Laboratory and field experiments on social dilemmas

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Proefschrift

ter verkrijging van de graad van doctor aan de Universiteit van Tilburg, op gezag van de rector magnificus, prof. dr. Ph. Eijlander, in het openbaar te verdedigen ten overstaan van een door het college voor promoties aangewezen commissie in de aula van de Universiteit op woensdag 16 maart 2011 om 14.15 uur door

Johannes Theodorus Roeland Stoop

geboren op 1 augustus 1982 te Goirle.

PROMOTORES: prof. dr. D.P. van Soest
prof. dr. C.N. Noussair

Preface

In front of you lays the final product of my first three years in the academic profession. For me, those three years as a PhD student have been among the best of my life. Unfortunately, an important reason for why those three years have been so awesome is not reflected in the chapters of this final product: the interactions with friends and colleagues which have inspired me to write a thesis like this. For that reason, I would like to devote the space in this preface to thank everyone who has had a significant impact on this thesis. The order in which the names of those who pass by is chronologically rather than in order of importance. I use a chronological order to avoid the problems that come along with putting weight on memorable suggestions or conversations. This preface is merely intended to put those who helped me in the limelight, rather than to be offensive to those who should have earned a spot nearer to the beginning.

The years as MPhil student

My first awareness of the existence of the academic world in economics is thanks to Prof. Sjak Smulders. At the end of my Bachelor program in Tilburg, I followed a course called 'Growth and Technology'. Sjak Smulders told me of the opportunity to take a Master that prepared me for a life as a researcher. The program was called the Master of Philosophy, a two-year program which is a preparation for a job as PhD student. Strange as it may seem now, back then I would never have thought that there was such a thing as an 'academic life'. In my mind, a Professor was just a Professor, whether he or she was an Assistant Professor, Associate Professor or Full Professor. Likewise, articles which I had to read for my classes were all of similar quality; I never paid attention to the journals in which they were published. Thanks to Sjak Smulders, I became aware of the exciting competition that the academic life represents.

Before starting the MPhil program that Sjak Smulders advised, I started my regular Master in Economics. Prof. Erwin Bulte and Dr. Frans de Vries supervised my thesis on the Porter Hypothesis. My first real contact with the academic scene was a result of this thesis: Prof. Cees Withagen invited me to become his research assistant for a couple of months, to try to convert my thesis into something publishable. I view that invitation as the birth of my academic career. Cees, many thanks again for giving me that opportunity.

After my period as a research assistant, I started the two-year MPhil program in Tilburg, hoping to get a job as a PhD student afterwards. It was a challenging program, but with the help of my classmates I was able to put myself through. For me, the best memories are all those times we made homework together (especially the Macro 1 assignments in room K414). The ones I spent the most time with were Salima Douhou, Alexandra van Geen, Thijs Griens, Jiehui Hu, Ting Jiang, Kenan Kalaycı, Martin Knaup, Kim Peijnenburg, Pedro Raposo, Marta Serra Garcia, Sotirios Vandoros and Peter van der Windt. Many thanks to all of you! Most likely, those good memories for me are still haunting the nightmares of some of the tutors of that time. Corrado Di Maria and Willem Woertman, thanks for never giving up on us.

The most important thing that happened to me during the MPhil program, was an invitation by Prof. Daan van Soest. He invited me to join a research team that he was assembling for an NWO research proposal. At that time, I had an increasing interest in everything that had to do with experimental economics and the environment. The topic of the proposal involved both my interests, so I did not hesitate a moment to accept his offer. Daan, I would like to thank you once again for the tremendous opportunity you gave to me. At the moment of the invitation, Daan was not yet a Full Professor, which meant that he was not allowed to become my promoter. For that reason, we asked Prof. Charles Noussair to become my promoter, a task which he accepted. On board of the research team were Prof. Johan Grasman, Dr. Jana Vysrastkeva and Andries Richter. Together with Jana, Daan and I would start the program with a paper on the evolution of counter-rewarding in a common pool resource environment, leading to Chapters 3 and 4. Jana, I vividly remember all the discussions we have had while interpreting the data of our experiments. Thanks for remaining so interested, and forcing me to present my ideas in clearer ways. Andries, I would like to thank you too for all the comments you have given on earlier versions of some of my chapters. Equally important, I would like to thank you for the fun times we had during the EAERE conference in Gothenburg. Johan, although we have not worked together closely, I would like to thank you for your comments on my work on occasions where we met.

A final important thing that happened to me during the MPhil program occurred during the NAKE Workshop, organized by Prof. Jenny Ligthart. First of all, I would like to thank Jenny Ligthart for organizing that week, it turned out to have quit an impact on the course of my life as a PhD student (although I don't have the counterfactual on what would have happened in case I did not attend the workshop, I assume that attending the week resulted in an improvement). During the NAKE workshop, I had a conversation with Prof. John List. I told him about the plans that I had for my thesis, and asked him if he could give me any tips on how to make improvements. His advice was brief: Including a field experiment on social dilemmas would be a major improvement of my plans. I took his advice seriously, maybe even a little bit too extreme. The search for a place to conduct a field experiment really became an obsession, but then in the good sense of the word. In the months that followed, everywhere I went, I asked myself the question if I could transform the place where I was to my 'field experimental lab'. At times this was a frustrating business, because many of the 'labs' I walked into turned out not to be suited for proper experimentation. However, the thought that I only needed one 'lab' pushed me to continue my search.

The years as PhD student

One of the tasks a PhD student has to fulfill, is teaching. I always enjoyed teaching a lot, and part of it is because I could work with colleagues who really wanted to teach me to become a better teacher. Katie Carman, thanks for all the time you have invested in me to become a good tutor for the course Institutions and Incentives. My teaching improved every year, and part of it is because of you. You were always fair in the way you distributed the grading load, and that meant a lot to me. I would also like to thank Prof. Aart de Zeeuw for his help in tutoring the course Environmental Economics. Although I only taught that course one year, it was my first teaching experience, and this was very valuable.

During my time as a PhD student, I have talked a lot about my research with others (whether they wanted to know about it or not). Especially for my colleagues, I must have been a constant plague. My first two go-to guys have always been Gerard van der Meijden and Peter van Oudheusden. From macro economists, it is hard to gain approval for micro economic ideas, so they provided an excellent benchmark for new ideas. Once my ideas had finally been granted the 'Geer and Peer'-seal of approval, it was time to fine tune my ideas. Patrick Hullegie and Nathanaël Vellekoop were never too shy to give me their thoughts on my research. I would like to thank all of you for helping me out with experimental design ideas and interpreting my data.

Once my ideas were developed into its final stages, and some data had been gathered, I consulted the more experienced colleagues. Many thanks go out to Eline van der Heijden, Wieland Müller, and Jan Potters. You have always listened very carefully to my research ideas, and your suggestions often turned out to result in great improvements. Eline, special thanks to you for your help with the statistical tests I used, especially in the early stages of my PhD track.

Also to friends I was talking non-stop about new ideas for research. On Tuesday nights (the 'Boys Night Out') the audience consisted of my close friends Ad van Amelsfoort, Marcel van Amstel, Roy Maas, Martijn van Steensel and Ferry Vermeer. Friday/Saturday nights (the 'Real Boys Night Out') were usually reserved for my dear friends Ramon Kool, Mark Ligtvoet, Bas Postema, Jeroen Remie and Maarten Rossou. Thanks guys, for all your comments and for never shutting down a conversation meant to improve the quality of science. I would like to spend some extra sentences to thank Mark Brouwers, a very good friend of mine. It was during an evening with him in which we speculated about the interpretation of experimental data of behavior in a common pool resource. The new interpretation was completely different from the one I presented in an earlier version of the paper. Our speculations seemed to be supported by the data, and the end result can be found in Chapter 4. Mark, thank you for your valuable input!

Former classmates of the 'Algemene Economie' Master proved to be a good soundboard as well. The trip to France was filled with conversations about social dilemmas. Thanks to you all: Vincent Bosgraaf, Magiel van der Groes, Sjoerd Kitzen, Maarten van Rossum, Emiel Suverain, Derk Timmer, Jeroen Udo, and Bart Verbeet.

After basketball practices, I also found ways to talk about my research with team mates. In many occasions, Joost de Bakker and Ardavan Farjami Haidari gave me very thoughtful comments which have influenced my research. Thank you very much!

Even at home I spent quite some time talking about my research. Thanks to all my 'Koetjeboe' house mates for all those conversations: Rogier Fakkeldij, Roel Ikink, Rogier van Kalmthout, Mathieu Ottevangers, David Prinsen Geerligs, Joost Verlaan and Reinier Willers. It was at home where I finally ended my search for an environment suitable for field experiments. The two friends responsible are Niels van den Broek and Magiel Driessers. Still obsessively searching for a place to conduct field experiments, I overheard a conversation between the two. They told me about a place where recreational fishermen spend their leisure to catch Rainbow Trout: 'de Biestse Oevers'. Their detailed talk about this pond convinced me to have a closer look and to investigate the possibilities to conducting a field experiment. The setting turned out to be ideal, and it resulted in Chapters 5 through 7. Niels and Magiel, I would like to thank you sincerely for telling me about this pond. Without your remarks, I might not have been able to conduct any field experiments at all. Most likely, I would have missed out on a lot of exciting adventures which made my years as a PhD student so colorful.

As a consequence of discovering the recreational fishing pond to conduct research, I had to make myself acquainted with the ins and outs of the world of sports fishing. The biggest source of help was provided by my dear friend Arjen Timmermans. Arjen, thanks for the enthusiastic help you gave me when I tried to get to know the fishing habits.

Of course, I could never have conducted the field experiments without the help of the owners of the fishing facility. Initially, the pond was owned by Ad and Thea van Oirschot. Thank you very much for giving me opportunity to conduct my field experiments, and for helping me out with letting things run smoothly during all the sessions. After a while, 'de Biestse Oevers' changed ownership to Ben and Shirley Willems. I would like to thank you too for allowing me to continue my research, and for your excellent assistance. Finally, I would also like to thank the other staff of the Biestse Oevers: Frans, Tim and Koen.

Conducting field experiments at the Biestse Oevers has always been very labor intensive, and impossible to do single handedly. Therefore, I would like to thank the Master students who helped me conducting the field experiments: Stef van Kessel, Mike Groels, Paul Ludeña Delgado and Menusch Khadjavi. Stef, it has been a pleasure to explore and discover all facets that come along with conducting experiments at an environment such as the pond. Your efforts really helped me to conduct the experiments smoothly. Mike, thanks to you too for your help at the pond. I was impressed by the speed with which you wrote your thesis. Paul, thank you for all the hard work you did. Especially the night we sacrificed in order to make last-minute changes to the experimental design was truly a blessing. Some of your comments were very influential for the experimental design. Menusch, also your help was greatly appreciated.

Traveling such a long way to help out meant a lot to me. Finally, I would like to thank my colleagues and friends who helped during the experimental sessions. Alexandra, Peter, Patrick, Roel and Sander Tuit, thank you once again!

Somewhere in my second year, Daan gave me the opportunity to co-organize the tenth Bioecon conference in Cambridge, UK. The conference was entitled 'The Effectiveness and Efficiency of Biodiversity Conservation Instruments' and matched perfectly the topic of my thesis. I would like thank Erwin Bulte and Andreas Kontoleon for being part of their team, and all the help and useful tips they gave me. Co-organizing the conference was a great pleasure for me, and it allowed me to get to know the people involved in the scientific community.

In my last year as a PhD student, I visited the University of Chicago. This trip was truly a great experience: I took classes from Nobel laureates, and I have met a lot of great researchers. First of all, I would like to thank Codrien Arsene for sharing his apartment with me when I was kicked out of my hotel (long story on miscommunication about the payment method. . .). Dave Herberich and Nikki Sullivan, thank you for showing me the city of Chicago, especially the American Football match and the shooting range were highlights. I would like to thank Dana Chandler and Johana Muriel Grajales for spending time with them in Chicago and talking about research. Finally, I would like to thank all the visitors of the two presentations I gave at the Becker Center. During those presentations, I had the impression that everyone had just one goal, and that was to improve my paper. I never had such an experience before, and it was truly amazing!

Special thanks to. . .

The last section of this Preface is devoted to the friends and colleagues who had an enduring influence on my PhD thesis. It was hard to fit them into the chronological order, because then I would have to mention them every other paragraph. First of all, I want to thank Daan van Soest for being my supervisor. Unlike many other supervisors, your door was always open for me whenever I had questions. Of course, being my supervisor, the way you think about economic problems has influenced me enormously and your critical and original view on my efforts to analyze data has always improved our papers. Working with you during all experimental sessions always went smoothly (both in the lab and at the pond), and after an experiment I always enjoyed your invitations to have a drink at your house. I hope that we can extend our work together in the future the way in which we collaborated during the past three years.

Secondly, I want to thank Charles Noussair for being my second supervisor. Charles, working with you was always a true pleasure for me. Your enthusiasm kept my spirit high in times when I believed that our field experiments yielded uninteresting results. Also, the quick way in which you are able to draw insightful and deep conclusions from the field data that we generated, has always impressed me. It would be great if we continue to work together in the future.

Thirdly, I want to thank John List for all that he has done for me. Thank you for pointing out where the research frontier is, thank you for giving me the opportunity to work together with you on tournament incentives, thank you for inviting me to visit the University of Chicago, and thank you for writing letters of recommendation for me. The talks that I had with you in Chicago were truly inspiring, and they pushed me to work harder than I had ever done before.

Fourthly, I would like to thank Christian Bogmans for being such an awesome room mate. Countless times, I had tears in my eyes of laughter. I really regret the fact that we will probably never share a room in the future (so long for our Hall of Fame…). Good luck with finishing your thesis. I am sure that your keen ability to translate real world problems into abstract economic models will get you a nice place to do research.

Fifthly, I would like to thank two persons: Chris Müris and David Voňka. During the MPhil program you helped me a lot with hard econometric courses, and during my PhD track you always were able to detect mistakes in my econometric analyses. Also the feedback you gave me when I talked about new ideas for experiments were really useful. It forced me to redesign my experiments more often than I dare to admit. Without your help, I am sure that I could not have achieved the things I have.

Sixthly, I would like to thank those who had helped me with all the LaTeXproblems I encountered. The ones I refer to are Hendri Adriaens, Marcel van Amstel, John Kleppe, Sander Tuit, and Ruud Hendrickx. Without you, my thesis would likely have been written in the evil M$ Word, and we all know what a mess that would have been…

Last, but certainly not least, I would like to thank my mom, dad, brother and sister for their support during those three years. Mom and dad, you always listened very carefully to what I told you about my research, and you even read very carefully some of the chapters of this thesis (shamefully, I have to admit that my mom even found a mistake I made in some of the math…). Loes thank you too for being interested in my research (I know you especially liked the stories from the pond, told in the beautiful Tilburg dialect). It was nice to see that the economic way of thinking influenced your Master thesis on the

History of Art. Bart, I really liked the conversations we had about life as a scientist. Those conversations always made me relativize the sometimes crazy world of academia.

With these final words of thanks, it is time to move on to the main part of this thesis. I hope that you will keep on reading the rest of my thesis, and that you may enjoy it as much as I have done writing it.

Contents

Chapter 1

Introduction

1.1 Social dilemmas and informal institutions

A variety of environmental problems are seen as the major contemporary challenges the world faces. Examples of these problems abound: Many fisheries are confronted with severe drops in stock levels, leading to the collapse of the Canadian cod stock (Milich (1999)), forests being reduced in size by thirteen million hectares each year (FAO (2005)), and the population of wild vertebrate species has fallen by 31% during 1970-2006 (GBO3 (2010)). One important cause of today's environmental problems is the lack of sufficiently well-defined or enforced property rights. It is this feature of renewable natural resources that transforms the environmental problems to a 'social dilemma'. A social dilemma is a situation in which private interests are not in line with group interests. Environmental problems are subject to this problem; whereas the returns from harvesting a resource accrue to the individual only, some of its costs are passed onto others (for example in the form of lower resource stocks). Selfish individuals will therefore make excessive use of the resource, although all would be better off mitigating their harvests. Explained in different terms, a selfish individual would like to free-ride on others by harvesting excessively, rather than to cooperate by providing the public good of maintaining the resource stock.

It falls to the government to overcome the problems associated with social dilemmas. A government has the right to define and enforce property rights, therefore, the government seems to be the right agency to deal with most of today's environmental problems. However, scholars have taken the view that a reduction of environmental problems can also be established through cooperation of the users of a resource. There are many examples of situations where community members can prevent the downfall of a resource stock (see for example Feeny et al. (1990), Baland and Platteau (1996), and Ostrom et al. (1999)). The key to which resources can be conserved without government intervention are 'informal institutions' (see for example Vyrastekova and van Soest (2005)). Informal institutions are defined as sets of self-enforcing local rules governing the behavior of resource users. Those self-enforcing rules can take many forms, such as ethical norms to which resource users live up to, but also sanctions or rewards for those who deviate from an established group norm.

For government policy purposes, relying on informal institutions can be an efficient tool. When community members find ways themselves to cooperate in a social dilemma, such as mitigating the harvests of a resource stock, then less appeal has to made to a government to intervene. Needless to say, a government can save costly expenditures when it merely has to encourage resource users to rely on informal institutions. The aim of this thesis is to gain insights into which informal institutions are effective in promoting cooperation in social dilemma situations. An answer is sought to the following research question:

'*How does behavior in social dilemmas, such as the conservation of renewable natural resources, depend on the informal institutions in place, and what are the implications for government policy design?*'

More generally, the aim of this thesis is to study how cooperation in social dilemmas is affected by informal institutions. By merely observing social dilemmas that are found in the real world, it is impossible to properly study the effects that informal institutions have. Social dilemmas are affected by a multitude of influences, all of which arise or disappear endogenously. Moreover, the outcomes of social dilemmas are likely to have feedback impacts on informal institutions, which then further influences behavior in social dilemmas. In order to study the causal effects of informal institutions, despite the complexity that is involved with social dilemmas in field settings, I will only focus on the effects that informal institutions have on social dilemmas. For that reason, I will try to seek an answer to the research question by means of experiments, both in the traditional laboratory, as in a field setting. The novel feature of experimentation is that informal institutions can be imposed exogenously, and therefore, a causal inference can be made on its influence on behavior in a social dilemma.

Conventional economic theory argues that social dilemmas could never be solved by informal institutions, if they are costly and all individuals involved are rational and selfish. To see why this is so, note that using informal institutions comes with a second-order problem of free-riding. If it is costly to implement a self-enforcing local rule to change the behavior of a fellow resource user, then all would like someone else to pay those costs. In doing so, an individual saves costs while still reaping the benefits of the behavior change of the fellow resource user. Of course, if all resource users are selfish, then all would think alike, and no one would make use of costly informal institutions. Anticipating that informal institutions are not used, then no one will face the negative or positive consequences of the self-enforcing rules. Therefore, it is to be expected that all individuals will act selfishly in the social dilemma and there will be no signs at all of altruistic behavior: 'behavior by an individual that increases the fitness of another individual while decreasing the fitness of the actor' (Bell (2008)). Conventional economic theory predicts that those who are altruistic will go extinct, because they have a lower fitness level than those who are not altruistic.

The predictions of classical economic theory come with one problem: There are many real life situations in which individuals are able to overcome widespread free-riding in social dilemmas. It seems as if humans in the real world are no strangers to cooperation. In Chapter 2 of this thesis, I come back to this issue. A review is given of theoretical arguments that scholars have made that show that individuals can overcome free-riding behavior in social dilemmas. Three mechanisms are discussed. This first mechanism by which altruism might survive as a strategy, is called 'kin selection'. The argument made is that altruistic acts towards family members can give indirect fitness advantages. Someone who is altruistic towards a brother can indirectly pass his genes on to the next generation, if that brother has enough descendants. Somehow, this mechanism is not satisfying, because in the real world, acts of altruism among non-related individuals can be found as well. This has led to the second mechanism of the survival of altruism: direct reciprocity. Direct reciprocity hinges on repeated interaction between two individuals. If one helps the other, then the other should give help in return at some later date. Both individuals will then be better off in the long run, and therefore altruism can survive. Still, the mechanism of direct reciprocity does not explain all acts of altruism found in real life, because altruistic acts seem to exist between individuals who never meet each other again. This observation has lead to the third mechanism: indirect reciprocity. The theory of indirect reciprocity predicts that altruism can survive if an altruistic act received from one individual is paid back to another individual. Of course, with such a mechanism the incentives to free-ride are huge. This problem is overcome by reputation; only those who have performed altruistic acts in the past become prone to receiving altruistic acts by others.

Kin selection, direct reciprocity and indirect reciprocity all involve interaction between just two persons. Free-riding by one leads to a loss of fitness to only one other individual. However, many social dilemmas in the real world, especially environmental problems, are in the context of groups. Free-riding by one leads to reduced fitness for more than just one individual. Therefore, a final way in which altruism can survive, is by considering informal institutions. In Chapter 2, the theories of altruistic punishment are reviewed. Altruistic punishment can work under some conditions. For example, if punishment is not too costly, or if a large enough share of the population are 'conditional cooperators'; individuals who cooperate in the social dilemma situation and who use costly punishment to sanction free-riders.

In this chapter, the outline of the remainder of this thesis is described. I will describe how I study the effects of informal institutions on social dilemmas, and the way it can be placed in the literature. Since the bulk of this thesis tries to give an answer to the research question by means of economic experiments, some background on the methodology of (laboratory) experiments is given in section 1.2. This chapter continues by describing earlier laboratory experimental literature on the effects of informal institutions on social dilemmas in section 1.3. In the final three chapters of this thesis, I leave the conventional laboratory to do experiments in a field setting. Therefore, section 1.4 provides some background information on the methodology of field experiments and presents a short overview of influential studies on social dilemmas in field studies. Finally, I describe how I test whether informal institutions can overcome free-riding behavior in a field experiment in section 1.5

1.2 The methodology of laboratory experiments

Economists have tried to test whether the predictions of standard economic theory, and those presented in Chapter 2 hold in practice. One popular way to test theories is by means of experiments conducted in controlled laboratory environments. In a laboratory experiment, the experimenter creates an artificial world, where subjects are put into an environment constructed for the purpose of research. The advantage of laboratory experiments is that the experimenter has substantial control over the environment; utility functions of agents can be induced, and all the relevant parameters of the environment can be carefully chosen (for example the institutional setting and the amount of information that is available). In order for an experimenter to conduct a proper experiment in the artificially created world, five conditions have to be met (for a more thorough discussion, see Smith (1976, 1982), Plott (1979), Wilde (1980), and

List (2006a)). The first is nonsatiation; more of the reward medium should preferred to less (usually money is the medium of reward in laboratory experiments). Second is salience; choices made in an experiment should be directly linked to payoffs in a manner understood by participants. The third condition is dominance; rewards in the experiment should be greater than subjective costs. Fourth is privacy; subjects receive information on their own payoff alternatives only. The final condition is parallelism; properties of behavior should translate to real world settings where similar ceteris paribus conditions hold.

Having designed a clean experiment, the researcher is then able to observe causal effects when changes are made to an environment. The main advantage of conducting experiments in the laboratory is that the proper counterfactual can be observed; the researcher knows what would have happened in an environment in case a certain treatment would not have been implemented. A necessary condition for this to hold, is that subjects are randomly allocated into different treatments. If this does not hold, then selection bias effects might confound the causal effect of a treatment. In real life, selection effects are hard to overcome, and hence it is hard to make causal inferences based on naturally occurring data when a policy measure is in effect. For example, consider the effects of class size on student performance. It is to be expected that smaller classes lead to greater student performance, because students receive more personal attention from the teacher. However, when looking at grades of students in small or large classes, they are more or less the same (for more details see Finn and Achilles (1990) and Krueger (1999)). One of the reasons is that smarter students are put into bigger classes; selection bias confounds the direct effects that class size have on student performance.

To see the problems of selection bias, consider the following (this analysis is based on Angrist and Pischke (2009)). Let Y_i represent the observed outcome of individual i and let Y_{1i} and Y_{0i} represent the potential outcome of an individual who has either undergone the treatment or not (represented by $D_i = 1$ or $D_i = 0$ respectively). Then:

$$
\begin{aligned}
\underbrace{E[Y_i|D_i = 1] - E[Y_i|D_i = 0]}_{\text{Observed difference between treated}} &= E[Y_{1i}|D_i = 1] - E[Y_{0i}|D_i = 0] \\
&- E[Y_{0i}|D_i = 1] + E[Y_{0i}|D_i = 1] \\
&= \underbrace{E[Y_{1i}|D_i = 1] - E[Y_{0i}|D_i = 1]}_{\text{Average effect of treatment}}
\end{aligned}
$$

$$+ \underbrace{E[Y_{0i}|D_i = 1] - E[Y_{0i}|D_i = 0]}_{\text{Selection bias}}.$$

The term $E[Y_{1i}|D_i = 1] - E[Y_{0i}|D_i = 1]$ is the average causal effect of the treatment. It shows the potential outcome of someone who has undergone the treatment, $E[Y_{1i}|D_i = 1]$, and the potential outcome of that same person in case he would not have undergone the treatment, $E[Y_{0i}|D_i = 1]$. Of course, in real life it is impossible to observe both outcomes at the same time. The term $E[Y_{0i}|D_i = 1] - E[Y_{0i}|D_i = 0]$ is the selection bias effect, it represents the average potential outcomes Y_{0i} of those who are and those who are not treated. In the example of class size and student performance, it could be the case that less smart students are more likely to be put in small classes. Therefore, those in smaller classes are likely to have worse values of Y_{0i}, which causes the selection bias to be negative in this example. This has the effect that the observed difference underestimates the true effects of class size.

By means of randomly assigning subjects to treatments, it is as if the causal effect could be observed. To see this, simply rearrange:

$$\begin{aligned}\underbrace{E[Y_i|D_i = 1] - E[Y_i|D_i = 0]}_{\text{Observed difference between treated}} &= E[Y_{1i}|D_i = 1] - E[Y_{0i}|D_i = 0] \\ &= E[Y_{1i}|D_i = 1] - E[Y_{0i}|D_i = 1] \\ &= \underbrace{E[Y_{1i} - Y_{0i}]}_{\text{Causal effect of treatment}}.\end{aligned}$$

The trick is that $E[Y_{0i}|D_i = 0]$ can be substituted for $E[Y_{0i}|D_i = 1]$, because randomization makes D_i independent of Y_{0i}. Therefore, by using randomization, the selection biases disappears from the equation, allowing the researcher to observe the causal effects of the treatment.

A large experimental literature has emerged on social dilemmas, comparing actual behavior of subjects to the predictions of conventional economic theory of zero cooperation. In the domain of group social dilemmas, such as those found with many environmental problems, two experimental games dominate the stream of research in economics. The two games are called the Voluntary Contribution Mechanism, also known as the Public Goods game, and the Common Pool Resource game. In the Public Goods game, N individuals each receive y tokens. Each token can be invested in either a private account, or a group account. Returns from the private account are for the investor only. Revenues of each token invested in the group account are divided equally among all individuals, irrespective whether or not someone contributed to the group account.

The profit equation of the public goods game usually has the following form:

$$\pi_i = y - x_i + \alpha \sum_{i=1}^{N} x_i, \qquad 0 < \alpha < 1 < N\alpha, \qquad 0 \leq x_i \leq y,$$

where x_i represents the amount of tokens invested in the group account. The preceding game represents a social dilemma if two conditions hold. The first is that individual returns of an investment in the private account are greater than individual returns of an investment in the group account ($\alpha < 1$). Secondly, the group as a whole earns the greatest payoffs when all contribute fully to the group account ($1 < N\alpha$). Because of the first condition, conventional economic theory predicts that no individual will invest any tokens in the group account when the game is played a finite number of times. For that reason, x_i is interpreted as a measure of cooperation.

The Common Pool Resource game is similar in structure as the Public Goods game; N agents can invest y tokens in either a private or group account. The main differences with the Public Goods game are threefold. First, the payoff function of the group account is non-linear. Second, the Nash equilibrium and the social optimum are in the interior. Third, the game is usually framed as a negative externality problem. An often used profit equation is the following:

$$\pi_i = y - x_i + \frac{x_i}{X}\left[AX - BX^2\right],$$

where X represents the sum of tokens put in by the N agents ($X = \sum_{i=1}^{N} x_i$). The term $AX - BX^2$ represents the yield that the common pool resource provides. Each agent i receives a share of the resource's yield equal to her share in aggregate extraction effort (x_i/X). Conventional economic theory predicts that each agent will invest in the common pool resource up to the point where private marginal costs are equal to private marginal benefits. This causes each agent to have investments equal to $x_i^{NE} = (A - w)/B(N + 1)$. However, since part of the costs of investing in the common pool resource are shifted onto others, agents have an incentive to invest more than is socially optimal. In case all agents would take into account the negative external effects they impose on others, the socially optimal investment levels are $x_i^{SO} = (A - w)/2BN$. In this game, $x_i^{NE} > x_i^{SO}$ if $N > 2$, therefore, the game represents a social dilemma.

Experimental evidence overwhelmingly shows that subjects in laboratory experiments behave differently than conventional economic theory predicts. The first experiments on the Public Goods Game appear at the end of the 1970's. Marwell and Ames (1979) conduct a variant of the Public Goods game described above; all members of the group received an equal bonus, provided enough was contributed to the group account. The authors find that subjects for whom it is less costly to contribute donate more to the public account. No evidence was found on the effects of group size; subjects donated similar amounts to the group account, irrespective of the number of group members. Isaac et al. (1985) also consider the effects of different costs of providing the public good, and they also find that subjects who can supply the public good cheaper are more willing to do so. In some treatments, the authors provide information to the subjects about the equilibrium outcomes. They find that this leads the subjects to contribute more to the group account. Isaac and Walker (1988b) delve deeper into the effects of group size. In their design, all members of the group either have big costs to contribute, or small costs to contribute. They find that groups with small costs contribute more than groups with big costs. However, the effects of group size are negligible, given the costs to contribute. One of earliest studies on the Common Pool Resource game is conducted by Walker et al. (1990). They find that, compared to the social optimum, very small payoffs are obtained. When subjects are given more tokens to invest into the group account, subjects do not hesitate to use them, leading to even lower payoffs for all involved.

Two stylized facts have emerged from hundreds of studies on the Public Goods game and Common Pool Resource game (see Ledyard (1995) for an overview of experiments conducted on the Public Goods game and Ostrom (2006) for an overview of the Common Pool Resource game). The first is that when the games are played repeatedly, considerable levels of cooperation are observed in the initial periods of the experiment; usually between forty and sixty percent of endowment is allocated to the group account. Secondly, a downward trend in cooperation is observed. As more and more periods are played, contributions to the group account become less, but many studies report substantial contributions in the last period.

The two stylized facts have influenced theorists to explain why behavior of humans differs from that of the rational and selfish actor usually assumed in theoretical models. The first fact, positive cooperation in early stages of the experiments, is interpreted as evidence that subjects have other-regarding preferences (see Rabin (1993), Fehr and Schmidt (1999), Bolton and Ockenfels (2000), and Andreoni and Samuelson (2006)). The second fact, a decline in cooperation over time, is interpreted as subjects making fewer errors over time (Palfrey and Prisbey (1996), Andreoni (1995), and Houser and Kurzban (2002)), reputation building (Andreoni (1988), Sonnemans et al. (1999), and Brandts and Schram (2001)), and as conditional cooperation (Neugebauer et al. (2007)).

1.3 Informal institutions in the lab

Although more cooperation is observed in the two social dilemma games than conventional economic theory predicts, economists have begun to search for factors that promote cooperation. Especially the search for informal instruments has spun a large literature. Among the most studied is peer-to-peer punishment, while peer-to-peer reward has received more attention lately. Punishment allows subjects to make a positive cost to reduce the earnings of other group members, after everyone learns about the contribution and earnings of each group member. Conventional economic theory predicts that the instrument will never be used. Agents would like to free-ride off the efforts of others. Free-riding is possible, because benefits of potential behavior changes in the social dilemma situation by someone who is punished, accrue to all agents, even to those who have not made the costs of punishment. If the game is finitely repeated, backward induction leads agents to refrain from using costly punishment. This works as follows: In the last period of the game, punishment cannot enforce future cooperation. Therefore, in the last period of the game, no punishment will be used. Anticipating this, agents will all free-ride in the last period. In the next to last period, agents anticipate that other group members will free-ride in the next period, and hence, costly punishment will have no effect on future play. Therefore, punishment will be ineffective in the next to last period. This reasoning continues all the way to the first period, causing punishment not to be used. A disadvantage of punishment is that welfare effects are ambiguous. Punishment might lead to an increase in cooperation, but since punishment reduces earnings of both the user and the receiver, it might be the case that all are worse off than in case of no punishment.

Yamagishi (1986) is the first to study punishment in the Public Goods game. In his design, subjects play a period of the Public Goods game first. Then, after learning who contributes least, everyone can invest in a new public pot. If enough contributions are made, then the public pot is used to reduce the earnings of the group member with the lowest contribution in the Public Goods stage. Yamagishi finds that contributions to the public pot are significant, contrary to what economic theory predicts. Moreover, subjects start to increase their contributions in the Public Goods game, hoping to avoid being punished. Fehr and Gächter (2000, 2002) implement a slightly different version of punishment. Rather than contributing to a public pot, each subject can spend resources to punish group members directly, independent of whether or not some threshold is reached. In their influential studies, the authors find that punishment is frequently imposed, and that contributions to the public good rise quickly. This finding is surprising, because groups change in composition between periods. A strategic motive of punishment seems therefore not plausible, because interaction with the same individual is ruled out. Punishment does not always work, it seems to depend on cultural aspects as well. Herrmann et al. (2008) repeat the punishment setup of Fehr and Gächter (2002) in sixteen different countries over the world. In most of those countries, punishment promotes cooperation. However, in some countries, perverse punishment is the rule rather than the exception; those who contribute most to the public good receive punishment. Punishment in the Common Pool Resource game is first studied by Ostrom et al. (1992). They find that punishment is effective in promoting cooperation. However, in combination with communication, almost full levels of cooperation are obtained by most groups.

Observing that punishment is effective in promoting cooperation, some researchers have studied how costs of punishment influence its use. Using a strangers matching protocol, Carpenter (2007) shows that punishment is like an ordinary good. After every three periods, the price of punishment changes; the cheaper punishment becomes, the more it is used. Nikiforakis and Normann (2008) find something similar, using a partner matching protocol. The authors find that punishment can lead to welfare improvements if it is sufficiently cheap. Only when the cost benefit ratio is 1:3 or better, does punishment lead to a welfare improvement.

The previous studies have shown that punishment is used often, and that it leads to considerable increases in cooperation. Contrary to what economic theory predicts, the second-order free-rider problem of punishment seems not to be an issue. The question then becomes how subjects respond to punishment when given the opportunity to take revenge, by allowing for counter-punishment. Conventional economic theory predicts that costly counter-punishment should not be feared, because free-riding motives will make individuals refrain from using it. Nikiforakis (2008) shows, by building on the design of Fehr and Gächter (2000), that revenge indeed is widespread. In his design, Nikiforakis adds a second punishment stage after the first punishment stage. Roughly a quarter of the subjects engage in costly counter-punishment. When allowing for counter-punishment, welfare levels are lower than when only one stage of punishment is present. A similar study to Nikiforakis (2008) is conducted by Denant-Boemont et al. (2007). They conduct a no-revenge treatment in which subjects do not learn who punished them, but everyone receives information on who punished other group members, and by how much. It turns out that subjects have no problem overcoming the third-order problem of free-riding; those who fail to punish others become punished themselves.

Given the success punishment has in establishing cooperation, a logical alternative instrument to consider is reward. Rewards have two major advantages over punishment. The first advantage is that rewards do not lead to losses in welfare. Whereas punishment is costly for both the sender and receiver, reward is costly for the sender, but those costs are offset by the gains to the receiver.[1] Secondly, in real life everyone is free to use rewards. No law hinders someone from giving money to another individual, or to provide help in knowledge specific tasks. The same cannot be said of punishment, because the right of coercion typically lies with the government. Hence, using punishment is in many societies not allowed.

Unlike the undivided succes of punishment, rewards do not lead to an unambiguous increase in cooperation. Failure or succes of rewards seem to depend crucially on the cost-benefit ratio. A mere transfer of rewards, those with a cost-benefit ratio of 1:1, does not promote cooperation. This has been shown by Sefton et al. (2007) in the Public Goods game. In their design, groups are formed using the partner matching protocol. Although initially rewards have a positive effect on cooperation, this effect does not last long. Interestingly, rewards were given to those who contributed more than the group average, but there was no correlation between the number of rewards received and the degree of above average contributions to the group account. Similar conclusions in the Common Pool Resource game are drawn from the study by Vyrastekova and van Soest (2008). Transfer rewards do not have a positive impact on cooperation when the same individuals meet each other in multiple periods. Things change when net-positive rewards are used; these are rewards with lower costs for the sender than the benefits for the receiver. Vyrastekova and van Soest (2008) use a 1:3 cost-benefit ratio and find a significant increase in cooperation compared to the baseline scenario without cooperation. Further evidence that net positive rewards promote cooperation is provided by Rand et al. (2009). In their Public Goods experiment, they form groups consisting of the same individuals who keep their identity each period. The authors find that rewards do a better job in promoting cooperation than punishment does. Sutter et al. (2010) compare punishment and reward in the Public Goods game, using either a 1:1 ratio, or a 1:3 ratio. Like the results of Vyrastekova and van Soest (2008), Sutter et al. (2010) find that net-positive rewards promote cooperation. In ad-

[1] An exception would be the case where the costs of a reward are bigger than its benefits, but such rewards have not been tested in laboratory experiments.

ditional treatments, subjects have the possibility to vote on whether they want to have a reward instrument or not, or a punishment instrument or not. Subjects vote more often in favor of reward than in favor of punishment. Groups who vote for reward attain greater levels of cooperation compared to a baseline without instruments. However, the greatest levels of cooperation are found in groups that vote for punishment. Contrary to the results of Sutter et al. (2010), Gürerk et al. (2004) find that an endogenous choice of reward does not promote cooperation. In their design, subjects can chose in which group they would like to participate, in a baseline group with no instruments, a punishment group or reward group. The authors find that contributions in the endogenous punishment treatment come close to the social optimum, while contributions in the endogenous reward treatment are even lower than those of the exogenous reward treatment.

All in all, most studies on net-positive rewards show that an increase in cooperation can be established. However, the way in which rewards are studied in the laboratory does not seem to fit the way it is likely to be used in the real world. Firstly, all of the above studies on rewards, with the exception of Rand et al. (2009), use a partner matching protocol where identity labels are shuffled between periods. This procedure ensures that subjects become anonymous the period after they have made their reward decisions. Conceptually this makes sense, because subjects have no way to base their reward decision other than on observed behavior in the social dilemma. However, when it comes to reward in real life, someone who uses rewards has all the incentives to reveal his identity and build a reputation. This would naturally translate into a partner matching design where identity labels are constant over the periods, like in Rand et al. (2009). Secondly, it is expected that in real life rewards are not artificially stopped after one opportunity. Like punishment, it seems realistic that the use of rewards calls for opportunities of immediate direct reciprocity.

In Chapter 3 of this thesis, a Common Pool Resource experiment is conducted with two, rather than one, reward opportunities. Adding an additional reward stage can have two effects. The first is that subjects are given an extra opportunity to show that they approve of cooperative play in the common pool resource. Anticipating this can lead subjects to be even more cooperative than in a game with only one stage of reward. The second effect can be that subjects lose their interest in the common pool resource, and engage in a (safer) bilateral exchange of reward tokens. A crucial factor of importance is the degree of anonymity. When subjects are able to track the identities of fellow group members over the periods, then it is more likely that reward and counter-rewards becomes attractive. However, when subjects are made anonymous between the periods, such a mechanism is impossible. It could be the case that in those scenarios, rewards are used in an enforcing way to promote cooperation; only those who cooperate receive rewards. The first part of the analysis in Chapter 3 considers the effects of two stages of reward in a partner matching protocol where identity labels are kept constant over the periods. We find overwhelming evidence that the second effect dominates the first; subjects try to engage in a bilateral exchange of reward tokens. Whereas the reward instrument is used almost maximally, cooperation in the common pool resource is virtually absent. The analysis proceeds by considering the partner matching protocol where identity labels are randomized between periods. It turns out that a large share of the subjects again engage in a bilateral exchange of reward tokens. Although our design hinders the ability to do so, subjects overcome this problem by systematically exerting the same effort levels in the common pool resource. This effort level serves as a 'signal' which is picked up by other users of the resource who are active in the bilateral exchange of rewards. Cooperation in this treatment is slightly greater than the selfish equilibrium, but not significantly so. The reason of the small increase in cooperation has nothing to do with an intrinsic motivation to cooperate, but it is because of the wide array of effort levels chosen by subjects to distinguish themselves from others. Finally, in another treatment the stranger matching protocol is used, where subjects are put into different groups after each period. In this treatment, the use of the reward instrument approaches zero, and cooperation levels are worse than the outcome predicted for the selfish optimum.

Chapter 4 of this thesis extends Chapter 3, by taking a closer look at the partner matching protocol with randomized identity labels. In this chapter, the two stages of reward treatment is compared by a treatment with only one stage of reward, taken from Vyrastekova and van Soest (2008). Interestingly, although rewards only promote cooperation in the treatment with one stage of reward, greater levels of rewards are used in the treatment with two reward stages. The reason is that in the treatment with two stages of reward, many subjects engage in a bilateral exchange of rewards. The way in which those rewards are used, in combination with the use of the common pool resource, gives insights into the social preferences of the subject pool. One-third of the subject pool behaves cooperatively in the common pool resource, while also being generous in terms of sending rewards and counter-rewards. Those subjects are classified as 'pro-social'. Half of the subject pool can be classified as 'strategic money maximizers', they are not acting cooperatively in the common pool resource, but do send out rewards in the first reward stage. When it comes to the second reward stage, they defect when it comes to counter-reward those who have rewarded them. Finally, one-sixth can be classified as homo economicus; these subjects show no signs of cooperation in the common pool resource and hardly use any reward tokens in either of the two reward stages.

1.4 The methodology of field experiments

Although laboratory experiments are a popular tool in an economist's tool kit, the use of it does not come without criticism. The most heard criticism is that of a lack of 'external validity'; laboratory experiments are too stylized, and are therefore not representative of the real world (see Falk and Heckman (2009) for a discussion). One way, for example, in which conventional laboratory experiments differ from real world scenarios, is the fact that in many studies, (mostly Western undergraduate) students are used as a subject pool. Students do not seem to be representative of the average population (see Henrich et al. (2010) for a review). Another way in which real world scenarios might differ from conventional laboratory experiments, is that subjects who participate in experiments are aware that they are being scrutinized. Especially in the area of social preferences, knowing that one is scrutinized by a researcher could influence subjects to make more pro-social decisions (see Levitt and List (2007, 2008) for an elaborate discussion). To address the criticism on conventional laboratory experiments, the use of field experiments is becoming more popular. Field experiments are experiments like laboratory experiments, but conducted in a natural environment. A drawback of moving to the field is that control is lost over the experiment; a researcher is not able to induce utility functions like in the laboratory. In return, field experiments have a better external validity because they are more realistic than conventional laboratory experiments.

To adequately measure behavior in a field experiment, the five conditions mentioned in section 1.2 have to be met. Another building block that is important in field experiments is randomization of subjects into treatments. Only then, causal inference can be made from observed data, because randomization allows the researcher to observe the proper counterfactual. Harrison and List (2004) propose six factors that are important to determine the field context of field experiments. These factors are the composition of the subject pool, the information that the subjects bring to the task, the commodity, the task or trading rules applied, the stakes, and the environment in which the subjects operate. Harrison and List (2004) use these six factors to propose a taxonomy of different field experiments. By adding more and more of the six elements to the conventional laboratory experiment, Harrison and List propose a methodological procedure to 'build a bridge from the laboratory to the field'; a step by step procedure that allows a researcher to study differences between conventional laboratory experiments and behavior in the field. The first step outside the conventional laboratory is what is termed an artifactual field experiment. An artifactual field experiment is the same as a conventional laboratory experiment, with the exception that the conventional student subject pool is replaced by a non-standard subject pool. The non-standard subject pool brings a different set of information to the experiment than students usually have. The second step in the bridge is the framed field experiment. In a framed field experiment, natural context is provided to the task of the artifactual field experiment. This is done by changing the nature of the commodities used in the experiment, the task or trading rules, and the stakes of the game. Finally, the last step of the bridge is termed a natural field experiment. A natural field experiment adds to the framed field experiment in the following way: The experiment is conducted in the natural environment known to the subjects, while they are not aware that they are being scrutinized. Natural field experiments are in a sense the most interesting experiments, because they use randomization and have natural realism of the task.

Comparison across the different types of field experiments allows a researcher to track differences between the conventional laboratory and naturally occurring real world settings. This makes the bridge proposed by Harrison and List ideal to gain insights in the external validity of conventional laboratory experiments. Differences in behavior between a conventional laboratory experiment and artifactual field experiment gives insights in the differences between students and subjects of the real world setting of interest. Comparing the results of an artifactual field experiment with a framed field experiment shows possible differences between a stylized context-free experiment and an experiment that has more context and more realistic commodities; an intermediate step towards the natural field experiment. Finally, differences in behavior between a framed field experiment and a natural field experiment shed light on what effects scrutiny have on behavior.

Some studies are conducted that address the external validity issues raised by critics of conventional laboratory experiments. In the domain of social dilemmas, studies that conduct artifactual field experiments on the Public Goods game or Common Pool Resource game show that behavior is similar to that observed in conventional laboratory experiments. For example, Barr (2001) conducts a one-shot Public Goods game with a sample of inhabitants of rural Zimbabwe. She finds that the subjects contribute positive amounts of tokens to the public good. When allowing for punishment, subjects cooperate more. Ruffle and Sosis (2007) conduct a one-shot public goods game with religious individuals in Israel. They find that the subjects have positive levels of cooperation, and that the more someone engages in religious activities, the greater the level of cooperation. Cardenas and Ostrom (2004) conduct a Common Pool Resource experiment with rural villagers of Colombia. They find positive levels of cooperation. As expected, individuals become less cooperative if they have experience with the game, and if they are less familiar with their group members. Fehr and Leibbrandt (2008) conduct a Public Goods game with Brazilian fishermen. Fehr and Leibbrandt compare the behavior of the fishermen in the laboratory to the mesh sizes of the fishing nets they use when they catch fish in their daily lives. Using nets with larger mesh sizes is interpreted as evidence for cooperation, because such nets cannot catch small fish. The authors find a positive correlation; those who cooperate in the laboratory are also more likely to cooperate in the field.

Two natural field experiments on social dilemmas deserve extra attention. The first is the study by Erev et al. (1993). The authors conduct an experiment at a fruit picking farm under three conditions. In the first condition, students are hired to pick oranges. The revenues that they make depend on the number of oranges they pick themselves. In the second condition, students have to pick oranges in teams. The team production has some features of a public goods game; all the revenues that the group make are shared equally. Finally, a treatment is conducted in which students are placed in teams where revenues are shared equally. A bonus is rewarded to the team with the greatest output. The results show that cooperation among students is possible; team production is greater when a bonus is provided. Teams in the bonus condition pick on average more oranges than students in the individual treatment. A second natural field experiment is conducted by Bandiera et al. (2005). They monitor fruit pickers under different circumstances. In one treatment, the earnings of each worker depend on own productivity only. They compare the results to a treatment where each worker's earnings are proportional to the total output. Therefore, a worker who picks more than an average worker imposes a negative externality on others. The authors find that workers internalize their externality by working less hard in the second treatment. Pure altruism is ruled out, because the finding disappears when the workers cannot be monitored by colleagues.

In Chapter 5 of this thesis, the external validity of the Public Goods game is explicitly tested by conducting a framed field experiment. External validity of the Public Goods game is tested by building the bridge proposed by Harrison and List (2004). The setting of the field experiment is a privately owned recreational fishing facility, called 'De Biestse Oevers'. At this fishing facility, regular costumers can pay a fixed amount to fish for four hours at rainbow trout. A convenient feature of this trout is that it is a hunting fis which actively pursues bait. Hence, a fisherman can catch more fish by exerting more effort; the process of constantly casting and reeling in bait. The properties of the rainbow trout make 'De Biestse Oevers' an ideal setting to conduct experiments. Not only can output be monitored, but also the effort levels that fishermen exert.

In the field experiment described in Chapter 5, fishermen are placed in groups of four. Each fisherman is allowed to catch up to two fish in each of the six periods. All fish caught are for the fishermen to take home, but a social dilemma is created by paying each other group member for each fish that a fisherman foregoes catching. This treatment is compared to a treatment where no incentives are provided to reduce the catch of fish. The results are very much in line with what conventional economic theory predicts: Fishermen fish with the same intensity and catch similar amounts of fish in the private incentive treatment as they do in the public goods treatment. The analysis continues by exploring the differences with behavior usually observed in Public Goods games in the lab; cooperation in early periods which diminishes as the periods go by. The field experiment is translated to a laboratory game, and played with students and a sample of the pool of fishermen. Finally, the laboratory game is played outside the traditional lab; at the recreational fishing site itself. The results show that behavior in all laboratory treatments is in accordance with the two stylized facts. Therefore, neither the subject pool, nor the physical environment cause the difference in behavior with the field setting. An additional field experiment is conducted which represents a dynamic version of the Common Pool Resource game. The advantage of this design is that the medium of reward is kept constant: Catching fish now results in less fish in the future, rather than less money for others. Again, we find no evidence of cooperation. The results show it is hard for cooperation to occur in situations which require effort of participants to free-ride, and where group members are anonymous and cannot communicate with each other.

1.5 Informal institutions in the field

To the best of my knowledge, no controlled field experiment on social dilemmas has ever been conducted that explicitly tests the effectiveness of informal instruments. The most popular and effective instrument that promotes cooperation in the laboratory is monetary punishment. Given the absence of cooperation in the field experiment of Chapter 5, the setting used there provides an extreme case in which to test monetary punishment. Two experiments on monetary punishment are presented in Chapter 6. The first experiment adds a monetary punishment stage to the Public Goods game conducted in Chapter 5. The experiment is divided into two parts, the first two periods are the baseline game with no punishment, followed by four periods with punishment opportunities. Secondly, monetary punishment is added to the dynamic version of the Common Pool Resource game. Subject play three periods of either a baseline game, or three periods of the same game with punishment. After each period, subjects receive feedback on the catch and earnings of each fellow group member. Then, they each receive an endowment of three euros, added to their earnings. The subjects are allowed to spend those three euros; each euro spent, reduces the earnings of a fellow group member with three euro. The results show that punishment has no effect on cooperation. Fishermen fish with the same intensity and catch similar amounts of fish, irrespective of punishment opportunities. Strikingly, almost no use of the punishment instrument is made. The data suggest that fishermen are averse to using monetary punishment. The effect that monetary punishment has in conventional laboratory experiments does not carry over to our field experiment. This means that there are situations where monetary punishment alone does not have the desired effects on cooperation.

Although monetary punishment is the natural candidate in the lab, it might seem strange in the field setting of Chapter 5 and 6. The media of reward in those experiments are fishing time, and money. For this reason, the effects of punishment and reward are further explored in Chapter 7. Rather than using money as a punishment mechanism, fishing time is the medium of exchange. In this chapter, a variant of the Public Goods game of Chapter 5 and 6 is presented. The experiment consists of two parts. In part 2, fishermen can fish unconstrained for up to 150 minutes. Each fish they catch is theirs to take home, and a bonus of two euros per fish is given in addition. After each half hour, the stock of fish is replenished to give the fishermen the opportunity to catch as much as possible. Part 1 consists of three periods of thirty minutes each. Fishermen are placed in groups of four subjects and each fisherman is allowed to catch up to two fish. Each fish that a fisherman catches is his to take home, but the consequence of each fish caught is that the other three group members face a ten minute reduction of fishing time in part 2. The results of this treatment are very similar to the results presented in Chapter 5 and 6; fishermen try to catch as much fish as possible, there are no signs of cooperation. In two additional treatments, the effects of punishment and reward are tested. At the end of each of the three periods in part 1, subjects receive information about the catch of each group member. Then, each subject is allowed to reduce his own fishing time in part 2 by up to three intervals of five minutes. In the punishment treatment, each interval used reduces the fishing time of a targeted group member with fifteen minutes. An increase of fifteen minutes can be provided to group members in the reward treatment. Note that a 1:3 ratio is used in both treatments. Experimental evidence from the laboratory shows that this ratio should be sufficient to establish an increase in cooperation. However, the results show no evidence at all of an increase in cooperation. Fishermen fish with the same intensity as they do in the baseline treatment with no informal institutions. Also when a different medium of reward is used, punishment and reward have no effect on cooperation in our field setting.

In Chapter 7, a closer look at the use of rewards and punishment is provided. Rewards are used more often, but only punishment is used in an intuitive way; those who catch more fish are punished more often. For rewards, this is not the case, there is no correlation between catch an rewards received. The use of punishment suggests that subjects do not use it hoping to change behavior of fellow group members. Rather, punishment seems to be used in order to vent some frustrations by the victims of free-riders. Research in the field of neuroeconomics shows that punishment in itself gives pleasure to subjects, because it feels nice to take revenge (see for example de Quervain et al. (2004), Singer et al. (2006) and Fehr and Camerer (2007)).

The final chapter of this thesis provides a short conclusion. The lessons learned are summarized, and some policy implications are presented as well as lessons to be learned for the scientific community. Finally, some attention is given to where future research is needed.

Chapter 2

Cooperation and Evolutionary Approaches

2.1 Introduction

One of the big puzzles in understanding today's society is the observation that many social systems are developed around, and built on cooperation among individuals.[1] For cooperation to exist, individuals have to engage in altruistic actions. Altruism is defined as 'behavior by an individual that increases the fitness of another individual while decreasing the fitness of the actor' (Bell 2008, p.367–368). The overwhelming evidence of cooperation in today's society seems to be at odds with the traditional view of natural selection: Only the strong who maximize their own fitness survive and reproduce. This interpretation of natural selection leaves no room for altruism and cooperation.

In this chapter, I will present a summary of three important mechanisms of the existence of altruism: kin selection, direct reciprocity and indirect reciprocity.[2] Kin selection theories are based on the notion that an altruistic act to someone genetically closely related yields indirect survival advantages. A sacrifice for family members increases the degree to which they are able to reproduce.

[1] *Science* Magazine ranked the question 'How did cooperative behavior evolve?' sixteenth in the top 25 questions that science faces the next twenty years (Pennisi (2005a)).

[2] Other mechanisms of the existence of altruism are explored in the literature as well. Examples are network reciprocity (see for example Lieberman et al. (2005) and Durrett and Levin (1996)), group selection (see for example Wilson (1975) and Wilson and Sober (1994)), 'green beard' models (see for example Riolo et al. (2001) and Jansen and van Baalen (2006)), and voluntary participation to the game (see for example Hauert et al. (2002b) and Hauert et al. (2002a)). Although each mechanism provides interesting insights on the emergence of altruism, they are far removed from the chapters that follow. The interested reader is referred to Nowak (2006).

In this way, the genes of the altruist are passed on to the next generation in an indirect way. Direct reciprocity works on the mechanism that if I help you now, you can help me in the future. If we can credibly commit to a promise to help in the future, then both of us will be better off in the long run. Indirect reciprocity occurs when an altruistic favor is not necessarily returned to the actor by the recipient, but by someone else. Reputation is the key to the evolution of altruism: Only those who help others are helped in return. Besides these three mechanisms of the evolution of altruism, altruistic punishment is considered. Altruistic punishment is not a mechanism, but an instrument that can empower agents to help others. Punishment is especially effective in group settings where one's actions reflect on unrelated group members, both directly and indirectly.

Before discussing the mechanisms of evolution, the dominant model showing why people might not want to engage in altruistic actions is discussed briefly. This model is known as the Prisoner's Dilemma game. Its elegant form has been used widely by economists, psychologists, and evolutionary biologists to model cooperation among humans. This model forms the basis underlying the three mechanisms addressed in this chapter. The Prisoner's Dilemma receives extra attention, because it is the underlying model behind some of the chapters that follow. In those chapters, I will present results on economic experiments which are designed to test cooperation of individuals.

2.2 The Prisoner's Dilemma: A model of a world without altruism

In the economics discipline, theoretical models are built around a specific actor: rational economic man.[3] The most important aspect of this actor is that he behaves 'rationally'. Rationality refers to the ability to make optimal choices. That means that rational economic man has the power to maximize his own wellbeing at minimum costs, given the information available. One advantage of assuming that agents are rational is that it makes theoretical models tractable and solvable. Another advantage is that the rationality assumption is closely related to the argument of natural selection: Only the strong and selfish shall survive. In the game theory literature, the rationality assumption is often interpreted as meaning that rational economic man is selfish. A rational agent

[3] The term economic man was used for the first time by Ingram (1888) to comment on earlier work by John Stuart Mill (1836). See Persky (1995) for an overview of the history of the term economic man.

only cares about his own wellbeing. However, including preferences for others into the wellbeing of a rational actor does not violate the assumption of an agent maximizing his utility. One can model the wellbeing of others into the wellbeing function of rational economic man. For the remainder of this chapter, I will use rationality as if it implies selfishness, as is most frequently done in the economics literature.

A powerful theoretical demonstration of this mechanism is provided by an influential paradigm called the Prisoner's Dilemma. It describes a game where individuals can choose to help each other, but helping comes at a cost.[4] In this game, two players are confronted with a dilemma: they simultaneously have to make a choice to either 'cooperate' or 'defect'. In its simplest form, choosing to cooperate comes at an individual cost c, but there are no personal benefits. Cooperation does give the other player a benefit b. Note that this is precisely like the definition of an altruistic action stated earlier. It is assumed that the recipient's benefits are greater than the costs made by the decision maker, so $b > c$. Therefore, if both players cooperate, each player earns a profit of $b-c > 0$. If both players defect, each player earns a profit of 0. The dilemma of this game is made apparent by the payoffs resulting from one player who defects, while the other cooperates. In this case, the cooperating player has a profit of $-c$, while the defecting player has a profit of b. This dilemma becomes a 'social dilemma' when the payoffs of two cooperators are greater than the payoffs of a cooperator and a defector; defection then results in a profit for the defector, but a loss to the population as a whole. Table 2.1 below presents the payoff matrix which summarizes the game. Only the payoffs for Player 1 are presented.

		Player 1	
		Defect	Cooperate
Player 2	Defect	0	$-c$
	Cooperate	b	$b-c$

Table 2.1 Payoffs for Player 1 in the Prisoner's Dilemma.

Assuming rationality, the predictions of this game are straightforward. For the moment, take as given that Player 2 commits to cooperate. In this case, it is in Player 1's best interest to defect, because the personal payoffs of defecting (b) are greater than the payoffs of cooperating ($b - c$). Now, let's assume that

[4]The structure of this simple game has been developed by Merill Flood and Melvin Dresher. Later, Albert Tucker converted the game to a situation in which two prisoners simultaneously were given the option to reduce their sentence at the cost of increasing the sentence of the other. See Poundstone (1992) for more details on the origin of this game.

Player 2 commits to defect. Also in this case Player 1 is better off to defect and have profits of 0, rather than to cooperate and have profits of $-c$. Hence, independent of the action of Player 2, Player 1 when defecting is always better off. Because the game is symmetric in payoffs to both players, Player 2 applies the same logic and chooses to defect no matter what Player 1 chooses. Although both players always defect, they both would be better off cooperating. The sum of payoffs when both defect is 0, while the sum of payoffs when both cooperate is $2 \times (b - c) > 0$.

For the game above, it can be shown that defecting in the Prisoner's Dilemma is an 'evolutionarily stable strategy' (ESS) in a population of agents who either always defect, or always cooperate. An ESS is a strategy which, if most agents in the population adopt it, no other strategy can yield greater payoffs (Smith and Price (1973)). To see that defecting is an ESS in the game above, consider the fitness of each strategy in a population with a fraction of p cooperators and $(1 - p)$ defectors (parts of the analysis are due to McElreath and Boyd (2007)). It is assumed that agents interact randomly in this population. The fitness of an agent who always cooperates is given by:

$$\begin{aligned} U(C) &= u_0 + p(b - c) + (1 - p) \cdot (-c) \\ &= u_0 + pb - c. \end{aligned} \tag{2.1}$$

Similarly, the fitness of a defector is given by:

$$\begin{aligned} U(D) &= u_0 + pb + (1 - p) \cdot 0 \\ &= u_0 + pb. \end{aligned} \tag{2.2}$$

Therefore, for any given level of cooperators, the fitness of a defector is always bigger. To model the evolution of frequencies of strategies in a population, usually the replicator equation is used (see Taylor and Jonker (1978)). The intuition behind the replicator equation is that natural selection favors those strategies which have a greater than average payoff. A general form of the replicator equation is given as follows:

$$\dot{p}_i = p_i[U_i(p) - \bar{U}(p)], \qquad \bar{U} = \sum_{i=1}^{N} p_i U_i(p),$$

where p_i is the fraction of agent's using strategy i in a population with N strategies. In the example above, the payoffs to defectors are greater than the payoffs to cooperators. Hence, defectors must have a bigger payoff than the average payoff, which eventually leads to the extinction of cooperators in the population.

The Prisoner's Dilemma yields stark predictions on how humans behave in a situation where altruism makes everyone better off. However, the problem is that there is much real world evidence that humans show altruistic behavior. Examples that humans are altruistic and protect the environment are overwhelming (see for example Ostrom (1990), Somanathan (1991), and Baland and Platteau (1996)), and studies with laboratory experiments provide a large body of evidence that humans are willing to forego profits to help others. Experiments with the two-player Prisoner's Dilemma show that humans do choose to cooperate (see for example Rapoport and Dale (1967), Andreoni and Miller (1993), and Tversky (2004)). One explanation that is offered as to why humans might want to show altruistic behavior is because humans care about those they are closely connected to. This idea is better known as kin selection.

2.3 Kin selection: Cooperation among related agents

The model described above shows that in a population of pure cooperators and pure defectors, random interaction between the two types causes defectors to have greater levels of fitness. Therefore, defectors will invade a population of cooperators; greater levels of fitness cause the defectors to produce more offspring. But how about the situation when cooperators interact with and care about their relatives? If a cooperator is not able to produce more offspring in a direct way, perhaps this is possible indirectly. When asked if he would sacrifice his life for a brother, John Haldane famously replied: 'No, but I would to save two brothers or eight cousins.' Haldane (1932, 1955) noted that an altruistic act could cause one's brother to produce more offspring, thereby indirectly passing genes along. For genes to pass on to the next generation, one could sacrifice his life for a brother, if this brother has two or more offspring. One could sacrifice his life for a nephew, if this nephew will have eight or more offspring.

With the previous analogy in mind, a population of cooperators can prosper when interaction between agents is not random, but conditional on type (this analysis is based on McElreath and Boyd (2007)). For this purpose, rewrite equation (2.1) and (2.2) as follows:

$$\begin{aligned} U(C) &= u_0 + \Pr[C|C](b-c) + \Pr[D|C] \cdot -c, && (2.3)\\ &= u_0 + \Pr[C|C]b - c, \\ U(D) &= u_0 + \Pr[C|D]b + \Pr[D|D] \cdot 0, && (2.4)\\ &= u_0 + \Pr[C|D]b, \end{aligned}$$

where $\Pr[C|D]$ represents the probability that someone is paired with a cooperator, given that he himself is a defector. Equation (2.3) can be rewritten by using that $\Pr[C|C] + \Pr[D|C] = 1$. In case cooperators gain a larger share in the population over time, the return to cooperation has to be greater than the returns to defecting. This is the case when:

$$\Pr[C|C]b - c > \Pr[C|D]b, \text{ or}$$
$$(\Pr[C|C] - \Pr[C|D])b > c. \tag{2.5}$$

The link to Haldane's observation can be seen by interpreting the conditional probabilities in terms of relatedness. Rather than interacting randomly, relatedness influences the probability that two agents meet. This is modeled by incorporating the variable r into the conditional probabilities. The variable r is a number between zero and one, a fraction that represents the probability that two agents share the same strategy because they stem from common descent. In terms of genes, r is one for full twins, and is one-half for full-siblings. For half-siblings r is one-fourth, and for cousins it is one-eighth. The probability that a cooperator meets another cooperator is given by $\Pr[C|C] = r + (1 - r)p$. The first term of the right hand side is the probability that two cooperators have the same strategy because they stem from the same parents. The second term presents the probability that two unrelated cooperators have the same strategy. In a large population, it is likely that some individuals happen to have the same strategy. The other conditional probabilities are given as follows:

$$\begin{aligned}\Pr[D|C] &= (1 - r)(1 - p),\\ \Pr[D|D] &= r + (1 - r)(1 - p),\\ \Pr[C|D] &= (1 - r)p.\end{aligned}$$

By substituting these probabilities into equation (2.5), we arrive at the following condition for cooperation to be a successful strategy:

$$rb > c. \tag{2.6}$$

This last equation is famously known as Hamilton's rule and was first established by William Hamilton (1964a, 1964b). The interpretation of Hamilton's rule is that altruism can sustain when the benefits of an altruistic act, discounted by the fraction of genes shared between the agents, are greater than the costs of the act.

