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THE EFFECT OF LEADERSHIP IN A PUBLIC BAD EXPERIMENT

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The effect of leadership in a public bad experiment

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Abstract

With regard to global or regional environmental problems, do countries that take unilateral actions inspire other countries to curtail emissions? In this paper this possibility is investigated by the use of a novel design of a laboratory public bad experiment with a leader. Twelve groups of five subjects played the game twice, with two treatments: ten rounds with a leader and ten rounds without a leader. The order of the treatments was varied over groups. A significant (within-subject) effect of leadership is found. Followers invest on average 15 percent less in the public bad when there is a leader setting the good example as opposed to a situation with no leader. Furthermore, total payoffs turn out to be significantly higher in the leader treatment than in the no-leader treatment.

Keywords: public bad, experiments, leadership, pollution.

JEL classification: C92, H41, Q30

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1. INTRODUCTION

Global and regional environmental problems can be described as commons problems. Prominent examples of such problems are climate change and ozone depletion. For each country, the marginal benefits of emitting pollutants typically exceed the extra environmental costs caused by relatively small own contributions to the total pollution. Still, for the entire region or globe, marginal environmental costs could be substantially higher than the marginal benefits. The nature of this kind of global environmental problems makes them hard to solve. Actions by one country generally hardly matter, as each country's own contribution to global pollution is small. Therefore, to solve global environmental problems, countries have to coordinate their actions. In addition, one probably needs institutions to enforce and supervise international decisions and agreements.

Although at first sight it seems that individual countries can do little to solve global environmental problems, (domestic) environmental interest groups often point out that unilateral actions undertaken by one country may be beneficial. The reason for this is that a unilateral reduction may 'set the good example' for other countries. But while this assertion is frequently put forward, there is not much empirical evidence to support this claim. Will unilateral reductions by a 'leader' country, which could also be a small country, indeed lead to lower emissions by other countries (the 'followers')? And, in the long run, will this induced imitation effect result in higher payoffs for everyone, i.e. reduced pollution at an acceptable cost? These are still largely open questions.

Hoel (1991) has analyzed the consequences of unilateral reductions of emissions in a game-theoretic framework, by looking for Nash equilibria for the noncooperative game and the Nash bargaining solution for the cooperative game. In the noncooperative game with n countries he finds that a unilateral reduction by one country leads to increased emissions by the other countries but also to lower *total* emissions and higher total welfare. In the cooperative case, total emissions could go in either direction depending on the parameters of the problem and the imposed restrictions. In case the leader is a small country, the effects become negligible.[1]

Although Hoel's analysis is very relevant and interesting, its focus is rather narrow. In particular, as he remarks himself, he will not consider "whether unilateral emission reductions by one country may lead to similar behavior by other countries", p.69. In this paper, we address exactly this question. That is, we want to investigate whether the presence of a leader country, which sets the good example by having low emissions, may induce other countries to imitate this behavior and to reduce emissions as well.[2]

We will study the effect of leaders by the use of a laboratory experiment of a repeated public bad game, where the public bad represents pollution. For the no-leader game, we use a traditional public bad game design, whereas for the leader game we implement a novel experimental design. For the leader game, the traditional public bad design is altered in such a way that in each round a 'leader' makes his or her decision first. The leader's decision is then communicated to the 'followers' before they make their decisions simultaneously. Cost functions are chosen such that the Nash solution becomes a corner solution, both when there is a leader and when there is not.[3] Each group of participants get to play the game twice, once with and once without a leader. This set-up enables within-subject (group) comparisons.

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[1] In this paper we focus on the competitive (noncooperative) case. The competitive case can also be seen as representative of a situation with a small initial group of signatories (leaders), which compete with independent nations or groups of nations that have not committed themselves to any treaty. If unilateral reductions turn out to have a positive influence on the behavior of others in a competitive situation, it is not unlikely that unilateral reductions could also speed up negotiations and lead to better treaties. This question is, however, left for further research.

[2] Discussing lessons from evolutionary game theory, Mailath (1998) points to the likely importance of learning by imitating observed successful behavior by others. Thus it seems pertinent to speculate whether a leading nation taking unilateral actions could come to influence directly the strategies of others. Huck et al. (1999) present experimental evidence that some subjects imitate successful behavior if they have the necessary information.

[3] Keser (1996) finds that over-contributions in a public good experiment with an interior Nash equilibrium are similar to those in a corner solution public good experiment. We have the same Nash corrner solution for the two treatments. In theory, the Nash-solution implied by our design rules out the possibility that followers increase their investments in the public bad as a response to a unilateral reduction by the leader (as in Hoel's interior solution). However, as we will see, average follower investments in the experiment are not equal to the Nash prediction in the no-leader treatment, which

Laboratory experiments of public good and public bad games typically show deviations from Nash equilibria (see Ledyard (1995) for a comprehensive overview of public good experiments). It should be noted that our experiment is not designed to reveal whether possible deviations from the Nash solution are caused by individuals applying utility functions that take account of more factors than the given monetary incentives or whether they fail to apply maximizing strategies.[4] We are predominantly interested in the question whether the presence of a leader, who sets a good example by investing little in the public bad, induces followers to reduce investments in the public bad as well.

As far as we know, no public good or public bad experiment has thus far been conducted in order to investigate the impact of a leader. Or, put differently, we have not seen any other leader-follower experiment with a decision protocol that is mixed sequential-simultaneous. A major reason for choosing this design is that any country is in principle free to pursue a leadership role within a region or on a global scale. Other, purely sequential game designs, assume that actors make decisions in an exogenously determined order (see e.g. Budescu et al. (1995), Erev and Rapoport (1990), Morris et al. (1995), and Rapoport (1993)). While there seems to be interesting applications for such designs, they do not seem to fit regional or global pollution problems where numerous policy initiatives are needed over long time intervals. Neither is there a natural sequential order in which countries make such decisions, nor can such an order easily be forced on countries. A good reason for countries to object to ordering is that theory predicts the first movers to have an advantage and the last movers to carry the largest burdens. These predictions are confirmed in sequential step-level public good games (see Erev and Rapoport (1990)) and in sequential common pool resource games (e.g. Budescu et al. 1995)). For our design, theory holds that there is neither a first mover advantage nor a last mover disadvantage.

