REPORT NO. 15/00

The effect of leadership in a public bad experiment

by

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SNF-Project No. 4375
Spredning av energi- og miljøpolitikk

The project as been funded by the Research Council of Norway
under the SAMRAM program

FOUNDATION FOR RESEARCH IN ECONOMICS AND BUSINESS ADMINISTRATION
Bergen, May 2000

ISBN 82-491-0042-5
ISSN 0803-4036

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FOREWORD

This is a report in a series dealing with diffusion of energy and environmental policies and effects of international leadership. Eline van der Heijden works at CentER at Tilburg University. The project is funded by the SAMRAM program of the Research Council of Norway.

1. INTRODUCTION

Global and regional environmental problems can be described as commons problems. Prominent examples 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. This situation calls for some sort of cooperation. In this report we investigate whether cooperation is encouraged by countries that take a leadership role and make unilateral emission reductions.

Our focus is on the competitive case. Will unilateral reductions by a leader, for instance a small country, lead to lower emissions by other nations? 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.

One aspect of the question we pose has been studied by the use of game theory, Hoel (1991). Hoel assumes that other nations than the leader choose policies dictated purely by their self-interest, and he seeks Nash equilibria of noncooperative games and the Nash bargaining solution in case of cooperation. In the noncooperative case he finds that unilateral actions lead to lower total emissions and increased total welfare. In the cooperative case, total emissions could go in either direction depending on the parameters of the problem. In case the leader is a small country, the effects become negligible.

Our focus is on other effects of unilateral reductions than those studied by Hoel. He explicitly states that he will not consider the possibility that unilateral actions by one nation "might lead to similar behavior from other countries", p.56. His choice is consistent with his assumption about individuals maximizing pure self-interest (in a narrow sense only considering direct economic effects of changes in emissions). However, both the assumption about narrow self-interest and the one about maximization are debatable. Laboratory experiments of public good and public bad games typically show deviations from Nash equilibria, Ledyard (1995). These could be caused by violations of one or both assumptions. First, consider utility functions[1]. Even within a broad set of utility functions based on self-interest, desires to be perceived as good, fair, innovative, clever etc. could stimulate leaders as well as followers to “unilateral reductions”.

Second, consider maximizing behavior. It is not obvious that actors have the appropriate information about themselves (e.g. costs, for which widely differing estimates exist), about others (e.g. their costs, utility functions, and beliefs), and that they are able to deduct maximizing strategies from such information. Discussing lessons from evolutionary game theory, Mailath (1998) points to the likely importance of learning by imitating observed successful behavior by others. He also points out that this perspective is relevant for the development of norms and conventions. Thus it seems pertinent to speculate whether a leading nation taking unilateral actions could come to influence directly the strategies of others. For instance environmental interest groups are likely to point to leading nations as having “successful strategies”.

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. The traditional design is altered in that in each round a leader makes his or her decision before the others, and then this decision is 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. Thus by design, we avoid potential confusion with the interior solutions found by Hoel for the competitive case[2]. Each group of participants get to play the game with and without a leader, enabling within-subject (group) comparisons. The experiment is not designed to reveal whether possible deviations from the Nash solution are caused by individuals applying utility functions which take account of more factors than the given monetary incentives or whether they fail to apply maximizing strategies.

As far as we know, no public good or public bad experiment has thus far been designed to investigate the impact of a leader. I.e. we have seen no other leader-follower experiment with a decision protocol that is mixed sequential-simultaneous. A major reason for choosing this design is that any nation is in principle free to pursue a leadership role

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1 Recently, several theories have been advanced in which considerations of fairness, altruism, reciprocity, equity et cetera are incorporated (see e.g. Fehr and Schmidt (1999), Levine (2000), Rabin (1993) Dufwenberg and Kirchsteiger (1998), and Bolton and Ockenfels (2000)).

