

Non-instrumental Behavior in an Experimental Public Goods Game

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Abstract

Economists have recently taken an interest in “social preferences”, meaning that each player’s welfare depends positively or negatively on the welfare of other agents. Previous research has demonstrated that such preferences explain behaviour in a number of games where standard economic theory produces anomalies.

This paper studies the infinitely repeated N-person Prisoners’ Dilemma experimentally. The experiment was conducted in an explicit context of climate change, using real money as payoffs. In each round the subjects were given an initial allocation, which they could either keep or invest in a public good. All investments were multiplied by a factor bigger than 1 but smaller than N, and divided equally on all group members. Thus, the proceeds from such investments represented a collective good for the group.

After the experiment, the subjects were asked to indicate (on a scale from 0 to 1) the weight they had placed on climate-related concerns relatively to pecuniary gains. The vast majority of the subjects reported that they had placed at least some weight on the climate. Moreover, we show that this “climate weight” is a powerful predictor of subjects’ propensity to invest in the public good, even when other relevant variables are controlled for. However, *nothing the subjects did during the experiment could possibly have any effect on global warming*. We conclude that our findings go some way towards challenging models based on purely instrumental behaviour, whether this behaviour is assumed to be grounded in self interest, or in some kind of social preferences.

1. Introduction

In this paper we report a somewhat curious empirical result obtained in a public goods experiment. The experiment was conducted in an explicit context of climate change, using real money as payoffs. The vast majority of the subjects reported that their behavior was motivated at least partially by a concern for the climate. However, nothing the participants did in the course of the experiment could possibly have any impact on the climate. Furthermore, pursuing a better climate in this (fruitless) way was costly in monetary terms for the individual. Thus, the behavior of these participants does not seem instrumental. As such, it is difficult to reconcile with existing models of public goods provision, be they based on purely self-seeking preferences or some kind of “social” preferences.

The paper is organized as follows. The next section offers a brief summary of recent research on public goods provision. In section three we describe the experiment. Section four presents our somewhat curious empirical finding in some detail. Finally, some apparent ways of resolving the puzzle are discussed in section five.

2. Models of public goods provision

It is well known that in fixed horizon public goods games – of which the fixed horizon Prisoners’ Dilemma is one variant – the unique equilibrium is that no player contributes. With an open horizon and not too severe discounting, things are more open-ended. In particular, folk theorems ensure that almost any observable behavior

may be sustained as a (subgame perfect) equilibrium. To obtain empirical cutting power, a more stringent equilibrium refinement must be introduced. Most existing models make the assumption of rational, self-regarding players.

A massive amount of experimental research has been carried out in order to test such models. Almost all the experiments implement a fixed horizon of either one or several rounds that is known to the subjects *ex ante*. Reviews of the literature are given by Dawes and Thaler (1988), Ledyard (1995), and by Fehr and Schmidt (1999:836-9). Main findings from these experiments can be summarised as follows. In single-round experiments, average contributions lie in the interval 40–60 percent. When the public goods game is repeated – usually for 10 or 20 rounds – average contributions in general start out as in the one-shot game but drop with time (though exceptions have been documented). The last period of the repeated game, in general, exhibits low levels of average contributions (high levels of free riding). When restarted, or played by subjects with prior experience from public goods games, average contributions typically start out as before and, in general, evolve as described. Hence, the observed pattern cannot easily be attributed to learning. Increasing the marginal per capita return on a contribution has a strong positive effect on average contribution levels. Controlling for this effect, the effect of group size is somewhat uncertain but, if anything, average contribution levels tend to increase with increasing group size (Isaac, McCue & Plott 1985, Isaac, Walker & Thomas 1984).¹ Moreover, pre-play communication tends to increase average contribution levels. Lumpy goods (i.e. goods possessing one or more provision thresholds) seem to induce higher contributions than continuous goods, although this effect is uncertain. High costs (low benefits) of contributing tend to decrease the average contribution level, other things being equal.²

As far as we know, the only public goods experiment with an open horizon (i.e., where the players cannot identify a final period with certainty) is described in Roth and Murnighan (1978). This is remarkable, since the bulk of theoretical research has focused on open horizon games, where equilibrium contribution levels generally might be higher than in fixed horizon games. The Roth and Murnighan study does not enable us to decide whether contributions tend to fall over time. Their main finding is that reducing the continuation probability reduces average contributions in the game. 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 subjects were rewarded solely by an opportunity to win 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. We study an N-person game (with no automatons), and payoffs are accumulated from combinations of actions taken over the rounds of play.