Kin selection provides powerful insights into patterns of altruism for insects (for example, see Foster et al. (2005)). Examples can be found in the case of ants (see for example Hölldobler and Wilson (1990)), or bees (see for example

Seely (1995)). When it comes to the human species, the theory has less succes in explaining real world behavior. One form of criticism is due to Sherwood Washburn (1978). It is known that humans share many alleles, and that these alleles don't change much over time.[5] This means that the relatedness among humans is very high, and that altruism should be observed in an overwhelming amount of instances. However, although altruism does occur in real life, it is less frequent than predicted by kin selection. Support for this claim is provided by Segal and Hershberger (1999), who conduct a laboratory experiment on the Prisoner's Dilemma game. As subjects, they use monozygotic and dizygotic twin pairs. In line with what kin selection predicts, more cooperation is found between monozygotic twins than between dizygotic twins. However, in contrast to kin selection theory, for both types of twins it is found that more defection has occurred than cooperation. Another study on the effects of relatedness is done by Charness and Gneezy (2008). They conduct a laboratory experiment on the dictator game, first conducted by Kahneman et al. (1986). In the dictator game, a subject receives an endowment of money. The subject can then decide how much to send to an anonymous recipient. In a way, this game is a stripped down version of the Prisoner's Dilemma game; only one player makes a decision to 'cooperate' or to 'defect'. A rational and selfish actor would never send any money to a recipient, but kin selection predicts that giving to related individuals might be possible. Two variants of the dictator game are played. In the first treatment, dictators are told that the possible recipient shares the same family name. The second treatment has no such information. Charness and Gneezy find that 42 percent of the dictators give at least half of their endowment to a recipient who shares the same family name. In contrast, in the treatment where this information is not provided, twenty percent of the subjects make such allocations. The results suggest that closer social distance does make dictators more altruistic. Note, however, that the subjects are not aware whom they are giving money to. Therefore, the data does not permit a stringent test of kin selection.

Another form of criticism is that kin selection does not explain all altruistic behavior observed real life situations (this holds for insects as well, see Wilson (2005)). Many acts of altruism in real world settings are among non-relatives. In these cases, the reward of an altruistic act in the form of passing on genes to the next generation are forgone.[6] Hence, understanding why strangers are

[5] *Science* Magazine ranked the question 'Why do humans have so few genes?' third in its top 25 challenges that science faces the next twenty years (Pennisi (2005b)).

[6] As noted by Trivers (1971), engaging in altruism because there is an indirect benefit of passing on genes is not in line with the strict definition of altruism. Pure altruism holds no

altruistic needs to go beyond explanations of being related. One explanation of altruism lies in the prospects of future interactions between agents: If I help you now, you can help me in some future time. The ideas around this principle are referred to as direct reciprocity.

2.4 Direct reciprocity: Returning favors as a mechanism of altruism

The analysis of the previous section shows that kin selection can help to sustain altruism in a population characterized by fixed strategies. One aspect of the analysis is that interaction between agents is one-shot and that relatedness influences the probability of an interaction. In real life situations however, it is likely that the same individuals meet more than once, while these individuals are not related to each other. An altruistic act by one individual towards another can be profitable if that favor is returned at some later date. This idea, known as direct reciprocity, has first been posed by Robert Trivers (1971). One important condition is that the benefits of an altruistic act to the receiver are greater than its costs for the actor. If this is the case, the net benefits of the two individuals will be positive and both will be better off helping each other.

The notion that it pays to engage in direct reciprocity has led scholars to search for strategies that maximize payoffs in a sequence of Prisoner's Dilemma games. One of the earliest ideas on such a strategy is due to the folk theorem: If a game is played repeatedly infinitely often, and the players do not discount future periods too much, an outcome can be reached in which all players have optimal payoffs. One way in which such an outcome can be reached is when all players play the grim trigger strategy. This strategy holds that players cooperate with each other until one player defects. From the period that one player defects, all players punish all other players by defecting for the remainder of the game.[7]

The disadvantage of the grim trigger strategy is that it is unforgiving: Cooperation cannot be restored after a defection. A more forgiving strategy could possibly attain greater profits. In the late seventies, Robert Axelrod organized a computer tournament where contestants could write a program which indicated a strategy of play in the Prisoner's Dilemma. In total, a program was played for a sequence of two hundred periods; each program played five periods against all

benefits at all for the actor, only benefits for the receiver.

[7]For a proof of the folk theorem, see Friedman (1971) and Rubinstein (1979). See Fudenberg and Maskin (1986) for a proof in the context of the Prisoner's Dilemma.

other programs. The strategy that attained the greatest average payoff was the so called Tit-For-Tat strategy (TFT), proposed by Anatol Rapoport (Axelrod and Hamilton (1981), Axelrod (1984)). This strategy holds that a player begins with cooperating and mimics the move of the other opponent in the next period. If the opponent has previously cooperated, TFT directs to cooperate, and vise versa. One of the successes of this strategy is that it defends against defectors by punishing uncooperative behavior. In addition, the strategy is successful in cooperating with cooperators. Especially when both players play the TFT strategy, cooperation will be maintained throughout the whole game. An important feature of the strategy is that defection by one player can be restored. All that has to happen is for that player to cooperate at some time in the future. However, this may be problematic because usually a player does not cooperate against an opponent who has defected in the previous period. In case such a defection is made unintended, for example due to an error by one of the players, the TFT strategy leads to suboptimal payoffs (Fudenberg and Maskin (1990)).

New strategies which overcome the problem of an erroneous defection have been developed. One of those is called Tit-For-Two-Tats (TFTT) (see Boyd and Lorberbaum (1987)). This strategy holds that defection is chosen when an opponent defects two consecutive times. This strategy outperforms TFT when occasional one-time defections are made. Another proposed strategy is Generous TFT (GTFT). Whenever the opponent has defected in the previous period, the GTFT strategy chooses to cooperate with a certain probability. Nowak and Sigmund (1992) show that a population of TFT strategies can be invaded by GTFT when there is a one percent probability that an agent mistakenly defects after having observed cooperative play by the opponent.

Finally, a strategy called Pavlov, or 'Win-Stay, Lose-Shift' is proposed (see Kraines and Kraines (1989, 1995, 2000), and Nowak and Sigmund (1993)). This strategy is to cooperate if both players have cooperated, or if both players have defected in the previous period. When in the previous period the player cooperates while the opponent defects, the player defects in the next period. Likewise, when in the previous period the player defects while the opponent cooperates, the player defects in the next period. An important feature of this strategy is that cooperation is easily restored when two Pavlov players meet. In case one player mistakenly defects, the other player will defect in the next period. The two defecting players will then switch back to cooperation in the following period. The Pavlov strategy resembles the 'carrot-stick' mechanism in bringing up children (Nowak and May (1995)). Good behavior remains in place, while bad behavior leads the child to reconsider and change its actions. Note that like the TFT strategy, TFTT and GTFT cannot invade a population of pure cooperators (Nowak and Sigmund (1989)). This is not true for the Pavlov strategy; a pure cooperator will be exploited because no switch will be made to cooperation. In contrast, Pavlov players do not fare well against pure defectors, because Pavlov players try to restore cooperation in every other period (Nowak and May (1995)).

To summarize, direct reciprocity explains why altruism can evolve. When altruistic actions are returned in kind on some future date, even cooperation among non-related individuals can be established. Many phenomena in real world settings can be described by acts of direct reciprocity. One could think of giving tips in restaurants, friends taking turns to babysit each other's children, carpooling, and the like. From experiments conducted in labs an overwhelming amount of evidence shows that individuals are willing to engage in direct reciprocity. For example, in the ultimatum game (Güth et al. (1982) and Roth (1995)), a proposer is asked to share an amount of money with a responder. The responder then has the power to veto the proposal, in which case neither of the two subjects receive any money. Typically, the responder offers an amount between twenty and fifty percent, while the responder rejects offers below twenty percent. In the trust game (Berg et al. (1995)), a proposer has to choose how much money of a given endowment to send to an anonymous responder. The experimenter triples the money, and gives the responder the possibility to send money back. Assuming rational agents, it is expected that no money is sent between two individuals. The typical finding is that the proposer sends around fifty percent of his endowment, and that the responder sends about fifty percent of the tripled amount back. Finally, in the gift exchange game (Fehr et al. (1993)), a proposer sends an amount of money to a responder, and the responder has the option of returning a costly favor. In this game, the general finding is that more money sent translates into more costly favors.

Although direct reciprocity is able to explain real world behavior which cannot be explained by kin selection, direct reciprocity does not cover all instances of altruism. In many real world situations, individuals engage in altruistic actions which cannot be directly returned. For example, giving to charity and volunteer services have in common that the actor makes a cost, while others reap the benefits. The ones who receive the benefits usually do not have a way to return the favor. Through an indirect way these altruistic actions can lead to a payoff for the actor. This principle is referred to as indirect reciprocity.

2.5 Indirect reciprocity: Reputation as a mechanism of altruism

The notion that altruism can be sustained through indirect reciprocity has first been put forward by Richard Alexander (1979, 1987). He notes that cooperation can be established, with reputation and status as driving factors. A necessity for this to occur is that humans constantly ‘assess and reassess each other’. Therefore, indirect reciprocity is closely related to the evolution of social norms.

In evolutionary game theory, indirect reciprocity is modeled as follows (see Nowak and Sigmund (2005), and Nowak (2006)). Two agents meet randomly, one in the role of possible donor and the other in the role of recipient. The donor can decide to make a costly transfer to the recipient. In doing so, the donor makes a cost of c, while the benefits to the recipient are given by b. As in the previous models, it is assumed that $b > c$, so the net effect of an altruistic action is positive. After the agents have met, each will be matched to a different agent in the population. Agents will never meet each other more than once to ensure indirect reciprocity: An altruistic act can never be returned in kind. In order to establish cooperation, a mechanism of reputation is needed. A reputation can be established by having agents, other than the decision maker, observe and judge the decision maker’s behavior. Basically, the mechanism of reputation forming concerns what is considered a good act, or what is considered a bad act. The way in which a reputation is built, depends on the level of sophistication of the population. Three levels are distinguished (Brandt and Sigmund (2004)).

First-order sophistication only looks at the decision of the decision maker. The decision is judged as good by bystanders when the decision maker provides the benefit, and it is judged as bad otherwise. In Boyd and Richerson’s (1989a) model, agents in a population are formed in a circle. Each agent has to make a decision to help the agent left of his. Different TFT-like strategies to cooperate are explored, depending on the actions of either the right neighbor or the left neighbor. The authors show that cooperation can be established only when unrealistic scenarios hold. For example, the number of agents in a circle must be small and the cycles of help have to be long lasting. However, Nowak and Sigmund (1998a, 1998b) drastically change the model of indirect reciprocity, and show that cooperation can be established under less stringent assumptions. In their influential model, agents are endowed with an ‘image score’ and interaction between agents is random. Whenever an agent decides to provide help, the score is increased by one. Refusing to provide help reduces the score by one. An agent’s strategy to donate is based on the image score of the recipient;

only those with at least a score of k are provided help. Nowak and Sigmund show that when all agents in the population can monitor each other, cooperation is stable. The strategy of giving help to recipients with at least an image score of zero invades all strategies in the population which direct to give to a recipient with an image score greater than zero. The same result holds when only a fraction of the population can observe each other, but the evolution of cooperation is slower in this case. Brandt and Sigmund (2004) show that when agents can form networks, first-order sophistication is enough for altruism to evolve. A network of an agent, that is, the number of acquaintances, grows each period and being in a network informs an agent of the status of a potential recipient. Discriminating agents can establish a cooperative equilibrium whenever the information available and the probability of a future period are big enough.

When it comes to first-order sophistication, not helping a defector lowers the status of the potential donor. For this reason, the evolutionary succes of an agent who discriminates against defectors may be in danger (Ferrière (1998)). But, it may be regarded as fair whenever someone refuses to help a recipient who has not helped others in the past. Second-order sophistication deals with this problem. Status is derived by considering the action of the decision maker, and by considering the status of the recipient. This idea has been proposed by Robert Sugden (1986), basing strategies on the notion of 'standing'. All players begin with good standing, but lose this reputation when help is refused to an agent with the standing status. The standing status can be gained back whenever help is provided to someone in good standing. Leimar and Hammerstein (2001) show that the standing strategy can result in the evolution of altruism. The standing strategy usually outperforms agents who base their decision on image scores alone. The reason is that when it comes to the standing strategy, it suffices to keep your own reputation just above the critical level of receiving help. Both image scorers and standing strategists will provide help to someone in good standing. However, agents who use the standing strategy can exploit the cooperative acts of image scorers who are always seeking for a better image.

Finally, third-order sophistication is explored. Assessing whether a decision maker's action is good is based on the act of the decision maker, the status of the recipient, and the status of the decision maker. Dealing with these three assessment points causes the number of strategies to become large in number. Ohtsuki and Iwasa (2004) evaluate in total 4,096 strategies, all based on one or more of the elements of third-order sophistication.[8] In their simulations, they

[8]Ohtsuki and Iwasa (2004) consider binary scores only. A strategy has two modules; an action module and an assessment module. The action module, deciding whether to give or not,

find that eight of them lead to a system in which altruism invades a population of agents. These so called 'leading eight' behavioral rules have two principles in common. The first principle is that helping altruists is considered good, while not helping them is considered bad. Second, defection against defectors is considered good. The standing strategy is classified as one of the leading eight, because it adheres to both rules. Image scoring does not address the second rule, and therefore it is not part of the leading eight rules to sustain cooperation. Brandt and Sigmund (2004) conduct a similar exercise. In their model, as opposed to Ohtsuki and Iwasa (2004), it is assumed that interactions are for a short time, rather than for an infinite time. Next to that, Brandt and Sigmund assume that the images derived from observing the act of the decision maker are private, whereas they are public in the model of Ohtsuki and Iwasa. The qualitative conclusions of both analyses are similar. Like Ohtsuki and Iwasa, Brandt and Sigmund conclude that agents directed by standing outperform agents directed by image scoring.

The theoretical prediction that indirect reciprocity leads to altruism has been tested with laboratory experiments. A widely used experiment is called the Helping game, with a setup very similar to the model of Nowak and Sigmund (1998b). Subjects are placed in a room and are matched in pairs of two in each period . The decision maker has to make a choice between cooperating at a cost c, while providing a benefit $b > c$. In case of a defection, both players earn zero. A considerable amount of laboratory evidence is gathered which supports indirect altruism as a mechanism for the evolution of cooperation. Information about donors' decisions are provided in the form of image scores.

Wedekind and Milinski (2000) and Wedekind and Braithwaite (2002) show that when students play the Helping game, the amount of money donated is correlated with the image score of the recipient. Milinski et al. (2002) provide similar evidence. In addition, they find that when decision makers contribute to charity (UNICEF), they tend to attract more rewards from others. All three studies provide the entire history of the recipient. Bolton et al. (2004) conduct the Helping game with varying amounts of information, mimicking first and second-order sophistication. When decision makers don't have any information about the recipient, a surprising amount of positive cooperation is observed.

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depends on the decision maker's own score (good or bad) and the recipients score. Since there are 4 combinations of the action module, there are $2^4 = 16$ action modules. The assessment module, deciding how to assess the action of both agents when they were decision makers in the previous period, depends on the score of the decision maker (two possibilities), the score of the recipient (two possibilities), and the action of the decision maker (two possibilities). Hence, there are eight combinations possible and therefore $2^8 = 256$ assessment rules. In total, the number of strategies becomes $16 \times 256 = 4,096$.

Information that resembles first-order sophistication leads decision makers to provide more help, but most help is provided when subjects have second-order information. Finally, Seinen and Schram (2006) find evidence of indirect reciprocity when long periods of play are considered and players have first-order information. They show that different norms evolve in different groups; what some groups interpret as a fair reputation is different from other groups. These studies are in line with the image score model of Nowak and Sigmund.

Evidence is mixed regarding the robustness of the image score model of Nowak and Sigmund against the standing strategy proposed by Leimar and Hammerstein (2001). Engelmann and Fischbacher (2009) vary the degree to which information about a decision maker is available to others. They show that when past actions of decision makers are made public, they cooperate about twice as much as when this information is held private. It can be concluded from this evidence that donors cooperate because of strategic concerns. Rather than trying to 'do the right thing', donors are looking for a return on their investment. Engelmann and Fischbacher find that 15% of the subjects act in a purely strategic way and are not reciprocal. Interestingly, these subjects have the greatest payoffs of all participants of the experiment. Evolution is likely to favor the subjects who play this strategy, rather than the agents who are reciprocal. Milinski et al. (2001) conduct the Helping game where they provide first-order information in one treatment, and second-order information in the other. In both treatments, subjects are equally averse to donate to players who have defected in their last period. This is in line with a strategy based on image scoring, but not with the standing strategy. The authors conclude that subjects have difficulties in dealing with the vast amount of second-order information. Therefore, the subjects return to the easier rule of image scoring.

Up to know, we have seen that kin-selection, direct reciprocity and indirect reciprocity can lead to the evolution of cooperation among humans. Although examples of the three approaches are found abundantly in real life situations, the fact that humans directly and frequently interact in group settings is not captured fully in these models. Kin-selection and direct reciprocity assumes that individuals meet in pairs of two. Indirect reciprocity comes closer to group interaction, but it leaves out the possibility of direct interaction between two agents in a group. In the next section, altruistic behavior in group settings is considered. It is shown that altruism can be sustained in groups when agents have the possibility to punish each other.

2.6 Altruistic punishment as an instrument to sustain cooperation

Many real world situations resemble a Prisoner's Dilemma game, with the exception that more than two players are involved. These are situations in which the actions of one individual have positive or negative consequences for a group. For instance, one could think of a fisherman catching a fish in a lake, a factory polluting the environment to produce output, or someone driving a car from home to work. Like in a two-player Prisoner's Dilemma, each individual facing an n-player Prisoner's Dilemma has an incentive to defect rather than to cooperate. Cooperation is hard to establish in such cases, because strategies like Tit-For-Tat have unwanted side effects. Punishing defectors by defecting in kind, has the undesirable property that those who don't deserve to be punished face the consequences as well. Yet, there are many examples of situations where humans who interact in groups repeatedly engage in cooperative behavior. Hardin (1968) mentioned that 'mutual coercion, mutually agreed upon' can ensure that humans act in the best interest of the group, rather than in their own best interest. Such mutual coercion is referred to as 'altruistic punishment'; behavior in which individuals punish others at a cost to themselves in order to provide a public good (see for example Fowler et al. (2005)).

Adding punishment to an n-player Prisoner's Dilemma changes the payoffs of the game. Consider a population of n agents, of which a fraction p cooperates, thereby giving benefits b to all other agents at a personal cost c. The benefits of a cooperator are given by $bp - c$, while the benefits of a defector are given by bp. After the actions of all agents have been observed by all, cooperating agents have the possibility to punish defectors. Punishment comes at a personal cost k, and reduces the payoffs of each defector by $x > k$. The payoffs to a cooperator in case of a punishment decision are given by $bp - c - k(1 - p)$, while the payoffs to a cooperator who does not punish are given by $bp - c$. The payoffs to defectors are given by $bp - xy$, where y is the number of cooperators who punish. In case the cost-benefit ratio of x and k is sufficient, punishing defectors can lead them to cooperate.

The game presented here faces two problems in sustaining cooperation (Sigmund (2007)). First, consider a population consisting of defectors, and cooperators who all must punish the defectors. In case the population consists of a few defectors and many punishers, defectors disappear from the population because of the tremendous amount of punishments they receive. However, when the population consists of a few punishers and many defectors, the punishers go extinct because they have to make too much punishment costs. Hence, a system of punishers cannot be invaded by defectors, but a system of defectors cannot be invaded by punishers either. Second, consider the case where cooperators can choose to punish. Payoffs to cooperators who do not punish are greater than payoffs to cooperators who punish. This so called 'second-order free-riding' problem causes cooperators to refrain from punishing, which causes all agents to defect when choosing to cooperate. A solution to the second-order free-riding problem is to give agents the possibility to punish those who have not punished defectors. However, this merely raises the question who will punish those who have failed to punish the non-punishers, and so on.

Boyd and Richerson (1992) show that cooperation can be established in a population of defectors, cooperators and punishers, provided that the costs of punishment are low and that the population is small. Cooperation can be established only under some unrealistic parameter conditions (for example, for certain cost-benefit ratios of punishment) When a population becomes too big, punishment becomes too expensive for punishers.

The result that punishment does not stabilize cooperation in large societies is quite unsatisfactory.[9] Large populations have more to gain from positive spill-overs, such as providing insurance or benefiting from the returns from schooling. For the punishment strategy to work in large populations, interactions between groups are needed. Boyd et al. (2003) argue that groups of agents can learn from each other and migration between groups can lead to the evolution of altruism. The intuition is that in populations where punishment is common, cooperators have a greater fitness level than defectors. Therefore, cooperation is especially likely to survive when cooperating agents of other populations migrate into a large population where punishment is common. Henrich and Boyd (2001) show that when agents randomly interact and copy strategies of agents from other groups, i-th order punishment can stabilize cooperation. For this to hold, it is assumed that strategies spread through a population because the most frequent strategies are copied, rather than the strategies that are most successful in terms of payoffs. In addition, if second-order free-riding is occurs because of rare mistakes, then the payoff differences between punishers and non-punishers is relatively small. The more stages of punishment added, the more unlikely it becomes that an agent mistakenly forgot to punish another agent who mistakenly forgot to punish. Hence, the more punishment stages, the smaller the payoff differences between punishers and i-th order free-riders. It becomes then

[9] Also in an n-player Prisoner's Dilemma where punishment is not allowed, cooperation is hard to sustain in large groups, see Boyd and Richerson (1989b).

more attractive to become a punisher, which makes it possible for cooperation to survive as a strategy.

Another way in which punishment can stabilize cooperation, is by implementing the probability of a natural disaster which wipes out an entire population. Gintis (2000) considers the repeated n-player Prisoner's Dilemma where each period there is a probability that the game ends. In such a setup, cooperation will emerge only when the probability that a new period emerges is sufficiently large. Add to the model the state of nature: a good state or a bad state. In the bad state, the probability that the game ends is larger than in a good state. When all agents defect, the game ends with certainty. This leads to a downward spiral. In a scenario where the probability of future interaction is small, defecting becomes an attractive strategy. More defection however, increases the probability that the game ends with certainty. Altruistic punishers can induce defectors in the bad state to switch to cooperation: When the costs of punishment are sufficiently small, defectors face a credible threat that their strategy makes them worse off. Self-interested agents respond by cooperating, which increases the probability of future interactions.

A more elaborate model on the interactions between cooperation and the collapse of a population is given by Sethi and Somanathan (1996). Rather than considering a simple n-player Prisoner's Dilemma, they consider the evolution of a natural renewable resource stock. Cooperation can be established in a population of defectors, punishers and cooperators but this is linked to the parameters of the model. When the revenues that the resource yields are greater, costs of harvesting are lower, or when costs of technology are lower, the gains from defecting are bigger.[10] Finally, Richter et al. (2008) consider the effects of nonmonetary punishment on the evolution of cooperation in a renewable resource. Agents harvest a resource and those who extract more face social consequences by means of signals of disapproval by others. These signals of disapproval can help to promote cooperation. However, when costs of disapproval are high, societies with a propensity to harvest more than is socially optimal cannot reverse this pattern and will deplete the resource.

The effects of punishment on cooperation in social dilemmas, like the n-player Prisoner's Dilemma, have been examined by numerous experiments. The main work horse is the so called Public Goods game (Marwell and Ames (1981)).

[10]For more on the effects of technology on harvesting levels, see Richter et al. (2009). In their model, they show that an initial increase in technology causes defectors to increase their harvest levels up to the point where they cannot exploit more due to a time constraint. A further increase in technology causes cooperators to increase their harvest levels as well, making payoff differences with defectors smaller. Through 'moral persuasion' by cooperators, defectors then start to decrease their harvest levels.

In this game, participants are placed in groups of usually four anonymous persons. Each person receives an endowment of tokens, which can be allocated to either a private or a public account. A token invested in the public account is multiplied by a number larger than one, and then evenly divided among all group members. For the investor, each token invested in the public account therefore yields one half token. A token invested in the private account yields a benefit of one token to the investor only. In a one-shot version of this game the dominant strategy is to invest all tokens in the private account, because the marginal benefits of each token are higher in that account. Usually, the experiment lasts for a number of periods ranging between one and fifty. The typical finding in this game is that subjects contribute between forty and sixty percent of their tokens to the public account in the first period. As the periods go by, contributions to the public good approach zero (Ledyard (1995)). This pattern can be explained by assuming that some agents in the population are purely selfish, while others are reciprocal altruists (Fehr and Fischbacher (2003)). Reciprocal altruists start by cooperating, but feel the urge to punish the selfish agents. The only way in which reciprocal altruists can do this, is by holding down on cooperation. A single selfish agent can therefore cause a breakdown of cooperation in an entire population.

Providing subjects with the possibility of punishment has dramatic effects on the establishment of cooperation. Yamagishi (1986) is the first to address the effects of punishment to the Public Goods game. After each subject has made a contribution to the public good, subjects can allocate tokens to another 'punishment' account. The tokens collected by this account are used to punish the group member who contributed the least to the public good. Selfish agents would never sacrifice their tokens to punish some else, hoping to free-ride on the efforts of others. However, a substantial amount of punishment is found by Yamagishi and punishment causes subjects to start to cooperate. Even when punishment is costly to perform, but merely acts as a signal of disapproval, the instrument causes subjects to become more cooperative in a public goods game (Masclet et al. (2003)). Ostrom et al. (1992) study the effects of punishment in the Common Pool Resource game, a non-linear version of the Public Goods game. The authors find that punishment causes subjects to harvest the resource less severe than the selfish equilibrium. In combination with communication, punishment causes almost all agents to harvest at socially optimal levels.

One reason for punishment to occur in the setup of Yamagishi (1986) and Ostrom et al. (1992) is that agents interact with each other for multiple periods. Strategic use of punishment can cause free-riders to reconsider their strategy and make them cooperative. However, punishment is effective even when subjects meet only once (Fehr and Gächter (2000, 2002)). Fehr and Gächter show that negative emotions play a crucial role in the decision process of subjects to engage in punishment. Economic principles play a role as well. Punishment is influenced by the cost-benefit ratio of the instrument, as well as the income of the punisher. Carpenter (2007) estimates a demand curve of punishment and find it is inelastic with respect to prices and income. The inelastic form of the demand function provides further evidence that social motives play a bigger role than economic motives. Although subjects use more punishment the cheaper it becomes, those with a greater income do not punish more than those with a low income.

Finally, some light is shed on the evolutionary stability of the sanctioning institution. This is shown in an experimental setup where subjects can choose each period whether they would like to have a sanctioning system present or not (Gürerk et al. (2006)). The authors find that although subjects are hesitant to favor punishment, after fifteen periods most of the subjects favor punishment, and levels of cooperation come close to the social optimum.

Chapter 3

On Rewards and Cooperation in Social Dilemmas: Carrots without Bite[1,2]

3.1 Introduction

Nowadays, the world is confronted with a variety of pressing environmental problems, including depletion of fisheries, tropical deforestation, and biodiversity loss. At the heart of these problems is the lack of sufficiently well-defined and enforced property rights which tend to result in overexploitation of the resource under consideration. The benefits of extracting an extra unit of the resource are private, whereas its costs (for example the increased scarcity because of lower levels of regeneration) are borne by all. Absent cooperation, each individual resource user ignores the costs she imposes on other resource users, and hence, from a social welfare point of view, puts too much effort into resource harvesting. This is observed to occur even if access to resources is limited to a specific group of individuals.

Over the past two decades, a substantial number of economic experiments have been conducted to assess the relative effectiveness of self-regulatory instruments in sustaining cooperation in multi-person social dilemma situations, such as linear public good games and non-linear common pool resource games. In-

[1]This chapter is co-authored with Daan van Soest and Jana Vyrastekova

[2]We would like to thank Chris Müris and David Voňka for their useful input.

struments tested in economic laboratory experiments include ostracism (Masclet (2003), Maier-Rigaud et al. (2010)), peer-to-peer rewards (Sefton et al. (2007), Vyrastekova and van Soest (2008), Rand et al. (2009)), and verbal expressions of approval or disapproval (Masclet et al. (2003)). Most attention, however, has been paid to the effectiveness of peer-to-peer punishments; see for example Yamagishi (1986), Ostrom et al. (1992), and Fehr and Gächter ((2000), (2002)). Offering subjects the opportunity to impose monetary sanctions on their peers significantly increases subjects' contributions to the public good, and this is even the case if punishments are not only costly to the punished, but also to the subject imposing them (Gächter et al. (2008)). To economists, these results are surprising because the experimental games are set up such that subjects should not be willing to provide the second-order public good of punishing free-riders in any of the periods, and hence efficiency in the social dilemma should be equally low with and without the opportunity to impose punishments.

The external validity (or real world relevance) of the experimental punishment mechanism results has been challenged on two grounds. The first is that punishments may not be used so eagerly if there is an opportunity for revenge. Nikiforakis (2008) conducted an experiment with two punishment stages (in addition to the social dilemma stage) rather than just one, so that subjects can use the second punishment stage to directly reciprocate to sanctions received in the first. The consequences for play in the social dilemma stage are quite dramatic. Faced with the threat of potential retaliation hardly any sanctions are imposed in the first punishment stage. The subjects correctly predict this happening and hence the efficiency in the social dilemma stage does not differ from the efficiency level that materializes absent any punishment stages (see also Denant-Boemont et al. (2007) and Nikiforakis and Engelmann (2008)). Hence, peer-to-peer punishments may be able to sustain cooperation in the real world, but only if punishers can hide their identity.

The second ground on the basis of which the real-world relevance of peer-to-peer punishments has been challenged is that in most societies, the use of force is the exclusive right of the government: Typically, individual citizens are not allowed to actually impose either physical or monetary punishments on their peers (Vyrastekova and van Soest (2008)). That means that peer-to-peer rewards may be empirically more relevant than peer-to-peer punishments, and a relatively small literature has emerged analyzing the effectiveness of rewards in sustaining cooperation (see for example, Sefton et al. (2007), Vyrastekova and van Soest (2008), Sutter et al. (2010) and Rand et al. (2009)). When using the same design features as the standard punishment experiment,[3] rewards are observed to increase cooperation in the social dilemma stage if and only if the benefits of receiving a reward are larger than the costs of giving it,[4] but less so than the punishment mechanism.

This chapter contributes to the experimental economics literature on the (relative) effectiveness of the reward mechanism in sustaining cooperation in multi-person social dilemmas by exploring to what extent offering subjects the opportunity to counter-reward increases or decreases the mechanism's effectiveness. While in the real world individual agents have incentives to hide their identity in case they punish another agent in a social dilemma, the opposite holds in case of rewards; the benefactor usually has good reasons to reveal her identity to the recipient. Also, in most real-world social dilemmas agents are likely to be well aware of the history of (at least a subset of) their fellow agents' behavior in the social dilemma as well as of the history of whom they received 'rewards' (in the form of gifts, or help). So when we conduct a social dilemma experiment in which we allow subjects to directly reciprocate not just to other subjects' behavior in the social dilemma but also to rewards received in the past and present, does this increase efficiency in the social dilemma even more?

While it may seem obvious that the answer to this question should be affirmative, we argue that the mechanism may not be so straightforward. If subjects systematically reward those who act cooperatively in the social dilemma stage, the returns of doing that can be twice as high than if there is just one single reward stage. But because the costs of sending a reward token are smaller than the benefits of receiving one, exchanging reward tokens is a profitable enterprize by itself. One may wonder to what extent subjects are willing to potentially jeopardize a mutually profitable (bilateral) exchange of reward tokens by withholding rewards if another agent decides to act less cooperatively in the social dilemma. There is a danger that the other agent may decide to stop sending reward tokens in response.

If subjects do not view the decrease in the number of reward tokens received as a just punishment for their acting less cooperative in the social dilemma

[3] By standard design, we mean a repeated public goods game with one reward stage after every period. The group composition remains the same, but the subject's identity labels change between periods.

[4] While this condition seems restrictive, it likely to be met in a large variety of situations. Note that the term 'reward' is typically thought to refer to a gift given (money, an object, or time) that increases the recipient's welfare. Because agents' marginal valuation of objects, time (for example, like assisting a community member in getting his harvest off the land) and even money can differ, the recipient's valuation of the 'reward' may well be higher than the provisioning cost incurred by the benefactor; see for example Vyrastekova and van Soest (2008).

stage, they may retaliate by withholding rewards too. In that sense, withholding rewards can be viewed as a second-order public good, and the question is whether or not subjects are willing to provide it.

In this chapter we analyze the behavior of subjects in a finitely repeated game in which, in every period, subjects first decide on their investments in a standard non-linear Common Pool Resource game, after which there are two stages in which they can send reward tokens to one or more of their fellow group members. Regarding the way in which groups are formed, we follow the literature in keeping group composition constant within and between periods, but we deviate from standard procedure by also keeping each subject's identity label unaltered both within and between periods. That means that, consistent with many real world instances, the design allows each subject to base her reward decisions not only on her fellow group members' behavior in the multi-person social dilemma in the current period, but also on the number of reward tokens she received from them in the current as well as in previous periods. We use the so-termed Partner Fixed (PF) matching, where Partner refers to the fact that group composition is constant throughout the experiment, while Fixed refers to the fact that each subject receives a unique identity label that remains constant throughout the experiment too.

The results regarding the effectiveness of rewards in sustaining cooperation are surprising. While we find that the average number of reward tokens sent by each subject is close to the maximum level in both reward stages in all periods but the last, efficiency in the non-linear Common Pool Resource game is low, even lower than predicted by standard game theory. Indeed, we find that subjects establish relationships with one another in which each partner systematically sends reward tokens to the other. These mutually profitable exchange relationships are formed early on in the experiment and are long-lasting. We also find that the establishment of these connections is largely independent of the partners' behavior in the social dilemma in the early periods of the experiment.

Based on this data, we hypothesize that subjects view the use of reward tokens as an alternative way of making money, rather than as a means to affect their peers' behavior in the social dilemma, and conjecture that rewards alone are unlikely to be able to sustain cooperation in real-world multi-person social dilemmas. To gain further insight in the underlying mechanism, we run the same experimental game using two alternative matching protocols. The first is Stranger matching, where group composition changes between periods (but not within periods), and the second is Partner Random matching, in which group composition remains fixed throughout the experiment but the subjects' identity labels are randomly reshuffled between periods. Contrary to Partner Fixed (PF) matching, Partner Random (PR) and Stranger (S) matching are not conducive to establishing multi-period bilateral exchange relationships, if rewards are given at all. As subjects are allocated randomly to sessions using one of the three matching protocols, we can infer whether the use of reward tokens is predominantly driven by altruistic considerations (as these are the only reason to send reward tokens in the Stranger sessions), by both altruistic and conditionally cooperative motivations (as they may both play a role in the PR sessions), or whether strategic money-maximizing reasons play a role too (as all three motivations can play a role in the PF sessions).

The evolution of play in the social dilemma stage in the Stranger and Partner Random sessions as well as the associated pattern of use of reward tokens in both session types strengthen our conclusion from the Partner Fixed sessions that rewards are used predominantly as a means to make money via bilateral exchange. Systematically using a social dilemma experiment without the option to reward as a baseline, we find that having two reward stages does not result in an increase in efficiency in the social dilemma stage in the Stranger matching protocol. Moreover, if rewards are given, many subjects spread them equally over all other members of their group and hence 'rewarding' is independent of their peers' behavior in the social dilemma stage of the same period. In the Partner Random sessions, subjects actually find a way to establish long-run bilateral exchange relationships despite the reshuffling of subject identifiers between periods: They use their decision in the social dilemma stage to signal their identity to the rest of the group. Therefore, we conclude that rewards are not likely to be effective in sustaining cooperation in multi-person social dilemma situations in the real world. The reason is that subjects systematically prefer establishing two-person relationships of cooperation to providing the second-order public good of using the reward stages to sustain cooperation in the multi-person social dilemma.

This chapter is organized as follows. In section 3.2 we present the two main treatments of this experiment as well as three matching protocols implemented. In section 3.3 we present the data for the Partner Fixed sessions as this matching protocol is empirically the most relevant one. In section 3.4 we present the results of the other two matching protocols, Partner Random and Stranger, as they provide additional support for our claim that rewards are not likely to be effective in sustaining cooperation in real-world social dilemmas. Section 3.5 provides a discussion of the results, and section 3.6 provides some conclusions.

3.2 The game and its experimental design

In this section we present the experimental design. Section 3.2.1 contains the model, and section 3.2.2 describes the experimental procedure.

3.2.1 The game

In line with the game developed by Ostrom et al. (1992), we implement a non-linear Common Pool Resource game with $N > 1$ identical players. The game is repeated $T \geq 1$ times, and in every period $t = 1, \ldots, T$ each player $i \in \{1, 2, \ldots, N\}$ can allocate a fixed amount of 'effort', e, between a social dilemma activity and an alternative economic activity, the outside option. The amount of effort player i puts into the social dilemma activity in period t is denoted by $x_{i,t}$ and is from the set of integers $\{0, 1, \ldots, e\}$. The marginal return on the amount of effort allocated to the outside option, $e - x_{i,t} \geq 0$, is constant and equal to w. The private marginal benefits of effort allocated to the social dilemma activity are equal to $A - BX_t$, where $X_t = \sum_{i=1}^{N} x_{i,t}$ is the aggregate amount of effort put into the social dilemma activity by all N players in period t. The baseline game consists of one stage only, the social dilemma stage, which we will refer to with superscript $s1$. Player i's total payoffs in stage $s1$ of period t are thus equal to:

$$\pi_{i,t}^{s1} = w(e - x_{i,t}) + [A - BX_t]\, x_{i,t}. \tag{3.1}$$

Because $\partial \pi_{i,t}^{s1} / \partial x_{j,t} < 0$ for all $j \neq i$, this game is a (non-linear) public bads game. If $T = 1$, the symmetric individual Nash effort level is $x^{NE} = (A - w)/B(N + 1)$, while the socially optimal individual effort level is equal to $x^{SO} = (A - w)/2BN$. Since $x^{NE} > x^{SO}$ if $N > 1$, there is a social dilemma. If the game is repeated a finite number of times ($T \geq 2$), the standard game-theoretic prediction is that all players choose the Nash equilibrium effort x^{NE} in all periods $1, \ldots, T$. Using backward induction, if it does not pay to cooperate in the last period of a finitely repeated game, then it also does not pay to cooperate in any previous period either.

The game described above captures a social dilemma in which there are no instruments to affect the behavior of one's peers other than one's own social dilemma effort level. Hence it serves as a baseline against which we can test the impact of players having the opportunity to reward their peers. We refer to this baseline game as 0SR, reflecting that there are zero reward stages.

The game that allows for rewarding is modeled as follows. The first stage ($s1$) in this game is identical to the (first) stage of the baseline game (0SR), and hence a player's payoffs in this stage are given by equation (3.1). The social dilemma stage is then followed by two identical reward stages, $s2$ and $s3$. We will refer to this game as 2SR, reflecting that there are two reward stages in this game. A reward stage is set up as follows. Each of the N players receive z reward tokens which she can keep herself, or give to one or more of her fellow group members. Every token that the player keeps, increases her payoffs by 1 point. Every token that is sent to a fellow group member, increases that group member's payoffs by r points, where $r > 1$. Player i's payoffs in stage s ($s = \{s2, s3\}$) in period t are therefore given by:

$$\pi^s_{i,t} = z - \sum_{j \neq i} p^s_{ij,t} + r \sum_{j \neq i} p^s_{ji,t}, \qquad s = \{s2, s3\}, \tag{3.2}$$

where $p^s_{ij,t}$ is the number of reward tokens that player i sends to player j ($j \neq i$) in stage s ($s = \{s2, s3\}$) in period t. Hence, the total individual payoffs in period t of the 2SR game are $\pi^{2SR}_{i,t} = \pi^{s1}_{i,t} + \pi^{s2}_{i,t} + \pi^{s3}_{i,t}$.

Aggregate payoffs are maximized if all player choose effort level $x^{SO} = (A - w)/2BN$ in every period, and if they always send all their z reward tokens in both reward stages to their fellow group members, because $r > 1$. The standard game-theoretic predictions are, however, that no reward tokens are sent in either $s2$ or $s3$ in any period (i.e., $p^{s2}_{ij,t} = p^{s3}_{ij,t} = 0$ for all $j \neq i$, and for all $t = \{1, \ldots, T\}$). Applying backward induction, there is no reason for a selfish player to send reward tokens in $s3$ of period T, and hence there is no reason to send reward tokens in stage $s2$ of that period either. As a result, there is no reason for selfish players to choose any effort level other than the Nash equilibrium one, x^{NE}, in period T, and hence there are no reasons for selfish players to send reward tokens in either of the two reward stages in period $T - 1$ either. That means that the game unravels, and efficiency in the social dilemma activity ($s1$) is expected to be equal to the non-cooperative level independent of whether players have the opportunity to send reward tokens.

According to social orientation tests, only about 30 percent of humans behaves consistently with the assumption of 'homo economicus' in laboratory experiment; see for example Fischbacher et al. (2001). Altruists may be willing to always give rewards because of the warm-glow associated with it, or because it increases group welfare; conditional cooperators may use the reward stages 'properly' by giving rewards to those player who act cooperatively in the social dilemma stage. Thus, if players are endowed with a richer set of preferences than homo economicus, the above standard game-theoretic predictions may be refuted. It may also be the case that players are predominantly interested in their own material welfare, but that they realized that others could be reciprocating to the rewards received. Hence, they may prefer to establish bilateral ties of cooperation by exchanging reward tokens. The use of reward stages to increase cooperation in the multi-person social dilemma is more complex in the sense that it requires cooperation; a single person seizing to reward in order to punish free-riding in the social dilemma not only remains exposed to the free-rider in the social dilemma, but also may jeopardize the future exchange of rewards with the free-rider in the bilateral reward exchange.

In real world social dilemmas agents typically have good knowledge of the (past and present) behavior of (at least a subset of) their fellow community members in the social dilemma activity, and also whether and from whom they received 'rewards' (in the form of gifts, or help) now or in the past. That means that from the range of matching protocols typically used in economic experiments, the Partner Fixed protocol is the most plausible one. In this matching protocol, group membership does not change throughout the experimental session, and also identity labels remain fixed not only within but also between periods. In this setup, all of the above reasons to send reward tokens may materialize, and we can assess the net result of their interaction by comparing the efficiency in the social dilemma stage in the 2SR treatment to that in the 0SR treatment. To have an adequate benchmark, we therefore have player play both the 0SR and 2SR treatments sequentially in every Partner Fixed (PF) session, with 0SR being played first.

However, we can gain additional insight into the relevance of the various potential strategies by having player play 0SR and 2SR using two alternative matching protocols. In one, group composition remains constant throughout the experiment but identity labels are randomly changed between periods (Partner Random, PR). In sessions with this matching protocol, player cannot base their reward decisions on whether or not they received rewards from a specific fellow group member in the past. In the other, new groups are formed randomly in every period (termed the Stranger (S) matching protocol), so that player with altruistic motivations are the only ones who are likely to give rewards. So, whereas the 0SR and 2SR treatments with PF matching are the most important ones, we also implement the two treatments using PR and S matching protocols.

3.2.2 Experimental design

The experiments were conducted at Tilburg University's CentER laboratory. Subjects were students of Tilburg University of different nationalities with a background in business, economics, law, or social sciences. Each subject participated in only one session. The experimental parametrization of the game is given in Table 3.1, and Table 3.2 summarizes the predictions of the associated socially optimal and Nash equilibrium levels of both effort and number of rewards sent. Subjects earned on average €16.86 including a show-up fee of €5 for a session lasting roughly two hours and interacted via computers, programmed using the software z-Tree (Fischbacher (2007)).

Variable	Description	Value
N	number of individuals per group	5
T	number of periods of the stage game	15
w	return on investments in the private activity	0.5
A	parameter of the social dilemma's revenue function	11.5
B	parameter of the social dilemma's revenue function	0.15
e	individual endowment of effort	13
z	individual endowment of 'reward' tokens	12
r	value of reward tokens received	3

Table 3.1 Experiment parametrization.

Variable	Description	Value
x^*	symmetric individual socially optimal extraction level	6
X^*	aggregate socially optimal extraction level	30
x^{NE}	individual Nash equilibrium extraction level	10
X^{NE}	aggregate Nash equilibrium extraction level	50
$p_{ij}^{SO,s}$	indiv. socially optimal no. of rewards sent in stage $s = \{s2, s3\}$	12
$p_{ij}^{NE,s}$	indiv. Nash equilibrium no. of reward tokens sent in stage $s = \{s2, s3\}$	0

Table 3.2 Social optimum and Nash equilibrium values of all decision variables for the given experiment parametrization.

In each session, subjects played both 0SR and 2SR, and both games were implemented using the same matching protocol (i.e., PF, PR or S). In the instructions participants were informed about the matching process in their session, and 0SR and 2SR were referred to as Task 1 and Task 2, respectively. Participants were told that they would participate in two tasks, but they received the instructions for Task 2 only after Task 1 was finished.[5] The tasks were framed neutrally. The effort decision was described as 'investing tokens in option 1 or 2', where options 1 and 2 were the social dilemma activity and the outside option (with constant marginal benefits w), respectively. In Task 1, subjects played 15 periods of the game 0SR. The experimenter read out loud the instructions for this task. Subjects were given the payoff function of effort in the social dilemma (see equation (3.1)), but they were also given a payoff table

[5]The instructions can be found in Appendix 3.A.1.

in which they could look up, for every aggregate amount of effort put in by the other group members, what payoffs they would earn for a specific amount of effort invested. We did not inform the subjects about the socially optimal or the Nash equilibrium effort levels. Before the start of the experiment subjects were presented with a short test; the participants answered all questions correctly without much difficulty. After Task 1, the same sequence of events took place for Task 2, consisting of 15 periods of the 2SR treatment.

The information structure in every period of Task 1 (0SR) and Task 2 (2SR) was as follows. At the end of stage 1 of Task 1, subjects were informed about the individual effort decisions of all other group members, and about their associated profits. In Task 2, subjects received the same information as in Task 1, but they were also informed, at the end of every reward stage, about the number of reward tokens they had received from other subjects as well as about the associated payoff consequences.

As explained above, the 0SR and 2SR treatments were run using three different matching protocols: PF, PR and S. The various sessions are summarized in Table 3.3.

Session	Subjects	Groups	Average Earnings
Partner, with fixed identity labels (PF)	50	10	€19.60
Partner, with random identity labels (PR)	55	11	€18.11
Stranger (S)	80	4 sessions	€14.30

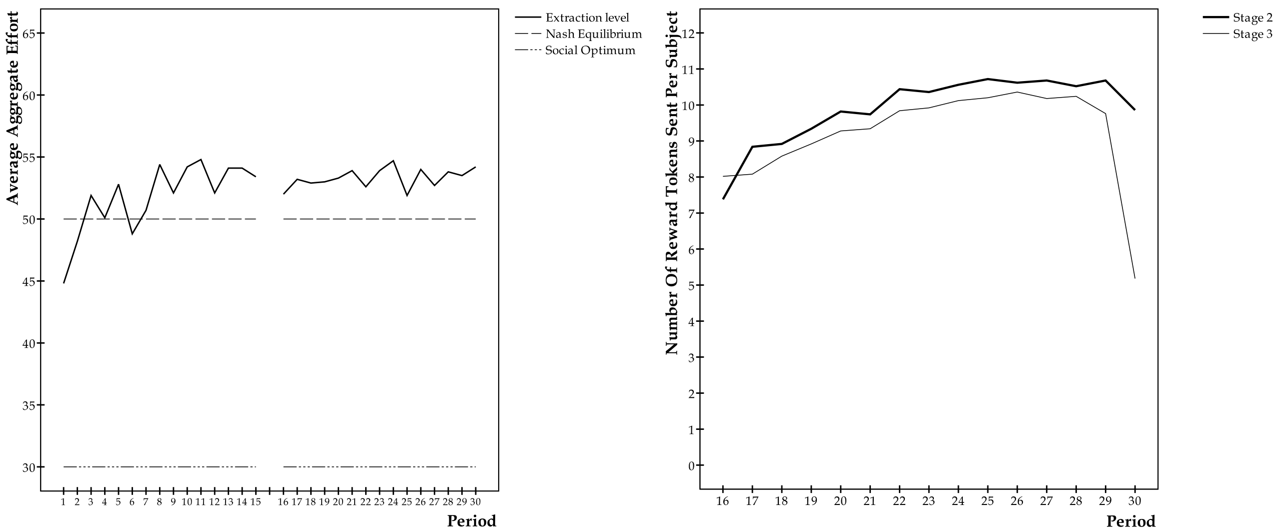
Table 3.3 Summary of all treatments.

3.3 Analysis of play in the PF sessions

In Figure 3.1(a) we present the aggregate effort (averaged over all groups) in the social dilemma stage in the 0SR treatment (periods 1-15) as well as that in the 2SR treatment (periods 16-30) that were obtained using the Partner Fixed matching protocol. This figure suggests that adding two reward stages to a standard Common Pool Resource game does not increase efficiency in the social dilemma. The average aggregate effort level in the 2SR treatment is above the Nash equilibrium level of 50 tokens in all 15 periods, and it is also not lower than average aggregate effort in the 0SR treatment.[6] Indeed, we do not even find the reward stages to have an impact at the moment that they are introduced: The

[6] When omitting the first three periods of 0SR, the Wilcoxon test on effort levels in 0SR and 2SR (with $N_1 = N_2 = 10$) yields a p-value of 0.58. When including the first three periods, average effort in the 0SR treatment only just fails to be significantly below that in 2SR as the p-value of the associated Wilcoxon test is 0.11.

average aggregate effort levels in period 15 and 16 are not statistically different (as the relevant Wilcoxon test with $N_1 = N_2 = 10$ yields a p-value of 0.72).



(a) **(b)**

Figure 3.1 **(a)** Average aggregate effort in the social dilemma stage in the PF sessions. **(b)** Average number of reward tokens sent per subject in stage 2 and stage 3 in the PF sessions.

The lack of impact of having two reward stages, however, cannot be attributed to subjects' refusing to use the reward options, as standard game theory would predict. On the contrary, Figure 3.1(b) shows that in period 16, on average, subjects give away about two-thirds of their endowment of reward tokens in both the first and second reward stage. The figure also shows that the number of reward tokens sent even increases towards the maximum of 12 tokens as the game proceeds. The average number of reward tokens sent in stage 3 of every period is only just below the average number sent in the first reward stage and follows the same trend over time, except for the very last period.

Additional support that the reward institution does not promote cooperation is obtained by means of a regression analysis, presented in Table 3.4. In the model, the difference in stage 1 effort between two consecutive periods, $x_{i,t+1} - x_{i,t}$, is the dependent variable. As independent variables, the sum of stage 2 reward tokens that subject i has received is included, as well as the sum of stage 3 reward tokens received. Furthermore, the difference in effort levels in period t between subject i and the average of the other group members, $x_{i,t} - x_{-i,t}$, is included which takes into account the regression to the mean which subjects might display, independent from the rewards they receive. The table shows that

Dependent variable: $x_{i,t+1} - x_{i,t}$	
$\sum_{i=1}^{N} p_{ji}^2$	-0.015
	(0.025)
$\sum_{i=1}^{N} p_{ji}^3$	-0.033
	(0.023)
$x_{i,t} - x_{-i,t}$	-0.283^{***}
	(0.070)
Constant	0.504^{***}
	(0.146)
N	700
R^2	0.1448

Table 3.4 OLS model to estimate the determinants of a change in stage 1 effort. Standard errors, clustered at the group level, are reported between parentheses. ***: significant at the 1%-level.

the two variables of the number of reward tokens received are insignificant. This means that the average subject is not willing to lower stage 1 efforts in the next period if he or she has received reward tokens in the current period. Subjects do tend to conform to the group average stage 1 effort levels. A subject who has greater effort levels than the group tends to lower his effort slightly in the next period. The reverse case also holds; those who invest less than the group average tend to increase their investments in the next period. The above analysis gives rise to the following three results.

Result 1 Behavior in the social dilemma stage in the first period of 2SR is, on average, even less cooperative than predicted by standard game theory, but the average subject gives away more than two-thirds of her endowment of reward tokens in the first reward stage in all periods.

Result 2 While efficiency in the social dilemma stage remains equally low over all 15 periods of 2SR, the propensity of subjects to send reward tokens increases over time.

Result 3 The number of reward tokens sent in the second reward stage of 2SR is smaller than that in the first reward stage in all but the first period, but not substantially so (except for the very last period).

Results 1 and 2 suggest that it is unlikely that the decision to send reward tokens is motivated by a desire to compensate one's peers for their cooperative behavior in the social dilemma. Instead, the temporal increase in rewards exchanged (result 2) and the fact that almost an equal number of reward tokens are sent in the second reward stage as in the first (result 3) suggest the following: Subjects recognize that exchanging reward tokens is profitable and that they base their decision to send reward tokens more on the history of reward tokens received than on the development of cooperation in the social dilemma stage.

However, these results are obtained on the basis of aggregate data, and these may hide important differences at the individual level. To uncover the underlying mechanism, we first analyze the persistence in the number of rewards exchanged between subjects. We introduce the following definition:

Definition Subjects i and j ($j \neq i$) are said to have a connection of length τ in period t, denoted by $Connection_{ij,t} = \tau$, if τ is the number of periods between periods 16 and t in which subject i sent a strictly positive number of reward tokens to subject j in both stage 2 and stage 3, and vice versa.

Figure 3.2(a) shows the frequency with which connections with a certain duration occur in the data, evaluated in period 30. Because we have 10 groups of 5 participants in the PF sessions, the maximum number of connections is equal to 100.[7]

Although there are quite a few short-run connections, the persistence in reward and counter-reward is remarkable. Of the maximum duration of 15 periods, almost fifty percent of all connections have a length of 10 periods, or more. This suggests that subjects are unwilling to provide the second-order public good of ceasing to send reward tokens to induce cooperation in the social dilemma stage. This is even more evident from the fact that all but one subject in the PF sessions had at least one connection with a length of 10 periods, or more. Consistent with intuition, Figure 3.2(b) indicates that the number of tokens sent is larger the longer the connection is in place.[8] This finding is intuitive: The longer a bilateral relationship exists, the more trustworthy the agents have proved to be, hence the more reward tokens they send to each other.

The persistence in rewarding raises the question how connections are formed. What is the role of the behavior in the social dilemma stage in every period?

[7]Each subject can start a connection with 4 other group members. Because connections consist of two subjects, the maximum number of connections is $(10 \times 5 \times 4)/2$.

[8]The Spearman correlation coefficient between the length of the connection and the average number of tokens sent is 0.90 in the first reward stage ($N = 100, p < 0.01$), and 0.91 for the second reward stage ($N = 100, p < 0.01$).

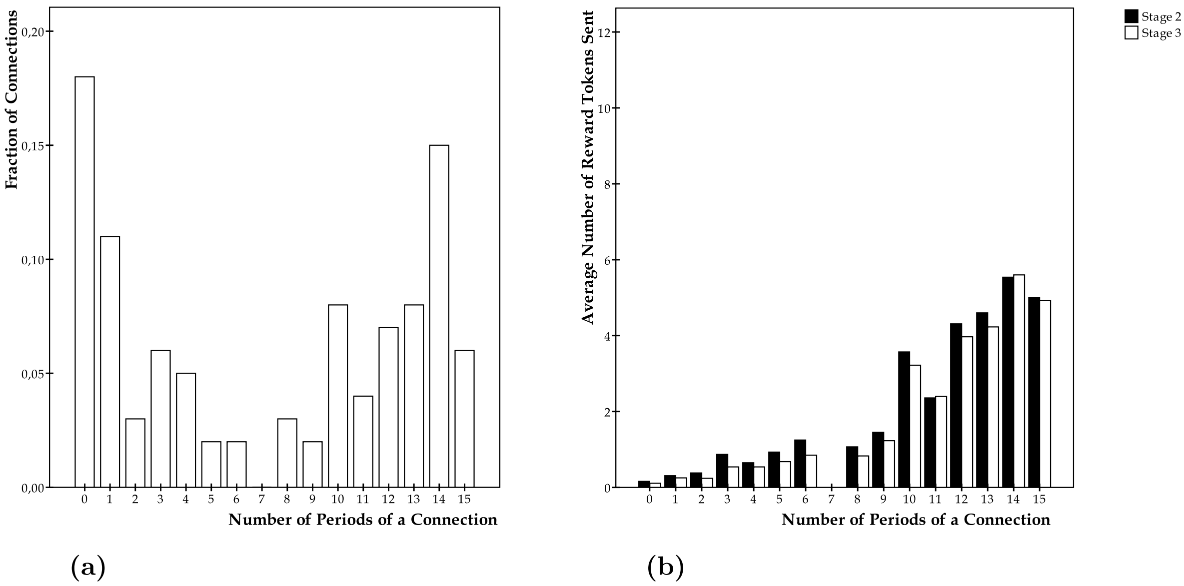


Figure 3.2 (a) Fraction of connections which last τ periods. **(b)** Average number of reward tokens sent between two subjects in a connection which lasts τ periods.

Is it really true that the number of reward tokens received is independent of a subject's effort decisions? To analyze this, we use regression analysis to explain the number of rewards sent in the two reward stages. To mitigate potential endogeneity problems, we run regressions for every period of 2SR separately.

Let us first analyze the decisions of subject i to send reward tokens to subject j ($j \neq i$) in the two reward stages of the first period ($t = 16$). The key explanatory variables here are whether or not subject j acted cooperatively in the social dilemma stage of the first period, where cooperation (non-cooperation) can be defined as subjects choosing an effort level that is below (above) their groups' average as measured by $\text{Max}\{0, \overline{x}_{-j} - x_j\}$ ($\text{Max}\{0, x_j - \overline{x}_{-j}\}$). These variables are included in the analysis of both reward stages ($s2$ and $s3$). In addition, we also include $p^2_{ji,t}$ (for $t = 16$) as an explanatory variable in the second reward stage ($s3$) of the first period, because the decision to send rewards in this stage may also depend on the number of rewards subject i received from subject j in the first reward stage.

The results are reported in columns (i) and (ii) of Table 3.5. We find that the subjects are quite prone to sending reward tokens in the first reward stage ($s2$), as evidenced by the magnitude of the intercept. However, they do so but less to subjects who put in more effort in the social dilemma stage than the av-

Dependent variable: Reward tokens sent by i to j in the PF protocol								
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
	Period 16		Period 17		Period 18		Period 25	
	$s2$	$s3$	$s2$	$s3$	$s2$	$s3$	$s2$	$s3$
$\text{Max}\{0, \overline{x}_{-j} - x_j\}$	0.049	0.023	0.258	0.196	0.249	0.196	0.068	0.006
	(0.070)	(0.052)	(0.144)	(0.191)	(0.145)	(0.148)	(0.075)	(0.026)
$\text{Max}\{0, x_j - \overline{x}_{-j}\}$	-0.242^{**}	-0.043	-0.241	-0.062	-0.040	0.042	-0.032	-0.025
	(0.103)	(0.107)	(0.168)	(0.186)	(0.103)	(0.139)	(0.054)	(0.041)
$p^2_{ji,t}$		0.461^{***}		0.572^{***}		0.671^{***}		0.602^{***}
		(0.107)		(0.096)		(0.138)		(0.173)
$p^3_{ji,t-1}$			0.541^{***}	0.107	0.785^{***}	0.209	0.944^{***}	0.365^{*}
			(0.059)	(0.091)	(0.081)	(0.122)	(0.036)	(0.179)
Constant	2.096^{***}	1.180^{***}	1.107^{***}	0.400	0.444^{**}	-0.001	0.248	0.035
	(0.222)	(0.204)	(0.305)	(0.294)	(0.165)	(0.169)	(0.151)	(0.118)
N	200	200	200	200	200	200	200	200
R^2	0.0241	0.1950	0.3342	0.4837	0.5999	0.7100	0.8371	0.8980

Table 3.5 OLS regression estimates of the number of reward tokens sent in the first reward stage ($s2$) and in the second reward stage ($s3$) in the first three periods of 2SR, and in its tenth period (i.e., $t = 16 - 18$, and 25). Standard errors, clustered at the group level, are reported between parentheses. ***: significant at the 1%-level, **: significant at the 5%-level, *: significant at he 10%-level.

erage other group member. We find no direct impact from subject j's effort decision on subject i's propensity to send reward tokens in stage 3.[9]

Next, we focus on behavior in the second period of 2SR ($t = 17$); see columns (iii) and (iv). Regarding sending behavior in the first reward stage ($s2$), we use the same controls as in column (i), but we also add the number of reward tokens subject i received from subject j in the second reward stage of period 16 (that is, $p^3_{ji,t-1}$ where $t = 17$). The results of columns (iii) and (iv) are striking: Behavior in the social dilemma stage no longer shows up significantly, while the coefficient on the number of rewards received in the previous reward stage is positive and significant.

We replicate this analysis for the third period of 2SR (t=18) and, arbitrarily, the tenth period (t=25), and the same pattern emerges. Behavior in the social dilemma stage of the current period does not affect the number of reward tokens exchanged; what matters is the number of reward tokens received from one's partner in the previous reward stage. Additional support for this conclusion comes from the temporal pattern of the magnitudes of the intercept (the constant term) and of the specifications' explanatory power (as measured by their R^2). The 'exogenous' propensity to send reward tokens decreases over time (as evidenced by the constant term becoming smaller),while the specifications'explanatory power increases. We summarize these results as follows.

Result 4 In the regressions explaining the number of reward tokens sent in the first and second reward stage in every period, we find that (i) effort only matters in the first period of 2SR, (ii) the coefficient on the number of reward tokens received from the other subject in the previous reward stage increases as the game proceeds, (iii) the 'exogenous' propensity to send reward tokens decreases over time, and (iv) the explanatory power of the models increases over time.

We conclude that the participants in the Partner Fixed sessions do not use the reward stages to enforce cooperation in the social dilemma stage, but to increase their own private earnings by establishing bilateral relationships.

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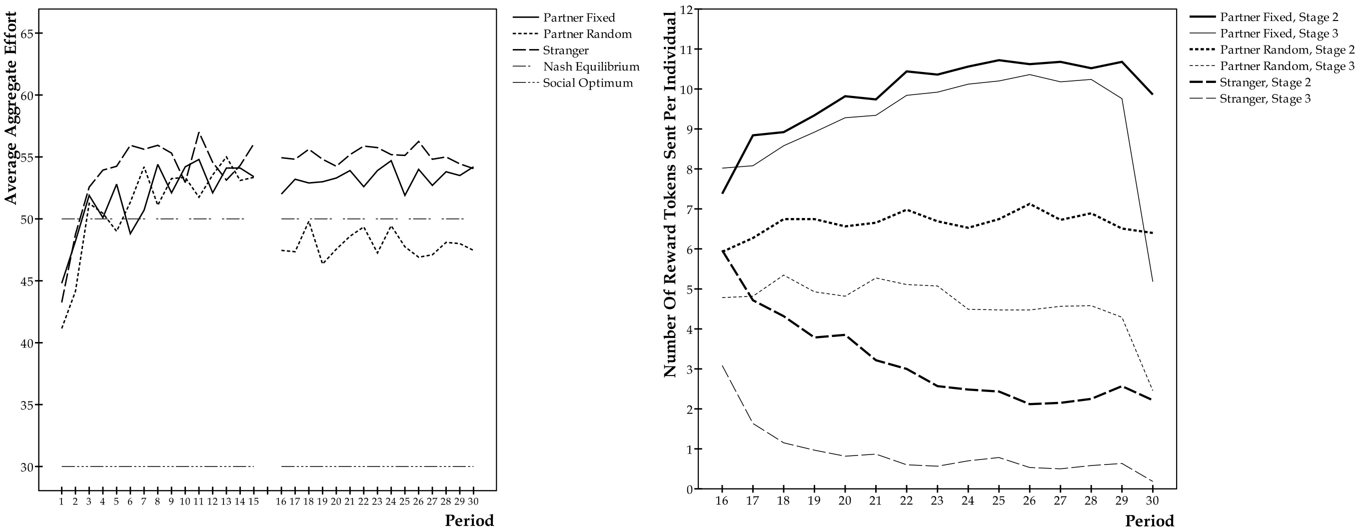
[9]There may be an indirect effect, though, because the number of reward tokens subject i receives from subject j in the first reward stage ($p^2_{ji,t}$) is likely to be affected by subject i's effort decision. This indirect effect is not likely to be very large, though, because of the very low explanatory power of the $s2$ regression.

3.4 Additional evidence on the motivation to use reward tokens

The analysis of the results obtained by using the PF matching protocol suggests that the reward institution is used predominantly to establish mutually profitable exchange relationships rather than as a true reward for cooperative behavior in the social dilemma stage. Let us now see whether we can find additional support for this conclusion when using the Partner Random (PR) and Stranger (S) matching protocols. To what extent are the results in the PF sessions driven by the fact that there is perfect information about the behavior of one's fellow group members in the social dilemma stage (current, and past) and in the reward stages (current and past)? In Figure 3.3(a) we present the results of the average aggregate amount of effort invested in the social dilemma stage $s1$ in the PR and S sessions, and the numbers of reward tokens sent in $s2$ and $s3$ are shown in Figure 3.3(b). Note that for ease of comparison we also include the results for the PF sessions.



(a) **(b)**

Figure 3.3 **(a)** Average aggregate effort in the social dilemma stage in the PF, PR and S sessions. **(b)** Average number of reward tokens sent per subject in stage 2 and stage 3 in the three matching protocols.

Starting with Figure 3.3(b), the number of reward tokens sent in the two reward stages differs between the three matching protocols, and the results are consistent with intuition. Most rewards are sent in the PF sessions and least in the S sessions, and within each session type, the number of reward tokens sent in the second reward stage is below that in the first reward stage.

