are directed towards non-contributors as well as to punishers and we show that the decision whether or not to exclude someone precedes the allocation of a rank. (ii) Furthermore, we observe that efficiency increases significantly only when endogenous grouping is combined with punishment. (iii) Under the endogenous grouping regime, higher contributions are achieved with less punishment.

The results support the proposition that heterogeneity in types and the threat of exclusion motivate subjects to signal their (good) type through contributions (Ones and Putterman, 2007; Van Vugt and Hardy, 2010). The readiness with which exclusion is used (in spite of the fact that for any belief about types, exclusion is dominated by allocating a low rank) shows that this fear is justified. In early periods, punishment could be used as a signal of being an altruistic punisher, but the upward trend in punishment contradicts this explanation. Given that many subjects contribute and punish even in the last period, their types seem to be intrinsic instead of strategic<sup>44</sup>.

The effect of punishment depends to a large extent on the composition of groups. Although the addition of punishment to endogenous grouping increases the efficiency at the group level,

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<sup>44</sup> See also Fudenberg and Pathak (2010) on unobserved punishment.

individual punishers are less preferred as partners<sup>45</sup>. The positive influence of punishment at the group level therefore is not reflected by higher attractiveness of individual punishers during the group formation phase. Free riders could be expected to assign lower ranks to punishers than to contributors; but among contributors this aversion for punishers can only be attributed to fear of antisocial punishment<sup>46</sup>. Irrespective of the reason for the aversion however, if punishers expected their attractiveness as a partner (for contributors) to decrease by inflicting punishment, they may have refrained from punishment and turned to the seemingly cheaper alternative: the exclusion of free riders. Whether this effect would be strong enough to eliminate punishment in the long run – and thereby the competitive advantage of punishment and the preference for it- remains to be assessed<sup>47</sup>. One way to tease out the effects of punishment types on group formation (and vice versa, the effect of group formation on punishment types) would be by explicitly introducing heterogeneity in the punishment costs or inflictions.

Altogether the lower punishment rates account for a large part of the higher efficiency of the combination of the two regimes. Having a say about one's interaction partner reduces the need for punishment, but preserves the threat. This complementary effect of punishment and endogenous grouping may well account for the level of cooperation we observe in real institutions.

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<sup>45</sup> See Rockenbach and Milinski (2011) who, on the contrary, find no effect of punishment in assessing eligibility for later periods.

<sup>46</sup> See Herrmann et al., 2008.

<sup>47</sup> Gürer et al. (2006) show that when given the choice between a regime with or without punishment, participants ultimately migrate to the former.



## Chapter 4: Group competition and punishment

### 4.1 Introduction<sup>48</sup>

Taking the group composition in a public goods game (see Chapter 3) as given, we now focus on the effects of the presence of other groups. As before, a public goods setting is characterized by the tension between the welfare of the group and the interests of the individual group members. As described in Chapter 1, the possibility to exert costly punishment has been shown to enforce contributions in public goods games. Therefore the exertion of altruistic punishment has been put forward as an explanation for the existence of cooperation in human societies (Fehr and Gächter, 2002).

Another institution that has been shown to promote cooperative behavior in social dilemmas is group competition (Gunnthorsdottir and Rapoport, 2006). The key to group competition is that the payoff to members of one group is contingent on their performance relative to another group's performance, as the groups that do better have additional advantages<sup>49</sup>. Competition adds two components to a public goods game. First, individuals may learn about the decisions of (members of) other groups; second, the earnings of the winning group may be increased at the expense of the losing groups. We refer to the former effect as 'group observation' and to the latter as 'group earnings competition'. Either of these alone has been shown to increase contribution levels relative to single groups (Bardsley and Sausgruber, 2005; Böhm and Rockenbach, 2013).

In this chapter, we will focus on the simultaneous occurrence of punishment and group competition. This is important because their net effect is not clear a priori. Although taken separately both effectively reduce free-riding in a group, punishment costs may be detrimental to a group's relative performance in competition with other groups. Costs incurred by the punisher and costs imposed on the punished accrue at the group level, making groups with

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<sup>48</sup> This chapter is based on Van den Broek, Egas, Gomes and Riedl (working paper, 2010).

<sup>49</sup> In the extreme case of lethal group competition, the losing groups leave the stage (Egas et al., 2013.).

more punishment worse off compared to groups where no punishment is exerted. In theory, groups in which the number of potential punishers is sufficiently large may outperform groups with fewer punishers in terms of cooperation rates (Boyd et al., 2003). The net effect of increased cooperation and increased punishment costs remains unclear, however. Hence it is still an open question whether exerting punishment contributes to winning a competition in terms of group earnings. This chapter addresses this question.

The effect of competition between groups on contributions in a public goods game has previously been studied without punishment options. This literature shows for prisoners' dilemma games that the implementation of competition for a prize increases cooperation levels (Bornstein, 1993; Bornstein et al. 2002; see also Nalbantian and Schotter, 1997). Moreover, group competition (without punishment) increases efficiency in social dilemmas (Puurttinen and Mappes, 2009; Egas et al., 2013).

As for the effects of punishment in social dilemmas, the notional detrimental effects of punishment on efficiency seem to disappear if the duration of the game is extended (Gächter et al., 2008). Yet, subjects do not immediately anticipate that the possibility to punish will increase their earnings: when given the choice between institutions, they initially prefer a setting without sanctions (Sutter et al., 2010). They quickly learn, however, and change their mind after experiencing the higher payoffs in the sanctioning regime (Gürerk et al., 2006). Lastly, the interaction of punishment with another institution promoting cooperation, to wit, indirect reciprocity, increases the efficiency of cooperation (Rockenbach and Milinski, 2006). We conclude from these recent insights that the effect of punishment on group welfare interacts with the cooperation-inducing mechanisms. Hence, the effects of punishment on earnings are as yet unpredictable if it occurs in combination with group competition on outcomes.

To our knowledge, there has been no systematic analysis of the combined effect of intergroup competition and intra-group punishment<sup>50</sup>. By filling this gap an important debate can be

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<sup>50</sup> Bornstein, Kugler and Goren have run a competitive public goods game with punishment, and found that the punishment option increased contribution levels to 50 percent (not published, personal communication).

resolved: whether it is in principle possible to sustain cooperation by means of punishment under the threat of competition (Boyd et al. 2003; Dreber et al, 2008). The main research questions in this chapter are thus:

- (i) Does intergroup competition lead to higher contributions?
- (ii) Does competition lead to higher levels of punishment?
- (iii) Does competition lead to higher levels of efficiency in terms of earnings?

Our results indicate that the mere presence of another group (even without formal intergroup competition) induces higher cooperation and punishment levels. Group competition attenuates punishing behavior, however. Nevertheless, this does not affect contribution levels, which remain equally high when groups compete as when they only observe each other. This leads us to the conclusion that intergroup<sup>51</sup> competition makes punishment more effective.

The remainder of this chapter is organized as follows: Section 2 describes the basic public goods game with extensions and theoretical predictions; section 3 gives the experimental design; section 4 reports the results and section 5 concludes.

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<sup>51</sup> We use competition as shorthand for intergroup competition.

## 4.2 Theoretical predictions

Consider the standard linear public goods game as introduced in Chapters 1 and 3. Punishment is implemented by the possibility for subject  $i$  to allocate  $p_{ki}$  points to a fellow group member  $k$  at a cost  $c$ , with an impact of  $d$  per allocated point (assuming  $p_{ii} = 0$ , i.e., a subject cannot punish himself). As a consequence, the payoff function becomes

$$\hat{\pi}_i = \omega - g_i + a \sum_{k=1}^m g_k - c \sum_{k=1}^m p_{ik} - d \sum_{k=1}^m p_{ki} \quad (1)$$

Group competition is modeled as a transfer  $t$  between the individuals of two groups. The payoff  $\pi_i$  for subject  $i \in K$  after group competition can now be written as:

$$\pi_i = \omega - g_i + a \sum_{k=1}^m g_k - c \sum_{k=1}^m p_{ik} - \sum_{k=1}^m p_{ki} + t_{i \in K} \quad (2)$$

with  $t_{i \in K}$  being the transfer to subject  $i \in K$  and the costs of punishing again set to 1. We model this by assuming that the transfer  $t_{i \in K}$  is a linear function of the difference in the two groups' aggregate payoffs before competition. Denoting groups by  $K$  and  $L$  (hence,  $m = |K| = |L|$ ), these payoffs are given by  $\hat{\Pi}_K$  and  $\hat{\Pi}_L$ , the earnings before group competition. Using a factor  $f$  to scale the impact of group competition this yields:

$$t_{i \in K} = f \frac{\hat{\Pi}_K - \hat{\Pi}_L}{m} \quad \text{with} \quad (3)$$

$$\hat{\Pi}_L = \sum_{i=1}^m \hat{\pi}_{i \in L} \quad \text{and} \quad \hat{\Pi}_K = \sum_{i=1}^m \hat{\pi}_{i \in K} \quad (4)$$

Conversely, the payoff for subject  $j \in L$  is:

$$\pi_j = \omega - g_j + a \sum_{l=1}^m g_l - c \sum_{l=1}^m p_{jl} - \sum_{l=1}^m p_{lj} + t_j \quad (5)$$

with

$$t_j = f \frac{\hat{\Pi}_L - \hat{\Pi}_K}{m} \quad (6)$$

Note that transfer is the same for every individual in a group and has opposing signs for group L and group K.

The aggregate payoff in a period now not only depends on contributions and punishment in a subject's own group, but also on the outcome of the group competition. As a consequence, the MPCR (Marginal Per Capita Return), which denotes relative earnings of investing in the public account versus investing in the private account, is no longer directly determined by parameter  $a$ . An increase in contribution to the public account now not only leads to a negative effect on the earnings before competition (because  $a < 1$ ), it also yields a positive expected return from group competition, because the group payoff  $\hat{\Pi}$  will increase (when  $ma > 1$ ).

We therefore proceed with determining the ‘adjusted’ MPCR. This gives the payoff change that occurs from contributing one monetary unit to the public good as opposed to keeping it. Aside from the direct MPCR, captured by  $a$ , the adjusted MPCR also captures the indirect effect through the change in relative group payoffs.

We first determine the derivative of the payoff after group competition (Eq. 5):

$$\frac{\partial \pi_j}{\partial g_j} = -1 + a + \frac{\partial t_j}{\partial g_j} \quad (7)$$

Rewriting Equation 6 in a term for  $j$  and one for  $i \neq j$

$$t_j = \frac{f}{m} [\hat{\pi}_j + \sum_{i \neq j}^m \hat{\pi}_i - \hat{\Pi}_K] \quad (6a)$$

Therefore,

$$\frac{\partial t_j}{\partial g_j} = \frac{f}{m} (-1 + a) + \frac{f}{m} (m - 1)a - 0 \quad (8)$$

Substituting Equation 8 in Equation 7:

$$\frac{\partial \pi_j}{\partial g_j} = -1 + a + \frac{f}{m} (-1 + a) + \frac{f}{m} (m - 1)a$$

The marginal incentive to contribute to the game with group competition is thus given by:

$$\frac{\delta \pi_j}{\delta g_j} = a - 1 + \frac{f}{m} (ma - 1) \quad (9)$$

The social dilemma is bounded by the parameters  $ma > 1$  as before. In addition, for this to constitute a social dilemma, it must be a dominant strategy for (self-interested)  $i$  to contribute nothing, i.e., the right hand side of (9) must be smaller than 0, which gives

$$a + \frac{f}{m} (ma - 1) < 1 \quad (10)$$

The standard equilibrium prediction assuming self-interested players is then once again that they contribute zero to the project. Since punishing lowers their payoff, such players never resort to punishing and therefore never expect to be punished, independently of the MPCR. Competition does not change the predictions under the standard rationality assumptions. Therefore we correct the MPCR for contribution incentives under competition, but we do not correct for the increased cost and impact of allocated punishment points.

