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Collective *versus* Random Fining: An Experimental Study on Controlling Ambient Pollution

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Abstract. This paper presents an experimental study of two different pollution compliance games: collective *vis-à-vis* random fining as a means to regulate non-point pollution. Using samples from both Costa Rican coffee mill managers and Costa Rican students, we find that the two games perform equivalently but, although they lead to efficient outcomes through Nash play in the majority of cases, the observed frequency of Nash play is lower than theoretically predicted. Moreover, we reject the hypothesis that managers and students behave equally. Off the equilibrium, managers tend to over-abate, whereas students tend to under-abate. This result suggests the importance of considering subject pool differences in the evaluation of environmental policies by means of experiments, particularly if those policies involve certain forms of management decisions.

Key words: environmental regulation, experimental economics, non-point pollution, subject pool

JEL classifications: B4, C9, Q28, H2

1. Introduction

Economic theory provides a wide array of policy instruments such as emission standards, effluent charges and tradable permits to control pollutants from point sources. The problem of efficient pollution abatement turns out to be more difficult under circumstances of non-point pollution, or in cases where the individual emissions of point sources can only be monitored at a prohibitively high cost, such that effectively only ambient pollution levels can be observed. A common suggestion to deal with this problem is the use of target-based mechanisms like forcing contracts. For example, all potential polluters can be collectively punished if ambient pollution exceeds a certain threshold, defined by the regulatory authority. Such a mechanism has been suggested by Segerson (1988) and independently by Meran and Schwalbe (1987). Alternatively, it would also be possible to subsidize the firms according to the marginal benefit of abatement. A special characteristic of mechanisms like these is that they are not budget balancing in the sense that the sum of charges exceeds the social damage, or the sum of subsidies paid to the firms exceeds the social benefit.

By contrast, Xepapadeas (1991) suggests a mechanism with subsidies per unit of abatement and random fines. If ambient pollution exceeds the socially optimal level or some other fixed threshold, one of the suspected polluters is picked out randomly and charged a fine, irrespective of being one of the true polluters. Thereafter, the fine is redistributed to the other suspects. Xepapadeas calls this mechanism budget balancing, since the total payments to/from dischargers in case of deviations between desired and observed ambient concentrations equal the corresponding society's valuation of reduced abatement. In addition to the random fining scheme, Xepapadeas also discusses a non-budget-balancing mechanism based on subsidies and collective penalties, similar to those suggested by Meran and Schwalbe (1987) and Segerson (1988).

From a more general point of view, the underlying problem is a typical problem of moral hazard in teams, which was first treated by Holmström (1982) in a general principal-agent environment. Holmström demonstrates that under fairly general conditions, particularly for risk neutral agents, no budget-balancing mechanism exists that implements the efficient outcome in Nash equilibrium. Kritikos (1993) and Herriges et al. (1994) point out that Xepapadeas' results regarding the efficiency of the random fining mechanism are in contradiction with Holmström's findings, since risk neutrality is an assumption in Xepapadeas' paper. Kritikos shows, however, that Xepapadeas' idea of random fining can be restored if the revenues from fining are not redistributed to the other suspects, i.e. by giving up budget balancing. In contrast, referring to Rasmusen's (1987) work on risk averse agents, Herriges et al. (1994) argue that a budget-balancing scheme of random fining can be maintained if polluters are risk averse.[1]

In this paper we present an experimental study of both mechanisms proposed by Xepapadeas (1991) for cases where only ambient pollution levels can be observed. The main issue is the analysis of the players' behavior under collective *vis-à-vis* random fining, where the expected payoff in both mechanisms is identical. We designed two non-cooperative games that closely reflect the proposed mechanisms. One of the games reflects the non-budget-balancing mechanism, where firms face collective penalties if ambient pollution exceeds the desired target. The second game mimics the mechanism, where one firm is randomly chosen to bear the fine whenever measured ambient pollution is not optimal. Following Kritikos, we did not incorporate budget balancing, i.e., we did not refund the fines to other players, mainly because we are interested in the response of agents to the scheme, and because risk neutrality cannot be excluded for small stakes.

We run the experiments with two different pools of subjects. Our first sample consists of managers of coffee mills in Costa Rica. Coffee milling is associated with water pollution because water is used to peel and wash the fruit. Although the pollution of rivers caused by coffee milling is not a typical non-point pollution problem, the fact that several mills generally share the same river basin, and the agency's lack of funds to monitor each mill individually, added realism to the experiments. In any case, our experiment was not constructed for any particular industry, and we view our sample of managers as a representative sample of this type of individual in any given industry. Our second sample consists of Costa Rican students,[2] where the participants were subjected to the very same treatment that was applied to the sample of managers.

Due to sample size constraints in the sample of managers, we were only able to form "teams" of two, i.e., there were only two potential polluters. Although in practice two polluters obviously do not constitute a non-point pollution problem, the fact that individual pollution is not observable is a central element of the design of this controlled experiment. In addition, even if we form larger groups each player will obtain information about the behavior of the other players, based on his/her own payoffs, apart from the fact that in case of deviation players do not know who has been deviating. Still we acknowledge that larger groups provide less detailed information about the behavior of the "rest of the team" and therefore might better capture a non-point pollution situation.

According to Smith (1982), the general view is that the evaluations of the performance of institutions, obtained from laboratory tests, should also apply to non-laboratory settings where similar conditions hold. Still, some authors have argued that different subject pools might introduce different conditions into the experiments, some of which are not possible to control for in the design (Coller and Harrison 1997). For example, students, although readily available and cheap to work with, might have a particular sociological behavior (Cunningham 1974), might use different heuristics to solve the exercise (Friedman and Sunder 1994), and exhibit different degrees of risk aversion than, for example, firm managers. This calls for special caution when generalizing the results from these experiments. We believe that our samples provide a unique opportunity to explore differences in the behavior of convenience samples and industry people, who are involved in decisions about discharging pollutants.

