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DOES THE STRUCTURE OF THE FINE MATTER?

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Does the structure of the fine matter?

Abstract: We study individual compliance behavior with respect to a legal norm in an experimental setting under two different regulatory instruments: emission standards and tradable pollution permits. Compliance to the same set of standards and expected permit holdings was induced with different structures of the fine schedule, namely: a linear and a strictly convex penalty function. Even though our design induces perfect compliance, we find that there are violations in both emissions standards and tradable permits systems, regardless of the penalty structure. Nevertheless, the extent of violations is affected by the penalty parameters under emissions standards, but not under a tradable pollution permits. Notwithstanding, we find that the penalty design has an effect on the average price of permits traded, its dispersion and the number of trades.

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

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

1. Introduction

The seminal work on the economic theory of crime by Becker (1968) inspired an uncountable number of studies concerned with the use of pecuniary penalties to induce economic agents to comply with established rules in different fields, ranging from criminal behavior to tax compliance and many other applications. A basic premise of this traditional line of work establishes that compliance rates will increase alongside with the severity of penalties.[1] But

[1] This conclusion was later questioned by several studies with different arguments. Block and Heneike (1975), for example, focus on the need to thoroughly specify the choice problems faced by agents and established that the conclusions widely accepted by economists were valid only in very special cases because they imposed strong restrictions in the preferences and the relative

surprisingly, to our knowledge, a relative small number of studies have focused on the role that different penalty schedules may have on the number and size of violations. One exception is Pencavel (1979), who focuses on the effect of altering the penalty function on tax declarations. He remarks that some implications of the standard tax evasion theory rely strongly in narrow assumptions. Nevertheless, in his model, because the regulator cannot observe the size of the violation (the amount of unreported income) without an audit, the marginal expected penalty does not depend on the size of the violation, as it is the case in environmental economics literature, but on the amount of declared income. Rodriguez (1992) studies the effects of establishing a speed limit for drivers. Considering different penalty structures he shows that a constant or decreasing fine schedule may induce an increase of speed for some drivers, compared with the unrestricted situation. This result is also possible with strictly increasing penalties if the marginal penalty is too low.

The questions that we seek to answer in this study are the following. First, does a penalty schedule that is increasing in the level of violation induces a different average level of violation in a system of tradable pollution permits than a penalty schedule that is linear in the level of violation? Second, does it induce a different rate of violations? And finally, are the answers to the above two questions different in the case of emission standards?

In the economic literature of the enforcement of environmental norms, the structure of the penalty schedule plays no marginal role in equilibrium. In the case of emission standards, risk-neutral, profit-maximizing firms will comply with an emission standard if and only if the marginal abatement cost evaluated at the standard is less or equal than the marginal expected fine (Harford, 1978). This is true whether the structure of the penalty schedule is linear or increasing in the size of violations. A similar argument is valid in the case of tradable pollution permits. In this case, what is needed to attain full compliance in equilibrium is that the regulator

magnitude of the parameters involved. Further limitations to the traditional analysis were introduced incorporating concepts from other fields such as psychology. Akerlof and Dickens (1982) and Dickens (1986) made use of the theory of cognitive dissonance to question the effectiveness of increasing punishment to increase compliance, and find that in certain circumstances higher penalties cause more violations to the rules when the opportunity arises.

sets the marginal fine (the fine for a slight violation of the permits holding) at least as large as the equilibrium price of the pollution permits, the firm´s marginal benefit from violating (Stranlund and Dhanda, 1999). Moreover, the structure of the penalty schedule should have no effect on the equilibrium price of the pollution permits, and therefore on the level of individual and aggregate level of emissions, the demand for permits and compliance levels.[2] This issue is important from a policy perspective because recent works in the literature establish that when enforcement costs are added to the abatement costs, cost-effectiveness of a pollution control program calls for perfect compliance (Stranlund (2007), Arguedas (2008)). In the case of tradable pollution permits, an additional necessary condition is that the fine schedule should be linear in the level of violations, i.e.: the marginal penalty should be flat (Caffera and Chávez (2011). Nevertheless, if firms actually react differently to linear penalties than to convex penalties, some penalty schedules may result in higher emissions and violations than others, altering the relative cost-effectiveness of inducing compliance, or the possibility of inducing compliance. Moreover, if the penalty structure affects the level of emissions and the demanded level of permits, it must do so through the permit price. If true, asking whether the penalty structure affects the equilibrium price of the tradable pollution permits may have implications in terms of the need and the design of the price "safety valve". Finally, on a simpler ground, while most of the theoretical literature uses a penalty function that is convex in the level of violation, constant marginal penalties are common in actual tradable emissions permits systems (see for example Boemare and Quirion (2002)) and Restiani and Betz (2010). Nevertheless, the relative effectiveness and cost-effectiveness of these alternatives systems has not been studied.

To our knowledge, the only exception to the above is the study by Restiani and Betz (2010). These authors explore the effect of three types of penalties on the compliance levels in an experimental market for pollution permits: a fixed rate (constant marginal penalty), a make-good provision system (off-set penalty), and a mixed penalty that combines both. They conclude that different penalty designs do not translate into different permit prices. Regarding compliance

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rate, they find that it is higher for the fixed rate penalty, despite theory predicts no difference provided the penalties levels are above equilibrium price. Nevertheless, their analysis assume that there is perfect monitoring. In contrast, we analyse the impact of different penalty structures on the performance of a transferable emissions permits system considering the possibility of imperfect monitoring –an important actual element of any environmental policy- together with variations in the structure of the penalty under consideration.

In this paper we compare the level of individual violations with respect to a legal norm in an experimental setting. Although the experiments were framed as a decision with respect to the level of production of an unspecified good, they were designed to mimic the choice of the level of emissions of a given pollutant by profit-maximizing firms, under two different regulatory instruments: emission standards and tradable pollution permits. In both types of experiments, the enforcement parameters (the probability of inspection and the value of the fine) were set such as to induce perfect compliance to all subjects, under the assumption of risk neutrality. Compliance to the same set of standards and expected permit holdings was induced with different structures of the fine schedule, namely: a linear and a convex penalty function.

