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RESEARCH ARTICLE

The deterrence effect of linear versus convex fines: laboratory evidence

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ABSTRACT
Using experiments in which participants play the role of polluting firms, we study compliance behavior with emissions limits under two types of fines and two different regulatory instruments. We find that the market price of pollution permits and the probability of violating permits holdings are higher with a fine that is convex in the level of violation than with one that is linear. This effect operates through an increase in the prices asked by sellers, not in the bids made by the buyers of permits. We do not observe an effect of the type of the fine on the average level of violation or the number of firms in violation in the case of emission standards. We conclude that the type of fines may affect the cost-effectiveness of pollution control programs based on tradable pollution permits.

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1. Introduction

According to the seminal applications of Becker (1968) classical economic theory of crime to the problem of enforcing environmental policy instruments, polluting firms will comply with environmental regulations if and only if the expected monetary fine for violating the regulation is higher than the corresponding benefit. (See, for example, Harford (1978 and 1987), Viscusi and Zeckhauser (1979), Harrington (1988), Malik (1990 and Malik, 1992), and Stranlund and Dhanda (1999)). The focus of these works was on the compliance behavior of polluting firms under different instruments for pollution regulation. In their setting, the structure of the penalty function (i.e.: whether it is linear or convex in the level of violation) was assumed to be exogenous to the environmental regulator and was relevant only for the possibility of obtaining corner solutions. It played no fundamental role in the policy recommendations obtained from these works. Nevertheless, the structure of penalties may determine the cost-effectiveness of inducing compliance or not. As shown by Stranlund (2007), for the case of tradable permits, and Arguedas (2008), for the case of pollution standards, allowing some level of noncompliance can be cost-effective (when not only abatement but also enforcement costs are considered) only if penalties are convex. More importantly, they also show that perfectly enforcing an emissions control program with a linear penalty function is always

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cheaper than a program that achieve the same level of emissions allowing some level of noncompliance with a convex penalty. Assuming heterogeneous costs of monitoring and sanctioning, Caffera and Chávez (2011) show that the cost minimizing design of a program that controls emissions, (a) induces perfect compliance, and (b) requires the penalty to be linear, or not as convex as to make cost ineffective to induce compliance to the firm with the lowest ratio of sanctioning costs to monitoring costs. In sum, these works show that the choice of the structure of the penalty may affect the overall expected costs of the pollution cap program.

As far as we know, a surprisingly low number of works have analyzed the impact of different structures of monetary fines in the lab (see the Literature review section). One of such studies is Anderson, DeAngelo, Emons, Freeborn, and Lang (2017), who studied the structure of penalties in other dimension: the number of past offenses. In another work, Restiani and Betz (2010) studied the effect of two types of penalties (fines tied to the permit´s price versus offsets) in a cap and trade program with perfect monitoring. We are not aware of any work testing the deterring effect of linear versus convex fines. This state of the literature leaves unanswered the question of the way in which we should fine violations to an emission limit as they increase in size, a relevant dimension of penalty structures. Given the role of fine structures in the cost-effectiveness of pollution control regulations and the importance of cost-effectiveness as a criterion for designing environmental policy, this is an important unanswered question. The objective of this work is to fill this gap in the literature by testing whether firms respond differently to linear versus convex penalties, when both types provide the same incentive on the margin. Should conventional theory fail and, for some reason, convex penalties have higher deterrence, this would mean that a regulator could enforce the same level of emissions with less monitoring resources than with linear penalties. On the other hand, if convex penalties increase violations, linear penalties would add another point in favor of their use, to the ones exposed above.

To test the hypothesis that convex and linear penalties produce the same level of emissions if they provide the same incentive in the margin, we use laboratory experiments with undergraduate students in which they play the role of producers of an unspecified profitable good and face a regulation aiming at capping the aggregate level of production of this good. Subjects can violate the regulation, at the risk of being caught and fined. We construct four different treatments over this basic scenario, by combining two regulatory instruments with two types of fines. The two instruments are: (a) an individual maximum limit of production, below the profit maximizing level, and (b) a market for production permits. The former mimics an emission standard and the latter mimics a cap and trade system. The two types of fines are: (a) a linear and (b) a convex function in the level of violation. The design of the experiment is such that, for a given firm, the expected profit-maximizing level of production is the same in the four treatments.

The issue of the deterrent effect of linear versus convex penalties has empirical significance also: the penalty guides remain silent on whether monetary fines should be linear or convex in the level of violation and, consistently, one can observe both types of penalties in the real world.

According to official sanctioning guides in the UK and the US, monetary fines in environmental policy should 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 the fine is to remove the economic benefit obtained from noncomplying. The goal of the gravity component is to deter future violations. "It should reflect the seriousness of the violation." (U.S.E.P.A. 1984, p. 3), which depends 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). In other words, according to official guides, the higher the level of pollution detected in excess of the legal permit, the higher should be the monetary fines. Nevertheless, there is no guideline on whether this should be achieved using linear or convex penalties.

Absent any guidelines, it is maybe unsurprising that we see several types of monetary fines in the real world. In both the European Union Emissions Trading Scheme (EUETS) and the US Acid Rain Program, regulators punish violators of individual holdings of allowances with linear penalties.[1] On the other hand, in the ETS pilot program of Beijing, China, the monetary penalty for non-compliance is convex for "key carbon emission units" (Zhang, 2015).[2] Korea ETS uses a similar fine (Kim and Yu, 2018). This could also be the case for the pilots in Shangai and Hubei, but since there has been no penalty applied yet, it is not clear whether Beijing´s pilot convex penalty will be legally binding in these two cases (Tang Jin, personal communication). Lastly, to enforce the California Cap and Trade Program, regulators impose penalty amounts between 1,000–10,000 USD per metric ton, after an entity fails to meet an untimely surrender obligation. This obligation consists of submitting " … four compliance instruments (only one quarter of which can be offsets) for each instrument the entity failed to surrender" (Partnership for Market Readiness (PMR) and International Carbon Action Partnership (ICAP), 2016). In determining the amount of the fine to impose, the court shall consider "the extent of harm caused by the violation".[3] Therefore, if harm increases with emissions, and does it at an increasing rate, California fines could be convex.