The two samples of managers and students played several rounds of a treatment mimicking the collective fine game first, and several rounds of a treatment mimicking the game with random fining second. Since both games can be designed such that their Nash equilibria induce the socially optimal outcomes, our first conjecture is that the two mechanisms lead to Nash play. Due to the small sample size and the categorical nature of the data, a formal statistical analysis within game play was precluded. Hence, we looked at the frequency of Nash play compared to that of other strategies. We find that the frequency of Nash outcomes is higher than the frequency of any other pair of strategies. In the games played second, we even observe Nash play in the absolute majority of cases. Still, with frequencies between 33% to 68% in the different rounds, the frequency of Nash play is still low compared to what is predicted by the theories of Xepapadeas and others.

Since the two games are theoretically equivalent if the players are risk neutral, our next, and stronger, hypothesis is that the behaviors of the players are equal in both types of treatments. This hypothesis seems to be rejected at first glance. Since it cannot be excluded, however, that the differences in play are due to order effects, we tested for them by convoking a second sample of students who played the two series of treatments in the reversed order; that is, they played the treatment with random fining first, and the collective fine treatment second. Unfortunately, it was not possible to convoke managers for a similar test. We employ the two student samples to compare the games after accounting for order effects. By using a chi-square test, we cannot reject the hypothesis that the two games of collective and random fining are equivalent. Applying, once again, a chi-square test to the two student samples, we reject the hypothesis that the order in which the games are played is irrelevant.

Finally, we tested for differences in the subject pool under the hypothesis that the behavior of managers is equivalent to the behavior of students. Employing another Chi-square test, we reject this hypothesis. Looking at the outcome tables in more detail, we even find that off the equilibrium, managers tend to over-abate, whereas students tend to under-abate. Furthermore, we find that managers and students react differently to the random fine.

The paper is organized as follows: in section 2 we briefly set up the theoretical background of the games of collective and random fining. In section 3 we describe the design of the experiment and state our hypotheses. Section 4 presents the results of our analyses. In section 5 we draw our conclusions.

2. Theoretical Background

Following Xepapadeas' (1991, section 3) simple framework, we assume a finite number n of identical polluters, where A_i denotes the abatement level of polluter i and $C(A_i)$ his or her abatement cost function, which is assumed to be increasing and convex. The regulator only observes ambient pollution, given by $W = \overline{W} - \sum_i A_i$, where $\overline{W}$ is the unregulated ambient pollution level. Let A^* denote the socially optimal abatement level of each firm, which by symmetry is unique and identical for each firm.

Xepapadeas explores two mechanisms in which the agency subsidizes abatement in case of collective compliance, but fines the firms in case of collective under-abatement, distinguishing between collective and random fines.

2.1. GAME WITH COLLECTIVE PENALTIES

Under collective penalties, the n-firms are punished collectively if the regulator observes aggregate under-abatement. Each player receives a uniform share θ of the total subsidy corresponding to the observed pollution reduction minus the fine F^c. If there is optimal or over-abatement, the firms receive the share of the subsidy σ, corresponding to the total optimal abatement level.

Compliance is an equilibrium if and only if, for any $\tilde{A} \neq A^*$, we have:

$$\Pi^0 - C(A^*) + \theta\sigma n A^* > \Pi^0 - C(\tilde{A}) + \theta[\sigma[\tilde{A} + (n-1)A^*] - F^c] \qquad (1)$$

where Π^0 denotes the firm's profits for a given output without subsidies or abatement. The incentive compatibility condition in Equation (1) is equivalent to

$$F^c > \frac{1}{\theta}[C(A^*) - C(\tilde{A})] + \sigma[\tilde{A} - A^*] \text{ for each } \tilde{A}. \qquad (2)$$

Obviously, F^C can be chosen sufficiently large to induce compliance with the optimal abatement level by each firm as a Nash equilibrium. However, compliance to the socially optimal outcome $(A^*, \ldots, A^*)$ is not the only equilibrium. First, there exists also an equilibrium where no firm fully complies, but rather sets its marginal abatement cost equal to the effective individual subsidy, i.e., $C'(\tilde{A}) = \theta\sigma$. This equilibrium is, of course, strictly Pareto-dominated be the symmetric compliance equilibrium $(A^*, \ldots, A^*)$. Second, any other strategy profile $(A_1, \ldots, A_n)$ with $\sum_{i=1}^{n} A_i = nA^*$ is also an equilibrium as long as inequality (2) holds for the firm with the highest A_i, and thus for the firm which incurs the highest abatement cost. However, if firms are symmetric, the socially optimal outcome can be considered as a focal point among all the equilibria with aggregate compliance.

2.2. GAME WITH RANDOM FINES

In this game, if firms do not abate optimally, one of the players is picked randomly with probability ξ and has to pay the exogenously determined fine F^R. The rest of the players receive the subsidy corresponding to the observed total abatement. In case of aggregate non-compliance, the expected profit of firm i is given by

$$E\Pi = \Pi^0 - C(A_i) + \xi[-F^R] + (1-\xi)\left[\theta_{n-1}\sigma \sum_{i=1}^{n} A_i\right], \qquad (3)$$

where θ_{n-1} is the share of social benefits distributed among the other firms. Note that in the case of only two players, the firm that is not fined receives the full benefit from the subsidy. The only difference with the payoff functions in Xepapadeas (1991) is that the fine F^R is not returned to the lucky firms. For the risk neutral firm, compliance to the socially optimal action is an equilibrium if and only if, for any $\tilde{A} \neq A^*$, we have:

$$\begin{aligned} -C(A^*) \ + \ \theta\sigma n A^* &> -C(\tilde{A}) - \xi F^R \\ &+ \ (1-\xi)\theta_{n-1}\sigma[\tilde{A} + (n-1)A^*] \text{ for any } \tilde{A}. \end{aligned} \qquad (4)$$

It is obvious again that F^R can be chosen sufficiently large to guarantee that the strategy profile where each firm complies with the socially optimal outcome is an equilibrium. Note again that this equilibrium is not unique.

