March, 2012

Firms' Compliance of Emissions Regulations under different Penalty Schemes

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Acknowledgments: We gratefully acknowledge financial support provided by the Agencia Nacional de Investigación e Innovación (ANII) - Fondo Clemente Estable - Uruguay, under Project FCE_2009_1_2801, Eduardo Cancela, Silvana Acosta and Juanita Bloomfield for the invaluable help in the programming and assistance.

This paper is an incomplete draft of a work in progress. Please do not quote or cite this paper without permission of the authors. Comments and suggestions for improving the work would be greatly appreciated.

Testing Firms' Compliance Behavior under Different Penalty Schemes and Policy Instruments for Pollution Control

Abstract: We present preliminary results of a series of laboratory experiments performed to test hypotheses on compliance behaviour of firms under different enforcement regimes in a program that caps aggregate emissions of a given pollutant. The enforcement design for the laboratory experiments considers inducing individual perfect compliance and also allow for violations, under both emissions standards and a tradable discharge permits system. Our results indicate that the theory predicts well the experimental behavior of subjects in the case of emission standards. In this case, the level of individual emissions is only a function of the marginal benefits of emissions and particularly it is not a function of the combination of monitoring probability and the standard. In the permits experiments, as theory predicts, we obtain the result that the regulator can induce the same level of individual violations with two different penalty structures. Contrary to theory, nevertheless, our experiments suggest that a regulator cannot manipulate the supply of permits and the monitoring probability accordingly so as to induce the same level of individual emissions, when the policy induces compliance as when induces violations.

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

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

1. Introduction

In this paper, we study firms' compliance behaviour under different penalty structures in a program that caps aggregate emissions of a given pollutant from a set of heterogeneous firms based on emissions standards and a tradable discharge permits system. We present preliminary results of the first of a series of laboratory experiments that we are running to explore enforcement and compliance results under a constant marginal penalty and an increasing marginal penalty considering different policy instruments. The enforcement design for the laboratory experiments considers inducing individual perfect compliance and also allow for violations.

Our main interest with these hypothesis is to test whether if, as theory predict, a regulator can induce a certain level of emissions to a set of heterogeneous firms by altering the level of the monitoring probability and the level of the individual emission standard accordingly, when emission standards is the regulatory instrument, or by altering the monitoring probability and the number of emission permits supplied to the market, when the regulatory instrument is emission tradable permits. Because the structure of the penalty function is central to the theoretical cost-effectiveness of enforcing emission standards and tradable permits (see for example Stranlund (2007), Arguedas (2008), Caffera and Chávez (2011)), we test the above issue with linear marginal penalties and increasing marginal penalties.

The paper is organized as follows. In section 2, we briefly present an individual firm´s compliance behaviour model under a system of a firm-specific emissions standards and a transferable emissions permit system. We use this model to derive the main hypothesis we want to evaluate by designing a laboratory experiment. Section 3 contains a description of the experimental design under an emissions standard system and under a transferable emissions permit system. We also present here the main experimental procedures. Section 4 presents the results we have obtained thus far. In Section 5, we put forward some preliminary concluding remarks from our work.

2. Conceptual Framework and Hypothesis

In this section we present the conceptual model of firm´s compliance behavior under different penalty structures and policy instruments for pollution control developed by Caffera and Chávez (2011). We then discuss the hypothesis we evaluate by designing a laboratory experiment.

2.1 Compliance behavior under different penalty structures and policy instruments

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 Q, exogenously determined by the regulatory authority.

We first consider the case of a command and control environmental policy in which each firm faces an emissions standard s. Under this policy the regulator defines for each firm the maximum allowable (legal) level of emissions. Emissions standards for all firms satisfy $\sum_i s_i = Q$. 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 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 $\gamma \geq 0$. We notice that when $\gamma = 0$, there is a linear penalty (constant marginal penalty), when $\gamma > 0$, the penalty is strictly convex (the marginal penalty is linear).

To ensure that perfect compliance is a possible outcome, we assume $-c'(s) \leq \varphi$; that is, the firm's marginal abatement cost evaluated at the standard is not greater than the marginal penalty for a slight violation. If the inequality was reversed, then the firm would choose to be noncompliant even if it was monitored with certainty, because the marginal benefit of violating the standard would be greater than the expected penalty for some level of violation.

Under an emissions standard, a firm chooses the level of emissions to minimize total expected compliance cost, which consists of its abatement costs plus the expected penalty.

On the other hand, under a transferable emissions permit system, a total of $L = Q$ licenses are issued by a regulatory authority, each of which confers the legal right to release one unit of emissions. 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 $q > l$.

Enforcement, from the firm's point of view, remains the same as under a system of emissions standards, which implies an audit probability and a penalty if audited and found in violation. As for the case of emissions standards, to allow for perfect compliance as a possible outcome under a transferable emissions permit system, we assume $p \leq \varphi$. For a transferable emissions permit system, a firm chooses its emissions and permit demand to minimize compliance costs – abatement costs, receipts or expenditures from buying or selling permits, and the expected penalty – taking the enforcement strategy as given.