If our interpretation of the PF data is incorrect and subjects use the reward options to affect their peers' behavior in the social dilemma stage, the patterns observed in Figure 3.3(b) should result in the average aggregate effort being highest in the Stranger sessions and lowest in the PF sessions. As is clear from Figure 3.3(a), these predictions do not play out in the experiment. Whereas the average group effort is indeed highest in the S sessions, it is lowest in the PR sessions, not in the PF sessions. Even more surprisingly, having two reward stages does not result in a significant reduction in the average aggregate effort in the PF and S sessions (with the p-values of the associated Wilcoxon tests being 0.58 and 0.72, respectively), but it does in the PR sessions (at $p < 0.03$).[10]

Additional support for the low effectiveness of the reward instrument is provided by means of a regression analysis, reported in Table 3.6. The regression analysis is meant to capture the effects of rewards received by subject i on that subject's next period effort levels, similar to the regression analysis of the PF matching design reported in Table 3.4. The dependent variable is the change in a subject's stage 1 effort between two consecutive periods, $x_{i,t+1} - x_{i,t}$. The same explanatory variables are used as in the regression analysis of the PF matching design: stage 2 rewards received, stage 3 rewards received and a regression to the mean variable. As can be seen from Table 3.6, the sum of stage 3 tokens received has a significant impact on stage 1 effort in the next period in the PR design. However, given its low coefficient, the economic significance of the variable is low as well. For every eighteen reward tokens received, a subject is willing to reduce stage 1 effort in the next period by only one token. The data reveals no instance of a subject who receives this amount of rewards in stage 2 in any period.[11] In the Stranger design, both the stage 2 and stage 3 reward variables are insignificant, showing that the reward instrument is ineffective in enforcing cooperation in the social dilemma stage. The above analysis gives rise to the following two results.

Result 5 The propensity to send reward tokens is lowest in the S sessions, and

[10] These tests are all based on comparison of the average aggregate effort data in 0SR and 2SR for each of the three matching protocols, omitting the first three periods of 0SR to control for learning.

[11] Taking the upper limit of the 95%-confidence interval shows that a subject is willing to reduce stage 1 effort with one token if ten reward tokens are received in stage 2. The data show that in twenty percent of all reward choices, a subject has received ten or more stage 3 reward tokens.

Dependent variable: $x_{i,t+1} - x_{i,t}$		
	PR	S
$\sum_{i=1}^{N} p_{ji}^2$	-0.024	0.026
	(0.020)	(0.043)
$\sum_{i=1}^{N} p_{ji}^3$	-0.057^{**}	0.020
	(0.020)	(0.050)
$x_{i,t} - x_{-i,t}$	-0.267^{***}	-0.167^{**}
	(0.051)	(0.045)
Constant	0.434^{**}	-0.116
	(0.182)	(0.1.5)
N	770	1,120
R^2	0.1192	0.719

Table 3.6 OLS model to estimate the determinants of a change in stage 1 effort. Standard errors, clustered at the group level for the PR matching and clustered at the session level for Stranger matching, are reported between parentheses. ***: significant at the 1%-level,**: significant at the 5%-level.

efficiency in the 2SR treatments is not higher than in the 0SR treatment in these sessions.

Result 6 Whereas the number of reward tokens sent in either reward stage of the PR sessions is about half the number sent in the same stage in the PF sessions, efficiency in 2SR is higher than in 0SR in the PR but not in the PF sessions.

To uncover the underlying mechanism, let us first have a closer look at the reward decisions made in the first period of 2SR ($t = 16$) in the three session types. The most obvious way to check whether subjects base their decisions in the first reward stage on their peers' behavior in the social dilemma stage is to analyze whether they sent reward tokens discriminatively, or not. Aggregate effort is high in the first period of 2SR in all three session types, but obviously this may hide large differences in individual effort decisions. Is it the case that subjects send their reward tokens to the ones investing least in the social dilemma stage? Or did most subjects spread their tokens equally over all four other group members in an attempt to find partners to exchange tokens with in the second reward stage (in all three session types) or also in later periods (which is feasible only in the PF sessions)?

Let us first calculate the share of subjects who equally divide whatever number of tokens they sent over all four other group members in the first reward stage of period 16. In the PF sessions this share is 0.34 while it is 0.42 in the S sessions. But this behavior occurs least frequently in the PR protocol, as only a share of 0.16 of the subjects indiscriminately spread their reward tokens over their peers.

So, the share of subjects using the first reward stage indiscriminately is quite high in both the PF and the S sessions, and especially so in the latter. In these S sessions, the average individual effort level in the first period of 2SR is 11 (and hence, is above the Nash equilibrium level and not lower than in period 15; see also Figure 3.3(a)). Still, more than 40 percent of the subjects decide to spread the rewards they sent in that period equally over all four other group members. More specifically, 58 percent of the subjects in the S matching protocol send reward tokens to all four other group members, and conditional on doing so, 72 percent of those decide to spread them equally. These observations are not consistent with subjects basing their reward decisions on their peers' behavior in the social dilemma.

Result 7 In $s2$ of period 16 of the S sessions, 58 percent of the subjects sends reward tokens to all four other group members, and 40 percent of the subjects sends an equal number of reward tokens to all four other group members. Because aggregate effort in period 16 is not significantly below that in period 15, this behavior is inconsistent with subjects rewarding their peers for their pro-social behavior in the social dilemma stage. It is consistent with subjects willing to incur small costs in the hope of finding other subjects willing to reciprocate in $s3$ to reward tokens received in $s2$.

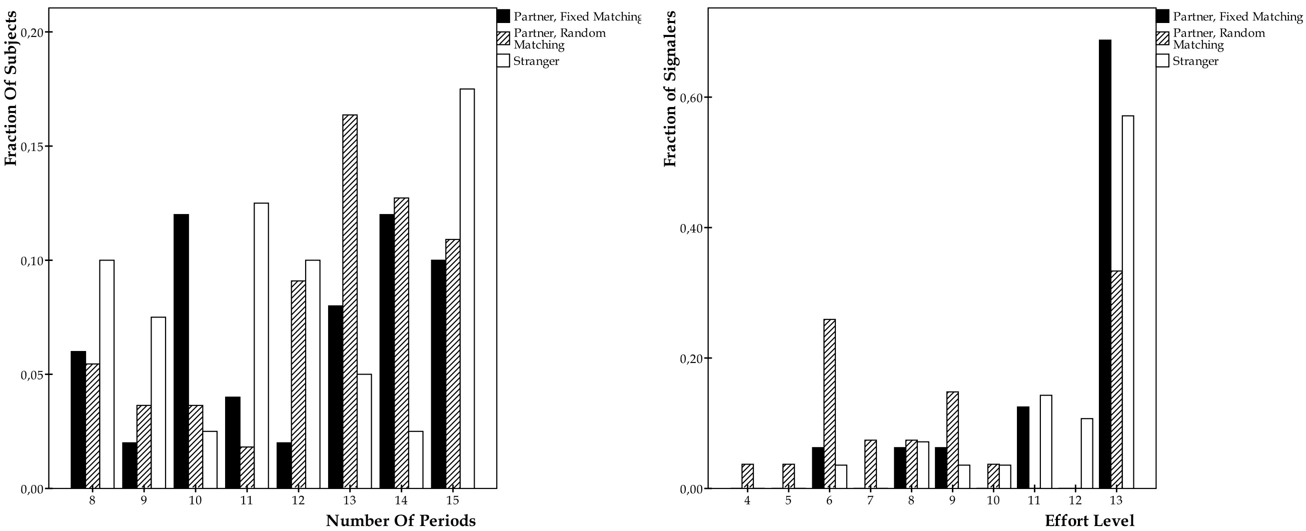
So we find that in period 16, rewards are sent most indiscriminately in the S sessions and most selectively in the PR sessions. Result 7 suggests that subjects try to find partners willing to reciprocate to rewards received within the same period, while result 6 suggests that there is real efficiency improvement associated with the more selective reward behavior in PR matching. Let us see whether we can reconcile the two results.

To do so, let us first have a closer look at the subjects' individual behavior in the social dilemma stage in each of the three session types. We calculate the variance in effort within groups over all 15 periods. The average variance of effort within groups is equal to 2.1, 2.5 and 1.9 in the PF, PR and S sessions, respectively. We find that the within-group variance is highest in the PR sessions,[12] and closer inspection of the temporal pattern (available upon request)

[12] Based on a Mann-Whitney test, taking each average variance in a period as an observation, we find that the variance in PR matching is higher than in the other two matching

reveals that it does not really decline over time either. This suggests that convergence to symmetric effort levels is least strong in the PR matching protocol. This is surprising because if rewards are used to induce cooperation in the social dilemma stage, we would expect the patterns and variances to be identical in the PR and PF sessions. If the reward stages are used to induce cooperation, all one needs is information on the effort decisions of one's peers in the current period, not of previous periods.[13]

To explore the differences in convergence of effort levels within groups between the three matching protocols, we calculate the number of periods in which a subject chooses a particular effort level in the social dilemma stage in each of the three, and also, conditional on choosing the same effort level for a number of periods, what effort level was chosen. The results are shown in Figure 3.4(a) and Figure 3.4(b), respectively.



(a) **(b)**

Figure 3.4 **(a)** Fraction of subjects who choose the same effort level in the social dilemma stage for 8 periods or more. **(b)** Distribution of effort levels chosen by subjects who choose the same effort level for 12 periods or more.

designs in the 2SR treatment. Comparing PR with PF yields a p-value smaller than 0.01 ($N_1 = N_2 = 15$). Variance in PR is also greater than in S ($N_1 = N_2 = 15, p < 0.01$).

[13]Even though subject labels are randomly reshuffled between periods in the PR sessions, group composition itself remains fixed. If other subjects are sensitive to changes in the number of reward tokens received, using the reward stages yield the same returns to the decision maker in the PR and PF sessions. Hence, if the reward stages are used exclusively to support cooperation in the social dilemma stage, plays should be identical in all stages of PR and PF.

In Figure 3.4(a) we present the frequency of subjects choosing the same effort level for eight periods or more for the three matching protocols. Focusing on the frequencies of subjects choosing the same effort level for 12 periods or more, the PR matching protocol is clearly overrepresented. Again, if rewards are used to induce cooperation in the social dilemma stage, we would expect the patters to be identical in the PF ad PR sessions. This conjecture does not play out in the data, though. Almost 50 percent of the subjects in the Partner Random sessions choose the same extraction effort level for 12 periods (out of a maximum of 15) or more, while the numbers in the PF and S sessions are 32 and 35 percent, respectively. Conditional on choosing the same effort level for 12 periods or more, Figure 3.4(b) presents the distribution of effort levels chosen. Here, the subjects of the S and PR sessions are clearly overrepresented at effort levels above the Nash equilibrium (i.e., $x > 10$), with frequencies of 82 percent and 81 percent, respectively. This is only 36 percent in the PR sessions: Subjects in the PR sessions pick a wide variety of effort levels between four and ten tokens (with a cumulative frequency of 64 percent).

So, of the three sessions types, we find that in the PR sessions (i) the variance in effort levels chosen in a group remains highest throughout 2SR, (ii) the persistence in choosing a specific effort level below the Nash equilibrium level is most pronounced, and (iii) the use of reward tokens in the first reward stage is most selective. Combined with the fact that the number of reward tokens sent in the first and second reward stages remains fairly constant throughout the experiment, one explanation might be that subjects use the social dilemma stage to signal their identity, in order to overcome the problem of subject identifiers being reshuffled between periods. This would explain the observations that the variance in effort levels chosen in groups is higher in PR than in PF.

We now supply two pieces of evidence supporting the hypothesis that the efficiency gain the PR sessions is due to signaling rather than to rewards being used to enforce cooperation (see also result 6). We do so by means of non-parametric tests regarding reciprocity in the number of reward tokens sent between 'signalers', and by means of a regression analysis aimed at explaining the use of reward tokens in the first and second reward stages.

Before we are able to present these additional pieces of evidence, we first need to define a 'signaler':

Definition A 'signaler' is a subject who chooses the same effort level in the social dilemma stage for twelve periods or more in the 2SR game.

Obviously, the cutoff point of 12 periods is arbitrary, but our results are very robust against using a cutoff point of 10 periods too (results available upon request).

Our first piece of evidence supporting the claim that the efficiency increase in the PR sessions is due to subjects signaling their identities is the way in which two signalers exchange reward tokens. If tokens are used as a way to sustain cooperation in the social dilemma, one expects the 'partner' with a higher (lower) effort level in the social dilemma stage to give more (fewer) reward tokens to the 'partner' systematically choosing a lower (higher) effort level. If subjects simply view their partner's effort level as a signal of their identity, there would be no systematic difference in the number of reward tokens sent by the two partners.

We test this by analyzing the number of reward tokens exchanged between all pairs of signalers with unequal effort levels in the social dilemma. We compare the number of rewards sent by the partner in each pair with the lower effort level to the number of rewards sent by the partner with the higher effort level.[14] Note that we include all subjects choosing the same effort level for 12 periods or more, independent of whether that level is below or above the Nash equilibrium level. The results speak against the enforcement-use of reward tokens as we cannot reject the null hypothesis of no difference in the number of rewards sent in either the first or the second reward stage, with p-values of 0.33 and 0.61 respectively (Wilcoxon tests, $N_1 = N_2 = 20$).

Result 8 Differences in effort levels chosen by two 'signalers' in the PR sessions does not affect the net flow of reward tokens sent between the partners in a 'signaling' relationship.

Second, we try to explain the number of reward tokens sent in both the first and second reward stage using regression analysis. Table 3.7 contains the results of a hurdle model explaining the number of reward tokens sent by subject i to subject j ($j \neq i$) in the first and in the second reward stage in period t. In the first step of the hurdle model we estimate the probability that subject i decides to send (or not to send) a strictly positive number of reward tokens to subject j, while the second step estimates the actual number of tokens sent. In columns (i)-(iv) of Table 3.7 we present the results of the hurdle models for the first and second reward stages while using just explanatory variables that are related to (relative) effort in the social dilemma stage. Additionally, we include a period variable to control for potential trends in the use of reward tokens. The controls

[14] From the 55 pairs of signalers, 35 are pairs in which both partners have the same effort level and 20 in which the effort levels chosen differs between the two partners.

Dependent variable: Reward tokens sent by i to j in the PR protocol	Stage 2		Stage 3		Stage 2		Stage 3	
	(i) Reward decision	(ii) Reward level	(iii) Reward decision	(iv) Reward level	(v) Reward decision	(vi) Reward level	(vii) Reward decision	(viii) Reward level
$\text{Max}\{0, \overline{x}_{-j} - x_j\}$	0.097***	1.931***	0.077***	2.166***	0.082***	1.761***	−0.021	0.086
	(0.027)	(0.510)	(0.017)	(0.686)	(0.030)	(0.421)	(0.026)	(0.087)
$\text{Max}\{0, x_j - \overline{x}_{-j}\}$	−0.091***	−1.053***	−0.070***	−1.060***	−0.052*	−0.589**	−0.001	0.050
	(0.027)	(0.240)	(0.025)	(0.322)	(0.031)	(0.267)	(0.017)	(0.146)
$\text{Max}\{0, x_i - x_j\}$	−0.100***	−1.605***	−0.090***	−1.836**	−0.107***	−1.509***	−0.002	0.016
	(0.026)	(0.515)	(0.020)	(0.743)	(0.029)	(0.487)	(0.023)	(0.236)
$\text{Max}\{0, x_j - x_i\}$	−0.020	0.064	0.005	0.308	−0.020	0.209	−0.013	−0.054
	(0.027)	(0.218)	(0.018)	(0.399)	(0.027)	(0.206)	(0.012)	(0.064)
I(Signal seen					0.525***	4.298**	0.205*	2.780***
					(0.168)	(2.102)	(0.115)	(0.779)
I(Signal seen) $\times x_j$					−0.049**	−0.418**	−0.025*	−0.283***
					(0.020)	(0.185)	(0.015)	(0.071)
p^2_{ji}							0.144***	0.840***
							(0.015)	(0.079)
Period	−0.004	0.054***	−0.019***	0.133	−0.005	0.032	−0.023***	−0.008
	(0.004)	(0.046)	(0.004)	(0.084)	(0.005)	(0.045)	(0.004)	(0.017)
Constant		−0.068***		−1.501		−0.588***		−0.621***
		(1.653)		(2.922)		(1.779)		(0.933)
N	3,080	1,880	3,080	1,216	4,800	1,993	4,800	630

Table 3.7 Hurdle model to explain the use of reward tokens in the PR sessions. The reward decision is estimated using a Probit specification. The number of rewards sent is estimated using truncated linear regression. The coefficients reported are marginal effects. Standard errors, clustered at the group level, are presented in parentheses. ***: significant at the 1%-level, **: significant at the 5%-level, *: significant at the 10%-level.

used are Max$\{0, \overline{x}_{-j} - x_j\}$ and Max$\{0, x_j - \overline{x}_{-j}\}$ as our measures of subjects acting more or less cooperatively in the social dilemma stage than the rest of their group. Also we include Max$\{0, x_i - x_j\}$ and Max$\{0, x_j - x_i\}$ as subjects may be reluctant to send reward tokens to other subjects acting not only much less cooperatively than they do themselves, but maybe also to subjects acting more cooperatively. Such subjects are unlikely to send reward tokens in return.

At first sight, the results seem to suggest that subjects indeed condition their decision to send reward tokens in both reward stages on the behavior of their peers in the social dilemma stage. In both reward stages, subjects are more likely to send rewards (and also a larger number of rewards) to fellow group members who put in less effort than the group's average. There is also some evidence that they are less inclined to send reward tokens to a fellow group member in the first reward stage the more aggressive that group member behaves in the social dilemma stage (relative to the group average). However, the effect is not very strong because of two reasons.

First, the coefficient estimates on Max$\{0, \overline{x}_{-j} - x_j\}$ are statistically highly significant but economically quite small. Comparing all effort levels within a group, the difference between an individual's and group's effort levels is equal to one or zero in about 78 percent of the cases. Therefore, the hurdle model predicts that the probability of sending a stage 2 (stage 3) reward token based on stage 1 behavior is in the order of nine (seven) percent in most cases. Conditional on sending reward tokens, the number sent is about 2 tokens. Second, the other two explanatory variables, Max$\{0, x_i - x_j\}$ and Max$\{0, x_j - x_i\}$ suggest that the larger the absolute difference between the effort levels of subjects i and j, the less likely subject i is to send rewards and also the fewer reward tokens sent, if any. The negative coefficient on Max$\{0, x_i - x_j\}$ may be consistent with subjects refusing to send reward tokens to subjects acting less cooperatively than they do. However, probably the only reason why one is less willing to send rewards to someone acting more cooperatively than one does oneself is that one does not expect rewards to receive in return. We summarize our findings below.

Result 9 Subjects are less prone to sending reward tokens to subjects acting more cooperatively than they do, the larger the difference in cooperation levels. This is inconsistent with subjects rewarding their peers for their cooperative behavior in the social dilemma.

Next, we extend our analysis in Table 3.7 by adding information on (potential) signaling behavior; see columns (v)-(viii). The key variable here is I(Signal seen). This is a dummy variable with a value of 1 when the effort level of subject j in period t is an effort level that has been invested in period $t-1$, and 0 otherwise. We use this dummy as a control variable, but we also interact it with x_j (to create $I(\text{Signal seen}) \times x_j$) to check whether the strength of the signal is inversely related to the level chosen: Sending a signal is more costly the lower the signal chosen, and hence the more trustworthy subject j may be. In addition, we also add $p^2_{ji,t}$ to take into account the effects of stage 3 rewards due to having received reward tokens in stage 2.

The consequences of adding these variables for the level of significance of the coefficients of the social dilemma stage variables are quite substantial. In terms of economic relevance, the signaling variables are much more important than the social dilemma ones. The magnitude of the variable I(Signal seen) is around fifty percent in stage 2. Together with the interaction term (evaluated at the average effort level of seven) the probability of attracting rewards using a signal of six or lower, is around six times greater in magnitude than the first four variables of Table 3.7. In the second reward stage these effects are slightly less strong, especially because part of the significance of being in a bilateral exchange relationship is accounted for by the variable p^2_{ji}.[15] We summarize our findings in result 10 below.

Result 10 Subjects are more likely to send reward tokens to a subject if that subject chose the same effort level as in the previous period. Because a large share of subjects hardly ever change their effort levels and because the subjects' identifiers are reshuffled between periods, subjects view unchanged effort levels as a signal of the decision maker's identity.

3.5 Discussion

In the real world behavior of individual agents is embedded in a system of interpersonal relations, where individual welfare depends on actions taken in activities that require multi-agent cooperation, and also on actions taken in alternative economic activities that only require bilateral cooperation (Granovetter (1985), Bowles and Gintis (2002)). The latter can take the form of the exchange of goods or services, for which marginal utilities may differ between individuals. Examples include assisting fellow community members with activities such as crop harvesting or child minding, where the recipients time constraint becomes

[15]Note that this is remarkable because we did not exclude the 36 percent subjects 'choosing effort levels of 11 or higher as a signal'; these agents most likely just play best response to the relatively low and highly stable effort levels chosen by the signalers in their group, which negatively affects the economic and statistical significance of the coefficients on the signaling variables.

less binding, and the recipients marginal value of time may well be above that of the donor. Refusing to provide these rewards is costly to the agent no longer receiving them, and hence having the opportunity to selectively engage in a mutually profitable exchange of rewards (and hence have the option to cease providing these rewards) may be viewed as a natural instrument to induce co-operation in the multi-agent social dilemma. If a community member does not act cooperatively in the social dilemma, the other members of the community may decide to selectively exclude that member from these alternative activities that positively contribute to welfare.

The results of our laboratory experiments suggest that having the option to selectively increase one's fellow group members' welfare does not result in an increase in efficiency in the multi-agent social dilemma, as is evident from Figure 3.1(a). However, Figure 3.1(b) shows that this is not due to subjects to increase their peers' welfare by providing a reward, as long as the other group members reciprocate such that mutually profitable exchanges of rewards are established. These claims are supported by the fact that play in the PR sessions differs substantially from that in the PF sessions. If agents would decide to provide rewards *selectively* based on their peers' behavior in the social stage, behavior in the two sessions should be identical. Even though subject labels are randomly reshuffled between periods in the PR sessions, group composition itself remains fixed. Therefore, subjects are equally well able to base their decisions to send reward tokens to some of their peers on their behavior in the social dilemma stage, and reap the fruits of these selective rewarding decisions in the subsequent periods as in the PF sessions. However, in the analysis of play in the PF sessions we find evidence that subjects base their decision to directly increase their peers' welfare more on whether they received rewards from their peers in the previous reward stage, than on their peers' behavior in the social dilemma stage. And from the analysis of play in the PR sessions we find that subjects attach more weight to establishing bilateral cooperation in the reward stages than to affecting their peers' behavior in the social dilemma stage. The reason is because they are willing to incur costs to signal their identity between periods by choosing a specific effort level in the social dilemma stage. Rather than trying to maximize their profits in the social dilemma activity by choosing a privately optimal effort level, they pick an effort level that serves as a subject label in the periods to come; see Figure 3.4.

So we find that subjects are not willing to provide the 'second-order public good' of (not) using the option to selectively affect their peers' welfare to induce cooperation in the social dilemma, but they are willing to cooperate bilaterally.

When subjects are able to select their partners in an activity that requires cooperation, efficiency tends to be much higher than if group composition is imposed (see for example Sutter et al. (2010)). Our results are consistent with that. The possibility for subjects to choose their partners in the 'reward activity' results in the exchange of reward tokens being very close to the socially optimal level (as the average number of reward tokens sent is above 10 (from an endowment of 12) in both reward stages in periods 23-29 in 2SR in the PF sessions. Hence, we cannot attribute our results to subjects' not being cooperative, all the evidence suggests that they are conditionally cooperative. The pattern of play in the social dilemma stage in 0SR in all three matching designs (as shown in Figure 3.3(a)) is consistent with subjects reciprocating to their peers' effort decisions in the previous period. This gives rise to a jagged pattern of effort levels in all groups (group-level data are available upon request). And from the pattern of rewarding in 2SR (especially in the PF and PR sessions) we find that subjects reciprocate in rewarding.

3.6 Conclusion

Previous literature shows that efficiency in a social dilemma situation can be increased considerably if subjects are allowed to either punish or reward their peers for non-cooperative (cooperative) behavior in the social dilemma. This effect vanishes, however, when the experimental design allows for counter-punishment (or retaliation). In this chapter, we demonstrate that the option to counter-reward also negatively affects the cooperation-enforcing effect if rewards are used as an instrument.

We study experimentally a non-linear public game (also known as the Common Pool Resource game) with two stages of reward. Rewards in such a setup can be either used in an 'enforcing' manner, linked to the decisions observed in the group-level social dilemma game, or in a 'bilateral' manner, fully independent of the observations made in the group-level social dilemma game. We set up three matching protocols affecting the length of subjects interaction across periods and compare the use of rewards in them. We implement our standard non-linear public bads game (with zero reward options; 0SR) and the same game with two reward stages (2SR) using three different matching protocols: sessions with Partner matching and fixed identity labels (PF), sessions with Partner treatment and randomized identity labels (PR), and sessions with (imperfect) Stranger matching (S).

We find that in the PF sessions, offering subjects two options to send rewards to their peers does not improve efficiency in the non-linear Common Pool Resource game as compared to the case with zero reward options. While efficiency in the social dilemma stage remains low throughout the experiment, the propensity to send reward tokens increases over time. We find that this increased propensity to send rewards is due to subjects positively reciprocating to the number of reward tokens received from fellow group members in the previous reward stage. Here, decisions in the social dilemma stage are found to play a role in the first period only, and even then we still find that more than one-third of the subjects just equally divide whatever number of reward tokens they give over all four other members of their group.

We delve deeper into the underlying mechanism by analyzing play in the PR and S sessions. With respect to the latter, we find subjects in the S sessions to also be prone to just equally divide whatever number of reward tokens they give over all four other members of their group, as more than 40 percent of the subjects display such behavior. Even though subjects are rematched into new groups at the beginning of every new period, subjects still try to earn extra money by sending reward tokens to fellow group members in the first reward stage in the hope of receiving back some in the second.

The results of comparing play in the PF and PR sessions are most telling, however. Because group composition remains fixed throughout the experiment, one would expect play in the social dilemma stage to be identical in the PF and PR sessions if rewards are used to enforce cooperation. All what is needed for such behavior to emerge is (i) being ensured of future payoffs if the reward recipients are sensitive to receiving rewards (that is, one needs to be in the same group as the recipients in the next period), and (ii) receiving information on the behavior of one's peers in the social dilemma stage of the current period. Both conditions are met in PF and PR. Still, we find marked differences between the two session types. We find that the within-group variance of effort levels chosen is larger in the PR sessions than in the PF sessions, while the within-subject variance in effort over all periods is smaller in PR matching than in the latter. The lack of convergence in effort levels in the PR sessions suggests that subjects see benefits to acting consistently during the social dilemma stage (even though it is costly if one chooses an effort level that is (much) below the Nash equilibrium level), whereas they do not perceive any benefits to such persistence in the PF sessions. Given that the only difference between the two matching protocols is that subject identifiers are reshuffled between periods in the one protocol and not in the other, it is natural to think of the persistence of effort levels in terms of a signal: Subjects in the PR sessions manage to overcome the problem of establishing multi-period, mutually profitable exchanges of reward tokens by choosing a specific ‘signal’ by means of their effort decisions in the social dilemma stage.

Together, we find strong evidence that subjects do not use reward tokens to endorse cooperative behavior in the social dilemma, but that they see the benefits of engaging in a less risky, more profitable exchange of reward opportunities. Because there are no discrete stages in real world social dilemma situations there is a continuous opportunity to engage in mutually profitable activities requiring bilateral cooperation, we conjecture that the presence of such opportunities is insufficient to support cooperation in the social dilemma itself.

3.A Appendix

3.A.1 Instructions for the two stages of reward treatment

In this appendix, the instructions of the Partner Fixed (PF), Partner Random (PR), and Stranger (S) treatments are displayed. The experimental instructions are kept the same where possible. Sentences that differ are placed in brackets, and each sentence is preceded by the abbreviation of the treatment.

Introduction:

You will now participate in an experiment on economic decision-making. The experiment will last approximately 1.5 hours. You will be paid after the experiment. No other experiment participant will learn how much you earned. You will be paid 5 Euros for your participation PLUS any additional earnings you will make in the experiment. How much you earn crucially depends on your decisions in the experiment.

During the whole experiment, you are not allowed to talk to other participants. Disobeying this rule results in your exclusion from the experiment. In the experiment, you will participate in two Tasks. You will earn points in each of them. At the end of the experiment, you will be paid for all the points you earned.

The exchange rate is: 100 points = 1 Euro, 1 point = 1 Euro-cent.

Experiment description:

The experiment consists of 15 rounds. In each round, you will be in a group with four other participants; a group therefore consists of five participants in total. Note that you will interact with the same four subjects in all rounds. The other members of your group will be identified by means of identity labels, 'x', 'xx', 'xxx' and 'xxxx'. [PF: These labels will remain the same within each round, and between rounds. To give an example, if another group member is labeled 'x' in some round, he/she is also labeled 'x' in the next round. / PR: These labels will remain the same within each round, but they will change between rounds. To give an example, if another group member is labeled 'x' in some round, he/she may be labeled 'xxxx' in the next round. That means that nobody is able to make a connection between another participant's decisions in different rounds. / S: After each round, all participants are randomly rematched into new groups.

Again, the labels ‘x’, ‘xx’, ‘xxx’ and ‘xxxx’ are randomly assigned to the other four members of your group. Because groups are rematched, ‘xx’ in some round is not the same participant as the group member labeled ‘xx’ in some other round. This means that nobody is able to make a connection between other participants’ decisions in different rounds.]

At the beginning of every round, you receive 13 tokens. You have to decide how to divide these 13 tokens between two options: option 1 and option 2. Observe that you have to divide all your 13 tokens. Therefore if you put X tokens into option 1, you put automatically 13 − X tokens into option 2. In this task, therefore, you will be asked to make one choice: how many tokens you put in option 1. It is then automatic that you put 13 − X tokens in option 2.

Now, we will explain how you earn points for the tokens you put in option 1 and option 2.

Earnings for tokens in option 1: The number of points you earn for the tokens in option 1 depends on how many tokens you put in option 1 and how many tokens the other four group members put in option 1. You receive 9.5 points for every token you put in option 1. You also have to pay costs when using option 1. The costs depend on how many tokens in total all group members (including you) put into option 1: for every token you put into option 1 you have to pay a cost of 0.15 points MULTIPLIED BY the total number of tokens in option 1.

Earnings for tokens in option 2: The number of points you earn for the tokens in option 2 depends only on how many tokens you put in option 2. For every token in option 2 you receive 0.5 points. There are no costs.

Your total earnings in this round are the sum of points you earn in option 1 and 2, that means

9.5*X − 0.15*Y*X + 0.5*(13 − X),

where X is the number of tokens you put into option 1, and Y is the total number of tokens that are put into option 1 (that is, by you and the four other group members).

When making your decisions, you can use the above formula, but you can also make use of the table below. The table contains the number of points you can earn for different combinations of the number of tokens you put in option 1 and the total number of tokens the other four group members put in option 1. Please, have a look at the table now. In the first column (in grey print), you find all possible numbers of tokens you may put in option 1. You can choose any integer number from 0 to 13, that means numbers $0, 1, \ldots, 12, 13$. In the first

	POINTS EARNED FOR TOKENS IN OPTION I AND OPTION II													
	Total number of tokens put in option 1 by the other FOUR group members													
	0	4	8	12	16	20	24	28	32	36	40	44	48	52
0	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5
1	15.3	14.7	14.1	13.5	12.9	12.3	11.7	11.1	10.5	9.9	9.3	8.7	8.1	7.5
2	23.9	22.7	21.5	20.3	19.1	17.9	16.7	15.5	14.3	13.1	11.9	10.7	9.5	8.3
3	32.1	30.3	28.5	26.7	24.9	23.1	21.3	19.5	17.7	15.9	14.1	12.3	10.5	8.7
4	40.1	37.7	35.3	32.9	30.5	28.1	25.7	23.3	20.9	18.5	16.1	13.7	11.3	8.9
5	47.7	44.7	41.7	38.7	35.7	32.7	29.7	26.7	23.7	20.7	17.7	14.7	11.7	8.7
6	55.1	51.5	47.9	44.3	40.7	37.1	33.5	29.9	26.3	22.7	19.1	15.5	11.9	8.3
7	62.1	57.9	53.7	49.5	45.3	41.1	36.9	32.7	28.5	24.3	20.1	15.9	11.7	7.5
8	68.9	64.1	59.3	54.5	49.7	44.9	40.1	35.3	30.5	25.7	20.9	16.1	11.3	6.5
9	75.3	69.9	64.5	59.1	53.7	48.3	42.9	37.5	32.1	26.7	21.3	15.9	10.5	5.1
10	81.5	75.5	69.5	63.5	57.5	51.5	45.5	39.5	33.5	27.5	21.5	15.5	9.5	3.5
11	87.3	80.7	74.1	67.5	60.9	54.3	47.7	41.1	34.5	27.9	21.3	14.7	8.1	1.5
12	92.9	85.7	78.5	71.3	64.1	56.9	49.7	42.5	35.3	28.1	20.9	13.7	6.5	-0.7
13	98.1	90.3	82.5	74.7	66.9	59.1	51.3	43.5	35.7	27.9	20.1	12.3	4.5	-3.3

Figure 3.5 The payoff table.

row (in grey print), you find the number of tokens the other four participants may (together) put in option 1. Your total payoff in one round depends on the combination of the number of tokens you put in option 1 and the number of tokens the other four participants (in total) put in option 1.

Example: Suppose you put 4 tokens in option 1. In the grey column, find the row that begins with 4 (tokens). And, suppose you think that the other four group members will put in total 12 tokens in option 1. In the grey row, find the column that begins with 12 (tokens). Look in the table for the intersection of the chosen row (4 tokens) and column (12 tokens). You find that if you put 4 tokens in option 1 and the other four members put in total 12 tokens in option 1, your total earnings in this round are 32.9 points.

In the table, observe the following. You can always make sure to earn 6.5 points in any round by putting zero tokens in option 1. You can, however, possibly earn more points if you put some tokens in option 1. How many points you earn, depends crucially on the choices of the other members of your group. If, for example, you put all 13 tokens in option 1, you can earn 98.1 points, if the other group members do not put any tokens in option 1. On the other hand, you can lose 3.3 points, if the other group members do the same as you, and put all their tokens in option 1.

Other group members affect how many points you earn, and you affect how many points the others earn.

Note that in the experiment, you and the other four members of your group will decide on the division of the tokens at the same time. Therefore, at the moment of your decision you do not know how many tokens the other members of your group will put in option 1. You can only guess.

After all group members made their decisions, you will receive information on how many tokens each group member has put in option 1 in this round, and how many points each group member earned.

We will now explain how the computer screens look like.

SCREEN 1

Have a look at Figure "Screen 1". Here you decide how many tokens you put into option 1. Use the keyboard to type in one of the numbers $0, 1, \ldots, 12, 13$ in the active field, and confirm your choice by pressing OK.

Warning: Before pressing OK, make sure your choice is correct. You cannot change your decision after you have pressed OK.

After having pressed OK, you will be asked to wait until all experiment participants have done the same. The experiment continues only after all experiment participants pressed OK. We therefore kindly ask you not to delay your decision too much. For every decision, a time indication of one minute is shown in the header. After this time expires, you are repeatedly asked to submit your decision, or press the OK button.

After pressing OK, a waiting screen will appear. After all experiment participants have pressed OK, Screen 2 will appear.

Figure 3.6 Screen 1

SCREEN 2

In the upper part of this screen you find a table with information on how many tokens each group member has put in option 1 in this round, and how many points he/she earned in this round.

Note that information about you is always given in the column denoted 'me'. Information in the columns denoted 'x', 'xx', 'xxx', and 'xxxx' is about the other four group members. [PF: Remember that the group member labeled 'x' will always be the same individual in all rounds. / PR: In each round, the labels of these four subjects will change. Therefore, for example, subject denoted 'x' in some period may be denoted 'xxx' or 'xx' or 'xxxx' in the next period. / S: Remember that all participants are rematched into new groups between all rounds. Therefore, group member 'xxx' in some round is not the same participant as the group member labeled 'xxx' in some other round.]

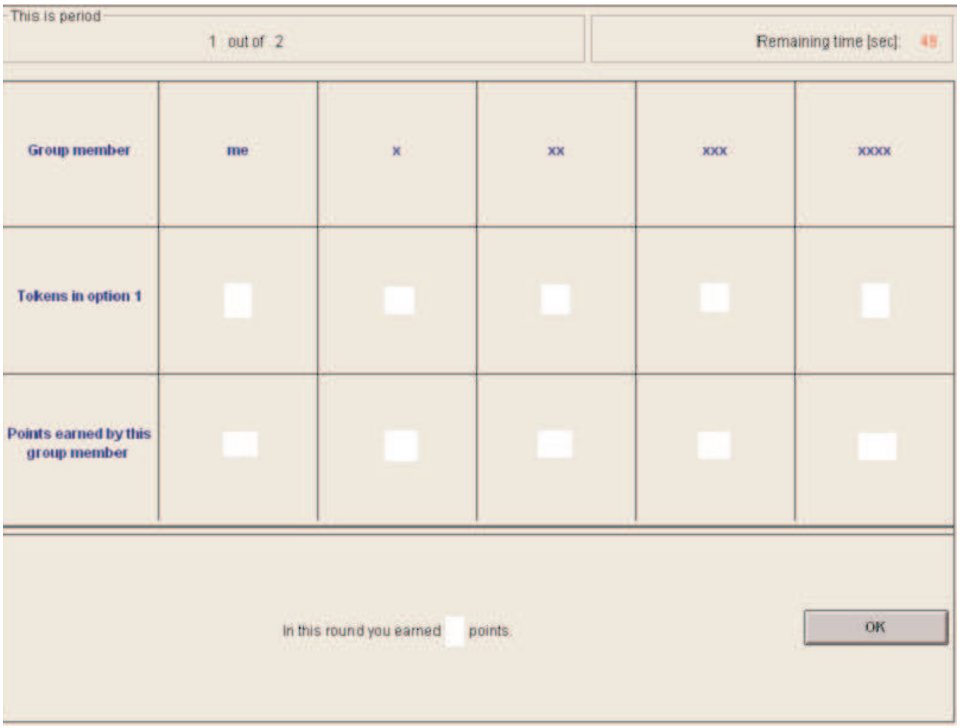


Figure 3.7 Screen 2

The experiment now starts with a short test to make sure that everybody understands how points are earned. Use your tables to answer the following questions. After all experiment participants answered all questions correctly, the experiment will begin.

TEST QUESTIONS:

Q1. I will be in a group with four other subjects. These will remain the same four subjects for all 15 rounds. [YES/NO]

Q2. When I choose 5 and the other four group members choose 1, 2, 9 and 0, then my payoff will be:. . . (use table to answer)

Q3. When a group member is denoted ‘xx’ in round 5, then it is certain that this is the same subject as the subject denoted ‘xx’ in round 6. [YES / NO]

Second part of the experiment:

You will now participate in the second and last part of the experiment. It also has 15 rounds. [PF: You will be matched with the same four group members as you were before. Again, these other participants remain member of your group in all 15 rounds. As before, the same individuals have the same labels. / PR: You will be matched with the same four group members as you were before. Again, these other participants remain member of your group in all 15 rounds. As before, the identity labels will remain the same within each round, but they will change between rounds. / R: As in the previous task, the participants of your group will not be the same between rounds. Again, the other four members of your group will be randomly assigned out of all the participants who take part in this experiment.] In this part of the experiment, however, every round consists of three stages, Stage I, Stage II and Stage III. Stage I of every round is the same as before. That means that you will receive 13 tokens and have to divide them between option 1 and option 2. The payoff table and payments are the same as before. Let us explain Stage II and Stage III.

Stage II:

In Stage II of a round (following immediately Stage I in each round), you will again receive a number of tokens — this time it is 12 tokens. Every token is worth 1 point to you. That means that you basically receive 12 points. You can now choose how many of these tokens to send to each of the other four members of your group. For every token you send to another subject in your group, your earnings will be reduced by 1 point. Every token you send to another subject in your group is worth 3 points to that subject. So, for every token you send to another subject in your group, the earnings of that subject will be increased by 3 points. You can decide to send any number of tokens (from $0, 1, 2, \ldots, 11, 12$) to any number of other group members (that is, 0, 1, 2, 3, or 4 other group members). The sum of the tokens you send must not exceed 12. Also, you are not allowed to send any tokens to yourself. All five group members make this decision at the same time.

Earning points in Stage II:

The number of points you earn in Stage II of every round is calculated as follows.

It is equal to:

- 12 points (for 12 tokens you received at the beginning of the round),
- MINUS as many points as the number of tokens you sent to the other four members of your group,
- PLUS three times the total number of tokens you received from the other four members in your group.

Stage III:

In Stage III of a round (following immediately Stage II in each round), you will again receive 12 tokens. Every token is again worth 1 point to you. You can now choose how many of these tokens to send to each of the other four members of your group. So, for every token you send to another subject in your group, his/her earnings increase by 3 points and your earnings will be reduced by 1 point. You can decide to send any number of tokens (from $0, 1, 2, \ldots, 11, 12$) to any number of other group members (that is, 0, 1, 2, 3, or 4 other group members). The sum of the tokens you send must not exceed 12. Also, you are not allowed to send any tokens to yourself. All five group members make this decision at the same time.

Earning points in Stage III:

The number of points you earn in Stage III of every round is calculated in the same way as in Stage II. It is equal to:

- 12 points (for 12 tokens you received at the beginning of the round),
- MINUS as many points as the number of tokens you sent to the other four members of your group,
- PLUS three times the total number of tokens you received from the other four members in your group.

Your total earnings in one round of the experiment are:

- The number of points you earned in Stage I, PLUS
- The number of points you earned in Stage II, PLUS
- The number of points you earned in Stage III.

We will now explain how the computer screens look like.

SCREEN 1

This is very similar to the decision screen as in the first part of the experiment:

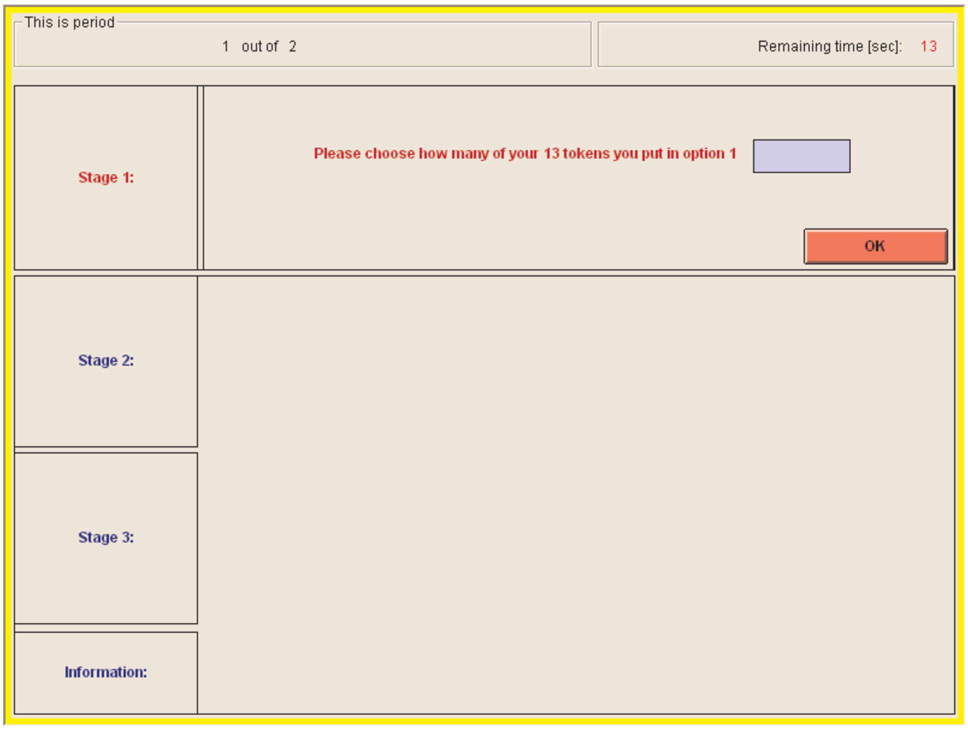


Figure 3.8 Screen 1

SCREEN 2

In the upper part of this screen you find a table with information on how many tokens each group member has put in option 1 in this round, and how many points he/she earned in Stage I. Your decision is in the column 'me'. Decisions of the other four group members are in the columns 'x', 'xx', 'xxx' and 'xxxx'. [PF: Remember that these labels remain constant within each round, and between rounds. So 'x' is the same subject in all three stages (Stage I, II and III) of the current round. Furthermore, a subject labeled 'x' in this round is also labeled 'x' in the next round. / PR: Remember that these labels remain constant within each round, but that they change between rounds. So 'x' is the same subject in all three stages (Stage I, II and III) of the current round, but label of subject labeled 'x' in this round may be different in another round. / S: Remember that these labels remain constant within each round, but that they change between rounds. So 'x' is the same participant in all three stages (Stage I, II and III) of the current round, but not between rounds.

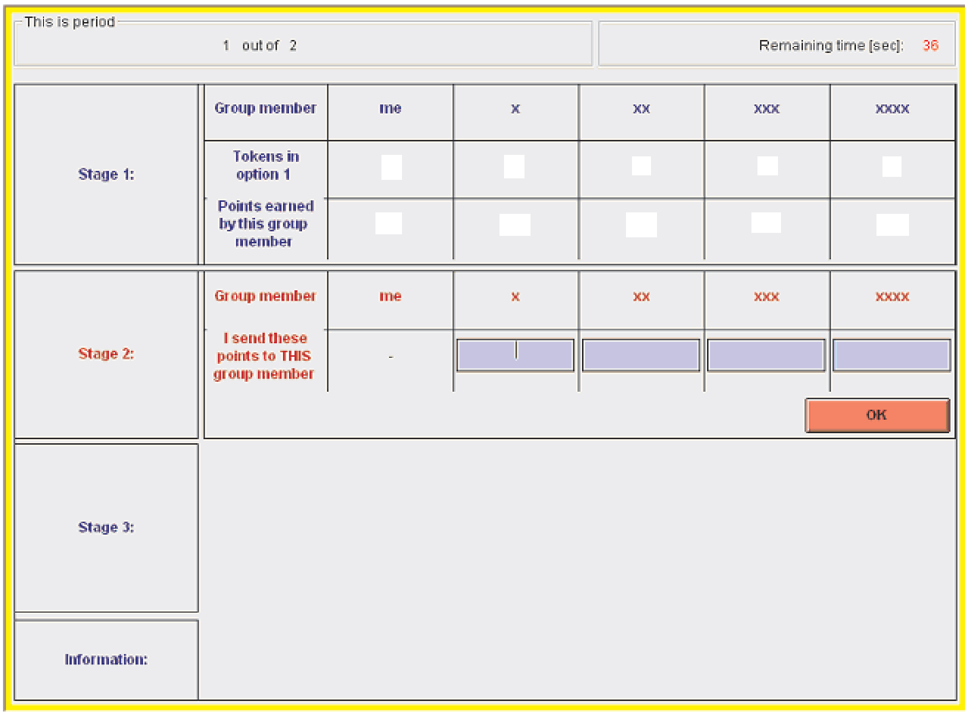


Figure 3.9 Screen 2

In the lower part of the screen, you are asked to make a decision how many tokens from your 12 tokens to send to each of the other four members of your group. For each group member, you have to put in a number; if you do not wish to send tokens to a particular group member, you type in '0'. The sum of these four numbers of tokens must not exceed the 12 tokens you received. Press OK, when you are ready to continue. A waiting screen will appear. The experiment continues only after all experiment participants have pressed OK, and therefore we kindly ask you not to delay your decision too much.

SCREEN 3

In this screen you find all information about how many tokens each of the other group members sent to you in Stage II. In the lower part of the screen, you are asked to make a decision in Stage III: how many tokens from your 12 tokens in Stage III you want to send to each of the other four group members. For each group member, you have to put in a number; if you do not wish to send tokens to a particular group member, you type in '0'. The sum of these four numbers of tokens must not exceed the 12 tokens you received. Note that (for example) subject denoted 'x' in Stage I is the same as subject denoted 'x' in Stage II and the same subject in Stage III.

Press OK, when you are ready to continue. A waiting screen will appear. The experiment continues only after all experiment participants have pressed OK, and therefore we kindly ask you not to delay your decision too much.

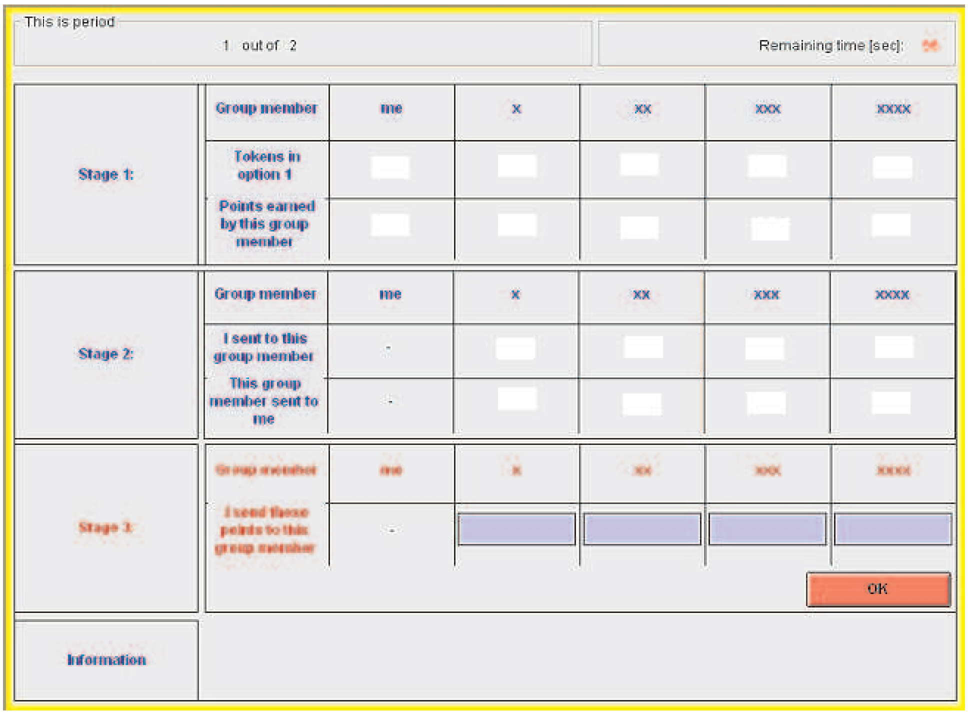


Figure 3.10 Screen 3

SCREEN 4

In this screen you find the information about all three stages, and you will also learn your final payoff for this round.

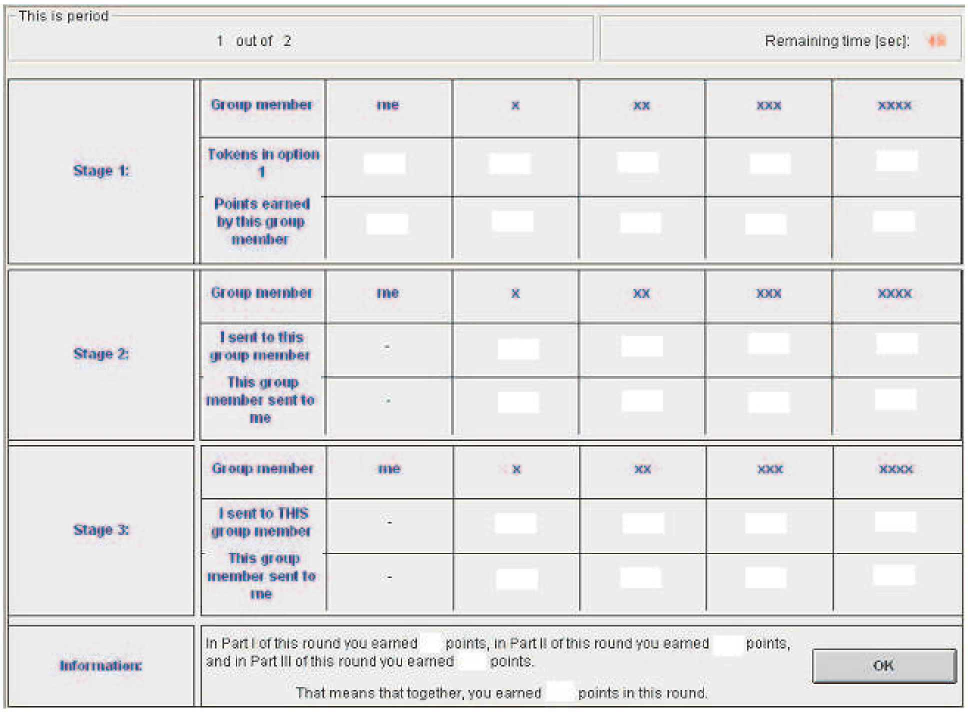


Figure 3.11 Screen 4

Please, raise your hand if you have questions at this moment.

The experiment now starts with a short test to make sure that everybody understands how points are earned. Use your tables to answer the following questions. After all experiment participants answered all questions correctly, the experiment will begin.

TEST QUESTIONS:

Q1. I will be in a group with four other subjects. These are the same subjects as in the first part of the experiment. [YES / NO]

Q2. I will be in a group with four other subjects. These subjects will remain the same four subjects for all 15 rounds. [YES / NO]

Q3. Have a look at screen 3. The subject denoted ‘xxx’ in Stage I and in Stage II and in Stage III is the same subject. [YES / NO]

Q4. When a subject is labeled ‘x’ in Stage I of round 3, then it is certain that this is the same subject as the subject labeled ‘x’ in Stage I of round 4. [YES / NO]

Chapter 4

A Tale of Two Carrots: The Effectiveness of Multiple Reward Stages in a Common Pool Resource Game[1,2]

Forthcoming in J. List and M. Price (eds., 2010) Handbook on Experimental Economics and the Environment, Edward Elgar.

4.1 Introduction

In Chapter 3, I have shown that adding two stages of rewards to a social dilemma game does not result in an increase in cooperation. This chapter takes a closer look on one of the matching designs, the partner design with randomized identity labels between periods. As is shown in the previous chapter, many subjects use their effort levels in the common pool resource as a signal, meant to attract rewards from trustworthy group members. In this chapter, the analysis of signaling behavior is studied in more detail. Additionally, it turns out that the partner matching design shows insights into the social orientation of participating subjects. This chapter gives a closer look on how this can be established.

[1]This chapter is co-authored with Daan van Soest and Jana Vyrastekova.

[2]Special thanks to Charles Noussair, Eline van der Heijden, Mark Brouwers, Chris Müris and David Voňka for their comments and suggestions on earlier versions of this paper.

Over the past two decades, many economic experiments have been conducted to assess the relative effectiveness of so-called self-regulatory instruments in sustaining cooperation in social dilemma situations, such as public goods games or common pool resource games. Instruments include ostracism (Masclet (2003)), peer-to-peer rewards (Sefton et al. (2007), Vyrastekova and van Soest 2008), and verbal expressions of approval or disapproval (Masclet et al. (2003)). However, most attention in the literature has been paid to the effectiveness of peer-to-peer punishments.

Yamagishi (1986), Ostrom et al. (1992), and Fehr and Gächter (2000) were the first to use economic experiments to analyze whether efficiency in multi-agent social dilemma situations increases if subjects are given the opportunity to impose sanctions on their peers. All studies find that the threat of sanctions is indeed very effective as the resulting level of efficiency in the social dilemma situation is very close to the socially optimal level. This is surprising as game theory predicts that rational self-interested agents would never engage in punishing their peers. The reason is that these experiments are set up such that imposing punishments is not only costly to the subject receiving the sanction, but also to the subject imposing it. That means that punishing is tantamount to providing a second-order public good. If the punishment is effective in changing the recipient's behavior, the benefits accrue to all group members whereas the costs are borne by the punisher. And if, on top of that, the game is finitely repeated, backward induction makes it even less likely that punishments will be imposed. Punishing non-cooperative behavior in the last period is a costly investment with zero future payoffs, hence punishing is not rational in any of the preceding periods either.

So, contrary to these game-theoretic predictions, Yamagishi (1986), Ostrom et al. (1992), and Fehr and Gächter (2000) observe that subjects frequently impose sanctions on those fellow group members that act non-cooperatively in the social dilemma situation.[3] But in real life there may be yet another reason not to impose punishments on one's peers, and that is that one makes oneself vulnerable to counterpunishment (or retaliation). This has been explored by Nikiforakis (2008), who added a second punishment stage to Fehr and Gächter's (2000) experiment. Those who get punished in the first punishment stage are

[3] Note that in these experiments subjects are free to impose sanctions on any of their peers. Conditional on being willing to impose sanctions it seems most logical to punish those who free-ride on the cooperative efforts of the others. That this is not necessarily the case is shown by Herrmann et al. (2008) who find that in some countries and cultures not only free-riders are sanctioned, but also sometimes those who contribute most to the public good.

given the opportunity to counterpunish in the second. The result is that the efficiency gains of punishments vanish: The threat of counterpunishment takes away the willingness to punish in the first stage, and hence cooperation unravels (see also Denant-Boemont et al. (2007)).

Peer-to-peer sanctioning is the most well-studied decentralized enforcement instrument in social dilemma experiments, but it is not the only one. Peer-to-peer rewards have been analyzed too; see for example Sefton et al. (2007) and Vyrastekova and van Soest (2008). These two studies use different impact ratios of rewards, and come to opposite conclusions regarding the efficiency consequences of rewards. Whereas Sefton et al. impose that it costs 1 experimental currency unit (ECU) to increase the payoffs by the reward recipient by 1 ECU (i.e., a 1:1 ratio), Vyrastekova and van Soest use a ratio of costs of 1 unit to increase the recipient's payoffs by 3 (i.e., a 1:3 ratio). The difference between 1:1 and 1:3 is crucial as an exchange of reward tokens between two subjects makes both better off in the 1:3 parameterization (as each subject's net gains are 2 points) whereas their payoffs remain unchanged in case of the 1:1 parameterization.[4]

Indeed, when using the 1:3 impact ratio, Vyrastekova and van Soest observe that efficiency in the social dilemma situation increases if subjects are given the opportunity to give rewards to their peers (whereas Sefton et al. do not observe any efficiency gain).[5] But even with a 1:3 impact ratio the improvement is less substantial than is typically the case with the single stage punishment mechanism. The natural question is then whether adding a second reward stage would result in efficiency being even closer to the social optimum, or not. At first sight, it seems that efficiency can only be improved taking as given that subjects are willing to incur costs to reward their peers, the benefits of acting cooperatively are increased.

In this chapter we examine whether the opportunity to 'counterreward' one's peers indeed increases efficiency in the social dilemma situation. We take the experiment by Vyrastekova and van Soest (2008) as a starting point and add a second reward stage. We follow the literature by using a Partner treatment

[4]Note that transfer rewards (1:1) are not necessarily more realistic or common than 'net-positive' rewards (1:3). A ratio greater than 1:1 can be defended by noting that in case of financial rewards marginal utility of income may differ between agents. But rewards may also take the form of helping each other (e.g., helping with the harvest, minding each others' children, etc.), and the opportunity cost of time may differ well between agents as well as over time (cf. Vyrastekova and van Soest (2008)).

[5]This finding that rewards are effective too is not only interesting from a scientific point of view. In most societies the right of coercion is restricted to the government, and hence peer-to-peer rewards may well be more relevant in real-world situations than peer-to-peer sanctioning.

where the subjects' identities are reshuffled between periods, so that reward cannot become a game in itself (see, Fehr and Gächter (2000), Nikiforakis (2008), Denant-Boemont et al. (2007), and Vyrastekova and van Soest (2008)).

Note that by adding a reward stage to the experiment of Vyrastekova and van Soest (2008), we actually introduce two changes even though the second reward stage is identical to the single reward stage in that earlier study. One change is that adding a second reward stage doubles the maximum number of rewards that can be given. That means that potential free-riders in the social dilemma situation face a larger carrot to act cooperatively. And from earlier work by van Soest and Vyrastekova (2004) we know that larger rewards tend to improve cooperation. But there is also a second change, which is relevant if subjects seek to establish mutually profitable bilateral exchange relationships. In treatments with just one reward stage, the only way in which subjects can signal their intention to cooperate is by behaving cooperatively in the social dilemma situation. With two reward stages, the first reward stage can be also used to signal one's willingness to engage in a bilateral exchange of reward tokens. That means that the relationship between behavior in the social dilemma and the reward activity becomes less important, or is even severed.

This chapter's contribution is twofold. The first is of direct relevance from an environmental policy point of view. Whereas Vyrastekova and van Soest (2008) find that efficiency in the social dilemma situation is greater than Nash with a single stage of rewards, we find that adding a second reward stage results in efficiency falling back to its Nash level. This is in spite of the fact that we also find that the opportunity for reciprocity in reward increases the subjects' propensity to exchange rewards. Taking into account the two changes adding a second reward stage gives rise to, the second dominates the first.

The second contribution is methodological in nature. We find that adding a second reward stage substantially increases insight into the social orientation of our subjects, allowing the experimenter to classify subjects into individuals holding pro-social preferences, strategic money maximizers, and 'homo economicus'. Identifying behavioral patterns is important if the literature takes experimental play seriously, and tries to come up with utility functions that better capture actual behavior in the lab. Several methods are available, including Fischbacher et al. (2001)'s strategy method and the decomposed games approach as developed by social scientists (Liebrand (1984)). Problems with these approaches are that (i) they do not capture reciprocal preferences in an interactive setting, (ii) play in these games may contaminate play in the actual experiment, and (iii) their predictive power is not always very strong. For example, van Soest and Vyrastekova (2006) use the decomposed game approach to analyze subjects' behavior in two treatments, one with 1:3 sanctions and one with 1:1 rewards. Whereas they find that the observed differences in behavior of the various behavioral types are in concordance with the theoretical predictions, they are in many instances not significantly different.

Indeed, we find that adding a second reward stage induces pro-social individuals reveal to themselves by behaving very differently from the subjects whose behavior is best described as strategic money maximizers or purely self-interested individuals. Indeed, we find that the extra reward stage induces subjects to behave much more consistently than in Vyrastekova and van Soest (2008). It is an interesting avenue for future research to consider whether adding extra stages to other games also allows researchers to classify their subject pool into the various behavioral types, thus enabling theorists to come up with models that better capture actual behavior.

The set-up of this chapter is as follows. Section 4.2 presents the game, the game theoretical predictions, and the experimental design. The impact of the second reward stage is analyzed in Section 4.3. In Section 4.4 we take a closer look at the impact of having a second reward stage on individual behavior, on the basis of which we can classify our subject pool into cooperative individuals, strategic money maximizers and 'homo economicus'. Finally, a conclusion is provided in Section 4.5.

4.2 The game and its experimental design

The social dilemma game that is studied in this chapter is the common pool resource (CPR) game. In the following subsection we present the game as well as our game theoretic predictions, and in the second subsection we introduce our experimental design.

4.2.1 The game

In the CPR game, each of the N agents is endowed with a fixed amount of effort e which she can allocate to common pool resource extraction, or to an alternative economic activity. The amount of effort agent i allocates to extraction is denoted by x_i, and hence $e - x_i$ is the amount of effort she allocates to the alternative activity. The game is finitely repeated over T periods. Using $s1$ to denote stage 1, agent i's total payoffs in period t are given by the following equation:

$$\pi_{i,t}^{s1} = w(e - x_{i,t}) + \frac{x_{i,t}}{X_t}\left[AX_t - BX_t^2\right], \qquad (4.1)$$

where X_t denotes the aggregate amount of effort put in by the N agents ($X_t = \sum_{i=1}^{N} x_{i,t}$). The marginal returns from the alternative activity are constant and equal to w, and hence, the profits generated by that activity are equal to $w(e - x_i)$. The returns from the extraction activity depend on the amount of effort subject i allocates to this activity ($x_{i,t}$), but also on the total amount of effort put in by the $N-1$ other agents ($X_{-i,t} = \sum_{j \neq i} x_{j,t}$). The total yield of the resource is equal to $AX_t - BX_t^2$, and agent i receives a share of the resource's yield equal to her share in aggregate extraction effort ($x_{i,t}/X_t$). The symmetric individual Nash equilibrium extraction effort level is $x_{i,t}^{NE} = (A-w)/B(N+1)$, while the socially optimal individual extraction effort level is equal to $x_{i,t}^{SO} = (A-w)/2BN$. Since $x_{i,t}^{NE} > x_{i,t}^{SO}$ if $N > 1$, there is a social dilemma. Given that this game is finitely repeated, the standard game-theoretic prediction is that all agents choose the Nash equilibrium extraction effort, because of backward induction.

The above game is the baseline treatment, and will be referred to as game 0SR (i.e., the zero reward stage treatment). We implement two additional treatments, the one stage reward treatment (1SR) and the two stage reward treatment (2SR). The first stage ($s1$) in these two treatments is identical to the (first) stage of 0SR, but is then followed by either one or two reward stages (in 1SR and 2SR, respectively). Each reward stage is set up as follows. Each of the N agents receive z reward tokens which she can keep herself, or give to one or more of the other agents. Every token that the agent keeps, is worth 1 point. Every token that is sent to a fellow group member is worth 3 points to that group member. Agent i's payoffs in stage $s = \{s2, s3\}$ in period t are therefore given by:

$$\pi_{i,t}^{s} = z - \sum_{j \neq i} p_{ij,t}^{s} + 3 \sum_{j \neq i} p_{ji,t}^{s}, \tag{4.2}$$

where $p_{ij,t}^{s}$ is the number of reward tokens that agent i sends to agent j ($j \neq i$) in stage $s = \{s2, s3\}$ in period t. The total individual payoffs in one period of the 0SR, 1SR and 2SR treatments are therefore $\pi_{i,t}^{0SR} = \pi_{i,t}^{s1}$, $\pi_{i,t}^{1SR} = \pi_{i,t}^{s1} + \pi_{i,t}^{s2}$ and $\pi_{i,t}^{2SR} = \pi_{i,t}^{s1} + \pi_{i,t}^{s2} + \pi_{i,t}^{s3}$, respectively.

