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Renegotiation Proofness and Climate Agreements: Some Experimental Evidence

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Abstract

The notion of renegotiation-proof equilibrium has become a cornerstone in non-cooperative models of international environmental agreements. Applying this solution concept to the infinitely repeated N-person Prisoners' Dilemma generates predictions that contradict intuition as well as conventional wisdom about public goods provision. This paper reports the results of an experiment designed to test two such predictions. The first is that the higher the cost of making a contribution, the more cooperation will materialize. The second is that the number of cooperators is independent of group size. Although the experiment was designed to replicate the assumptions of the model closely, our results lend very little support to the two predictions.

[1] We are indebted to an anonymous referee and to the participants at a CICERO seminar for helpful comments.

1. Introduction

The infinitely repeated Prisoners Dilemma (IRPD) is frequently used to study the conditions for provision of pure public goods, such as curbing global warming.[2] A number of applications of this model use the notion of renegotiation-proof equilibrium as solution concept.[3]

Loosely put, the idea behind renegotiation proofness is that a punishment that hurts all players will not be carried out. Unless an agreement is renegotiation-proof, a transgressor will be able to escape a threatened punishment by inviting the other players to renegotiate, rightfully pointing out that every player gains by skipping the punishment. Foreseeing this, a rational decision maker will not enter into an agreement that is susceptible to renegotiation.

Requiring renegotiation proofness in the N-player IRPD entails some rather pessimistic predictions about the prospects for effective cooperation. In several models, the number of per period contributions in a renegotiation-proof equilibrium is considerably smaller than what can be achieved with the weaker restriction of subgame perfection. A number of scholars have taken this as a basis for recommending measures to enhance cooperation, such as taxes, quotas, regional agreements, trade sanctions, technology standards, transfers and other issue linkages.[4]

A prime motivation for our paper follows directly from this literature. We want to test some implications of a model upon which strong policy recommendations are based (at least indirectly). Arguably, this is perhaps the most legitimate of reasons for conducting a policy experiment. We report the results of an experiment designed to examine two hypotheses that follow from the N-player IRPD game when weakly renegotiation-proof equilibrium is used as solution concept. The first hypothesis is that the higher the cost of making a contribution, the more cooperation will materialize. The second is that the number of cooperators is independent of group size.

Using experimental data to check the sensibility of equilibrium refinements is not a novel idea.[5] However, existing studies deal almost exclusively with one-shot or finitely

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[2]Greenhouse gases mix very quickly and almost perfectly in the atmosphere. As a result, constraining emissions of such gases benefits all countries in a non-excludable and non-rival way. This means that curbing global warming is a pure public good. Hence, every country faces a free-rider incentive, and all countries suffer if no measures are taken to curb climate change.

[3]The concept was introduced into the study of environmental politics by Barrett (1994, 1999a,b, 2001, 2002, 2003). Barrett's work has been followed up by a number of others (Asheim et al. 2006, Carraro 1999, Carraro and Siniscalco 1998, Ecchia and Mariotti 1998, Enders and Finus 2002, Finus 2001, Finus and Rundshagen 1998a,b, Wagner 2001).

[4]Asheim et al. 2006, Barrett 2001, 2003, Finus 2001, Finus and Rundshagen 1998a,b, Carraro 1999, Carraro and Siniscalco 1998, Wagner 2001.

[5]For instance, the literature includes a number of carefully controlled experiments aimed at evaluating various refinements in signalling games. See Banks et al. (1994), Brandts and Holt (1992, 1993, 1995) and

repeated games. Also, many studies only consider two-player games. In particular, the only previous study conducting an experimental test of renegotiation proofness in a PD-like game uses a two player, two-stage game (Davis and Holt 1999). In contrast, our study considers an N-player IRPD game. This is no doubt a more satisfactory approximation to most of the theorizing on global public goods provision.

2. The Model

The model tested in our experiment has N identical players. In every round of the game each player must choose to contribute (reduce emissions of greenhouse gases) or abstain (not reduce emissions). Let the periodic utility to player i be $D_i(m)=dm-c$ if he contributes and $B_i(m)=bm$ if he abstains,[6] where d and b are positive constants, c is the cost of making a contribution ($c>0$), and m ($m \leq N$) is the number of players that contribute. For simplicity, we assume $b=d$.[7]

For the stage game to be a PD, we must have that $D_i(m)<B_i(m) \forall m \in N$, and $D_i(N)>B_i(0)$. In our linear formulation this entails that $c>0$ and $c>d$.

Assume that all players pursue a strategy called "Penance" (PE).[8] PE instructs a participating player to contribute in a given round unless another participating player has been the sole deviator from PE in the previous round, in which case the player abstains. Non-participating players abstain after any history. The distinction between participating and non-participating players is explained below.

In order to form a weakly renegotiation-proof equilibrium (WRPE), a strategy vector must satisfy two requirements.[9] First, it must form a subgame perfect equilibrium. Second, this subgame perfect equilibrium must be renegotiation proof. In any repeated game with discounting a necessary and sufficient condition for a strategy vector to be

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Partow and Schotter (1996), Potters and van Winden (1996, 2000). Camerer (2003) provides a thorough review and discussion.

6 It has been shown that replacing linear utility functions with functions showing decreasing returns from cooperation does not change the main predictions of the model (Asheim et al. 2006: Appendix A).

7 It has been shown that assuming increasing returns of cooperation ($b<d$) may (but need not) produce more contributions in an WRPE (Barrett 1999).