Before continuing with our design, we briefly discuss the implications of three alternative models, which incorporate social preferences. First, for a person who is inequity averse (Fehr and Schmidt, 1999), the motivation to contribute in a competitive setting is unclear. Competition makes it even harder to predict the levels of punishment; there is a multiplicity of equilibria and therefore we do not discuss all possibilities. Note that by contributing to the in-group, an agent raises the income of the in-group members, but at the same time creates inequality between the in-group and the out-group members. Hence, the predicted outcome depends on who the agent sees as its reference agents. Second, agents with welfare maximizing preferences (Charness and Rabin, 2002) will contribute but not punish. Competition does not change these predictions, since a transfer does not change the overall welfare. Third, introducing spite into the model (Andreoni and Miller, 2002) increases the incentive to hurt the out-group.

### ***4.3 Experimental design and procedures***

The experiment was conducted at the CREED laboratory at the University of Amsterdam in 2006. A total of 231 subjects participated in 16 sessions, mostly undergraduate students at the University of Amsterdam. Participants majored in economics (34%) and in a variety of other fields. No subject was allowed to participate in more than one session. Their average age was 22 and 33% of the participants were women. Average earnings were 19.50 euro (plus 5 euro show-up fee). The experiment lasted for one hour. Subjects were paid anonymously and no communication was allowed during the experiment. The experiment was computerized and written in zTree 2.1.0 (Fischbacher 2007). Written instructions (see Appendix A for a translation) were provided to the subjects. We checked for understanding by means of a questionnaire; subjects with incorrect answers or with questions were individually advised. After the experiment we acquired personal information and conducted a personality test before the anonymous payment took place.

The basic design of our experiment consists of the three-person public goods game presented in Chapter 3, repeated for 30 periods. Group composition was constant across periods and subjects were not informed about the identity of the other group members or other players in the game. We are interested in the effect of punishment and group competition and the interaction between these mechanisms. For group competition, as argued in the introduction, we need to distinguish between group observation and group earnings competition. Because the latter is impossible without group observation, we isolate the effect of observation by introducing a treatment with only group observation. All in all, we systematically varied (1) the opportunity to punish fellow group members and (2) group interaction (no group interaction, group observation without competition, and group observation with competition) in a complete 2x3 design. This leads to 6 treatment conditions (see table 4.1).

**Table 4.1 Treatment conditions**

	<b>no group interaction</b>	<b>group observation</b>	<b>group competition</b>
<b>without punishment</b>	Base (7 groups, 3 subjects)	Obs (8 groups, 2x3 subjects)	Comp (8 groups, 2x3 subjects)
<b>with punishment</b>	BaseP (6 groups, 3 subjects)	ObsP (8 groups, 2x3 subjects)	CompP (8 groups, 2x3 subjects)

*Notes.* Treatment conditions and number of independent observations

The treatments without group interaction (Base and BaseP) serve as control treatments, where one group of three subjects plays a public goods game respectively with (BaseP) or without (Base) punishment opportunities. In the group observation treatments (Obs and ObsP) we investigate the effect of observation without monetary consequences. With these treatments we can identify without confounds the effect of observing another group's earnings, which is, as argued above, an inherent component of group competition. In the observation treatments, two groups are paired for the duration of the whole experiment. Group members learn in a specific subset of periods the total group earnings of the group they have been paired with, without any consequences for their own payoffs. In the treatments with group competition (Comp and CompP), the individuals in the group with the higher aggregate earnings each receive the difference between the two groups' total earnings in that period. These earnings are deducted from the individuals in the group with the lower total earnings. Details are given below.

Linear group competition has large effects on incentives and therefore may drive up contributions to the maximum. In order to downscale the effect of competition and to attenuate these ceiling effects, we chose to let group interaction occur with a certain probability in each period. Groups met with a probability of  $q=0.2$  in each period; the group interaction periods were randomly determined beforehand for the purpose of comparison across treatments. Subjects did not know in which periods group interaction would occur, only that it would occur with a given probability. Importantly, they are told whether or not group interaction will take place only after the contribution and punishment decisions have been made.

In all treatments, every period of the game consists of a decision stage and a punishment stage (if applicable), followed by an ‘information stage’ in case of a group interaction period. In the decision stage, subjects decide simultaneously and independently how much of their endowment (20 points) they want to contribute to the public good. We use an MPCR of 0.4 in treatments without competition, which is adjusted for the competition treatments as described below. At the end of the decision stage, the participants receive information about the individual contributions of all group members, the total return from the project, the personal return from the project, personal earnings, and the total earnings of the group. Subsequently, subjects in the punishment treatments can simultaneously and independently decide to assign at most a total of 10 punishment points to any other group member. A punishment point (called ‘subtraction point’ during the experiment) costs 1 point to the individual allocating it and 3 points to the group member receiving it. At the end of the punishment stage, subjects are informed about punishment points involving themselves, the incurred costs and the payoff consequences. Individual subjects are not recognizable across periods. The treatments with group observation and group competition are characterized by an additional subsequent information stage in periods of group interaction. Subjects are informed about the total earnings of the group they are paired with, the difference with their own group and the transfer of earnings resulting from group competition (in the competition treatments only).

The implementation of group competition with probability  $q$  per period changes the marginal incentive to contribute (eq. 9) to:

$$\frac{\delta \pi_j}{\delta g_j} = a - 1 + \frac{qf}{m} (ma - 1) \quad (11)$$

The experiment was designed to capture group competition in its simplest form. To keep the MPCR constant across contribution levels, we chose a linear transfer between the two groups<sup>52</sup>. Furthermore, we were careful to implement group competition such that the material

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<sup>52</sup> The linear function implies that every subject in the winning group receives a scaled difference between the groups from a subject in the losing group. A different functional form of the difference between the two would imply that the MPCR in the group competition treatments would not be a constant.

incentives were equal in treatments with and without competition. Although theoretically the difference in the MPCR between both cases will not affect choices, it has been shown that the MPCR does strongly affect contribution, even when it is predicted not to do so (Isaac and Walker, 1988a; Isaac and Walker, 1988b). If we were to observe higher contributions with group competition, this could then be ascribed to the mere effect of increasing the MPCR to the expression in (9). To disentangle this effect from a ‘pure competition effect’, we used an adjusted MPCR in the treatments with group competition. The adjustment needed was determined as follows. Since subjects in the competition treatment could not predict in which periods group competition would occur, the marginal incentive to contribute in each period is defined by equation (11). The MPCRs were equalized across treatments to keep marginal incentives across treatments identical, i.e.,  $a$ ’s were chosen such that the equation for the marginal incentive to contribute in the treatments without group competition,  $\frac{\delta\pi_j}{\delta g_j} = a - 1$ , equals the marginal incentive for the group competition treatments, equation (11). Given MPCR  $a$  in the treatment without group competition, for the group competition treatment we chose  $a'$  such that:

$$a' = \frac{a + \frac{qf}{m}}{1 + qf} \quad (12)$$

We set the transfer factor  $f$  such that  $a'$  is smaller than  $a$ .<sup>53</sup>

Other parameters for the public good game were set as follows. For the endowment, we use  $\omega = 20$ , the MPCR ( $a$ ) = 0.4, and group size ( $m$ ) = 3. The transfer factor between groups ( $f$ ) was set to 3, which essentially implies that each group member of the winning group receives the difference in the two group’s earnings from a member of the losing group. A steeper or flatter linear transfer rate would have meant either that in many cases, money from previous periods would have to be deducted after losing the competition, or that there was no noticeable effect of the competition at all. Negative earnings in a period were deducted from the total account; with the above parameters, however, negative earnings are not possible in the treatment

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<sup>53</sup> We did so to ensure that if we were to observe higher contributions with group competition, it would not be due to the material incentives by itself.

without punishment.<sup>54</sup> The interaction periods were predetermined at period 3, 6, 13, 17, 21 and 27 in all treatments and replicates. Subjects could allocate up to 10 punishment points per period. To compensate for the change in incentives for contribution as a result of group competition as described above, the MPCR for the competition treatments ( $a'$ ) was set at 0.375 (eq. 12).

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<sup>54</sup> Without punishment, the lowest earnings for a participant occur in a competition period if he is the only person contributing in his group and everyone contributes in the other group. He then earns  $\omega - g_i + 0.4 \cdot \sum(g)$ , or  $(20 - 20 + 0.4 \cdot (20 + 0 + 0))$  or 8 points, from which the difference between groups is deducted  $(8 - ((8 + 28 + 28) - 72)) = 0$ . This occurs in expectation in 6 out of 30 periods (during which he earns 7.5 in the worst case).

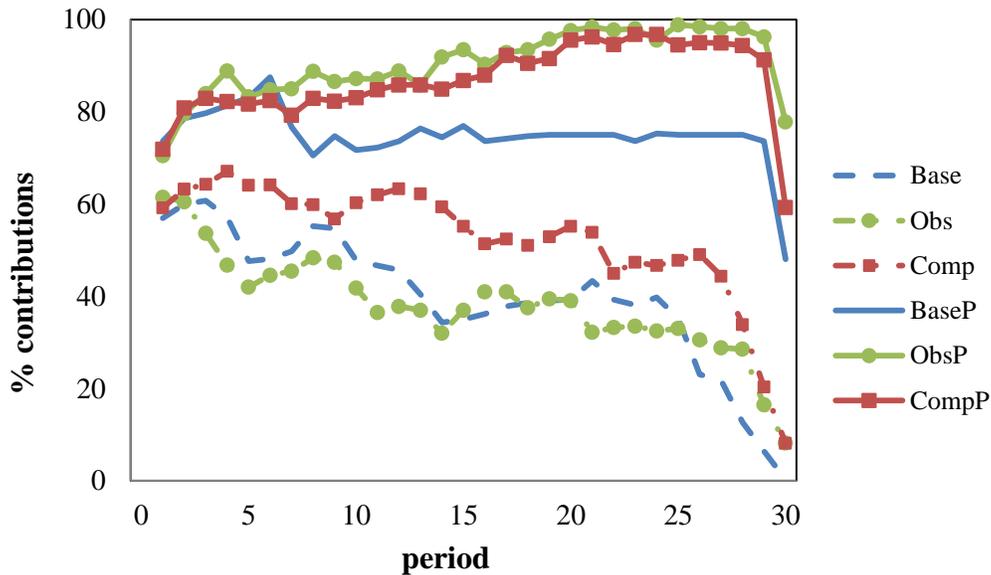
## ***4.4 Experimental results***

This section is divided into four subsections. After an overview of contributions in the different treatments, we describe how the exertion of punishment and subjects' reactions to the received punishment differ across treatments. We conclude with the effects of group competition and punishment on the efficiency of the public good provision.

