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The deterrence effect of linear versus convex penalties in environmental policy: laboratory evidence [1]

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Abstract: We study the individual compliance behavior of polluting firms in an experimental setting under two different penalty functions (a linear and a strictly convex) and two different regulatory instruments (emission standards and tradable pollution permits). We find that a convex penalty, as compared to a linear penalty, increases the market price of pollution permits and the violation rate of firms. The effect of the structure of the fine on the price of permits operates through an increase in the ask-prices of sellers, not on the bids by suppliers. With convex penalties, sellers are not willing to sell a permit at a price as low as with linear penalties. We do not observe an effect of convex penalties on the compliance status of firms with emission standards. These results call for attention on the possible effect that the type of penalties may have on the cost-effectiveness of pollution control programs based on tradable pollution permits.

***Keywords*:** Environmental policy, enforcement, penalty structure, emissions standards, emissions trading, laboratory experiments

***JEL Classification*:** C91, L51, Q58, K42

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1 Introduction

Monetary fines in environmental policy commonly have two components: a financial (or economic) benefit component and a gravity (or deterrent) component. (See, for example, the "Guidance for Enforcement and Sanctions" of the U.K. Environment Agency (2015) and the "Policy on Civil Penalties: EPA General Enforcement Policy", U.S.E.P.A. (1984)). The goal of the economic benefit component of a fine is to remove the economic benefit of noncompliance. The goal of the gravity component is to deter future violations. It is an additional amount "to ensure that the violator is worse off than if it had obeyed the law. This additional amount should reflect the seriousness of the violation." (U.S.E.P.A. 1984, p. 3). The seriousness of the violation depends, in part, on the actual or possible harm. The assessment of this harm should take into consideration the amount of pollution. (U.S.E.P.A. 1984). Whether the fine should be linear or convex (progressive) in the level of the violation, is something that these official sanction guidance documents leave for the discretion of the enforcer.

The issue of whether to use a penalty that is linear or convex in the level of violation is not addressed also in the seminal works of the economic literature of enforcing environmental regulation (Harford (1978 and 1987), Viscusi and Zeckhauser (1979), Harrington (1988), Malik (1990 and 1992), and Stranlund and Dhanda (1999)).[2] The reason may be that, according to seminal work by Becker (1968), the structure of the penalty function is irrelevant to the compliance decision. What matters is that the expected marginal fine for the "first unit" of violation is higher than the marginal

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[2] Although some authors have considered the case of decreasing marginal penalties (Keeler (1991), for example), we do not deal with decreasing marginal penalties in this work.

benefit. The objective of this work is to test this irrelevance. To perform these tests, we compare the predicted versus the actual individual and aggregate level of violations using laboratory experiments with undergrad students. In the experiments the enforcement level is “just enough” to induce an expected–profit maximizer to comply with the norm. The norm takes the form of an emission standard in a set of experiments and a market for emission permits in another set.

An important policy implication arise from testing whether firms respond differently to linear versus convex penalties, when both induce the same level of emissions. As showed by Stranlund (2007) for the case of tradable permits and Arguedas (2008) for the case of pollution standards, minimizing the expected social costs of reaching an emissions target requires full compliance with a linear penalty. However, if the firms react differently to a linear penalty than to a convex penalty, the relative cost-effectiveness of linear versus convex penalties may need a revaluation. In other words, because the type of the penalty determines the overall expected cost of the pollution control program, testing the response of firms to different penalties may give regulators information on the expected cost of the program, which could differ from the theoretical expected costs if firms react differently to linear versus convex penalty.

To our knowledge, the only work that tests the effect of penalties on the compliance levels of firms in an experimental setting is Restiani and Betz (2010).[3] They test three types of penalties in a market for pollution permits: a fixed rate (constant

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[3] Anderson et al. (2017) studied the deterrence effect of different penalty structures for repeat offenders. In this framework, penalties are increasing, decreasing or constant in the number of previous discovered offenses, and the nature of the model is dynamic. In our case, penalties are decreasing, increasing or constant in the level of the violation and the nature of the model and the decision is static.

marginal penalty), a make-good provision system (offset penalty) and a mixed penalty that combines both. They conclude that different penalty designs do not translate into different permit prices and that the compliance rate is higher for the fixed rate penalty, despite theory predicts no difference. Nevertheless, their design includes perfect monitoring; i.e. the regulator can observe the firms' emissions at every moment. In contrast, we analyze the impact of different penalty structures under imperfect monitoring; i.e. the regulator audits the firms' emissions with a certain probability. This is an important actual element of any environmental policy.

We organize the paper as follows. In section 2, we present the main hypotheses we want to evaluate and the theory behind them. Section 3 contains a description of the experimental design. Section 4 describes the experimental procedures. Section 5 presents the results. Finally, in Section 6, we put forward a discussion and concluding remarks from our work.

2 Compliance Behavior and Hypotheses

Assume a polluting firm operating under either an emissions standard or a competitive transferable permits system, along with a fixed number of other heterogeneous firms. The firm's abatement cost function is $c(q)$, which is strictly decreasing and convex in the firm's emissions q [$c'(q) < 0$ and $c''(q) > 0$].[4] In line with the vast majority of theoretical work in this area, we assume that firm's objective is to maximize expected profits.