As it is shown by Caffera and Chávez (2011) as well as by many others [Heyes (2000), Malik (1992) Harford (1978)] a firm will be compliant whenever it chooses a level of emissions consistent with $-c'(s) \leq \pi \varphi$. Thus, an individual firm's compliance choice requires the expected marginal penalty to be no lower than the marginal abatement cost associated with an emissions level equivalent to the emissions standard.

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 $-c'(q) = \pi[\varphi + \gamma(q - s)]$.

In the case of a transferable emissions permit system, we know that a firm is compliant if and only if: $-c'(l) \leq \pi\varphi$. (For details, 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 permit market equilibrium condition is $\sum_{i=1}^{n} l_i(p) = L = Q$, which implicitly defines the equilibrium permit price as a function of the total number of licenses; that is, $p(Q)$. Hence, under a transferable emissions permit system, a firm will be compliant whenever $p(Q) \leq \pi\varphi$; suggesting that a firm will comply with the regulation when the expected marginal penalty is not lower than the equilibrium price obtained in a competitive permits market.

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 $\sum_{i=1}^{n} l_i(p, \pi, \varphi, \gamma) = L < Q$, which implicitly defines the equilibrium permit price as a function of the total number of licenses and enforcement parameters; that is, $p^{nc}(L, \boldsymbol{\pi}, \varphi, \gamma)$, where $\boldsymbol{\pi}$ is a vector of monitoring probabilities on regulated firms.

2.2 Hypotheses

We now present the main hypothesis we evaluate by designing a laboratory experiment. A brief discussion is included.

Our first hypothesis is that individual firm's compliance decision does not depend neither on the regulatory instrument nor on the penalty structure. That is,

H1. *If the marginal expected cost of violation is larger than the marginal benefit of violation, a risk-neutral firm complies. Otherwise, it does not. This does not depend on the instrument chosen for regulating emissions (emission standards vs tradable permits), or the fine structure (constant or increasing in the margin).*

As previously discussed, under emissions standards a firm comply only if $-c'(q = s) \leq \pi\phi$, while under a transferable emissions permit system the firm comply only if $-c'(q = l) = p \leq \pi\varphi$. By simple inspection of these conditions, we conclude that neither the penalty structure nor the pollution control instrument play a role in the compliance decision.

H2. *The individual level of violations does not depend on the structure of the penalty function: a regulator that induces a certain level of individual violations with a penalty function* $f(q - x) = \varphi(q - x) + (\gamma/2)(q - x)^2$*, where x is the legal level of emissions,* $\varphi>0$ *and* $\gamma>0$*, can induce the same level of emissions and violations with a penalty function* $f(q - x) = (\gamma/2)(q - x)^2$*, provided that the marginal expected penalty at the desired level of emissions is the same.*

Suppose that the firm is violating an emissions standard, from Caffera and Chávez (2011) we know that the firm is going to choose a level of emissions $q(s, \pi) > s$, where $q(s, \pi, \varphi, \gamma)$ is the solution to $-c'(q) = \pi[\varphi + \gamma(q - s)]$, for a given monitoring probability and marginal abatement cost, it is always possible to choose a combination of φ and γ, which generate the same marginal penalty, therefore, the emissions choice and violation is invariant to the penalty structure. As for a transferable emissions permit system, 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 $p = \pi[\varphi + \gamma(q(p) - l)]$. Given an equilibrium price of permits and a monitoring probability, it is possible to choose a combination of φ and γ, which generate the same marginal penalty at the desired level of emissions, therefore, the demand of permits and violation is invariant to the penalty structure and the level of violation.

H3. With standards, the regulator can maintain the level of emissions constant by decreasing the standard and the monitoring probability accordingly (but allowing for violations).

As discussed before, under an emissions standard system, the firm is going to choose a level of emissions $q(s, \pi) > s$, where $q(s, \pi, \varphi, \gamma)$ is the solution to $-c'(q) = \pi[\varphi + \gamma(q - s)]$, for a given marginal abatement cost and penalty structure, it is always possible to reduce the monitoring probability π and the emissions standard s, generating the same marginal penalty, therefore, the emissions choice remain the same. Because of the reduction in the emissions standard, the firm will be noncompliant.

H4. *Under tradable permits the regulator can maintain the level of emissions constant by decreasing L and the monitoring probability accordingly.*

Assume that the compliance conditions under a transferable emissions permits system holds. Then, it is always possible to reduce both, the supply of permits Q and the monitoring probability π, such that the equilibrium price of permits remain the same. If this is the case, then the choice of emissions remains the same.

3. Experimental Design and Procedures

3.1. Experimental Design

We framed the experiments as a neutral production decision of an unspecified fictitious good. Subjects obtained benefits from the production of the fictitious good *q*. Every subject had a production capacity of 10 units (they could only produce whole numbers), but the benefits of production from these units differ between subjects. There were four possible "ladders" of marginal benefits, depicted in Table 1. These were obtained from Cason and Gangadharan (2006). The four ladders gave place to four "types" of subjects. Each experiment had eight subjects. There were two subjects of each type in each experiment.

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 9 different treatments for these experiments varying the following variables: (1) the regulatory instrument (standard / tradable permits), (2) the marginal expected penalty function, and (3) the level of the emission standards or the number of permits supplied.