The paper is organized as follows. In section 2, we present the main hypotheses we want to evaluate with our laboratory experiments. Section 3 contains a description of the experimental design and procedures. Section 4 presents the results. Finally, in Section 5, we put forward concluding remarks from our work.

2. Compliance Behavior and Hypotheses

To analyse the individual firm's compliance behaviour, we consider a risk-neutral firm operating either under 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 $c''(q) > 0$]. We index firms by i and denote the total number of firms as n (whenever possible, we avoid the use of a specific firm index for simplicity). The environmental target is a fixed aggregate level of emissions $\sum_{i=1}^{n} q_i = \overline{Q}$, exogenously determined by the regulatory authority.

2.1 Emissions Standards

We first consider the case of a prescriptive environmental policy in which each firm faces an emissions standard s. This is a maximum allowable (legal) level of emissions for each firm. Emissions standards for all firms satisfy $\sum_i s_i = \overline{Q}$. In this context, an emissions violation v occurs when the firm's emissions exceed the emissions standard: $v = q - s > 0$. The firm is compliant otherwise. The firm is audited with a random exogenous probability π. An audit provides the regulator perfect information about firms' compliance status. If the firm is audited and found in violation, a penalty $f(v)$ is imposed. Following Stranlund (2007), we assume that the structure of the penalty function is $f(q - s) = \varphi(q - s) + (\gamma/2)(q - s)^2$, with $\varphi > 0$ and $\gamma \geq 0$. When $\varphi > 0$ and $\gamma = 0$ the penalty is linear.[3]

Under an emissions standard, a firm chooses the level of emissions to minimize its total expected compliance cost, which consists 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 $-c'(s) \leq \pi f'(0)$ [Heyes (2000), Malik (1992), Harford (1978)]. For the penalty structures considered in this work, the marginal penalty is $f'(v) = \varphi + \gamma v$, and the condition turns to $-c'(s) \leq \pi\varphi$. Thus, a firm will be compliant with the emission standard if the expected penalty for a marginal violation is no lower than the marginal abatement cost at that level of emissions (the benefit of a marginal violation). Otherwise, the firm is going to choose a level of emissions $q(s,\pi) > s$, where $q(s, \pi, \varphi, \gamma)$ is the solution to $-c'(q) = \pi[\varphi + \gamma(q - s)]$.

[3] We follow Arguedas (2008) and call the coefficient φ of the fine "the linear gravity component" and the coefficient γ the "progressive gravity component".

2.2 Transferable Emission Permits System

Under a system of transferable emissions permits, a total of $L = \overline{Q}$ licenses are issued by a regulatory authority, each of which confers the legal right to release one unit of emissions to the firm that possesses it. Each individual firm is a perfect competitor in the license market, so the license market generates an equilibrium license price p. 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 (v) is $v = q - l > 0$, for $q > l$.

In a transferable emission permits system, a firm chooses its emissions and permits to minimize compliance costs: abatement costs, receipts or expenditures from selling or buying permits, and the expected penalty – taking the enforcement strategy as given. We know that in this system a firm is compliant if and only if $-c'(l) \leq \pi f'(0)$. (See for example, Malik (1990) and Stranlund and Dhanda (1999)). We also know, that the optimal choice of emissions requires $-c'(q) = p$, which implicitly defines $q(p)$. If compliant, the choice of emissions for firm i equals its demand of permits, that is $q_i(p) = l_i(p)$. The condition for the perfect – compliance equilibrium in the market for pollution permits is $\sum_{i=1}^{n} l_i(p) = L = \overline{Q} = \sum_{i=1}^{n} q_i$, which implicitly defines the perfect-compliance equilibrium price of permits as a function of the total number of licenses; that is, $p^c(L)$. Hence, under a transferable emissions permit system, a firm will be compliant whenever $p(L) \leq \pi\varphi$; i.e: when the expected marginal penalty is not lower than the price of a permit.

When the firm is noncompliant, it is going to choose the demand of permits $l(p, \pi, \varphi, \gamma) < q(p)$, where $l(p, \pi, \varphi, \gamma)$ is the solution to $p = \pi[\varphi + \gamma(q(p) - l)]$, and the level of violation is $v(p, \pi, \varphi, \gamma) = q(p) - l(p, \pi, \varphi, \gamma)$. The permit market equilibrium condition when violations occurs is $\sum_{i=1}^{n} l_i(p, \pi, \varphi, \gamma) = L = \overline{Q} < \sum_{i=1}^{n} q_i$, which implicitly defines the equilibrium permit price as a function of the total number of licenses and enforcement parameters; that is, $p^{nc}(L, \bar{\pi}, \varphi, \gamma)$, where $\bar{\pi}$ is a vector of monitoring probabilities on regulated firms.

2.3 Hypotheses

Hypothesis 1: *In a tradable emission permits system designed to induce perfect compliance, the compliance level of the polluting firms does not depend on the penalty structure.*

As previously discussed, under a transferable emissions permit system the firm comply if and only if $-c'(q = l) = p \leq f'(0)$. This can be obtained with any penalty structure. In other words, it does not depend on whether $f\ (v)\ =\ \varphi \times v\ +\ \gamma/2 \times v^2$, or $f(v) = \varphi \times v$. (Where $v = e - l$ if $e > l$ and zero otherwise). Consequently, according to the standard theoretical model of the enforcement of environmental regulations, we should not expect the percentage of violations, or the average level of violation, or the aggregate level of violations to differ when perfect compliance is induced by a penalty schedule that has a linear and a progressive gravity component, as compared to when perfect compliance is induced by a penalty schedule that has only a linear gravity component, provided that both schemes are designed such that $-c'(q = l) = p < \pi \times f'(0) = \pi \times\ \varphi$.