8 See Asheim et al. 2006.

9 Several competing concepts of renegotiation-proof equilibrium exist in the literature (see Bergin and MacLeod 1993 for a discussion). When we use the concept in this article we always refer to the concept of weakly renegotiation-proof equilibrium, formalized for 2x2 games by Farrell and Maskin (1989:330-31), and extended to N-person games by Barrett (1994) and by Finus and Rundshagen (1998).

subgame perfect is that no player can gain by a one-period deviation after some history (Abreu 1988:390).

In our model there are two histories to consider. In the first there is no single deviation in the previous period. PE then instructs each player to play contribute. For PE to be a best reply given that all other players use PE, every player must (weakly) prefer to play contribute, rather than to make a one-period deviation from PE by abstaining. Formally, we must have that $dm-c+\delta(dm-c)\geq d(m-1)+\delta(d-c)$, where $0<\delta<1$ is the players' common discount factor.[10]

Solving for m gives

(1) $m\geq[(\delta-1)/\delta]+(c/\delta d)$

If condition (1) holds, the strategy vector in which all players play PE is a Nash equilibrium. For this equilibrium to be subgame perfect, two additional requirements have to be met. First, it must be (weakly) profitable for the punishing players to follow through with the punishment prescribed by PE. This requirement is fulfilled if $d\geq 2d-c$, i.e., if $c\geq d$, which is met by assumption in the PD game. Second, the punished player must (weakly) prefer to accept the punishment by paying penance (i.e., being the only player who contributes) in the following period, rather than to deviate in one more period by abstaining. The second requirement is fulfilled if $d-c+\delta(dm-c)\geq d(0)+\delta(d-c)$. Solving for m gives (1), which is the binding condition for subgame perfection.

For the strategy vector in which all players play PE to be a WRPE we must require that not all players strictly gain by restarting cooperation immediately after a violation, rather than implementing the punishment prescribed by PE. Formally this requires that $d\geq dm-c$. Solving for m gives

(2) $(d+c)/d\geq m$

Combining conditions (1) and (2) gives the following condition for the strategy vector in which all players play PE to form an WRPE:

(3) $(d+c)/d\geq m\geq[(\delta-1)/\delta]+c/\delta d$

By a participating player we understand a player $i\in m$ that contributes, while a non-participating player is a player $j\in(N-m)$ that abstains. Thus the distinction between participating and non-participating players is endogenous to the concept of a WRPE.

[10]Note that we need only compare payoffs in the periods where a deviation from PE occurs.

The focus on the PE strategy might be justified in at least two different ways. First, PE is arguably better able to sustain stable cooperation than other strategies which have received much attention in the repeated-game literature, such as Grim Trigger or Tit-for-tat. PE sustains cooperation as a weakly renegotiation-proof equilibrium. In contrast, Grim Trigger only sustains cooperation as a subgame perfect equilibrium (i.e., this equilibrium is not renegotiation-proof), and Tit-for-tat only sustains cooperation as a Nash equilibrium (i.e., this equilibrium is not even subgame perfect).

Second, the enforcement mechanism used by PE bears some resemblance to the enforcement mechanism used by the Kyoto Protocol. Both mechanisms require that a country that is in non-compliance in period t is required to bear a larger share of the abatement burden in period t+1. However, whereas Kyoto requires the non-compliant country to abate more in period t+1, Penance instructs compliant countries to abate less in period t+1. Note that the enforcement mechanisms used by Grim Trigger and Tit-for-tat do not even remotely resemble Kyoto's enforcement mechanism.

A number of implications follows from the model outlined above. Our ambition is to check whether two main implications of the model, evaluated as directional predictions, obtain empirical support:[11]

Implication 1: The higher the cost of making a contribution, the more players will contribute.

Whenever condition (3) can be satisfied (so that a WRPE with a positive number of contributions exists), it imposes a restriction on the number of participating players. This number is small if the gap between the payoff curves is narrow and larger if the gap is wide. In other words, the number of participating players in equilibrium increases if the cost of playing contribute increases. Most readers are likely to find implication 1 strongly counterintuitive. For example, economists have emphasized the efficiency of markets for tradable emission permits. A market for permits helps reduce abatement costs, which is presumed to enhance cooperation. Acting upon this advice, a number of countries and states are already operating in such markets. Implication 1 suggests that, if anything, such markets tend to reduce the number of countries that are willing to abate.

Implication 2: The number of contributors is independent of group size.

Within the literature on public goods provision, several authors have argued that cooperation is likely to be adversely related to group size (e.g., Olson 1965, Chamberlin 1982, Esteban and Ray 2001). In contrast, if (1) is satisfied, the number of participating players in the WRPE depends only on the slopes of the utility curves and the distance

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[11]For a discussion of the prominence of directional predictions (as opposed to point predictions) in the social sciences, see Hovi et al. (2003) and Fiorina (1996).

between them (the cost of making a contribution). In particular, the number of contributors does not depend on the overall number of players, meaning that group size should have no impact on cooperation.[12]

3. Previous Research

Theoretical Research

The idea of a WRPE for repeated games was formulated by Farrell (1983) and formalized by Farrell and Maskin (1989) for 2×2 games. In particular, Farrell and Maskin identify the restrictions on discounted payoffs imposed by the requirement that a strategy vector is a WRPE (1989:335ff.). Van Damme (1989) considers a strategy vector called "Getting even" (GE) in an infinitely repeated two-player, symmetric PD game. GE instructs a player i to cooperate unless i has played defect more often than any of the other players in the past.