3.1.1. Standards

In the standards experiments subjects faced a maximum allowable level of production (the standard), and had to decide how much to produce. After their decision, at the end of each period, the subjects were audited with a known, pre-determined and exogenous probability π_i. If audited, the number of units produced by the subject in that period was compared with the legal maximum level of production (the standard) set for its type. If the level of production chosen was superior to the standard, the subject was automatically fined.

Before making the decision the subjects had the information on the marginal expected fine for every level of violation in their screens. The marginal expected penalty function for subject i is $\pi_i \times f(q_i - s_i)$, where π_i is the exogenous, known monitoring probability faced by subject i, q_i is the quantity of units produced by subject i, s_i is the production limit faced by subject i (the standard). As in Caffera and Chávez (2011), we use the following penalty function structure: $f(v_i) = \varphi \times v_i + \frac{\gamma}{2} \times {v_i}^2$, where v_i is the violation level of subject i and φ and γ are non-negative parameters. Following Arguedas (2007), we call φ the linear component of the fine and γ the progressive component of the fine.

The idea of the experiments is to test whether a regulator can maintain a certain level of individual (and therefore aggregate) emissions by varying the level of the individual emission standards and the monitoring probability accordingly, and that it can do this using for the case in which φ and γ are both positives and the case in which one of them is zero. Therefore, we constructed 5 treatments for standards. These are treatments 5 through 9 in Table 2. In treatment number 5, the emission standards are 7, 6, 4 and 3 for types 1 to 4, respectively, the monitoring probabilities are all in the 0,60s for all the firms, and violations are fined with a penalty structure with a positive linear and a positive progressive component ($\varphi = 100$ and $\gamma = 66{,}67$). This policy induces compliance for expected profit maximizing subjects, so the expected aggregate level of production is 40 units in a group of 8 subjects, two of each type ($7 \times 2 + 6 \times 2 + 4 \times 2 + 3 \times 2$). Treatment 6 is the same as Treatment 5, except that noncompliance is fined with a linear marginal penalty ($\varphi = 133$ and $\gamma = 0$). In Treatment 7, the standards are decreased for every type of subject, so that the aggregate cap of emissions is 20, but monitoring probabilities are decreased accordingly so as to keep the level of emissions of expected profit maximizing subjects in the same level as in Treatment 5 and 6. Therefore, Treatment 7 induces violations. Treatments 8 and 9, were designed to produce the same level of emissions as every treatment in these experiments, inducing violations as Treatment 7, but with different penalty structures (and according probabilities). In Treatment 8 noncompliance is sanctioned with a constant marginal penalty ($\varphi = 100$ and $\gamma = 0$). In Treatment 9 noncompliance is sanctioned with a progressive only component ($\varphi = 0$ and $\gamma = 66{,}67$).

3.1.2. Tradable permits

In the permits experiments, subjects had to buy a permit in order to be legally able to produce one unit of the good. Subjects in this case had to decide how much to produce of the fictitious good and how many permits to buy or sell. The auditing procedure was exactly the same as in the case of standards, except that in the case of tradable permits a violation is defined as $q_i - l_i > 0$, where l_i is the number of permits possessed by subject *i* at the end of the period.

We constructed 4 treatments for the case of markets for permits. (See the first 4 rows of table 2). In Treatment 1 the total number of tradable permits supplied for each group of 8 subjects was 40. The initial allocation was 4 permits for subjects of type 1 and 2, and 6 permits for subjects of type 3 and 4. The enforcement of the market was implemented through an increasing marginal penalty with both a linear and a progressive component ($\varphi = 100$ and $\gamma = 66{,}67$). Treatment 2 was exactly the same as treatment 1, except that the structure of the penalty function was different. In this case the marginal penalty was linear ($\gamma = 0$), with the value of the parameter φ adjusted accordingly ($\varphi = 133$) to induce compliance for risk neutral subjects.

In contrast to Treatments 1 and 2, Treatments 3 and 4 induce or allow a certain level of violations of the permits holdings, but such that the expected level of aggregate of emissions would be 40 units. This is done by decreasing the total number of permits supplied to 20 (initial allocations halved) and by decreasing the monitoring probability from 0.6 to 0.32). In Treatment 3 we used the same penalty structure as in Treatment 1. In Treatment 4 we use a progressive-only marginal penalty ($\varphi = 0$ and $\gamma = 100$). The 4 Treatments were calibrated such that the expected equilibrium price is the same, between $E 74 and $E80.