In sum, we observe both linear and convex penalties in the most notorious cap-and-trade programs in the world. Therefore, given the sizes of these markets, a substantial amount of resources may be at stake if firms react differently to linear than to convex penalties.

Our results indicate that the market price of pollution permits and the number of firms in violation are higher with a convex than with a linear penalty, when they should

[1]In the case of the EUETS, "(t)he excess emissions penalty shall be EUR 100 for each tonne of carbon dioxide equivalent emitted for which the operator or aircraft operator has not surrendered allowances" (Directive 2003/87/EC, amended by Directive 2008/101/EC of the European Parliament and of the Council of 19 November 2008). "The excess emissions penalty relating to allowances issued from 1 January 2013 onwards shall increase in accordance with the European index of consumer prices" (Directive 2003/87/EC, amended by Directive 2009/29/EC of the European Parliament and of the Council of 23 April 2009). A source that does not hold enough allowances to cover its emissions of SO2 in the US Acid Rain Program must pay the equivalent of 1990 US$ 2,000 per ton in excess, adjusted by inflation. (EPA, 2002).

[2]" … In the Beijing pilot, depending on the extent of noncompliance, entities are subject to fines equal to three to five times the prevailing average market prices over the past six months for each shortfall allowance. A fine of three times the average market prices is imposed if the emissions of non-complying entities exceed less than 10% of their emissions allowances, while a fine of five times the average market prices is applied if non-complying entities emit 20% more than their emissions allowances, with a fine of four times the average market prices imposed in between the two cases (Beijing Municipality Development and Reform Commission, 2014a)" (Zhang, 2015).

[3]California Health and Safety Code, División 26 Air Resources, Part 4. Non-Vehicular Air Pollution Control, Chapter 4. Enforcement, Article 3. Penalties, Section 42,400.8

not be, according to basic economic theory. Moreover, this effect operates through an increase in the prices asked by sellers, not on the bids put by the buyers of permits. We find that a possible mechanism at work behind this result is that a convex fine increases the value of the production permits for the buyers and the sellers take advantage of this, asking for higher prices, on average, that sellers accept. In other words, a convex penalty increases the bargaining power of sellers in the auction market for permits. This interpretation is reinforced by the fact that we do not observe an effect of the type of fines on the average level of violation (intensive margin) or the proportion of firms in violation (extensive margin) in the case of emission standards. In sum, we conclude that the type of fines may affect the expected costs of pollution control programs based on tradable pollution permits.

2. Literature review

As stated in the introduction, monetary fines are a usual tool in deterring violations to environmental norms. Economic theory and actual manuals support fines that increase with the size of the violation. Both linear and convex penalties could achieve this mandate. Nevertheless, manuals remain silent which type of penalty should be used, leaving the choice to policy makers and officials. On the other hand, in theory, whether the fine is linear or convex should make no difference in the number and extent of violations that we observe, if the marginal incentive is the same. The applied literature does not provide guidance to policy makers on this issue, either. As far as we know, there is no empirical work analyzing how linear versus convex fines affect compliance.[4] Anderson et al. (2017) conducted lab experiments to study the structure of penalties with respect to another possible determinant: the number of past offenses. Their experiments were a two-stage game, in which subjects with an initial fixed endowment had to decide on whether to commit an illegal activity with a fixed return in two rounds. All subjects faced a fixed probability of apprehension in both stages, but they faced fines that could be higher for the first offense than for the second, the other way around or flat. The amount of the fines was substantial: up to 90% of the initial endowment. In this setting, their results showed that decreasing fine structures are more effective as deterring mechanisms. The question of punishing repeat offenders more harshly or not is an important one, but leaves unanswered the relevant question of how subjects react to linear versus convex penalties, when these are increasing in size of the violation. We address this question in this work in a different setting in which subjects could not only choose whether to violate a norm or not, but also the extent of the violation.

Restiani and Betz (2010) are the only work that we are aware of that analyzes the effects of different penalty structures in the context of pollution control program a cap and trade program. In their experiments, these authors show that a make-good provision type of penalty provides stronger compliance incentives than (a) a penalty tied to the permit prices, and (b) a mixed penalty. They also found no evidence that the type of

[4]The literature that has used lab experiments to explore compliance behavior with environmental regulations is large and increasing; see for example, Cason and Gangadharan (2006), Murphy and Stranlund (2006), Murphy and Stranlund (2007), Stranlund, Murphy, and Spraggon (2011), Friesen and Gangadharan (2013), Stranlund, Murphy, and Spraggon (2013), Caffera and Chávez (2016). Reviews of the literature on enforcement and compliance with environmental regulations are presented in Stranlund (2017), Shimshack (2014) and Gray and Shimshack (2011).

penalty affected the permit price. Nevertheless, in their experiments, among other differences, monitoring was perfect. That is, the probability of an audit equaled one. In contrast, we analyze the impact of different penalty structures under imperfect monitoring; i.e. the regulator audits the firms' emissions with a probability less than one. This is an important actual element of any environmental policy.

3. Theory and hypotheses

In this section, we present the hypotheses we tested with our laboratory experiments. These hypotheses are based on the positive theoretical literature on the behavior of polluting firms under transferable emission permit systems and emission standards (Malik 1990, Arguedas, 2008; Caffera & Chávez, 2011; Stranlund, 2007; Stranlund & Dhanda, 1999).

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 gross profit function is$b(q)$, which is strictly increasing and concave in the firm's emissions q [$b'(q) > 0$ and $b''(q) < 0$].[5] In line with the vast majority of theoretical work in this area, we assume that firm's objective is to maximize expected profits.

The environmental policy target is a fixed aggregate level of emissions$\bar{Q}$. The regulator audits polluting firms with probabilityπ, that is exogenous the level of emissions or the compliance history of the firm. The regulator announces this probability to all firms. An audit provides the regulator with perfect information about the firms' compliance status. A firm is in violation (v) when its units of emissions exceed its permit holdings or its emission standard. The regulator automatically fines a firm discovered in violation with the monetary amount$f(v) = \varphi v + \frac{\gamma}{2} v^2$, where $\varphi$is a strictly positive parameter and$\gamma \geq 0$.

