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COMPLEMENTARITY OF INSPECTIONS AND PERMITS AS LEVERAGES FOR CAPPING EMISSIONS: EXPERIMENTAL EVIDENCE

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Abstract: Recent analysis on the cost-effectiveness of inducing perfect compliance in cap and trade programs is based on the possibility that a regulator has of inducing each individual firm to emit the same level of pollution by altering the supply of permits and the monitoring probability according to theoretical models that assume rational and risk-neutral agents. In this paper we test this possibility based on a series of laboratory experiments. Contrary to what theory predicts, our experiments suggest that a regulator cannot manipulate the supply of permits and the monitoring probability as suggested by these models and keep the level of emissions of each individual firm constant. This result does not depend on whether or not we control for risk aversion. Policy implications are discussed.

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

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

1. Introduction

Cost-effectiveness has been an important criterion for policy design in the environmental economics literature. Notwithstanding, the literature has only recently dealt with the use of non-compliance as a possible cost-decreasing strategy for regulators to attain the desired policy target. Stranlund (2007) was the first to address this issue for the case of a cap and trade program. The question he addressed is the following: If a regulator wants to cap the aggregate level of emissions from a set of *n* firms at a certain level *E*, what would be cheaper: to set the legal cap at *E* and perfectly enforce the program, or to set the legal cap at a lower level, but allow violations such that the aggregate level of emissions is *E*? Because in both alternatives each individual firm emits the same amount, the differences between both strategies is given by the monitoring and sanctioning costs borne by the regulator. Stranlund shows that (a) whether it is cost-effective or not for the regulator to induce perfect compliance depends on the relative costs of monitoring and sanctioning firms, which in turn depends on the fine structure, and (b) if the regulator can choose the fine structure, and it can observe the perfect-compliance equilibrium price of the permits market, inducing perfect compliance with a marginal penalty tied to the equilibrium price of permits minimizes the expected costs. Arguedas (2008) proved that the condition under which is cost-effective for a regulator to induce a single firm perfect compliance to an emission standard is the same as in the case of tradable permits, and that if the regulator can choose the penalty structure cost-effectiveness call for perfect compliance.

In Caffera and Chávez (2011), we proved that these conclusions are also valid for the case in which a regulator caps the emissions of *n* firms with emission standards, and not only abatement but also monitoring and sanctioning costs differ between firms. We also compare the total cost of a cap and trade program with that of an expected cost-minimizing allocation of abatement responsibilities (emission standards) and monitoring probabilities. This is done for the case in which, given the relative cost of sanctioning and monitoring each firm, it is cost-effective for the regulator to induce perfect compliance, and for the case in which it is not. Our results show that (a) when it is cost-effective to induce perfect compliance, a cap and trade program does not minimize the total cost, unless monitoring costs are the same across firms, or the marginal penalty for violations is constant, and (b) when it is not cost-effective to induce perfect compliance, the conditions under which a cap and trade program minimizes total costs are implausible.

A fundamental tool for the cost comparison of inducing perfect compliance or not in these models is the possibility that a regulator has to induce each individual firm to emit the same level of pollution by changing the aggregate supply of emission permits (or the emission standards) and the monitoring probability according to the assumption of cost-minimizing, risk-neutral firms. In this paper, we present results of a series of laboratory experiments designed to test this possibility. We explore the compliance behavior of firms when the regulator induces perfect compliance and when it induces the same level of emissions as in the perfect compliance case, but allowing a certain level of violation. We do this for two different regulatory programs: cap and trade and emissions standards. Our experiments suggest that, contrary to what theory predicts, a regulator with perfect information on the firms abatement cost functions, cannot induce the same level of individual emissions manipulating the supply of permits (or the emission standards) and the monitoring probability as suggested by a theoretical model based on rational, risk-neutral agents.

Several aspects of the enforcement of cap and trade systems and individual emissions standards have been previously examined in the experimental literature. Among these aspects are the existence and extent of a direct and indirect (through the permit price) effect of enforcement on emissions trading programs (Murphy and Stranlund, 2006), the possibility of targeting enforcement in emissions trading programs and emissions standards programs (Stranlund and Murphy, 2007), the effect of environmental framing (Cason and Raymond, 2010), the perception of policy fairness as a driver of the subjects' truthfulness in emissions reports and compliance behavior (Cason and Raymond, 2011), and the level of violations, emissions, and prices of permits in the context of dynamic enforcement, banking and random emissions shocks (Cason and Gangadharan, 2006). Nevertheless, none of these papers were designed to test the theoretical complementarity of inspections and emission permits/standards as leverages for capping emissions. Testing this complementarity in the lab is the objective of this work.

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

2. Hypotheses

In this section we present the main hypotheses that we evaluate with our laboratory experiments. These follow directly from the works of Stranlund (2007), Arguedas (2008) and Caffera and Chávez (2011), and we refer the reader to these papers and others cited in the following paragraphs for a detailed exposition of the conceptual framework from which our hypotheses are derived.

Our first hypothesis concerns a system of tradable pollution permits. Assume a perfectly competitive system of tradable pollution permits that is enforced by random inspections and fines. Denote π the probability that a firm is inspected. The amount of pollution emitted by a firm is denoted q and the quantity of permits demanded l. We assume throughout the paper that a violation (defined as $q > l$) is penalized according to the following penalty function: $f(q - l) = \varphi(q - l) + (\gamma/2)(q - l)^2$, with $\varphi > 0$ and $\gamma > 0$. Assume further that there are n firms in the permits market, L_0 represents the number of permits supplied, and Q represents the desired cap of emissions. If π is chosen such that every firm complies, then the equation $\sum_{i=1}^{n} l_i(p,\pi,\varphi,\gamma) = L_0 = Q$ represents the full compliance equilibrium condition of the market for permits. In this condition, $l_i(p,\pi,\varphi,\gamma)$ represents the firm's i permits demand function. (See Malik (1990), and Stranlund and Dhanda (1999) for the derivation of $l_i(p,\pi,\varphi,\gamma)$ and the signs of its partial derivatives).