Hypothesis 2: *In system of emissions standards that is designed to induce perfect compliance, the compliance level of the polluting firms does not depend on the penalty structure.*

The reasoning for the case of emission standards is exactly 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, a firm i complies with the emission standard s_i, if and only if $-c_i'(q_i - s_i) \leq \pi \times f'(0) = \pi \times\ \varphi,$ where the sub-index i indicates that the standard, the monitoring probability, and the abatement cost function are firm specific, but the fine is not. As it is the case with tradable permits, this compliance condition does not depend on whether f is convex or linear in the level of violation.

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 from these units differ between subjects (see Table 1). The four marginal benefits (obtained from Cason and Gangadharan (2006)) gave place to four "types" of subjects.

Table 1: Assigned marginal benefits of production of the fictitious good

	Marginal Benefits of Production			
Units produced	Type 1: subjects 1 and 2	Type 2: subjects 3 and 4	Type 3: subjects 5 and 6	Type 4: subjects 7 and 8
1	161	151	129	125
2	145	134	113	105
3	130	119	98	88
4	116	106	84	74
5	103	95	73	63
6	91	86	63	54
7	80	79	53	47
8	70	74	44	42
9	61	70	35	38
10	53	67	27	35

These schedules of marginal benefits were the same through all the experiments and were randomly assigned between subjects.

We constructed 4 different treatments for these experiments, varying the following variables: (1) the regulatory instrument (standards / tradable permits) and (2) the structure of the penalty function.

3.1 Tradable permits

In the permits experiments, subjects had to possess a permit in order to be legally able to produce one unit of the good. Consequently, subjects had to decide how much to produce of the fictitious good and how many permits to buy or sell. In order to buy or sell permits, subjects participated in a double-auction market, one permit at a time. A market was comprised by 8 subjects, 2 of each type. After their decision, at the end of each period, the subjects were audited with a known homogeneous predetermined and exogenous probabilityπ. 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 (l_i) at the end of the period. If the level of production chosen was higher than the number of permits possessed, the subject was automatically fined. The subjects had the information on the 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 2 treatments for the case of markets for permits (see Table 2) designed to induce compliance (M1 and M2). In Treatment M1, the total number of tradable permits supplied to each group of 8 subjects was 40. The initial allocation was 4 permits for subjects of type 1 and 2, the prospective buyers, and 6 permits for subjects of type 3 and 4, the prospective sellers. 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 is 10). The enforcement parameters took the values $\varphi = 100$, $\gamma = 66{,}67$ and $\pi = 0.6$ in M1. These values are sufficient to induce all types of firms to comply with their permit holdings. The resulting perfect-compliance equilibrium price of the market is expected to be between 74 experimental pesos (E\$) and E\$ 80. Treatment M2 is exactly the same as Treatment M1, except for the fine schedule. More precisely, in Treatment M2 $\varphi = 133$, and $\gamma = 0$. With this parameterization, the Treatment M2 induces the same equilibrium price of permits and individual level of emissions as the treatment M1 does.[4] Hence, the expected level of aggregate emissions remains in 40 units. This is a unique feature of our design.

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

3.2 Standards

In the standards experiments subjects faced a maximum allowable level of emissions (the standard), and had to decide how much to emit. The auditing procedure was exactly the same as in the case of tradable permits; except that in the case of standards a violation is defined as $q_i - s_i > 0$, where s_i is the standard for type i. Similar to the case of tradable permits, we constructed 2 treatments for the case of emission standards. These are labeled S1 and S2 in Table 2. In treatment S1, the emission standards are 7, 6, 4 and 3 for firms' types 1 to 4, respectively. The monitoring probabilities are 0.6, 0.65, 0.63 and 0.66. Finally, violations are fined with the same penalty function used in M1; $\varphi = 100$ and $\gamma = 66{,}67$. This policy induces, so the expected aggregate level of production is 40 units in a group of 8 subjects.[5] In Treatment S2, the standards and monitoring probabilities are the same as in S1, so that the aggregate cap of emissions is 40, but we change the structure of the penalty to the constant marginal penalty used in M2; $\varphi = 100$ and $\gamma = 0$. Treatment S2 induces perfect compliance, as S1, but with a linear penalty schedule.

[5] In the standards experiments, not all groups had 8 subjects, and therefore the number of subjects showing up for a session was not always multiple of 8. This was not a problem because in these experiments the subjects do not interact with each other in any form.

Table 2: Summary of Treatment design

PERMITS MARKET TREATMENTS										
	Penalty function: $\varphi v + \frac{\gamma}{2}v^2$		**π**	**Total permits in the market**	**Equilibrium**				**Policy induces**	**Equilibrium price**
	φ	γ			Type	q	L	v		
Treatment M1	100	66,666 7	0,60	40	1	7	7	0	Compliance	74-80
					2	6	6	0		
					3	4	4	0		
					4	3	3	0		
Treatment M2	133	0	0,60	40	1	7	7	0	Compliance	74-80
					2	6	6	0		
					3	4	4	0		
					4	3	3	0		

STANDARDS TREATMENTS									
	Penalty function: $\varphi v + \frac{\gamma}{2}v^2$		**π**	**Aggregate Standard**	**Equilibrium**				**Policy induces**
	φ	γ			Type	q	l	v	
Treatment S1	100	66,666 7	Type 1: 0,602	40	1	7	7	0	Compliance
			Type 2: 0,647		2	6	6	0	
			Type 3: 0,630		3	4	4	0	
			Type 4: 0,662		4	3	3	0	
Treatment S2	133	0	Type 1: 0,602	40	1	7	7	0	Compliance
			Type 2: 0,647		2	6	6	0	
			Type 3: 0,630		3	4	4	0	
			Type 4: 0,662		4	3	3	0	

4. Experimental Procedures

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

Participants were recruited 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 sessions, all subjects that showed up that day at that time played two treatments of tradable permits or two treatments of emission standards. We allocated the standards and permits sessions evenly in the mornings and afternoon, and on different days of the week to minimize any possible selection bias.