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gives followers the opportunity to increase investments in the public bad in the leader treatment. Obviously, this does not happen (see Section 4).

[4] Recently, several theories have been advanced, which incorporate considerations of fairness, altruism, reciprocity, equity et cetera (see e.g., respectively Fehr and Schmidt (1999), Levine (1998), Rabin (1993) and Dufwenberg and Kirchsteiger (1998), and Bolton and Ockenfels (2000)).

The remainder of this paper is organized as follows. The next section outlines the public bad game and the experimental treatments and it formulates the main hypothesis. Section 3 describes the experimental procedure in more detail. Section 4 presents the results of the leader and the no-leader treatments. The main finding is that leaders on average have a statistically significant effect on the followers, and that follower decisions tend to vary with variations in leader decisions. Otherwise, the results of the no-leader cases are consistent with previous findings for public bad games. We also find that the order of the treatments does not affect the results. The last section contains a concluding discussion and gives some lines for further research.

2. EXPERIMENT AND HYPOTHESIS

2.1. The public bad game

The basic experimental design is similar to the public bad framework used by Andreoni (1995), however with different parameter values. The main features of our public bad game are as follows. Subjects play in groups of five. In each round, subjects are endowed with 20 tokens, which they can allocate between two projects: project A (the public bad) and project B. Investing in project A gives a direct private return of 0.7 per token invested. Investing in project B gives a private return of 0.4 per token invested. However, investing in project A has also a negative external effect: each token invested in project A yields a negative return of 0.1 to all group members. So, payoff p_i to individual *i* (*i*=1, 2, ..,5) when she or he invests x_i^A in project A and x_i^B in project B reads

$$p_i = 0.7x_i^A + 0.4x_i^B - 0.1\sum_{j=1}^{5} x_j^A \qquad (1)$$

where x_j^A denotes the investment in project A (the public bad) by subject *j* (*j*=1, 2, ..,5), and $x_i^A + x_i^B = 20$. Using this last equality, we can rewrite (1) as

$$p_i = 8.0 + 0.2 x_i^A - 0.1 \sum_{j \neq i} x_j^A \tag{2}$$

From Equation 2 it follows directly that purely selfish, money-maximizing subjects have a dominant strategy to invest their total endowment in the public bad. That is, the unique Nash equilibrium is $x_i^A = 20 \ \forall i$, which yields a total investment of 100 tokens in project A and a payoff $p_i = 4 \ \forall i$. However, higher payoffs can be obtained if subjects invest in project B. If all members of a group decide to invest their total endowment in project B, i.e. $x_i^A = 0 \ \forall i$, the payoff to each individual would be twice as much, namely 8, which is the socially optimal outcome. Thus, the welfare-maximizing solution is obtained by full cooperation.[5]

The parameters of the game, summarized by the marginal per capita return (MPCR), are likely to influence the outcome. For instance, Isaac and Walker (1988a) find that contributions vary with MPCRs in a public good game. This indicates that our choice of MPCR could influence the magnitude of the leader effect. Since all players face the same MPCR in both treatments, the leadership effects we find are for that particular set of parameters. Our game has a MPCR of 1/3, which is similar to the MPCR of 0.5 used by Andreoni (1988) in a public bad game.[6]

The parameters (costs) in our game have been set such that investments in project A are clearly profitable, for given investments by the others. From Equation 2 we see that individuals gain 4 tokens by going from fully social ($x_i^A = 0$) to fully selfish investments ($x_i^A = 20$). In relative terms this means that if all others have invested socially ($x_j^A = 0$), subject *i* reduces his or her profits by 33 percent by being cooperative instead of selfish (from 12 to 8).

[5] The outcome $x_i^A = 0, \forall i$ is also the unique symmetric Pareto efficient outcome. Note, however, that many other, asymmetric Pareto efficient outcomes exist. For instance, the outcome in which 4 players contribute everything to the public bad and one player contributes nothing to the public bad is Pareto efficient as in this situation no player can be made better off without making a player worse off.

If all others have invested selfishly (x_j^A = 20), the relative loss for subject *i* is 100 percent by being fully social (from 4 to 0). These are rather large relative amounts, which should stimulate money-maximizing subjects to choose selfish investments and ignore leader decisions. Compared to actual gains from choosing polluting options as opposed to abatement options, the relative gains in the experiment seem high. Particularly the first abatement investments to move away from "business as usual" are likely to be smaller than in the experiment. If "business as usual" means that the economy is in an efficient state, marginal costs of the two investment options are equal. Hence, in reality, there should be virtually nothing to gain by acting selfishly and not making the first marginal reductions in pollution.

Groups remain the same during the session (the partner design by Andreoni (1988)). Thus the game mimics a persistent environmental problem, where decisions are made repeatedly at certain time intervals. This produces a stronger incentive for participants to influence others toward cooperation, than what is found in one period games.

2.2. The experimental treatments

In the experiment we have employed two experimental treatments: a leader treatment and a control treatment. In the control treatment, also called the no-leader treatment, all subjects in a group make their investment decisions simultaneously, as in standard public good and public bad experiments. There is, however, one difference between our control treatment and the standard games: usually individuals are only informed about their own payoff and the *aggregate* (group) contributions, whereas in our control treatment subjects also get feedback on the *individual* decisions of their group members. This has been done to avoid informational asymmetries between the leader, whose investment has to be revealed, and the others. Experimental evidence suggests that there is no significant effect of this extra

[6] Using Equation 1, in our game MPCR= $\dfrac{d(-0.1\sum_{j=1}^{5} x_j^A)/dx_i^A}{d(0.7x_i^A + 0.4(20 - x_i^A))/dx_i^A} = \dfrac{0.1}{0.3} = \dfrac{1}{3}$.

information on average investments in a public bad experiment (Van der Heijden and Moxnes (1999)).