Table 2: Treatments

<table>
<tr><th rowspan="2">Treatment</th><th rowspan="2">Regulation</th><th colspan="4">Monitoring Probability by firm´s type</th><th colspan="2">Fine parameter values</th><th rowspan="2">Policy Induces</th><th rowspan="2">Number of tradable permits supplied / Aggregate Standard</th><th rowspan="2">Equilibrium price / Emission standards</th><th rowspan="2">Expected Aggregate level of emissions</th></tr>
<tr><th>Type 1</th><th>Type 2</th><th>Type 3</th><th>Type 4</th><th>Phi</th><th>Gamma</th></tr>
<tr><td>1</td><td rowspan="4">Tradable Permits</td><td>0,60</td><td>0,60</td><td>0,60</td><td>0,60</td><td>100</td><td>66,66</td><td rowspan="2">Compliance</td><td rowspan="2">40</td><td rowspan="4">80 - 74</td><td rowspan="9">40</td></tr>
<tr><td>2</td><td>0,60</td><td>0,60</td><td>0,60</td><td>0,60</td><td>133</td><td>0</td></tr>
<tr><td>3</td><td>0,30</td><td>0,30</td><td>0,30</td><td>0,30</td><td>100</td><td>66,66</td><td rowspan="2">Violations</td><td rowspan="2">20</td></tr>
<tr><td>4</td><td>0,32</td><td>0,32</td><td>0,32</td><td>0,32</td><td>0</td><td>100</td></tr>
<tr><td>5</td><td rowspan="5">Standards</td><td>0,60</td><td>0,65</td><td>0,63</td><td>0,66</td><td>100</td><td>66,66</td><td rowspan="2">Compliance</td><td rowspan="2">40</td><td rowspan="2">Type 1 = 7;
Type 2 = 6;
Type 3 = 4;
Type 4 = 3</td></tr>
<tr><td>6</td><td>0,60</td><td>0,65</td><td>0,63</td><td>0,66</td><td>133</td><td>0</td></tr>
<tr><td>7</td><td>0,24</td><td>0,26</td><td>0,32</td><td>0,31</td><td>100</td><td>66,66</td><td rowspan="3">Violations</td><td rowspan="3">20</td><td rowspan="3">Type 1 = 4;
Type 2 = 3;
Type 3 = 2;
Type 4 = 1</td></tr>
<tr><td>8</td><td>0,75</td><td>0,84</td><td>0,8</td><td>0,8</td><td>100</td><td>0</td></tr>
<tr><td>9</td><td>0,34</td><td>0,37</td><td>0,50</td><td>0,53</td><td>0</td><td>66,66</td></tr>
</table>

3.2. Experimental Procedures

Participants were recruited mainly from the undergrad student population of the School of Business and Economics of the University of Montevideo, Uruguay. But we also invited students from the same schools of the University of the Republic and the ORT University. The experiments were conducted in a computer lab at the University of Montevideo, specifically conditioned for these experiments. The experiments were programmed and conducted with the software z-Tree (Fischbacher 2007).

Subjects that participated first in standards sessions were then invited to participate in another session in which we plan, without their knowledge, to run permits, and vice versa. We allocate standards and permits sessions evenly in mornings and afternoon, and days of the week to prevent any possible selection bias of subjects. A total of 59 subjects participated in the experiments ran in December 2011. 21 of these subjects participated in both a standard and permits sessions. So we conducted a total of 10 eight-subject experiments, 5 for standards and 5 for permits. (80 experimental subjects). Each eight-subject experiment consisted of 20 rounds. In the first 10 rounds subjects participated in one treatment. In the second 10 rounds they participate in another treatment. Both treatment were either standards or permits. The order of treatments differ between groups. In the standards experiments, 16 subjects played Treatment 5 and then Treatment 7, and 24 played in reverse order. In the permits experiments, 16 subjects played first Treatment 1 and then Treatment 3, 16 the opposite, and 8 subjects played Treatment 2 and then Treatment 4.

Before the beginning of the experiments, instructions were read aloud and questions were answered. Prior to the first round in the first treatment, subjects played 2 trial rounds in the case of standards, and trial experiment of 5 rounds in the case of permits. This trial experiment contained the same features of the real experiments but a different set of parameters. In the standards experiments each period lasted 2 minutes. In the permits experiments 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. After all subjects 8 subjects in the experiment had made their decision, the computer program automatically produced a random number between 0 and 1. If this number was below the informed probability of being monitored, the subject was inspected. This procedure was independent for each subject. Subjects were informed in their screen about the result of this random process: if 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.

Subjects were paid around 7 US$ ($U 150) for showing up on time in the experiments sessions and then earned more money from their participation in the experiment. 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. These payments ranged between $U 507 (around US$ 25) and $U 100 (around US$ 5) with a mean value of $U 345 (around US$ 17) and a standard deviation of $U 86 around US$ 4).

At the end of the experiment subjects answered a questionnaire. The questionnaire included questions related to the university the student attends, school, major, sex, age, income, political tendency (form 1 to 10, with 1 being the "most left" and 10 the "most right"), attitudes towards cheating and a Holt and Laury (2002) type of test to assess the subject risk aversion. In our test the subjects were confronted to 10 choices between a certain amount of money (Option A, fixed across the 10 choices), and a lottery (option B) with increasing probabilities of the larger prize. The larger prize was larger than the fixed amount offered in option A and has a probability equal to 1 in the tenth choice, so every subject should choose option B in the 10^{th} choice.

4. Results

We begin our analysis of the results of these first experiments by comparing these results with what theory predicted. We do this for markets for permits and for standards. We then test our hypothesis using panel data estimations.

Table 3 compares the summary statistics of key variables in the markets experiments with what theory predicts for the case of cost-minimizing, risk-neutral, agents. General observations are the following. First, the individual level of violations is similar to the predicted level in the compliance treatments (1 and 2). These same numbers are somehow below the predicted values in the violations treatments (3 and 4). Second the equilibrium price is exactly as predicted in Treatment 1, below in Treatment 2, and above in Treatments 3 and 4 (the latter are violation treatments). Across all experiments the number of transactions per period was above the predicted level, 3.