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 all the subjects played a treatment in which we induce perfect compliance (M1 or M2 in a permits session; S1 or S2 in a standards session). In the other treatment all the subjects played a treatment in which the probability of being inspected was lower, and therefore violations were induced or allowed. The order of treatments differed between groups in a session. Approximately half of the people that showed up in the room for that session played the compliance treatment first, and the other half played the violation treatment first. For this work, we use the data generated in the compliance treatments.

Before the beginning of the experiments, instructions were handed out to subjects. The instructions were read aloud and questions were answered. Prior to the first round of the first treatment, subjects played 2 trial rounds of the first treatment in the standards sessions, and 3 trial rounds of the first treatment 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 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 probability of being monitored, 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.

The exchange rate between the experimental and Uruguayan pesos was set in order to produce 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. Subjects were paid around 7 US$ for showing up on time in the experiments sessions and earned more money from their participation in the experiment.[6] Including this show-up fee, the average total earning for both the permits and standards experiments was 28 US$. The median payoff was also US$28 in the permits experiments, and US$ 26.7 for the case of standards. The minimum payoff was US$ 14 in the standards experiments and US$ 17.2 in the permits experiments. The maxima were US$ 34 and US$ 38, respectively. The total number of experimental subjects that participated in the permits experiments was 328. The total number of experimental subjects that participated in the standards experiments was 401.

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 Tradable Permits

[redacted]Descriptive statistics

In this section we present the results derived from the two permits experiments. Once again, both treatments induce perfect compliance, but Treatment M1 does so with an increasing marginal penalty, whereas Treatment M2 does so with a constant marginal penalty. Both schedules imply a marginal penalty of 133.33 for the first unit of violation.

In Table 3 we report average and median values of emissions, permits holdings and violations. We notice, on average, violations are positive for all types of firms in both treatments. The empirical result of positive average levels of violations for schemes designed to induce

[6] In the first session of the standards experiments we paid US$ 5 as a show up fee. After this first session we decided to increase the show up fee to US$ 7 to increase the incentive of showing up.

compliance has already been reported in the literature. Murphy and Stranlund's (2007) experiments of tradable permits markets report levels of violation between 0.1 to 0.4 units for different treatments designed where the competitive equilibrium is to comply. Cason and Gangadharan (2005) study compliance behavior in the framework of dynamic repeated game, where past compliance behavior determines whether the person is assigned to a more severe enforcement group. They find that violation rates are close to 20% in treatments where the violation rate is expected to be zero.

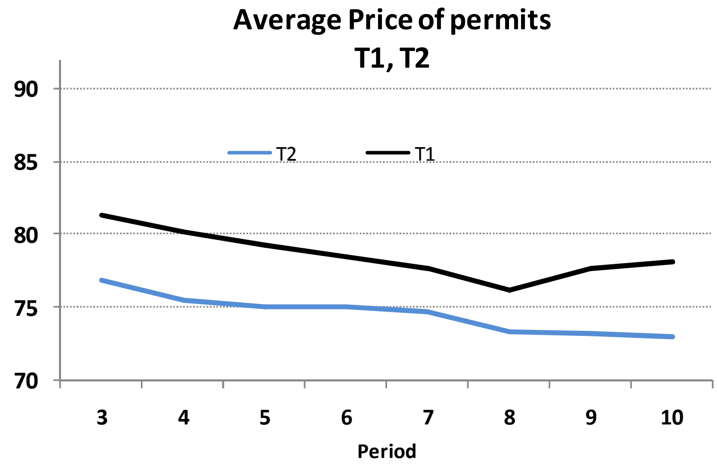
In spite of the average positive levels of violations, the median level of violation is zero for all type of firms in both treatments. All in all, for the permits treatments, the compliance rate is around 70% (see Graph 1 in the appendix).

Table 3: Descriptive statistics Permits treatments

PERMIT MARKET TREATMENTS		Mean price per period	Number of transactions per period	Type 1			Type 2			Type 3			Type 4		
				q	l	v	q	l	v	q	l	v	q	l	v
TREATMENT 1															
	Theory	**74-80**	**10**	**7**	**7**	**0**	**6**	**6**	**0**	**4**	**4**	**0**	**3**	**3**	**0**
Experiments															
	Mean	80.1	8.5	6.50	5.65	0.85	6.51	6.10	0.41	4.77	4.38	0.39	4.24	3.88	0.36
	Median	79.5	8.0	7.00	6.00	0.00	6.00	6.00	0.00	5.00	4.00	0.00	4.00	4.00	0.00
	Std Dev	15.3	2.4	1.34	1.71	1.54	1.33	1.16	0.81	1.16	1.00	0.55	1.31	1.10	0.63
	Nº obs	88.0		176			176			176			176		
TREATMENT 2															
	Theory	**74-80**	**10**	**7**	**7**	**0**	**6**	**6**	**0**	**4**	**4**	**0**	**3**	**3**	**0**
Experiments															
	Mean	74.6	10.0	7.06	6.27	0.80	7.04	5.59	1.45	4.98	4.40	0.57	3.97	3.73	0.24
	Median	75.0	10.0	7.00	7.00	0.00	7.00	6.00	0.00	5.00	4.00	0.00	4.00	4.00	0.00
	Std Dev	5.1	2.8	1.31	1.58	1.76	1.75	2.06	2.63	1.26	1.09	1.44	1.22	1.03	0.57
	Nº obs	120.0		240			240			240			240		

Following the analysis of the descriptive statistics, the positive levels of violations are observed together with higher than expected average quantities of permits demanded for the of the firms that were prospective sellers (type 3 and type 4 firms) in both treatments. The other side of the coin was that the final holdings of permits for prospective buyers' (type 1 and type 2 firms) was, on average, lower than expected (with one exception). The average price was within the predicted range (74 – 80 experimental pesos) in both treatments, although it was in the upper limit in the case of markets enforced with an increasing marginal penalty schedule (M1) and in the lower limit in the case of the markets enforced with a flat marginal penalty schedule (M2).