Van Damme demonstrates that the GE vector is a WRPE for sufficiently high discount factors. More generally, he shows that the requirement of WRPE does not further restrict the set of equilibrium payoffs in this game, compared to the restriction imposed by the requirement of subgame perfection.[13]

Barrett (1994) verifies that the strategy vector where all players play GE is also a WRPE in the N-person IRPD, provided that the discount factor is arbitrarily close to 1. Moreover, he shows that this WRPE places a cap on the number of parties to an agreement. Finally, Finus and Rundshagen (1998a) extends this argument to the case where discount factors are not arbitrarily close to one. Asheim et al. (2006:97) show that PE simplifies GE in ways that are intuitively and analytically appealing.

Experimental Research

A massive amount of experimental research has been carried out on public goods games, of which the Prisoners' Dilemma is one variant. However, almost all such experiments consider a fixed number of rounds (either one or several) that is known to the subjects ex ante. Reviews of the literature are given by Dawes and Thaler (1988), Ledyard (1995), and Fehr and Schmidt (1999:836-9).

The main findings from this research can be summarized as follows. Even though finitely repeated public goods games have a unique equilibrium in which no player contributes in any period, single-round experiments average contributions in the interval

[12]With an alternative concept of collective rationality Barrett (2002) establishes a link between equilibrium behaviour and the size of the player set. This alternative concept is less developed in the formal literature than the renegotiation-proof equilibrium considered here, and will not be discussed further.

[13]Thereby proving wrong the conjecture put forward by Farrell (1983) that only repetition of the stage game equilibrium of a discounted PD game under an open horizon could be renegotiation proof.

40-60 percent. When a public goods game is repeated - usually for 10 or 20 rounds - average contributions typically start out as in the one-shot game, but drop over time (even though exceptions have been documented) and the final period tends to exhibit low levels of cooperation. When restarted, or played by subjects with prior experience from public goods games, average contributions start out as before and in general evolve as described. Hence, the observed pattern cannot easily be attributed to learning.

Pre-play communication tends to increase average contribution levels. Moreover, increasing the marginal per capita return of a contribution has a strong positive effect on average contribution levels. Controlling for this effect, the impact of group size is somewhat uncertain, but, if anything, average contribution levels tend to increase with increasing group size.[14] High costs (low benefits) of contributing tend to decrease the average contribution level, other things being equal.[15] Lumpiness (provision thresholds) seem to increase the average level of contributions, but this effect is uncertain.

A large and growing literature claim that subject pools exhibit considerable heterogeneity with respect to other-regarding preferences, and that a sizable fraction of subjects act upon such preferences in finite public goods experiments (Camerer 2003, Fehr and Fischbacher 2003).[16] However, such explanations of observed deviations from a model based on self interest, though popular, remain controversial. Samuelson (2005) forcefully argue that observed deviations is better viewed as resulting from self-regarding players with bounded rationality.

As far as we know, the only previous study of an indefinitely repeated public goods experiment is Roth and Murnighan (1978). This almost complete absence of experiments with games of indefinite repetition is remarkable, in light of the fact that the bulk of theoretical research has focused on indefinitely repeated games, where equilibrium contribution levels generally are higher than in fixed horizon games. Roth and Murnighan's main finding is that reducing the continuation probability reduces average contributions in the game.[17]

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[14]Isaac, Walker and Thomas (1984) and Isaac, Walker and Williams (1994) are seminal studies.

[15]See especially Isaac, McCue and Plott (1985) and Marwell and Ames (1979).

[16]Henrich et al. (2004), Ockenfels and Weimann (1999) and Burlando and Hey (1997) demonstrate cultural variation in cooperation in public goods games. The differences are ascribed to variations in social preferences over societies. The importance of informal social sanctions (naming and blaming) in public goods games are demonstrated by Andreoni and Petri (2004), Rege and Telle (2004) and Cinyabugma, Page and Putterman (2005). Altruistic punishments in public goods games are investigated by Fehr and Gächter (2000, 2002). General overviews of the research on social preferences and public goods games can also be found in Gintis et al. (2005), Ledyard (1995), Gächter and Fehr (1999). There even exists some neurological evidence from finite public good experiments, indicating that subjects derive utility from mutual cooperation in and of itself (Rilling et al. 2002, 2004).

[17]Unfortunatley the Roth and Murnigham study does not enable us to decide whether contributions tend to fall over time in the manner observed in fixed horizon games.

The aim of Roth and Murnighan is different from ours. They explore whether variation in continuation probabilities (discount factors) has an impact on behavior. Costs and benefits of the stage game are kept constant over treatments, and the study is limited to a two-player game (in which one of the players is an automaton). Furthermore, the generality of their results may be questioned on behalf of the reward structure implemented. Subjects were rewarded solely by an opportunity to win a fixed sum (10 USD) in a final game. In our experiment the continuation probability is kept constant, while the cost of making a contribution is systematically varied over treatments. Also, we study an N-player game (with no automatons) where the player group is varied systematically, and rewards are accumulated over all rounds of play.

Of particular interest for our purposes is the experimental study carried out by Davis and Holt (1999). They study a two player, two stage game. In the first stage a unique sub-optimal equilibrium exists. The question asked is whether a cooperative outcome of the first stage can be supported by threats in the second stage game. Davis and Holt vary the structure of threats available in the second stage game, making them non-Nash, Pareto-dominated and Pareto-undominated respectively. In WRPE, cooperation in the first stage should be largest in the treatment with Pareto-undominated threats. Furthermore, cooperation in the first stage should not be supported by non-Nash threats. Davis's and Holt's results do not support these conjectures, however. In fact, they find that the second stage equilibria are not primary determinants of the rate of cooperation in the first stage. Very low rates of cooperation are sometimes observed in treatments with access to Pareto-undominated threats, while very high rates of cooperation are sometimes observed in treatments with access to non-Nash threats. In other words, WRPE does not explain behaviour well in the Davis and Holt design.