### *4.4.1 Contributions*

Figure 4.1 shows the average percentage of contributions per period for the different treatments. The same data are summarized in Table 4.2. These results show that the control treatments with and without punishment, Base and BaseP, display similar patterns to those observed in previous studies on public goods games (Fehr and Gächter, 2000), namely a steady decrease in contributions in Base and for the punishment case (BaseP) an increase in contributions in early periods, followed by stable levels at approximately 70 % of the endowment. Across all treatments, those that include the option of punishment show higher initial and average contributions than any of the treatments without punishment opportunity. The introduction of competition increases contributions (Comp), but less so than punishment (compare, for example, the difference Comp–Base to the difference BaseP–Base). Observation (Obs) only increases contributions in combination with punishment (Obs and Base are indistinguishable but ObsP shows higher contribution levels than BaseP). It also appears from figure 4.1 that all treatments exhibit end game effects. Because we are not interested in this effect per se, we will exclude period 30 from all of the tests to be presented in this section.

**Figure 4.1. Contributions**



*Notes.* Average percentage contributions over 30 periods in the control treatment (Base), Observation (Obs) and Competition (Comp), with (P) and without punishment.

Furthermore, we infer from visual inspection that the last third of the periods (20-29) is most stable (see Table 4.2 for a numerical comparison of treatments). In earlier periods there is an upward trend noticeable for the treatments with group interaction and punishment (ObsP and CompP); all other treatments are either downward sloping or approximately constant. Lastly, standard deviations of group contributions differ substantially across treatments (cf. the last row of Table 4.2). Most diverse are groups in the competition treatment without punishment.

**Table 4.2. Contributions in numbers**

<b>Treatment</b>	<b>Base</b>	<b>Obs</b>	<b>Comp</b>	<b>BaseP</b>	<b>ObsP</b>	<b>CompP</b>
<b>Av. % 1-30</b>	39.70	38.24	52.68	74.81	90.41	86.96
<b>Av. % 1-29</b>	41.06	39.27	54.22	75.73	90.84	87.92
<b>Av. % 20-29</b>	29.88	30.79	44.35	74.75	97.68	94.99
<b>Corr. Contr. /</b>	-0.36***	-0.31***	-0.4***	0.03	0.33***	0.36***
<b>Period</b>						
<b>% Full contr</b>	25.12	25.22	15.30	60.92	70.26	61.85
<b>% Free riders</b>	37.93	40.80	28.52	12.26	2.85	3.74
<b>Av. SD</b>	22.91	15.08	38.29	15.08	18.55	21.33

*Notes.* Columns indicate treatments without group comparison (Base), with group observation (Obs) and with group competition (Comp), with (P) and without punishment. Av.% 1-30 is the percentage of endowment contributed over all periods; Av.% 1-29 over the first 29 periods; Av. % 20-29 over period 20-29; Corr. Contr. /Period is the correlation (Spearman's  $\rho$ ) between contributions and periods, \*\*\* indicates statistical significance at  $p < 0.001$  with the group as unit of analysis; % Full contr is the percentage of groups that contributed fully in a certain period (1-29); % Free riders is the percentage of individuals that contributed 0 in a period (1-29); Av. SD is the average standard deviation of contributions across groups (period 1-29).

#### 4.4.1.1 *Effects of punishment on contributions*

Average contributions differ across treatments (Kruskal-Wallis for group averages<sup>55</sup> across 29 periods,  $p < 0.01$ ). In the treatments without punishment, pairwise comparisons reveal that contributions do not differ significantly between any pair of the three treatments (Base, Obs and Comp). Similarly, the treatments with punishment show no pairwise significant differences in contributions either<sup>56</sup>. The overall effect of punishment on contributions in the control condition is only marginally significant (MW (Base-BaseP),  $p = 0.09$ ). The effect of punishment in the observation treatments is significant (MW  $p < 0.01$ ). Lastly, the punishment opportunity also increases contributions in the competition treatments (MW,  $p < 0.01$ ). The

<sup>55</sup> In all statistical tests the group is the unit of observation, unless stated otherwise. For the group interaction treatments (CompP, ObsP, Comp, Obs), a statistical group consists of 6 subjects, because their actions are interdependent; for the BaseP and the Base treatment, a group consists of 3 subjects. Unless stated otherwise, two-sided Mann-Whitney test statistics with group averages over 29 periods are reported; for multiple comparisons the significant p-value has been adjusted.

<sup>56</sup> Neither do contributions in period 20-29 vary significantly across any of the punishment treatments.

difference in contributions between these two treatments, Comp and CompP, is already significant in the first period (MW,  $p < 0.05$ ).

#### *4.4.1.2 Effects of punishment compared to observation and competition*

The increase in contributions (in periods 1-29) caused by the punishment institution separately (84%, BaseP compared to Base; recall that this is marginally significant) is larger than that of observation (-4% increase for Obs compared to Base; recall that this is not significant). The difference between the contributions in these two treatments is significant (MW BaseP-Obs,  $p < 0.04$ ). Analogously, if we compare the increase in contributions caused by the punishment institution with the increase caused by the competition treatment (a 32% increase compared to Base; recall that this is not significant), we do not observe a significant difference (MW BaseP-Comp,  $p = 0.12$ ). The combination of punishment and competition increases contributions by 114% compared to Base, which is a significant increase (MW CompP-Base,  $p = 0.02$ ).

#### *4.4.1.3 Effects of observation and competition on contributions*

Further analysis reveals that although average contributions in ObsP and CompP are very similar, the proportion of groups where all members unanimously contributed in a period is significantly higher in the observation treatment (Two sample proportion test, ObsP-CompP,  $p < 0.01$ ). We find the same effect in the treatment without punishment (Two sample proportion test, Obs-Comp,  $p < 0.01$ ).

To better understand the underlying differences between the two-group interaction treatments with punishment, we distinguish between ‘winning’ and ‘losing’ groups. Winning (losing) groups are defined as groups with the higher (lower) earnings of the two matched groups in the majority of the 29 periods. The winning groups in the competition treatment contributed a significantly higher percentage of their endowment (98.64%) than those in the observation treatment (91.62%, MW ObsP - CompP,  $p < 0.01$ ); for the losing groups, the difference in contributions between treatments was larger and went in the opposite direction (88.85% in ObsP versus 77.19% in CompP,  $p < 0.10$ ). Competition thus drives up the difference between two competing groups more than observation.

**Results 1:** *Without punishment, neither observation nor competition affects contributions. Punishment alone increases contributions more than observation or competition alone. Punishment increases contributions in the observation and competition treatments. The increase is largest when punishment and observation or competition are combined: in that case the contribution rate increases to 100 percent in 60-70 percent of the groups across periods.*

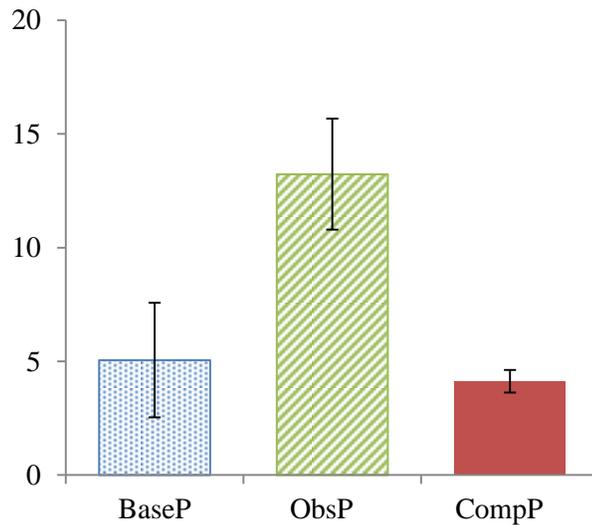
#### *4.4.2 Punishment behavior*

We now turn to a comparison of punishment behavior across treatments. After presenting an overview of the allocated punishment points and their impact in treatments with and without competition, we focus on the determinants and responses to punishment and how these differ in the presence of competition.

##### *4.4.2.1 Average allocated punishment points*

The average numbers across treatments of punishment points allocated are shown in figure 4.2. The highest degree of punishment (on average 13.22 points in 29 periods) is observed in the observation only treatment (ObsP) and the lowest (4.13) in the competition treatment (CompP), with BaseP (5.05) in between. Subjects allocated more than three times as many punishment points in ObsP than in CompP, but the difference is not significant at the group level (MW,  $z = 1.42$ ;  $p = 0.15$ ) because of large variance differences. The (smaller) difference between punishment allocation in ObsP and BaseP is marginally significant (MW,  $z = 1.81$ ,  $p = 0.07$ ).

**Figure 4.2. Average punishment points**



*Notes.* Average number of total punishment points allocated per person in the first 29 periods; lines denote standard errors; BaseP = control treatment, ObsP = observation only, CompP = competition.

Not surprisingly, the observed difference in punishment between ObsP and CompP can be attributed to the groups that contributed relatively less. Across 29 periods, we summed the number of times a group has the higher average contributions of the two; if contributions were on par, neither of the groups was defined as ‘winning’<sup>57</sup>. The group that won in the majority of periods is the ‘high contribution group’. Sorting into ‘high contribution’ and ‘low contribution’ groups reveals that the levels of punishment in the high contribution ObsP and CompP groups are both low, respectively 3.91 and 2.33 (MW,  $z = 1.00$ ,  $p = 0.32$ ); in contrast, we find a large difference between average punishment levels in the low contribution groups in ObsP (22.54) compared to CompP (5.91) (MW,  $z = -1.89$ ,  $p = 0.06$ ). The low contribution groups in ObsP also punish (marginally significantly) more than the BaseP groups (MW,  $z = 1.90$ ,  $p = 0.06$ ).

**Result 2:** *Punishment is lowest in the competition treatment. Groups with relatively low contribution levels punish more in the observation treatment than in the competition treatment.*

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<sup>57</sup> Note that we define winning here in terms of contributions, to avoid taking punishment into account, as opposed to the earlier definition that was formulated in terms of earnings.

#### 4.4.2.2 Punishment efficiency

So far, we have seen that the combination of a competitive setting with a punishment institution (CompP) increases contributions compared to a situation with punishment but without competition (BaseP), and at the same time slightly decreases the number of allocated punishment points. To further analyze the effect of punishment we consider the impact per punishment point, measured as the difference between the average contributions in the punishment treatment and the accompanying treatment without punishment, divided by the average number of punishment points allocated in the punishment treatment. In formula:

$$\rho_x = \frac{\text{averageContributions}(xP) - \text{averageContributions}(x)}{\text{averagePunishment}(xP)}$$

where  $x \in \{\text{Base}, \text{Obs}, \text{Comp}\}$ . Our data give  $\rho_{\text{Base}} = 2.50$ ,  $\rho_{\text{Obs}} = 1.46$ ,  $\rho_{\text{Comp}} = 4.42$ . Hence, subjects generate highest contribution growth per punishment point used in the group competition treatment.