Assuming that subjects only care about their own payoffs and are able to apply backward induction, the game theoretic predictions regarding play in the 1SR and 2SR treatments are straightforward. Given that the game is played for a finite number of periods, giving a reward in the (last) reward stage of period T does not affect future behavior anymore. That means that rational, self-interested agents do not send reward tokens in the last reward stage ($s2$ in 1SR, and $s3$ in 2SR). That means that in 2SR there is also no reason to send any reward tokens in the first reward stage ($s2$) because the recipients have no incentives to reciprocate in the second reward stage ($s3$). And given that no rewards will be given in the last period, agents choose the Nash equilibrium amount of effort in the CPR stage ($s1$) in both 1SR or 2SR. In turn this implies that a positive use of rewards in the (last) reward stage of period $T-1$ (i.e., $s2$ in 1SR, and $s3$ in 2SR) does not influence behavior in stage 1 of period T. Continuing reasoning backward we deduce that no rewards are given in any of the T periods in either 1SR or 2SR, and cooperation unravels. So, in our finitely repeated game the standard game theoretic prediction is that no rewards are being sent in any period, and also that the amount of effort allocated to CPR extraction is always equal to the Nash equilibrium level.

The game theoretic predictions change when the standard assumptions of rational, self-interested agents are relaxed. In case other-regarding preferences play a role, reward tokens may be used. For example, individuals with reciprocal preferences may be present in the subject pool. And these reciprocal individuals are willing to incur costs to reward some group members for their cooperative behavior in $s1$ and $s2$, and to punish others for their noncooperative behavior by refusing to send them reward tokens. The effectiveness of rewards in improving efficiency in the CPR stage is likely to be greater in the 2SR treatment than in the 1SR treatment, as the benefits of cooperation are unambiguously larger in the latter treatment.

4.2.2 Experimental design

The experiments were conducted at Tilburg University. The sessions for the one reward stage institution (1SR) were held during the Spring semester of 2005. Here 40 subjects participated, forming 8 groups of 5 participants, that is, $N = 5$. The sessions for the two reward stages (2SR) took place during the Spring and Fall semester of 2007. In total 55 subjects participated, resulting in 11 groups. Each session lasted about two hours. All 95 participants were students at Tilburg University with different nationalities and different academic backgrounds (economics, law, management, social sciences), and were recruited via e-mail. Interaction between subjects was mediated via computers, and the games were programmed in z-Tree (Fischbacher (2007)). In each session, 15 or 20 subjects participated who were randomly assigned to computer terminals. The experimental parameterization is shown in Table 4.1, and the resulting Nash equilibrium and socially optimal levels of extraction effort and reward are shown in Table 4.2.

Every session started with the experimenter reading out aloud the instructions of the 0SR treatment (and students were invited to read along), next there were test questions, and then the 0SR treatment was implemented.[6] To facilitate the calculations of the profits in the common pool resource activity, all subjects were given a payoff table. No information was given about either the socially optimal or Nash equilibrium extraction effort levels.

Variable	Description	Value
N	number of individuals per group	5
T	number of periods of the stage game	15
w	return on investments in the alternative activity	0.5
A	parameter of the CPR's revenue function	11.5
B	parameter of the CPR's revenue function	0.15
e	individual endowment of effort	13
z	individual endowment of 'reward' tokens	12
r	value of reward tokens received	3

Table 4.1 Experiment parameterization.

Variable	Description	Value
x^*	symmetric individual socially optimal effort level	6
X^*	aggregate socially optimal effort level	30
x^{NE}	individual Nash equilibrium extraction effort	10
X^{NE}	aggregate Nash equilibrium extraction effort	50
$p_{ij}^{NE,s}$	Nash equilibrium number of reward tokens given in stage s	0

Table 4.2 Socially optimal and Nash equilibrium levels of all variables of the stage game.

At the beginning of the experiment subjects were randomly matched into groups of 5 participants. The group composition remained constant for the rest of the experiment. At the beginning of every new period an identity label was assigned to each group member, and this label remained constant throughout the three stages of that period. To prevent reward becoming a game in itself, we follow the literature (Fehr and Gächter (2000), Nikiforakis (2008), Denant-Boemont et al. (2007), and Vyrastekova and van Soest (2008)) by randomly reshuffling identity labels between periods.

Upon completion of 0SR, the instructions for the session's second (and last) treatment were distributed and read out. This second treatment was either 1SR or 2SR. In the 1SR treatment each subject received information about the stage

[6] The instructions can be found in Appendix 3.A.1.

1 extraction effort decisions of all other group members (and their payoffs), the number of reward tokens he/she received from each other group member, and the number of reward tokens that group members did not distribute. Because of screen size limitations, the subjects in 2SR received the same information except for the information on how many tokens each group member kept for him or herself.

In the sessions implementing the one reward stage institution participants earned on average €15.90 (including a showup fee of €5.–), whereas the subjects in the two reward stage sessions earned, on average, €18.11.

4.3 The efficiency consequences of allowing for reciprocity in rewards

Let us first have a look at the aggregate amount of effort put into CPR extraction, averaged over all groups; see Figure 4.1. The average aggregate extraction effort levels in the 0SR treatments of all sessions are shown in periods 1 to 15, whereas the average aggregate extraction effort levels in the two reward treatments are shown in periods 16-30.

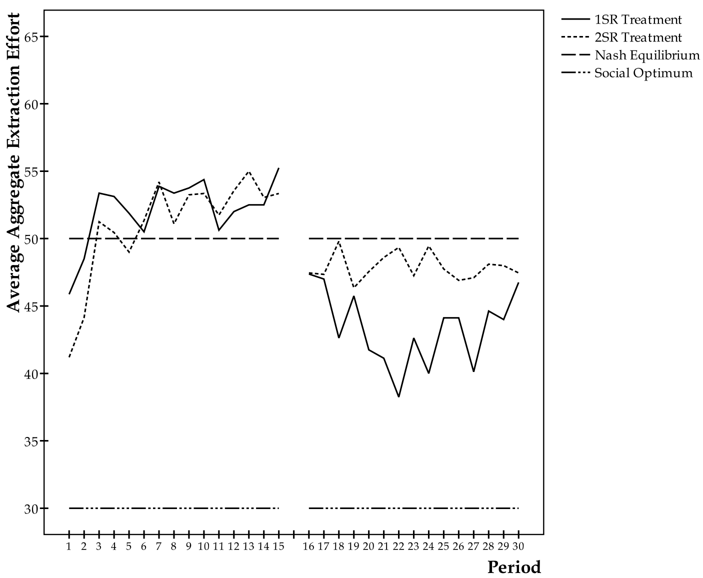


Figure 4.1 Average aggregate extraction effort levels in the first stage.

In the 0SR treatment, presented in periods 1 to 15, group extraction effort starts on average at a level in between the socially optimal level (30) and the Nash equilibrium level (50), but increases rapidly in the early periods. There is no difference between the two subject pools with respect to their play in 0SR as the relevant two-sided Mann-Whitney U test (with 8 and 11 groups in the 1SR and 2SR treatments) yields a p-value of 0.395.

The impact of introducing either one or two reward stages are markedly different, though; see periods 16-30. Whereas the instantaneous reduction in average aggregate extraction effort levels in the CPR stage is identical in 1SR and 2SR, play evolves very differently in the two treatments. Indeed, the single reward stage reduces average aggregate extraction effort levels significantly as compared to the 0SR treatment (as the two-sided Wilcoxon matched pairs test with 8 groups yields a p-value of 0.01), but the two stage reward treatment does not (11 groups, $p = 0.21$).

Result 1 Compared with 0SR, adding one reward stage significantly reduces the aggregate amount of effort put into CPR extraction, but adding two reward stages does not.

Next, we turn to analyzing reward behavior. The average individual reward effort is presented in Figure 4.2.

The first observation is that, on average, the number of reward tokens sent (and received) is lower in the 1SR treatment than in either of the two stages in 2SR. Whereas this difference is not significant for the last stage of the two treatments ($s2$ in 1SR and $s3$ in 2SR; $p = 0.35$), it is significantly higher in $s2$ in 2SR than in either $s3$ in 2SR or $s2$ in 1SR (with p-values of 0.01 and 0.02, respectively).

Result 2 On average, the total number of reward tokens sent is greater in 2SR than in 1SR.

As stated in the introduction, adding a second reward stage implies that the 2SR treatment differs from the 1SR treatment in two respects. In the first place the maximum number of reward tokens that can be exchanged is twice as high in 2SR, and hence subjects can offer their peers a larger carrot to sustain cooperative behavior in the CPR stage. A payoff-equivalent treatment would be one with a single reward stage, but with an impact ratio of 2:6. And from earlier work by van Soest and Vyrastekova (2004) we know that increasing the net profitability of rewards results in behavior in the social dilemma situation becoming more cooperative.

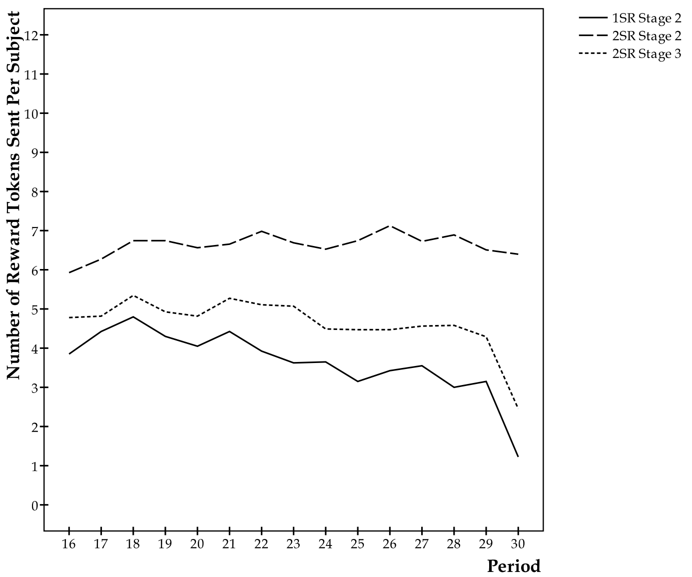


Figure 4.2 Average individual number of reward tokens sent in stage 2 and stage 3 in 1SR and 2SR.

The second change is that the presence of a second reward stage allows subjects to signal their trustworthiness not only in their behavior in the CPR stage, but also in the first reward stage. In both the 1SR and 2SR there are two motivations for choosing lower extraction effort levels in the first stage. By putting relatively little effort into extraction one is more eligible for receiving rewards, but one also signals one's willingness to act cooperatively. Hence the odds for other subjects sending a reward to receive reward tokens in return, are also larger. Clearly in the 1SR treatment the only way one can signal one's willingness to engage in a bilateral exchange of reward tokens is by acting cooperatively in the extraction stage ($s1$). But in the 2SR treatment one can signal one's trustworthiness in the CPR stage, but also by one's behavior in the first reward stage. That means that the link between acting cooperatively and the reward activity is weaker in 2SR than in 1SR.

Given that we find that efficiency in the social dilemma situation is at the Nash level in the 2SR treatment, clearly the second effect dominates the first. This is evidenced by the fact that the number of rewards exchanged is identical in the last reward stage in both treatments ($s3$ in 2SR and in $s2$ in 1SR), whereas it is much greater in the 2SR's first reward stage ($s2$). Hence, the link between behavior in the CPR stage and the last reward stage becomes less strong, and cooperation in the social dilemma unravels.

Comparing the effectiveness of rewards and sanctions in one or two stage settings, the verdict on the effectiveness of sanctions versus rewards in sustaining cooperation seems to lean towards sanctions. The two instruments are completely symmetric in the sense that just having one decentralized enforcement stage (sanctions, or rewards) proves to be efficiency increasing, but adding a second enforcement stage makes this efficiency gain disappear.

As argued in the introduction, in principle rewards are likely to occur more often in real life situations than sanctions, if only because the right of coercion usually lies with the government. Rewards can always be given,[7] but that also means that reciprocity in rewards cannot be ruled out either. There is no natural mechanism that prevents people from engaging in 'rewarding' and 'counterrewarding'. It may be less likely that an individual will seek out another individual who acts non-cooperatively in social dilemma situations to establish a mutually beneficial exchange of good and services with, but if the other makes the first gesture, the exchange relationship is established after all.

Whereas peer-to-peer sanctions may not occur very frequently in the real world (although there are quite a few case studies showing that they are being used in practice, see for example Taylor (1987) and Cordell and McKean (1992)), they may prove to be effective if they do. Individuals who are punished in a community may sometimes have the opportunity to retaliate, but clearly there are many situations in which this is not really possible, if only because it is natural for punishers to form a collective to prevent retaliation in these matters. It is less likely that the punished, at least if the punishment was justified, are able to do so too.

Peer-to-peer sanctions may occur in the real world in the form of 'one-stage' and 'two-stage' situations, but peer-to-peer rewards are likely appear in the form of 'two-stage' rewards only. As such, the prospects for peer-to-peer rewards being an effective decentralized enforcement mechanism are bleak.

4.4 Measuring social preferences

One of the most important challenges economists are nowadays confronted with is to improve the predictive power of their models. The assumption of humans being exclusively interested in maximizing their own material welfare predicts well when analyzing behavior in the market place, but not in situations where

[7]Keeping in mind the crucial differences between rewards and bribes, as the latter always imply a violation of property rights whereas the former does not.

cooperation is called for, as is the case in social dilemma situations. Experimental economics can help create data about human behavior from which better specifications of utility functions for various behavioral types (conditional cooperators, inequity averse individuals, strategic money maximizers, etc) can be derived. Various methods have been developed including the strategy method of Fischbacher et al. (2001) and the decomposed game technique developed by social psychologists (Liebrand (1984), see also Offerman et al. (1996)).

The attempt by van Soest and Vyrastekova (2006) to predict behavior in a social dilemma situation using the Decomposed Game approach is of particular interest here, as the actual experiment used is the 1SR treatment (albeit with impact factor 1:1; i.e., where rewards are effectively transfers). The Decomposed Game approach consists of 24 independent decision situations, which have actual financial consequences for the subject who makes the decision, as well as for one other (anonymous) participant the subject is matched with in the experimental session. The decision situations present dilemmas because they consist of choices between a payoff combination with large benefits for the decision maker and small benefits to the other participant, and a payoff combination where the sum of benefits is larger but with smaller private benefits to the decision maker. By choosing between the two payoff allocations in each decision situation, the decision maker has to weigh his/her own payoff gains/losses against those of the other, anonymous, participant. This approach allows the experimenter to label subjects individualistic (if they maximize their own final payoff), competitive (if they end up with a positive number of points for themselves and a negative one for the other participant), or cooperative (if they end up with both a positive number of points for themselves as well as for the other participant).

The predictive power of this test turned out to be very low in the experiment under consideration. When using the classifications thus derived to explain behavior in the CPR stage, van Soest and Vyrastekova find that the differences in behavior have the expected signs, but are generally not significant. Clearly, behavior in the 1SR treatment is too complex to be captured by a simple classification obtained via a game like the Decomposed Game approach. One of the main reasons is of course that the approach does not allow for reciprocity as all subjects make their decisions simultaneously, and, to avoid contamination, only learn the payoff consequences after the main experiment has been implemented. The strategy method of Fischbacher et al. (2001) is less susceptible to this criticism, but the fact that subjects have to answer multiple 'what if' questions tends to make them act more strategic then they would do in a situation with 'hot' interaction.

Interestingly enough, we find that our 2SR treatment may provide an alternative method for eliciting social preferences. When comparing behavior in 2SR with that in 1SR, we find that whereas it is really difficult to classify our subject pool into various behavioral types (cf. van Soest and Vyrastekova (2006)), behavior in the 2SR treatments allows for a very simple and straightforward classification of subjects. Let us first take a look at the aggregate CPR data.

When comparing CPR extraction behavior in 1SR and 2SR in Figure 4.1, the first thing to note is that behavior is much more variable in the former than in the latter. On average, the standard deviation in a group in the 1SR treatment is 1.35. The average standard deviation in the 2SR treatment is 0.64. This observation is supported by the relevant two-sided Mann-Whitney U test on differences in the standard deviations between the two treatments ($N_1 = 8, N_2 = 11$), which yields a p-value of less than 0.01.

Result 3 The variance in extraction behavior is significantly higher in treatment 1SR than in treatment 2SR.

One hypothesis explaining why behavior is much more constant in 2SR than in 1SR is that subjects try to overcome the fact that reshuffling identities between periods prevents them building a reputation as being trustworthy in exchanging reward tokens. By choosing the same extraction effort levels in a series of periods, subjects signal their identity to their fellow group members.

Let us check whether indeed subjects behave in such a way. We do so in two steps. First we check subjects' persistency in the amount of effort put into CPR extraction in the three treatments (0SR, 1SR, and 2SR). And then we check whether the difference in persistency between 1SR and 2SR give rise to differences in reward behavior.

Figure 4.3 presents the share of subjects who choose a specific level of extraction effort for eight periods, or more. The figure shows a striking difference between the three treatments. In the 0SR treatment, about 27% of the subjects put in the same amount of effort into CPR extraction in eight periods or more, whereas these percentages are 55% in the 1SR treatment and 64% in the 2SR treatment. The differences between the treatments become even more transparent when the cut-off point is set at twelve periods or more. In the 0SR treatment, 7% of the subjects choose a specific level of extraction effort for twelve periods or more. In the 1SR treatment, the percentage of subjects is 13% but it is 49% in the 2SR treatment.
Result 4 restates these findings:

Result 4 Almost 50% of the subjects in the 2SR treatment extract the same

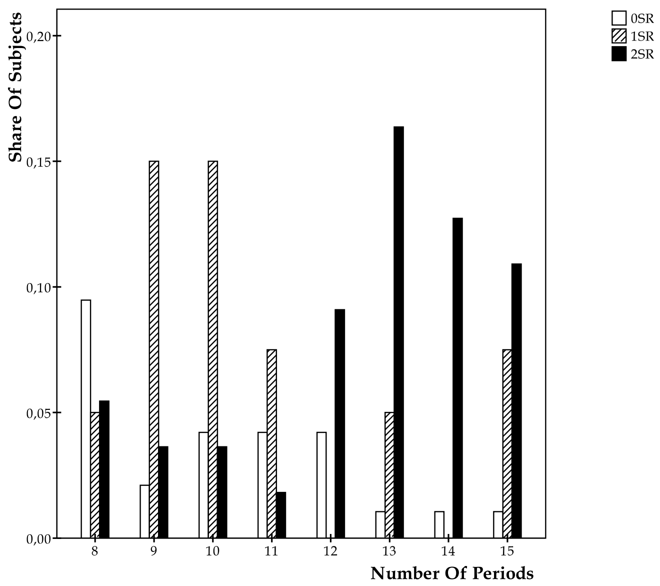


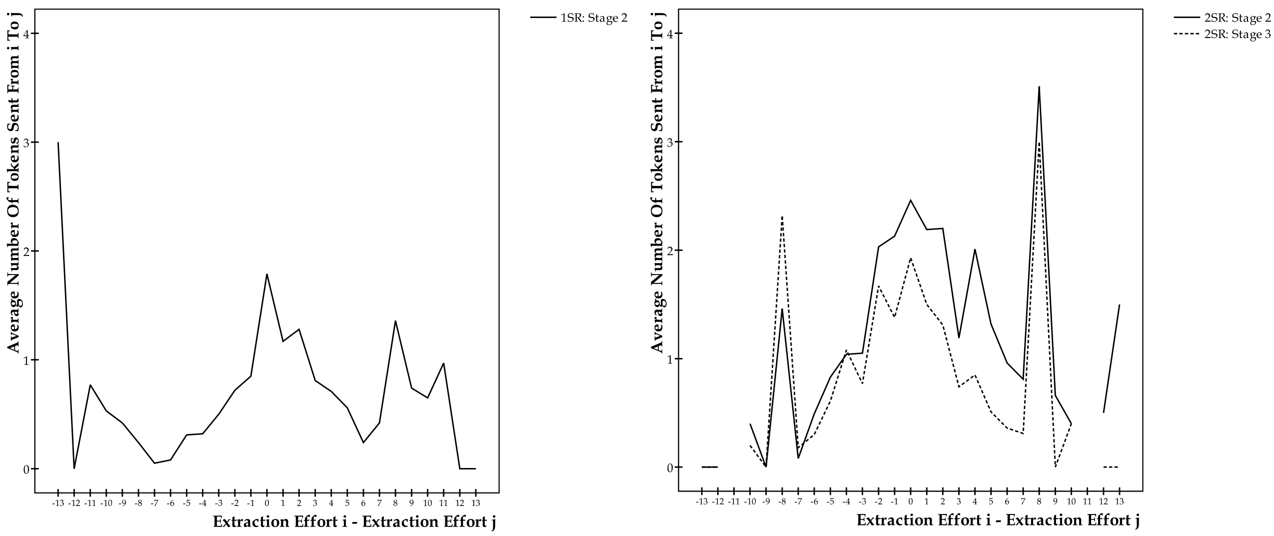
Figure 4.3 Share of subjects who keep their stage 1 extraction effort levels constant in the three treatments for 8-15 periods.

number of stage 1 tokens for twelve periods or more. In contrast, less than 15% display such persistency in extraction effort in the 0SR or 1SR treatment.

So, we find that subjects are more persistent in their choice of the amount of extraction effort chosen in stage 1 in the 2SR treatment than in the 1SR treatment. Let us now have a look at whether this difference in persistency gives rise to differences in reward behavior.

Here we have to distinguish between the two motivations for sending reward tokens. The first is that subjects use rewards as 'intended', that is to induce fellow group members to choose lower levels of extraction effort in the social dilemma situation. The second is that subjects try to establish a mutually beneficial exchange of reward tokens with fellow group members.

In Figures 4.4(a) and 4.4(b) we plot the number of reward tokens given by sender i to recipient j as a function of the difference in extraction effort put in by sender i and recipient j ($x_i - x_j$). If rewards would be used predominantly to mitigate payoff inequalities resulting from differences in stage 1 behavior, one would expect the lines both panels of Figure 4.4 to be upward-sloping. If the main aim is to establish a mutually beneficial exchange of reward tokens, one would expect the number of tokens exchanged to be an increasing function of $x_i - x_j$ in the range where $x_i < x_j$, and a decreasing function of $x_i - x_j$ for all levels where $x_i > x_j$.



(a) (b)

Figure 4.4 Average number of reward tokens sent by subject i to subject j, as a function of the difference in extraction effort $(x_i - x_j)$, for (a) the 1SR treatment, and (b) the 2SR treatment.

Clearly, straightforward rewarding cooperative extraction behavior is not the most important motivation behind sending reward tokens in either 1SR or 2SR. Subjects tend to send more reward tokens to those subjects who choose the same extraction effort level in the first stage as they do themselves. Although both treatments show peaks at differences other than zero too, these are the result of just a few decisions in case of 1SR but are much more persistent in case of 2SR. For example, the peak at -13 in 1SR is the result of one subject's decision to choose effort level 0 and send 3 reward tokens to a fellow group member who chose effort level 13. Obviously, this was just a one-time decision. But in 2SR it happens fairly frequently that subjects choosing different levels of extraction effort establish an exchange relationship. For instance, the peaks at $+8$ and -8 are the result of two subjects, one choosing effort level 13 and the other choosing level 5, sending reward tokens to each other for 12 periods. Obviously, this is one reason why efficiency in 2SR is essentially at the Nash equilibrium. The link between CPR extraction and the exchange of reward tokens is driven more by the desire to establish mutually profitable bilateral exchange relationships than to influence CPR extraction behavior itself.

So we find that a substantial share of our subjects choose the same level of extraction effort in 12 periods, or more, and also that there is some evidence that the persistence in extraction behavior facilitates the exchange of rewards.

Having established that a substantial share of our subjects tries to signal their identity, the next question is to whether these 'signalers' behave more cooperatively than 'non-signalers', or not. In the current setting cooperative behavior consists of three actions: (i) choosing lower levels of extraction effort than average, (ii) being willing to forego profitable exchange relationships with subjects that free-ride in the CPR stage (even though it may be privately profitable to do so), and (iii) positively reciprocate to the number of reward tokens received in stage 2 by sending back rewards in stage 3 (even though identity labels are re-shuffled upon completion of stage 3).

To test whether this is the case, let us first define 'signalers' as follows:

Definition 1 A 'signaler' is a subject who chooses the same stage 1 extraction effort level for twelve periods or more.[8]

This definition results in 27 subjects (of the 55 in the subject pool) to be classified as a signaler, and hence 28 as being non-signalers. Now let us have a look at the first aspect of cooperative behavior: what extraction effort levels signalers and non-signalers tend to choose. We do so in Figure 4.5(a) and 4.5(b) respectively.

In Figure 4.5(a) we see that of the signalers nine subjects choose extraction effort 13 for twelve periods or more, whereas the other 18 subjects choose levels of 10 and lower. That means that two-thirds of the signalers tend to behave cooperatively in the CPR stage, but one-third does not.

Comparing 4.5(a) and 4.5(b), it is evident that signalers, on average, tend to behave more cooperatively in the social dilemma situation than non-signalers. On average, the extraction effort level chosen by signalers is below Nash (even when including the nine subjects choosing 13), and hence the average effort level chosen by non-signalers is greater. Support for this claim is given by a Student t-test, taking the average extraction of an individual over all periods as an independent observation. The test compares whether the extractions are different from ten tokens, the symmetric Nash equilibrium. Taking all signalers into account, the average extraction level is 9.03, which is statistically different

[8]The cut-off point of twelve periods is arbitrary. However, the results that follow are more or less the same as when a cut-off point of eleven or thirteen periods is chosen.

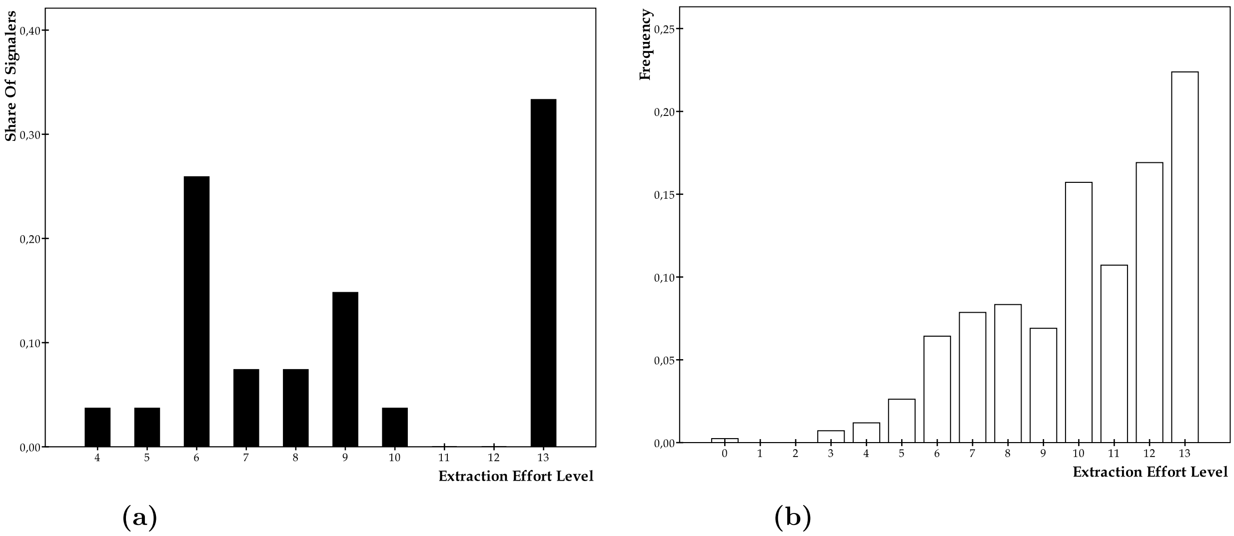


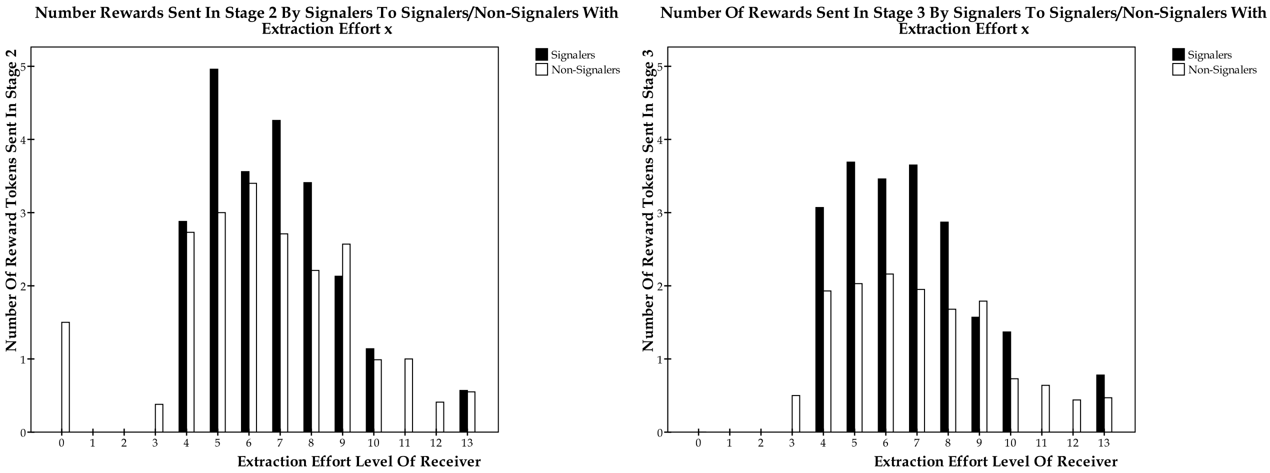
Figure 4.5 (a) The distribution of signalers over the various extraction effort levels. (b) Frequency with which the extraction effort levels are chosen by the non-signalers.

from ten tokens ($N = 27, p = 0.096$). When the signalers with an extraction level of 13 are taken out, the average extraction level is 7.22, and is statistically different from 10 ($N = 18, p < 0.01$). The average extraction level of non-signalers is 10.11. A Student t-test shows no statistical difference with the symmetric Nash equilibrium ($N = 28, p = 0.778$).

Result 5 On average, the extraction effort level chosen by signalers is below Nash, and the average effort level chosen by non-signalers is greater.

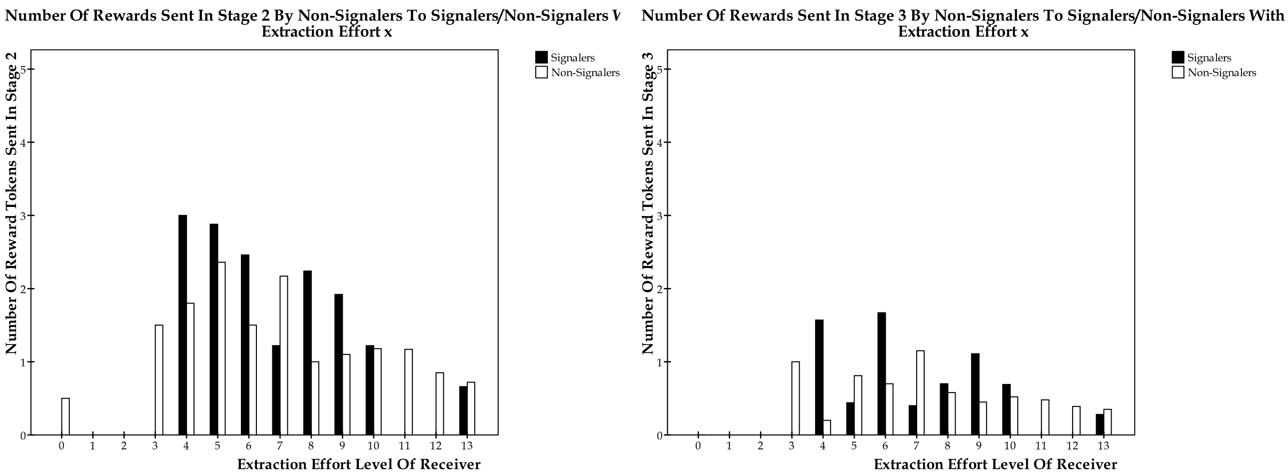
Next, let us have a look at the second aspect of 'cooperative behavior', and that is whether subjects are willing to forego a profitable exchange of reward tokens with subjects who free-ride in the social dilemma stage. We make a distinction between the number of reward tokens sent by signalers and non-signalers; see Figures 4.6 and 4.7.

These four panels are revealing. First, there is a clear downward sloping pattern in the number of reward tokens signalers send to other signalers and non-signalers in stage 2 as well as in stage 3, with the number of tokens falling the more effort the recipient puts into CPR extraction. Non-signalers, however, display such behavior in stage 2 when sending reward tokens to signalers, but



(a) **(b)**

Figure 4.6 Rewards sent by signalers in: (a) stage 2 (b) stage 3.



(a) **(b)**

Figure 4.7 Rewards sent by non-signalers in: (a) stage 2 (b) stage 3.

less so when sending rewards to non-signalers (remember that the bulk of the extraction effort chosen by non-signalers is 10 or higher). And in stage 3 there is hardly any detectable pattern. Therefore, signalers tend to be more willing to provide 'the second-order public good', i.e. to forego private mutually beneficial exchange of reward tokens with free-riders in the CPR stage. And these conclusions are largely confirmed when calculating the Spearman correlation coefficients between rewards and stage 1 extraction effort of the receiver; see Table 4.3.[9]

	Stage 2	Stage 3	Stage 2	Stage 3
	Signalers to:		Non-Signalers to:	
Signalers	−0.46	−0.37	−0.47	−0.21
Non-Signalers	−0.48	−0.31	−0.20	−0.10

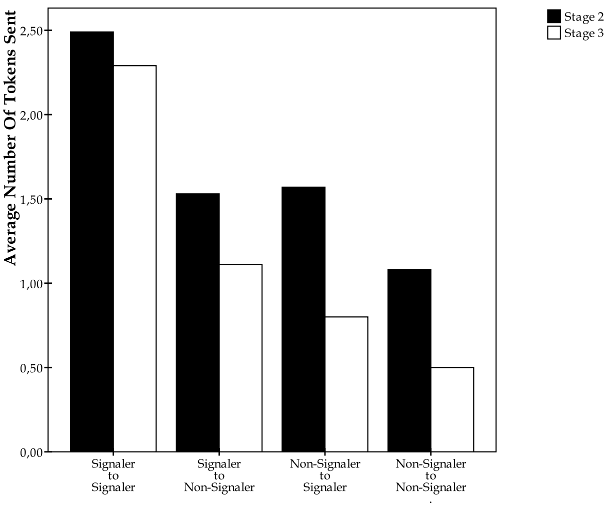
Table 4.3 Spearman's correlation coefficient of relation between sending rewards and stage 1 extraction effort levels of the receiver. All coefficients are significant at the 1% level.

Finally, let us have a look at the third element of cooperation, and that is how 'trustworthy' signalers and non-signalers are in positively reciprocating in stage 3 to the number of rewards received in stage 2. Figure 4.8 shows information on trustworthiness.

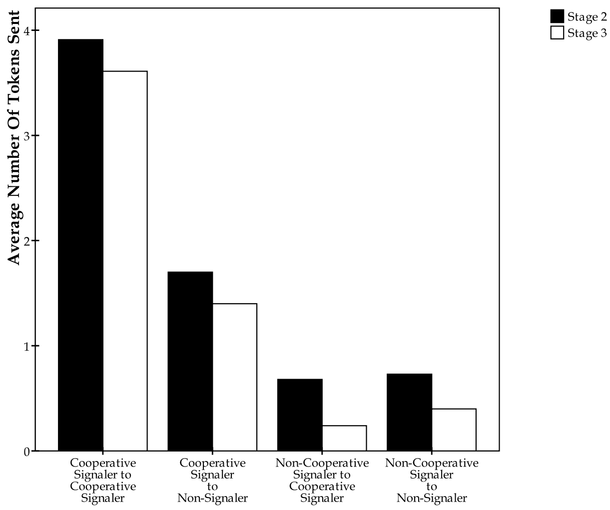
Again, these panels are revealing. Figure 4.8(a) shows the average number of reward tokens sent by signalers and non-signalers, distinguishing between the recipients being signalers and non-signalers, in the two stages. Both signalers and non-signalers send more tokens to signalers than to non-signalers, albeit that signalers tend to send more. More interesting though is the drop in the number of tokens sent from stage 3 as compared to stage 2. Whereas the number sent in stage 3 is about half of the number sent in stage 2 in case of non-signalers, the drop is much less pronounced in the case of signalers. Here it is important to remember too that our definition of signalers includes not only those subjects that persistently chose a relatively low extraction effort level (18 of the 27 signalers), but also those subjects that chose effort level 13 for twelve periods or more (the other 9 signalers). We find a crucial difference between the two types as the majority of the non-cooperative signalers send, on average, no tokens. When taking out these 9 subjects, the first four columns of Figure 4.8(a) change dramatically; see Figure 4.8(b).[10] If we only focus on those signalers that put in 10 tokens or less, we find that they act very cooperatively in the reward stage too. They tend to send an equal number of reward tokens to both signalers and non-signalers in stage 2 and stage 3.

[9] To compute the correlation coefficients, each reward decision is taken as independent, yielding 3,300 observations in total.

[10] From the non-cooperative signalers, one signaler does exchange reward tokens with another signaler (a finding we have already reported in Figure 4.4(b)). This subject is omitted from Figure 4.8(b).



(a)



(b)

Figure 4.8 **(a)** The average amount of stage 2 and stage 3 tokens that are sent between subjects of the two types, averaged over all subjects and all periods. **(b)** The average amount of stage 2 and stage 3 tokens that are sent by cooperative signalers (with $x_i \leq 10$) and non-cooperative signalers (with $x_i = 13$) to cooperative signalers and non-signalers.

Combined, Figures 4.5-4.8 suggest that indeed just adding the second reward stage allows for a full classification of our subject pool into pro-social individuals, strategic money maximizers, and individuals who behave in line with the assumption of 'homo economicus'.

The 18 subjects (= 1/3 of the pool) who are signalers and put in maximally 10 units of effort into CPR extraction are pro-social individuals. They put in less extraction effort than the average subject, they tend to send fewer reward tokens to fellow group members who put more effort into extraction, and they tend to give about the same number of tokens in stage 3 as they did in stage 2.

The 28 subjects (= 1/2 of the pool) who were labeled as non-signalers are strategic money maximizers. They adjust their extraction effort level in stage 1 to maximize their profits, they tend to send their reward tokens to the more cooperative signalers (that is, the ones with the lower extraction effort levels) because they bank on receiving reward tokens in stage 3, while they themselves tend not to send any reward tokens in stage 3.

And the remaining 9 signalers consistently choosing extraction effort level 13 (= 1/6 of our subject pool) are best labeled as 'homo economicus'. Given the lower extraction effort levels chosen by the other signalers level 13 is often the best-response level, and they do not really engage in exchanging reward tokens at all.

Having seen the evidence that some subjects try act pro-socially, the question arises what signals their trustworthiness best; is it their behavior in stage 1, stage 2, or stage 3? For the remainder of the analysis, the following definition of a successful bilateral relationship is used:

Definition 2 A 'connection' is a bilateral relationship between two subjects, such that both subjects reward each other in both stage 2 and stage 3 for eleven periods or more.[11]

It is expected that mainly signalers form connections with other signalers. Moreover, it is not clear that subjects who have a connection with a non-signaler are aware of this, since they have no way of distinguishing non-signalers. Table 4.4 shows the ratio of actual connections compared to the total number of connections that are possible between the subject types.

	Possible Connections	Actual Connections	Ratio
signaler with signaler	26	13	0.50
signaler with Non-signaler	56	6	0.11
Non-signaler with Non-signaler	28	2	0.07

Table 4.4 Existence of connections between the subject types.

The table reveals that half of the signalers are active in a connection with other signalers. Furthermore, the results show that a small fraction of non-signalers is active in a connection.

To find out what determines the creation of a connection, a natural candidate seems to be extraction and reward effort in the first period. Table 4.5 gives the average number of reward tokens that a subject has sent and received in period 16 and period 17 to 30. The table divides subjects who are in a connection and subjects who are not. The table shows that subjects who are in a connection give and receive substantially more reward tokens than subjects who are not in a connection. This finding is not very surprising, since the purpose of establish a connection is to exchange reward tokens. What is more surprising is that the differences in the number of reward tokens exchanged within connections and outside connections are already visible in the first period. This finding suggests that the first period is crucial for the development of connections.

[11] Again, the cut-off point of eleven periods seems rather arbitrary. However, robustness checks are performed which have indicated that the main results do not change much.

	Period 16				Period 17–30			
	p^2_{ij}	p^3_{ij}	p^2_{ji}	p^3_{ji}	p^2_{ij}	p^3_{ij}	p^2_{ji}	p^3_{ji}
Connection	8.4	7.7	8.1	7.4	10.0	8.5	9.8	7.7
Non-Connection	3.9	2.3	4.1	2.6	3.9	1.4	4.1	2.0

Table 4.5 Average rewards sent and received for subjects in a connection and for subjects not in a connection.

So what is the key signal on the basis of which subjects decide that they are trustworthy, so that they decide to form a connection? Is it extraction effort, is it stage 2 reward behavior or stage 3 reward behavior? We test this by estimating the following Probit model:

$$\begin{aligned} y^*_{ij} &= \beta_0 + \beta_1 I(i \text{ and } j \text{ are signalers}) + \beta_2 \left(\frac{x_{i,t=16} + x_{j,t=16}}{2} \right) + \\ &\quad \beta_3 (p^2_{ij,t=16} + p^2_{ji,t=16}) + \beta_4 (p^3_{ij,t=16} + p^3_{ji,t=16}) + \varepsilon_{ij}, \end{aligned} \tag{4.3}$$

where

$$y_{ij} = \begin{cases} 1, & \text{if } y^*_{ij} > 0; \\ 0, & \text{otherwise.} \end{cases}$$

In the model, y_{ij} is a dummy variable indicating whether a connection starts in period 2, or not. That means, it has a value of 1 when the connection is established in period 2 and zero otherwise (with the two subjects giving each other tokens in both stage 2 and stage 3 for ten periods or more). The variable $I(i \text{ and } j \text{ are signalers})$ is a dummy variable which has value 1 if the subjects in the connection are signalers, and 0 otherwise. All the other variables are self-explanatory except for the variable $0.5(x_{i,t=16} + x_{j,t=16})$. It is expected that the initial choice of stage 1 extraction effort levels heavily influences the reward effort in stage 2 and 3 of period 16, and hence affects the dependent variable directly, or indirectly by affecting stage 2 and stage 3 rewards. An indirect effect of stage 1 on the dependent variable is however not a problem. This will become a problem when stage 1 simultaneously has a direct effect on the dependent variable. In order to test for this, a measure of the level of stage 1 extraction effort levels is included in the model. The average stage 1 extraction effort level of subject i and j is taken as a proxy.[12] Table 4.6 gives the estimation results of the model.

The regression results show that when two subjects are signalers in period

[12] In addition, a number of other measures for the impact of stage 1 are tested as robustness checks (results are not reported). For example, including the maximum and minimum of stage 1 of i and j, or adding the extraction effort levels of both subjects. More or less all the results are qualitatively the same.

Dependent variable: I(Connection i and j starts in period 2)	
I(i and j are signalers)	2.105^{***}
	(0.524)
$(p^2_{ij,t=16} + p^2_{ji,t=16})$	0.090
	(0.141)
$(p^3_{ij,t=16} + p^3_{ji,t=16})$	0.515^{***}
	(0.175)
$(x_{i,t=16} + x_{j,t=16})/2$	0.111
	(0.122)
Intercept	-4.075^{***}
	(1.439)
Pseudo-R^2	0.4887

Table 4.6 Probit regression results for the existence of a connection between subject i and subject j. In total, there are $11 \times 10 = 110$ possible connections.

16 to 30, they have a bigger probability of forming a connection. This result is intuitive, and supports the idea that subjects signal in order to bilaterally exchange reward tokens. Furthermore, the results indicate that the number of rewards sent in stage 2 of period 16 has no direct significant impact on the development of a connection in future periods. The effects of stage 3 rewards in period 1 are however significantly positive. The more reward tokens a subject sends to another subject in this stage, the bigger the probability that the two subjects will give each other tokens in future periods.

These findings can be interpreted as supporting the following mechanism. Two subjects, who reward each other heavily in stage 3 of the first period, have shown each other that they are trustworthy. Both subjects have an interest in maintaining each other's mutual trust, since receiving reward tokens is lucrative. The only way to recognize the group member with whom it is relatively safe to exchange reward tokens is by his or her stage 1 extraction effort levels, but the true test of trustworthiness is whether she defaults in stage 3, yes or no.

4.5 Conclusion

In the experimental economics literature, a substantial amount of research has been dedicated to exploring the effectiveness of 'decentralized' regulation mechanisms in sustaining cooperation in social dilemma situations. Decentralized mechanisms are those mechanisms where the agents involved try to solve the social dilemma situation themselves rather than by relying on some sort of centralized intervention from, say, a governmental regulatory body. Many situations where there are positive or negative externalities qualify as a social dilemma, and hence this literature is very relevant to the environmental economics profession. And if self-regulatory instruments are found to be effective in sustaining cooperation, their presence obviates the need for (expensive and sometimes cumbersome) government intervention.

In this chapter we address the issue whether peer-to-peer rewards can reduce aggregate extraction effort in a common pool resource game towards the socially optimal level. Past research has shown that peer-to-peer rewards can indeed improve efficiency in the social dilemma situation if the benefits of receiving a reward are larger than the costs of giving it; see Vyrastekova and van Soest (2008). However, the experiment in their paper was set up such that reward cannot become a game in itself; subject identifiers were reshuffled between periods, and there was just one opportunity to simultaneously exchange reward tokens in every period.

While the reshuffling of identities can well be defended on the basis of experimental practice, there are not many instances in the real world in which there is no opportunity for direct reciprocity in rewards. This can be remedied in the laboratory by adding a second reward stage to the experiment by Vyrastekova and van Soest (2008). This chapter reports the efficiency consequences of adding such a second reward stage, and the results are not very encouraging. Despite the fact that more rewards are being exchanged in the two-stage setup than in the one-stage setup, efficiency in the social dilemma situation is lower (and about equal to Nash).

That means that rewards may not be effective in sustaining cooperation in social dilemma situations after all. Unless the institutional setting is such that agents cannot positively reciprocate to rewards received from their peers, the possibility to engaging in a privately beneficial exchange of reward activities severs the link between the social dilemma situation and the reward activity, and cooperation in the social dilemma situation unravels.

However, the prospect for rewards may not be as bleak as the above analysis suggests. In this chapter we also tried to see to what extent adding this second reward stage facilitates classification of our subject pool into the various behavioral types, such as pro-social individuals, ‘homo economicus’, and strategic money maximizers. Whereas behavior in the one-stage reward setup is very noisy, there is much more consistency in our subjects’ behavior in the two-stage reward setup. Indeed, we find that we can classify one-third of our subjects as pro-social, one half as strategic money maximizers, and one sixth as 'homo economicus'. From an experimental economics point of view this is interesting because it suggests that adding a stage that allows for direct reciprocity helps identify the participants' social orientation, thus facilitating the use of this experimental data in the development of new models that better capture behavior in social dilemma situations than those based on the assumption of all agents being rational and interested exclusively in their own material payoff.

But it also suggests that the extent to which rewards can sustain cooperation in the social dilemma situation crucially depends on the behavioral composition of the group. If a large share of the agents involved in the social dilemma situation have pro-social preferences, and if the amount of effort the non-pro-social agents can put into the social dilemma situation is limited, as is the case in our experiment, efficiency in the social dilemma situation can be greater than Nash. Indeed, the group that achieved the greatest efficiency level in the common pool resource stage consisted of 4 subjects that were labeled as pro-social, 1 as strategic money maximizer and 0 were labeled as homo economicus. Whereas the Nash efficiency level is at 66% of the social optimum, the efficiency level achieved by this group was equal to 98% of the social optimum. In contrast, a group with one of the lowest levels of efficiency (42% of the social optimum) consisted of 0 subjects that were labeled as pro-social, 3 as strategic money maximizers and 2 were labeled as homo economicus.

That means that rewards may be able to sustain cooperation in a social dilemma situation after all, but only if the share of pro-social individuals is sufficiently high indeed.

Intermezzo: Visserslatijn[1]

This short intermezzo serves as a recollection of some memorable quotes that I have heard at the fishing pond, while conducting the field experiments that are described in the following chapters. Part of the fun of those quotes is that they are spoken in a Dutch dialect, 'Tilburgs'. For that reason, I have chosen to write this intermezzo in Dutch. This intermezzo is meant to have entertainment value only, and not to provide any scientific insights.

Naast experimenten in het traditionele laboratorium, heb ik gedurende mijn tijd als promovendus ook experimenten uitgevoerd 'in het veld'. De komende drie hoofdstukken gaan in op de resultaten van die onderzoeken. Voor het zo ver is, wil ik graag in het kort enkele leuke situaties beschrijven die symbool staan voor de omgeving waar ik ongeveer twee jaar lang zo leuk heb kunnen werken. Deze omgeving is een recreatieve visvijver genaamd 'de Biestse Oevers' in het plaatsje Biest-Houtakker ('dun Biest'). Biest-Houtakker ligt dicht bij de stad Tilburg, vandaar dat er over het algemeen in het Tilburgs met elkaar wordt gecommuniceerd. Omdat het Tilburgse dialect mij niet vreemd is, lukt het me vrij aardig om de doorgaans vele conversaties die de vissers met elkaar hebben, te volgen.

Mijn Tilburgse communicatieve vaardigheden zijn vooral van nut gebleken tijdens de vooronderzoeken die ik uitgevoerd heb met Stef van Kessel, Mike Groels en Paul Ludeña Delgado. In die vooronderzoeken heb ik namelijk veel geleerd

[1]Mijn dank gaat uit naar Jan van Steensel en Gerard Steijns voor hun hulp bij het correct schrijven van de Tilburgse citaten.

over de kunst van het vissen. Deels leer je de kunst door te kijken naar hoe anderen vissen. Echter, de beste inzichten verkrijg je door gewoonweg te vragen hoe het moet. Dan blijkt dat het altijd lastig is om te voorspellen waar de meeste vis gevangen kan worden. Vaak gehoorde antwoorden op de vraag waar de vis zich in de vijver bevindt, zijn:

„De miste vis zit tussen de kòp en de stèrt."
en
„Witte wèt is? Meer waoter as vis."

Het aanleggen van het aas is een factor van cruciaal belang. De meeste vissers zeggen dat het vooral belangrijk is dat het aas „droait as ene tierelier". Maar natuurlijk zijn ook de weersomstandigheden van invloed. Vooral de windrichting zou verklaren waarom er veel of weinig gevangen wordt, getuige de verklaring van een sportvisser:

„Tis Ostewènd vandaog, dès niks wèrd. De wènd moet op et zuije staon, dan aoze ze. Ôoh!"

Als het dan allemaal mee zit, en een visser heeft veel vissen gevangen, dan gaat zoiets niet onopgemerkt voorbij. Zo antwoordde een visser toen hem gevraagd werd naar zijn visvangst:

„Zette gij mar en pènneke klaor, meej heel veul booter!"

Mijn vele bezoeken aan de Biestse Oevers zijn niet onopgemerkt gebleven. Ik vertel geïnteresseerden slechts dat wij van de Universiteit van Tilburg komen, maar zo weinig mogelijk over hetgeen we daar doen. Sommigen zijn nieuwsgierig genoeg om te vragen naar wat wij allemaal van plan zijn met het onderzoek. Bang om het gedrag van de vissers in toekomstige onderzoeken te sturen, is mijn antwoord steevast dat ik niet in kan gaan op vragen over de aard van ons onderzoek. Dit houdt enig gespeculeer van de vissers natuurlijk niet tegen:

„Ik weet waor ze vur zèèn, des gehèèm. Woarschijnlik ist vur Balkenende!"
„Ôoh jè, ik kèn oe wel. Göllie zèèt van die studentenvereeneging, ist nie?"
„Göllie zult onderhaand al wel dirrekteur zèèn van die univèrsietèèt."

Een van mijn leukste herinneringen bewaar ik aan de ochtenden waarin we het restaurant van de Biestse Oevers veranderde in een laboratorium. Voor de vissers was het waarschijnlijk de eerste keer dat ze aan soortgelijk onderzoek meededen. Bij aanvang kwamen de vissers ietwat onwennig het lab binnen. Eén visser rondde nog snel een telefoongesprek af:

„Eej, ik goa hange. Sebiet aanderhalf uur teejorieles."

Na afloop bleek dat niet alleen wij het onderzoek geslaagd vonden, ook de vissers vonden het leuk om mee te doen. Sommigen vroegen zich af of er nog een herhaling in het verschiet zou liggen:

„Wanneer is de reeunie?"

Tot slot nog een opmerking over het landschap rondom de Biestse Oevers. De Biestse Oevers is omringd door prachtige natuur, met bomen, beekjes en weilanden waar koeien, paarden en schapen grazen. Vooral laatstgenoemde dieren hebben moeite met het vangen van vissen. . . Tenminste, als we een visser mogen geloven die een conversatie opving tussen twee schapen:

„Zeet diejen eene schoap tegen diejen aandere:
'Hèdde gij nòg we gevonge?'
'Nèèh-èh-èh-èh'"

Chapter 5

From the Lab to the Field: Cooperation Among Fishermen[1,2]

5.1 Introduction

A large literature in experimental economics has focused on the extent to which individuals cooperate in social dilemmas. Social dilemmas are group interactions, in which an individual maximizes his own payoff when he does not cooperate, but where attaining the social optimum requires cooperation. One experimental paradigm commonly employed to study social dilemmas is the Voluntary Contribution Mechanism (VCM). In a canonical version of this game, each member of a group receives an endowment of money. The members of the group then simultaneously choose to contribute any portion of their endowment to a group account. Contributions to the group account benefit all members of the group. The tradeoffs are specified so that each individual has a dominant strategy to place his entire endowment in his private account, but the social optimum is attained only if all individuals contribute their entire endowment to the group account. Thus, classical economic theory, which maintains

[1] This chapter is co-authored with Charles Noussair and Daan van Soest.

[2] We would like to thank Ad and Thea van Oirschot for use of their fishing facility, and Stef van Kessel, Mike Groels, and Paul Ludeña Delgado for excellent research assistance. We are grateful to Niels van den Broek, Machiel Driesser, Patrick Hullegie, Sander Tuit, Alexandra van Geen and Peter van Oudheusden for their help in conducting the field sessions. We also thank Wieland Müller, Jan Potters, Ernan Haruvy, Amrita Ray Chaudury, Eline van der Heijden, David Voňka, Cees Withagen and especially Amos Zemel for their comments and suggestions on an earlier version of this chapter.

the assumptions of exclusively self-interested motivation and rational decision making, predicts that all individuals allocate their entire endowments to their private accounts.[3] The percentage of endowment placed in the group account can be readily interpreted as a measure of cooperation.

The behavior of individuals who repeatedly play the VCM has been shown to exhibit two robust patterns (for a survey, see Ledyard (1995)). The first pattern is that individuals' initial average contributions to the group account are significantly different from both zero and 100 percent of their endowment. This reveals positive, but less than full, cooperation on the part of the average individual entering a new social dilemma. The second pattern is that a decline in the level of cooperation occurs as the game is repeated (see, for example Isaac et al. (1985), Andreoni (1988), and Isaac and Walker (1988b)). The two patterns found in the laboratory are interpreted as evidence that behavior of individuals is systematically different from that of self-interested rational agents. Explaining these patterns has been a focus of a number of models. The positive level of cooperation at the outset of interaction is one of the stylized facts motivating the modeling of other-regarding preferences (see for example Rabin (1993), Fehr and Schmidt (1999), Bolton and Ockenfels (2000), and Andreoni and Samuelson (2006)). The decline in cooperation with repetition of the game has been interpreted as a reduction of errors over time (Palfrey and Prisbey (1996), Andreoni (1995), and Houser and Kurzban (2002)), as reputation building (Andreoni (1988), Sonnemans et al. (1999), and Brandts and Schram (2001)), and as a result of a self-serving bias accompanying conditional cooperation (see for example Neugebauer et al. (2007)).

In this chapter, we consider whether these two patterns appear in a framed field experimental environment. The setting of our field experiment is a privately owned fishing pond where recreational fishermen can catch rainbow trout. We create a social dilemma similar in structure to the VCM. The fishermen are assigned to anonymous groups of four persons, who interact for six forty-minute periods. In each period, each fisherman is allowed to catch a maximum of two fish, which are his to keep. However, for each fish an individual foregoes catching, each of the three other members of the group receives a cash payment. Thus, a social dilemma is created in that each individual has a dominant strategy to catch two fish in each period, while the social optimum requires all individuals to forego their catches. Cooperation measures are derived from the actual catch

[3]If the game is repeated a finite number of times, the only subgame perfect equilibrium is for each individual to place his entire endowment in his private account in every period, regardless of the history of play. The social optimum requires all individuals to place their entire endowment in the group account in every period.

of fish, and from the effort made to catch fish, relative to a control treatment in which no collective incentives exist to reduce the catch of fish.[4]

As described in section 5.2, we find strong support for classical economic theory in our field experiment. There is no evidence of cooperation, even in the initial periods. Beginning in the first period, and continuing throughout the sessions, fishermen in the treatment with group-level gains from cooperation fish with the same effort and catch the same average number of fish as those in the treatment without such potential gains. To explore the source of the difference between our setting and received results from the laboratory, we conduct four additional treatments. Three of these treatments are implemented in the laboratory, and the fourth one is an additional field treatment. These treatments are described and reported in sections 5.3 and 5.4.

These four treatments establish that the discrepancy in cooperation is not due to the fact that: (i) the framing is contextualized in the field experiment, (ii) the subject pool differs, (iii) the field experiment is conducted in a natural rather than in a structured laboratory setting, or (iv) the group benefits and private costs of cooperation are denominated in terms of different units (money and fish) in the field experiment. Rather, the data from these treatments suggest that the key difference between the laboratory and our field setting is the decision variable, the activity that must be undertaken in order to cooperate. When cooperation requires a reduction of fishing, individual behavior conforms to classical economic theory, and there is no cooperation. This is independent of whether the reduction in catch results in more money, as is the case in the field experiment described above, or into more fishing opportunities for the group.

The treatment developed to test the last claim (iv), FieldDyna, is of particular interest for two reasons. The first reason is that the assumption that a social

[4] Our work bears a relation to a number of other field experiments that focus on cooperation. An active literature is investigating influences on charitable giving (see for example List and Lucking-Reiley (2002), Frey and Meier (2004), Martiny and Randal (2005), Alpizar et al. (2008) and Croson and Shang (2008)). Another strand of research uses artefactual field methods to study behavior of non-student subject pools in the VCM game (see for example Barr (2001) and Ruffle and Sosis (2007)), and a closely related paradigm, the common pool resource game (see for example Cardenas (2003), Cardenas (2004), Cardenas and Ostrom (2004), and Rodriguez-Sickert et al. (2008)Rodriguez-Sickert, C.). These studies all find positive cooperation in the VCM game among the subject pools studied. The available evidence from framed and natural field experiments is mixed. Erev et al. (1993) find considerable evidence of free-riding when students pick oranges under team incentives. When groups act individually, subjects pick thirty percent fewer oranges than when a bonus is given to the group with the highest output. In a one-shot social dilemma setting in a restaurant, Gneezy et al. (2004) find that students choose more expensive meals when the costs are split with five other students, than when each pays for her own meal, and thus exhibit a considerable tendency toward free-riding. Bandiera et al. (2005) report a substantial degree of cooperation in a fruit picking firm, but only when the subjects are able to monitor each other.

dilemma exists depends on no assumptions other than that individuals prefer to catch more fish to less fish (as described in section 2, the initial field experiment described above constitutes a social dilemma only under certain, albeit in our view weak, assumptions on the relative value of fishing and monetary payments). The second reason is that FieldDyna constitutes the first experimental test of the canonical renewable resource model (see for example Brown (2000)) with human participants.

Our framed field experiment can be viewed a controlled test of the external validity of an artefactual field experiment. This is the case because we observe members of the same non-student pool of subjects in the laboratory, as well as in the field, performing a similar task. Several other field experiments have documented a positive relationship between individuals' cooperativeness in an experimental VCM game and pro-social behavior in another activity (see for example Carpenter and Seki (2005), Laury and Taylor (2005), Benz and Meier (2008), Fehr and Leibbrandt (2008), Cardenas (2004), Henrich et al. (2004), and Ruffle and Sosis (2007)). However, there are other studies that do not find such a relationship. For example, List (2006b) and Karlan (2005) find that subjects act more cooperatively in laboratory settings than they do outside the laboratory. These latter papers suggest that the laboratory may not always be well-suited to test the effectiveness of policy interventions to promote cooperation. Here, we also find that cooperative behavior in an artefactual field experiment does not carry over to a similar field setting, in this case a framed field experiment.

Levitt and List (2007, 2008) have taken the view that social preferences appear with different prominence in the laboratory and in field settings. Our results are consistent with this view. Furthermore, for the particular game we study, we are able to identify several distinct sources of differences in cooperativeness between the laboratory and the field. Our fishermen exhibit more cooperation than student subjects when making decisions in a laboratory environment, fishermen display more cooperation when making decisions in a natural environment than in a laboratory setting, and making the fishing task real rather than virtual reduces cooperation. Nevertheless, the absence of cooperation in our framed field experiment can only be attributed to the fishing task being real rather than virtual, since the effects of subject pool and of the structured laboratory setting operate in the opposite direction.

We make no claims that our field experiment is any more generic than the traditional experiment conducted in the laboratory, or that commercial fishermen would necessarily behave in a similar manner to recreational fishermen. Rather, we claim only to support the contention that the typical empirical pattern observed in a common laboratory implementation of a social dilemma is not universal, and that the behavior of non-student subjects in a contextualized laboratory experiment is not necessarily predictive of their behavior in the field.

5.2 The FieldVCM treatment

The first pair of treatments we describe consist of a field implementation of the Voluntary Contributions Mechanism, and a control treatment. The treatments, which constitute a framed field experiment in the sense of Harrison and List (2004), are described in section 5.2.1. In sections 5.2.2 and 5.2.3 we consider methodological issues that arise under our design. We present the analysis of the data in section 5.2.4.

5.2.1 The setting, game, and experimental design

The sessions were conducted at a commercial trout fishing facility called 'De Biestse Oevers', located in the village of Biest-Houtakker.[5] This village lies in close proximity to Tilburg, in Noord-Brabant province, in the south of the Netherlands. De Biestse Oevers is privately owned, and comprises three separate fishing ponds with surface areas of about 12,000 square feet each. One of these ponds served as the venue for our experiment. On a typical day, when no experiment is taking place, a customer can fish for four hours for €12.50. The pond has space for twenty fishermen at a time. For each paying customer, four rainbow trout are put into the pond (for an extra fee, salmon trout, a larger variety of trout, can also be thrown in). There are strict rules regarding the fishing gear and type of bait that may be used, but a customer is allowed to catch as many fish as possible. Also, because of sanitary considerations with respect to the remaining fish, any trout caught cannot be thrown back into the pond and must be taken away from the site (presumably home). Customers therefore have experience with negative externalities, since when an individual catches a fish he reduces the number of fish available for others. The typical customer (and our typical participant) is Dutch, male, and over fifty years old.

Participants were recruited for our experiment two weeks in advance by distributing flyers on site which informed customers of the opportunity to take part in a study conducted by Tilburg University. A maximum of sixteen people was allowed to participate in each session.

Two treatments, FieldVCM and FieldPI, were conducted under the following conditions. A session consisted of six consecutive periods of forty minutes

[5]See www.biestse-oevers.nl for pictures of the site.

each, and therefore took four hours to complete. Within a session, each period proceeded under identical rules. Participants were assigned to groups of four, and group membership remained fixed throughout the session. Subjects were not informed at any time of the identity of the other members of their group. At the end of each period, each participant was informed privately of the total number of fish caught by his group.

Before a session began, two rainbow trout per participant were put into the pond, plus an additional six trout. For a session with 16 participants, we thus threw in 38 rainbow trout. The number of fish we put into the pond was common knowledge. Before the first period, the participants were randomly assigned a spot at the pond by picking a numbered spot tag out of a bag. This random assignment procedure was repeated before periods three and five. The rotation of positions was intended to create a degree of procedural fairness, since many fishermen believe that their physical position at the pond influences their probability of catching a fish.[6]

Each participant was allowed to catch a maximum of two fish per period (rainbow trout or salmon trout, because the latter could still be present because of previous use of the pond). Any fish caught was his to keep, as the standard rules and regulations of De Biestse Oevers prohibit throwing trout back into the pond. At the beginning of each session, we released 38 trout (instead of 32), in an attempt to ensure that, at least in principle, all individuals would be able to catch their quota of two fish each. Once a participant had caught his maximum quota, he was required to wait until the next period began to resume fishing. At the beginning of the next period, a number of trout equal to the total catch of the previous period was put into the water. Therefore, the total number of fish in the pond was the same at the beginning of each period within a given session, and this information was explained explicitly to the participants. Communication among subjects was strictly prohibited.

The above is a complete description of the FieldPI treatment; the FieldVCM treatment differed only in that a social dilemma was created by introducing group incentives for reducing the number of fish caught within each group.[7] Each fish that a participant did not catch below his maximum quota of two

[6]Our data show no actual significant relationship between location and the number of fish caught, suggesting that this belief may be incorrect or exaggerated; see appendix 5.A.1 for more details.