3.1. Transferable emission permits system

Under a system of transferable emissions permits, the regulator issues a total of $L = \bar{Q}$ licenses. Each of these licenses confers the legal right to release one unit of emissions to the possessor. Under the assumption of perfect competition, the market for licenses generates a unique equilibrium 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 (q) exceed the number of licenses (l) it holds and the level of its violation is $v = q - l > 0$.

A firm chooses its emissions (q) and permits (l) to maximize expected profits. These are comprised of gross profits minus expenditures from buying permits (plus earnings from selling permits) and the expected penalty.[6] We know that in this system a risk-neutral firm is compliant if and only if $b'(l) \leq \pi f'(0) = \pi\varphi$ (see for example, Malik

[5]The function $b(q)$ is the (maximum) profits function of the firm. That is, a function that tells us the level of profits that the firm obtains when it chooses the output and inputs levels to maximize profits, while holding its emissions to a level q. Embedded in this function are the production and abatement technologies. If assumed to be in the short run, these technologies could be given. If assumed to be in the long run, the choice of these technologies is part of the optimization problem. In any case, the function$b(q)$ implicitly includes all the abatement possibilities: hiring inputs to decrease emissions while holding the level of output constant, decreasing the level of output or a combination of both.

(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$b'(q) = p$, 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, $\sum_{i=1}^{n} l_i(p) = L = \bar{Q} = \sum_{i=1}^{n} q_i$. 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, $p^{nc}(L, \pi, \varphi, \gamma)$.[7] Accordingly, the first hypothesis to be evaluated is as follows:

Hypothesis 1: *In a system of transferable emission permits, where the value of 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.*

Proof: As seen previously, under a system of transferable emission permits, a firm complies if and only if $b'(q = l) = p(L) \leq \pi f'(0) = \pi\varphi$. 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 $f(v) = \varphi v + (\gamma/2)v^2$ (with $\varphi > 0$ and $\gamma > 0$), or a linear penalty function$f(v) = \varphi v$, we should expect no differences in violations between both schemes.

3.2. Emissions standards

We consider now the case in which each firm i faces an emissions standards_i. This is a maximum allowable (legal) level of emissions for each firm. Emissions standards for all firms satisfy $\sum_{i=1}^{n} s_i = \bar{Q}$. Under an emissions standard, a firm chooses the level of emissions to maximize its total expected net profits, which consist of its gross profits minus the expected penalty.[8] As it is known, a risk-neutral firm will be compliant ($q = s$) if and only if $b'(s) \leq \pi f'(0) = \pi\varphi$ (Harford, 1978; Heyes, 2000; Malik, 1992). This condition is also necessary and sufficient to induce compliance in the case of risk-averse managers.[9]

[6]The risk-neutral individual firm's expected profits under a transferable emissions permit system is the result of adding the expected net profit when the firm is monitored and when it is not, that is, $\pi[b(q) - p[l - lo] - \left(\varphi(q - l) + \frac{\gamma}{2}\left(q - l\right)^2\right]\right) + (1 - \pi)[b(q) - p[l - lo]]$.

[7]We notice here that the initial allocation of permits does not affect the individual demand of permits regardless of the compliance status of the firm. Consequently, the initial allocation of permits has no role in the equilibrium of a competitive market for tradable emissions neither under perfect compliance nor under violation. However, due to a lower level of permits demand, the equilibrium price of emission permits is expected to be lower under violation than under perfect compliance (Malik (1990), Stranlund and Dhanda (1999)).

[8]The risk-neutral individual firm's expected net profits under a system of emissions standards is the result of adding the expected net profit when the firm is monitored and when it is not, that is,$\pi[b(q) - \left(\varphi(q - s) + \frac{\gamma}{2}\left(q - s\right)^2\right]\right) + (1 - \pi)[b(q)]$.

If $b'(s) > \pi\varphi$, 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 $b'(q) = \pi[\varphi + \gamma(q - s)]$. Based on this theory, we now present the second hypothesis as follows:

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 individual and aggregate violations is independent of the penalty structure.*

Proof: 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.

4. Experimental design

Instead of framing our experiments as a decision on how much to pollute, we framed them in a more neutral fashion. We told subjects that they were producers of an unspecified good *q*, and that this activity yielded *b(q)* net benefits to them. In spite of the framing choice, the production of *q* in the experiments is intended to mimic the production of emissions, as in the model above. For this reason, in what follows, we refer indistinctively to the level of production or the level of emissions.

Each subject had a production capacity of 10 units of the good, but the benefits derived from the production of this good were not the same for every subject. Throughout the experiments, we use four different schedules of marginal benefits of production ($b'(q)$, in the model above), taken from Cason and Gangadharan (2006). (See Table 1).

To assign one of the four schedules of marginal benefits to each subject, we randomly assigned subjects to groups of eight individuals and then we randomly assigned each of the four types of schedules to two of the subjects in the group. Each of these groups of eight subjects represent a group of *n* polluters whose aggregate level of emissions a regulator is interested in capping. In other words, *n* = *8* in the model above. Because

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

	Marginal Benefits of Production			
Units produced	Type 1: subjects 1 & 2	Type 2: subjects 3 & 4	Type 3: subjects 5 & 6	Type 4: subjects 7 & 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

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

each subject had a production capacity of 10 units of emissions, the unregulated aggregate level of emissions for every group of eight firms was 80 units. The cap was set at 40 units.

We constructed four treatments to test our hypotheses (See Table 2), combining two different regulatory instruments to cap the production of q, and two penalty structures. The two regulatory instruments were (a) a cap-and-trade system (a market for emission permits) and (b) maximum individual limits of production (emission standards). These type-specific emissions standards were set at the levels of emissions that were predicted in the market for permits (see column F of Table 2). The two penalty structures were (a) a linear penalty and (b) a convex penalty (see column C of Table 2).