Given the amount of anomalies revealed by experimental data, it is not surprising that exploring the implications of social preferences in public goods games has become a growth industry in economics. Social preference models assume that people are not only (or even primarily) motivated by self-interest, but also care about the payoffs of others. Several attempts have been made at modeling social preferences rigorously (Fehr and Schmidt 1999, Charness and Rabin 2001, Bolton and Ockenfels 2000, see also Camerer 2003). At least three main categories of models may be distinguished: “*Difference aversion* models assume that players are motivated to reduce differences between theirs and others’ payoffs; *social welfare models* assume that people like to increase social surplus, caring especially about helping those (themselves or others) with low payoffs; *reciprocity models* assume that the desire to raise or lower others’ payoffs depends on how fairly those others are behaving” (Charness and Rabin 2001:1).³

A large number of experimental confrontations of models based on social preferences have been carried out. It is fair to say that the jury is still out on the question of how well these models tackle the anomalies created by standard public goods models. The point to note here is that, in one sense, social preference models represent a rather modest departure from standard models of public goods provision based on self-interested players. Both kinds of models assume that people are instrumental in the sense that they try to further given aims in a sensible way, by rationing on scarce resources and taking other players’ beliefs and actions into account. The disagreement concerns what precisely those aims are.

¹The marginal per capita return must not be confused with the marginal gains of contributing or abstaining (the parameters *b* and *d* in Barrett’s model). Write the subject’s utility function on the following form:

$U_i = f(z_i - c_i) + g/N(c_i + \sum_{j \neq i} c_j)$, where z_i is the private wealth of subject i , c_i is the contribution of i , and N is the number of players. The optimal contribution to the good is given by $U_i'(c_i) = 0 \Rightarrow f = g/N$, where f is the marginal return of resources used on the private good, while g/N is the marginal per capita return on resources used on the collective good. In our experiment $z_i \in \{6, 12, 21\}$ depending on the treatment, $c_i \in \{0, z_i\}$, $f = 1$ and $g = 3$. So the marginal per capita return is $3/5$ if $N=5$ and $3/10$ if $N=10$. With $f = 1$ it is a dominant strategy in the stage game not to contribute if $g/N < 1$. This is the case in our experiment. As we can see, it is not possible to keep d and b constant (as our design demands), and at the same time hold the marginal per capita returns constant while increasing N .

²See especially Isaac, McCue & Plott (1985) and Marwell & Ames (1979).

³Our italics.

By contrast, the findings we present in this paper indicate that subjects in a laboratory experiment sometimes deviate more fundamentally from the standard model. Specifically, a majority of our subjects reported that their behaviour in the experiment was at least partly motivated by a concern for the global climate, *even though nothing the subjects did in the experiment could possibly have any impact whatsoever on the climate*. At the same time, pursuing a better climate was costly in monetary terms. Hence, the subjects' behaviour did not seem to be instrumental.

3. The experiment

The purpose of the experiment was to test a model proposed by Scott Barrett (1999, 2003). Using weakly renegotiation proof equilibrium as solution concept, Barrett shows that, other things being equal, *more* players will contribute to the public good the higher the cost of a contribution. A second implication of his model is that the number of players contributing does not depend on the total number of players. Elsewhere we have questioned the empirical relevance of these conjectures, based on our experimental data (Helland and Hovi 2003).⁴ In the present paper we limit ourselves to discussing a by-product of the experiment: The evidence of non-instrumental behaviour. We now describe the experiment in some detail.