**Result 3.** *Punishment is more effective in the group competition treatment than in the other treatments.*

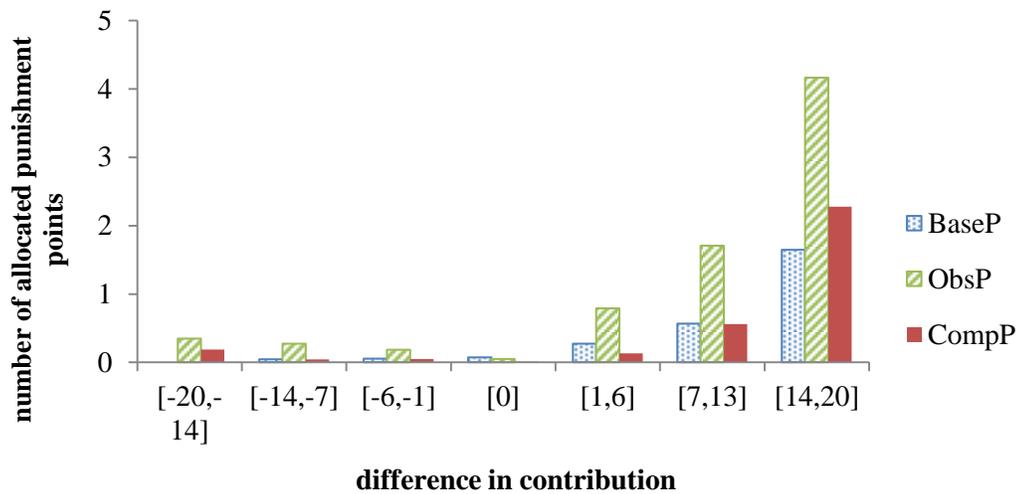
This increased effectiveness may have two causes. Firstly, people may assign fewer punishment points because competition in itself keeps up contributions. We find only weak evidence for this explanation, since competition does not yield higher contributions when there is no punishment (cf. the analysis following figure 4.1 and table 4.2). Alternatively, received punishment points may have a larger impact on subsequent individual contributions through the interaction with group competition. To investigate this, we first consider the individual determinants of punishing; then we analyze the effects of punishment on others' behavior in subsequent periods to establish the effects of received punishment points on contributions.

#### 4.4.2.3 Factors determining punishment behavior

Why do we observe fewer punishment points being allocated in the competition treatment, CompP, compared to the observation treatment without competition, ObsP? A potential explanation for the higher punishment in ObsP is that the differences in contribution across individuals may be larger, which could lead to more punishment. This is not the case: we observe a lower average variance in the contribution of participants per group (of three subjects) in ObsP (8.78 points) than in CompP (9.94 points; MW,  $z = -1.74$ ,  $p = 0.08$ ). On average, then, the difference in contribution between punisher and punished is larger in CompP. We therefore reject this explanation for the difference in punishment.

A second possible explanation for the difference in punishment points is that competition may mitigate punishing in response to the differences in contributions between the punisher and the punished. If this dampening effect is causing the difference, we should on the one hand observe in all three treatments an increase in punishment points with an increase in the difference in contributions, but on the other hand see a more pronounced increase in ObsP than in CompP.

**Figure 4.3. Punishment allocation**



*Notes.* Pairwise differences in contribution between punisher and punished, with resulting average punishment points allocated per category, in the competition treatment (CompP), the observation treatment (ObsP), and the control treatment (BaseP).

This is indeed what we observe (see figure 4.3). When the punisher has previously contributed more than the (potentially) punished – i.e., the difference in contributions is positive – the number of allocated punishment points increase with the difference in contribution for all treatments. This correlation is much higher in the observation treatment (ObsP) than in CompP (MW, group-level Spearman correlations between positive deviation in contribution and allocated punishment,  $p < 0.04$ ). This observation confirms that group competition dampens the effect that differences in contributions have on the allocation of punishment points.

To better understand the factors determining the individual decision to punish another group member, we use a Tobit regression estimation. To correct for statistical dependencies within statistical groups, robust standard errors are clustered on these groups. For these regression equations we use data from all 30 periods, since a dummy for the final period can account for most of the end game effects. The results are presented in table 4.3. They show that a positive and a negative difference in contribution between the punisher and the punished both significantly increase the number of punishment points allocated. The slope of the increase after a positive deviation in ObsP and CompP is steeper than in the baseline, indicating a stronger reaction to deviations, but not significantly so ( $p = 0.16$ ). Aside from these effects the observation treatment dummy is positive and large (but not significant), reflecting that a subject in the observation treatment on average starts punishing at a lower absolute deviation than a subject in the baseline treatment; the competition treatment dummy is negative, but not significant. Whether or not a group had encountered the other group in the previous period does not significantly affect punishment behavior (as it should not, since it was explained to the subjects that encounters are based on a random sequence). There is a negative but non-significant trend over periods (not surprising given the positive trend in contributions over periods (cf. Table 4.1)), except for the last period, where we observe a large increase in punishment points following the drop in contributions in this period<sup>58</sup>.

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<sup>58</sup> Note that punishment in period 30 is not strategic; see f.i. Nikiforakis (2008) and Vyrastekova, Funaki, Takeuchi (2008).

**Table 4.3. Deviation in contribution leads to punishment**

<b>Dep. var: punishment points allocated by <math>i</math> to <math>j</math> in <math>t</math></b>		
<b>Indep vars</b>	<b>Coefficient</b>	<b>Std err</b>
<b>Contr <math>i</math> – contr <math>j</math> (<math>&gt;0</math>)</b>	0.64**	0.26
<b>Contr <math>i</math> – contr <math>j</math> (<math>&lt;0</math>)</b>	0.20*	0.11
<b>Tobservation</b>	1.90	1.45
<b>Tcompetition</b>	-0.91	1.27
<b>deviationXobs</b>	0.19	0.25
<b>deviationXcomp</b>	0.04	0.24
<b>Av. Contr in subgroup</b>	-0.19**	0.09
<b>Group interaction period</b>	0.30	0.35
<b>Period</b>	-0.13***	0.05
<b>Period30</b>	3.96**	1.68
Constant	-7.46***	2.72
<i># observations (groups)</i>	6838 (22)	
<i>log likelihood</i>	-1589.13	
<i>R2</i>	0.23 ***	

*Notes.* The table presents the results of a Tobit regression used to explain the allocation of punishment points to  $j$  by  $i$  in  $t$ . *Contr  $i$  – contr  $j$  ( $>0$ )* = positive difference in contribution between punisher and punished; *Contr  $i$  – contr  $j$  ( $<0$ )* = negative difference in contribution between punisher and punished; *Tobservation* = dummy for Obs treatment; *Tcompetition* = dummy for Comp treatment; *deviationXobs (comp)* ( $>0$ ) = the interaction between the positive difference and a dummy for the Obs (Comp) treatment; *Av contr in subgroup* = the average contribution in a subgroup of 3; *group interaction period* = a dummy for whether the previous period was a group interaction period. The first line per variable denotes the coefficient (\*, \*\*, \*\*\* indicating significance at the 0.10, 0.05, and 0.01 level, respectively); the second line denotes the standard error.

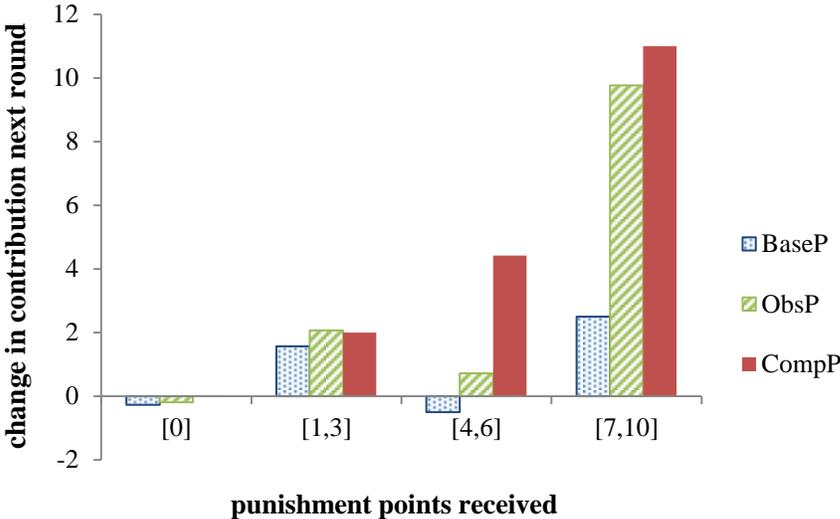
**Result 4:** *More punishment takes place in ObsP than in CompP.*

A probable reason for this result is that competition extenuates the allocation of punishment points in reaction to differences in contribution.

4.4.2.4 Individual reactions to punishment

Having discussed the decision to punish, we now turn to the effects of punishment and how these vary across treatments. The effects on contributions in the period subsequent to punishment differ across treatments (see figure 4.4). Most noticeably, subjects in BaseP react less to punishment than subjects in the other two treatments. Secondly, taken as a function of the number of punishment points received, the increase in contribution compared to the previous period kicks in at a lower number of received punishment points in the treatment with competition (namely, at 4-6 punishment points instead of at more than 7) than in the other treatments.

Figure 4.4 Reactions to punishment



Notes. Change in contribution in next period as a result of the punishment points received for CompP (competition), ObsP (observation) and BaseP (control).

For a more systematic understanding, we again present the results of a Tobit regression model with robust standard errors clustered on statistical groups (Table 4.4). Here, the dependent

variable is the non-negative increase in contribution in the subsequent period<sup>59</sup>. In the baseline treatment the received punishment significantly increases the contribution in the next period; the interaction dummy of the CompP treatment with the number of punishment points received has a large positive coefficient (1.69,  $p < 0.10$ ), but does not differ significantly from the ObsP interaction dummy ( $p = 0.34$ ). This result quantifies the previous observation that impact of punishment points on contribution increases more sharply in the competition treatment and can be compared to the finding that subjects in the competition treatment react more sensitively to punishment (see Figure 4.4). The ObsP treatment dummy is negative and significant compared to the baseline treatment ( $p < 0.05$ ), denoting that in the observation treatment a higher level of punishment is needed to affect behavior. The competition treatment dummy does not differ significantly from the baseline. The negative coefficient of the average contribution of the other group members in the previous period suggests that, other things being equal, people are less inclined to increase their contribution as the others contribute more. Whether group interaction took place in the previous period has no effect.

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<sup>59</sup> We exclude decreases in contribution after being punished from this analysis, since these cannot be explained as a reaction to group pressure.