Table 3 – Comparison of predicted results with summary statistics for Permits Experiments

(Market) Treatment			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**
1	**Theory**		**74-80**	**5**	**7**	**7**	**0**	**6**	**6**	**0**	**4**	**4**	**0**	**3**	**3**	**0**
	Experiments	Mean	78.8	14.0	6.3	5.5	0.8	6.6	6.2	0.4	4.7	4.4	0.3	4.4	4.0	0.4
		Median	80.0	11.0	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.	8.1	6.8	1.2	1.6	1.6	1.2	1.0	0.7	1.2	1.0	0.6	1.5	1.3	0.6
		# Obs.	50.0	50.0	160.0	160.0	160.0	160.0	160.0	160.0	160.0	160.0	160.0	160.0	160.0	160.0
2	**Theory**		**74-80**	**5**	**7**	**7**	**0**	**6**	**6**	**0**	**4**	**4**	**0**	**3**	**3**	**0**
	Experiments	Mean	76.0	22.5	6.8	6.2	0.6	7.1	5.7	1.4	4.9	4.4	0.6	4.0	3.8	0.2
		Median	76.8	23.0	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.	4.1	6.4	1.2	1.3	1.3	1.5	2.0	2.5	1.3	1.1	1.4	1.1	1.0	0.6
		# Obs.	60.0	60.0	280.0	280.0	280.0	280.0	280.0	280.0	280.0	280.0	280.0	280.0	280.0	280.0
3	**Theory**		**74-80**	**3**	**7**	**4**	**3**	**6**	**3**	**3**	**4**	**2**	**2**	**3**	**1**	**2**
	Experiments	Mean	103.6	9.3	5.6	3.0	2.6	4.7	2.7	1.9	3.8	2.3	1.6	3.5	2.0	1.4
		Median	105.2	7.0	5.0	3.0	2.0	4.0	3.0	2.0	3.0	2.0	2.0	3.0	2.0	1.0
		Std. Dev.	11.0	5.2	1.7	1.6	2.1	1.7	1.0	1.8	1.7	1.2	1.2	1.4	0.9	1.2
		# Obs.	40.0	40.0	140.0	140.0	140.0	140.0	140.0	140.0	140.0	140.0	140.0	140.0	140.0	140.0
4	**Theory**		**74-80**	**3**	**7**	**4**	**3**	**6**	**3**	**3**	**4**	**2**	**2**	**3**	**1**	**2**
	Experiments	Mean	102.1	8.3	5.8	3.3	2.5	4.9	2.5	2.5	4.8	2.4	2.3	3.2	1.8	1.4
		Median	94.0	8.0	5.0	3.0	3.0	5.0	2.0	2.0	4.0	3.0	2.0	3.0	2.0	1.0
		Std. Dev.	13.4	2.0	1.6	1.5	1.3	2.3	1.1	2.4	1.7	1.1	1.2	1.2	0.7	1.0
		# Obs.	20.0	20.0	60.0	60.0	60.0	60.0	60.0	60.0	60.0	60.0	60.0	60.0	60.0	60.0

Table 4 – Comparison of predicted results with summary statistics for Standards Experiments

(Standard) Treatment			Level of violation				
			Type 1	Type 2	Type 3	Type 4	Total
5	**Theory**		**0**	**0**	**0**	**0**	**0**
	Experiments	Mean	1,1	1,3	0,8	1,2	
		Median	1,0	0,0	0,0	1,0	
		Std. Dev.	1,1	1,7	1,4	1,4	
6	**Theory**		**0**	**0**	**0**	**0**	**0**
	Experiments	Mean					
		Median					
		Std. Dev.					
7	**Theory**		**3**	**3**	**2**	**2**	**20**
	Experiments	Mean	2,7	2,5	1,2	2,3	
		Median	3,0	2,0	1,0	2,0	
		Std. Dev.	1,4	1,9	1,1	2,4	
8	**Theory**		**3**	**3**	**2**	**2**	**20**
	Experiments	Mean					
		Median					
		Std. Dev.					

Table 4 shows that on average, subjects violated more, as predicted, in Treatment 7 than in Treatment 5. Nevertheless, as standards halved in Treatment 7, as compared to Treatment 5, this table is also saying that emissions may have decreased more than predicted. This is shown in our econometric results.

4.1. Results for Hypothesis 2

Hypothesis 2 states: *The individual level of violations does not depend on the structure of the penalty function: a regulator that induces a certain level of individual violations with a penalty function* $f(q - x) = \varphi(q - x) + (\gamma/2)(q - x)^2$*, where x is the legal level of emissions,* $\varphi>0$ *and* $\gamma>0$*, can induce the same level of emissions and violations with a penalty function* $f(q - x) = (\gamma/2)(q - x)^2$*, provided that the marginal expected penalty at the desired level of emissions is the same.*

A first test for this hypothesis

Insert and discuss descriptive statistics for vi(T3) = vi(T4) per each subject's type.