Table I. Abatement cost schedule

Abated emissions	Marginal abatement costs (MA)	Total abatement costs (AC)
0	0	0
1	20	20
2	40	60
3	60	120

By referring to these models we implicitly assume a deterministic relationship between individual emissions and ambient pollution. This is in line with Xepapadeas (1991) and with other papers in this field. In real situations, ambient pollution will certainly be subject to stochastic influences. It is not clear, though, that the introduction of a stochastic component or incomplete information would significantly change the results of the games. A regulatory authority might respond to these characteristics by adopting a less strict regulatory target that protects the firms from stochastic fluctuations, while the other properties of the regulatory mechanisms may still be the same. Note also that the number of firms is assumed to be exogenous in these models. Clearly different mechanisms may induce different numbers of firms in the long run through entry or exit if firms are risk averse.

3. Design of the Experiment, Hypotheses and Application

3.1. EXPERIMENTAL DESIGN

The games were constructed to be played repeatedly in a non-cooperative setting with two players (*A* and *B*). In principle, both games could be designed to be played by more than two individuals, given some minor adjustments. We decided to have two players only, mainly due to limitations in sample size, particularly for the managers. We think that, given that we guarantee the players that their actions are unobservable in the sense that the fines depend only on ambient pollution and not on individual abatement effort, our design still keeps the basic features of pollution problems with unobservable point emissions and of the enforcement mechanisms suggested by Xepapadeas. We acknowledge that larger groups might introduce new interesting features, particularly with regard to the information available to the participants. Due to the limitation of our sample size concerning the group of managers the study of the impact of the number of players on the abatement strategy must be left for future experimental research.

Throughout the experiment we set the default profit to $\Pi_i^0 = 34$. Table I contains the abatement cost schedule assumed in the experimental design of this exercise, which is the same for both players in both games.

Table II. Payoff matrix for treatment with collective penalty. In each cell, the numbers in the lower left corner determine the payoffs to player A, in the upper right corner the payoffs to player B

Abated emissions		Player B			
		0	1	2	3
Player A	0	17 17	22 42	7 67	–28 92
	1	42 22	47 47	32 72	14 114
	2	67 7	72 32	74 74	39 99
	3	92 –28	114 14	99 39	64 64

The social benefit per unit of abatement is assumed to be constant and equal to 50. Then, in the social planner's optimal solution, each polluter should abate 2 units in each period (round of the game). Hence we set the subsidy $\sigma = 50$ per unit of reduction in ambient pollution. If ambient pollution is not reduced by a total of 4 units, the fine will be set at $F^C = F^R = 34$. In this setting, each firm has full information about its own payoff function and about that of the other firms in the industry (firms are homogeneous). In addition, the observed ambient concentrations provide information about the behavior of the "other" firms in each monitoring round.

3.1.1. *Treatment with collective penalties*

In each round of this treatment, the two players (firms) are punished collectively by $F^C = 34$ if there is under-abatement. Each player receives the corresponding uniform share $\theta = 1/2$ of the total subsidy minus the fine. Table II provides the payoff matrix resulting from this profit function.

One easily verifies by inspection that there are two Nash equilibria: (1,1) and (2,2), where the latter one Pareto dominates the first one.

3.1.2. *Treatment with random fines*

In this treatment, if firms do not abate optimally, one of the players is picked randomly with probability $\xi = 0.5$ and has to pay the fine, $F^R = 34$. Moreover, we choose $\theta_{n=1} = 1$. The payoffs are presented in Table III. Each cell of the matrix contains three possible payoffs depending on total observable abatement. The smallest (top) value is the result of being randomly selected to pay the fine. The highest (bottom) value is the result of not being fined and receiving the full subsidy. If $\sum_i A_i > 4$, both values are the same since no fine is imposed. The

Table III. Payoff matrix for treatment with random fines. In each cell, the numbers in the lower left corner determine the payoffs to player A, in the upper right corner the payoffs to player B. Top number: payoff in case of being fined, bottom number: payoff in case of not being fined, bold number: expected payoff

Abated emissions		Player B			
		0	1	2	3
Player A	0	0 **17** 34 0 **17** 34	–20 **22** 64 0 **42** 84	–60 7 74 0 67 134	–120 –28 64 0 92 184
	1	0 **42** 84 –20 **22** 64	–20 **47** 114 –20 **47** 114	–60 **32** 124 –20 **72** 164	14 14 14 114 114 114
	2	0 **67** 134 –60 **7** 74	–20 **72** 164 –60 **32** 124	74 **74** 74 74 **74** 74	39 39 39 99 99 99
	3	0 92 184 –120 –28 64	114 114 114 14 14 14	99 99 99 39 39 39	64 64 64 64 64 64

middle value is the *expected payoff* for choosing a certain amount of emission reduction.

Taking expected payoffs, the game generated by this matrix has the same Nash equilibria as under collective fining: (1,1) and (2,2).