**Table 4.4. Increase in contribution**

<b>Dep. var: increase in contribution from <math>t</math> to <math>t+1</math></b>		
<b>Indep vars</b>	<b>Coefficient</b>	<b>Std err</b>
<b>Received punishment in <math>t</math></b>	1.14**	0.51
<b>ppxCompP</b>	1.69*	0.96
<b>ppxObsP</b>	1.77	0.55
<b>ObsP</b>	-4.20**	1.95
<b>CompP</b>	-1.60	1.74
<b>Av. contribution others</b>	-1.60***	0.19
<b>Group interaction in <math>t</math></b>	-0.05	0.30
<b>Period <math>t</math></b>	-0.07	0.05
Constant	26.00***	2.44
<i># observations (groups)</i>	2500 (22)	
<i>log likelihood</i>	-2953.50	
<i>R2</i>	0.24***	

*Notes.* The model (Tobit regression explaining non-negative increase in contribution, limited between 0 and 20, robust standard errors clustered on group) includes the treatments, the interaction of treatments with the total number of punishment points received in the previous round, and controls for the average contribution of others. \*, \*\*, \*\*\* indicates significance at the 0.10, 0.05, and 0.01 level, respectively; the second column denotes standard errors.

**Result 5:** *A punishment point received in the previous period has a larger impact on subsequent contributions in the competition treatment than in the baseline treatment.*

#### 4.4.3 Earnings

Average earnings are an indication of the efficiency of various mechanisms. They do not differ significantly across treatments ( $\chi^2(5) = 3.29$ ,  $p=0.65$ ); see table 4.5. We constructed a

measure of efficiency that normalizes actual earnings by relating them to earnings realized in the social optimum (max *Earnings*) and earnings in the no-contribution Nash equilibrium (min *Earnings*):

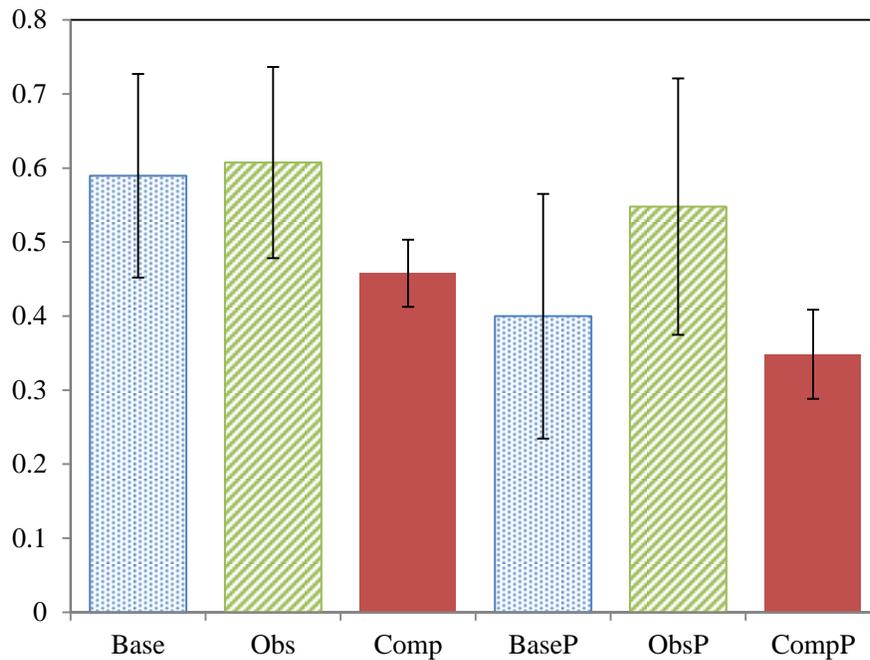
$$\varepsilon = \frac{\max Earnings - actualEarnings}{\max Earnings - \min Earnings}$$

**Table 4.5. Average earnings and efficiency**

<b>Treatment</b>	<b>Average earnings (29)</b>	<b>SD</b>	<b>Spearman's rho (p&lt;0.01)</b>	<b>% of max</b>	<b>% of min</b>	<b>Efficiency (ε)</b>
<b>Base</b>	18.83	1.27	-0.88	90	108	0.59
<b>Obs</b>	18.77	1.27	-0.90	90	107	0.61
<b>Comp</b>	18.58	0.28	-0.91	95	107	0.46
<b>BaseP</b>	19.49	1.54	0.44	93	112	0.40
<b>ObsP</b>	18.97	1.70	0.60	91	109	0.55
<b>CompP</b>	18.82	0.37	0.57	96	108	0.35

*Notes.* For each treatment denoted in the first column, the remaining columns show: average earnings over 29 periods; standard deviation; spearman's rho (the correlation between earnings and period number); the percentage of the maximum and minimum earnings; and efficiency for the 6 treatments.

**Figure 4.5. Efficiency of treatments**



*Notes.* Average efficiency ((maximum earnings – actual earnings)/(maximum – minimum earnings))  $\pm$  se, per treatment. Treatments are control without punishment (Base), Observation (Obs), Competition (Comp), control with punishment (BaseP), Observation with punishment (ObsP), and Competition with punishment (CompP).

For all treatments with punishment, the Spearman’s rho of earnings over periods was positive, in contrast with the treatments without punishment. A comparison of the efficiency between the treatments with and without punishment shows that the punishment institution was less efficient (MW Base-BaseP,  $p < 0.001$ ; MW Obs-ObsP,  $p < 0.01$ ; MW Comp-CompP,  $p < 0.001$ ). There is no significant difference between the efficiency of the competition treatments and the observation treatments, either with or without punishment (Obs-Comp:  $p = 0.10$ ; ObsP-CompP:  $p = 0.40$ ). Neither did competition with punishment or observation with punishment increase efficiency compared to the baseline treatment (CompB-Base:  $p = 0.12$ ; ObsP-Base:  $p = 0.99$ ).

**Result 6:** *Earnings do not differ across treatments. Efficiency in the baseline with punishment treatment is lower than the baseline without punishment treatment; similarly, efficiency in the competition with punishment treatment is lower than in the competition without punishment treatment.*

## *4.5 Discussion*

Both group competition and costly punishment have been put forward as explanations for sustained cooperation in groups, but the interaction of these two mechanisms has not previously been investigated. This chapter experimentally investigates their interplay by systematically comparing how the presence of another group and competition between groups affects contribution levels, punishment and earnings.

Altruistic punishment has been observed widely (Henrich et al., 2006) and competition between groups has taken place throughout the ages (Bowles, 2009). Therefore how people behave when these two institutions occur together is an important question. Our main result is that competition makes punishment more effective. Although competition reduces the number of allocated punishment points, it increases at the same time their impact on contributions in the subsequent period. Observation without financial consequences drives up the contributions to a public good as much as competition does. The fact that this does not result in significantly more efficient outcomes may be due to the nature of the public goods game implemented, specifically the relatively low MPCR, in combination with the higher punishment levels. A longer time span may lead to larger differences in efficiency (Gächter et al., 2008).

We did not find support for the idea that efficiency increases with competition as reported by Puurtinen and Mappes (2009); Sääkasvuori et al., 2011). This may be due to differences between their setup and ours. The linearity of competition effectively increases the MPCR, thus increasing the incentive to contribute, which may explain the efficiency increase reported by Puurtinen and Mappes (2009) and Sääkasvuori et al. (2011). In our experiments, we correct for this change by lowering the MPCR in the competition treatments, and find no significant effect of competition on earnings. The corrected MPCR may have had an effect on punishment, but how exactly punishment depends on the MPCR and other parameters has not yet been extensively studied (Egas and Riedl, 2008). Competition may play a role, too, in the reaction to punishment. Since people are responsive to non-monetary punishment as well, they may be sensitive to other aspects of punishment than the monetary impact (Masclet, 2003). Punishment is more effective when it communicates a transgression of norms, and costliness

has been shown to increase the strength of this signal (Xiao, 2013). Therefore, the trade-off that punishers face under competition (by punishing a group member, they decrease their group's chance of winning the competition on the short term) may have increased the perceived strength of the punishment.

Our results may also be considered from an evolutionary group selection perspective. Evolutionary (cultural) group selection models predict that groups in which punishers are common have a selective advantage over groups with lower numbers of punishers, and for this reason may have won the evolutionary battle (Boyd et al., 2003). Recent findings, however, show that within-group punishment does not pay for the individual (i.e., it is probably maladaptive at the individual level to punish free riders; Dreber et al., 2008). Nevertheless, agent-based models suggest that in human prehistoric context the selection effect of group competition may have been strong enough to sustain altruistic punishment (Bowles and Gintis, 2004). On the other hand, group competition has been reported to induce not only behavior that is beneficial to the group welfare. From rent seeking games we know that group contests can turn out to be very destructive for group welfare, especially when punishment opportunities are added (Abbink et al., 2009). Such out-group aggression could have translated into high punishment rates towards in-group free riders in our setting, but we observe the opposite effect. Therefore, our findings are in line with the predictions in Boyd et al. (2003).

One promising direction for further research on competition may be to extend inequality aversion models (Fehr and Schmidt, 1999). In these models agents incorporate the difference between the wellbeing of a reference agent and themselves in their utility function. Diversifying reference agents by in-group and out-group modifiers of altruism and spite may generate testable predictions and spur further experiments (see for instance Yamagishi and Mifune, 2009; Harris et al., 2009). Another way to disentangle subjects' motivations for contributing and punishing in a competitive setting may be to implement a punishment institution in only one of the groups.

When faced with the choice, subjects voluntarily choose a punishment institution after experiencing its monetary advantages (Gürerk et al., 2006). This prompted the question

whether such a regime results in winning a competition on earnings when subjects are aware of the competition. Even with our conservative implementation of competition, we show that the interaction between the two mechanisms makes punishment lose its destructive effect on earnings, without losing its deterring force. Therefore group competition removes rather than amplifies the efficiency dilemma of costly punishment.



## Chapter 5: Conclusion

The tension between mine and thine, or between self-interest and the group's collective interest, is at the heart of many social interactions. Natural selection tends to favor individuals who maximize their own resources (as markets favor firms that maximize their own profits); however, in many social dilemmas, like voting, housekeeping, and academic peer review, we display cooperative behavior. Both economists and biologists, among others, have studied such social dilemmas. They share an interest in the aggregated effects of individual behavior (cooperation, social behavior), apply the same methods to study these effects (evolutionary game theory, controlled laboratory experiments), and even show some overlap in their vocabulary – although when confronted with an economist who uses the term ‘evolution’ for a gradual learning process in the lab, a biologist might bring up the span of a single human reproductive cycle. In this thesis I build on both the economics and biology literature to study how the institutional environment steers behavior in situations in which an individual's interest is not in line with the general interest. I use laboratory experiments to assess the effect of information about (others') previous behavior on cooperative decisions. With a series of laboratory experiments that capture the nature of the tradeoff between two individuals, I show that both reputation and own experience jointly influence helping behavior. In Chapter 2 and 3 I turn to groups of three and larger to show how voluntary grouping boosts cooperation rates, and how competition together with punishment increases cooperation rates.