Davis and Holt speculate that "when the game is modified to make the notion of punishing clearer, for example, by allowing pre-play communication or by increasing the number of periods, participants may be much more sensitive to the structure of punishment outcomes" (1999:109). In our experiment there is an indefinite number of rounds, in the sense that there is always a positive probability that the game continues at least one more round. Even though our experiment did not allow pre-play communication, we did introduce a treatment (described below) aimed at solving the inherent coordination problems facing our subjects. This treatment might be viewed as a coarse substitute for pre-play communication. Interestingly, despite these differences between Davis's and Holt's design and our own, our results basically support Davis's and Holt's conclusion.

We do not know of any previous experimental studies designed to test the notion of an WRPE in an infinitely repeated PD or public good game. Given the apparent enthusiasm for the WRPE concept in the literature, it certainly seems worthwhile to attempt to fill this gap. In light of the fact that the concept of WRPE is widely employed in theoretical research on a future global climate agreement, experimental confrontations seem particularly appropriate. This is so since the Kyoto Protocol has not yet completed its first commitment period. Testing the implications of theoretical models on field data is therefore not (yet) a viable option.[18]

4. Design

Our experiment was undertaken over two consecutive days, with 20 students participating each day.[19] Subjects were recruited throughout the campus of the University of Oslo. More than 50 students signalled an interest in participating. Around half were students in political science, whereas the others came from a number of other departments. From the set of volunteers, 10 political science students and 10 students from other departments were randomly drawn to participate in the experiment each day.[20]

Upon arrival, the students were asked to fill in a short questionnaire pertaining to background variables. Together with the questionnaire, the subjects received written instructions explaining the details of the experiment. Included in the instructions was a set of control questions that enabled us to verify that the students had properly understood the structure of the game. Two trial runs (without monetary payoffs) enabled the subjects to become familiar with the software.

The experiment was conducted in a number of sessions. In every session, subjects played an N-player IRPD-game with linear payoff functions in groups of either 5 or 10 subjects. After each round of play, a random draw decided with probability 0.9 that the game would proceed to the next round, and with probability 0.1 that the game would stop after the current round. The resulting number of rounds varied from 1 to 48, with an average of 12.3 rounds.

Group size, the cost of cooperation, and the information communicated to the students were systematically varied. On day 1 the game was played in four groups of five subjects. In sessions 1-3 the cost of cooperation was "low," whereas in sessions 4-5, the cost was "high." After the fifth session the solution based on the PE strategy was explained in some detail to the subjects, and subjects were allowed to ask questions about the nature of this solution. Then we ran two new low-cost and two new high-cost sessions. On day 2 this basic design was repeated (with a new set of subjects), with two important differences. First, the subjects now played in two groups of 10. Second, additional

[18]As far as we know, the only example of empirical research on the level of cooperation with a global climate agreement is Fredriksson and Gaston 2000. They estimate the conditional probability of countries ratifying the UN Framework Convention on Climate Change. The study does not address the content of the convention, or the chances that signatories to the convention will eventually comply with it.

[19]The experiment was conducted using the software Z-tree (Fischbacher 1999).

[20]In the event that some participants would not show up, we also invited three potential substitutes for each day. Only one of these was actually used.

sessions with a "very high" cost were included both before and after the students were informed of the solution based on the PE strategy.

To make sure that subjects could condition their behavior only on the history of the current session (i.e., not those of previous sessions), we implemented a double randomization. First, subjects were randomly distributed to groups before each new session. Second, subjects were randomly assigned a participant number in the group at the beginning of each new session (a number between 1 and 5 in the small group treatment, and a number between 1 and 10 in the large group treatment).

It is well known that the context imposed on an experiment may have a significant impact on the results. Scholars have drawn two very different conclusions from this observation. One view is that the experimenter should try to impose as little context as possible. The competing view is that the idea of context-free experiments is naive, because if the experimenter does not impose a particular context, subjects will choose their own, leaving the experimenter even less in control (e.g., Loewenstein 1999). In accordance with the latter view, the subjects in our experiment were explicitly told in the invitation, in the general introduction, and in other instructions, that the purpose of the experiment was to test a set of hypotheses derived from a game-theoretic model that tries to identify conditions for international cooperation to curb climate change. We made clear to the subjects that they would not be informed about the nature of these hypotheses, explaining that this information might influence the results. Our findings should be interpreted with the context of climate change in mind. Without further research it is difficult to decide to what extent generalizations to other contexts can be made.[21]

All subjects received a show-up fee of NOK 300, in addition to the money made in the experiment. For most Norwegian students the show-up fee would roughly approximate the salary for two hours work. For convenience all costs and rewards were denominated in an experimental currency, which was denoted "schillings" in our experiment.[22]

The exchange rate of 0.3 NOK to a schilling was made public knowledge at the start of the experiment. The latter was done to approximate the assumption of complete information in the model.

In the beginning of every round each subject received either 6, 12 or 21 schillings depending on the treatment. They could either keep or contribute (all of) this endowment to the provision of a public good. In all sessions the marginal gains of contributing and abstaining respectively were kept constant at d=b=3. Thus, a contribution always raised every group member's payoff by 3 schillings. Since the minimum cost of a contribution was 6 schillings, however, abstaining was invariably a dominant strategy in the stage game.