Before we discuss the leader treatment, we want to make clear what we think are important characteristics of a leader, given our research agenda. First, a leader is one who sets the good example by making small investments in the public bad. Second, it seems important that the leader acts as a first mover and commits him- or herself for the coming round. In reality this would correspond to enacting policies and making abatement investments that are not easily reversed. Having set the good example in the last round, is no guarantee that the leader will do the same in the next round. Commitment removes uncertainty about that issue. Third, an active leader, who wants others to follow suit, must announce and make visible his or her actions. While such information could be seen as a minimum requirement for the leader to have an effect, it is also possible that increased visibility of the leader could have a framing effect which for instance reduces the weight put on other data.

In the leader treatment, one subject in each group is a *leader*. The leader decides first on his or her investment. As a result, the leader is a first mover and makes a commitment for the particular round. This decision is communicated to the other four members in the group, the *followers*, after which they make their decisions simultaneously. Like in the control treatment, all players are informed about the individual investments of the group members before the next decision is made. The incentives for all subjects in both treatments are given by Equation 1. Consequently, the main difference between the two treatments is that in the no-leader treatment all subjects decide simultaneously, whereas in the leader treatment the leader decides first and then the followers decide simultaneously (i.e. the decision-making protocol is mixed sequential-simultaneous). No matter what leaders do, the followers are still faced with a unique Nash corner solution.

2.3. The hypothesis

Our focus is on the total effect of leadership in a public bad game. Our null hypothesis is given by the Nash solution:

Hypothesis 0: *There is no effect of a leader in a public bad game*

The alternative hypothesis is that there is an effect of leadership. Public good and public bad experiments often observe behavior that deviates from Nash predictions (see Ledyard (1995)). The same mechanisms that cause these deviations could also be important for the effects of a leader.

We do not formulate a hypothesis about which of the three characteristics of a leader are (most) important for the eventual effect of leadership. In fact, the experimental design only allows for a partial exploration of this issue since all three characteristics change from the no-leader to the leader treatment. From one treatment to the next, the subject that is selected as a leader can change investment levels, is or is not a first mover, and does or does not receive particular attention by the others. In the discussion section we will suggest new experimental designs for further research on this issue. We reason that our strategy is an economical one. If we find no effect of leaders in our experiment, more detailed experiments will probably not be very interesting.

All this being said, time-series data from the present experiment can still give some indications about why leaders have an effect. Regressions will be used to analyze average follower investments as a function of both leader investments and previous round investments for followers with sufficiently low investments to qualify as "leaders". Thus we will be able to compare the effects of highly visible leader investments made in the present round to "less visible" follower investments made in the previous round.

3. EXPERIMENTAL PROCEDURE

We ran four experimental sessions with three groups of five subjects in each session in April and November 1999. In each session we ran both the leader and the no-leader treatment, i.e. we employed a within-subject (group) design. To control for order effects we had two sessions with first 10 rounds of the no-leader treatment and then 10 rounds of the leader treatment and two sessions in which this order was reversed. The subjects were undergraduate students from the Norwegian School of Economics and Business Administration in Bergen who were recruited from classes. No subject participated in more than one session. Subjects were told that they could earn between NOK 100 and 180 (about $12.50 to $22.50) in about one hour. They knew that rewards were contingent on performance.

Upon arrival subjects were randomly seated behind computers, which were separated by curtains. Groups were formed in such a way that subjects could not identify the other members in their group. Instructions (in Norwegian) were divided and read aloud by the experimenter (see the Appendix for an English translation of the instructions). In the instructions, we did not talk about contributions to the public bad or public good, but we talked about investments in project A or B. Subjects were encouraged to ask questions. Few questions were asked. After that the experiment started. In each treatment, all parameters of the experiment were common knowledge to all subjects. In particular, subjects knew that they would play 10 rounds in the experiment, and then another 10 rounds with a slightly different design.

Subjects made their decisions on linked computers. In the first round of the no-leader treatment, each subject decided how much of the endowment he or she wanted to invest in project A by typing the number; the remaining amount was automatically invested in project B. The total group investment, the individual investment decisions and the private payoffs were calculated and communicated to the subjects, all via the computers. Subjects were then asked to make their decisions for the next round etc.

The procedure in the leader treatment was similar except for the fact that in this treatment the leader decided first how much he or she wanted to invest in the public bad in that round. The followers were informed about the leader's decision, after which they had to make their decisions for that round. Like in the no-leader treatment, all subjects (including the leader) got feedback on the individual investment decisions of each group member, the total group investment and their private payoff via the computer.

After the last round of a session, subjects were privately paid their earnings from all rounds. Each session lasted for about one hour. Average earnings were NOK 120.2 (≈$15), including NOK 20 for showing up.

To control leader investments, we used instructed leaders, i.e. the leader in each group was not randomly selected. Rather we used young research assistants from SNF, who were placed as early arrivers behind their curtains as the followers started to arrive. The other subjects were not told about this. No comment indicated that they suspected that leaders were instructed. To the extent that some subjects still were suspicious, most probably the experiment shows too small effects of leadership, i.e. if anything the results are on the conservative side. While we are convinced that it is important to avoid manipulating subjects in experiments as much as possible[7], we saw manipulation as a last resort in our case. In a parallel experiment we did not use instructed leaders, but the randomly selected leaders did not set very good examples in these sessions (Van der Heijden and Moxnes (2000)). This becomes clear by comparing the leader investments in the two experiments. On average, non-instructed leaders invested 15.89 in the public bad while the instructed leaders invested 3.1.

These low average leader investments occurred because the leaders (or rather confederates) were instructed not to invest more than 6 tokens in project A in the leader treatment, and to

[7] The general consensus among economists seems to be that deception in experiments should be avoided. However, many economic experiments can be found that in fact do deceive participants (e.g. Weimann (1994) and Isaac and Walker (1988a)). In a recent paper, Bonetti (1998) concludes from a review of the literature on the consequences of deception that there is little support for the argument that deception should be avoided. Bonetti argues, furthermore, that there are potential gains from deception in data validity and experimental control.

vary investments over rounds to enable regression analysis. Otherwise they were asked to do whatever they felt could stimulate the others to follow suit. In the no-leader treatment, the leaders were asked to invest the last round's average of the followers. In such a way, we tried to minimize the impact of the instructed subjects on the results in that treatment. These are important instructions for our test of the main hypothesis about the total effect of leadership.