In Chapter 2, our results show how people integrate direct and indirect information about past social interactions, confirming the commonly held belief that people use both personal experience and reputations to decide whether or not to help others. We build on simulation studies that suggest that evolution has driven our preference to help others at our own cost. In these simulations, agents can help each other at a cost to themselves. Reciprocal altruism (“I help you and you help me”) accounts for many of the instances of cooperation in the animal kingdom. People, on the other hand, offer favors to others who they may never meet again (“I help you and someone else helps me”, or indirect reciprocity). It has been shown that agents selectively helping only those who have a reputation of being nice, fare relatively well. Both memory and gossip from others may influence this helping tendency. In our study we offer

people the choice between costly direct and indirect information about their partner's history of cooperative behavior. We show that people use both types of information when deciding about a reciprocal gift, even when the indirect information (e.g., gossip) is known to be less reliable but more abundant. We find that in such a noisy environment, the helping levels remain the same; but people compensate for the noisiness of the indirect information by switching to the more reliable direct information. In environments without noise, too, direct information is on average more decisive for helping.

In Chapter 3, we turn to social dilemmas in groups. In situations in which group structure has been imposed, like in the army or at the office, non-contributors have the chance to hurt the group welfare and punishing them improves efficiency. Groups, however, are often formed voluntarily, for instance in the case of treaties or food gatherers. The exclusion of free riders is often a more straightforward option than punishing them. To study the interplay between voluntary grouping and punishment, we conducted an experiment in which people can rank and exclude their fellow players based on their previous behavior, including both contribution and punishment levels in previous periods. Our results show that endogenous grouping increases contributions more than the punishment option does. The combination of the two mechanisms increases contributions even further, since subjects use the possibility to rank and exclude others and therefore punish less. Exclusion and the allocation of low ranks are directed towards non-contributors as well as to punishers. Altogether the lower punishment rates account for a large part for the higher efficiency of the combination of the two regimes. In other words, having a say about one's interaction partner reduces the need for punishment, but preserves the threat. This complementary effect of punishment and endogenous grouping may well account for the level of cooperation we observe in real institutions.

In Chapter 4, I zoom out further and study the interaction of two groups of players who face a social dilemma. Competition is known to spur higher helping rates within a group. If punishing is costly, however, group members may hesitate to resort to punishing free riders, out of fear of reducing their group's chances of winning the competition even further- as in a football game in which the star player runs solo, but gets away with it. Although our implementation of competition is very conservative and linear (winning group members

receive  $1/3$  of the difference between the groups), we show that competition reduces the number of allocated punishment points. The interaction between the two mechanisms makes punishment lose its destructive effect on earnings, without losing its deterring force. Indeed, observation without any financial consequences drives up the contributions to a public good as much as competition does. Although competition reduces the number of allocated punishment points, it increases at the same time the impact per punishment point on contributions in the subsequent period.

The results presented in this thesis show that the subtle interplay between two institutions amplifies their effect on human social behavior. First, since participants in the lab substitute noisy reputational information with direct information, reputation appears to be a multilayered construct that cannot be studied in isolation. Theory on either direct or indirect reciprocity has been shown to have large predictive powers, but further study of human cooperative behavior requires assumptions on how the two types of information, direct experience and reputation, are integrated in social decision making. Second, if a punishment institution is bolstered by partner choice, higher contributions are achieved with less punishment. This means that the puzzle of the evolution of costly punishment perhaps has shrunk a little; or even that the theoretical puzzle has been inflated from the start, by isolating the social dilemma from its supporting institutions. Third, competing with another group - even if the competition takes place only in the minds of the participants - increases the impact of punishment. Merely evoking the mental scheme of an institution may be enough to bring forth a stronger reaction to another institution.

In all the cases described above, unweaving social institutions has led to a systematic underestimation of their effects on human social behavior. A potential reason is that incorporating a specific stylized behavioral phenomenon by extending the preference function (for instance with spiteful or competitive preferences) overestimates the modularity of human cognition. Social dilemmas in real life evoke, depending on the framing, various psychological schemes that do not add up. Our results suggest that incentive-neutral changes to laboratory settings alter the behavior of participants by tapping into other psychological schemes: “is this my partner”, “what would she do to me”, “did I choose him personally”; “are we in the same

boat?” These contextual characteristics have been decisive in social relations throughout human history and therefore deserve a role in economic theory – and therefore also in the laboratory.

The institutions above – reputation, punishment, competition, partner selection - build on memory and comparison between partners or groups. More importantly, our results show that people apply these mental faculties recursively: we are self-conscious and know others will judge us in comparison to others. It is in this self-consciousness that egoism and altruism meet. Of all psychological mechanisms that play a role in such situations, *keeping up appearances* may be the killer app<sup>60</sup> of humankind.

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<sup>60</sup> “A computer application of such great value or popularity that it assures the success of the technology with which it is associated” (Merriam-Webster, [www.m-w.com](http://www.m-w.com)).

## Samenvatting

De spanning tussen mijn en dijn, tussen ons eigenbelang en dat van een ander, speelt in ons dagelijks leven een grote rol. Natuurlijke selectie bevoordeelt individuen die hun eigen welvaart maximaliseren, zoals markten de bedrijven bevoordelen die hun winst maximaliseren. Toch stellen wij mensen ons in veel alledaagse sociale dilemma's cooperatief op: we doen het huishouden, gaan stemmen en beoordelen wetenschappelijke artikelen van onbekenden. Zulke 'sociale dilemma's', waarin het groepsbelang tegengesteld is aan het eigenbelang, worden bestudeerd door zowel economen als biologen. Zij delen de interesse in geaggregeerde effecten van individueel gedrag (samenwerking, competitie), ze passen dezelfde methodes toe om die effecten te bestuderen (evolutionaire speltheorie, gecontroleerde laboratoriumexperimenten), en hun vocabulaire overlapt- hoewel sommige economen ontwikkelingen in het lab 'evolutie' noemen, terwijl een bioloog zou opmerken dat één reproductiecyclus van de mens al langer duurt dan een laboratoriumexperiment.

In dit proefschrift bouw ik voort op de economische en biologische literatuur. Ik onderzoek hoe de institutionele omgeving (de 'spelregels' van het dagelijks leven) menselijk gedrag stuurt in situaties waarin het individuele belang niet in lijn is met het algemene belang. Specifiek onderzoek ik met laboratoriumexperimenten het effect van informatie over andermans' eerdere gedrag op daaropvolgende beslissingen. In de inleiding introduceer ik verschillende vormen van samenwerking, speltheorie en de onderzoeksmethode. Hoofdstuk 2 draait om twee individuen die elkaar kunnen 'helpen' door geld weg te geven. We laten zien hoe de combinatie van reputatie en het eigen geheugen het hulpgedrag jegens een ander beïnvloedt. In hoofdstuk 3 en 4 onderzoeken we groepen van drie en meer mensen. Daarin tonen we aan dat als de keuze voor partners vrijwillig is, er meer onderlinge behulpzaamheid is en dat competitie tussen groepen in combinatie met de mogelijkheid tot straffen de samenwerking binnen die groepen vergroot.

In hoofdstuk 2 onderzoeken we op basis van welke informatie mensen de beslissing nemen om een ander te helpen tegen bepaalde kosten. Veel voorbeelden van altruïsme in het dierenrijk kunnen aan de hand van 'wederzijds altruïsme' worden verklaard: ik help jou, jij helpt mij.

Mensen daarentegen helpen ook soortgenoten die ze nooit meer tegenkomen: ik help jou, jij helpt iemand anders, oftewel indirecte reciprociteit. Resultaten van eerdere simulaties suggereren dat het vermogen om selectief te zijn in wie je helpt, door alleen behulpzame anderen te helpen, evolutionair voordeel biedt. Wij laten met simulaties zien dat de combinatie van die twee strategieën (direct en indirecte reciprociteit) ook evolutionair voordeel biedt. In een laboratoriumexperiment plaatsen we mensen in de schoenen van de gesimuleerde agents. Deelnemers krijgen iedere ronde een vast bedrag en de mogelijkheid om een deel daarvan aan een ander te geven, die dan de gift plus een extra bedrag ontvangt. De beslissing om een ander iets te geven kan zowel door het geheugen (directe informatie over hoe de ander hem heeft bejegend) als roddels van anderen (indirecte informatie) beïnvloed worden. In ons onderzoek bieden we mensen de keuze tussen die twee soorten informatie over hun partners' samenwerkingsgedrag. We tonen aan dat mensen beide types informatie gebruiken bij de beslissing over een gift, zelfs wanneer de indirecte informatie, net als roddel, minder betrouwbaar is, maar wel overvloediger aanwezig. In een dergelijke 'onbetrouwbare' omgeving blijft het gemiddelde hulpniveau gehandhaafd, maar compenseren mensen voor de onbetrouwbaarheid door meer directe (betrouwbare) informatie op te vragen.

In hoofdstuk 3 verschuift de focus van samenwerking tussen twee personen naar sociale dilemma's in groepen. In situaties waarin de groepssamenstelling opgedrongen is, zoals in het leger of op kantoor, kunnen *freeriders* (degenen die niet bijdragen aan het groepsbelang) de prestaties van de groep omlaagtrekken. Deelnemers de mogelijkheid bieden om tegen kosten geld van elkaar te vernietigen, oftewel elkaar te straffen, is een manier om freeriders in toom te houden. De meeste groepen worden vrijwillig gevormd, bijvoorbeeld bij internationale verdragen of bij jagers of verzamelaars. In zulke gevallen ligt het uitsluiten van freeriders meer voor de hand dan hen straffen. Om die combinatie van vrijwillige groepsvorming en strafgedrag te analyseren, voeren wij een experiment uit waarin mensen hun medespelers kunnen *ranken* en uitsluiten, op basis van de bijdrages en strafpunten die die anderen in eerdere rondes uitdeelden. Uit de resultaten blijkt dat de mogelijkheid tot vrijwillige groepsvorming tot hogere bijdrages leidt dan de mogelijkheid om te straffen. De combinatie van de twee mechanismes leidt tot nog hogere bijdrages, omdat proefpersonen de mogelijkheid tot ranking en uitsluiting van anderen gebruiken en daardoor minder vaak

straffen. Zowel freeriders als mensen die veel straffen worden uitgesloten en krijgen een lage rank toebedeeld. Doordat er minder gestraft wordt, is de combinatie van de twee regimes efficiënter; oftewel, het kunnen selecteren van je groepsgenoten verlaagt de noodzaak tot straffen, maar de dreiging blijft bestaan. Dit complementaire effect van straffen en vrijwillige groepsvorming zorgt in de buitenwereld waarschijnlijk ook voor een hoger samenwerkingsniveau.

In hoofdstuk 4 zoomen we verder uit en analyseren we de interactie tussen twee groepen spelers terwijl in beide groepen een sociaal dilemma speelt. Competitie tussen groepen wakkert hogere bijdrages binnen een groep aan. Omdat straffen geld kost, aarzelen groepsleden om de freeriders te straffen, omdat ze daarmee de winkansen van hun groep (en daarmee hun eigen verdiensten) nog verder verkleinen - het straffen van een groepsgenoot valt te vergelijken met het op de reservebank zetten van een sterspeler die zijn doelpunten in *alleingang* maakt. Hoewel we competitie lineair en erg conservatief hebben geïmplementeerd (de winnende groepsleden krijgen 1/3 van het verschil tussen de twee groepen), vermindert de competitie het aantal uitgedeelde strafpunten. De interactie tussen de twee mechanismes zorgt ervoor dat straffen niet meer het destructieve effect op verdiensten heeft, maar wel zijn afschrikwekkende functie behoudt. Inderdaad heeft het observeren van de andere groep zonder enige financiële consequenties dezelfde effecten als competitie. Ook uit deze resultaten blijkt dat twee instituties een versterkt effect op menselijk sociaal gedrag hebben doordat ze subtiel op elkaar inwerken.