The experiment was undertaken over two consecutive days, with 20 students participating each day. Upon arrival, the students were asked to fill out a short questionnaire asking them about their name, sex, age, university courses, and prior knowledge of game theory. They were also asked a few questions intended to tap potentially relevant aspects of their value system. One of these questions was designed to tap the respondents' "green attitudes": "Please indicate your agreement or disagreement with the following statement: I am willing to sacrifice goods and services that I presently consume, if by doing so I would contribute to preserving our natural resources". The response alternatives were: "I completely agree", "I agree", "I find it impossible to indicate agreement/disagreement", "I disagree" and "I completely disagree". These alternatives were coded 0 to 4 for degree of "greenness". The responses varied from 0 to 4 with a mean of 3.05 and a standard deviation of 0.86.

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 problem at hand.

The experiment was conducted in a number of sessions. In each session, each of 2 or 4 groups played an infinitely repeated N-person Prisoners' Dilemma with linear payoff functions. Each group consisted of 5 or 10 subjects. After each round of play, a random device included in the software decided with probability 0.9 that the game would proceed to another round, and with probability 0.1 that the game would stop. The resulting number of rounds varied from 1 to 48, for an average of 12.3.

Throughout the experiments, we systematically varied group size, the cost of cooperation, and the information communicated to the students. 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 a proposed equilibrium solution (the weakly renegotiation proof way to play the game) was explained in some detail to the subjects. In each group, a certain number of subjects (corresponding to the number of cooperating parties in the renegotiation proof equilibrium) were randomly drawn as "signatories" at the start of each new session. The numbers of subjects selected as "signatories" were communicated to the group on the computer screen, in order to facilitate coordination on the renegotiation proof equilibrium. Then two new low-cost and two new high-cost sessions were undertaken. On day 2 this basic design was repeated (with a new set of subjects). However the subjects now played in two groups of 10 (as opposed to four groups of 5 on the first day). Additional sessions with a "very high" cost were also included both before and after the students were informed of the proposed solution.

In order to maximize independence between sessions we used 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). The purpose was to ensure that subjects could only condition their behaviour on other subjects' behaviour *in the same session*, not others' behaviour in *previous* sessions.

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 other is that the idea of context-free experiments is naïve. If the experimenter does not impose a particular context, the 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

⁴A copy of Helland and Hovi (2003), as well as the detailed experimental instructions, can be obtained at: <http://www.bi.no/users/a0111218/>

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.

In the beginning of every round each subject received a certain amount of schillings (6, 12 or 21), which they could either keep or contribute in its entirety to a public good. Any contribution raised every group member's payoff by 3 schillings. However, as the minimum cost of a contribution was 6 schillings, abstaining was invariably a dominant strategy in the stage game. The unique Nash equilibrium of the stage game is therefore that all subjects abstain. Furthermore, this equilibrium is Pareto inefficient. For example, in the low-cost case with 5 members in each group, each subject would receive 15 schillings if all group members contributed, as opposed to 6 schillings if all subjects abstained. However, the net (short-term) loss incurred by making a contribution differed significantly between treatments, depending on whether the cost of a contribution was 6, 12 or 21 schillings.

All subjects received a show-up fee of NOK 300, in addition to real money made in the experiment.⁵ All monetary payoffs were denominated in an experimental currency, called "schillings." 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 made in Barrett's model. 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 got this information, and (iii) using control questions to verify that the information was 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 a new round $t > 1$. This statistic contained information about the choices of every subject in every previous round of the session. Moreover, a private piece of subject specific information was revealed on the screen as a reminder: the subject's own conjecture about the number of contributors for the present round. The subjects were also reminded of their own monetary payoff in the preceding round and the total number of contributions from the group 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.

After the final session the subjects were given a second questionnaire, prompting them to indicate, on a 0–1 scale, the emphasis they had placed on the climate dimension of the experiment relative to monetary benefits. The answers varied from 0 to 1, with an average of 0.36 and a standard deviation of 0.29.