To formally test Hypothesis 2 for the case of a transferable emissions permit system, using the observed subjects' behavior, we estimate the following general model,

$$v_{it} = f(TREATMENT_i, FIRM\text{-}TYPE_i, PRICE_t, RISK_i) \qquad [1]$$

where v_{it} is the level of permit violation of subject *i* during round *t*. *TREATMENT* is a dummy variable, with *TREATMENT* = 1 if treatment 3 (permit market, allowing permit violation, increasing marginal penalty with $\phi > 0$, and low level of aggregated emissions) and *TREATMENT* = 0 for observation generated during the sessions under treatment 4 (permit market, allowing permit violation, increasing marginal penalty with $\phi = 0$, and low level of aggregated emissions); *FIRM-TYPE* is a set of four dummy variables to control for firm type according to marginal abatement costs' functions; *PRICE* is the mean permit price in each round which is estimated by an auxiliary regression using instrumental variable methods. Finally, *RISK* is a risk aversion index which represents subjects' risk preferences according to Holt and Laury's lottery.

We estimated this specification as a random effect model considering all sample observations, and excluding observations from sessions where individual went to bankruptcy. We also estimated the model excluding the first two rounds in each session. Because the results turned to be robust in all these estimations, we present results based on the use of all sample observations for treatments 3 and 4, according to the experimental design. Table X shows the results for four model specifications. Models 1 and 2 do not consider interaction between mean permit price and subjects' (firms') type. Models 2 and 4 include implementation of instrumental variable procedure to correct for possible bias caused by permit price endogeneity.

The results allow us to fail to reject Hypothesis 2 for the case of an emissions permit market. The experimental evidence suggests that subjects' violations in a transferable emissions permit system with incomplete enforcement turned out to be independent of the penalty structure we used in the experimental design. As it is shown in Table X, the estimated coefficient for variable *TREATMENT 3* is not statistically significant. Consistent with the theoretical literature on enforcement and compliance in transferable emissions permit systems, the *PRICE* variable has a positive and significant effect on the individual level of violation. However, this result disappears when controlling for potential interactions between *PRICE* and *FIRM-TYPE* variables.

Table X. Random Effect Models of Individual Violations in Market Treatments

	Model 1	Model 2	Model 3	Model 4
TREATMENT 3	-0.278	-0.292	-0.278	-0.335
	(0.195)	(0.190)	(0.195)	(0.205)
TYPE2	0.060	0.061	-0.844	-1,854
	(0.292)	(0.263)	(0.816)	(28,966)
TYPE3	-0.291	-0.291	-0.841	17,478
	(0.294)	(0.267)	(1,197)	(24,978)
TYPE4	-0.404	-0.404	-2.070***	2,771
	(0.277)	(0.273)	(0.783)	(19,155)
PRICE	0.008*	0.011**	-0.001	0.069
	(0.005)	(0.005)	(0.007)	(0.198)
RISK	-0.084	-0.083	-0.083	-0.063
	(0.060)	(0.056)	(0.061)	(0.066)
PRICExTYPE2			0.011	0.024
			(0.011)	(0.358)
PRICExTYPE3			0.007	-0.219
			(0.016)	(0.308)
PRUCExTYPE4			0.021*	-0.039
			(0.011)	(0.236)
CONSTANT	2.111***	1.884***	2.851***	-2,930
	(0.525)	(0.581)	(0.724)	(16,310)
Number of Observations	1740	1740	1740	1740
Number of clusters	174		174	

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$.

4.2. Hypothesis 3

In Table 5 we present the results of the random effects estimation to test our Hypothesis 3. The variables "Type" refer to the Type of subject, according to their marginal benefits of Table. Type 1 subjects are those with larger marginal benefits of production, then Type 2 subjects, and so on. *Treatment 5* is a dummy variable equal to 1 if the treatment played was Treatment 5 and 0 if it was Treatment 7. Treatment 5 induced perfect compliance of laxer emissions standards while Treatment 7 induced violations of stricter emission standards through lower monitoring probabilities. Both treatments induce the same level of emissions for a risk-neutral subject.

The variable *"Risk Aversion"* varies from 1 to 10 according the number of the lottery in which they switched to option B. The larger the number the lower the level of risk aversion.

The dependent variable in all four regressions presented in Table 5 is the level of emissions of subject *i* in period *t*. Regression 1 presents a simple model in which the level of emissions is a function of the firm type and the Treatment (that is, the combination of the levels of emission standard and monitoring probability). According to the theory, risk-neutral subjects should not change their level of emissions between treatments, so we expect this variable to be statistically insignificant. In regression 2 we control for the level of risk aversion. The treatment variable is statistically significant in these two first specifications. In regression 3 we add an interaction between the variables Treatment 5 and Risk Aversion. We expected this interaction variable to the Treatment 5 variable statistically insignificant, as it does. Nevertheless, we also expected it to take all the variation of the dependent variable that is attributed to Risk Aversion in the Regression 2, but it does not. Notwithstanding, we obtain this result when we include, all the interaction effects. In this final regression 4, the level of individual emissions is only a function of the marginal benefits of emissions and particularly it is not a function of the combination of monitoring probability and the standard.