Now assume that the regulator asks itself whether it is less costly to decrease the quantity of permits supplied to the market and the probability of inspection such that the individual level of emissions of each firm remains constant (and therefore the aggregate level of emissions remains equal to the cap Q) but there is noncompliance. This requires the regulator to adjust the supply of permits and the monitoring probability such the equilibrium price of the permits market remains constant at its full compliance equilibrium level (see

Stranlund and Dhanda, 1999). Moreover, if emissions remain constant, abatement costs will not vary. Therefore, the difference in costs from inducing compliance or not will come from monitoring and sanctioning costs. What is the relative amount in which the regulator must decrease the supply of permits and decrease the monitoring probability such that the equilibrium price of the pollution permits remains at the equilibrium level? Towards answering this question, and from the full compliance equilibrium condition of the market for permits, it is easy to show that

$$\frac{\partial p}{\partial L_0} = \frac{1}{\sum_{i=1}^{n} \frac{\partial l_i}{\partial p}} < 0$$

and that

$$\frac{\partial p}{\partial \pi} = -\frac{\sum_{i=1}^{n} \frac{\partial l_i}{\partial \pi}}{\sum_{i=1}^{n} \frac{\partial l_i}{\partial p}} > 0 .$$

Writing the equilibrium price of permits as $p(L_0, \pi)$, totally differentiating it, imposing $dp = 0$, and using the above two expressions, we obtain $\frac{d\pi}{dL_o} = \frac{1}{\sum_{i=1}^{n} \frac{\partial l_i}{\partial \pi}} > 0.$[1] From existing results in the literature, we know that $\frac{\partial l_i}{\partial \pi} = \frac{f'}{\pi f''}$. Therefore,

$$\frac{d\pi}{dL_o} = \frac{1}{\sum_{i=1}^{n} \frac{f'}{\pi f''}} = \frac{\pi f''}{n f'} > 0, \qquad (1)$$

where f' and f'' are the first and second derivative of the penalty function. Now we are ready to state our first hypothesis:

[1] This result holds under the assumption of a perfectly competitive market for pollution permits. If this is not the case, $\frac{\partial l_i}{\partial \pi} = \frac{f'}{\pi f''} + \frac{\frac{\partial p}{\partial \pi}}{\pi f''}$.

Hypothesis 1: *Under a system of tradable pollution permits the regulator can maintain the individual level of emissions constant by altering the aggregate supply of permits and the monitoring probability according to equation* (1).

In a similar fashion, under a system of emission standards, the regulator can decrease the level of emission standards and the monitoring probability such that the level of emissions of each individual firm, and therefore the desired aggregate level, remains constant. Let $-c'(q_i)$ denote the marginal abatement cost function of firm *i*. The optimal choice of emissions in a system of emission standards is given by the condition $-c'(q_i) = \pi_i f'(q_i - s_i)$, which implicitly defines the firm's optimal choice of emissions as a function of the monitoring probability π_i and the emission standard s_i, $q_i = q_i(\pi_i, s_i)$ (see Harford (1978), Garvie and Keeler (1994) and Malik (1992)). Totally differentiating this function, and imposing $dq_i = 0$, we obtain $\frac{d\pi_i}{ds_i} = \frac{-\frac{\partial q_i}{\partial s_i}}{\frac{\partial q}{\partial \pi_i}}$. Substituting the numerator and denominator for expressions obtained from the condition above (see Caffera and Chávez (2011)), we obtain:

$$\frac{d\pi_i}{ds_i} = \frac{\pi_i f''}{f'} > 0 \qquad (2)$$

We are now ready to state our second hypothesis:

Hypothesis 2: *Under a system of emissions standards the regulator can maintain the individual level of emissions constant by altering the individual emission standards and monitoring probabilities according to equation* (2).

3. Experimental Design and Procedures

3.1. Experimental Design

We framed the experiments as a neutral production decision of an unspecified fictitious good *q*, from which the subjects obtained benefits. Every subject had a production capacity of 10 units (whole numbers), but the benefits of production from these units differ between subjects (see Table 1). The four marginal benefits (obtained from Cason and Gangadharan (2006)) gave place to four "types" of subjects.

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

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

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

We constructed 4 different treatments for these experiments, varying the following variables: (1) the regulatory instrument (standards / tradable permits) and (2) the level of the standards / the number of permits supplied and (3) the monitoring probability.

3.1.1. Tradable permits

In the permits experiments, subjects had to possess a permit in order to be legally able to produce one unit of the good. Consequently, subjects had to decide how much to produce of the fictitious good and how many permits to buy or sell. In order to buy or sell permits, subjects participated in a double-auction market, one permit at a time. A market was formed by 8 subjects, 2 of each type. After their decision, at the end of each period, the subjects were audited with a known homogeneous predetermined and exogenous probability π. If audited, the number of units produced by the subject i in that period (q_i) was compared with the number of permits possessed by the subject i (l_i) at the end of the period. If the level of production chosen was higher than the number of permits possessed, the subject was automatically fined according to the penalty function described in section 2. The subjects had the information on the probability of inspection that they faced and on the marginal fine for every level of violation in their screens at every moment before making their decisions.