4. RESULTS

As the leaders were instructed in our experiment, we focus our attention on the behavior of the followers. First we analyze the average investments over all rounds. The tests performed are weak and conservative since we disregard the fact that the games are repeated over 10 rounds in each treatment.[8] We also look at the monetary consequences for followers and leaders. Next we look at the evolution of investments over rounds. Finally we briefly consider the data at the individual level, and we perform regressions using round data. These last results are likely to overstate the accuracy of the findings since observations of groups over rounds are not totally independent. Correct measures of accuracy are likely to lie in-between aggregate and time-series results.

4.1 Aggregate results

Tables 1a and 1b present the average investments by followers in project A (the public bad) by session (A, B, C, and D) and by group (subscripts 1, 2, and 3). Investments are averaged over rounds. Table 1a depicts the results of sessions A and D, which had first ten rounds of the no-leader treatment and then another ten rounds of the leader treatment. Table 1b shows the results of sessions B and C in which the order of the treatments was reversed. To simplify comparisons between the two tables, both tables show the no-leader treatment first, irrespective of the actual sequence used in the experiment. From the tables we can make the following three observations:

[8] It is more interesting that 10 groups follow the leader in one round than that one group follows the leader in 10 rounds.

Observation 1: On average, subjects behave as weak free riders. Investment levels are closer to the Nash prediction of 20 than to the socially efficient level (investing 0).

Observation 2: The order of the treatments does not lead to significantly different outcomes.

Observation 3: Follower investments in the public bad are significantly lower in the leader treatment than in the no-leader treatment.

Table 1a: Average investment in the public bad by followers. Order: no-leader, leader.

	Treatment	
Group	No-leader	Leader
A_1	16.55 (2.43)	14.60 (5.01)
A_2	17.75 (1.40)	16.43 (2.07)
A_3	17.98 (2.46)	4.65 (6.29)
D_1	17.00 (2.36)	17.93 (1.67)
D_2	18.03 (2.22)	17.48 (2.04)
D_3	17.60 (1.92)	13.75 (2.59)
Average	17.48 (2.14)	14.14 (5.75)

Note: standard deviations between parentheses

Table 1b: Average investment in the public bad by followers. Order: leader, no-leader.

	Treatment	
Group	No-leader	Leader
B_1	17.93 (1.92)	16.18 (2.49)
B_2	19.05 (1.53)	14.88 (2.60)
B_3	17.53 (3.01)	14.95 (2.95)
C_1	17.25 (2.35)	16.38 (2.66)
C_2	17.25 (1.68)	16.15 (2.23)
C_3	14.33 (2.67)	13.75 (2.59)
Average	17.22 (2.60)	15.38 (2.66)

Note: standard deviations between parentheses

The first observation is in line with the findings of other public good and public bad studies (e.g. Weimann (1994), Andreoni (1995), Burlando and Hey (1997)). In particular, the average level of cooperation in our no-leader treatment is 13%, which is similar to the 16% found by Andreoni (1995) in his public bad experiment and to the 13%, Burlando and Hey (1997) found for the English subjects in the partners treatment of their public bad experiment (but distinct from the 29% they obtained for the Italian partners.)

Observation 2 states that the average investment by the followers is not affected by the order of the treatments. For the no-leader treatment, there is no significant difference between the average investments in the sessions in which the no-leader treatments comes first (sessions A and D) or second (sessions B and C): a non-parametric Mann-Whitney U test with group averages as units of observation results in $p=0.75$ ($n_1=6$, $n_2=6$). Similarly, for the leader treatment we cannot reject the hypothesis that the average investment over the first ten rounds is equal to the average investment over the second ten rounds ($p=0.26$, $n_1=6$, $n_2=6$). These results suggest that we can pool the data of all sessions.

The most important result is Observation 3, which states that the average investments by followers in the public bad are significantly lower when there is a leader than when there is no leader. Considering all sessions, a non-parametric Wilcoxon test with group averages as units of observation reveals that this difference is statistically significant ($p=0.01$, $n_1=12$, $n_2=12$). The average follower investments in the public bad are about 15 percent lower when there is a leader compared to a situation without a leader (i.e. 17.35 versus 14.76). Looking at the individual groups, we find that in 11 groups the average investments are lower when there is a leader, and in one session they are a bit higher. On the basis of Observation 3 we can reject Hypothesis 1 that there is no effect of a leader.

Next, it is pertinent to look at the monetary consequences. Do unilateral reductions lead to higher total welfare? It turns out that the average sum of the payoffs of the followers and the leaders is 227.5 in the no-leader treatment versus 273.3 in the leader treatment. Using Wilcoxon tests with group averages as units of observations it turns out that this difference is highly significant ($p<0.01$, $n_1=12$, $n_2=12$). Obviously, total welfare is increased when there is a leader who sets the good example. This apparent positive outcome is accompanied by a negative effect for the leaders themselves, though. While the followers earn on average significantly more in the leader treatment than in the no-leader treatment (61.9 versus 45.8, a

Wilcoxon test on the differences gives $p<0.01$, $n_1=12$, $n_2=12$), leaders have to pay the costs for their good behavior by earning significantly less in the leader treatment (25.8 versus 44, $p<0.02$, $n_1=12$, $n_2=12$). We can summarize this result as Observation 4:

Observation 4: Total payoffs are significantly higher in the leader treatment than in the no-leader treatment.

4.2 Developments over rounds

Further results can be obtained if we look at the development over time. To that end, Figures 1a and 1b show the evolution of the average investment in the public bad. Figure 1a depicts the average investment in the public bad by the followers in each round for the case when the order of the treatments is no-leader, leader. The leader treatment is shown by the solid line and the no-leader treatment by the dashed line. Figure 1b gives an analogous picture for the sessions with the reversed order: leader, no-leader. Note that each data point represents the average of 24 investment decisions. As can be seen, the average investments in the leader treatment are lower than in the no-leader treatment with the exception of one round in both cases.