3.2. HYPOTHESIS

Theoretically, both games described above capture efficient mechanisms between the regulator and a polluting industry for the case where individual emissions are unknown to the agency. An efficient mechanism "... induces the dischargers to adopt optimal abatement policy in the absence of effective individual monitoring by the agency. The agency can monitor only whether the dischargers as a group follow the optimal policy, by monitoring the deviations between desired and observed ambient concentrations" (Xepapadeas 1991: 120). Since both games are theoretically equivalent for risk neutral players,[3] our first hypothesis is that both treatments successfully achieve the optimal pollution reduction ($A_i = 2$, $i = 1, 2$) and that there are no differences between the subjects' performances when facing the two treatments.

Our second hypothesis concerns the behavior of the sample of students *versus* the sample of coffee mill managers which constitute a more realistic sample of potential affected parties from this type of regulation. Most experimental studies use students as a representative sample and use the conclusions from such studies to draw conclusions about the behavior of real decision makers (Smith 1982; Plot 1982[4]). Hence, our hypothesis is that both of our samples should render similar results for both games in terms of pollution reduction and Nash behavior.

3.3. APPLICATION OF THE TREATMENTS

The treatments were applied to three different samples. One sample consisted of 16 pairs of coffee mill managers in Costa Rica (i.e., a total of 32 individuals) who participated in a one-day seminar organized by ICAFE (Coffee Institute,[5] Costa Rica). The participants were told in advance that part of the seminar, approximately 2 hours, was going to be dedicated to experimental sessions that involved non-negative monetary payments. The second sample consisted of 21 pairs of students from the University of Costa Rica who responded to the advertising of the treatments which was posted all around the university campus. The students were convoked for a two-hour experimental session of treatments involving non-negative payments. In both of these cases, the two treatments discussed here were part of a larger set of exercises which included trust experiments. Approximately 30 minutes of the 2 hours were used for the 2 treatments described in this paper. Finally, the third sample consisted of 16 pairs of students from the same university, which were convoked in the same way as above. This session lasted for approximately one hour and was intended to allow for testing for order or learning effects.

All sessions were conducted in large rooms that allowed us to physically separate the group of players type A from players type B.[6] The participants were randomly assigned to one of the groups. In the session with students, the participants who came accompanied were assigned to the same group. Additionally, we repeatedly requested that there should be no communication during the experiments.[7]

Each treatment was carefully explained in Spanish and the experimenters provided several examples, putting care into not biasing the results by choosing specific numbers. Special emphasis was added to the fact that the regulator was unable to monitor individual emissions and was therefore limited to ambient emissions monitoring. The actual treatment did not start until all questions were answered and the researchers were fully satisfied with the level of understanding. The same individuals conducted the experiments for all samples, following the same script.

Another important element of the treatments was the monetary compensation. We explained that there would be a monetary payment according to the results of the treatments. We also explained that one of the rounds was going to be randomly selected for payment and that each person would be paid individually and privately. Beattie and Loomes (1997) called this practice a *random problem selection procedure*[8] which is intended to encourage the respondents to treat each problem/round in isolation. In this way, the researcher is able to control for income effects, and, most importantly, each response can be treated as independent of the others, hence allowing for "direct within-subjects tests" (p. 156). Finally, the obvious differences in income level between our samples of students and coffee mill managers called for a different monetary compensation for each group. For the case of coffee mill managers, we decided to have an exchange rate of 12 units per US$l (hence a payoff of 74 units was equivalent to approximately US$6), and for the students this rate was 120 units per US$l. We are convinced that the amount of money at stake was non-negligible both for the managers and the students, such that dominance[9] was achieved.

We also described the dynamics of the treatments. Each of the two treatments would be played for five initial rounds. At the end of the fifth round a flip of a coin would decide if another round would take place.[10] After each round the experimentalists would collect the decision sheets and the payoffs for that round would be calculated.[11] The sheets would then be returned to the players for the next round. The players would keep their unidentified randomly selected partner through all rounds of a treatment.

Finally, for the sample of managers and the sample of 21 pairs of students (henceforth students-1), the treatments were played in the following order: first, treatment I with collective penalties and second, treatment II with random fines. We acknowledge that it would have been better to have each sample play just one treatment, but the number of observations obtained from the sample of managers would have been greatly reduced. Hence we decided to have each sample play both treatments. Still, we were interested in testing the impact of playing the treatments in a different order. Therefore, in the sample of 16 pairs of students (henceforth students-2), we played the treatments in the inverse order, i.e., first, the treatment with random fines and second, the treatment with collective fines.

4. Analysis of the Results

4.1. "WITHIN SAMPLE–WITHIN TREATMENT" ANALYSIS

We start our analysis by exploring the different ways in which both the managers and the students played both treatments. The primary objective is to determine whether the treatments achieve the optimal Nash equilibrium. We should recall

Table IV. Structure of responses in treatments with collective penalties (C1–C5) and random fines (R1–R5): sample of managers. The bold numbers indicate the frequency of the Nash outcome

Strategy	C1	C2	C3	C4	C5	R1	R2	R3	R4	R5
% in 3-3	12.5	6.25	18.75	12.5						
% in 3-2	25	31.25	31.25	12.5	25	25	25	6.25	12.5	12.5
% in 3-1	6.25		12.5	12.5						6.25
% in 2-2	**31.25**	**18.75**	**12.5**	**25**	**37.5**	**50**	**56.25**	**68.75**	**68.75**	**56.25**
% in 3-0		6.25			6.25				6.25	
% in 2-1	25	31.25	18.75	18.75	12.5	18.75	6.25	18.75	6.25	12.5
% in 2-0		6.25			6.25	6.25	12.5	6.25	6.25	6.25
% in 1-1				12.5	6.25					6.25
% in 1-0			6.25	6.25	6.25					
% in 0-0										

from the previous section that optimal abatement occurs when both firms reduce pollution by two units. Furthermore, the (2,2) outcome is the optimal Nash equilibrium in both treatments. As mentioned in section 3.2, our conjecture is that both treatments are efficient in achieving optimal abatement. The categorical nature of the data and the small sample size preclude a formal statistical analysis of this conjecture.[12] Nevertheless, a qualitative analysis of the data, in particular by looking at the frequencies of Nash play compared to other outcomes, gives some interesting insights.