We constructed 2 treatments for the case of markets for permits (see Table 2). In Treatment M1, the total number of tradable permits supplied to each group of 8 subjects was 40. The initial allocation was 4 permits for subjects of type 1 and 2, the prospective buyers, and 6 permits for subjects of type 3 and 4, the prospective sellers. We chose this initial allocation of permits as opposed to a homogeneous allocation (5-each) as a way to foster the market activity (the number of expected trades is 10). The enforcement parameters took the values $\varphi = 100$, $\gamma = 66{,}67$ and $\pi = 0.6$. This probability is sufficient to induce all types of firms to comply with their permit holdings under the assumption of risk-neutrality. The resulting perfect-compliance equilibrium price of the market is expected to be between 74 experimental pesos (E$) and E$ 80. In contrast, Treatment M2 induces violations of the permits holdings. This is done by decreasing the total number of permits supplied to 20 (initial allocations and expected number of trades halved) and by decreasing the monitoring probability from 0.6 to 0.30. With this parameterization, the Treatment M2 induces the same equilibrium price of permits and individual level of emissions as the treatment M1 does.[2] Hence, the expected level of aggregate emissions remains in 40 units. This is a unique feature of our design. Another unique feature of our design is that each subject participates in both the M1 and M2 treatments.

3.1.2. Standards

In the standards experiments subjects faced a maximum allowable level of production (the standard), and had to decide how much to produce. The auditing procedure was exactly the same as in the case of tradable permits; except that in the case of standards a violation is defined as $q_i - s_i > 0$, where s_i is legal maximum level of production (the standard) set for its type. Similarly to the case of tradable permits, we constructed 2 treatments for the case of emission standards. These are labeled S1 and S2 in Table 2. In treatment S1, the emission standards are 7, 6, 4 and 3 for firms' types 1 to 4, respectively. The monitoring probabilities are 0.6, 0.65, 0.63 and 0.66 (violations are fined with the same penalty function; $\varphi = 100$ 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.[3] In Treatment S2, the standards are decreased for every type of subject, so that the aggregate cap of emissions is 20, but monitoring probabilities are decreased so as to keep the predicted level of emissions at 40 units, the same level as in Treatment S1. Therefore, Treatment S2 induces violations.

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

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

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 (φ)</th><th>Gamma (γ)</th></tr>
<tr><td>M1</td><td rowspan="2">Tradable Permits</td><td>0.60</td><td>0.60</td><td>0.60</td><td>0.60</td><td rowspan="4">100</td><td rowspan="4">66.66</td><td>Compliance</td><td>40</td><td rowspan="2">80 - 74</td><td rowspan="4">40</td></tr>
<tr><td>M2</td><td>0.30</td><td>0.30</td><td>0.30</td><td>0.30</td><td>Violations</td><td>20</td></tr>
<tr><td>S1</td><td rowspan="2">Standards</td><td>0.60</td><td>0.65</td><td>0.63</td><td>0.66</td><td>Compliance</td><td>40</td><td>Type 1 = 7
Type 2 = 6
Type 3 = 4
Type 4 = 3</td></tr>
<tr><td>S2</td><td>0.24</td><td>0.26</td><td>0.32</td><td>0.31</td><td>Violations</td><td>20</td><td>Type 1 = 4
Type 2 = 3
Type 3 = 2
Type 4 = 1</td></tr>
</table>

3.2. Experimental Procedures

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

Participants were recruited from the undergrad student population of the University of Montevideo, the University of the Republic, the Catholic University, ORT University and UDE University, all in the city of Montevideo, Uruguay. A total of 90 different subjects participated in the sessions of the experiments whose results we report in this paper. Each experimental session consisted of either two market-treatments or two standards-treatments. Fifty (50) subjects participated in the permits sessions and 55 subjects in the standards sessions. Fifteen (15) subjects participated in both types of sessions. The maximum number of sessions that a subject participated in was three. We allocated the standards and permits sessions evenly in the mornings and afternoon, and on different days of the week to minimize any possible selection bias.

Subjects participating in a session were randomly assigned into groups of 8 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 participated in another treatment. In one treatment all the subjects played the compliance treatment (either M1 or S1), and in the other treatment all the subjects played the violation treatment (either M2 or S2). The order of treatments differed between groups in a session. Approximately half of the people that showed up in the room for that session played the compliance treatment first, and the other half played the violation treatment first. More specifically, of the 61 experimental subjects that participated in the standards sessions, 24 played S1 (the compliance treatment) first and 37 played S2 (the violation treatment) first. Of the 64 subjects that participated in the permits sessions, half played T1 (the compliance treatment) first.

Before the beginning of the experiments, instructions were handed out to subjects.[4] The instructions were read aloud and questions were answered. Prior to the first round of the first treatment, subjects played 2 trial rounds of the first treatment in the standards sessions, and 3 trial rounds of the first treatment in the permits sessions. In the standards sessions each period lasted 2 minutes. In the permits sessions each period lasted 5 minutes, to give subjects time to make their bids, asks, and to decide how many units to produce and how many permits to buy or sell.

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

With one exception, the different types of firms incur in no losses in any given period and treatment, if behaved as an expected profit-maximizer. The exception is given by firms of type 4 in treatment S2. These lose E$ 15 if behaved as an expected profit-maximizer and inspected. Because of this, all subjects were given an initial allocation of

[4] The instructions for the tradable permits experiments are available at http://www2.um.edu.uy/marcaffera/investigacion/InstructionsTradablePermits.pdf

525 E$. This endowment allowed firms of type 4 to cover 35 periods of losses if they behaved as expected-profit maximizers, and are audited and fined in every period in this treatment. Also, the endowment allows all firms to finance a number of units of violation in excess of the expected violation. In the compliance treatments, for example, the endowment allows types 1 and 2 firms to finance losses due to any level of violation, and allows types 3 and 4 firms to finance 4 and 5 units of violation.