Since this is a repeated game it is worth looking if there is some evolution of price levels as the rounds progress. The data shows a declining pattern of price levels in both treatments, although there is a rebound in treatment M1 in period 8.



Our interest is not in the absolute values of the variables, but in the difference between treatments. Nevertheless, as an informative first step, we present the results of a series of tests of equality comparing the average values with their theoretical predictions for variables of interest. In table 4 we report the results for the tests performed to the average across round for presentation reasons but the tests for individual rounds show the same results. We find that the observed mean levels are different form predicted levels at a 5% of significance, with the exception of the emissions for type 1 firms in Treatment M2.

Table 4: Comparison of experimental values with theoretical predictions

	Null hypothesis	p-value	Conclusion (level of significance 5%)
Individual violations			
Treatment 1			
All types of firms	v=0	0.0000	Reject null hypothesis
Treatment 2			
All types of firms	v=0	0.0000	Reject null hypothesis
Individual emissions			
Treatment 1			
Type 1	q=7	0.0331	Reject null hypothesis
Type 2	q=6	0.0477	Reject null hypothesis
Type 3	q=4	0.0022	Reject null hypothesis
Type 4	q=3	0.0000	Reject null hypothesis
Treatment 2			
Type 1	q=7	0.7456	Cannot reject null hypothesis
Type 2	q=6	0.0009	Reject null hypothesis
Type 3	q=4	0.0000	Reject null hypothesis
Type 4	q=3	0.0000	Reject null hypothesis
Permits holdings			
Treatment 1			
Type 1	l=7	0.0007	Reject null hypothesis
Type 2	l=6	0.6648	Cannot reject null hypothesis
Type 3	l=4	0.0571	Cannot reject null hypothesis
Type 4	l=3	0.0001	Reject null hypothesis
Treatment 2			
Type 1	l=7	0.0022	Reject null hypothesis
Type 2	l=6	0.2585	Cannot reject null hypothesis
Type 3	l=4	0.0185	Reject null hypothesis
Type 4	l=3	0.0000	Reject null hypothesis
Aggregated violations			
Treatment 1	V=0	0.0000	Reject null hypothesis
Treatment 2	V=0	0.0000	Reject null hypothesis
Aggregated emissions			
Treatment 1	Q=40	0.0017	Reject null hypothesis
Treatment 2	Q=40	0.0000	Reject null hypothesis

[redacted] The effect of the fine structure

Now we turn to the comparison of variables between the two treatments applied. The experiments were designed to induce the same level of emissions, violations and prices, with a different penalty structure. For that purpose we use two non-parametric tests that involve a minimum of assumptions, not requiring a normal distribution. The Wilcoxon rank sum test evaluates the hypothesis that two independent samples are from populations with the same distribution. Additionally, we perform the test of equality of medians, where the null hypothesis states that the samples were drawn from populations with the same median.

The first two rounds of the game are excluded because of possible learning effects. So we retain eight observations per subject (rounds 3 to 10).

Table 5: Comparison of experimental values between Treatments

Effects of penalty design- PERMITS TREATMENTS		
	Two-sample Wilcoxon rank-sum (Mann-Whitney) test (1)	**Median Test (2)**
	Prob > |z|	Pearson chi2 probability (continuity corrected)
EMISSIONS		
Compliance treatments (T1 and T2)		
Average rounds 3 to 10		
Type 1	0.171200	0.784000
Type 2	0.133800	0.520000
Type 3	0.298900	0.689000
Type 4	0.437900	0.927000
PERMITS HOLDINGS		
Compliance treatments (T1 and T2)		
Average rounds 3 to 10		
Type 1	0.260500	0.784000
Type 2	0.820700	0.976000
Type 3	0.452300	0.563000
Type 4	0.569700	0.831000
AVERAGE PRICES		
Compliance treatments (T1 and T2)		
Average rounds 3 to 10	0.000000	0.000000
INDIVIDUAL VIOLATIONS		
Compliance treatments (T1 and T2)		
Average rounds 3 to 10		
Type 1	0.6254	0.411
Type 2	0.8399	0.877
Type 3	0.1890	0.605
Type 4	0.3895	0.605
AGGREGATE EMISSIONS		
Compliance treatments (T1 and T2)		
Average rounds 3 to 10	0.298800	0.394000
AGGREGATE LEVEL OF VIOLATIONS		
Compliance treatments (T1 and T2)		
Average rounds 3 to 10	0.557600	0.259000

(1) Null hypothesis: violation(treatment==a) ~ violation(treatment==b)

(2) Null hypothesis: the 2 samples were drawn from populations with the same median.

These tests indicate that we cannot reject the hypothesis that emissions and violations in both treatments have the same median. However, the price levels seem to have significant differences. In fact, the results of the median test indicate that the median price for Treatment M1 is higher than the one for Treatment M2.

These conclusions are robust to a regression analysis in which we run random effects models conditioning the effect of the fine schedule on the unconstrained level of violation (first column of Table 6) and the censored-at-zero level of violation on observables. First, we include variables for the type of firm, since the theoretical predicted values vary across firm's types. We also include controls for the round being played in order to take into account any changes that may occur as the game develops in repeated periods. Following Caffera and Chávez (2012) who found some evidence of an effect of whether the treatment was played first or second (in every session each subject played two treatments), we included this control in our regressions. Finally, we include controls for risk preferences of the players. To elicit the risk preferences of the participants, they were presented with a questionnaire replicating the lottery designed by Holt and Laury (2002). These authors design a menu of choices between lotteries in order to obtain a measure of the degree of risk aversion. Therefore every subject is assigned with an index according to the choices made in this lottery. This is a categorical variable that takes the values from zero to ten, the value zero being assigned to the most risk preferring attitude.