2 The internal solution is not a problem in itself. Keser (1996) finds that contributions in a public good experiment with an interior Nash equilibrium are similar to those in a corner solution public good experiment.

within a region or on a global scale. Other, purely sequential game designs[3], assume that actors make decisions in an exogenously determined order. 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 nations make such decisions, nor can such an order be forced on nations. A good reason for nations 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[4] and in sequential common pool resource games[5]. In our design, theory holds that there is neither a first mover advantage nor a last mover disadvantage.

The next section outlines the hypothesis and describes the public bad game. Section 3 describes the experimental procedure. Section 4 presents the main results with 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, and we find that the order of the treatments is not important. The last section contains a concluding discussion and gives some lines for further research.

3 See e.g. Budescu et al. (1995), Erev and Rapoport (1990), Morris et al. (1995), and Rapoport (1993).

4 See Erev and Rapoport (1990).

5 E.g. Budescu et al. (1995).

2. HYPOTHESIS AND EXPERIMENT

Our focus is on the effect of a leader in a public bad game. As mentioned in the introduction, we will use an experiment which has a corner solution for the Nash equilibrium. Assuming selfish (in a narrow sense including only direct costs and benefits) and maximizing subjects, we should expect that there is no effect of a leader. Hence our null hypothesis reads:

H_0: *There is no effect of a leader in a public bad game with a Nash corner solution*

When we want to test this hypothesis it is because of the objections to standard assumptions raised in the introduction. We are also inspired by empirical findings in public good and public bad experiments of behavior that deviates from Nash predictions. The same mechanisms that cause these deviations could also be important for the effects of a leader. The hypothesis will be tested both with respect to average effects over many rounds and with regard to effects from round to round.

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 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 also has a negative external effect: each token invested in project A yields a negative return of 0.1 to all group members. So, payoff π_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

$$\pi_i = 0.7 x_i^A + 0.4 x_i^B - 0.1 \sum_{j=1}^{5} x_j^A \tag{1}$$

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

$$\pi_i = 8.0 + 0.2x_i^A - 0.1\sum_{j\neq i} x_j^A \qquad (2)$$

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, which gives a total investment of 100 tokens in project A and a payoff π_i = 4. However, higher payoffs can be obtained if subjects invest in project B. If all members of a group decide to invest the total endowment in project B, i.e. x_i^A = 0, 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[6].

The parameters of the game are summarized by the marginal per capita return, MPCR[7]. Since all players face the same MPCR, the leadership effects we find are for that particular set of parameters. In a public good game, Isaac and Walker (1988b) find that contributions vary with MPCRs. This indicates that our choice of MPCR could influence the magnitude of the leader effect. Our game has a MPCR of 0.33, which is similar to the MPCR used by Andreoni (1988) in his public bad game, 0.5. In other words, the parameters (costs) 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). If all others have invested selfishly (x_j^A = 20), the 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

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6 The outcome x_i^A = 0, 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.

7 Using Equation 1 we find: $MPCR = \dfrac{d(0.1\sum_{j=1}^{5} x_j^A)/dx_i^A}{d(0.7x_i^A + 0.4(20 - x_i^A))/dx_i^A} = \dfrac{0.1}{0.3}$.

efficient state, marginal costs of the two investment options are equal. Hence, to begin with, there is nothing to gain by acting selfishly.

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 the aggregate investments via their own payoff, whereas in our control treatment subjects get feedback on the individual decisions of their group members. This has been done to avoid asymmetries between the leader, for which the investment has to be revealed, and the others. An experiment indicates that there is no significant effect of this extra information on average investments, Van der Heijden and Moxnes (1999).

In the leader treatment, one subject in each group is a leader. The leader decides first on his or her investment. This decision is then 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 players in both treatments are given by Equation 1. Thus, the main difference between the 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.

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 students from the Norwegian School of Economics and Business Administration 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 in about one hour. They knew that rewards were contingent on performance.

Upon arrival subjects were randomly seated behind computers such that groups were formed in a random way. The computers were separated by curtains, and the subjects could not identify the other members in their group (members of all groups were mixed). Groups remained the same during the session (the partner design by Andreoni (1988)). Instructions (in Norwegian) were divided and read aloud by the experimenter[8]. 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. Subjects knew that they would play 10 rounds in the experiment, and then another 10 rounds with a different design.