Subjects were paid around 7 US$ for showing up on time in the experiments sessions and earned more money from their participation in the experiment.[5] 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. Total payments ranged between US$ 30 and US$ 5 in the standards sessions, with a mean value of US$ 19, a median of US$ 18 and a standard deviation of US$ 4. For the case of the tradable permits sessions, payments ranged between US$ 24 and US$ 14, with a mean value of US$ 20, a median of US$ 20 and a standard deviation of US$ 2.

4. Results

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

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

4.1. Overall results of the market experiments

Table 3 presents the summary statistics of key variables in the market experiments with the values that theory predicts for the case of cost-minimizing, risk-neutral agents.[6] It can be seen first that the average price of permits traded is approximately as predicted in treatment M1, but it is above its predicted level in treatment M2 (the violation treatment). In the treatment M1, the average price of permits traded was E$ 81.2, very close to its predicted full-compliance level, and below the expected penalty for a one unit violation (E$ 100). Still, violations are slightly above zero, on average, for the four types of firms. However, the mode of violations is zero for the four types of firms (69% of all the observations) and 90% of violations are zero or one units. Moreover, 41% of the subjects complied in every round, 84% of the subjects had an average violation level equal to 1 or less, and 98% had an average violation of 1.7 or less.

In the treatment M1 we can note further that although, on average, emissions and permits holdings are higher than predicted for the firms of types 2, 3 and 4, and are smaller than predicted for firms of type 1, the modal behavior is consistent with the theory. We cannot say the same thing for the case of the treatment M2, though. In this case, the average violations are below their predicted values, but also de mode violations are below the theoretical values (for three of the four types of firms). In the case of types 1 and 2 firms (the expected buyers of permits) the lower than predicted violation levels seems to be

[6] To perform the calculations we dropped the first two periods of each treatment from the sample. This was done to avoid possible effects due to learning. In the case of the market treatments, we also dropped from the sample a group of eight subjects from the sample of eight groups that played M1 and M2. This was due to bankruptcy of one subject in period 2 of the treatment M2 in this group that we exclude.

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

Market Treatment 1 - Compliance Treatment		Mean Price per Period	Number of transactions per period	**Type 1 (l_0=4)**			**Type 2 (l_0=4)**			**Type 3 (l_0=6)**			**Type 4 (l_0=6)**		
				q	**l**	**v**	**q**	**l**	**v**	**q**	**l**	**v**	**q**	**l**	**v**
Theory		**74-80**	**10**	**7**	**7**	**0**	**6**	**6**	**0**	**4**	**4**	**0**	**3**	**3**	**0**
Experiments	Mean	81.2	9.1	6.6	5.7	0.9	6.6	6.3	0.3	4.5	4.2	0.3	4.3	3.9	0.4
	Std. Dev.	3.7	1.7	1.3	1.6	1.8	1.0	0.8	0.6	1.1	0.9	0.6	1.5	1.3	0.7
	Mode	79.5/84.2	8	7	6	0	6	6	0	4	4	0	3	3	0
	Median	80.7	9	7	6	0	6	6	0	4	4	0	4	3	0
	# Obs.	56	56	112	112	112	112	112	112	112	112	112	112	112	112
Market Treatment 2 - Violation Treatment		Mean Price per Period	Number of transactions per period	**Type 1 (l_0=2)**			**Type 2 (l_0=2)**			**Type 3 (l_0=3)**			**Type 4 (l_0=3)**		
				q	**l**	**v**	**q**	**l**	**v**	**q**	**l**	**v**	**q**	**l**	**v**
Theory		**74-80**	**6**	**7**	**4**	**3**	**6**	**3**	**3**	**4**	**2**	**2**	**3**	**1**	**2**
Experiments	Mean	105.5	5.0	5.7	3.0	2.7	4.8	2.7	2.0	3.7	2.2	1.6	3.6	2.1	1.5
	Std. Dev.	9.5	1.8	1.6	1.6	2.1	1.8	1.0	1.9	1.6	1.2	1.1	1.4	0.9	1.2
	Mode	87.2/108	5	5	4	1	4	3	2	3	3	2	3	2	1
	Median	107	5	5	3	2	4	3	2	3	2	2	3	2	1
	# Obs.	56	56	112	112	112	112	112	112	112	112	112	112	112	112

driven by lower than predicted emission levels. (Although the modal quantities of permits demanded are equal to the theoretical values, a similar frequency of observations showed a one unit less than the predicted demand for permits). For the case of firms of types 3 and 4, the expected sellers, it could be said that the average and the mode values of permit holdings are higher than those predicted (while the respective values for the quantity produced are closer to their theoretical values). We can conclude that expected sellers tended not to sell as many permits as predicted, which is consistent with observing a lower than expected number of transactions per period.

In general, these results are consistent with previous works. For example, Murphy and Stranlund (2007) and Stranlund, Murphy and Spraggon (forthcoming) found that subjects marginally violate on average in treatments that induce compliance, but in treatments that induce violations average violations are lower than predicted. Murphy and Stranlund (2007) also found that expected buyers violate more than expected sellers. Raymond and Cason (2010) found that subjects under-report "well below" the level of what would be predicted for a risk-neutral or even risk-averse subject.

4.2. On the complementarity of inspections and permits as leverages for pollution control

Our first hypothesis to be evaluated affirms that *under a system of tradable pollution permits the regulator can maintain the individual level of emissions constant by altering the aggregate supply of permits and the monitoring probability according to equation* (1).