4. A curious result

We present our results in two steps. First we analyze data at the level of subjects ($N=40$). Thereafter we analyze data at the level of decisions ($N=3766$). Note first that only five subjects (12.5%) indicated that they placed no weight whatsoever on the climate when taking decisions in the experiment. Thus, only for a small fraction of the subjects can we rule out non-instrumental behavior of the type described in the previous section. For the 5 subjects reporting a purely instrumental motivation the mean fraction of contributions – taken over all decisions made by these subjects during the course of the experiment – was 0.34. By contrast, the corresponding mean for the 35 subjects that placed at least some weight on the climate was 0.43.

The small number of subjects with undisputable instrumental motivations makes statistical comparisons of differences in means infeasible. For this reason, we allow for degrees of instrumentality in motivations, and – somewhat arbitrarily – introduce the following convention: Subjects reporting a climate weight less than 0.25 are said to have "instrumental" motivation, while subjects reporting a climate weight larger than 0.25 are said to have "non-instrumental" motivation. When we use this convention, both subgroups become large enough to allow for statistical comparisons. In the group with instrumental motivation the mean fraction of contributions was 0.31 ($N=19$), while in the group with non-instrumental motivation the mean fraction of contributions was 0.51 ($N=21$). Table 1 shows the results of an independent samples test for the difference in means.

	Difference in means	P-value for difference in means	F-statistics homogenous variance (p-value)
Homogenous variance	0.20	0.007	4.28 (0.046)
Heterogenous variance	0.20	0.006	–

Table 1: Independent samples test for difference in means.

⁵At the time the experiment was carried out (29.04.03 and 30.04.03), 1 USD bought approximately 7.07 NOK.

The reported F-statistics of the Levene's test implies that the null hypothesis (that the distribution of contributions have equal variance in the two groups) may be rejected at the five percent level. Similarly, assuming heterogenous variance of contributions, the difference in the fraction of contributions between the two groups is significantly different from zero well below conventional levels. Thus, on average subjects with non-instrumental motivation contribute more often than subjects with instrumental motivation, and the difference is statistically significant. We now report the results of a simple multivariate analysis. Table 2 presents the results of two linear regression equations.

Independents	Dependent: Average contributions	Dependent: Average profits
Constant	0.17 (0.001)	11.3** (1.054)
Climate weight	0.61** (0.099)	-2.98** (1.035)
Group size	-0.02 (0.056)	8.35** (0.579)
Green attitude	0.00 (0.33)	0.26 (0.384)
F-statistics	13.7**	75.8**
Adjusted R ²	0.49	0.85
N	40	40

Table 2: Regression equations. Subjects. Coefficients (standard errors).

** Significantly different from zero at the 1 percent level.

* Significantly different from zero at the 5 percent level.

In Table 2, the independent variables are *climate weight*, a dummy for *group size* (scoring 0 for groups with 5 members and 1 for groups with 10 members) and *green attitudes* (using the five point scale described in the previous section). We ran the regressions on two different dependent variables: (i) the average fraction of contribution taken over all rounds in which the subject participated, and (ii) the average monetary payoff earned taken over all rounds in which the subject participated.

The results can be summarized as follows. Climate weight significantly affects the average fraction of contributions. Other things being equal, increasing a subject's climate from zero to one increases the average fraction of contributions by 0.61. This is a sizeable effect. Group size and green attitudes does not significantly contribute to the explanation of this dependent variable.

Climate weight also significantly explains average profits. Other things being equal, increasing a subject's climate weight from zero to one decreases average profits per round by 2.98 schilling. Thus, acting on non-instrumental motivation is costly. Note that group size now matters. The reason is simple. A given fraction of contributions will produce higher profits in a large than in a small group. Green attitudes do not contribute significantly to the explanation of average profits.

Interestingly, the correlation between climate weight and green attitudes is moderate. Pearson's r scores only 0.26 (and is just significantly different from zero at the 10 percent level). Table 3 shows the bivariate distribution for green attitudes and climate weight (measured as a trichotomy).