Table 5: Random Effects Estimation of the Level of Individual Emissions (Standards)

	Regression 1	Regression 2	Regression 3	Regression 4
Type1	3.660***	3.693***	3.693***	4.738***
	(0.383)	(0.411)	(0.414)	(1.622)
Type2	2.770***	2.860***	2.860***	8.054***
	(0.383)	(0.419)	(0.422)	(1.751)
Type3	0.575	0.696*	0.696*	2.466
	(0.383)	(0.403)	(0.406)	(1.615)
Treatment 5	1.127***	1.057***	0.698	0.652
	(0.271)	(0.286)	(0.923)	(0.960)
Risk Aversion		-0.174**	-0.202**	0.145
		(0.0735)	(0.102)	(0.243)
Treatment*RiskAversion			0.0572	0.0829
			(0.140)	(0.140)
Type1*Treatment5				0.142
				(0.785)
Type2*Treatment5				-0.462
				(0.799)
Type3*Treatment5				-0.151
				(0.769)
Type1*RiskAversion				-0.192
				(0.271)
Type2*RiskAversion				-0.793***
				(0.276)
Type3*RiskAversion				-0.311
				(0.256)
Constant	3.036***	4.183***	4.363***	2.347
	(0.303)	(0.523)	(0.685)	-1.431
Observations	800	700	700	700
Number of subjects	80	70	70	70
Standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

4.3. Hypothesis 4

Hypothesis 4 states that *under tradable permits the regulator can maintain the level of emissions constant by decreasing L and the monitoring probability accordingly*. We test this hypothesis comparing the individual level of emissions in a treatment in which the regulator induces perfect compliance in a system of tradable permits with those in a treatment in which the regulator decreases the level of supplied permits and the monitoring probability accordingly, so as to induce the same level of emissions as in the previous treatment.

Table x: Treatment 1 vs Treatment 3 test of equal average level of emissions by type (Ho: Difference = 0 ; Ha: Difference ≠ 0)

Sample	Market Treatment	Observations per Type	Mean Level of Emissions			
			Type 1	Type 2	Type 3	Type 4
All	1	160	6.3	6.6	4.7	4.4
	3	160	5.2	4.8	3.8	3.6
Difference			-1.1	-1.8	-0.9	-0.8
Pr(|T| > |t|)			0.0000	0.0000	0.0000	0.0000
Without groups with bankrupt	1	160	6.3	6.6	4.7	4.4
	3	140	5.6	4.7	3.8	3.5
Difference			-0.7	-1.9	-0.9	-0.9
Pr(|T| > |t|)			0.0000	0.0000	0.0000	0.0000

A first two sample t test (assuming equal variances) leads us to reject Hypothesis 4. According to our experiment, it is no possible for a regulator to induce the same level of emissions when it designs a system of transferable permits to induce perfect compliance as when it designs the system to induce the same level of emissions but with violations of the permits holdings. These violations are generating by decreasing the amount of permits offered and a decrease in the level of the monitoring probability accordingly, so as to generate the same equilibrium price of the market of permits and therefore the same level of emissions.

As a second test for hypothesis 4, we ran the following random effect model

$$q_{i,t} = f(Treatment_i, Type_i, Price\ of\ permits_t, Level\ of\ risk\ aversion_i) \qquad (4)$$

where the level of emissions of the firm *i* in period *t* ($q_{i,t}$) is a function of: (a) the strictness of the enforcement (indicated by the variable *Treatment*, which is equal to one if the treatment induces violation (Treatment 3) and zero otherwise, (b) the type of the firm (given by its marginal benefits of emissions), (c) the price of the pollution permits, (d) a control for the level of risk aversion of the subject, and interactions.

Because the average price of the permits in a period ($Price\ of\ permits_t$) is endogenous to the subjects' emissions choice, we first ran an auxiliary regression to obtain an instrument for the average price of permits in the period. The instruments chosen are: (a) an indicator variable for the group of eight subjects that make the market in which the subject is interacting, (b) the number of transactions in the period, (c) the standard deviation of the price in the period, and (d) the number of the period. We present the first stage estimation results of the price equation in Table 6 below.

Table 6: First-stage G2SLS regression for the average price per period
All sample observations