[7]Informing subjects that they are matched into groups is awkward in a setting in which individual outcomes are completely independent of others' actions. Nevertheless, we wanted to check whether framing the FieldPI treatment as a group exercise has an impact on behavior. Therefore, we conducted one of the FieldPI sessions without informing subjects about any matching procedures. We did not detect any differences in behavior resulting from the different framing.

per period resulted in a cash payment of €2 to each of the other three group members. Therefore, a participant faced a tradeoff in the FieldVCM treatment between catching a fish for himself, or providing a surplus of €6, to be divided equally among the three other members of his group. Note that this game differs from the standard VCM game in that cooperation yields a pure externality; the decision maker does not get any private return to the investments he makes. We imposed this simplification in order to make the social dilemma more obvious to subjects. At the end of each period, participants in the FieldVCM treatment were informed of the group catch in that period, the amount of money they had earned in that period, and their cumulative earnings. The average earnings of a participant in the FieldVCM treatment over a session equalled €49.60.

One round of sessions of the FieldPI and FieldVCM treatments was carried out in June 2008, and a second round was conducted in September and October 2008. The season influences the number of fish caught. In June the water temperature is too high for trout to bite in large numbers, while this is typically not the case in September and October. Therefore, the data from each of the two seasons are analyzed separately. The data from June will be described as having been conducted in the Low season and will be designated as FieldVCML and FieldPIL. Those data acquired in September and October will be said to have been gathered in the High season and will be referred to as FieldVCMH and FieldPIH. All sessions of the field treatments were conducted between 8 AM and noon (with the instructions starting at about 7.40 AM).

5.2.2 Establishing the existence of a social dilemma

In the FieldVCM treatment, a social dilemma exists if the private benefit of the right to catch an extra fish is smaller than the amount of money received by the other three members if that fish is not caught. In other words, a social dilemma exists if participants value the right to catch one additional fish at less than €6.

There is market evidence that the marginal valuation of the act of catching a rainbow trout is less than or equal to €3. We identified five recreational fishing ponds within a 90 minute drive from our site, where fishermen are charged only for the number of fish caught. Thus the fee per fish can be viewed as the price for the right to catch an additional fish. The fees that are charged for each fish caught in these five facilities range from €1.95 to €3; the one that is closest to Biest-Houtakker, just 40 minutes away, charges €2.40 per fish. The fact that our participants are regular customers of the Biestse Oevers and not of these other facilities is the first piece of evidence that their marginal valuation of the act of catching a fish is less than €3.

The second piece of evidence is obtained by calculating the upper bound for the value of non-cooperation as follows. First, note that the private value of the right to catch a fish has two components: the value of the fish itself and the utility of fishing. The price of rainbow trout in local fishmongers' shops varies from €4.85 to €10 per kilo, and the average rainbow trout weighs around 400 grams. This translates into a price range from €1.95 to €4 per fish. Because an equivalent fish can be purchased nearby for at most €4, it is an upper bound for the value of a fish itself.

To place a value on the utility of fishing, recall that our subjects are regular customers at the fishing pond, so that the value of the marginal half-day of fishing is close to the market price of €12.50. This is an upper bound of the utility of the act of fishing itself, since individuals typically are able to take home some fish after four hours of fishing. Thus, a generous upper-bound for the total value of acting non-cooperatively in our experiment is then €4/fish $\times$ 12 fish + €12.50 = €60.50, though the actual private value is likely to be much lower. If we suppose that the usual fee of €12.50 is paid with an expectation of catching four fish on average, the amount typically thrown into the pond per paying customer, the experiment gives participants an opportunity to catch eight additional fish. Under this assumption, the value of acting non-cooperatively for an entire session (again assuming that the value of each fish is the highest price available in the area) is €4/fish $\times$ 8 fish + €12.50 = €44.50.

Regarding the benefits of cooperation, all subjects would each go home with €72 if they cooperate fully and catch zero fish during all six periods, which is substantially more money than the private value of fishing as calculated above.[8] Indeed, it would be enough to go fishing five times at 'De Biestse Oevers', and have €9.50 remaining, or alternatively to buy twelve fish in a fishmonger's shop and have €24 remaining.[9]

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[8] We are aware of only one study that estimates the total surplus of recreational fishing (rather than the marginal value of a fishing trip), and that is the paper by Toivonen et al. (2004). They estimate the total surplus recreational fishermen in five Nordic countries obtain from all fishing trips they make per year. The estimates are fairly consistent across these five countries in that they range between 1.30 and 1.54 times actual fishing expenses. If we apply the maximum ratio (1.54, measured in Norway), to our case, the amount of compensation needed for not being allowed to fish equals €19.25 (= 1.54 times the entrance fee) plus €32 (as an upper bound for the consumption value of the eight extra fish one can catch in our experiment). The calculation indicates that, even when using total surplus of fishing rather than the marginal value, the total estimate of the private value of a half-day of fishing of €51.25, is well below the monetary returns to cooperating of €72.

[9] In addition, there are various ways to decrease the opportunity cost of acting cooperatively. For example, fishermen can decide to cooperate at least partially by fishing leisurely rather than at full force, and thus enjoying the act of fishing while reducing the chances of actually catching two fish per forty-minute time period. Alternatively, they can decide to just fish for, say, four periods rather than six, or they can decide to voluntarily limit their catch

A third test of whether our game is correctly parameterized is a survey of members of our subject pool. On a day when no experiments were conducted 24 fishermen were surveyed. Using the strategy method, we asked the fishermen their maximum willingness to pay for the right to catch fish. We asked a fisherman how much fish he would like to catch, given that he would be charged €0.50 for each fish caught. If a fisherman allocated a non-zero value to this price, we asked how much he would like to catch if he would be charged €1 for each fish caught. This procedure was repeated in increments of €0.50 until a fisherman indicated that the fee exceeded his willingness to pay. The survey shows the monetary value a fisherman assigns to the act of fishing and the value of a fish combined. The data does not permit us to disentangle the two values, but that is not necessary to assess whether a social dilemma exists in FieldVCM.

The results of the survey are the following. Four fishermen indicated that they would not participate in a scheme where a fee was charged per fish caught. Therefore, we are not able to derive a maximum willingness to pay for these four fishermen. The remaining twenty fishermen had an average maximum willingness to pay for the first fish they catch of €3.50. One fisherman indicated that he was willing to pay €15 to catch one fish, while another indicated he would pay €6, and the rest indicated a willingness to pay smaller than €6. This means that ninety percent of the fishermen had a value of less than €6 for the act of fishing and the first fish they catch. For all of the fishermen, the marginal value of each fish beyond the first was always non-increasing. Thus, we are confident that our the parametrization used in our experiment indeed poses a social dilemma.

5.2.3 Measuring cooperation

The measurement of cooperation in this setting raises methodological issues that do not usually appear in laboratory experiments. The number of fish caught depends on exogenous factors, such as weather conditions, as well as on the level of cooperativeness. Here, results obtained in the FieldPI treatment serve as the non-cooperative benchmark, as FieldPI provides the same incentives to catch the quota of two fish as FieldVCM does if agents are acting non-cooperatively.

Comparing catch in FieldPI and FieldVCM during a given season (High or Low) provides one measure of cooperation. Cooperation corresponds to a smaller catch of fish in FieldVCM than in FieldPI in the same season. We call the magnitude of this difference the *Catch* measure of cooperation. The level

to just one fish per period. We deliberately specified the strategy space as zero, one or two fish (rather than just zero or one) to allow for partial cooperation.

of cooperation in the FieldVCM treatment in the Low season, according to the Catch measure, is thus:

$$C = 4\sum_i x_{it}^{FieldPIL}/n - \sum_i x_{it}^{FieldVCML^j}, \tag{5.1}$$

where $\sum_i x_{it}^{FieldVCML^j}$ is the total catch of group j in period t of the FieldVCML treatment, and $4\sum_i x_{it}^{FieldPIL}/n$ is the average catch of 4 of the n total number of individuals in the FieldPIL treatment. An analogous measure is defined for the High season. A value of C equal to 0 would indicate zero cooperation, and a positive level would indicate the presence of cooperation.

A second measure of cooperation is the number of times an average fisherman casts his fishing rod per minute. There are several advantages of this 'input' measure of cooperation. First, casting a rod is a conscious decision of a fisherman. A fisherman can deliberately 'work harder' to catch more fish. In appendix 5.A.1, we show that there is a significantly positive effect of effort on the number of fish caught. Second, the measure yields a clear measure of cooperation. Whereas catching zero fish might be a consequence of bad luck, not casting a rod cannot be reasonably interpreted in a manner other than as indicating cooperation. To measure cooperation, we take the average number of casts per minute registered by members of the group in FieldVCM, and compare it to FieldPI in the same season. If the average is lower in FieldVCM than in FieldPI, we interpret the difference as an indication that cooperation is observed. We refer to the magnitude of the difference between treatments as the *Effort* measure of cooperation. The data on casts per minute were gathered by two experimenters continuously scoring the number of casts of the 16 fishermen at the pond, with each experimenter monitoring eight individuals. This monitoring serves to increase the level of experimenter scrutiny in both FieldVCM and FieldPI — a factor that Levitt and List (2007) have identified as one that fosters pro-social behavior.

5.2.4 Results from the FieldVCM treatment

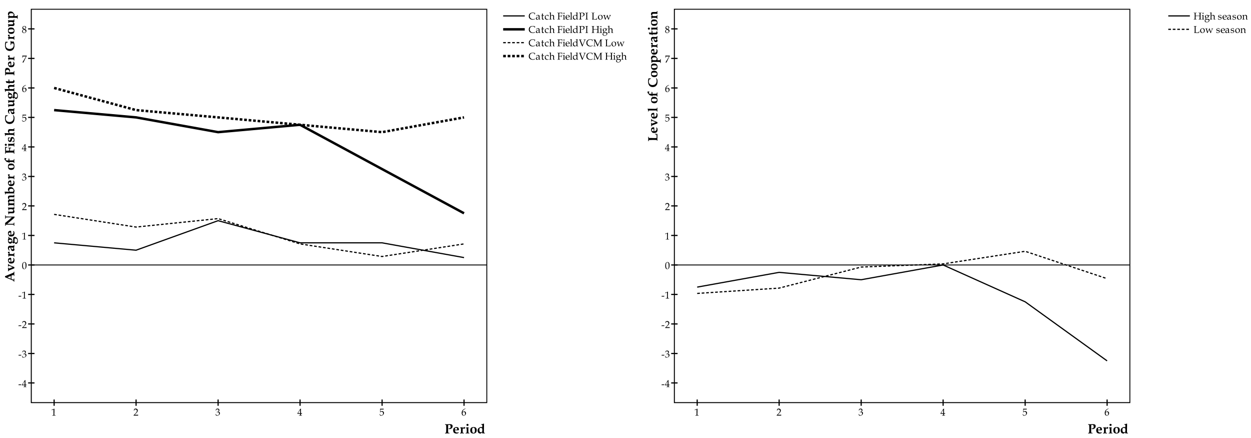
Table 5.1 illustrates the structure of the Field treatments and indicates the amount of data available. Unless noted otherwise, in the analysis of the data, we treat the activity of each group of four subjects over an entire session as one observation. This gives us a minimum of four observations per treatment.

Figure 5.1(a) presents the average aggregate number of fish caught in a group, while Figure 5.1(b) displays the level of cooperation as calculated according to equation (5.1). The average in each of the two seasons is indicated as

Treatment	Groups	Main feature	Average Earnings
FieldPIH	4	Determine maximum fishing activity in the high season	–
FieldPIL	4	Determine maximum fishing activity in the low season	–
FieldVCMH	4	Diff. from FieldPIH measures cooperation in the high season	€26.63
FieldVCML	7	Diff. from FieldPIL measures cooperation in the low season	€62.71

Table 5.1 Number of groups, main feature, and average earnings in the Field Voluntary Contribution Mechanism treatment (FieldVCM) and Field Private Incentive treatment (FieldPI) in the Low and High season.

a separate series. In Figure 5.1(a), higher catch reflects less cooperation. Two patterns are obvious in Figure 5.1(a). The first is that, in a given season, the average number of fish a group catches in FieldVCM is at least as great as in FieldPI. Second, whereas the number of fish caught falls over time, the decrease is not more pronounced in FieldVCM than in FieldPI. This is shown by the relatively stable level of cooperation, as calculated according to equation (5.1), in all periods in Figure 5.1(b) (with the exception of the last period in the high season).



(a) **(b)**

Figure 5.1 (a) Average group catch by period for FieldVCM and FieldPI, in the High and Low season. **(b)** Average level of cooperation by period, in the High and Low season.

Our second measure of cooperation, effort as captured in the number of casts per minute, is shown in Figure 5.2. The figure shows that the four treatments yield similar behavior. On average, the fishermen cast their rod 0.59 times per minute in FieldPI, compared to 0.63 in FieldVCM. The Effort measure is not appreciably different between the Low and the High season. This finding

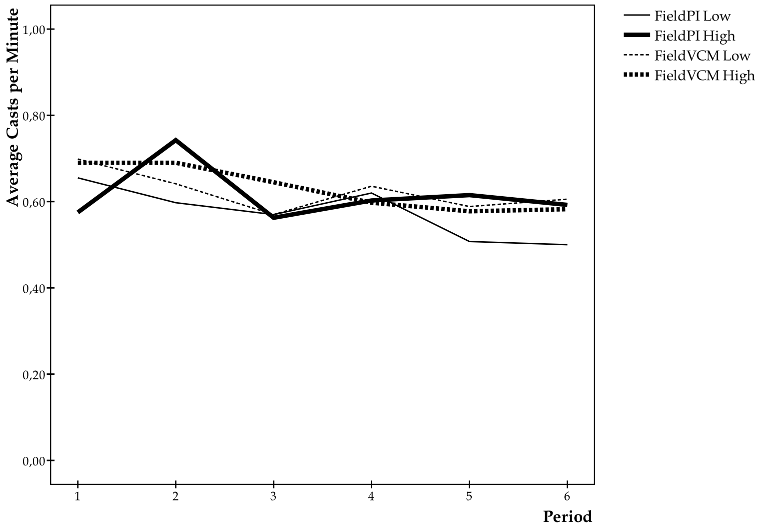


Figure 5.2 Average individual casts per minute by period, FieldVCM and FieldPI, High and Low season.

is important in interpreting the catch data presented in Figure 5.1(a), which shows that not all fish are caught in the VCM treatments. The finding that not all fish are caught in the VCM treatments suggests cooperative play by the fishermen. However, the effort levels show that the lack of catch must be due to exogenous factors, rather than to a conscious decision of the fishermen to stop catching: Fishermen in the VCM treatments try as hard as the fishermen in the PI treatments to catch fish. Thus, by both the Catch and the Effort measures, Figures 5.1 and 5.2 show no evidence of cooperation. The support for result 1 below provides the statistical basis for this claim.

Result 1 In our social dilemma experiment conducted in the field, FieldVCM, no cooperation is observed.

Support for result 1: We first consider cooperation measured in terms of catch. On average, the catch of fish is actually higher in the FieldVCML and FieldVCMH treatments than in the corresponding FieldPIL and FieldPIH treatments. A Mann-Whitney test, taking each group's activity over a session as one observation, and comparing the catch of fish in the Low season, fails to reject the hypothesis of equal catch in the two treatments ($N_1 = 4, N_2 = 7, p = 0.164$). In the High season, the Mann-Whitney test indicates that more fish are caught in the FieldVCMH treatment than in FieldPIH ($N_1 = 4, N_2 = 4, p = 0.057$).

Consider now the Effort measure. Here, the appropriate Mann-Whitney test indicates no significant differences in casts per minute between FieldVCM and FieldPI, neither in the Low season ($N_1 = 4, N_2 = 7, p = 0.412$) nor in the High season ($N_1 = 4, N_2 = 4, p = 0.886$). There is no evidence of cooperation by either of our two measures.

Another place to look for evidence of cooperation, is to consider the effort levels associated with attempting to catch a second fish, conditional on having caught one fish already in the current period. The quota of catching two fish gives the fishermen the opportunity to cooperate partially, by catching one fish — thus enjoying fishing while still earning money for the other members of one's group. Such cooperation would be revealed in lower effort in trying to catch a second fish in FieldVCM than in FieldPI. However, we find no evidence of a difference in effort to catch a second fish between FieldPI and FieldVCM (Mann-Whitney test, $N_1 = 19, N_2 = 33, p = 0.50$, taking the average effort levels of each subject over the course of the entire session as an independent observation).[10] ■

Thus, we find no evidence of cooperation in the FieldVCM treatment. We now consider whether there is a trend in cooperation over time. A downward trend in the number of fish caught is evident in Figure 5.1(a), which could indicate an increase in cooperation. However, the decrease is similar in the two treatments, although it is more pronounced in FieldPI than in FieldVCM in late periods of the High season. This shows that cooperation becomes even more negative over time in FieldVCMH, as can be seen from the level of cooperation depicted in Figure 5.1(b). The visual impression gained from Figure 5.2 is that there is no discernible trend in effort levels. For both Catch and Effort we test whether the measure of cooperation is different between early and late periods, and the weight of the evidence favors result 2.

Result 2 There is no change in the level of cooperation over time.

Support for result 2: For purpose of this analysis, the *early* periods of a session consist of periods 1 and 2, while periods 5 and 6 are considered the

[10] We also test for differences in the variance of the number of casts between FieldPI and FieldVCM. A Mann-Whitney test cannot reject the hypothesis of an equal variance across the two treatments ($N_1 = 8, N_2 = 11, p = 0.60$). There is no evidence of a diminishing variance over time in either treatment. Comparing the variance in period 1 and 2 with the variance in period 5 and 6, a Wilcoxon test yields a p-value of 0.58 in the FieldPI treatment ($N_1 = N_2 = 8$) and a p-value of 0.18 in the FieldVCM treatment ($N_1 = N_2 = 11$). The similarity between the two treatments is further evidence that the incentive to cooperate does not influence behavior.

late periods. The average group catch and effort over all groups in the first two periods of the FieldPI treatment in a given season are taken as the zero cooperation baselines for early periods. Similar baselines are constructed for the late periods. Using $k = \{L, H\}$ to denote the season, the early baseline is subtracted from group catch in the first two periods for each group in the FieldVCMk treatment separately, and the late baseline from group catch in periods 5 and 6 for each group in FieldVCMk. Thus, the difference between each group's catch (effort) in FieldVCMk and the average catch (effort) in FieldPIk is an observation. If the catch (effort) in an observation of FieldVCMk exceeds the average in FieldPIk, we assign the observation a cooperation level of zero. We then test whether cooperation is the same in the early and late periods in either season, treating each group's catch as a matched pair.

The number of fish caught in both early and late periods on FieldVCMk exceeds the average in the same periods of FieldPIk in every session, so the Catch measure indicates zero cooperation in both early and late periods. For the Effort measure, we find that the difference in cooperation between early and late periods is insignificant in the Low season (Wilcoxon test, $N_1 = N_2 = 7, p = 0.11$), as well as in the High season ($N_1 = N_2 = 4, p = 0.85$). ■

5.3 Bridging the gap between the laboratory and the field

Section 5.2 shows that the pattern of cooperation in FieldVCM is very different from the pattern of behavior observed in traditional VCM experiments conducted in the laboratory. However, the two conditions differ in several major aspects, and hence there are a number of candidate causes for the differences in results. These include the subject pool participating, whether the experiment is conducted within or outside the laboratory, and characteristics of the game itself, such as the decision variable (fish or money), and the framing of the task. To isolate the effect of the subject pool and the laboratory setting, we conduct three treatments, called StuLab, FisherLab and FisherPond. We will refer to these collectively as the *Lab* treatments because of their relatively close adherence to traditional laboratory experimental procedures.

In section 5.3.1 we describe the procedures that are common to the three treatments. Section 5.3.2 describes differences between the three treatments. The results are presented in section 5.3.3.

5.3.1 The laboratory version of our social dilemma game

As in the FieldVCM treatment, participants in the three lab treatments were assigned to groups of four subjects. Each group's composition remained constant throughout the six-period sessions. Sessions were conducted by hand using pen and paper. Participants were asked to decide how many virtual fish to catch in each period, with a maximum of two fish per period. Each fish that a participant decided to catch, yielded her a real cash payment of €1; each fish that the participant did not catch yielded €0.50 to each of the other three group members. The earnings of an individual are given by the following:

$$\pi_{it} = €1 \times x_{it} + €0.50 \sum_{j \neq i} (2 - x_{jt}), \tag{5.2}$$

where π_{it} are the earnings in Euros of subject i in period t, and $x_{it} \in \{0, 1, 2\}$ is the catch of subject i in period t. There is a dominant strategy to catch two virtual fish, yielding individual payoffs of €2 per period. The social optimum, with each group member receiving €3 per period, can be reached only if all players choose to catch zero fish. The duration of a session of the lab treatments takes about one fourth of the duration of a session of the field treatment. Therefore, earnings in the lab treatments are scaled down by a factor 4 to make the earnings comparable to the field treatments.

In contrast to the traditional laboratory experiment, the language of the instructions was contextualized to approximate a virtual implementation of the FieldVCM treatment. For example, the terms 'fish', 'catch' and 'pond' were used, rather than terms such as 'tokens', 'account', and 'project'. After the instructions were read out loud, the participants had to answer some test questions, which they answered without much difficulty.

After each period the experimenter informed all participants about the decisions of all subjects in the session by writing down all individuals' catch decisions, next to their identification numbers. This meant that each subject was able to monitor and track every other individual subject's decisions over time. However, none of the subjects were informed about which of the other session participants were in his own group, and there were either twelve or sixteen subjects in each session. This approximated the content and precision of the information available to participants in the FieldVCM and FieldPI treatments, in which individuals could observe others, but did not know who was in their group. After each period, subjects were informed, in private, of their earnings in that period as well as of the sum of the total group catch. Communication between participants was strictly forbidden, which was respected in all sessions.

5.3.2 Constructing the bridge from the laboratory to the field

The first treatment, StuLab, was a conventional lab treatment conducted with student participants in the CentER laboratory at Tilburg University. We specifically and exclusively invited students with a Dutch nationality to participate. This restriction was intended to control for cultural factors, which could potentially influence the results (see for example, Brandts et al. (2004), and Herrmann et al. (2008)). In total, 32 students participated in the StuLab treatment, yielding eight groups of four subjects. All of the students were economics, law or psychology majors. On average, the participants in this treatment earned €12.98 in the experiment.

The second lab treatment, FisherLab, was identical to the StuLab treatment except for the subject population, who were customers of 'De Biestse Oevers', the same subject pool sampled for the FieldVCM and FieldPI treatments. Thus, FisherLab can be classified as an artefactual field treatment according to the definitions of Harrison and List (2004). The treatment was conducted in the restaurant of De Biestse Oevers, which was temporarily transformed into an experimental lab. We rearranged the restaurant so that it closely resembled a standard experimental laboratory. We brought folding tables (normally used as exam tables for students taking large-scale written examinations at Tilburg University), and placed them in rows well apart from each other. This ensured that subjects could not read their neighbors' decision sheets. We installed a blackboard in front of the rows of tables on which decisions could be recorded. We applied the procedures customary to sessions conducted in our laboratory. In total, 32 fishermen participated in this treatment, comprising eight groups of four participants, and thus yielding eight independent observations. On average, the participants in this treatment earned €13.65.

The third lab treatment, FisherPond, was identical to the FisherLab treatment, except that the FisherPond treatment was conducted while participants were actually fishing at the pond. Recruitment took place by approaching fishermen at the pond and asking them if they would be willing to participate in a research study conducted by Tilburg University. We deliberately approached fishermen located at some distance from other participants, in order to exclude the possibility of participants contacting each other. Once we had recruited all participants, the rules were explained to all of them simultaneously at a central location. This was intended to ensure common knowledge and comprehension of the task among all participants. This was the only time during a session that the participants were not at their designated fishing spots. Participants were given a typed summary of the instructions, and listened to the experimenter reading out aloud the full version of the instructions.

After instruction, the fishermen returned to their fishing spots, and resumed fishing. An experimenter circulated among the subjects collecting their decisions and providing information about others' decisions and outcomes, while the participants continued fishing. As in StuLab and FisherLab, participants were informed in each period about the decisions of all other subjects in the session, but also (privately) about the decisions of the other members of their group and their own earnings.

After period six was completed, each participant was paid his earnings and then continued fishing for the remainder of the morning. The average earnings for the participants in this treatment were €14.30. Table 5.2 summarizes the number of groups, main feature and average individual earnings in each treatment.

Treatment	Groups	Main feature	Average Earnings
Students in the lab (StuLab)	8	Isolate effects of contextualization	€12.98
Fishermen in the lab (FisherLab)	8	Isolate effects of fishermen subject pool	€13.65
Fishermen at the pond (FisherPond)	7	Isolate effects of physical environment	€14.30

Table 5.2 Number of groups, main feature, and average individual earnings in the lab treatments.

5.3.3 Results in the StuLab, FisherLab and FisherPond treatments

Figure 5.3 shows the average levels of cooperation over time in the three lab treatments, StuLab, FisherLab and FisherPond. Cooperation is measured as the average number of fish not caught per group. That is, the level of cooperation is the maximum possible group catch in a period, eight, minus the actual (though virtual) catch. The figure shows that, as in prior controlled laboratory studies, the level of cooperation is positive in the early periods of the game, and decreases as the game progresses. Therefore, we obtain the following result:

Result 3 Contribution patterns in the StuLab treatment conform to the usual patterns observed in the VCM game as typically implemented in the laboratory. The lack of cooperation in FieldVCM is therefore not due to the contextualization of the decision.

Support for result 3: Figure 5.3 shows that in early periods of the StuLab treatments, students cooperate in the first period, but increasingly less so in the

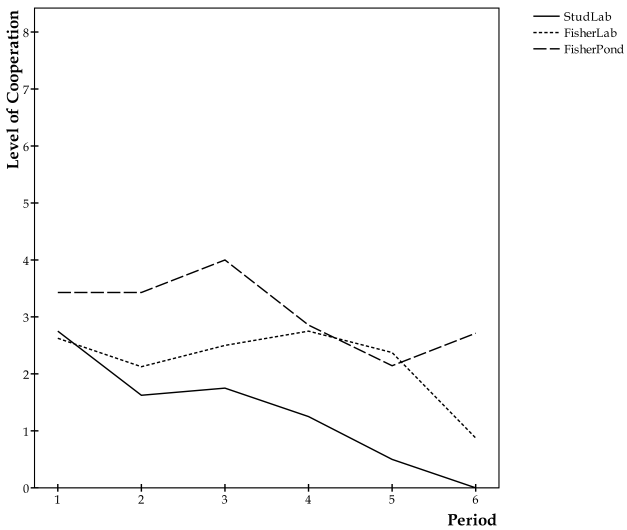


Figure 5.3 Levels of cooperation (maximum possible group catch minus actual catch) in the lab treatments by period, averaged over all groups.

later periods. A t-test shows that in the StuLab treatment, the cooperation level is significantly different from zero in period 1 ($N = 32, p < 0.01$). In this test, the choice of an individual, rather than a group's average contribution, is taken as an independent observation (because in the first period, there are no intragroup dependencies resulting from the history of play). A Wilcoxon test comparing 'early' and 'late' play, taking the group average contribution over periods 1 and 2 as an observation of early play and the group average over periods 5 and 6 as an observation of late play, yields a p-value of 0.01 ($N_1 = N_2 = 8$) for the StuLab treatment. Hence, cooperation decreases significantly over time. ■

Thus, we find that the lack of cooperation in the first periods of FieldVCM is not a result of contextualization itself. Next, we test whether the lack of cooperation found in the field treatments is due to differences in the subject pool. It may be the case that fishermen are systematically less cooperative than students, and that such a difference accounts for the behavior we observe in the field treatments. However, when comparing play in the StuLab and the FisherLab treatments — which are identical except for the characteristics of the subjects that participate — we find that, if anything, recreational fishermen are more cooperative than students. This is reported as result 4.

Result 4 Cooperation is greater in FisherLab than in StuLab. The lack of cooperation in FieldVCM is therefore not due to recreational fishermen being intrinsically less cooperative than students.

Support for result 4: Figure 5.3 shows that students exhibit a lower level of cooperation than the fishermen in the laboratory, especially in the later periods of the game. This is supported by a Mann-Whitney test ($N_1 = 8, N_2 = 8, p = 0.02$), that rejects the hypothesis of equal cooperation in both treatments.[11] ■

Thus, the behavior of recreational fishermen in the laboratory experiment is not predictive of their behavior in the field. One may argue that this is not unexpected because recreational fishermen are likely to have competitive instincts: They will try to catch more fish than their peers and hence it is not surprising that we find no evidence of cooperation in the field. On the other hand, it is striking that fishermen act cooperatively, even more so than students, in a contextualized laboratory experiment. A necessary condition for laboratory experiments to be reliable tests of policy interventions is that people bring their true preferences into the laboratory; comparison of the results of FisherLab and FieldVCM suggests that this is not always the case.[12]

Furthermore, the above shows that subject pool composition alone does not account for the lack of cooperation in FieldVCM: both students and fishermen display positive levels of cooperation in the lab. We now consider whether the laboratory setting itself has an effect on the cooperation levels that the fishermen exhibit. We do so by comparing behavior in the FisherLab and FisherPond treatments. These two treatments are identical except that one is conducted in a synthetic environment very similar to an experimental laboratory, while the other is conducted in more natural conditions. From this comparison, we obtain result 5.

Result 5 Cooperation in the FisherPond treatment is greater than in the FisherLab treatment. Cooperation is reduced by the laboratory setting.

[11] Initial cooperation is also significantly different from zero for the FisherLab treatment. The Student t-test shows that individual cooperation levels are significantly different from zero in period 1 in the FisherLab treatment ($N = 32, p < 0.01$). In this treatment, average group cooperation decreases over time, but not significantly. A Wilcoxon test comparing the group average of period 1 and 2 to that of period 5 and 6, yields a p-value of 0.23 ($N_1 = N_2 = 8$).

[12] There is some evidence that high-sea professional fishermen, a distinct group from recreational fishermen, are particularly competitive. Two quotes illustrate this point. Analyzing the catch decisions of Norwegian fishermen targeting Blue Whiting, Gezelius (2007) quotes a skipper stating that "[the choice of technology is not dependent so much on] a question of cost, but of fishing more than your neighbor." Similarly, in his analysis of fishing behavior by Dutch high-sea fishermen, van Ginkel (2009) states that "the deep-seated core value of the fishing game [is] the fisherman's desire to catch more than his neighbors."

Support for result 5: Figure 5.3 shows that the average level of cooperation in the FisherPond treatment is higher than in FisherLab. A Mann-Whitney test shows that this difference is statistically significant ($N_1 = 8, N_2 = 7, p = 0.04$).[13] ■

This result suggests that the formally structured laboratory setting itself reduces cooperative behavior, at least for our subject pool of recreational fishermen. Therefore, the fact that our experiment is conducted outside of the laboratory cannot, on its own, account for the lack of cooperation we have observed in FieldVCM.

5.4 The FieldDyna treatment: A dynamic social dilemma

The treatments reported in section 5.3 show that the difference between our field results and traditional laboratory results persist when the effects of subject pool and the laboratory are removed. The source of the discrepancy in results must lie in differences between our field and the traditional laboratory implementations of the VCM. While there are several substantive differences, we believe that the most salient is the decision variable that must be modified in order to cooperate. In FieldVCM players cooperate by fishing less, while in the lab treatments, they cooperate by giving up money.

There are two separate mechanisms whereby the decision variable could affect the level of cooperation. The first is the possibility that the decision variable itself influences cooperation. It may be that if a reduction in fishing is required to achieve cooperation, individuals are less cooperative. The second is that when group benefits and private costs of cooperation are measured in different units, as in the FieldVCM treatment (money versus fish not caught rather than the money versus money trade-off in the lab treatment), individuals are less cooperative. Different units of account might introduce self-serving biases in beliefs about the tradeoffs between the two units. For example, individuals may convince themselves that other players prefer to fish rather than to have money, and thus that failure to reduce one's own fishing is compatible with attaining

[13] As in the other lab treatments, cooperation in the first round is also significantly different from zero for the FisherPond treatment, as indicated by a standard t-test, taking each individual catch decision as an independent observation ($N = 28, p < 0.01$). In this treatment there is also a significant decrease of cooperation over time. A Wilcoxon test comparing the group average cooperation of period 1 and 2 to that of period 5 and 6, yields a p-value of 0.03 ($N_1 = N_2 = 7$).

the social optimum.

To investigate whether the decision variable is the key factor influencing behavior, and to distinguish between the first and second possibilities of the manner in which it influences behavior, we construct an additional field treatment, called FieldDyna. In this treatment, both the private costs and group benefits of cooperation are measured in terms of fishing. If we find an absence of cooperation, we would rule out the second explanation, but not the first.

The FieldDyna treatment is a dynamic game. In the first period, fishermen are divided into groups of four. Each group has the opportunity to catch a group maximum of eight fish in the first period, as was the case in the FieldVCM treatment. In contrast to the FieldVCM treatment, however, there are no individual constraints on catching fish in FieldDyna, as long as the group as a whole does not catch more than eight fish. The total number of fish the group can catch in the second period, however, depends on the total number of fish the group catches in the first period. A quadratic (hump-shaped) growth function relates the increase in the number of fish that the group is allowed to catch in the next period to the stock remaining at the end of the current period. Hence, catching too many fish in the current period results in the group being allowed to catch fewer fish in the next. The social dilemma is entirely in terms of fish: An individual who catches a fish reduces the number of fish available to other members of his group in the current period. Typically also the number of fish available for the group in the subsequent periods is reduced, depending on the actual quantity of allowable catch remaining.

This treatment is interesting for at least three reasons. First, as stated above, it controls for the impact of the benefits and costs of cooperation being measured in different units. As such, it isolates potential factors causing the qualitative differences in play between the laboratory and the field, as captured in the difference between FisherPond and FieldVCM. Second, if there is any doubt about whether our parametrization in FieldVCM constitutes a social dilemma, it is obvious that FieldDyna unambiguously does so; fish caught by one fisherman reduces the current number of fish remaining and hence affects the fishing opportunities available to the group in both the current and future periods. Third, the FieldDyna treatment is the first experimental field test of the canonical renewable resource model used in the environmental and resource economics literature (for an overview, see for example Brown (2000)).

This section is organized as follows. Section 5.4.1 describes the structure of the game. Section 5.4.2 presents the experimental design and discusses some methodological issues. Section 5.4.3 presents the main findings.

5.4.1 Description of the game

Consider the following model, which is the basis of the FieldDyna treatment. A finite number of agents ($n \geq 2$) has access to a renewable resource. Each agent aims to maximize his net present value of resource harvesting, taking into account the dynamics of the renewable resource as well as the behavior of his $n-1$ fellow agents harvesting the resource. That is, agent i faces the following maximization problem:

$$\max_{x_i(t)} \quad V_i = \int_{t=0}^{T} \bar{p} x_i(t) e^{-rt} dt \tag{5.3}$$

$$\text{s.t.} \quad 0 \leq x_i(t) \leq \overline{x}, \tag{5.4}$$

$$\dot{S}(t) = Q(S(t)) - x_i(t), \tag{5.5}$$

$$Q(S(t)) = G(S(t)) - \Sigma_{j \neq i} x_j(t). \tag{5.6}$$

Here, T is the number of time periods ($t = 1, \ldots, T$) the game lasts, $\bar{p}$ denotes the constant net revenues of selling a unit of the resource, and $x_i(t)$ is the quantity of resource agent i harvests in period t. Next, r is the private discount rate, possibly the interest rate. $S(t)$ is the stock of the resource in period t, and $\dot{S}(t)$ denotes the change in the stock of the resource over time. $G(S(t))$ is the natural regeneration of the resource, whose rate depends only on the size of the current stock, and $Q(S(t))$ is the change in stock resulting from natural regeneration net of the amount extracted by all agents other than the decision maker. We assume that there is a maximum number of units of the resource that an agent can harvest per period ($\bar{x}$; see (5.4)). As constraints (5.5) and (5.6) describe, the change in the stock of the resource in period t, $\dot{S}(t)$, is equal to the natural regeneration of the resource $G(S(t))$, minus the total quantity of resource harvested by the n agents ($\Sigma_{j \neq i} x_j(t) + x_i(t)$).

In the renewable resource literature, the natural regeneration function $G(S)$ is usually specified as follows:

$$G(S(t)) \quad = \quad \gamma S(t) \left(1 - \frac{S(t)}{K}\right). \tag{5.7}$$

Here, $K > 0$ is the maximum possible stock of the resource, also referred to as the carrying capacity. $\gamma > 0$ is the maximum rate at which the resource regenerates, and is usually referred to as the intrinsic growth rate. Note that $G(0) = G(K) = 0$, and that the increment in population size is largest at $S = K/2$, where $dG(S)/dS = 0$. This stock level is usually referred to as the maximum sustainable yield stock (i.e., $S_{MSY} = K/2$).[14] For a sufficiently high

[14]Note that absent harvesting, equations (5.5) and (5.7) combined would result in the size

$\bar{x}$, the total number of fish caught is maximized if aggregate effort is chosen such that the stock is kept at this level in periods $t = 1, \ldots, T-1$, while all remaining fish are caught in period T.[15] This level also maximizes group benefits in this model if and only if $r = 0$. Hence, the socially optimal steady state resource stock, S^*, is equal to S_{MSY} ($=K/2$) if $r = 0$. For any non-negative discount rate, however, the unique Nash equilibrium steady state stock is equal to zero; absent cooperation, all agents commit maximum effort until the stock is depleted. In appendix 5.A.2, the social optimal and subgame perfect Nash equilibrium harvesting paths are derived and characterized.

5.4.2 Experimental design and parameters

As in the FieldVCM and FieldPI treatments, there were sixteen participants in a session, assigned to groups of four with fixed membership. In each period, the four fishermen in a group faced a group quota which could change from period to period. The quota for period t — also referred to as the total allowable catch in that period — is denoted by Z_t, and any fisherman in the group was allowed to catch as many fish he or she wanted (or was able to) in that period as long as $X_t \equiv \Sigma x_{it} \leq Z_t$. The total allowable catch remaining for the group at the end of period t, $S_t \equiv Z_t - X_t$, determined the number of new fish the group was permitted to catch, $G(S_t)$. Therefore, the available quota for period $t+1$ was equal to $Z_{t+1} = S_t + G(S_t)$. Thus, a group's total allowable catch remaining at the end of period t satisfied:

$$S_t = S_{t-1} + G(S_{t-1}) - X_t. \qquad (5.8)$$

In order to facilitate the implementation of the experiment, we modified the model of section 5.4.1 as follows. First, the model (5.3)-(5.5) assumes that there are constant benefits of catching fish (equal to $\bar{p}$). However, in the field, the marginal utility of fish may be declining. In the experiment, we ensured that the benefits of catching fish were always strictly positive, by not only allowing fishermen to keep any fish caught, but also by paying them an additional €5

of the resource stock growing over time according to an S-shaped function; the stock develops logistically. Starting from a very small population size, the stock increases very slowly in the first periods (in the case of fish, because the number of mating pairs is small), then increases and reaches its maximum increment at $S_{MSY} = K/2$. For stocks larger than this level, resource growth tapers off because of increased competition between individuals in the population for food and basic resources. Eventually, the resource would reach its natural equilibrium size K, where net growth is zero as the number of offspring would equal natural mortality.

[15] That is, in all periods $t < T$ aggregate catch should be equal to (i) zero, (ii) the maximum amount ($n\bar{x}$), or (iii) $G(S)/n$, if the current stock is smaller than, larger than, or equal to S_{MSY}. In period T, $\Sigma_i x_i(T) = S_{MSY}$

for every fish they caught.[16] Second, the rate of time preference, r, was set equal to zero.[17] Third, in the experiment, the continuous growth function (5.7) of the model was approximated by a discrete function. The values chosen are represented by the solid line in Figure 5.4; they were such that $K = 8$ and $S_{MSY} = 4$. Fourth, we set the number of periods equal to four ($t = 1, \ldots, 4$), and, as in the FieldVCM treatment, we set the total allowable catch for period 1 equal to eight fish for each group ($Z_1 = 8$).[18]

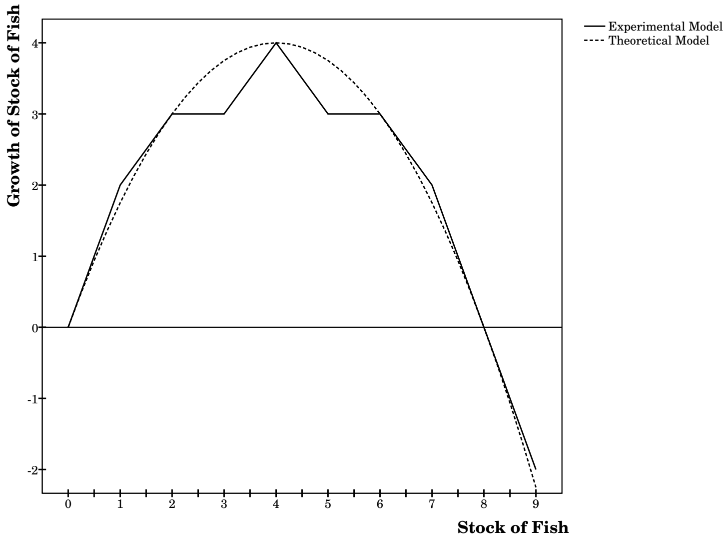


Figure 5.4 Theoretical specification of the regeneration function (with $\gamma = 2$ and $K = 8$) and the discrete experimental parametrization thereof.

For the parameter values we use, the socially optimal harvesting path is the following. Because $r = 0$, the group's benefits are largest if the group harvest is maximized. To do this, a group should catch four fish in the first three periods, and it should catch the remaining eight fish in the fourth (that is, $X_t^* = K - S^* = K/2$ in periods $t = 1, 2, 3$, and $X_4^* = K$). Note that the discrete parameterization of the logistic growth function as shown in Figure

[16]Note that because harvesting costs are zero and independent of the size of the stock, neither the socially optimal nor the Nash equilibrium harvesting paths are affected by the level of $\bar{p}$ as long as it is positive.

[17]In a four hour experiment the natural value of r is zero. Participants may prefer to catch fish sooner than later because of strategic considerations, but for any given number of fish caught during a session, participants are not likely to prefer to catch them all in the first few periods. We could have induced $r > 0$ by paying interest, but at the cost of (i) longer instructions, and (ii) a lower probability of subjects being able to infer the correct level of S^*.

[18]Hence, we implicitly assume that the group's fish stock was initially equal to the carrying capacity, K.

5.4 is chosen to increase the salience of the maximum sustainable yield stock as the cooperative solution — the fit of the discrete function would have been better if we had set $G(S) = 4$ for $S = \{3, 4, 5\}$ rather than just for $S = \{4\}$. The subgame perfect equilibrium path is $x_{i,t} = \bar{x}$ for all i, t as long as $Z_t > 0$. That is, the equilibrium outcome is that the entire allowable catch is taken in the first period, and there are no fish available to the group afterwards. Since $G(0) = 0$, the session would end after the first period, and the members of the group would be required to leave the pond area. Because each period is one hour long, in the social optimum, a group can fish for four hours, catch twenty fish and receive €100. In the subgame perfect Nash equilibrium harvesting path, a group receives eight fish and €40, and can fish for at most one hour.[19]

In all sessions of FieldDyna, 16 subjects participated, divided into four groups of four participants. At the beginning of the first period 38 rainbow trout were released into the pond (two per participant, plus an additional six, as was the case in FieldVCM). At the beginning of each subsequent period, a quantity of fish was released equal to the number caught in the previous period by all groups in the session that were still active in the current period. Hence, the actual number of fish in the pond, per fisherman still participating, was the same at the beginning of each period, while the dynamics of the total allowable catch remaining for each of the four groups are described by equation (5.8). Replacing the fish caught avoids the possibility that one group's harvesting path affected the feasibility of other groups in the same session following their intended path.

In the FieldDyna treatment, participants were aware of which other individuals were in their group. Fishermen wore colored ribbons identifying their group. We gave this information because the model presented in section 5.4.1 has a closed-loop solution (see appendix 5.A.2), which requires fishermen to be aware of the size of the remaining quota (Z_t) at any moment. We believe that if this feature of the design affects behavior, it would enhance cooperativeness. If fishermen are able to monitor the development of the remaining quota over time, it may induce them to cease fishing when they see that the remaining quota is getting too small. Hence, if we do not find any evidence of cooperation, the results would be even more convincing than in the absence of the group affiliation information.

At the end of each period, subjects were informed of (i) their total earnings

[19] The reader may argue that it is no surprise that there is no cooperation in FieldVCM because 'fishing is fun'. If anything, this argument should result in more cooperation in FieldDyna because the more cooperative the group's fishing behavior, the longer one is allowed to fish, the larger the number of fish caught, and the larger the amount of money earned.

in the period, (ii) total group catch in the period, X_t, (iii) the total group quota still remaining, S_t, (iv) the increase in the group's quota, $G(S_t)$, and (v) the size of the resulting allowable catch for the next period, $Z_{t+1} = S_t + G(S_t)$.

As in FieldPI and FieldVCM, the instructions were read out aloud by the experimenter at a central location, participants were provided with a handout summarizing the instructions, and communication was strictly forbidden. We explicitly tested the participants' understanding of the dynamic game by having them answer test questions before the start of the session. The sessions of the FieldDyna treatment were conducted in April 2009. Average earnings of the participants in this experiment were €15.30.

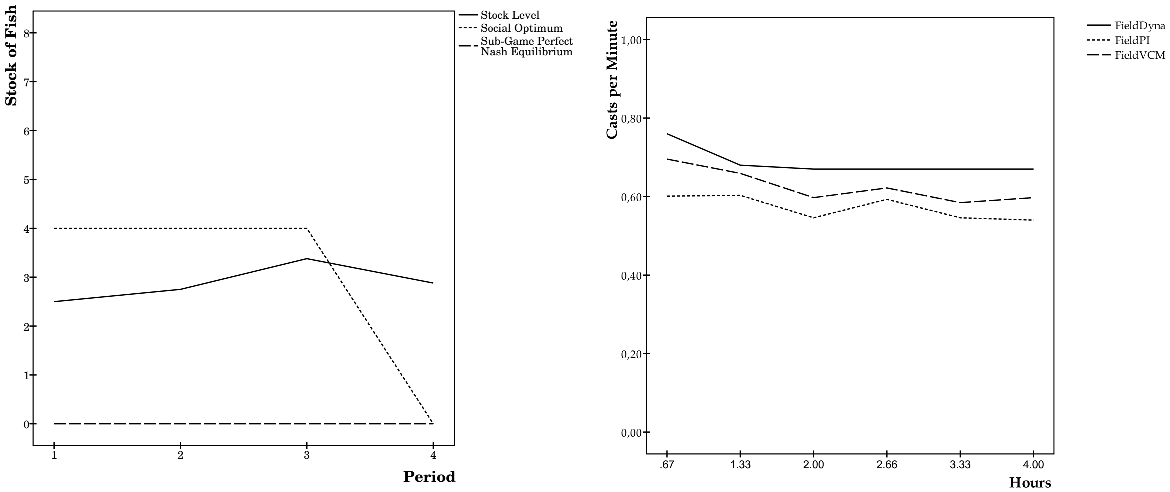
5.4.3 Results of the FieldDyna treatment

There are two patterns that we use to distinguish cooperation from non-cooperation in this treatment.[20] The first is that, under non-cooperative behavior, there would be no difference in behavior over the four periods. Players would fish with the same, maximal, effort in all periods. Under the social optimum, however, effort would be greater in the last period, relative to the first three periods. This would indicate an attempt to reduce catch in periods 1-3 to below the maximum feasible level. The second pattern is that, under cooperative behavior, effort would exhibit a dependence on the number of fish remaining in the group's quota. If individuals fish less intensely when there are fewer fish in the pond in periods 1-3, it is consistent with cooperation. If they exert less effort when the stock of fish is below the socially optimal level than when it is above, it is consistent with a targeting of the social optimum. If they fish with the same intensity regardless of the social cost, we interpret it as evidence of non-cooperative behavior.

The results of the FieldDyna treatment are presented in Figure 5.5, where panel (a) shows the stock of fish remaining at the end of each period (S_t), and panel (b) shows the associated effort, averaged over all active groups, in the four periods. For comparability we have also included the average effort levels observed in FieldPI and FieldVCM in Figure 5.5(b).

At first glance, Figure 5.5(a) seems to suggest that participants acted fairly cooperatively; the size of the remaining stock at the end of each of the first three periods is positive. Indeed, of the eight groups participating in this treatment,

[20]Because it may not be feasible to catch the subgame perfect equilibrium quantity of fish in a one hour period, comparing the absolute stock of fish remaining with the point predictions of the two models may give a misleading impression of support for either model. In particular, if the remaining allowable catch is close to the socially optimal level, it may be a consequence of a binding feasibility constraint rather than an intention to cooperate.



(a) **(b)**

Figure 5.5 **(a)** Amount of allowable catch remaining (ACR) at the end of a period in the FieldDyna treatment, averaged over all groups. **(b)** Casts per minute in the FieldDyna treatment, averaged over all active groups.

only two actually depleted their quota before the final period, and only one group caught the total allowable catch in the first period. However, there is evidence from the catch data that the stock was not depleted in most sessions because catching the full quota in a period of one hour was not feasible. The allowable catch remaining at the end of the fourth period, Z_4, is greater than zero in six of eight groups. Furthermore, the allowable catch remaining (ACR) at the end of periods 1-3 is on average very close to the ACR at the end of period 4.

If cooperation is occurring, the average level of effort should be at a similar level in periods 1 to 3, and then increase in period 4. Figure 5.5(b), however, shows that effort starts at a high level in the first period, decreases slightly in the second, and remains approximately constant between the second, third and fourth periods. Furthermore, as the figure illustrates, effort in FieldDyna is not lower than effort in the FieldPI treatment, further suggesting that participants did not voluntarily limit their effort.

We also find that effort is independent of the current stock of fish. The model in section 5.4.1 and appendix 5.A.2 suggests that if players are cooperating, effort in periods 1-3 would be as great as possible if $S > S^* = 4$, and zero if $S \leq S^* = 4$. The relationship between the number of casts and the ACR is shown in Figure 5.6. The figure shows the average individual number of casts in the five minute intervals during which a specific stock level is reached.[21]

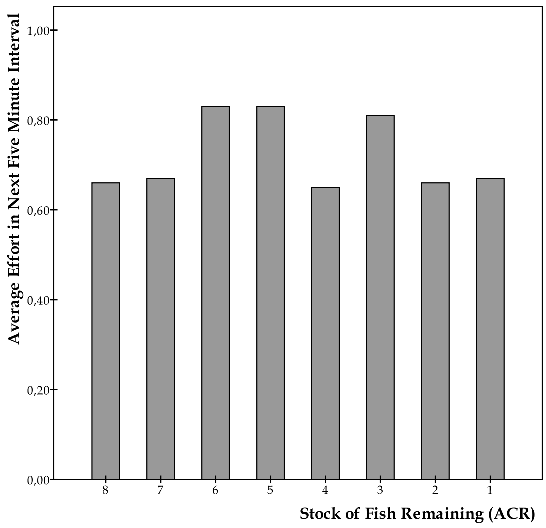


Figure 5.6 Average individual effort conditional on the allowable catch remaining (ACR) in period 1-3. Each 5 minute interval is an observation.

The figure reveals that the average effort level in a group is independent of the allowable catch remaining. There is no evidence that effort is greater for $S > 4$ than for $0 < S \leq 4$. The following result summarizes our findings with regard to cooperation in FieldDyna:

Result 6 In the dynamic social dilemma treatment, FieldDyna, no cooperation is observed. The lack of cooperation in FieldVCM is not specific to that particular treatment nor to a setting in which private and group gains are denominated in different units.

Support for result 6: Consider the differences in effort levels over the four one-hour periods (see Figure 5.5(b)). A Wilcoxon test indicates no difference in

[21]Data from the first ten minutes of a period are not included in the figure. During the first ten minutes of each period fishermen have the tendency to put in more effort than in the subsequent time intervals within a period. An explanation for this effect could be that fishermen are more eager to fish at the start of a new period. Alternatively, given their new spot at the pond, fishermen have to adjust the optimal settings of their rods by trial and error. Since all groups begin with a stock of eight fish in the first period, effort levels for this particular stock of fish are higher when the first ten minutes are included. Inclusion of the first ten minutes causes the average effort at a stock size of eight to equal 0.88 rather than 0.66. Excluding the first ten minutes of each period does not appreciably change the average effort at the other stock levels.

effort between the fourth period and the first period ($N_1 = N_2 = 6, p = 0.75$), taking the average effort levels of each group as an independent observation. Similar results are found when the fourth period is compared with either the second period ($N_1 = N_2 = 6, p = 0.67$), or the third period ($N_1 = N_2 = 6, p = 0.60$). Six observations are used, because two groups caught their allowable catch in a period before the fourth (one in period 1 and one in period 3).

Now consider the allowable catch remaining at the end of each of the four periods (see Figure 5.5(a)). A series of Wilcoxon tests indicate that a group's allowable catch remaining at the end of period 4 does not differ from that remaining at the end of each of the first three periods. The allowable catch remaining at the end of period 4 does not differ from that in period 1 ($N_1 = N_2 = 8, p = 0.60$), period 2 ($N_1 = N_2 = 8, p = 0.89$), or period 3 ($N_1 = N_2 = 8, p = 0.40$).

The difference in effort between the range of allowable catches remaining where it is in a group's interest to catch more fish (if the ACR is 5 or higher), and where it is socially harmful to catch fish (at four and lower), is investigated with a fixed effects panel data regression model. We only use the data for periods 1-3 because effort should be maximal in period 4, independently of the allowable catch remaining. The Fixed Effects estimates, which show no significant differences in effort between these two ranges of ACR, are presented in Table 5.3.

Effort levels in period 1, 2 and 3 are estimated as a function of ACR. Each period is divided into twelve five-minute intervals. We regress the amount of effort exerted by fisherman i in time interval $s+1$ on the ACR in interval s. The ACR is captured by a series of dummy variables capturing whether it is equal to 4, 3, 2, or 1. We use an indicator function $I(\text{ACR} = h), h \in \{1, 2, 3, 4\}$ which has value 1 if $S_s = h$, and zero otherwise. Therefore, the baseline against which each dummy variable should be interpreted is the range at which the ACR is five or greater. In addition, we insert a within-period time interval variable (Time) and dummies accounting for the influences of between-period effects. The variable Time is included to correct for a trend of effort within a period.

All ACR dummy variables are insignificant. Hence, irrespective of the amount of allowable catch remaining, fishermen fish with the same intensity as they do so when it is both individually and socially desirable to exert full effort. They make no attempt to replenish the resource when ACR levels are critically low. The only significant variables in this model are the variable Time and the dummy variable for the second period. The negative sign of Time indicates that fishermen exert less effort later in the one hour period, controlling for

Dependent Variable: Effort in interval $s+1$	
$I(\text{ACR}=4)$	-0.052
	(0.042)
$I(\text{ACR}=3)$	0.012
	(0.089)
$I(\text{ACR}=2)$	-0.062
	(0.082)
$I(\text{ACR}=1)$	-0.147
	(0.128)
Time	-0.014^{**}
	(0.005)
$I(\text{Period}=2)$	-0.090^{**}
	(0.038)
$I(\text{Period}=3)$	-0.097
	(0.069)
Constant	0.871^{***}
	(0.053)
N	937

Table 5.3 Fixed Effects regression on impact of amount of allowable catch remaining (ACR) on individual effort. Standard errors, clustered at the group level, are reported between parentheses. ***: significant at the 1%-level, **: significant at the 5%-level.

the amount of allowable catch remaining. Overall, we find that our subject pool of recreational fishermen displays a similar lack of cooperation in the FieldDyna treatment as in the FieldVCM treatment. ■

5.5 Conclusion

Our data are consistent with the predictions of classical game theory. In FieldVCM and FieldDyna, behavior conforms to the subgame perfect equilibria of the games that we created. Accordingly, we find no evidence of cooperative behavior in our framed field social dilemma experiment. We can detect no difference in behavior between a situation in which refraining from fishing yields a large positive externality to the group (the FieldVCM treatment) and when it does not (the FieldPI treatment). This conclusion contrasts sharply with results from studies of the VCM game when it is implemented in the laboratory. In such laboratory settings, cooperation is typically positive at the outset of a group's interaction, and declines over time. Therefore, our results shed doubt on the external validity of behavior observed in this type of laboratory experiment. While the behavior of recreational fishermen may not be not of special economic interest in itself, it is striking to see the difference in their behavior in the field compared to a contextualized laboratory environment.

Additional treatments allow us to explore potential causes of the difference between the results we have observed and those from previous laboratory studies. The treatments permit us to rule out four would-be explanations: (i) differences in contextualization between the game we implemented in the field and the standard VCM implemented in the laboratory, (ii) differences in the subject pool (students versus recreational fishermen), (iii) differences between the settings in which the experiments are conducted (the laboratory versus a more natural environment, the recreational fishing pond), and (iv) differences in the units in which the benefits and costs of cooperation are measured (money versus money, or money versus fish).

When implementing our modified version of the VCM game in the laboratory using student subjects, we find a pattern of behavior very similar to that typically observed in standard VCM lab experiments. In addition, we find that using students as participants lowers cooperation compared to our subject pool of recreational fishermen. Therefore, the use of students alone cannot account for the greater cooperation observed in received laboratory experiments than in FieldVCM. Conducting the experiments in the structured and formal setting of an experimental laboratory decreases cooperation among our subjects. They are more cooperative when participating in a voluntary contributions game while they are fishing than when they are in the laboratory. Therefore, the fact that the experiment is conducted outside the laboratory, cannot on its own account for the lack of cooperation.

The most plausible remaining explanation of the poor external validity of our laboratory experiments is the nature of the decision variable. Our subjects are unwilling to forego fishing to yield benefits to the group, even when group benefits are also in terms of fishing. Nevertheless, subjects from the same pool are willing to cooperate if it involves sacrificing own monetary earnings for the benefit of the group. Taken together, our data are consistent with the assertion that cooperativeness depends on the decision variable, the activity that must be modified in order to yield a benefit to the group. This statement is not to deny the importance of other factors; for example, whether similar results would apply to professional high sea fishermen remains an open question.

Some readers of this chapter have suggested that a demand effect may exist in the experiment, in that 'fishermen participate in the experiment to fish', and that when individuals find themselves at the fishing pond, the desire to fish overwhelms the money that we offer the group not to fish. However, we note that a similar effect exists with students who participate in traditional laboratory experiments: Students presumably participate in such experiments with the primary motivation of earning money for themselves. While fishermen might be disposed to feel that the pond is a place to fish, subjects in the laboratory presumably are disposed to view it as a place to earn money for themselves. Furthermore, in the FieldDyna treatment, payoffs are entirely in terms of fishing. Reducing one's own fishing increases the overall fishing opportunities available to the group. Thus, the tradeoff is fully in terms of the reward medium that is typically associated with the venue. As described earlier, we find no cooperation in FieldDyna, in agreement with standard economic theory, indicating that a demand effect of the type described above could not account for the lack of cooperation that we observe.

It has been shown in some field experiments that decentralized cooperation can be successful (see for example Erev et al. (1993) and Bandiera et al. (2005)). Cooperation can be found in naturally-occurring social dilemmas as well (see for example Ostrom (1990)). However, our results suggest that this successful cooperation does not spontaneously arise. When there is no contact possible between agents facing a social dilemma, the mere presence of potential group-level gains resulting from the sacrifice of private payoffs does not guarantee cooperation — even if the group concerned is small in number. The propensity to cooperate appears to depend on the nature of the activity that individuals must undertake, or refrain from, in order to increase group payoffs. It may be the case that to reliably achieve cooperation in a setting such as ours, some additional structure is required. This structure might be an effective avenue of communication between individuals (Isaac and Walker (1988a)), a system of punishment of non-cooperators (Fehr and Gächter (2000)), or a mechanism for increasing and maintaining social cohesion (Gächter and Fehr (1999); Masclet et al. (2003)). All of these factors have been found to increase the level of cooperation in laboratory social dilemmas. It is thus reasonable to conjecture that presence of one or more of these instruments may be necessary, or at least make it more likely, to achieve cooperation in some inhospitable field settings, such as the one we have studied here. On the other hand, cooperation may be so difficult to achieve in our setting that even these instruments may not be effective.

5.A Appendix

5.A.1 Statistical analysis of the effect of effort on catch

This appendix shows that the number of casts per minute, our Effort measure of cooperation, is correlated with the number of fish caught, which is used to calculate our Catch measure of cooperation. Thus, we establish that casts per minute is a legitimate measure of cooperation: A higher casting frequency increases expected private payoff and decreases expected group payoff.

An Ordered Probit model is used to estimate the effects of fishing effort on the number of fish caught, as presented in Table 5.4. The dependent variable is an individual's catch of fish in a period. Table 5.4 contains estimates of the pooled data from the FieldVCM and FieldPI treatments.

Dependent variable: Number of fish caught in a period	
Effort	0.739***
	(0.301)
I(High Season)	1.522***
	(0.127)
Quadrant Fixed Effects	Yes
N	456
pseudo-R^2	0.1928

Table 5.4 Ordered Probit estimation on the relationship between individual effort and individual catch. Standard errors, clustered at the subject level, are reported between parentheses. ***: significant at the 1%-level.

The model shows a clear positive and significant effect of our measure of effort, casts per minute, on the catch of fish. The dummy variable I(High Season) has a value of 1 when an observation is taken from the high season. The quadrant fixed effects are dummy variables that capture the position at the pond at which a fisherman is fishing. The quadrant dummy variables are insignificant, indicating that each position offers equal chances to catch fish.

5.A.2 The socially optimum and subgame perfect equilibrium extraction paths of the dynamic game

In this appendix we derive the socially optimal and closed-loop Nash equilibrium harvesting paths for the theoretical model in section 5.4.1. Assuming homogenous agents, we have a social dilemma if $\bar{X} \equiv n\bar{x} > G(S_{MSY})$; that is, if $\bar{X} > G(S)$ for all $0 \leq S \leq K$. This can be shown as follows. Consider first the social optimum in case of an infinite time horizon problem. The social welfare function can be derived by inserting (5.6) into (5.5), noting that this implies that $X \equiv \sum_{i=1}^{n} x_i = G(S) - \dot{S}$, and then summing V_i over all $i = 1, \ldots, n$:

$$\max_{X(t)} V = \int_{t=0}^{\infty} \bar{p}\left(G(S) - \dot{S}(t)\right) e^{-rt} dt. \tag{5.9}$$

Integrating by parts allows us to rewrite the objective function as:

$$\max_{X(t)} \left[\bar{p}S_0 + \bar{p}\int_{t=0}^{\infty} \left(G(S) - rS(t)\right) e^{-rt} dt\right]. \tag{5.10}$$

Objective function (5.10) is maximized if $dG(S)/dS$ and r are equated as quickly as possible. Using (5.7), we find that the socially optimal steady state stock is equal to $S^* = \max[0, K(\gamma - r)/(2\gamma)]$ and that the socially optimal harvesting levels equal $X = \bar{X}$ if $S > S^*$, $X = G(S)$ if $S = S^*$, and $X = 0$ if $S < S^*$.[22] In our dynamic field experiment r is equal to zero, and hence $S^* = K/2$ $(= S_{MSY})$. Also, if the time horizon is limited to T periods and assuming that $\bar{X} > K$, the socially optimal stock is $S^* = K/2$ in periods $1, \ldots, T-1$ and 0 in period T; to maximize group welfare, regeneration should be maximized in all periods apart from the last one, and the stock should be depleted in that terminal period.

Next we derive the unique Nash equilibrium harvesting path. The solution is straightforward: All $n \geq 2$ agents harvest the resource at maximum effort level $(\bar{x})$ until it is depleted, even if $\gamma > r$. This can be shown as follows. Consider $\widetilde{S}$ as a candidate interior equilibrium stock size (i.e., $\widetilde{S} \in \langle[0, K]\rangle$), which may or may not be equal to S^*. For $\widetilde{S}$ to be an interior steady state, all agents $j = 1, \ldots, n$ must harvest at $x_j = G(\widetilde{S})/n$ if $S = \widetilde{S}$, and choose $x_j = 0$ $(x_j = \bar{x})$ if $S < \widetilde{S}$ $(S > \widetilde{S})$.

That means that the amount of net regeneration agent i faces for any stock level S, $Q(S)$, equals:

$$\begin{aligned} Q(S) &= G(S) - (n-1)\bar{x} \text{ if } S > \widetilde{S}; \\ Q(S) &= G(\widetilde{S})/n \text{ if } S = \widetilde{S}; \\ Q(S) &= G(S) \text{ if } S < \widetilde{S}. \end{aligned}$$

[22] Hence, the socially optimal transition path towards the steady state is a so-called Most Rapid Approach Path (see for example Hartl and Feichtinger (1987)).

That means that if agent i decreases the stock infinitesimally below $\widetilde{S}$, she would increase the regeneration she faces ($Q(S)$) by almost a factor n (from $G(\widetilde{S})/n$ to infinitesimally less than $G(\widetilde{S})$). Forever harvesting at a rate such that the stock remains infinitesimally below $\widetilde{S}$ would then yield a present value of (almost) $\bar{p}G(\widetilde{S})/r$ for agent i and a zero payoff for all other agents $j \neq i$ in an infinite time horizon model. Clearly, this holds for all agents $i = 1, \ldots, n$ and for all $\widetilde{S} \in \langle 0, K]$, and hence the only steady state equilibrium stock is $S = 0$ for all $t = 1, \ldots, \infty$. This means that if one or more of the $n-1$ agents are greedy and harvest at maximum rate, no individual agent is able to keep the resource stock at the desired level, and hence each agent's best response is to harvest at maximum rate too (see also Clark (1980), or Dockner et al. (2000, p. 333-335)). Because $G(S) = 0$ if $S = 0$, the game ends in period 1. And if the unique closed-loop Nash equilibrium path is to deplete the resource in one period in the infinite time horizon model, it is also the unique subgame perfect equilibrium path in the finite time horizon model implemented in the field.