Green attitudes	Climate weight		
	Low (0, .33)	Medium (.33, .67)	High (.67, 1.00)
Very weak (0)	1	0	0
Weak (1)	1	0	0
Undecided (2)	4	0	1
Strong (3)	12	6	3
Very strong (4)	5	3	4

Table 3: Distribution of subjects on climate weight and green attitudes

Table 3 shows that 33 of the 40 subjects (83 percent) have strong or very strong green attitudes. More than half of these "green" subjects reported a fairly low climate weight – from .33 and downwards. This, of course, is by no means inconsistent. A subject may well have strong green attitudes, and at the same time realize that nothing he or she does in the experiment can be of any help to the environment. Of the subjects with

medium to high climate weights all except one have strong or very strong green attitudes. In this group some subjects may have let their strong green attitudes guide behavior in the experiment, even though such behavior cannot further their preference for green policies. We discuss this in more detail in the final section. Given the distribution in table 3, it is not surprising that green attitudes do not explain the variations in contributions and payoffs. Strong green attitudes are widely held in the subject population, both among subjects that let their behavior be guided by non-instrumental motivations and among subjects that does not.

Table 4 shows the results of an analysis at the level of individual decisions. A logistical regression is used to estimate the probability of contributing. The probability is conditioned on nine independent variables. The climate weight, the dummy for group size and the green attitudes are defined as in the regression in table 2. There are two dummies for costs. The first scores one if the cost of making a contribution is 12 schillings and zero otherwise. The second scores one if the cost of making a contribution is 21 schillings and zero otherwise. The dummy for type draw takes the value 1 if the decision was made by a subject that had been designated a “signatory” after a random draw (and thus were expected to contribute in a weakly renegotiation proof equilibrium), and zero otherwise. The dummy for gender takes value 1 for male and value zero for female. The two final variables are session number (per day), and round number (per session), which are included in order to control for dynamics. Using the same set of independent variables, we also ran an OLS regression with payoff per round as the dependent variable.

	Logistical regression: Contribution	OLS: Profit
Constant	-1.74** (0.190)	9.97** (0.392)
Climate weight	0.33** (0.097)	-2.97** (0.297)
Dummy high cost	-1.66** (0.121)	1.34** (0.221)
Dummy very high cost	-1.47** (0.163)	4.60** (0.302)
Dummy group size	0.33** (0.097)	7.71** (0.204)
Dummy type draw	0.58** (0.112)	-1.09** (0.235)
Dummy gender	0.51** (0.084)	-1.24** (0.169)
Green attitudes	0.06 (0.042)	0.27** (0.087)
Round number	0.00 (0.004)	-0.00 (0.009)
Session number	-0.02 (0.017)	0.25** (0.037)
χ^2	830.8**	n.a.
Percent correct	72.0	n.a.
F-statistics	n.a.	365.3**
Adjusted R ²	n.a.	0.46
N	3766	3765

Table 4: Regression equations. Individual decisions. Coefficients (standard errors)

** Significantly different from zero at the 1 percent level.

* Significantly different from zero at the 5 percent level.

The analysis of individual decisions largely corroborates the findings in table 2. Climate weight significantly explains the probability of contributing, controlled for cost structure, type draw, group size, gender, green attitudes and dynamics. By contrast, green attitudes do not explain the probability of contributing. High costs and small groups imply a lower probability of contributing than low costs and large groups. Men are more likely to contribute than women. Being singled out as a signatory significantly increases the probability of a contribution. Finally, no dynamic pattern is present in the data. Given that the length of each session was decided by a random mechanism, and hence basically unpredictable, this is not surprising.

The results for the OLS regression using profits per round as dependent variable are also broadly in accordance with the results presented in table 2. Non-instrumental motivation carry a cost. Again, the expected sacrifice in terms of payoffs forgone is close to 3 Schillings per round for a subject placing all weight on the

climate, compared to one placing no weight on the climate, other things being equal. Large groups and a high cost of contributing increase expected profits. The group size effect may be explained in the same way as before. That expected payoffs increase with higher costs reflects the fact that the value of a given fraction of contributions is higher for higher costs. Men have lower expected payoffs than women. There are weak, but significant, effects of session number and green attitudes. Round numbers have no significant effect.