	Model 2	Model 4			
	Average Price	Average Price	Average Price* Type2	Average Price* Type3	Average Price* Type4
TREATMENT INDUCES VIOLATION	-0.0000 (0.08038)	0.0000 (0.2502)	0.0000 (0.3206)	0.0000 (0.3255)	-0.0000 (0.2971)
TYPE 2	0.0000 (1.1272)	0.0000 (0.3510)	84.0805*** (0.4496)	0.5282 (0.4564)	0.5086 (0.4166)
TYPE 3	0.0000 (1.1541)	0.0000 (0.3594)	0.8170* (0.4603)	84.0737*** (0.4673)	0.5843 (0.4266)
TYPE 4	0.0000 (1.1333)	0.0000 (0.3529)	-0.1868 (0.4520)	-0.0049 (0.4589)	85.592*** (0.4189)
RISK AVERSION	0.0000 (0.2998)	0.0000 (0.0934)	0.6036*** (0.1196)	-0.0384 (0.1214)	0.2769** (0.1108)
NUMBER OF TRANSACTIONS	-0.2086*** 0.0592	-0.2086*** 0.0592	-0.0047 (0.0758)	0.0058 (0.0770)	-0.0847 (0.0703)
PERIOD	-0.2821*** (0.0644)	-0.2821*** (0.0644)	-(0.0427) (0.0825)	-0.0462 (0.0838)	-0.0938 (0.0765)
STD. DEV. PRICE	-0.6720*** (0.0330)	-0.6720*** (0.0330)	-0.1739*** (0.0423)	-0.1719*** (0.0429)	-0.1462*** (0.0392)
CONSTANT	113.4637*** (3.153.7)	113.4637*** (2.2462)	1.8583 (2.8771)	4.2803 (2.9209)	5.6838** 2.6662
Observations	784	784	784	784	784
Wald chi	(15): 2405	(15): 6087	(15): 50710	(15): 46168	(15): 61575
Prob > chi2	0.0000	0.0000	0.0000	0.0000	0.0000
*** p<0.01, ** p<0.05, * p<0.1					

Table X below shows the results for two specifications and two estimations techniques of our equation (4). Models 1 and 2 do not consider interaction between mean permit price and each firm's type. Models 2 and 4 include implementation of instrumental variable procedure to correct for possible bias caused by permit price endogeneity. In all models we have eliminated the first two periods to allow for learning. We also estimated the specified models excluding observations from sessions where one individual went bankrupt. Because the results turned to be robust in all these estimations, we present results based on the use of all sample observations.

The results allow us to reject Hypothesis 4. The experimental evidence suggests that subjects' emissions in a transferable emissions permit system are lower when the aggregate supply of permits is reduced along with the monitoring effort so as to allow for incomplete enforcement.

Table X. Random Effect Models of Individual Emissions in Market Treatments

	Model 1 (GLS regression)	Model 2 (G2SLS IV regression)	Model 3 (GLS regression)	Model 4 (G2SLS IV regression)
TREATMENT INDUCES VIOLATION	-1.059***	-1.059***	-1.059***	-1.059***
	(0.233)	(0.239)	(0.229)	(0.179)
TYPE2	-0.008	-0.012	0.893	-2.599
	(0.318)	(0.330)	(1.417)	(12.190)
TYPE3	-1.807***	-1.811***	-0.058	40.269
	(0.294)	(0.338)	(1.186)	(28.902)
TYPE4	-1.535***	-1.534***	-1.367	14.698
	(0.379)	(0.333)	(1.393)	(20.176)
AVERAGE PERIOD PRICE	0.010**	0.008	0.018	0.164
	(0.005)	(0.006)	(0.012)	(0.147)
RISK AVERSION	-0.152*	-0.154**	-0.148*	-0.104
	(0.082)	(0.074)	(0.079)	(0.115)
*PRICE*TYPE2*			-0.011	0.035
			(0.016)	(0.143)
*PRICE*TYPE3*			-0.021	-0.499
			(0.013)	(0.342)
*PRICE*TYPE4*			-0.002	-0.190
			(0.015)	(0.236)
CONSTANT	6.458***	6.629***	5.766***	-7.019
	(0.746)	(0.808)	-1.190	-13.240
Number of Observations	784	784	784	784
Number of clusters	98	98	98	98
R – sq:				
Within	0.0050	0.0050	0.0039	0.0002
Between	0.4507	0.4508	0.4702	0.2398
Overall	0.3227	0.3227	0.3360	0.1195
Wald chi2	(6): 131.20	(6): 76.38	(9): 162.63	(9): 137.44
Prob > chi2	0.0000	0.0000	0.0000	0.0000

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$.

5. Conclusions

In this draft we present preliminary results of the first of a series of experiments that we are running to test hypothesis with respect to profit maximizing firms in environmental regulatory frameworks.

Our main interest with these hypothesis was to test whether if, as theory predict, a regulator can induce a certain level of emissions to a set of heterogeneous firms by altering the level of the monitoring probability and the level of the individual emission standard accordingly, when emission standards is the regulatory instrument, or by altering the monitoring probability and the number of emission permits supplied to the market, when the regulatory instrument is emission tradable permits. Because the structure of the penalty function is central to the theoretical cost-effectiveness of enforcing emission standards and tradable permits, we test the above issue with linear marginal penalties and increasing marginal penalties.

Our results with this first set of data indicate that the theory predicts well the experimental behavior of subjects in the case of emission standards. In this case, the level of individual emissions is only a function of the marginal benefits of emissions and particularly it is not a function of the combination of monitoring probability and the standard.

In the permits experiments the results are less close to what the theory predicts. In this case, the first result that we observe and that theory does not predict is bankruptcy. A subject went bankrupt in one of the two treatments in 4 of the 8 experiments that we ran. If we do not take this into account, we find that emissions seem to be affected only by the price of the permits. But if we drop all the observations of the groups in which a subject went bankrupt in a given treatment, we obtain the predicted result that emissions are a function of the types of the firms, but at least for Treatment 1, the differences in the level of emissions between types is affected by the treatment. Another result obtained in this last case is that the level of risk aversion of the subjects explains their differences in emissions, and this is not irrespective of all treatments and types.

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