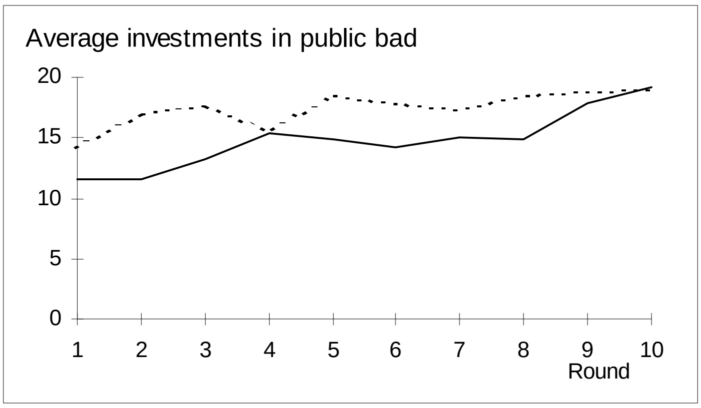

Figure 1a: Average investments in the public bad by followers. Treatment order: no-leader (dashed line); leader (solid line), sessions A and D.

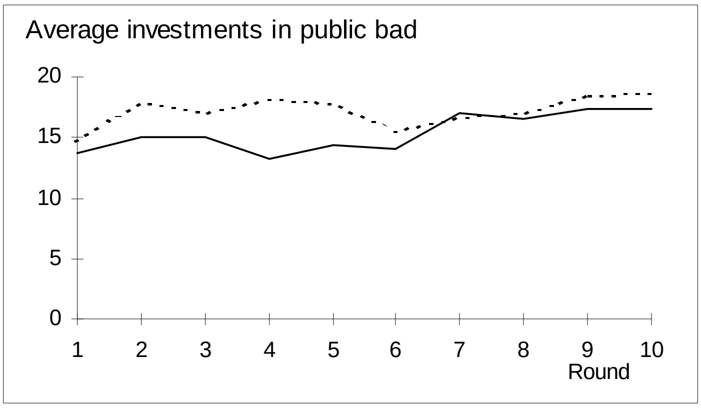

Figure 1b: Average investments in the public bad by followers. Treatment order: leader (solid line); no-leader (dashed line), sessions B and C.

The patterns observed in Figures 1a and 1b correspond to the typical (public good and public bad) finding that cooperation decreases over time. Furthermore, when the game is restarted with the second treatment, average investments in the public bad in the first round after the restart decrease significantly relative to the last round before the restart. This feature is independent of the order of the treatments[9], and resembles the so-called restart effect, which is often observed when a game is repeated with the same treatment.[10]

Figures 1a and 1b also indicate that the absolute effect of leadership decreases over rounds[11], as indicated by the distance between the two curves. The relative effect of leaders seems to be sustained over time, though. By relative effect we mean the increase in investments in project B caused by having a leader rather than having no leader, divided by the average investment

[9] When the no-leader treatment comes first, the difference is significant at $p < 0.03$. When the leader treatment comes first the level of significance is $p < 0.04$.
[10] Evidence for restart effects can be found in several public good and public bad experiments (e.g. Andreoni (1988), Croson (1996), Croson (1997), Burlando and Hey (1997), and Van der Heijden and Moxnes (1999)). Although a restart effect usually refers to a situation in which an additional number of rounds of the same game or treatment are played after a restart, in our experiment the set-up was slightly different in the second set of rounds (as in Burlando and Hey (1997)). In this respect, it would perhaps be better if we talked about behavior in the first and second treatment instead of before and after the restart. See Van der Heijden and Moxnes (1999) for a further discussion.
[11] Wilcoxon tests on the differences in the average follower investment per round by group on the pooled data reveal that in six of the ten rounds the average investments are significantly higher in the no-leader treatment than in the leader treatment. In the last four rounds the difference is only once marginally significant.

in project B in the leader treatment.[12] The percentages are shown in Figure 2 for the two orders of treatments. Simple OLS regressions show that there is no time trend in any of the two curves.

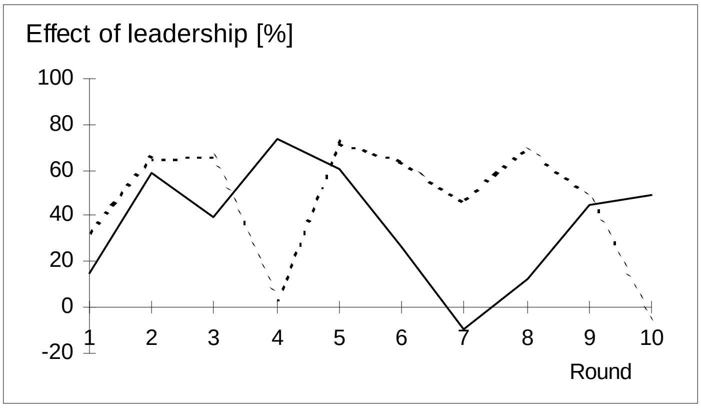

Figure 2: Relative effect of leadership. Treatment order: Leader first (solid line), no-leader first (dashed line).

4.3 A closer look at the individual data

Figure 3 uses pooled data of all sessions and all individual followers to depict the distribution of the investments by the followers. Clearly, the modal class is the investment interval 16-20, but in the leader treatment the frequency of these investments is 10 percentage points less than in the no-leader treatment. Furthermore, it can be observed that very low investments, i.e. in the interval 0-5 occur more frequently in the leader treatment; the difference is 12 percentage points.[13]

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[12] The exact formula is $100\,(\bar{x}_t^{B,L} - \bar{x}_t^{B,N}) / \bar{x}_t^{B,L}$ where $\bar{x}_t^{B,L}$ is the average investment in project B in round t in the leader treatment, and $\bar{x}_t^{B,N}$ is the average investment in project B in round t in the no-leader treatment.

[13] Some information about the dispersion of the the investments can also be derived from the standard deviations, reported in Tables 1a and 1b. Wilcoxon tests on the pooled data confirm that investments are more spread in the leader treatment: the hypothesis that the standard deviations are equal in the two treatments can be rejected at the 5% level.