Consider the cap-and-trade treatments first. In these treatments, subjects had to possess a permit in order to be legally able to produce a unit of the good. At the beginning of the experiment, each subject received an initial number of permits (l_0) without cost. Subjects could also buy or sell permits in a permit-by-permit double-auction market comprised by the eight subjects in the group. The total number of tradable permits supplied to each group of eight subjects was 40. The initial allocation of (free) permits was four to subjects of type-1 and type-2, the prospective buyers, and six permits for subjects of type-3 and type-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. In theory, the initial allocation of permits should not affect the equilibrium of a competitive market. Given the marginal benefits of production of Table 1, this initial allocation of permits gave rise to the demand and supply of permits depicted in Figure 1. The perfect-compliance equilibrium price of this market, $p^*(L)$, is between 74 and 80 experimental pesos (E$) (column G). The number of expected trades consistent with this initial allocation is 10. (See Figure 1).

In the first of the market treatments (Treatment M1), the penalty schedule was convex in the level of the violation. More precisely, the enforcement parameters took the values $\varphi = 100$, $\gamma = 66$. (See column C in Table 2). In the second of the market treatments (Treatment M2), the penalty schedule was linear ($\varphi = 133$, and $\gamma = 0$). In both treatments, the exogenous monitoring probability $\pi = \frac{80}{133}$. Therefore, $p^*(L) \leq \pi f(1) = 80$ in

Table 2. Summary of treatment design.

Policy Instrument (A)	Treatment (B)	Penalty function: $\varphi v + \frac{\gamma}{2} v^2$ (C) φ	γ	π(D)	Cap (E)	Predicted Behavior (F)	Predicted Equilibrium Price (G)
MARKET FOR EMISSION PERMITS	M1 M2	100 133	66 0	$\frac{80}{133}$	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
EMISSION STANDARDS	S1 S2	100 133	66 0	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	

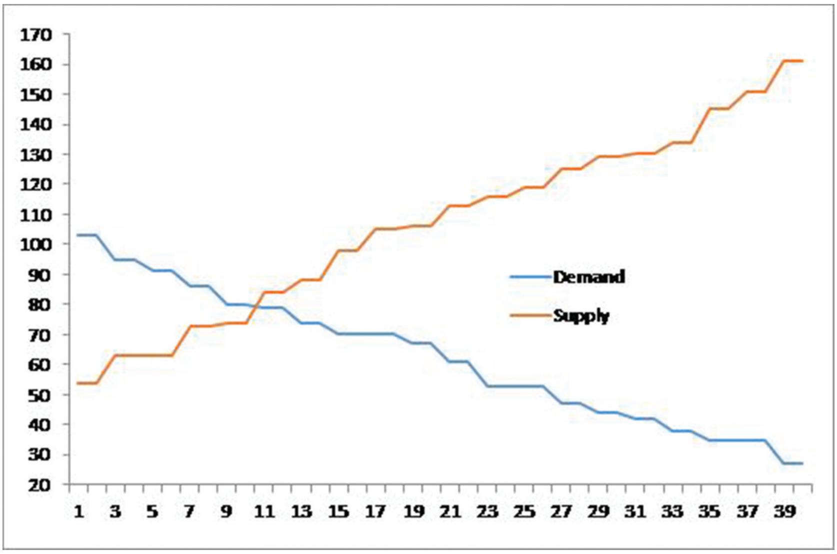

Figure 1. Supply and demand in the market for permits.

both treatments, which means that both treatments should induce compliance. Moreover, they should induce the same individual level of emissions. Hence, the expected level of aggregate emissions was 40 units for both treatments.

In the standards experiments, subjects faced a maximum allowable level of emissions (the standard) and had to decide how much to emit. The emission standards (s) were set at 7, 6, 4 and 3 units of production for firms of types 1 to 4, respectively (See column F in Table 2). These are the cost-effective levels of emissions, and are the same levels the firms are expected to emit in the perfect equilibrium of the market treatments. As in the case of standards perfect-compliance requires targeting inspections according to the marginal abatement costs of each type of firm, we set four different auditing probabilities with values 0.6, 0.65, 0.63 and 0.66 for firms of types 1 to 4, respectively. As with the cap-and-trade, we constructed two treatments for the case of emission standards, labeled S1 and S2 in Table 2. In the treatment S1, violations are fined with the same convex penalty function used in M1; $\varphi = 100$ and$\gamma = 66$. In the treatment S2, violations are fined with the same linear penalty schedule of M2; $\varphi = 133$ and$\gamma = 0$. Both of these fines induce compliance with the set of emission standards, so the expected aggregate level of production is 40 units in a group of eight subjects in both treatments.

5. Experimental procedures

We programmed the experiments in z-Tree (Fischbacher, 2007) and conducted them in a computer lab at the University of Montevideo, specifically conditioned for these experiments. 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. For that purpose, we built a web page where students could register as candidates who wished to receive invitations to attend an experimental session. The schedule of sessions was available on this webpage.

Days before the date, we invited all previously registered students to show up for the scheduled session.

Our computer lab had a capacity of 40 subjects. The subjects showing up for a permit or standards session were randomly assigned into groups of 8 individuals.[10] For each eight-subject-group in the room, an experimental session consisted of 20 rounds, during which the subjects played two treatments (10 rounds per treatment). Both treatments in a session consisted of either tradable permits or standards. In one of the two treatments, we induce perfect compliance with a linear or a convex penalty. In the other treatment, we lowered the inspection probability, inducing violations. The results presented in this paper are from the treatments in which we induced perfect compliance. The order of treatments differed between groups in a session. Approximately half of the groups 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.[11] 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 round lasted 2 minutes. As commented above, in the permits sessions, subjects had not only to decide how many units to produce, but also how many permits to buy or sell. To give subjects time to make their bids, asks, and to decide how many units to produce, in the market experiments each round lasted 5 minutes.