Although the dependence of observations precludes us from performing a formal t-test, it can be seen in Table 3 that on average the level of emissions (q) of the different types of firms were not the same between treatments. Moreover, emissions were, on average, higher for all four types of firms in the treatment M1 than in the treatment M2.

To perform a formal test for Hypothesis 1, we estimate a linear random effects model of the form

$$e_{it} = f(VIOLATION\ TREATMENT_{it}, FIRM\text{-}TYPE_{i}, PERIOD_{t}, OTHER\ CONTROLS) \qquad [1]$$

where e_{it} is the level of emissions of subject *i* in round *t*, *VIOLATION TREATMENT* is a dummy variable equal to 1 if the treatment is M2, and equal to zero if the treatment is M1; *FIRM-TYPE* is a set of three dummy variables to control for firm type, according to marginal abatement costs' functions, and $PERIOD_t$ is another indicator variable for each of the eight periods used in these regressions. The other controls employed in the regressions depend of the specification. In specification 1, we included a dummy variable to control for the order of the treatments (*First* equals 1 if the treatment to which the observation belongs was played first), and its interaction with "*Violation Treatment*". This interaction was included as a way to disentangle any possible difference in the order effect between violation and compliance treatments. In specification 2, we included a set of dummy variables to control for possible group (of eight subjects) effects. In both cases we included random individual effects, and we clustered errors by subjects.

Finally, we ran both specifications with and without controlling for risk aversion. When we control for risk aversion we include one indicator variable for each possible level of risk aversion of the subjects. We opted for this approach instead of including a variable measuring the level of risk aversion of the subjects to allow non-linear effects of risk aversion on the individual choices of emissions. We constructed the level of risk aversion for each subject using a Holt and Laury (2002) type of test at the end of each session. 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). The lottery was between an amount of money lower than the certain value in option A and an amount higher. The probability of winning the higher prize varied from 0.1 to 1 between choice 1 and 10. Our measure of risk aversion is the number of the choice in which the subject switches to Option B. It then varies between 1 and 10, with 10 being the highest value of risk aversion. (In the tenth choice the higher prize of the lottery, higher than the certain amount in option A, has a probability equal to 1, so every subject should choose the lottery in the 10^{th} choice). A risk neutral subject should switch from option A (the certain amount) to option B (the lottery) in the 5^{th} or 6^{th} choice.[7]

It can be seen in Table 4 that the results with respect to our variable of interest indicate that, everything else equal, the level of individual emissions is lower in the violation treatment (M2) than in the treatment that induces compliance (M1). Recall that the aggregate supply of permits and the monitoring probability are lower in M2 than in M1, but such that the individual and aggregate levels of emissions are the same under the assumption of risk-neutral, cost-minimizing subjects (see Table 2). Notwithstanding, we find that, depending on the specification used, individual emissions are approximately between 1 and 1.3 units lower in the violation treatment M2 than in the compliance treatment M1. This result leads us to reject Hypothesis 1.

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[7] We do not include the price as an explanatory variable because this is a fundamental channel by which the enforcement regime affects emissions and violations choices in a cap and trade system. Moreover, according to theory (Malik, 1990) and experimental evidence (Murphy and Stranlund, 2006) an increase in the enforcement effort by the regulator does not directly affect the level of emissions of the firms, but only through the permit's price. See also section 4.4 below.

Table 4: Linear Random Effect Models for Hypothesis 1 (Market Treatments)

Dependent variable: Level of emissions	(1)	(2)	(3)	(3)
	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
Violation Treatment (M2)	-1.289*** (0.296)	-1.224*** (0.303)	-1.047*** (0.181)	-0.985*** (0.208)
First	-0.383 (0.255)	-0.147 (0.283)		
Violation Treatment * First	0.520 (0.439)	0.458 (0.477)		
Type 2	-0.451 (0.316)	-0.240 (0.271)	-0.451* (0.264)	-0.361 (0.305)
Type 3	-1.987*** (0.316)	-1.934*** (0.327)	-1.987*** (0.288)	-1.998*** (0.301)
Type 4	-2.183*** (0.299)	-1.789*** (0.283)	-2.183*** (0.317)	-1.829*** (0.286)
Period 4	-0.071 (0.113)	0.049 (0.123)	-0.071 (0.113)	0.049 (0.123)
Period 5	-0.018 (0.108)	0.122 (0.116)	-0.018 (0.108)	0.122 (0.116)
Period 6	0.098 (0.110)	0.244** (0.111)	0.098 (0.110)	0.244** (0.111)
Period 7	0.089 (0.158)	0.244 (0.189)	0.089 (0.158)	0.244 (0.190)
Period 8	0.054 (0.129)	0.159 (0.127)	0.054 (0.130)	0.159 (0.127)
Period 9	0.152 (0.162)	0.354** (0.163)	0.152 (0.163)	0.354** (0.163)
Period 10	0.098 (0.153)	0.134 (0.160)	0.098 (0.153)	0.134 (0.160)
Risk aversion = 5		0.594 (0.431)		0.942 (0.724)
Risk aversion = 6		-1.177** (0.529)		-0.671 (0.649)
Risk aversion = 7		-0.884* (0.502)		-0.406 (0.638)
Risk aversion = 8		-1.212** (0.493)		-0.639 (0.629)
Risk aversion = 9		-0.853 (0.628)		-0.361 (0.662)
Risk aversion = 10		-1.251** (0.564)		-0.503 (0.769)
Group 2			-0.359 (0.383)	-0.349 (0.581)
Group 3			-0.500 (0.341)	-0.540 (0.451)
Group 4			-0.555* (0.336)	-0.847** (0.408)
Group 5			-0.656** (0.305)	-0.654 (0.477)
Group 6			0.031 (0.513)	-0.593 (0.534)
Group 7			-0.914** (0.426)	-0.745 (0.530)
Constant	6.753*** (0.255)	7.200*** (0.436)	7.011*** (0.311)	7.236*** (0.432)
N	896	656	896	656
N_clust	56	41	56	41
* p<0.1, ** p<.05, *** p<.01				