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[21]Including a separate treatment for context would drastically complicate our design, and was therefore left to future research.

[22]At the time the experiment was carried out, 1 USD bought approximately 7.07 NOK.

With a continuation probability of 0.9, condition (3) identifies a WRPE in each of the cost treatments.[23]

In the treatment where costs are 6 schilling, condition (3) places m between 2.1 and 3 inclusive. Since the number of players must be an integer, the WRPE has exactly 3 players contributing. In the treatment where costs are 12 schillings, condition (3) places m between 4.3 and 5 inclusive, so that the WRPE has exactly 5 players contributing. In the treatment where costs are 21 schillings, condition (3) places m between 7.7 and 8 inclusive, meaning that the WPRE has exactly 8 players contributing.[24]

These various WRPEs give rise to a simple comparative static, from which our directional predictions can be derived.

In the model, information is assumed to be complete and "almost perfect." In a game of complete information, the structure of the game (strategy sets and preferences) is common knowledge. This assumption is impossible to replicate completely in the laboratory. However, the structure of the game was made public knowledge by (i) providing all subjects with payoff matrixes and other relevant information about the game, (ii) making sure that all participants could observe that all participants received this information, and (iii) using control questions to verify that the information was understood, as well as announcing in public that everyone had understood. Almost perfect information implies that the history of the game up to and including round t - 1 must be common knowledge when subjects make their decisions in period t. In order to approximate this assumption in the lab, an updated statistic was generated on the screen of the subjects' computers before they entered the decision phase of the next round. This statistic contained information about the choices of every subject in every previous round of the session. The subjects were also reminded of their own monetary payoff in the preceding round and the total number of contributions in that round.

After entering the decision phase of a new round, all subjects had continued access to the complete history of all the subjects' choices in the session up to that round. While making decisions, subjects were also reminded of the cost of contributing. In addition, each subject had a paper copy of the relevant payoff matrix at hand.

In the treatment with a random draw of signatories, a list of subject numbers matched with each subject's "type" ("Signatory" or "Non-signatory") was presented to the subjects within each group before every round of the session. The number of signatories (held constant over the session) corresponded to the number of contributors in the WRPE (3 signatories for low costs, 5 for high costs and 8 for very high costs). This treatment provided subjects with a powerful "focal point", facilitating coordination on a set of participating players (i.e. "signatories") that where expected to contribute in the

[23]In addition to the three WRPE equilibria with positive contributions, playing the equilibrium of the stage game (based on the strategy vector "always abstain") is always an WRPE.

[24]The last equilibrium is of course only relevant for the large group.

relevant WRPE. During the experiment, the subjects were only allowed to communicate through their computers. The idea was that, because the notion of WRPE considered in the experiment does not include an explicit theory of communication, subjects should not be allowed explicit communication either.[25]

As can be seen, our experiment combines elements from an across-subjects design (group size) with elements from a within-subjects design (cost treatments). Conventionally, across-subjects designs dominate in experimental economics, although such a dominance does not seem to be well founded.[26] A potential drawback of the across-subjects design is the risk that comparisons across subjects are blurred by uncontrolled, subject-specific differences, pertaining for instance to group heterogeneity in social preferences and varying degrees of previous experience with laboratory experiments.

5. Results

We present our results in three steps. First, we look at aggregate contribution levels for all sessions, conditioned on group size, the cost of contribution, and whether subjects were explained the nature of the PE equilibrium. Second, we estimate the probability of an individual contribution based on micro data, conditioned on the same variables as in the aggregate analysis, and controlling for various cross-sectional and time-dependent influences. Finally, we carry out an analysis at the level of group interactions. The

[25]Views differ as to whether this restriction might influence the external validity of the experiment. On the one hand, Barrett (1999) suggests that cheap talk between rounds might facilitate renegotiation-proofness, and this conjecture is supported by the findings of at least one recent experimental study (Andersson and Wengström 2007). On the other hand, van Damme (1989:207, note 1) claims that "[explicit communication]… is irrelevant: Even if no player can articulate the proposal, the logic underlying the argument should convince … players not to punish each other (and themselves)." In choosing not to include communication as a treatment we are in good company. In particular, the experimental study of renegotiation proof equilibrium in two-player, two stage-games by Davis and Holt (1999) does not include explicit communication as a treatment. At least two other points are relevant in this context. First, in violation of the Nash program, neither Barrett nor Davis and Holt model communicative moves explicitly. Because of this, it is arguably still uncertain exactly how such moves would influence equilibrium behavior in their models. In other words, our experiment is at least consistent with the formal structure of the model we are aiming to test. Second, in our experiment we included treatments with a public signal (the signatories draw), which arguably solved the coordination problem that the participants otherwise would have faced. These treatments served (at least in part) as compensation for the fact that the subjects were precluded from coordinating through pre-play (as well as intra-play) communication.

[26]Camerer (2003:42) writes: "There is a curious bias against within-subjects designs in experimental economics (not so in experimental psychology). I don't know why there is a bias, and I can't think of a compelling reason always to eschew such designs. One possible reason is that exposing subjects to multiple conditions heightens their sensitivity to the differences in conditions. This hypothesis can be tested, however, by comparing results from within- and between-subjects designs, which is rarely done." An example of a mixed design where such comparisons can be found is Sutter (2003), where the equilibrium prediction is not supported in an across-subjects design, but achieves a fair amount of support in a within-subjects design.

dependent variable is now the average group contributions per round, conditioned on the same variables as before. This three-step approach was chosen to ensure that our findings are not artefacts of aggregation, but consistent over various levels of analysis.