We implemented the market for permits in the way of a permit-by-permit double-auction market. From the beginning of the round, a subject could put a bid for a permit or submit an asking price to sell a permit. The rest of the subjects, who had the value of the lowest asking price and the highest bid at all times in their screen, could outbid or accept the lowest asking price, if buyers, or ask for a lower price or accept the highest bid, if sellers. If a seller accepted a bid or a buyer accepted an asking price, the auction for that permit closed and the market moved to the next permit. The auction for the next permit began from zero. The auction floor remained open for the entire 5 minutes of the round. Subjects could also change the desired level of production at any time within these 5 minutes.

At every moment, on their screen, subjects had the information on the probability of inspection that they faced and the marginal fine for every level of violation. At the end of each round, after all subjects in the group had made their decision, the computer program automatically generated a random number between zero and one for each subject, independently. If this number was below the informed monitoring probability, the subject was inspected. Consistent with the theoretical model, this randomly generated number did not depend on the compliance history of the subject, or any other factor. If audited, the software automatically compared the number of units produced by the subject i in that period (q_i) with the number of permits it possessed (l_i) or its emission

[10]The number of subjects showing up for a session was not always multiple of eight. This was not a problem in the standards experiments, because in these experiments the subjects do not relate with each other in any form. In the case of market treatments, we completed groups of eight subjects by order of arrival. Excess subjects were paid the show-up fee.

[11]Instructions of the experiment are available in the Online Appendix, at http://www2.um.edu.uy/marcaffera/investigacion/Online%20Appendix%20_Structure_of_penalties.pdf.

Also, starting each session subjects signed a consent in which we informed them that the data gathered in the experiments was going to be treated privately and only for academic purposes.

standard (s_i), depending on the treatment. If the former was higher than the latter, the subject was automatically fined. Subjects were informed in their screen whether they had been selected for inspection or not. They were also informed about the result of the inspection (violation level, total fine and net profits after inspection). After this, a new screen informed subjects about the history of their decisions in the game, the history of inspections and the history of profits, up to the last round 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 207 in the standards experiments. Due a thin pool of subjects, we allow subjects to participate in more than one session. As we discuss in section 5, our analysis takes appropriate control of these reappearances. One hundred and ninety six (196) of the 216 students participating in the market experiments and 186 of the 207 that participated in the standards experiments majored in economics or business.[12]

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.[13] 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.

6. Results

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

6.1. Results for market experiments

6.1.1. Descriptive statistics for market experiments

We report basic descriptive statistics of relevant variables in Table 3, by treatment. We cluster the statistics for the level of emissions (q), the number of permits hold (l) and the level of violations ($v = q - l$) at the type-of-firm level. The first thing to notice is that violations are positive, on average, for all types of firms in both treatments. Nevertheless, all average levels of violations are below one unit, except that of type-2 firms in the treatment M2. This result is not new. The literature has already reported violations in

[12]We are aware of the evidence that students majoring in economics or business behave in a more profit/oriented or in less cooperative way than students majoring in other disciplines, when participating in economic experiments. Economics students contribute less than high school students in a public good game (Marwell & Ames, 1981), accept less as a receiver and give less as a proposer as compared to non-economics majors in the ultimatum game (Carter & Irons, 1991), defect more than non-economics students in the prisoner's dilemma game (Frank, Gilovich, & Regan, 1993), and send less as player 1 and give back less as player 2, as compared to non-economics students in the trust game (Núñez, Miranda, & Scavia, 2009). (See Caffera, Zipitria, and Arboleya (2010) for a more complete analysis of this issue). We believe this is not a major problem for the external validity of our experiments; unless one is willing to assume that the polluting firms our students represented in the lab are not mainly profit-oriented

[13]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.

Table 3. Descriptive statistics permits treatments.

		Average Price per period	Number of Transactions per period	Type 1 firms($l_0 = 4$)			Type 2 firms($l_0 = 4$)			Type 3 firms($l_0 = 6$)			Type 4 firms($l_0 = 6$)		
				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.

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 (2006) violation rates were between 15% and 37%, depending on the relative costs and benefits of compliance. In spite of the 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%.

We can also see in Table 3 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 a convex penalty (M1) than in the case of a linear penalty (M2). The difference is persistent across periods (see Figure 2). Lastly, we observe an average number of transactions in the treatment M1 (8.5) that is lower that the predicted level (10) and lower than the average number of transaction in M2 (9.7).

6.1.2. Tests for hypothesis 1 (market experiments)

6.1.2.1. Non-parametric tests. Recall that the experimental markets mimic the situation of a market for pollution permits where the regulator has perfect information on the marginal abatement costs of the firms and uses this information to set the marginal penalty to induce perfect compliance cost-effectively. According to Hypothesis 1, the level of individual and aggregate violations should be the same if the regulator uses a convex or a linear penalty. As the levels of emissions of individuals that interact in the same market are not independent, each experimental market provides only one independent observation (Davis & Holst, 1993). Therefore, we formulate Hypothesis 1 as $V^{M1} = V^{M2}$, where V is the sum of violations of the eight subjects that comprise a market, averaged across the ten periods, and the superscript M1 or M2 indicate whether the market is enforced with a convex or a linear penalty.

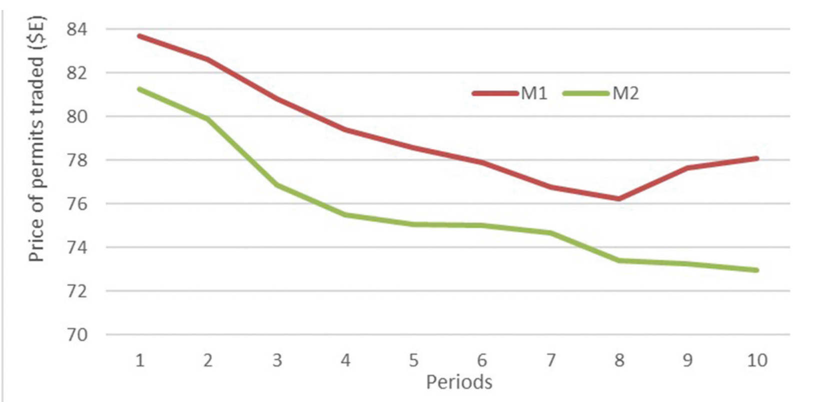

Figure 2. Evolution of average price by treatment.