5.A.3 Promotional material

This appendix contains a translation of the flyer, which we used to recruit participants in the Field treatments. The original flyer is available upon request from the authors.

TILBURG UNIVERSITY REQUESTS YOUR PARTICIPATION IN A STUDY

DATE: ...

TIME: PRESENT AT 7.15 A.M., START OF THE STUDY AT 7.45 A.M.

- Duration: 4 hours.
- Participation is free.
- You can earn money during the study.
- Each participant can catch at most twelve fish.
- You can take home all of the fish you catch.
- You should use your own fishing equipment and bait.
- We will use pond 3.
- You will fish according to the standard rules of the trout fishing facility plus some modifications.
- You will be allocated more than one spot.
- You are not allowed to talk during the session.

5.A.4 General rules at the trout fishing facility

This appendix gives the rules for fishing which apply at the trout fishing facility. They are copied from their website, and translated into English.

Rules and regulations at the trout fishing facility

Everyone is cordially invited to fish at our recreational fishing facility. You are obliged to abide by the following rules.

- Entering the site is at your own risk.
- Do not cause unnecessary noise nuisance.
- Each person fishes with only one rod at a time.
- Fly-fishing is only allowed when there is enough space (we decide if this is so).
- Feeding the fish, in any way or form, is prohibited.
- Fishing is only allowed with natural bait and/or Trout Dough (no fish).
- Fishing with artificial bait, twister, dreg, jigs, shiner, etc., is not allowed.
- Throwing back trout (rainbow trout and salmon trout) into the pond is not allowed.
- All trout caught (rainbow trout and salmon trout) must be taken home.
- Using a keepnet is not allowed.
- Using a scoopnet to catch fish is not allowed.
- Any grass carp or catfish caught should be thrown back into the pond.
- Fishing at a different pond than the one you have selected upon entering is not permitted.
- You are allowed to clean fish only at the designated cleaning area.
- Everyone should keep the area clean, including the fish cleaning area.
- Damage to rented material due to incompetent use must be compensated for.
- We cannot be held accountable for theft, accidents, etc., which take place on our property.

5.A.5 Instructions for the FieldPI treatment

This appendix contains a translation of the instructions for the FieldPI treatment. Part (a) is the summary of the rules handed out to participants, who could reread it throughout the session. Part (b) is the text of the instructions read aloud at the beginning of the session.

a) Summary of the rules

Group formation

- You are placed in groups of 4 persons.
- The groups will remain the same throughout the entire session.
- We do not tell you who belongs to your group and you are not allowed to exchange information with other participants.

Timing

- The session lasts four hours, from around 8.00 a.m. until noon.
- If we begin later, we will stop later.
- The four hours will be divided into 6 periods of 40 minutes.

Stocking of the pond

- In the first period, we will put (16 x 2) + 6 = 38 rainbow trout into the pond.
- You are allowed to take home each fish you catch.
- We make sure that an equal number of rainbow trout is in the pond at the beginning of each period. We do this by putting in a new rainbow trout at the beginning of a new period for each fish that is caught in the previous period.
- In each period of 40 minutes, you are allowed to catch at most 2 fish. If you catch a salmon trout, it also counts as one fish.

In each period

- The number of rainbow trout put into the pond is equal to the number of fish caught in the previous period.

- You are not allowed to talk with the other participants.

In periods 1, 3 and 5:

- The fishing spots are determined by participants picking a numbered spot tag from a black linen bag.

At the end of the session:

- You will receive 5 euro for your participation.

(b) Instructions read aloud by the experimenter

Introduction

Welcome to this study by Tilburg University. Before we start, we want to point out two things. Firstly, this study is independent of the owners of the trout fishing facility. We are grateful that we are allowed to conduct this study here, but the owners have nothing to do with this project. All responsibility lies with Tilburg University. Secondly, we want to make clear that this study has nothing to do with animal welfare issues or the like. As researchers, we accept the rules and habits of recreational fishing as practiced at the trout fishing facility. We cannot tell you the exact aim of this study. We do want to stress that your privacy is guaranteed; none of the results we report can be traced back to individual participants.

As you know, you do not have to pay to take part in this study. The entrance fees are paid by Tilburg University. You are allowed to take home all fish you catch.

We ask you to abide strictly by the rules which we impose.

The session

In the next four hours, we ask you to adhere to the following rules. First, all rules that normally apply at the trout fishing facility remain in place. This means that it is not permitted to throw fish you catch back into the pond, you are only allowed to fish with one rod, you are only allowed to use a scoop net to set fish ashore, you are only allowed to use the usual bait, etc.

You are placed in a group with 3 other participants during the session. A group therefore consists of 4 persons. Your group remains the same for the whole session, but we do not inform you about who is in your group, and who is not. We urgently ask you not to talk to other people during the study. This is so important to us that we will exclude you from the session if you violate this rule.

The session takes a total of four hours, from about 8.00 a.m. until noon. If we start a little later, we will stop a little later. The study consists of 6 periods of 40 minutes. In the first period, we will put 2 rainbow trout into the pond for each participant. In addition, we put another 6 rainbow trout into the pond. There are 16 participants, and hence we will put (16 x 2) + 6 = 38 rainbow trout into the pond. You are allowed to take home all fish that you catch. We make sure that, at the beginning of each period, the number of trout in the pond is always equal to that at the beginning of all other periods. We do this by putting in a number of rainbow trout, at the beginning of a new period, equal to the total number of trout caught in the previous period.

In each 40 minute period you are allowed to catch a maximum of 2 fish. Any trout caught counts as one fish, whether it is a rainbow trout or a salmon trout. Whenever you have caught 2 fish and the period is not finished yet, you have to take your fishing line out of the pond. You then have to wait until the next period begins. You are not allowed to talk with others while you are waiting.

The spot at which you are located may influence the number of fish you can catch. Fishing spots are assigned in periods 1, 3 and 5. That means that you will be located for two periods of 40 minutes at each of your three spots. Spots are assigned by having participants pick a numbered spot tag out of a black linen bag.

Questions

If you have any questions regarding the session, you can ask them now, but also during the session. We do not answer questions about how to act in this study — all decisions you take are yours. We also do not answer questions about the purpose of this study. When we have analyzed the data, we will inform you about its results.

5.A.6 Instructions for the FieldVCM treatment

This appendix contains a translation of the instructions for the FieldVCM treatment. Part (a) is the summary of the rules handed out to the participants, who could reread it throughout the session. Part (b) is the text of the instructions read aloud at the beginning of the session.

a) Summary of the rules

Group formation

- You are placed in groups of 4 persons.
- The groups will remain the same throughout the entire session.
- We do not tell you who belongs to your group and you are not allowed to exchange information with other participants.

Timing

- The session lasts four hours, from around 8.00 a.m. until noon.
- If we begin later, we will stop later.
- The four hours will be divided into 6 periods of 40 minutes.

Stocking the pond

- In the first period, we will put $(16 \times 2) + 6 = 38$ rainbow trout into the pond.
- You are allowed to take home each fish you catch.
- We make sure that an equal number of rainbow trout is in the pond at the beginning of each period. We do this by putting in a new rainbow trout at the beginning of a new period for each fish that is caught in the previous period.
- In each period of 40 minutes, you are allowed to catch at most 2 fish. If you catch a salmon trout, it also counts as one fish.
- If you (decide to) catch less than two fish, we give money to the other three participants of your group.

- If you catch fewer fish than the two you can catch maximally, we divide 6 euro equally among the other 3 participants in your group for each fish you did not catch.
- Hence, for each fish you do not catch (or decide not to catch), each of the other 3 participants in your group receives 2 euro. This means that:
  - If you catch 2 fishes, the other 3 participants in your group do not receive any money.
  - If you catch 1 fish, each of the other 3 participants in your group receives 2 euros.
  - If you catch 0 fishes, each of the other 3 participants in your group receives 4 euros.
- This holds for all participants. This means that you will receive 2 euro for each fish that is left in the pond by the other participants in your group.

In each period

- The number of rainbow trout put into the pond is equal to the number of fish caught in the previous period.
- You are not allowed to talk with the other participants.

In periods 1, 3 and 5:

- The fishing spots are determined by participants picking a numbered spot tag from a black linen bag.

At the end of the session:

- You receive €2 for every fish not caught by the other three members of your group over the 6 periods.

(b) Instructions read aloud by the experimenter

Introduction

Welcome to this study by Tilburg University. Before we start, we want to point out two things. Firstly, this study is independent of the owners of the trout fishing facility. We are grateful that we are allowed to conduct this study here, but the owners have nothing to do with this project. All responsibility lies with Tilburg University. Secondly, we want to make clear that this study has nothing to do with animal welfare issues or the like. As researchers, we accept the rules and habits of recreational fishing as practiced at the trout fishing facility. We cannot tell you the exact aim of this study. We do want to stress that your privacy is guaranteed; none of the results we report can be traced back to individual participants.

As you know, you do not have to pay to take part in this study. The entrance fees are paid by Tilburg University. You are allowed to take home all fish you catch. In addition, you can earn money. We ask you to abide strictly by the rules which we impose.

The session

In the next four hours, we ask you to adhere to the following rules. First, all rules that normally apply at the trout fishing facility remain in place. This means that it is not permitted to throw fish you catch back into the pond, you are only allowed to fish with one rod, you are only allowed to use a scoop net to set fish ashore, you are only allowed to use the usual types of bait, and so on.

You are placed in a group with 3 other participants during the session. A group therefore consists of 4 persons. Your group remains the same for the whole session, but we do not inform you about who is in your group, and who is not. We urgently ask you not to talk to other people during the study. This is so important to us that we will exclude you from the session if you violate this rule.

The session takes a total of four hours, from about 8.00 a.m. until noon. If we start a little later, we will stop a little later. The study consists of 6 periods of 40 minutes. All the money you earn during the study is paid to you at the end. In the first period, we will put 2 rainbow trout into the pond for each participant. In addition, we put another 6 rainbow trout into the pond. There are 16 participants, and hence we will put $(16 \times 2) + 6 = 38$ rainbow trout into the pond. You are allowed to take home all fish that you catch. We make sure that, at the beginning of each period, the number of trout in the pond is always equal to that at the beginning of all other periods. We do this by putting in a number of rainbow trout, at the beginning of a new period, equal to the total number of trout caught in the previous period.

In each 40 minute period you are allowed to catch a maximum of 2 fish. Any trout caught counts as one fish, whether it is a rainbow trout or a salmon trout. If you catch fewer than 2 fish, or decide to catch fewer than 2 fish, the other members of your group receive money. For each fish you catch fewer than 2, the other members of your group receive, in total, 6 euros, to be divided equally among the three of them; they thus receive 2 euros each.

This means that if you catch 2 rainbow trout, the other 3 participants in your group do not receive any money. If you catch 1 rainbow trout, each of the other 3 participants in your group receives 2 euro. If you catch 0 rainbow trout, each of the other 3 participants in your group receives 4 euro. This rule holds for all participants. Hence, you receive 2 euro for every fish anyone of your three group members decides to catch fewer than 2 in any period.

The spot at which you are located may influence the number of fish you can catch. Fishing spots are assigned in periods 1, 3 and 5. That means that you will be located for two periods of 40 minutes at each of your three spots. Spots are assigned by having participants pick a numbered spot tag out of a black linen bag.

Questions

If you have any questions regarding the session, you can ask them now, but also during the session. We do not answer questions about how to act in this study — all decisions you take are yours. We also do not answer questions about the purpose of this study. When we have analyzed the data, we will inform you about its results.

5.A.7 Instructions for the StuLab, FisherLab and FisherPond treatments

Part (a) of this appendix presents the translation of the instructions for the StuLab treatment. Part (b) indicates how the instructions for the FisherLab and FisherPond treatments differed from those of the StuLab treatment.

a) Instructions for the StuLab treatment

Introduction

Welcome to this study by Tilburg University. You can earn money during the study. The amount of money you can earn depends on your decisions during the session and on the decisions of others. We will read out aloud the instructions now, and you are invited to read along.

The session

In this session you are placed in a group with 3 other participants during the session. A group therefore consists of 4 persons. Your group remains the same for the whole session, but we do not inform you about who is in your group, and who is not. We urgently ask you not to talk with others during the study. This is so important to us that we will exclude you from the session if you violate this rule.

The study consists of 6 periods in which we mimic a scenario at a fishing pond. However, instead of really catching fish, you are requested to decide how many fish you would like to catch. The rules of the study are as follows. In each of the 6 periods, you are allowed to catch a maximum of 2 fish. You receive 1 euro for each fish you catch. For each fish you catch fewer than 2, the other members of your group receive, in total, €1.50, to be divided equally among the three of them; they thus receive €0.50 each.

That means that if you catch 2 fish, you receive €2 and the other three members of your group do not receive any money. If you catch 1 fish, you receive €1 and the other three members of your group receive 1 × €0.50 = €0.50 each. If you catch 0 fish, you receive €0 and the other three members of your group receive 2 × €0.50 = €1 each. This rule holds for all participants. Hence, you receive €0.50 for every fish anyone of your three group members decides to catch fewer than 2 in any period.

Examples

Suppose that you and all your other group members catch 0 fish. You will earn €3. This amount consists of the 2 × €0.50 = €1 as a consequence of the choice of each of your other group members. Because there are 3 other group members, you will earn 3 × €1 = €3. Because you have not caught a fish yourself, you will earn nothing due to your own fishing activities.

If you catch 2 fish while all of your other group members catch 0, you will earn €5. This amount consists of the 3 × €1 as a consequence of no catch of your other group members plus the earnings from your own fishing activities, 2 × €1 = €2.

Suppose that you and all your other group members catch 2 fish. You will earn €2. This amount consists of the 2 × €1 of the two fish you have caught. Because all your other group members have also caught 2 fish, you will earn no money.

If you catch 0 fish and all of your other group members catch 2 fish, then you will earn €0. You will earn nothing as a consequence of your own fishing activities. Because no other group member leaves a fish, you will earn nothing.

Filling in the form

You can indicate your choices for each of the 6 periods on the form we handed out. We will now explain how you can do this. Please look at the form now.

In the upper right corner please fill in your participant number. This number is the one marked on the sticker on your table. Please make sure to fill in the correct number; we need this in order to make the payments at the end of the session.

On the form you find the choices you can make. Below these options, we have printed the numbers 0, 1, and 2 next to each period. We ask you simply to circle your choice for each period.

When you have made your choice for period 1, please put the form, face down, on your desk, so that we know you are done making your decision for the period. When all participants have made their choice, we collect all of the forms. We calculate how much you receive in this period, and fill the information out on the form. We then return the form to you, so that you know how much you have earned. On the form, we also write the total number of fish your group has caught. In addition, we write down the decisions of all participants in the session on the white board in front of you. Note that the white board does not provide information on which participant is placed in your group.

After this procedure, period 2 starts. We ask you to make your choice for this period by circling the appropriate number, and place your form face down on top of your desk. When everyone has made their choice, we collect the forms, and calculate how much money each participant receives in period 2. We inform each participant about the decisions of their group members, and how much money he/she receives. Also, we again write the decisions of all participants on the white board. This procedure is repeated until all 6 periods are finished. Note that you should not make a choice for a period that has not yet begun.

At the end of the session, we give all participants their receipts in private by inviting them, one by one, to the adjacent room. When you have collected your earnings, the session is finished. There is no reason for you to return to this room, so please take all your belongings when your name is called.

Questions

If you have any questions regarding the session, you can ask them now, but also during the session. We do not answer questions how to act in this study — all decisions you take are yours. We also do not answer questions about the purpose of this study. When we have analyzed the data, we will inform you about the results.

Test questions

We now proceed with a short test. Once all participants have answered these questions correctly, the session will begin.

1. With how many other participants will you be placed in a group?

2. How much money will you earn due to your own fishing activity when you decide to catch 2 fish in a period?

3. How much money will you earn when the following decisions are made in a period?

- You catch 1 fish,
- Two other members of your group catch 0 fish,

- One other member of your group catches 2 fish.

The form

The session consists of 6 periods. In each period, we ask you to choose one of the following options.

(0) You catch 0 fish. You receive €0 and the other members of your group receive 2 × €0.50 = €1 each;
(1) You catch 1 fish. You receive €1 and the other members of your group receive 1 × €0.50 each);
(2) You catch 2 fish. You receive €2 and the other members of your group each receive €0.

	Your choice			Total group catch	Your earnings
Period 1	0	1	2		
Period 2	0	1	2		
Period 3	0	1	2		
Period 4	0	1	2		
Period 5	0	1	2		
Period 6	0	1	2		

b) Instructions for the FisherLab and FisherPond treatments

The instructions for the FisherLab treatment are identical to those for the StuLab treatment. The instructions for the FisherPond treatment only differ from those of StuLab and FisherLab treatments with respect to the mechanics of the experiment's implementation. In the FisherPond treatment decisions sheets were to be handed back to the experimenter (rather than put on the participant's table for the experimenter to collect) and information about the decisions of the other participants in the session were shown on a paper sheet for the participant to peruse rather than recorded on a white board.

5.A.8 Survey about the value of fishing

Dear participant,

We are conducting a study on behalf of Tilburg University. We would like you to answer the following questions. What we ask of you, is to answer the following questions, which relate to a hypothetical scenario we describe now.

Suppose that you are fishing for rainbow trout. However, there is an additional rule to the usual rules that apply here at the Biestse Oevers: you have to pay for each rainbow trout that you catch. You are not allowed to put fish that have been caught back into the pond. What price would be the most you would pay to catch rainbow trout?

Price per rainbow trout caught	I would like to catch ... fish
€0.50	> 10/9/8/7/6/5/4/3/2/1/0
€1	> 10/9/8/7/6/5/4/3/2/1/0
€1.50	> 10/9/8/7/6/5/4/3/2/1/0
...	...
€15	> 10/9/8/7/6/5/4/3/2/1/0

5.A.9 Instructions for the FieldDyna treatment

This appendix is a translation of the instructions for the FieldDyna treatment. Part (a) is the summary of the rules handed out to participants, who could reread it throughout the session. Part (b) is the quiz that participants took before their session began. Part (c) is the text of the instructions read aloud at the beginning of the session.

(a) Summary of the rules of the study

Group formation

- You are placed in groups of 4 persons.
- These groups remain the same throughout the entire session.
- The other members of your group wear a ribbon of the same color as you do.
- You are not allowed to talk to other people during the study.

Timing

- The session lasts four hours, from around 8.00 a.m. until noon.
- If we begin later, we will stop later.
- The four hours are divided into 4 periods of 1 hour each.

Stocking the pond

- In the first period, we will put $(4 \times 8) + 6 = 38$ rainbow trout into the pond.
- You are allowed to take home each fish you catch.
- At the beginning of each new period, we put fish into the pond equal to the total number of fish caught by the present participants in the previous period.
- The number of fish in the pond per active participant is therefore equal at the start of each period.

Catching fish

- Each fish you catch is yours to take home. For every fish you catch, you also receive €5. Rainbow trout and salmon trout count both as one fish.

Available fish for your group

- Each period, you and your other group members allowed to catch at most the number of fish available to your group.
- In the first period this is 8 fish.
- The number of fish available for your group does NOT depend on the number of fish caught by other groups.
- The number of available fish for your group in the current period depends ONLY on the number of fish your group left in the pond at the end of the previous period. See the table below.
- Whenever a group catches all fish that were available to that group, all members of that group have to stop fishing and are requested to leave the pond area.
- We pay your earnings at the end of the period in which your session ends.

- Example:
  - In the first period the total number of fish available to your group equals 8.
    - The maximum number of fish that your group is allowed to catch in the first period, is 8 fish.
    - When 8 fish are caught, all members of your group have to take their fishing lines out of the water and have to leave the pond area.
  - Suppose your group catches 6 fish in the first period.
    - At the beginning of the second period, there are 2 fish left from the first period and 3 new fish are put into the pond; see the table.
    - Your group is then allowed to catch $2 + 3 = 5$ fish in the second period.
    - If your group catches all 5 fish in the second period, all members of your group have to take their fishing lines out of the water and have to leave the pond area.
  - If your group catches less than 5 fish, new fish will be placed into the pond, as indicated in the table.
    - Suppose your group catches 1 fish in the second period.
    - Then, at the end of the period there are $5 - 1 = 4$ fish left for your group.
    - The available number of fish for your group in the next period is then raised by 4 fish (see the table), and the total number of available fish in period 3 is $4 + 4 = 8$.

Your fishing spot

- Each group has four spots, each member of the group fishes for one period at each of these four spots.
- You draw a numbered spot tag out of a black linen bag which indicates your spot for the first period.
- You receive a ribbon at the beginning of the session.
- At the end of each period of one hour, we inform you at what spot you will be fishing in the next period.

Number of fish left at the end of the previous period by your group	Increase in the number of fish available to your group	Number of fish available to your group in the coming period
0	0	0
1	2	3
2	3	5
3	3	6
4	4	8
5	3	8
6	3	9
7	2	9
8	0	8
9	2 fish subtracted	7

- You are not allowed to talk to other people during the session.

(b) Test questions

1. How many other group members are in your group, besides you?

2. Suppose your group catches 6 fish in the first period.
a. What is the number of fish still available to your group at the end of the first period?
b. The number of fish available to your group is then increased by how many fish in period 2?
c. What is the maximum number of fish your group is allowed to catch in period 2?
Suppose next that you and the other participants of your group catch all available fish in period 2.
d. What is the maximum number of fish your group is allowed to catch in the third period?
e. What is the maximum number of fish your group is allowed to catch in the fourth period?

3 a. What is the maximum number of fish your group is allowed to catch in the first period?
b. How many fish should your group have left at the end of the first period in order to have the largest increase in fish available to your group at the start of period 2?
c. How much is this increase?
d. What is the total number of fish available for your group in the next period?

(c) Instructions read aloud by the experimenter

Introduction

Welcome to this study by Tilburg University. Before we start, we want to point out two things. Firstly, this study is independent of the owners of the trout fishing facility. We are grateful that we are allowed to conduct this study here, but the owners have nothing to do with this project. All responsibility lies with Tilburg University. Secondly, we want to make clear that this study has nothing to do with animal welfare issues or the like. As researchers, we accept the rules and habits of recreational fishing as practiced at the trout fishing facility. We cannot tell you the exact aim of this study. We do want to stress that your privacy is guaranteed; none of the results we report can be traced back to individual participants.

As you know, you do not have to pay to take part in this study. The entrance fees are paid by Tilburg University. You are allowed to take home all fish you catch. In addition, you can earn money. We ask you to abide strictly by the rules which we impose.

The session

In the next four hours, we ask you to adhere to the following rules. First, all rules that normally apply at the trout fishing facility remain in place. That it is not permitted to throw fish you have caught back into the pond, you are only allowed to fish with one rod, you are only allowed to use a scoop net to set fish ashore, you are only allowed to use the usual types of bait, etc.

You are placed in a group with 3 other participants during the session. A group therefore consists of 4 persons. The group remains the same throughout the study. Each participant receives a ribbon. The members of your group have the same color ribbon as you have. We urgently ask you not to talk to other people during the study. This is so important to us that we will exclude you from the session if you violate this rule.

The session consists of four periods of one hour. The session therefore takes four hours, from 8.00 a.m. until 12.00 a.m. If we start a little later, we will stop a little later.

In the first period, we put 8 rainbow trout into the pond for each of the four groups. In addition, we put another 6 rainbow trout into the pond, and hence we put $(4 \times 8) + 6 = 38$ rainbow trout into the pond. At the beginning of period 2, 3, and 4 we put a number of fish into the pond equal to the number of fish caught in the previous period by all active participants. This means that the number of fish in the pond for each active fisherman is the same at the beginning of each period.

Each fish you catch is yours to take home. In addition, you receive €5 for each fish you catch. During the study, rainbow trout and salmon trout both count for 1 fish.

Although the number of fish per participant remains constant over all periods, you are not allowed to catch fish without limit. Each period of one hour, you and your group members are not allowed to catch more than the maximum number of fish available to your group. In the first period, this maximum number of fish available is 8 fish, and you and your group members are not allowed to catch more than 8 fish in this period. Keep in mind that each fisherman is allowed to catch as many fish he or she can or wants to, as long as the total number caught by the group in the first hour is not more than 8 fish.

In the second period, the number of fish available to your group changes. The number of fish available to your group does not depend on the number of fish caught by participants of other groups. The number of fish available to your group in the next period depends only on the number of fish still available to your group at the end of the current period.

The way in which this works is indicated in the table.

Whenever the number of fish caught is such that the number of fish available to your group at the end of the period equals zero, the number of available fish is not increased and your group is not allowed to catch any more fish. The session is over for your group. We pay your earnings at the end of the period in which your group has caught its maximum available fish.

Whenever the number of fish caught is such that at the end of a period 1 fish is left to your group, the number of fish available to your group is raised by 2, and your group is allowed to maximally catch 3 fish in the next period.

Whenever the number of fish caught is such that at the end of a period the number of fish available to your group equals 4, the number of fish available to your group is raised by 4, and your group is allowed to maximally catch 8 fish in the next period.

Whenever the number of fish caught is such that at the end of a period the number of fish available to your group equals 6, the number of fish available to your group is raised by 3, and your group is allowed to maximally catch 9 fish in the next period.

Let us consider an example. In the first period, the total number of fish available to your group equals 8. This means that your group is allowed to catch at most 8 fish in this period. When the 8th fish is caught, all members of the group have to take their fishing lines out of the water. The session is then over for your group and you have to leave the pond area.

Suppose your group does not catch 8 fish in the first period, but rather 6. In that case there are $8 - 6 = 2$ fish still available to your group at the end of the first period. The table shows that when 2 fish are left to a group at the end of a period, the total number of fish this group is allowed to catch is increased by 3 fish at the beginning of the second period. At the beginning of the second period there are hence $2 + 3 = 5$ fish available for your group. Once again, the number of fish caught by other groups has no influence on the number of fish your group is allowed to catch.

When your group catches 5 fish in the second period, all members of your group have to take their fishing lines out of the water at the moment the fifth fish is caught. If your group catches fewer than five fish in the second period, the number of fish available to your group is again increased. The increase in the number of fish available for your group depends on the number of fish left at the end of the second period, as indicated in the table. Suppose your group catches one fish in period 2, then the total number of fish still available to your group at the end of that period is $5 - 1 = 4$ fish. In the table you can see that in that case, the number of available fish is raised by 4 fish. The number of available fish for your group is 8 in period 3, in this example.

Each group of fishermen is allocated 4 fishing spots. You will be located for one period at each of those group spots. You will draw a number out of a bag which indicates the spot at which you will be located during the first period. You will receive a ribbon before the session starts. At the end of each period we inform you at which spot you will be located in the next period. We want to stress again that it is important that you are not allowed to talk during the session. This is of such importance, that we will exclude you from the session if you violate this rule.

Questions

If you have any questions regarding the session, you can ask them now, but also during the session. We do not answer questions about how to act in this study — all decisions you take are yours. We also do not answer questions about the purpose of this study. When we have analyzed the data, we will inform you about its results.

Before the session starts, we ask you to answer some test questions. You can do this at the spot at which you will be fishing in the first period. Only when all participants have answered all questions correctly, the session starts.

Chapter 6

Monetary Punishment in the Field[1]

6.1 Introduction

Over the past two decades, scholars have had an increasing interest in human behavior in social dilemmas. The main work horse for social dilemmas in group settings is the voluntary contribution mechanism. In this game, agents face a situation in which they can engage in a (selfish) activity that benefits themselves only, or they can engage in a (pro-social) activity that benefits all members of the group. The greatest total payoff can be obtained when all members cooperate, by devoting their efforts fully to the pro-social activity. The dilemma is then created as follows: All agents profit equally from the total pro-social investments, even the ones who have not cooperated. However, for each agent, the private returns from the selfish activity are greater than the private returns from the pro-social activity. Assuming that agents are rational and play the game a finite number of times, the prediction is that no agent will ever contribute to the pro-social activity, hoping to free ride on the activities of others.

A large body of literature has tested the predictions of the voluntary contribution mechanism using laboratory experiments (see for example Isaac et al. (1985), Andreoni (1988), and Isaac and Walker (1988b)). Unlike the dire prediction that all agents act purely selfishly, the majority of laboratory results shows two robust patterns (Ledyard (1995)): (i) initially, agents contribute between

[1]I would like to thank Ben and Shirley Willems for use of their fishing facility, and Paul Ludeña Delgado, Joris Hoendervangers, Stef van Kessel, and Menusch Khadjavi for excellent research assistance.

forty and sixty percent of their efforts to the pro-social activity, and (ii) as the game is repeated, more and more effort is allocated towards the selfish activity.

In the previous chapter the robustness of these findings is challenged by conducting a field experiment, closely related to the voluntary contribution mechanism. In this field experiment, fishermen can catch up to two fish in each of the six periods. Each fish not caught yields a monetary payoff of €2 to each of the other three group members. Unlike in the lab, no cooperation is observed in any of the six periods of the game. Fishermen catch as much fish, and exert as much effort in the social dilemma game as in a control treatment where no incentives are provided to reduce the catch of fish. By conducting a series of additional laboratory and field treatments, it is ruled out that the subject pool, the physical environment, or the fact that the medium of reward (money versus fish) explains the lack of cooperation.

Following the results reported by laboratory experiments on the decline in cooperation in social dilemmas, the literature has focused on the question how to promote cooperation. A particular interest is shown in instruments that are used on a peer-to-peer basis, rather than on instruments used by a formal institution such as a government. Among the most studied instruments is peer-to-peer punishment. In laboratory experiments, punishment is implemented as follows: After each agent has made a decision at what level to cooperate to the pro-social activity, all agents learn about each other's decisions. Then, they are given the opportunity to make a costly investment to reduce the earnings of other group members. The theoretical prediction, assuming rational agents, is that no use of the instrument is made. Since punishment is costly, those who punish achieve lower payoffs than those who do not. Agents prefer to free ride on the punishment activities of others, a phenomenon called second-order problem of free riding (Henrich and Boyd (2001)). Likewise, if the social dilemma game is played a finite number of times, backward induction causes subjects to refrain from using costly punishment or reward. In the last period, no use of the instrument will be made, because future behavior cannot be changed. Anticipating that no use of the instrument is made in the last period, free riding will prevail in the last period. In the next-to-last period, no use of the costly instrument will be made as well, because agents will not defer from free riding in the last period. Hence, free riding will occur in this period as well. This logic can be extended all the way to the first period. In contrast to the theoretical predictions, not only is costly punishment used in the laboratory on a large scale, the instrument succeeds in promoting cooperation. Punishment has been found to be extremely effective, as reported in studies of Yamagishi (1986), Ostrom et al. (1992), and Fehr and Gächter (2000,2002).[2]

The effects of monetary punishment in a field setting are tested in this chapter. This is done by adding a punishment stage to the FieldVCM and FieldDyna treatments presented in Chapter 5. Studying punishment in the field setting of Chapter 5 is interesting for two reasons. First of all, there has never been conducted an experiment before that explicitly tests the effects of punishment in a non-laboratory environment. Taking traditional laboratory methods to field settings is a relatively new phenomenon, and this chapter extends the field experimental literature in a new direction. Secondly, the recreational fishing pond is interesting because it is likely that fishermen who catch a lot of fish gain respect of fellow fishermen. It is possible that those who are uncooperative are seen as 'good fishermen', because they are able to catch a lot of fish. Consequentially, it could be the case that uncooperative fishermen do not attract a lot of punishment. In a laboratory experiment on the Ultimatum Game, responders approve of much lower payoffs when the allocator has earned the right to become an allocator (see Hoffman et al. (1994)). Allocators have proven to be more skillful in a task conducted in the laboratory (scoring high on a knowledge quiz), and the respect that they have earned with this translates into a larger cash inflow. A similar mechanism can uphold in the field setting under study. The main advantage is that levels of respect come naturally, rather than that this is created in an artificial way.

Compared to the previous chapter, the setup of the FieldVCM and FieldDyna experiments is changed slightly. The six periods of which the FieldVCM treatment exists, are divided into two initial periods of the baseline game, and then four later periods of the game with punishment. A treatment of the FieldDyna experiments consists of three periods of 35 minutes, rather than the four one-hour periods of the FieldDyna experiment from Chapter 5. The way in which subjects can punish each other in FieldVCM and FieldDyna is very similar to the way it is implemented in traditional laboratory experiments. After each period, subjects receive information about the catch and earnings of each fellow group member. Then, all subjects receive an endowment of €3, which they can use to reduce the monetary earnings of other group members. Each euro spent reduces the earnings of a group member by three euro. After each subject has made his or her decision in private, feedback is given on the total amount of punishment received by the other group members. Subjects are also informed on the total punishment received by each of the group members.

[2] Not in all societies is punishment effective. Herrmann et al. (2008) show that in some societies punishment leads to a breakdown of cooperation.

Unlike the findings of traditional laboratory experiments, we find no evidence that monetary punishment promotes cooperation. In the FieldVCM treatment, fishermen fish with the same intensity, whether punishment opportunities are present or not. Effort levels are greater than those exerted in the FieldPI treatment reported in Chapter 5. Any evidence of partial cooperation is absent as well; fishermen always try to catch their first fish, and conditional on catching one fish, they fish with the same intensity to catch the second fish. Likewise, there is no evidence that monetary punishment promotes cooperation in the FieldDyna treatment. The fishermen provide equal effort levels to catch fish, independent of the stock level remaining.

The setup of this chapter is as follows. Section 6.2 describes the experimental design and experimental procedure. The procedure deserves extra attention, because weather influences caused us to change the way in which we gathered data. During one of our sessions, the weather was too warm for the fish to bite in large numbers. In another session, a sudden storm prevented us from completing all periods. The threat of lightning caused us to abort the session due to safety concerns for the participants. In section 6.3 the data are presented, and section 6.4 gives some concluding remarks.

6.2 Experimental design and procedure

In this section, the experimental designs of the FieldVCMPun and FieldDynaPun treatments are described.

6.2.1 Experimental design of FieldVCMPun

The experiments were conducted at the privately owned recreational fishing site 'de Biestse Oevers'. A detailed description of this field setting can be found in Chapter 5. Recruitment of the participants was done two weeks in advance, by handing out flyers at the Biestse Oevers. A maximum of sixteen participants was allowed to take part in a session.

The FieldVCMPun session consisted of six periods of 35 minutes each. The session was divided into two treatments, the baseline and punishment treatment. In the first two periods, the baseline treatment was conducted while the remaining four periods consisted of the punishment treatment.

In the baseline treatment, participants were placed in groups of four. Each fisherman was allowed to catch up to two fish in each period; each fish caught had to be taken home. Once a participant caught the second fish, he or she had to wait until the start of the next period. The dilemma was created as follows:

For each fish that a fisherman did not catch, the other three group members received €2. Each participant therefore faced a tradeoff; either take home one fish and experience the utility of catching it, or forego the act of fishing and raise the earnings of the other group members with €6 in total. The subgame perfect Nash equilibrium of this game is that all agents catch two fish in each period. However, the social optimum requires all agents to forego their entire catch. For a detailed discussion of the establishment of a social dilemma in this game, see Chapter 5, section 2.2. At the start of the experiment, $16 \times 2 + 6 = 38$ rainbow trout were released into the pond. After each period, a new rainbow trout was put into the pond for each fish caught. This means that in every period, the pond was stocked with 38 rainbow trout, plus the stock of fish that was already present from past days. At the end of each period, all participants received feedback on the catch and earnings of each of the other group members.

The description of the punishment treatment is similar to the baseline treatment, with the exception that participants were given the opportunity to sanction fellow group members. Subjects were placed in the same groups as in the baseline treatment. They were given this information at the start of the punishment treatment. At the end of each of the four periods, subjects received an endowment of €3. Subjects then received information on the individual catch and earnings of each of the other group members. A subject could decide to spend €1 to reduce the earnings of a fellow group member by €3. No restrictions were placed on how punishment could be targeted, as long as no less than €0 and no more than €3 was spent. Each euro that a subject did not spent, was added to his or her total earnings. In case a subject's earnings within a period would be reduced to below €0, then the earnings of previous periods would be reduced, including those of the baseline treatment. If a subject had negative earnings, then the total earnings were set to €0. A subject could never have a 'debt'; future earnings were not reduced in case a subject had negative earnings at the end of any period. For example, if a subject would have negative earnings at the end of period 3, but earned money in period 4, then the total earnings of that subject were equal to the earnings made in period 4. Finally, after each subject made his or her punishment decisions, but before the new period began, individual feedback was given on the total amount of punishment received. Subjects were also informed on the catch and sum of punishment received by each of the other group members. This kind of feedback differs from usual practice, where no feedback is given on the sum of punishment received by other group members. Note that because punishment is costly, backward induction causes rational agents to never use it in a repeated game, and the subgame perfect

Nash equilibrium is the same as the one of the baseline treatment.

The instructions of the baseline treatment were explained at a central place.[3] All subjects were informed that they would play two periods of the baseline treatment, after which new instructions for part 2 were explained at the central location. After the instructions for part 1 were explained, all subjects were requested to randomly draw a spot tag out of a black linen bag to determine their fishing spot for the first two periods. Before period 1 began, each participant had to make test questions at their fishing spot. Only when all subjects correctly answered all questions the experiment would continue. Test questions were also made before the beginning of the second part of the experiment. At the end of the second period, fishermen were told that their new fishing spot was six places to the left of their current one. Similarly, at the end of the fourth period, subjects were told that their new fishing spot was again shifted six places to their left side.

6.2.2 Experimental design of FieldDynaPun

As an additional way to study the influences of monetary punishment on cooperation in a social dilemma, the FieldDyna treatment is considered. A complete description of that experiment and the theoretical predictions can be found in section 5.4.1 and 5.4.2. Some changes have been made in the sessions of the FieldDynaPun treatment, which are described below.

The FieldDynaPun sessions consisted of two parts, each consisting of three periods of 35 minutes. One part consisted of the baseline treatment, and one part consisted of the punishment treatment. In the FieldDynaPun session, subjects were placed in groups of four. In total, sixteen subjects participated in a session. All participants were told with whom they were grouped by issuing each player with colored ribbons. In the baseline treatment, a group was allowed to catch up to eight fish in the first period. Each fish caught was his to keep and yielded earnings of €3 to the one who caught it. The logistic growth function described in section 5.4.2 was used to link catch in one period to the group quota in the next period. Assuming rational agents, the subgame perfect Nash equilibrium predicts that all eight fish are caught in the first period, and that no fish can be caught in the second and third period. The social optimum can be reached if group catch is limited to four fish in the first two periods, so that eight fish can be caught in period 3. A score board was used so that all subjects could see the available catch for his or her group. This score board was used so that

[3]The instructions of the FieldVCMPun session can be found in the appendix.

subjects were not confused about the remaining stock of fish; in case we find no evidence of cooperation, then this information makes it even more convincing that subjects act out of self interest. At the end of a period, individual feedback was given on the following: i) the total individual earnings, ii) the total group catch, iii) the catch and earnings of each other group members, iv) the group quota left at the end of the previous period, v) the increase of the group quota, and vi) the size of the resulting allowable catch for the next period. At the beginning of the new period, a number of rainbow trout was put into the pond equal to the number of fish caught in the previous period by fishermen who are active in the new period. This makes sure that the intended harvesting path of one group does not influence the intended harvesting path of another group in the same session.

The description of the punishment treatment is similar as the baseline treatment, with the exception of a punishment opportunity. Punishment was implemented the same way as in the FieldVCMPun treatment: After each period, subjects received feedback on the catch and earnings of their group members and had the opportunity to reduce their earnings. Subjects received a budget of €3, each €1 spent reduced the earnings of a group member with €3. A subject could never have negative earnings; in case a subject would have negative earnings in a period, past earnings would be reduced. Like in the FieldVCMPun treatment, future earnings were not reduced in order to recover from negative earnings from past periods. The procedure in which the subjects made their punishment decisions was very similar as in the FieldVCMPun treatment: After each subject had made their punishment decisions, feedback was provided on the sum of punishment received. Feedback was also provided on the catch and sum of punishment received by each of the other group members. Since punishment is costly and the game is finitely repeated, the subgame perfect Nash equilibrium is not changed; agents would never use punishment and all eight fish are caught in the first period.

At the start of a session, subjects were requested to randomly draw a spot tag out of a black linen bag. The spot tag indicated their fishing spot for the first three periods of the session. The participants were then told that the instructions would be given in a decentralized way, in groups of four participants (the decentralized instructions were given to four members of different groups).[4] All participants were told that everyone would receive the same instructions. Instructions for the second part were also provided in a decentralized fashion in groups of four participants. At the end of the first part, the subjects were

[4] The instructions of the FieldDynaPun sessions can be found in the appendix.

informed that their new fishing spot was located six fishing spots to the left of their current one. Before each part began, subjects had to answer test questions. Once all subjects had answered all test questions correctly, the session started.

6.2.3 Experimental procedure

Some extra attention is paid to the experimental procedure. One FieldVCMPun session is carried out end of March 2010, and three sessions with the FieldDyna setup are carried out end of March and beginning of April 2010. Due to unforseen weather conditions, the way we conducted the experiments deviated form the way we planned it. In the first session, two sequences of the baseline treatments were conducted. The second session was run with the baseline treatment first, followed by the punishment treatment. The second treatment of this session was terminated halfway the second period, because of a sudden storm. Data is available for only one punishment decision, made at the end of the first period. Finally, the third session began with the punishment treatment, but this was followed by what we term the 'Unconstrained' treatment. In this treatment, subjects could fish unconstrained for a period of 35 minutes. No negative externalities could be imposed on others (except for the fact that the stock of fish is finite; one fish caught by one fisherman cannot be caught by another within a period). Table 6.1 shows the collection of the data. The table shows the number of groups, the order in which the treatments were carried out, the number of periods, and the average individual earnings.

Treatment	Groups	Order (# Periods)	Average Earnings
FieldVCMPun	4	Baseline (2) - Punishment (4)	€34.13
FieldDyna	4	Baseline (3) - Baseline (3)	€9
FieldDynaPun	4	Baseline (3) - Punishment ($1\frac{1}{2}$)	€12.31
FieldDynaPun	4	Punishment (3) - Unconstrained (1)	€12.88

Table 6.1 Number of groups, order of treatments, and average earnings in the FieldVCMPun and FieldDynaPun treatments.

6.3 Data analysis

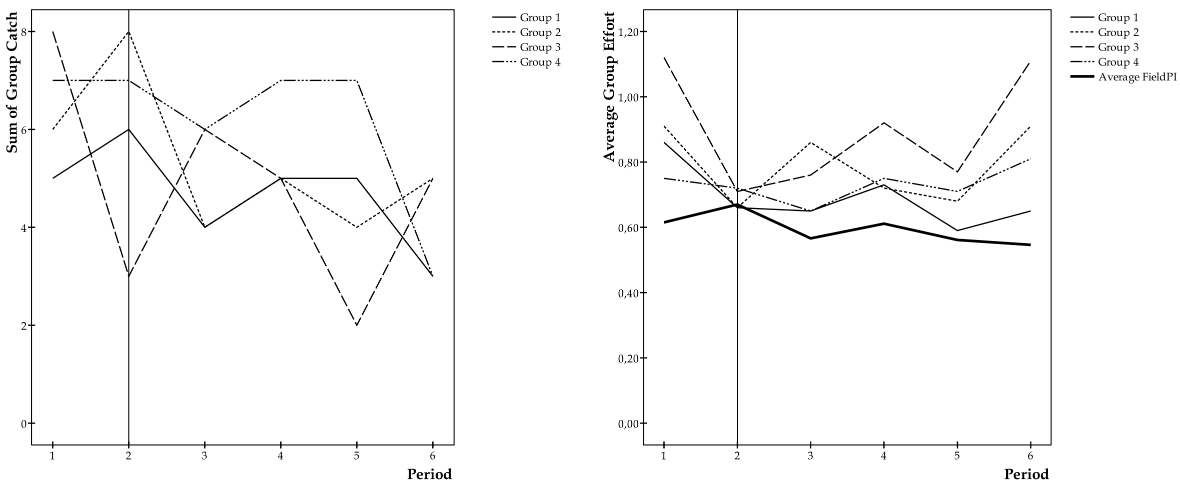
In this section, the data are presented. Section 6.3.1 covers the behavior of the fishermen in the social dilemma stage, and the effect that punishment has on cooperation. Section 6.3.2 shows how the subjects use punishment.

6.3.1 Cooperation in the social dilemma stages

This section covers the development of cooperation in the two field experiments. First, the development of play in the FieldVCMPun treatment is examined after which the FieldDynaPun treatment is considered.

Cooperation in the FieldVCMPun experiment

As a starting point of the analysis, the catch and effort levels in the social dilemma stage are presented. Figure 6.1(a) shows the sum of fish caught per group and Figure 6.1(b) shows the average effort levels of the four groups.[5] For ease of comparison, the average effort level of all groups from the FieldPI treatments (both high and low season) is included in the figure as well. A detailed description of the FieldPI treatment can be found in section 5.2.1



(a) **(b)**

Figure 6.1 **(a)** Sum of fish caught per group in the FieldVCMPun session. Periods 1-2 form the baseline treatment and periods 3-6 form the punishment treatment. **(b)** Average effort per group in the FieldVCMPun treatment (average of all groups from the FieldPI treatment (see Chapter 5) is also included).

Figure 6.1 shows three important things. The first observation is that the catch of fish is declining slightly after the first two periods. This decline in catch can be interpreted as a tendency for subjects to cooperate more when punishment is introduced. However, the effort levels show whether the fishermen caught fewer fish on purpose, or that a decline in catch is bad luck. Bad luck

[5]In appendix 6.A.1, we show that effort and catch are positively correlated.

seems to be ruled out from the second observation; unlike the catch of fish, effort levels are relatively stable over the periods. Even when subjects can punish each other, they try as hard in every period to catch fish as without the option of being punished.[6] The final observation is that the average effort levels of the FieldPI treatment are lower than those observed in the FieldVCMPun treatment; punishment does not lead fishermen to be more cooperative. All in all, punishment seems to have no effect on promoting cooperation. This observation is in contrast to the results of many laboratory experiments. In result 1, we summarize our finding:

Result 1 Monetary punishment does not promote cooperation in the Field-VCMPun treatment.

Support for result 1: The first piece of evidence comes from comparing the effort levels in the baseline to those in the punishment treatment. As an independent observation, we take the average effort level of a group in both treatments. A Wilcoxon matched pairs test shows no difference in effort levels ($N_1 = N_2 = 4, p = 0.273$). The same test is repeated at the subject level, given the limited amount of observations. We find a similar result: Effort levels are not significantly different from the baseline and punishment treatment ($N_1 = N_2 = 16, p = 0.569$).

When we apply the same procedure to the catch of fish, we find that fewer fish are caught in the punishment treatment. This is shown by a Wilcoxon matched pairs test, taking each group as an independent observation ($N_1 = N_2 = 4, p = 0.066$). At the individual level, this finding is confirmed ($N_1 = N_2 = 16, p = 0.011$). However, given that the effort levels are very similar in all periods, it must be the case that the lower catch of fish in later periods is due to bad luck. For that reason, we conclude that punishment does not promote cooperation.

Additional support for result 1 is provided by means of a regression analysis, presented in Table 6.2. The dependent variable of the regression analysis is the change in catch (column (i)) or effort (column (ii)) of subject i, between period t and $t+1$. The main independent variable is the number of punishment tokens received, $\sum_{j\neq}^{N} p_{ji}$. Also a variable that takes into account regression to the mean is included $x_{i,t} - x_{-i,t}$, this variable controls for declines in catch that are not caused by the punishment instrument. This variable is not included for

[6] Note that, in general, effort levels show a declining trend, because fishermen become physically tired of the act of fishing. Although this is not clearly visible from the effort levels exerted in the FieldVCMPun treatment shown in Figure 6.1(b), results from Chapter 5 support this view.

the regression on effort, because subjects are not given feedback on the effort levels of other group members. Table 6.2 shows that the impact of punish-

Dependent variable: $x_{i,t+1} - x_{i,t}$ (i), $e_{i,t+1} - e_{i,t}$ (ii)		
	(i) Catch	(ii) Effort
$\sum_{j \neq i}^{N} p_{ji}$	-0.768	-0.104
	(0.429)	(0.179)
$x_{i,t} - x_{-i,t}$	-0.554^{***}	
	(0.070)	
Constant	0.129	0.071
	(0.075)	(0.061)
N	48	48
R^2	0.4011	0.0307

Table 6.2 OLS model to estimate the determinants of a change in catch (column (i), or a change in effort (column (ii)). Standard errors, group-level clustered, are reported between parentheses.***: significant at the 1%-level.

ment is insignificant, both for the catch measure and for the effort measure. This provides additional evidence that punishment does not enforce subjects to become more cooperative, unlike many results from laboratory experiments. ■

Having shown that punishment does not promote cooperation, we will show that there are no signs of cooperation at all. Fishermen always try to catch two fish in each period, just like we have reported in Chapter 5. We summarize below:

Result 2 In the FieldVCMPun session, there is no evidence of cooperation, neither in the baseline treatment, nor in the punishment treatment.

Support for result 2: The first piece of evidence comes from the observation that fishermen have positive levels of effort to catch the first fish, in all periods. When fishermen are exerting effort to catch fish, they show a revealed preference for catching a fish. A series of two-sided Student t-tests, taking a subject's average effort levels in a period as an independent observation, show that the effort levels to catch the first fish are always greater than zero. For each of the six periods, the t-test has a p-value smaller than 0.01 ($N = 16$).

The second piece of evidence comes from comparing the effort levels in FieldVCMPun with those observed in FieldPI. A reduction in effort as compared to the FieldPI treatment suggests that fishermen in the FieldVCMPun treatment are trying to catch fewer fish, which might be interpreted as evidence of cooperation.[7] A Mann-Whitney test shows that effort levels in the FieldVCMPun treatment are greater than those in the FieldPI treatment. Taking each group as an independent observation, average effort is greater in the first two periods of FieldVCMPun (the baseline treatment) than in the first two periods of FieldPI ($N_1 = 4, N_2 = 8, p = 0.048$). Similarly, average effort levels in periods 3-6 in the FieldVCMPun treatment are greater than in the same periods in FieldPI ($N_1 = 4, N_2 = 8, p = 0.016$).

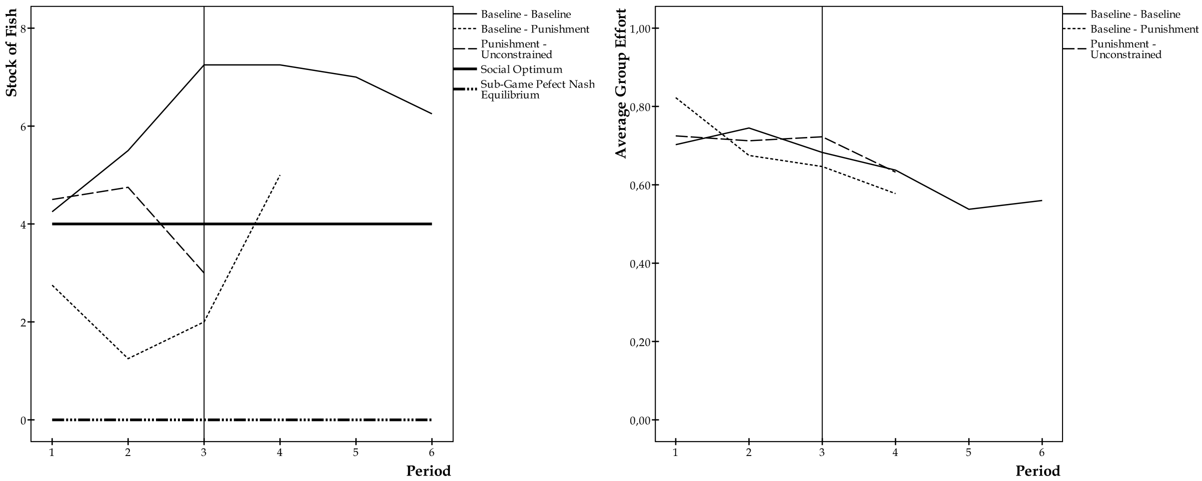
The final piece of evidence is obtained by considering the effort levels to catch the second fish in both treatments of the FieldVCMPun session. If fishermen are acting partially cooperatively, then they could do this by lowering their effort levels for their second fish, conditional on having caught the first fish. We find no such effect. We test this by means of a Wilcoxon matched pairs test, taking effort levels for fishermen who have caught at least one fish in a period. The matched pair consists of the effort level exerted to catch the first fish, and the effort level exerted to catch the second fish. In the baseline treatment of FieldVCMPun, average effort levels for the second fish are even greater than those for the first fish ($N_1 = N_2 = 27, p = 0.012$). In the punishment treatment, average effort levels for the first and second fish are the same ($N_1 = N_2 = 48, p = 0.529$). ■

Cooperation in the FieldDynaPun experiment

Additional evidence that monetary punishment does not promote cooperation is obtained by conducting the FieldDyna experiment. Let us begin by showing the average stock level and effort levels in the FieldDynaPun treatments. Figure 6.2(a) shows the average stock levels at the end of each period in each of the three sessions, while Figure 6.2(b) shows the corresponding average effort levels in each period.

Our first aim is to establish the effect that punishment has on promoting cooperation. The Baseline-Punishment session shows that the stock levels in the baseline treatment are lower than the stock level in the punishment treatment (note that we only have one full period as an observation in this treatment). A similar observation can be made for the effort levels. The combination of these two findings is consistent with the conclusion that punishment has a pos-

[7] Note that we do not compare the catch levels, since the two experiments have been conducted over a year from each other. For this reason, we do not control for the seasonal effects when considering catch. This is not a problem for effort, because effort is independent of seasonal effects.



(a) **(b)**

Figure 6.2 **(a)** Average stock of fish at the end of each period in the FieldDynaPun sessions. Periods 1-3 form part 1 of each treatment (either the baseline or the punishment treatment) and periods 4-6 form part 2 of the experiment (either the baseline, punishment or unconstrained treatment). **(b)** Average effort per group in the FieldVCMPun treatment.

itive effect on promoting cooperation. However, this finding can be explained by two factors: The results of the previous analysis and those of the FieldPI treatment of Chapter 5 show that fish are less likely to bite in later periods of the experiment. Furthermore, fishermen experience lower marginal benefits of the act of fishing, or become physically tired of the act of fishing, as indicated by the steady drop in effort in all the sessions (including those of Chapter 5). The Punishment-Unconstrained treatment illustrates this point; although stock levels of fish are above the social optimum in period 1 and 2, effort levels in the Unconstrained treatment are on average slightly lower (but not significantly so) than those in period 3 of the Punishment treatment. We state an intermediate result below for the Punishment-Unconstrained session, coming back to the Baseline-Punishment session later.

Result 3 Monetary punishment does not promote cooperation in the Punishment-Unconstrained session.

Support for result 3: If subjects intend to target catch at the social optimum, it is to be expected that in the final period the stock is depleted, or at least has levels lower than those in periods 1 or 2.[8] The first support for our claim that monetary punishment does not promote cooperation, comes from observing that the stock levels at the end of each period, though seemingly to be targeted at the social optimum, are equal in all three periods. We find no evidence that the stock level at the end of period 3 is lower than those in periods 1 or 2, as indicated by a Wilcoxon matched pairs test. As an independent observation, we take the end stock level of a group in period 1 and compare it to the end stock level in period 3. The test shows that the stock levels are equal ($N_1 = N_2 = 4, p = 0.886$). A similar result is obtained when the difference in end stock levels of period 2 and 3 are compared ($N_1 = N_2 = 4, p = 1.000$).

Finally, average effort levels in the Unconstrained treatment are slightly (but insignificantly) lower than those in any period of the punishment treatment. In the Unconstrained treatment, fishermen are expected to fish at their maximum (selfish) optimum. Finding effort levels which are slightly lower than in any period of the Punishment treatment is suggestive that fishermen fish at their (selfish) optimum in the Punishment treatment as well.[9] A Wilcoxon matched pairs test shows that effort levels are statistically significantly lower in period 1-3 as compared to the Unconstrained treatment, taking each group as an independent observation (for each test, $N_1 = N_2 = 4$ and $p = 0.068$). Due to the limited number of observations, the test is repeated taking each individual effort level as an independent observation. This results in an insignificant difference between periods 1-3 on the one hand and the unconstrained treatment on the other (for each test, $N_1 = N_2 = 16$ and the lowest p-value is 0.179).

Additional support for result 3 is provided by a regression analysis, presented in Table 6.3. The regression analysis estimates a subject's change in catch from one period to the next, $x_{i,t+1} - x_{i,t}$ (column (i)), or the change in

[8] Even in the last period, a fisherman might fear being punished when, for example, he catches more than his 'fair' share of the fish remaining. Even so, it is expected that more fish are caught in total than in any of the two preceding periods in case fishermen are not hindered by bad luck.

[9] Note that we do not have evidence that supports the view that fishermen fish with maximum efforts in the punishment treatment. Data from other fishing sessions shows that fishermen become physically tired as the periods go by. It might be the case that although fishermen exert slightly more effort in the punishment treatment than in the Unconstrained treatment, the fishermen could have exerted even more effort in the punishment treatment. This is consistent with the view that fishermen are pro-social after all. However, we think that this is unlikely; given that fishermen have all the incentives to catch as much as possible in the Unconstrained treatment, it is to be expected that they fish with the optimal effort level (which might be at the top of their physical fitness). If fishermen could fish at a much faster pace, then it might be the case that their rods 'outrun' the rainbow trout. Therefore, although it might be possible that fishermen could have fished with greater effort levels in the punishment treatment, the fact that effort in the Unconstrained treatment is so similar to effort in the punishment treatment suggests that they fished with the optimal effort levels in both treatments, intending to catch as much as possible, irrespective of the threat of punishment.

effort, $e_{i,t+1} - e_{i,t}$ (column (ii)). This analysis is similar to the one presented in Table 6.2. Variables included are the effects of the total number of punishment tokens received by the other group members, $\sum_{j \neq i}^{N} p_{ji}$, and a variable that takes into account regression to the mean for the catch measure only. In contrast to the analysis reported in Table 6.2, a dummy variable is included for period 3. The last period of the FieldDyna treatment is different from the earlier ones, because it is socially optimal to deplete the resource stock. However, there still might be some effects of punishment received in period 2 that carry over to period 3. Table 6.3 shows that, like the results reported in Table 6.2, there is

Dependent variable: $x_{i,t+1} - x_{i,t}$ (i), $e_{i,t+1} - e_{i,t}$ (ii)		
	(i) Catch	(ii) Effort
$\sum_{j \neq i}^{N} p_{ji}$	−0.085	0.037
	(0.273)	(0.056)
$x_{i,t} - x_{-i,t}$	-0.568^{***}	
	(0.065)	
$I(\text{Period} = 3)$	0.345	0.029
	(0.638)	(0.049)
Constant	−0.024	−0.027
	(0.347)	(0.02)
N	32	32
R^2	0.2599	0.0146

Table 6.3 OLS model to estimate the determinants of a change in catch (column (i)), or a change in effort (column (ii)). Standard errors, group level-clustered, are reported between parentheses.***: significant at the 1%-level.

no effect of punishment on cooperation. Fishermen do not catch less fish, nor provide smaller effort levels, when they have received punishment in the previous period. Like the results of the FieldVCM treatment, the variable that takes into account regression to the mean is significantly negative. This shows that the fishermen converge towards some norm of the number of fish that are being caught. ■

The data we have for the Baseline-Punishment session are not rich enough to give an undisputable conclusion about the effectiveness of punishment. Due to a sudden storm, the session was terminated halfway during the second period of the punishment session, leaving us with complete data on the first period only, and some effort levels for the second period. However, the data that we do have for this session, do not show any evidence of punishment being effective. The logic we apply is as follows: If there are no signs of cooperation, then it must be the case that punishment is ineffective. Absence of cooperation is observed when fishermen fish with the same intensity when the allowable catch remaining (ACR) is four or lower, as they do when the ACR is five or greater. When fishermen intend to cooperate by maximizing group catch, it is to be expected that they stop fishing when the stock level is four. Figure 6.3 shows that fishermen keep fishing with the same intensity, irrespective of the size of the ACR. In the figure, the average effort levels are reported conditional on the stock of fish for all three sessions of the FieldDyna experiments. Only the first two periods are included, because there is no social dilemma in the final period. To be more precise, each period is divided into seven intervals of five minutes. The five minute interval after which a group reaches a certain stock level is included in the figure.[10] As can be seen, effort levels when the ACR is four or lower are always greater than zero.[11] As the positive effort levels show when the stock level is three, the fact that a stock level of two and one is not reached must be due to bad luck, rather than the fishermen not trying to reach that stock level. We summarize our result below.

Result 4 In all FieldDynaPun experiments, we cannot find any evidence of cooperation, neither in the baseline treatments, nor in the punishment treatments.

Support for result 4: The first piece of evidence of absence of cooperation is obtained by comparing the effort levels in period 1 and 2 of a treatment to those of period 3 of the same treatment. If fishermen are acting cooperatively, then it is expected that more effort is exerted in period 3 than in either period 1 or 2. In total, three baseline treatments are conducted (two in the Baseline-Baseline session and one in the Baseline-Punishment session). This gives us twelve observations. However, in one of the sessions a group depleted their stock in period 2, leaving us with eleven independent observations. The observation that is lost, provides in itself a clear example that fishermen are not acting

[10] The first ten minutes of a period are not considered, because at the start of a period, fishermen are more eager to fish. Since all fishermen start with an ACR of eight, the effort levels associated with this stock level are slightly biased. Including the first ten minutes of a period in the baseline treatment causes the effort levels to increase from 0.62 to 0.68. In the punishment treatment, including the first ten minutes of a period increases effort from 0.72 to 0.79.

[11] An exception contains the ACR of one and two in the punishment treatments of the Punishment-Unconstrained session and the Baseline-Punishment session. Fishermen in the two punishment sessions never reached those stock levels, which is why effort levels are missing for those ACR.

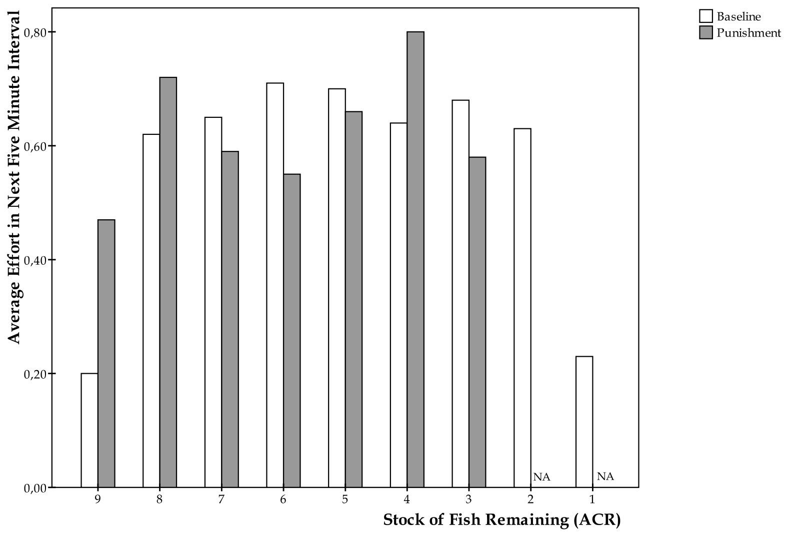


Figure 6.3 Average individual effort conditional on the allowable catch remaining (ACR) in period 1 and 2. Each 5 minute interval is an observation. The white bars contain data from the baseline treatments of the Baseline-Baseline and Baseline-Punishment sessions. The grey bars contain data from the punishment treatments of the Punishment-Unconstrained and Baseline-Punishment sessions.

cooperatively. A Wilcoxon matched pairs test shows that effort levels in period 1 are greater than those in period 3 ($N_1 = N_2 = 11, p = 0.005$). Effort levels in period 2 are similar to those in period 3 ($N_1 = N_2 = 11, p = 0.212$). The two tests show that fishermen fish with at least the same intensity in the period without the social dilemma as they do in the periods with a social dilemma. The same test cannot be performed for the punishment treatment in the Baseline-Punishment session, because the data is incomplete for the second period and missing for the third period due to a sudden storm.

The final piece of evidence in favor of a total absence of cooperation is obtained by considering the effort levels, conditional on the ACR. Table 6.4 shows a fixed effects regression analysis where the effects of the stock level on effort are measured. Each period of 35 minutes is divided into seven intervals of five minutes. The independent variable is the individual effort level, five minutes after a certain stock level is reached. Dummy variables are included to take account for the stock level; the benchmark is the stock level at which it is socially optimal to catch an additional fish (a stock level of five or greater). Also included is a dummy variable for the second period, to account for between period effects in effort. Likewise, the variable Time is included to take account of trends in effort within a period.

Dependent variable: Effort in interval $s+1$	Baseline Treatment	Punishment Treatment
$I(\text{ACR}=4)$	0.140**	0.129
	(0.051)	(0.102)
$I(\text{ACR}=3)$	0.175*	−0.075
	(0.077)	(0.049)
$I(\text{ACR}=2)$	0.212	NA
	(0.118)	
$I(\text{ACR}=1)$	−0.318***	NA
	(0.060)	
Time	−0.052**	−0.009*
	(0.020)	(0.005)
$I(\text{Period}=2)$	−0.075	−0.002
	(0.044)	(0.022)
Constant	0.802*	0.614***
	(0.060)	(0.012)
N	512	352

Table 6.4 Fixed effects regression model to estimate the influence of stock level on effort. Standard errors (clustered at the group level) are reported between parentheses. ***: significant at the 1%-level, **: significant at the 5%-level, *: significant at the 10%-level.