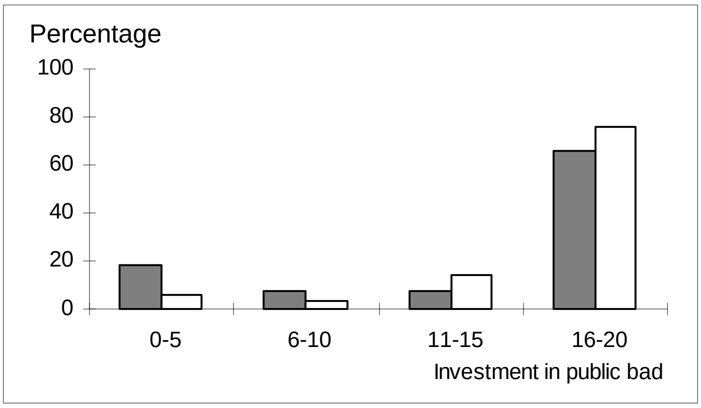

Figure 3: Frequency distribution of investments in the public bad by the followers in both treatments. Gray: Leader treatment; blank: no-leader treatment.

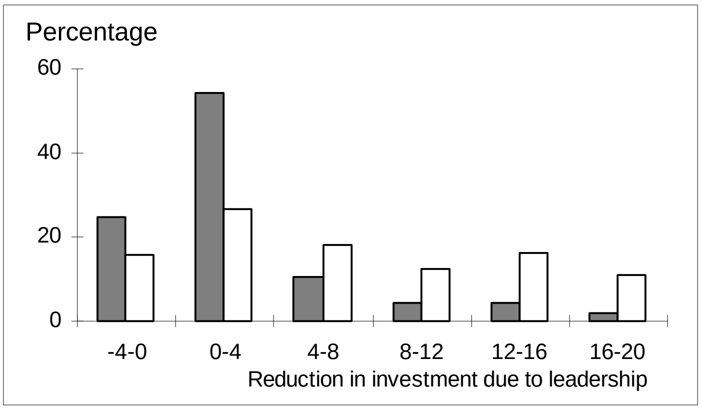

Figure 4: Distribution of the effect of leadership over individuals. The effect is measured by the reduction in average investments in the public bad over rounds due to leadership. Gray: number of subjects; blank: contribution to the total effect of leadership.

However, from this information it is still not unequivocal whether the lower level of investments in the leader treatment is caused by the fact that all subjects contribute somewhat less, or that some subjects contribute considerably less in this treatment. To investigate this point further, Figure 4 depicts the distribution of the reduction in average investments in the public bad due to leadership. The gray bars denote the percentage of subjects that fall into the different categories for reductions. The blank bars denote the percentage contribution of the subjects in each category to the total reduction in investments. Clearly, there is a considerable effect of the few that make large reductions due to leadership. However, the major contribution still comes from the subgroup that makes reductions in investments in the range from 0 to 4. We also note that some subjects increase their investments in the public bad when a leader is present. Eight of these cases take place when the leader treatment comes last, four when it comes first.

Next we run a simple OLS regression including most of the factors analyzed separately above, however using round by round data. All the time-series data are pooled. As before, our main interest is in the effect of leader investments on follower investments. The dependent variable is therefore a vector consisting of the average follower investment in the public bad $x_{i,t}^F$ for group *i* in round *t*. We then estimate the following model

$$x_{i,t}^F = \alpha_0 + \alpha_1 L_i + \alpha_2 L_i x_{i,t}^L + \alpha_3 R_i + \alpha_4 (1 - R_i) t + \alpha_5 R_i t + e_{i,t} \qquad (3)$$

where the dummy variable L_i takes the value 1 if it concerns the leader treatment (and 0 otherwise), $x_{i,t}^L$ is the investment in the public bad by the leader in the same round, and the dummy variable R_i takes the value 1 if it concerns rounds after the restart, i.e. in the second treatment of a session (and 0 otherwise). To allow for different time trends in the two treatments we include two variables for the round. In accordance with the instructions to the leaders, we observe no significant time trend in any of the leader investments. This should give sharper estimates since multicollinearity is avoided, i.e. there is no correlation between $x_{i,t}^L$ and *t*. The regression results are depicted in Table 2.[14]

The results confirm the previous findings. There is a highly significant upward trend in investments over rounds, as seen in Figures 1a and 1b. This trend is practically the same in the first and second treatment of a session, as witnessed by the fact that the estimates of α_4 and α_5 are virtually identical. The restart dummy is not significant. Together these findings confirm our claim that the order of the treatments is not important, i.e. the behavioral pattern

[14] The regression results seem robust to the specification chosen. For instance, they do not change when fixed effects for the groups are included or when a forward or stepwise selection procedure is used.

in the leader and in the no-leader treatment does not depend on the order of the treatments within a session.

Table 2: Estimation results for average follower investments in the public bad.

Variable	Symbol	Coefficient value	p-value
Constant	a_0	15.20	0.000
Effect of leader, constant	a_1	-3.56	0.000
Effect of leader, slope	a_2	0.42	0.005
Restart, constant	a_3	-0.46	0.630
Round effect first treatment	a_4	0.44	0.000
Round effect second treatment	a_5	0.45	0.000

Note: Adjusted R^2=0.21, number of observations *n*=240.

The effect of a leader is highly significant as indicated by the estimates of α_1 and α_2.[15] As expected, the p-value of α_1, showing the strength of the mere effect of having a leader, is smaller than in the previous test, which was based on averages. Most interesting is the new information coming out of the regression, namely that the effect of leadership also depends on the size of leader investments. When the leader reduces his or her investment by one token, the followers on average reduce their investments by 0.42 tokens, implying a total follower reduction of $4\times0.42=1.68$ tokens. Or, put differently, 42 percent of the leader variations in investments are imitated by the followers.