According to both the Wilcoxon rank-sum (Mann-Whitney) test ($p = 0.43, z = -0.781, n = 27$) and the median test($p = 0.547, Pearsonchi2 = 0.3635, n = 27$), we cannot reject the null hypothesis that the aggregate level of violations is the same between treatments with convex and linear penalties.[14]

6.1.2.2. Regressions. As commented above, we allowed subjects to participate in more than one session due to a thin population.[15] Repeating subjects introduce the possibility of dependence between observations. A 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 groups. Instead, we conduct an econometric analysis controlling for observations belonging to a subject that is repeating participation.[16]

In our econometric analysis, we estimated two different models, depending on the dependent variable used. Our first model is the following basic linear panel data model

$$y^*_{i,t} = \beta_0 + \lambda_i + x_{i,t}\beta + \mu_{i,t} \tag{1}$$

In the equation above, $i = 1, \ldots, n$ indexes individuals and $t = 1, \ldots, T_i$ indexes the rounds played by subject i. The outcome variable $y^*_{i,t}$ is the *level* of the violation of individual i in the t^{th} round he or she played.[17] The second term,λ_i, is the individual *unobserved* effect. In the third term, $x_{i,t}$ is a vector of dimension $1 \times K$ of control variables, and β is the associated vector of dimension $K \times 1$ of parameters to be estimated. The list of explanatory variables included in the vector $x_{i,t}$ is the following:

- $Linearpenalty_{i,t}$: is a dummy variable, indicating whether the observation corresponds to a subject that was facing a linear penalty or a convex penalty. In the first case, the variable takes the value 1. In the second, it takes the value 0.
- $Firsttreatmentinsession_{i,t}$: is a dummy variable, indicating whether the observation corresponds to the first treatment in the session. Recall from above that the data set used in this work corresponds to treatments in which we induced perfect compliance. Nevertheless, the experimental sessions that subjects played consisted not only of this compliance treatment, but also a treatment in which we lowered the inspection probability, inducing violations. The order of treatments differed between groups in a session. Approximately half of the groups played the compliance treatment first and the other half played the violation treatment first.
- An interaction between the above two variables
- $Riskcategory_i$: This variable categorizes the individual to which the observation belongs as risk lover, risk-neutral and risk-averse. To construct this categorical

[14] 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.

[15] Of the 120 different subjects that participated in the market 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).

[16] We identify subjects by matching the amount of profits earned in the session with the amount of the payment in the receipts. This procedure, nevertheless, fails when we have two subjects in the same session that made the same amount of profits. This issue prevented us from identifying eight subjects.

[17] We do have observations in which subjects over-complied. Nevertheless, results do not change if the dependent variable is the level of violation censored-at-zero. The reason is that over-compliance occurred only in a few cases.

variable, we asked subjects to answer a Holt and Laury (2002) type of questionnaire, at the end of the session. In this questionnaire, subjects had to make 10 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). In the 10^{th} choice, the probability of winning the higher prize in the lottery was equal to one. Therefore, extreme risk averse subjects, who had preferred the certain amount instead of the lottery up to the 9^{th} choice, should prefer the lottery in the 10^{th} choice (see Online Appendix). We classified the subjects as risk lovers, risk neutral or risk averse, based on the number of the choice in which subjects switched from preferring the certain amount to preferring the lottery. As expected, 80% of the individuals that participated in the experiments exhibited some degree of risk aversion (switched to prefer the lottery after the 6^{th} choice).

- $Inconsistentriskpreferences_i$: Subjects made inconsistent choices in the Holt and Laury questionnaire in 45 out of 216 cases. A subject´s choice were inconsistent if he or she switched back to the certain amount after switching to the lottery. In the case of those subjects that made inconsistent choices every time they participated (once or more than once), we imputed them with the average category of risk aversion of the whole sample. In the case of those subjects that 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.
- $Type_{i,t}$: This is a variable indicating which of the four marginal benefits schedule we assigned to the subject.
- $Period_{i,t}$: This variable indicates the number of the period that the subject i was playing in that observation. This variable takes the value 1 to 10 for subjects that participated only once in our sessions, 1 to 20 for subjects that participated twice, and so on.
- $Group_{i,t}$: This is an indicator variable for the group of eight subjects comprising the same market for permits.

Finally, $\mu_{i,t}$ is an error term. We assumed λ_i uncorrelated with the observed explanatory variables$x_{i,t}$. In other words, we ran a *random effects model* to estimate equation (1). By using such a model, instead of a fixed effects model, we avoided losing the observations from those subjects that participated only once (the majority of them) due to perfect collinearity between the individual effect and the variable measuring the number of past participations. To perform the random effect estimation, we specified cluster-robust standard errors, nested within each of the groups of eight subjects that comprised a market. We used Stata for all the estimations presented in this paper.

The second econometric model that we estimated is the random effects probit model

$$P\left(y_{i,t} = 1|x_{i,t}, \lambda_i\right) = \Phi\left(x_{i,t} + \lambda_i\right) \qquad (2)$$

for $i = 1, \ldots, n$ indexes individuals and $t = 1, \ldots, T_i$, $\Phi(.)$ denotes the standard normal cumulative distribution function. The underlying model is

$$y_{i,t}^* = \beta_0 + \lambda_i + x_{i,t}\beta + \mu_{i,t} \qquad (3)$$

where $\mu_{i,t}\tilde{N}(0,1)$, independently of λ_i, and $y_{i,t} = 1$ if $y_{i,t}^* > 0$ and 0 otherwise. In other words, $y_{i,t}$ is the violation *status* of the individual, taking the value of one in the case of positive violations and zero otherwise. In this regression, we also specify standard errors robust to heteroskedasticity, at the group level. We use a random effects specification of the binary choice model for the same reason that we use a linear random effects model to estimate equation (1).