As expected, some degree of risk aversion prevails in around 85% of the individuals that participated in the experiments.

In column 1 we present the results for the random effect regression and in column 2 the results fo the censored model, were the dependent variable is the level of violation censored at zero. We find that there is no statistically significant effect of the penalty schedule on the average level of violations. (See Table 6).

Table 6: Regression on the violation levels

Dependent variable: Level of violations	Random Effects model Coefficient (Std error)	Tobit RE model Coefficient (Std error)
Compliance treatment with constant marginal penalty (T2)	0.136	0.136
	-0.212	-0.193
First	0.03	0.031
	-0.194	-0.222
Type= 2	0.241	0.241
	-0.255	-0.2
Type= 3	-0.12	-0.121
	-0.191	-0.207
Type= 4	-0.196	-0.197
	-0.206	-0.217
Period=3	-0.373***	-0.376***
	-0.108	-0.08
Period=4	-0.187**	-0.187**
	-0.088	-0.08
Period=5	-0.199**	-0.201**
	-0.084	-0.08
Period=6	-0.211**	-0.211***
	-0.096	-0.08
Period=7	-0.260***	-0.260***
	-0.099	-0.081
Period=8	-0.145*	-0.145*
	-0.074	-0.08
Period=9	-0.12	-0.12
	-0.083	-0.08
Risk Aversion=3 *	1.753	1.754***
	-1.366	-0.554
Risk Aversion=4	-0.361	-0.362
	-1.179	-0.542
Risk Aversion=5	-0.872	-0.871*
	-1.086	-0.507
Risk Aversion=6	-1.085	-1.085**
	-1.092	-0.475
Risk Aversion=7	-0.902	-0.903*
	-1.095	-0.482
Risk Aversion=8	-0.878	-0.878*
	-1.087	-0.499
Risk Aversion=9	-0.475	-0.48
	-1.173	-0.541
_cons	1.414	1.415***
	-1.142	-0.494
N	1321	1321
N_clust	166	
* p<0.1, ** p<.05, *** p<.01		
Base cathegories: type of firm: type 1; period: period 10; index of risk aversion: risk_av =0 (highly risk loving)		

As we did with non-parametric tests, what also find a statistically significant effect of the penalty schedule on the average price of permits traded, according to a random effects model whose results we can see in Table 7. Conditioning on whether the compliance treatment of interest was played before or after the violation treatment in the session, we can observe in Table 7 that the average value of permits traded was lower in every period of the treatment in which the market was enforced by a constant marginal penalty as compared with the average value of permits traded in the treatment in which the market was enforced with an increasing marginal penalty. The random effects analysis allows us also to explore the effect of the penalty schedule on the dispersion of the price of permits traded. Observing the results in column 2 of Table 7, we do not observe such an effect when we analyze the dispersion of prices. The penalty structure appears to affect the mean price of permits traded but not the standard deviation of these prices.

Table 7: Regressions on the average price and standard deviation of prices

	RE model Coefficient (Std. Error)	RE model Coefficient (Std. Error)
Dep variable	**Average Price**	**Std. Dev. Price**
Compliance treatmente with constant marginal penalty (T2)	-6.506***	-0.867
	-2.386	-0.884
First	-3.585	-1.765
	-3.177	-1.115
First*Flat marginal penalty	5.635	-0.189
	-3.887	-1.297
Period 3	3.605***	2.820***
	-0.896	-1.021
Period 4	2.335***	0.965**
	-0.705	-0.404
Period 5	1.733***	0.795**
	-0.599	-0.388
Period 6	1.335**	0.286
	-0.575	-0.254
Period 7	0.906*	0.298
	-0.534	-0.357
Period 8	-0.533	1.569
	-0.654	-1.294
Period 9	-0.005	0.341
	-0.405	-0.868
Constant	79.096***	3.754***
	-1.717	-0.659
N	207	207
N_clust	26	26
* p<0.1, ** p<.05, *** p<.01		
Base cathegories for period: period 10		

Moreover, performing the same regressions but with the average ask (Column 1 of Table 8) and the average Bid (Column 2 of table 8), we can conclude that the negative effect of a flat marginal penalty on the average price of permits traded (or the positive effect of an increasing marginal penalty) seems to be driven by the effect of the penalty on bids and not asks (See Table 8). In other words, the effect of the penalty schedule on the price of permits traded seems to be driven by the reluctance of sellers (those endowed with an initial number of permits above their predicted final holdings at the end of the period) to sell at the values they sell in the market with a flat marginal penalty. More precisely, the average bid price is $E7 higher in the treatment with increasing marginal penalty than in the treatment with a constant marginal penalty.

Table 8: Regression on the average bid and ask

	RE model Coefficient (Std. Error)	RE model Coefficient (Std. Error)
Dependent variable	**Average Ask**	**Average Bid**
Compliance treatment with constant marginal penalty (T2)	-5.822	-6.966**
	-12.361	-2.916
First	-1.973	-1.543
	-13.326	-3.563
First*T2	2.626	6.081
	-18.500	-4.487
Period 3	-16.019	0.055
	-9.826	-1.660
Period 4	-17.578**	-0.178
	-7.783	-1.208
Period 5	-14.556	0.071
	-8.990	-0.814
Period 6	-5.004	0.980
	-14.830	-1.093
Period 7	-15.292	0.791
	-11.752	-0.990
Period 8	-7.528	-0.549
	-8.900	-0.824
Period 9	10.608	-1.025
	-12.640	-1.262
Constant	111.777***	73.389***
	-11.575	-1.938
N	207	207
N_clust	26	26
* p<0.1, ** p<.05, *** p<.01		
Base cathegory for period: period 10		

To sum up, whether the market for pollution permits is perfectly enforced with an increasing marginal penalty or a constant marginal penalty seems to have no effect on the compliance behavior of firms, although a market that is perfectly enforced with a constant marginal penalty clears for smaller prices. This latter result seems to be explained by the behavior a reluctance of sellers in the market enforced with a increasing marginal penalty to sell at the same price as they do in the market with a with a constant marginal penalty, as can be concluded by the observation of a higher average value of bids in the former.