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[4] The abatement cost function is defined as $c(q) = b(q^u) - b(q)$, where $b(.)$ is the profit function, $b'(q) > 0$, $b''(q) < 0$ and $q^u = argmax\ b(q)$. Under the previous assumptions, $q^u > q$.

The environmental policy target is a fixed aggregate level of emissions $\overline{Q}$. The regulator audits polluting firms with a random exogenous probability π. An audit provides the regulator perfect information about firms' compliance status. A firm is in violation (v) when its units of emissions exceed its permit holdings or its emission standard (more on this below). If the firm is audited and found in violation, a penalty $f(v)$ is imposed.

2.1 Transferable Emission Permits System

Under a system of transferable emissions permits, a total of $L = \overline{Q}$ licenses are issued by the regulator, each of which confers the legal right to release one unit of emissions to the possessor. Under the assumption of perfect competition, the market for permits generates a unique equilibrium price p of licenses. Let l_0 be the initial allocation of licenses to the firm and let l be the number of licenses that the firm holds after trade. When a firm is non-compliant, its emissions exceed the number of licenses it holds and the level of its violation is $v = q - l > 0$ for $q > l$, 0 otherwise.

A firm chooses its emissions and permits to minimize expected costs. These are comprised of abatement costs, expenditures from buying permits minus earnings from selling permits and the expected penalty. If the firm's choice of emissions q equals its demand of permits $l(p)$, the firm is in compliance. We know that in this system a risk-neutral firm is compliant if and only if $-c'(l) \leq \pi f'(0) = \pi\varphi$ (see for example, Malik (1990) or Stranlund and Dhanda (1999)). Stranlund (2008) shows that this condition is also necessary and sufficient to induce compliance in the case of risk-averse managers. We also know from the literature that the optimal choice of emissions implies $-c'(q) = p$, which implicitly defines $q(p)$. If the number of firms that participate in the market is n, the equilibrium price of permits with perfect-compliance $p(L)$ is implicitly defined by the perfect – compliance equilibrium condition for the market for pollution permits, $\sum_{i=1}^{n} l_i(p) = L = \overline{Q} = \sum_{i=1}^{n} q_i$. Hence, under a transferable emissions permit system, perfect compliance requires $p(L) \leq \pi\varphi$. If this condition is not met, the firm chooses to demand a number of permits equal to $l(p, \pi, \varphi, \gamma) < q(p)$. This number of permits $l(p, \pi, \varphi, \gamma)$ is the solution to $p = \pi[\varphi + \gamma(q(p) - l)]$. The permit market equilibrium condition when violations occurs is $\sum_{i=1}^{n} l_i(p, \pi, \varphi, \gamma) = L < Q$, which implicitly defines the non-compliance equilibrium permit price as a function of the total number of licenses and enforcement parameters; that is, $p^{nc}(L, \boldsymbol{\pi}, \varphi, \gamma)$, where $\boldsymbol{\pi}$ is a vector of monitoring probabilities on regulated firms.

2.2 Emissions Standards

We consider now the case in which each firm i faces an emissions standard s_i. This is a maximum allowable (legal) level of emissions for each firm. Emissions standards for all firms satisfy $\sum_{i=1}^{n} s_i = \overline{Q}$. Under an emissions standard, a firm chooses the level of emissions to minimize its total expected compliance costs, which consist of its abatement costs plus the expected penalty. As it is known, a risk-neutral firm will be compliant ($q = s$) if and only if $-c'(s) \leq \pi f'(0) = \pi\varphi$ (Heyes 2000; Malik 1992; Harford 1978). This condition is also necessary and sufficient to induce compliance in the case of risk-averse managers.[5] If $-c'(s) > \pi\varphi$, the firm is going to choose a level of emissions $q(s, \pi, \varphi, \gamma) > s$, where $q(s, \pi, \varphi, \gamma)$ is the solution to $-c'(q) = \pi[\varphi + \gamma(q - s)]$.

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[5] The derivation of this result is available upon request.

2.3 Hypotheses

We are now ready to present the main hypotheses that we evaluate with our laboratory experiments.

Hypothesis 1: *In a system of transferable emission permits where the expected marginal penalty is just enough to induce compliance by expected profit maximizers firms, the level of individual and aggregate violations is independent of the penalty structure.*

As previously discussed, under a system of transferable emission permits, a firm complies if and only if $-c'(q = l) = p(L) < \pi f'(0) = \pi\varphi$. Assume $\pi\varphi = p(L) + \varepsilon$, where $\varepsilon > 0$ is an arbitrarily small amount. Because this condition can be obtained with a convex penalty function $f(v) = \varphi \times v + \gamma/2 \times v^2$ (with $\varphi > 0$ and $\gamma \geq 0$), or a linear penalty function $f(v) = \varphi \times v$, we should expect no differences in violations between both schemes.

Hypothesis 2: *In a system of emissions standards where the expected marginal penalty is just enough to induce compliance by expected profit maximizers firms, the level of violations is independent of the penalty structure.*

The reasoning for the case of emission standards is the same as for the case of tradable permits, except that in the case of emissions standards the compliance condition is firm specific. More specifically, the enforcement level must be such that $\pi_i\varphi = -c_i'(s_i) + \varepsilon$ for all i.