The regression shows that in the baseline treatment, effort levels are even greater when the stock is coming close to depletion. An exception is the effects of a stock level of only one fish; fishermen then reduce their effort levels as compared to stock levels of five or greater. However, the observations from a stock level of one come from the only group who depleted their stock entirely before the end. So, although the fishermen in that group slightly reduced their effort to catch the last fish remaining, they actually caught it. Therefore, even the negative sign of effort cannot be interpreted as evidence in favor of cooperation.

In the punishment treatment, effort is unaffected by remaining stock levels; fishermen fish with the same intensity irrespective of the stock level. The threat of punishment does not cause the fishermen to start to cooperate. Unfortunately, the data are not rich enough to observe what would have happened when the stock levels were lower than three. ■

6.3.2 The use of punishment

In this section, the mechanism behind punishment is presented. First, the use of punishment in the FieldVCMPun treatment is presented. Finally, punishment in the FieldDynaPun sessions is considered.

Punishment in FieldVCMPun

As is shown in results 1 and 2, monetary punishment is not an effective instrument to promote cooperation in our field FieldVCM experiment. To understand this result in more detail, we analyze how the instrument is used. Figure 6.4 shows the sum of punishment used for each of the four groups. The figure

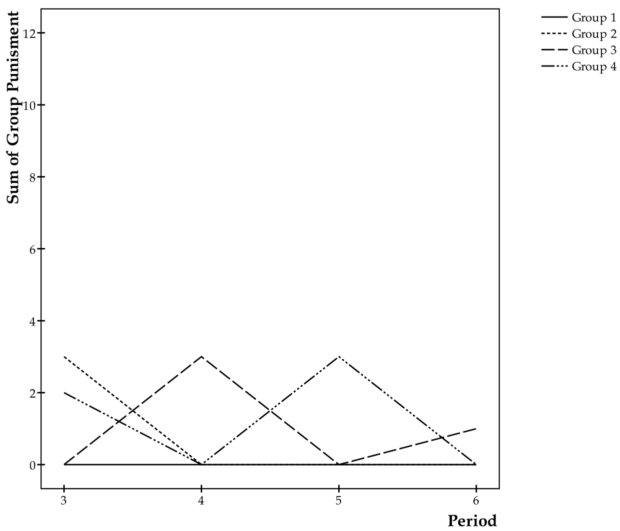


Figure 6.4 Sum of group punishment used for each of the four groups in the FieldVCMPun treatment.

shows that with an average of 0.75 tokens per period, monetary punishment is hardly used. Moreover, the rare use of punishment is stable in any of the periods, although in the last period only one punishment token is used in total. The pattern of punishment found in the FieldVCMPun treatment is in sharp contrast to earlier laboratory results on punishment, where punishment is high in early periods and declines over time (see for example Gürerk et al. (2004), Denant-Boemont et al. (2007), Nikiforakis (2008), Rand et al. (2009)). Group 1 never used the punishment instrument at all, while the sum of punishment tokens used in the other groups never exceeds three in a given period. A closer look at the data shows that out of the 4 periods × 4 subjects × 4 groups = 64 punishment decisions, only in five of those cases a subject directed punishment to one or more group members. Furthermore, out of the 64 × 3 = 192 punishment tokens that could have been used, only 12 tokens (6.25%) are used. As a result, we state the following:

Result 5 In the FieldVCMPun treatment, monetary punishment is used rarely by the subjects. There is no upward or downward trend in the use of monetary punishment.

Support for result 5: The trend in time is considered by comparing the sum of punishment in period 1 with period 4 of the punishment treatment. As an independent observation, we take the sum of punishment used at the group level. A Wilcoxon matched pairs test shows no significant difference between the two periods ($N_1 = N_2 = 4, p = 0.285$). Repeating the analysis at the individual level, the same result is obtained ($N_1 = N_2 = 16, p = 0.285$). ■

Finally, we present some further insights into how punishment is used in the FieldVCMPun treatment. One would expect that punishment is mainly targeted at those who cause the largest negative externalities, the mechanism of punishment found in many laboratory experiments. In our field experiment, it is hard to find evidence for an unambiguous mechanism, because the instrument is used so rarely. Out of the five punishment decisions that are made in total, four came from different subjects, and only one subject used punishment in two periods. In three of the instances in which a subject punished, all three tokens were evenly divided among all group members. These instances of punishment seem rather arbitrary, given that in two of those instances the targeted group member did not have an equal catch. Combined with the big catch and effort levels presented in the previous section, it seems that acts of punishment bear no relation to the observed behavior in the social dilemma stage.[12] This conclusion is reported in result 6:

Result 6 Monetary punishment is not correlated with catch of the recipient in the FieldVCMPun treatment.

[12]Punishment received in one period might also have a relation with punishment sent in the next period. Those who have received a lot of punishment might 'retaliate' by using much punishment in the next period. We find that the sum of punishment received in period t, and the sum sent in period $t + 1$ has no significant correlation. This is evident from a Pearson correlation coefficient, with an insignificant value of -0.130, taking the sum each subject's punishment decisions per period as an observation ($N = 48, p = 0.379$).

Support for result 6: A Hurdle regression or Tobit regression is not feasible, given the low number of punishment decisions. Therefore, we present the Pearson's correlation coefficient. A unit of observation consists of the catch of a subject, summed over the four periods in the punishment treatment. Linked to the catch measure is a punishment measure; punishment received by the same subject, summed over the four periods. The Pearson's correlation coefficient has a value of 0.274, but is statistically insignificant ($N = 16, p = 0.304$). ■

Punishment in FieldDynaPun

Even though monetary punishment is not an effective instrument to promote cooperation in the dynamic field experiment, it is still interesting to observe how the instrument is used. Figure 6.5 shows the use of the instruments over the three periods in the Baseline-Punishment session and the Punishment-Unconstrained session. Two observations can be made from Figure 6.5. Firstly,

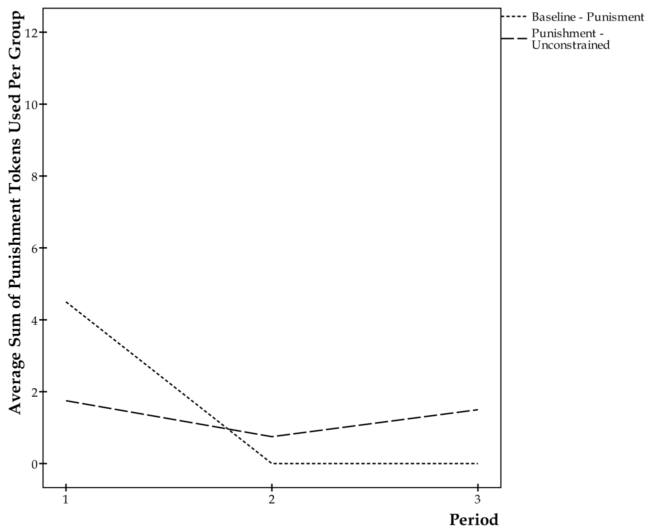


Figure 6.5 Average sum of punishment used per group in the Baseline-Punishment session and the Punishment-Unconstrained session. Data for period 2 and 3 is missing for the Baseline-Punishment session.

monetary punishment is used more often in the FieldDynaPun treatments than in the FieldVCMPun treatment. Out of the 32 subjects that participated in one of the two FieldDynaPun treatments, 12 subjects used punishment, a rate which is double than that in FieldVCMPun. In total, like in FieldVCMPun, 64 punishment decisions were made in FieldDynaPun, of which 14 times a subject sent more than zero tokens to a group member. These 14 punishment decisions yielded a total of 34 punishment tokens to be used, which is almost three times as much as in FieldVCMPun. Furthermore, punishment seems to be stable over the three periods in the Punishment-Unconstrained session. Punishment is as high in period 1 as it is in period 2 or 3. In result 7, we summarize our findings:

Result 7 In the FieldDynaPun experiments, monetary punishment is rarely used by the subjects. There is no upward or downward trend in the use of monetary punishment in the Punishment-Unconstrained session.

Support for result 7: The trend over time is considered by comparing the sum of punishment in period 1 with period 3, which is only possible for the Punishment-Unconstrained session. A Wilcoxon matched pairs test, taking the sum of punishment used at the group level, shows no significant difference between the two periods ($N_1 = N_2 = 4, p = 0.713$). Because of the low number of observations, the analysis is repeated at the individual level, which has the same result ($N_1 = N_2 = 16, p = 0.713$). ■

As a final part of the analysis, we consider the mechanism of monetary punishment in the FieldDynaPun treatment. Also in the FieldDynaPun treatment, it is expected that those who catch most fish are punished because they cause the largest negative externalities onto others. However, like in the FieldVCM-Pun treatment, punishment is used rarely, making it hard to uncover a clear mechanism of punishment.

In the FieldVCMPun treatment, three out of the five times that punishment was used, it was divided equally among all group members. The data for FieldDynaPun show that this is the case in five out of fourteen cases. Although this is half of the amounts observed in FieldVCMPun, still a large amount of punishment in FieldDynaPun seems to be targeted rather arbitrary; ten of the victims of equal punishment caught zero fish. All in all, we cannot find evidence in favor of a correlation between catch and punishment, as is observed in many laboratory experiments.[13] We summarize our last finding below:

Result 8 Monetary punishment is not correlated with catch of the recipient in the FieldDynaPun treatment.

Support for result 8: The first piece of evidence is obtained by estimating the correlation between catch of fish and sum of punishment received. In

[13] The sum of punishment received in period t, and the sum sent in period $t + 1$ also bear no significant correlation in the FieldDynaPun treatment. This is supported by the Pearson correlation coefficient, which has an insignificant value of 0.013, taking the sum each subject's punishment decisions per period as an observation ($N = 32, p = 0.945$).

the Baseline-Punishment session, we are limited to catch and punishment received in one period only. We sum the catch and punishment tokens received over the three periods in the Punishment-Unconstrained session. The Pearson's correlation coefficient has an insignificant value of 0.067 ($N = 32, p = 0.716$).

The second piece of evidence is obtained by estimating a Hurdle regression model and a Tobit model. The dependent variable is the number of punishment tokens sent by subject i to subject j in period t. As independent variables, we use the absolute difference in catch between subject i and j, and the absolute difference in catch between subject j and the group average catch (without taking subject j into account). Finally, a period variable is added to take the trend of punishment into account. Data for both punishment sessions are pooled, and the results are reported in Table 6.5. The Hurdle model is presented in columns (i) and (ii), while the Tobit model is presented in column (iii).

Dependent variable: # punishment tokens subject i sends to j			
	(i) Punishment Decision	(ii) Punishment Level	(iii) Tobit
$\max\{0, c_i - c_j\}$	0.020	−0.003	0.165
	(0.031)	(0.017)	(0.230)
$\max\{0, c_j - c_i\}$	0.068	0.030	0.643
	(0.111)	(0.055)	(0.879)
$\max\{0, c_j - \bar{c}_{-j}\}$	−0.082	−0.016	−0.758
	(0.125)	(0.074)	(0.993)
$\max\{0, \bar{c}_{-j} - c_j\}$	−0.136	-0.260^{***}	-1.382^{*}
	(0.091)	(0.072)	(0.821)
Period	0.073	0.060	0.685
	(0.030)	(0.060)	(0.173)
Constant		1.065^{***}	-3.195^{***}
		(0.175)	(0.922)
N	192	29	192

Table 6.5 Hurdle model (columns (i)-(ii)) to estimate the determinants of punishment. The punishment decision is estimated using a Probit specification. The punishment level is estimated using truncated linear regression. Tobit model (column (iii)) to estimate the determinants of punishment. Marginal effects are reported in the columns for punishment decision. Standard errors (clustered at the group level) are reported between parentheses. ***: significant at the 1%-level, *: significant at the 10%-level.

The regression results show that when subjects decide whether or not to punish a group member (column (i)), none of the independent variables are significant. This means that when subjects decide to punish a group member, the difference in catch does not matter. However, conditional on sending punishment tokens, subjects who catch less than the group average receive less punishment tokens (column (ii)). Some intuition behind punishment seems to be used after all, a result found in the estimates of the Tobit model as well. However, we still conclude that no widely used mechanism is underlying the punishment instrument, because the economic significance of the parameter value is of little meaning. The maximum difference in catch between the group average and subject j is found to be 2.67 fish, which translates into withholding less than three fourths of a token. The estimates of the Tobit model show that slightly less than half a token is withheld if this would occur. ■

6.4 Conclusion

The results of this chapter confirm the findings reported in Chapter 5; no evidence of cooperative behavior is found in the FieldVCM treatment or FieldDyna treatment. In the FieldVCM treatment, subjects show no signs of cooperation. The main evidence for this claim is provided by looking at the effort levels exerted, as measured by the amount of times a fisherman casts his fishing rod. In the FieldVCM treatment, subjects try as hard to catch the first fish, as they do to catch the second fish. Results of the FieldDyna treatment are similar, fishermen try as hard to catch an additional fish, independent of the allowable catch remaining.

When providing subjects with the opportunity to punish group members, then no increase in cooperation is observed, neither in the FieldVCM treatment, nor in the FieldDyna treatment. The current chapter shows that when punishment is in the form of a monetary reduction of earnings, then fishermen do not behave differently than when punishment opportunities are absent. Our results are in contrast to what has been reported in many laboratory experiments on punishment. Experiments conducted in the laboratory show that punishment often leads to an increase in cooperation. This holds for monetary punishment, as well as non-monetary punishment (for monetary punishment, see Yamagishi (1986), Ostrom et al. (1992), Fehr and Gächter (2000, 2002), for non-monetary punishment, see Masclet et al. (2003)).

The use of monetary punishment is rare in the FieldVCMPun treatment. Moreover, there seems to be no clear correlation between the degree a group member is anti-social and the degree of punishment assigned to him or her. As a motive to punish, it seems implausible that subjects believe they can change the behavior of fellow group members. A more plausible motivation seems to be that subjects simply want to take revenge; it makes them feel good to punish a free-riding group member. Taking revenge on free-riding group members might give joy, as is reported on studies that measure brain activity (see de Quervain et al. (2004) and Fehr and Camerer (2007)). The results reported in the current chapter are consistent with that view; it might be the case that the rare events of punishment observed in the FieldVCM treatment are born out of frustration, and that venting this frustration derives joy for the punisher. Of course, this argument applies to the FieldDyna treatment as well.

However, given the low frequency of punishment (especially in the FieldVCM treatment) and the absence of a correlation with the catch of fish (in both the FieldVCMPun and FieldDynaPun treatments), different explanations are also possible. One explanation is the following: The fact that so few subjects sanction fellow group members suggests that subject do not derive any joy of punishment. Rather, the data seem to be more consistent with the view that subjects are averse to use punishment; they might not like the idea that someone is able to reduce one's earnings. Alternatively, the subjects might not like the idea of reducing someone else's monetary earnings. This intuition seems strange given the observation that no cooperation is observed in the social dilemma. One would expect that if fishermen do not like to reduce earnings of fellow group members, then much more cooperation should have been observed. However, there are important differences between reducing someone's earnings due to catching fish or due to punishment. Perhaps the most important difference between the two is that in the social dilemma stage *potential earnings* are reduced, while in the sanctioning stage *actual earnings* are reduced. The aversion to reduce someone's actual earnings might be a feature of the specific subject pool we have. It could be the case that more punishment is used if the same field experiment is played with, for example, students.

Another explanation for the low use of the punishment instrument might be that fishermen are averse to punish group members who caught many fish, because such group members are good fishermen. When deciding to punish someone, it might be considered unfair to punish good fishermen, because they deserve to catch a lot. Results from laboratory experiments on, for example, the Ultimatum Game show that when the allocator has earned the right to be an allocator, then the responder approves of much lower rates than would be the case if this right is not earned (Hoffman et al. (1994)).

The results reported in this chapter suggest that relying on monetary punishment alone to overcome free-riding in social dilemma settings could prove harmful. Perhaps an interplay with other factors, such as communication, or a well established group norm might be necessary in order for punishment to be effective in non-lab settings. In the next chapter, a different kind of punishment is considered; punishment which reduces someone's fishing time.

6.A Appendix

6.A.1 Statistical analysis of the effect of effort on catch

In this appendix, we show the statistical relationship between our effort measure and catch of fish. We use an Ordered Probit model to estimate the effects of effort on catch, presented in Table 6.6. The dependent variable is the amount of fish caught by an individual. As an independent variable we use our measure of effort, the ratio of casts per minute. Furthermore, we include quadrant fixed effects; dummy variables which take into account the effects of the fishing spot at the pond. Table 6.6 contains pooled data for the FieldVCMPun session and FieldDynaPun sessions.

The model shows that effort has a positive and significant influence on the number of fish caught by an individual. The quadrant fixed effects are all insignificant, indicating that the spot at which an individual fishes has no influence on the number of fish caught.

Dependent variable: Number of fish caught in a period	
Effort	1.076***
	(0.283)
Quadrant Fixed Effects	Yes
N	332
pseudo-R^2	0.0308

Table 6.6 Ordered Probit estimation on the relationship between individual effort and individual catch. Standard errors, clustered at the subject level, are reported between parentheses. ***: significant at the 1%-level.

6.A.2 Instructions for the FieldVCMPun treatment

This appendix contains a translation of the FieldVCMPun treatment. Part (a) and (b) is the text of the instructions read aloud at of the baseline treatment, of which part (b) is handed out to the participants. Part (c) is the summary of the rules of the punishment treatment.

(a) Instructions read aloud by the experimenter

Word of welcome

Welcome to this study by Tilburg University. Before we start, we want to point out two things. Firstly, this study is independent of the owners of the trout fishing facility. We are grateful that we are allowed to conduct this study here, but the owners have nothing to do with this project. All responsibility lies with Tilburg University. Secondly, we want to make clear that this study has nothing to do with animal welfare issues or the like. As researchers, we accept the rules and habits of recreational fishing as practiced at the trout fishing facility. We cannot tell you the exact aim of this study. We do want to stress that your privacy is guaranteed; none of the results we report can be traced back to individual participants.

As you know, you do not have to pay to take part in this study. The entrance fees are paid by Tilburg University. You are allowed to take home all fish you catch. In addition, you can earn money. We ask you to abide strictly by the rules which we impose.

The study

In the next four hours, we ask you to fish according to the following rules. First, all rules that normally apply at the trout fishing facility remain in place. This means that it is not permitted to throw fish you catch back into the pond, you are only allowed to fish with one rod, you are only allowed to use a scoop net to set fish ashore, etc.

(b) Summary of the baseline treatment rules

Group formation

- You are placed in groups of 4 persons.
- The groups will remain the same throughout the entire session.

- We do not tell you who belongs to your group and you are not allowed to exchange information with other participants.

Timing

- The session lasts five hours, from around 10.00 a.m. until around 15.00.
- If we begin later, we will stop later.
- The session is divided into two parts, part 1 and part 2.
- Part 1 takes 70 minutes and is divided into two periods of 35 minutes.
- When part 1 is finished, we will give instructions for par 2.

Stocking the pond

- In the first period, we will put $(16 \times 2) + 6 = 38$ rainbow trout into the pond.
- You are allowed to take home each fish you catch.
- We make sure that an equal number of rainbow trout is in the pond at the beginning of each period. We do this by putting in a new rainbow trout at the beginning of a new period for each fish that is caught in the previous period.
- In each period of 35 minutes, you are allowed to catch at most 2 fish. If you catch a salmon trout, it also counts as one fish.
- If you (decide to) catch less than two fish, we give money to the other three participants of your group.
- If you catch fewer fish than the two you can catch maximally, we divide 6 euro equally among the other 3 participants in your group for each fish you did not catch.
- Hence, for each fish you do not catch (or decide not to catch), each of the other 3 participants in your group receives 2 euro. This means that:
  - If you catch 2 fishes, the other 3 participants in your group do not receive any money.
  - If you catch 1 fish, each of the other 3 participants in your group receives 2 euros.

  - If you catch 0 fishes, each of the other 3 participants in your group receives 4 euros.

- This holds for all participants. This means that you will receive 2 euro for each fish that is left in the pond by the other participants in your group.

In each period

- The number of rainbow trout put into the pond is equal to the number of fish caught in the previous period.
- You are not allowed to talk with the other participants.
- At the end of each period, you receive information on how much money you have earned. We also tell you how many fish each of the other group members has caught, and how much money he or she has earned.
- The information on catch and earnings of other group members is always presented in a random order. This means that you cannot link the catch and earnings of one group member in one period to the catch and earnings of that same group member in another period.

In period 1

- Your fishing spot in part 1 is determined by participants picking a numbered spot tag from a black linen bag.

Questions

If you have any questions regarding the session, you can ask them now, but also during the session. We do not answer questions about how to act in this study — all decisions you take are yours. We also do not answer questions about the purpose of this study. When we have analyzed the data, we will inform you about its results.

Test questions

We will now have a short test. When all participants have correctly answered all questions, the study will begin.

1. With how many other participants are you placed in a group?
2. How many euros do you receive, due to your own activities, when you catch one fish in a period?

3. Suppose the following situation has occurred:
   - You catch 2 fish
   - The other members of your group each catch 1 fish
     - How many euros do you receive?
     - How many euros do each of the other group members receive?

(c) Summary of the punishment treatment rules

Timing

- Part 2 consists of 4 periods of 35 minutes.

Group formation

- You are placed in a group with 4 persons.
- Your group members are the same as those in part 1.
- We do not tell you who belongs to your group and you are not allowed to exchange information with other participants.

The rules of part 1 are in place in part 2 as well

New rules in part 2

- The main difference with part 1 is that in part 2 you can indicate from which participants you want to reduce earnings.
- You can instruct us to reduce the earnings of one or more other group members with €3, or more.
- If you instruct us to reduce the earnings of another group member with €3, then this costs you €1.
- In each period you receive a budget of €3 that you can use to reduce the earnings of another group member. You can reduce the earnings of another group member with maximally €9. However, you can also reduce the earnings of each the other three group members with €3.
- You do not have to instruct us to reduce the earnings of other group members. If you decide not to reduce the earnings of other group members, the budget of €3 is added to your total earnings.

- The total amount of money that is reduced from a group member can never be bigger than the total earnings of that individual up to that period (including the earnings of part 1).
- At the end of each period we inform you about the number of fish each of the other three group members have caught, and their individual earnings. Then we ask you how many of the €3 you would like to spend to reduce the earnings of one or more group members, and if so, with how much you want the earnings to be reduced.
- At the beginning of the next period we inform you about all decisions of your other group members, and we tell you how much money each of the other group members have earned.
- Note that the information presented to you is always in a random order.
- An example:
  - Suppose that a group member has caught 2 fish and that his earnings in that period are €10. If you decide to reduce the earnings of that group member with €6, then this costs you €2 of your budget.
    - The one whose earnings you have lowered then has earnings equal to: 2 fish plus €10 – €6 = €4.
    - You then have €1 left in your budget. If you do not choose to reduce the earnings of another group member, this euro is added to your total earnings.
  - Suppose another group member has caught 1 fish and his earnings in that period are €2. If you decide to spend €1 for this group member, then his earnings in this period are €2 – €3 = –€1. In that case, the total earnings of that group member up to that period will be lowered with €1. In case those earnings are €0, then they will remain €0.

In period 3 and 5 of part 2

- We will tell you where you will fish in period 3. In period 4 you have to fish at the same spot. In period 5 we will tell you at which new spot you will fish. This is the same spot as period 6.

6.A.3 Instructions for the FieldDynaPun treatment

This appendix contains a translation of the FieldDynaPun treatment. Part (a) is the text of the instructions read aloud at the beginning of the session. Part (b) is the text that is handed out to the participants. Part (c) is the text of the instructions read aloud at the beginning of part 2. Part (d) is the text that is handed out to the participants.

(a) Instructions read aloud by the experimenter for part 1

Word of welcome

Welcome to this study by Tilburg University. Before we start, we want to point out two things. Firstly, this study is independent of the owners of the trout fishing facility. We are grateful that we are allowed to conduct this study here, but the owners have nothing to do with this project. All responsibility lies with Tilburg University. Secondly, we want to make clear that this study has nothing to do with animal welfare issues or the like. As researchers, we accept the rules and habits of recreational fishing as practiced at the trout fishing facility. We cannot tell you the exact aim of this study. We do want to stress that your privacy is guaranteed; none of the results we report can be traced back to individual participants.

As you know, you do not have to pay to take part in this study. The entrance fees are paid by Tilburg University. You are allowed to take home all fish you catch. In addition, you can earn money. We ask you to abide strictly by the rules which we impose.

The study

In the next four hours, we ask you to fish according to the following rules. First, all rules that normally apply at the trout fishing facility remain in place. This means that it is not permitted to throw fish you catch back into the pond, you are only allowed to fish with one rod, you are only allowed to use a scoop net to set fish ashore, you are only allowed to use the usual types of bait, etc.

Group formation

You are placed in a group of 4 participants. The group remains the same during part 1 of the study. Each participant receives a colored ribbon. The participants in your group have the same color ribbon as you have. You are not allowed to talk with other participants. This rule is so important that we exclude you of the study if you do not abide by this rule.

Timing

The study consists of two parts, part 1 and part 2. Part 1 takes about two hours, until about 12.00 hours.

Earnings from fishing

You are allowed to take home each fish you catch. In addition you earn €3 for each fish you catch. During the study, rainbow trout and Salmon trout count both as one fish.

Number of periods in part 1

The first part consists of three periods of a half hour each.

Stocking the pond

In the first period we put 38 rainbow trout into the pond.

Total available fish for your group

You are not allowed to catch as much fish as possible. In each period, you and your group members are not allowed to catch more fish than is available for your group. In the first period, there are 8 fish available for your group. Each participant is allowed to catch as much fish as possible, but the total catch in your group can not be more than 8 fish in the first period.

The amount of fish available for your group does not depend on the amount of fish caught by other participants at the pond. The amount of fish available for your group in a period only depends on the amount of fish that was present for your group at the end of the previous period. And this amount depends on the amounts available in all previous periods. How this exactly works is presented in the following table:

If, due to catch in the previous period and the periods before that, on a given moment no fish are available for your group (in other words, if at any

Number of fish left at the end of the previous period by your group (see score board)	Increase in the number of fish available to your group	Number of fish available to your group in the coming period
0	0	0
1	2	3
2	3	5
3	3	6
4	4	8
5	3	8
6	3	9
7	2	9
8	0	8
9	2 fish subtracted	7

given time a 0 appears on the score board), then you and your group members have to stop fishing immediately. In the second column of the table you see that no extra fish are available for your group, and you are required to wait until the start of the second part of the study.

If at the end of a period 1 fish is available for your group (in other words, if the score board indicates a 1 at the end of a period), then the amount of fish that your group is allowed to catch in the next period is raised by 2. Hence, there are 3 fish available for your group in the next period.

If at the end of the previous period 4 fish are available for your group, then the amount of fish available for your group in the next period is raised by 4 fish. This means that your group can catch 8 fish in the next period.

If at the end of a period 6 fish are available for your group, then the amount of fish available for your group in the next period is raised by 3 fish. This means that your group can catch 9 fish in the next period.

How to read the table

Suppose that at any given moment there are 8 fish available for your group. If you and your group members catch 1 fish in total in that period, then the total available catch in the next period raises with 2. If you and your group members catch 2 fish in total in that period, then the total available catch in the next period raises with 3.

Fishing spot

You will draw a spot tag out of a black linen bag. Before the study begins, you will receive a colored ribbon. In all period of part 1 you will fish at the same spot. We want to stress again that it is very important that you do not talk with other participants. This is so important that we will exclude you from the study if you do not abide by this rule.

Questions

If you have any questions regarding the session, you can ask them now, but also during the session. We do not answer questions about how to act in this study — all decisions you take are yours. We also do not answer questions about the purpose of this study. When we have analyzed the data, we will inform you about its results.

We will now have a short test. When all participants have correctly answered all questions, the study will begin.

(b) Summary of the instructions for part 1

Group formation

- You are placed in groups of 4, recognizable by the colored ribbons.
- You are not allowed to talk during the study.

Timing

- The study takes place from around 8.00 until 12.00 hours.
- The first part consists of 3 periods of 30 minutes each.

Earnings

- You are allowed to take home each fish you catch.
- You receive €3 for each fish you catch.

Total available fish for your group

- Each period you and your group members are allowed to catch what is available for your group (as indicated on the score board).
- In the first period, there are 8 fish available for your group.
- The available fish for **your** group ONLY depends on the decisions made in **your** group.

Number of fish left at the end of the previous period by your group (see score board)	Increase in the number of fish available to your group	Number of fish available to your group in the coming period
0	0	0
1	2	3
2	3	5
3	3	6
4	4	8
5	3	8
6	3	9
7	2	9
8	0	8
9	2 fish subtracted	7

Test questions

1. What is the maximum amount of fish that your group is allowed to catch in period 1?
2. If your group catches 3 fish in the first period, how many fish are available for your group in the next period?
3. If your group catches 4 fish in the first period, how many fish are available for your group in the next period?
4. If your group catches 5 fish in the first period, how many fish are available for your group in the next period?
5. If your group catches 8 fish in the first period, how many fish are available for your group in the next period?

(c) Instructions read aloud by the experimenter for part 2

Group formation

You are placed in groups of 4 participants. The group remains the same during part 2 of the study. The group members of part 1 are the same as those in part 2. You keep your colored ribbon. We urge you not to communicate with other participants. This is so important that we exclude you from the study if you do not abide by this rule.

Timing

Part 2 takes two hours, until about 14.00 hours.

Earnings

You are allowed to take home each fish you catch. In addition you receive €3 for each fish you catch. During the study, both rainbow trout and Salmon trout count as one fish.

Number of periods in part 2

The second part consist of three periods of 30 minutes each, like in part 1.

Available fish for your group

Like in part 1, in part 2 you are not allowed to catch more than the total available fish for your group. This is indicated on the score board. The total available fish for your group in the next period depends on the total available fish at the end of the previous period, as indicated on the score board. Changes as compared to part 1:

- The main difference with part 1 is that in part 2 you can indicate from which participants you want to reduce earnings.
- You can instruct us to reduce the earnings of one or more other group members with €3, or more.
- If you instruct us to reduce the earnings of another group member with €3, then this costs you €1.
- In each period you receive a budget of €3 that you can use to reduce the earnings of another group member. You can reduce the earnings of another group member with maximally €9. However, you can also reduce the earnings of each the other three group members with €3.
- You do not have to instruct us to reduce the earnings of other group members. If you decide not to reduce the earnings of other group members, the budget of €3 is added to your total earnings.

- The total amount of money that is reduced from a group member can never be bigger than the total earnings of that individual up to that period (including the earnings of part 1).
- At the end of each period we inform you about the number of fish each of the other three group members have caught, and their individual earnings. Then we ask you how many of the €3 you would like to spend to reduce the earnings of one or more group members, and if so, with how much you want the earnings to be reduced.
- At the beginning of the next period we inform you about all decisions of your other group members, and we tell you how much money each of the other group members have earned.
- Note that the information presented to you is always in a random order.

Questions

If you have any questions regarding the session, you can ask them now, but also during the session. We do not answer questions about how to act in this study — all decisions you take are yours. We also do not answer questions about the purpose of this study. When we have analyzed the data, we will inform you about its results.

We will now have a short test. When all participants have correctly answered all questions, the study will begin.

(d) Summary of the instructions for part 2

Timing

- Part 2 consists of 3 periods of 30 minutes.

Group formation

- Your group in part 2 is the same as your group in part 2.

The rules of part 1 are in place in part 2 as well

New rules in part 2

- You can instruct us to reduce the earnings of one or more other group members with €3, or more.
- If you instruct us to reduce the earnings of another group member with €3, then this costs you €1.
- In each period you receive a budget of €3 that you can use to reduce the earnings of another group member. You can reduce the earnings of another group member with maximally €9. However, you can also reduce the earnings of each the other three group members with €3.
- You do not have to instruct us to reduce the earnings of other group members. If you decide not to reduce the earnings of other group members, the budget of €3 is added to your total earnings.

In period 3 of part 2

- We will tell you where to fish in the next period. You will fish at the same spot in the remainder of this study.

Test questions

We will now have a short test. When all participants have correctly answered all questions, the study will begin.

1. How big is your budget that you receive at the end of each period to reduce the earnings of other group members?
2. Are the following statements correct or false?
    - When I spend all of my budget, I can reduce the earnings of each other group member with €3. C / F

- When I spend all of my budget, I can reduce the earnings of maximally 1 other participant with €9. C /F
- I do not need to spend my entire budget; the budget is then added to my total earnings. C / F

Chapter 7

Non-Monetary Punishment and Rewards in the Field[1]

7.1 Introduction

In Chapter 6, it is shown that monetary punishment does not lead subjects to become more cooperative as compared to the FieldVCM treatment. Fishermen catch similar amounts of fish, and fish with similar intensity in the FieldVCM treatment as they do in FieldPI. Having monetary punishment as a device to enforce cooperation has no effect in that setup. As an additional experiment, a dynamic social dilemma experiment is conducted, termed FieldDyna. In this experiment, overharvesting by a group in one period leads to a considerable loss in the stock of fish in future periods. We find no evidence of cooperation; fishermen fish with maximum intensity, independent of the possibility to punish.

In the search for instruments that promote cooperation, the literature on laboratory experiments has focussed on rewards as well (for example see Chapters 3 and 4 of this thesis). The success of rewards depends crucially on the cost-benefit ratio. When rewards merely represent a one-to-one transfer of one agent to the other, it has no effect on cooperation (see Sefton et al. (2007), Vyrastekova and van Soest (2008), and Sutter et al. (2010)). Matters change when the costs for the sender are lower than the benefits of the receiver (see Vyrastekova and van Soest (2008), Rand et al. (2009), and Sutter et al. (2010)). Real life examples of such so called net-positive rewards can be found when two agents help each other in knowledge intensive tasks. It costs little effort for an

[1] I would like to thank Ben and Shirley Willems for use of their fishing facility, and Paul Ludeña Delgado, Joris Hoendervangers, Stef van Kessel, and Menusch Khadjavi for excellent research assistance.

expert to fix someone's boat, but the payoff to the recipient are likely be greater than the effort exerted by the expert.

In the current chapter, the effects of punishment are reconsidered, taking the effects of rewards into account as well. One important difference between punishment used in the field setting of Chapter 6 and punishment in the lab, is the payoff medium. In the lab, punishment is usually monetary in nature while money is also the payoff medium in the social dilemma stage. In the field experiments reported in Chapter 5 and 6, money is the medium in which punishment is used, like in the lab, but the payoff media in the social dilemma stage are fish and fishing time. In the current chapter, both the earnings from the social dilemma game and the medium of punishment or rewards are the same; fishing time. The setup of such a social dilemma game has two advantages. Firstly, the results of this chapter provide a robustness check on the results of the voluntary contribution mechanism, presented in Chapter 5. A self-serving bias which subjects might use to convince themselves that others do not value money as much, is very unlikely to hold in the setup of this chapter. If an absence of cooperation is found in a similar way as in the FieldVCM treatment, than that provides further evidence of a poor external validity of the laboratory VCM game. Secondly, it might be the case that a difference in the payoff media causes punishment to have no effects on cooperation. Perhaps punishment or rewards might only promote cooperation when the payoff medium is similar to the payoff medium in the social dilemma stage. Therefore, the results of this chapter serve as a robustness check on the results of Chapter 6. Note that any motives of not punishing skillful fishermen seems unlikely in the setup of this chapter, while such motives might have played a role in Chapter 6. Fishermen might think it is fair that good fishermen earn more money. However, it is hard to see why fishermen would approve of any behavior of good fishermen that reduces the fishing time of others. Good fishermen have nothing to lose by the presence of less skilled fishermen.

The setup of the social dilemma experiment, termed the baseline game, is as follows. The baseline game is divided into two parts, and fishermen are placed into groups of four. In the second part, fishermen can fish unconstrained for up to 150 minutes and earn a monetary bonus of €2 per fish caught. The first part is divided into three periods of thirty minutes each in which each fisherman is allowed to catch up to two fish. A fisherman is allowed to keep each fish that is caught, but for each fish caught, ten minutes of fishing time in part 2 are subtracted from each of the other three group members. In two different treatments, punishment and reward opportunities are added to the baseline game. At the end of each period in part 1, fishermen receive feedback on the catch of each of the group members. Then, the fishermen have the opportunity to reduce their own fishing time in part 2 by up to three times five minutes; each interval of five minutes spent reduces the fishing time in part 2 of a group member with fifteen minutes in the punishment treatment. In the reward treatment, a fisherman can spend five minutes of his own time to increase fishing time of a group member by fifteen minutes. Based on previous laboratory experiments, it has been shown that transfer rewards have no effects on cooperation. Therefore, in order to give rewards a fair chance to establish cooperation, a net-positive nature of the instrument is considered.

In contrast to results reported from laboratory experiments, we do not find evidence that punishment or rewards promotes cooperation. Fishermen fish with the same intensity in the baseline treatment as they do in either the punishment or the reward treatment. Moreover, in the baseline treatment there are no signs of partially cooperative behavior in any of the three periods. Unlike in the lab, the subjects in our field experiment hardly make use of punishment opportunities. In the rare cases where punishment is used, it is targeted at those who catch most fish. Rewards are used more often than punishment, and in a different way. Rather than sending rewards to the group member with the lowest catch, in many instances, subjects divide their rewards equally among all group members.

The remainder of this paper is organized as follows: In section 7.2, the setting in which the field experiment is described, as well as the experimental design and procedure. Section 7.3 presents the data analysis, and section 7.4 concludes.

7.2 The setting, experimental design and experimental procedure

This section covers the experimental design and procedure of the baseline, punishment and reward treatments. Special attention is given to the measurement of cooperation, but first the setting of the field experiment is described.

7.2.1 The setting of the field experiment

The field experiment is conducted at a privately owned recreational fishing facility 'De Biestse Oevers'.[2] The fishing site consists of three ponds which are all

[2]For photos of the facility, see www.biestse-oevers.nl.

roughly fifty meters long and thirty meters in width. Each pond has room for twenty fishermen, ten of those spots are located at the east side and ten on the west side. The fee that is charged at the Biestse Oevers is either €12.50 or €15. For €12.50, four rainbow trout are put into the pond while for €15, two rainbow trout and one salmon trout are set out. A fisherman is then allowed to catch as many fish as possible, but no compensation is provided in case a fisherman does not catch the fish that are released on his behalf. There are strict rules when it comes to catching fish. For example, it is prohibited to use more than one rod at the same time, no scoop net can be used to catch fish, and each fish caught has to be taken away from the pond (presumably home). Because fishermen take away the fish they catch, they are all experienced with imposing negative externalities on other fishermen; each fish caught by one fisherman cannot be caught by another. Depending on the season, a fisherman can either fish four hours (from April to October) or five hours (from November to March).

7.2.2 The design of the Baseline treatment

In the baseline treatment, participants are placed in groups of four. Group composition is fixed throughout the entire experiment, but no participant is informed about the identity of his or her group members. A session of the baseline treatment consists of two parts: part 1 and part 2. Part 1 consists of three periods of thirty minutes, while part 2 is consists of one period which takes 150 minutes maximally. However, the duration in which a fisherman can fish in part 2 depends on what happens in his group in part 1.

Let us begin by explaining the rules of part 2. A fisherman can fish unconstrained in part 2, and each fish caught is his to keep and additionally yields a bonus of €2. After each half hour in part 2, the number of fish caught by fishermen who are allowed to fish in the next half hour is replenished. This rule is intended to keep the ratio of fish per fisherman constant. The spot at which a fisherman fishes in part 1 and part 2 is determined by a lottery; each participant has to draw a spot tag out of a black bag.

At the beginning of part 1, two fish per participant are put into the pond, plus an additional six to increase the probability of catching fish. In each of the three periods of thirty minutes, each fisherman is allowed to catch up to two fish. When the second fish is caught, the fisherman has to wait until the start of the next period. All fish caught can be kept, but no monetary bonus is attached to catching fish in part 1. At the end of each period, each fisherman is informed on the total number of fish caught by each individual group member. Over the periods, this information is presented in random order to ensure that a fisherman cannot link the catch of one fisherman from one period to another. At the start of each subsequent period, the stock of fish is replenished. This means that at the start of each period in a given session an equal number of rainbow trout is present in the pond.

The social dilemma is introduced by the following rule: Each fish that a fisherman catches in part 1 reduces the amount of time in part 2 for each of the other three group members by ten minutes. Assuming that a fisherman has monotonically increasing preferences over fishing time and money, fishing the full time in part 2, rather than fishing fully in part 1, makes each fisherman better off for the following reasons: (i) the duration of part 2 is an hour longer than part 1, (ii) in part 2 each fisherman can fish unconstrained in terms of the number of fish he is allowed to catch, and (iii), in part 2 each fish yields a monetary bonus of €2 which is absent in part 1.

One important observation is that for a group to fully reap the benefits of fishing in part 2, it is required that all group members give up fishing in part 1. However, the subgame perfect Nash equilibrium strategy for each fisherman is not to give up fishing, but to fish at full force in each period of part 1; for each individual fisherman there are no negative effects associated with catching fish, only other group members are harmed. To make sure that part 2 dominates part 1 even when the costs of waiting are included, we have conducted a questionnaire. On a day in which no experiment was conducted, we asked 21 fishermen which of the following two options they preferred. Option 1 was to fish for ninety minutes and be allowed to catch six fish. Option 2 was to wait for ninety minutes and then to fish for 150 minutes unconstrained. In case a fisherman indicated to prefer option 1, we repeated the question, but then indicating that €0.50 is earned per fish caught in option 2. This procedure was repeated until a fisherman indicated to prefer option 2 over option 1. Out of 21 fishermen, 14 indicated that they preferred option 2 over option 1 even if no money could be earned in option 2. Two fishermen indicated that they never preferred to wait for ninety minutes, they always preferred option 1 over option 2. The remaining five fishermen preferred option 2, but only if compensated well enough; the fisherman with the highest willingness to accept demanded a price of €1.50 per fish caught in part 2. For nineteen out of 21 fishermen this means that our parametrization indeed causes the social optimum to require all fishermen not to fish in part 1. Therefore, we are confident to claim that the tradeoff in our experiment resembles a social dilemma. In Appendix 7.A.2 we have included the questionnaire.

7.2.3 The design of the Punishment and Reward treatment

The rules described for the baseline treatment are effective in both the punishment and reward treatment. However, in the punishment and reward treatment subjects are given an opportunity to directly reciprocate the behavior of group members. At the end of each period in part 1, subjects receive information about the catch of each of the other group members. The order in which this information is provided is randomized over the periods, as is the case in the baseline treatment. Then, subjects can choose to reduce the length of their own fishing time in part 2 by up to three intervals of five minutes. Each interval of five minutes spent, reduces the fishing time in part 2 of a designated group member by fifteen minutes in the punishment treatment. In the reward treatment, each interval of five minutes spent, increases the fishing time in part 2 of a group member by fifteen minutes. Subjects can choose how to allocate the intervals of five minutes, as long as no less than zero and no more than three intervals are used. If a subject has no time left in part 2, then neither the punishment nor the reward instrument can be used. After each subject has made his decision, feedback is given on: (i) the total number of five minute intervals the other three group members have directed towards the subject, (ii) the total amount of time available in part 2 as a result of the decisions of the group members, (iii) the total number of five minute intervals directed towards each of the other group members. No information is provided on the total length of fishing time each of the other group members has left. This is done in order to let reward and punishment decisions be based on the actions of group members in the previous period only, and also to prevent retaliation taking place.

For each subject and in all treatments, the total length of part 2 cannot be smaller than zero minutes or greater than 150 minutes. This poses a problem in the reward treatment, since it is possible that subjects end up with more than 150 minutes of fishing time. For that reason, we have told the subjects in the reward treatment that they receive coupons in case the 150 minutes are binding. The coupons can be used at De Biestse Oevers at any date of choice in order to reduce the entrance fee that is normally charged. Because it is impossible for us to track when the fishermen would use these coupons, we could not pay €2 for every fish they caught. To make up for this loss, we gave a coupon worth of €3 for every thirty minutes in excess of the 150 minutes of fishing time in part 2. This is equivalent to a compensation of two minutes fishing time on another day for every one minute a fisherman has in excess of 150 minutes.

Table 7.1 provides a summary of the experimental design of the baseline, punishment and reward treatments.

Description	Value
Group size	4
Part 1	
Periods	3
Length of each period	30 minutes
Maximum catch per period	2
Earnings per fish caught	€0
Reduction others in part 2 per fish caught	10 minutes
Part 2	
Maximum length	150 minutes
Maximum catch	no maximum
Earnings per fish caught	€2
Punishment/Reward	
Costs to punish/reward	5 minutes
Impact of punishment/reward	15 minutes
Maximum amount to punish/reward per period	3 × 5 minutes

Table 7.1 Experimental parametrization. Part 1 and Part 2 are effective for the baseline, punishment and reward treatment.

7.2.4 Measurement of cooperation

As compared to traditional laboratory experiments, measuring cooperation in our field setting comes with some difficulties. Ideally, the number of fish not caught reflects the level of cooperation within a group. The problem with this approach is that catching fish is subject to external influences such as weather conditions and fishing skills of the fishermen, as well as on the degree of co-operation. Therefore, interpreting the absolute catch of fish as a measure of cooperation in the baseline treatment is misleading; it is unclear whether a fish not caught is due to cooperativeness of the subjects or simply bad luck. The catch of fish in the punishment or reward treatment compared to the baseline treatment does provide a direct measure of the effectiveness of the instruments. Our first measure of cooperativeness, the Catch measure, is therefore given as follows:

$$C = \sum_i x_{it}^{Baseline}/n - \sum_i x_{it}^{\{Pun,Rew\}^j}, \tag{7.1}$$

where $\sum_i x_{it}^{\{Pun,Rew\}^j}$ is the total catch in period t of group j in either the Punishment or Reward treatment, and $\sum_i x_{it}^{Baseline}/n$ is the average catch in period t of all groups in the baseline treatment. A value of C equal to 0 would indicate zero cooperation, and a positive level would indicate the presence of cooperation in the punishment or reward treatments.

Our second measure of cooperativeness is to consider the input process of catching fish: fishing effort. Rainbow trout is a predator fish that actively pursues bait. Therefore, a fisherman can increase his catch by constantly casting bait and reeling it back in. The movement of the bait naturally attracts the fish and the more the bait moves, the bigger the probability of catching fish. Two experimenters continuously gathered information on the effort levels of the sixteen fishermen. One experimenter monitored eight fishermen at the east side of the pond while the other experimenter monitored eight fishermen at the west side. Measuring effort rather than output has three advantages in our experiment. The first advantage is that a fisherman can consciously 'work harder' by casting his bait at a faster pace. In appendix 7.A.1 we show that there is a positive correlation between effort and catch; the more a fisherman casts his rod, the more fish are caught. The second advantage is that, unlike catching fish, casting bait is independent of weather influences, with the exception of extreme weather conditions such as a heavy storm. Finally, the most important advantage is that casting bait provides an unambiguous measure of cooperation. Whereas a fish not caught might be interpreted as cooperation or bad luck, not casting bait cannot be interpreted differently than evidence in favor of cooperation. To measure cooperation, we report the number of times a fisherman casts his bait per minute in each of the treatments. In case an absolute level of casts per minute greater than zero is found in the baseline treatment, this is interpreted as evidence against cooperation. The comparison of effort levels in the punishment and reward treatment relative to the baseline treatment give insights in how effective the instruments are in promoting cooperation. We will use the same measure of cooperation presented in equation (7.1) for the effort levels, using average casts per minute rather than catch of fish, and term this the Effort measure of cooperation.

7.2.5 Experimental procedure

The experiments have all been conducted in April and May of 2010. Recruitment was done two weeks in advance by handing out flyers at the site. The flyer informed interested fishermen of the possibility to take part in an experiment of Tilburg University, and a registration list was available at the canteen of the Biestse Oevers. A maximum of sixteen participants was allowed to take part in each session.

Upon arrival of the pond, all sixteen participants in each session were given a word of welcome after which fishing spots were randomly assigned for part 1 of the experiment. The fishermen were collectively told that instructions would be given at the fishing spot in groups of four, and that all participants received the same information. It was stressed that the group in which the instructions were explained need not be the group members once the experiment began; any of the other fifteen participants are equally likely to be group members. We have chosen to provide information in a decentralized way in order to prevent attempts of fishermen to communicate strategies in using punishment or reward. We are confident that we have succeeded in our approach, since all fishermen abided by the rules not to communicate.

The information provided to the fishermen was mediated through a laptop. After each period, the experiment briefly stopped and two experimenters told each fishermen the relevant information. In the punishment and reward treatments the experimenters went by twice, once to gather the punishment or reward decisions of the fishermen and once to give feedback on the decisions made by the other group members.

In Table 7.2 we provide a summary of the three treatments. The table reports the number of groups per treatment and the main feature of the treatment. In the analysis that follows, each group is taken as an independent observation.

Treatment	Groups	Main feature
Baseline	12	Cooperation in the absence of instruments
Punishment	8	Effectiveness of punishment on cooperation
Reward	8	Effectiveness of rewards on cooperation

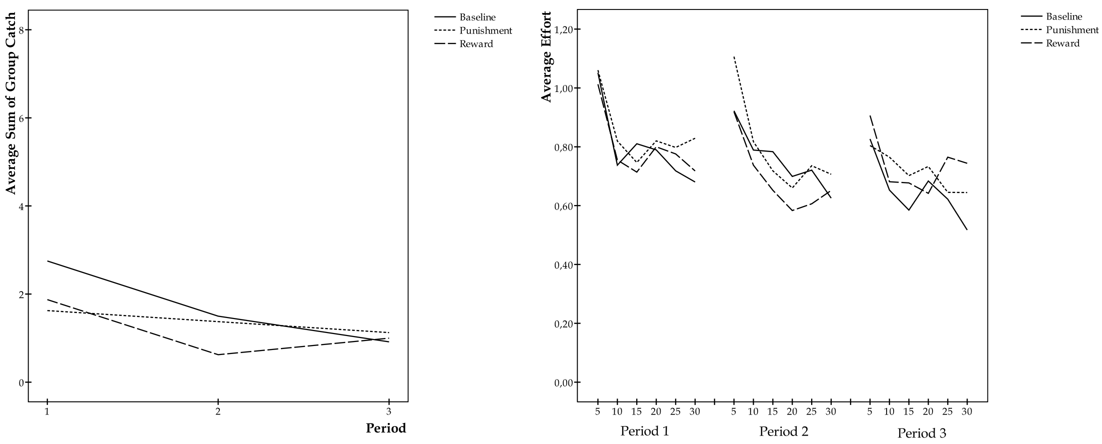
Table 7.2 Number of groups and main feature for the baseline, punishment and reward treatments.

7.3 Data analysis

In this section, the data are presented. Section 7.3.1 shows the development of play in the social dilemma stage, while section 7.3.2 shows how the reward and punishment instruments are used.

7.3.1 The social dilemma stage

In many laboratory experiments, it is shown that punishment or reward opportunities have a positive impact on the development of play in the social dilemma stage. If such effects carry over to our field setting, we would expect a clear difference in catch and effort between the baseline treatment on the one hand, and the reward and punishment treatments on the other. Figure 7.1 shows that this is not the case, neither for the amount of fish caught (panel a), nor for the effort exerted (panel b).



(a) (b)

Figure 7.1 (a) Average sum of fish caught per period in the baseline, punishment, and reward treatment. **(b)** Average effort per group in the three treatments. Effort is reported at five minute intervals within each period.

From Figure 7.1 we can make two important observations. The first is that the absolute levels of catch and effort in the baseline, punishment and reward treatments are very similar over the three periods. The second observation is that there is a decline in the number of fish caught, as well as in the level of effort, as the periods go by. This decline in catch and effort is similar in all treatments, which implies that neither punishment nor reward has any effect on cooperation relative to the baseline treatment. This observation is in stark contrast to results of many experiments conducted in the lab. We summarize our finding below:

Result 1 Average catch and effort is the same in the first three periods in all three treatments. Neither punishment, nor rewards cause an overall increase in cooperation.

Support for result 1: Support for result 1 is given by a series of Mann-Whitney tests. As an independent observation, we use the average catch or effort levels at the group level. Catch in the punishment treatment does not differ from catch in the baseline treatment ($N_1 = 12, N_2 = 8, p = 0.473$). Likewise, there is no significant difference between catch in the baseline and reward treatment ($N_1 = 12, N_2 = 8, p = 0.427$). As the former tests suggest, we cannot find a significant difference in catch between the punishment and reward treatment either ($N_1 = 8, N_2 = 8, p = 0.195$).

With regards to the effort levels, there is no significant difference between the punishment and baseline treatment ($N_1 = 12, N_2 = 8, p = 0.521$), nor between the reward and baseline treatment ($N_1 = 12, N_2 = 8, p = 0.851$). Finally, there is no difference in effort between the punishment and reward treatment ($N_1 = 8, N_2 = 8, p = 0.798$).

Additional support for result 1 is given by means of a regression analysis, provided in Table 7.3. The dependent variable is subject i's change in catch $x_{i,t+1} - x_{i,t}$ (columns (i) and (iii)), or change in effort $e_{i,t+1} - e_{i,t}$ (columns (ii) and (iv)) from period t to period $t+1$. The independent variables are the sum of punishment or rewards received by subject i, $\sum_{j \neq i}^{N} p_{ji}$. In case of catch, the variable $x_{i,t} - x_{-i,t}$ is included. This variable takes into account the effects of regression to the mean, independent of the punishment or reward instrument. Note that all subjects are given information on each group member's catch, but not on each group member's effort. Therefore, the regression to the mean variable is not included for the case of effort. Table 7.3 shows that neither

Dependent variable: $x_{i,t+1} - x_{i,t}$ ((i) and (iii)), $e_{i,t+1} - e_{i,t}$ ((ii) and (iv))				
	(i)	(ii)	(iii)	(iv)
	Punishment		Reward	
$\sum_{j \neq i}^{N} p_{ji}$	0.358^{***}	0.038	0.128^{**}	0.024
	(0.071)	(0.021)	(0.061)	(0.044)
$x_{i,t} - x_{-i,t}$	-0.67^{***}		-0.355^{***}	
	(0.157)		(0.077)	
Constant	-0.216^{*}	-0.064^{***}	-0.235^{**}	-0.051
	(0.108)	(0.013)	(0.1)	(0.051)
N	64	64	64	64
R^2	0.3797	0.0396	0.2685	0.0114

Table 7.3 OLS model to estimate the determinants of a change in catch (columns (i) and (iii), or a change in effort (columns (ii) and (iv))). Standard errors, clustered at the group level, are reported between parentheses.***: significant at the 1%-level, **: significant at the 5%-level.

punishment, nor rewards cause subjects to catch less fish, or exert less effort in the next period. Moreover, the signs of the punishment and reward coefficients are opposite of what is expected; the more punishment or rewards a subject receives, the more fish he catches in the next period. The enforcing influence that punishment or rewards show to have in many laboratory experiments are not found in our field setting. Regression to the mean is only found when catch is considered. This shows that there is some conformity in the number of fish that group members catch. The variable does not tell whether the conformity is towards cooperative levels, or uncooperative levels. ■

We present further insights into the declining trend in catch and effort in the treatments. Visual inspection suggests that the decline in catch and effort is similar over the treatments, although it seems that the decline is more pronounced in the baseline treatment. As we will show below, and summarize in result 2, the decline of catch and effort is indeed smaller in the punishment and reward treatment than in the baseline treatment. This again confirms our finding that neither punishment nor reward has any positive effect on promoting cooperation.

Result 2 The decline in catch and effort levels over the periods in the punishment and reward treatments is not greater than in the baseline treatment.

Support for result 2: Support for result 2 is given by performing a series of Wilcoxon matched pairs tests with the Catch and Effort measure of cooperation, as stated in section 7.2.4. The trend in catch is computed by matching period 1 with period 3 of the same group in the punishment or reward treatment. As a benchmark of cooperation, we take the average catch in period 1 of all twelve groups in the baseline treatment and the average catch of those groups in period 3. This decline in catch provides the trend of cooperation in absence of the enforcement instruments. A matched pair in the punishment treatment is created by subtracting group k's catch in period 1 from the average catch in period 1 of the twelve groups in the baseline treatment. To complete the matched pair, the catch of group k in period 3 is subtracted from the average catch in period 3 of the twelve groups in the baseline treatment. A similar procedure is used for the reward treatment, giving us eight observations in both treatments. In the punishment treatment, we find a marginally significant difference in the decline in catch over time; the decline in catch is slightly less in the punishment treatment than in the baseline treatment ($N_1 = N_2 = 8, p = 0.092$). In the reward treatment we find no significant difference using our catch measure of effort ($N_1 = N_2 = 8, p = 0.118$).

When we apply the same procedure to our Effort measure of cooperation, we do find a significant difference in trends. Compared to the baseline treatment, the decline in effort is less pronounced in both the punishment treatment ($N_1 = N_2 = 8, p = 0.046$) and the reward treatment ($N_1 = N_2 = 8, p = 0.036$). Since the decline in effort levels is smaller in the punishment and reward treatment, we conclude that both enforcement instruments do not lead the fishermen to become more cooperative over time, on average. ■

Let us elaborate some more on the declining trend observed in all treatments. Two explanations are consistent with the observed decline of effort. The first explanation is that fishermen might experience decreasing marginal benefits of fishing, or that they become physically tired from the act of fishing. It might be the case that the fishermen always fish at full force in all of the periods, but that they cannot keep up the levels of effort provided in the early stages of the experiment. Such a view would leave no room for any cooperation in any period in any of the treatments. In Chapter 5, it is shown that fishermen fish with the same intensity in the social dilemma as they do in the Private Incentives treatment. This is a treatment without a social dilemma, intended to provide fishermen with maximum incentives to catch fish. The reduction in effort reported in that treatment is consistent with fishermen experiencing declining marginal benefits of fishing, or becoming physically tired. The second explanation for a decline in effort over the periods is a desire of subjects to partially cooperate, irrespective of whether or not punishment or reward opportunities are present. Partial cooperation can occur in a number of ways, two of which are: i) subjects fish with maximum effort in the early periods, and withhold fishing in later periods, ii) subjects fish with maximum effort in all periods to catch the first fish, but provide no effort to catch the second fish. We focus on effort levels only, rather than catch levels, since effort provides an unambiguous measure of cooperation. If at any time a fisherman has his rod in the water, then this cannot be interpreted differently than a revealed preference to catch an additional fish. As stated in the following result, we cannot find evidence of partial cooperation in any of the treatments:

Result 3 There is no partial cooperation in any of the periods. This holds for the baseline, the punishment and the reward treatment.

Support for result 3: Let us start the support for result 3 by considering the effort levels of the subjects to catch the first fish. A two-sided Student t-test, taking each individual effort level in a period as an observation, shows that the effort levels to catch the first fish are always greater than zero in all periods. This holds for the baseline treatment ($N = 48, p_{t=1} = p_{t=2} = p_{t=3} < 0.01$, where $p_{t=x}$ is the p-value of the Student t-test in period $x \in \{1, 2, 3\}$). Similarly, effort levels to catch the first fish are always greater than zero in the punishment treatment ($N = 32, p_{t=1} = p_{t=2} = p_{t=3} < 0.01$) and the reward treatment ($N = 32, p_{t=1} = p_{t=2} = p_{t=3} < 0.01$).

The second piece of evidence is obtained by comparing effort levels to catch the first fish to effort levels to catch the second fish. Fishermen might be partially cooperative by taking their rod out of the pond once the first fish has been caught. However, a series of Wilcoxon matched pair tests shows that this is not the case; effort levels remain the same. As an independent observation, we use the effort level of a fisherman in a period in which he has caught at least one fish. The matched pair consists of the effort level to catch the first fish, and the effort level to catch the second fish. In the baseline treatment, effort levels for the first fish are similar to those to catch the second fish in period 1 ($N_1 = N_2 = 24, p = 0.768$), in period 2 ($N_1 = N_2 = 15, p = 0.198$), and in period 3 ($N_1 = N_2 = 9, p = 0.401$). Likewise, in the punishment treatment effort levels to catch the second fish are equal to those to catch the first fish in period 1 ($N_1 = N_2 = 9, p = 0.594$), period 2 ($N_1 = N_2 = 8, p = 1.000$) and period 3 ($N_1 = N_2 = 7, p = 0.310$). The same holds for the reward treatment where effort levels to catch the second fish are similar to those to catch the first fish in period 1 ($N_1 = N_2 = 10, p = 0.575$), period 2 ($N_1 = N_2 = 4, p = 0.273$) and period 3 ($N_1 = N_2 = 6, p = 0.345$). ■

Finally, we present some evidence in favor of the explanation that fishermen become physically tired of catching fish. We do so by considering the effort levels observed in part 2 of the experiment. It is expected that if fishermen become tired, then they should exert less effort over time, even in part 2. Note that in part 2 of the experiment, fishermen are expected to fish at their maximum ability, because they can fish unconstrained and they earn money for each fish caught. The evidence presented below is also consistent with the fishermen experiencing a decline in marginal benefits of fishing. It is impossible to disentangle the tiredness explanation from the declining marginal benefits explanation. However, fishermen who show to have higher marginal benefits of fishing in part 1 by fishing at faster pace, show that they have no regard for others. Note that if fishermen exert more effort in part 2 than in period 3 of part 1, then the decline in effort in part 1 is consistent with an increasing willingness to cooperate after all. We find no such effect, and summarize our finding in result 4:

Result 4 Fishermen exert less effort in part 2 of the experiment than in part 1 of the experiment.

Support for result 4: Support for result 4 is given by a Wilcoxon matched pairs test, taking as an independent observation the average group effort levels in period 3 of part 1 and the average group effort levels in part 2. Effort levels in period 3 of part 1 are greater in the baseline treatment ($N_1 = N_2 = 12, p = 0.002$), the punishment treatment ($N_1 = N_2 = 8, p = 0.024$), and in the reward treatment ($N_1 = N_2 = 12, p = 0.018$). ■

7.3.2 The use of the instruments

As we have shown, neither punishment nor rewards have any positive effects on promoting cooperation. This stands in sharp contrast to the effects usually reported in the lab. In order to gain insights into why this might be the case, it is important to take a look at how punishment and reward is used. Figure 7.2 shows the average number of five minute intervals used per group over the three periods in the punishment and reward treatment. The first observation

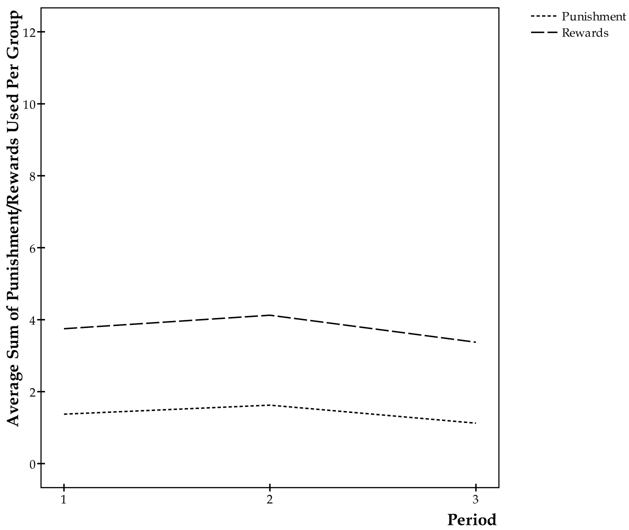


Figure 7.2 Average sum of five minute intervals used per group in the punishment or reward treatment.

that can be made, is that rewards are used more often than punishment. This finding is in line with findings from laboratory experiments on punishment and reward. For example, Vyrastekova and van Soest (2008) and Rand et al. (2009)

show that rewards are used more frequently than punishment. We summarize this finding in result 5:

Result 5 Rewards are used more often than punishment.

Support for result 5: Support for result 5 is given by a Mann-Whitney test, taking the sum of tokens used per group over the three periods as an independent observation ($N_1 = 8, N_2 = 8, p = 0.065$). ■

Another observation that follows from Figure 7.2, is that punishment and reward are relatively constant over the three periods. In the lab, punishment is typically used in early periods of the game, and decreases over time (see for example Gürerk et al. (2004), Denant-Boemont et al. (2007), Nikiforakis (2008), Rand et al. (2009)). The threat of punishment usually causes subjects to behave cooperatively, and therefore its use diminishes over the periods. In our field experiment, we do not find such an effect; punishment is used rarely and subjects remain selfish in all periods. Therefore, the threatening effects of punishment observed in many laboratory experiments do not play a role in our field setting. When it comes to rewards in the lab, usually the level of reward is increasing over the periods (see for example Chapter 3, Vyrastekova and van Soest (2008), and Rand et al. (2009)). Although rewards are used in our field experiment as well, the data do not suggest that subjects use more rewards as the periods go by. There is no evidence of either an increasing or decreasing trend in the use of the instrument.

Result 6 Punishment and reward assignment does not follow an upward or downward trend.

Support for result 6: Support for result 6 is given by a Wilcoxon matched pairs test. The pairs consist of the sum of five minute intervals in period 1 of a treatment and the sum of five minute intervals used in period 3 of the same treatment. We take each group as an independent observation. In the baseline treatment, groups use punishment equally often in period 1 as in period 3 ($N_1 = N_2 = 8, p = 0.593$). Similarly, in the reward treatment, there is no trend over time ($N_1 = N_2 = 8, p = 0.599$). ■

Many studies of punishment or reward in the laboratory show that the use of the instruments is based on reciprocity; those who free-ride are punished while those who cooperate are rewarded. Using a Hurdle regression analysis (columns i-iv) and a Tobit regression analysis (columns v-vi), we find moderate support for this mechanism. In the Hurdle model, we first estimate the decision that subject i uses the instrument. Then, conditional on using the instrument, the level of the decision is estimated. Two variables are included that take into account the individual differences in catch between the two subjects. Additionally, two variables are included that take into account how subject j differs from subject i. Finally, a variable is taken into account that measures the trend of use of the instruments over the periods. Table 7.4 shows the results.