Aggregate analysis

Consider tables 1, 2a and 2b. Table 1 shows the percentage of players expected to contribute in the relevant WRPE under four different combinations of group size and cost of contributing.

Table 1: *Expected contributions by group size and cost.*
Percentage of players expected to contribute in a WRPE

	Small group (n=5)	Large group (n=10)
Low cost (6 schillings)	60 (3 of 5)	30 (3 of 10)
High cost (12 schillings)	100 (5 of 5)	50 (5 of 10)

While, according to implication 2, the absolute number of contributors in a group should not vary if costs are kept constant, the percentage of players that contribute should vary. In particular, this percentage ought to decrease with increasing group size. This effect is in line with one interpretation of Olson's size principle (Olson 1965, Barrett 1999). More interesting perhaps, the percentage of players that contribute should increase with increasing cost of contributing, holding group size constant. This is the effect suggested in implication 1. What are the results of the aggregate analysis? Consider first the case where subjects were not given any information about the nature of equilibrium play (and where no signatories therefore were drawn). The results are reported in table 2a.

Table 2a: *Contributions by group size and cost.*
Percent of players who contributed (N=Decisions).

NO SIGNATORIES DRAWN	Small group (n=5)	Large group (n=10)
Low cost (6 schillings)	42.7 (700)	49.5 (400)
High cost (12 schillings)	17.1 (340)	33.0 (100)

The table clearly shows that implication 1 is not supported by the data. To the contrary, the average percentage of contributions falls with increasing costs of contributions, holding group size constant. With a group of five, an average of 42.7 percent of the players contribute (17.3 percentage points below the contribution level expected in equilibrium) in the low-cost treatment. Moving to the high-cost treatment, contributions fall to 17.1 percent on average. With a large group, contributions again fall when going from low to high costs, contrary to the model predictions. In this case, the average contribution level for low cost is 49.5 percent (19.5 percentage points above the contribution level in equilibrium) and drops to 33 percent for high cost.

In the case of large groups, we also ran sessions with "very high" costs (21 schillings, not reported in the table). For very high costs the equilibrium level of contributing players is 80 percent (8 out of 10 subjects contribute in equilibrium). However, the average contribution level observed for this treatment was only 22.7 percent (N=340). Thus, increasing the cost from high (12 schillings) to very high (21 schillings), reduced the average contribution level by 10.3 percentage points, discrediting implication 1 further.

Observation 1a: In sessions carried out before subjects were explained the nature of equilibrium play - and given clues as to which subjects were expected to contribute in equilibrium - average contribution levels fell markedly with rising costs.

Although the PE vector admits only one WRPE with positive contributions in each of our experimental controls, this equilibrium can usually be achieved by many different combinations of specific subjects as signatories and non-signatories.[27] A possible explanation for implication 1a is that the subjects were simply unable to coordinate on one particular of the (often) bewilderingly large number of ways to play the equilibrium. We therefore conducted a number of sessions in which the subjects (i) were explained the nature of the WRPE based on the PE vector, and (ii) an appropriate number of "signatories", corresponding to the number of participants (m) in the relevant WRPE, were randomly selected at the beginning of each session. In these sessions, the administrators de facto solved the coordination problem faced by the subjects. Every

[27]With low cost there exists - by the binomial formula - 10 ways to play the equilibrium in the small group. With low cost and a large group, the number of ways to play the equilibrium increases to 120. With high cost, there is one way to play the equilibrium in the small group, while the number of ways to play the equilibrium in the large group is 252. If costs are very high (only relevant for the large group), the number of ways to play the equilibrium is again 120. Since it is more profitable to be a non- contributor than a contributor in equilibrium, the subjects face formidable coordination problems in trying to find a way to play the renegotiation-proof equilibrium. The exception is the high-cost, small-group treatment. While in this treatment average contribution levels fall short of the equilibrium expectation by far, we do not hold this observation against the theory, since it builds on a point and not a directional hypothesis. Inability to solve the coordination problems may of course destroy our directional predictions.

subject (signatories as well as non-signatories) were of course free to choose as they liked. The results from these sessions are reported in table 2b.

Table 2b: *Contributions by group size and cost. Percent of players who contributed (N=Decisions).*

SIGNATORIES DRAWN	Small group (n=5)	Large group (n=10)
Low cost (6 schillings)	54.1 (1060)	52.4 (700)
High cost (12 schillings)	16.4 (360)	29.0 (780)

Table 2b shows that implication 1 does not hold even when the subjects are explained the nature of the WRPE and the administrators single out the subjects expected to contribute in this equilibrium (thereby giving the equilibrium concept considerable help). Again, the average number of players that contribute decreases with increasing costs of playing contribute, holding group size constant. In small groups, while an average of 54.1 percent of the players contributed when costs were low, only 16.4 percent contributed when costs were high.[28]

In large groups, the contribution level was 52.4 percent with low costs, and 29.0 percent with high costs. In short, increasing the cost of making a contribution caused the overall level of cooperation to drop.

In the large group condition, we ran additional sessions with very high costs (21 schillings). The average contribution level observed for this treatment was only 23.8 percent (N=240). Thus, increasing the cost from high (12 schillings) to very high (21 schillings) further reduced the average contribution level by 5.2 percentage points.

Observation 1b: After subjects were told about the WRPE way to play the game - and given clues as to which subjects were expected to contribute in equilibrium - average contribution levels still fell markedly with increasing costs.