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; the remaining amount was automatically invested in project B. The total group investment and the private payoffs were calculated and communicated to the subjects together with the individual investment decisions. 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 first decided how much he or she wanted to invest in the public bad in that round. The followers were informed about this decision, after which they had to make their decision for that round. Like in the no-leader treatment, subjects got feedback on the individual investment decisions of all group members, the total group investment and their private payoff.

8 An English translation of the instructions is available upon request from the authors.

After the sessions, subjects were privately paid their earnings from all rounds. Each session lasted for about one hour. Average earnings were NOK 120.2, including NOK 20 for showing up.

To control leader investments, we used instructed leaders. That is, 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, the experiment should show too small effects of leadership, i.e. it should be conservative. While it seems important to avoid manipulating subjects in experiments[9], we saw manipulation as a last resort in our case. In a parallel experiment we did not use instructed leaders, and the endogenous leaders did not set very good examples, Van der Heijden and Moxnes (2000).

The leaders were instructed not to invest more than 6 tokens in project A in the leader treatment, and to vary investments over rounds to enable regressions. 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. Thus we tried to minimize the impact of the instructed subject on the results in that treatment.

9 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.

4. RESULTS

As the leaders were instructed, we focus on the behavior of the followers. First we analyze averages over rounds. The tests performed are weak and conservative since we disregard the fact that the games are repeated over 10 rounds in each treatment. Next we perform regressions over all rounds. These are likely to overstate the accuracy of the same findings since observations of groups over rounds are not totally independent observations[10]. Correct measures of accuracy are likely to lie in-between the two results.

Tables 1 and 2 present the average investments by followers in project A (the public bad) by session (A, B, C, and D) and by group (1, 2, and 3). Investments are averaged over rounds. Table 1 depicts the results of sessions A and D , which had first ten rounds of the no-leader treatment and then, after a restart, another ten rounds of the leader treatment. Table 2 shows the results of sessions B and C in which the order of the treatments was reversed. From these tables we can make the following three observations[11]:

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

Group	Treatment No-leader	Leader
A_1	16.55 (4.45)	14.60 (7.53)
A_2	17.75 (5.04)	16.43 (6.39)
A_3	17.98 (3.77)	4.65 (6.82)
D_1	17.00 (4.46)	17.93 (4.18)
D_2	18.03 (3.03)	17.48 (4.96)
D_3	17.60 (4.22)	17.60 (5.28)
Average	17.48 (2.14)	14.78 (5.89)

Note: standard deviations between parentheses

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

11 To simplify comparisons of the two tables, both tables show the no-leader treatment first, irrespective of the sequence used in the experiment.

Table 2: Average investment in the public bad by followers. Order: leader, no-leader.

Group	Treatment No-leader	Leader
B_1	17.93 (5.18)	16.18 (6.68)
B_2	19.05 (3.37)	14.88 (7.47)
B_3	17.53 (5.94)	14.95 (7.65)
C_1	17.25 (5.76)	16.38 (5.45)
C_2	17.25 (4.84)	16.15 (5.31)
C_3	14.33 (8.61)	13.75 (8.21)
Average	17.22 (2.60)	15.38 (2.66)

Note: standard deviations between parentheses

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.

The first observation is in line with the findings of other public good and public bad studies (e.g. Weimann (1994), Andreoni (1995)).

Observation 2 states that the investment decisions made by followers are 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 the average investment decisions are not affected by the fact that they were made before and after the restart ($p=0.26$, $n_1=6$, $n_2=6$). These results suggest that we may 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 investments in the public bad are about 13 percent lower when there is a leader compared to a situation without a leader (i.e. 17.35 versus 15.07). Looking at the individual groups, we find that in 10 groups the average investments are lower when there is a leader, in one session they are the same, and in one they are a bit higher.

Further results can be obtained if we look at the development over time. To that end, Figure 1 depicts the average investment in the public bad by the followers in each round for the case when the order of the treatments is leader, no-leader. The leader treatment is shown by a solid line and the no-leader treatment by a dashed line. Figure 2 gives the same picture for the sessions with the reversed order: no-leader, 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.