5.2 Standards

[redacted] Descriptive statistics

In this section we compare a treatment with increasing marginal penalty (S1) versus one with constant marginal penalty (S2). In both cases 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.

In Table 9 we present the descriptive statistics for these treatments. It can be seen that average level of violations for every type of firm in both treatments is above zero, as it was in the case of tradable discharge permits. Taking a closer look at this result we see that violation occurs in approximately 40% of cases, and for the 60% remaining subjects choose to comply (see Graph 2 in the appendix).

Table 9: Descriptive statistics for the standards treatments

STANDARDS TREATMENTS										
		Type 1		Type 2		Type 3		Type 4		
		q	v	q	v	q	v	q	v	
TREATMENT 5		s =7		s =6		s =4		s =4		
	Theory	**7**	**0**	**6**	**0**	**4**	**0**	**3**	**0**	
Experiments										
	Mean	7,52	0,52	6,62	0,62	4,62	0,62	3,61	0,61	
	Median	7,00	0,00	6,00	0,00	4,00	0,00	3,00	0,00	
	Std Dev	0,94	0,94	0,95	0,95	0,89	0,89	1,00	1,00	
	Nº obs	272	272	272	272	272	272	248	248	
TREATMENT 6		s =7		s =6		s =4		s =3		
	Theory	**7**	**0**	**6**	**0**	**4**	**0**	**3**	**0**	
Experiments										
	Mean	7,63	0,63	6,84	0,84	4,86	0,82	3,57	0,57	
	Median	7,00	0,00	6,00	0,00	4,00	0,00	3,00	0,00	
	Std Dev	1,07	1,07	1,42	1,42	1,33	1,33	1,10	1,10	
	Nº obs	144	144	144	144	136	136	128	128	

Again, we compute a series of mean comparison tests (t-tests) to test if the variables behave as expected. In table 10 we report the results for the tests performed to the average of violations across rounds, but the tests for individual rounds show the same results. We find that the observed mean levels are different form predicted levels at a 5% of significance. The same results are obtained for individual emissions and aggregated violations and emissions (not shown).

Table 10: Comparison of experimental values with theoretical predictions

	Null hypothesis	p-value	Conclusion (level of significance 5%)
Individual violations			
Treatment 5			
All types of firms	v=0	0.0000	Reject null hypothesis
Treatment 6			
All types of firms	v=0	0.0000	Reject null hypothesis

[redacted] The effect of the fine structure

Now turning to the comparison between treatments S1 and S2, the results of these tests show no statistically significant differences across treatments.

Table 11: Comparison of experimental values between Treatments

Effects of penalty design- STANDARDS TREATMENTS		
	Two-sample Wilcoxon rank-sum (Mann-Whitney) test (1)	**Median Test (2)**
	Prob > |z|	Pearson chi2 probability (continuity corrected)
VIOLATIONS		
Compliance treatments (T5 and T6)		
Average rounds 3 to 10		
Type 1	0.2782	0.752
Type 2	0.9690	0.771
Type 3	0.7404	0.921
Type 4	0.7086	0.848

(1) Null hypothesis: violation(treatment==a) ~ violation(treatment==b)

(2) Null hypothesis: the 2 samples were drawn from populations with the same median.

In Table 12 we present the results of our econometric analysis. In column 1 we present the random effect regression and in column 2 for the censored model. These estimations show that, when we account for negative violations, there is no statistically significant difference on the level of violations between the treatment that induces compliance with an increasing marginal penalty and the treatment that induces compliance with a flat marginal penalty. Nevertheless, when we truncate violations in zero, the coefficient of the control for the treatments becomes statistically significant, indicating that when we eliminate the possibility of over compliance, violations are around 0.3 units larger in the treatment with flat marginal penalty. This effect exceeds a matter of risk preferences from the part of individuals.

Table 12: Regressions on the level of violations

Dependent variable: Level of violations	Random Effects model Coefficient (Std error)	Tobit RE model Coefficient (Std error)
Compliance treatment with flat marginal penalty (T6)	0.328	0.327*
	(0.242)	(0.175)
First	0.101	0.099
	(0.114)	(0.133)
First*T6	-0.202	-0.202
	(0.295)	(0.227)
Type= 2	0.101	0.105
	(0.154)	(0.143)
Type= 3	0.230*	0.227
	(0.137)	(0.150)
Type= 4	0.113	0.112
	(0.147)	(0.150)
Period=3	-0.374***	-0.374***
	(0.093)	(0.084)
Period=4	-0.276***	-0.276***
	(0.084)	(0.084)
Period=5	-0.270***	-0.270***
	(0.097)	(0.084)
Period=6	-0.196**	-0.186**
	(0.090)	(0.084)
Period=7	-0.202**	-0.202**
	(0.083)	(0.084)
Period=8	-0.147	-0.147*
	(0.100)	(0.084)
Period=9	-0.074	-0.074
	(0.087)	(0.084)
Risk Aversion=3	1067	1.064**
	(0.654)	(0.522)
Risk Aversion=4	-0.228	-0.226
	(0.571)	(0.503)
Risk Aversion=5	-0.375	-0.376
	(0.556)	(0.488)
Risk Aversion=6	-0.254	-0.254
	(0.548)	(0.474)
Risk Aversion=7	-0.413	-0.414
	(0.543)	(0.476)
Risk Aversion=8	-0.352	-0.353
	(0.549)	(0.490)
Risk Aversion=9	-0.426	-0.409
	(0.550)	(0.506)
_cons	0.829	0.829*
	(0.535)	(0.463)
N	1304	1304
N_clust	163	
* p<0.1, ** p<.05, *** p<.01		

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6. Conclusions

Our results can be summarized as follows. First, we have found that the structure of the penalty schedule affects the average size of violations in the case of an emission standard, but not in the case of tradable permits. More specifically, in the case of emission standards a flat marginal penalty induces a larger violation, on average, than in the increasing marginal penalty case.