We present the results of two regressions in Table 4.

Table 4 shows that the structure of the penalty function does not affect the individual *level* of violation in a statistically significant way. In the second column of Table 4, the estimated coefficient of our variable of interest (the dummy "Linear penalty") is not statistically different from zero. Nevertheless, according to the results shown in column 3, the structure of the penalty may affect the violation *status* of firms. Notwithstanding, the statistical significance of the latter effect depends on whether subjects played the treatments we are analyzing before or after the treatment in which we induce violations. Recall that subjects played two treatments in a session. In one treatment, we set the enforcement monitoring frequency to set the expected marginal penalty high enough to induce compliance in an expected profit-maximizing subject. In the other treatment, we lowered the monitoring frequency to induce the same subject to violate its permit holdings. We used the same structure of the penalty in both the "compliance" and the "violation" treatment. What we see in column 3 of Table 4 is that a linear penalty decreased the probability of a violation, on average, with respect to the convex penalty, in those subjects that played the violation treatment first, at the 5% significance level (coefficient = −2.666). In other words, when subjects played a low enforcement treatment

Table 4. 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)
Number of observations	1,806	1,806
Number of groups	112	112
* p < 0.1, ** p <.05, *** p <.01		

Note: robust standard errors, clustered at the group level.

first, the convex penalty produced more violations (in the compliance treatment we are analyzing) than the linear penalty. The effect seems to be weaker if subjects played the compliance treatment first. In this case, the effect is lower in size ($-2.667 + 2.64 - 1.126 = -1.15$) and it is not statistically different from zero (p-value = 0.3272). 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 individual levels of violations in a market for pollution permits, but it affects the individual compliance status. These results suggest that the effect of the penalty structure may operate at the margin. Effectively, 87.6% of the individual average levels of violations were lower or equal to one unit. Moreover, 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, not explained by conventional theory? A possible explanation is that fines may act as "focal point" for price setting. Monetary fines are "close relatives" of safety valves (Jacoby & 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). In other words, monetary fines set a cap on the willingness to pay for a permit. An alternative hypothesis is that fines may act as focal points to sellers, facilitating a tacit collusion on prices above the equilibrium level. The problem with this explanation is that the experimental evidence does not support it. This is true in the case of classical double-auction experimental markets (Isaac and Plott, 1981; Smith and Williams (1981)) and in the case experiments designed to facilitate collusion (Engelmann and Müller (2011).

In our experiments, convex and linear penalties provided the same incentive in the margin (that is, they provide the same incentive for the first unit of violation). Nevertheless, compared with linear penalties, convex penalties impose a higher expected penalty for violating the permits holding for more than one unit. Recall that buyers received an initial endowment of permits that was two or three units below the expected – profit maximizing level of emissions. Based on this, we think the following explanation is more plausible. The equilibrium price of permits in a market for pollution permits is a function of the number of permits issued by the regulator, the abatement costs of the firms and the level of the enforcement parameters. Recall that, according to theory, and the design of the experiment, the equilibrium price of permits should be between E$ 74 and E$80 in both treatments. As we see in Table 3, the average price of traded permits in both treatment were within these bounds. Nevertheless, Table 3 also shows that the average price was higher with a convex penalty (E$ 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, clustered at the group level.

We can see in Table 5 that, a linear penalty decrease the price of traded permits by E$ 6.5 with respect to an identical market enforced with a convex penalty, at the 1% significance level. This happens when it should not, because in both markets marginal penalties are set high enough to induce compliance. In the third column of Table 5 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, at the 5% significance level.

In sum, the results in Table 5 provide strong evidence that a linear penalty decreases the price of the permits with respect to a convex penalty.

The channels by which the structure of the penalty affects the prices of the permits are the bids and asks of permits. Therefore, we perform a series of regressions similar to those presented in Table 5, 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

Table 5. 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)
Treatment played First	−8.684***	2.689***
	(2.363)	(0.942)
First*Linear penalty	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)
Number of observations	267	267
Number of groups	27	27
* p < 0.1, ** p <.05, *** p <.01		

Note: robust standard errors, clustered at the group level.

[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.

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

statistically significant effect on the structure of the penalty on bids. On the other hand, we do find effects on asks. A convex penalty increases the minimum ask observed in a given market and period by E$ 5.8 at the 1% significant level. It also increases the lower 25% percentile of asks by a similar amount at the same level of statistical significance. Finally, it increases the median ask by E$ 4.8 at the 5% significance level. It does not have a statistically significant effect on the 75% percentile and maximum ask. Moreover, we do not find any statistically significant effect of the structure of the fine on the accepted bids, but 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 a convex penalty seems have caused potential sellers to increase, on average, the price at which they are willing to sell their permits, and this drove the prices up. As we do not observe a similar result on bids by potential buyers, we conclude that it was the supply side of the market, not the demand side, which drives the effect of penalties on prices. The simplest explanation for what we observe is that convex penalties increased the value of the permits for the buyers, by increasing the lowest willingness to pay for the average permit. This gave sellers more bargaining power, and they seem to have used it.

Finally, we explore whether the structure of the fine changes the number of "expected transactions" (potential sellers selling to potential buyers) or 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 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.

6.2. Results for standards experiments

6.2.1. Descriptive statistics for standards experiments

In this section, we present 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. For the rest, the standards experiments are the same as the market experiments. As explained above, we set four different emission standards, one for each type of firm, at the predicted level of emissions (and demand for permits) that each type of firm chooses in the market for tradable permits, in equilibrium. In addition, we induce perfect compliance with the same convex penalty in one treatment (S1) and linear penalty in the other treatment (S2).

Table 6 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 zero and one units, across subjects and periods for every type of firm in both treatments. 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%.

6.2.2. Tests for hypothesis 2 (standard experiments)

6.2.2.1. 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

Table 6. Descriptive statistics for the standards treatments.