Figure 1 shows an example of a probability curve drawn on the basis of the logistical regression results reported in table 4. In the example we look at a male subject facing high costs in a big group. It is round 15 in session 6, and the subject has very strong green attitudes (score 4). We let the climate weight vary in its domain. As can be seen, the climate weight has a strong controlled effect on the probability of contributing.

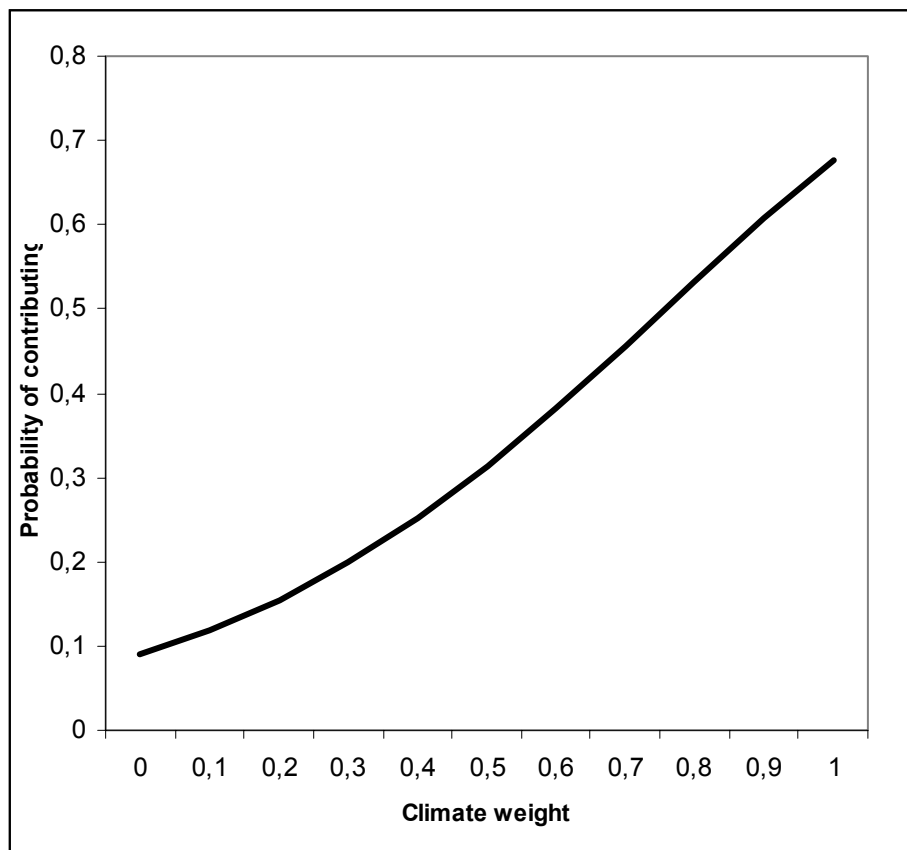


Figure 1: Probability of contributing as a function of the climate weight.

We sum up our findings in a few observations

Observation 1:

Only a *small* fraction of the subjects place no weight whatsoever on the climate.

Observation 2:

The variance in climate weight can *not* be explained by variance in the subjects' green attitudes.

Observation 3:

On average, subjects that report a high climate weight contribute *more often* than others. The effect is sizeable.

Observation 4:

To the extent that subjects act out of consideration for the climate, they *pay a price* in the form of reduced monetary payoffs.

5. Discussion

Given that nothing the subjects did in the experiment could possibly influence the climate, it is puzzling that the vast majority of the subjects nevertheless report that their behavior was influenced by climate-related considerations. This section briefly considers some possible explanations for this apparent puzzle.

Ignorance: A first idea might be that many subjects simply did not realize that their behavior in the experiment was completely unrelated to the climate. While we have no hard evidence that enables us to completely rule out this possibility, we find it extremely unlikely that subjects should really believe that the results of a simple experiment carried out in a PC laboratory could possibly have any impact on the climate.