Second, we find that the structure of the penalty schedule affects the average price of permits traded by affecting the willingness to pay of net buyers. More specifically, the average price of permits traded is higher in the case of increasing marginal penalties than in the case of a flat marginal penalty. Although in our experiments the change in the price was not apparently of enough magnitude so as to induce a change in the level of emissions and violations, this possibility cannot be ruled out if the parameters of the fine schedule produce a larger increase in the price. In this case, the structure of the penalty schedule may affect the compliance level in a cap and trade system. In this respect we think it may be worth analyzing in further detail the out of equilibrium dynamics of the price formation mechanism in a cap and trade system to accurately predict its effect on the level of emissions and violations. This latter issue is important because if the allocation of emissions is not that predicted by the standard model, the structure of the penalty schedule may have an effect on the overall costs of the program, and therefore the relative cost-effectiveness of tradable permits with respect to emission standards.

7. References

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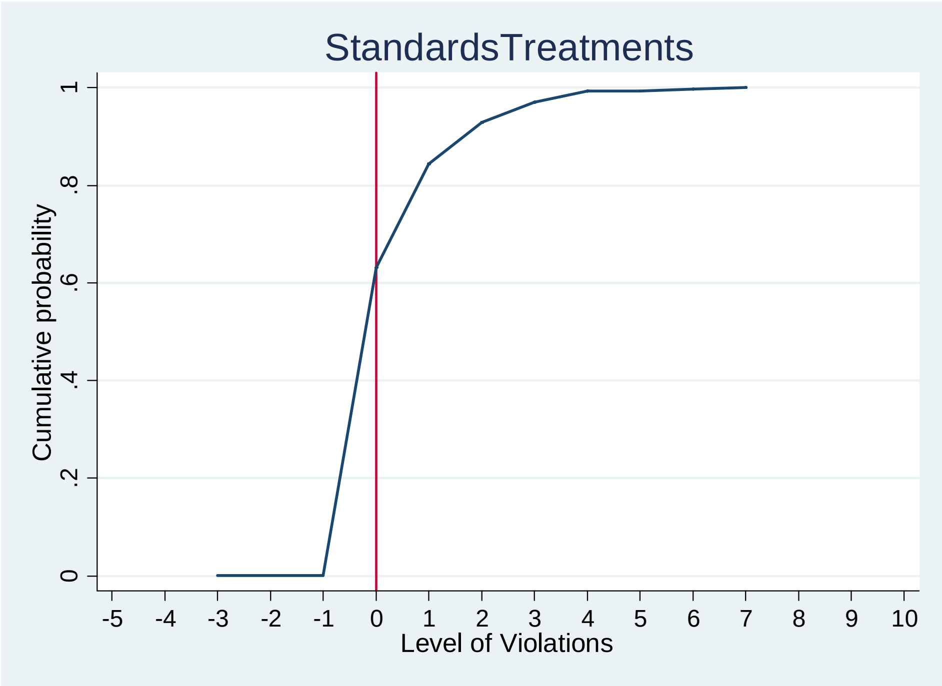
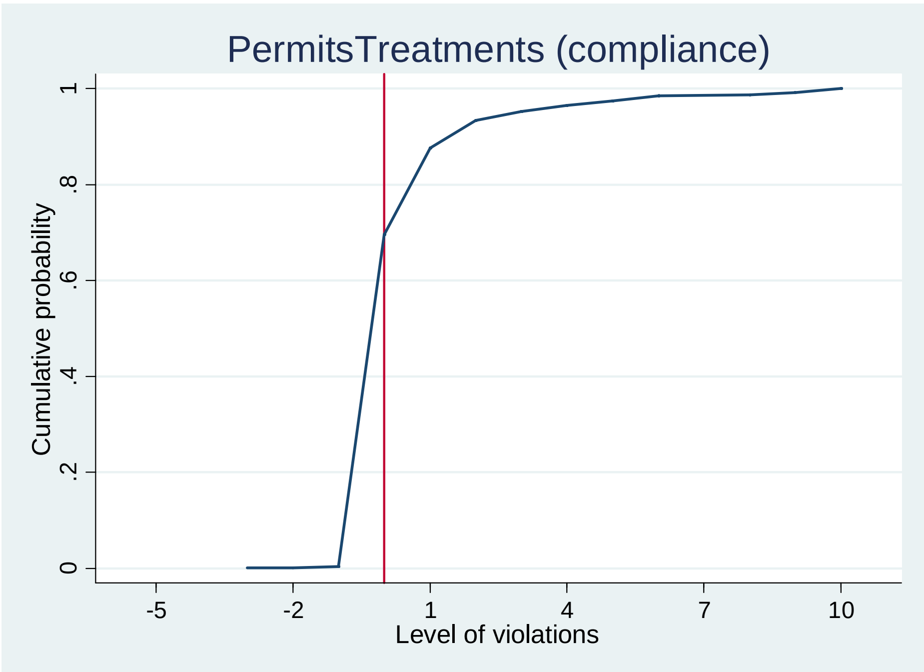
APPENDIX

Graph 1


StandardsTreatments
Cumulative probability
0
.2
.4
.6
.8
1
-5
-4
-3
-2
-1
0
1
2
3
4
5
6
7
8
9
10
Level of Violations

Graph 2


PermitsTreatments (compliance)
Cumulative probability
0
.2
.4
.6
.8
1
-5
-2
1
4
7
10
Level of violations