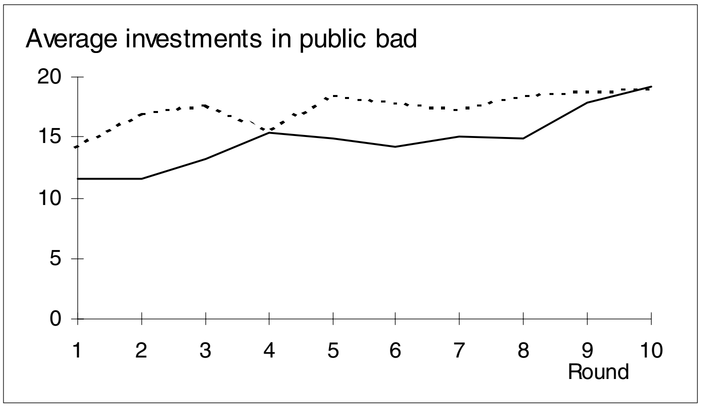

Figure 1: Average investments in the public bad by followers. Order: No-leader (dashed line); leader (solid line), sessions A and D.

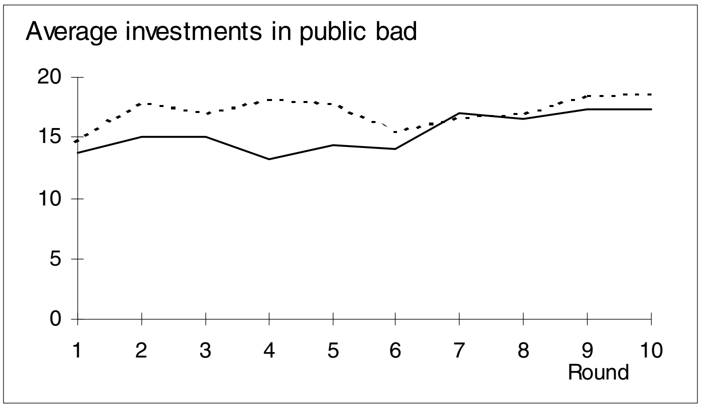

Figure 2: Average investments in the public bad by followers. Order: Leader (solid line); no-leader (dashed line), sessions B and C.

The typical (public good) finding of decreasing cooperation over time is also found here, i.e. investments increase over rounds. When the game is restarted with the second treatment, average investments in the public bad in the first round after the restart are significantly lower than in the last round before the restart[12]. The effect is independent

[12] 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 $p < 0.04$.

of the order of the treatments. The effect resembles the so-called restart effect observed when a game is repeated with the same treatment[13].

Figures 1 and 2 also indicate that the absolute effect of leadership decreases over rounds, as indicated by the distance between the two curves. The relative effect of leaders seems to be sustained over time. By relative effect we mean the extra investments in project B caused by having a leader rather than having no leader, divided by the total investments in project B when having a leader[14]. The percentage is shown in Figure 3 for the two treatment orders. OLS regressions show that there is no time trend in any of the two curves.

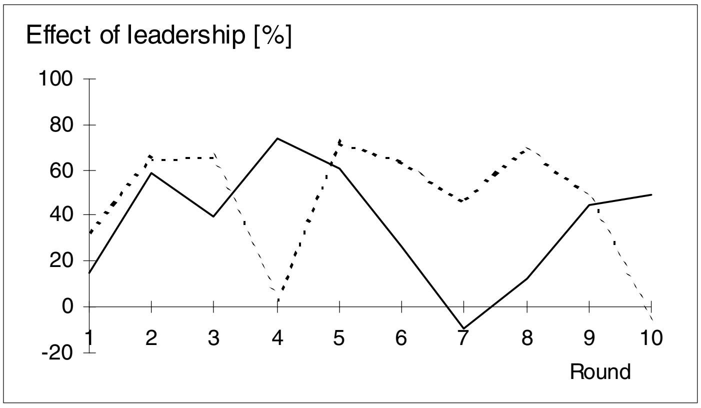

Figure 3: Relative effect of leadership. Order: Leader first (solid line), no-leader first (dashed line).