3 Experimental Design

We framed the experiments as a neutral production decision of an unspecified fictitious good q, from which the subjects obtained benefits. Every subject had a production capacity of 10 units (whole numbers), but the benefits of production differed between subjects, giving place to four types of subjects: two with "high" marginal benefits and two with "low" marginal benefits. These schedules of marginal benefits, taken from Cason and Gangadharan (2006), were the same through all the experiments and we assigned them randomly across subjects.[6]

3.1 Tradable permits

In the permits experiments, subjects had to decide how much to produce of the fictitious good but they had to possess one permit in order to be legally able to produce each unit of the good. Each subject received an initial number of permits without cost at the beginning of the experiments. Subjects could also buy or sell permits in a permit-by-permit double-auction market comprised by 8 subjects, 2 of each type. In these auctions every subject could either make a bid for a permit, submit an asking price to sell a permit, or accept a bid or an asking price made by another subject. Each experiment consisted of 10 identical rounds. At the end of each production round, the subjects were audited with a known homogeneous predetermined and exogenous probability π. If audited, the number of units produced by the subject i in that period (q_i) was compared with the number of permits possessed by the subject i at the end of the period (l_i). If $q_i > l_i$, the subject was automatically fined. The subjects had the information on the

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[6] See Table A.1 in the Online Appendix, available at http://www2.um.edu.uy/marcaffera/investigacion/OnlineAppendix_Structure_of_penalties.pdf

probability of inspection that they faced and on the marginal fine for every level of violation in their screens at every moment before making their decisions.

We constructed two treatments for the case of markets for permits, both of them designed to induce compliance (see Table 1). In Treatment M1, the total number of tradable permits supplied to each group of 8 subjects was 40. Four (4) permits were initially allocated free of charge to subjects of type 1 and 2, the prospective buyers, and 6 permits for subjects of type 3 and 4, the prospective sellers.[7] The enforcement parameters took the values $\varphi = 100$, $\gamma = 66$ and $\pi = \frac{80}{133}$. Treatment M2 is the same as Treatment M1, except for the fine schedule. More precisely, in Treatment M2 $\varphi = 133$, and $\gamma = 0$. The resulting perfect-compliance equilibrium price of the market in both cases is expected to be between 74 and 80 experimental pesos (E$). Therefore, $p^*(L) \leq \pi f(1) = 80$ in both treatments. Therefore, both treatments should induce the same perfect-compliance equilibrium price of permits and the same individual level of emissions.[8] Hence, the expected level of aggregate emissions is also 40 units for both treatments.

3.2 Standards

Similar to the case of tradable permits, we constructed two treatments for the case of emission standards; labeled S1 and S2 in Table 1. In the standards experiments subjects faced a maximum allowable level of emissions (the standard) and had to decide

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[7] We chose this initial allocation of permits as opposed to a homogeneous allocation (5-each) as a way to foster the market activity. The number of expected trades consistent with this initial allocation is 5.

[8] We call "emissions" the output chosen by the subjects although, as we have already mentioned, we framed the experiment as a neutral production decision.

how much to emit. In the treatment S1, the emission standards were 7, 6, 4 and 3 for firms' types 1 to 4, respectively. (These are the cost-effective levels of emissions). The auditing procedure was as in the case of tradable permits, with the exception that in the case of standards perfect-compliance requires targeting inspections according to the marginal abatement costs of the firms. Accordingly, the auditing probabilities were 0.6, 0.65, 0.63 and 0.66 for types 1 to 4, respectively. Finally, violations are fined with the same penalty function used in M1; $\varphi = 100$ and $\gamma = 66$. This policy induces compliance, so the expected aggregate level of production is 40 units in a group of 8 subjects.[9] In the treatment S2, everything is equal to S1, except the structure of the penalty, which is that of M2; $\varphi = 133$ and $\gamma = 0$. In other words, treatment S2 induces perfect compliance, as S1, but with a linear penalty schedule.

Table 1: Summary of Treatment design

Policy Instrument	**Treatment**	**Penalty function: $\varphi v + \frac{\gamma}{2} v^2$**		**π**	**Cap**	**Predicted Behavior**	**Predicted Equilibrium price**
		φ	γ				
MARKET FOR EMISSION PERMITS	M1	100	66	$\frac{80}{133}$	40	Type 1: q = l = 7, v = 0 Type 2: q = l = 6, v = 0 Type 3: q = l = 4, v = 0 Type 4: q = l = 3, v = 0	\$74 - \$80
	M2	133	0				
EMISSION STANDARDS	S1	100	66	Type 1: 0.60 Type 2: 0.65 Type 3: 0.63 Type 4: 0.66	40	Type 1: q=s=7, v=0 Type 2: q=s=6, v=0 Type 3: q=s=4, v=0 Type 4: q=s=3, v=0	
	S2	133	0				

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[9] The number of subjects showing up for standards experiments was not multiple of eight in five of the eight sessions. We allowed this to avoid disappointing subjects in a thin pool.

4 Experimental Procedures

We programmed the experiments in z-Tree (Fischbacher, 2007) and conducted them in a computer lab specifically designed for these experiments at the University of Montevideo, between December 2011 and April 2012.