Dependent variable: # five minute intervals subject i sends to j	(i) Punishment Decision	(ii) Punishment Level	(iii) Reward Decision	(iv) Reward Level	(v) Punishment Treatment	(vi) Reward Treatment
$\max\{0, c_i - c_j\}$	0.079**	−0.542	−0.178***	0.001	3.336***	−0.645**
	(0.039)	(0.531)	(0.065)	(0.058)	(1.191)	(0.326)
$\max\{0, c_j - c_i\}$	0.015	0.891*	0.213	−0.006	0.869	0.768
	(0.024)	(0.535)	(0.204)	(0.058)	(1.090)	(0.778)
$\max\{0, c_j - \bar{c}_{-j}\}$	0.042	−1.416***	−0.172	0.115	1.601	−0.555
	(0.039)	(0.211)	(0.198)	(0.087)	(1.400)	(0.737)
$\max\{0, \bar{c}_{-j} - c_j\}$	−0.157**	0.105	0.331**	0.202***	−7.060***	1.283*
	(0.075)	(0.881)	(0.140)	(0.076)	(2.521)	(0.661)
Period	−0.011	0.379***	0.006	−0.005	−0.372	0.028
	(0.008)	(0.076)	(0.044)	(0.038)	(0.366)	(0.138)
Constant		1.255***		0.961***	−4.263***	−0.872*
		(0.378)		(0.078)	(1.485)	(0.486)
N	288	24	288	87	288	288

Table 7.4 Hurdle model (columns i-iv) to estimate the determinants of punishment and reward. The punishment and reward decision is estimated using a Probit specification. The punishment and reward level is estimated using truncated linear regression. Tobit model (columns v-vi) to estimate the determinants of punishment and reward. Marginal effects are reported in the columns for punishment and reward decision. Standard errors (clustered at the group level) are reported between parentheses. ***: significant at the 1%-level, **: significant at the 5%-level, *: significant at the 10%-level.

The results show that subjects who catch less than the group average, receive less punishment tokens and more reward tokens. But, given that a subject who catches less does receive punishment, those punishment tokens are likely to be sent by group members who caught much fish. Likewise, given that a subject who catches less fish than the group average, rewards are not given by those group members who caught a lot of fish. It should be noted that although the regression analysis shows that some variables have a statistically significant impact, the magnitudes of the coefficients are such that the economic significance is low. For example, when a subject has caught two fish more than the group average, then there is only a 31.4% chance that he receives punishment.

Another way in which subjects can display reciprocal behavior, is by providing rewards in the next period, when they are given rewards in the current period. Similarly, subjects might be inclined to make more use of the punishment instrument once they find out that they have been punished in the previous period. A Pearson correlation coefficient shows evidence of reciprocal behavior in case of rewards, but not in case of punishment. The Pearson correlation coefficient has a value of 0.309 in the reward treatment, taking each decision of each subject as an observation ($N = 64, p = 0.013$). In the punishment treatment, this value is 0.019 and insignificant ($N = 64, p = 0.879$). Note that the use of rewards suggests a similar mechanism as the one presented in Chapter 3. A positive correlation between rewards received now, and rewards sent in the next period, suggests that subjects would like to exchange rewards with other group members, rather than focussing on the group social dilemma.

Let us elaborate further on the different use of punishment or reward. In Table 7.5, a breakdown of two interpretations of the mechanism of punishment and reward is given. In the first row of the punishment and reward block, the table shows the fraction of subjects who spend at least one of their five minute intervals. The second row presents the fraction of subjects who use the instrument in a way that resembles reciprocity; by sending most punishment to those group members who catch most, or by sending most rewards to those group members who catch least. In the last row of each block, the table shows the fraction of subjects who divide their tokens equally among their group members, conditional on making use of reward or punishment.

	Period 1	Period 2	Period 3
Punishment			
% Subjects who spend more than zero intervals	22	28	16
% Subjects who spend most intervals on those who catch most fish	86	89	100
% Subjects who divide intervals equally, conditional on spending	0	11	0
Reward			
% Subjects who spend more than zero intervals	44	41	38
% Subjects who spend most intervals to those who catch fewest fish	36	23	42
% Subjects who divide tokes equally, conditional on spending	36	69	33

Table 7.5 Mechanisms of punishment and reward in more detail.

As the table shows, almost all of the instances of punishment are reciprocally; punishment is directed to those group members who catch the most fish. This is in line with the regression results reported in Table 7.4. In only one period, a fisherman decided to punish all three group members equally (while all group members caught zero fish). Although a considerable amount of rewards are based on reciprocal motives, many rewards are divided equally. This suggests that subjects use rewards as an investment, hoping that fellow group members will reward them in return. In Chapters 3 and 4 on rewards it is shown that some subjects follow such a strategy. However, it is hard how to provide clean evidence for this claim, because in the total eighteen times a subject divided rewards equally, in ten of those instances all group members caught zero fish. It might be the case that the decision to divide the rewards equally is based on reciprocal motives. A clear distinction between either a reciprocal use, or an 'equal division' use is cumbersome in those cases, because the data are consistent with both views.

7.4 Conclusion

The results report no evidence at all of cooperation in a social dilemma experiment conducted in a field setting. Fishermen played a game in which catching fish imposes a negative externality on three other fishermen. In a treatment that allowed subjects to punish, no improvement of cooperation was found as compared to the baseline setting. A similar result holds for a treatment that allows subjects to reward each other. Our findings are in line with those of Chapter 6, but in contrast to a vast amount of literature using laboratory experiments that test those instruments in social dilemmas.

We find no evidence of partial cooperation; fishermen try to as hard as they can to catch as much fish as they can, in early periods as well as in later periods. This finding is in line with the results from chapter 5 and 6, but in contrast to the classical results from the lab, where in early periods cooperation is positive. The main evidence for this claim is provided by measuring the effort levels of the fishermen to catch fish. Effort is measured as the total number of times a fisherman casts his rod in the water. At no time in part 1 do any of the participants stop fishing. Besides the absence of cooperation in the baseline treatment, we cannot find any evidence that punishment or reward has a positive effect on cooperation; fishermen catch as much fish, and exert as much effort in the punishment or reward treatments as in the baseline treatment. Additionally, we find that punishment is rarely used, in contrast to many laboratory experiments. Rewards on the other hand are used slightly more, but many subjects refrain from using it in any of the three periods.

The mechanism of both punishment and reward observed in many laboratory experiments is mainly characterized by two features. First, punishment is used by cooperators, directed at free riders, while rewards are used by cooperators directed at cooperators. Second, the more an agent free rides, the more punishment is targeted to him or her. A similar finding is present in the case of rewards; the more an agent cooperates, the more reward tokens are received. The mechanism of punishment that is used in our field experiment has a similar mechanism as reported in the lab; those who catch most fish attract the most punishment. In our field experiment, rewards are used more often than punishment, but there is no significant correlation between catch of fish and attraction of rewards.

The results of this chapter, and the previous one, are important in a number of ways. The first is that the vast body of laboratory experiments have influenced theorists to give a theoretical basis for the finding that using punishment or reward promotes cooperation. Examples of theoretical work show that punishment strategies can be evolutionarily stable in social dilemmas with small groups (Boyd and Richerson (1992)), in large groups with inter-group migration (Henrich and Boyd (2001)), and in social dilemmas with uncertain futures (Sethi and Somanathan (1996) and Gintis (2000)). In the non-lab setting presented in this and the previous chapter, there is no evidence at all of cooperation when allowing for punishment or reward. This shows that there are settings in which the external validity of laboratory results of punishment or reward is low. Doubting the degree in which laboratory results carry over to the field environments makes the grounds on which the theoretical models are built shaky.

A second way in which the results of this and the previous chapter contribute to the literature is by considering why subjects use punishment or rewards. Given that punishment and reward are used, it remains a puzzle why it is used. By means of theoretical models, two different motivations are distinguished, mainly targeted at explaining the use of punishment. The first reason why agents might use punishment is to eliminate payoff differences between free riders and cooperators. An aversion against payoff differences might stem from inequality aversion, as modeled by Fehr and Schmidt (1999), Bolton and Ockenfels (2000) and Fowler et al. (2005). Another reason why payoff differences might matter, comes from the notion that if cooperation should have any chance of surviving as a strategy, then it has to be the case that payoff differences with free riders are completely gone. Punishment is hence performed in order to achieve greater profits in future interactions (see for example van Lange (1999), Charness and Rabin (2002) and Price et al. (2002)). In this line of reasoning, the outcome of play in a social dilemma is more important than the intentions to free ride. The second reason why punishment and rewards might be used, has to do with the intentions of free riding, and is called ‘strong reciprocity’;

punishment is used to communicate that one does not approve of the unfair intentions others have to deviate from the social norm (see for example Gintis (2000), Fehr and Fischbacher (2003) and Gintis et al. (2003)). With this argument, the intentions are more important than the final outcome observed in the social dilemma.

The underlying assumption why agents punish or reward, is that there are some 'hard wired' preferences to use the instruments (Price et al. (2002)). Using punishment or reward could give agents some utility. This holds regardless of the argument that punishment or reward is used to mitigate payoff differences, or that it is used to enforce future cooperation. Only recently, evidence that humans do enjoy the act of punishment has come from neuroeconomics (for an overview, see for example Fehr and Camerer (2007)). Using a positron emission tomography camera, the activity in the brain of a human subject can be analyzed. de Quervain et al. (2004) show that when subjects punish defectors in a trust game, the dorsal striatum is activated, the part of the brain that processes rewards when goal directed actions are achieved. The authors find that the greater the punishment given, the more the brain activates the dorsal striatum. Singer et al. (2006) show that when free riders in a sequential Prisoner's Dilemma game are punished with electric shocks, male subjects show activation in the left ventral stratum. This part of the brain is also linked with reward processing.

The results of this chapter and the previous one add insights into the question why humans punish, although the results are mixed. The fact that some use of the instruments is maintained throughout all periods, while cooperation is absent in both the punishment and reward treatments, suggests that subjects do not use the instruments to promote cooperation. This is consistent with the view that humans are inequity averse, but it is also consistent with strong reciprocity. However, the main underlying motivation of punishment in strong reciprocity is an unfair intention. The current field experiment provides a unique insight in the intentions to free ride, because all subjects could monitor each other's fishing activity. Since at no time a fisherman ceased fishing, the subjects were fully aware of the negative intentions of others. Therefore, if intentions are really driving forces of punishment and reward, then one would expect that punishment is used more often than reward. This prediction is the opposite of what is observed in the current field experiments. One interpretation of the results of the current chapter, is that subjects use non-monetary punishment because it gives pleasure to do so. Even though subjects can conjecture that no cooperation will be achieved, utility is derived from the fact that one can take revenge. Revenge is mainly targeted to those who catch fish, rather than on those who intend to catch fish. In case of rewards, such an interpretation is less clear because many subjects divide their reward tokens equally among the other group members. This does not suggest that subjects derive joy from the act of rewarding, but rather that they use rewards opportunistically, hoping to attract rewards form others.

Work by, for example, Ostrom (1990) and Casari and Plott (2003) shows that decentralized punishment does promote cooperation in field settings. The results of this chapter, and the previ ous one, suggest that for punishment or reward opportunities to be effective, an interplay with other factors might be necessary. In field settings, for example, peers are likely to be able to communicate with each other, or are likely to meet each other in other facets of life. In case those opportunities are absent, depending on the mere presence of punishment or rewards might not give the desirable results that laboratory experiments predict.

7.A Appendix

7.A.1 Statistical analysis of the effect of effort on catch

In this appendix, we show the correlation between catch and effort, as measured by the number of times a fisherman casts his rod per minute. We find a positive correlation: More effort leads to more fish, and therefore we interpret our measure of effort as a legitimate proxy for cooperativeness. An ordered Probit model is used, taking a fisherman's catch and effort in each of the four periods as an independent observation.

As Table 7.6 shows, the coefficient of Effort is highly significant. The quadrant dummy variables correct for the spot at which a fisherman is fishing. Two of the quadrant variables are significant, indicating that the probability to catch fish at some spots is greater than at other spots.

Dependent variable: Number of fish caught in a period	
Effort	0.871***
	(0.158)
Quadrant Fixed Effects	Yes
N	448
pseudo-R^2	0.0411

Table 7.6 Ordered Probit estimation on the relationship between individual effort and individual catch. Standard errors, clustered at the subject level, are reported between parentheses. ***: significant at the 1%-level.

7.A.2 Questionnaire results

This appendix presents the questionnaire that we used to determine whether a social dilemma exists.

Dear Fisherman,

On behalf of Tilburg University we would like your cooperation to fill in a questionnaire. We ask you to indicate which of the following two options is your preferred option.

Option 1	Option 2
Fish 1.5 hours at maximally 6 fish	Wait for 1.5 hours at your fishing spot and then fish for 2.5 unlimitedly
Fish 1.5 hours at maximally 6 fish	Wait for 1.5 hours at your fishing spot and then fish for 2.5 unlimitedly, receiving €0.50 for each fish caught
Fish 1.5 hours at maximally 6 fish	Wait for 1.5 hours at your fishing spot and then fish for 2.5 unlimitedly, receiving €1 for each fish caught
...	...
...	...
Fish 1.5 hours at maximally 6 fish	Wait for 1.5 hours at your fishing spot and then fish for 2.5 unlimitedly, receiving €15 for each fish caught

7.A.3 Instructions for the Baseline treatment

Word of welcome

Welcome to this study by Tilburg University. Before we start, we want to point out two things. Firstly, this study is independent of the owners of the trout fishing facility. We are grateful that we are allowed to conduct this study here, but the owners have nothing to do with this project. All responsibility lies with Tilburg University. Secondly, we want to make clear that this study has nothing to do with animal welfare issues or the like. As researchers, we accept the rules and habits of recreational fishing as practiced at the trout fishing facility. We cannot tell you the exact aim of this study. We do want to stress that your privacy is guaranteed; none of the results we report can be traced back to individual participants.

As you know, you do not have to pay to take part in this study. The entrance fees are paid by Tilburg University. You are allowed to take home all fish you catch. In addition, you can earn money. We ask you to abide strictly by the rules which we impose.

The study

In the next four hours, we ask you to fish according to the following rules. First, all rules that normally apply at the trout fishing facility remain in place. This means that it is not permitted to throw fish you catch back into the pond, you are only allowed to fish with one rod, you are only allowed to use a scoop net to set fish ashore, you are only allowed to use the usual types of bait, etc.

Timing

- The study takes place over four hours, from around 8.00 a.m. till noon.
- In case we start a little later, we will end a little later.
- The morning is divided into two parts, part 1 and part 2.
- Part 1 takes 90 minutes and is divided into three periods of 30 minutes.
- Part 2 consists of 1 period in which you can fish freely.

Group formation

- You are placed in a group with 3 other participants.

- Your group consists of the same 4 participants in part 1 and part 2 of this study; you and the 3 other participants.
- We do not tell you in which group you are placed, and you are not allowed to communicate with other participants.

Part 2 of the study

- In part 2 you are allowed to catch as much fish as possible
- You can take home each fish you catch and you receive €2 for each fish caught.
- After each half hour we put new fish equal to the amount caught in the previous half hour, by those who are allowed to fish in the next half hour.
- Part 2 takes maximally 2.5 hours.
- The length of time you are allowed to fish in part 2 depends on what happens in your group in part 1.

Part 1 of the study

- In the first period, we put $(16 \times 2) + 6 = 38$ rainbow trout into the pond.
- You can take home each fish you catch, you do not receive money for each fish caught.
- In each period of 30 minutes you are allowed to catch a maximum of 2 fish. If you catch salmon trout, this also counts as 1 fish.
- Whenever you have caught your second fish, you have to stop fishing; you are allowed to go on fishing when the next period starts.
- After each period, we put in a number of rainbow trout equal to the total catch in the previous period.
- Each fish you catch reduces the time with 10 minutes of the other members of your group in part 2 of the study.
- The previous rule holds for all participants. That means that for each fish another member of your group catches, the time you are allowed to fish in part 2 is reduced by 10 minutes.

In each period of part 1

- You are not allowed to communicate with other participants.
- You receive information about how much each of your group members have caught.
- This information is always presented in random order. You cannot link the catch of a group member in one period to this group member's catch in another period.

Other information

- The spot you fish at in part 1 is determined randomly. You draw a spot number from a bag.
- Your spot at part 2 of the experiment is determined randomly as well.
- When part 2 is finished for you, we pay your earnings and the study stops for you.

Test questions

We ask you to fill in the test questions. When each participant has correctly answered all questions, the study will begin.

1. With how many other participants are you in a group?
2. How much money do you receive for each fish caught in part 1?
   How much money do you receive for each fish caught in part 2?
3. Suppose that the following situation emerges at the end of part 1 of the study:
   You have caught 3 fish.
   The other three members of your group have each caught 2 fish.
   How long are you allowed to fish in part 2?
   Answer: 2.5 hours minus ____ minutes.
   How long are each of your group members allowed to fish in part 2?
   Answer: 2.5 hours minus ____ minutes.

7.A.4 Instructions for the Punishment treatment

Word of welcome

Welcome to this study by Tilburg University. Before we start, we want to point out two things. Firstly, this study is independent of the owners of the trout fishing facility. We are grateful that we are allowed to conduct this study here, but the owners have nothing to do with this project. All responsibility lies with Tilburg University. Secondly, we want to make clear that this study has nothing to do with animal welfare issues or the like. As researchers, we accept the rules and habits of recreational fishing as practiced at the trout fishing facility. We cannot tell you the exact aim of this study. We do want to stress that your privacy is guaranteed; none of the results we report can be traced back to individual participants.

As you know, you do not have to pay to take part in this study. The entrance fees are paid by Tilburg University. You are allowed to take home all fish you catch. In addition, you can earn money. We ask you to abide strictly by the rules which we impose.

The study

In the next four hours, we ask you to fish according to the following rules. First, all rules that normally apply at the trout fishing facility remain in place. This means that it is not permitted to throw fish you catch back into the pond, you are only allowed to fish with one rod, you are only allowed to use a scoop net to set fish ashore, you are only allowed to use the usual types of bait, etc.

Timing

- The study takes place over four hours, from around 8.00 a.m. until noon.
- In case we start a little later, we will end a little later.
- The morning is divided into two parts, part 1 and part 2.
- Part 1 takes 90 minutes and is divided into three periods of 30 minutes.
- Part 2 consists of 1 period in which you can fish freely.

Group formation

- You are placed in a group with 3 other participants.

- Your group consists of the same 4 participants in part 1 and part 2 of this study; you and the 3 other participants.
- We do not tell you in which group you are placed, and you are not allowed to communicate with other participants.

Part 2 of the study

- In part 2 you are allowed to catch as much fish as possible
- You can take home each fish you catch and you receive €2 for each fish caught.
- After each half hour we put new fish equal to the amount caught in the previous half hour, by those who are allowed to fish in the next half hour.
- Part 2 takes maximally 2.5 hours.
- The length of time you are allowed to fish in part 2 depends on what happens in your group in part 1.

Part 1 of the study

- In the first period, we put $(16 \times 2) + 6 = 38$ rainbow trout into the pond.
- You can take home each fish you catch, you do not receive money for each fish caught.
- In each period of 30 minutes you are allowed to catch a maximum of 2 fish. If you catch salmon trout, this also counts as 1 fish.
- Whenever you have caught your second fish, you have to stop fishing; you are allowed to go on fishing when the next period starts.
- After each period, we put in a number of rainbow trout equal to the total catch in the previous period.
- Each fish you catch reduces the time with 10 minutes of the other members of your group in part 2 of the study.
- The previous rule holds for all participants. That means that for each fish another member of your group catches, the time you are allowed to fish in part 2 is reduced by 10 minutes.

At the end of each period in part 1

- You can give us an order to reduce the fishing time of a group member in part 2 with 15 minutes, or more.
- If you give us such an order, this costs you 5 minutes of your own fishing time in part 2.
- In each period you can spend maximally 3 times 5 minutes of your own fishing time in part 2 to reduce the fishing time of a group member. If you spend 15 minutes of your own fishing time, then you can reduce the fishing time of one group member with 45 minutes, but you can also reduce the fishing time of each group member with 15 minutes.
- You can choose not to give us an order to reduce the fishing time of another group member / other group members.
- If you do not have time left in part 2 then you cannot give us an order to reduce fishing time of a group member.
- At the end of a period we inform you on the number of fish each of the three other group members have caught and how many minutes you have in part 2.
- Then we ask you to tell us if you would like to spend 0, 5, 10, or 15 minutes of your time in part 2 to reduce the time of one or more of your group members.
- At the beginning of the next period we inform you about all the choices of your group members, and we tell you the time you can fish in part 2.
- This information is presented in a random order, so you cannot link the catch or decision of a group member in one period to this group member's catch or decision in another period.
- It holds for each group member that the fishing time in part 2 can not be lower than 0 minutes or higher than 2,5 hour.

Example

- If you decide to reduce the fishing time in part 2 of a group member with 30 minutes, then this costs you 10 minutes of your own fishing time in part 2.

Other information

- The spot you fish at in part 1 is determined randomly. You draw a spot number from a bag.
- Your spot at part 2 of the experiment is determined randomly as well.
- When part 2 is finished for you, we pay your earnings and the study stops for you.

Test questions
We ask you to fill in the test questions. When each participant has correctly answered all questions, the study will begin.

1. With how many other participants are you in a group?
2. Suppose that the following situation emerges at the end of part 1 of the study:
   You have caught 4 fish.
   The other three members of your group have each caught 1 fish.
   How long are you allowed to fish in part 2?
   Answer: 2.5 hours minus ____ minutes.
   How long are each of your group members allowed to fish in part 2?
   Answer: 2.5 hours minus ____ minutes.
3. How many blocks of 5 minutes are you allowed to use to reduce the fishing time in part 2 of another group member?
4. If you spend a block of 5 minutes, with how many minutes do you reduce the fishing time in part 2 of another group member?

7.A.5 Instructions for the Reward treatment

Word of welcome

Welcome to this study by Tilburg University. Before we start, we want to point out two things. Firstly, this study is independent of the owners of the trout fishing facility. We are grateful that we are allowed to conduct this study here, but the owners have nothing to do with this project. All responsibility lies with Tilburg University. Secondly, we want to make clear that this study has nothing to do with animal welfare issues or the like. As researchers, we accept the rules and habits of recreational fishing as practiced at the trout fishing facility. We cannot tell you the exact aim of this study. We do want to stress that your privacy is guaranteed; none of the results we report can be traced back to individual participants.

As you know, you do not have to pay to take part in this study. The entrance fees are paid by Tilburg University. You are allowed to take home all fish you catch. In addition, you can earn money. We ask you to abide strictly by the rules which we impose.

The study

In the next four hours, we ask you to fish according to the following rules. First, all rules that normally apply at the trout fishing facility remain in place. This means that it is not permitted to throw fish you catch back into the pond, you are only allowed to fish with one rod, you are only allowed to use a scoop net to set fish ashore, you are only allowed to use the usual types of bait, etc.

Timing

- The study takes place over four hours, from around 8.00 a.m. until noon.
- In case we start a little later, we will end a little later.
- The morning is divided into two parts, part 1 and part 2.
- Part 1 takes 90 minutes and is divided into three periods of 30 minutes.
- Part 2 consists of 1 period in which you can fish freely.

Group formation

- You are placed in a group with 3 other participants.

- Your group consists of the same 4 participants in part 1 and part 2 of this study; you and the 3 other participants.
- We do not tell you in which group you are placed, and you are not allowed to communicate with other participants.

Part 2 of the study

- In part 2 you are allowed to catch as much fish as possible
- You can take home each fish you catch and you receive €2 for each fish caught.
- After each half hour we put new fish equal to the amount caught in the previous half hour, by those who are allowed to fish in the next half hour.
- Part 2 takes maximally 2.5 hours.
- The length of time you are allowed to fish in part 2 depends on what happens in your group in part 1.

Part 1 of the study

- In the first period, we put $(16 \times 2) + 6 = 38$ rainbow trout into the pond.
- You can take home each fish you catch, you do not receive money for each fish caught.
- In each period of 30 minutes you are allowed to catch a maximum of 2 fish. If you catch salmon trout, this also counts as 1 fish.
- Whenever you have caught your second fish, you have to stop fishing; you are allowed to go on fishing when the next period starts.
- After each period, we put in a number of rainbow trout equal to the total catch in the previous period.
- Each fish you catch reduces the time with 10 minutes of the other members of your group in part 2 of the study.
- The previous rule holds for all participants. That means that for each fish another member of your group catches, the time you are allowed to fish in part 2 is reduced by 10 minutes.

At the end of each period in part 1

- You can give us an order to increase the fishing time of a group member in part 2 with 15 minutes, or more.
- If you give us such an order, this costs you 5 minutes of your own fishing time in part 2.
- In each period you can spend maximally 3 times 5 minutes of your own fishing time in part 2 to increase the fishing time of a group member. If you spend 15 minutes of your own fishing time, then you can increase the fishing time of one group member with 45 minutes, but you can also increase the fishing time of each group member with 15 minutes.
- You can choose not to give us an order to increase the fishing time of another group member / other group members.
- If you do not have time left in part 2 then you cannot give us an order to increase fishing time of a group member.
- At the end of a period we inform you on the number of fish each of the three other group members have caught and how many minutes you have in part 2.
- Then we ask you to tell us if you would like to spend 0, 5, 10, or 15 minutes of your time in part 2 to increase the time of one or more of your group members.
- At the beginning of the next period we inform you about all the choices of your group members, and we tell you the time you can fish in part 2.
- This information is presented in a random order, so you cannot link the catch or decision of a group member in one period to this group member's catch or decision in another period.
- It holds for each group member that the fishing time in part 2 can not be lower than 0 minutes or higher than 2,5 hour.
- In case your fishing time exceeds 2,5 hours (due to the decisions of your group members), you receive a coupon which allows you to make up the fishing time at another date.
- For each 30 minutes you are allowed to fish longer than 2,5 hours, you receive a coupon of €3 (so, for each minute you are allowed to fish longer, you receive a coupon of €0.10).

Example

- If you decide to increase the fishing time in part 2 of a group member with 30 minutes, then this costs you 10 minutes of your own fishing time in part 2.

Other information

- The spot you fish at in part 1 is determined randomly. You draw a spot number from a bag.
- Your spot at part 2 of the experiment is determined randomly as well.
- When part 2 is finished for you, we pay your earnings and the study stops for you.

Test questions We ask you to fill in the test questions. When each participant has correctly answered all questions, the study will begin.

1. With how many other participants are you in a group?
2. Suppose that the following situation emerges at the end of part 1 of the study:
   You have caught 4 fish.
   The other three members of your group have each caught 1 fish.
   How long are you allowed to fish in part 2?
   Answer: 2.5 hours minus ____ minutes.
   How long are each of your group members allowed to fish in part 2?
   Answer: 2.5 hours minus ____ minutes.
3. How many blocks of 5 minutes are you allowed to use to increase the fishing time in part 2 of another group member?
4. If you spend a block of 5 minutes, with how many minutes do you increase the fishing time in part 2 of another group member?

Chapter 8

Conclusion

In this thesis, the effects of informal institutions on behavior in social dilemmas are studied. Chapter 2 shows a review of some of the most influential studies that illustrate how cooperation can be established in a social dilemma, without government intervention. In Chapters 3 and 4, the effects of rewards and counter-rewards on cooperation in a renewable natural resource are examined by means of a laboratory experiment. Chapter 5 addresses a field experiment of the Public Goods game, meant to study the external validity of this influential laboratory experimental game. Using a similar setup, the effects of monetary punishment on cooperation in a field experiment are studied in Chapter 6. Finally, Chapter 7 studies the effects of non-monetary punishment and reward on cooperation in a different version of the Public Goods game as presented in Chapter 5 and 6. In this chapter, the main findings of the results of this thesis are briefly summarized. Furthermore, the results are related to policy recommendations, and some ideas for future research are presented.

In Chapter 3, the effects of rewards are studied in a laboratory Common Pool Resource experiment. Subjects are placed in groups of five, and each subject has to choose how much to extract from a common pool resource. Overharvesting by one leads to smaller payoffs to others in a non-linear way. After the extraction stage, subjects receive feedback on the extraction decisions of others. Then, subjects receive an endowment of money which can be spent to reward other group members. Feedback is provided about the number of rewards that are received by other group members, although no information is available on how fellow group members have rewarded others. After this first reward stage, a second opportunity to reward is provided which has the same structure as the first stage. Chapter 3 adds to the existing literature in a number of ways. Because subject identities remain fixed throughout the periods, subjects are given the ability to build a reputation. This is a feature that is not implemented often in laboratory experiments, but it is likely to be a key feature in field settings. In addition, the possibility to counter-reward is provided. No other study on rewards has this feature, usually the number of reward stages is artificially reduced to one (see for example Ostrom et al. (1992), Sefton et al. (2007), Vyrastekova and van Soest (2008), and Rand et al. (2009)). Using rewards in field settings might be done in order to receive the favor of rewards in the future. Therefore, having two stages of rewards, rather than one, adds to the realism of the reward instrument.

The results show that although no cooperation is observed in the social dilemma game, almost full cooperation occurs in both reward stages when subject identity labels are kept constant over the periods. Subjects show a willingness to cooperate in the 'bilateral' cooperation game, while they massively free-ride in the 'group' cooperation game. In an additional treatment, subjects are made more anonymous by shuffling their identity labels between periods. Behavior in this treatment is qualitatively similar to the one where identity labels are fixed. No cooperation is observed in the common pool resource, but a considerable degree of cooperation is found in the reward stages. By consistently having the same extraction levels over the periods, many subjects overcome the barrier of anonymity and engage in a bilateral exchange of reward tokens. A final treatment considers the effects of counterrewards in a stranger treatment where new groups are formed between each period. No cooperation is observed at all in this treatment, neither in the social dilemma stage, nor in the reward stages. In early periods of the game, many subject choose to divide their reward tokens evenly among their group members. This strategy seems to be used in order to attract reward tokens in the second reward stage. However, because of the high levels of defection in that stage, the use of reward tokens in both stages approaches zero as the end of the experiment comes near.

Based on the results, the policy implications that follow from Chapter 3 are twofold. Firstly, the results suggest that depending solely on a system of rewards and counter-rewards to overcome free-riding in a social dilemma might be hazardous. Such a conclusion might seem odd, given that it is hard for a government to implement a system of rewards and counter-rewards. However, a different way to look at the experiment, is that the two reward stages represent an outside economic activity. When resource users have a way to earn money besides a common pool resource, then the results suggest that the individuals lean heavily on the outside option, neglecting the long run consequences of the natural resource. However, because the agents neglected cooperation in absence of such an economic activity anyway, a government cannot make matters worse when promoting the use of outside options. Secondly, and put in a different perspective, the behavior observed Chapter 3 has some positive insights as well. In the treatments where the same resource users interact with each other, be it with known identities or changing identities, a lot of cooperation is occurring when it comes to the use of rewards. The data show that group social dilemmas cannot be solved, but bilateral social dilemmas can. A government could try to exploit this feature in a small scale society where resource users frequently interact on a bilateral basis. For example, a government could try to convince resource users that free-riding in a natural renewable resource mostly hurts a neighbor. Shifting the way resource users think about the consequences of their actions might cause them to be more cooperative. In a situation where new and unfamiliar resource users frequently meet, such an approach seems fruitless, given the results of the stranger design. Alternatively, the resource users have the option to choose their partners in the bilateral social dilemma, whereas they might have no choice in a group social dilemma. Perhaps if the government provides opportunities to resource users for them to choose with whom they interact, this might increase cooperation in the large scale social dilemma (see for example Gürerk et al. (2004) and Sutter et al. (2010)).

One important direction in which future research can shed more light on the effects of counter-rewards is to consider the effects of information. In the experiment conducted in Chapter 3, subjects are only told whom they have received rewards from. No information has been given on how others reward others. Therefore, subjects might miss out on some crucial information regarding the mechanism of reward in a group. Knowing that cooperative group members have attracted a lot of rewards might make others become more cooperative as well. The effects of counter-rewards might be reversed when subjects receive more information.

Chapter 4 is an extension of Chapter 3. The partner matching design where identity labels are shuffled between periods is considered in more detail. Those results are compared to an identical treatment with only one stage of reward, taken from Vyrastekova and van Soest (2008). Interestingly, although less rewards are used in the treatment with only one stage of reward, more cooperation is found in that treatment. A large part of the results section of the treatment with two stages of reward shows how subjects behave in more detail. It turns out that the way in which rewards are used, provides insights in the social orientation of the subjects. About one-sixth of the subjects behaves as rational economic man would behave; they do not cooperate in the common pool resource and do not make use of the reward instrument. Half of the subjects behave as strategic money maximizers. They free-ride in the common pool resource, but they make considerable use of the rewards in the first stage. These subjects defect from sending rewards back in the second reward stage. One-third of the subjects behave cooperatively in both the common pool resource, and both reward stages.

Future research could extend this work in at least two ways. The first way is by extending the research on the social orientation of subjects. In some studies, researchers have tried to measure the social orientation of subjects directly (see for example Fischbacher et al. (2001)). It turns out that the counter-reward setup used in Chapter 4 also provides clear insights into the social orientation. Therefore, it might be interesting to repeat the analysis of Chapter 4 while subjects undergo the direct tests of social orientation proposed in the literature. It is interesting to observe which measures correlate with the social orientation blue print provided by the results of the counter-reward experiment. A second way in which future research can be extended, is by making use of the different social orientation methods. If the measures correlate, this might be interesting for a government. The direct measures on social orientation are likely to be gathered at relatively low costs. A survey conducted among villages where reward and counter-reward is the norm (for example, in the form of helping each other with outside economic alternatives) could tell a government what proportion of the subjects are making an unsustainable use of a natural renewable resource. Government policy can be improved by having it targeted directly at those resource users. However, before a government can rely on laboratory methods, the external validity of laboratory experiments has to be understood in greater detail. In chapters 5 through 7, I show that there are field settings in which the results from the laboratory do not carry over.

A direct test of the degree in which laboratory results carry over to a field setting is provided in Chapter 5. In this chapter, the external validity of the Public Goods game is studied. A vast body of literature on the Public Goods game shows two robust patterns; cooperation is positive at the outset of the game, but declines as more and more periods are played (Ledyard (1995)). To test whether such a pattern also exists in a non-laboratory setting, a related field experiment is conducted at a privately owned recreational fishing facility. Fishermen are placed in groups of four and play a game consisting of six periods of forty minutes each. In each period, a fisherman can catch up to two fish. Each fish caught is for the fisherman to take home. A social dilemma is created by giving incentives to the fishermen not to catch fish; each fish not caught yields a monetary payoff of €2 for each of the other three group members. Therefore, if all fishermen cooperate by not catching fish, each goes home with €72. The results of this treatment are compared to a private incentive treatment, a treatment with no incentives to reduce the catch of fish. Each fish caught in the six periods is for the fishermen to take home, but no payoff is provided to other fishermen for fish not caught.

In contrast with the received results from previous research on the Public Goods game in the laboratory, there is no evidence of cooperation in any of the periods in the field experiment. Fishermen catch the same amount of fish in the social dilemma treatment as they do in the private incentive treatment. Besides the catch measure, an effort measure provides additional support for the conclusion. The fish in the pond are rainbow trout, a hunting fish. Therefore, the number of times a fisherman casts his rod is correlated with catch. The results show that fishermen cast their rod with an equal pace in both treatments. In order to isolate factors that might cause a difference between the classical laboratory results and the field experiment, a series of laboratory experiments are conducted. The social dilemma field experiment is translated to a traditional laboratory experiment, and conducted with students and a sample of the fishermen population. When both subject pools are brought into the laboratory, fishermen show to be more cooperative than students. Therefore, the subject pool does not explain the lack of cooperation observed in the field. Additionally, the translated laboratory game is conducted while fishermen are fishing in their leisure time. Compared to the situation in which fishermen play the game in the laboratory, more cooperation is found when the game is played at the pond. The physical environment is therefore ruled out as an explanation for the total absence of cooperation in the field. Two explanations remain: the difference of the activity (filling in a number versus catching real fish), or the difference of the medium of reward (money versus money in the laboratory, but money versus fish in the field) might cause the absence of cooperation in the field. By conducting an additional treatment in the field, the difference in reward medium is ruled out as an explanation for the absence of cooperation. The treatment is a dynamic social dilemma game; catching more fish in one period results in a loss of fish in the next period. Also in this treatment, fishermen do not attempt to lower their effort levels and catch as much as they can.

The lack of cooperation in the field experiment, as opposed to positive but diminishing cooperation in the laboratory, has one important policy implication. Some scholars have suggested that sustainable use of a resource might be possible without government intervention. However, the results of Chapter 5 show that in scenarios where no communication is possible, or norms cannot be enforced otherwise, cooperation might not spontaneously emerge. Although the results do not provide insights which factors might improve cooperation, the result do suggest that a government should try to avoid that a natural renewable resource is confronted with an institutional setup like the one tested in Chapter 5. The results of Chapter 5 do not only relate to natural resources, an absence of cooperation might be found in all social dilemmas in field settings where communication is not possible, or where norms are absent.

Future research on social dilemmas in field settings is necessary to better understand the external validity of laboratory experiments. The setting used in Chapter 5 provides one environment in which external validity does not hold, but there might be other settings where the laboratory does have great predictive power of the field. Research from laboratory experiments has shown that, in contrast the theoretical predictions of zero cooperation, subjects become more cooperative in social dilemmas if the gains from doing so increase (see for example Marwell and Ames (1979), and Carpenter (2007)). These results might carry over to the field experiment of Chapter 5, fishermen might become more cooperative when the incentives to do so change. Other avenues of future research could lie in the use of formal or informal instruments that can enforce cooperation. There have been no previous attempts to study the effects of informal institutions on behavior in social dilemmas in a setting outside a laboratory. In chapters 6 and 7, the research on social dilemmas is extended by explicitly testing the effects of punishment and reward in a field setting. The bulk of the laboratory experiments on punishment and reward studies the effects of monetary punishment (see, for example, Fehr and Gächter (2000, 2002), Nikiforakis (2008), and Rand et al. (2009)). Subjects are endowed with money, to reduce or increase the monetary earnings of other group members. In chapter 6, the robustness of monetary punishment is tested by adding a punishment stage to the setup described in Chapter 5. Then, in chapter 7, a different social dilemma game is conducted where subjects have a non-monetary way to punish or reward other group members. The subjects in the experiment of Chapter 7 are allowed to reduce or increase the fishing time available to others. Let me address the two chapters in more detail.

Chapter 6 studies the effects of monetary punishment on cooperation in the static and dynamic field experiments conducted in Chapter 5. After each period, subjects receive information on the catch and individual earnings of each group member. Subjects then receive an endowment of €3 which they can keep, or spend to reduce the earnings of group members. Each euro spent, reduces the earnings of a group member with €3.

The results show that, unlike many laboratory results, punishment does not promote cooperation. Subjects fish with the same intensity, and catch similar amounts of fish, in a treatment with punishment as they do in a treatment without punishment. Monetary punishment is used rarely by the subjects, which suggests an aversion against the use of it. It seems as if the subjects have no problems to reduce the potential earnings of group members, because no cooperation is observed in the social dilemma phase. However, when it comes to reducing actual earnings, the subjects don't do so. An alternative explanation for the lack of punishment might be due to respect that fishermen have for skillful group members. Perhaps the participants believe that skillful fishermen deserve to catch a lot of fish, and that punishing them is unfair. Such effects are found to play a role in the Ultimatum Game (see Hoffman et al. (1994)).

The results of Chapter 6 give rise to some policy implications, as well as scope for future research. In contrast to conventional results from the laboratory, a government should be warned that relying on monetary punishment alone might not yield the desired effects. If the conjecture is true that subjects are averse to use monetary punishment, and if it extends to different environments with different subject pools as well, then a breakdown of cooperation in a social dilemma situation can be expected in some situations. Anticipating that no use of monetary punishment will be made, the users of a resource have all the incentives to harvest at unsustainable levels. In real life however, an interplay of different informal institutions can be expected. For example, in a situation where resource users can punish each other, it is expected that they can also communicate. From past laboratory research, it is known that the combination of punishment and communication can be extremely effective in promoting cooperation (see Ostrom et al. (1992)). Perhaps this result carries over to the field experiment of Chapter 6 as well. Past laboratory research also shows that subjects are sensitive to the cost-benefit ratio of the punishment instrument (see for example Carpenter (2007), and Nikiforakis and Normann (2008)). It might be the case that when punishment has a bigger impact, more subjects fear that they will be the target. Although the use of the instrument can still be at relatively low levels, anticipating an extreme reduction of income might persuade resource users to act cooperatively. Alternatively, when subjects have the opportunity to reduce a group member's stock of fish, rather than money earned, could have the desired effect on cooperation. Finally, it is possible that the rare use of punishment is due to characteristics of the fishermen. Laboratory experiments that study monetary punishment in a social dilemma could test the degree in which this influences the results of Chapter 6. Those results can be directly compared to a similar experiment where students form the subject pool.

Chapter 7 extends the literature by considering an alternative punishment or reward instrument: Time rather than money is the medium of reward. In almost all laboratory experiments, money is the medium of reward. However, in real life, time is a scarce resource that is of great value as well. Punishment or rewards are as easily provided in terms of time, as they are provided in terms of money. The social dilemma game studied in Chapter 7 resembles the Public Goods game, but it is different than the setup studied in Chapters 5 and 6. Fishermen are placed in groups of four, and play a game that consists of two parts. In part 2 of the game, each fisherman is allowed to catch as many fish as possible, and each fish yields an additional bonus of €2. After each half hour, the stock of fish is replenished to give the fishermen all the opportunities to catch as much as they can. Part 2 takes up to 150 minutes, but the time which a fisherman is allowed to fish, depends on the behavior of the other group members in part 1. The social dilemma aspect of the experiment is introduced in the three thirty minute periods that make up part 1. In each period, each fisherman is allowed to catch up to two fish; each fish caught must be taken away from the pond. Catching fish has a negative externality, because each catch results in a ten minute reduction of fishing time in part 2 for each of the other three group members.

Like the results reported in Chapter 5 and 6, there is no evidence of (partial) cooperation in this baseline setup. Fishermen always try to catch the first fish, and conditional on catching one fish, fishermen always try to catch their second fish. Moreover, effort levels in part 1 of the experiment are greater than those observed in part 2. The fact that fishermen do not exert more effort in part 2 provides extra support for the lack of cooperation found in part 1. In two additional treatments, the effects of either punishment, or rewards are examined. At the end of each period in part 1, subjects receive feedback on the individual catch of each group member. Then, fishermen can decide to spend up to three five-minute intervals of part 2 fishing time to reciprocate fellow group members. In the punishment treatment, each five-minute interval spent reduces the part 2 fishing time of a targeted group member with fifteen minutes. A five-minute interval spent in the reward treatment results in an increase of fifteen minutes. Again, like the results reported in Chapter 6, also non-monetary punishment has no influence on cooperation. Fishermen fish with the same intensity and catch similar amounts of fish in the punishment treatment as they do in the baseline treatment. Similar results hold for the reward treatment; no increase in cooperation is found, neither in terms of catch nor in terms of effort. Interestingly, rewards are used more often than punishment. However, a detailed analysis on the mechanism behind both instruments shows that punishment is directed towards free-riders. No correlation between free-riding behavior and rewards is found. Some studies in the area of neuroeconomics show that subjects derive joy out of the act of punishment (see for example de Quervain et al. (2004), and Fehr and Camerer (2007)). The results reported in Chapter 7 are consistent with this view; subjects observe that they cannot enforce cooperation, yet they do punish group members who catch many fish.

The results from Chapter 7 provide a robustness check on the results of Chapter 5 and 6; when fishing time is the medium of reward, rather than money, no cooperation is observed. Additionally, also non-monetary punishment or reward does not promote cooperation. Hence, there are field settings in which the results from the laboratory do not hold. Policy implications derived from this chapter are therefore much in line with those of Chapter 6. Relying on a mechanism of punishment or reward might prove ineffective. In scenarios where resource users are anonymous and cannot interact with each other, perhaps formal institutions are the only way in which sustainable use of a resource can be established.

Future research should provide more insights in the latter claim. An interplay of different informal institutions can shift behavior to the social optimum. Communication has proved to be very successful in the laboratory, and this might well be the case in the field setting of Chapter 7. Alternatively, giving resource users the option to vote to exile free-riding group members could provide the necessary incentives to act in line with the social optimum (see for example Cinyabuguma et al. (2005)). A mix of all those informal institutions is likely to be found in field settings, rather than just one institution in isolation. The field setting of Chapter 7 provides ample opportunities to dig deeper into the question how cooperation can be established in a controlled field setting. Finally, since the effects of time as the medium of reward are not well understood in laboratory experiments, future research should address this issue as well. Perhaps the results found in Chapter 7 can be replicated in the laboratory when somehow choices in a social dilemma game effect time use, rather than monetary earnings.

The debate on whether cooperation can be found in social dilemmas in the absence of government intervention is still ongoing. Although many scholars take the view that cooperation is possible in such cases, this thesis challenges this view by providing some examples in which cooperation breaks down. Conventional results from laboratory experiments, which show that peer-to-peer regulation can enforce cooperation, are not robust to just any change. This point has been made before in the case of punishment; allowing for the possibility to take revenge causes a breakdown of cooperation (see Denant-Boemont et al. (2007)), and Nikiforakis (2008)). In the case of rewards the message is similar, as is shown in Chapters 3 and 4. Counter-rewards do not lead to an improvement in cooperation, because rewards shift the focus away from the social dilemma towards a bilateral exchange of rewards with other group members. This finding is robust to several design changes in which the anonymity of subjects is varied. Additional support for the view that cooperation does not arise spontaneously in field settings is provided by Chapters 5 through 7. Using a non-laboratory field setting, a recreational fishing pond, the results of Chapter 5 show no evidence at all of cooperation in a voluntary contribution game. The reason for its absence is the decision variable; whereas subjects do display cooperative behavior when doing so requires the sacrifice of a monetary payoff only, cooperation disappears when a reduction in fishing effort is needed. In the final two chapters of this thesis, the issue is raised how the effects of informal institutions, which have proven to work well in laboratory experiments, carry over to a non-laboratory setting. The two instruments that are considered are punishment and reward. When taken to the field setting, the effectiveness of both instruments vanishes. The results show that subjects rarely use punishment or rewards, and that any punishment or rewards received does not change their behavior in the social dilemma. Proponents of peer-to-peer policy design should keep in mind that the external validity of laboratory experiments is poor in settings where the decision to cooperate or not involves a laborious activity. The instances of such social dilemma situations, such as many environmental problems, are widespread. Therefore, relying on peer-to-peer policy design could prove to be dangerous in some situations.

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Laboratorium- en veldexperimenten over sociale dilemma's

Samenvatting

In dit proefschrift wordt een antwoord gezocht op de volgende vraag: „Hoe hangt gedrag in sociale dilemma's, zoals de conservatie van vernieuwbare natuurlijke hulpbronnen, af van de geldende informele instituties en wat zijn de implicaties voor overheidsbeleid?" Meer algemeen wordt er een antwoord gezocht op de vraag hoe het gedrag in sociale dilemma situaties afhangt van informele instituties. Een sociaal dilemma doet zich voor wanneer het belang van een individu niet overeenkomt met de belangen van een groep. De problematiek die zich voordoet bij veel situaties aangaande het milieu is een voorbeeld van een sociaal dilemma. Wanneer een individu een vis vangt, dan zullen delen van de kosten afgewenteld worden op de maatschappij. Het wordt voor anderen moeilijker om een vis te vangen, omdat er minder vissen voorradig zijn. Daarnaast zal de visstand minder snel groter worden naarmate er minder vissen aanwezig zijn. Omdat een deel van de kosten voor het vangen van een vis niet voor rekening zijn van degene die de vis vangt, is het in het belang van de visser om meer vissen te vangen dan wat vanuit sociaal oogpunt optimaal is. In economisch jargon wordt gesteld dat een individu prikkels heeft om te 'free-riden' in plaats van om samen te werken. De overheid is een voorname kandidaat om er voor te zorgen dat individuen niet te zelfzuchtig handelen. Door middel van bijvoorbeeld belastingen of boetes kan er voor gezorgd worden dat de belangen van

een individu overeenkomen met de belangen van een groep (bijvoorbeeld omwonenden rondom een meer met vissen). Echter, inzichten uit wetenschappelijke studies en voorbeelden uit de praktijk laten zien dat individuen onderling, in afwezigheid van een overheid, tot een oplossing van een sociaal dilemma kunnen komen. Een belangrijke schakel die zorgt voor een uitkomst zijn zogenaamde informele instituties: de set van zelf-opgelegde locale regels die het gedrag van gebruikers van een hulpbron stuurt. In dit proefschrift wordt een antwoord gezocht op de hoofdvraag door middel van het uitvoeren van economische experimenten. Hoofdstukken 3 en 4 zullen economische experimenten beschrijven die uitgevoerd zijn in het traditionele laboratorium. Vanaf Hoofdstuk 5 zullen de conventionele methodes van laboratorium experimenten getest worden in een veldsituatie, een recreatieve visvijver.

In Hoofdstuk 2 van dit proefschrift wordt de literatuur samengevat die ingaat op de vraag hoe individuen samen kunnen werken in een sociaal dilemma in afwezigheid van een overheid of informele instituties. In een sociaal dilemma heeft een individu altijd een prikkel om niet samen te werken. Daarom zullen sociale dilemma's niet gekenmerkt worden door altruïstisch gedrag: gedrag van een individu dat zorgt voor een toename in fitness voor een ander individu, ten koste van een afname van de fitness van de weldoener. Sterker nog, klassieke economische theorie voorspelt dat altruïsten een lagere kans hebben om te overleven, omdat zij kosten maken die 'free-riders' niet maken. In de literatuur worden drie argumenten gegeven waarom het op de lange termijn toch loont om samen te werken. 'Kin-selection' is de eerste verklaring: wanneer een familielid voor voldoende nakomelingen zorgt, dan kan altruïsme in iemands belang zijn. Op indirecte manier worden dan de genen van een altruïst doorgegeven naar de volgende generatie. De tweede verklaring wordt 'directe reciprociteit' genoemd. Dit is gebouwd op het principe dat wanneer ik iemand nu help, deze persoon mij in de toekomst helpt. Zodoende kunnen we allebei beter af zijn op de lange termijn. De derde verklaring is 'indirecte reciprociteit'. Het geldende principe in deze stroom in de literatuur is dat wanneer ik iemand help, iemand anders mij in de toekomst helpt. Reputatie is het bindende element, alleen degenen die een goede reputatie hebben verworven zullen hulp ontvangen van anderen. Tot slot wordt in Hoofdstuk 2 ingegaan op de informele institutie straffen. Wanneer individuen de mogelijkheid hebben om asociaal gedrag van anderen te straffen, dan kan altruïstisch gedrag overleven. De reden is dat 'free-riders' een lagere fitness hebben, omdat ze gestraft worden door altruïsten.

In Hoofdstuk 3 wordt ingegaan op de effecten van belonen en wederbelonen op samenwerking in een sociaal dilemma, een common pool resource. In een laboratorium worden studenten geplaatst in groepjes van vijf. Elke deelnemer heeft de mogelijkheid om tot 13 eenheden uit een natuurlijke hulpbron te putten. De winsten die behaald kunnen worden uit de hulpbron hangen af van de mate van extractie van anderen; hoe minder de extractie van anderen, des te hoger de opbrengsten voor het individu dat een hoge extractie heeft. Echter, als alle deelnemers een hoge mate van extractie hebben, dan zijn de totale winsten lager dan wanneer iedereen een matige extractie heeft. Het experiment bestaat uit twee delen. In het eerste deel worden vijftien periodes van het bovengenoemde spel gespeeld. Het tweede deel is een herhaling van het eerste, met de uitzondering dat aan het einde van elke periode de mogelijkheid tot belonen en wederbelonen wordt gegeven. Elke deelnemer krijgt informatie over de extractiekeuzes van alle groepsgenoten. Daarnaast ontvangt ieder twaalf tokens die ingezet kunnen worden om groepsgenoten te belonen. Ieder token dat aan een groepsgenoot wordt gegeven, levert drie tokens op voor de ontvanger. Na de eerste mogelijkheid tot belonen, ontvangt elke deelnemer informatie over hoeveel beloningen ontvangen zijn door anderen. Dan volgt een tweede fase waarin iedereen opnieuw twaalf tokens ontvangt die gebruikt kunnen worden om groepsgenoten te belonen. De invloed van de mate van anonimiteit op het beloningsgedrag vormt de basis van Hoofdstuk 3. In een treatment blijven dezelfde deelnemers in dezelfde groepjes van vijf. De identiteit van iedere deelnemer blijft onveranderd gedurende alle periodes. In een tweede treatment blijven de deelnemers gepaard met dezelfde groepsgenoten, maar de identiteit van iedere deelnemer wordt veranderd na afloop van elke periode. In een laatste treatment wordt de samenstelling van de groepen veranderd na afloop van elk van de vijftien periodes. De resultaten van het experiment wijzen uit dat belonen en wederbelonen niet voor samenwerking in het sociale dilemma zorgen. In de eerste twee treatments proberen de deelnemers met elkaar uitwisselingsverbanden van beloningen aan te gaan. Dit is opmerkelijk in de treatment waar de identiteit van de deelnemers wordt veranderd tussen de periodes. De subjecten lossen dit probleem op door in de common pool resource altijd dezelfde mate van extractie te hebben. Daardoor kunnen ze kenbaar maken wie ze zijn en kan op veilige wijze een uitruil van beloningen aangegaan worden. Het gevolg is dat de mate waarin beloningen gebruikt worden erg hoog is en dat daardoor de deelnemers niet meer afhankelijk zijn van winsten uit de common pool resource. In de treatment waarin deelnemers steeds in nieuwe groepen worden geplaatst, is deze strategie niet mogelijk. Dientengevolge worden beloningen nauwelijks gebruikt en is er geen samenwerking zichtbaar in de common pool resource. Een groot deel van de subjecten die wel beloningen gebruiken, verdeelt ze gelijkmatig over alle groepsgenoten in de eerste beloningsfase. Op deze manier hopen deze subjecten dat hun investeringen worden terugverdiend in de tweede beloningsfase.

Hoofdstuk 4 is een uitbreiding van Hoofdstuk 3. Er wordt dieper ingegaan op de treatment waarin deelnemers in ongewijzigde groepen blijven, maar waarin de identiteit van elke deelnemer wordt veranderd na afloop van alle periodes. De resultaten van deze treatment worden vergeleken met een treatment met slechts een fase van belonen (die resultaten zijn genomen uit een eerdere studie). Twee bevindingen springen uit het oog. Ten eerste, hoewel meer beloningen gebruikt worden in de treatment met twee fases van beloningen, is de samenwerking tussen groepsgenoten hoger in de treatment met slechts een fase van belonen. Ten tweede, het blijkt dat het beloningsgedrag inzicht geeft in de sociale oriëntatie van de deelnemers. Een zesde van de deelnemers gedraagt zich als homo economicus, zij werken nooit samen in de common pool resource en ze maken nooit gebruik van het beloningsinstrument. De helft van de deelnemers gedraagt zich als strategische winstmaximaliseerders, zij werken niet samen in de common pool resource, maar ze belonen wel in de eerste fase van belonen. Vervolgens maken ze geen gebruik van de tweede fase van belonen, om zodoende te profiteren van diegenen die hen wel belonen in die tweede fase. Een derde van de deelnemers is pro-sociaal, zij zijn sociaal in de common pool resource en ze maken gebruik van beide mogelijkheden om te belonen.

In Hoofdstuk 5 wordt bestudeerd in welke mate het gedrag in sociale dilemma's van studenten in het laboratorium voorspellend is voor gedrag in niet-laboratorium omgevingen. De Public Goods game, een populair sociaal dilemma experiment uit het laboratorium, wordt uitgevoerd aan een privaat beheerde recreative visvijver om een antwoord te krijgen op deze vraag. Uit de vele voorgaande laboratorium onderzoeken van de Public Goods game blijken twee robuuste patronen. Deelnemers beginnen met gematigd sociaal gedrag, maar naarmate de periodes vorderen, handelt men steeds meer naar het eigenbelang. Om te onderzoeken of dergelijk gedrag zich ook voordoet in een niet-laboratorium omgeving, worden vissers in groepen van vier geplaatst. Er wordt een spel gespeeld dat bestaat uit zes rondes van elk veertig minuten, waarin de deelnemers niet mogen communiceren. In elke ronde is iedereen toegestaan om tot twee regenboogforellen te vangen. Elke vis die gevangen wordt, moet door de visser mee naar huis worden genomen. Echter, elke vis die een visser laat zwemmen, levert €2 op voor elk van de andere groepsgenoten. Een visser wordt daarom gedwongen om een afruil te maken: of een visser vangt een vis voor eigen plezier, of de visser werkt samen en ziet af van het eigen plezier om de winsten van anderen te vergroten. Aan de vijver vissen de vissers op regenboogforel, een vis die actief op aas jaagt. Dit kenmerk geeft de mogelijkheid om moeite van vissers te meten. Hoe vaker een visser zijn aas uitgooit, des te meer vissen er worden gevangen. Om samenwerking te meten, worden de resultaten van deze treatment vergeleken met een treatment waarin geen prikkels worden gegeven om de visvangst te verminderen. Elke vis die gevangen wordt, moet mee naar huis genomen worden, maar er wordt geen geldbedrag gegeven aan groepsgenoten als een vis niet wordt gevangen.

In tegenstelling tot wat meestal wordt gerapporteerd uit laboratorium onderzoeken, is er geen enkele vorm van samenwerking te vinden in het veldexperiment. Vissers vissen met dezelfde intensiteit en vangen dezelfde hoeveelheid vis in de sociale dilemma treatment als in de treatment zonder dilemma. Mogelijke verklaringen voor het verschil in gedrag tussen het laboratorium en aan de vijver zijn de aard van de subjecten (studenten versus vissers) en de fysieke omgeving waarin het onderzoek wordt gehouden (laboratorium versus vijver). In drie extra treatments worden de invloeden van elk van die factoren onderzocht. Het spel in het veldexperiment wordt vertaald naar een 'pen en papier' laboratorium experiment. Dit experiment wordt vervolgens uitgevoerd onder studenten in een laboratorium, vissers in een laboratorium en vissers aan de vijver terwijl ze aan het vissen zijn. Uit de resultaten van deze laboratorium experimenten blijkt dat alle deelnemers zich gedragen zoals meestal wordt geobserveerd: men werkt samen in het begin, maar naarmate de periodes vorderen, handelt men meer en meer naar het eigenbelang. Twee laatste verschillen tussen het laboratorium- en veldexperiment blijven bestaan. In het laboratorium is de afruil die gemaakt wordt altijd in termen van geld, of men samenwerkt of niet. Echter, in het veldexperiment is deze afruil verschillend, handelen in het eigenbelang levert visplezier op, terwijl samenwerken geld oplevert voor anderen. Een ander verschil is dat in het laboratorium geen activiteit ondergaan wordt, er hoeft slechts een cijfer omcirkeld te worden. In het veldexperiment moet moeite gedaan worden om een vis te vangen. Om te onderzoeken welk van deze twee factoren bepalend is voor het verschil in gedrag, wordt een laatste veldexperiment uitgevoerd. Dit experiment bestaat uit vier periodes van een uur en vissers worden geplaatst in groepen van vier. In het eerste uur mag een groep maximaal acht vissen vangen, hoe dat verdeeld wordt binnen de groep maakt niet uit, maar er mag niet worden gecommuniceerd. De hoeveelheid vis die gevangen wordt, bepaalt het aantal beschikbare vissen voor de volgende periode. Hoe meer er wordt gevangen, des te minder er beschikbaar is. Dit spel is een dynamische variant van de common pool resource game uit Hoofdstuk 3 en 4. In dit spel is de afruil die de deelnemers hebben van gelijke aard, in plaats van geld en visplezier is de afruil hier louter in termen van visplezier. De resultaten van dit experiment geven eenzelfde beeld van samenwerking als die in de Public Goods game. Vissers doen maximaal hun best om zo veel mogelijk vis te vangen, ongeacht de hoeveelheid die nog beschikbaar is. De conclusie van deze treatment is dat het verschil in gedrag tussen het laboratorium en het veld moet zitten in de activiteit. Zodra samenwerken gepaard gaat met (de afwezigheid van) een activiteit kan het zijn dat samenwerking niet wordt bewerkstelligd. In tegenstelling tot de resultaten uit het laboratorium zijn er dus veldsituaties waarin samenwerking niet spontaan wordt bewerkstelligd.

In Hoofdstuk 6 wordt onderzocht of het informele instituut straffen voor samenwerking kan zorgen in de twee veldexperimenten uit Hoofdstuk 5. Aan het einde van iedere periode wordt aan de deelnemers verteld hoeveel elk van de groepsgenoten heeft gevangen en wat hun verdiensten zijn geweest. Vervolgens ontvangt elke deelnemer een bedrag van €3. Elke euro die ingezet wordt, verlaagt de verdiensten van een gekozen groepslid met €3. Niet elke euro hoeft te worden gebruikt, geld dat niet wordt gebruikt wordt toegebracht aan de totale verdiensten van een deelnemer. Het maakt niet uit hoe een deelnemer het budget verdeeld, zolang niet minder dan €0 en niet meer dan €3 wordt gespendeerd. Uit onderzoeken die gedaan zijn in het laboratorium, blijkt dat dit instrument erg succesvol is in het verhogen van samenwerking. Echter, het blijkt dat de vissers aan de vijver net zo veel moeite doen en net zo veel vissen vangen in de treatment met straffen als dat zij doen in treatments zonder straffen. Dit resultaat geldt voor zowel de Public Goods game, als het dynamische sociale dilemma spel.

Tot slot worden de invloeden van belonen en straffen in een variant van de Public Goods game uit Hoofdstuk 5 en 6 bestudeerd. Het instrument bestudeerd in Hoofdstuk 6 is er een van monetaire aard, terwijl een deel van de keuzes die de vissers maken een andere aard heeft: visplezier. In Hoofdstuk 7 wordt de invloed van straffen op gedrag in sociale dilemma's bestudeerd wanneer de aard van straffen die van visplezier is. Daarnaast worden de invloeden van belonen op sociaal gedrag gemeten.

Het experiment uit Hoofdstuk 7 bestaat uit twee delen. In het tweede deel mogen vissers onbeperkt vissen voor maximaal 150 minuten. Elke vis die wordt gevangen, moet mee naar huis genomen worden en levert tevens €2 op. De voorraad vis wordt om het half uur aangevuld om de vissers alle mogelijkheid te geven om zo veel te vangen als mogelijk is. Echter, de tijd die beschikbaar is voor de vissers in deel 2 hangt af van wat er gebeurt in deel 1. Deel 1 duurt drie periodes van dertig minuten en vissers worden geplaatst in groepen van vier. Communicatie is niet toegestaan. In elk van de drie periodes mag een visser maximaal twee vissen vangen. Elke vis die wordt gevangen, moet mee naar huis worden genomen, maar de tijd waarin elk van de drie groepsgenoten mogen wissen in deel 2 wordt met tien minuten verkort. Een visser heeft daarom de volgende afruil: of er wordt naar het eigenbelang gehandeld door te vissen, of er wordt niet gevist, zodat anderen langer kunnen vissen in deel 2. De resultaten van dit experiment komen overeen met de resultaten uit Hoofdstukken 5 en 6: er zijn geen sporen van samenwerking. Vissers proberen altijd de eerste vis te vangen en, conditioneel op het vangen van de eerste vis, proberen ze altijd de tweede vis te vangen.

In twee andere treatments worden de effecten van belonen en straffen gemeten. Aan het einde van elk van de periodes in deel 1, ontvangt iedere deelnemer informatie over hoeveel vis elk van de groepsgenoten heeft gevangen. Vervolgens wordt de mogelijkheid geboden om tot drie intervallen van vijf minuten uit deel 2 te spenderen. In de straftreatment verlaagt elk interval dat wordt gespendeerd de vistijd in deel 2 van een gekozen groepslid met vijftien minuten. In de beloningstreatment verhoogt elk interval de vistijd van een gekozen groepslid met vijftien minuten. Het maakt niet uit hoe een deelnemer de intervallen verdeeld, zolang niet minder dan nul intervallen en niet meer dan drie intervallen worden gebruikt in elke periode. De resultaten van deze niet-monetaire instrumenten zijn overeenkomstig met die uit Hoofdstuk 6. In de treatment waarin groepsgenoten elkaar kunnen straffen, wordt geen samenwerking gevonden. Vissers vissen met dezelfde moeite en vangen dezelfde hoeveelheid vis in de straftreatment als in de treatment zonder straffen. Dezelfde conclusie wordt getrokken voor de beloningstreatment.

Een gedetailleerde analyse van het gebruik van beloningen wijst uit dat er geen verband bestaat tussen het geven van beloningen en de vangst van vis. Een deel van de beloningen wordt gelijk verdeeld over de deelnemers, wat suggereert dat vissers anderen belonen in de hoop om zo beloningen terug te krijgen. Het gebruik van straffen komt overeen met eerdere bevindingen van laboratorium onderzoeken: degenen die de meeste vissen vangen ten opzichte van het groepsgemiddelde, ontvangen de meeste straffen. Het feit dat vissers het strafinstrument blijven gebruiken, terwijl ze ondervinden dat het geen effect heeft om samenwerking, is interessant. Dit suggereert dat vissers het strafinstrument gebruiken, omdat het goed voelt. Straffen geeft de deelnemers de mogelijkheid om wraak te nemen op groepsgenoten die niet zo sociaal zijn als anderen.

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