Tables IV, V, and VI contain the percentage of strategy pairs that played each possible outcome in each round (1, 2, 3, 4, 5) of both treatments (c-collective and r-random), for the sample of managers, students-1 and students-2, respectively.[13]

In Table IV, we observe an interesting pattern in the responses of the managers. In the collective penalty treatment we observe that the efficient Nash strategy profile is played far below 50%. Still, apart from round C2 and C3, we observe that the efficient Nash strategy profile is played at least in the majority of cases. Moreover, the initial rounds of the treatments are characterized by many pairs over-abating (i.e., playing (3,3) or (3,2)). In round C3, the percentage of pairs over-abating even reaches 50%. The share of pairs that over-abate remains stable at 25% after round C4 and falls toward the end of the treatment with random penalties. This pattern might possibly reflect a cautious behavior by the managers. Also, with the exception of rounds C2, C4 and C5, the share of pairs in non-compliance ranges from 19% to 25% throughout both treatments. Finally in the random treatment, which in this case was played second, we observe the efficient Nash outcome steadily increasing from 50% in R1 to almost 70% in R4 with a slight drop at the end round. Thus, random punishment seems to perform better than collective punishment.

Table V. Structure of responses in treatments with collective penalties (C1–C5) and random fines (R1–R5): sample of students-1. The bold numbers indicate the frequency of the Nash outcome

Strategy	C1	C2	C3	C4	C5	R1	R2	R3	R4	R5
% in 3-3	4.76				4.76					
% in 3-2	4.76	9.52	14.29	4.76	4.76	9.52	19.05	9.52	4.76	9.52
% in 3-1	4.76	9.52			9.52			4.76		
% in 2-2	**38.10**	**33.33**	**33.33**	**33.33**	**38.10**	**57.14**	**42.86**	**47.62**	**52.38**	**33.33**
% in 3-0		4.76		9.52				4.76	4.76	4.76
% in 2-1	28.57	28.57	23.81	19.05	14.29	4.76	19.05	4.76	4.76	4.76
% in 2-0			4.76	4.76		19.05	4.76	14.29	14.29	19.05
% in 1-1	14.29	9.52	14.29	9.52	23.81	4.76			9.52	4.76
% in 1-0	4.76	4.76	4.76	19.05	4.76	4.76	4.76	14.29		14.29
% in 0-0			4.76				9.52		9.52	9.52

A different pattern of responses can be established for the student sample. Most striking is the observation that the share of pairs over-abating is very low throughout all rounds of both treatments. On the other hand, the share of pairs in non-compliance is very high, reaching even 52% and 62% in rounds C3 and C4, respectively. Moreover, with the exception of the end round, R5, the treatment with random penalties exhibits smaller shares of non-compliance. Similar to the managers' pattern of behavior, the Nash equilibrium is played far below 50% in the collective punishment treatment and is played more frequently (around 50%) under the random punishment treatment, but with a worse performance than observed with the managers. Nevertheless, also here we observe the efficient Nash equilibrium being played in the (simple) majority of cases in both treatments.

Finally, we turn our attention to the second sample of students, who received the treatments in reversed order. These results seem to indicate that order effects, rather than treatment effects, are responsible for the changes from one treatment to the other that we observe in Tables IV and V. This result is consistent with our hypothesis since both treatments provide the same incentives if players are risk neutral, and theoretically their expected results are equivalent. Disregarding the type of treatment we are discussing, we observe the same structure of strategies in Tables V and VI for both student samples. Again, the share of pairs over-abating is very low, compared to the share of pairs that under-abate. This consistent pattern seems to be in contrast to the behavior of the managers, who tend to over- and under-abate in approximately equal proportions. Once again, we find that the treatments achieve optimal abatement in the majority of cases, but for far below 50% of the cases for the treatment that was played first, and between 50% and 62.5% in the cases for the treatment being played second.

Table VI. Structure of responses in treatments with collective penalties (C1–C5) and random fines (R1–R5): sample of students-2. The bold numbers indicate the frequency of the Nash outcome

Strategy	R1	R2	R3	R4	R5	C1	C2	C3	C4	C5
% in 3-3				6.25						
% in 3-2	18.75	18.75	6.25		6.25	6.25	12.50			
% in 3-1	6.25	6.25			6.25					
% in 2-2	**37.50**	**37.50**	**37.50**	**31.25**	**37.50**	**62.50**	**56.25**	**50.00**	**62.50**	**62.50**
% in 3-0		6.25	6.25	6.25						
% in 2-1	12.25	18.75	37.50	31.25		25.00	18.75	31.25	12.50	12.50
% in 2-0	25.00	6.25	6.25	12.50	31.25			6.25	12.50	12.50
% in 1-1			6.25		6.25	6.25	12.50	6.25	12.50	6.25
% in 1-0		6.25		6.25				6.25		6.25
% in 0-0				6.25	12.50					

In summary, although we find the efficient outcomes being played in the (simple) majority of cases for both treatments in all three samples, the frequency of the efficient outcomes is still far below 50% in the treatments that were played first. However, if the treatment was played second, i.e., after possible learning effects, the efficient outcome could be observed in the absolute majority of cases but is still low compared to the prediction of the theory. This must be conceded even if one accepts that players make mistakes at some stage of their optimization process, in particular in situations where the outcome of the game depends on more information than is provided by its strategic form, e.g., experience, culture, expectations about the game, and about the other players' behavior (see Fudenberg and Tirole 1991: 13). Thus some skepticism about the effectiveness of the mechanisms as suggested in the theoretical literature is in place. We also obtain some indication of possible differences between students and managers, and possible order/learning effects rather than treatment effects.