Interestingly, the rejection of Hypothesis 1 does not depend on whether or not we include indicator variables to control for the subjects' different levels of risk aversion.[8] It can also be seen in Table 4 that the group of people with which the subject interacts seems to be important to explain its level of emissions, in some cases. Furthermore, controlling for group effects make the risk aversion variables statistically insignificant. This suggests that there may be some interdependencies among subjects' behaviors in a group that may be correlated to the level of risk aversion of at least some of the subjects in that group.

Finally, we do not observe an order effect statistically different from zero in the level of emissions.[9]

4.3. The effect of inducing non-compliance on the price of permits

We have provided experimental evidence against the hypothesis that a regulator has the possibility of inducing a given level of emissions to each of the regulated firms by manipulating the supply of permits and the monitoring probability in the manner suggested by the theoretical models of enforcement in cap and trade schemes. Nevertheless, we have not explored the channel by which this effect takes place. One obvious channel is the price

[8] When we include the risk aversion dummies we lose the observations of 14 subjects that made inconsistent choices in the Holt and Laury test. We also dropped from the sample the observations of an additional individual that revealed an extreme preference for risk in the test (opted for the lottery in the ten choices, risk aversion = 1) but did not behave consistent with this choice, biasing the estimation of some of the other risk aversion dummies. Among the remaining 41 subjects, the mean level of risk aversion is 7.4, the median 7.0, the minimum 4 and the maximum 10.

[9] We did observe an order effect in the level of violations. In estimations not shown in this paper (available upon request) we found that the order of the treatment had a statistically significant effect on the observed *level* of the violations of the compliance treatment (M1). Subjects tended to violate more in M1 if the violation treatment (M2) was played first, as compared to what they violate in M1 if M1 was played first. On the other hand, the level of violations of the firms in the M2 treatment did not depend on whether this treatment was played before or after the treatment M1. The order effect in the compliance treatment may be seen as an "anchoring effect" (Tversky and Kahnema, 1974; Ariely, et al, 2003) in enforcement regimes. In terms of policy implication, it may suggest that a regulator that previously allowed violations in a cap and trade program needs a relatively more stringent enforcement strategy and/or more time to induce perfect compliance, than a regulator that induced perfect compliance in the first place.

of the pollution permits. As it is well known, the price of the pollution permits determines the level of emissions of the firms that participate in a cap and trade scheme, together with the individual characteristics of the firms (particularly, their marginal pollution benefits) (Stranlund and Dhanda, 1999). To explore this channel, we ran two additional regressions whose results we present in the Table 5. The panel variable in these regressions is the group of 8 subjects that comprise the market, and the time variable is the round. In the first of these regressions, whose results we show in the second column of Table 5, the dependent variable is the average price at which the permits were traded. We can see that, although both treatments were designed to produce the same equilibrium price, the average price of the permits traded was higher in the treatment that induces violation (M2) than in the treatment that induces compliance (M1). The size of this difference depends on the order in which the treatments were played. In the M2 treatment the average price was around 15.5 experimental pesos higher than the average price in the M1 treatment when the M2 was played after the M1 treatment, and it was around 27.8 (= 15.574 - 3.974 + 16.247) experimental pesos higher in the M2 treatment if this was played before M1. Moreover, the order effect has a negative sign for the compliance treatment (-3.974) and a positive sign for the violation treatment (-3.974+16.247). This result suggests that there may be a partial anchoring effect of the price in the first treatment over the price of the second treatment, irrespective of which treatment was played first.

In the third column of Table 5 we show the results of our second additional regression. In this regression the dependent variable is the number of trades in the period. The results can be summarized in the following way. Recall that by design, the number of trades in the M1 treatment should be 10 and in the M2 treatment they should be 6. The results in the third column of Table 5 show that if the compliance treatment M1 is played first there were around 10 trades $(8.643 + 1.417)$ in the M1 treatment but $8.643 - 4.542 \cong 4.1$ in the violation treatment M2. On the other hand, if the violation treatment is played first the number of trades in this treatment is 5.5 ($\cong 8.643 - 4.542 + 1.417$), but the number of trades in the M1 treatment is 8.6.

Table 5: Linear Random Effect Models for Prices and Trades (Market Treatments)

	Dependent variable:	
	Average price of permits traded in the period	Number of trades in the period
Control Variables:	Coefficient (Std. Error)	Coefficient (Std. Error)
Violation Treatment (M2)	15.574*** -5.277	-4.542*** (0.862)
First	-3.974*** -1.164	1.417* (0.770)
Violation Treatment * First	16.247** -6.336	0.469 -1.366
Period 4	0.028 (0.653)	-0.286 (0.559)
Period 5	-0.985** (0.432)	-0.357 (0.453)
Period 6	-1.079 (0.799)	-0.214 (0.547)
Period 7	-2.662*** (0.852)	-0.071 (0.960)
Period 8	-2.642* -1.574	-0.214 (0.470)
Period 9	-4.433*** -1.506	0.071 (0.312)
Period 10	-3.244* -1.719	-0.071 (0.332)
Constant	84.811*** (0.444)	8.643*** (0.859)
N	112	112
N_clust	7	7
* p<0.1, ** p<.05, *** p<.01		

In sum, the number of trades is approximately the one that theory predicts when the treatment is played first, but it is below this theoretical level if the treatment is played second. Moreover, diminishing the supply of permits (and the monitoring probability) decreases the number of trades more than it is predicted (from 10 to 4.1 trades), while increasing the supply of permits (and the monitoring probability) increases the number of trades less than it is predicted (from 6 to 8.6).