Dit proefschrift toont aan dat het samenspel van twee instituties onverwachte effecten op menselijk gedrag heeft. De deelnemers aan het eerste experiment substitueerden onbetrouwbare reputatie voor betrouwbaardere informatie uit de eerste hand. Dat impliceert dat reputatie een meerlagig begrip is. Hoewel de afzonderlijke theoretische modellen van directe en indirecte reciprociteit veel inzichten hebben opgeleverd, vereist verdere studie van samenwerkingsgedrag aannames over hoe mensen de twee types informatie integreren in hun beslissingen. Ten tweede leidde de combinatie van de mogelijkheid tot straffen en het uitzoeken van partners tot hogere bijdragen aan een publiek goed. Doordat er tussen zelfgekozen partners minder gestraft wordt, is de evolutionaire puzzel rondom het ontstaan

van straffen een stukje kleiner geworden- misschien was, als we het sociale dilemma in de context van andere instituties zien, de puzzel niet zo groot. Ten slotte versterkt competitie met een andere groep, zelfs als die alleen in gedachten plaatsvindt, het effect van straffen: de gestrafte persoon draagt nog meer bij. De gedachte aan een institutie verandert dus de reactie op een andere institutie.

In alle bovenstaande voorbeelden leidde het uiteenrafelen van sociale instituties tot een systematische onderschatting van hun effect op menselijk gedrag. Een verklaring daarvoor is dat we door specifieke gedragsfenomenen in te bouwen in de preferentiefunctie (bijvoorbeeld kwade wil of competitiviteit) de modulariteit van de menselijke cognitie overschatten. Een sociaal dilemma roept, afhankelijk van de framing, een scala aan psychologische schemata op, die soms tegengestelde reacties opwekken. De resultaten van dit proefschrift suggereren dat veranderingen in de spelopzet het gedrag van participanten beïnvloeden doordat ze aan psychologische schemata raken: “is dit mijn partner”, “wat zou zij voor mij doen”, “ik heb hem zelf uitgezocht”, “zitten we in hetzelfde schuitje?” Zulke context is bepalend geweest voor de sociale verhoudingen in de hele menselijke geschiedenis en verdient daarom een rol in economische theorie – en misschien ook wel in het laboratorium.

Reputatie, competitie en partnerkeuze, de belangrijkste concepten uit dit proefschrift, vereisen mentale capaciteiten zoals geheugen en het met elkaar vergelijken van partners of groepen. Deze geestelijke capaciteiten passen we recursief toe: we zijn ons bewust van de indruk die we maken en weten dat onze partners ons zullen beoordelen in vergelijking tot anderen. In dat zelfbewustzijn komen egoïsme en altruïsme bij elkaar. Van alle psychologische mechanismes die bij menselijke samenwerking een rol spelen, is *keeping up appearances* misschien wel de *killer app*<sup>61</sup>.

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<sup>61</sup> Een computerprogramma dat door zijn grote waarde of populariteit het success van de geassocieerde technologie verzekert.





# Appendices

## *Appendix A1: Instructions “Direct and indirect reciprocity”*

Welcome to this experiment on decision making. The experiment will last for approximately 90 minutes. During the session it is not allowed to talk or communicate with the other participants. If you have a question, please raise your hand and one of us will come to you to answer it. During this experiment you will make money. How much you earn depends of your decisions and the decisions of others. At the end of the experiment the amount you have earned, plus a show up fee of 7 euros, will be paid to you in cash. These payments are anonymous; you will be paid individually in the reception room. Please stay seated at the end of the experiment until your desk number is called. We will not inform any of the other participants about your earnings. It is impossible for us to associate your desk number with your identity. You start out with an amount of 3000 points; 300 points are worth 1 euro.

### **Experimental procedure**

1. Instructions
2. Quiz to verify if you understand the experiment
3. The experiment
4. Questionnaire
5. Payment

The experiment consists of at least 100 rounds. From round 100 upon, there is a chance of 90% that a new round starts. Every round, you will be paired with another participant in the room. Everybody stays anonymous; you will not be informed about the identity of the participant you are paired with.

The chance to be paired with a particular participant is for all participants the same in every round. Hence, the chance to be paired with the same participant twice in a row is very small.

Every round you will be assigned a role (A or B). You only have to make a decision when you are assigned role A. If you are assigned role B, you do not have to do anything.

### **Choices**

If you are assigned role A, you can choose between two alternatives. If you choose ‘yellow’, participant B, who is paired with you, receives 250 points. You lose 150 points. If you choose ‘blue’, the participant paired with you receives nothing, and you do not lose any points.

Summarized:

Yellow has cost 150 for participant A. Participant B receives 250 points.

Blue yields 0 for both participants.

### **Information**

Before you make your decision, you have the opportunity to request information on the decisions of participant B in earlier rounds. By clicking the boxes you obtain a summary of the actions of participant B in the past **6 rounds**. You can request two kinds of information.

1. Information about what participant B decided, in the role of A, when s/he was paired with **you**
2. Information about what participant B decided, in the role of A, when s/he was paired with **others**

This information is obtained by clicking the boxes. The information will be displayed on the screen like this:

1. Participant B decided in earlier rounds, when s/he was paired with **you** in role A: ... times yellow and ... times blue
2. Participant B decided in earlier rounds, when s/he was paired with **others** in role A: ... times yellow and ... times blue

Requesting information about participant B comes at a **cost**. This amounts to 5 points per requested information item. If you request both kinds of information, it costs 10 points. Only the participant clicking on the buttons obtains this information. The number of decisions of B you can see is 6 at maximum. If participant B has not been in the role of A, you will see a 0.

*--- The next paragraph is only shown in the Noise treatment ---*

The information about what player B decided when s/he was paired with **you** is **perfectly reliable**. Information about what participant B decided when s/he was paired with **others** is **not perfectly reliable**. In  $1/6^{\text{th}}$  of the cases, a 'blue' choice is displayed as 'yellow', or a 'yellow' choice is displayed as 'blue'. Thus, the information on what participant B did, when s/he was paired with others, is not completely reliable.

*--- End of Noise treatment paragraph ---*

When participant A has made his decision, both participants will be informed about their payoffs in this round. This is the end of the round. In the next round you will be paired with another randomly drawn participant. The roles of A and B are randomly assigned.

Please stay seated at the end of the experiment. We will call your desk number, so you can be paid in the reception room individually.



## ***Appendix A2: Instructions “Public goods and private aversions”***

Welcome to this experiment on decision making. Please read these instructions carefully. Every participant receives exactly the same instructions. The instructions are easy; if you follow them carefully, you can make quite some money. How much you earn depends on your decisions and those of the other participants in this experiment. At the end of the experiment, your earnings will be paid in euros.

During this experiment we will compute your earnings in ‘francs’ instead of euros. At the end of the experiment your earnings in francs will be exchanged to euros at the exchange rate of 1 franc = €0.03 euro.

Please note that it is not allowed to communicate with the other participants. Please raise your hand if you have a question; one of us will come to your desk to answer your question.

There will be two experiments today. The first will take approximately 10 minutes. Afterwards you will receive the instructions for the second experiment.

### **First experiment**

In the first experiment you will be asked to make one choice. The choice determines the allocation of a sum (in francs) to you and to a randomly chosen other participant.

Below you see the computer screen for the first experiment.

Cirkel

instructies  
**Kies een punt op de cirkel. Gebruik daarvoor uw muis. Gebruik de knoppen links- en rechtsonder voor meer precisie.**

De "ander" is willekeurig gekozen.

beslissing

verdienste	
voor u:	77,3
voor de ander:	63,4

Bevestig

Every point at the circle defines a sum you receive (+) or lose (-) and a sum the other receives (+) or loses (-); losing means the sum will be deducted from this person's earnings. Your choice therefore influences whether you and/or the other person earn or lose money.

You can click on a point on the circle with the mouse pointer. An arrow indicates your choice. The corresponding amounts for you and for the other will be shown in the window on the right. In the above example you chose 77,3 francs for you and 63,4 for the other.

You can adjust your choice by clicking at another point on the circle, either with your mouse or with the buttons underneath the circle. If you are satisfied with your choice, please click the confirmation button.

Your earnings consist of the sum of what you allocated to yourself and what another participant allocated to you, by exactly the same procedure. Note that the participant to whom you allocate a sum will probably be another than the one who allocates a sum to you. Any of

these could be negative or positive. Only after the second experiment you will learn what the other allocated to you.

If you have any questions, please raise your hand. If not, please click on 'ready'. When everyone is ready with the instructions we will start with the first experiment.

### **The first part of the second experiment**

The first part of the second experiment consists of 10 rounds. At the start of a period, participants are divided in groups of 3. All group members are anonymous, which means you don't know who is in your group and others don't know whether you are in their group. Between the periods the constellation of your group changes- you will play with other participants every period.

At the end of the period the francs you have earned will be put on a savings account. The account and the period number are shown at the upper left of the screen.

In these instructions we will show numerical examples. These are chosen randomly. They do not give information about what you can expect during the experiment.

### **The project**

At the start of each period every participant receives 20 francs. Each participant may decide whether he or she wants to contribute to a group project. You can only choose between contributing all (20) francs or nothing. The other two group members decide at the same moment.

### **Earnings**

The contributions of the three group members to the group project will be added. The experimenters will increase the total sum with 50% and distribute the final sum evenly over all group members. The project earnings are thus for every group member  $\frac{1}{3}$  of 150 % of the

total contribution to the project. This means that every franc you (or someone else) contributed will result in an additional income of 0.5 franc for you and for every other group member.

Your earnings after the first decision in a period are the sum of

1. The number of francs you did not contribute (either 0 or 20)
2. Your income from the project ( $0.5 \times$  total contribution to the project)

These earnings are calculated the same way for every group member.

After everyone has reached a decision, all group members will learn whether the other group members have contributed, what the total contribution to the project is, and what the earnings of the group members are. This information will appear on a window in the center of the screen.

### **Second decision**

You now know whether the other group members have contributed to the project, and they know whether you did. Now you have the opportunity to lower the earnings of the other group members by allocating deduction points. You can decide for each of the two other group members separately whether you want to allocate a deduction point to them; one is the maximum.

What happens if you allocate a deduction point?

1. If you decide to allocate a deduction point to another group member, you lower their income with 3 francs. If you decide not to, their earnings remain unchanged.
2. Every deduction point you allocate to someone costs you 1 franc. If you decide not to allocate deduction points, it doesn't cost you anything.

The other group members simultaneously decide whether they want to allocate deduction points to anyone, including you. For every deduction point you receive, your earnings are lowered with 3 points. If you do not receive any reduction points, your earnings will remain unchanged.

The group members' contributions remain visible while you are deciding on deduction points.