Finally, we will use time-series data to explore the question of what characteristics of leadership are (most) important for the leader effect. In particular, we want to see if low previous round follower investments have the same effect on follower investments in the present round as low present round leader investments. To estimate the effect of low follower investments, we add the dummy variable $F_{i,t-1}$ to the model of equation (3). $F_{i,t-1}$ takes the value 1 if at least one follower in group *i* invested less than or equal to 15 in the previous

[15] The estimates of the effect of leadership are virtually identical when we add fixed effects for the groups (α_1 changes from –3.56 to –3.51 and α_2 from 0.42 to 0.41). Furthermore, both leadership effects remain if we add the average follower investment of group *i* in round *t-1* as an explanatory variable. Although the estimated coefficient of this variable is significantly positive (0.26), it basically makes the round effects insignificant, whereas the leadership effects remain highly

round, and 0 otherwise. The following model is estimated:

$$x^F_{i,t} = \beta_0 + \beta_1 L_i + \beta_2 L_i x^L_{i,t} + \beta_3 R_i + \beta_4 (1 - R_i)t + \beta_5 R_i t + \beta_6 F_{i,t-1} + \varepsilon_{i,t} \qquad (4)$$

The regression results are depicted in Table 3.[16] The estimated coefficient for the effect of follower investments in the previous round is 0.13, but highly insignificant with a p-value of almost 0.60. The other estimates are very similar to the earlier estimates as depicted in Table 2. This indicates that low last round investments by followers do not have the same effect as leader investments. Still one might question this result on the ground that average follower investments are on average larger than the average instructed leader investments. The experiment we carried out with non-instructed leaders (Van der Heijden and Moxnes (2000)), who invested at least as much as the selected followers here (namely on average 15.89), provides an interesting reference for the effect of variations in leader investments. A sharp estimate for the slope of 0.28 was found (see results in Table 2 in that paper for the "same-costs-treatment" in Van der Heijden and Moxnes (2000)). A similar result should have appeared here if last round follower investments had the same effect as leader investments.

Table 3: Estimation results for average follower investments in the public bad, including the possible effect of a good follower.

Variable	Symbol	Coefficient value	p-value
Constant	β_0	14.98	0.000
Effect of leader, constant	β_1	-3.57	0.000
Effect of leader, slope	β_2	0.42	0.005
Restart, constant	β_3	-0.41	0.670
Round effect first treatment	β_4	0.44	0.000
Round effect second treatment	β_5	0.45	0.000
Effect of previous round follower	β_6	0.13	0.578

Note: Adjusted R^2=0.21, number of observations *n*=240.

significant (α_2 =-3.15, p=0.000 and α_1=0.47, p=0.000). The effect of leadership is thus very robust.

[16] Almost one quarter of all follower investments (225 out of 960 observations) of the pooled data is less than or equal to 15. The estimation results do not depend on the chosen investment level of 15; i.e. regressions with other cut-off values yield the same results. We have also performed regressions with a dummy variable indicating the number of followers with investments less than or equal to a certain level as explanatory variable, but, again, this does not affect the results. Also regressions that include the investments of those followers with average investments below 16 as explanatory variables show no significant effect of last round follower investments.

4.4 Discussion

Figure 5 summarizes the effect of leadership on follower investments in the public bad as follows from the regression on pooled data, see Table 2. The dashed line represents the average investments for the no-leader treatment, 17.35 tokens. In the case of leadership (solid line), follower investments are set 3.56 tokens below the no-leader investments when the leader investment equals zero. At the average leader investment, 3.10 tokens, followers invest 2.26 tokens less than in the no-leader case. This is close to the difference of 2.59 (=17.35-14.76) found earlier using average investments over rounds.

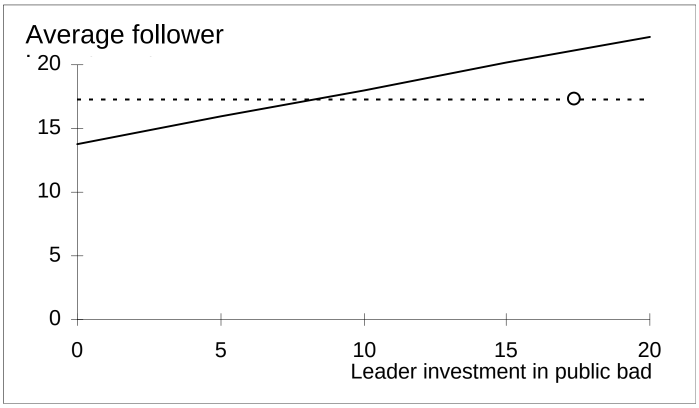

Figure 5: Average follower investments as a function of leader investments in the public bad (solid line) and the average investments without a leader (dashed line). The circle denotes the point where leader and follower investments equal the average investments in the case without a leader.

Recall that leaders were instructed to keep investments in the public bad equal to or below 6 tokens. Hence, the experimental data are only representative for this range. If in spite of this, the reaction curve is extrapolated beyond the region where it applies, we see that follower investments increase above the no-leader investments for leader investments above 8.5 tokens. It seems likely that the curve should come close to the small circle in the figure, where leader and follower investments equal the average investments in the case without a leader. When it does not, it seems natural to propose that the effect of leadership is non-linear, with almost no effect at moderate and high leader investments in the public bad. However, the results of a parallel experiment by Van der Heijden and Moxnes (2000) suggest that there exists also a small effect of leadership at moderate leader investments in the public bad. They argue that the two results could be seen as consistent if the effect of leadership has two components, one involving considerations of fairness (with a limited potential with respect to reductions in follower investments), and another involving imitation or learning.

Our main message here is that leadership matters. What do the tests say about the importance of leader characteristics? Clearly the level of investment is important, i.e. the example must be good. Next, the results suggest that an "anonymous" example set by a follower in the last round is of little importance compared to a visible example set by a leader in the present round.[17] What is more important of visibility and timing we cannot say anything about.

For further research, the following experimental designs could provide more conclusive evidence about the importance of leader characteristics. An experiment where confederates choose low investments in both the leader and the no-leader treatments will give better data about the difference between "anonymous" last round followers and visible present round leaders. These data would enable us to derive whether followers are responding to the low investments of the leader in the same round or whether they are just reacting to the low choice signal. To separate the characteristics of visibility and timing, the new experiment could also be carried out with heightened visibility of the confederate follower – it seems not possible to make the leader "anonymous" since his or her investment has to be communicated separately as a new round starts.

[17] Data from a previous public bad study (Van der Heijden and Moxnes (1999)) lend some additional support for this conclusion. In particular, the data suggest that having one (or two) player(s) showing cooperative behavior did not induce other players to reduce investments as well. For example, although in one group three players observed that two players were making very low contributions, they were not following this good behavior by lowering their investments.