Leading question: Pre-play instructions made it clear that the purpose of the experiment was to test a model designed to explain international cooperation to curb climate change. Hence, the context imposed on the experiment was explicitly one of climate change. Because of this, the subjects may have felt they were *expected* to report a positive number when asked to what extent a concern for the climate had an impact on their behavior. In other words, the question posed to the subjects after the experiment may have been *leading*. There is certainly something to this. However, it is hard to see why subjects who felt they were expected to report a positive climate weight should also be more likely than others to cooperate during the experiment. Unless one can come up with a convincing reason for the existence of such a link, this explanation is not very persuasive either.

Modest monetary incentives: The maximum aggregate payoff achieved by a subject in the experiment was around US\$ 150. Many students are likely to see this as a significant sum, especially in light of the fact that the experiment only lasted a few hours. Nevertheless, the monetary incentives *in each round* were undeniably modest. Thus, it is possible that some subjects largely disregarded the monetary incentives and rather used the experiment as an opportunity to act out their green attitudes. However, as we have already seen, the subjects' climate weights were only moderately related to the extent to which they had green attitudes. At the same time, the variance in green attitudes did not explain much of the variance in behavior. Hence, this account has at best limited explanatory value.

Boredom: An experiment of this kind involves the subjects in iterated decisions that are likely to quickly become routine or even boring. We cannot rule out the possibility that this may have had a bearing on behavior. Again, however, it is difficult to see why effects of this type would produce a strong link between climate weights and the propensity to cooperate. Also, to the extent that this effect was real, it would presumably become more and more apparent as the experiment progressed. In general, no dynamic pattern is discernable in the data.

Expressive motivation: March & Olsen's (1989) useful distinction between the logic of consequences and the logic of appropriateness is well known. As we have seen, it is difficult to account for the behavior of the subjects in our experiment via explanations based on the logic of consequences. The behavior is not consistent with a model where subjects maximize monetary payoffs. Nor can it be explained in terms of a concern for climate-related consequences. However, it is also difficult to provide a plausible answer via the logic of appropriateness. The latter type of explanation seems to assume motivation of a genuinely social character, which presupposes that behavior can be observed by other people. In our experiment, by contrast, behavior was basically anonymous. Hence, there was no way that the participants could demonstrate to others that they behaved in a socially or morally appropriate way.⁶

Ex post rationalization: While the statistical analysis in the previous section sees subjects' experimental behavior as being determined (at least in part) by climate weights, it is also possible that it was in fact the other way around, i.e., that climate weights were determined by the subjects' behavior. Recall that the subjects were not asked about climate weights until *after* the experiment had been completed. Although at that stage each subject had not yet been informed about his or her exact aggregate monetary payoff, most subjects probably had a reasonably good idea about their own performance. Thus, subjects that did well (in monetary terms) may simply have tried to highlight their own "success" by stressing that making money was their main objective in the experiment, whereas subjects that made less money used the opportunity to excuse their "failure" by emphasizing that they never tried to maximize monetary payoffs in the first place. Taking the opportunity to use this "excuse" may have seemed all the more attractive (to those that did relatively poorly in monetary terms), as being motivated by a concern for the climate probably stood out as the morally superior alternative. Hence, to report a concern for the climate rather than a desire to maximize monetary gains may have been a way for subjects that did poorly (in monetary terms) to rationalize their performance after the fact in a way that seemed morally attractive *as well as* logically consistent with their own behavior.

⁶ It cannot be ruled out, however, that some participants wanted to prove to themselves that they were not of a type that took advantage of a chance to be a free rider.

6. Conclusion

This paper has reported a somewhat curious result from a public good experiment carried out in a PC laboratory. The experiment used real monetary payoffs, and was conducted in the explicit setting of international cooperation to curb climate change. The puzzle is that after the experiment, a vast majority of the subjects reported to have been motivated by a concern for the climate, even though *nothing the students did in the experiment could possibly have any impact on the climate*. Having described this result in some detail, we briefly discussed a number of possible explanations. Although some of these accounts (such as the possibility of ex post rationalization) may carry some explanatory value, we feel that the basic puzzle largely remains. While recent research on public goods provision has gone beyond the traditional assumption of rational, self interested actors, the assumption of instrumental behavior is typically preserved. The result reported in this paper goes some way towards challenging the validity of this assumption.

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