4.2. "BETWEEN SAMPLES–BETWEEN TREATMENTS" ANALYSIS

In order to further explore the possible differences between students and managers, as well as the presence of order *versus* treatment effects, we would need to do between sample and between treatment comparisons. Since our data consist of frequencies in discrete categories, we can use a chi-square test to statistically determine any presence of differences between the two independent groups from which those frequencies originated. If the groups were different, then we would expect that the frequency with which the subjects in each group enter each category would also differ more than we would expect from usual random deviations (Siegel and Castellan 1988; Gibbons and Chakraborti 1992).[14] The chi-square test is valid

Table VII. Pooled frequencies in all three categories

	Managers		Students-1		Students-2	
	Collective penalty	Random penalty	Collective penalty	Random penalty	Random penalty	Collective penalty
Subopt. compl. and over-abating	33	14	15	14	15	3
Optimal abating	20	48	37	49	37	47
Under-abating	28	18	53	44	53	30

for independent samples and is likely to fail if the number of occurrences in each category is too low. Therefore, we cannot use the categories presented in Tables IV to VI, and we need to pool similar categories following reasonable criteria. Following the information about net social benefits provided in Appendix 1, we decided to have three categories, namely: (i) suboptimal compliers (3,1 and 1,3) and over-abaters (3,3; 3,2; and 2,3), i.e., those who meet the target but do not maximize net social benefits; (ii) optimal abaters (2,2) and (iii) under-abaters (3,0; 2,1; ... ; 0,0), i.e., those that do not even meet the target. Additionally, the observations in each category are pooled across the rounds of each treatment. This effectively assumes that the rounds of a treatment are independent, which is granted by the use of the *random problem selection procedure* described in section 3.3.[15] The test will then be based on the following Table VII, where each column can be intuitively perceived as a summary of how the treatment was played.

We start our analysis based on the chi-square test by continuing to explore the hypothesis that both treatments, i.e., the treatment with collective penalties and the one with random fines, are efficient in the sense that they achieve optimal abatement as the Nash equilibrium. In the previous analysis we observed that both treatments achieve the Nash outcome at least in a simple majority of cases in each round. Still, the original hypothesis was stricter since it originated from the fact that both treatments are theoretically equivalent. However, Tables IV and V for the manager and students-1 sample seem to indicate that the random fines treatments, which were played second, is more efficient in achieving the optimal Nash outcome since this outcome was chosen even in the absolute majority of cases. Nevertheless, Table VI shows exactly the opposite result, i.e. the treatment with collective penalties, which was played second this time, seems to be more efficient. Apparently, the observed differences between treatments are due to the sequence of playing rather than due to actual differences. Table VIII contains the formal statistical analysis based on the chi-square. The critical value in all cases (2 degrees of freedom)[16] is 5.99 (9.21) at a 5% (1%) confidence level. The p-values are provided in parentheses in all tables.

Table VIII. Chi-square test for testing order *versus* treatment effects

Managers: collective fines treatment is equivalent to random fines treatment	21.37 (0.0002)
Students-1: collective fines treatment is equivalent to random fines treatment	2.84 (0.417)
Students-2: collective fines treatment is equivalent to random fines treatment	10.84 (0.0042)
Collective fines treatment (first, as in students-1) is equivalent to random fine treatment (first, as in students-2)	0 (1.000)
Random fines treatment (second, as in students-1) is equivalent to collective fines treatment (second, as in students-2)	4.79 (0.0911)

The first three rows of the table contain a test that compares both treatments within a given sample. This is granted by the fact that the subjects were told that they would have a new partner in each of the two treatments and, once again, by the *random problem selection procedure*. Based on this test, we conclude that for the sample of managers and students-2, the pattern of responses in one treatment is significantly different from the pattern in the other treatment. However, the last two rows of Table VIII contain a comparison based on the order in which the treatments where played. Hence, we compare the treatments that were played first and detect no significant difference in the pattern of responses. The same is done for the treatments that were played second, and we obtain a similar result. In summary, the set of tests presented in Table VIII contribute to not rejecting the hypothesis that both types of treatments are equivalent, and that all perceived differences are due to ordering effects. We could also test for order effects within treatments by comparing both samples of students. This is done in Table IX.

We detect a significant difference in the pattern of responses only for the treatment with collective penalties. Certainly, the treatment with random fines is more complex than the one with collective penalties, which might account for the weak learning effect in the first compared to the second.

Next we turn our attention to subject pool effects. As we recall from section 3.2, the hypothesis now is that there are no observable differences between the subject pool of managers and that of the more usual, convenience sample of students. If this is the case, then the chi-square test should not detect any significant difference in the pattern of responses between the two groups. Table X contains the test statistics for the hypothesis of no subject pool effects.

In both cases, we reject the hypothesis that there are no differences in the pattern of responses arising from subject pool differences. In previous paragraphs, we already hinted at the source of this difference; in Table VII we observe that in

Table IX. Chi-square test for testing within treatment order/learning effects

Hypothesis: no order/learning effects	Statistic
Treatment with collective fines when played first (students-1) is equivalent to the collective fines treatment when played second (students-2)	12.41 (0.0020)
Treatment with random fines when played first (students-2) is equivalent to the random fines treatment when played second (students-1)	2.08 (0.3534)

Table X. Chi-square test for subject pool effects

Hypothesis: no subject pool effects	Statistic
Collective fines treatment: the behavior of managers does not differ from that of students-1	16.72 (0.0002)
Random fines treatment: the behavior of managers does not differ from that of students-1	7.83 (0.0199)

the treatment with collective penalties the managers have similar shares in all three categories. This is particularly so for the suboptimal categories of under- and over-abatement. In the sample of students, we definitely observe a different pattern of responses characterized by a very high share of under-abatement. A similar structure holds for the treatment with random fines. Again the managers have similarly low frequencies in the sub-optimal categories, which is in clear contrast to the sample of students who have a large frequency of pairs that under-abate.