We recruited the participants from the undergrad student population of the University of Montevideo, the University of the Republic, the Catholic University and ORT University; all in the city of Montevideo, Uruguay. In a given experimental session, subjects played two treatments of tradable permits or two treatments of emission standards. Each session consisted of 20 rounds. In the first 10 rounds subjects participated in one treatment. In the second 10 rounds they participated in another treatment. In one treatment we induce perfect compliance (M1 or M2 in a permits session; S1 or S2 in a standards session). The results presented here are from these treatments. In the other treatment, the probability of being inspected was lower, inducing violations. The order of treatments differed between groups in a session. Approximately half of the people that showed up in the room for a session played the compliance treatment first, and the other half played the violation treatment first.

Before the beginning of the experiments, we handed out instructions to subjects and we read them aloud, after which we answered questions in private.[10] Prior to the first round of the first treatment, subjects played 2 trial rounds in the standards sessions and 3 trial rounds in the permits sessions. In the standards sessions each period lasted 2 minutes. In the permits sessions each period lasted 5 minutes, to give subjects time to

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[10] Instructions of the market experiments available in the Online Appendix.

make their bids, asks, and to decide how many units to produce and how many permits to buy or sell.

After all subjects in the group had made their decision, the computer program automatically produced a random number between 0 and 1 for each subject. If this number was below the informed monitoring probability, the subject was inspected, as explained in the instructions. Subjects were informed in their screen whether they had been selected for inspection or not, and the result of the inspection (violation level, total fine and net profits after inspection). After this, subjects were informed in their screen the history of their decisions in the game, the history of inspections and the history of profits, up to the last period just played. After 20 seconds in this screen, the next period began automatically.

Two hundreds and sixteen (216) experimental subjects participated in the permits experiments and 219 in the standards experiments. Due a thin pool of subjects, we allow subjects to participate in more than one session. Eliminating reappearances, the total number of different subjects that participated in the permits experiments was 120 and the total number of different subjects that participated in the standards experiments was 113.

We set the exchange rate between experimental and Uruguayan pesos in 40 ($E 40 = $U 1). The value produced an average expected payment for the participation in the experiment that was similar to what an advanced student could earn in the market for two hours of work (the duration of the sessions), including a showing up fee of around US$ 7.[11] Total payments ranged between US$ 16.8 and US$ 5.1 in the tradable permits sessions, with a mean value of US$ 13.7, a median of US$ 14.1 and a standard deviation of US$ 2.1. In the standards sessions, payments ranged between US$ 5.1 and US$ 30.3, with a mean value of US$ 20.2, a median of US$ 18.9 and a standard deviation of US$ 5.3.

5 Results

In this section, we present the results of our work. We present the outcomes of the permits experiments first, then those of the standards experiments and finally we compare results between instruments.

5.1 Descriptive Statistics of Market Experiments

We report basic descriptive statistics of relevant variables in Table 2. The first thing to notice is that violations are positive, on average, for all types of firms in both treatments. With the exception of firms of type 2 in the treatment M2, all average levels of violations are below one unit. This result is not new. The literature has already reported positive average levels of violations in experiments of tradable permits designed to induce perfect compliance in equilibrium. Murphy and Stranlund (2007) report levels of violation between 0.1 to 0.4 units for firms with production capacity of 8 or 17 units, depending on the firm's type. In Cason and Gangadharan (2005) violation rates were between 15% and 37%, depending on the relative costs and benefits of compliance, in treatments where the violation rate is expected to be zero. In spite of the

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[11] We paid US$ 5 as a show up fee in the first sessions of the experiments. We decided to increase it to US$ 7 due to our thin pool of subjects.

average positive levels of violations, the median level of violation is zero for all type of firms in both treatments. Overall, the compliance rate is 70.0%.