It is important to note that, taking together both regressions, we can conclude that the treatment that induces violations (M2) increases the price at which permits are traded independently of the fact that the number of trades are less or equal to what the theory predicts. Even in those cases were the number of trades is what we expected, the prices are higher than expected. It seems that buyers are willing to pay a premium on the price to avoid the possibility of being caught in violation.

4.4 Overall results of the standards experiments

In this subsection we present the results for the standards experiments. Recall that in these experiments the design is similar to the one of the market experiments, but in the standards experiments the regulator sets by fiat the expected-cost-minimizing allocation of emissions in the form of individual standards, instead of trusting a market for pollution permits for this task.[10]

Table 6 compares the summary statistics of key variables in the emissions standards experiments with what theory predicts for the case of cost-minimizing, risk-neutral agents.[11] The results indicate that, on average, subjects violated more than what is predicted by the expected profit maximizer model in Treatment S1; the compliance model. Nevertheless, the modal behavior of subjects was according to theory. At the same time, in

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[10] It is not an issue of analysis here, but for this to be possible, the regulator should perfectly observe the marginal benefits of the firms. The relative advantage of a market for pollution permits to perform the task of allocating emission responsibilities in a cost-minimizing way is based precisely on the fact that regulators do not have perfect information on the firms' marginal benefits of pollution.

[11] We eliminated the observations of six subjects that went bankrupt in a given period of the session. Three of these six subjects were type 4 subjects and two were type 3 subjects (recall that type 3 and 4 subjects were those with lower marginal benefits. The sixth subject was a type 1 subject. In the standards experiments we discarded the observations of each subject instead of the observations of the whole group of eight subjects as we did in the case of the markets experiments because in the case of the standards experiments the subjects do not interact with one another.

the violation treatment (S2), the level of violations turned out to be lower than those predicted by the same model, but only for firm's type 1 and type 3. For the case of subjects of type 2 and 4, the resulting level of violations was higher than predicted, although very close in the case of type 2. If we look to the modes, types 1 and 3 subjects performed as the theory predicts, and types 2 and 4 emitted one unit less than predicted. It seems that the expected profit maximizer model does better in predicting the average behavior of firms in the violation treatment for the case of emission standards than for the case of tradable pollution permits, while it does basically the same job in the compliance treatment.

4.5. On the complementarity of inspections and standards as leverages for pollution control

We now address the test of Hypothesis 2, which states that *under a system of emissions standards a regulator can maintain the individual level of emissions constant by altering the individual emission standards and monitoring probabilities according to equation* (2).

If we look at the average levels of emissions of subjects in the treatment S1 vs. treatment S2 in Table 6, the comparison is not as clean as in the case of tradable permits. For the cases of subjects of type 1 and type 3, the average level of emissions is more than one unit lower under the "*Violation Treatment*" (S2) than under the "*Compliance Treatment*" (S1). However, the average level of emissions for subjects' type 2 and 4 is much closer to the theoretical prediction (0.3 and 0.4 units, respectively).

Table 6: Comparison of predicted results with summary statistics for Emissions Standards Experiments

Treatment S1		Type 1 (s=7)		Type 2 (s=6)		Type 3 (s=4)		Type 4 (s=3)	
		q	**v**	**q**	**V**	**q**	**v**	**q**	**v**
Theory		**7**	**0**	**6**	**0**	**4**	**0**	**3**	**0**
Experiments	Mean	7.7	0.7	6.5	0.5	4.8	0.8	3.7	0.7
	Std. Dev.	1.1	1.1	1.1	1.1	0.8	0.8	1.0	1.0
	Mode	7	0	6	0	4-5	0-1	3	0
	Median	7	0	6	0	5	1	3	0
	# Obs.	104	104	112	112	96	96	88	88
Treatment S2		**Type 1 (s=4)**		**Type 2 (s=3)**		**Type 3 (s=2)**		**Type 4 (s=1)**	
		q	**v**	**q**	**V**	**q**	**v**	**q**	**v**
Theory		**7**	**3**	**6**	**3**	**4**	**2**	**3**	**2**
Experiments	Mean	6.6	2.6	6.1	3.1	3.6	1.6	3.4	2.4
	Std. Dev.	1.3	1.3	2.3	2.3	0.9	0.9	2.2	2.2
	Model	7	3	5	2	4	2	2	1
	Median	7	3	6	3	4	2	3	2
	# Obs.	104	104	112	112	96	96	88	88

To perform a formal test of Hypothesis 2, we estimated a linear random effects model. The specification of our econometric model for the case of standards mimics the specification 1 of our econometric model for the case of tradable permits. We do not run a specification 2 in this case since there is no reason to control for group effects in the standards experiments. Table 7 presents the results with and without controlling for risk aversion.[12]

Our econometric analysis shows that, everything else equal, the level of emissions is around 0.7 units lower in the treatment S2 (the treatment that induces violations with lower emissions standards and monitoring probabilities) than in the treatment S1 (the treatment

[12] Similarly to what we did in the case of tradable permits, in the case of standards we discard the observations of 10 subjects that made inconsistent choices in the Holt and Laury test of risk aversion, and one more subject that revealed an extreme preference for risk in the test (risk aversion = 1) but did not behave consistently with this choice, biasing the estimation of some of the other risk aversion dummies. Among the remaining 39 subjects, the mean level of risk aversion is 7.1, the mean 7.0, the minimum 4 and the maximum 10.

that induces compliance with higher emissions standards and monitoring probabilities). This result is not driven by risk aversion, given that it does not change when we include the risk aversion indicators in the set of controls. Consequently, we reject Hypothesis 2. The result is the same one we obtained in the case of tradable permits. According to this, we can conclude that regulators may not be able to induce the same level of emissions pooling the leverages of the supply of permits or the emission standards and the monitoring probabilities in the quantities suggested by the standard theoretical models of enforcement.