Next, we want to compare the frequency distributions of the investments in the no-leader and leader treatment. Figure 4 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 much more frequently in the leader treatment; the difference is 12 percentage points.

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13 E.g. Andreoni (1988), Croson (1996), and Croson (1997).

14 The exact formula is $100\,(\overline{x}_t^{B,L} - \overline{x}_t^{B,N})\,/\,\overline{x}_t^{B,L}$ where $\overline{x}_t^{B,L}$ is the average investment in project B in round t when having a leader, and $\overline{x}_t^{B,N}$ is the average investment in project B in round t when having no leader.

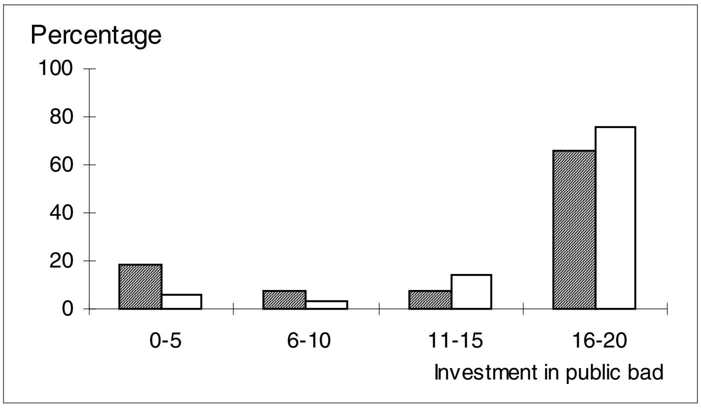

Figure 4: Frequency distribution of investments in public bad by the followers in both treatments. Gray: Leader treatment; blank: no-leader treatment.

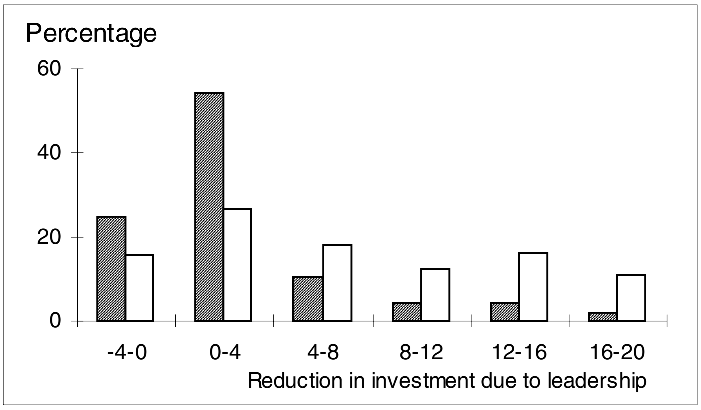

Figure 5: Distribution of the effect of leadership over individuals. The effect is measured by the reduction in average investments in public bad over rounds due to leadership. Gray: number of subjects; blank: contribution to the total effect of leadership.

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. To investigate this point further, Figure 5 shows 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.

We conclude the result section by using a simple OLS regression including most of the factors analyzed separately above, however using data by round. Our main interest is in the effect of leader investments on follower investments. The dependent variable is 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

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

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 by the leader in the same round, and the dummy variable R_i takes the value 1 if it concerns the second treatment in a session (and 0 otherwise). 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 multicolinearity is avoided, i.e. there is no correlation between $x_{i,t}^{L}$ and *t*. The regression results are depicted in Table 3[15].

Table 3: Estimation results for average follower investments in the public bad.

Variable	symbol	Coefficient value	p-value
Constant	α_0	15.2	0.000
Effect of leader, constant	α_1	-3.56	0.000
Effect of leader, slope	α_2	0.42	0.005
Second treatment dummy	α_3	-0.46	0.630
Round first treatment	α_4	0.44	0.000
Round second treatment	α_5	0.45	0.000

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

The results confirm the previous findings. There is a highly significant upward trend in investments over rounds, as seen in Figures 1 and 2. This trend is practically the same in the first and second treatment of a session. The second treatment dummy is not significant. Together these findings confirm our claim that the order of the treatments is not important.