Table 2: Descriptive Statistics Permits treatments

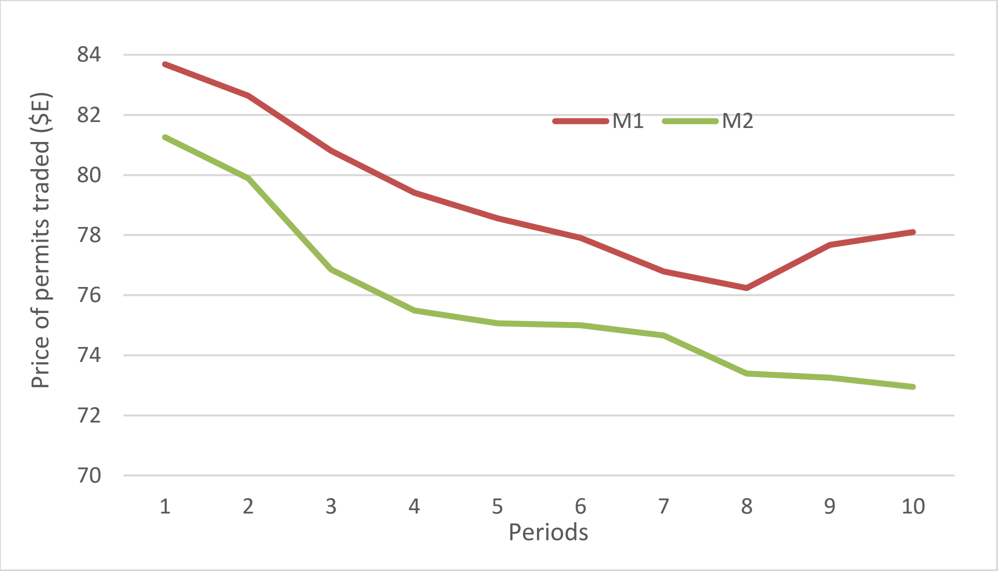
		Average Price per period	Number of Transactions per period	Type 1 ($l_0 = 4$)			Type 2 ($l_0 = 4$)			Type 3 ($l_0 = 6$)			Type 4 ($l_0 = 6$)		
				q	**l**	**v**	**q**	**L**	**v**	**q**	**l**	**v**	**q**	**l**	**v**
TREATMENT M1 - Increasing marginal penalty															
Theory		**74-80**	**10**	**7**	**7**	**0**	**6**	**6**	**0**	**4**	**4**	**0**	**3**	**3**	**0**
Experiments	Mean	79.2	8.5	6.5	5.7	0.8	6.5	5.9	0.6	4.8	4.4	0.4	4.3	3.9	0.3
	Median	79.6	8.0	6.0	6.0	0.0	6.0	6.0	0.0	5.0	4.0	0.0	4.0	4.0	0.0
	Std. Dev.	7.1	2.6	1.3	1.7	1.4	1.4	1.4	1.4	1.2	1.0	0.6	1.4	1.2	0.6
	# obs.	117	117	234	234	234	234	234	234	234	234	234	234	234	234
TREATMENT M2 - Constant marginal penalty															
Theory		**74-80**	**10**	**7**	**7**	**0**	**6**	**6**	**0**	**4**	**4**	**0**	**3**	**3**	**0**
Experiments	Mean	75.8	9.7	6.9	6.2	0.7	6.9	5.5	1.3	5.0	4.4	0.5	4.1	3.8	0.2
	Median	75.7	10.0	7.0	7.0	0.0	7.0	6.0	0.0	5.0	4.0	0.0	4.0	4.0	0.0
	Std. Dev.	6.1	2.9	1.3	1.5	1.6	1.8	2.0	2.5	1.3	1.1	1.4	1.2	1.1	0.6
	# obs.	150	150	300	300	300	300	300	300	300	300	300	300	300	300

Notes: we dropped one transaction with a price of $E 752. A subject in a group went bankrupt in round 7 of Treatment M1. We dropped the observations of rounds 8 – 10 of that group.

We can also see that prospective sellers (firms of type 3 and type 4) withhold a higher-than-expected number of permits on average in both treatments. The other side of the coin is that the final holdings of permits for prospective buyers' (type 1 and type 2 firms) was, on average, lower than expected.

The average price of the permits traded was within the predicted range (74 – 80 experimental pesos) in both treatments, but it was E$ 3.4 higher in the case of the treatment with the increasing marginal penalty (M1). The difference is persistent across periods (see Figure 1). We also observe an average number of transactions in treatment M1 (8.5) that is lower that the predicted level (10) and lower than the average in M2 (9.7).

Figure 1: Evolution of average prices by treatment



5.2 Hypothesis 1 Tests (Market Experiments)

5.2.1 Non-parametric tests

Recall that the experimental markets mimic the situation of a market for pollution permits where the regulator has perfect information of the marginal abatement costs of the firms and uses this information to set the marginal penalty to induce perfect compliance cost-effectively. Hypothesis 1 establishes that in this setting the level of individual and aggregate violations should be the same if the regulator uses a convex or a linear penalty. In an experiment were subjects do not participate in more than one session, each experimental market provides only one independent observation (Davis and Holt, 1993). In this case, Hypothesis 1 is $V^{M1} = V^{M2}$, where V is the sum of violations of the eight subjects that comprise a market, averaged across ten periods, and the superscript M1 or M2 indicate whether the market is enforced with a convex or linear penalty.

According to both the Wilcoxon rank-sum (Mann-Whitney) test ($p = 0.43, z = -0.781, n = 27$) and the median test ($p = 0.547, Pearson\ chi2 = 0.3635, n = 27$), we cannot reject the null hypothesis that the aggregate level of violations is the same between treatments with convex and linear penalties.[12]

5.2.2 Regressions

As commented above, we did allow subjects to participate in more than one session due to a thin population.[13] A natural solution would be to perform the same tests using only the markets in which no subject was repeating, but we have a small number of such

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[12] In the Wilcoxon test the null hypothesis is, more formally, that the distribution of violations is the same in both markets. In the Median test, the null is that the two samples are drawn from populations with the same median violation. We treat the samples as unrelated, because the same group of subjects did not participate in the two treatments.

[13] Of the 120 different subjects that participated in the experiments, 66 subjects showed up only once and 54 subjects showed up more than once (25 subjects showed up 2 times, 19 subjects showed up 3 times, 7 subjects showed up 4 times and 3 subjects showed up 5 times).

groups. Instead, we conduct an econometric analysis controlling for observations belonging to a subject that is repeating participation.[14] Other controls that we included are the following indicators: linear penalty, treatment was first in session (and an interaction with the former), type of subject, period of play and the group of eight (market) in which the subject participated. We add the category of risk aversion of the subject to these indicators. The categories of risk aversion are the following: risk lover, risk-neutral and risk-averse.[15] Finally, we also include an indicator for the subjects that made inconsistent risk choices.[16]

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[14] We identify subjects by matching the amount of total profits made in the session with the amount of the payment in the receipt, where the name of the subject was. This procedure, nevertheless, fails in the case two subjects in the same session made the same amount of profits. This issue prevented us from identifying 8 subjects.