Another interesting difference between managers and students, related to the previous discussion on differences in their patterns of responses, lies in their reaction to the fine in the treatment with random fines. In the sample of managers, 100% of those who received the fine in any given round subsequently increased their stated abatement in the rest of rounds. Even more, 67% of them reduced pollution optimally in all of the rounds left of the treatment. The picture is very different for both student samples. More or less 50% of the fined players increased abatement in the next period, but for approximately half of them, this increase was still sub-optimal and, most interestingly, only temporary (for one round). This points out two potential differences between the two subject pools. The first one is a possibly higher degree of risk aversion in the sample of managers. The second relates to the strong learning effects observed in the experiments. We believe that receiving the fine in the treatment with random fines could be seen as a revelation of information regarding the true consequences of cheating in that treatment. From this perspective, it seems that 100% of the managers successfully incorporated that information into their decision-making process, whereas a much smaller share of students did so, and most of them only temporarily. In this sense, one could hypothesize about differences in the ability of both subject pools to incorporate new information into their decision-making process.

5. Conclusions

We presented an experimental study to test different mechanisms to regulate non-point pollution. In particular, we tested two efficient mechanisms proposed by Xepapadeas (1991) to deal with this type of pollution. One of them involves the combination of collective fines and subsidies for pollution abatement, whereas the other combines the same subsidies with random fining.

Regarding our first hypothesis, we do not find significant differences in the treatments. Most of the observed differences in the strategies employed by the players can be linked to order/learning effects. Further, we observed a considerable share of deviations from the efficient Nash equilibria in the treatments being played first and, despite of learning effects and despite observing efficient pollution reduction in the absolute majority of cases for the treatment being played second, the frequency of the efficient Nash outcomes is still low compared to the theoretical predictions by Xepapadeas and others. These observations suggest the need to perform further tests of the proposed mechanisms but also the need to look for other, more effective mechanisms that could then be compared to the ones explored in this paper. Moreover, it might be perceived as unfair that a firm receives the full fine given that its individual pollution was in line with the desired target. Some participants in the experiments privately expressed this opinion about the treatment with random fines. In such a case, a system of collective fines would possibly be perceived as ethically preferred.

Another interesting conclusion is the importance of learning effects. In those treatments that were played second, compliance with the desired pollution reduction was significantly higher, and this outcome dominated all others by an absolute majority in each round. This result indicates the need for providing a suitable learning or maturing period for a newly implemented policy, before evaluating its performance. The importance of learning behavior is discussed by Starmer (1999), who argues that, if the proper incentives are provided to encourage learning, then "... actual behavior might gravitate towards optimal solutions over time" (p. F12).

In addition, our results indicate that firms can understand and adapt their behavior to elaborate regulatory contracts after a suitable learning period, leading to efficient outcomes. This result is particularly relevant for the developing country context from which our sample was drawn, where skepticism to any, not to say an elaborate, regulatory contract is widespread.

A second issue in this paper was to test for behavioral differences among "real" decision makers and convenience samples such as students. In all cases, our analysis allows us to reject the hypothesis that managers and students performed similarly in this experiment. The main difference lies in the observation that the managers tend to over- and under-abate in similarly low proportions, whereas both samples of students show a clear pattern of high under-abatement in all rounds of the treatments. The observed differences between subject pools are in line with similar exercises in the experimental economics literature. This evidence seems to suggest that professional experience might create a behavioral gap between the managers and the students (Ball and Cech 1996; Binmore 1999). Additionally, a related argument is that experienced managers have different attitudes towards risk than students. Although this study was not designed to test for risk aversion, the fact that the frequency of students "cheating" is high compared to the managers could be an indication of different degrees of risk aversion.

Ball and Cech (1996) argue based on an extensive review of the literature that the validity of using a convenience sample of students as surrogates for a more realistic sample of individuals will depend on the nature of the study. Our experiment studies environmental regulation under non-point pollution, and is hence a case in which "information subtleties or behavioral nuances" are relevant for the results. Very few students, if any, have any experience with environmental regulators and regulation, i.e., taxes, command and control measures, etc. Therefore, the evidence presented in this paper is in line with Ball and Cech and confirms that students cannot immediately jump into the shoes of firm managers.

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Notes

1. We note that the proposed solutions to the problem of moral hazard in teams explored in this paper have different theoretical origins. Whereas the tax/subsidy scheme draws on Holmström's (1982) mechanism, the random penalty scheme is based on Rasmusen (1987). We thank an anonymous referee for this hint.
2. We conducted the experiments with students from Costa Rica instead of recruiting students from Germany in order to exclude cultural differences.
3. If the agents were risk averse, the fine chosen for the random game would provide a stronger incentive to comply with the regulation.
4. "General theories must apply to special cases. [. . .] Theories which do not apply to the special cases are not general theories and cannot be advocated as such" (Plot 1982: 1522).
5. This institute is the organizing body in charge of coffee production and processing, as well as regulation of the industry.
6. A pair of players always consisted of a member of group A and a member of group B.
7. This request was particularly difficult for the sample of coffee mill managers – mostly senior businessmen not used to being silenced. We downgraded our request to not discussing the games or the strategies under the promise of a later explanation. We believe that this strategy was more successful, and we never heard discussions about the optimal strategies. Nevertheless, this

possibility cannot be rejected. Note that these were within player-type conversations. The groups of players were physically separated.