5. CONCLUSIONS

With regard to global or regional environmental problems, do countries that take unilateral actions inspire other countries to curtail emissions as well? Hoel (1991) has shown that in a noncooperative situation, unilateral actions will both improve global welfare and lead to lower total emissions. Hoel explicitly leaves out the possibility that unilateral actions “might lead to similar behavior from other countries”. It is exactly this question that has been addressed in this paper. We have investigated this issue by conducting a novel public bad experiment with a leader. Twelve groups of five subjects played the game twice, once with a leader and once without a leader. In the no-leader treatment, all subjects make their decisions simultaneously, while in the leader treatment the leader decides first after which the followers decide simultaneously. Our set-up allows for within-subject (group) comparisons. The order of the two treatments was varied over groups.

The no-leader treatment leads to the same type of behavior found in earlier studies of public bad games: subjects behave as weak free riders, closer to the Nash equilibrium than to the social optimum, and the level of cooperation decreases over rounds. There is no significant effect of the order of the treatments. The new and interesting finding is that there is a small but significant effect of a leader setting the good example. On average, investments in the public bad are lower when there is a leader, and the effect on the followers is stronger the more the leaders cut back on their investments in the public bad. Hence we reject the null hypothesis that leadership does not affect followers’ behavior. Furthermore, the presence of a leader is conducive to higher payoffs: total payoffs are significantly higher in the leader treatment than in the no-leader treatment.

A regression analysis shows that the level of investment is important, i.e. the example must be good. Next, the results suggest that an "anonymous" example set by a follower in the last round is of little importance compared to a visible example set by a leader in the present round. What is more important of visibility and timing we cannot say anything about. Hence further research is needed to get more precise indications about what characteristics of leadership are important for the effect of leaders.[18]

While the experiment demonstrates that an effect of leadership should be expected, one should be careful in applying the exact numbers to given environmental problems. The only point we want to make here is that the effect is not necessarily negligible. On average, the leader induces a 15 percent reduction in average follower investments in the public bad.

While the effect we have measured is likely to reflect some common trait of human decision making, other factors may also be important. In laboratory experiments, contributions to public goods are found to increase with identification of and eye contact with other players (Bohnnet and Frey (1995)), and there are positive effects of free and costly communication (see e.g. Isaac et al. (1985), Isaac and Walker (1988b) and Isaac and Walker (1991)). In reality, one should also expect effects of NGOs (environmental groups and polluter organizations), environmental costs, abatement costs, income levels etc. The way, in which these factors influence the effect of leaders in regimes with competition or with negotiations, are questions for further research.

[18] It is not exactly clear why followers are reacting to the good example. It might be that they feel a kind of moral obligation toward the leader since he or she is putting in large effort. Also other motivations could play a role, like fairness, inequity aversion, and altruism (see references in the introduction). We do not want to discuss these issues here. First we wanted to establish whether there is any effect of leadership; a next step could be to see why such an effect exists.

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APPENDIX: INSTRUCTIONS FOR THE EXPERIMENT

This appendix contains an English translation of the instructions for the public bad experiment without a leader.

Welcome

This is an experiment to investigate investing behaviour. The instructions are simple. If you follow them carefully, you can earn a considerable amount of money. Earnings will be paid in cash immediately after the experiment. In addition, you receive NOK 20 for showing up. The money has been supplied by the Norwegian Research Council.

In the experiment you first have to make 10 investment decisions, one in each round. After that we will change the design slightly and there will be 10 additional decision rounds. The payoffs of all 20 rounds determine your total earnings.

During the experiment you belong to a group together with four other subjects, which are the same persons all the time. Your payoffs will depend on your own decisions and on what the other four members of your group decide. The design is such that nobody is able to find out which persons belong to a group and what decisions persons have made. In other words, you are anonymous.

Investments and payoffs

In each round, you (and all others in your group) can invest an endowment of NOK 20. So, in total, NOK 100 per round is invested. You can invest in two different projects: project A and project B. You write down how much you want to invest in project A, the rest of your endowment is then automatically invested in project B. Project A yields a direct payoff of NOK 0.70 per crown invested. Note, however, that in addition to the payoff for yourself, investing in project A yields an additional cost of NOK 0.10 per crown invested for you and the other four members in your group. Similarly, investments in project A by the other members gives a cost for you and the other subjects. Project B yields a direct payoff of NOK 0.40 per crown invested. Investments in project B have no (indirect) impact on the payoffs for others. All persons in a group are in the same position.

To make it easier to see the consequences for your payoffs of the investment decisions made by you and the others, we have computed the payoffs for several combinations. To limit the size of the table we only mention investments in steps of 5. However, you can use all integers from 0 up to and including 20 when you choose your investment in project A.

An example shows how the numbers in the table are computed. Assume that you invest NOK 10 in project A. Then you receive a direct payoff of NOK 7 from project A. The rest of your endowment, NOK 10, is automatically invested in project B, and yields a payoff of NOK 4. Together this generates a direct payoff of NOK 11. Assume furthermore that the other four persons in your group invest on average NOK 5 in project A. That gives a total investment of NOK 20 in project A for these four persons. Together with your own investment of NOK 10 this gives a total investment in project A of NOK 30. This yields a cost of NOK 3 for you (and for the others). A direct payoff of NOK minus a cost of NOK 3 gives your own payoff of NOK 8.

Table to calculate your own payoff per round

Average investment in project A by others	Your own investment in project A				
	0	5	10	15	20
0	8	9	10	11	12
5	6	7	8	9	10
10	4	5	6	7	8
15	2	3	4	5	6
20	0	1	2	3	4

Practical design

Every round you receive a form on which you write down how much you want to invest in project A (integers from 0 up to and including 20). The forms are collected by the experimenters. For each round after the first one, on this form you get also information about the previous round: your own payoff , the investment in project A by each member of your group, and the total investment in project A by all persons in the group (at most NOK 100 per round). You can show that you have made your decision by putting your form upside down on your desk. After 10 rounds we inform you about the changes in the rules for the last 10 rounds.

Do not communicate with the other participants and try to avoid the possibility that others see your form!

You can now ask questions for clarification.

Good luck.