[15] To elicit the risk preferences of the participants, we asked them to answer a Holt and Laury (2002) type of questionnaire. In this questionnaire, subjects made consecutive choices between a certain amount of money and lotteries. The certain amount of money (U$ 800) remained fixed over the consecutive choices, while the lotteries had increasing probabilities of wining the higher prize (U$ 1300) over the lower one (U$300) (see Online Appendix). Based on these choices, we constructed a categorical variable for risk lovers, risk neutral and risk averse individuals, based on the number of the choice in which the subject switched to preferring the lottery.

[16] Subjects made inconsistent choices in 45/216 cases. We assigned a missing value to the risk aversion category in these cases. If the subject participated in more than one session and did not exhibit inconsistent choices every time it participated, we calculated the average risk-aversion category of the subject in the non-missing observations and input it to its missing observations. If the subject had inconsistent choices in all observations, we input the subject the average category of risk aversion of the whole sample. As expected, 80% of the individuals that participated in the experiments exhibited some degree of risk aversion.

We present the results of two regressions in Table 3. In the second column, the dependent variable is the level of individual violations per period, allowing this level to be negative for the case of over-compliance.[17] In this case, we ran a random effects model with robust standard errors. In the third column, we present a random effects Probit model, where the dependent variable is the violation status, a variable that takes the value of one in the case of positive violations and zero otherwise.

Table 3: Violation Regressions

	Random Effects model	**Probit RE model**
Dependent variable:	**Level of individual violation**	**Violation status**
	Coefficient (Std error)	Coefficient (Std error)
Linear penalty	0.895	-2.666**
	(1.178)	(1.250)
First treatment in session	0.641	-1.126
	(0.791)	(1.070)
First treat. * Linear penalty	-1.091	2.640*
	(1.255)	(1.379)
Risk category	-0.639*	-0.482
	(0.330)	(0.326)
Inconsistent risk preferences	0.546**	0.363
	(0.277)	(0.354)
# times participated before	-0.0310	0.205
	(0.0915)	(0.163)
Type indicator	Yes	Yes
Period indicator	Yes	Yes
Group indicator	Yes	Yes
Constant	1.473**	1.607
	(0.714)	(1.382)
N	1,806	1,806
N_clust	112	112
* p<0.1, ** p<.05, *** p<.01		

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[17] Results do not change if the dependent variable is the level of violation censored-at-zero. The reason is that over-compliance was observed only in a few cases.

Table 3 shows that the individual level of violations is not affected by the structure of the penalty function in a statistical significant way. The structure of the penalty does affect the violation status, though. A linear penalty induces more compliance than the convex penalty at the 1% significance level. The effect seems to be somewhat weaker (in economic and statistical senses) if subjects play first the treatments in question.

These results are robust to different specifications and its combinations. More specifically, both results are robust to indicating with a dummy variable whether the subject had participated in an experiment before or not, instead of a variable counting the number of times she participated before. They are also robust to measuring risk preferences in a scale from one to ten, instead of three classes.

The overall results of the tests for Hypothesis 1 is that the penalty structure, as parametrized in our experiments, does not seem to affect in a statistical significant way the aggregate and individual levels of violations in a market for pollution permits but it affects the individual compliance status. Across subjects and periods, the violation rate was higher with a convex penalty (32.3%) than with a linear penalty (27.9%). As expected, 87.6% of the individual levels of violations observed were lower or equal to one unit. This suggests that the effect of the penalty structure may operate at the margin. Effectively, while the distribution of the levels of violations is somewhat more skewed to the right in the case of a linear penalty than in the case of a convex penalty (over compliance is negligible in both cases), the main difference between both treatments occur with the 0-unit and 1-unit levels of violations. Linearizing the penalty for violations produces the 0-unit violations (perfect compliance) to increase from 67% to 72% and the 1-unit to decrease from 22% to 14%, while providing the same incentive at the margin.

What could be driving this result? Although standard theory does not explain the observed differences, it may be of help to answer this question. The equilibrium price of permits in a market for pollution permits is a function of the number of permits issued by the regulator, of the abatement costs parameters of the firms and of the level of the enforcement parameters. According to theory, and the design of the experiment, the equilibrium price of permits should be between E$ 74 and E$80 in both treatments. As we see in Table 2, the average price of traded permits in both treatment were within these bounds. Nevertheless, Table 2 also shows that the average price was higher with a convex penalty (E$ 79.2) than with a linear penalty (E$ 75.8). An increase in the price of permits, for whatever reason, causes a net increase in the cost of compliance. Therefore, an increase in the price of permits may be an obvious channel for the increase in violations. We explore this channel below. To do it, we compare the average market price of permits with linear and convex penalties running a random-effects regression, conditioning on the structure of the penalty, the order of the treatment, the period and the number of the subjects in the experimental market that are not participating for the first time.[18] Standard errors are robust to heteroscedasticity.

We can see in Table 3 that, a linear penalty decrease the price of traded permits between by E$ 6.5 with respect to an identical market enforced with a convex penalty at the 1% significant level. This happens when it should not, because in both markets

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[18] Results do not change if we use a dummy variable equal to one if at least one subject in the market had participated before in a session. They do not change either if we use the market aggregate level of experience (the sum of the number of times the subjects in the market had participated before). They change at the decimal level if we do not condition on the whether subjects in the market had experience or not.

marginal penalties are set high enough to induce compliance. In the third column of table 4 we included the result of a similar regression but with the total number of permits traded in a given period and market as the dependent variable. One can see that the number of trades increases by 2.5 with a linear penalty, but at the 5% significance level.