### Total earnings in one period

Your earnings in one period consist of

- Your earnings after the first decision
- Lowered by the costs of the deduction points you allocated to others
- Lowered by the costs of the deduction points you received

These earnings are computed similarly for all group members. Your earnings from this period will be transferred to your 'savings account'.

The result of a period will be summarized as below:

Er zijn 1 aftrekpunt(en) aan u uitgedeeld door andere deelnemers. U verloor daardoor 3 franc(s).  
Uw gemaakte kosten voor het uitdelen van aftrekpunten zijn 0.  
Uw verdiensten uit de eerste fase waren 20.  
Uw totale verdiensten uit deze ronde zijn 17 franc(s).

Verder

	U	deelnemer 1	deelnemer 2
bijdrage	ja	nee	ja
verdiensten	20	40	20
aftrekpunten door u		0	0
aftrekpunten door deelnemer 1	0		0
aftrekpunten door deelnemer 2	1	0	
verdienste ronde	17	40	19

## **Second part of the second experiment**

The decisions in Part II of this experiment resemble the decisions in Part I. The difference between the parts is that groups are composed in a different way. The composition of groups is no longer random. From now on you can state a preference over your fellow group members.

Every group member will be allocated to either role A or role B in every period. 6 participants in role B and 3 in role A are pooled together, from which 3 groups of 3 can be composed. Every group consists of one A and 2 B's. Your role may change across periods.

If you are in role A, you can express a preference over the 6 B's with whom you could form a group. You will be asked to rank the participants B from 1 to 6. If you are undecided, you may give two B participants the same number. If you do not want to be in one group with a certain B participant, you can allocate a "-" to him or her.

Suppose you prioritize the 6 B participants as below:

B1	2
B2	1
B3	-
B4	2
B5	3
B6	4

This means you prefer to be with B2 in a group, and you are indifferent between B1 and B4. If necessary you would form a group with B5 and/or B6; you do not want to be in a group with B3.

### **Information for participant A**

Information about the B-participants will be shown on the screen to facilitate your choice. This information concerns their contribution, the deduction points they allocated and their earnings. If there is no information available, a "-" will be shown. The information for the previous round is given immediately, but if you want you can browse through earlier periods

by means of the arrows on the left. Note that you can only go back to the first period of Part 2; no information is given about part I.

The information window is shown below.

Kies ronde		bijdrage	afrekpunten uitgedeeld	verdiensden	voorkeur
<input type="button" value="◀"/>	<input type="button" value="▶"/>	nee	0	27	<input type="button" value="◀"/>
		ja	0	30	<input type="button" value="▶"/>
		ja	2	8	<input type="button" value="◀"/>
		ja	1	26	<input type="button" value="▶"/>

Once the groups have formed, you will be shown the same information at the moment you have to decide about your contribution.

At the deduction points stage, however, the only available information is about behavior in this period. You can no longer match a participant to the information shown before. The only thing you see is what was shown in part 1: information on contribution and earnings in this periods.

### Information for participant B

A participant in role B can express his or her preference for a group composition in another way. He or she can indicate for every a participant whether he/she wants to be in a group with the A participant, if this person would agree.

As a B participant you will be shown information for every A participant about contribution, deduction points and the earnings in previous periods. If no information is available, “-” will be displayed.

Kies ronde		bijdrage	afrekpunten uitgedeeld	verdiensden	voorkeur
<input type="button" value="◀"/>	<input type="button" value="▶"/>	nee	0	40	<input type="radio"/> ja <input type="radio"/> nee
		nee	2	35	<input type="radio"/> ja <input type="radio"/> nee

After the groups have been formed, you will receive the similar information when you have to decide about your contribution. B participants, like the A participants, can see only information about contributions in the previous period while deciding to allocate deduction points.

### **Group formation**

Participants A and B will be matched according to their preferences by the following procedure.

When all participants have made their decisions, one A participant will be chosen randomly. This person will be matched to B participants according to his or her preferences, but only if the B participants agree to be in a group with this person. If such a match does not exist, the A participant will not be allocated to a group. Then a second A participant will be randomly chosen and matched to B participants through the same process. After the procedure has been applied to all A participants, it ends; B participants that have not been 'matched' will not be allocated to a group. The group stage will then proceed exactly as in part I.

If you are not allocated to a group, you will receive the 20 francs at the beginning of the period, but you will not receive any income from the group project and you cannot make a decision about contribution or deduction points.

Please raise your hand if you have any questions.

## ***Appendix A3: Instructions “Group competition and punishment”***

### **Introduction**

Welcome to this experiment on decision making. In this experiment you will earn money. The amount depends on your choices and on the choices of other participants present here.

The experiment is organized by the ‘Institute for Biodiversity and Ecosystem Dynamics (IBED)’ of the Science Department of the University of Amsterdam, in collaboration with the ‘Center for Research in Experimental Economics and political Decision-making (CREED)’ from the Department for Economics and Business. The results of the experiment will be used only for scientific purposes.

The next 1.5 hours are organized as follows:

1. Instructions for this experiment. Please read them carefully, it is of utter importance for this research project that you understand what will happen.
2. Practice questions. If you can answer these questions properly, the researchers can be convinced that you understand the experiment correctly.
3. A short questionnaire
4. The experiment itself
5. A questionnaire at the end
6. Payment. After the experiment, everyone will be paid individually and anonymously. The participants will be called to the reception room to be paid.

### **Instructions for the experiment**

All participants receive the same instructions. The experiment consists of 30 periods. In every period you will be asked to make a decision of the same kind.

At the beginning of the experiment, all participants will be divided into groups of three. The group members are anonymous. The composition of your group does not change- you will remain in your group with the same participants during the entire experiment. The same applies to the other groups.

Earnings are denoted in fiches. At the beginning of the experiment, everyone will receive an initial endowment of fiches. The experiment consists of 30 periods and every period consists of two stages. In every stage, you and the other group members make a decision. After every period, your earnings will be transferred to your saving account. The total number of fiches at your saving account after 30 periods will be changed into euros.

1 fiche = 3 eurocent.

During the experiment you are not allowed to communicate with the other participants in any way. If you have a question, please raise your hand. One of us will come to your table and answer your question.

Please find an explanation of the stages of the experiment below.

#### Stage 1- contributions to the group project

At the beginning of every period, every participant receives 20 fiches. Each period you may decide how many of your fiches you want to contribute to the project and how many you want to keep. The other two members of your group simultaneously make the same decision.

#### Consequences of your decision

All contributions to the group project are then summed. The experimenters will increase the total amount with 1/8 and divide this amount evenly over all group members. The income from the project is thus for every group member 1/3 of 112,5 percent of the total contribution to the project. This means that every fiche that you (or someone else) invest in the project will yield 0.375 fiches, both for yourself and for the other members in your group.

Once everyone has made their decision, you will learn what the other group members' contributions were, how much has been contributed in total, and the sum of the group member's earnings.

Your earnings at stage 1 are thus:

1. The number of fiches you did not contribute ( $20 - \text{your contribution}$ )
2. Your income from the project ( $0.375 \times \text{total contributions to the project}$ )

These earnings are calculated similarly for every participant.

*(Only in treatments with punishment- BaseP, ObsP, CompP)*

## **Stage 2: the second decision**

You now know how much each of the two other members has contributed to the project. Similarly, the two other members know what you have contributed. During stage 2, you have the opportunity to decrease the earnings of each other member by awarding points to them. You can award at most 10 points to every group member. Every member takes at the same moment a similar decision with regard to the other group members. This means they can decide to attribute points to you, too.

What happens if you award points?

- For every point that you give to someone, their earnings diminish by 3 fiches. You don't have to award points. In the case you do not award points, the other's earnings remain unchanged.
- Every point that you give to someone, lowers your income with 1 fiche. If you do not award any points, you do not incur any costs.

Similarly, every point that is awarded to you will lower your earnings with 3 fiches. If there are no points awarded to you, your earnings do not change.

Note that only the results affecting you will be displayed after this period, e.g., how many points have been awarded to you.

Your earnings after Stage 2 are now composed of

Earnings from stage 1

Minus the costs of awarded points to others, if any ( $1 \times \text{total number of points you awarded}$ )

Minus the fiches that were deducted because of points received from others ( $3 \times \text{total number of points received}$ )

*(Only in treatments with encounters- Obs, ObsP, Comp, CompP)*

### **Stage 3 – encounter with another group**

At the end of each period, there is a chance that you and the other group members ‘encounter’ another group of 3. The chance that this happens is 20% in every period; on average you can expect to encounter another group every 5<sup>th</sup> period. The encounters are determined randomly by the computer. Note that the actual number of periods between encounters is not necessarily the average, since the encounters are randomly determined by the computer program. Every period the chance to encounter another group is 20%.

If you and the other group members encounter another group of three, everyone in your group receives information about the total earnings of the other group in this period. The members of the other group also get to know what the total earnings of the members in your group have been. No one else receives this information. You will always encounter the same other group.

*(ONLY in treatments with competition- Comp, CompP)*

The encounter with another group has the following consequences: The group with the highest total earnings will receive fiches from the group with the lowest total earnings. The number of fiches that is exchanged is 3 x the difference in total earnings. These fiches will be distributed evenly among the 3 members of the group with the highest total earnings. You will receive the difference in total earnings if you are a member of the group with the highest total earnings, but you will lose the difference in total earnings if you are a member of the group with the lowest earnings.

At the end of a period in which you have encountered a group are the sum of

1. your earnings from stage 1
2. your costs from stage 2 (if any)
3. your share in the earnings resulting from the difference in this period between your group and the other group (positive or negative)

Your earnings at the end of a period in which you do not encounter another group is thus the sum of

1. Your earnings from Stage 1
2. Your costs from Stage 2 (if any)

The earnings are computed similarly for every group member. Your earnings will be added to your savings.

It is in principle possible that you have negative earnings at the end of a period. This negative amount will be subtracted from your saving account.

Please raise your hand if you have any questions.



## *Appendix B: Decision to include*

<b>Model</b>	<b>I</b>	<b>II</b>
<b>Treatment</b>	<b>Baseline</b>	<b>Punishment</b>
<b>Contribution i (t-1)</b>	-2.64(0.58)***	-0.66 (0.54)
<b>Contribution j (t-1)</b>	-0.95 (2.53)	1.59 (1.45)
<b>Punishment points i (t-1)</b>		0.30 (0.54)
<b>Punishment points j (t-1)</b>		1.08 (1.49)
<b>Period</b>	4.83 (1.80)***	0.02 (0.06)
<b>Inclusive value</b>	-9.56 (3.53)***	-1.16 (1.00)
<b>Constant</b>	2.65(0.63)***	0.95(0.40)*
<b># observations (groups)</b>	957(4)	862 (6)
<b>R<sup>2</sup></b>	0.59	0.36

*Notes.* The table presents the results of two binomial logit regressions, used to explain the inclusion of  $j$  (a responder) by  $i$  (a proposer). Formally, it gives the  $\beta$ -coefficients of a vector of independent variables relating to  $i$  and  $j$  in  $t$  as described in the first column of the table. A (white noise) matching-group-specific error corrects for the dependencies within matching groups.



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