8. Starmer and Sugden (1991) call this practice *random lottery incentive system*. They find evidence that subject responses are not significantly different between designs based on this system and single choice designs.
9. Smith (1982) states that dominance is achieved when: "The reward structure dominates any subjective costs (or values) associated with the participation in the activities of the experiment" (p. 934).
10. This strategy was used to reduce the number of deviations from the Nash strategy in the last round of each game. Since the two games described in this paper were the last to be played during the session, we took the liberty of stopping the second of our games after the fifth round, without flipping a coin. The rest of the games ended according to the flip of the coin. Merely by chance, all the games played first also ended after round 5.
11. The use of computers would certainly simplify this. Computers were not available.
12. Table A1 in Appendix 1 contains the net social benefits associated with all possible outcomes and categories of the games.
13. Empty cells in the following tables stand for 0%.
14. A limitation of this test is that it does not provide any information about the likely explanations for the observed differences.
15. The alternative is to do tests for each round and aggregate the results into one overall conclusion, possibly adjusting the significance of each individual test by dividing it by the number of tests to be aggregated, i.e. doing a Bonferroni adjustment of significance. Still we believe that not much can be gained by opting for this alternative, which in turn precludes the use of the chi-square test due to small frequencies.
16. The degrees of freedom is calculated as $df = (r - 1)^*(c - 1)$, where r is the number of categories (3 in our case) and c is the number of columns to be compared (2 in our case).

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Appendix 1

Table A1. Calculation of net social benefits

Possible strategies by each pair			Social benefits	Total costs	Net social benefits	Categories
	3,3		300	240	60	
3,2		2,3	250	180	70	Over-compliers and
3,1		1,3	200	140	60	suboptimal compliers
	2,2		**200**	**120**	**80**	**Optimal compliers**
3,0		0,3	150	120	30	
2,1		1,2	150	80	70	
2,0		0,2	100	60	40	
	1,1		100	40	60	Under-compliers
1,0		0,1	50	20	30	
	0,0		0	0	0	

Appendix 2

Sample instructions (original in Spanish)
(Treatment with random penalties)

Thank you for participating in this experiment. For your participation today, you will earn some positive amount of money. This amount of money will be determined by the way that the decisions that you make interact with the decisions of others.

There will be at least five rounds in this experiment. After round 5, a coin will be flipped to see if the session continues. If the result of the coin flip is "heads", another round will follow, with another coin flip after this round. The session ends when the result of a coin flip is "tails". In this exercise, you will be paired with an individual randomly selected from the other group. Your partner will be the *same* person throughout this experiment. You will never be informed of the identity of the person with whom you are paired, either during or after the experiment; similarly, no one will know that they are, or have been, paired with you at any point in time.

In each round, you will be asked to choose a level of pollution abatement – either 0, 1, 2, or 3 units abated. Since pollution is costly, your payoffs decrease as you abate pollution. If there is not enough abatement (fewer than 4 total units abated), one of the players is picked randomly with probability 1/2 and has to pay a fine F. But, the regulator cannot observe your individual abatement level – hence the probability of being caught does not depend on individual abatement effort, because the regulator cannot observe it. However, the regulator can observe *total* abatement effort and if it is less than 4 units of pollution abated (2 units per person), one of the players is randomly selected and given the fine. Note that you could abate the proper amount and still receive the fine if your partner does not abate the proper amount.

The person with whom you are paired will simultaneously choose abatement levels without knowing the action you have selected. The payoffs you receive are shown in the game matrix below:

(*See Table III in main text*)

All payoffs are in a currency called *Pesetas* – where 12 *Pesetas* equal \$1. The payoff in the bottom left hand corner is your payoff; the payoff in the upper right hand corner is the payoff for the other player (player Y). You will notice that there are 3 payoffs in both corners – the upper payoff is the least you can earn with that choice, the middle payoff is what you should expect to earn, and the lower payoff is the most you can earn with that choice. This is because the payoffs for each choice are different. **For example, if the summation of your choice and the other player's choice is less than 4 units abated, one of the players is picked randomly with probability 1/2 and has to give up some *Pesetas*.**

Here are a few examples to get you started: if both players choose abatement of 0, then the two choices sum to 0, which is less than 4 units abated, thus one of you will have to give up some *Pesetas*. If you are randomly chosen to give up the *Pesetas*, then you receive 0 (lowest payoff in 0,0 cell) and the other player receives 34 *Pesetas* (highest payoff in 0,0 cell). On the other hand, if the other player is chosen to give up the *Pesetas*, then you receive 34 *Pesetas* while they receive 0 – since the probability is 1/2, the expected value is 17 *Pesetas* (middle payoff cell). Suppose you choose 0 and player Y chooses 1, then again they sum to less than 4 and if you are randomly selected you will receive 0 while player Y receives 64 *Pesetas*. If player Y is selected, then you receive 84 and player Y receives –20. Note, that your individual probability of getting chosen for the fine does not depend on the level of your abatement effort.

Finally, suppose you both choose 3. Then, both players receive 64 Pesetas with certainty. This is because the sum 3 + 3 is not less than 4. We will go over these possibilities after everyone has read the instructions, to ensure that they are understood.

In front of you should be a decision sheet. Each subject has a decision sheet exactly like the one in front of you. After everybody has made a decision, these sheets will be collected and payoffs will be calculated. At the end of each round, you will be informed of the choice of the player with whom you were paired, and the outcome.

At the end of the game, we will randomly select one of the rounds for payment, and each person will be paid individually and privately. Please feel free to ask questions before the beginning of this experiment. After the experiment begins, there is to be no communication between the participants in the experiment. Are there any questions?