		Type 1		Type 2		Type 3		Type 4	
		q	v	Q	V	q	v	q	v
TREATMENT S1		$s = 7$		$s = 6$		$s = 4$		$s = 4$	
Increasing marginal penalty									
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		$s = 7$		$s = 6$		$s = 4$		$s = 3$	
Constant marginal penalty									
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: In several sessions, the number of subjects that showed up was not multiple of eight. Because the subjects in the standards experiments do not interact with one another, we used two type-4 subjects from the sessions with surplus of subjects to complete two groups in those sessions with shortage of subjects. We also replaced the observations of three bankrupted type-4 subjects with another three subjects from the sessions with surplus subjects.

a regulator uses a convex or a linear penalty, when both penalties induce the same incentive to comply in the margin. 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($p = 0.92, Pearsonchi2 = 0.01, n = 207$), we cannot reject the null hypothesis that the average individual levels of violations are the same between treatments with convex and linear penalties.

6.2.2.2. Regressions. 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 estimating the same 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 7 below. As we did for the case of marketable permits, 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 another one with the violation *status* as the dependent variable (in the third column). According to these estimations, there is no statistically significant difference on the level or status of violations between treatments.[21]

According to both the non-parametric tests and the econometric analysis, we cannot reject Hypothesis 2. We conclude that the structure of the monetary fine does not affect the firm´s individual levels of emissions, when these firms face emission standards and

[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.

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

Table 7. 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)
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

the monetary penalties are high enough to induce compliance to expected-profit-maximizers firms.

7. Conclusion and discussion

As theory predicts, we find that the structure of the monetary fine for punishing violators (whether it is linear or convex in the violation level) has no effect on the individual levels of emissions when these are subject to emission standards. Nevertheless, conventional theory does not support in such a straightforward manner the results that we obtain when we test this same hypothesis for the case of a cap and trade regulatory system. In this case, we found that, as compared to a linear penalty, a convex penalty might increase the violation rate of firms in spite of not affecting the level of violations. The fact that the effect of the fine structure operates at the compliance margin explains this apparently contradictory result. Effectively, we find that a linear penalty increases compliance and decreases 1-unit violations. This effect turned out to be (statistically) significant in a regulatory system designed to induce compliance, as was the case of our experiments, because in this case almost 90% of the individual average levels of violations were lower or equal to one unit. The history of the enforcement seems to affect this result, notwithstanding. Effectively, according to our experiments, the convex penalties increase the violation rates at the margin more significantly when subjects were exposed to a low enforcement regime first.

The fact the we observe that convex monetary fines may increase the violation rate of polluting firms in market for permits but we observe no effect of the structure of monetary fines in the violation rates of firms in the case of emission standards, suggests that the channel by which a convex penalty increases noncompliance is the price of permits. We found evidence for this latter. Convex penalties increase the average price of permits and decrease the number of transactions in the market. The effect of the structure of the fine on prices, we found, operates through an increase in the asking prices of sellers, not on the bids by suppliers. More specifically, our data reveals that convex penalties increased the floor and the median of the asked prices. This evidence is consistent with the conclusion that, by increasing the buyer´s average lowest willingness to pay for a permit, a convex penalty gave sellers more bargaining power and they seem to have used it.

These results have important policy implications. First, and most importantly, a regulator interested in minimizing the overall costs of enforcing a cap and trade program should use linear penalties. Using convex penalties, according to our results, may put upward pressure on permit prices, violation rates, and enforcement costs. In other words, relative to linear penalties, the regulator may need to inspect firms more frequently in order to enforce the same cap of emissions when using convex penalties.

If the structure of the penalty is not among the levers that the environmental regulator can pull, our results raise the issue of how to minimize the extra resources needed to attain perfect compliance when using convex penalties. In this respect, there seems to be two alternatives for the regulator. One is to direct the extra monitoring effort at firms with the lowest ratio of number of initially allocated permits to the number of the final expected demand for permits. Alternatively, the regulator could manipulate the initial allocation of permits. If, as indicated by our experiments, a convex penalty increases the bargaining power of sellers, decreases the number of trades and increases the price, skewing the initial allocation of permits towards the would-be buyers could curb these effects.[22] This would indicate that in the presence of convex penalties, the initial allocation of permits does affect the result of the market. In practice, nevertheless, manipulating the initial allocation of permits in this way would require the regulator to have good information on the firms´ abatement costs. Obtaining this information may be costly. To avoid these costs, the regulator could auction the permits itself. In sum, it seems that having the option to affect the structure of the penalty may be less cumbersome for the regulator.

Finally, conducting lab experiments always brings up the issue of the external validity of its results and policy implications. We know lab experiments cannot replicate every aspect of real life regulation of polluting firms. In spite of this, lab experiments could be good tools for analyzing real life situations if they replicate the fundamental issues regarding the hypotheses in question. If they do not, however, it may be that the theoretical model in which they rest is to blame, not the experiments. Take for example the assumption of minimal transactions costs in the market for permits. In reality, transaction costs (searching, formalization of transactions, etc.) could be substantial (Stavins, 1995). This could jeopardize the external validity of the experiments in some cases.[23] Another issue that affects the external validity of lab experiments is whether experimental subjects behave in the same manner an actual polluter that faces that incentives would. This is an important point. In their widely cited work, Levitt and List

[22]This implication is consistent with Hahn's (1984) result, but for a very different reason.

[23]In the case of our experiments, we believe that transaction costs are likely to increase the effect that we find, as it would another source of friction that would increase prices and reduce transactions.

(2007) state that the nature and extent of scrutiny is one of the key factors influencing the behavior in the lab. In their behavioral model, the moral costs of violating a social norm depends on the degree of scrutiny of the subject´s behavior. The more the scrutiny, the more the moral costs. Following their model, knowing that experimenters are recording their actions, subjects participating in experiments may behave differently in the lab than outside the lab. Barmettler, Fehr, and Zehnder (2012) test the effect of anonymity in the degree of pro-social behavior in the laboratory using three typical games used in the literature of pro-social behavior. They find that the introduction of complete anonymity had no statistically significant effect in any of the three games.

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 for penalties to depend also on the compliance history of firms and consider make good provisions, features that we did not consider in our analysis. In addition, 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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