Table 4: Regressions on the average price and number of transactions

Random effects		
Dependent variable:	**Average price**	**# of Transactions**
	Coefficient (Std. Error)	Coefficient (Std. Error)
Linear Penalty	-6.509***	2.503**
	(1.663)	(1.048)
First	-8.684***	2.689***
	(2.363)	(0.942)
First*M2	6.048*	-2.677*
	(3.337)	(1.468)
# subjects that repeat in the group	0.610**	-0.109
	(0.283)	(0.113)
Period dummies	Yes	Yes
Constant	78.93***	7.871***
	(1.515)	(1.105)
N	267	267
N_clust	27	27
* p<0.1, ** p<.05, *** p<.01		

In sum, the results in Table 4 provide strong evidence that a linear penalty decreases the price of the permits with respect to a convex penalty. Providing a comprehensive explanation of what is driving this phenomenon is beyond the objective of this paper; but we perform some explanatory analysis below.

The explored channels by which the structure of the penalty affects prices of permits are bids and asks of permits. To do it, we perform a series of regressions similar to those presented in Table 4, except that we do not condition on the experience of subjects, to compare different statistics of bids made by buyers (type 1 and 2 firms) and asks made by sellers (type 3 and 4 firms) with linear and convex penalties.[19]

We do not find any statistically significant effect on the structure of the penalty on bids. We do find effects on asks. A linear penalty decreases the minimum ask observed in a given market and period by E$ 5.8 at the 1% significant level. It also decreases the lower 25% ask by a similar amount at the same level of statistical significance. Finally, it decreases the median ask by E$ 4.8 at the 5% significance level. It does not have a statistically significant effect on the upper 75% and maximum ask. Moreover, we do not find any statistically significant effect of the structure of the fine on the accepted bids and we do find it for the case of accepted asks for the same statistics (the minimum, the lower 25% and the median). The conclusion is that potential sellers increase the price at which they are willing to sell with a convex penalty and that is what driving the prices up. We do not observe a similar result on bids by potential buyers. It is the supply side of the market, not the demand side, which drives the effect of penalties on prices.

Finally, we explore whether the structure of the fine changes the number of "expected transactions" (potential sellers selling to potential buyers) or a change in the number of unexpected transactions (potential sellers buying or potential buyers selling). To do this we perform similar regressions to the ones performed for the case of bids and asks, but with "expected transactions" and "unexpected transactions" as the dependent

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[19] We do not present tables with the full results of these regressions for space reasons. They are available upon request.

variable. We do not show the full results for space reasons, but we find that what drives all the effect of the fine on the number of transactions is its effect on "expected transactions". Although we observe secondary market, unexpected transactions, the structure of the penalty does not affect the number of these transactions in any statistically significant magnitude.

5.3 Descriptive Statistics for Standards Experiments

We present in this section the results of the standards experiments. In these experiments, we recall, subjects face an emission standard (maximum legal level of emissions) instead of a market for pollution permits. We set four different emission standards, one for each type of firm, at the theoretical level of emissions (and demand for permits) of each type of firm in the market for tradable permits equilibrium. For the rest, the standards experiments are the same as the market experiments. In particular, we induce perfect compliance with a convex penalty in one treatment (S1) and with a linear penalty in the other treatment (S2). More specifically, the initial level for the marginal penalty is 133.33 for the first unit of violation, but for the following levels the penalty is more severe for S1.

Table 5 shows that, as it was the case with tradable permits, cost-effective perfect enforcement does not produce zero violations. The average level of violations is between 0 and 1 across subjects and periods for every type of firm in both treatments. Also as in the case of tradable permits, the median violation is zero in every case, notwithstanding. Overall, across subjects, periods and treatments, the compliance rate was 62.75%.

Table 5: Descriptive statistics for the standards treatments

	Type 1		Type 2		Type 3		Type 4	
	q	v	q	v	q	v	q	v
TREATMENT S1 Increasing marginal penalty	s =7		s =6		s =4		s =4	
Theory	**7**	**0**	**6**	**0**	**4**	**0**	**3**	**0**
Experiments								
Mean	7.5	0.5	6.6	0.6	4.6	0.6	3.6	0.6
Median	7.0	0.0	6.0	0.0	4.0	0.0	3.0	0.0
StdDev	0.9	0.9	0.9	0.9	0.9	0.9	0.9	0.9
Nº obs	340	340	340	340	340	340	340	340
TREATMENT S2 Constant marginal penalty	s =7		s =6		s =4		s =3	
Theory	**7**	**0**	**6**	**0**	**4**	**0**	**3**	**0**
Experiments								
Mean	7.6	0.6	6.8	0.8	4.7	0.7	3.6	0.6
Median	7.0	0.0	6.0	0.0	4.0	0.0	3.0	0.0
StdDev	1.0	1.0	1.4	1.4	1.2	1.2	1.1	1.1
Nº obs	180	180	180	180	170	170	180	180

Notes: Seven subjects showed up in a session. In this session, we had a type-4 subject missing. Fourteen subjects showed up in another session, where we had a type-3 and a type-4 subjects missing. In three more session, fourteen subjects showed up in excess of a multiple of eight. The program allocated these subjects a type 4. Three subjects went bankrupt in two different sessions while playing S1 treatment. They were all type 4. We dropped their observations. We replaced the observations of the 2 missing 3 and bankrupted type-4 subjects with five of the type-4 subjects from the sessions with excess of subjects.