Apart from the main result previously commented it is interesting to note that we do not observe the order or anchoring effect in the case of standards. Both coefficients are not statistically significant, and we cannot reject that the sum of both is equal to zero. On average, the firms tend to violate the same level in both treatments, independently of whether the treatment was played in the first 10 rounds or in the second 10 rounds of the session. Finally, by design, the type of firm affects the level of emissions; firms with lower abatement costs tend to emit less as compared with the highest marginal abatement cost firm type.

Table 7: Linear Random Effect Model (Standards Treatments)

Dependent var.: Level of emissions	Linear Model Coeff. (Std. Err.)	Linear Model Coeff. (Std. Err.)
Violation Treatment	-0.743** (0.360)	-0.722** (0.306)
First	0.275 (0.218)	0.091 (0.194)
Violation Treatment * First	-0.107 (0.530)	0.267 (0.434)
Type 2	-0.843** (0.384)	-1.176*** (0.329)
Type 3	-2.980*** (0.253)	-2.936*** (0.415)
Type 4	-3.625*** (0.296)	-3.663*** (0.349)
Period 4	-0.190 (0.146)	-0.282 (0.180)
Period 5	-0.110 (0.132)	-0.128 (0.166)
Period 6	-0.200 (0.153)	-0.282 (0.192)
Period 7	0.030 (0.172)	-0.141 (0.200)
Period 8	-0.020 (0.147)	-0.179 (0.164)
Period 9	0.050 (0.210)	-0.090 (0.257)
Period 10	0.250 (0.231)	0.141 (0.278)
Risk aversion = 5		-1.899** (0.827)
Risk aversion = 6		-1.644* (0.841)
Risk aversion = 7		-1.251 (0.779)
Risk aversion = 8		-1.570** (0.795)
Risk aversion = 9		-1.434* (0.763)
Risk aversion = 10		-1.856* (0.949)
Constant	7.435*** (0.288)	9.063*** (0.718)
N	800	624
N_clust	50	39
* p<0.1, ** p<.05, *** p<.01		

5. Conclusions

We study the compliance behavior of firms under a system of transferable emission permits and under a system of emissions standards by using economics experiments with different enforcement regimes. We evaluate whether a regulator can induce a given level of emissions on individual firms by different combinations of the aggregate supply of emission permits (or the emission standards) and the monitoring probability, as suggested by the conventional theoretical models of compliance, even though in one combination the regulator induces perfect compliance, while in the other it allows violations.

Our results provide experimental evidence against this possibility in the case of transferable permits. Although we construct two treatments that in theory produce the same equilibrium price (and individual emissions) by manipulating the supply of permits and the monitoring probability, we find that emissions are lower in the violation treatment. Consistent with this result, we also find that in the violation treatment the average price of the permits traded increases.

For the case of emission standards, the results are somewhat less conclusive. Although we do reject Hypothesis 2 using econometric techniques, without controlling for any other difference, the level of emissions of some type of firms remain close to constant between treatments.

If externally valid, these results have direct implications on the relative cost-effectiveness of inducing compliance. According to our results, the aggregate level of emissions is about 10% higher than the predicted level in the compliance treatment, both in transferable permits and emission standards. On the contrary, in the violation treatment emissions are about 10% lower than the predicted level in the market experiment and about the predicted level in the standards experiments. Consequently, the aggregate level of violations is lower than expected in the market violation treatment, but it is about the expected one in the standard violation treatment. The result from the market violation treatment implies two effects on the overall costs of the program working in opposite directions. On the one hand, a program that is designed to allow a certain level of violations would have higher than expected abatement costs because firms would not violate as much as predicted. On the other hand, because firms violate less, the regulator would expend less on imposing fines. Depending on the relative costs of abating emissions and imposing fines, a cap and trade program that is designed to allow a certain level of violations would be more or less costly than a program that is designed to induce perfect compliance.

This observation is not as conclusive in the case of emissions standards. Although we reject Hypothesis 2 on individual emissions, it appears that it might be possible to use the standards along with monitoring effort to keep the aggregate level of emissions constant. Nevertheless, this may or may not imply an opportunity for a cost-effective choice of noncompliance. According to our experiments, this would depend on the relative costs and benefits implied by the reallocation of emissions and violations that occur among the different types of firms.

Albeit less robust, another result that we observe in our experimental settings is a type of anchoring effect when switching enforcement regimes in tradable permits. The order effect does not affect emissions but it affects violations. Firms tend to violate more in the perfect compliance treatment if this is played after a violation treatment (as opposed to be played before). It appears that the effect operates through the permits market. The treatment that induces violations increases the price at which permits are traded independently of the fact that the number of trades are less or equal to what the theory predicts. It seems that buyers are willing to pay a premium on the price to avoid the possibility of being caught in violation. This result deserves to be studied further, as it may suggest that a regulator that previously allowed violations in a cap and trade program may need a relatively more stringent enforcement strategy, or more time, to induce perfect compliance than a regulator that induced perfect compliance in the first place. Puzzlingly, we do not observe the same effect for emission standards.

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