What about implication 2? Observing table 2a and 2b, we see that the average level of contributions tends to increase when moving from small to large groups, holding cost levels constant. In the low-cost treatment, however, this effect is somewhat ambiguous. If no signatories are drawn (table 2a), the contribution level increases by 6.8 percentage points when moving from small to large groups. If signatories are drawn (table 2b), contribution levels decrease by 1.7 percentage points when moving from small to large

[28]It may be noticed that this is 5.9 percentage points below the contribution level expected in equilibrium.

groups. When costs are high, the effect is more pronounced: If no signatories are drawn, moving from the small to the large group increases the average level of contribution by 15.9 percentage points. The comparable number when signatories are drawn is an increase of 12.6 percentage points.

Observation 2: Moving from small to large groups increases the average number of contributions markedly if costs are high. If costs are low, the effect of increasing group size is ambiguous.

What is clear from observation 2 is that the independence conjectured in implication 2 is not observed for large groups. Increasing group size has an effect on the average level of contributions, and the effect is strong and positive for high costs, implying an interaction between costs and group size. We also note that the tendency observed (at least for high costs) goes in the opposite direction of that predicted by Olson (1965).

How can the observed pattern be explained? In our design the marginal per capita return is higher in the small group treatment than in the large group treatment. Other things being equal, one should therefore - based on previous experiments - expect average contributions to be smaller in a large group than in a small group (Ledyard 1995:137-41;149-55, Isaac, Walker and Thomas 1984, Isaac, Walker and Williams 1994). However, we observe an effect in the opposite direction. By implication 2, increasing group size should not lead to changes in observed behavior. For all of our 10 experimental treatments, a non-parametric test (based on the binomial distribution) was carried out. The test was designed to check whether the observed distribution of contributions differed significantly from the one expected in the WRPE. In all 10 treatments, we tested the null hypothesis that the actual distributions correspond to the distribution expected in the WRPE. In every case, the null hypothesis was rejected at significance levels at or below 0.001.

Observation 3: The distribution of contributions is significantly different from the ones one would expect to see if the players had played the PE vector in support of the relevant WRPE.

Analysis of micro data

We now turn to the results of our micro level analysis. Table 3 shows the estimates of two logistical regressions. The equations control for cross-sectional (subject specific and inter-group specific variation) and time-specific (session numbers and round numbers) variance (not reported).[29]

[29] The expected distributions in the renegotiation-proof equilibria are given in table 1 for low and high costs, and in the text for very high costs.

What we want to estimate is the probability that a subject will play contribute, given group size and cost of contribution. Both equations include the variables GROUP SIZE, HIGH COST and VERY HIGH COST. The first of these variables is a dummy taking the value one if the group is large (N=10), and zero otherwise. The second is a dummy taking the value one if costs are high (12 schillings) and zero otherwise. The third variable is a dummy scoring one if costs are very high (21 schillings) and zero otherwise. The first equation (model 1) contains only cases in which subjects have been told how to play the WRPE. The variable TYPE takes the value one if the subject was singled out as a signatory and zero otherwise. The second equation (model 2) contains only cases in which subjects were not told how to play the WRPE. In this equation the variable TYPE has no meaning, since no random draw of signatories was carried out.

Table 3: *Dependent variable: individual decision (contribution=1). Logistical regression coefficients (standard errors). Controls for subject dummies, group dummies, round numbers and session numbers not reported.*

	Model 1	**Model 2**
CONSTANT	2.37 (1.60)	-0.86 (.644)
GROUP SIZE	1.79** (.613)	2.05*** (.626)
HIGH COST	-1.73** (.259)	-1.48*** (.361)
VERY HIGH COST	-1.29* (.534)	-1.94*** (.483)
TYPE	0.71*** (.161)	–
Correctly predicted (%)	79.8	77.8
Model Chi-square	1611.8***	771.8***
N	3140	1880

*** $p < 0.001$
** $p < 0.05$
* $p < 0.10$

As can be seen from model 1 and model 2, the regression coefficients for group size and cost dummies are highly significant and have signs that are in disagreement with implications 1 and 2, controlling for other cross-sectional and time-specific variance.[30]

[30]See Köningstein 1998 for details about the use of dummies to control for cross sectional variance and temporal variance in experimental data.

Moving from a small to a large group significantly increases the probability of an individual contribution. Furthermore, moving from low to high cost significantly decreases the probability of an individual contribution. Finally, moving from low to very high cost also significantly decreases the probability of an individual contribution.

In regards to model 1, we see that solving the coordination problem for the subjects - by instructing them how to play the WRPE and drawing signatories - does not alter these conclusions. Being drawn as a signatory significantly increases the probability of a contribution to the collective good, but the size of the effect is moderate. To provide an illustration, we use the small group, low cost, no type draw treatment as a reference group. A shift to high costs from this reference group reduces the probability of a contribution by .26. In comparison a shift to type draw from this reference group increases the probability of a contribution by .04. This is quite remarkable in light of the fact that in this experimental treatment the WRPE is given considerable help. Despite this, the results offer very little support for implication 1. Giving the model a considerable amount of "help" - by explaining to the subjects how to play the WRPE and solving the coordination problems involved - does not produce much additional support for the central model prediction (implication 1).

Observation 4: Other things being equal, a subject is less likely to contribute, the higher the cost of a contribution.
Observation 5: Other things being equal, a subject is more likely to contribute in a large group than in a small group.