5.4 Hypothesis 2 Tests (Standard Experiments)

5.4.1 Non-parametric tests

We now turn to the test of Hypothesis 2. Recalling, this hypothesis states that there should be no difference in individual levels of violations if a regulator uses a convex or a linear penalty, when both penalties produce the same incentive in the margin and this is enough to induce compliance. More formally, Hypothesis 2 is $v^{S1} = v^{S2}$, where v is the individual level of violation of a given type of firm, averaged across ten periods, and the superscript S1 or S2 indicate whether the firm faces a convex or linear penalty. According to both the Wilcoxon rank-sum (Mann-Whitney) test ($p = 0.87, z = -0.16, n = 207$) and the median test ($p = 0.92, Pearson\ chi2 = 0.01, n =$

207), we cannot reject the null hypothesis that the average individual levels of violations are the same between treatments with convex and linear penalties.

5.4.2 Regressions

As in the case of the market experiments, the non-parametric tests of the previous section may be biased because, as commented in Section 4, we allow subjects to participate in the sessions more than once. The 207 observations in the above tests are not decisions of 207 different subjects but of 113.[20] We tackle this as we did in the previous section with the market experiments: comparing the level of individual violations with a linear and a convex penalty by using a random-effects regression where we control for the number of times the subject participated in an experiment before the one in question. We present the results of this econometric analysis in Table 6 below.

Table 6: Violation Regressions

	Random Effects model	**Probit RE model**
Dependent variable:	**Level of individual violation**	**Violation status**
	Coefficient (Std error)	Coefficient (Std error)
Linear Penalty	-0.216	-0.411
	(0.237)	(0.481)
First	-0.0508	0.229
	(0.211)	(0.487)
First treat. * Linear Penalty	0.181	0.0287
	(0.311)	(0.592)
Risk category	-0.158	0.0569
	(0.144)	(0.228)
Inconsistent risk preferences	0.162	0.451*
	(0.117)	(0.250)
# times participated before	-0.0781*	-0.295***
	(0.0409)	(0.0964)

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[20] Of the 113 different subjects that participated in the standards experiments, 65 subjects showed up only once, 19 subjects showed up 2 times, 18 subjects showed up 3 times, 5 subjects showed up 4 times and 6 subjects showed up 5 times.

Type indicator	Yes	Yes
Period indicator	Yes	Yes
Group Indicator	Yes	Yes
Constant	1.262***	-0.0461
	(0.438)	(0.810)
Number of observations	1,860	1,860
Number of subjects	108	108
* p<0.1, ** p<.05, *** p<.01		

Replicating the analysis we presented for the market experiments, we present the results of two random effects regressions with robust standard errors: one with the uncensored level of individual violation as the dependent variable (in the second column) and a Probit model with the violation status as the dependent variable (in the third one). According to these estimations, there is no statistically significant difference on the level or status of violations between treatments.[21]

6 Discussion and conclusions

We have found that a convex penalty, as compared to a linear penalty, increases the market price of pollution permits and increases the violation rate of firms. We do not observe this effect of convex penalties on the compliance status of firms with emission standards. The latter suggests that the channel by which a convex penalty increases noncompliance is through the increase in the prices of permits.

The effect of the structure of the fine on the price of permits operates through an increase in the asking prices of sellers, not on the bids by suppliers. With convex penalties, sellers are not willing to sell a permit at a price as low as with linear penalties.

These results have important policy implications. First, if firms´ rate of compliance in a cap and trade program are lower with convex penalties (relative to

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[21] We have only 2 observations in 1860 in which a subject decided to over comply.

linear penalties), the regulator may need inspect firms more frequently in order to attain the same cap of emissions when using convex penalties. This means that using linear penalties would be cost effective with respect to using convex penalties, even when perfectly enforcing the cap.

Relatedly, it has been recognized long ago that financial penalties are “close relatives” of safety valves (Jacoby and Ellerman, 2004). In the words of Sigman (2012): “(i)n an emission trading system, non- Draconian fines can play the role of a “safety valve,” allowing polluters to avoid buying permits during price spikes and, thus, effectively setting a ceiling on the marginal cost of carbon reductions” (p. 216). Our results are consistent with these observations.

Finally, our results may have implications for the initial allocation of permits. Given a convex penalty, a regulator who seeks to design a cost-effective program should try to increase the allocation of permits to would be buyers firms and to reduce the initial allocation of permits to would be sellers firms. This will reduce the upward pressure on permit prices, reduce the violation rates of firms, and consequently reducing enforcement costs.

This work can be extended in different ways. Most notably, additional experimental designs that include other penalty structures can help to understand how the enforcement structures affect incentives for compliance with regulations. For example, it is common that penaties depends also on past compliance behavior of firms and consider make good provisions, features that we did not consider in our analysis. Also, variation in the parameters could help us to shed light on the ways that enforcement and monitoring effort provides incentives for compliance under different penalty structures and regulatory instruments.

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