The effect of a leader is highly significant as indicated by the parameters α_1 and α_2. As expected the p-level is lower than in the test based on averages. Most interesting is the new information coming out of the regression, namely that the effect of leadership depends on the size of leader investments. When the leader reduces his or her investments by one token, the followers on average reduce their investments by 0.42 tokens.

15 The regression results seem robust to the specification chosen. For instance, they do not change when we include fixed effects for the groups or if we use a forward or stepwise selection procedure.

(Lack of independence between rounds is not a problem for this test). This implies that 42 percent of the leader variations in investments are imitated by the followers.

Figure 6 summarizes the effect of leadership on follower investments in the public bad as follows from the regression. The circle represents the average investments for the no-leader treatment, 17.4 tokens. It seems reasonable to place the circle at a leader investment which is equal to the average follower investments[16]. 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, 3.1 tokens, followers invest 2.26 tokens less than in the no-leader case. This is consistent with the difference of 2.28 found earlier using average investments over rounds.

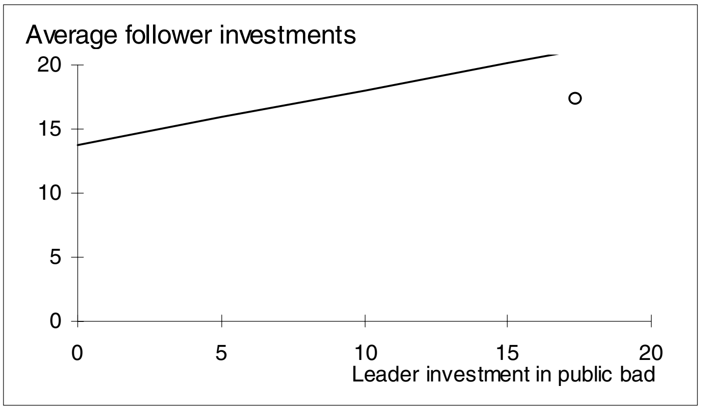

Figure 6: Average follower investments as a function of leader investments in public bad (solid line). Circle denotes point where leader and follower investments equal the average investments in the case without a leader.

Recall that leaders were instructed to keep investments in the public bad equal to or below 6 tokens. Hence, the experiment is 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 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 of leadership at moderate and high leader investments in the public bad. However, the parallel experiment by Van der Heijden and Moxnes (2000) shows that the effect of leadership is considerable also at moderate leader investments in the public bad. They argue that the two results could be seen as

16 This is close to what has been found in a parallel experiment where average leader investments were similar to average follower investments, Van der Heijden and Moxnes (2000).

consistent if the effect of leadership has two components, one involving a limited budget for sacrifices caused by considerations of fairness, and another involving imitation or learning.

5. CONCLUSIONS

With regard to global or regional environmental problems, do nations that take unilateral actions inspire other countries to curtail emissions as well? Using game theory, Hoel (1991) has shown that in a competitive situation, unilateral actions will both improve global welfare and lead to lower total emissions. For small leading nations, however, the effect is negligible. Hoel explicitly leaves out the possibility that unilateral actions "might lead to similar behavior from other countries." We have investigated this question by a public bad experiment with a leader (mixed sequential-simultaneous protocol). Hoel's effect of leadership is ruled out by the design of the experiment. Twelve groups of five subjects played the game twice, with a leader and without a leader. This enabled within-subject (group) comparisons. The order of the two treatments was varied over groups.

The no-leader treatment lead 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 investments in the public bad increase over rounds. There was no significant effect of the order of the treatments. The new and interesting finding is that there is a significant effect of a leader that sets 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 there is no effect of leader decisions on follower behavior.

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 will make here is that the effect is not necessarily negligible. On average the leaders cause a 13 percent reduction in 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 are also 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), there are positive effects of free and costly communication, Isaac et al. (1985) and Isaac and Walker (1991) respectively. 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.

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