Analysis of group interactions
We now focus on the level of strategic interaction, that is, the group in which the subjects actually interacted. Table 4 presents the results of the analysis.

Table 4: *Dependent variable: average group contributions per round. Unstandardized regression -coefficients (standard errors).*

	OLS	GLS
Constant	0.44*** (.019)	0.58*** (.010)
ROUND NUMBER	0.00 (.001)	0.00 (.001)
GROUP SIZE	0.05*** (.022)	0.08*** (.010)
HIGH COST	-0.29*** (.022)	-0.31*** (.011)
VERY HIGH COST	-0.29*** (.041)	-0.23*** (.022)
TYPE DRAW	0.06*** (.022)	0.12*** (.010)
F-statistics	45.3***	187.0***
Adjusted R^2	0.23	0.73
N	748	748

*** $p < 0.001$
** $p < 0.050$
* $p < 0.100$

The dependent variable in table 4 is the average percentage of players that contributed in each round. As independent variables we use the cost-dummies, the group-size dummy, and TYPE as defined above. To account for dynamics, we control for round number. Since a Spearman rank correlation test indicated signs of heteroscedasticity, a generalized least squares regression (GLS) was run in addition to ordinary least squares (OLS). As can be seen, the difference between the two is marginal.

The results support our previous findings. Moving one step up the cost ladder reduces the average fraction of players that contributed by roughly 20 to 30 percentage points. Moving from small to large groups increases the average percentage of players that contributed by roughly 5 to 10 percentage points. Being explained the nature of the WRPE leads to an increase in contributions of roughly the same magnitude. No dynamic effects are discernible.

The latter point is worth elaborating on. In PD games with a fixed horizon, a robust experimental finding is that contributions tend to decrease over time, and that free riding is especially pronounced in the last few rounds. In our data, there is no such tendency of more pronounced free riding towards the end of each session.[31] In fact, there

[31]Inspecting plots of the development of average contributions within groups supports this conclusion. This also holds for our longest sessions in the small group and the large group sessions. The longest session in the small group treatment ended after 48 rounds, and the longest session in the large group treatment ended

is no particular dynamic pattern at all. The reason may well be precisely that the time horizon is not fixed.

Observation 6: Other things being equal, increasing the cost of a contribution reduces the average fraction of group members that contribute.
Observation 7: Other things being equal, increasing group size increases the average fraction of group members that contribute. This effect is moderate.
Observation 8: Group behavior does not change systematically over time.
Observation 9: Explaining the nature of the WRPE to the subjects, and solving the coordination problem for the group, increases the average fraction of group members that contribute, other things being equal. This effect is moderate.

A likely comment at this stage is that the results obtained in our experiment might be due to the presence of some kind of social preferences. This, however is far from evident. There are two main types of social preference models, intentions-based and outcome-based. Intentions-based models are problematic in the N-player PD game, where it is impossible for a player to reciprocate a particular player's unfriendly (or friendly) behavior without also punishing (or rewarding) other players. Intentions-based models have so far been worked out only for games in which punishments and rewards can be accurately targeted.[32]

What about outcome-based models? The most elaborate models of this kind are developed by Fehr and Schmidt (1999) and Bolton and Ockenfels (2000). For the parameters in our experiment, however, Bolton and Ockenfels's model rules out stage-game equilibria where a fraction of the subjects contribute to the public good.[33]
We are then left with the model proposed by Fehr and Schmidt (FS). As we have shown elsewhere, if discount factors are high enough, the subgame perfect equilibrium behavior of FS-players and purely self-regarding players is not likely to be observationally distinct in the N-player IRPD (Helland and Hovi 2007).[34]

It still remains to be shown that the same holds true in a WRPE of the N-player IRPD. Our conjecture is that it does. For these reasons, we do not believe social preferences explain our findings.

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after 37 rounds. In the experimental literature, conclusions about convergence are in general drawn on the basis of observed behaviour over 10 to 20 rounds in fixed horizon games. Our longest sessions are considerably longer than this, but convergence is still absent. More interestingly, the longest sessions are representative, in the sense that no convergence can be found in any of the other sessions in the experiment either.

[32]Rabin 1993, Falk and Fischbacher 2006, Dufwenberg and Kirchsteiger 1998, Charness and Rabin 2002.

[33]More precisely, for public goods games with $N \geq 2$ and $a < 1$ no positive contribution equilibrium exists in the Bolton and Ockenfels model.

[34]More precisely, we show that other-regarding players are also subject to a folk theorem in this game.

6. Conclusion

The notion of WRPE is frequently used as a solution concept in non-cooperative models of international environmental cooperation in general and climate agreements in particular. Requiring WRPE leads to very pessimistic predictions about cooperation in the N-player IRPD game. Also, some of these predictions are highly counterintuitive and contradict conventional wisdom in the theory of public goods provision. In particular, this is true for the predictions that higher costs of making a contribution induce more cooperation, and that the number of contributors is independent of group size.

This paper has reported the results of an experiment designed to test these two predictions in a setting that carefully replicates a WRPE in the N-player IRPD game. The main findings are that high costs have a strong negative effect, and group size a marked (although weaker) positive effect, on cooperation.

Needless to say, there is a long way from individual behavior in the laboratory to governmental decisions on the international scene. The results of experiments such as the one reported in this paper should therefore be interpreted with care. Yet one should take seriously the fact that our findings are not easily reconciled with the predictions generated by the underlying model. Supporting previous experimental research on weak renegotiation proofness, our results challenge the empirical relevance of this notion, which underlies a good deal of recent theorizing on international climate agreements.

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